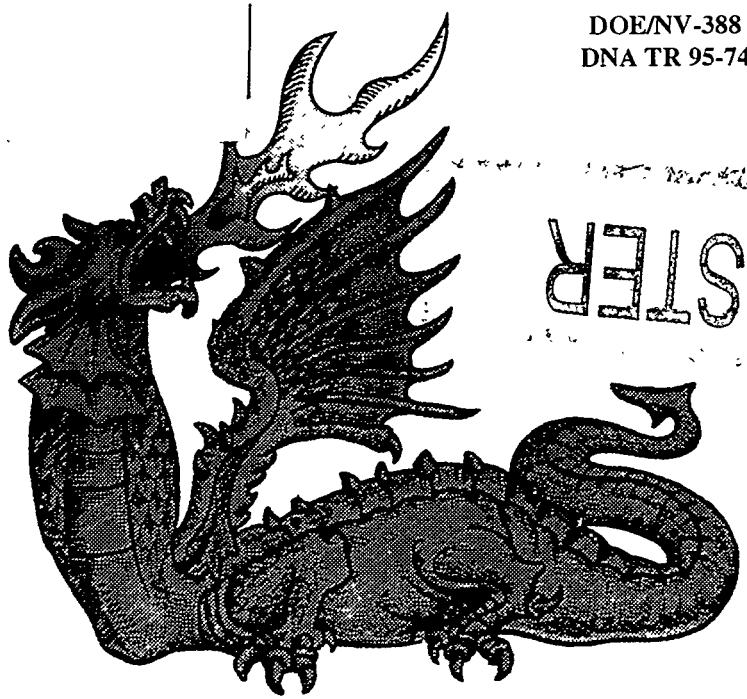
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Caging the Dragon

The Containment of Underground Nuclear Explosions

DOE/NV-388
DNA TR 95-74



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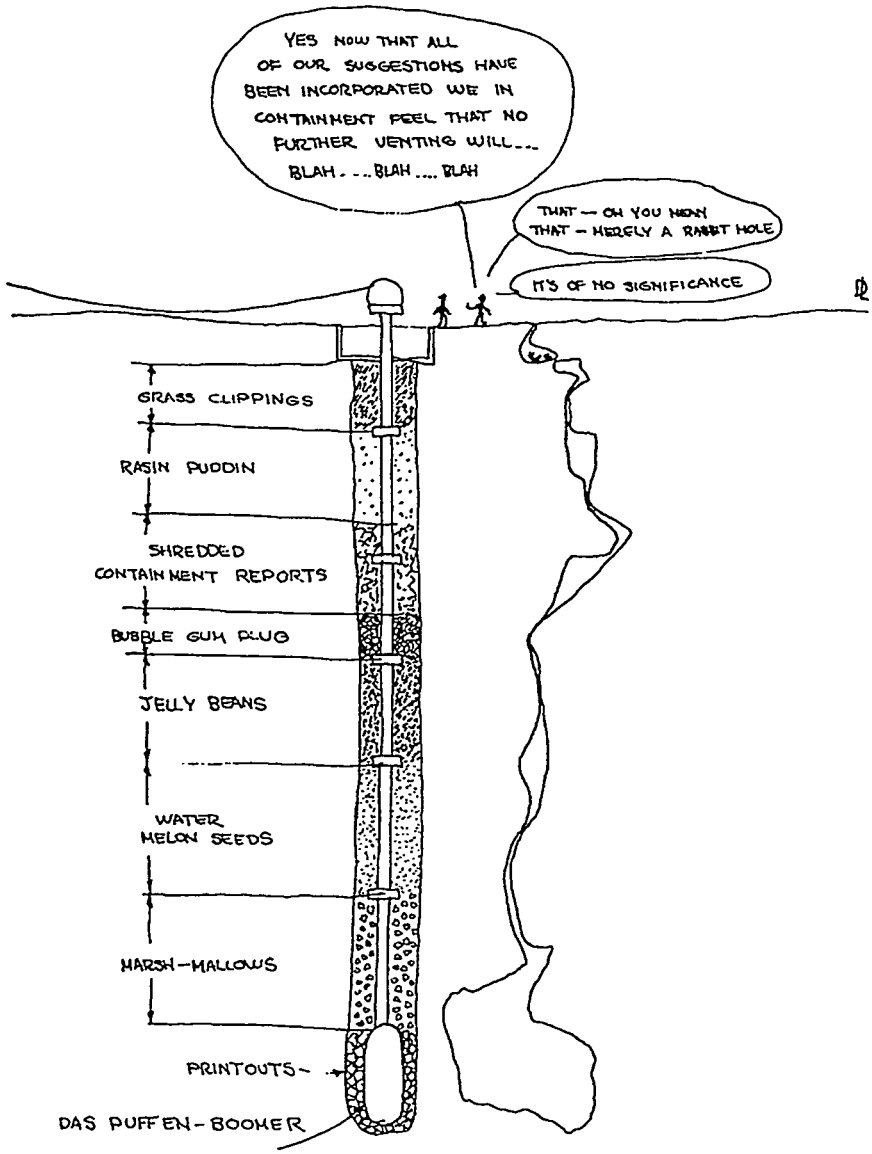
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YES NOW THAT ALL OF OUR SUGGESTIONS HAVE BEEN INCORPORATED WE IN CONTAINMENT FEEL THAT NO FURTHER VENTING WILL... BLAH... BLAH.... BLAH
THAT — OH YOU MEAN THAT — MERELY A RABBIT HOLE
IT'S OF NO SIGNIFICANCE
GRASS CLIPPINGS
RASIN PUDDIN
SHREDDED CONTAINMENT REPORTS
BUBBLE GUM PLUG
JELLY BEANS
WATER MELON SEEDS
MARSH-MALLOWS
PRINTOUTS
DAS PUFFEN-BOOKER

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Preface

Robert Brownlee, in a talk given at the Monterey Containment Symposium on August 26, 1981, said:

"It has been said that there is no such thing as history, only biography. Assuming this to be true, a description of the evolution of containment would contain the story of the people involved - - their experiments, beliefs, motivations, successes, failures, foibles, and idiosyncrasies. We might then be able to understand our current faith and practice, and their origins, in a far better way.

"Even for the earliest moments of containment of underground nuclear tests, when the number of individuals involved in the subject was very few, the complexity of the subject, and the parallel and relatively independent pursuits of Los Alamos and Livermore, make a retrieval of biographical knowledge quite impossible."

In that context, this book is an attempt to approach that impossibility. It has been my pleasure to have had the opportunity to talk with people I know who played a role in the development of our current faith and practice. This book is really theirs, and that is shown in the extensive quotations from people who have spent much of their professional careers dealing with the truly difficult problems encountered. This book does not deal with the formulae and the mathematics, the charts and graphs that make up the structure of the scientific and engineering practice of the containment of underground nuclear explosives. Those things can be found in the documents and reports written by many of the individuals who have worked in the field during the past thirty-five years or so.

Here there are only the recollections, memories, opinions, and stories of some of those many people. Recollections can be faulty, memories fade, opinions change, and stories often become better in the telling, but taken as a whole they may convey something of how we came to be where we are in the containment world.

One regret I have is that the quoted printed word does not capture the emotional content of the spoken word - - the humor, satire, frustration, sincerity that I heard during these talks that we had. All are muted in a quotation on the printed page. The inflection of a single word can change the way a statement is to be taken, but how to convey that? The only way I know to attempt it is to give some brief background on each of the people, in their own words.

That doesn't have to do directly with containment, but it does have to do with perhaps putting the statements of that person in a personal context.

As for the context of myself, I went in 1952 from being a newly graduated graduate student who had done his thesis work at the U. C. Radiation Laboratory, to work with Herb York on what he initially described to me, somewhat vaguely, as a "small project." I somehow got the impression that it would be in Berkeley, and would deal with controlled fusion as a source of power, and indeed that was what I started to do. A few months later I had moved to Livermore with my family, and by then I was aware that the "small project" was a second nuclear weapons design Laboratory. Nine years later the Lawrence Livermore Radiation Laboratory had a staff of perhaps 5000 people, and I became involved with nuclear test work.

Since that time in 1961 I have been associated with the test program of the United States in various capacities. First as the Division Leader of L Division, the people at Livermore responsible for the design and fielding of the diagnostic measurements on Livermore nuclear experiments. Later as the person responsible for the overall Livermore Test Program, and since 1971 as the Chairman of the DOE-NVO Containment Evaluation Panel, whose function is better explained later in this book.

I can say from my own knowledge that the people, my friends, whose words are quoted in this book, are all dedicated individuals who grappled with the dragon, and eventually caged him, albeit uneasily, because they retained their sense of perspective, and of the limitations of their knowledge, while doing so. I did not say "subdued him", because they know, as I know, that whenever a nuclear explosion occurs he is there, just as enormously strong, clever, and dangerous as ever. Those who may be called upon someday to do an underground detonation should remember that. The amount of energy released by a "small" one kiloton nuclear device is simply beyond human experience and comprehension, except possibly that of the unfortunate people in Japan who were near the second and third nuclear detonations.

It was the Department of Energy Nevada Operations Office which supported this work, and to the people there, particularly Richard Navarro, I give my grateful acknowledgement. Byron Ristvet of the Department of Defense Defense Nuclear Agency was principally responsible for arranging the support required for the printing of the book. Without his interest and the Defense Nuclear Agency's support, the publication of this work might well not have happened.

The people I talked with were always cooperative in giving me their time for the interviews and for editing the transcripts, and my thanks go to each of those quoted in the text. Gary Higgins and Bob Brownlee were generous with their time in reviewing the book and offered many valuable suggestions on various points.

The table on page 572 was compiled by Gregory Van der Vink of OTA. The pictures in the book were provided by Roger Meade of Los Alamos, Steve Wofford of Livermore, and John Weydert of Sandia. My appreciation also goes to the unknown Livermore artist who, in the early seventies, captured the feeling of many people who were grappling with containment problems.

Particular thanks are due two people. Beverly Babcock assisted with many of the interviews and transcriptions. She was also most helpful in such matters as arranging times and places for the interviews, and gently encouraging the interviewees to finish and return their edits. Aside from providing photographs for the illustrations, Steve Wofford gave unfailing support on many questions of how best to arrange the chapters and format the text. He deserves my thanks for helping in a very substantial way on this project.

Introduction

The science of the containment of the radioactive by-products of a nuclear detonation exists only because there was a period of from 1957 to 1992 when nuclear detonations were carried out underground by the United States, the Soviet Union, the United Kingdom, and France.

The elements of several scientific and engineering fields are inextricably intertwined when people attempt to understand, calculate, and predict what will happen when a nuclear detonation occurs underground. The interactions which occur do so in regimes of material interactions, times, temperatures, and pressures that are never encountered in any other field.

The earth, from the surface to the mile or so in depth that has been used in underground nuclear testing is an inhomogeneous body of materials. Such things as the density, the strength , the chemical composition, and the water content of the rocks vary in a three dimensional fashion over almost any dimensional element that is chosen, ranging from molecular size to kilometers. Given the volume over which significant effects take place, the expense of obtaining sufficient representative samples to test in the laboratory, and the fact that laboratory measurements cannot reproduce many of the regimes of interest, it is not possible to know all, or even most, of the details of the medium where the detonation takes place.

So, empirical rules are developed, approximations are made and are used in computer codes to model the behavior of the earth materials following a detonation, but there is a further complication. Important processes occur during a time span that ranges from fractions of a microsecond to hours. Different measurement techniques and different calculational codes are required for different parts of this time span, and somehow must be linked together to try to understand the overall picture of what happens.

In such a situation experience and empirical evidence from previous detonations assumes a considerable importance when trying to judge what will happen when a particular detonation takes place in some specific location. The experience and evidence that there is has been gathered over the years, sometimes in a costly fashion. Experience and its role in judgement is difficult to codify and make available to people who might be newly charged with the responsibility to detonate a device, obtain the necessary data from

it, and simultaneously "successfully contain" the radioactive materials produced. Such a situation may never arise; if it does perhaps the words here may be helpful.

1

The Origins of Containment

To discuss the containment of nuclear explosions it would be helpful to have an understanding of what "containment" is. Unfortunately, there is no simple definition, or indeed, no uniform agreement as to what it is. Basically, it is whatever someone in the appropriate position of authority says it is, as is the case with many politically defined terms. And that also means that what it is can change from time to time.

There are documents which shed some light on this. The most important is the Nuclear Test Ban Treaty, signed on August 5, 1963 by the Soviet Union, the United Kingdom, and the United States. This Treaty called for the signatory nations to conduct nuclear detonations only underground, and in such a way that there would be no nuclear debris beyond the boundaries of the State which conducted the detonation. The operative article of the Treaty which relates to what would become "containment", as it is currently known in the United States, is Article I, Section 1. of the English version.

Article I

1. Each of the Parties to this Treaty undertakes to prohibit, to prevent, and not to carry out any nuclear weapon test explosion, or any other nuclear explosion, at any place under its jurisdiction or control:

(a) in the atmosphere; beyond its limits, including outer space; or underwater, including territorial waters or high seas; or

(b) in any other environment if such explosion causes radioactive debris to be present outside the territorial limits of the State under whose jurisdiction or control such explosion is conducted.

This seems clear enough, but there are some things that, on careful examination of subparagraph (b), are open to interpretation. The first, and most important of these, are the words " . . . causes

radioactive debris to be present . . . " What comprises the radioactive debris of a nuclear explosion? Is it any radioactive product produced by the explosion? Or is it only those radioactive products which will ultimately be deposited on the ground, and thereby become "debris" - - the dictionary definition of which is: "The scattered remains of something broken or destroyed; ruins"? This could be interpreted as meaning that if you cannot go about the ground and find "scattered remains," or fallout particles, you have not violated the Treaty. Hence, any release of noble gases, which dilute in the atmosphere, which are biologically inert, and which do not deposit on the ground, do not count. The answer to this question of interpretation is of considerable importance to the people who are charged with conducting a nuclear detonation, and at the same time with complying with the terms of the Treaty.

With one interpretation, a seepage of gases from a detonation, however large, would not be considered a violation, no matter where or how detected, because they would not be considered "debris." Using the other interpretation, such a seepage would be a violation, if large enough to be detected outside the State boundaries.

Now consider the words ". . . to be present outside the territorial limits of the State under whose jurisdiction or control . . ." In order for something to exist in this context, somebody has to know it's there. If radioactive material did cross the border of the State conducting the detonation, and someone, with some instrument, did detect the activity, then the Treaty has been violated. If the material is not detected outside the territorial limits, for whatever reason, it is difficult, or impossible to claim that a violation has occurred.

Another document that can be considered as defining containment in the United States is the Charter of the Containment Evaluation Panel. The relevant passages concerning containment itself are Articles III, subparagraphs A and C, and Article VIII subparagraph F. These are:

III A Emplacement and firing of each nuclear device will be conducted in a manner that conforms with United States obligations under all Nuclear Test Treaties.

III C Each test will be designed to be successfully contained. Special cases will be referred to DOE/Deputy Assistant Secretary for Military Application (DASMA), for approval.

VIII F Successful Containment: Containment such that a test results in no radioactivity detectable off site as measured by normal monitoring equipment and no unanticipated release of radioactivity on site within a 24 hour period following execution. Detection of noble gases which appear on site at long times after an event due to changing atmospheric conditions is not unanticipated. Anticipated releases will be designed to conform to specific guidance from DOE/DASMA (NV-176, Revision 5, Planning Directive for Underground Nuclear Tests at the Nevada Test Site (U)).

Note that the word "debris" does not appear. For there to be successful containment, it is "radioactivity" that is not to be detected off site, and this term certainly includes the noble gases. The boundaries of the Test Site are much closer to the event than the borders of the United States, hence "successful containment" is a much more rigorous standard than that given by using either interpretation of "debris" in the Treaty. Further, there should be "no unanticipated release of radioactivity on site within a 24 hour period following execution." The implication is that an unanticipated release of any amount of radioactivity within the 24 hour period is a failure to achieve successful containment. The monitoring equipment which might be used to detect such an unanticipated release is not specified, unlike the case of detection off site where "normal monitoring equipment," whatever that is, is to be used.

What occurred between 1945 and 1963 that led to the Treaty, generally known as the Partial Test Ban Treaty?

> It's almost always true of any organization that there are outside influences that make that organization change. It seldom comes from within. V. Leimbach

And so it was with the Atomic Energy Commission, the Laboratories, and the field test organizations.

Trinity, the first nuclear detonation, was carried out on July 16, 1945, atop a 100 foot tower. For the next many years that was one of the basic methods for doing experiments with nuclear devices. There were variations, of course; the air drop and the underwater detonations in Crossroads are examples. Sometimes the tower was short, or non-existent, and the device was detonated on the surface. Sometimes a plane dropped the device to detonate in the air. Or sometimes a balloon, or rocket, lifted the device to a desired altitude, there to be detonated.

For the scientists seeking information about the performance of some aspect of the device, there were trade-offs. The turnaround between experiments could be markedly decreased by using planes or balloons, but it was not possible to do experiments that depended on accurately viewing some particular area of the the device where phenomena of interest were taking place. Towers allowed that, but it took a long time to build the towers, and install the carefully collimated and aligned pipes through which instruments viewed a particular area, and recorded the data from there.

There were other differences among the ways in which the experiments were done, and these related to what happened to the radioactive material that was produced by the detonation. It was these considerations which gradually shaped the way in which experiments could be carried out, and eventually led to the firing of all devices underground in such a way that no radioactivity entered the atmosphere.

Initially, the approach to the radioactive products of the detonation was to disperse and dilute them, hopefully to a degree that made them of little biological consequence to people who might encounter them. It was an application of a belief once commonly held, not only by those detonating nuclear devices but by those running factories and other industrial sites which produced unpleasant and possibly dangerous by-products of the materials they produced:

The solution to pollution is dilution.

With this approach, if you were dumping waste chemicals into a river, and the river became badly fouled, what you needed was a bigger river, so there would be more dilution. The concept of controlling by-products of an activity at the source came slowly, and only as a result of public concerns.

After the end of World War II the United States conducted nuclear experiments at Bikini, and later at Enewetak as well. There was the Crossroads operation at Bikini in 1946, and the Sandstone operation at Enewetak in 1948.

Crossroads consisted of two 21 kiloton detonations; one airburst on June 30, and an underwater detonation on July 24, 1946. These were weapons effects tests, to investigate effects of a nuclear detonation on ships and other military equipment. In Sandstone there were three devices of various yields fired, all on towers, between April 14 and May 14, 1948. There was a significant difference from the focus on the effects of the Crossroads detonations - - information about the performance of the devices themselves was an integral part of the Sandstone operation.

Crossroads and Sandstone were basically ship-based with minimal support facilities on the atolls themselves. By 1951, when the Greenhouse operation was held from April 7 to June 24, 1951, permanent facilities had been built on Enewetak.

Bob Campbell became one of the Los Alamos Test Directors, and although he did not participate in either Crossroads or Sandstone, he later had extensive experience in both the Pacific, and in Nevada, starting with Operation Ivy, in the Pacific, in 1952.

Campbell: Enewetak was first used in '48, for Sandstone, and the whole nine yards of that thing was done by the Corps of Engineers; U.S. Army types. And there were a number of lessons learned on that. The AEC made their imprint on Greenhouse. The operation itself was in '51, but there was well over a year and a half buildup. They had a big structures program, and all the housing, warehousing - - essentially everything out there that we used for Greenhouse - - was built by the AEC. They did a much better job than the Corps of Engineers, because they had the idea that they were going to operate these things for ever and a day. It wasn't going to be done in the style of a Task Force campaign.

You can go back and look at the testing. The reason for Trinity was obvious; to see if the thing would work once. Then there were the Japanese things. Then there was a big hue and cry by the Navy, and so there was Crossroads. That was a Navy show; all this Lab did was provide the detonation service. And the Navy did themselves proud with Crossroads.

Then it was the Army's turn with Sandstone, but by that time the Lab had an interest in it too, because they had some new designs to try. So, it was more or less a joint venture. In fact, it was a little more than a joint venture. The Army really acted as support to the Lab on Sandstone.

The Laboratory group who did those operations was formed the same way as it had been in the past. You take somebody from this division, somebody from that division, and somebody from over here, put together a campaign, and go do it. Everybody comes back and then goes back to their regular jobs. At the end of Sandstone, Darol Froman, who had been the senior Lab person there, realized that wasn't going to cut it. It was going to go on and on, and so in '49, the year after Sandstone, they formed J Division, a permanent testing division, in the Laboratory.

Froman saw the need of it, and I've seen a fair amount of his correspondence on it. He wrote some rather persuasive papers on why it would be better if they faced up to it and said, "Here it is; we're going to be doing this for the rest of our lives." And I think the AEC was right in listening to him, and going along with a permanent plant at Enewetak atoll.

Operations in the Pacific, at what was called the Pacific Proving Ground (the PPG), were expensive, time consuming, and required considerable military resources to support the operation, the civilian construction workers who built the camps, the bunkers, and the towers, and the scientific teams who came to install the devices and the diagnostics. The construction started a year to a year and a half before the actual tests began.

Gerry Johnson, after a short time as a weapons designer, became responsible for the Livermore field efforts, and then became one of the Livermore Test Directors.

Johnson: Shooting in the atmosphere required big task forces, and as a consequence we could not have continuous operations. You had to mobilize, put things together, shoot them all in an interval, then return to the Laboratories and try to figure out what happened, rework the designs, and design new experiments.

In addition to that, the operations were complicated, unduly complicated, because there were thousands of people in the field. They were spectacular shows, people liked to see them, and so they

dreamed up all sorts of reasons for being there. That meant if you were trying to manage the operations, you had several thousand people to try to keep track of. If anything went wrong, any confusion, you had a hell of a time getting them out of there, and getting it straightened out so you could do your work.

After Sandstone there were no more shots in the Pacific for almost three years, until the first event of Greenhouse on April 7, 1951. In the meantime there was an exploration for a possible location in the United States where low yield detonations could be carried out, without the cost and time required for the Pacific tests.

The Korean War led to the declaration of a national emergency by President Truman on December 16, 1950. Two days after that declaration the President authorized the AEC to establish a proving ground for nuclear tests on the Las Vegas-Tonopah Test Range. Various locations had been looked at during the 1948-1949 period. Ultimately a choice had to be made.

Brownlee: The Nevada Test Site location was selected by Al Graves. He got on an airplane with somebody, they flew around, and he found this nice area . You could put some boundaries around it, there was a road to it on the south side, and it looked like it would be easy to build roads.

So, the criteria used for the selection of the Test Site had nothing to do with whether there would be atmospheric or underground shots. It was just a place we could get our hands on. And it was a place that had a road to it, and a place where you could land airplanes; it was an accessible place.

And there is another thing. When Al was selecting the Site, he was selecting a place where Los Alamos could go to do interim kinds of things, and a place where we could have our failures. We could have a failure there, because if it didn't work we could come back here, and in a few days have another thing ready to try. Once we got the various problems worked out, then we would go to the Pacific to do the real experiment. That was the concept.

So, the Nevada Test Site was selected by Los Alamos as a place where you could do certain experiments before you went to the Pacific to the permanent test site - - the Pacific Proving Ground.

When we removed the people from Enewetak, in the Marshall Islands, that was expected to be, at one point in time, permanent. We didn't anticipate them going back.

The object of the NTS was to do experiments close to home so you could go over to the Pacific to do the real thing. So, you did the low yield here, and you'd find out what wouldn't work. When you got ready to do one that really worked, you went to the Pacific. That concept was believed, and held, and fostered by people here at Los Alamos for, I would guess, four years. That was a long time in those days. And then we realized that Nevada was good enough that we could do a lot of things there that were not originally intended to be done there.

With the approval to do continental testing, things moved rapidly. The Ranger operation consisted of five airdrop detonations which were done in eleven days from January 27 to February 6, 1951 at the (then) Nevada Proving Grounds. There were two devices with a yield of 1 kiloton, two with yields of 8 kilotons, and one of 22 kilotons. All were detonated at 1000 feet or more altitude.

There was also the first of the things which would lead to todays's world of "successful containment." Fallout from one of the Ranger events left measurable amounts of radiation in Rochester, New York, deposited during a snowstorm.

Of course, from 1951, when the first tests were done at the Nevada Proving Grounds, until 1963, there was no Nuclear Test Ban Treaty, and from 1951 until 1961 there were no requirements for any type of containment in the conduct of a test at the (now) Nevada Test Site. If the concept of containment is considered in a broad context, it relates fundamentally to a way to mitigate the effects of the radioactivity produced during a nuclear explosion. These effects can be quite close to the place of the detonation, they can take place at considerable distances, they can be global in extent.

Such effects began to be a problem soon after tests began in Nevada. By 1953, in the operation called Upshot-Knothole, airdrops were used less and less, and devices with yields up to 32 kt (Harry), and 43 kt (Simon) were fired on towers. Both of those events caused off site fallout problems. In the case of Simon, some off site cars were contaminated, and had to be washed down. In the case of Harry, the people of St. George, Utah were told to stay indoors from nine until noon, to reduce exposures from the fallout

on the community, and the passage of the radioactive cloud. There was fallout in Troy, New York, deposited in rain which fell. There were reports (and later lawsuits) that hundreds of sheep in the Nancy and Harry fallout patterns had died, presumably due to exposure to the radioactive products of those events. The general public began to be aware of the actuality of, and the hazards associated with, the radioactive material from the nuclear tests at the NTS.

Worldwide attention was drawn to the dangers of fallout when the 15 megaton Bravo event of Operation Castle was fired on February 28, 1954. A Japanese fishing vessel, the Lucky Dragon, was some 80 miles from the detonation, and was in the fallout pattern. By the time it returned to Japan several members of the crew required hospitalization for the effects of the exposures they had received, and one died during treatment. Some 236 Marshallese and 31 weather service personnel who were on downwind atolls, well removed from Enewetak and Bikini, were also exposed, as were personnel on the ships of the Task Force.

Bill Ross was a participant during Castle, responsible for the mechanical hardware that was to be used for Livermore measurements on the Los Alamos Bravo event.

Ross: I was on the Curtis. We were issued the glasses the night before, told to wear long sleeved shirts, and that sort of thing. We were about thirty-five miles away. You kind of wondered, you know. There'd been the orderly room speculation about setting the atmosphere on fire, splitting the world in half into two pieces, and all that. And you began to wonder whether they really were seriously talking about what actually might happen.

Of course, the shot point was below the horizon. There was this tremendous light and heat, although you couldn't feel the heat at first. There was just the tremendous light. Even with the very dark glasses you were squinting. And then the heat came as the fireball got above the horizon, and it just got hotter, and hotter, and hotter. We had been warned to hang on because of the shock wave. By that time it was just so spectacular I'd forgotten all about that warning, and I was just standing there. All of a sudden it looked like a gauze curtain coming at you, smoothing out the little ripples of the water. There was a bang, and from then on there was just a roar, a tremendous roaring that seemed to go on for a long time. Finally

the light got so dim that you were opening your eyes wide and straining, and then you remembered, oh, I've got these goggles on. When you took the goggles off it was still very bright. The goggles were a darn sight darker than welding goggles. It was very impressive.

We started to get into the fallout, and the commanding officer of the Curtis got out from under the fallout. But, the Navy didn't like their ships all going in different directions, each commanding officer deciding which way to go, so they were all told to regroup around the Estes, where the task force commander was, which was right under the cloud, so they all went back into the fallout. We just had a little bit of fallout and got out from under it, and they washed the decks down. We were back out on the deck again when they got ordered to regroup. We went back in, and here it comes down again. Then we were locked in, and they were hosing down.

My stateroom was right near where the swabbies were going out on the deck and cleaning up, and they had a monitoring station there. These guys would go out in their raincoats and boots, and they would hose and sweep. When they came in they would strip and pile the stuff on the floor, step over two feet, and a guy would go over them with a counter. It was into the showers if you showed more than 2 mR above background. All this time the pile of clothing background is growing. At one point they were looking for the difference between 100 and 102 mR per hour. They were doing the monitoring in a 100 mR per hour field.

I don't remember how long we had to stay inside, but it was many, many hours. This thing went off at five o'clock in the morning, and we didn't get fallout coming down on us until after we had eaten, as I remember. We got a little bit, got out from under it, cleaned up, and were out on the deck around nine or ten, and then it was back in again. I don't remember when we got out. Herb Weidner and some of the other guys who were over on the aircraft carrier Bairoko, they were in a high field for days. The hanger deck was running, if I remember, a number of hundred mR per hour for hours, and then they were down around 5 mR per hour for days afterwards. They got a lot of exposure. In fact, the Bairoko never did get cleaned up.

I didn't see the fallout, but I was told by the guys who were sweeping it up and hosing it off that it was like a white dust. It was calcined coral. An awful lot of stuff went up, and it falls out, it definitely does. If you read the story of the Lucky Dragon, they got caught in the same kind of mess.

After Bravo, and the Lucky Dragon, fallout was no longer just a local concern, or a U.S. concern. Prime Minister Nehru, of India, on April 2, 1954 called for a testing moratorium. Concerns in the United States led to the Atomic Energy Commission finally to release some information about fallout. Before this there had only been releases saying, in essence, that whatever exposure people had received from the fallout from detonations at the Test Site were not large enough to cause any problems. No mention was made of the levels of possible exposures, of what radioactive isotopes were involved, or what areas were in the overall fallout pattern. It was on February 12, 1955 that the Commision released a report titled "A Report by the United Stastes Atomic Energy Commission on the Effects of High Yield Nuclear Explsions". This report did talk about both the Bravo test and the fallout from Nevada tests, but did little, if anything to reduce the concerns of the public; in fact, it may have exacerbated them. Commissioner William Libby did make a public statement about the problems of radiation exposure, and released scientific data about fallout in a talk he gave in June of 1955 to the alumni of the University of Chicago, wherein he made the statement that fallout did not "constitute any real hazard to the immediate health" of members of the public.

People working at the Test Site were having their own problems with both local and off-site deposition of radioactive material.

Campbell: After the St. George business, and after Bravo, it became obvious that we couldn't continue to have that kind of off site fallout. If we wanted to get our job done, we were going to have to find different ways of doing things. I don't think it was the people in Washington. It was really an internal recognition that we had to do something. And on site it was our people who were getting exposed making the recoveries. Believe me, radiation readings in the fallout patterns were much higher in Area 3 than they were in Utah, and we had to go in to get the data. The rule then was that you could work people forever in 10 mr per hour fields. In Nevada

I don't believe I ever went into a field that was over 50 R. Fifty R per hour is a lot, but you could get people who had not had much exposure, and you could say, "The operation is almost over, you're going home, and this is just for one time."

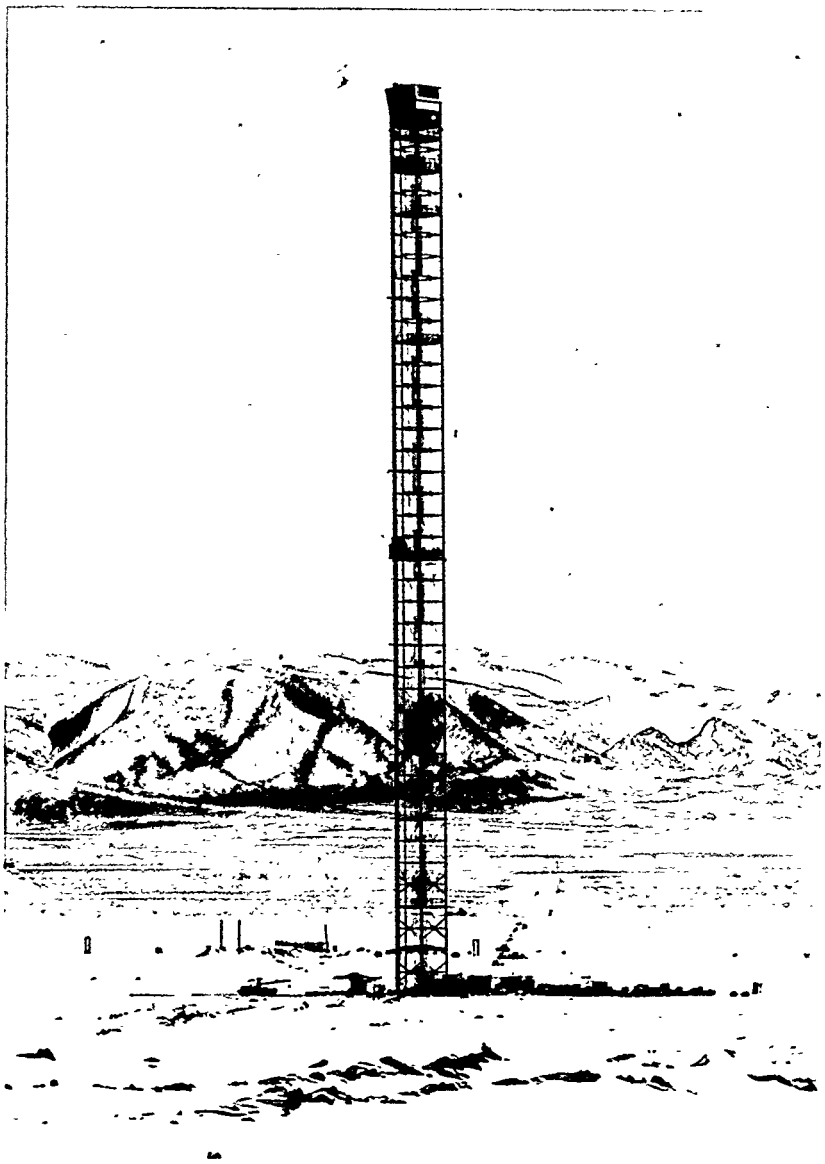
Operationally a number of things were tried, with the idea being to protect our own people. In so doing there was, of course, a benefit off site. They tried making large blacktop pads around tower bases, asphalt pads, to keep from entraining so much dirt. You could keep down quite a bit of the dirt that came flying up otherwise. And then there were areas where boron was put down, to reduce the soil activation. That was in about '55, or even before.

Towers we bought by the foot; we'd buy pieces for several thousand feet of towers. Then you take bits and pieces, like an erector set, and put up a two hundred foot, or three hundred foot tower. We had towers that were triangular in cross section, and towers that were square in cross section. The triangular things twisted too easily in torsion, and wouldn't bear enough load either. There was always the business of Herman Hoerlin wanting more lead, or Ernie Krause wanting more of something else in the cab. We tried aluminum towers to get away from the steel, but they didn't work worth a damn Do you want a little steel, or do you want a lot of aluminum? It ended up that the aluminum just did not have the strength and rigidity. It wasn't too popular, so we were always trying to find ways to use the aluminum towers we had in stock.

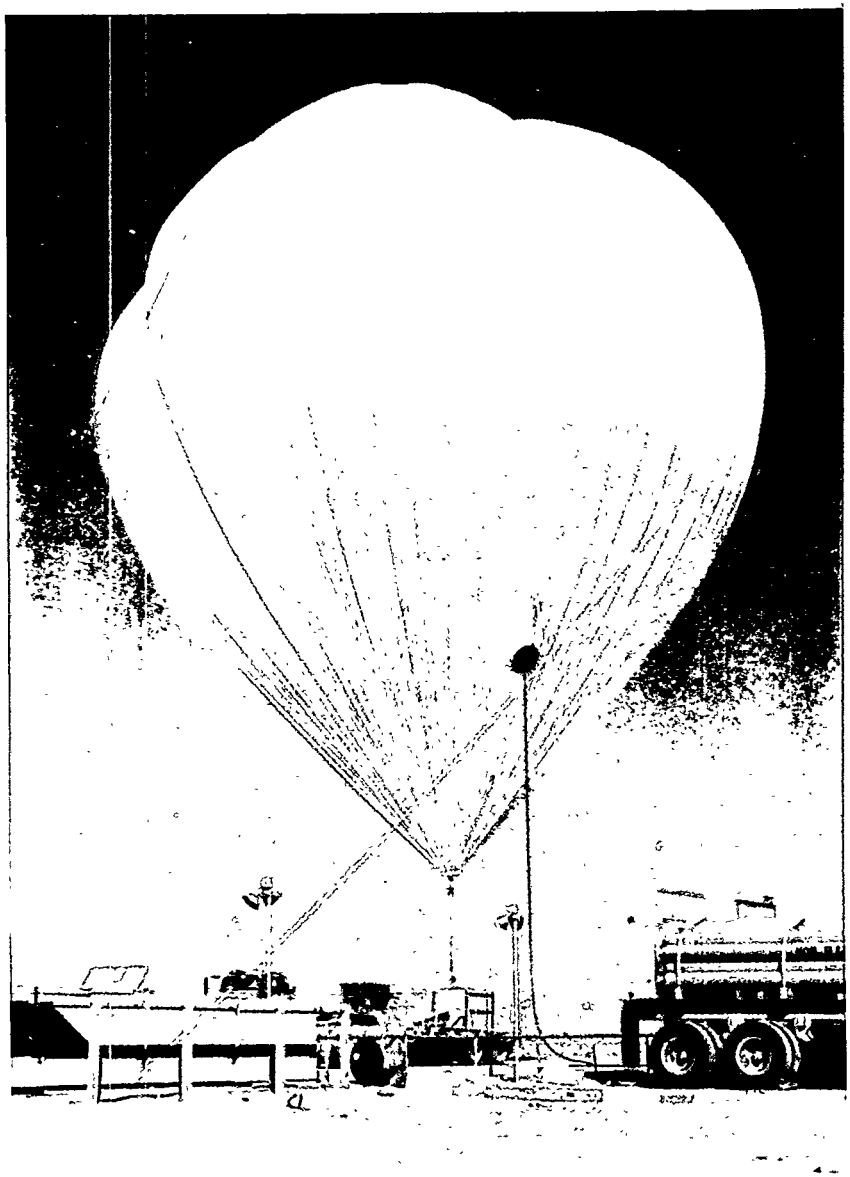
By 1956 people in the testing community were beginning to consider seriously the possibilities of conducting tests underground. This was a major shift in the thinking about the problem of fallout. Previous efforts had been directed basically to dispersal and dilution; firing underground would be an attempt to control at the source, to keep the radioactive material in one place, and not to let it disperse.

Edward Teller and Dave Griggs in 1956 wrote a brief paper (UCRL-1659) titled "Deep Underground Test Shots". In it they concluded:

"1. The cost of drilling a hole sufficiently large and deep to emplace and contain kiloton shots is comparable to the cost of erecting a tower for such shots.



Typical shot tower. Operation Teapot, 1955.



In an effort to reduce fallout, balloons were used in both the Plumbbob test series, 1957, and the Hardtack II test series, 1958, to lift test devices to approximately 1,000 feet for atmospheric detonation. Above is a typical example from the Plumbbob series.

"2. A depth of 3000 feet is ample to be sure of no surface eruption from 30 kt and small-to-zero emanation of volatile radioactive elements. One thousand feet will suffice for 1 kt.
"3. Yield can be determined within 5 to 10% by seismic and time-of-shock arrival, with suitable calibration.
"4. Radiochemistry of the explosion products may be done by core drilling the molten sphere. This may be expensive.
"5. Diagnostic experiments may have to be restricted to the determination of the time-dependent gamma flux.
"6. Using an open hole, visual observation and interesting neutron experiments may become possible.
"7. The seismic hazard to off site structures is nil.
"8. The long-term radiologic hazard is nil."

And, they recommended that in connection with the next Nevada test series, (which would be Plumbbob, in 1957) a low yield shot be detonated at the Test Site "at such a depth that it will be contained."

At Los Alamos, Al Graves, head of the test effort, had arrived at the same conclusion; the possibility of doing tests underground had to be explored, because nuclear tests were going to have to be done underground if testing in the United States, at the Nevada Test Site, was to continue. No such events had ever been conducted, and the state of ignorance was vast. There were no equations of state for earth materials, and no codes into which to put them if they had existed, and by today's standards, primitive computers to run them on if the codes had existed. No one knew how big a cavity would be formed, or what the post-shot cavity conditions would be. No one knew what a safe burial depth was. No one knew what the ground motion and seismic effects would be. And so on.

In 1956, Graves asked Bob Brownlee to look into what might happen if a device were detonated underground.

Brownlee: One reason I admired Al Graves was because he was so inordinately farsighted. He anticipated problems long before other people. Where he came to have these ideas I have no idea; whether they came from his colleagues, or whether they came from the sky I don't know. He was the first one to my knowledge to ask questions of a far-reaching kind about the hazards of testing. For example, "Is there any chance that I will knock a piece of the shelf

off the reef, which will then slide down the edge of the atoll and start a big wave of some kind? What's the chance of doing that?" As people talked it over, they decided that could actually happen, and so we moved some shots. But those kinds of unanswerable questions were frequently asked by Al first, at least to my knowledge.

One experience with testing in Nevada which must have influenced him mightily was that in '55 there was a civil defense test where they sat out over two weeks before the weather was right. They were very carefully watching where the fallout would go, and if it was predicted to go over places where there were people who couldn't be warned, and evacuated if need be, the shot was cancelled for that day. That shot was scheduled every day, and then cancelled, for nineteen days, and Al was the one making those decisions. Now, there were a lot of civil defense people, and press people, and others, whose hotel reservations were running out and so on, and they were very impatient with all this. And Al was taking that pressure.

He said to me, in 1956, "There isn't any doubt about it. If testing is to proceed, we're going to have to go underground. It's got to be done, whether we want to or not. Would you start working on what it might be like to have a fireball underground?"

I was tied up for the '56 tests in the Pacific, but once they were over Al said again, "Please go to work on underground things. It's inevitable. We're going to have to do that, and the question is, 'Can you contain anything at all? If you put the device underground, does it just all blow out, or what?'" It was a very interesting question, and I began doing some machine calculations. We were doing work on the IBM 704's, which were quite new then.

Carothers: You were using, by today's standards, a rather small machine. You were using a computer that had less capability than the one you probably have at home now.

Brownlee: Oh, you can now carry around, in your shirt pocket, something with more memory than the 704's had. We coded everything in machine language in order to save memory. And we had bits in the words which we used as flags, because you never did any multiplication or division until the end, because that was so slow. The programs were incredibly sophisticated in adapting anything in the world to a little bit of memory, and to the machine's characteristics. You spent all of your time doing that rather than

working on the problem. I had this big deck of cards that I would feed into the machine, and if there was a card upside down it was rejected. It was a very slow, laborious process, but that's what we had in '57.

The earliest work I did was try to calculate the creation of a cavity. I had the equations of state of four materials; aluminum, uranium, air, and water. I said, "That's the old Greek concept of earth, air, fire, and water. Earth was aluminum, fire was uranium, and there was air, and water. With those four equations of state I started trying to calculate what might happen underground. Now, very quickly we began to get more refined equations of state, but from those four I tried to make an equation of state for some fake material. I tried to guess in what direction earth might be different from aluminum, and started to change the various parameters. I finally evolved what I called the equation of state of NTS dirt.

I look back on it all now in amazement. How could anybody pay me to do such absolutely worthless calculations? And yet, the fact is, they weren't all that bad. I created, in my initial calculations, an elliptical cavity; I didn't really get a round cavity. That was because of the inadequacies of my equations of state. Of course, I didn't know enough to know what the answer should be, so, just like every other theoretician, I fudged the numbers to make them kind of match what I saw. By modern day standards it was an abomination, but for the time it wasn't all that bad, and we were educating ourselves.

Incidently, I feel very strongly about that. Machine calculations you should use to teach you how to think. You don't pay any attention to the numbers, but they teach you how to think, and how to see what is more important than something else. And that's exactly what I was doing. I was getting a very good education. I wasn't contributing anything profound to the system, but I sure was getting a education about how to think about things. That's the real value of that kind of work.

So, I did my first primitive calculations in '56. And I actually calculated one test, Bernalillo, which we did in '58. That's how I got into the underground business, and that was strictly due to Al Graves, who recognized the necessity to go underground. There are

a lot of people who don't realize that we were doing the initial work for underground tests as early as 1956. Now, remember, we didn't do that until '63, totally.

One theme that was present in the early underground experiments was that there was a definite self-interest for the Laboratories' test organizations in reducing the fallout from the shots. There was a need and a desire to reduce the fallout off site, and to respond to the mounting public concerns, but also there was the need to reduce the local fallout in the vicinity of the shot itself for operational reasons.

Campbell: The first thing we at LASL did in a hole was called Pascal-A. It was 500 feet deep, in a cased hole. We put the bomb in the bottom of it, and we didn't stem it. So, we fired it. Biggest damn Roman candle I ever saw! It was beautiful. Big blue glow in the sky. I was up in the CP office, and that was fired from a little handset, out at the B-J Y.

Carothers: You mean somebody sat out there, and as I've seen in Tom Mix movies, pushed the plunger to blow up the dynamite and foil the Bad Guys?

Campbell: Well, pretty close to that, but not quite. He had a little hand firing set. The shot was in Area 3, down by 3-300. The firing point was the nearest timing station of any size to Area 3, and so the shot was between the people out there and the CP.

Bill Ogle was out there, in that timing station. When he saw that come out of the ground he knew he couldn't come south the way he came north, because he was going to get into trouble. Bill was more excited that evening than I ever heard him before or since. He was really excited about how they were going to get back. They went way out east on roads that didn't exist, came back around into Yucca Lake, and came in that way. You've heard people say, "His eyes bulged out like a stomped-on toad"? That's what Ogle looked like when he came into the J Division office that night. He was really excited, and talked a mile a minute. They were damn lucky they didn't go right through that cloud.

Carothers: Why didn't you stem it?

Campbell: Didn't need to. We did have a lid on the hole. Nobody's seen that since. We never did find that. On that lid was one of Johnny Malik's detectors, and we wanted a line of sight to see if we could measure some of the reactions. There was a kind of plug in the hole. It was a couple of hundred feet off the bottom, as I remember. All it was, was a concrete cylinder with a hole through the center of it, so the detector could look through. And it had an annulus, so it wouldn't bind anywhere going down. It was suspended from the harness that was holding the bomb. It was a collimator, not a plug that was supposed to stem the hole. We never found that collimator either, and it was about five feet thick.

We had a half dozen of those holes drilled in an arc around station 3-300, our alpha station. We were in the business of making the transition from towers that were looked at from the station. All our scopes were in there, and we were trying to get something where we could use the same recording gear without having to move it.

But anyhow, bad as it was, spectacular as it was, there was only about a tenth of the radiation on the ground around there that there would have been if we had done it on the surface. And we considered a factor of ten reduction to be wonderful. We thought we had made a real gain. A factor of ten meant we could get back, and get set up and fire again more quickly. We were very happy with the results, and we did it all over again on Pascal-B. That one doesn't stick in my mind like that first blue one. That was our initiation.

The reduction in off site fallout was an effect that was appreciated by the AEC, and the people who worried about off site safety. What we were worried about was being put out of business if we had too many people pounding on the gates. And, we wanted to reduce the local fallout, the contamination of the area that we were using.

Jumping ahead to the moratorium, it turned out that we had a little money, and we drilled holes against the day that we might come out of the moratorium. We really thought that was the direction we were going to go.

Bob Brownlee, who had been asked to look at the possibility of firing shots underground, helped to design the Pascal experiments, and attempted to approach the problem in an orderly fashion. That was sometimes difficult.

Brownlee: Our first underground tests were done in '57. There was Pascal-A, and Pascal-B, and Pascal-C. And there were several others in '58, during Hardtack II. Al asked the following question, "If I take a 48 inch casing, and I put a bomb a couple of hundred feet down, by how much will the fallout be reduced?" We discovered it was a factor greater than ten. And that was just an open hole. So, he then said, "If we put some plugs in the hole, does that cut it even further? And if so, how much?" So we did that. Then he said, "Let's put a plug right down on top of the bomb, and then let's put a plug half way down. Does that make any difference?" And yes, it does a better job if you put the plug right on top of the bomb. "Well, suppose we put in some dirt. Does that help?"

We started exactly that way. We were still doing atmospheric shots, so the question was a very simple one. "If you do this, or that, how much will you cut the fallout?" And we determined that experimentally. The answer to the fallout question was, "We'll measure it and see." On the other hand, the calculations I did calculated the time the shock would get to the top, what kind of top you might put on the hole to hold things in, what would the pressures be there, how big might the cavity get, how does it cool, and what happens to all that pressure? Does it lift the ground? Those kinds of questions.

Pascal-B and Pascal-C had plugs, but Pascal-A did not, although it had a concrete collimator in it for the detector at the surface. The guys had been working trying to get it ready, and there had been a number of troubles. They finally got it down hole, by my recollection, about ten o'clock or so at night. There wasn't much time to go back into Mercury, go to bed, and get up the next morning to shoot it, so somebody said, "Why don't we just shoot it now, and then go in?" And it was the world's finest Roman candle, because at night it was all visible. Blue fire shot hundreds of feet in the air. Everybody was down in the area, and they all jumped in their cars and drove like crazy, not even counting who was there and who came out of the area. Today it would give the Test Controller and his Panel total apoplexy - - they would become totally insensate.

It wasn't done quite as logically as I have indicated, but there was a thread of logic from shot to shot. We saw what happened on one, and decided what to do next, but in the meantime we would have another one. So, the chronology is not as perfect as you'd like to think it was.

One of the things we were annoyed about in '57 and '58 - - I remember being annoyed, and I say we because I think there were a number of us - - was that we'd do an underground shot, and the radioactivity from an atmospheric test would be floating by at such high levels we'd never know what came out. The object of the underground shot was to see how much we could reduce the fallout, but we couldn't differentiate that fallout from the fallout of the atmospheric shot, which was so much greater. So, I thought, "Why in thunder are we doing this? The whole object is to find out what happened on this underground shot, and after it's over we don't know whether it leaked or not, or how much. This is absurd. Why don't those guys knock it off?" I remember having that kind of an attitude, and I think there were several of us that were annoyed. But the right hand usually doesn't know what the left hand is doing, and all that.

At the Livermore Laboratory, stimulated by the Teller and Griggs report, work was being done to fire a low yield device in a tunnel, with the object of completely containing all the debris produced. Gerry Johnson was in charge of the Livermore testing program, and was the Livermore Test Director.

Johnson: It was becoming increasingly difficult to carry out tests in Nevada because of the fallout constraints, and the public furor over the fallout. There was a rising public concern that kept growing through those years. In Nevada, from an operator's point of view, we were only interested in getting the developmental information. Actually, 1956 was when we began to think about underground shots, and we were interested from an operational point of view. We felt if we could go underground and get the data, then we could treat it as an extension of the Laboratory. We'd go out and shoot whenever we were ready to shoot, without this big Task Force and large numbers of people, because as you know, underground shots are pretty dull to look at. And the duller the better.

Carothers: Somebody said, "Watching an underground shot is like watching a submarine race."

Johnson: I've never heard that, but you're right. That's a good way to put it. One of the big questions we had was how to seal the tunnel, but out of a lot of stewing around the Rainier experiment was finally designed, and we fired it in September of '57. And it did contain.

The Rainier event was fired in B-tunnel on September 19, 1957. Even by today's definitions, Rainier was successfully contained. The dragon was caged, and his foul breath no longer polluted the air. Considering the lack of knowledge at that time about the phenomenology of an underground detonation, that fact is somewhat remarkable. After Rainier, perhaps containment even seemed easy.

hubris: n. Excessive pride, arrogance. From the Greek.

Meanwhile, two different paths were leading to changes in the way in which nuclear tests were conducted. Since the Bravo fallout problems, opposition to continued testing had been increasing in the United States, with wide publicity given to the anti-testing or anti-bomb views of Linus Pauling, Albert Schweitzer, Paul Jacobs, and others. There were anti-bomb demonstrations in England, West Germany, and Japan. Politically, the issue of testing arose during the 1956 presidential campaign, and influenced the steps that were being taken to negotiate a disarmament treaty with the Soviet Union. In August of 1958 President Eisenhower announced that United States would suspend testing for a year once test ban negotiations were begun on November 1, 1958, in Geneva.

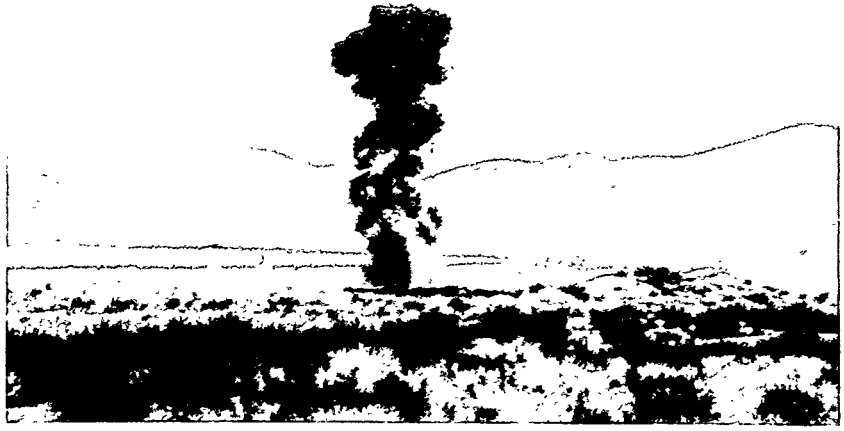
The Hardtack operation had been conducted at the PPG from April to the middle of August in 1958. With the announcement of a moratorium to begin at the end of October, Hardtack Phase II began some thirty days after the last shot in the Pacific.

During Hardtack Phase II Los Alamos conducted six safety shots in unstemmed holes, with yields ranging from zero to a few tens of tons. These events in unstemmed holes were not designed to be completely contained; the objective was still to reduce contamination in the immediate vicinity of the ground zero, and to experiment with various plug and stemming locations and configurations.

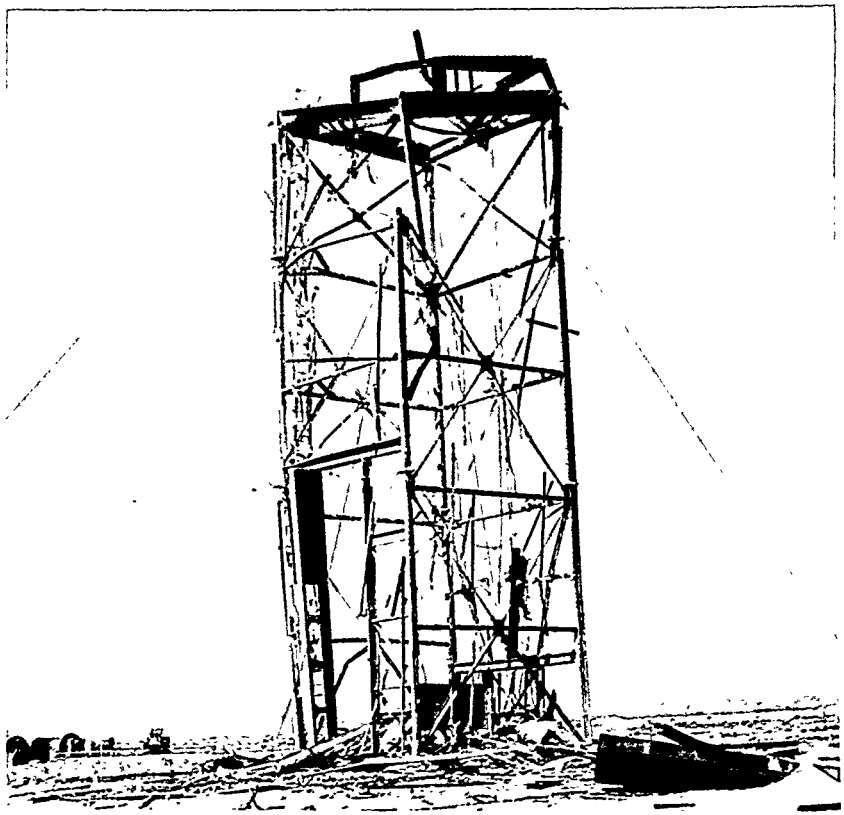
Livermore did seven tunnel events during this period. There was one tunnel event which introduced those people interested in containment to the possibility of an unexpectedly high yield, or as



September 12, 1958, Otero event, unstemmed hole.



September 12, 1958, Otero event, unstemmed hole.



Surface structure for Otero event.

some people might say, the unreliability of designers. Neptune was fired on October 14, 1958, as a safety experiment with an expected yield of zero, but with a possible yield of 10 tons or so. It was fired in a tunnel, with a working point that was under the sloping face of the mesa, with a vertical distance of 110 feet to the surface, and a slant range of 100 feet to the closest point of the mesa. The yield of 116 tons was unexpected, the shot vented, and produced a crater. The fact that some of the radioactivity was released was not of real concern; Hardtack II was, after all, principally a series of atmospheric shots, and the day before Neptune the Lea event, with a 1.4 kt device suspended from a balloon, had been fired. The Livermore people, showing considerable flexibility in their thinking, promptly called Neptune a nuclear cratering experiment, and in a future report (UCRL-5766 The Neptune Event; A Nuclear Cratering Experiment) discussed the "major contributions of the data to the theory and prediction of cratering phenomenology."

Carothers: In '58 Livermore fired a shot, called Neptune, in a tunnel. It turned out that it gave somewhat more yield than was expected, and it vented out the side of the mountain. People have said to me, "That Gerry Johnson, he was probably the world's foremost optimist. We don't know how he did it, but he could take a disaster and convince everybody it was a great success. On Neptune he just didn't pay any attention to the idea that shot was supposed to be contained. He said, 'Well, that's our first nuclear cratering experiment.'"

Is that a true story?

Johnson: Yes, that's correct. I was told the maximum possible yield was ten tons. That was the absolute tops. So we designed it for ten tons, and it went a hundred or so.

It was a lousy cratering experiment. It was on a sloping hill, but it was a point on the curve. But you're right - - you'll find that listed with the cratering shots in the Plowshare program, and it had lots of analyses done on it.

Gary Higgins, at Livermore, was beginning to explore the possibility of collecting what were called prompt radchem samples. The thought was that if some kind of pipe could be designed that would be emplaced in such a way as to look directly at the device, allow a flow of some very small fraction, but no more, of the device

debris to a collector on the surface, the expense and time delay of the post-shot drilling for samples of the device debris could be avoided. Dick Heckman was part of the group that was to field and collect the samples which might be obtained.

Heckman: We started off with a few of the safety shots. The one incident that I remember in particular was the Neptune event, in Hardtack Phase II. There was about a one-inch diameter pipe which ran down into the roof of the room. The hole was drilled vertically, preshot, and this one-inch pipe was inserted and grouted into place. We had a plywood box built on top with a two foot by two foot aircraft-type filter material with the appropriate screen, and with just a discharge on up. With the yield that was anticipated, everything should really be kind of nice after the shot.

We then backed off down to the Area 12 CP. I requisitioned a pair of binoculars, and braced myself on my vehicle so I could spot in on the location. With binoculars I could see the little sampling box, and since our success hadn't been all that good on safety shots, I thought if something were to happen, maybe I could follow the trajectory of the box so I'd know where to go and find it. The event went off, and the yield was quite a bit higher than they expected. As a matter of fact, that was one of the first cratering shots that the Plowshare program takes credit for. The binoculars did no good, because the ground shock hit the surface, raised a dust cloud, and I couldn't see a thing.

I got a bunch of radiochemists, and we went up and we saw the the filter box was there, in the crater. I argued like a Dutch uncle, and got permission, which in retrospect was a dumb thing to do, but I got permission to get a rope tied around myself, and to be let down into the crater. We were pretty motivated in those days.

So I crawled down, and indeed found the filter box. I tore the filter paper out, but the pipe had shut off and so we had no sample. Now, when I was given permission to go down into this crater, it was, "Under no circumstances will you go into a field which is greater than 1 R per hour," because they expected this big fallout.

Well, going down there with my survey meter, I found out that the activity was incredibly low - - a few tens of mR per hour. In hindsight, as a result of attempting to do that recovery, we got some very important information that really excited some of the Plowshare people. When I came back and reported this, Vay Shelton and

Gerry Johnson happened to be down there at the time, and their eyes lit up. It was sort of, "You've made the discovery, clearly you would want to publish the paper." At this point, the sampling system didn't work, so as far as I was concerned there were other things to worry about. But it was my crew in that recovery who were the first ones to discover that it was possible to do a cratering shot and trap the gross radioactivity down in the ground. Remember that all the previous experience had been with military cratering shots, which were underburied.

Two of the tunnel events in the Hardtack II operation were designed to give appreciable yield. Logan, fired two days after Neptune, produced about five kilotons, and was successfully contained. Blanca, fired on October 30, 1958 produced 22 kilotons, and like Neptune, but in a more spectacular fashion, vented out the face of the mesa.

The Logan event was interesting for several reasons. It was an event in a tunnel, designed to investigate the effects of the nuclear radiation on various materials. There was a horizontal vacuum line-of-sight pipe which extended for 150 feet from the device, opening to two feet in diameter at the far end. From there two six-inch diameter pipes extended another 75 feet. The design team started with some money, with very little Laboratory manpower support available due to the heavy shot schedule already planned, support from some contractors, a pad of blank paper, a tunnel that was still being dug, and six weeks to design the experiments and the diagnostics, fabricate the hardware, and have everything installed for the shot. That is an incredibly short time scale by today's standards. And, Logan was successfully contained. Arnold Clark was the project physicist for Logan.

Clark: We had six weeks, because we had to shoot two weeks before the end of October. We were going to shoot in a tunnel, which hadn't been finished being dug yet, where an important shot, Blanca, was going to be shot in another part. So, they had to have two more weeks after we shot to finish off the cabling for Blanca. They would finish digging out a side drift place for us, and they'd pull cable for us. Our biggest problem was that we wanted a vacuum pipe in the tunnel. Here we were, starting with a blank piece of paper, and we had five weeks to have that pipe finished, installed, and pumped down.

They said, "How long do you want it?" We looked at our blank piece of paper, and said, "A hundred and fifty feet." "How big around?" "Oh, about this big," making a cirle with our arms. And that was the process we went through to specify it. So, we had a 150 foot vacuum pipe, maximum diameter of two feet, made by NRL in Washington. It was flown out, installed, and evacuated. And it held a vacuum ! In five weeks !

Lockheed made a very fancy, very strong steel sample holder to put at the 150 foot station. Then people had second thoughts about that station, and said, "That is not going to survive. Or maybe it's not going to survive." They didn't know. "Maybe we better go out farther." So, we extended the pipe by adding two pipes, 6 inches in diameter, to the back end of the big one, to go out another 75 feet. And that's all that survived; the 225 foot stuff. We never saw any of that 150 foot station after the shot. That was where the container of very special steel, made by Lockheed, had been. It was a huge thing, about the size of a really good-sized safe, just essentially solid steel. And it was a very special steel alloy that was supposed to survive. Well, it didn't. There was very little from that station.

We had a quite elaborate closure on the front end. There was a very fine theoretical physicist, Harold Hall, working for Montgomery Johnson in early '58. They were worrying about this containment problem, and Harold came up with the idea of a Box A type closure, as they call it now. This was a brand new idea. Harold Hall did some calculations, and so did Montgomery Johnson, and they said, "Ah, yes !" So a Box A type closure was used for the first time on Logan, and it worked very well. I think the front end was a foot in diameter, which is pretty big. Maybe it was ten inches.

When they were digging back after the shot they also drilled back at different areas around the zero room, and found that the really highly radioactive area, I guess you would call it the cavity today, was pear shaped. It wasn't circular. Some activity had come down the tunnel, but not very far except for a few cracks that went out as much as 150 feet. So, it did contain completely.

However, it knocked in the side of the drift where Blanca was supposed to be, and there wasn't time to clean out that drift, so instead of being shot underneath the mesa where it was supposed to be, Blanca was shot beneath the very steep face of the mesa, out



October 14, 1958, Neptune event, 116 tons, tunnel U12c.



October 30, 1958, Blanca event, 22 kt, tunnel U12b.

where the overburden was maybe half of what it would have been. I watched it, and I thought the side of the mountain was going to come right towards me and hit me. I was only two miles away.

The days of unrestricted atmoshperic testing at the Nevada Test Site came to an end on October 31, 1958, at midnight. As midnight came and went a Livermore device, ready to be fired, hung suspended from a balloon, and there it remained until the balloon was brought down and the device removed.

Duane Sewell, who later became the Deputy Director of the Livermore Laboratory, was the Scientific Adviser to the Operations Manager for Hardtack Phase II, and made the recommendation not to fire.

Sewell: We left one device unfired, and I remember that night very well. I had about fifteen hundred people who really were upset with me because I didn't tell the AEC to go ahead and fire that device. I told them not to fire it, because it was obvious we were going to have trouble, but not from fallout. The wind pattern was in a direction that was not going to give us trouble, and that last shot was a balloon shot, so there was not going to be a great deal of dirt picked up, and local fallout from that. But the wind pattern was such that there was a potential for a pressure impulse into Las Vegas that was strong enough to possibly break plate glass windows. We obviously didn't want to hurt anybody, and didn't want to break windows either.

We were testing with shots of a half ton of high explosive mounted on one of the hills a short distance from the CP. We'd fired a number of those during the evening, and it was a double bounce. The shock wave bounced down around Indian Springs, then the next bounce was into Las Vegas, and it was rather sharply focused. We had trouble getting enough high explosive; I was blowing up all the high explosives on the site to make those measurements every half hour to forty-five minutes. The scheduled deadline was midnight on October 31st, Halloween night. I remember a lot of masks around the place.

Dodd Starbird was the Director of Military Applications at the time that operation was going on. I got on the phone with him, and I said, "That's midnight Washington time, not Greenwich time when

we start the moratorium." We agreed on that. That gave us an extra five or six hours. When it got to that point I said, "No, it's really midnight here," and I got him to agree to that. Then I tried to get him to agree to midnight within the United States, which would mean Hawaii, but he wouldn't buy that. He wouldn't go that far, so Pacific Standard Time was what we finally had to go on.

We fired the last HE shot about eleven-thirty that night. I was in the microbarograph room, and we had people out in the field with mobile measuring systems. The people there called in and said, "My God, what did you fire that time?" It really shook them. Apparently we had them just at the focus, and I thought, "Boy, if a half a ton can be heard that far, I'm not going to fire." The last thing we wanted was to have any sort of damage, or the potential of harming people in Las Vegas. That's why I made the decision I did. I advised Jim Reeves not to fire and he went along with it. That's why we left that thing hanging on the balloon that night.

Louis Wouters was one of the Livermore scientists waiting for the shot to be fired.

Wouters: We ended up with one shot, Adams, being The Last, the last of that particular series. It was going to be shot on October 31st, but something didn't go the right way, and we didn't fire the shot. If the politicians had any sense at all they would have let us shoot it, because it turned out that two days later the Soviets went ahead and fired one more shot anyway, after the beginning of the moratorium. They weren't as picky about those things as we were.

2

The Rainier Event

The first nuclear detonation that was designed to be completely contained was the Rainier event, fired in B-tunnel in Rainier Mesa on September 19,1957, during the Plumbbob operation. It had a yield of 1.7 kilotons, and for the first time there was a nuclear detonation that did not release radioactive material into the atmosphere.

During the test moratorium that started in 1958 there were extensive explorations of the cavity region and the surrounding materials. It was from the information obtained during these reentry operations that many of the early ideas of cavity formation, growth, size, and so forth originated.

Gerry Johnson was, at that time, the Test Director for Livermore events, and was the person who caused the Rainier detonation to take place.

Johnson: The operational constraints, which were increasing each year, were bugging us, and we were looking for a way out. Then Teller and Griggs did some back of the envelope calculations and said, "Look, it ought to be possible to shoot a shot underground, and if you had a thousand feet of overburden, you probably could shoot a few kilotons or so." I was interested in that, and I said, "Well, we'll examine that. We'll get some people looking at it and thinking about it, and see what comes out of it." Which we did.

That was in '57. The Teller and Griggs suggestion was about a year previous. They wrote a memo on it, describing the concept. Two of the big questions we had were whether you could contain it, and would the radiochemistry be any good. As usual, we got into big arguments with Los Alamos on all issues from technical to cost.

The chemists here felt they could do the chemistry. We had questions about the sampling. We didn't know if we'd have a pool of molten rock, or what we would to get into. Before the event we had lots of speculation on what would really happen. There were some calculations made in terms of what you might expect in ground shock, and surface motion, and so on.

We choose the site based on topography. We decided on a tunnel geometry because we thought that would be the best way to do diagnostics. And that's how we finally ended up with Rainier Mesa. We ended up in tuff, which was good stuff to dig in, but we didn't know anything about it. We didn't know what tuff was when it was first mentioned to us.

But then we began to get into public information trouble. A number of us were interacting with the geophysical community, which we always had done, for all sorts of reasons. Dave Griggs made the suggestion, "Look, if you are going to fire a shot like this, for the first time we'll have a shot closely coupled to the ground. We'll know the yield, we'll know the coordinates, and we ought to make this information available to the geophysical world, so they can take advantage of it. In fact, you ought to announce it ahead of time."

Well, we went through this, and were told to hold the time of firing to a tenth of a second at some predetermined time, which we agreed to do. If for any reason we were delayed, and couldn't meet that time, we agreed to wait twenty-four hours and try again. And we published this. That was fine. It was very altruistic and lovely, and in the right spirit of technical cooperation. But about a month or six weeks before we were going to be ready, an international geophysical meeting took place in Toronto, and by then this event was getting lots of interest on the part of the seismic geophysical community. At this meeting some guy made some statement about Livermore planning to fire an "earthquake maker," and it hit the headlines, and of course got the Atomic Energy Commission's attention.

Carothers: "AEC TO FIRE EARTHQUAKE BOMB ! ! !" I can see the headlines.

Johnson: That's right. Well, that did it. Strauss, who was then Chairman of the AEC, called and said, "What in the hell are you guys doing out there?" I said, "Nothing."

So I went in to talk with the Atomic Energy Commission, and I said, "We've gone through all the calculations of what the seismic effects might be. This is a very low yield thing that we're trying to shoot; 1.7 kilotons." That seemed quite small to us. "And we've done all these calculations." Strauss said, "That's not good enough. I'll tell you what you've got to do before I'll authorize this shot. You

have to assure the Commission that the shot itself will not cause an earthquake. Number two, that it will not trigger an earthquake, and number three, if a natural earthquake occurs at the same time, you have to prove you didn't do it."

So we put together a committee. We got Perry Byerly, somebody out of Cal Tech, I guess Dave Griggs was on it, a fellow named Roland Beers whom none of us knew, and somebody from back east. They met. And we told them what we were going to do, and the whole thing, and Byerly's first remark was, "You shouldn't be so presumptuous. One point seven kilotons? That will do nothing seismically." I said, "I'm not arguing with you."

I called up Strauss an appropriate time later and I said, "We've gone through this thing. This board of experts got together, now we want to come in and talk to you." Strauss said, "Who's on that committee?" I told him, and he said, "I don't want any West Coast people on it." This was a setback, because the West Coast seismic mafia was most of it. It turned out that the only guy who was acceptable to them to be at this presentation was this guy Roland Beers, whom we didn't know.

Beers came to the meeting. He was a soft-spoken guy, and didn't seem to know what was going on. I thought, "We've lost the shot." I muttered to my partners, "I don't think we're going to win this one. I don't know this guy; he didn't say anything when we were meeting, and I don't know who he is."

So we go assemble with the Commission and have the meeting. I go through my pitch, describing the experiment and so on, what we were doing, and what the conclusion of this panel was. Strauss then looked at Beers and said, "Beers, what do you think is the largest explosion that you could safely fire in Nevada, underground?" I thought, "Oh God, what's he going to say?" And he said, so quietly Strauss could barely hear him across this enormous table, "About a megaton." Strauss said, "What !" Beers said, "About a megaton, sir." That was all he said, and Strauss said, "You fellows get out of here. The Commission and I are going into an executive session." Which they did, and they decided favorably. "Okay, but be careful."

So off we go, and by then the furor had gotten to the state of Nevada. The day before the test somebody from the Governor's office came to the Test Site to serve an injunction on the AEC to

stop the shot. The Governor said, "We'll hold the AEC directly responsible for any damage to public works in the state of Nevada." He wasn't going to take any responsibility. Well, bless Jim Reeves, who was the AEC Area Manager for the Site. The day the guy came out to serve the summons, Jim had to make extensive surveys of the upper end of the Test Site. He was unreachable.

Carothers: Well, he was just doing his job. He has to go see what's going on, once in a while.

Johnson: Sure, he's the Manager. And we were going to fire the next day.

We had a technical advisory board, with respect to the containment. These were vulcanologists, geophysicists, I don't know who all, but distinguished people. The night before the shot we had a final review. Shall we go ahead, or is there something else we should do? And the conclusion was everything is fine, go ahead.

We arrived at the CP early in the morning; I forget what time we were to fire, but it was during daylight so we'd get good photography. I was there, and one of the members of the advisory group came up. We were about an hour away from firing. This was a fellow named Fran Porzel, who was an expert in ground shock, and shock measurements, and so on. He was from Battelle, in Chicago. He came up and said, "Gerry, I'm nervous about that tunnel, about the containment. There are only thirteen feet of sandbags in there." I said, "Oh yes, we all know that." He said, "I'm not sure that's going to hold."

And then he began to pace back and forth. And he kept talking and walking beside me. He said, "Can't you just hold the shot for a few days? We'll go back in and put some more sandbags in." I said, "How many sandbags would you put in? What would you do?" and so on. Well, he wasn't sure. I said, "Well, Fran, I'll tell you. We've worked on this thing for a year. We've had the best advice we could get, including last night. If we open that tunnel up to do anything, we have to start over, repeat all our dry runs, and check everything out again. We'd have to do everything. I don't know how long it would take us to get it straightened out so we could get back to a shot day. And this is the end of the operation. We're holding the operation to get this shot off, and it's an experiment. We could easily lose the whole thing, administratively, and I don't want to do that."

He said, "You know if that blows out, everybody here will say they knew it was going to happen, and it will be your neck that will be out." I said, "Well, that's my job. But there's just no way that I can see to postpone. We're committed now. We have to go ahead." I said, "I appreciate your bringing it up. There really isn't a choice. If we cancel it now we might not get another crack at it." But he really put the heat on me.

And of course, as it turned out, it worked perfectly, but that's just a bit of history, and he could have turned out to be right. But we had done everything we knew to do. And God knows what would have happened if we had shut down. I think if we hadn't fired at that time we probably would not have gone to underground testing. I think it's unlikely. The next year we entered into the nuclear test moratorium. We wouldn't have had time to do a test, set it up, and do enough to learn any more about it.

But we did fire it, and it was well established by the end of the next year, as a technique. We were lucky in hindsight, as it turns out. The seal was just a simple spiral. We only had those thirteen feet of sandbags, and a steel door to stop gases, but the stemming worked perfectly. We got overconfident later and had some problems, but Rainier did work very well.

And as it turned out, we recovered the radiochemical samples. The rock had frozen right away because the cavity collapsed, so we never did find molten rock. But we were concerned about tapping into molten rock. The question was, "How can we test that out?" Naturally we went to the vulcanologists, and they told us that no one ever drilled into a molten zone. The way they got their samples was to wait for the molten rock to come to the surface so they could scoop it out in a bucket.

About that time there was an eruption on Kilauea Iki in which a pool of lava some three hundred feet deep was formed, and later a thin crust about a twenty feet or so thick formed. So we sent some guys from the Lab to drill through the crust and collect samples, which they did. They only had to drill through twenty feet of stuff, but to get to a suitable location they had to walk out on this crusty lava flow for several hundred feet. Don Rawson headed the group out there. They had a contract driller and crew, but they were with them. That experience convinced me that at Livermore you could get somebody to volunteer for anything.

Carothers: Gary, when did you first became involved in the containment business?

Higgins: It was at the Laboratory, not at the Test Site, and it was almost coincident with the firing of Rainier. Gerry Johnson, who was then the Division Leader of the test organization, I think then called Test Division, was working with Bill Ogle and Al Graves from Los Alamos, who were deeply involved in the conduct of the whole Plumbbob operation. Gerry went to Chemistry Division, and what Gerry wanted was someone to look into the question of how you would do a radiochemical yield measurement on Rainier, or a test like Rainier. I was in the radiochemistry group of the Chemistry Division, and in the heavy elements part of the group. My responsibility was the separation of the plutonium and transplutonic elements from the debris samples from the atmospheric shots.

So, I came into this picture just about the month Rainier was fired. I didn't know a thing about what the underground effects of a nuclear detonation would be, so I thought I would go talk to some experts, who obviously would know. And so I began to talk to experts at Los Alamos and at Livermore. It turned out that all of the experts had not come to a consensus. There was a range of expectations. At one extreme was the prediction, or guess, that Rainier would produce a bubble of molten rock about a meter in radius, and that the debris would all be contained in that lava.

Carothers: But Gary, you can just look at the calories available and know that there will be more molten rock than a few tons.

Higgins: You'd think so. At the other extreme there was the expectation that there would be something like a 100 meter void, and the debris would be contained in a thin shell of glass lining that void. This was about the period of time when the French science fiction writer, Camille Rougeron, who made his living selling these Jules Vern type ideas to the popular press, published an article that said if you detonated a nuclear explosion underground, in rock, you'd get a glass bubble full of steam, and you could then power generators with that steam for a very long time. That was before we'd ever done anything in the Plowshare program.

Carothers: Who were these experts you talked to?

Higgins: Gene Pelsor was the one in Livermore that I particularly remember, because his prediction was, within the uncertainty of the yield, correct about both the size of the void that would be produced, and the approximate amount of shock-melted material. His arithmetic, the details of how he arrived at the numbers were incorrect, but with self-canceling errors there were enough wrong things that his conclusions ended up being pretty close to right.

Carothers: If you have enough wrong things some of them will make the answer too big, and some of them will have the effect of making it too small, so you might come close to the right answer?

Higgins: Yes. The guys who were really far off were the ones who made one mistake and got everything else perfectly to maybe four significant figures.

One of the people who made an estimate was Stanley Ulam, at Los Alamos, who was a theoretical type person. He made one very simple mistake, and I'm inferring this from what other people said; he did not say this to me. His error was to neglect the vaporization of rock, in that he went directly from a solid to a Fermi gas, and back to a solid. That neglects the region of condensed molecular gases. Half the energy of vaporization of rock is in the phase transitions from solid to vapor. There's another half that takes the rock from vapor to ionized gas. So, the first half is a very important step function in the pressure-volume relationship, but it's easy to leave it out because nothing very important physically is going on except the change of phase. That was the small estimate.

The very largest estimate came from Bill Libby, and his was not very different from Gene Pelsor's. The reason it was larger was because he did not leave any strength in the solid. Gene let the solid be an elastic solid forever; what Libby did was pretend it was a liquid with a back pressure, but no strength. The way to say that correctly is to say he used a Poisson's ratio of 0.5 instead of 0.3, as it really is. Which is kind of a dumb thing, but that does make the cavity get bigger.

Carothers: He would have been correct if Rainier had been fired in water.

Higgins: Yes. It would have been precise in water until the rebound occurred. Rebound occurs in water too, and it causes a recompression, so the bubble rings. It oscillates with a period that is proportional to the depth, which is a kind of restoring force.

Carothers: If the energy from the device wasn't going to melt much rock, where did they think that energy was going to go?

Higgins: Well, you and I think it's self-evident that there would be a lot of melt. But, naively, people thought that all of the energy would go out in the seismic wave. If you fired a kiloton explosion you'd get a kiloton seismic wave. If the earth were perfectly elastic, that's what would happen. But it's not perfectly elastic, and that isn't what happens. It's rather fortunate that only something like one part in ten to the fourth of the total energy ultimately gets into the seismic wave as energy.

Dave Griggs, who has passed away, was active in the seismic community, and was the author of the first paper that made an absolute calibration of the seismic magnitudes of earthquakes translated into energy. It was based on the nuclear explosions carried out in the South Pacific - - I believe it was the 1954 series. If the conversion were not so small, the convergence of the waves at the antipode of the explosion would have been sufficient to cause an eruption, like a volcano.

That was if all the energy had gone into seismic energy. The people in the seismic community had calculated that if all the energy went out around the world and came back into the same place at the antipode, and none of it were lost, there would be another explosion. It wouldn't be any bigger than the detonation, but if the energy went out one hundred percent elastically, it would be as big as - - or a little smaller than - - the original explosion. So all you would do then, if you wanted to destroy a target, was to go to its exact seismic antipode, fire off the appropriate energy device, and say, "Who, me?"

Carothers: That sort of thing sounds like the days of the high altitude tests, where the thought was that you would detonate a device at some altitude here, and all the ionized particles would going running down the magnetic field lines and cover up the enemy's radar over there.

Higgins: Right. You got it. But the business of the earthquake, and the elastic world was a real concern. They still compute the elastic equivalence of earthquake yields as the the absolute magnitude. If you take the elastically coupled value for a magnitude six earthquake, it's way less than one kiloton. And so there was real concern that one kiloton, if elastically coupled, would be like a magnitude seven earthquake. A magnitude seven earthquake, it causes some damage. But the real world is not elastic, which is of some annoyance to those who like to calculate things, because it would be much simpler if it were.

By the time Rainier was fired there was a group of consultants who were assembled, ad hoc at first, and then that group was was formalized more or less, to advise the AEC, or the Manager of the Nevada Operations Office, about such matters as safety. Dave Griggs was on that committee. He got in by being in the seismic community, and being an Air Force consultant. He brought George Kennedy along because George had been a student of George Morey's, and knew about the melting of rocks and so on.

Carothers: People certainly knew some things about the response of the earth, because for years and years lots and lots of people had set off thousands and thousands of explosive charges. All kinds of sizes, and in all kinds of places, and they knew the earth didn't respond that elastically. So what were these people in such an uproar about?

Higgins: Well, precisely the same thing that they were in such an uproar about on things like Three Mile Island. It was the unknown feature. And the people involved in the Test Program at that time really weren't in the same community as the people who had all of this experience with high explosives. There were a few individuals who carried that experience over. One was a guy named Roy Goranson, in the very early days at the Laboratory, who had spent a lot of his life with high pressure steam, and steam explosions, and equations of state of water and rocks.

Gerry Johnson had the experience of working with artillery in the Navy, and he knew from his experience what the detonation of a thousand pounds of TNT would do, and how it would scale. He had HE experiments done prior to Rainier. They tried to produce containment, and discovered one of the differences between TNT and nuclear, which is the residual gas.

You can't contain high explosives unless you can also contain lots of residual gas. For every pound of high explosive, you produce a pound or so of residual gas. In a nuclear explosion the rock vaporizes and does all of its mechanical work, then as soon as it cools off it goes back to be some kind of rock again, and the gas pressure is gone. So, the containment of the nuclear debris is a much simpler, although more sophisticated, problem than the containment of a high explosive charge.

It's really extremely difficult to contain high explosives. People in oil fields and in mining are painfully aware of that problem. For that reason they have criteria for safety and for detonations that are very different from those for the safety and containment of nuclear explosions. The difference is understood by a few people, but most people who grow up in one community don't comprehend the problems that people in the other community face.

People who have grown up thinking nuclear containment cannot understand why the oil field people want explosives with the highest possible specific energy with the lowest possible residual gases - - they're extremely fond of nitroglycerine, for example, which is terribly hazardous to handle. So you say, "Why don't you use something like ammonium nitrate? It's a lot safer." And they say, "Yeah, but we can't get enough in there to shatter the rock." "But why do you want to shatter the rock? That just makes little tiny particles, and they'll plug up. What you want are fractures." They say, "Yeah, but if we do that, it blows out the top of the hole." "Well, then why don't you stem it?" "Oh, you can't stem it."

They don't shoot stemmed shots. They put the explosive down, detonate it, and let it blow out. They don't try, because they have never been successful in containing the gases. Therefore, they don't use some of the most valuable products of the explosion. The high pressure gas would do them more benefit than the shockwave, but they don't use it.

But, back to the rocks. It was difficult to select which expert to believe, except I could reject there would be no bubble. All of them shared one thing; there would be molten rock, and I believed that. The first conclusion I came to was that it was reasonable from all points of view to expect the debris to be in fused rock. And if the molten rock cooled, there would be glass. If it stayed molten, then the question would be how would you sample it. The obvious

answer was, you would need to drill into it. But, without measuring we had no idea how complete or how good the samples would be, or how efficient or effective the sampling would be.

We had some fused rock from the ground surface of a number of near-surface bursts, including Trinity, so we could do the chemistry on fused rock. We had done all of those things with samples picked up from the surface. But we had no idea what concentration of debris to expect in the samples we hoped to get. We didn't know whether we were going to need a gram or a kilogram. And that, of course, depended on how much rock got melted per kiloton. We did do some sensible estimates, again using Gene Pelsor's calculations primarily.

I believe Gene was asked to attempt to fully contain the explosion. Not maybe for the reasons that we want it contained now, but that was his objective. The stemming procedure on Rainier had been designed as a rather elaborate spiral buttonhook. The philosophy, expressed in different ways by different people, was that the radioactive debris would be charging around the tunnel at velocity V, and by the time it went around the spiral, the seismic shockwave would have come across and closed off the tunnel, trapping the radioactive debris. The placement of the sandbag plug, I believe, was to stop jets. The idea that the buttonhook would achieve containment neglected a lot of things. It worked for all the wrong reasons, but it worked, that one time at least, very well, and it established that containment could happen. I believe that a lot of Gene's work was not recognized as being as good as it was, considering how little anybody really knew.

Carothers: Somebody wanted to try to contain the shot, and that was probably Gerry Johnson.

Higgins: Yes. I think it was Gerry, although Al Graves had made the statement, before this was done, that we weren't going to be able to continue to carry out atmospheric nuclear tests forever, and we really ought to find an alternative method. He didn't say it should be underground, or in deep space, or how. There were actually four ideas that were kicked around in '56 and '57. Underground was one, deep space was two, deep ocean was three. Under the ice cap, either in the Antarctic or under the Greenland ice cap, was the fourth possible way of carrying out tests without

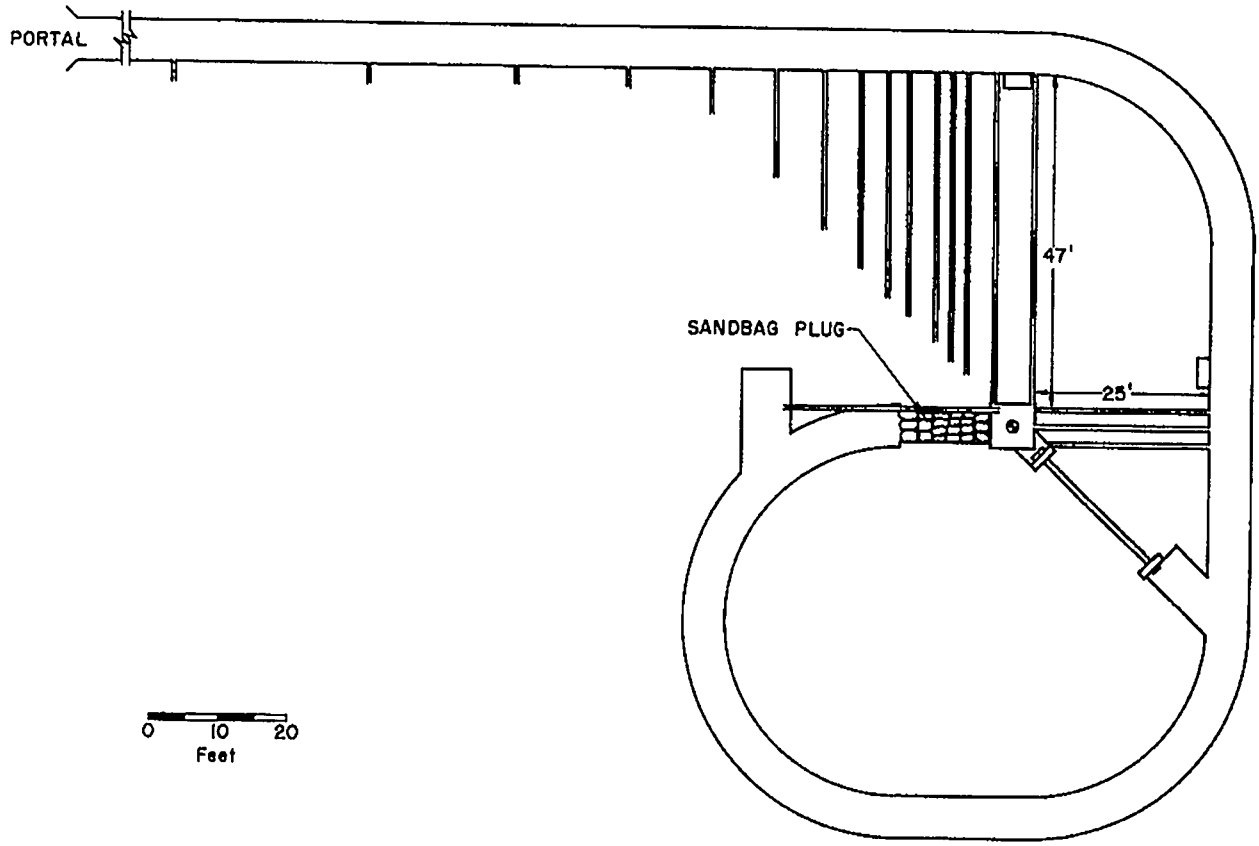
contaminating the environment in any gross way. We might criticize the ice cap or the ocean ways as contaminating, but at that time, in that period, that looked like complete containment.

Well, Rainier was fired. The next thing then was to find someone who could drill into it. In the fifties a lot of our drilling was done by contract drillers, and most of the early drilling underground was by E. J. Longyear people, and people that they hired. The Longyear people were having real difficulty, because the drillers had to be cleared; you had to have a green badge to work with the radioactive debris in those days. To find drillers that they could get a long enough history on to get them a Q clearance was not easy. Drillers, by habit, or choice, or circumstance, don't stay in one place for very many months at a time. They go from crew to crew, and place to place, wherever the work is good and their fancy takes them.

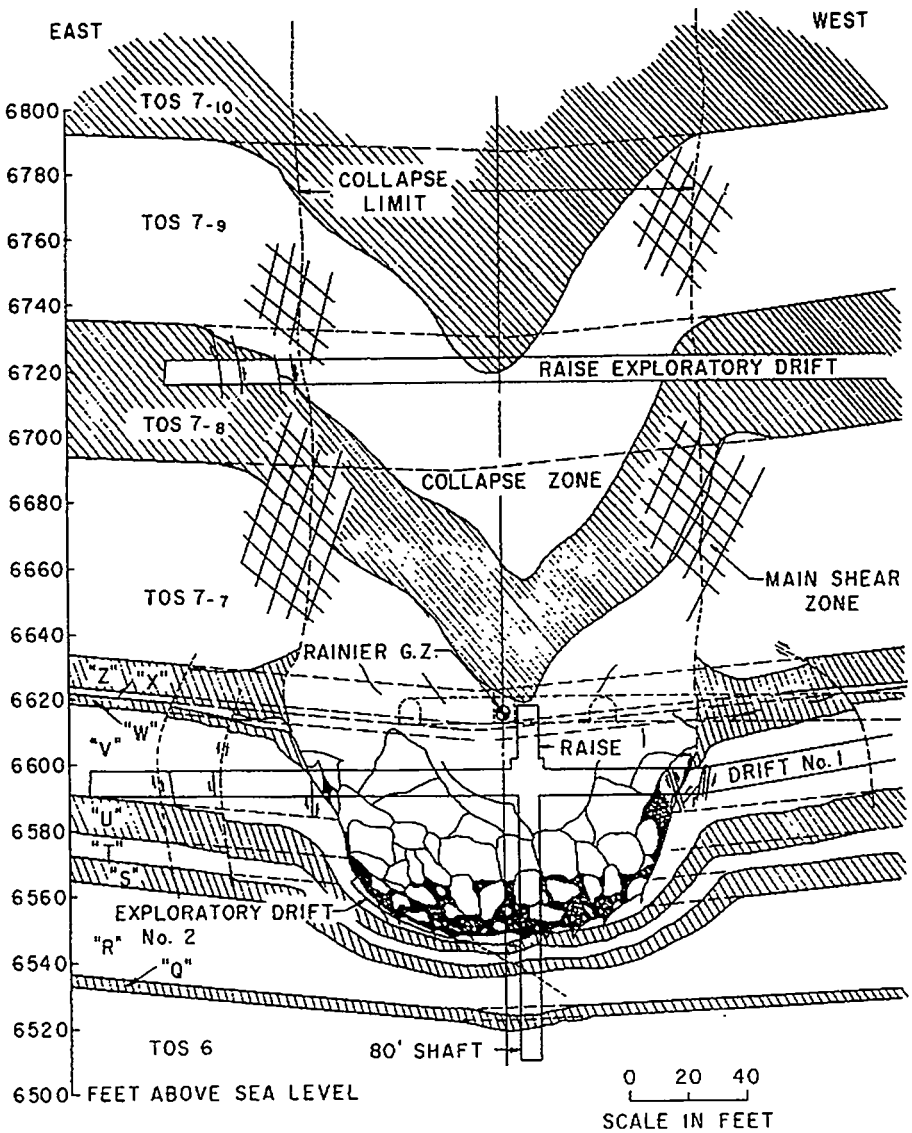
Diamond drillers, who are a group that we found were experienced in the small drills we needed for the underground rigs, were used to doing ore deposit definition for the mining companies all over the world. So, most of the drillers we had were non-U.S. citizens, which made it even harder to get clearances for them. We had a real problem getting three men to handle each of the three shifts - - actually it means four shifts because we were going to go seven days a week.

Finding that number of drillers who were Q-cleared was really very difficult. Add to that the gossip that was going back and forth in the union halls, or in the beer halls maybe, about the possibility of thousands of pounds per square inch of steam, and such high radiation that they'd be sterilized forever. One fellow told me he was told that the samples they would recover, if they ever did get to where they were supposed to, were going to be so radioactive that the whole crew on that shift was going to be killed. Well, it makes it real hard to get people to do that, no matter how much you try to convince them, or talk about what to expect. And, we weren't all that sure ourselves. We knew the business about the radioactivity wasn't true, but beyond that we didn't really know what the conditions would be when we got there.

Carothers: What was your role on the reentry?



Preshot tunnel configuration in region of detonation.



Geological cross section at Rainier.

Higgins: People didn't have administratively designated roles in those days. My role was sort of keeping tabs of what was observed, and reporting it, and asking questions. I talked to the drillers, and the geologists. We had a geologist by then, and the Geological Survey was involved. And I did chemistry measurements. A lot of those. I still did that part of it.

Our first attempts were to go into the tunnel, establish an alcove, and drill horizontally. I found out drills don't easily do that; they don't drill horizontally, because the drill stem droops. So, there was the issue of, well, where is the drill?.

Before we had penetrated the radioactive zone in the tunnel we had started drilling from the surface, but that was 860 feet up. For reasons I've never been able to understand, they cored all the way from the surface instead of just drilling in, and then switching to a core bit. The communication between ourselves and the construction people in the field was not good. Perhaps we asked them to core from the surface, not realizing that they could very easily switch from a spade bit that would have drilled much faster and have gotten down to the ground zero zone very early, to a core bit.

However, the hole from the surface never intercepted any of the radioactive debris because it came out in the chimney, and all the drilling fluid ran out of the hole. The drillers maintained that they could not drill without fluid because the drill bits would not survive if there was no fluid in the hole. In those days they didn't have reverse circulation drilling. They only had forward circulation, which meant that the fluid came out behind the bit. So, if there was nothing around the bit to confine the drilling fluid, it was not cooling the bit; it was cooling the rock wherever it ran to. We really had to learn the drilling business before we could ask the right questions, and we didn't know them then.

I also found out that the progress in drilling in the tunnel was painfully slow. The drill would be turning around and around for days on end, but it never got anywhere. I found out the reason that it wasn't getting anywhere was that the drillers didn't want it to. As I said, there were rumors, including the one that this cavity might contain thousands of pounds per square inch of steam.

Carothers: Well, Gary, the drillers felt they were going to drill into a volcano, filled with radioactive steam and molten rock. Would you want to drill into something like that, which would spew all over you and kill you and all of the members of your drill crew?

Higgins: Of course not. So, they would turn the drill, but they'd never push. We had a huge cavern worn out of the side of the alcove into the tuff, but it went in only a few feet. I think there was a lot more gossip and misinformation than we in the Laboratory ever heard. I do know that there were drillers who would make all kinds of excuses for not going on that particular drilling crew.

Flangas: Well, there was always some concern over the unknown, but for those of us who came out of the mining business, we were used to some risk. Now, there is certainly a difference between intelligent risk and recklessness, and some of us know the difference. Gary Higgins became an integral part of that crew, and I personally had a great deal of confidence in his judgment and his experience. He didn't try to butt into the actual mechanics of what we were doing, but he was there to advise us on the things we didn't know about. It was a very, very close relationship. We trusted him, and his judgment was good.

Carothers: His story is that it took a long time to drill back into the cavity, because the drillers weren't very anxious to get there.

Flangas: There may have been some of that - - some of the miners were that way. Occasionally you would run into somebody who would be a little bit spooked, but once it was explained to me, and I had a fairly decent grasp of what to expect, and as long as the leadership was confident in what they were doing, our people just followed.

Higgins: Well, finally, after a couple of months of drilling, and I think it was close to a year after Rainier was fired, because we didn't start immediately, the drillers penetrated, unexpectedly, a radioactive zone. That got radioactive debris into the tunnel, and we had to shut down because the rad-safe people said, "You've contaminated everything here." It was great news to me, but sad news to the drillers.

I went down and tried to find some of the debris, along with some of the people from the NTS-LLL contingent. We finally sorted out a bunch of sand and stuff, and when we took it all apart a grain

at a time we could find some little black pieces of glass that seemed to be more radioactive then the rest. It was radioactive enough to be an annoyance, but not a big enough sample to do any kind of measurements on. Maybe we could have, but we didn't try. But the key thing that had happened was that we had penetrated into a radioactive zone, and there was no high pressure steam in it. Now, it was hot - - it was hot enough so if they shut off the circulation water it would almost boil. The water that was coming out was too hot to hold your hand in. But the drillers then had great confidence it wasn't going to erupt, and so, within the next two weeks they finally hit a mass of lava. It was black frothy rock, and they got cores of it.

We found a piece of core that was gray and gunky, but it had a radioactive peak in it. And I said, "I wonder why that is?" So, I put a rubber glove on and squished it. I found little glass beads that were pendant shaped. From the shape, and the fact that it was now solid glass, we could infer that it had been hanging from something at some time. We said, "If it was hanging from something, it must have been in a void space. It was liquid, and it has the shape of a liquid drop, so there couldn't have been anything against it."

And so we began to reconstruct that after the cavity had grown underground it had stood there for a little while - - at least long enough for these glass beads to solidify. By a process of reconstruction we worked out about how long that had to have been. We confirmed that by measuring the ratio of some of the gas-precursed radioisotopes that were included in those glass pendants to what they were in unfractionated radioactive debris. It worked out that it was something between one and a half and four minutes that this glass had been pseudostable. The whole thing stood there before the roof caved in.

We also looked at the amount of water that was dissolved in the glass. Roy Goranson had produced a table, and published it back in the thirties, of the solubility of steam in silica glass. If you quenched a glass in water at ten bars of pressure, what percent of that glass would be water, and how much water would remain as vapor? He had the whole table of solubility of water in silica glass, and we got the pressure of the Rainier cavity as being about 45 bars. We observed that 45 bars was not all that different from the overburden pressure, at that depth of burst. If one hypothesizes that the steam expands until it's in equilibrium with the external

pressure, then all of the pieces balance. So, that became an adopted hypothesis for containment; that the cavity size is such that the pressure would equal the overburden pressure. It turns out that's probably not right, exactly, but it was a good working hypothesis. We now say stress instead of pressure, and it is probably even correct.

The Rainier cavity was about fifty-five or fifty-six feet in radius. That was the size it would have been if the material in the first meter or so, around the explosion, were transformed into steam and other gases, and they expanded until the pressure was somewhere near the overburden pressure. That was the concept of a balloon, or bubble, blowing up inside a pile of blocks, with no rock strength involved, and that was pretty much what the model was that was used for a lot of the early evaluations of containment. It's wrong, and it's wrong in a lot of different ways, but it was extremely useful.

About a year or so after Rainier the Hardtack II series started, and we sampled several other underground shots - - Logan, Blanca, Evans, Neptune. We did not explore, in any detail, any of those shots. We simply drilled enough to get rad-chem samples. We were still going into the tunnel and drilling horizontally.

Carothers: When you were drilling horizontally, were you then getting your samples from the bottom of the cavity, or from the sides?

Higgins: They came from the bottom of the cavity. From what we discovered on Rainier we designed the scaling law that says the radius of the cavity is 55 times W to the 1/3 feet, with W, the yield, in kilotons. That was where we found the puddle that gave us a good, big sample. So, the target we drilled for was based on design yield and 55 times W to the 1/3 feet. We usually aimed a little above that, with the idea that if we missed it on the high side, as the drill progressed across the cavity it would go through the puddle on the far side. And we would carefully log, and almost always saw two blips on the radioactivity versus depth of penetration plot. And we then said, "That's the cavity boundary." And on the far side we'd say, "Well, the drill probably carried some radioactivity along with it, so the far side is probably a little too far." So you subtract a little from that, and do things like that.

Those measurements were recorded, of course, and people began to say, "My, isn't this interesting that these things scale together?" Then they said, "What is the yield that you get from the cavity radius by using the 55 W to the 1/3 feet law backwards?" It wasn't very good, but it was a number.

We went through Plumbbob and Hardtack II without really understanding anything about containment. After Hardtack there was the moratorium, and during that time we did the post-shot exploration of Rainier, in great detail. That was to measure accurately the boundaries of the chimney region and the cavity region, and all of the physical parameters of the shot. We wanted to measure things like the temperature, integrate the thermal energy, and locate where all of the energy was deposited permanently. We balanced the total release, as we measured it from the rad-chem yield, to the thermal energy to within about 92 percent or so. We inferred that the energy that went into producing fractures, which we couldn't measure, was in addition to that. The seismic energy then was some number that was very small, which from measuring the seismic wave you could also say was true. So, in a sense, we balanced the total energy of the shot to within the precision of the various measurements. Which is a satisfying thing for scientists to do.

We began to understand, in the course of those drillings, where the radioactive debris was distributed. Not only the kind we wanted for the rad-chem samples, but also there were, in these logs of radioactivity versus distance, blips that were clearly at larger radii than the inferred cavity radius.

Carothers: You did find cavity material at some distance from the cavity boundary? Did that material go along bedding planes, or did you think there were fractures in the rock itself?

Higgins: Well, we didn't recognize bedding planes then. We did recognize faults. And there was a huge fault not far from the Rainier shot point. At the time that the tunnel was mined some drift in the B tunnel complex - - I believe it was 12B-02 - - was terminated. It was designed to go into the mountain a little further than it did, but it ran into this fault that was so large that you could look down it. You could literally bend over and look in, and here was this hole in the mountain that went off in the distance and you couldn't see how far it went. It was a real fault, not like the things

we map these days; it was empty. What we did was back up from this big open fault and mine the buttonhook, and they put the muck from the ground zero room down into the fault. And it just disappeared down there. We didn't have to haul it out to the portal of the tunnel. That was a big fault.

The reason I bring the fault up is that in the post-shot exploration that we did in such detail, we found that the center of all of the energy, both the radioactive radius from ground zero, and the thermal regime, was displaced by a couple of meters toward the fault. It was clear that the presence of that fault influenced the growth of the cavity, and there wasn't real symmetry. One would like to say everything was symmetric about the detonation point, but it really wasn't.

We also found that if you looked in detail, this cavity that we were fond of drawing with a compass as a nice sphere really had bumps and wiggles, and had cracks that went out. Some of those cracks were filled with various and sundry bits of what had been molten rock. We also found evidence of enough hot vapor having gone out into some of the fractures to change the color of the rock on each side of the fracture. It had boiled water out of the rock, but there was no melt there. So, we knew that the simple picture of a glass lined sphere, like a Japanese fishing float, the kind you see hung in the seafood restaurants, really wasn't what the inside of the cavity looked like. It was really pretty bumpy and wiggly, and probably very leaky.

When we had the first core holes we saw the blip on each side of where the cavity was, but when we mined that out we found a jumble of slabs. They were mostly planar slabs of melt-covered rocks, folded over each other. When the geologist identified where these slabs had come from, it turned out they had come from a hundred or so meters above the detonation point. Then we began to have a picture that there was the growth of the cavity, then a pseudo-stable period when it sat there and nothing happened except some leaking of the high pressure gases pushing out, and then slabs and bits and pieces falling in, jumbling in helter-skelter, and the steam being quenched by pieces that were fairly large. It was not a hail of small pieces of sand, but pretty big pieces that were falling in, and the steam probably migrated some distance upward. In fact we found evidence for some gas radioactivities in the rubble three or four cavity radii above the detonation point.

Carothers: This picture you're giving of these slabs of material falling in doesn't fit very well with the accepted picture of a collapse. "The geophones were quiet, and then it collapsed, and the collapse progressed upward at whatever feet per second." It's not exactly a plug falling in, but it occurs very rapidly, and the picture is that the layers of rock would still be basically intact, just displaced down some distance. You don't describe anything like that.

Higgins: No. That's right. What we observed, and I would say in an almost differential sense, was quite different from what we inferred from the readings on the instruments. And I see still a discrepancy between the detailed reentry mining observations from Rainier, and from the general picture we get from the observations of cables breaking and from the surface. I think that discrepancy still exists to a degree. And you identify it very specifically.

I would put this point up, and it's one that has disturbed me and continues to disturb me. We have only investigated in great detail one event, and that's Rainier. We've never investigated in great detail any other one event. In the first place it's quite costly. It cost us about as much to do the kind of post-shot investigation that we did on Rainier as it did to fire the shot in the first place. So, it like doubled the cost.

Now, I must say that in recent years the line-of-sight pipe tunnel explorations have in some respects exceeded the information that was learned from Rainier. But it is not so much about the containment of the shot as about the containment of the pipe, and the phenomena associated with the pipe closure. When I said we've never explored another shot in such detail, I meant in all the containment aspects in general. In other, detailed areas, I think DNA has exceeded Rainier by quite a bit.

Carothers: Well, you had the moratorium going for you, Gary. People didn't have anything else to do. We wanted to keep the miners busy, we wanted to keep Gary Higgins busy, and so we let them go dig around in the mountain.

Higgins: Precisely. And keeping the miners occupied was a very important thing. During the moratorium a number of professional people decided to abandon the Test Program. That disturbed a lot of people who felt an obligation to maintain the defense posture that we had because of our nuclear weapons capability.

And so they asked themselves, "How far can this loss of personnel go before we lose the capability to resume, should we decide to resume?"

There were a number of answers to the question, but among the answers that emerged was the fact that there were other skills than physics and mathematics and chemistry that we would be losing, and one of those was our mining capability and our drilling capability. Both of those skills had evolved well beyond, and different from, the common industrial practice. In other words, any miner wasn't adequate. Or any driller. Witness the fact that we'd sat there and turned to the right with no forward progress on that first Rainier hole for two months or so. It was a question of having other kind of skills that were as important as the scientific skills.

So, during the moratorium, we spent a lot of effort trying to understand what had happened in the Rainier cavity. The business of what goes on in a cavity went through a history like that in a lot of technical fields. There was the first evaluation, and a simple model was generated, or invented, or selected from among a lot of proposals. That model fit a lot of observations, so we said, "Okay, we understand this part of the explosion phenomenology. We won't devote much time to doing a lot more investigations, because they are very difficult to do."

And they are difficult because the stress levels within the area where the cavity is formed run not just to kilobars, but to megabars and above. So, the measurement techniques must be very sophisticated. The region that's involved is small, and things are diverging very rapidly in space, so any measurement instrument has to be kind of tiny. And everything goes on in extremely short periods of time, so getting signals that are meaningful out from that region is extremely difficult. Getting a fast signal out means a big co-ax, and a big co-ax means a big void or something like that in the very small region. That is kind of contradictory to the idea of measuring what is happening in that region without disturbing it. There are a lot of contradictory requirements, or conflicting requirements, when you try to make such measurements.

Carothers: In your work on Rainier there were probably several things you wanted to do. Certainly you wanted to do radiochemical analyses to get the yield. What effort was devoted to

trying to understand what happened to the rock materials themselves under the high pressures and high temperatures that had existed?

Higgins: The primary charge we had was to be able to do on underground shots the same measurements we'd been doing in atmospheric testing. So, that was the primary purpose of our efforts. In order to fulfill that primary objective we wanted to know something about the mechanics, and the chemistry, of how the samples we were recovering had been created. The basic purpose was still to diagnose the performance of the explosive, not to know how to contain it. The containment concern really didn't come up until much later.

We were extremely curious about what had happened to the native material, and we did a lot of different measurements. One of the first things we found on Rainier was a lot of glass, which was the tuff that had been melted and then quenched. We did radiochemical analyses for a lot of different chemical species to determine how much total rock had been melted, and how well mixed that melted rock was with the device components themselves. Those conditions influenced how the device components would behave after the shot, and what they would be like when we went back and found the samples. We pretty well knew, from all kinds of laboratory and atmospheric test experience, what the immediate surroundings were going to be, and what temperatures and pressures things were going to be heated to. It wasn't like working in total darkness. We knew that the initial temperatures and pressures were going to be so high that the material present would be disassociated into electrons and nuclei, and that there really wouldn't be any material properties, other than those of a so-called Fermi gas.

Carothers: That doesn't last long.

Higgins: It doesn't last even a microsecond. Some reports on containment describe what's going on in the first microsecond as if that's a very short time. That's a long, long time compared to some of the things that go on. The Fermi gas very quickly, in the first tenth of a microsecond, probably has begun to expand enough so a genuine shock has developed. That shock is a really strong shock, well above a megabar. The rock is vaporized by it, and even though the gas may not be fully ionized, it's still partly ionized, at least once or twice, so the chemistry's still not important.

Somewhere out about a meter or two meters from one kiloton enough energy has been absorbed, and there's been enough spherical divergence of the shock wave so the pressure level has gone down to where the kind of rock that's there is important. In the model, that first simple minded model, what we used to do was say, "Okay, the first meter that surrounds the explosion is made out of iron." We had a fairly good equation of state for iron, and we knew what pressures would be developed if you shocked iron to ten megabars. So, we started all our calculations, whether the detonation was in limestone, or oil shale, or Nevada tuff, or alluvium, with iron out to the first meter. We put the whole energy of the explosion into that. Of course, if you do that, for most of the explosives we talk about that means the composition of the explosive itself doesn't really make a lot of difference.

A sphere of iron with a one meter radius is like ten times pi tons, so you've got thirty or forty tons of iron to mix with the device. You mix in a small number of pounds of whatever and it doesn't make a lot of difference. So, that assumption was very useful for generating the correct shock out in the rock where we could make decent measurements and the coaxial cables didn't get banged so quick that we couldn't get the signals out. They confirmed that what we'd done by putting in the meter of iron was right. So, okay, what was in that first meter didn't make any difference.

All of that model is correct, except that after the material has been shocked, it does something. It's left behind as very high pressure atoms and electrons, but it doesn't stay that way. The electrons and atoms that have been disassociated by the shock, and other things, are going to recombine, and they don't really care what form they were in before they were disassociated. They go back to a form that is consistent with their environment at the time they are being born. The electrons don't care that they were in tuff to start with; they're very happy going back and becoming methane, for instance.

The little bubbles that were frozen inside the glass on Rainier were microsamples of the cosmos in which they were formed. You don't know in the stage of expansion when that glass becomes solid and the bubbles were trapped, but you do know that whenever it did get solid, it was a closed sample. So, the analysis of those glass samples showed us a number of things.

We took the glass, broke it into little chips, and examined them under the microscope to find which had closed bubbles. We put those in a vacuum system, heated them, and when the glass melted the bubbles burst, and then we analyzed what the bubbles contained. It turned out that what was in them was mostly water vapor, which, I would say, was not surprising.

Carothers: You refer to the material you recover from the cavity as "glass." Why do you call it that? It doesn't look like glass.

Higgins: No, it doesn't look like what we think of as glass, but in fact it is glass. We had established that early through some work with consultants at the Laboratory, in several ways. One was to take some of the initial material we had recovered from Rainier, and do physical measurements on it; measure its density, its index of refraction, and so on.

When you look at it through a low power microscope, it is just like window glass. The reason, when we look at it in a gross sense, that it is all black is that it has a whole range of size of tiny bubbules in it that absorb all the wave lengths of light. Plus there are some inclusions of metals, and other things. If you look at it in a thin section it doesn't look black any more. First, it looks sort of dark green. As you get it thinner it begins to look yellow, and then when you get it down very thin it's perfectly transparent. You can see through it, with the individual bubbles in it visible. Those bubbles are remnants of the steam that was in excess of that required for saturation.

Professor George Morey of the United States Geological Survey, who was then in his late seventies or early eighties, was intrigued with the whole of the phenomenology of the creation of lava. He had worked for many years as a geochemist, first in the Geologic Survey, and then after his retirement, at the Carnegie Institute. Then, when they forced him to retire, he went back as Emeritus Scientist for the USGS.

He was very intrigued with the geochemical processes that go on in ground water, and how hot water around volcanos and fumaroles really transports earth from place to place at a very large rate - - a lot larger than we mortals, who are here for an instant in geologic time, realize. If that water is flowing from there to here, it's also bringing along huge quantities of rock. And pretty soon, as the water evaporates and goes away, the rock will grow here, and

it will grow in whatever form best fits this environment. Professor Morey spent the last twenty years of his retirement searching out and quantifying these effects.

Well, what was going on in Rainier, and the underground explosions in general, was a rapid speeding up all the processes he was interested in. So, he was intrigued by the kind of glass we would form from an ash. Volcanic tuff was spewed out of the ground as ash. But on Rainier it had recondensed, after the shot, as glass. Why did it come out to be glass, and not go back to being ash? So he got involved in this study of the glass.

One of his students, George Kennedy, from the UCLA Institute of Geophysics, also got involved. And there was another fellow, named David Griggs, who had been involved in the test program from before Hiroshima and Nagasaki He was the principal geoscientist involved with the Air Force advisory panel. Professor Morey, George Kennedy, and Dave Griggs were involved in not only determining that glass was produced from the condensation of the molten rock, but also in measuring its index of refraction, and the amount of water vapor that was dissolved in it. From that, and the radius of the cavity, we deduced what the steam pressure must have been to make that kind of glass.

To form glass you need some silica sand. As long as the ratio of silica to the other common earth forming oxides, such as aluminum and calcium and magnesium, is large, the melt when cooled quickly from its liquid state, or quenched, will always form glass. The rate at which that glass changes back to being crystal silica, and alumina, and calcium, depends on how much silica there is. The more silica the longer it will stay glass, but it will change. That process of changing from glass to crystalline form is devitrification.

Glass is a metastable liquid, but it takes a long time to devitrify, and for silica glasses that time is measured in hundreds of thousands of years. At the concentration of silica in the tuffs at the Nevada Test Site, the glass would prefer to be crystalline quartz plus felspars, but the process takes around five hundred thousand to a million years. Those tuffs, as we know from many lead isotope ratio studies, and the fact that they're there as minerals and not as glass

today, are like two, three, four, up to tens of millions of years old. Even so, there are still remnant glasses from the original volcanic outpouring. Not a lot, but there are some.

A nuclear explosion converts the rock close around it to glass, with minor, minor exceptions. And so the generic term is that the "glass" is the initially molten material, from the shot, that cooled very quickly. The amount of glass produced is like a kiloton per kiloton of yield, and that's not too surprising. The energy in the nuclear explosion is just about right so one kiloton of energy will make one kiloton of molten rock. And that's what we find out.

Going back to what goes on the cavity, the other thing we found in those little bubbles in the glass was hydrogen, and oxygen, and a little bit of carbon monoxide, and a little bit of carbon dioxide. There really isn't much carbon in the tuff; there wasn't in the Rainier ground zero area. But, the timbers that held up the tunnel were wood, and all the electronics had rubber and plastic insulation, and plastic foam as a dielectric. If you added it all up, there was enough carbon in the environment to explain the carbon dioxide in the bubbles.

Now, how did the carbon get from plastic to carbon dioxide? Well, if you have this big sea of electrons and atoms, the atom doesn't know whether it came from plastic or rock. A lot of what's around is water, which is hydrogen and oxygen. So, the carbon has a high probability of combining with either oxygen, or even more probably with hydrogen, because there are two hydrogens for every oxygen, so hydrogen is the major material around. So, when you put hydrogen with carbon, you get methane. The carbons have some affinity for each other, so a lot of them go around as two's. And when two's go together, then you get ethane. Sometimes there's an oxygen, so that makes methyl alcohol, or methyl formaldahyde. A whole suite of hydrocarbons gets formed, not because they were there as hydrocarbons to begin with, but because it's probable that they're going to become that in this sea of mostly oxygen and hydrogen with the occasional carbon.

More frequently than carbon there's a silicon, or an aluminum here and there, but not many. For every four oxygens there's one aluminum, or iron, or silicon. So they go back together, and then as they cool they continue to react with each other. One of the things that Russ Duff has noted, and I think he is onto a very

important clue, is that what is happening in the cavity, even at long times, like months, is that these gases are finding each other and reacting.

A not very probable reaction, but an easy example, is where a methane finds a water molecule, a steam molecule, and reacts with it. The oxygen from the water will go with the carbon in the methane, and two hydrogens will get formed. This happens at only very, very high temperatures; as the temperature cools, that reaction goes the other way. Water and methane are the natural products, hydrogen and carbon monoxide are the starting reactants. That particular reaction occurs at high temperatures, but stops abruptly at like 1300 degrees centigrade.

If you analyze a lot of these products, you can look at the ratios of the chemical compounds and derive a temperature where they must have been "frozen." They call it "frozen equilibrium," because the rates of reaction are exponential. There's an old rule of thumb which we chemists use, which is not quite accurate, but it demonstrates the principle: for each ten degrees increase in temperature, you double the rate of reaction. So, it doesn't take a very big change in temperature to have a reaction proceed extremely rapidly, as in seconds or milliseconds, or extremely slowly, as in hours or days. That change can take place as the temperature changes a hundred degrees or so.

The simplest ratio that gives a temperature is the carbon monoxide to carbon dioxide ratio at a given pressure of oxygen and hydrogen. If you look at the ratio of hydrogen to oxygen to water, that gives another method of calculating a temperature. If those two temperatures disagree, then you have a phenomenon you have to explain. It turns out they don't usually disagree, and they haven't in the tests where we have made measurements. They all give a temperature which is consistent with the cavity sample that we had frozen out in the bubbles on Rainier, which was about 900 to a 1000 degrees centigrade. That also turns out to be about where the melting, or softening point of the rock is. So, all of this holds together, sort of.

In retrospect it's what we should have expected, but we still tend to treat the material that the shockwave traverses near the explosion as if it were iron, or rock, or aluminum, or plastic. We forget that the world, and the environment around the explosion,

if you average all the stuff together, is almost half water. Normal tuff, they say, is fifteen percent water, twenty percent water. That's by weight. The molecular weight of water is eighteen. The molecular weight of rock is like sixty or seventy. So, if you take twenty percent of something with a molecular weight of eighteen, and mix it with eighty percent of something with a molecular weight of seventy, the result is that there are more molecules of water than molecules of rock. And so, if something is going to react, the odds are just about even that it is going to react with something from water, and something from rock. So, anything that's going to happen is dominated by the water.

One thing we found on Rainier was some fragments of glass that were formed by having been blown down a fracture, which then squished off. We found such a fracture, and again we didn't recognize its importance. We had this model of a smooth, round cavity with a glass lining; we ignored the fact that at two and a half cavity radii was a fracture containing some glass.

We found this fracture, and said, "Isn't that interesting? I wonder how that glass got down there. Well, it must have been a fracture." And, everybody said, "Yes, it must have been a fracture." But in all of the literature you don't find mention of the glass-lined cavity having spikes radiating out from it, containing products from the center. In the model we mentally smoothed the ball off, and forgot that there were fractures from it.

The point was that in those fractures were glass fragments that were frozen out while probably it was still in contact with the cavity, and they had elemental iron, elemental copper, elemental uranium in them. These metals are extremely reactive. With this sea of oxygen atoms we should have said, "They shouldn't be there." But they were there. Again, we ignored that. It was the exception that should have said our general model was too gross. Chemically, a sea of electrons is about the most reducing thing there can be. In fact, you couldn't get the average chemist to comprehend what a mole of electrons, just electrons, would do.

So, the clues were there. When the cavity forms dynamically, this high stress shockwave goes out, running way ahead of the material. And we know, for example, that shock velocity is greater than particle velocity, almost no matter how high the stress level of

the shockwave is. So, as the shock goes out from the explosion center, it runs ahead of the material, but the material that is behind it is moving at still a pretty high velocity. The shockwave is irreversible; it leaves a portion of its energy behind as heat, which causes this ionization-disassociation that's going on. There are more electrons around than anything else, so everything wants to be reduced to the elemental state, and then start combining. Most of the atoms that are present are oxygen, so most things end up as oxides. It may sound contradictory to say that oxides are reduced, but carbon monoxide is the reduced form, relative to carbon dioxide, and elemental carbon, or graphite, or diamond, is even more reduced.

So, the state of the cavity is highly reducing, and so, for example, if there is copper around the copper will stay pretty much as elemental copper. You don't see big globs of it because it's all vapor, and when it condenses, it condenses a few atoms at a time, dispersed throughout the glass. The black color of the glass is not due to radiation, and it's not due to carbon; it's mostly due to elemental lead and iron in the form of single, or a few, atoms.

What we should have learned, and should have known from the Rainier fractures is that there was a period of time when the cavity was growing, the boundaries were open, fractures were going out, and the volume being interacted with was considerably larger than that which we found when we calculated the steam pressure, and calculated 50, or 55, W to the 1/3rd as the cavity radius.

3

The Moratorium and the Return to Testing

The 1958-1961 moratorium followed Hardtack II. During the moratorium Los Alamos drilled some stockpile holes in Yucca, and Livermore continued with excavations in B tunnel and E tunnel, in Rainier Mesa. Considerable reentry work and explorations were done at the site of the Rainier event. And, little known until many years later, a series of experiments took place which contributed to the knowledge about containment.

Brownlee: There was something that went on during the moratorium which used to be supersecret but isn't anymore. There have been announcements about it, and newspaper stories. That was a series of one-point kind of experiments, and so we had a rather active underground experimental program here at Los Alamos. You didn't see towers, and you didn't see smoke, and you didn't see a lot of things. But out in TA-49 we put things down holes, and fired them.

The yields were just the high explosive yield, essentially, but it was during that period I saw my first stemming collapses, from a whole series of those things. It always happened. We'd shoot one of these things off, and a little while later the stemming would fall down the hole. We were doing them in tuff, so the holes tended to stand, and the stemming would go down. So, it was during the moratorium that I began to appreciate chimneys, and stemming falls.

Bob Newman and I spent an appreciable time fussing about scaling laws. How big a cavity would we make? How much stemming did we have to have to keep everything contained? The difficulty was that the number of people who knew about that program in Los Alamos was minimal. In J Division there was Westerfelt, Newman, Campbell, and a few others, including myself. And of course, in W Division there were the people who were making the devices.

Because of these experiments I continued to get an education in containment during the moratorium, which if you stop to think about it is odd. But it was kept so close that only Campbell and

Newman would talk to me, and they didn't talk to many others at all. I was not allowed to know very many details. The part of it that I knew was that we were doing things that required stemming and containment, and we didn't dare make a mistake. It had to be contained, and we had therefore to be super-conservative. It wasn't like the Test Site. If something floats around here in Los Alamos, everybody in town knows it. There's no way you can escape it. The argument was that we didn't dare go to the Test Site. I thought that was a bit odd, but that was my understanding. We had to do it here because the Russians would know we were doing something if we went somewhere else.

So, at Los Alamos we were learning a little something about underground containment. We talked a lot about scaling laws. We debated whether we needed a depth of burial where there wouldn't be a crater, or what it was we did need. My recollection is we kept debating what it meant, but with people like Campbell in the works those kinds of subtleties were ofttimes scorned. Obviously what we meant was that nothing comes out. So, at those very early times we had already, in a way, defined containment as not one atom out. There was nobody who told us to do it that way.

The scaling laws you could find in the literature were, of course, for chemical explosions, which is actually what we were dealing with, in a practical sense. So they were relevant, in a way. As a result of all that we came to '61 with the conviction that 400 feet times the 1/3rd power of the yield in kilotons was conservative, and worked.

In summary, I would say that more happened during that moratorium that's relevant to containment than you might think. Even though it was hidden, and there weren't very many people involved, there was a continuation of thought. I think we were more ready to test underground than people remember.

There were other activities, at the Test Site, which contributed to the ability to resume testing, should the need arise. Interestingly enough, this effort, on the part of both Laboratories, went into the preparation of underground sites, although the Nuclear Test Ban Treaty was still several years in the future.

Carothers: During the moratorium Livermore had the LRL-Nevada people working at the Test Site. They got some amounts of money, and I presume the Los Alamos testing organization did too. The Livermore people were digging tunnels against the time when there might be something to do with them. What were the Los Alamos people doing?

Brownlee: We stockpiled some vertical holes. When the moratorium was over we had holes in which we could shoot, right away. We had made the decision early on, I think, that our vertical holes would take a 48-inch diameter casing. To my memory they were all drilled to accommodate such a casing.

We were in alluvium in Area 3, and the alluvium we saw was pretty loose. When we drilled a hole, there were layers of what I call hourglass sand - - it would flow like the sand in an hourglass. Any fool knew that you would have to case those holes, or they would just fill up, particularly if they were going to stand there for a long time. And so, there was a policy here that you had to shoot in a cased hole, because you would lose the bomb and everything else if you didn't. After we resumed testing we used to have that argument with Livermore, regularly.

Carothers: Well, Livermore shot in cased holes for some years. It didn't occur to anybody to ask, "Los Alamos drills holes and cases them. Why do they do that? We're in a different area. Is it the same? Should we do that?" So, Livermore cased holes. Why? Well, because Los Alamos did, and that's the way it was done. I think that is an interesting example of something being done in one place in one way for a particular reason, and that becomes dogma. In different place at a different time the same things are done without regard to the fact that it is different place, and other ways might be better.

Brownlee: That's right. Had we started up on Pahute mesa, for example, the dogma would have been utterly different, I think. In Area 3 we did have the sand flow. In one of the shafts we put down later, the hourglass sand trickled down between the boards of the lagging for three or four months. It was a steady little stream, just like an hourglass. I don't think Livermore has ever seen anything like that in the north part of the valley.

Roy Miller was the drilling superintendent for Livermore for many years, and had a different view.

Miller: The problems that LASL had, and we had, on several holes, was that the alluvium-tuff contact is where they tended to cave in. There are places where that sand zone acts like a fluid. It just pours in there like sand in an hourglass.

We have the same zone, only it's deeper than in the Los Alamo area. As you get up in the northern part of Yucca Flat, we've had dozens of holes that caved in at the alluvium-tuff contact. We've repaired a bunch of them and used them; filled them full of cement and drilled back through.

To give you an example of how massive those cave-ins are, there was a hole called 10r, back when we were drilling with air-foam direct circulation. We drilled the hole to 1600 feet, pulled the drilling assembly out of the hole, ran a caliper log all the way to the bottom, 1600 feet, and were logging up. When the caliper log was at about 400 feet - - you run the caliper log from the bottom up - - it was like an explosion had occurred. Air roared out of the hole like a volcano. I wasn't there, but the stories that were told about that . . . It broke all the arms off the caliper log, but they pulled it on out. Didn't lose it. They repaired the caliper log, and went back in to 1050 feet, so they had lost 600 feet of hole. This was a sixty-four inch hole, and essentially this was instantaneous. They ran the bit back in, cleaned it out without difficulty, all the way to 1650. Then we pulled the bit out, went back in with the caliper log, and it stopped at 1050. It did that two more times.

It was that hourglass sand that LASL keeps talking about. The first time it was a massive cave-in. The other two times it was very slow. They weren't aware it happened until they went back in. The same thing happened in Area 2 on the west side of the road. We drilled down to below the water table, and set a liner to have a dry hole. It caved in above the liner and filled the liner up. We went in, cemented it up, drilled back down, and fortunately hit the liner. Anyway, those formations that LASL talk about down there occur up in Area 2 and 10, only at a deeper depth.

Brownlee: I think we did cased holes in Area 3 for perfectly rational reasons, in light of the things we were seeing. It was only after we had this big quarrel with Livermore, some years later, after

they went to uncased holes and were pointing fingers at us for spending too much money casing holes, that we really examined the fact that even in the alluvium in Area 3 the holes lasted a long time if you didn't mess around in them. That was very hard for Campbell to accept.

Also, during the moratorium, there was a doctrine to keep the testing community intact.

Carothers: You might almost call it a readiness program.

Brownlee: Yes, you might. And the way they planned to keep it intact was to let people work on whatever they wanted to. We had said, before the moratorium, that during the moratorium we would rework and reduce all the data we had collected in those frantic years of tests. In fact that really didn't happen. There were a few people who worked on data, but there were people they didn't want to lose who didn't want to work on data. They were allowed to work on other things, so in truth, even though people were around, they had other interests and evolved to other programs.

And so, when the moratorium was over and we went back to testing in '61, we really had, it's fair to say, a different set of people. Not entirely of course, but there were different groupings of people, and so there was not a lot of carryover from the things we did in '57 and '58, as far as containment was concerned, into the '61 time-frame.

Louis Wouters, by 1958, was one of the senior scientists in the Livermore testing program. He remained with the program until his retirement. As with the comments of John Foster cited after, his remarks do not have to do with containment, but they are interesting to consider in the light of Bob Brownlee's words about maintaining a testing, or containment capability when there is nothing to test, or to contain.

Wouters: The day the moratorium started, L Division ceased to exist in the minds of management. What do we need these people for? We have no tests to shoot. The general attitude we lived with for almost a year was, "Well, we're paying them, aren't they happy with that? We haven't fired them, after all. Good God, what are they complaining about? They haven't got anything to do except plan, and think, and look a old data. That seems to us that is an an idyllic situation." Well, the kind of guys we had at that time in Test

Division were a bit more motivated and a bit more ambitious than that, ambitious in the technical sense. They wanted to go out and do things. They were young men, and they wanted to do things. They didn't like being cooped up in an office.

The first year there were a number of things to clean up. There was data from Hardtack II, and also Hardtack I, to get into some kind of shape. Only about half of that work actually got done, because there was no interest from the design divisions, none whatever.

I think it was in 1959 that I went over to England to look into a number of things connected with the Joint Working Group we had with them, and also go to one of the photomultiplier plants of EMI to see what they had to offer. The people at AWRE were very nice, and they took me through their test program building and their laboratories. And let me tell you, you think we were in trouble. Any of the offices that had anybody in them - - and there weren't many, there were a lot of empty offices - - had a zombie. There was just no motivation. There was one guy who was excited, because he was working on image converter replacements for cameras, and he was able to use it on HE shots. All the others, they were just sitting there, waiting for the worm to turn, or whatever. It was dreadful.

In retrospect, what it tells you is that it is not unique to us when something like that happens. It seems to be a universal kind of syndrome. They don't want to spend money on us because they don't see the point. I, at that time, with a vengeance, came to the conclusion that if you don't have anything worthwhile for people to do, close the program down, put them on something else with a long string, and if the need arises, pull them back. They'll be happier and more useful to you than if you let them sit and rot in their offices.

John Foster was the Director of the Livermore Laboratory in 1963, when the Nuclear Test Ban Treaty was signed. One of the things that was considered to be important when the Treaty was signed was that there should be a readiness program - - a formal program to maintain a capability to resume atmospheric testing should such testing, for whatever reason, become necessary. The following words by Johnny Foster relate to that readiness, not to containment.

Foster: I can remember, when we got to the atmospheric test ban, going to the Joint Chiefs of Staff and trying to argue for the four safeguards that had been worked out with Scoop Jackson. The day I made this pitch to the JCS was the day that Curtis LeMay was, I think, Acting Chairman. I went through the four safeguards, and the one safeguard that LeMay hung up on was the one of readiness. He said to me, "You will never be able to maintain readiness." I was absolutely thunderstruck. Here was the guy who had created the Strategic Air Command that had maintained readiness, and he was telling me, "You will not be able to maintain readiness." I was too shocked to ask him why.

He was dead right. Only a few years later I was working in the Pentagon, (Ed. - - as Director, Defense Research and Engineering) and cancelling the very programs that I had fought for. I was cancelling them because the plans were made up by people who didn't understand what they were doing. The people who did had left to go work on things that would be more productive. And, if the plans didn't make any sense, you just simply couldn't afford to keep pouring money into them.

There were some experiments that could be done during the moratorium. In particular, the were a number of high explosive experiments done to look at crater formation from various yields of explosives in various media. One notable such experiment was the Scooter detonation, which was done at the Test Site. It involved the detonation of 500 tons of TNT which was stacked in a spherical shape at a depth of 125 feet. One of the problems with Scooter was that when the signal to fire was sent, the TNT did not ignite and so there was no detonation.

Bob Bass, of Sandia, was the project officer for the various ground response measurements that were to made.

Bass: We started putting the HE in the ground in May or June. That million pounds of TNT had to be loaded down 125 feet. We could never do that today. For example, one of the problems they're having right now with the Chemical Kiloton is how to have a safety plan for transporting the ammonium nitrate from Mercury out to Area 12. Don Larson had one of his people find out how they transported gasoline on the Site, so they could use that plan. Turns out, there is no safety plan for transporting gasoline, or flammable

material on the Test Site, on the Mercury highway. That's okay, but you can't move ammonium nitrate, because people have thought about it. And that's the current kind of stuff we're stuck with.

Anyway, we transported all the HE for Scooter, a million pounds, down from Hawthorne in twenty ton loads on commercial trucks. It came in blocks - - it had all been melted and cast. Hawthorne had so much of that stuff that it was unbelievable. We also had a whole bunch of spheres made up, and Sandia has used them for containment tests ever since - - two thousand pounds down to eight pounds.

Well, we put in all of our instrumentation. We had a trailer nearby that had a revetement around it to keep the air blast from hurting it, and the rocks from falling on it. In addition to our instrumentation we provided the electronics and the place to record and handle the firing system performance, and people's checkouts of all that. I was not responsible for the firing, but in a sense I was involved because I helped hook up the firing set. Bernie Shoemaker did it, and I helped him with that. Scooter was to be fired with a pentalite booster block in the center of the charge. That block was put in when the sphere was halfway installed. The detonators were sent up from Albuquerque, and they were supposedly war reserve detonators to be used with a regular firing set, and the people who did this were the people who would ordinarily do a regular test, a regular operation. There were extra dets for backups, and so on.

The trouble was somebody sent out sugar loads. They were dummy dets that didn't have any booster in them. There was no active final little blue booster to set off the pentalite; they just had the little wires across the back. These were what was put in. Everything was fine, except there was no explosive in the dets. We found out, after they were in, and the HE was on top of them, what had happened.

So, they sent out some more dets, of the same type. We took them down to our trailer and said, "Let's fire these things and see what happens." Bob Burton was in charge of doing this. The thought was, could we put enough energy in there, to that little wire, that we would get the pentalite to go. That was the idea, and we tried. And so we proceeded on. On shot day Neal Thompston, then head of AWRE was there, and whoever was head of the Atomic Energy Commission at the time was there. Everybody was there.

Carothers: What you're telling me, if I understand you correctly is . . .

Bass: That we knew damn well it wouldn't go. We would have been stunned if it had gone. That would have been the surprise of surprises. We knew it wasn't going to go, but we wanted to try it, because there wasn't anything else to do. The explosives were all stemmed in, and it would have been a terrible job to try to get them out. And, as we expected, it didn't go.

An investigation group was set up, and Mel Cook, a Utah explosive expert, was called in to head the committee to see what to do. They met and met and met, and decided there was only one approach, and that was to melt our way back down. So they set up a group to do this, and an explosive safety board to supervise it. We could never do this today, never in a million years.

What we did was to put a safety perimeter around the shot, which was established as soon as it didn't fire. About half way back toward the Area 10 highway where the access was to the area, we set up a remote control area to remotely drill back. We moved a drill rig in, drilled down to the top of the HE, remotely done. When we got to the top of the HE, then we put in a steel billet, which had hot water piped to it - - I don't think it was steam; I think it was just hot water - - to melt our way back down, through the explosive, to the center. When this was done, the guts of the billet were pulled out, and a pentalite booster block was lowered inside this billet, which now sat in the middle of the Scooter charge.

I was scared to death of the whole operation, but we were out there monitoring all the time. We were also worrying very much about our instrumentation cables, because we had all these storms and rainy periods. We were using white field wire, which was just laying out on top of the ground. It wasn't waterproof wiring at all, so we ended up with almost complete shorts in all of our cabling, in addition to the shorts in all the amplifiers, which were ruined.

So we sat there, burning out all our cabling, all this time. And we also had some cabling that went into the HE to measure the HE burning rate. There were concerns about how much current we could put into the cables and not be a danger to the HE, and all that. So, we had to monitor the things very carefully. A lot of thought went into it. We sat there with low currents, just burning out these

cables for three months. We were in the danger area, burning out our cable the whole damn time. And they dried out finally. All but the pressure measurements.

So, what did we end up with? We ended up with a lot of radial accelerometer data that was outstanding. We ended up with some good horizontal velocity gauge data. We were using the old SRI-Sandia DX velocity gauge, which was capable of outstanding measurements. It's not used anymore, because it's far too hard to use. There were some surface measurements too. We made some surface velocity measurements, and there were all kinds of photography done. Scooter was really a very good experiment.

Carothers: There were a number of HE shots during the moratorium.

Bass: Yes, and Sandia was doing all of those. There was the Buckboard series in hard rock, for instance, during that period. And there were a lot at Fort Peck. There is a lot of stuff in the literature on those, but there is very, very little instrumentation data. Mostly there are photographs of before and after, and throwout measurements - - sticky-paper trays, and things like that. Vortman put out beads all over everywhere, and they counted beads in various samples they took after the shot. There were a lot of people, including ones at Livermore, who got very excited about how the crater lips were formed, and that sort of thing. Cratering was a big thrust. Vortman was digging canals, out on the Yucca dry lake. I stayed as far away from that program as I could; I wasn't too interested in that.

The moratorium on testing ended in September of 1961. Following the atmospheric detonation of a Soviet device with a yield of over 50 megatons as the first of a series of Soviet atmospheric tests, President Kennedy ordered the resumption of testing at the Test Site. There was the proviso that the tests should be carried out underground, unless a specific exception was approved. The first event at the NTS following the moratorium was the Livermore 2.6 kiloton Antler test, fired on September 15, 1961 in a tunnel. It was followed by Shrew, a Los Alamos safety test in a drill hole, fired on September 16, 1961. Both events released measurable amounts of activity; the activity released from Antler was detected off site, that from Shrew was not.

During the next few months the experience of both Laboratories showed that the containment of the radioactive materials produced by an underground detonation was not a trivial task, whether the device was emplaced in a tunnel, or in a drill hole. The first eleven events all released activity. During the first year there were 43 shots fired in emplacement holes by Los Alamos and Livermore. One, Eel, released some 1,900,000 curies, and the activity was detected off site. Twenty-one released material that was detected only on site. Twenty-one are not recorded as having released activity.

Carothers: When the moratorium ended Los Alamos used drill holes for their shots, and Livermore did their shots in the tunnels. Was there any kind of agreement, or understanding that Los Alamos would do shots in drill holes, and Livermore would do tunnel shots, so there would be experience with both ways of doing the experiments?

Brownlee: I don't know that there was anything like that. Probably there was no reason for it at all. But, at the time I thought there was a reason. Our perception at Los Alamos, and mine, which came a lot from Al Graves, and some of Al's obviously came from Norris, was that Los Alamos had concluded it didn't make any difference what the facts were, peaceful uses of nuclear energy would never come to anything. If Livermore wanted to waste their time with Peaceful Nuclear Explosives - - PNE things - - that was Livermore's prerogative. But we at Los Alamos would, as a matter of policy, not devote any of our thinking to PNE type things, and tunnels smelled of PNE.

Our interest was bombs, and testing bombs, and for that vertical holes were quite sufficient. If you were going to make harbors and things like that you had to have answers to certain kinds of questions which tunnels helped you answer. But everybody knew - - Los Alamos thinking - - that the best place to test bombs was right where we were; Area 3. So, don't go near those mountains where who knows what evils lurk. We'll stay right here, thank you. So, the impression I had was that PNE was what separated them. Now, in fact, I do not know what Livermore was thinking, and I do not know whether PNE figured in Livermore's thinking or not. I don't know. But I think that's kind of how we saw it, early on anyway.

I would like to remember, but it's probably totally incorrect, that I was a bit more objective than some of the other people at Los Alamos. I was never quite so quick to pick up the party line. I always got along well with Livermore people. But there was a party line; thou shall not go near Livermore people, because they're all terribly bad. When I got permission to go to visit Rainier I was the only Los Alamos person who went and mixed with the Livermore people. I didn't mind that, but there were other people who didn't approve of that.

Carothers: I'll tell you a story I heard about why Los Alamos never had tunnels. I can't vouch for its truth, but it goes like this. Once upon a time Norris Bradbury visited the Test Site, during the moratorium. Livermore was busily digging tunnels, having nothing else to do. As part of Norris' tour of the Site, Livermore people took him to the tunnels. They got into one of the little mining cars and rattled back into the tunnel, which was poorly lighted, wet, noisy, dirty, and all the sorts of things tunnels sometimes are when mining is going on. When they came back out Norris said, "My people will never work under those conditions." And that was that for tunnels.

Brownlee: That's entirely consistent with Norris. I can believe that. That's the way Norris was. But it's also consistent with what I told you; tunnels were unneccesary, unneeded, and we would do our work in vertical holes.

But I was always very curious about the tunnels shots. I had seen those sandbags in Rainier that had turned into rock, and the other things that had happened in the tunnel, and I thought that was very interesting stuff. I went up and visited whenever I could, which wasn't all that often. Campbell, for example, didn't approve of Livermore, or tunnels. If you were going to drive up there you better not let Campbell discover that you drove one of his AEC cars up there. You had no business being up there. You were supposed to stay in Area 3. So, whenever I went there I was either on the q.t., or I had some special dispensation. I don't know that there was any reason for that. That's just the way it was.

Well, the Russians terminated the moratorium. Incidentally, I believe that was done perfectly legally. You hear that the Russians violated the agreement. I believe the understanding was, "We will

tell you before we shoot again." And they did. They told us the day before. I think they did what was perfectly legal in the eyes of the State Department. We had the same option.

They certainly didn't try to conceal it. But Kennedy was irate, and he called here and said, "How soon can you get a bomb off?" We must have gotten that call the first week in September, and I believe our answer was, "We can do an underground shot in one of our vertical holes in a week." The problem was, that was in no way a quid pro quo. To do a few kilotons in an underground shot in Nevada was certainly not equivalent to fifty megatons or so. But, that's what we were ready to do, that's what we said we could do, and Shrew, our first shot, was not very long after that.

The point is, we were ready to do that very quickly because we did indeed have vertical holes ready. And, we knew, or guessed, how big a yield we could fire in them.

So, through '61 and '62 we did some shots, and we were gathering information. Before we had the underground treaty in '63 we had satisfied ourselves that we could get the necessary data we wanted by testing underground. We had gotten enough information to know how to do that. And that was due to Al Graves, and Campbell, and Newman, in my view. I would name those three people as having done the necessary thinking and preliminary work to allow us to go that way fairly easily, and in a straightforward manner.

One of the projects that was significant for containment was the attempt by people at Livermore to develop a way to collect so-called prompt rad chem samples. The concept was that there would be an open pipe running from the device to some collecting station on the surface outside the tunnel, or by the top of the emplacement hole. There a sample of the device debris would be collected, essentially at the time of the detonation, and returned to the Laboratory for analysis. The work following the moratorium was basically a continuation of the work Gary Higgins had started during Hardtack II.

There is little question that this effort led to at least two major ventings. Dick Heckman, a chemical engineer, was in charge of the field effort to design the pipes and other hardware that were to collect these samples.

Heckman: After the moratorium I went back to the underground sampling business. There was what I called the fast sampling, which was an attempt to get fast, or prompt samples, where what I was trying to do was to get refractory bomb debris. In other words, the kind of bomb debris you would normally get from post-shot drilling, where the activity is trapped in the melted rock, which is the standard sort of thing. What I was initially trying to do was develop a competitive process to that.

Carothers: You did your first tries on the shots that Livermore did in the tunnels, like Antler? You were the guy who was ruining the containment on those?

Heckman: Yes. Well, I didn't have anything to do with Antler's failure, because we didn't have time to get the sampling system set up. I think that you have to give Mike Heusinkveld a lot of the credit for the ideas. In other words, I'm only guilty as being the field guy who carried out the concepts that Mike had.

On Gnome there was such an experiment, a fast sampling experiment. We had a vacuum system with a pipe ten inches in diameter down to the shot room. It was a beautiful straight, vertical hole. You could go down into the shot room at Gnome, look up through that pipe, and at noon you could see stars. It really does work. You could see stars.

Carothers: You know, I've heard that story, and I have done a little simple-minded calculation about the solid angle and what fraction of the sky you see, and how many visible stars there are, and the probability of there being a star in that patch of sky is so small that I don't believe you.

Heckman: Fine. I understand all your arguments, and all the rest of it, but I was there, and my recollection is I saw stars. I'm convinced I saw stars. Anyway, the point is that is was very straight.

Carothers: It was straight, I know that. You could look from top to bottom. Did you ever look down and see the stars down at the bottom?

Heckman: I have acrophobia. I don't like to look down much.

So we had the sampling pipe, and fortunately, it didn't work. We had enough problems on Gnome as it was, but if that sampling pipe had really worked, we could have had another Des Moines.

Higgins: On Gnome there was a ten-inch diameter hole pointed directly at the device. It went to the surface, and it was open all the way. It not only sealed up, but we probed the inside of it with a radiation detector down to within two cavity radii, and were unable to detect the fact that there had been a nuclear explosion there. There was no activity, not even gaseous activity. To me that was, and is still, rather surprising, because there was plenty of tritium tracer around the Gnome explosion, and it was everywhere else, but not in the rad chem sampling hole, believe it or not. It certainly went into the tunnel.

Carothers: Well, there were people, Gary, and I'm sure you're familiar with this, who believed that the way to ensure sealing and containment on cables and small diameter holes was to always, on all drawings, and when discussing them, speak of them as rad chem sampling devices. Then, the evidence was, nothing would ever come up them. You'd never see an atom.

Higgins: Not even one. You're right, I'm familiar with that approach.

Heckman: The concept behind all of this sampling work was that the bomb was going to go off, some of the debris would fly into the pipe, the ground shock would then squeeze off the end of the pipe, and now I would have a pressure pulse, and it would be just like a shock tube.

These were vacuum pipes that looked directly at the device, and so you put a slug of gas in, and it's equivalent to puncturing an aluminum diaphragm and allowing a pressure wave to travel down the pipe. You can very easily show that if indeed it behaves like that, with the pipe shut off by the ground shock, there's a certain maximum pressure wave that will arrive at the other end. So you design a system that will withstand that kind of pressure.

The chemical engineers devised several ingenious schemes to keep the pipe open, and Dick Heckman describes what was done on Eel, in May, 1962, and on Des Moines, in June, 1962. Both were major ventings. The reported release on Eel was 1.9 megacuries; on Des Moines, 11 megacuries.

Heckman: My good friend Heusinkveld wanted to use slifers as a way of getting a quick yield measurement, and he came up with this great idea where he just drilled a satellite hole, filled it with drilling mud, and stuck his slifer cable in it.

Carothers: And the mud was going to keep it open?

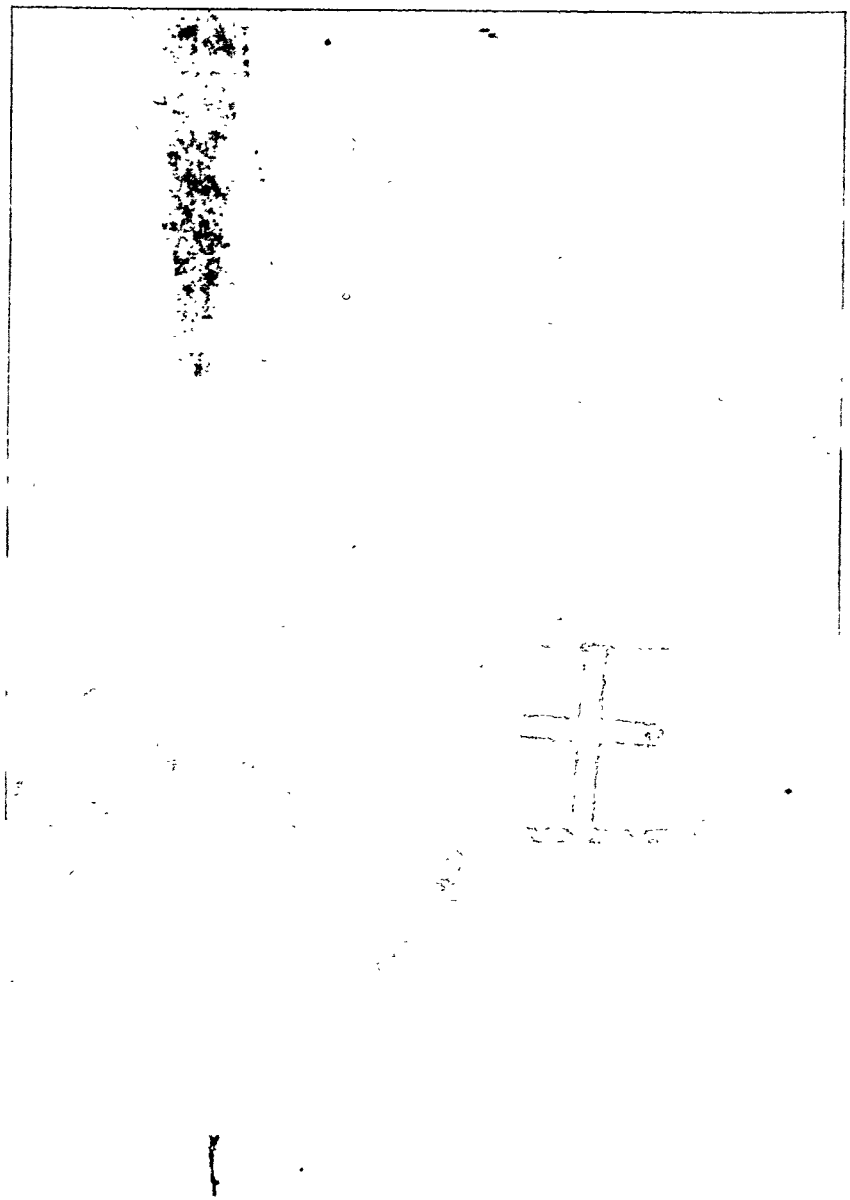
Heckman: Well, he didn't think about what the mud was going to do. He just knew the mud was going to transmit the shock wave as it went out. On that same shot, which was Eel, I had decided that maybe I could get an explosive that would get detonated by the shock wave. I wanted something that would burn pretty slowly, and nitromethane logically comes to the fore. And so we indeed did that.

Well, Mike's slifer cable worked fine, but immediately there was this 150 or 200 foot high column of mud that spewed out of his slifer hole. Our sampling system worked and we got samples out of it, but it didn't close off either, so Eel would have vented even if Mike's slifer hole hadn't been there.

We were not looking at the device itself. These were now satellite holes. People said that the device goes off, and the cavity grows out in this length of time, and our thought was that if we could connect up with that initial vaporized zone, we'd stay connected. Then we could build very sturdy systems that would take the thousand psi or so of pressure, with cyclone separators we could bury underground, and then we could pull samples out of them.

So, we tried a straight nitromethane tube, but what we found there was that when you look at the burn velocity of the nitromethane, it burned faster than the ground shock coming through the alluvium. We probably were exploding the pipe; we were putting pressure inside at the wrong time. So then we had them wind us up a helical pipe, where the spacing on the pitch changed as you went up, and we tried that. This was also nitromethane filled.

We seemed to get a pretty good sample out of it, but the problem we had was that when the nitromethane went off, razor blade size pieces of steel just spalled off, and that ended up clogging up our system. So that clearly wouldn't work. Well, we got to thinking about it, because Mike had had a spectacularly successful connection to the cavity on Eel.



Eel venting through pipes intersecting the cavity. May 19, 1962.



Eel event - the black cloud behind the white plume is the mud and cables ejected from the hydrodynamic yield hole.

So we started looking into it, and we ended up going back to the basic viscosity rules and discovered dilatant fluids. That's something in which the apparent viscosity is proportional to the rate of shear. To put it in simple terms, if I could fill a pipe with a fluid so while the shock wave was going through it the fluid had the viscosity of solid concrete, it would keep the pipe from crushing, and then as the shock wave went past, the stuff would act like a fluid.

In looking around we realized that ordinary starch and water would do this. And we added a gel to it. So, we did some tests and it all looked good in the laboratory. I remember one spectacular experiment I did. We had a beaker sitting on the table, and I said, "Okay, if this is really working, what I am supposed to be able to do is stick a spatula in it, and if I lift it rapidly, it will set up and I'll be able to lift the whole beaker up.

Carothers: Be sure you don't stop lifting.

Heckman: Well, that was the problem. You can only lift to as long as your arm is, and that could be right over your head. However, that demonstration, as far as I was concerned, was a very practical one.

Carothers: I recall you and Heusinkveld had a sampling pipe on Des Moines. What clever scheme did you use there to breach the stemming?

Heckman: On Des Moines we built a section of two-foot diameter pipe, and what we did is we packed it with polyethylene tubes, polyethylene pipe, and ran it through the stemming. Mike put a slifer cable right next to our inlet section, and he put a slifer cable over along the tunnel wall, and then, of course, that part of the tunnel was all packed with sandbags. Well, as you remember, Des Moines was one of the more spectacular containment failures.

Carothers: When you designed this horizontal pipe for your inlet experiment, and stuffed it with the polyethylene tubes which would vaporize and explode and keep the pipe open, what was going to close it? If you had deliberately prevented the ground shock from closing it, what was going to close it?

Heckman: Well, Mike didn't really think that one completely through, and it never occurred to me to worry about it, because we had that big gas-tight door, right? You were going to get a little

activity out, sure. Remember, this was all kind of back-of-the-envelope, and so you didn't really think about what kind of pulse that was going to be put out.

Well, when we looked at the signals from the slifers, the slifer he put by the tunnel we never did get a signal out of. The one on the pipe just took off, and clearly was moving at about two to three times the free field velocity. When you tried to look at the signal that was coming off of the slifer on the side of the tunnel, comparing that with the free field slifers that they had installed in other locations, it was just very clear that the shock wave coming out of our pipe was just blowing it up.

It became also very obvious at this point that you don't get just a little bit of the dragon's breath. Once you connect with the dragon, he keeps blowing. So, as you remember, the blast door that was sealing the tunnel came flying out.

Carothers: Richard, everything came flying out.

Heckman: Yes. And it's just very clear that Mike Heusinkveld and I were responsible for the Des Moines fiasco.

Carothers: Well, you can't really claim all the credit. There was a vertical rad chem sampling hole that looked from the top of the mesa down to the device, and pictures from the fast cameras show that vented immediately, in less than a millisecond. I do believe that your attempt to keep the pipe in the tunnel open succeeded, and led to the venting out the portal. But even if that hadn't happened, Des Moines would have had a big release due to that vertical sampling pipe. So, maybe we should give Des Moines to the chemists in general, rather than to you in particular.

The following pictures of the Des Moines venting, on June 13, 1962, were taken by the author with a hand-held camera. The pictures were taken at irregular time intervals; the elapsed time between the first and last is probably about ten minutes. The total release is recorded as 11,000,000 curies, which is one of the largest releases from any underground event. Regardless of what definition is chosen, Des Moines was not successfully contained. It is instructive to observe the amount of material ejected from the tunnel by the energy release from what was a rather low yield device.

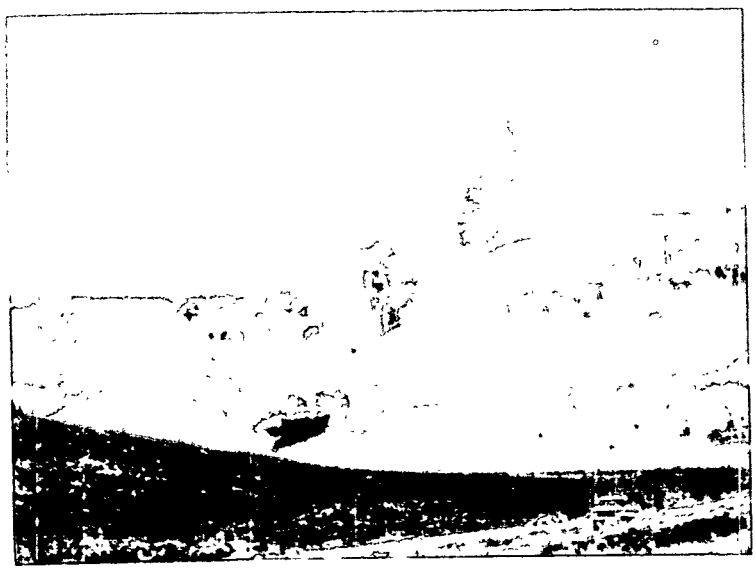
In the pictures there are three distinct venting paths that can be seen, The first is from the rad chem sampling hole that led to the mesa top. As was mentioned above, material was released there within the first millisecond. The second release occured through a hole that ran from the the face of the mesa down to the tunnel, and can be seen as a plume that appears before the venting from the portal develops. The purpose of this hole was basically to protect the diagnostic film in the trailers near the portal. The thought was that if there was venting into the tunnel, the pressure would be relieved by having an open hole from the tunnel to the mesa face. Hopefully, such pressure relief would allow the door near the portal to remain intact, and so prevent radioactive material from blackening the diagnostic films in the trailers near the portal. The third, and major release, was from the portal after the gas-seal door had been forceably ejected.



Des Moines; Fired in Tunnel U12j on June 13, 1962. The plume from the initial venting through the vertical radiochemistry sampling hole, which occurred within millisconds, can be seen at the top of the mesa.



The second release path was through the pressure relief hole, and occurred within seconds. Material is beginning to vent from the portal, lower left.

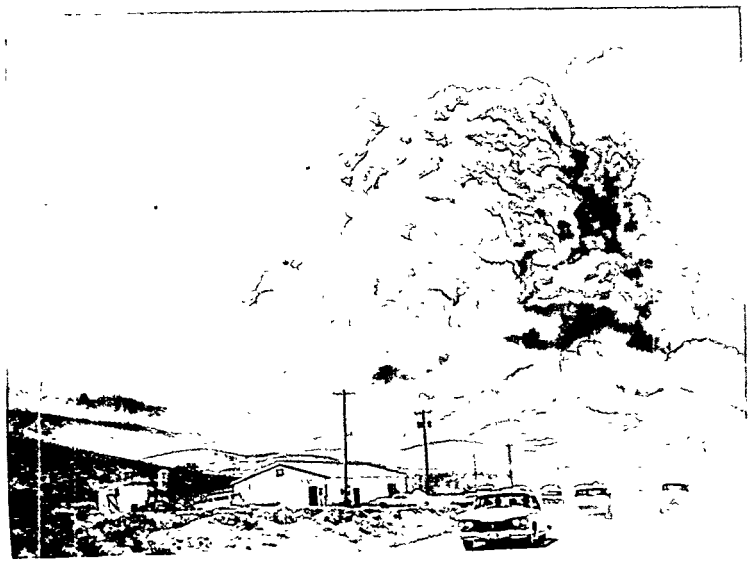


As the venting was established out the portal, the first two vent paths became less important.



There was essentially no cloud rise. The sandbag stemming and the material scoured from the tunnel itself stayed close to the ground.





There were between one and two hundred people in the area at shot time. At about ten minutes it seemed prudent to leave the scene.

However, neither the Des Moines venting, nor that of Platte (about 2 megacuries) in April of 1962, nor that of Eel (about 2 megacuries) in May of 1962, caused significant problems to the overall test program. They were significant problems to the people at Livermore, particularly the people trying to collect data on film, but there was no stoppage of testing while the causes of the ventings were explored, there were no changes in field procedures, and so on. Des Moines was detonated on June 13, 1962, and the Dominic operation was actively being carried out in the Christmas Island area. In the week preceding and the week following Des Moines, there were a total of seven airdrops of devices of intermediate or low megaton yield. If 10 megacuries is taken as the H+12 hour activity from 1 kiloton of fission, the Des Moines release was about that of a 1 kiloton atmospheric shot. That was trivial compared to the activity being released in the Pacific, and perhaps that influenced the AEC. On the other hand, it was close to home, and the people whose data were lost were not happy.

In all, Livermore fired seven tunnel events after the resumption of testing in 1961, and only one, the Madison event was contained. The last Livermore tunnel event was Yuba, fired on June 15, 1963. It was not contained and released material that was detected off the Test Site.

It was in 1963 that the Nuclear Test Ban Treaty, widely known as the Limited Test Ban Treaty, or the Partial Test Ban Treaty was signed. As observed in the first chapter, the Treaty did not say any event had to be designed to contain all the radioactive products that were produced - - only that the radioactive debris should not cross the border of, in this case, the United States. For example, nuclear cratering experiments continued until December 8, 1968, when the 30 kt cratering event Schooner was fired, presumably under meteorological conditions that would retain the vented activity with the boundaries of the United States for some indeterminate time.

Containment failures, as defined today, occurred both before and after the Treaty was signed. Most of them were minor seepages, but some were major failures, particularly for the experimenters trying to collect information from the detonation. There were a variety of reasons for the ventings, and it was not always easy to determine the cause of those failures.

Brownlee: There was Bandicoot, in 1962, in about the first year. There was nothing wrong with the containment design, nothing wrong with the emplacement, or anything like that. I think that was all done right. We had everything placed assuming the yield we were told it would go would in fact be the yield. The hole was deep enough for that yield, but I believe there's no doubt it went well above that. We had enough hydrodynamic data that we were convinced of that. And so, the hole was just too shallow, and it vented. It was just that there was this enormously surprising yield. Now, why was the Bandicoot yield so surprising? Well, it was a type of device where nobody can estimate yield very well. Of course, what we should have done was put it much deeper, just to be conservative. But, you see, we took the designers word for it; what the yield would be, and what the max cred was.

That as one of the few times when the yield was, in effect, dictated by a committee, which paid little attention to data that didn't fit the desired results. It was designed to be so many kilotons, so that is what it was. And it wasn't. It just wasn't.

Carothers: I have noticed, at the CEP meetings, that you are often skeptical of numbers we get from the designers.

Brownlee: You've noticed that?

Carothers: I have noticed that. Perhaps that's because of your experiences with Bandicoot and Pike.

Brownlee: It's more than that. We've had a number of times where I have seen idiocies promulgated as fact. And it's done for political reasons. You understand how that goes; we have promised the Navy, or the Army that this thing is going to be so many kilotons. And, that's what they are going to get. Never mind what the yield really looks like.

Carothers: Well, Bob, you must realize that having promised a particular yield, over the course of this many year development through the Phase 3 and into the stockpile, there has been lots and lots of money and time spent on targeting plans, training manuals, and so forth, all based on that yield. Now, you're not going to come in at some late date and tell them the yield isn't what you promised them, are you?

Brownlee: Yes, if it is different. I do understand the cycle you've described, but I do believe that the quicker the country finds out that something is different from what they thought it was, the better off the country is.

I was bitter about Bandicoot. I had done what I had been told to do, which was to contain this shot where the yield was going to be thus and so, and that's it. After the fact they insisted that's what it was, and it was just a lie. I had my own hydrodynamic yield measurements, and those measurements gave a much different number, and I have no reason to take them back today; they were good measurements. Now, we have relooked at Bandicoot. We went back and drilled for more samples. This was during Eric Jones' stay. Eric reviewed all my data, and when he got all through he said, "The hydrodynamic data are correct." By this time we had gotten rid of the guy who was the problem, and so we got them to concede that the yield really was a lot higher than it was originally reported.

In early 1963 an informal meeting was held between Los Alamos and Livermore test principals to evaluate individual tests. The intention was to share procedures, plans, and lessons learned,

but containment was not a major part of the discussions. After the Limited Test Ban Treaty was ratified in October in October, 1963, the Eagle event vented. Considered as a probable violation of the treaty, the event triggered additional discussions on containment at various levels in the Laboratories, and in the AEC. From these, the Test Evaluation Panel was formally established in December.

The Panel consisted of consultants, and persons furnished by LASL, LRL, Sandia, DOD, the Public Health Service, and the AEC. The USGS furnished the geologic information. The purpose of the Panel was to "review all data pertinent to the containment aspects of each planned nuclear test; then, based on these data, to assign the test to one of the risk categories defined below."

The TEP had three categories, much as the CEP does, but there the similarity ends. The TEP Category A, in 1966, was "Underground nuclear tests which, on the basis of experience, should not release a significant amount of radioactive material. It must be understood that, even in this category, unforeseen conditions may develop which result in the release of detectable levels of radioactivity at the border." The NVO Planning Directive for 1964 said, "The emplacement and firing of devices will be designed to result in containment in all cases where this requirement is not inconsistent with the technical objectives."

Cliff Olsen, long time Livermore containment scientist, made these comments about the TEP:

Olsen: The people who tended to be at the TEP meetings were the Test Group Directors, and they presented the shots. The presentations were very rudimentary. There was a data sheet, and maybe a line-of-sight pipe layout, or a stemming drawing. Often there was no stemming drawing, because we had generic stemming plans. There was LASL 5, or LASL 2 at that time. We would have our stemming plan, which was pea gravel with fifty feet of sand halfway up, and fifty feet of sand at the surface, and that was our stemming plan. So, there was no need for a drawing, because they were all the same. The TEP got into reviewing the designs of particular features a lot more than the CEP does. In a sense, they would suggest changes, and they would actually review mechanical designs - - why don't you do this, why don't you do that. And design changes to the hardware were made as a result of the TEP.

One of the things with the TEP, which I guess was sort of indicative of the climate at the time, was there were three categories, A, B, C, and C was, "Underground nuclear tests which are expected to release a significant amount of radioactive material." There was no particular onus to getting a C. It simply meant that you made different notifications before you shot it. It had nothing to do with whether you were going to execute the event. It wasn't that somebody in Washington or Germantown was going to have a hemorrhage when he saw it. It was just the design of the event.

One venting in particular, from the Pike event, fired March 13, 1964, has had a major and continuing impact on the Test Program. The fallout projections for following events have been based on the "Pike Model," which means that the possible fallout from the proposed shot is scaled to the readings that were obtained in the Pike fallout pattern according to the yield ratio of the two events. The basic assumption is that the proposed shot will release the same fraction of the activity that Pike did.

Brownlee: Pike has cost all of us enormous amounts of time, and effort, and money, and I think needlessly. That is a thing I have never been able to communicate to NVO in modern times. You see, one of the things everybody forgets is that we had a line-of-sight pipe on Pike. It didn't come to the surface, so people forget that it was there. And, since it didn't come to the surface, although it went a substantial distance, it had no closures or anything. That pipe was one of the key factors. Another was that Pike was expected to have a maximum credible yield of a certain value - - not very large, but definitely not a safety shot. Well, it went over one and half times the max cred. And then, it was in a very shallow hole; 400 feet or so.

What I knew about it was that it had this predicted max cred, and the pipe was so long and so big. I said, "A lot of energy is going to come to the top of that pipe." I did not realize that there was any chance that the yield could go higher than what I was told, or I would have hollered. I knew that it was in a shallow hole, so that bothered me as it was. Another thing I did not know was that when they drilled that hole they had run into what I called hourglass sand. And guess where that layer of sand was - - which I found out after the shot. It was right at the top of that pipe.

Now, there's no chance in the world we will ever duplicate Pike. First of all, we won't shoot anything at 400 feet. Secondly, we won't have a pipe on it like that. Thirdly, in a medium where the sand was running like water - - we'll never do that. And finally, having a max cred yield as bad as that is kind of unthinkable. I say, "Kind of unthinkable." When you combine all those things, Pike was a lead-pipe cinch to be spectacular.

My argument is that to treat every shot like Pike is absurd. It's just absurd. There isn't anything that's going to vent like Pike, because we don't do those things anymore. We will never duplicate Pike, and yet we pretend to the world, and to society, and to the President of the United States, that this shot we're considering could come out like Pike did. We don't intend to communicate that message, but that's what we do, and it's just not true. It's not going to come out like Pike, because Pike had too many great oddities.

Pike is what really brought us to every detail going through the hands of the containment people. We said, "No more are we going to take anybody's word for anything." The Pike experience was profound for us, because that's when we realized that no one person was knowledgeable about everything on a shot. I was responsible for certain things, but not everything. After Pike we began to function in what I'll say is a modern way. We had a meeting in which the bomb designer had to come and swear he knew what the yield would be. That really came as a result of Pike. Before, it was by chance. You knew what you knew by who you happened to talk to, but if people were on vacation, or you were on vacation, you didn't talk to them, and you didn't know whatever it was they could have told you. I learned a bitter lesson on Pike, which was that I thought I knew what the shot was and I didn't. I didn't know the yield, I didn't know the geologic setting, and I didn't know about the sand. All these things came out in the wash.

Bob Bass, Sandia, Albuquerque, was doing instrumentation work on the hydrodynamic yield measurements that Los Alamos was doing at that time. He also had information that Brownlee didn't have.

Bass: Pike could have been foretold. I was on the instrumentation crew. We were doing hydrodynamic yield measurements on Pike, and we had three satellite holes. Our job was to instrument these satellite holes. We had a guy named Jim Greenwald, and he liked to play with TV. He was our installation engineer when we were lowering these slifers and/or time of arrival gauges down these holes. Pike, of course, wasn't very deep. Well, Jim called me and said, "You've never seen such a mess in your entire life. I lower my TV camera down there, and it's a cavern. I've got communication between every one of my satellite holes, all the way down. I go a hundred feet down and there's no sandpile down there. There's nothing but a labyrinth of tunnels."

Well, we tried to put in enough stemming to fill up that cavern, but we didn't get it done. We flat didn't get it done. We got it done for a while, but then it would start again. We knew that site was Swiss cheese. That shot was sitting there waiting to vent.

Brownlee The first political fallout was about the fallout. It was on Las Vegas, and it also went straight toward Mexico City. I think somebody in the embassy read something and didn't know what he read, and it never really did get reported in a sensible way. I don't believe you'll find any record of a measurement having been made in Mexico City, but I believe there was.

Then Al Graves had a meeting, and I went, and Westerfelt, and for the first time we put together all the things that were wrong. And I was appalled. So we, at the second level, bared our breasts and said, "Well, we had done this, and we had not done that, and the yield was quite a bit higher than we were expecting, etc." Then Washington came down hard on us on all those points, but they knew these things because we told them about them.

The thing they knew was that it had crossed the border. We made lots of promises of brand new procedures, which indeed we did initiate. And we really changed our working relationships after Pike, between the people at the Laboratory, and the people in the field doing the engineering and drilling. I remember that as being profound. We said, "This will never happen again."

4

The Beginnings of Containment Programs

As time went by, the tolerance of releases of radioactivity from the underground detonations at the NTS diminished until it became obvious to the Laboratories and the DNA that serious effort must be given to all the questions that such releases raised. And the list of questions was daunting. What was it that prevented such enormous energy releases from rupturing the ground and thereby releasing the gases, steam, and radioactivity to the atmosphere? Obviously it was related to the amount of material over the detonation. In what way did the necessary amount of material depend on what the material was? How was it related to the chemical composition of the material, the strength of the material, which in turn is related to the amount of water in the material? How does the material react to pressures of millions of atmospheres, and to temperatures of many tens of thousands of degrees? Does it matter if the material is fractured or faulted, and if does, how? What pressures and temperatures actually are created in the material, and how do they decay with time?

The original thoughts about doing experiments underground arose from pressures to reduce off-site fallout, and desires to make the operations easier to carry out. Contamination of the shot sites, and radiation exposures to people working in the field gave additional incentives to the Laboratories to find different ways to conduct the tests. In a similar way, the releases that occured on some of the underground shots were a problem to those trying to collect experimental data. At first, political pressures to achieve better containment were minimal, but by the time of Baneberry in 1970 they became controlling.

Olsen: It must have been late '65 that I started to do things in containment. There had been several leaks, but the political climate of the time was sort of, "So what?" But the Test Program people really didn't like getting trailer parks exposed, because virtually all the shot data was on film, which turned black if it got irradiated, and there went the data. If you put the trailer park upwind, nobody really cared if you leaked a little bit. But the AEC

people eventually began to think that we ought to be a little more careful. The straw that broke the camel's back was a thing called Diluted Waters, which was shot in Frenchman Flat in June of '65.

I remember I was working on something in Yucca at the time. I was driving over the old Burma Road, and heard the countdown for Diluted Waters on the net, so I parked the car and watched it. At zero time there was a little dust, and a few seconds later a big cloud came out. A few other cars had stopped, and the guys watched for a while, and then we decided, "Oh well, another one," and we started our cars and went out to Yucca Flat.

After I got back to Livermore, my division leader, Jim Carothers, called me into his office, and asked me if I would be interested in something called containment. It seemed the AEC had gotten a little worried that we were having some problems, and Diluted Waters had kind of sensitized enough people that the AEC was going to form an investigating committee to look into it, because we had guaranteed we had solved the problems. We were involved even though it was a DASA shot. Now, we thought we had solved some of the line-of-sight problems on earlier things, going back to Eagle, in '63.

Carothers: Mr. Olsen, since Eagle released enough energy to create an explosion at the surface which completely destroyed the surface structure and the experiments thereon, I cannot say that I would use Eagle as an example of how you had solved things.

Olsen: Well, no, but it led us to things that needed solving, let us say.

So, in '65 this Jim Carothers asked me to look into containment, and it turned out at that time it had to do primarily with line-of-sight shots and the diagnostics thereof. So, we went scrounging for recording equipment for slow diagnostics, as compared to reaction history. We were looking at tens of microseconds, and millisecond response rather than nanosecond things. We got help from EG&G, primarily Santa Barbara. And some from EG&G Albuquerque, which at that time existed.

We were looking basically at flow in the pipe itself; time of arrival, pressures, as well as radiation coming up the stemming and the pipe itself. We were looking at how you could attenuate flow in a pipe. If you want to do that you obviously have to look at what's going on.

Carothers: How did you do that? Pipe flow is still an interesting, difficult problem.

Olsen: That is true. We tried lots of things, some of which worked, and some of which didn't. We used ordinary pin switches, and pressure pins. We used pressure transducers. We used optical time of arrival things, slifer cables both inside and outside the pipe. We used radiation detectors.

Carothers: And lo and behold, you discovered that indeed there was a lot of pipe flow, and it often came right out the top of the pipe.

Olsen: That's right. And it got to where we were measuring differential pressures, we hoped, across closure mechanisms. If you saw zero above, and a lot below, it said that thing really closed, and really worked. These were man-made closures, as opposed to ground shock driven. There were high explosive driven, and mechanically driven, closures. And there were lots of varieties of those. There were ball valves, and flapper valves, and so on.

Carothers: Did any of them work?

Olsen: Yes, some of them did, although some of them didn't. In fact, some of them were probably worse than if they hadn't been there. Probably the worst one we put in was a thing called HE flaps. They were dimples that had been cut in the pipe at alternating spots. You put little pads of HE on them, and shoved pieces of the pipe in, rather than trying to close it symmetrically. The idea was to obscure the pipe by pushing things in. One version of these flaps was to cut the pipe at the bottom of the flap, and shove this flap in so that something coming up the pipe would come to the area of the pipe where this piece had been pushed across, and the flow would then just go out into the stemming.

Carothers: Say, that sounds clever.

Olsen: That was really clever. Unfortunately, this thing weakened the pipe so much that what it did was put a tab of material out in the flow, and that tab could rip off very easily. So, the whole thing went right on up the pipe.

We had a few other disasters on line-of-sight pipes. Some, like Tapestry, weren't too bad. The reason it leaked was that some valves at surface ground zero jammed a bit and didn't close all the way.

Carothers: Did you ever have a successfully contained pipe shot?

Olsen: Oh yes. Probably the best ones were Crew and Flax. They were unusual in that the pipe terminated underground, and we had the things there that we wanted to expose and follow for a time. Obviously you had to close the pipe, or there wouldn't be anything there to look at. Both of those events were quite successful. Packard was another one where we had exposure stations about halfway down the pipe, and we wanted to pull them up the pipe to recover them. That was quite successful. We closed everything off below the exposure stations.

By then we knew about things that didn't work, like the HE flaps that just put more mass into the flow. We knew not to put an HE closure, even though the closure worked, in too close, because the ground shock could simply go around it, as if it weren't even there, and still have enough energy to pour energy into the line-of-sight pipe. So, you have to put even a fast HE closure far enough out so the ground shock doesn't just envelop it and keep going. We learned that closer isn't necessarily better. We learned how to build valves that would seat in the environment. We learned how to decouple them, if necessary, with joints and things like that, to modify the environment so they would survive.

We had a better capability, by then, to look at the energy in the front end, and to look at things where we could limit the energy going in. Often, in the early shots the experimenters wanted everything they could get. So, they wanted bigger and bigger apertures.

Carothers: That's still true. The experimenters always want more than they can have.

Olsen: That wasn't always true though. On Flax, for example, we put a segment of a pie-type collimator in the front of the pipe to cut down on the flux. That, of course, made it easier to close,

because part of the path was already plugged. When the experimenters got to be a little less grabby about wanting everything it sometimes made things easier. But a lot of it was trial and error.

The Livermore Hupmobile event, fired on January 18, 1968, released activity that resulted in a major loss of data, and radioactivity was detected off-site. It did not result in the kind of long-lasting operational changes that Pike did, but it did lead to the formation of a separate group, responsible for the design of the containment plan, at Livermore. Cliff Olsen and Billy Hudson became two of the first members of that group.

Olsen: In those days I think ninety percent of the reason for expending effort on containment related to data loss, rather than pressure from Washington. That begin to change probably around '67 to '68. It may have been as a result of Hupmobile, because the people in Washington who supplied the money, even though they were not so worried about the loss of data as the experimenters, got antsy about dumping money into these things and not getting anything in return. Hupmobile was quite expensive for the time. I think that may have been the first thing beyond strictly experimenters wondering why their film was black.

Hudson: Hupmobile turned out to be a containment fiasco, in that a lot of the film data was lost due to the radiation release. The decision was made at the Associate Director level to form a containment group, and to try to put some serious effort into understanding containment and saving the film. Jim Carothers asked me to join that group, and it appeared to me to be an offer I couldn't refuse.

I believe it was in late 1968 that happened. For about the first two years Bill McMaster and I used to have some words now and then about how this surely wouldn't be more than a two year problem, and then we could get back to doing some science. "We'll figure this out, won't take more than two years, then we'll get back to interesting physics." Fortunately, it got more and more interesting, because it turned out to be much more than a two year problem.

Carothers: I'm surprised that the containment group came along so late, because there were a number of Livermore events earlier that had been, by today's standards, quite catastrophic containment failures.

Hudson: Personally, it was my impression that we became interested in having a containment group because so much data was being lost on experiments like Hupmobile. On the earlier events there weren't that many experiments, so it was a relatively small loss, even though they perhaps lost a major fraction of what they had on the event. It was a small loss compared to the loss on Hupmobile. And, programmatic people decided that since this was the direction they wanted to go, bigger and more comprehensive experiments, something had to be done about containment so they would have some confidence that after spending all that money on the test they would get the data back. The primary problem then was to protect the film so the prompt diagnostics folks could go back to the Laboratory, read the film, and tell the bomb designers what they did right or what they did wrong. It was not to protect the environment; it was to protect the data.

The Partial Test Ban Treaty had been signed in 1963, several years earlier. The Treaty said we were not to do any experiments where radioactive material would go beyond the national boundaries of the U.S. That was a primary guideline; no radiation across our international borders. But in fact, measures had already been taken to pretty much limit the escape across the border. Just by the act of putting a few hundred feet of dirt over the device you almost always eliminated radiation getting to the border. There were a few events after the early sixties that released material that may have gone across the border, but they were very few. It was mostly a local problem, because the radiation leakage would be confined almost to the site of the event itself, or maybe a little larger. But, it was that local radiation that was causing the damage to the film containing the data, and that was the kind of problem most often encountered.

If we had a release that got up to the neighborhood of ten thousand curies there was a possibility of activity getting off site. Less than a thousand curies was of little or no concern to the general public, or the people in Washington. However, it was of great concern to the people whose film was in the recording trailers.

In the late 1960's, early 1970's, they were doing some exposure experiments, with an open line-of-sight pipe to the surface. It took a few tries before the hardware was properly designed to stop the rush of hot gases and refractory products to the surface, but that problem was pretty well solved by the time the



The Baneberry event, detonated December 18, 1970. 10Kt, hole U8d.

containment group was formed. I don't think people realized it, but we didn't see much more of the Hupmobile type releases on our line-of-sight shots after we formed the containment group.

The Baneberry event, detonated on December 18, 1970 with a yield of 10 kilotons, was the watershed in the history of containment. It was fired in an emplacement hole in Area 8, and had a vertical, non-divergent line-of-sight pipe. It vented spectacularly through a fissure, a little over three minutes after the device was fired. The cloud of dust and debris rose some 12,000 feet, and was reported to have been seen by people at the NVO offices in Las Vegas. The total release is today given as 6,900,000 curies (H+12 hours). Interestingly, almost all of the activity was the volatile and gaseous elements, so there was little fallout deposition from Baneberry. The integrated total activity in the fallout pattern, on the ground, was a small fraction of that in the Pike pattern.

The wind patterns before the shot indicated the transport of any effluent to the northeast, and so the Area 12 camp, to the west of the shot site, had not been cleared of the people staying there. However, surface winds carried some of the activity to the west. During the time it took to alert the people in the camp, and to clear the area, a number of people received radiation exposures, and some of those filed lawsuits in the following years, alleging damage to their health and longevity.

The AEC allowed no more detonations for some six months while a committee, called the Vinceguerra Committee, after the Chairman, examined the causes of the venting, and the method of operations at the Test Site. In the report of the committee several recommendations were made for changes in the way future test operations should be carried out, and how improvements could be made in the way the containment aspects of an event were evaluated. One of the recommendations was that the Test Evaluation Panel should be reconstituted, and a new Charter developed for the new Panel. The Containment Evaluation Panel, as the new Panel was called, consisted of a Chairman, one member and an alternate nominated by each of LASL, LRL, Sandia, DNA, USGS, and the Desert Research Institute. In addition, provision was made for the Manager, NVO, to appoint one or more consultants. Members

nominated by particular organizations, or consultants recommended by the Chairman, were formally appointed by the Manager, NVO, to serve on what was an advisory Panel to him.

Carter Broyles, Sandia, was one of the first members of the CEP.

Broyles: I think the members of the Panel all recognized there was a political need to be met, to prove to the nation that we were paying attention. And clearly it was evident in the series of proposed charters, and hassling that went on between Nevada and the Labs and Washington on just what the charter should say. I think I viewed from the very beginning that the CEP took it's role as a technical judgment body seriously, and more than just political window dressing.

In fact, I think some members perhaps were over-enthralled. Not so much over-zealous, but perhaps they did not have a full appreciation of the limits of our technical knowledge, and therefore tended to give themselves more credit for how sure they were of any technical facts than we really were. They didn't necessarily recognize the technical limitations, and the lack of knowledge of geophysics and geo-engineering, and what the characteristics of the real world were, how variable they were, and the limitations of the calculations.

Clearly various parts of the structure looked different from different perspectives, and the CEP, I think, was many different things to many different people. But the Panel itself, from the very beginning took its role seriously, and took it as a technical challenge to do the best job they could, because it was obvious that the world was going to be different after Baneberry.

The Livermore containment group had been in existence for some two years when the Baneberry venting occurred. During those years they were supported as part of the overall testing effort, but their authority to affect a particular shot was questionable. That changed significantly after Baneberry.

Hudson: Following Baneberry the Test Program was shut down for six months, and the people who designed bombs and wanted to get data back were suddenly aware that containment was a very important factor to be considered. It was the beginning of a

movement directed toward the idea that we shouldn't have anything out at all. If it was above background, it was too much. It was clear that was where people were headed.

There were about a dozen people in the containment group at that time, as I recall, and I would guess that two-thirds of them were involved with calculations. This is when the major effort was directed at adapting the codes that first had been used for bombs, later to describe what's going on in the pipe, to describing what's going on in the earth. It was clear that the interaction between the bomb and the ground might be the ultimate worry, not just the interaction between the bomb and the pipe.

Carothers: Clearly demonstrated by the Baneberry venting. Why didn't the containment group prevent that?

Hudson: Well, that is an interesting question. At that time the containment program was really under the umbrella of the Test Director and the operational folks. We didn't have a Containment Evaluation Panel. In those days we had the Test Evaluation Panel, and the Test Evaluation Panel was more concerned with having a successful experiment than they were with containment. As a result, when containment aspects of an event were considered, they were presented by the operational side of the program.

We in the containment group were operating in a support mode. If they wanted to pay attention to us they did. If they thought that the concerns we had wouldn't lead to an expensive loss of data, then they didn't. The objective was still to bring back the data. And as a matter of fact, they brought back data on Baneberry.

What we did say was that we should run some logs in that hole, and find out what kind of densities and velocities we really were shooting in. We did ask for them, but we didn't get them, because we couldn't make a good enough case for it. We couldn't say, "Hey, if the velocity is below this, or the density is below this or above that, we're going to have a release problem, or a vent." We just knew there were questions we would like to have had answered before the event. We knew there were some things that were new and different, and that we didn't understand.

If we could have said, "Hey, you're going to lose a lot of data," then we would have gotten their attention. But as far as containment per se is concerned, we didn't have a lot of leverage. And, we didn't know we were going to have a horrific containment problem. We

just knew that there were some things about the site that were unusual, and that was our cause for worry. But we didn't have any theory as to why we were worried. We were just worried because it was new and different. Without having a really logical, well thought out reason for delaying things, it was hard to give credence to our fears.

Carothers: You might contrast that with today, almost twenty-five years later. Today if the containment group said, "We have fears and we don't know the answers," you would be listened to more, I believe.

Hudson: I think there's no doubt about it. The attitude today is that we have to demonstrate why somebody's fears aren't really a problem. In those days somebody had to demonstrate why a fear was a problem. Today we're almost in the position of having to prove negatives. Just the opposite was true in the past. Then we had to prove that there was a problem. Today we have to demonstrate that there's not a problem - - as well as we can.

Recognizing the changes that were taking place, and the strong requirements that were being developed for complete containment, Los Alamos organized a formal containment group in 1970. Bob Brownlee had been working on the containment of underground events since 1956. In 1966 he was joined by Carl Keller, and they did a number of calculations and experiments related to line-of-sight shots, but it was not until after Baneberry that a containment group, per se, was formed.

Carothers: When did there get to be somebody working on containment besides you, or when did there get to be a defined containment activity?

Brownlee: It was at Baneberry time that we actually formed a containment group, and I became the group leader. We finally decided that between Baneberry and when we started testing again. The first step in that direction was actually back in 1966, when Ogle said, "Get somebody and teach them." So, I hired Carl Keller. He was young then. I'm the same age now as then, but he's older for some reason. I hired Carl, and just spent time with him. We started going through things, and learning, and doing things, and I started transferring jobs to him. Before that some people were named as doing containment. That is, there was somebody in J-6, and there

was somebody somewhere else, there was me and Carl. These people stayed in their own organizations, but they were supposed to work on containment. In effect we had a very small containment group scattered around the Lab.

Carothers: When you say, "formed a containment group," does that mean these people now physically came to work in one area?

Brownlee: Yes. J-9 was formed at that time. Jack House was in J-8. He had a little training in geology, so I latched on to Jack right away, and got Jack into the group. Then, after we had that group, I hired Fred App. I started hiring people for the purpose of containment. Bob Sharp and Tom Weaver were both in J-9, the containment group. And so we had some pretty good guys, and for the first time we had some geologists.

So, the containment effort, as you see it now, is really derived from that containment group, J-9. That's when we started down that path.

House: I had never actually heard anything about containment until that December morning in 1970 when Baneberry vented. I happened to be in Mercury in the J-3 operations group office, waiting to ride into town with Bob Newman, who was then one of our Test Directors, to get a plane back to Los Alamos. There was this ominous gray cloud rising up over the Gate 200 pass. I didn't know what it was, but Newman proceeded to tell me that LRL had a really bad leak. We could see the cloud all the way into town. I happened to be sitting on the left side of the aircraft as we flew towards Albuquerque, and I could still see that cloud when we were clear out over the Grand Canyon, until the sight angle became diminished to the point where you could no longer see it. That was my first introduction to containment.

About two months later I got a call from Ogle saying that I was being temporarily reassigned to a new group that was being formed under Bob Brownlee. It was to be a containment group called J-9. Apparently Ogle told Brownlee that he had to form up a purpose-oriented containment group, and he could pick any of the J group numbers not in use, and Bob picked J-9.

The people who were in J-9 were Brownlee, Bob Sharp, Carl Keller, and a few other folks, probably less than ten, that Brownlee had assembled from other groups in the Laboratory. So we had this little cadre of dedicated personnel who were to do "containment," whatever that was. I didn't know anything about containment except probably how to spell it.

At that time, in early 1971, we were in the six months test moratorium mandated by the Atomic Energy Commission post-Baneberry. Now we were supposed to have information, geological information, about the shot sites. All the emplacement holes that Los Alamos had in inventory were cased. How can we do site characterization and examine the material properties in a cased hole? So, we initiated a drilling program for exploratory holes, in close proximity to the emplacement holes, that could be sampled and logged.

We initiated our program of exploratory drilling, and sampling, and so forth. Because we didn't have the necessary expertise to do the geologic analysis, the data went to the USGS at Denver, where Evan Jenkins and Paul Orkild and their people did the analysis. The USGS would then put together a site characterization package - - the cross sections and the whole nine yards. Livermore at that time was able to do those kinds of things in-house, because they had the necessary personnel. Billy Hudson and Cliff Olsen had been doing containment work for a few years, and they were our distant colleagues in this new, for me, world.

It was a real circus in those early days of 1971 while we were still learning the containment business. We didn't have any designated presenter for the events that came before the new Containment Evaluation Panel, as Livermore did. As I recall, Billy Hudson was the designated presenter. And we had no containment scientists as we know today, or event managers, as some people call them when they're trying to figure out what a containment scientist is. Well, how do we do this thing, which we had never done?

The very first event that was presented by Los Alamos to the CEP was a shot in Area 3. Bob Brownlee sat at the CEP table and read the prospectus to the Panel. I was sitting in the audience along with essentially all the rest of J-9, there being only a few of us, and that's how the presentation was made. The USGS sat in the

audience and responded to whatever geological or geophysical questions were posed by the Panel. Bill Twenhofel was on the Panel, and he was the USGS representative.

Carothers: No. Bill was on the Panel, but Jack, there aren't representatives of organizations on the Panel. There are independent experts. They may make their living by working for some organization, but they don't represent that organization.

House: Yes. I do understand that you do work hard to try to maintain that distinction.

Anyhow, Brownlee soon became dissatisfied with this mechanism of reading the prospectus to the Panel, and he concluded that Los Alamos would need a designated presenter. He also decided that we needed individuals who would be assigned to prepare the prospectus. They were to pull everything together from all the different venues, like the engineering folks who were doing the diagnostics rack design, and the operations folks who were drilling the holes, and backfilling them, and so forth. Carl Keller had been writing the prospectuses. Then one day Brownlee came to myself and Roy Saunders, and said, "Okay, we have a couple of these one-point safety tests, and they're in these little shallow holes, and they're not very complicated. Roy, you write one up, and Jack, you do the other one." And so we did.

Carl made the presentations for a while. Then, I guess he decided that really wasn't his cup of tea. So Brownlee called me one evening at home, and dropped this little nugget in my lap, saying that I was going to start presenting all the events. I was not real comfortable with that. Bob prevailed, as Bob always has, at least in my case, and lo and behold, not too terribly long thereafter I was standing at the podium presenting an event to the CEP. Lacking any experience, I mimiced Billy Hudson in my presentation. As time evolved, the containment prospectus preparation and presentation became my task, with a lot of support from my colleagues.

We continued on with the USGS supplying our geologic packages until about 1974 or 1975, somewhere along in there. That relationship was not always comfortable for the USGS people up in Denver, because we didn't know, in the early days, what we really wanted or needed. So, those guys didn't know quite how to respond to our needs. As a result, we had some interesting meetings, hosted by the Lawrence Radiation Lab, about things like

grain density - - what did we need to measure in grain densities, and how should we do it? Should we use the air pycnometer, or should we use this other kind of trap, or what? And what should we expect in alluvium, and what should we expect in tuff? And how about the lavas in Pahute Mesa? So, there were problems in determining data needs.

Then, in late 1974 the USGS made an incredibly gross error in the assessment of a Paleozoic scarp location near a hole in Area 4. That caused us to have to drill a special exploratory hole, do sidetracking, and so on. The J Division management, Brownlee in particular - - as an aside, Brownlee had been moved on from being the J-9 group leader to being an associate or assistant division leader in the J Division office - - decreed that J-9 should hire a geologist, and diminish our dependency on the USGS.

Where were we going to get a geologist who knew anything about the Test Site? Well, we had Fenix and Sisson geologists who supported Los Alamos as our "well-sitters." They sat the emplacement holes, or the exploratory holes, as they were being drilled on the Test Site. So we thought, "Let's pick the guy assigned to Los Alamos, and let's hire that guy. He's got to know something about the geology of our test holes."

We hired a young man named Mike Ray from F&S. Mike came to work with me, and we then hired a geophysicist to do well-log analysis on the Birdwell logs supplied to us. Gradually Los Alamos developed enough of a geoscience capability that we were able to tell the USGS that we didn't need their geologic packages anymore, because we were going to do that in-house.

We, I think, separated ourselves from the USGS without acrimony. I think, quite frankly, the Survey was relieved to get out of that production mode. That's not their cup of tea. They are primarily a research organization, and they don't like to be called and told, "Look, we needed this yesterday. Where is it?" And then to be called back and told, "Well, we got it, but it's not right. Now you need to do this, and that." There weren't many of those occasions, but still, that didn't fit the Survey's view of themselves.

So, here we are in the mid-seventies now, and we have our own geosciences capability. House is making all the presentations and writing all the documents, and so forth. That went on until 1979, when we changed Lab Directors. Harold Agnew left the Laboratory,

and Don Kerr, a former J Division staffer from years before, took over as Director. One of the very first things he did was to dissolve J Division, the field test organization. He spread out the groups that were in J Division to other divisions, such as WX, weapons engineering. Then they looked at the containment group and said, "What shall we do with these guys?" Well, Brownlee was then the geosciences division leader, and that seemed like the right place to be. We do geoscience stuff, and Brownlee is known in our Laboratory as the father of containment at Los Alamos, and so let's put these guys, the J-9 guys, over there in G Division, and call them G something or other.

I've forgotten why they chose to dissolve J Division. It was much to the dismay of the people in J Division. We all ended up working for other existing divisions in the Laboratory. The diagnostic guys were all put in the Physics Division, and they were called the weapons physics guys. The field engineering and rack design guys were put in WX, under a management they had not previously been associated with.

Kunkle: I was hired by J-9, but when the paperwork was done the group called itself G-6. By the time I showed up in 1980 it was calling itself G-5. According to rumor that was because Don Kerr, the then Director, decided to get rid of J Division, the field testing division, in the fall of 1979 because he was concerned that a comprehensive test ban would soon be enacted, and a field testing division would be something easily clipped out of the budget. So he decided to - - I wouldn't say hide - - submerge those activities in other divisions. One of the divisions created, an artificial division, was G, the Geology Division, and Bob Brownlee became the division leader of that.

House: Actually, J-9, the containment group, came out best, because we were reassociated with our former boss, and that worked out reasonably well for us, and for the containment organization.

There were three supporting groups, discipline oriented. There was geology and geochemistry, geophysics, and something called geoanalysis, which we just called computer jocks. So we were in some common organization which has metamorphosed through being called Geosciences, then Earth and Space Sciences, to now being called Earth and Environmental Sciences.

Then the Containment Project Office was created, and I was named as the Deputy Project Leader. In late 1980 the gentleman who had lasted but eight months as the Principal Project Leader decided to seek other employment, so he bailed out and went to another division which had nothing whatsoever to do with containment. As a result of that Bob Brownlee called me into his office one day and said, "I intend to make you the Containment Project Manager." I was either too stupid, or too stunned, to say no, and so that became my task, in addition to making the presentations, and writing the documents, and all this other large load of responsibilities.

After two or three months, maybe as many as six, I went to Brownlee and said, "I can't do all this. It's too much." He said, "Well, what do you want to do then? How do you want to structure this?" I said, "I want to follow Livermore's model of having containment scientists, or event managers. I want to make a selection of people, and for openers I'll pick Fred App, who's one of our CEP members, and Eric Jones, and Nancy Maruzak, and we'll make them into containment scientists. They will be responsible for the event from the time I assign it to them, and we lay out the parameters for it, with the yield and the location. And they'll carry it through the presentation, and ultimately the post-shot report, to the Panel." Brownlee said, "Okay, we'll try it," and so we did.

And that's where we are today. except with far fewer people. At one time in our glorious past, which, as I recall, was fiscal year 1984, the Containment Program had 34 FTE's, which represented about 42 to 45 actual personnel. That was pretty big, and it was pretty much paralleled by our colleagues at Livermore in their containment program.

Brownlee: When the containment goups were large, and even before, there were a good many opportunities for Los Alamos and Livermore to work together toward common goals. There were also many opportunities for disagreements. At any given time, both kinds of activities were on-going. It was therefore possible to believe that no cooperation ever occurred, or that good togetherness was possible, depending upon just where one happened to sit. I happened to have one foot in each activity, and remember a few occasions when Los Alamos asked cerftain questions that caused some difficulty for Livermore in public meetings, yet the result

enhanced certain arguments that Livermore's containment people could not win at home. So, some debates were based on what I will call "a non-obvious agenda".

House: A sidelight that I would like to bring to your attention is the incredible acrimony that existed between Lawrence Livermore guys and Los Alamos containment guys in the early, if not almost all the way through, the seventies. It seemed to be initially precipitated by two adversaries across the table, who shall remain nameless, who got into a shouting match one day at a CEP meeting. I remember it as well as if it were last week. Those two gentlemen were summarily removed from the Panel by the Chairman. One of them, by his choice, no longer works at Livermore, and the other one, by his choice, is retired from Los Alamos. But for a long time there was an incredible acrimony; there was a real bad - - them guys at Livermore, and vice versa - - attitude.

Carothers: I know the two gentlemen to whom you refer. I remember the situation, and I did remove them from the Panel. I must say I had a little difficulty with Los Alamos. I talked to Brownlee first, and he was understanding of my position. Then I got a call from Dr. Charles I. Brown, who informed me that the Los Alamos Scientific Laboratory would decide who would be their representative on the Panel, and that was not something that was within my purview. I explained to Dr. Brown that Los Alamos did not have a representative on the Panel; that was not the way the Panel was constituted. The Laboratories, and other organizations, nominated people they felt were reasonably expert in the field, and subject to the Manager's approval, and mine, those people could serve on the Panel. Anyway, I won.

House: Yes. I noticed that you did, and I have remembered that. And, quite frankly, the tension level was reduced dramatically as a result of that change of personnel. Anyway, to carry on with this, and not to beat a dead horse, it wasn't until Larry McKague became Livermore's containment project leader, then succeeded by Frank Morrison, that we started working together to try to reduce this tension and acrimony. I remember Carl Smith sitting at the CEP table one day and commenting about the acrimony that apparently existed between the two Labs. That caused me to think about how we could defuse this. We were viewing the attitude of our

containment colleagues at Livermore as a "hassle LASL" attitude. I thought that was no good, and that we ought to do something to fix that.

The first real case of a friendly gesture was when the late Frank Morrison invited me out to sit in on a Livermore pre-CEP meeting. This was absolutely unprecedented ! Sit in on a pre-CEP? That's inviting the enemy into your camp; the fox into the hen house. But I went, and it was great, although I was a little uncomfortable, needless to say. Then after the pre-CEP, that evening I was invited to Frank's home, and he had some of the Livermore containment folks over for dinner. That was what really broke the ice, I think. After Frank's tragic demise I continued to work with his successors, up to and including Norm Burkhard, the current program leader. We have, I think, maintained a much better attitude. We don't hold hands, per se, but we do talk to each other, and when the Laboratories independently review each other's containment prospectuses prior to presentation to the CEP we try to air all our dirty laundry, behind the scenes and before the CEP meeting, so we don't get in there and have one of these acrimonious activities.

Carothers: Do you think that's proper?

House: The reviewing of each others shots? I think that's a very important part of the checks and balances that seems to be built into the containment community. And of course you must understand, it doesn't exist just between the two Laboratories. We get comments from the USGS, whose primary focus is on the site characterization package. When Russ Duff, of S-Cubed, was on the Panel, he would call me up with a concern, and we would discuss it. Sometimes it was a simple question that needed explaining, and perhaps Russ didn't feel he might want to raise it in the forum of the CEP, but he really wanted an answer. As far as the two Laboratories looking over each others shoulders, and as one wag has been known to say, "keeping each other honest," I think it's a very important part of the way we do business.

There have been occasions when either Lab has served notice on the other one's event, via the prospectus mode and response. "Maybe you guys ought to look at this," or "Have you really calculated that, and do you really believe those numbers?" So I think it's incredibly important, and it's been something we've continued. The way it works is quite simple. We transmit the

prospectus to the containment community, and of course the other Laboratory is included, and we expect a response within a week or two. It usually comes as a FAX, and is prepared by one of the principal containment members of the organization. In many cases it's the CEP member, like Cliff Olsen at Livermore, or Tom Kunkle at Los Alamos. And there will be questions and comments on the event. And then we interact, and it's most helpful.

Carothers: Then why do you need a CEP? And I do not mean that as a frivolous question.

House: No, I don't take it as such. Why do we need a CEP? The Panel represents a rather broad scientific experience base. Hydrologists, geologists, radiochemists, people who are well versed and have expertise in the calculational side of the house, and people we might refer to as phenomenologists. These folks are looking at the sponsoring Laboratory's containment plan from, hopefully, an independent viewpoint. So you have nine or ten individuals reviewing and discussing and questioning the plan of the sponsoring Laboratory. It provides a review that is, in my experience, unparalleled for plans of operations that are going to go forward, especially with something as critical as an underground nuclear test.

The Containment Panel review has a distinct ES&H aspect to it. Back in the days before Admiral Watkins we didn't call it ES&H, but it certainly is environment, safety, and health oriented, and it's a big part of the whole thing. There have been occasions when the sponsoring Laboratory had an event reviewed by the Containment Evaluation Panel, and has had to step back and say, "Well, maybe we didn't do this quite right. Maybe that hole isn't suitable for that event." And so, appropriate steps and responses are taken. Once again checks and balances are in play.

Carothers: There are some people who feel that the process has gotten to be pretty cut and dried, and that it has become a kind of ritualistic process that you have to go through. And that the CEP doesn't really do much, other than providing a public facade of reviewing the Laboratories' and DNA's activities.

House: I think you would find, if you ask any seasoned member of the containment staff at either Los Alamos or Livermore, or at the DNA, they would strenuously object to that. To some it may seem like a rote process, where you go to the Panel, and you stand

up and you present the standard set of viewgraphs, and you make the standard apple pie and Chevrolet arguments. But the fact remains that you are having your containment plan reviewed by, not a peer group, but a group of experts in the field of underground testing. While it may seem like the same old stuff, every time we go to a Containment Evaluation Panel presentation, granted it is repetitive, you will find that each containment scientist is extremely sensitized and concerned about the design they have put together. And not just to get it approved.

Carothers: When a person does a presentation, in front of friends and peers from his or her own Laboratory, and people from a couple of other, may I say possibly competing organizations, and from various other places, there are going to be questions about various aspects of the plan. Basically that person doesn't want to look stupid. I wouldn't want to stand up there and make a fool of myself.

House: I know what you mean. I've been there, Jim. But consider this. Both Laboratory containment staffs have what we call pre-CEP meetings. Or you could refer to them as dry runs for the presentation. The way we like to view it is that it's a heck of a lot easier to take flak here at home, from your peer group, and be prepared, and be able to answer the majority of questions that presumably might be posed to you, versus doing it in front of the Panel. I likened the CEP presentation, to Brownlee, and mind you this was back when I was doing them all, in many cases one a month, especially during the high yield test series in '76, to be like defending your doctoral thesis once a month.

It's a pretty stressful situation, and sure you don't want to look bad, and sure you'd like to get your event properly categorized and approved. But by golly, when push comes to shove and the Panel, as a whole, or as a individual Panel member, finds something that is unsuitable, whether it isn't understood, or what have you, we better to step back and take another look rather than try to move forward with something that might cause a problem at shot time.

And, you don't want to present something that might not make it through the detonation authority process. I can remember one time when the Chairman's recommendation to the Manager, and

the Manager's subsequent forwarding of the detonation authority package back to Germantown didn't guarantee the event was going to get approved by Headquarters.

Carothers: It has happened once only that the DOE Headquarters has refused the Manager's request for detonation authority. That was for the Kawich event, and I consider that to have been an embarrassing failure on my part. I went to the Manager and apologized for having put him in that situation.

House: Well, it is an awful feeling, and as I said, I have been there, to stand up in front of the Panel and get put on the run. Once I got a post-presentation viewgraph from my Livermore colleagues about the "wounded rabbit" syndrome. Or, to have an event come up to categorization and have someone on the Panel give it a dissenting vote. It stops everything dead in the water.

The DNA took a different route than the Laboratories in approaching the problem of containment. They were doing both vertical and horizontal line-of-sight shots, with limited success in containing the radioactive products of the detonation. And, like the Laboratories, they were losing experimental data. Joe LaComb, DNA, had much of the responsibility for the way the events were designed and constructed, but he had no person designated as responsible for containment.

LaComb: By 1966 we cared about containment, and I cared about it, because it was the same as it is today. If we don't keep all the detonation products in close we don't accomplish what we want to accomplish. We lose our experiments. On Double Play, which was in June of '66, after we had problems with Red Hot in March, Discus Thrower in May, and Pile Driver in June, Jack Noyer came out and said, "How long will it take you to build an overburden plug?" So, we built an overburden plug in five days. We put that plug in because our containment record wasn't very good. We already had a gas-seal door in the drift, so when we ended up we had, in general, the same kind of configuration we do nowadays, although we didn't have a lot of the things we do now, like cable gas blocks. That plug was strictly for public safety and health. It wasn't going to help our experiments at all.

I don't think at that time DASA had anybody who was designated as the person to be concerned about containment. There wasn't really anyone who was given that responsibility, other than Jack Noyer. Being the kind of person he was, he tried to worry about it all. A person I listened to was Wendell Weart, from Sandia. He was the one who came out when we had questions regarding how should the stemming be placed, should the hook drift be left open or should it be backfilled - - those kinds of things. Mel Merritt was another one who helped.

It was about that time that they started doing calculations with a bunch of folks who were with General Atomics. It started out with some GA folks involved, and there was, right after Double Play, some RAND people involved. For Door Mist, in '67, it was those contractors who were doing the calculations, and were saying we want this kind of grout with this kind of strength, and so forth. As far as the overburden plug and the gas-seal door went, that was more or less our engineering problem. There were no real criteria.

So, there wasn't anybody in DASA, in the Door Mist time frame, in 1967, saying it was this or that, that DASA wanted. The contractors were saying, and saying more or less directly to myself and the Test Group Director, "This is what DASA wants." Somebody said they wanted a plug, or particular kinds of grout, but it wasn't my job to define those things, only from the standpoint that I tried to make sure we got the materials that the "experts" thought they wanted.

Right after Door Mist, where we had more problems, Noyer told me, "That won't happen again. You're going to take care of this." I said, "Yes sir." We could see we were going to have to pay some significant attention to protecting the experiments, and to stopping leaks. So, after Door Mist, for every test I sat down, and I wrote out the criteria for the grouts; what the velocity should be, the strength should be at least this, and so on. And I set down criteria as to how things were going to be done, and what should be done. I worked fairly closely with the people doing the calculations, although I didn't understand what they were doing, at that time.

It was, in our program, always a constant threat that the people funding the experiments would decide that the possibility that they would lose a lot of their data was too big to take a chance on. That's one of the reasons DASA tried to turn things around so rapidly after

the four in a row in '66. I don't think it was so much a big concern about the fact that we were releasing a little radiation to the atmosphere. It was the loss of the experiments, and our credibility. I think that has always been a factor, and still is.

About that time we formed what we called the Stemming And Containment Panel Junior, or SACPAN Junior, which was a working group. It was a real mixture. There was Court McFarland from headquarters DNA, who was an aeronautical engineer, but very interested in materials behavior. There was Ben Grody, who had a doctor's degree in geology, and Bob Bjork, who was, and is, an excellent physicist, myself, and Jerry Kent. That group, in my opinion, really turned our containment program around. I think that group was the real foundation of the DNA containment program.

When Baneberry happened we were working on Misty North. We were also getting ready to field Diagonal Line. We had to go and present a risk-benefit analysis for the Diagonal Line shot to get permission to fire it, because some guy named Jim Carothers, on the Panel, said it was going to leak. Actually that turned out to be good, because it did leak, and we had gone through it, and they had okayed it with that possibility in mind. So, they weren't surprised. We also had to do that for Misty North, because there wasn't a whole lot of confidence. Fortunately, that one contained.

We also had the presentations we had to make to the CEP, and there were problems with some of the early shots when we presented our material, in getting our ideas across about what we were trying to do. I was talking to Carl Keller one day, and we were kicking ideas about this problem back and forth. He said something about vessels, and I said, "You know, it might be worth thinking about that." So, it was when he and I were talking that the seed was planted, I think. I decided there had to be a logical way to present this material, so you could say, "That will be coming in this section here." So, I sat down and I said, "Okay, we've got three vessels, nested together, and each one backing up the ones inside it." So, I started writing up the presentation based on the three vessel concept. I made out the outlines, and then went back and started writing how you would do it. They're still using some of the same words today.

In '74 DNA hired Carl Keller to be the Containment Scientist, and early on he began to develop an experimental and calculational program to try to understand some things about what was going on. Through the years, thirty to forty percent of our effort with Pac Tech and S-Cubed has been in research, to do something new and different, to find out something. We've got to do our production work that's associated with the test, but it's essential to me that we still keep enough effort in there to try to find out if there isn't something there, something we're missing. I always have the feeling there's a shadow lurking around the corner.

Keller: When I came to DNA I think the title of the position was Containment Scientist, and there was a job description associated with that, as the Civil Service requires. That job description had been developed by Jay Davis, my predecessor at DNA by more than a year, and by the Director of the Test Directorate. This was a new job description, with their new concept of what the Containment Scientist ought to be doing.

At that time DNA had numerous problems, and they had decided that they needed a heavier gun in the Containment Scientist position. They upgraded the position from a GS-14 to a GS-15, which meant they could offer more pay. I know that they had solicited several senior people in the containment business to take that job - - people far more senior than I was. Those people, I suspect, were already above that pay grade, and probably well established in the Laboratories. I wouldn't say I was the bottom of the barrel, but I was certainly not their first choice for the position.

It was an interesting environment at DNA. They hadn't had any recent leaks, but they had had some real encounters with the Containment Evaluation Panel. I remember one of their presentations to the Panel where they had decided not to present any of the mechanical closures, because they felt those were only relevant to sample protection. Therefore, the DNA people who were presenting the event refused to present any details about the closures, because that was irrelevant to containment. Well, the Panel refused to categorize the shot, because they thought the closures were relevant to containment. Then Phil Opedahl, who was the Test Group Director on that event - - I believe it was Husky Ace - - stood up and said, "Just a minute. Mr. Chairman, could we have a short recess?"

After the recess it was, "We'll provide you with any of the information you want." It was pretty clear where the concept that the mechanical closures were not containment features came from, because for many years thereafter Joe LaComb still insisted that the MAC's were not containment features. I never agreed with him on that point, and so we always described them fully in the CEP documents.

One of the concerns was that it handicapped the Test Director to have all these non-containment features included in the containment presentation. But it was decided by the Panel that those features were important to containment. And the Panel was right. You can say with confidence that after Mighty Oak those features were thought to be very important. So, that's an old concept that's been abandoned, but I don't think DNA totally abandoned it until a couple of years ago.

When I came to DNA their containment program was really being managed by S-Cubed. There was no Containment Scientist, and had not been one for over a year. DNA was doing as well as they could with the few military people they had, some of whom were quite new to the business. I wouldn't say they were desperate, but they were really being controlled by the contractors. DNA had very little in the way of technical capability in-house, so they really relied almost completely on their contractors, and S-Cubed was happy to step in and supply all the advice the DNA needed.

For me it was a totally new environment, dealing with contractors, because when I was at Los Alamos contractors were considered second-class citizens; rude, mercenary, science-for-hire kind of people. They are still mentioned with a sneer. It was at DNA that I discovered that contractors did offer far more than you'd ever believe from the way they were considered at Los Alamos. I found they really were responsive, partly because you controlled the purse strings, but also because they were very capable. My whole staff was essentially contractors, and over the next ten years I gained a great deal of respect for them.

The whole concept of contracting for support does isolate you a bit from your staff, but you do define what the deliverable is to be, and what the price will be, and what the schedule will be. I found that I got results from the contractors much more predictably than, say, a program manager at Los Alamos would get from his staff.

That's because his staff might be scattered all over the Laboratory, and he was always competing with other programs in the Laboratory for the attention of those people he needed.

I found I very much enjoyed the contracting process as a way of doing a program. It really worked, and we got a lot of good results, though I was always worried when the contractors agreed with me. Was it because I had control of the money, or was it because they thought I was correct? And I was never sure.

5

The Nevada Test Site

During 1948 and 1949 a committee headed by Lt. Gen. E. R. Quesada developed a list of five potential sites that might be used for continental nuclear tests. The candidates were the White Sands Proving Grounds in New Mexico, Pimlico Sound in North Carolina, Dugway-Wendover Proving Grounds in Utah, an area between Fallon and Tonopah in central Nevada, and the Las Vegas-Tonopah Gunnery Range in Nevada. In 1950, after the approval by President Truman of continental testing, some 1360 square miles of the Gunnery Range were turned over to the AEC for the conduct of nuclear tests. The Nevada Proving Grounds, now kown as the Nevada Test Site, currently has an area slightly larger than the state of Rhode Island, and lies some 70 miles northwest of Las Vegas.

There are various criteria which could be used in the selection of an area to be used for nuclear testing, the importance of each of which would depend on the judgment of the person or persons making the selection. Remoteness from populated areas, availability of air, rail, and highway transport, security, and so on are important criteria. Another thing that would be of importance is the type of nuclear tests to be conducted, or which might be conducted, although that did not appear to be a factor in either the selection of the Pacific Proving Ground, or the Nevada Proving Grounds. Enewetak and Bikini, for instance, were too small to do experiments to explore the effects of the detonations on structures or military hardware, Crossroads notwithstanding. The possibility of underground detonations was not considered when the selection of the Nevada Proving Grounds was made.

What would be the geologic characteristics of a suitable site, where underground detonations were to occur, and the radioactive products were to be contained?

Brownlee: I'm of the opinion that we have not actually had to address that problem, thanks to the fact that we are where we are. In other words, we were blessed, in a sense, by being put down in a place not of our choosing; the Nevada Test Site. We have made the best of that without having the freedom of choosing where in the world we'd like to go to do the best underground testing.

As we've understood from the Soviets, and from the French, and from anybody else who's tried to test, we've had a wealth of different sites and different media, with opportunities for different kinds of tests. If we want to test in granite, we can; we don't have to go to North Africa. If we want to test below the water table, we can. If we want to test above the water table, we can. We can test in various kinds of alluvium, and in various kinds of tuff. Most places in the world are not blessed with all of those opportunities. So, without having had any opportunity to select the site, we have been lucky, if lucky is the right word, in being able to find a great variety of media within the confines of the Test Site.

Carothers: Do you mean that if in the fifties someone had said, "We want you to test underground. And, not one atom out. Pick a good spot, in the United States, and you can have it," we probably wouldn't have picked a place as good as the Test Site?

Brownlee: Absolutely. If we had used our heads we would have been in terrible trouble. We were very slow to learn all of the different opportunities we had for the different kinds of tests that we wanted to do. For example, did we want to mine a big room? At the Test Site we could. Did we want to do something and throw a little dirt on it? We could. Did we want to lay something out on the surface and shoot it? We could. We could do that because in those very early years we were sufficiently removed from anybody that we essentially had everything around us, and a long way around us, under our control, with a few exceptions.

When we went underground the number of milk cows in the fallout pattern we might have would sometimes be three, or six. The number of people for whom you would have to provide the means of evacuation, were that to be needed, would be twelve, or twenty. What's happened with the Test Site since those days is that people have moved right to the boundaries of the Test Site, and there are literally hundreds and thousands of everything and anything. But people see the Test Site as it is, and they don't understand that it was selected as it was.

Early, it seems to me, we had an emplacement hole and we used it. Even after Baneberry we still used the hole that was there. We had them stockpiled, and we tended to use them. It took us quite a little while after Baneberry before we really selected a site we wanted for the shot. I think it's only in relatively recent times that

the containment people have had some input, really, as to which hole they wanted for a given shot. There are times when we will have a deep hole, and shoot near the top of it. That deep hole was drilled for something else, but we make use of it. I can find all kinds of examples, still, where we don't select the site as logically as we are able. We do things in tuff that might better be done in alluvium, and so forth.

There is one other thing, and that's the water flow. I think it's bad business to get plutonium into an aquifer. It happens that at the Test Site the water doesn't go anywhere. To the first approximation, it just sits there. Al Graves didn't know that either, when he selected the Test Site. And so from a radiological point of view, a hydrologic point of view, the Nevada Test Site is peculiarly able to support testing.

On the other hand, on two or three occasions we've found debris from shots at places that we had no expectation of it being. That tells me it's entirely possible that things are going on down there for which we have only had an occasional whiff. And that we are not anything like as knowledgeable about the water flow as we think we are.

Carothers: I have talked with the folks who calculate what might occur in different types of rocks, if you were to shoot in them. They don't like very hard rocks like granite because of the tensile cracks that show in the calculations. And they don't like very weak rocks because they don't sustain the residual stress fields thought to be important for containment. Something like tuff or alluvium turns out to be just about the best you can do. Isn't that surprising? Believe the calculations or not, they imply that if we were in some other site it probably wouldn't be nearly as good.

Brownlee: We couldn't have possibly done as well, because I don't think there's any other place that could be as good. Now, that's said out of ignorance too, but let's look at Pahrump, which was actually considered. The whole history of the world would have been different. The water table there is so high that all of our testing would have been different, and we would probably have had the Russian experience of having something come out from each test. Or we'd have spent so much money that we couldn't have afforded it. I bet you anything that if we had started out testing in Pahrump either we would have abandoned it, or we would have still been

there and, for example, we would never have had room for the British. All kinds of things would have been different. It's really true, I think, that the selection of the Test Site actually was a branching point in history. We took that new road without ever knowing why or what we were doing, but I feel it was a very fundamental action.

As an aside, we are in the process of losing the Test Site for underground testing, and no one seems to know or care. The majority of the NVO budget is now devoted to things other than underground testing. And their interests are in Waste Management and various other kinds of things. They don't mind bringing in all kinds of people, and building new things, and doing new experiments. They would sell off any part of the Test Site to keep NVO green, and they have so cluttered up the Test Site with other kinds of activities, which are now sacred, that it's increasingly difficult to get a shot off.

And you haven't seen anything yet. I actually challenged one of the Assistant Managers at NVO a few months ago, and said, "I believe the biggest threat that we have to the Test Site is NVO. It's not the people on the borders. It's not Las Vegas. It's not the anti's. It's ourselves. We're our biggest enemies." I didn't know whether I could get away with saying that, but he pondered it a while and said, "I see what you mean, and I believe you're right. We really have plans for doing all kinds of things." They're preparing for a moratorium, and so they're going to do all kinds of things.

They imported a lot of activities during the last moratorium. And when we go into another test moratorium, which I figure will come one of these days, I don't believe we'll ever go back to being able to use the Test Site again. I've said what I think are the characteristics of the Site that are to our advantage, and I believe that there is hardly anybody in NVO who understands them. They do not appreciate what they have. They do not appreciate what it means to have such a place.

I believe that as long as there's a nuclear stockpile we have to have the ability to address questions which may arise. That might be continued testing, or it might not. It might mean a nuclear test, or it might not. I'm talking about questions which may arise, and we have to have a place where we can go to answer them. Nowadays, I visualize that being underground. Even chemical

experiments could be underground, and not on the surface. We need a variety of ways to be ready to answer a variety of questions. So, we need alluvium, and granite, and tuff, and shale, and dry, and wet, and space, and mesas, and valleys. We need it all, because we do not know which part of it we can give up. I believe we will have a nuclear stockpile for at least the next four decades. Forty years. I can't conceive of getting rid of it in less than forty years time. I would like to think we can, but I can't believe that we'll be able to do that.

If you look at what's happened at NTS in the last forty years, it's an exponential curve. And if we have any kind of a moratorium it will get worse. With Baneberry we stopped testing, and at the end of six months, every week we delayed it was harder to start. We almost didn't get started up again; it got harder and harder as time went on. I think that if there is any kind of a moratorium, it will be that way again. And so, I almost despair over the loss of our Test Site, because I think it's happening, and NVO couldn't care less because they have no appreciation of what I've been trying to say.

I shouldn't say, "no appreciation" because I've been lecturing to them, and waving my arms at them, and writing things down and showing it to them. But it doesn't take. After all, we don't have to pay any attention to what Brownlee's saying. We've got this waste management program, and the President has said, Secretary Watkins has said, "We've got to clean up. That's the urgent thing." So, NVO is no longer interested in stockpiles and testing; that's just way down on the list. You noticed in the memo we got from Watkins, where he outlined all the things DOE did, that he never once mentioned nuclear tests at all? It's not listed in his long list of things that had to be looked at. Nuclear test is not mentioned. So, the DOE has little appreciation of what I'm saying.

And what's worse, neither do the Laboratories. This question that you've asked me about the Test Site I think is an exceedingly important question. We ought to recognize what the Site means to us, and I think we don't. Notice that I've said, "Despite all the opportunities we've had to use it cleverly, we haven't." And now I'm saying, "Even though we've learned as much as we have, we're getting ready to throw it away."

That's the reason why I spent quite a few months writing a document on the preservation of the Test Site. And I got Troy Wade to finally enunciate the DOE policy on the preservation of the Test Site, which in effect says to NVO and ALOO that any decision made about the day-to-day operation of the Test Site has to be made with the idea that we're going to preserve it for testing. I was hoping by that means to get something down on paper which then I could wave in NVO's face, saying, "When you bring in these rug merchants, that is contrary to the policy of the preservation of the Test Site."

But doggone it, there is something kind of sacred about the Test Site nowadays. Put away the conservationists, and the preservationists, and the purists. We have done some things there that are special in history. They are. In the history of the world, a thousand years from now, that's going to be something kind of special. We ought to have that in mind as we act, but our concern is only this year's budget. That's all. And we ought to be bigger than that; we ought to be thinking more broadly than that. I'm saying, "Here we have the Test Site. We could use it much more cleverly than we do, but we're only interested in the current budget with this fiscal year's shots. That's our only concern, and therefore, we just do things as they come." I think that's a grave mistake. I doubt that we will ever do it any other way because that's the way our government is, and that's the way we are. But I wish it were otherwise, and I would like to really understand more about containment by doing things of various kinds at the Test Site.

Another point I want to make. I only learned in recent years that when it comes to space you don't need a nuclear test to affect it. You can seriously affect space with just a little bit of energy. If you have what the United States government assumes is empty space, a vacuum, it doesn't take very much mass there to change it. Actually, the United States government is mistaken, because it's not empty at all. It's already cluttered up with a lot of things, and so when we put something else up there and put a little energy there, it interacts with the stuff that's already there that we've put there. This is not appreciated. Well, I believe it's a grave mistake to do experiments in space that can be done underground, or in tanks, or in other places. And the NTS has the capacity to allow us to do many space-like experiments. Now, they say, "Oh, you can't have a mountain that will hold a vacuum." A lot of the key questions that

you need to answer in space are not those questions, and they could be answered easily in Nevada. So, I think we ought to preserve the Test Site for space research, strangely enough.

Carothers: That does seem strange.

Brownlee: But let me use this analogy; I said we originally thought of using the Test Site on the way to the Pacific. I think we ought to use the Test Site on the way to space. And so I'm not really talking about space experiments, I'm talking about some stepping stones on the way there. And for reasons that are lost to me, there's almost nobody who understands that yet, although I think they will in time.

DNA is leading us there. I think they're leading us in that direction, and eventually these things will occur to people. Now, when they do, will the Test Site be available? I'm afraid it won't be, and so that gives me grave concern.

What I'm doing here is taking an exceedingly broad view, and I know that. Let me summarize all this up by saying, "I think we need to look at the Test Site with much broader, much wider-angled glasses than we have the habit of doing." I feel very strongly about that, but unfortunately I can't convert anybody.

From the first detonations at the Test Site in 1951 until 1957, when the first underground shots were fired, the geologic structure of the Test Site was of little importance. There were a number of air drops, devices were placed on towers, suspended from balloons, fired on the surface, and two that were emplaced at the modest depths of 12 and 67 feet. The only information about the geology that was required was enough to allow the design of the footings for the towers.

In the Plumbbob operation in 1957 the first underground events were fired. It began to matter what lay beneath the surface, for the tunnels that were being dug and the emplacement holes that were being drilled.

Twenhofel: In the early days there were two aspects of Survey work here. One was ground water contamination. What was the water table like, and where was it. So there was some drilling done, and ground water testing. The contamination of water was the impetus for that. The other thing was mapping. There were some early explorers who came through the country, and there were

geologists attached to them sometimes, so there was a general knowledge of the Test Site area. There had been some mining in this region, but there were no geologic maps. The first overall report on the geology of the Nevada Test Site was done by two geologists named Johnson and Hibbard, who were assigned out here with the Army. That was probably in about 1952. No comprehensive study had been done until those two Army guys mapped the Test Site.

On their map they plotted the kinds of rocks that occur at the surface. If the rocks dip at an angle, you plot on the map that dip. You end up with a map that shows the occurrence of the various rocks that you can see at the surface. You have to have a good base map so you know where you are, so you can be relatively accurate about where the various formations are. The U.S. Geological Survey published that report later, (Geology of the Atomic Energy Commission Nevada Proving Grounds Area, Nevada, Geological Survey Bulletin 1021-K) when the interest in underground events began to grow.

The first thing that happened when the underground program began to materialize was that the USGS said, "Well, we've got to have modern topographic maps." Those are the quadrangle maps that are used today. So, that part of the USGS which is called the Topographic Mapping Division flew the area for aerial photographs and then made the topographic maps. Those were done, and the next thing was to map the surface geology on the new topographic maps. It was in the late 1950's to early 1960's when the geologic mapping was done. And, they're the maps that are still used today.

Orkild: In '58 we opened an office in Denver, and that same year they said, "Ah ha, you're a photo-geologist. We have this big Nevada Test Site out there. We want you to analyze the western part of it, using photos." I said, "All right," never having looked at volcanic rocks before, but sure, we can do anything. If we can find uranium, we can find volcanic rocks. So that's how I got involved in Test Site work. In 1961 I joined what they called the Special Projects Branch, which was formed to do work on the Test Site. And then I took over and started doing photo-geologic techniques for the mapping of the Test Site.

The USGS had been involved in '57, '58 in the tunnel work out in the Rainier Mesa area doing some of the pioneer work for doing shots in tunnels, and containment in tunnels. Prior to Rainier the

USGS did two high explosive shots in tunnels in Rainier Mesa. Those were right where the miners' camp is now. They were in east Rainier Mesa; not Rainier Mesa proper. They were a little to the west of the Area 12 camp.

Twenhofel: The Survey shot the first contained high explosive tests at the Test Site. There were two; ten tons and fifty tons. They were done in Rainier Mesa, in what were called the USGS tunnels. As far as I know, those ten and fifty ton shots were the only high explosive tests done at that time. They were done entirely by the USGS, for containment purposes. They were steps in scaling up to Rainier.

Carothers: Do you recall how the USGS become involved in detonating rather large amounts of high explosives?

Orkild: I guess they were sitting there, minding their own business, and one day got called by somebody in the AEC who said, "We need somebody who has experience in mining. You dig holes in the earth, so you must know something about it." Of course, nobody knew anything about it. Including me.

But the first reason we got involved with the Test Site was to understand what the rocks were. And that's how the mapping started up in the Rainier Mesa area. The USGS started mapping in quadrangles, so to speak. The Rainier Mesa was done first, and then it expanded from there. During the moratorium was when we did most of the geologic mapping in the northern part of the Test Site; Rainier Mesa, and over toward Oak Spring Butte, and the Climax Stock. That's when the quadrangle series mapping started; it started out to be three quadrangles. I think they're called Tippapah, Rainier, and White Rock Spring. When we finished that we had a big celebration and said, "Hey, we're done with the Test Site. We'll never have to come back. We finished all this mapping, and we're done." That was in '60 or '61, and then, lo and behold, the moratorium was over, and things picked up very actively.

Carothers: Do you know how it was that Rainier Mesa was chosen for the Rainier event? Do you think it just that the mesa happened to be there, or was there a particular geologic reason to pick it?

Orkild: Well, I think both. I think it was there, it was a nice mesa, and it had very mineable rocks. I don't think anybody worried about the physical properties of the rocks; it was just a place to put a hole in the ground. The tuffs are easy to mine; they're very competent, they hold up quite well, and you can make a tunnel in them very easily. It's easy mining really, better than mining in very hard rock.

By 1957 the USGS had a role at the NTS that has continued to the present time. Both Los Alamos and Livermore have had a continuing involvement with the Site, and both of the Laboratories established organizations with people permanently stationed in Nevada. However, the USGS did not.

Twenhofel: None of us ever moved to Las Vegas. There was a lot of pressure from the AEC to move the USGS group, because it was a fairly sizable group as things developed in underground testing, but we resisted that. Jim Reeves was the Manager, from Albuquerque, and he tried to exert pressure to have the USGS group move here. Studies were made about the costs of air travel, and per diem, and so on, but the move never came about. It was not the people who resisted; the organization resisted. The USGS is somewhat paranoid about becoming beholden to outside money. We do take it, but we're going to keep our independence and our objectivity. There was a real fear that if this group, assigned to work at the Test Site, would go there and be officed near or in the AEC, the people in it might lose their independence and their objectivity. That's a strong feeling in the USGS. So we never moved down here; we commuted and lived at Mercury.

At one time we had one person stationed here, and we rotated. We had a liaison office, you might call it. The guys would come down here for a month and be in the liaison position, but it just didn't work. That person had no authority.

Jenkins: Since 1966 I have put my roots down on the Test Site. I think the geologic work at the Test Site is fantastically interesting. There are very few places where so much drilling has been done, and so much data have been collected. There are a lot of concepts being developed as a result of the exploration at the Site. That is what makes it a really fascinating place to work.

In the Flats there are about nine hundred holes that have given information we can use. Up on the Mesa, perhaps a hundred. Nowhere else in the world do you have a buried volcanic caldera with that much exploration. It's just unreal. Nobody else can afford it.

Carothers: Probably not. Well, the Department of Energy hasn't done that just for you geologists, out of the goodness of its heart. Do you get a good amount of information out of those holes?

Jenkins: Oh yes. And as time goes on, more information is gotten out of them. When I first got here we were getting caliper logs and drill-hole bit cuttings; occasionally some core. That was about it. Now we know something about the magnetic properties of the rock. We have good information on the density of the units, in situ, and of course the electrical resistivity logging has developed as time has gone on, but we don't really use all that we could of that. We can get the in-situ water contents now. There's the thorium-potassium ratio from the logs, and in our work that helps in determining which units have clay. The technology has developed by leaps and bounds since I got here. It's a real opportunity.

Carothers: From the point of view of the person selecting a site for an event, it appears that you could consider the Test Site as made up of three general areas. There's the Yucca Flat area, which is deep alluvium over tuffs, there's Rainier Mesa, which has extensive layers of tuffs, and then there is Pahute Mesa, which has various lava flows throughout.

Orkild: That's correct.

Carothers: How would you describe Rainier Mesa?

Orkild: It's a mesa of layered volcanic rocks. They were laid down essentially horizontally; some by water and some by air, and compacted into a very cohesive mass of rock.

Off to the west there were a couple of volcanos, and they were spewing ash and debris out. Some of that was flowing with the wind and settling out as dust. Other material that was blown up further came down as big clots, and some came down as hot glowing ash. These volcanos were to the west of the Test Site proper, over in what we call the Timber Mountain area, but the actual source for those rocks we don't know. There were never actual lava flows on Rainier, because it's too far away from the sources.

The later rocks, like the Rainier Mesa tuff, came from the Timber Mountain Caldera. The Grouse Canyon tuffs came from the Silent Canyon Caldera. Those calderas are very close — only ten, twenty miles away. On Pahute Mesa you're very close to the sources of all of the lavas, so you do have flows and pillows — one going out to the west, one going to the east, some going to the south, some going over the top of others, and so on.

What you're looking at in Rainier are the outflow sheets from those volcanic features. Some of the material rolled and surged down the mountainside, and came to rest in the place where Rainier Mesa is today. Time went on, and some thirteen, fourteen million years ago, maybe ten, Yucca Flat started to subside, and left Rainier Mesa as a high monolith, essentially as you see it. Its formation was accelerated by erosion, and the cliffs formed, and the rocks from the face fell into the flats. In Yucca, the faults that go down through the valley occasionally move over time, and form scarps, and the valley spreads a little more and settles some more. It is still moving today.

Carothers: On Rainier Mesa there is a layer of hard rocks - - the cap rock. Is that why Rainier Mesa is there?

Orkild: That's right. It has preserved Rainier Mesa itself, being a hard rock. Essentially what the Rainier Mesa cap rock is doing is protecting the very vitric, soft Paintbrush Tuff beneath it. Now, that cap rock, the Rainier Mesa member, has a lot of vertical fractures in it, due to the way it was formed. As it cooled, it shrunk and formed into square blocks and polygonal blocks, and that's very typical of that type of rock unit deposit. You see the same thing on Pahute Mesa. You would see the same thing beneath Yucca Flat, if you could see through the alluvium. It is what happened to any of the units that are welded, or have some form of welding. They start as a very hot layered mass, which sticks together and compacts, and then as it cools it cracks.

Carothers: There have been a number of tunnels that have been mined into Rainier. Are they all being put into the same block of material?

Orkild: Essentially. They're all in the Tunnel Beds. There are very different units in the Tunnel Beds, but essentially they are all the same rock types. Except P tunnel, which is much higher in the stratigraphic section. It's up in what we call the Paintbrush, which

is above the Tunnel Beds. But I don't think those tuffs are very different. The physical properties are very, very similar. The porosity might be higher in some of them, especially as you get into the upper units that are not as welded, not as altered. This is also true of any event down in Yucca Flat, or in Pahute.

Carothers: What are Paleozoic rocks?

Orkild: They are the older rocks that form the basement of the Yucca Flat, and the whole Test Site, essentially. They are the limestones, or the dolomites, or the shales. They were there before the volcanos — many, many millions of years before the volcanos.

Carothers: There might have been a time when I could have walked around on them. What would they have looked like?

Orkild: Just like the Rocky Mountains. And then there were the eruptions which filled the various valleys and troughs.

Carothers: In Yucca Flat, are the tuffs below the alluvium the same rocks that are in Rainier Mesa?

Orkild: Essentially. The tuffs have exactly the same stratigraphic sequence on Rainier Mesa as you have on Yucca Flat. Once upon a time they were connected. The Grouse Canyon was a layer that was deposited probably all over Yucca Flat and Rainier Mesa. Now you find it on the top of Oak Spring Butte, and also many thousands of feet below in Yucca Flat.

Carothers: So, most of the things that you say about the tuff units on Rainier Mesa should also be true of the tuff units in Yucca.

Orkild: That's correct. The alteration is very much the same. The physical properties are very, very similar. And very likely there are blocks just like those in Rainier Mesa.

Jenkins: Right under the alluvium in Yucca Flat is the Rainier Mesa member, which is the same ashflow tuff that you find on Rainier Mesa and on Pahute.

Carroll: With one exception, which is the alteration phenomenon. That stuff has been there for twelve, fourteen million years. Having been there that length of time, there's another imprint that goes upon the rock. That's the effect of moisture, and of heat. There is water coming down and creating accessory minerals — the clays, and the zeolites. And although one argues in certain places

that this is Tunnel Bed A, and that is Tunnel Bed A, in Area 3 it may not be altered as opposed to the bed in Area 9. Stratigraphy to me has always been a problem, because I don't like people to tell me the name of a rock. Like "metasediment," which is a popular term, I think, in the Soviet Union now. That name means nothing to me as a geophysicist. What is it? Tell me the density, tell me the porosity, tell me something more than a name you've made up.

Rambo: In terms of material properties, those rocks beneath the alluvium in Yucca may have had a whole different experience than the ones in Rainier. It's the same ashfall, but from the materials properties view I think there are some differences. If you do shear strength measurements on the tuffs in the tunnels, they will look quite a bit different from the measurements we do out in the Flat. And take the Grouse Canyon layer. Out in the Flat that means something highly porous, and usually to us means something very weak. In the tunnels it is a very strong, highly welded member, and I wouldn't say it had anywhere near the same gas-filled porosity as in the Flats. But it's the same low density unit.

Miller: I never did consider the tuff directly beneath the alluvium in Yucca to be the same as Rainier Mesa tuff. It drills differently. The Rainier Mesa tuff you run into in Area 12, and Areas 19 and 20, is one hard rock. Whatever is underneath the alluvium in Yucca Flat is not a hard rock. It's not that much more difficult to drill than the alluvium. The fact is, often times the penetration rate was not that much different than the alluvium. In Yucca, where you usually hit the hard drilling is when you hit the Paleozoic; when you hit the limestone or dolomite. The tuff underneath the alluvium in Yucca Flat is a different rock than you find in Area 20.

Carothers: In Yucca, how thick is this layer of Rainier Mesa tuff?

Jenkins: It's quite variable. On the east side of the valley it's quite thin — maybe a hundred feet. In the thickest part of the Flat, where the unit is thickest, probably close to five hundred feet. And on Pahute Mesa it's very much a thousand feet thick all over. The Rainier is the surface unit there.

Carothers: If I were to go to some place in Yucca Flat, and drill down through the alluvium, I might be able to drill several hundred feet into this Rainier Mesa member, and have the working point in it?

Jenkins: Right. It's been done often.

Carothers: Well, it would seem that with the number of joints, or cracks that is in that rock, there would be a lot of pathways for gas to get to the top of that Rainier Mesa layer.

Jenkins: It's true they could be quite convenient pathways.

Carothers: Yucca Flat and Pahute Mesa are two very different regions, aren't they, in terms of structure? Pahute is composed largely of lavas, and Yucca is mostly tuffs of one kind and another, comvered by the alluvium.

Jenkins: Yes, I agree with you, but the same generic units, from the same volcanic centers, are in both places. And you have almost the same rock on Rainier as caps a lot of Pahute Mesa. Now, the stratigraphic section on Rainier is rather compressed as compared to that on Pahute Mesa. In other words, there are units on Rainier that are very lithologically similar to those we find in Pahute Mesa, but they're compressed. They aren't quite as thick. There was not as deep a hole to fill, if you will.

Carothers: Pahute was added to the original Test Site in 1964. Why was that area chosen, aside from the fact that it was directly adjacent to the existing site?

Jenkins: I think the biggest factor that led to the identification of Pahute Mesa as a testing area was the gravity data work. That work identified low density material, at depth, in this big circular situation. And of course, the good thinkers could look to the south and see Timber Mountain, which is an exposed caldera. And the good thinker said, "Well, this must be another caldera. Therefore it has a variety of volcanic rocks at depth, instead of the Paleozoic rocks which we find underneath Yucca Flat".

There were a number of exploratory drill holes that the USGS did. Pahute Mesa 1 and 2, Ue20f, Ue20j - the water well, Ue19c, — and Ue19b. All of them were quite deep. Ue20f was fifteen thousand or so feet, and a lot of them went greater than five thousand. And so, we got a pretty good picture of what we were dealing with there.

Carothers:I think of Pahute as being a different type of containment structure from Rainier Mesa, or Yucca Flat, because it has layers which are very hard rock.

Jenkins: The lavas. Well, the lavas are probably the only difference, because you have densely welded ashflow tuffs in all areas. The Grouse Canyon in the Flat is probably airfall or non-welded ashflow. Of course, it's thin. Under parts of Rainier Mesa, and under Pahute Mesa, it's densely welded.

Orkild: Pahute Mesa is where the lava was molten, and flowed out from an active volcano, which was very, very close - - within miles. Normally the hot lavas don't go more than ten miles away from their source. Being a viscous, gooey mass, they stay very close. They went in one direction, then that would clog up, and then they went in another direction. That's what really generated some of the blobbier structures.

Carothers: What's between the various lava flows on Pahute?

Orkild: Generally material that was ejected out of the volcano as ash or brechiated rock, rock that is broken up. Ash is hot volcanic material that is blown into the air and then cooled. The molten rock comes out, and when it gets to the atmosphere it vesiculates into a nice big frothy ball that becomes disaggregated, and falls back to the surface.

The deposits that are what we call airfall, dropping onto the ground, will form a very thin rind. Normally what you will see if you have any extreme topography between various flows is that the rains have washed this airfall material into gullies and other low spots. Many times you can see peculiar dips on the Mesa, and they are on those slopes where the material has been deposited, and then washed off.

Carothers: There would be considerable differences in the properties of the material in the lava flows, and in those places filled with the ash, wouldn't there? Density differences, for example.

Orkild: Sure. The lavas could be very dense, and the ashflows could be very low density. There are a number of density contrasts, especially going out of the very dense lavas into the very vitric tuffs, back into dense rock, back into soft rock, and then back into hard rock.

Carothers: In Yucca, which in a sense is a somewhat less complex structure, people took a lot of logging data and sample data, but in the first years they didn't do all of that on Pahute. The geologists would say, "Well, the properties are extrapolated in." How can you go to a structure like Pahute, which has pillows of lava, and many kinds of layers, and do that?

Jenkins: Because on Pahute Mesa the units have better identification, and therefore a unit there, a subunit, whatever, can be projected much better under Pahute Mesa than it can be on the Flats where you really don't have that good a handle on what the units are.

Carothers: Is that another way of saying that the units on Pahute Mesa are so different, one from the other, that you can see them readily?

Jenkins: Yes. The units are so different, one from another, that they can be easily distinguished one from another. That's a good statement. In the Flats you have the fallout of all of this volcanic activity, and it's just very hard to distinguish among them. Now of course, that depends on what kind of a scale you want your physical properties on. If it's a very wide scale, then the whole unit between the Timber Mountain tuff and the Paleozoic rocks could be generalized. On Pahute you'd have to have parts of it here, and parts of it there, and other parts of it over there in order to make that statement.

Orkild: I think that, as far as we're talking about the physical parameters, there's no longer a difference in the data gathering. It's true that most of the physical properties were extrapolated from the key fifteen exploratory holes that were drilled up there. You saw the same type of unit in the next hole, and said, "All right, this is very similar. Therefore we'll extrapolate." And there was really nothing wrong with that. It was very successful for thirty-four events.

Carothers: That depends on with whom you want to argue. There are some who might say, "Yes, but all of those events were high enough in yield to be the kind of shots that don't leak. You're just lucky that you only shoot high yield shots in that very complex, little known, variable density medium. You try to shoot low yield shots, as you do in Yucca, who knows what would happen? At nine hundred feet or so for a ten kiloton shot, it might not be the same.

Orkild: I would be very nervous with something like that, because of the Rainier Mesa tuff unit that's at the surface there. That Rainier Mesa material has cooling cracks all through it. It cooled as one unit. It came out in multiple flows, but it all stacked up, very thick, and compressed. As it cooled, the cracks formed in the vitrophyre in maybe a different pattern than they formed in the upper part of it, but those cracks are essentially through-going. That layer is about a thousand feet thick, and that means those fractures go down that far. Once you drop below that layer, unless you're near a fault you have very few fractures of that kind.

Carothers: Then if I shoot at sixteen hundred feet or so, I'm only a few hundred feet below that layer. I don't have to go very far to get to those fractures. And then the gases can move rather freely through the fractures, even if any single one doesn't extend all the way through the unit.

Orkild: That's right, if you only have the Rainier Mesa member as the rock at the top. That's in Area 19. When you go over into Area 20, you have other units above the Rainier. The events where you see late-time gas seepage are mostly over in Area 19, and they are directly related to whether the Rainier Mesa layer was near the surface. West of there, other units, the Thirsty Canyon and the other ashflows, sit on top of it, and those cracks are essentially sealed off at the upper part. Late-time gases certainly came up into them somewhere, but they didn't get to the surface because they could disperse into those thin, very porous layers that were intertongued with denser units.

One of the features of the Test Site that has been considered as possibly adversly affecting the probability of successful containment are the faults that occur throughout the site. Generally the faults with substantial displacements are avoided - - when they are known. Some faults, such as the one known as the Carpetbag fault, are not detected until some movement occurs as a result of a nearby detonation. How dangerous faults are with respect to containment is largely unknown, and is a matter of individual judgment.

Carothers: At the Test Site there are many faults, cracks that show that movement has taken place.

Orkild: Yes, many of them, some with up to forty meters of displacement in Rainier Mesa, where we can see them in the tunnels. That's a large fault for Rainier Mesa, and it would probably be the largest that you'll see there. And you do see displacements down to inches in the tunnels. In Yucca there are faults with much more displacement than that, but on those the displacement has to be inferred from seismic surveys and exploratory holes. Normally when you see a fault it's not one fault plane, but a series of fault planes that might make up a total displacement of maybe ten feet, distributed on five or six of these faults.

These faults are held very tightly together by the overburden. There are no open standing fractures. They are a plane of weakness, but I don't think they're a plane of transport. I don't think faults necessarily have to be bad for containment, but I think the system should be aware of them, and plan accordingly.

It would be nice to be able to try a shot on the Yucca fault. Many years ago, on Pahute Mesa, there was an event very close to a structure over in Area 20. That was back in the Rae Blossom days. He said, "Let's try it. Let's see what happens." Nothing happened. The fault moved very nicely, very handily, something like five or six feet. But there was no reason to think it affected the containment. There was no release.

Carothers: The closest shot to the Yucca fault that I can remember was in '72. It was called Oscuro, which was a Los Alamos shot fired on the east side of the Yucca fault, fairly close.

Orkild: And close to a very large northeast fault. My personal feeling is that it would have vented if it hadn't collapsed when it did. I remember going out and looking at the post-shot effects, and I said, "I don't know how this thing stayed in the ground." Everything was standing open. The fracture that broke to the northeast was standing open a good foot and a half or two feet at the surface, beyond where the collapse had occurred. That northeast fault is in tension, and that's the thing that would stand open, because there's nothing to close it up. I think that was a very, very close experience. The only thing I think that saved it was the collapse. If that had sat there for any length of time, I think we'd have been hit.

Carothers: How do you know whether a fault is under compression or is in tension?

Orkild: In certain cases I think we know based on the overall structure. When we look at some of the northeast trending faults off the Yucca Fault, we can say that very likely they are in tension — they're pulling apart. That's how the basin-range formed in the first place. There are areas within the Test Site that are under those conditions. That's based on the physical parameters that they have found on drilling on the Mesa, finding out the principal stress.

Carothers: You mentioned the east side of the Yucca fault as an area that is in tension. Does that mean the west side is in compression?

Orkild: No. They both could be under compression, because one side is coming up, and the other is going down, and they're both pushing together. It's the subsidiary faults that come off of the Yucca fault that are being pulled apart.

The upside of the Yucca Fault has always been steered away from because you see all of these open, standing fractures, which means that something has to be under tension and it's pulling apart. You would assume that some of the downside would be under compression because it's being pushed down. But there's another wrinkle to the Yucca Fault — it also has lateral motion. One side is moving laterally with respect to the other, and as you have that motion you can have tension along those northeast fractures.

Carothers: Would this same situation obtain up on Rainier Mesa, where you might have tension and compression areas? It's not a very big block.

Orkild: It's not a very big block, and probably what it's doing is that all of it is moving radially toward it's edge. That's the way you would think it would happen — that it would move toward the open-faced surface, toward Yucca or toward Pahute Mesa.

Carothers: Bill, you have said many times over the years that you don't see that faults really cause a containment problem, and they don't concern you particularly.

Twenhofel: I don't think faults are a problem unless they move on the shot.

Carothers: How am I going to know whether that will happen or not?

Twenhofel: Well, we don't really know that too well, but we know which ones have moved on past shots. The Yucca fault, for instance, moves quite a bit, and there are others. We try to avoid those because we have the concept, and I think it's valid, that a fault isn't a perfect plane, so when they do move there may be openings created, and that's a possible release path if you're near enough.

So I concur with the idea of avoiding the Yucca fault, because it moves. But many of the faults are not going to move. There are two kinds of faults. There are tectonic faults that are created by earth stresses, and they tend to be big things. Then there are a lot of subsidence and compaction faulting that only occurs because the rock is compressed a little bit here by the weight of the overlying rock, and so it subsides a little bit — there's a little fault. Those things don't move, and I don't think they're a factor.

I think that if a fault goes right through the stress cage it's going to be compacted and tightened right there, so it can't possibly be a path. When it's farther out where it can move, I think it can be a factor, but I don't get alarmed by many of these faults. But we can't be very quantitative about it. It's very subjective.

Weart: Well, Pin Stripe was an early vertical line-of-sight pipe shot that vented through a fault. It was conducted in Area 5, and rather than being in a drill hole, it was in a shaft that had been excavated. It had the latest in closures; ball valves, HE closures, everything. But, as we found out when we did an investigation after it vented in a very massive way, all those features had been circumvented. There was a fault that came into the shaft below these features, and it provided an easy release path to the surface.

Carothers: The release was through the fault itself?

Weart: Yes, we know it was. It was a very clear example. When we reentered the top of the line-of-sight pipe the seals were closed, and it was clean. And we could trace the fault path on the surface of the ground.

It wasn't a fault that released material directly from the cavity, and I'm not sure that the Baneberry fault did either, although on Baneberry the fault was much more closely associated with the cavity than the fault on Pin Stripe. Whether or not Pin Stripe would

have contained if the fault hadn't been there I can't say for sure, but it certainly was the easiest path. The shaft was clean above that point.

Carothers: There are people who say, "The geology at the Test Site really doesn't matter as far as containment goes, except in exceptional circumstances. We have fired so many shots that we've probably encountered just about every kind of material and situation that you can imagine. If it mattered it would have got us by now." I emphasize they mean that statement only with respect to the Test Site, not the world in general.

Orkild: I agree that if you bury shots deep enough you don't have any problems.

Carothers: Well, that's true, I think. If you have all the money you want, and all the time you want, you can certainly do that. But coaxial cable is expensive. Casing is expensive. Drill holes are expensive. And when you get down below the water table it begins to get very expensive. So, that's the kind of statement that is true, but it's not very helpful in the real world. But certainly we have encountered many different kinds of geologic situations, wouldn't you say, on those many shots that haven't leaked?

Orkild: Yes, but there are certain combinations of geologic conditions that can get you. Baneberry is an example.

Rimer: Take Barnwell. There was a case where the containment lore about geology doesn't matter came close to being disproved. John Rambo was the containment scientist. It scared him enough that he had people go there and take cores, and measure strength. I'm sure he got a lot of flak. He did a number of calculations, and we did hydrofracture calculations, and everything said that the thing was going to be contained. We came to the CEP with that information, and I forget who, but somebody said, "We've never had a problem with a shot of this size at that depth of burial. Therefore, we don't need to listen to these calculations."

The bottom line was, the thing was going to be contained, but it was going to be contained with the potential for a hydrofracture going much higher in the section that we had ever hypothesized before from a tamped event. I emphasize tamped event, rather than a cavity event.

Based on drilling rates John knew there was something funny, and what was funny was, you had this strong material right above the shot, which kept the cavity size small, and therefore kept the cavity pressure up. Above that material was a layer of weaker material which wouldn't support a high residual stress. So you had high cavity pressure, and low strength rock above. That's the worse possible case you could have, other than an open fracture leading to the surface. The empiricist said, "Why should this matter?" Well, when it was shot, radiation got very quickly up to the last plug.

Carothers: Higher than we've ever seen it on a shot of that yield.

Rimer: And you know what? It wasn't a coincidence. Geology mattered.

Brownlee: I think what I've learned is that the geology is only important when I'm on the edge. Then it becomes important. But if I've got normal margins of safety, the geology can be almost anything. I know that's true, because we've shot in almost any geology, safely. If you've done your containment right, you don't have to hang on the geology to determine what happens. But if you've done things wrong, a trivial thing in the geology can make all the difference.

Now, don't misunderstand me — I just believe that we ought to be so conservative that geology never matters, and most of the time that's true. We are so conservative that the geology doesn't matter. And so, I get very bored when they go into details that are of no importance to this shot; none whatsoever. But they go into it because after all, they've done this work, and they've got this geologic business to talk about. Well, they don't understand why it isn't important, and there's no way I can teach it to them. They have to learn it themselves.

Orkild: Many times, I agree that you could ignore all the geology; many, many times the geology is benign. That is, you have flat beds, you have low water content, you have good porous rock. There is no nearby structure that would affect the containment. And, a rock type where we have a good handle on the physical properties, and they are well within the range that we are familiar with. And the water content, the same thing; it's within a range that we know for this particular lithologic unit that's being tested in. And that there's no large body of clay at the working point.

Carothers: We've never seen a large body of clay, except for the Baneberry site, have we?

Orkild: There was the site for the Stutz event that had a pretty large volume of clay. I think there has to be a particular set of circumstances to get a big pod of clay. On Baneberry there was opportunity for a large amount of water moving through the formation, and the right lithology that could alter, and a thick zone. You've got to have water, and you've got to have a vitric tuff that will readily turn to clay, and the right chemical conditions to start the process.

We've seen that situation twice. I don't think that any other time we have drilled into a situation like that. Normally the clay we see is at the interface between two units, which is stratigraphic, and very, very thin. Theoretically, in a long strike it could get thicker, but not more than ten feet or so.

Carothers: On Baneberry that clay was what, a few hundred feet thick?

Orkild: Yes. It would be very nice if we could excavate that and see what it really looked like. We've looked on outcrops and looked for situations like that. Where would you go to look, to see if you could find a situation like that? There should be an analogue at the surface somewhere. The closest we could find to something like that was in what they call a chinle formation on the Colorado plateau, That was essentially volcanic ash that was deposited in a shallow ocean or pond, and altered. It's hundreds of feet thick. But we never have we seen anything like that on the Test Site.

Carothers: With data from the hundreds of drill holes that are on the Test Site you must be able to plot everything everywhere, from hole to hole.

Orkild: Yes, we have 2-D and 3-D programs where we can do that. We call up the data base and plot the holes. That's how we do the siting. The program looks at a site and plots the geology, the water table, the lithologic units, and plots in the known faults, and their distance.

The Laboratory people come in with a set of coordinates, and the parameters for the hole. We plug that into the program and crank out what they call a prediction report. And then we do the

same thing with the gravity. We show what the configuration of the Paleozoic surface will be, and send it on to the Labs. That eventually gets incorporated into the prospectus and the presentation.

Sometimes we reach a point where we do not agree. Or we might point out certain problems, like a lava mass being close to the site. We went through one of those in Area 20, where the two exploratory holes drilled did confirm there was a blob out there. It turned out that it wasn't really any problem, because it turned out to be far enough away.

Of course, as the hole is drilled you develop more data. It's an ongoing process. Now, I think, they've gone overboard, and collect data that nobody seems to understand. Especially water content. We here still think something's fishy between the results of the two water content logs - - the epithermal neutron logs that Livermore is showing these days, and the one they used to show.

Carothers: Why?

Orkild: It has to do with the bound water. It's hard to tell which standard to use, because the Lab seems to pick the one that fits best. Which is okay, I guess, but that really doesn't solve the problem of understanding why you have this bound water and additional water. The water contents on Pahute are up what — ten percent more? — than we ever used in the projections. On some of the recent shots where they're using the new logs, the water contents are up in the twenty's — twenty-five percent, twenty-four percent. Which might be real, but we really don't know. This is one of the outgrowths of all the data gathering that's been going on.

Carothers: These new numbers are coming from the neutron log aren't they?

Orkild: Yes, and I think it's a positive step. But I think it's going to take a lot more work before they really understand it, and everybody agrees with it. Of course, getting everybody to agree will probably never happen, but you could certainly get to where a majority agreed.

Hydrology

Fenske: Hazelton Nuclear Science Corporation got a contract to do hydrologic studies on the Test Site in 1962. Hazelton had two labs for work with radioisotopes; a pretty high level lab, and a low level one. They they were doing all kinds of things for the Atomic Energy Commission, and the National Institute of Health, and people like that who were interested in radioactivity. They were hiring people, and in about 1965 I went to work for them. The program was to find out whether underground testing was going to contaminate ground water in such a way that it would cause a serious problem. I don't recall what a "serious problem" would have been at that time, but I think it came out that it would be if any radioactivity would leave the Test Site.

Where was the water going to go? Nobody really knew very much about the transport of radioactivity in ground water, and not too much was known about hydrology, in that sense. At that time hydrology was centered around how much water could be produced from a well. So, hydrology was drilling a hole into an aquifer, and producing water so you could water the livestock, or irrigate the field, or something like that. That was the hydrology the USGS was steeped in at that time. It was always drilling holes and finding out how much water could be produced.

To do that, in the final analysis what you really do is pump on the well. In a nice isotropic, homogeneous medium the production is an exponentially decreasing curve. You plot it on semi-log paper, and it's a straight line. When the line comes out so many years in the future going to zero production, you know that's it - - that's the end of that well, probably. Of course that assumes things are isotropic and homogeneous and all those nice things. Which they never are, but that's about as good as you can do. The longer you run the pumping test the more confidence you have in the results, but, as on some of the wells at the Test Site that the USGS tested, you can find that the slope of the curve changes. It goes along nicely, and all of a sudden it starts diving. It has what they call a boundary effect.

So, hydrology was a developing field, from the point of view of transport of water for a long distance. It started out with the idea that there is an aquifer, there's a gradient in the aquifer, and therefore the water is moving down the gradient at a certain rate.

Essentially you drill a couple of holes, and if the water comes up to a certain level in one hole, and comes up to a lower level in another, you figure there's a hydraulic gradient in that direction. Early on, people tried to figure what the hydraulic gradient was, and what the direction was, and what the permeability was so they could say, "This contamination is going to go in that direction, and will travel this far in so many years."

As things progressed, people realized that the hydrology in an area wasn't that simple. The Test Site is not at all simple; it has a very complex hydrology, and we still don't know much about it, really. And, radionuclides are adsorbed on rocks. Then we found out that the process isn't really symmetrical; they weren't desorbed at the same rate they were adsorbed, and things like that. All kinds of problems like that occurred, but it didn't change the basic conclusion. That was, except for the tritium, the radionuclides just weren't moving very much. At least that was so for the rocks we were dealing with.

Now, the carbonate aquifers are different, because things like strontium and calcium are ionically similar, and strontium will move in the carbonate aquifers. In the alluvium you just didn't find any real movement of that material. In alluvium we never have found movement, except for the tritium, which moves as fast as, or maybe faster than the average velocity of the ground water.

Carothers: How can something in the water move faster than the average velocity of the water?

Fenske: Well, you try to calculate the velocity of the water on the basis of the pore structure and the permeability, which are the two things you have to have to get the velocity of the water. That's an average value for the movement of the water in the aquifer. Now, that aquifer extends over a broad region, and there may be localized regions where the pore structure is different from the the one you used to calculate the average velocity. An old, buried stream bed, for example. If you happen to drill into that when you are measuring the movement of the tritium, you might find a faster velocity than you have calculated as an average. So, it's not that easy. The velocity of diffusion depends on pore size, and the pores can be of different sizes in different places, so it gets to be complex.

Carothers: One of the things I have heard about the Test Site, which is said to be unusual, is that the water table is very deep, that there are very few places where you will have to go down 1600 feet before you get to the water. Is that true?

Fenske: Well, if you're talking about Yucca Valley, yes. If you're talking about Pahute Mesa, no. On Pahute the water is pretty far down, but very often in regions at higher elevations you will find deeper water. In lower regions, like Yucca Valley, you find shallower water. In fact, a lot of valleys in Nevada have swampy areas in them. Ruby Marshes is a good example. Or they have springs; Hot Creek Valley has springs. That would be the normal situation. But in Yucca Valley it's different.

I had one of the fellows at DRI, a number of years ago, draw a map of the elevation of the water in all of the valley bottoms in Nevada. The reason for this was because I felt if I had the elevation of the water in all the valley bottoms I would have an idea of what the regional flow structure looked like in Nevada. Well, you find everything is very regular, and it all moves down towards Death Valley — until it hits the Test Site, where Yucca Valley is. Then, there are very steep gradients going into Yucca Valley. There's something else going on; Yucca Valley is underlain in many areas by the carbonate rocks, which are fractured, and transmissive of water.

Another thing you have in Yucca is that the alluvium has a higher water saturation, higher in the section, than you would expect in a dry valley. All the way to the surface you have some water saturations that are higher than you normally would expect. In some places you find 20, 30, 40 percent water saturations; much higher than you would expect to see if the water was always down at the level where it is now. The impression you get is that at one time the water was up near the surface, but now it isn't any more.

What I think has happened is that the carbonates which underlie the valley have acted as a huge drain. So, the water is basically moving down to the carbonate, and out to the springs in Amargossa. The water in all the rest of the Nevada area is moving in sort of a normal fashion; in Yucca it's being drained, and has been drained, so it's not up near the surface anymore. It's down closer to the level of the water in the Amargossa.

If you look at the vertical gradients in the carbonate, they are much less than the vertical gradients in the alluvium, so the normal direction of movement would be downward through the alluvium into the carbonates in the Yucca valley. Then the water moves south through Frenchman out to the springs in the Amargossa Desert.

Some of the water beneath Pahute may be draining into Yucca, but whether it is or not is the subject of some argument. The Survey, which has done most of the work on that, think that the Eleana formation, which comes along the side of Yucca Valley, on the east side of Pahute, is a pretty effective barrier to movement into Yucca Flat. Everything that comes off the west side of Pahute Mesa goes down underneath Forty Mile Canyon toward Lathrop Wells. So, not much of any water gets into Yucca from Pahute. Some probably does, but I think it would be small.

I don't think there's any recharge at all in Yucca. We ran an investigation there once where we looked at tritium in the dirt. It's pretty dry dirt, and it's pretty hard to dig it. I guess about the farthest they could reach was about as far as you could reach with your hand to grab a handful of dirt. We found that the amount of tritium pretty well decreased with depth, so there doesn't appear to have been any recent recharge there. By the time you got down about a meter you just didn't find any tritium any more, in the dirt. There was a lot of tritium in the rain in the sixties from the atmospheric tests; there was a peak during those years. You don't find that at depth when you look at it in Yucca valley, or on the slopes around the valley. So, I don't think there's much recharge going on there. I don't think it's going from the surface of the valley down 2000 feet.

Now, it may reach an equilibrium point where the gradient around the sides of the valley is increased enough to replenish the water about as fast as it's running out. There are steep gradients going down into the valley, and the lower you make the water, the steeper those gradients get, and the more water you bring into the valley from the sides. It may have reached an equilibrium point, but I don't know if it has or not. You'd have to watch for a long time, a hundred years, or two hundred years, to be able to tell.

Carothers: A few years ago, in the LANL area, radioactivity was found when they were drilling an emplacement hole. There was the thought that perhaps this activity had been transported from the

expended Sandreef or Aleman sites, to the north. If that was so, it had traveled laterally a lot further than people had expected. What do you think accounted for the transport that was observed? It did happen.

Fenske: Yes, it did. There are a lot of things we can't explain that we see once in a while. The only thing I can think is that there was pressure in the cavity, and material was driven down a fracture that momentarily opened up. As I recall, there were some radionuclides out there that had gaseous precursors, and they wouldn't have gotten that far if they hadn't been shoved over by a pulse of gas, or something like that.

Generally, the water above the carbonates, in the allluvium and the tuffs, drains down. It is when it gets into the common aquifer that it drains to the south. I would think that the amount of lateral transport, above the carbonates, is pretty small. That doesn't mean there can't be some, but I think it's pretty small.

Carothers: On another subject, what is perched water?

Fenske: Well, it's something that you may well have in Yucca valley, if once you had a higher water table and now you have a lower water table. Say that a thousand years ago you had a water table close to the surface. If that water table drops, there will be water left in various places — on top of layers of less permeable material, for instance. That water is just sitting up there — perched up there. It is not like a lake; it's just a more saturated region of the rocks.

In the conventional sense of perched water, you'd be in a more humid region than the NTS, and you'd have a water table at some level. The water that's deposited on the surface infiltrates down. But, there may be some fairly shallow little clay lenses. So, some of that water sits on top of these clay lenses. Then it runs off the edges, and down to the water table, but there's a time delay. In that situation, given a certain amount of rainfall on the average, there's always a lens of water that's perched up there on top of this clay lens. It's in equilibration between the amount of water that's running off the edges of the lens, and the amount of rainfall that deposited.

Carothers: Perched water was one of the things that was discussed in the first report about what happened on Baneberry. The impression was given that the explosion cracked a rock layer, and a lot of water ran into the cavity. In the situation you describe, that couldn't happen.

Fenske: No, it couldn't. At least not rapidly, not in minutes. It might do it in months, or years, or decades, or something like that, depending on the permeabilities of the materials.

Carothers: What about "water mounds," that are thought to be produced by nuclear detonations?

Fenske: You will get Fenske's version, which is that I don't really believe in water mounds. I do believe in potential mounds. What I mean by that is, in material which has a low permeability, such as the alluvium, the water maybe moves ten feet a year when it's moving laterally. I don't think that, instantaneously, you can move huge volumes of water up meters in height over big areas. I think that what happens is that the relationship between grain pressure and pore pressure is changed.

Say you drill a hole down into the formation, and measure the water level. After the shot you have a higher pore pressure there than you had before; so the water comes up to equalize that pore pressure. And so, every place you drill a hole you have water coming up to higher levels than it did before the shot. You can say you have a water mound there, but it's not the water that's a mound — it's a potential mound. What you are really measuring is the potential of water in that formation at that point. Now, the water's flowing, because of the gradients and the higher potentials, from one place to another. But it's not a real mound of water. The holes you are drilling are really acting as piezometers; they're measuring the water pressure at that point, which is higher than you would consider at that depth, or higher than it was before. There really isn't any more water there than there was before. It's just that the pressure, in the water, is higher than it was before the shot, so the water just comes up higher in the piezometer tube that you have put in there. And that's because, in shaking the ground, you have transferred the grain pressure to pore pressure.

The material is originally in an equilibrium situation, where all the sand grains are impinging on one another, and they hold the pores open to a certain degree. If you took all the water out, there

would still be some pores in there. Now, if you shift the sand grains a little bit, they can go together and reduce the porosity. Then, because you have reduced the porosity, there isn't the space for the water that there was before, but there's still the pressure of the overlying rocks. By reducing the porosity you have increased the pore pressure, or the water pressure in the rocks.

The same thing happens, incidently, in a landslide. You have a water saturated material. A little shift of the grains, for one reason or another, will start increasing the pore pressure, and decreasing the grain pressure. When you do that you decrease the resistance to flow; the material becomes liquefied, and downhill it goes. Spontaneous liquefaction is basically that kind of a mechanism. You've been to the beach and patted the sands?

Carothers: Sure. And water comes up to the top.

Fenske: Yes. You're compacting the sand, and the water comes up. When you compact it, that material becomes liquified; it becomes mushy.

If you start calculating the amount of water that would have to be in a water mound, given a reasonable porosity, and how much you had to move in a fairly short period of time, that's a lot of water. And then you have to ask, "Where did it come from?"

We have run into situations where perched water was a consideration, and it has caused all kinds of problems. They were not necessarily containment problems; they were problems with emplacing the device, just getting the thing down in the hole. For example, there have been cases where the Labs have put down a liner, and the water has come over the top of the liner. That has to do with hydrology, and you ought to understand more about it. Sometimes that's a perched water problem.

Carothers: No it's not. It's a stupidity problem. They say, "And the water level was tagged at 636 meters. So, we've put in a liner whose top is at 636.5 meters." They seem to think that all they need is a liner that is an inch, or a foot higher than their tag, and everything will be swell. And then they say, "Oh my gosh, the water is running over the top. How distressing." What I think is, "How expensive to fix. Fire them."

Fenske: Well, there are other cases where water has run over the top for other reasons. There is the possibility that there really is perched water. When you go through a zone where there is water, and it's not all necessarily held in by capillarity, it can run out. And there are cases where there is excess pressure in the water, as in the ground water mound. There was one shot where the water pressure was high enough to collapse the casing, due to ground water mounding, or the pressure in the water, as I think of it.

Carothers: So, suppose I am drilling down through the alluvium, and the tuffs, and it's dry. I am careful to stay above the standing water level, but I notice water running into the hole. As I understand it, that could happen because there's perched water, or as you would say it, there's a high pressure zone. If I case that hole, the casing has to be able to withstand a pressure that is at least equal to whatever the pore pressure of that water is. If it can't do that, it could collapse, even though that casing is above the standing water level.

Fenske: Yes. There's another way you could have perched water, and at the Test Site it would be water running into the valley. If you happen to have a tongue of something like clay, that's lower permeability, which extends into the Valley, water may run along the top of it rather than run down to the water table. It may run along the top, and then drip over the edge. Up to that point you may have perched water. Around the valley sides, more than around the center, you could possibly have perched water. You have a source that keeps on running, because it's recharge water from, let's say, Pahute Mesa that's coming through the system. It's just taking a little different path. A good example of that is some of the springs that you see. They're basically perched water. Water enters the system, flows down some impermeable layer, and comes out in the form of a spring where the layer intersects the surface.

Carothers: Do you think we've made any difference to the flow of the water, or the drainage of the water? Have we upset the hydrology of the valley?

Fenske: I don't think so, except locally. around the cavity, and for some small distance out from that. I looked at one thing though, at one time, which was the number of uncased bore holes that went

into the Paleozoics. There turned out to be a fairly large number. At some level those holes enhance the flow of water from the alluvium down into the carbonates. Of course, you have to start with how much water is leaking through there, without the holes, to see what that enhancement might be, and we don't know that.

6

Earth Materials and Their Properties

The geologic materials in which a device is detonated determine the details of the phenomenology that occurs. Hence, the properties of these materials are required as input for any of the codes used to calculate the expected cavity size, hydrofractures that may occur, the various stresses that occur, the ground motions, and so on. Unfortunately, many difficulties beset the determination of these properties.

In emplacement holes there is no access to the materials, or to information about the geologic materials around the shot point, other than that provided by logging tools, or various tools which can retrieve small samples. In the tunnels samples can be taken fairly readily, and laboratory measurements can be made on them to determine various quantities such as density, porosity, and water content. However, such laboratory measurements cover only a small range of the conditions the material is subjected to near the detonation, and tell nothing of how the material will respond to a shock pressure of 500 kilobars, for example, or to simultaneous radial and tangential stresses.

Laboratory measurements are made on small, competent samples of rock, while the energy of the detonation interacts with the entire mass of the surrounding earth, which can include fractures, faults, and layers of different materials of different composition and properties. The behavior of the overall surrounding materials may be quite different from what would be expected from the the properites of a small laboratory sample.

Rocks that have been subjected to the high shock pressures generated by the energy released in the detonation can be damaged, to various degrees, depending on the shock pressure, and their properties are not the same after the shock wave has passed as they were before. Therefore, they do not respond as they did before, but things important to containment are still occurring.

There is not agreement among those working in the field of containment as to what the properties important to containment are.

Carothers: Los Alamos had drilled some holes during the moratorium, and after the resumption of testing in 1961 that activity picked up considerably. What sort of logging did you do, or what kind of geologic information did you look for? Or did it just accumulate peripherally to the actual drilling?

Brownlee: Well, I hate to go on record as saying this, but when I was dealing with the engineers those kinds of questions had nothing to do with it at all. They picked the sites, and told us there was a hole there. We could ask whatever questions we chose about it, but they were not obligated to tell us anything. The truth is, they didn't know anything about the site.

Carothers: If you asked, "What are the rocks like at the bottom of the hole?" Or "What sort of layers of rock have you drilled through?"

Brownlee: No answer. Now, they very well may have had some information, but they certainly didn't feel obligated to share it with a non-engineer. The one fact they acknowledged was that it was possible to hit water. And so, I usually knew if we had gotten to the water table.

I knew the depth, of course. And if they were in tuff, they would tell me that as opposed to being in alluvium. But I didn't know much about tuff, because the alluvium in Area 3 is very deep. So, tuff was not a common kind of occurrence until we got toward the edge of the valley.

But logging was not a requirement. The only requirement was to drill a straight hole, and so they did a lot of worrying about not drilling crooked holes. All the emphasis was on the mechanics, on the engineering aspects of the drilling. That's the way I remember it.

I finally derived four standard materials, based on how close I was to the water table. I had very dry, dry, wet and very wet. The very dry was for the top of the alluvium in Area 3, and the very wet was at the water table. And, I had a couple of other standard curves. So, I found that the equations of state were some strange function of depth, but it wasn't depth from the surface, it was really distance from the water table.

So, the question I would ask was, "Where is the water table?" And Rae Blossom would fume and say, "You don't have to know that. There's no reason why we should spend a dollar to find out where the water table is. Just knock it off. What difference does it make to you anyway? We tagged the water table at such and such a depth over there, so that is what it is. How could it be any different here?" And so, my questions always ended in big arguments about non-relevant things like budgets, and time, and money, and "You're bothering the engineers. Get the hell out of here."

I'd go have these terrible arguments with Rae, and shout and wave my arms, and we were enemies forever, and then Rae would go out and get the answer to my question because he would recognize I really was serious. You see, if you didn't persevere, you weren't serious. But if you persevered, he'd go get the information you wanted. So, I have to admit that on a number of occasions Rae did make an attempt to get answers to some of my questions. He was in a position where he could order the rest of them to do it, but it was always like pulling teeth to get that done.

So, I'm afraid that in those very earliest times we knew next to nothing about the medium or its properties after the hole was drilled. The guys who drilled would record that the drilling rate changed, but you didn't know why. I only found out about some of these things when we were actually doing the tests. Livermore did a better job than we, because when they were doing the tunnels they were asking the right kinds of questions. I did get permission to go out and see what was there, and I was educated more by Livermore guys than by people here, in the very earliest times.

Carothers: Pre-Baneberry there was, apparently, no real requirement at Los Alamos to take logs and samples.

Scolman: I don't recall that there was. My feeling is that to the extent that there was logging done, or we studied the lithology of the hole, it had to do mostly with drilling. How do you best drill it? Anything that came out in terms of geologic information was almost incidental to that procedure. We did run some logs. We ran caliper logs, for example.

Carothers: Because you wanted to put the casing down the hole, and you wanted to know how much cement you would need.

Scolman: Exactly. Because you had to put the casing down, And, of course, we ran cement logs, so we could say, "Yes, it is cemented." We calculated how much cement we should have put in and then made sure that it was reasonable with regard to that.

Carothers: What attention did the Livermore people pay to the medium the event was in?

Olsen: That really didn't come until Baneberry. There were some of us in the containment group, in '69 and '70, who started to appreciate some things. In particular, what we looked at was CO2 content, because there was a shot that conventional wisdom said, at that time, was deep enough, and had enough yield that it shouldn't have leaked, but it did. That was Nash. Well, the obvious thing about Nash was that it was in high carbonate rock. As soon as you think about it, if you make a lot of CO2, it doesn't go away, so it just keeps pushing. So, one of the things we started thinking about early, when there came some sensitivity about seeping, was carbonate content. We didn't go much beyond that, although we started to, until Baneberry.

Carothers: Was there any logging or sampling program to look at the various media you might be shooting in?

Olsen: We did some. It was not much. I don't remember that we did downhole sampling. We took cutting samples, as they drilled. One of the problems, in that era, was that there were not big-hole logging tools, which are now the standard. If you wanted to do any geophysical logging you had to drill a small diameter hole, so there was a lot more exploratory drilling then than there is now. We did take cores, and we had, basically, oil field geophysical tools to measure density and things like that. So, we had some logs then, and there were a few people who were beginning to look at those things. The cratering people, who were still in business at the time, were interested in knowing densities and things like that as input to their computational models, but they weren't interested in containment, obviously, if they were interested in blowing a crater.

One way to determine something about the material properties in the emplacement holes is to obtain samples of the rocks at various depths, and to do various tests on them, in the laboratory. Such

samples may or may not be representative of the actual material, but they are a start. Obtaining such samples is not an easy, nor inexpensive matter, as the drillers see it.

Miller: One thing I'd like to tell you is about all the sampling deals that came up after Baneberry. I was thinking about that when you called and said you would like to talk with me. I thought,"I wonder if he ever heard some of my tirades about all the sampling we did, back then."

Carothers: All the sampling? Just those few little side-wall samples here and there, you mean?

Miller: Yeah, just a few. In fact I wrote a technical paper on it. It was years ago when they were designing the tool to go out and take a side wall core, not a side wall sample. I said, "You've got to find a way of getting this information with logs." And they finally did. I was happy about that, because they caused us so many problems with that sampling. Drilling the hole is difficult enough.

Preshot measurements of material properties are important in the prediction of the behavior of the surrounding medium when the shot occurs. Measurements on cores and samples taken after the drilling of a hole, and downhole logging data from emplacement holes provide some information. However, there is not uniform agreement as to the value, or necessity, of the various kinds of data that can, or should be obtained.

Carothers: Bill, you chaired a committee to look at various material properties to make recommendations about which ones were important, or how they should be measured.

Twenhofel: It goes back to Baneberry, when the properties of the medium had a contributory effect in the release. So the system said, "We've got to have some way to find out what's down there. And we want to have that in numbers, we want to quantify it."

There were certain measurements that could be made at that time. You could get samples, and you could measure the water, the grain density, the bulk density from either samples or from logging tools, and you could measure the carbon dioxide. Those measurements are relatively easy to make. They're cheap, they're routine, and they tell you something about the material down there. Then

you can calculate other properties, some of which may be related to containment, like gas-filled porosity. You can do that, and it's cheap, so you do that.

There was a committee set up at that time, and then there was a report that said, "We are now going to collect these data, and make these measurements. At the very least, if there are any more surprises which are likely to cause another Baneberry, these measurements will probably tell us about those surprises."

Carothers: You make it sound as though the measurement of material properties started along the lines of, "Well, we've got to do something. Here are some things we can do, so let's do those."

Twenhofel: Pretty close. That's the impression I'm trying to give. And that wasn't foolish. We were scared. And there was another thing; the physicists liked it, because they had numbers now. And again I'm being a little facetious, but not completely.

Carothers: Well, Bill, what is a physicist going to do with information like, "At 1326 feet there are fossilized tree trunks mixed with gravel and sand." How do you put that into a code to calculate anything?

Twenhofel: I know. I realize that. But anyway, that's how it got started. Then the next thing that happened was that some years ago you appointed a Data Needs Subcommittee for the Panel, and I was the Chairman.

Carothers: Well, there had been a certain amount of grumbling, among some Panel members, who would occasionally say, "Why are you showing me all this?" And then there was grumbling on the part of other people who said, "I think it's absurd that you show me data that shows a water content of 100% and a saturation of 120%. How can there be 120% saturation? That just tells me you don't know what you're doing. Why are you doing this?"

Twenhofel: Yes. So, that subcommittee was appointed, and it's purpose was to look at what data was being collected to see whether there was additional data that ought to be collected, or whether we could stop collecting some of it. Well, we expanded that charter a little bit to include, "What kind of data ought to be presented to the CEP." One of our recommendations was that we add a section on phenomenology, and a section on a discussion of

the containment aspects of the event. So, I think we made a pretty good contribution in terms of what data ought the CEP see, and what ought to be in a containment package.

In my personal opinion, I think we badly goofed when we failed to eliminate much of the data we're collecting on material properties. We failed to do that. I tried, and some other members of the Panel tried, to get some of that data eliminated, and the Labs would not stand still for it.

Carothers: What, for example, do you think are some things that don't need to be collected?

Twenhofel: Grain-density. Bulk density. And consequent calculations based on them. I just don't think they are directly relevant to containment. I think that those data do not need to be collected. I've always thought that.

Now, I think that the electrical log is a really valuable tool because it tells you something about whether the rock is different from the norm, whatever the norm is. There's a norm for alluvium, and there's norm for tuff. If there's something different, when the electric log tells you that then you can go and look at the samples very carefully and see whether there is a bunch of clay or not, for example.

I think we know enough now about Yucca Flat and Pahute Mesa that we can drill a hole, take a few simple logs, and treat those areas just like the Sandpile. In certain places along the edges of the valley there may be another Baneberry surprise, but we'd catch it with simple logging. Then we could go into it in detail. This concept didn't prevail in the Data Needs Subcommittee because of the opposition of the Labs.

Carothers: What sort of arguments did the Labs make? You'd think they would latch onto that and say, "Here's our chance to save a few bucks."

Twenhofel: Well, they didn't. I think there were two reasons. If you have numbers, and you have data, that gives you some appearance of having done a good job. You've gone to the best of your capabilities. Also, the calculators do like to have some of those numbers. I'm not trying to downgrade their attitude at all; they had

a good attitude. It just differed a little bit from mine. I think it's time to reopen the whole question of what's needed, and to look at it again.

Carothers: Which properties do you think do matter to containment?

Twenhofel: You ought to know the carbonate content. You don't have to know it precisely, but you ought to know if it gets over six or seven percent. I think you ought to know if there are some big acoustic interfaces. The Paleozoic location is only of concern because it's an acoustic interface. I don't see any reason to give water content, I really don't, unless you want to know it for coupling, and placement of instruments.

One more thing about physical properties, or material properties. I think the histograms that are presented are probably unnecessary. We now have such a wealth of experience, since Baneberry, with all those grain densities, water content, gas-filled porosity, and all that, that what we say is, "Well, they're within experience. They're within successful experience, every property." Well, of course they are. We've covered a span now from 5% water to 27% water, so any value we get is going to fall within our successful experience. We're deluding ourselves with the magic of these numbers and the histograms, in thinking that they are relevant to anything. Again, I'm making an extreme statement to make a point.

Carothers: Or,I could put it as, "We've shot in just about every kind of geologic medium there is at the Test Site, and so the material properties of a new site are almost sure to fall in the range you've observed before."

Twenhofel: And many of them don't matter.

As contrasted to the measurement of the material properties preshot, either in-situ or in the laboratory, measurements can be attempted of the response of the material to the shock pressures and motions produced by the detonation.

Bass: There was a program at Sandia that was beginning to get started during the moratorium. Luke Vortman, Lou Perret, and Al Chabai had a very nice program set up . They asked me if I would be willing to go to work with Al Chabai on this, as his assistant. I

said, "Sure," and I got involved with Hugoniot determinations for earth materials. The main thing we were going to do was write a report on close-in effects of buried underground explosions, be they nuclear or chemical made no difference. We wanted to look at the pressures generated close by, the temperatures generated, whatever was there.

Livermore was heavily involved in this kind of work through the PNE program. Dave Lombard was doing this at Livermore. He was doing a lot of very good Hugoniot work. Bob McQueen was doing it at Los Alamos, and Al Chabai and I were doing it at Sandia. We said, "Let's measure Hugoniots, let's measure elastic waves. Let's look at granite, let's look at alluvium." Nobody wanted to look at alluvium very much. We looked at tuff a little bit; nobody really wanted to look at tuff. We were much more interested in oil shales, sandstones, and things like that.

Anyway, we got going on it. We did explosive work out at Coyote Canyon, behind Manzano. We had an explosive site out there that I was in charge of, and I had a crew of about eight or ten. It wasn't a big facility; it was sort of an ad hoc thing that we put together. So we started doing this, and we were chugging along merrily, measuring shock velocities and things like that. Mainly we were doing Hugoniot work, and gauge development.

Carothers: What shock pressures were you trying to reach?

Bass: My job was to get as high as we could. We wanted to get to a megabar. At that time Altschuler's work was coming out, and Altschuler was getting up toward a megabar. Everybody said, "How in the world is he doing it?" The answer was obvious to us all what he was doing, but it was never in the literature. It has been finally admitted he was using nuclear sources. He was ahead of everybody, there's no question about that. He did some great work, and I think he's still around and still doing some pretty good work. McQueen had started doing some flyer plate work at Los Alamos, where he was getting up towards a megabar.

It's no problem at all to get to a couple of megabars in brass, or steels, or maybe even aluminum. Getting a geologic material up there is a little tougher, because its impedance is so much lower. We did the best we could; we would run flyer plates five or six inches, and planarity was going to hell on us; we were generally using eight inch flyer plates as plane wave generators. We evacuated the path

between the flyer plate and the target, trying to cut down on the air shock that would build up and screw up our instrumentation. So we would fire the things in a vacuum. We would have to glue the plates to the explosive to keep them from bowing away when we pumped them down. It wasn't a very good vacuum, but you certainly could bow an eight inch plate; eighty mils was a typical thickness for the flyer plates. We followed McQueen's work on this directly. I'd say we were getting up to a megabar. The other high pressure work that was being done was being done by Bill Isbell, at General Motors at the time.

Higgins: The results of the Livermore work on Rainier, and that work was very much focused just on Rainier, caused us to modify our experimental measurements program. We had three or four people who were spending a lot of time on designing measurement techniques for the megabar, or many hundreds of kilobars, pressure regime, and we dropped all of those except one confirmatory measurement that was done on the Antler experiment in 1961. That was done by Dave Lombard, and it is the only megabar level active measurement that's been done on a shot.

I can remember standing up at a meeting at Rand Corporation, in Santa Monica, and making an impassioned plea. "Please stop spending all this money on ten megabar equations of state because it doesn't make a bit of difference. It's all electrons anyway." And I wasn't the only one making that argument. That point of view prevailed, and so all that work stopped. And that was wrong. It was a terrible mistake.

Carothers: And you talked them into it?

Higgins: Well, I was one of those who did. I've thought of things I've done wrong, and that was certainly one of them. The consequence of that decision was that the measurements program centered on the things like Bob Bass has done for the last thirty years, measuring stress levels in the tens to low hundred kilobar range, where plastic failure, and brittle failure, and that kind of thing is happening. Of course, that region is important not only for containment purposes; it's important for structures effects purposes. It's a region where the mechanical engineers are very uncomfortable designing things like bunkers, and missile silos, and very crucial elements of an offensive or defensive system. Whether

the stresses come from nuclear or not nuclear things, that stress region is important. So, it wasn't totally a mistake, but it was a mistake from the standpoint that we would today understand more about what goes on in the explosion than we do.

Bass: I did not go to that meeting in Santa Monica. I was in Rio that week, and if I had a choice, I would be in Rio de Janiero. I would say that where Gary now feels we are lacking is not necessarily in the megabar region, but in the hundred kilobar, two hundred kilobar regime. That's because the phase changes that are going on in all of our native materials have been terribly handled theoretically. The various contractors who have worked with that have really botched that job badly.

When you get into a porous geologic material, apparently the phase change can move down in pressure, down into the seventy or eighty kilobar region, because of the temperature that's involved. Alluvium does funny things. Alluvium starts expanding when you get above a hundred kilobars when you hit it. On a Hugoniot plot of pressure versus volume, it starts expanding when you get up there, because you're moving back in the temperature curve. It's a mess, and I don't pretend to really understand what's happening there.

It would be nice to have data in the megabar region, but I'd rather have them in the hundred kilobar regime. Shell Schuster, who used to be at Livermore said, "Don't measure me another Hugoniot, for God's sake. I can draw them." And I think he's right. You can draw the Hugoniot, but you can't write the equation of state.

I think we've got a better handle on some of these things than a lot of people realize. There have been two decent sources of data in recent years. The containment program has provided a wealth of data. Frankly, I think most of it recently has come from DNA, and Sandia. I think that's because of where they test. The DNA tunnel events give you the opportunity to make a decent measurement, because you can get there. You know exactly where your gauge is, you know exactly where it's pointing, you can orient it with a transit. You don't have to dangle it down a hole, or put it in a satellite hole.

The other great source of data has been the hydrodynamic yield program, and this a tremendously overlooked source of data. We got seventy-five pressure measurements in tuffs and alluviums

in the period when Los Alamos was making hydrodynamic yield measurements. Individually they're pretty damn bad, but as a whole they're pretty good. There's a wealth of data in there in the 500 kilobar to 10 kilobar pressure regime. There are awfully good data on events in alluvium up to 500 kilobars. I know they're good because I made the measurements, and I'm very happy with them. They were not measurements of wave shape, however. They were measures of peak pressure, and at 500 kilobars what else can you have?

Another batch of data came from Livermore. Clyde Seismore came up with a marvelous pressure gauge that worked in this pressure regime. It was a bulb of plexiglass; it turns out that when you shock plexiglass it puts out a charge. So, Clyde put this bulb of plexiglass downhole, and he made some outstanding measurements.

App: DNA has the best opportunity to look at material properties in-situ, and at what material damage has been caused by the shot because they can go into the tunnels preshot, and can reenter after the shot. On a reentry they can go back in and obtain core samples of rock that has been shocked to a kilobar, or five kilobars. They can obtain the damaged samples, send them to Terra Tek and have them measure the residual strength.

Carothers: And it's different from the strength of rock that hasn't been shocked.

App: Yes it is. For the cases I have seen on reentry, the tuff appears to damage more than the grout. Preshot, both the rock and the grout give off a ringing sound when hit by a rock hammer. Postshot, the grout still rings, but the tuff gives off a dull thud. The postshot tuff can be pulled from the walls by hand, and it crumbles.

DNA has taken cores from damaged rock to Terra Tek, and the failure properties they measure are way, way down. Of course, it's a function of range. At five kilobars the damage is severe, at one kilobar it is just beginning, and at five hundred bars it is virtually nonexistent. Damage decreases significantly with increasing range.

As a practical matter, on the vertical shots we can't obtain such core, so we have to estimate damage based on the tunnel rock observations. Or we can try to pseudo-damage rocks in the laboratory them by straining them, and determine how they weaken

with strain. You can do that up to a point, but you can't get the really large, twenty percent type shear strains that occur in an underground test. And, you can't replicate the strain rates either. So, we're not really replicating what is going on in the ground with laboratory tests.

However, with laboratory tests we can get some feeling whether a material is going to damage easily or not. DNA has gone to a lot of effort to try to simulate damage in the lab, but with a lab sample, once you get a through-going shear failure, you've lost your experiment. They can't achieve twenty percent strains, although the people at Waterways and Terra Tek are trying. They're coming up with new schemes, and who knows? They might be successful; it would be very valuable to the calculational community if they were.

Carothers: Suppose you get a sample from a tunnel location, and you send it to Terra Tek. They say its compressive strength is such and so. There are folks who would say, "That's the value for a small, competent piece of material. The region the device energy is interacting with is much larger, and that much larger volume will have things in it like fractures, changes in porosity, and so on. Therefore the lab measurements are not really representative of the world the device energy is going to interact with."

App: That's right. But, for certain types of rocks apparently that measurement is fairly representative. The Tunnel Beds tuff in the tunnels is perhaps one of those rock types. The DNA modelers feel that the in-situ fractures don't modify the overall properties much from what one obtains from the small samples. The reason they believe this is that they can put the Terra Tek test results into their models, and replicate the outgoing shock wave fairly well. Now, I said outgoing shock wave; late time residual stress is a different story.

Other rock types such as alluvium and welded tuff also are a different story. For these materials, what you're saying is absolutely right. You cannot go from the laboratory measurements to a calculation that agrees with the field data. This is a big problem for us, and the approach we have taken is to start a systematic study of events that have had a lot of free field measurements associated with them, and infer the response properties of the rock mass from them. Merlin alluvium is an example. The Merlin event was heavily

instrumented by Sandia. There were a lot of working point level gauges, some horizontally out, and some vertically up from the working point. Unfortunately, the Merlin samples are the only ones recovered for alluvium core properties measurements, so that's the only case where we can make direct comparison to the calculations.

We're trying to create a library of properties based on inferences made from modeling, as opposed to from core. I don't know what else to do. This approach does take into account the larger volume. Exactly what you've said is what prompted us to take this course of action. We do not get a unique solution from these calculations, but the more measurements we have, the closer to unique it becomes. So, we now have a standard equation of state for Area 3 alluvium. It's not based on mechanical measurements, because you can't obtain core in alluvium. If you do get core it's only the more competent parts of the material, so it comes back to your argument that it's not representative.

What is representative is what we see in the wave forms, and if we can infer properties by using forward modeling techniques, then we can come up with an equation of state for Area 3 alluvium. When we have our next shot in Area 3 we will take those properties from Merlin, look at the comparisons of the physical properties, such as density, and perhaps make a few adjustments, but keep the same basic response model. Then we will use that in the new location where we're trying to do a site evaluation.

We're currently taking this approach with granite, for the verification program. We have the same problem for verification. And so, we're trying systematically to calculate a number of events. We've been doing this for years now, as time permits. It is not something we have recently started. There are three of us working on this; myself, Wendee Brunish, and Jim Camm. We've calculated some Pahute Mesa tests, and in Yucca Flat we've done a lot of work on the Hearts event.

Carothers: I would think that Pahute Mesa would be your most difficult area. On Pahute you have fairly soft layers of ashfall or ashflow rocks, and you have pillow of lavas, so you have hard layers and soft layers. All of those presumably interact with the outgoing wave. How do you handle that?

App: It's a difficult problem, and we're not happy with our Pahute Mesa results in comparison with the experimental data. We've been able to model certain aspects of them; we're able to see the same kind of rarefactions, in the model, coming back from the hard to soft transitions that we see in the measurements. But they are only qualitatively similar. Quantitatively, no. I said something about uniqueness earlier. When you have a layered situation, it is extremely difficult. From the modelling standpoint what we'd really like to be able to find is a Pahute Mesa site that doesn't have any soft layers, but then we'd be afraid to conduct the event, from a containment standpoint, because the site would be different from any we've used before.

Carothers: When we talk about differences in the materials, over what physical dimension do things have to be different to produce changes in the response?

App: The size of the feature compared to the wavelength is probably the most relevant thing. If a feature that is different is quite small compared to the wavelength, it may not be very important. This is not always true, however. It would not take a very large open fracture to seriously attenuate the signal. The wavelength is going to increase with increasing distance, so by the time you get out to where the wave becomes elastic, it's going to take a fairly large feature to cause a serious perturbation.

Carothers: Let's consider closer to the device. There is the thought that it doesn't matter much what's close to the device, because the energy release is so large that it just overwhelms any minor geologic features. Some folks think that's not a very good argument.

App: I'm not one of those people. Close in, in the true shock regime where the wave is supersonic, I don't think response properties of the solid rock much matter. For rock that is vaporized, it does make a difference. Now, again, these opinions are based on modeling. I think properties do matter beyond where the eventual edge of the cavity will be, at the few kilobar level and below. The exact range would be somewhat material dependent.

Two and three dimensional effects are important, but our serious difficulties lie with material response.

Carothers: But that's something you cannot find out in the laboratory with the tests that you can make today.

App: You're right. Not to the degree that we need to. I think we need more measurements in the field, and from that data, backing out the response models would be the way we would go. These would be inferred properties, and once again, there is a uniqueness problem. But, if you get enough data, using reasonable assumptions about how a material behaves, augmented by mechanical tests in the laboratory, you might be able to back out how particular classes of materials behave.

That takes an experimental program, and in fact that is part of the emphasis right now in verification, to do more well controlled in-situ experiments. Modeling plays an important role in the experimental setup. In order to maximize our ability to infer bulk response properties of the rock, instruments must be located well into the inelastic zone, and at numerous locations.

To characterize various classes of rock in this way will be an expensive process, but once having developed a library of properties, we should have more confidence in modeling, and understanding the more important effects of layering, such as exists at Pahute Mesa. Currently it is very difficult to sort out the effects of layering.

The Pahute Mesa event named Houston was a rare example of a site where there's considerable thickness of hard rock without softer interlayered rock. The soft layers are halfway up the hole. If we can have another shot in that area, where we can get instrumentation strings deep, and study the propagation of the initial outgoing wave, then we can learn something about the properties of a heretofore difficult material to characterize.

Carothers: Carl, when you make the measurements you're making, I'm sure you must need to know properties of the materials. Where do you get that information?

Smith: DNA sends cores to Terra Tek principally, where they are squeezed. The data from that goes principally to Pac Tech, where Dan Patch does most of the stemming calculations for DNA. The other big source of information is Maggie Baldwin and the people at DNA, because they're the ones who have done the exploratory geophysics work in those holes.

Carothers: Carl you have said, when talking about the equation of state work, that measurements in the field are not necessarily at all like those made in the laboratory on samples. Couldn't the same criticism be made of the data from the cores?

Smith: It's a question of economics. With a fixed pot of dollars you could spend it all on investigating one little area, and know more and more about less and less until you know everything about nothing. Or you could take that fixed pot of dollars and explore a larger area with selected measurements. This question has always been a bugaboo for DNA; you've got all these variations on the core measurements, but everyone wants to treat the material as uniform, because you do all your calculations with one material.

Actually, for years DNA has been very successful in treating all this shot area, this material they excite, as roughly uniform material with some faults and fractures through it. But now, as you go to smaller and smaller shots, and maybe go into a new tunnel, and maybe get into places that are not zeolitized, now maybe these variable things come back to haunt you, and you can't treat them as a single element. The scale is no longer the high yields where you overwhelm the geology. If the scale is that for only half a kiloton, maybe that fault is going to eat you alive.

Carothers: Instead of taking all these cores, which means you have to drill a hole, why don't you run logging tools? That's cheaper.

Smith: Logs give you one type of information, the cores give you different types of information. Cores give you information up to four kilobars, and the calculators like very much to have that.

Calculations of many kinds are done. One of the important questions for containment is how the gases in the cavity, originally at very high temperatures and pressures, flow out into the surrounding medium. Central to that is the permeability of the materials in the earth itself, and in the column of stemming materials. One of the first efforts to measure, and to calculate that was made by Carl Keller, then at Los Alamos, in the early seventies.

Keller: The flow paths of concern were the stemming column, the chimney, the hypothetical hydrofracture, and that was about it. The characterization of the medium required for hydrofrac calculations was never done. The permeabilities were not measured. The

in-situ stresses were not measured. There were no serious measurements even of stresses near the events. There were serious efforts, but the data certainly weren't of the quality that there is now. Cavity pressures were not measured. They were inferred from LOS pipe measurements of pressure, and those weren't too bad. Now they seem to be what they were thought to be then; a lower bound on the cavity pressure.

Carothers: How could you run a gas flow, or hydrofracture code? You just listed a number of the important parameters and said you didn't know them.

Keller: Cowles and Yerba were the first two line-of-sight pipe events after Baneberry. Yerba was in a shaft, and so we had access. So, one of the things we did, having that state of ignorance, was that I designed a permeability measurement scheme for the Yerba shaft, and the J-6 folks built the hardware, installed it, and made the measurements. Every hundred feet in the Yerba shaft we made permeability measurements, and those are still the only permeability measurements with that kind of resolution in existence.

Those were done with two drill holes. One was the air injection hole, and you measured the flow rate and the pressure history at the bottom of the hole. They were essentially packed off so you only had a small volume at the bottom, which was the gas source, that was free to leak into the medium. And given the pressure history and the flow rate, you can determine a permeability. The second hole measured the pressure of the flow field. That's a check, a redundant check, and from that you can also deduce a permeability. And so with that over-constrained system we could tell whether or not it was really spherical flow, and we could tell whether it had come to equilibrium, and some of those kind of things.

The Yerba measurements have been invoked countless times as characteristic of alluvium. Well, alluvium is a highly variable material. One of the most glaring examples of the differences in alluvium are the Agrini crater, which was 200 feet deep and bigger at the bottom than at the top, versus Pike, where the alluvium just fell in like a big sand pile. Some of the alluviums crater very gracefully; they just fall in and there's a big flow, a big slump down to the bottom.

There have been other permeability measurements made by other people. Frank Morrison and the Livermore folks tried to deduce permeabilities from pressure histories measured during the stemming process, which is kind of a clever way of doing it. It's pretty complicated, but in principle you can do it with enough math, and get a measure of the permeability.

Another way was to measure pressure histories in drill-back holes, and from that try to measure overall permeabilities. I think those are fine for a real gross measurement, but there are serious problems with them. For one, the volume of the hole can be a real problem because you have to fill the hole. It's not like you have this ideal pressure probe which does not influence the flow. You have to fill the hole, and these flows are very small with that kind of driver, so it takes time to fill the hole. And then the hole can actually leak off, because they're not all cased, and most of them are not grouted even if they do have a casing in them. And so you never know, because you can get strange flows. You can come up in this hole, and run down to the bottom of that one, and shortcut the medium. There are a lot of problems with those measurements. So, it gives you a very gross measure. There are better ways of making measurements.

You can even infer permeabilities. If you presume that you know the noncondensable gas source, then you can, from the arrival times at the surfaces for those that have leaked, infer chimney permeability.

Carothers: Well, along these same lines, in the CEP you often hear somebody say something like, "Well, you may get a fracture to here, or there may be gas transport to there, but this layer has a lot of permeability and porosity, and so it will just soak everything up.

Keller: Yes, you do hear that a lot. The people making those statements are not very quantitative in those areas, but they could be. Those kinds of statements could be supported completely with simple noncondensable gas flow calculations. Or you can even do hydrofrac calculations. Generally people infer that if you have a high porosity, high air void content, then you have a high permeability. That's sometimes true, but not necessarily so.

And the permeability is never measured; it's always inferred from other characteristics. That's bothered me forever. There have been some pretty strong statements about pore space available, but

it is the permeability that determines whether it's really available. Some air voids are not available; they're in pumice shards, for instance, and they're sealed off.

Near the detonation point the shock pressures, the stresses and the strains the material undergoes, and the time scales on which they occur are beyond those that can be created in the laboratory. Data from instruments located in the material near the shot point can give some information, but the environment is severe, and such experiments are extremely difficult to do. Carl Smith has done extensive work on developing ways to make such measurements in the field, on both nuclear events and high explosive experiments.

Smith: I have principally done gauging work, working on gauge development techniques, trying to make in-situ equation of state measurements. Equation of state measurements typically are made in the laboratory on small samples. Of course, if the sample breaks you discard it, and get an intact sample. But field work invariably involves fractures, and faults, and things like that, and so the big push for many years was how to develop techniques, and how to make measurements for in-situ equation of state type work.

The equation of state measurements principally revolve around the area from the near elastic into the shock wave regime, so you're through the yield range of soft rocks, like tuffs. That's from like half a kilobar up to the ten kilobar regime, where the yield effects take place, and that's where the unknowns are in the equations of states.

In such work you need measurements of both motion and stress, because of the three dimensionality of the meaurements in the field. In gas-gun work you have one dimension, and you can take stress measurements and get the motion measurements out of them, through the Hugoniot equations of state. On a gas-gun type of shot you slice the rock, put in these material gauges, which can be as thin as mils, and then glue all the layers back together. The shock passes through the rock from one end to the other, and so it's one dimensional.

In measurements in the field, because of the spherical divergence, you need the hoop stresses, the radial strains and the radial stresses, and particle velocity measurements. And so, very quickly, you become aware that the Achilles heel of all that work is in developing instruments to make viable measurements.

The seventies were the days when we started developing the so-called ytterbium gauge. Ytterbium is an odd-ball element that sits off the periodic chart, and has a very strange electronic structure. As discovered by Bridgeman and others, it has a very wild stress-piezoresistive effect. In other words, as you squeeze, it changes it's resistance.

On a field event you don't have the ability to build an in-material gauge, as you do on a gas-gun shot. You have to drill holes, insert the gauges, and put in grouting material. The concern then is, does the grout material match the host in some way. In particular, you want a measurement that is representative of the free-field stress in the rock, in a material that is not the rock itself. That's the so-called inclusion problem that people have worked on for numerous years.

Carothers: You say you want the grout to match the rock in some way; which characteristics are most important?

Smith: Compressibility. In other words, does the grout deform in the same way as the host rock.

Carothers: When you get into the ten kilobar range, you're in the region where the tuffs are plastic. That means you're near the edge of the cavity, where it's still growing. How do you make things survive?

Smith: They don't. Principally it's the cables and electrical leads that are destroyed. Sometimes we can go in on a mine-back and find these gauges. When we find them, the first thing we do is see if the gauge is still intact. Almost invariably it is, but the leads that have been severed, or torn off the package, or somewhere something like a fault has moved differentially and sheared the cables.

What we're getting out of the gauges now is the arrival time, a rise to peak, and then a little bit of unloading, enough stress wave unloading so we can say that we have indeed captured the peak, rather than having it go up and stop before it reaches the top. One of the efforts nowadays is to enhance those recordings, and to incease the recording times by building armored cables, and so on.

To build stonger cables we're now using a technique that uses wire-rope. The first time we did that we took a wire rope, took off the outside strands, replaced the center core with the electrical

cable, and then wrapped the outside wires back on the rope. Now we've gotten sophisticated, and we're going to a wire-rope manufacturer to have the cables built that way. At the end of a production run on something of about the right size, we stick our spool of cable at the back of the machine, and have the wire-rope made with our electrical cable as the center.

The shock wave damages the materials through which it passes, and changes their properties. Those properties are of importance in what occurs in the later time processes around the cavity. Attempting to reproduce in the laboratory the damage that occurs due to the shock loading is extremely difficult to do, and the material properties measured on such damaged rock samples as can be produced may be quite different than those of materials near the detonation. Persons attempting to develop models to predict the ground response must often infer the material properties by trying to match their calculations to data such as arrival times, peak values, and decay times of the pressure pulse.

Keller: One of the really interesting experiments we did was on one of Sandia's shots called One Ton, which was done by Carl Smith. We obtained, from the working point region of One Ton, big, twelve inch core. We took that to SRI, cut it, machined it, and put it in our HE charges with the wires, to measure the response of that tuff from the working point of the One Ton HE shot. We also took core to Terra Tek, and measured, in the laboratory, the strength properties of that same material.

Both S-Cubed and Pac Tech did calculations of the SRI test, and of One Ton. This was before the shot. Then Sandia shot One Ton, and made good stress measurements all around it. I asked Sandia not to tell the calculators what the results were.

I had asked the calculators to take the properties of the cores that Terra Tek had measured, and calculate the SRI test. Then they could see the SRI test results, and they could take those results and modify their equation-of-state if they wanted to, and then they were to predict the One Ton results.

We met at S-Cubed, and they had their viewgraphs of the stress histories at the various ranges, which had been pre-selected on the SRI tests and on the One Ton tests. We had pre-selected the scales

to use in the plots of the results, so you could overlay them. Sandia would put down the measurement from One Ton, and S-Cubed would put down their stress history, and then Pac Tech would put down their stress history, right on top. The correlation between the predictions and the measurements was really quite good. It wasn't equally good at all ranges. The calculations were better close-in than they were far-out.

We discovered, out of that series, that the way Terra Tek was measuring the pressure versus volume curve was not good. They just put the sample in the holder and squeezed it. From that they would get a pressure versus volume curve. However, if they put the sample in the holder, squeezed it up to the overburden stress, where the samples had been obtained, and let it sit there for a while, it would creep to a lower volume. Doing that sort of replaced the sample in the mountain, and now when they ran their pressure versus volume curve they got much lower compaction. It turned out that you needed lower compaction in order to match the results. That was a really instructive series, where we compared our predictions and our procedures to reality. Sandia was very helpful on that.

Patch: The most important things that go into the models are the mechanical test data that are done in the laboratory, on cores. And those tests have some serious limitations that everybody understands. The people doing the tests certainly do, and the users do as well. One of the most serious limitations is that they are limited in the total amount of strain, because they can only squash the rock so much. Nuclear bombs, near where the bomb is, have a way of scrunching the rock a whole lot. That strain path is just not accessible in the laboratory.

Carothers: My impression is that they can go up to about four kilobars.

Patch: Yes, they can go to about four kilobars. We have gotten up to six kilobars at Terra Tek, and I think eight kilobars is doable in a Terra Tek type test. They can go up to twenty-five kilobars, but the problem is, you don't get out the data you need. What you're really looking for is the response of the rock in terms of its deviatoric response, and so on. Just pushing on a rock and measuring how

much it squeezes gives you some data, but there are many other things you want to know. So, it doesn't help just to go to some high stress level.

The other factor is that you can load the core to four kilobars by loading it axially, but you can only deform it so much before you reach the limits of the machine. The problem is that in the ground, rock that's squeezed to four kilobars subsequently moves out a long ways, and undergoes a lot of strain. It laterally stretches and it compresses axially, and that occurs at much less than four kilobars. A great deal of that motion might be at only a quarter or half, or maybe three-quarters of a kilobar. A lot of that deformation goes on at low levels, and one could track that in the laboratory, except that there are mechanical limitations on the machinery.

The other problem we have with mechanical test data, and in some ways it's almost more serious, is that in the ground, when the material is deformed there is a funny kind of lateral constraint. To first order the material is forced to move out spherically symmetrically. Maybe block motion happens later on, and other funny things, but by and large, if you go in and look at any given piece of material, you can pretty much convince yourself that it's been homogeneously moved out and stretched. To the zeroth order it's an isovolumetric strain path. If you try to do that on a sample in the laboratory, you can do the compression part of it. Once you try to mimic the part of the strain path that amounts to stretching it laterally, and taking up that so it is kind of isovolumetric, the rock wants to fracture along a shear plane. It wants to form these shear planes, and now suddenly it's not a continuum material anymore. You're doing a friction test in a way, and you get data out of the test that looks reasonable. The only problem is, it doesn't have any relationship to the way the material is behaving either in the field or in any kind of continuum sense. That's one of the serious problems, and we have to finesse our way around that.

Rimer: I've been working for a number of years, trying to understand how the rock gets damaged. I have not been able to get data in tuff, because its permeability is so low, for effective stress modeling. So, I assume the laboratory data we have has the pore pressure built into it, because the strengths are lower because of the saturation. I am still trying to get measures of how much of the material is damaged from the shot.

We tried to do a laboratory material properties test at Terra Tek that would go along the strain paths. Unfortunately, you cannot confine the material in the laboratory like it is underground. Underground it's confined by adjacent material doing the same thing. You have membranes around it in the laboratory, with pressures on them, but you can only measure the strains in a couple of locations around the circumference. And, you don't even know what path you're on. As you start to unload the material, you get a through-going fracture, so those samples are worthless for material tests after that. So, that work was unsuccessful.

There is another set of data on reentries and core samples taken at the time of Hybla Gold, which was near Dining Car. These show that the samples that were near Dining Car were damaged greatly. Their strengths were extremely low, much lower than we can reproduce in the laboratory by damaging the material to the same peak stress levels. So, my hypothesis was that the total shear-strain the material has seen is greatly different, based on calculations, than with any model. And that's the difference; we should make the damage that we see a function of shear-strain.

In late 1990 I and Bill Proffer, who did the calculations for me, used that Terra Tek data to do some residual stress calculations. Those calculations give grossly different residual stresses. The peak in the residual stress is further out, but it's still considerably higher than the cavity pressure. Even though the material is now much weaker, peak stresses are the same as with the other model. So are peak velocities, so are cavity pressures, and cavity size. The material goes out more, comes back more, and ends up at about the same place. But, it undergoes a lot more plastic work. It gives low peak residual stresses further out, but gives almost no residual stresses out to, let's say, the range of the FAC, the Fast Acting Closure. The other model would say a third of the way from the cavity to FAC you've got strong residual stresses, much higher than cavity pressure.

I think that's why we see radiation as far as the FAC on many of the DNA events. There's a nice closure, but it's permeable, and the residual stresses aren't there to keep it closed. And so, you get a little seep of material through that grout to the FAC. It doesn't influence containment because there's a gas-tight closure further

down, but there's a little seep. Operationally it means we can't examine and take out the FAC anymore. But I think that's in line with the new calculations I've been doing with this new model.

We know that post-shot we have much lower strength in the material. When this strength reduction occurs is anyone's guess. My guess is that it doesn't occur when the peak stress is reached. The material continues to strain all the while it's moving out, and the strains it gets to may be a factor of three higher than the strain it sees at the peak stress of the shock wave.

I recently saw some interesting data. Terra Tek had taken preshot samples from Disko Elm, and they did the normal tests on them. Then they did SEM tests, the scanning electron microscope tests. Then they used what they call a Wood's Metal approach, where they use melted metal, which gets into the open pores and fractures of the sample. They shine a laser on the sample, and they get marvelous color pictures of the microstructure at different scales, even better than they get from the SEM pictures.

They did the same thing to materials they took post-shot, at the same stress levels. The pictures are totally different. At two kilobars, from samples that were damaged in the laboratory by squeezing, you see some signs of pore crush-up, but just a little bit. Once in a while you see a little fracture. At two kilobars, in the in-situ, shot damaged material, there are fractures throughout it. It looks like a totally different process has occurred.

Carothers: Well, sure. The material near the shot doesn't get just compressed. It also gets stretched tangentially, because it's moving out.

Rimer: That's right. Exactly. And that's true even at two kilobars. That's what I mean by the strain test. I'm using shear-strain, because mathematically it's a principal invariant. The lateral strain is tensile, the radial strain is compressive. They add together, and you get four percent, roughly, at four kilobars peak stress. That's almost all radial strain, but then it keeps stretching as it moves out almost incompressibly. The strain is enormous; you can get twelve percent strain, and that's what I'm trying to model. I don't know the numbers, the parameters, but when it reaches ten percent strain I think it's mush. And we've seen plenty of mush near the cavity. These results that Terra Tek showed are another confirmation of that.

Ristvet: You get into totally microfailed material as you get about a quarter of a cavity radius away from the cavity boundary. You start seeing isolated pockets of this material from about two cavity radii in, and it is basically like cohesive silt. I would say its unconfined strength is only a few hundred psi, as a result of the microfracturing. We've documented that at Terra Tek and USGS, and we see it in the shear wave drop, and so forth. It's for real.

Rimer: Terra Tek also did uniaxial strain, and triaxials on those damaged samples from Disko Elm. They have much lower strength, and the strengths get lower and lower, within the scatter of the tuff, as it's been hit harder. I don't know if strain is the right thing to use, but it's much better than stress.

App: In the effective stress there are theories that the pressure of the water, after it has been shocked and some unloading has occurred, exceeds the stress in the matrix. Then, essentially the response of the whole aggregate is determined by the response properties of the water. And the more water you have, the more that's going to occur. In an effective stress model, the strength, after the material has been loaded up to a certain point, comes back to zero. There's no strength left, and it is the water in the pores that determines the response of the material.

One reason the effective stress models have not been adopted universally is their extreme sensitivity to small changes in mechanical behavior such as dilation, porosity increase due to shear induced microfractures. Pore pressure, and therefore shear strength, is very sensitive to such increases in porosity. Yet, in the field, we do not observe huge variations in observed phenomena from site to site, at least not at the scale that is suggestd could occur due to dilation.

Carothers: In P tunnel there was a small change in something that made a big change in the response of the ground.

App: Well, yes. DNA does have a prime example that is contrary to what I was just saying. Why was Mission Cyber so different from Disko Elm? Those were two shots that were very similar. If I'd thought of that a minute ago when I started on that little spiel about not seeing much difference, I might not have said it. There is apparently some change so hidden that nobody's been able to identify it. Thus far, the only difference that has been identified is the minerology, and we cannot determine how that

would alter the phenomenology. One site has been altered to zeolite, and the other hasn't. The mechanical properties from both sites are about the same. There is no known answer at this time.

Rimer: With the things we usually successfully measure, the free-field ground motion, the calculational results do not tell you which model is better. Residual stresses would, and we're still trying to measure them; real hard we're trying to measure them. The problem has been gauge breakage, and cable breakage.

Carothers: How about a self-contained, hardened instrument that you recover after the shot?

Rimer: Great idea ! We tried that with the SCEMS, the Self Contained Environment Measurement System, a big heavy piece of equipment that does that. And the batteries went dead on it.

There's a paper by a guy named Starfield, in which he talks about the limits of our ability to understand rocks, and classifies calculations by how much data is available on the material; how much data is available to isolate the physical models that are important. It's a very interesting paper. It really tells you how limited you are in rock mechanics, in your understanding. That's not to say you don't learn anything about how materials behave by looking at measurements, and trying to match measurements. You need to know as much as you can about the properties of the rock, and I get very exercised every time I'm at a CEP, because they go into enormous detail about sonic velocities, and physical properties, but they don't talk about strength. And containment is, to zeroth order, a strength phenomenon. Water matters, for cavity pressure, and it has an effect on strength. Gas porosity matters in attenuating peak motions, and surface velocities. Why anyone cares about surface velocities for these deeply buried shots I don't know. I guess it's easy to measure.

Carothers: I don't think anybody knows how to measure the in-situ strength, unfortunately.

Rimer: That's true. Now, Bob Schock looked at how you could determine strength from the measurements we have. He found a strong correlation with the shear modulus, the modulus of rigidity, the shear wave velocity. Any one of those, because they all use the same quantity, really. I always thought an improvement would be to measure the in-situ shear wave velocity, because you can get a

shear modulus, and from that maybe get an idea of the strength. Not the full story, but a feeling. Now, John Rambo, at Livermore, looks at drilling rates as a measure of strength. He's come up with some interesting correlations.

Carothers: But the drilling rate depends on a lot of things you don't know. How sharp is the bit, how much weight is on it, are the drillers pushing today, or taking it easy.

Rimer: I understand. But it's something that's worth looking at.

Another thing we spent a great deal of time on was Pile Driver. I must have done thirty or more one-dimensional calculations, and a number of 2-D calculations, to develop a model for the in-situ strength of the Pile Driver granite. The intact rock was very strong, but the pulse width measurements that Perret and Bass, at Sandia, did, and SRI did showed wider pulse widths, which shows weaker material. By pulse width I mean velocity versus time.

Carothers: There are folks who might say, "The rock is very strong. We've taken good, intact cores to the lab, checked them out, and it's strong rock all right. No doubt about it." And there are other folks who might say, "That's all very well, but that's a mountain there, which is not intact. It's full of cracks and fractures which weakens the rock."

Rimer: A one-foot joint spacing.

Carothers: For example. And so, you have all these numbers from these unfractured cores, but you've got deal with all this fractured rubble, to exaggerate a little.

Rimer: I spent a considerable period of my life dealing with that. Ted Cherry's idea, and he first thought of it back at Livermore, was that there was water in the fractures. At this point I think it's more likely there's clay there. Either way it results in a weaker, lubricated joint system. Ted modeled that with an effective stress model. We tried a number of things, and that's what I spent a lot of time on. What could we do that was reasonable, where we used the laboratory strength of the granite, which was superstrong, and then brought in some physical process to reduce the strength? We used the effective stress model, and ran 2-D calculations which we calibrated to pieces of data; Perret's underground particle velocity

measurements, the geologic structure, which was a weak weathered layer, then a layer where Perret measured the wave speeds to be slightly less, and then the working point material.

We were able to match all the ground motion measurements, both underground and free surface, with that model. The peaks were a little different, but the SRI data is from a different azimuth than the Perret data, and that may explain it. We were able to match, with a 2-D calculation, that data, but I don't believe it. I don't believe that effective stress is the true model. I think it's more something that happens in the joints, and that could be tied in with the pore pressure in the joints. We had a program, which DARPA funded, thats consisted of small-scale explosive tests at SRI, using 3/8 of a gram of HE, to look at this.

These were in granite cylinders. I was doing calculations, supervising the experiments that Alex Florence was doing up at SRI, and having special laboratory material properties tests done by Chris Schultz, at LaMont Dougherty Geologic Observatory at Columbia University. In these experiments SRI was measuring particle velocities, looking at wet versus dry, where they measured the pore fluid pressures preshot.

We had overburden pressures on those cylinders. We put everything in a balloon, pumped up the gas pressure, and then blew the balloon. The granite just splintered into pieces. Then we put lead shot around the cylinder to let it go out slowly, and the granite microfractured. That fracture spacing, when the cube root of the yield was scaled up to Pile Driver, gave us, within a factor of two, that one-foot joint spacing. We were trying to get to the bottom of this question, and we spent three or four years on it.

I finally concluded that the strain rate effects in the small scale experiments were too great. They decreased the strength so much that they were not relevant to Pile Driver. However, they still showed an effect of water, but not as strong an effect as I believed to be in-situ.

As the number of events increased more and more information accumulated about the events that were taking place. New people joined the program, perhaps an old-timer ot two left, and there was increasing difficulty in relating a current shot to the experience on an earlier one. What previous experience had there been? Had there been a similar geologic setting for a shot, similar material proper-

ties, a similar yield at a similar depth? Eventually there was recognition of the need to bring together in some accessable fashion what was beginning to be a large amount of data.

Rambo: In the late sixties, while I was still involved with slifer measurements, I and a lady by the name of Mary Lou Higuera were set together in a nice large room in Building 111, and told to start collecting all the data on our shots. So we started collecting data, and I wrote a simple version of a data base that would work. I decided what the logic should be, and interestingly over the years that piece of logic has still remained as one of the ways of getting the data out.

Carothers: What sort of things did you have in your data bank?

Rambo: Yield mostly, at first. The groups that I was working with were very interested in yield, because seismic happened to be a big thing at that time. So we had different kinds of seismic yields in there. The old slifer yields were put in there as sort of a comparison, and there was some thought of going back and reworking all of the old seismic data. And, it went further than that. We tried to put a little bit of geology in also.

Then, after Baneberry, the tone of it changed dramatically. Then it became very interesting as to what caused things to leak, and were there any clues that could be put together to extrapolate to serious problems of that sort. I recall the day after Baneberry happened, of looking in the database, and gee, there seemed to be a definite correlation of shooting shots shallower than six hundred feet and leakages showing up. So I wrote a very limited memo to about four or five people. Billy Hudson then took some of that data and extrapolated it in a more formal sense, and that became policy. Some of those data bank runs that we did in those early days really did cause the development of some of the procedures that we use nowadays.

That data collection is still being carried on. Now Los Alamos information is included as well. The two Laboratories do trade this information to update both of their data banks. Los Alamos independently started a data bank about the time of Baneberry, and did find some similar correlations to what we found.

Keller: One of the things I did before Baneberry was to develop a library of shot data. So, I evolved the first data bank at Los Alamos, and I got it to print out in regular book format so I could trim the printouts and bind them. Then I had a data book in which I had all the shot names, and the dates, and the depths of burial, the yields, and everything else that was known about them. That was one of the things that was picked up very quickly, and they decided to expand that data book to include all the Lab data on the shots. The device designers also had their own shot data book, but it was more crude; it had been developed much earlier.

At that time, as I remember, there were like 72 underground events, total. And there were only Los Alamos events included in the data. We didn't even think about Livermore; somehow that was irrelevant experience. We were really pretty parochial. And the Livermore data wasn't readily available either. So, I only put together those Los Alamos events, and I remember the highest yield event I had was Halfbeak, and the lowest yield was Solendon.

7

Logging and Logging Tools

Paul Fenske, before he turned to hydrology, spent several years working for oil companies, doing logging on holes thought to have penetrated an oil bearing formation. These were small diameter, cased, fluid-filled holes often drilled to depths of many thousands of feet. Here the problem was not to obtain the kinds of data about rock properties needed for code calculations, but to determine where, if at all, the oil bearing regions were so the casing could be perforated there, and the oil pumped out.

Fenske: There was a standard set of geophysical logs; there was a resistivity log, a neutron log, and what was basically a conductivity log. It was one of those logs where you had two coils, and we transmitted from one coil to the other. The ability to transmit from one coil to the other was given by the conductivity of the formation. We would run those induction logs, I guess they would call them today.

The neutron log was a porosity log, essentially. We looked for the hydrogen content of the rocks. If you have a real clean formation you would find a difference between the gas in the well, and the hydrogen content, but most of the time you couldn't depend on that, because most of the time the formation wasn't that uniform. It wasn't isotropic or homogeneous, and so you couldn't depend on that. There have been a lot of advances made in the logs, how you interpret the data, and what kind of logs you use since that time. This was in 1952, and we had, by today's technology, some rather simple logs; induction logs, neutron logs, resistivity logs.

We used those for the purpose of defining what the structure was in the area, and also interpreted them in terms of where the pay zones, the high porosity zones, were. Basically, we were trying to determine if there was porosity there or not. At that time the logs were not good enough to determine if there was really oil there or not. You could tell if there was porosity, and you could tell if you were dealing with a shale, or dealing with limestone, or sandstone. You could tell where the formation tops were, and the formation bottoms, and things like that. But you could not really tell, from the

logs, where you had oil. What you can do, because you also run the resistivity log, and oil is essentially a nonconductor compared to water, you can by a combination of those logs make a pretty good guess as to whether you have oil, if you have a combination of high porosity and high resistivity.

The gamma ray log, which we also used, will show you that you are not in a shale, because shale has higher radioactivity than a limestone, for example. And, the neutron log will show you that you have a rock that has a lot of holes in it — high porosity. The resistivity log will show that there is something in those holes other than just water. Basically, the induction log was better for that than a resistivity log. When you had the induction log you could do pretty well in wells that you knew something about. If you were in an area, and you knew something about the area because you had taken cores, and had measured the porosity, you could do pretty well.

Joe Hearst has been a central figure in the development of logging tools and methods at the Livermore Laboratory. The initial impetus, at the Test Site, for ways of determining the various in-situ properties of different materials encountered in drill holes came from the Plowshare program. In particular, the use of nuclear explosives to form craters of different sizes was envisaged as a means for creating harbors and canals. To predict the yield of the explosive required at what depth in a particular formation to produce the desired result required both the development of computer codes, and a means of obtaining the the properties of the rocks involved as input data for those codes. The oil companies had developed various tools to measure properties associated with the presence of oil when an exploratory hole seeking an oil-bearing formation was drilled, and it was from this base of experience that the development of instruments that could be used in the nuclear programs came.

Hearst: When I was working on the Plowshare program I had to calculate an event; I think it was Danny Boy, a cratering shot. One of the things you had to put in the code as one of the rock properties was the sound speed. Well, I discovered when I was given the sound speed from laboratory measurements, the calculated signal arrived at the surface in about half the time it did in real life.

I thought perhaps something was wrong with the numbers I'd been given. So, I decided I should go to the field and measure the sound speed. I went to the field, and I reinvented what is known as the uphole survey. What you do is you make a noise like an explosion, underground, and you time the signal coming to the surface. I also reinvented the refraction survey. There you hit a hammer on the ground and listen to the signal coming back.

Of course, refraction surveys had been standard for years, but I didn't know that. I invented it again out of ignorance. Then I decided maybe I didn't believe the density numbers either, and I believe that I reinvented the density logging tool, or re-conceived it, using gamma ray reflection, or gamma ray back-scattering. That's how I got into the logging business. I had all these numbers that I didn't believe, that didn't work, and so I started reinventing some of these things. And then I discovered, first of all, that there was a logging group at the Test Site, which I hadn't known about. And, secondly, that there was a logging industry, but I didn't know that either, at the time.

Then, one day - - I was in a ride pool with Don Rawson, who was at the time the head of geology in K Division - - Don said, "Joe, how would you like to take charge of logging for K Division, and be in charge of the logging effort in Nevada?" I almost said, "What's logging?"

But that's how I got into it. I needed the data. I don't remember when I found out about the logging group in Nevada, but at first I didn't even know about them. They were developing seismic measurements, and improving on them, and they were using commercial logging companies, which I had never heard of, like Birdwell, and Wellex. I was reinventing all this stuff in a vacuum.

Carothers: One of the things researchers are supposed to do is look at the literature, Joe.

Hearst: I didn't even know there was a literature.

The logging people that I knew in Nevada were in support of Plowshare, because the Plowshare people were interested in breaking up the rocks, and when or where the signal came to the surface. They were doing cratering shots, and they were worried about damage, and earthquakes. The Panama Canal effort was what was funding all this, so that was what the Nevada group was working on.

Rambo: In the Nevada group we were trying to develop new logging tools. And, we were evaluating the commercially available tools from Birdwell, and I think Wellex. We weren't very happy with what we were seeing, because those tools were all borrowed from the oil patch, and those people were not interested in the same things we were, at the time. We were interested more in physical properties than in blips on an electric log. On the cratering events we were able to drill a lot of holes and pull out samples, and make measurements on those, and get material properties in that way.

So, during this time we were developing logging tools to measure these unknowns, and density was one of the big items we were looking at. We had just gotten one of the old 1620 IBM computers, and that was a miracle machine at that time. I learned all about programming that. We bought the second version of the Rand Tablet, which was a digitizing device, which had etched lines on it. I had a stylus, and you could digitize logs with this electronic pencil. For every point you got, it would punch a number in a piece of paper tape. So, I wrote programs to do this translation, and I wrote the programs for the IBM 1620. We would put on a reel of paper tape that was maybe about eight to ten inches in diameter, turn it on just before we went home, and this thing would run all night long digitizing a density log. Then we'd process it in a Cal Comp plotter, which was an old version of a plotter, and convert what was kilocounts at one time to density, which was the real thing.

Carothers: What was the source of the input data? What kind of an instrument were you using?

Rambo: What it was for the density log was a cobalt 60 source. The gammas would backscatter from the formation after the source was held up against the wall of a hole at various locations. It was a gamma-gamma density; the backscatter was the indication of the density. I forget whether there was two or three feet of separation between the sensor and receiver. You couldn't get the receiver too far away, or you wouldn't sense anything; if it was too close you'd only sense the source. You then had to go through various calibrations to get the density.

We were also dealing at that time with a firm called Birdwell, which was big in the logging field, and which did the Test Site logging. We were trying to do our processing in-house, and to

develop a whole logging program. That eventually became the modern logging programming we now have. Those were the early days of developing those sort of things.

Carothers: Why didn't the Lab use the commercial tools? Why build up an in-house logging capability? There were companies that had logged hundreds of miles of holes.

Hearst: Because the conditions were different. The commercial tools were developed for deep, small holes that would be fluid-filled because they were below the water table. We were logging in emplacement holes, so first of all, we were logging above the water table, and almost all commercial tools were, and are, designed to work in water-filled holes, or liquid-filled holes.

When we were trying to do seismic surveys, at first we tried to couple them with water. We'd drill an eight-inch hole, dump a truckload of water into it, and it would flush like a toilet. We learned you can't do that. Even for seismic surveys we had to develop a new method of stemming with sand and things like that, because the commercial methods wouldn't work in the holes we had.

So, we needed methods for a dry hole, and a big hole. First a dry hole. For Plowshare we didn't need big hole tools, we needed dry hole tools. And so we developed dry hole methods. We also needed higher accuracy for many of these things than was available from the commercial tools at the time. First of all, velocity; our first paper was on an uphole survey, which was a standard procedure, which was an order of magnitude higher accuracy than industry used. For the short distances which were used on the cratering shots, we needed the higher accuracy. Or, at least we thought we did. For density, we had rough, dry holes, and we had to develop tools that would work in them.

The first thing we worked on was a lock-in geophone, to get better accuracy in measuring velocities. I didn't work on that much; that was done by Dick Carlson and the people in Nevada. We used that for downhole surveys rather than uphole. The geophones would lock into the borehole, and measure the travel time from an HE shot on the surface. We were measuring the sound speed for the code calculations. One of the reasons for that is that laboratory measurements on samples, especially for sound speed, have nothing to do with field measurements.

For laboratory measurements you take a core sample, a nice solid core which doesn't have any cracks in it, and which doesn't fall apart. In the field things aren't like that. I remember the Sulky event, where you could look down the hole and see cracks you could put your arm into. And of course, the acoustic signal has to come through that broken up, fractured material. The Test Site is very nasty that way. And, the fractures at the Test Site are not filled with water — they're filled with air, which gives a tremendous attenuation for acoustic signals. That's why we had to make those measurements with the geophones. There is that factor of two that I mentioned, between the laboratory and field measurements. People still fall into that trap sometimes.

Don Larsen developed some very thin velocity gauges, and we then could look at the velocity history of rock samples in the lab, using very small HE charges. We also worked a lot on stress gauges, and we're still working on them. Checking calculations with actual measurements is a lifetime program.

Carothers: The Buggy event, which used five simultaneous detonations, was a great success, in that it made a real ditch. It demonstrated that you could actually calculate these row charge effects.

Hearst: Before Buggy was Palanquin, which demonstrated you couldn't calculate everything.

Carothers: The only real work that was being done was being done for Plowshare, and basically being done for the cratering shots. Now, the Plowshare people had various other ideas, such gas stimulation. Did you do any work on those kind of things?

Hearst: Logging was logging, but for most of the other things commercial logs could be used. Another thing was verification. There was Salmon, in Mississippi, and Dick Carlson especially did a lot of logging work on Salmon. In the second place, on Salmon, if you recall, the ground shock caused much more damage to buildings than people had anticipated. It was a real surprise. We then started bringing seismic people into our group, and we started getting involved in what we would now call verification work. We then did logging for that, as well as calculations and lab experiments.

Carothers: On device development shots, as differentiated from Plowshare events, were there samples or logs taken?

Hearst: I don't think the test program people did much of that. We didn't get involved with the test program until Baneberry. Plowshare was vanishing, and the Lab also had the first big reduction-in-force. Just about that time, very providentially, along came Baneberry, and that put us back in business, logging for the test program. I was still doing calculations at that time.

Carothers: What logs could you do at that time?

Hearst: We could do almost anything that we can do now. There were very sophisticated acoustic things, which weren't useful at the Test Site, but as soon as we started working on verification, then we could use the conventional stuff for things like Salmon and Sterling, and so on. I recall doing lots of seismic surveys on Pahute Mesa events, and I think it was pre-Baneberry.

Now, the quality wasn't as good. The measurements weren't as sophisticated as they are today, but the techniques were available in the sixties. There's very little new that has come along since then. The only thing, really, is borehole gravity, which was conceived in the fifties, but not used in the field until later. And it's still not very commercial. You could, in the sixties, do acoustic, density, electrical logs.

The epithermal neutron log was a commercial tool; we just used it. We did invent one density logging tool, which was for Plowshare, and we subsequently stopped using it. It was a rugosity insensitive density logging tool. When we got into bigger holes we stopped using it, and started using commercial tools, and calibrating them, and living with the rugosity effects.

So, it was all commercial tools, and we had to make them work. That was the switch from Plowshare to verification and test. We started making commercial tools work. The only tool we developed that is still in use is the dry-hole acoustic log. The other tools could be made to work; basically, they had to be calibrated for dry holes. The thing that I did with the epithermal neutron tool was, after many years of effort, and learning to run Monte Carlo codes, and things like that, was to convince management to build me a calibrator for dry holes. It was boxes of carefully mixed materials, and those boxes are expensive.

Carothers: Boxes with dirt in them?

Hearst: They had to be big boxes, because the neutrons go long distances. And the dirt had to be carefully designed to give you what you wanted, and to give it uniformly and accurately. Actually, that calibrator didn't work very well.

What we ended up doing, because of engineering and money constraints, was, we made cells a foot square and six feet high so we could put them together to make a rectangular parallelepiped, to be technical, which was six feet high, by three feet by five feet. Basically it was a slab.

We put carefully measured amounts of material in each one of the boxes, and shook them to get it uniform. We calculated what materials we needed, and mixed them. We used sand, and marbles - - we actually had a million marbles, a whole truck load of marbles - - and aluminum oxide. Among other things we had to control the density, and so we had to make mixes of materials of different sizes to get it dense enough to do what we wanted. We used marbles, and sand to get higher densities. And aluminum oxide, to get even higher densities. To a neutron, aluminum looks very much like silicon. Then we poured in water, and we also had some activated alumina, which could soak up some water.

We did all that, and it was still not well done. Part of the problem was that the mixes were made here, and they were sealed in these aluminum cans, and then they were trucked over the Sierra. That made the cans expand, because of the low pressure as they went over the mountains. And so, when the cans got to Nevada they were bulging. Consequently, they never fit well together.

Carothers: All you had to do was to put a little pinhole in them.

Hearst: They didn't think of it. Remember, they were sealed to keep the water in there. There were actually reinforcing rods in them, but that didn't work well enough, and so they bulged. When you put them together and squeezed as hard as you could, they still weren't flat; they had gaps, and bumps, and wiggles. And so, they were never satisfactory. But it took a lot of persuasion to get management to let me build that facility, and that was what I contributed — the calibration facility.

There were problems with our first calibrations. We did two procedures. Our main effort, which was to simulate a big hole, was this three by five foot wall. And, we took one box out of the middle of the fifteen boxes to mock up a small hole; that hole, of course, was square.

Those results are quite different from those in the small cylindrical hole we now have in our in our new calibrator. A logging tool is cylindrical, and is up against a wall that is either cylindrical or flat, and the major effect is right in the front of the tool. One of the things we discovered was that even in a 72-inch hole there is a hole-size effect on a neutron log. That's why we had to build this ENS — the Epithermal Neutron Special, instead of the ENP — the Epithermal Neutron Porosity — to take care of the hole-size effect. The Geologic Survey people are unhappy because we almost always get higher values with the ENS.

The ENS was special because it had more shielding. We put that bigger shielding on to compensate because we were up against the slab. The slab was to simulate the big hole — infinite radius. But because it wasn't right, wasn't really effectively infinite, we had to put in this extra shielding. We also pulled out one of the boxes in the middle to simulate a small hole. Now that we've built our new system, we've found that neither of those simulations is particularly good.

Our new calibrator is two cylinders, fifteen feet in diameter, with a six foot diameter hole in the middle. They are vertical cylinders, six or eight feet high, and somewhere between twelve and fifteen feet outside diameter, with a six foot diameter hole in the middle. They are made of pie-shaped wedges; each of the two cylinders has six cells filled with the material. It cost us like a quarter of a million dollars to fill them — REECO prices. You have to fill them very, very carefully, and we did a lot of studying of the mixing of solids. We even sent our engineer to a meeting on the subject, in Southern California. We came to the conclusion that we could not make uniform mixes of the solids we wanted to mix. The technology does not exist to make good uniform mixes of solids of different sizes, or even of the same size.

Carothers: My mother can do that when she makes sticky buns with raisins in them. She gets a pretty uniform mix.

Hearst: Well, probably on that scale you can do it. But we concluded that we just could not guarantee a uniform mix, with dry particulates. So what we did was, we made layers. Each cell has fifteen layers, and we know what's in each layer, so we know that at least on that scale the mix is uniform. We use fifteen layers of the same mix. The layers may not each be completely homogeneous, but the neutrons see more than one layer.

The layers are all the same recipe, mixed in a concrete mixer. The problem is that the concrete mixer may not necessarily get things uniform, but it makes it uniform on the scale that the neutrons see. We put these mixes in place, and then vibrated the cells to get the right density, because we had to have a known density as well as a known water content. So, we vibrated these huge cells each time we put in a layer, to settle it to get the right density. We did all sorts of experiments on that sort of thing. We did experiments where we would pour stuff into a container after we mixed it, then shake it to settle it to get the right density, and it would separate.

Carothers: Well, sure. The heavy things fall down to the bottom. It's the shaking that's doing it.

Hearst: Yes, but otherwise you can't get the right density. So, it's probably still not uniform. Afterwards we made all kinds of measurements with logging tools, and other things. I've got a book an inch and a half thick describing these mixes. How to mix solids is an unsolved problem, and there are conferences on the subject. It's important to places like cookie companies, and places like that.

I think the solution is that you put in liquid, and then you can mix it. If you make a slurry you can mix it, apparently. But as long as it's a dry solid, you can't. That seems to be the story. This was a major problem that we spent a lot of time and money on.

There are calibration facilities at places like Bendix, in Grand Junction, where they tried to make mixes of radioactive concrete to calibrate gamma ray logs. It took them years to discover that they got it wrong. There are American Petroleum Institute test pits in Houston that are not right, because they couldn't mix it well; they're not uniform. Mixes just don't do very well, and these test pits where they tried to mix radioactive concrete don't work. And so, when we built our gamma ray calibrator we used six foot high, three foot diameter pieces of granite.

Commercial tools are calibrated in American Petroleum test beds in Houston, in saturated limestone, and things like that. They are small, water-filled holes, and we have dry big holes, and dry small holes. And so, we had to simulate that. And also, we have a much bigger range of water contents and densities. One thing that I did invent was the idea that you had to compensate the neutron log for density. That's not a problem in the oil industry, because any time there's a density change there is also a water content change, because everything is saturated. The holes they log are deep, and also they're in places where there is a shallow water table.

So, we had to develop calibrations to account for that, and we did. We developed ways of correcting for all those features they don't worry about in industry. We didn't have to develop tools, we just had to develop calibrations and corrections; ways to use those tools. That saved lots of effort.

Carothers: This new calibrator you have is bigger, better, and so forth compared to the old square cells. Presumably it was more expensive also. How was the management persuaded to spend that quarter of a million dollars?

Hearst: Actually, it ended up being more expensive than that. But, partly it was the DOE management that spent it. I think we succeeded because there is still the tradition of getting better data, and because there was money in the budget, the DOE budget, to do these things.

When I was working with Frank Morrison I was in charge of research for the containment program. I had lunch with Frank one day at the bowling alley at the Test Site. I said, "Frank, we don't need any more research in the containment program. We're doing our job, and we're not hurting. We have a budget, and there lot's of interesting things we could do that would give us more accurate measurements — nicer, warmer fuzzy feelings — but they don't improve the containment of the event one bit."

Carothers: I was wondering if there was something new that had occurred; if for some reason better numbers were needed. For instance, perhaps the verification folks needed better numbers.

Hearst: No. There was available money, and we could show the things that were wrong with the existing calibrator. So, we have better data now. This business of the correction for the bound water, that's an improvement in the correctness of the numbers, even though it's not very important.

Carothers: Your epithermal neutron log doesn't really measure water; it measures the hydrogen that makes up the water. And you assume that all the hydrogen is associated with water.

Hearst: That's correct.

Carothers: Well, your measurements seem to bother the geologists, because you measure not only the free water, but the bound water. As far as I know, they measure the free water. They never measure the bound water.

Hearst: That's correct, but they could if they tried. They measure the water in samples, and if you heat the samples hot enough the bound water will come off.

Carothers: But the problem is that all the data in the data banks that we have that relate to the Test Site only report the free water. Now you're reporting free water and bound water, and so there's always more than there is reported in the data bank.

Hearst: Not always. Only in places where there is water that is bound, and that's in zeolitic materials, as far as I know. Or clays, or things that have some clay in them. But yes, the neutron log seems to give higher values than the sample data, and generally it should. That should only happen where there's bound water. But, we have a method for correcting for bound water. We can measure it with nuclear magnetic resonance — from samples only, which is a little bit cheating, as a reviewer from a journal pointed out to me. It's cheating to interpolate between samples. There is nuclear magnetic resonance logging, but it's never been successful. I recently read a proposal for something that might work, but they aren't there yet. That tool has also existed since the sixties, but it's never been very good, and it certainly wouldn't work in big holes.

But there is a problem there, and I'm not sure the solution is complete; that is, that we can explain away all the differences between the sample measurements and the log measurements. But,

we think we understand most of it, and yes, the data bank does have just the free water, and we can now compare free water measurements if we wish.

Carothers: But you do that by cheating a little bit.

Hearst: Yes. Incidently, the epithermal neutron log was a Birdwell tool. It was abandoned by the industry, because it didn't get enough signal, until very recently. Now epithermal neutron logs are coming back into fashion in industry, and they are using them in creative ways. Maybe it's just more recognition of neutron poisons, which is the reason we used epithermal neutrons — the fact that there are things out there that absorb thermal neutrons. And maybe it's that industry is getting into more materials where they care about it. But also they've found constructive ways of using the tool.

The problem, with the neutron log in particular, and the density log, is that our calibration at zero gap, and even at a small gap, is excellent. But the correction for gap, when we measure some gap, is still very poor, because we're doing that badly, somehow. I don't know why. I think it's poor because I'm measuring the gap at some place other than the spot where I'm making the neutron measurement. The hole is rough, and we're making the measurement a foot away from the source, because the gap measuring device is somewhere else on the tool. That isn't right, but we don't know how to do it otherwise. We're probably getting the water content wrong; we're probably overestimating it in many cases.

Carothers: There are members of the Panel who have said that they really don't care about all those numbers, and the geology, unless there is something unusual about it. For instance, they, and I, feel that the histograms of material properties that are presented are meaningless, because there have been so many measurements taken that what is at the Test Site has been bracketed, and what you measure always falls within those limits.

Hearst: Yes. Of course. Norm Burkhard gave a paper at the containment symposium before last about the rockpile concept — you should just assume these numbers. I think that's quite reasonable.

Carothers: In 1978 there was a session of the Panel called to consider the question posed by Ink Gates, the then NVO Manager, as to whether there were ways to reduce the containment related costs. Without compromising the probability of successful containment, of course.

One of the suggestions that was made in 1978 was that Livermore should regard a large section of the areas they used in Yucca Flat as LANL regards the Sandpile — call it the gravel pile, or whatever. There is plenty of data to do that, and when a new hole is drilled, just extrapolate in the data from adjacent holes. The Livermore Laboratory, for whatever reason, has not chosen to do that.

Hearst: In 20ax, the containment scientist wanted to do that, and suggested that we look at the 20ax data and compare them to the data from nearby holes. We did actually try that for that event. Well, it turned out that in many of the lithologic units the error bars for the measured 20ax data lay outside the error bars for the nearby data. They didn't agree. But, so what?

Carothers: I don't believe, these days, that the CEP is the organization that drives the data collection. You've got people who do calculations, and to do calculations you have to have numbers, and if you don't have numbers people criticize you for having so many knobs to twiddle in your code that the results are meaningless. And so, you have to have numbers. And to get the numbers you either have to have samples, or logging tools. Samples are expensive.

Hearst: And they're not very good anyhow.

Carothers: So, we have to have logging tools, so we have to have people to do that.

Hearst: I consider it a ritual, but I earn my living at it, and it's interesting work. As long as you're going to do it you might as well try to do it well. Although, we wouldn't use the tools we are using if we were starting now; we'd use higher technology. We're still using 1960's technology in much of our stuff at the Test Site.

Carothers: With a logging tool there are two things you can do, and presumably you could do them both at once. One is, you might not care what the absolute value is; you might only care about where and how the value changes. The other is that you really want to know what the absolute value is. How do you deal with that?

Hearst: Well, in the first place, you should actually design the tool differently for the two different uses. For almost any tool, the larger the source-detector spacing the further you're averaging over, so the more accurate number you're going to get, but the less definition you're going to get of a boundary. There is a basic problem, before you start a logging program, of deciding what you want, and why. There's always a balance between accuracy of the value and accuracy of the depth, which compete, and cost.

For the Soviet test site a U.S. committee got together and decided what logs they wanted to verify Soviet tests. There was this list of logs that were wanted, and we made decisions about the necessary logging tools to send over to Russia. This commitee would say, "We want these logs." And maybe, "We want them to this accuracy," but usually not. But not, "We want them because." The cortex people wanted them for one thing, the Geological Survey wanted them for another thing. The first time people went over there they spent a great deal of money, and effort, and time getting these data. And, nobody has ever used the data, as far as I can tell.

Carothers: Why do you think that is?

Hearst: I don't know. Probably they didn't think it through. Dick Carlson is the guy who went to Russia to do it, and the last time I talked to him nobody had ever made any use of his work. And he does a very good job of getting good data.

It's very difficult to persuade people, including the CEP, to think hard about what numbers they want, to what accuracy, and why. You can say, "I want the density to two percent accuracy over the range." Then I come back and say, "Why? What are you going to do with those numbers? That's very expensive. If you really mean you want that accuracy, I probably would want to run three different tools. But, you probably don't really mean that, because you're not going to use those numbers that accurately."

Each person, each organization will say, "I need this measurement," and they will always specify some accuracy which is good as they could possibly use, without ever thinking how difficult they make it to get the data, and how much more costly it is, and how it's competing with someone else's desires. You really have to think about what you're going to do with those numbers.

Let me say, the CEP doesn't need accurate numbers. They're just talking about how their grandfather did it, and 10% accuracy would be wonderful for them. We put big error bars on the data we present, and nobody cares.

Carothers: Well, in defense of the CEP, I constituted a subcommittee of the CEP a number of years ago, chaired by Bill Twenhofel. It was called the Data Needs Subcommittee. That subcommittee came back and said the CEP didn't need various kinds of data. The Laboratories paid no attention at all, and continued to get those data anyway. Why? Well, they've got guys like Joe Hearst, and John Rambo, and Fred App who are calculating various things, and they want numbers.

Hearst: That's right. Calculators need numbers. But I think we are getting data that are too accurate. Or too precise — they are probably not that accurate. I think we are wasting time with too many decimal places which nobody uses.

Carothers: You now have a tool that measures the hydrogen in the rock, and you assume all the hydrogen is there as water, so let's say you measure the water in the rock. Why did you develop a tool to do that?

Hearst: We didn't. We hired it. That's the tool that was available. But also, one of the important parameters for containment is the total water. That's one of the key parameters, and if you need to know numbers at all, that's one of the numbers you need to know. When we first started we looked at the available methods of measuring water content, and decided this was the best. But we didn't want to measure only free water. We'll continue to report total water and these other parameters, the porosity and saturation, which the whole world tries to measure, by the way. The objective of the industry in running all these logging tools is to measure

porosity and saturation. That's what the tools are built for; that's what they were invented for. All those methods assume that the formation is saturated with some conductive liquid.

We looked at density tools, and we just demonstrated that these tools are worthless in the seventeen-inch holes for the groundwater characterization program. We've shown that they're not good. For 20ax we had problems with a density tool in a seventeen-inch hole, and we had to build a new calibrator for it. This was blocks of various metals like aluminum and magnesium. That's the way you calibrate a density tool, and that's one reason density tools are much easier to calibrate. We discovered that these automatic, two-receiver compensated density tools get wrong answers if they are tilted ever so slightly in a seventeen-inch hole. They work all right in an eight-inch hole, which is what they were designed for, but they have to be recalibrated for the bigger holes. That's still being worked, but we have now demonstrated what we surmised on 20ax; the logs are coming out wrong.

Carothers: The first log I see at the CEP is the density log. You measure that with a gamma ray logging tool. What happened to the dry-hole acoustic log?

Hearst: It is used, and in fact you see it, but you just don't pay attention. It's the DHAL, and it's shown every time we show logs. It's the acoustic velocity. First comes the caliper log, and then comes the dry hole acoustic. That's something we invented ourselves, because there were none in the world. We needed it for Plowshare at the time, because we had no way of measuring acoustic velocity except by seismic surveys.

We went to Don Rawson's back yard one day, with a couple of acoustic transducers. Dick Carlson and I had thought of attaching cones to transducers that we had bought. We put them on Rawson's fireplace, and sure enough, we got a signal through the fireplace, horizontally. Then we tried it on trees, as well. We got acoustic signals, and we had invented a dry-hole acoustic log. The reason nobody in the world uses it is because it's not continuous. A logging tool to be useful in the industry, where drilling costs are immense, must run continuously as you pull it up the hole. There are now a couple of logs that do that, but there weren't at the time.

Carothers: Why isn't your acoustic log continuous?

Hearst: Because it has to dig into the wall of the hole. You have to push it hard up against the wall, and push these points into the wall. That's why nobody else ever invented it. It wasn't that we were these brilliant geniuses; it was just that nobody else could use it in their business.

Then we discovered a problem with it, which is why we call it a relative measurement. In a small hole it agrees quite well with seismic measurements of velocity. In a big hole it usually gives us velocities that are too low. The reason for this, apparently, is that the material near the wall of a big hole, or any hole, is broken up by the drilling process. In the case of a big hole the depth to which it is broken up is about the same as the depth to which the acoustic signal goes, so we're just measuring the region which is broken up by the drilling process.

The least time path is what you measure. So, the higher velocity material gives you less time, but if you have to go through a large amount of low velocity material to get to the high velocity material, that doesn't work. The way we proved all this was to build a tool with two receivers, which is the standard way done in the industry. If we used the measurement between the last two receivers, it was faster than between the source and one receiver. That's because the passage through the broken up material is cancelled out. Actually, in the industry now they may use up to twenty receivers.

So, we get the acoustic velocity, and then the density, which we get from the gamma log, and then we show the acoustic impedance, which is the product. And that's probably why we still show that log - - to show the impedance mismatches.

For the water content we use the epithermal neutron log, and correct for gap between the neutron sonde and the wall of the hole. Los Alamos does not.

Carothers: So you ought to get different answers, in the same hole.

Hearst: Not only that, but if you look at the calibration curves, they're different.

And, we also show the CO2 content, which is still measured from samples. And we show the clay content, which is done with x-rays, occasionally.

Carothers: You also show the resistivity log. How do you do that?

Hearst: Well, resistivity is the standard log in the oil industry. That was the first log invented, and it was the only thing available for many years. You put a source of current at the surface, and you look at the voltages generated by it, downhole. Nowadays I think some of them have a source of current and voltage detectors in the hole. Some of them use induction instead, because then there's no contact problem.

Again, it's very difficult to do in big holes. In the big, dry holes none of the standard methods work. We did one time develop an induction tool — huge coils for a big hole — but we never made it standard. What we have for our dry hole resistivity log, which is the only thing we can use in big, dry holes, is a bunch of wheels — padded cloth wheels — saturated with copper sulphate solution. They roll up the wall of the hole, and they're saturated with conductive solution. They make contact with the wall of the hole. The basic problem is that sometimes they make good contact, and sometimes they make bad contact as they roll up the hole, and so you get indifferent results. That's our attempt at duplicating the standard things that are used in liquid filled holes. The current source is in one of the wheels, and you measure the voltage between the two wheels.

Carothers: Why is it useful for the CEP?

Hearst: Well, clay is conductive because it's has water in it, and it's got all kinds of ions in it. So, clay is more conductive than alluvium or tuff. Supposedly a resistivity log tells the CEP if there is clay, but there have been a number of studies done, and none of them link resistivity to clay. There have been a number of papers which show there is really no connection. Nevertheless, since we care very much about clay because of Baneberry, it is traditional to present a resistivity log, and to worry very much if there is a very low resistivity somewhere. Then you have to go get a sample, despite the fact that Gayle Palawski has written a couple of papers showing the lack of connection between log resistivity and clay content.

The log resistivity is proportional to the conductivity of the rock, which depends, among other things, on the amount of water, the amount of clay, and the kind of rock. But it depends even more,

I believe, on the amount of contact between these wheels and the wall of the hole. There are many brand names of these resistivity, or electric, or E logs. And there are many configurations of the electrodes, depending on who does it.

Carothers: How about the seismic velocity. How is that measured?

Hearst: I got into that when I first got into logging. The way it's measured now is with an air gun. There has been a lot of work on that, and I don't know too much about how it's done today. There are problems with getting good contact between the air gun and the rock. The air gun puts a big pulse into the ground, at the surface, and you have detectors clamped into the hole, downhole, and they sense the signal. So, you're measuring the entire depth of the hole, down to the detector.

Carothers: So if I want to know the velocity in a particular layer I have to subtract out all the others above it. It sounds as though the deeper I go the worse the measurement would get.

Hearst: Well, this is an acoustic signal, not going through liquid, and you measure the arrival time of this acoustic signal, The signal has to be some amplitude that you can see. Therefore, the arrival time really depends on the contact between the detector and the wall. You look at the analog trace, and you pick the arrival time. If you have less sensitivity you will see the signal later, because the signal is not a step function; it rises from zero to full value in some amount of time, and when you can see the arrival depends on the sensitivity of the detector. It's a smoothly rising signal, and you pick the time when you can see it. That's the trouble with automatic picking procedures; they depend on the amplitude.

So, from all this, the measured velocity depends on the contact between the detector and the wall. Again, this is a problem with our dry holes, which is not much of a problem in industry, where they have liquids and the contact doesn't matter.

Carothers: There are other logs, one of which is presented to the CEP as the gravimeter. Tell me about that.

Hearst: A gravimeter is a device that measures gravity, and it's used routinely in the industry to make subsurface maps. I got interested in borehole gravity when it was first being thought about in the 1950's. The first paper was published in 1950, and I was one of the first people to use it for anything.

The tool measures gravity in different places, and you attribute variations to changes that are underground. The idea of measuring rock density with borehole gravity was very intriguing to me, and as soon as a borehole gravity meter became available I started using one to measure density that way. You put the tool downhole, and measure at various stations at various depths, and you can calculate the density of uniform slabs, if you assume the world is made up of uniform slabs.

That's exceedingly uninteresting, but if you measure the difference between the gravity measurements and the density log, and you believe them both, you can infer things about the structure of the earth, underground. And that's what it's used for. There's now a fair industry; I was at a large meeting in Chicago last year where people were talking about improving the measurements. There were maybe twenty or thirty experts there.

Carothers: The changes you are looking for in the gravitational field must be very small, and so the instrument must be very sensitive.

Hearst: It is a very sensitive instrument. One of the questions raised at this meeting was, "Do we need greater sensitivity?" The conclusion was that the instrument is sensitive enough to do the job. Basically, it has a mass on an arm, and it measures the angle of the arm as the field changes. That's the physics principle; the trick is to get it to work in real life. People have done this, and there's one company that does it well. The interesting conclusion of that meeting was that what they wanted was to make the measurement faster, and make the equipment more rugged and more reliable. But they didn't need more sensitivity.

Carothers: How do you infer things about the structure?

Hearst: You make a calculational model of the structure, calculate what the gravity would be with that model, from that calculate the difference in gravity that you would see at different depths, and compare that to what you observe. There are, of

course, infinitely many structures that would give you the same result, and all you can do is to use the measurements to choose between proposed models.

It has apparently worked very well in the oil industry to find oil some distance from a hole. Again, their density logs are much more accurate than ours, because they satisfy all the assumptions — good contact with the hole, no gap, and it's a small hole. They can use very small density differences to infer useful things. We can't, because our measurements aren't that good because of our big, rough holes. But that's what we use it for.

There is also a thing called a gravity gradient measurement, where you're measuring the change in gravity with depth. You can build instruments which measure the gradient, but it turns out that gravity measurements are sensitive to one over R squared of the mass. The gradient measurement is sensitive to one over R cubed, and the gradiometer is so sensitive to changes in the hole configuration and things like that, that you don't buy anything by building a gradiometer.

Carothers: What's the free air gradient that is always measured when you're doing gravity measurements?

Hearst: If you calculate the density, using a gravity meter, there is a constant term, an additive constant, that is in the formula for the gravimetric density. It is the change in gravity with depth which is caused by the fact that you're getting closer to the center of the earth. It's called the free air gradient because originally it was the change in gravity measured as you got closer to the surface of the earth, in the air. When we started working with this, we decided we ought to measure this free air gradient by making gravity measurements on a tower. Well, a lot of people in the field said that was a bad way of doing it, because that measurement is very sensitive to things that are close to the surface. In fact, that measurement is now used to look for tunnels and things like that which are near the surface.

You get a much better measurement of the free air gradient by measuring the gravity at the surface over a wide area, and doing a transformation to calculate the free air gradient. Norm and I finally got persuaded by a number of publications by other people that is indeed the correct way to do it. We were not doing it right, and so we now do it that way. We no longer measure it directly. If you're

making measurements near the surface, yes, you should measure it above the surface. But when you're measuring at depth, as we are, in general you're better off by calculating it from a number of surface measurements.

Incidently, one of the things you have to correct for is tide, and the first time I asked for tide tables for the Nevada Test Site people thought I was crazy. If you set a gravity meter out on the ground, it changes with time, because the sun and the moon affect it. There are earth tides, and that's what you have to correct for. That's automatic now.

The seismic survey business is another huge industry, and it's been a very successful one. The surveys show you where reflecting layers are, below the surface. I have never been able to interpret the measurements with any comfort. I think it requires a great deal of imagination to interpret those surveys, but people do it successfully, and get paid very well for it. It is a universally used procedure, and that's how all this information we get about the structure of the earth comes to us.

It is another technique which is standard in the oil industry, but which is exceedingly difficult to use at the Test Site. The highly porous rocks near the surface are highly absorbing for the acoustic signals. We used to hear stories of how some world expert in seismic measurements would come to the Test Site and go out with our technician. The expert would start setting off small explosions and get no signal. Finally our technician would say, "You have to use two sticks of dynamite instead of one detonator to get a signal here." For many years companies would come out and produce thick reports about why they failed..

Norm Burkhard got his Morrison Award because he was the first person to do a successful seismic survey at the Test Site. He used a procedure which I don't quite understand, where he used a fairly small charge. He got it to work; it had to do with using the right source-detector spacing, and the right type of charge, and all sorts of things like that, which he said he learned in school.

It is difficult technique to use at the Test Site, but we do have seismic surveys now, and they are used usually to look at cross sections, to interpret them. A number of them have been done, but nothing like the number that have been done in the oil patch. They're quite expensive, but you can call in a crew, and they'll do

it. My problem is in interpreting them, but people do it. You can see things, but figuring out what they mean is another story. Now again, there's a huge amount of software that's been developed to improve these things, and there's all kinds of difficulties converting time, which is what you measure, to distance, which is what you want.

By the way, another way people in the industry measure porosity is velocity. These velocity logs in industry, in the right circumstances, get porosity from velocity, if you make the right assumptions. In a clean, water filled sandstone, all you need is the velocity. As far as I know, every formula that's used assumes clean, water filled sandstone. So, a velocity log is called a porosity tool, and that's what it was developed for. There was recently an issue of one of the journals published on the use of velocity logs to infer porosity and permeability and things like that in rocks like granite. Now people are starting to measure fractures with velocity. You can do all kinds of neat things with acoustic signals, in a water filled hole. You can actually make a picture of the wall of the hole and look at the fractures, and things like that. You can even see some depth into the wall, and see fractures.

Carothers: One of the things people on the Panel, from time to time, ask about is the stress state of the rock, and about the shear strength. What can be done there?

Hearst: We are, in fact, developing a method of measuring strength, compressional strength. I have spent a fair amount of time, from time to time, trying to figure out how to do that downhole. I have not yet found a method we could field. There are methods that I have looked at that are used, even some that are done in the tunnels, that are very difficult to do remotely. For example, putting two pins in the wall of the hole, measuring the distance between them very accurately somehow, then taking a saw and making a slot in the wall between those two pins, and then measuring the distance between them again. We've spent some money looking at things like that. One of the major problems is that the borehole causes a major change to the in-situ stress, and so whatever you measure in the wall of the borehole may not have a great deal to do with what's out in the rock. But we've looked at a number of methods for that.

Carothers: I think of it because one of the things people are touting these days, maybe correctly, maybe not, is the following argument. There isn't, necessarily, any residual stress field around the cavity. There would be in a uniform medium, or world, but we don't shoot shots in such a world. Blocks move, here and there, and what really contains shots is hydrofractures, which drive out into the rock a short distance, dump a lot of steam, cool the cavity down, and that's it.

Hearst: That's quite possible. Now, we have worked on measuring shock induced stress. In fact, we just had a failure on the Bristol event, where we got numbers that were mostly strain. It is exceedingly difficult to measure shock induced stress. Part of the problem is that the shock damages the gauges. The biggest problem is that it's very easy to measure a stress in the stress transducer, but relating that to the stress in the rock is very difficult indeed. If you could, in fact, put the transducer in direct, intimate contact with the rock, you could do it. But you can't. You have to drill a hole, you have to put the transducer in a package, you have to put the package is some kind of stemming material, and all of that makes a big difference in the measurement. We've worked quite hard on that. We've developed procedures for reducing the data, and they haven't worked very well either.

Carothers: How about measurements where you could say, "Yes, there is a residual stress field, because before the shot I measured the stress in this region, and thirty seconds after the shot, here's what that stress field was, and it was different."

Hearst: We've had some little hints of that in these measurements, but one of the major problems is that every stress transducer you can build is also affected by strain. You can't distinguish between stress and strain easily, and so we have not been able to prove that what we have seen is actually residual stress. We have seen signals that have stayed up for long periods of time, but we can't prove what they are.

Carothers: As contrasted to post-shot stress, there is a lot of interest, by people who are interested in the hydrofracing model, in in-situ stress.

Hearst: Attempts have been made, and papers have been published, even about work at the Test Site. Again, it's something that's done routinely in small holes in mines, where you can get at the rock, where you can drill a small hole and put an instrument in it. Even then there are difficulties.

While we have not developed a method for measuring in-situ stress remotely, we are developing a method to measure strength. It's very difficult, again, to calibrate. It is known that the penetration of a projectile into a material, such as a rock, depends on the strength of the rock, among other things. We did a series of experiments in concretes, and things like that, where we demonstrated this. And, there's been a great deal of work done on it because of penetrating weapons, by Sandia and Waterways Experiment Station. They have developed a whole bunch of complicated formulas for calculating the penetration.

I discovered that a formula developed in 1765, or something like that, by Euler, was much better than any of the formulas developed in modern times, and he used very simple math. At any rate, we now have a device, built, which is capable of being put down hole. It fires a projectile into the wall of the hole, by remote control, measures the decceleration, and then retracts. It can then be used to repeat. This device exists, but the equipment to lower it down the hole doesn't exist. There are a lot of difficulties with it.

One of the major problems, of course, is in calibration. You can calibrate it in concrete fine, and that's what we're working on. Calibrating it in rock is extremely difficult. We're going to take it down to the tunnels, and we've done this once before with a kluge. Now we're doing it with the real apparatus. The problem, of course, is knowing the right answer. When you fire it into a rock, and measure the decceleration, what is the strength of that rock? You get a core sample, and you measure the strength of that core sample. You hope that if you measure it six inches from the place you're measuring with the tool that it is at least similar. But if you take two or three core samples, and you measure the strength of them, they're wildly different. And if you shoot in two or three places in this piece of rock, you get different penetrations. I think we'll be lucky if we get a factor of two accuracy; we'll be happy if we get a

factor of two accuracy. But this tool I am very pleased with. It's calibrating pretty well in grouts, and I'm looking forward to doing it this summer in the tunnels. It's a lovely piece of apparatus.

Carothers: Maybe the strength varies by a factor of two over short distances.

Hearst: Quite possibly. At any rate, we have actually built this apparatus, which is on wheels at the moment. We have designed a device to lower it. It's designed to work in a big hole, to clamp up against the wall of a big hole and fire the projectile into the wall.

John Rambo uses the drilling rate as a measure, of some kind, of the strength, but it also measures other properties. Among other things it depends on how the drillers are working, and how much weight is on the bit, and how sharp the bit is. But it is another measure.

John also believes that the velocity is another measure of the strength. Remember that I told you that the velocity depends on how much the rock is broken up by the drilling? Well, if it's broken up less, it's stronger, and so the velocity is higher. A lot of other things will make the velocity higher also. We may have to use all of these methods together to infer a strength. But since strength makes a great deal of difference in a calculation, it's important to get it.

8

Energy Coupling and Partition

A nuclear detonation produces ten to the twelth calories per kiloton, by definition. All of that energy is deposited in the earth, and ultimately, over a long period of time, results in making the earth as a whole somewhat warmer. Over the short term, the energy deposition can cause many different things to take place. The question of what that energy deposition does, and what fraction goes into each phenomenon is an open one, subject to many variables. Some amount causes surrounding rock to vaporize and to melt. Another amount causes the surrounding material to move, giving rise to motions in the ground. Some causes open pores to collapse, some gives rise to stresses in the rock, some is carried away by elastic waves that propagate to large distances. The amount of the energy that goes into each of the various channels determines the phenomena that are produced by the detonation. Some are easily seen; that which goes into the seismic wave can be detected worldwide. The amount that melts rock stays close to the origin; the rock cools, solidifies, and can only be seen if a costly reentry is made to the vicinity of the detonation point.

App: When you are looking at the coupling of the energy, and ground motions, there is the issue of how much energy actually gets coupled into the rock, as opposed to what remains behind in the cavity. This deals with the shock Hugoniot and the release properties of the vaporized rock. Butkovitch, in 1974, determined that there are large differences in the kind of energy coupling between low and high density rock. He looked at the refractories in the melt puddle and assumed perfect mixing, and from that inferred how much melt had been generated. That gave a value for how much energy had stayed behind in the cavity. What he showed was that for a dense rock you get twice as much, or maybe more than twice as much, of the energy into the shock wave as you do for a shot in low density, like 1.6 grams per cc, rock. And so, starting off one looks like a bigger bomb than the other, but it doesn't really change the waveform characteristics, just the amplitude of the signal. It looks like a bigger bomb.

Now, the porosity, and a number of other things change how large the bomb appears to be; how much energy goes into the solid rock. One can make the argument that there could be a factor of two in how much energy goes into the stress wave, just from the Butkovitch work. We should be able to go back and systematically look at cortex data to determine the hydrodynamic coupling for different materials. That's essentially what cortex is sampling - - the energy that goes into the shock wave. If we could combine that with additional rad chem analyses of melt puddles, we might be able to come up with some relationship between such coupling and the working point material.

The other part is, as we move farther out, there's this other phase of coupling, where the strength of the materials comes into effect and changes both the wave shape and the amplitude. That's the regime where the properties of the rock can modify the wave form to make it look like maybe something else, another type of source.

Carothers: Does that matter to containment?

App: I think it matters. Anything we can learn about how much energy gets coupled into the ground, and how it gets coupled in, I think is relevant to containment. If a bomb is going to put twice as much energy into ground shock because it's in this material rather than in that one, that's relevant. It's relevant to containment because we worry about the yield of the bomb, and that's the yield of the bomb, as far as the ground shock is concerned.

Higgins: There was a recent tunnel experiment that was identical in almost every respect to a test that had been fired six months before. The results show that the same explosive yield, in the same configuration, created a seismic signal that was one-half as large, or even a little bit less than half as large, in one case as in the other. That doesn't disturb anyone, because everyone knows that the seismic wave is kind of a vague and various thing. But when people began to examine the close-in strong motion measurements, they too were half as large, or less. And, as were the accelerations, as was the tunnel damage. If you went to a distance like a hundred meters from each of these two explosions, in one case there was nearly total destruction of everything. The tunnel was collapsed, and so forth. In the other case there was almost no observable effect; there were displacements, but they were modest.

As the data are examined, one of the suggestions, and it looks now to me to be the most likely suggestion, is that the mechanisms for coupling energy, in that region where melting and vaporization was going on, was very different in the two cases. If you think of the total explosion, very close to the explosion rock is melted and heated to extremely high temperatures. So, there is a part of the total explosion energy that goes into heating the immediate surroundings, and that part goes into forming the cavity. There's another fraction of the total energy that goes into deformation of the rock in an elastic-plastic sense. And finally, way out at longer distances, there's an elastic wave which creates a seismic wave.

We've long said that about fifty percent or so of the total bomb energy goes into the thermal cavity region, that another large fraction, also about fifty percent, goes into the plastic deformation region, and a very tiny part - - one percent or less - - goes into the seismic signal. What these two shots, and the measurements since then, suggest is that this roughly equal partition between the molten and the plastic deformation is variable, and a lot more variable than we thought. And that, in turn, affects the one percent or so that's left over for the seismic wave by a rather large factor.

For example, look at the amount of energy that is stored in what we call the containment cage. Take from one cavity radius to three cavity radii and say that is the containment cage region. That's a very crude set of definitions, but if you put two bars of stress in that spherical shell, that amounts to thirty percent of the initial device energy, using the compression curves that we are measuring. That amount of energy in the containment cage is a significantly large fraction of the device energy, and things that go on to perturb it are big things, not little things.

Carothers: What would lead to variability between the ratio of device energy that goes into cavity formation and the elastic-plastic type of deformation?

Higgins: There are quite a lot of things, it turns out. We have started to look at that, but I don't think the subject has been adequately studied, certainly not exhaustively. The most obvious thing that changes the ratio is irreversible pore collapse. Suppose you built the test medium out of fiberglass foam, or frothy pumice-like blocks, with fifty percent air-filled void. The crushing of those voids would consume huge amounts of energy. Of course, the

material gets very hot when it's compressed, but we don't measure temperature from a distance, so we don't know how hot it gets. All we know is how much of the compressive wave got transmitted, and if you're crushing the material, you're not transmitting any wave.

So, air-filled voids are one thing that can change the ratio. There are other kinds of things, such as phase transitions. Everybody is familiar with the ice cube in the drink, and the fact that you have a phase transition going on. It keeps the drink cold even though there is almost no volume change. The same thing happens in an even more pronounced way in some solids, like rocks. There are phase transitions that go on where minerals hydrate, or dehydrate, or melt, or vaporize, or change from loose open structures to dense compact structures. A common one is the transition of carbon to diamond, where there is a big density change. Silica does the same thing. It goes from an orthorhombic eightfold symmetry to cubic symmetry at very high pressures, and the volume change that accompanies that is like a factor of two. So the amount of energy that can be stored, just by going from an open loose structure to a high density structure is huge.

Carothers: You mean that just to generically call the Rainier Mesa rocks 'tuff' doesn't tell you what you need to know?

Higgins: Right, and it doesn't even tell you what you need to know if you identify it as being Tunnel Bed Four, because it turns out that the degree of zeolitization, the minerals of Tunnel Bed Four, are quite different in different places.

Carothers: So, to say that you have Tunnel Bed Four here, and in a different location you also have Tunnel Bed Four, as the geologists do, is not adequate to determine what's going to happen when the device goes off, at least close-in.

Higgins: That's a conclusion that appears to be true. I've got an analogy, which isn't exact. The business of containment, the interaction of a nuclear explosion with the earth, is somewhat like atomic physics was at the turn of the century. People were beginning to discover the difference between the orbital electrons in the various atoms. Then they discovered there was a nucleus, and there was the atomic structure. Then there is the nuclear structure, and they discovered that makes a difference; all nuclei aren't just the same nuclei. There are levels in those nuclei and there are

particles in there. And then, there are particles in the particles. I think that going from the picture of the earth as homogeneous is just like the transition when they said, "You know, the atom isn't a pudding. It's more complicated than that."

We're at the point of knowing things are more complicated, but not exactly what all of the complications are. That is still an open question. It's not open to the degree that we don't have some pretty good containment rules, but it is open to the degree that we can't say we can test in every conceivable situation with complete certainty. There are questions that have to be answered in every case. I think we can answer them. I don't see any insurmountable technological problems, but it's more complicated than we first thought, by quite a bit.

The amount of melt is one of the interesting numbers to look at. I really do believe, and I think most of us in the business believe, that energy is conserved. Ten to the twelfth calories in a glacier, or in the Greenland ice cap, will melt a fixed amount of ice, and it doesn't make a lot of difference if it does it by crushing or whatever. One of the rules of thermodynamics is that the paths are not important; the end states will be the same no matter what path you take. There is a certain amount of ice transformed into a certain amount of water. If you know the total energy, you know the total amount of water regardless of the path.

When you consider those kinds of things, and then you observe such different results in the seismic signal from two different events, you have to say, "It's clear that there have been differences in the thermodynamic path, and that must be related to the materials involved, and in the structure." We know that the total has to be the same.

Take the differences between P tunnel, and N and T tunnel. N and T turn out to be almost twins, but P is different. When we ask the question, "Well, what is different?" what turns out to be different is the degree of zeolitization, although the stratigraphic units are the same. That's a fancy way of saying to what degree the original volcanic glass has been transformed into some kind of a clay mineral. There are units in both tunnels that have the same amount of clay formation, but the clay occurs at different levels in the stratigraphic section. In other words, the geologists have layered the cake differently than the physics does.

Carothers: There was a man, Rick Warren, who gave a presentation at one of the CEP meetings about identifying the rock structures by mineral analysis. He felt that was the way you should identify the layers. His didn't correspond to the conventional units, but his point was that he could tell you that the rock at this depth in this hole is like that rock at a different depth in that hole.

Higgins: Right. DNA had him to do a special set of examinations, and it was through his work that this analysis of the P tunnel versus N tunnel came out. There are like fivefold differences in the amounts of some of the minerals.

Carothers: Might one say, "Over and over again we learn that the earth is an inhomogeneous body of materials. There's no reason to be surprised by differences in the response of the rocks to shots in different locations, because if you don't think of the rocks just as Tunnel Beds Four, and instead look at their mineralographic makeup, you're in a different medium in those locations?"

Higgins: That's right. That's what you would conclude. And that has to do with the history of the two areas we've been trying to understand, and their history with water. One is closer to the edge of the old original pile of ash.

In the first years of underground testing the radiochemists had a difficlt time determining the fraction of the yield that resulted from the fusion reactions. The samples obtained from the post-shot drill-backs sufficed for measuring the number of fission reactions, but the number of fusion reactions was difficult problem. In this circumstance other methods of measuring the yield, or energy release, of the device were sought.

The yield could, in principle, be determined by measuring the velocity of the outgoing shock wave in the earth materials surrounding the device. Small diameter holes drilled near the emplacement hole were used to place various instruments in, hopefully, known locations with respect to the device so the shock velocity could be measured. Information about the behavior of the medium itself could also be determined by instruments placed in the same satellite holes to measure the pressures and accelerations produced by the shock as it passed

Two of the important tools for obtaining information about shock velocities are what are called the "slifer," and the "corrtex." In the slifer, a long length of coaxial cable acts as the inductance in an oscillator circuit. When the cable is placed in an environment where the cable is progressively crushed, and thereby electrically shorted by some external pressure, the frequency of the circuit changes. Measurement of the frequency of the oscillator as a function of time will then give the rate at which the cable is being electrically shortened. In the corrtex there is also a long length of cable, whose length is determined by sending short electrical pulses down it and measuring the time it takes for them to reflect from the shorted end.

SLIFER - Shorted Location Indicator by Frequency of Electrical Resonance.

CORRTEX - Continuous Reflectometry for Radius versus Time Experiment.

Bass: I first got involved with underground measurements when I was asked to head an instrumentation section to make the close-in earth motion measurements on Scooter. We had stations at 25 feet, 50 feet, 100 feet, 200 feet, in vertical drill holes at shot depth. Then we had some instruments above them, making some vertical measurements, and then we had a few surface measurements. Scooter provided absolutely fantastic data that probably is not equaled today.

Carothers: Were these the first attempts at such measurements?

Bass: No. There were very good measurements on Rainier. Bill Perret did those. Rainier was an outstanding experiment and it was very well measured. Actually, some of the measurements were fantastic. Go back and look at the work that Fran Porzell did. He had left Los Alamos, and was at Armour Research at that time.He was attempting to measure hydrodynamic yield. and he had measurements that are now on what I am going to say is the cutting edge of what Los Alamos is now trying to do . There was a Doppler system radar on Rainier to measure the shock wave arrival which actually was as good, or just as far advanced, as Los Alamos is doing on the hydrodynamic yield programs today.

Also, on Scooter we tried to make pressure measurements, and we got next to nothing. We put in a few hydrophones, which are underwater pressure gauges, which we tried to adapt to underground, in soil, measurements. These hydrophones, which were made by Atlantic Research Corporation, were all Navy type equipment. They were a little batch of barium titanate crystals, I think, in a sack, and they drove a cathode follower, which was an emitter follower which drove a line driver. We tried to put those in a pressure chamber, and tried to calibrate them. We then put the gauge, which looked like your finger with a little bulb at the bottom, in a plastic sack of sand. We then put this in a metal frame, lowered it down the hole, and poured matching grout around it. The idea was that we would activate the crystals in the chamber. The return was zero.

The reason the return was zero was that during the long period of time when Scooter misfired, and then finally went, we had snows and rains and everything else on the Test Site. The emitter-follower boxes were right at the surface, and they all got wet, and shorted out. As project officer I was at fault for not having them moved. That shot went about the first week of October, I believe, instead of July, for reasons we have talked about.

Brownlee: Right after the moratorium I was doing hydrodynamic yield measurements in satellite holes. Ray Blossom picked the site for the shot, and Bob Newman told them how deep to drill the hole, using his little scaling law. Then I came along and said, "Okay, let's drill a hole here, and a hole there, and a hole over there, so for that yield range I can measure the hydrodynamic yield."

In order to do that I would go talk to the designers, and spend time with them. I would say, "You've got this down as 10 kt. What's the chances it will go 15 kt? What's the chances it will go five? What's the chance it will only give us a hundred tons?" I would listen to everything I was told, and I would say to myself, "Well, they say it's going to go ten, but it's clearly not going to do that." So, I would locate the satellite holes so if it went three kilotons I could get a good yield measurement. So, I'd have one or two in close, and do the third one farther out. I would put the holes where I was guessing would be right for the yields. We didn't have a design yield or a max cred yield then. We had a design yield in the sense that

they were hoping it would give ten kt, or whatever. Newman would have the hole for that. But that's not necessarily the yield I would use to place the satellite holes.

Carothers: If you were trying to get the yields hydrodynamically, that must have made the question of what the material around the shot point was important to you.

Brownlee: Oh, yes. That's where I came up with the four standards. There is a supersonic part of the arrival time curve, then it becomes sonic. And so, the shot would tell me what the sonic velocity of the material was. I had these curves, four altogether. I would say, "Is the sonic velocity most like this one?" Then I would use that one to derive the yield.

On one shot the curve would go sonic at this place, and then I could say, "This is very wet." On another shot it would curve over at another place, and I could say, "This is very dry." So, when I'd gotten enough facts I could say, "There, that's what it does. The shot itself is telling me the sonic velocity." So, I was able to construct a particular curve. Now, after you've done that, you can go back and restructure all the other curves and say, "Well, I can have any kind of equation of state here, depending on how much water is there." Then you do the trial and error fitting, and let that try to tell you the yield. I abandoned the four standard curves in time, but I needed data to show me that.

But you're exactly right. The reason I got interested in the water content, and what the rocks were like, and the porosity, and whether the porosity was filled with water or not, was in order to determine the yield. Hindsight says we were doing a better job of determining the yields than we had any right to expect. They were really pretty good, but I didn't know that. We finally stopped because the rad chem people said they were getting the yields well enough. It costs money to drill those satellite holes, so we finally stopped it. On the other hand, it's a good way to get the yield, and you really can do a pretty good job in a medium that you understand.

But remember, it's the shot itself, when you shoot it, that tells you what the medium is like. We never measured, ahead of time, the correct sonic velocity. We determined it from the shot, and it was always different from the pre-shot measurement.

That was the point I was trying to make at one of the CEP to these lads who were sitting there, who persist in believing that what they measure is the truth. They insist upon that, but it's never been true when you find out what the truth really is; it's never right. But there's the, "That's what I went to school to learn. I did all the things they told me to do, so this must be the number." So, you're quite right. It was the hydrodynamic yield measurements for those earliest shots, more than for containment, that forced me to understand something about the material.

Bass: After the moratorium Los Alamos was drilling a lot of holes in Nevada. They would drill a hole, and we would locate three satellite holes for making hydrodynamic yield measurements. Bob Brownlee had a formula to locate them, and we were working in the sonic region, because we thought that was the only place we could really understand. Also, in that region we were far enough away that the range errors - - the errors in distance between where we thought the device was and where our instruments were - - weren't killing us. But, every now and then something would happen, and they would end up putting a higher yield device down than they had originally planned. So, we would be in the hydrodynamic region, and we started getting some hydrodynamic data out of our first satellite hole, which was supposed to have been at around ten kilobars. Sometimes we were getting up to a hundred kilobars, or even two or three hundred kilobars.

When Bill Ogle said, "Let's start this hydro yield program," he gave Sandia carte blanc. Sandia and Los Alamos started their program, and Sandia did all the experimental work on that. The people in Livermore went off on their own, and started their own program. At one time Johnny Foster came to Sandia and asked Sandia to get involved in the Livermore program, but it never got implemented. It was probably a good thing, because I think there was too much work to be done as it was.

Carothers: It was expensive to drill those instrument holes, and Livermore gave up such measurements as the chemists developed their own methods for better yield measurements.

Bass: Los Alamos quit it too, because we got into a medium that was badly layered and we weren't getting decent results. The results were garbage, so we all quit the thing. But, before that we did get some useful data. Our agreement was this - - we would

provide Los Alamos with time of arrival information, if they would let us make, in their facility, pressure measurements. Chabai and I wanted the pressure measurements. They wanted the time of arrival data. Art Cox, Bob Brownlee, and I worked on this constantly. I was at Los Alamos every week during that period.

We were also working rather closely with Fred Holzer at that time, on Madison. He had a big containment program there. Madison had a huge room, and a drift off to the side. He wanted to find out how the energy partitioned down that, and could you close off the tunnel with that drift. He courteously invited us out to look at the whole thing, and go over all his data.

Then he came up to us at the CP one day and said, "I've got something you ought to get involved with," and he handed us a drawing of the slifer that they had come up with. We looked at it and said, "This is outstanding," because at that time we were using peizoelectric crystals, rather than a cable. That's the same thing the Russians are using today, although they finally went to a slifer.

We immediately started putting down slifers. I think the first one went down within a week. I took the drawing down to our trailer area, to an electronics guy, and said, "Hey, build us one." He said, "I'm not going to put those tubes down there," because it was a hard-tube oscillator. In about twelve hours he had one working with two transistors. There's just an oscillator and a line driver; that's all there is to it. That is still the same slifer design that Sandia uses to this date. We have never changed that design, from that day in the CP in 1962. When the Soviets looked at that they said, "You guys are kidding. Is this how you measure hydrodynamic yield? You use this?" because the transistors were circa '61.

We put slifers down right away, and we loved them. We used them ten times as much as Livermore ever did, and we still use them to this day. There are two slifers installed on the outside of the pipe on all DNA events, to measure pipe flow.

Brownlee and I put them on the inside of a pipe one time only. Al Graves gave us Mataco, and said we could do anything we wanted on Mataco, because he wanted to know if they could do these line-of-sight experiments. One of the things we did was to put a slifer cable inside the pipe, and one outside. We found that they read exactly the same thing.

So we started using these slifers. Taking the slifer data, and the Hugoniots of the earth materials, you can do the integrations, and put them all together, and you will end up with a beautiful pressure-distance curve. And it matches the pressure data. The whole thing falls together. It really falls together when you do it for granite. You can end up with a pressure curve from two megabars down to two hundred kilobars, in granite, from the slifer cables, that match the pressure data. It's a very good test, and the sanity check is solid. Those all go together, the pressure data, the slifer data, and everything else we've ever put together.

So, we were making these time-of-arrival measurements on Los Alamos events, and compiling the data. That's one thing I've always done through the years. I say, "All these individual data are poor, but when you put them together, there's some sense to them." And Brownlee's a real advocate of this; you better believe the data, because they're telling you something. They're always telling you something.

We started putting together the data we had taken on the LASL shots where we were in the hydrodynamic region and we discovered, lo and behold, it didn't make any difference what we were shooting in. There was a straight line function in everything. If we were in granite, if we were in alluvium, if we were in tuff, it made no difference in the strong shock region. All these materials worked the same way. What we had found is now called the Universal Relation. Now, marble is an exception. There are exceptions always to rules like that.

And this has been ignored, I think for one reason. The Livermore jealousy concerning the Los Alamos hydro yield program has been incredible. Livermore has been very negative on that program from the very beginning, because they have been oriented more toward seismic measurements for yield. So, they have not been much in favor of the cortex measurement program and the slifer program.

Incidently, I think cortex is the greatest sales job in the history of the program. If the Lord above had told you how to do this, he would have said, "Cortex first, and then slifer is the improvement over cortex," because slifer is continuous, and cortex is discrete, and not too solid. Now, the new cortex gets rid of this problem by

looking for the phase change of a standing wave on a cable. Fran Porzell had done that on Rainier. It didn't work on Rainier, but it was the same thing.

We first started the idea of using slifers for hydrodynamic yield on the PNE and the Threshold Test Ban Treaty, using this Universal Relation. It says that shock propagation in most geologic materials can be described by one power-law formula. It's called the Universal Relationship, or the Los Alamos Relationship. Al Chabai and I developed it, and Los Alamos has used it ever since.

We were pushing this as the way to measure the yield of PNE events. The reason you have to use the Universal Relationship on PNE's is that you don't know where the source is. The treaty we had, and still do have, with the Soviet Union says that the canister can be, then ten meters long, now twelve meters long. And, they can put the device anywhere they want in those twelve meters.

Carothers: You mean, you don't know where the center of energy is.

Bass: That's right. That's a better way to put it. So, your system has to tell you where the center of energy is. This is where you use the Universal Relation, because you know what the slope of the function must be. All you have to do is make a measurement in the emplacement hole, although it also works in a satellite hole. In the emplacement hole you don't need to know where the source is below you, but you do know from the Universal Relation that the slope of the shot curve has to be a certain value. Theoretically it should be 0.4, on a log-log plot. It turns out it 0.459, or something like that, because theory doesn't work here. It's just strictly accidental empiricism, or quackery. That's a proper dictionary definition of an empiric, isn't it - - a quack? Anyhow, the Universal Relationship works beautifully for this.

So, that's where PNE monitoring became possible, because we could, through the use of the Universal Relationship, and with the proper spacing of the cable above the canister so we would know we were in the hydrodynamic region, get the yield for any event we wanted to measure. If you had a satellite hole that went deeper than the device emplacement, that would tell you directly, but at that time there were not going to be any satellite holes. Everything on the PNE treaty was main hole, because it costs a million dollars to drill a satellite hole.

Rambo: I was hired on, in November of 1963, by John Ellis, who was then in charge of a small group developing, as a group, how to measure slifer yields for the nuclear test program. As I got familiar with the operation I would design where they would be located. I was one of the first people to say, "We've got to run the cable below the working point, so we can see the first crush on it." We wanted the first arrival because the elevation of that first arrival is where we thought the shot horizon would be. That was of interest for us because it would tell where the working point was, and there was a lot of uncertainty in that.

Carothers: You know where the emplacement hole started, on the surface, and it may wander a bit, but not much. Then you know where the device itself was put, rather accurately.

Rambo: Fairly accurately, although in the early days it wasn't always stated if they changed that location, and other times that information did not get back to us. On the satellite holes we thought the location accuracy was about two feet per thousand feet of depth, on the average. The Sperry Sun people did those surveys. They would run two or three runs, and we'd get different answers from each run. Two feet per thousand feet was average, but it could exceed that. You could get systematically bad information. Occasionally you'd get one survey that was five or six feet different from the others.

So, there was some uncertainty there. There was also some uncertainty in the depth, and there was the distance from the emplacement hole to the satellite hole; you had to make sure that was correct. One time I discovered that my data wouldn't fit no matter what I tried to do to it. So I went back to an aerial photograph, found the size of the pad, and then was able to determine how far away the satellite hole was. The surveyors had made a ten foot error. Then I was able to analyze that data. That was the kind of thing you could run up against from time to time.

In those days we were usually using one satellite hole. There was one or two shots that had three holes, but it was usually just one, and we didn't always know where the satellite hole was. So, there were surveys, and sometimes there were errors in where the cables might be located. So, we would make corrections after the fact to our data.

Carothers: After people told you what the yield was supposed to be?

Rambo: I've been accused of that quite often, so that question doesn't come as a surprise.

The results were quite sensitive to separation. A few feet made a difference. At the low yields we were doing in those days, it was very sensitive. In fact, I could easily be in the thirty to fifty percent range sometimes, certainly thirty percent. When you made the statement that if I knew the yield I could determine a good slifer yield, sometimes I did that, but more or less to determine what went wrong with the experiment. I was looking for systematic problems. I'd look back at the yield, and I'd say, "In order to get this yield, what would I have needed to change?" And so I would learn something about the experimental procedure, hopefully, to improve it. But I can't say I was completely oblivious to the fact that I sometimes knew what the yield was before I published the yield that I had gotten.

Carothers: If you were going to do that kind of measurement, you needed to know something about the material properties of the medium in which you were shooting - - whether it was tuff, or alluvium, or below the water table, or whatever. How did you get that kind of information? When you started the slifer measurements they weren't logging the holes were they?

Rambo: We weren't getting anything. The geologists would go down, look at the cuttings, and say things like, "There's rocks down there," or, "This is highly porous stuff". They weren't very good descriptions for what I needed.

We had four or five curves that we would compare our data to, to get the yield. It's called similar explosion scaling. These curves were labeled Wet, Damp, Dry, Very Dry. The tail of these curves would fold over flatter if they were dry, and they would be steeper at the end if they were wet. I would take these curves, and I would compare the tails of these things after the fact, and with some knowledge that we were in a wet hole or a dry hole area, I would ask the geologists for what specific information they had. From that I would try to figure out which curve was the one I was to use. It is not the best way, and it is certainly not as good a way as we do nowadays.

Carothers: Bob Brownlee did measurements of that sort for some time for Los Alamos. Did you have any contact with him at all?

Rambo: No, I don't think so; not in those days. The curves I was just mentioning came from Los Alamos, and they had come from calculations they had done. We inherited those nomenclatures. We were at least self-consistent in terms, between the Laboratories.

Carothers: Why did you use satellite holes, which are expensive? Why not put the slifer cables down the emplacement hole?

Rambo: We did. We did them in both places, but what was happening during those days was we were usually measuring fairly low yields, and there were often large diagnostic line-of-sight pipes, at that time. Close in to the device, where you needed to be with the slifer cables, there was a lot of radiation and energy that was going up to hit doghouses and things like that. Often what I'd see on the slifer cables was doghouses exploding. I could see all this detail going on, but it wasn't very conducive to doing a good yield measurement. So, I kept pushing for satellite holes. That was an additional expense, and I think toward the end of this early era they were trying to save money, and the other methods of yield determination were getting better, so slifers sort of ceased to exist at Livermore, probably around 1964 to 1965.

Carothers: What's the difference between a slifer and a cortex measurement?

Rambo: Really nothing, in terms of what the data looks like. In a slifer, the cable is the inductive leg of a tuned oscillator. When the shock crushes the cable, and shorts it, the inductance changes, which changes the oscillator frequency. Los Alamos, at a later time, decided to use a slightly different way to measure the cable length. What they would do was send an electrical signal down the cable, let it reflect off the crush point, and come back. So, they would measure the transit time. This is what the cortex method is. Essentially we measured the same thing, but by slightly different methods.

There are some things that a slifer does in sensing a change more rapidly in the speed of the crush going up the cable, because you don't have to wait for the transit time of the signal. There are some advantages to the slifer, and they're still being used on the tunnel shots. I am not sure which method is better. I think cortex is a little bit freer of noise, and in some instances, because the technology of electronics have changed, it's better in that sense. It suffers from the same problems we had in the early days of doing slifers. It's just that when they brought more information to bear on the problem in these recent times, it's a little easier to get better solutions from the data. But there are still problems that are very hard to deal with.

I have looked at a lot of data where I would see a time of arrival up above the device which seemed shorter than it did off on the horizontal, where we were looking at the horizontal arrival of the shockwave. Then after a while, the two curves from those locations would come together. This was two separate cables, of course. I saw that more often than not. If there was a baffle, or some sort of metal plate, or something like that above the device, it looked like the shock was coming from that source. Or if there was a large opening for a brief distance above the device you could almost see that looking like a source.

So, there was this problem of where the center of energy looked like it was, very close-in. People like to measure close-in, near the center; the material properties don't matter as much there because of the very high pressures. But what does matter is the minute geometry of what's going on with the explosion, in terms of where the energy flows. So, you've got a trade-off taking place at that point. It's almost better to look at the data farther out, but at that point you're worried about material properties more.

If you looked at the entire curve on these things, sometimes you could determine what errors you were looking at. If your cable was further down than you thought it was, you could see that kind of error, because it was a constant difference from what you should be reading. And if you were comparing it to an emplacement hole, sometimes you could determine that. If the satellite hole were farther away than you thought, you could compare it to the emplacement hole, and sometimes you would get a feel for that kind of an error. These were all techniques, and some of them I had developed in the early days, like looking at where the first crush

point was on the satellite hole to try and iron out some of those difficulties. A lot of that is now taking place more professionally with current methods than I was able to do.

The earth in which the energy of the detonation is deposited is not an infinite, homogeneous material, unfortunately for those who would calculate and predict what will occur. The various layers of rocks of different properties, the faults, the presence or absence of water all affect the ground response. One example is the surface ground motion produced by an event called Tybo, which was detonated in an emplacement hole in Pahute Mesa. The surface motion, and the measured ground shock was, unexpectedly, the highest that had ever been seen at the Test Site. John Rambo tried to model the geologic setting, and in his calculations determine why this should have been so.

Rambo: I started to wonder about these peculiar ground motions when there were two shots that were fired quite close together physically, and also in time. One was nine kilotons, and it was located below the water table at about six hundred and eighty-eight meters. The upper one was at about four hundred and thirty-some meters, and it was about thirty-five kilotons. The interesting thing was that the free surface velocity for the deeper one was about one point one meters per second, and the free surface velocity for the upper one, which was higher in yield and much closer to the surface, was about one meter per second. These were actually measured, and because there was about thirty seconds between the detonations it was easy to see separate signals.

Carothers: The thirty-five kilotons closer to the surface gave less motion than the nine kilotons deeper down?

Rambo: That's right. And so, that was certainly a puzzle.

Carothers: No puzzle. The lower one was below the water table. The coupling is higher there.

Rambo: But above the water you had all this porous material, for quite a ways. The shock was running through much more porous material from the more deeply emplaced lower yield shot than the shock was from the upper yield shot. The perception at that time was that this was something to wonder about.

It turns out, from what we saw in the calculations, that being below the water table tended to give what I call a focusing effect that changed the attenuation rate of the signal, even though you're going through the same porous material. The shock is being absorbed right above the water table; there is quite a lot of attenuation at that point. But there is another effect. The shape of the wave shows where it comes from, or where it looks like it comes from, and that is important. If it becomes more planer, it's going to attenuate less than the spherical wave you get if everything is a uniform medium.

I didn't know this until we did the Tybo shot. That was an event that had very high ground motion. It was about nine point eight meters per second.

Carothers: That is the highest ground motion we have ever seen on a contained shot in Nevada, if I remember correctly.

Rambo: That's right. Tybo was certainly a mystery because of the high ground motion. There was at one time some TV footage of what it looked like from the side, when it went off. It showed this huge mound rising up, and you could see the curvature quite clearly. The containment scientist related to me that it looked like it was going to come out of the ground. It just looked like a cratering shot, and gave you that impression, it was so rounded.

So, there was a lot of interest in why this could have occurred. I went back and I ran fifty or more 1-D calculations, and I couldn't get anything close to what happened, no matter what I did. Even if I ran it saturated to the surface, I couldn't get anything like that. And, it just was not in the realm of material properties, and I tried a lot of them. Even increasing the yield to the maximum credible yield, and going to extreme material properties I could not get a match to that kind of a signal.

Carothers: That merely illustrates the deficiencies of your code.

Rambo: You're right, and the deficiency I found out about was that a 1-D calculation didn't take into account a flat water table effect in the soil.

Carothers: Of course not, because in a 1-D calculation the device sits in a sphere of saturated material. So, the shock goes out spherically, and it doesn't care what the interfaces are, except a little energy may get reflected back, and it stays spherical.

Rambo: You did a better job than I could do to describe it.

To look at the surface signals on Tybo we had surface arrays that went off in a couple of different directions. Without those arrays I doubt if we would have been able to unravel it. Looking at that data, you could see that the plastic part of the wave was traveling in different pathways than the elastic part which was just going straight through the formation on constant rays. From that I got the idea that a 2-D calculation would probably show the effect, So I switched over to a 2-D calculation, and I definitely saw the effect; there was at least a factor of two between a 1-D and 2-D calculation.

It's a Snell's law kind of an effect. What happens is that there is a change in the shape of the outgoing wave when it hits this porous surface. It becomes very broad and very shallow, so it looks like the source is much deeper. That means it's going to attenuate less because it's progressing now more like a plane wave rather than like a highly spherically divergent one. It's still spherical, of course, but it's not as divergent as it was, because the radius is now much bigger. The calculations showed this enhancement, so you get a much higher free surface velocity then you would with normal spherical kinds of geometries.

I think that was the first time we had discovered that this huge variety of ground motions could indeed be due to a focused effect from the layering. In the case of Tybo it happened to be the water table, but there could be certainly other cases where you'd see things of that nature. When you go from something that is saturated to something that's highly porous, and maybe there's some strength in that rock as well, the the signal is not attenuated very much in the porous material, and so you may get a focusing effect.

Carothers: This effect occurs when you're going from a medium with a relatively high sonic velocity into something where it's slower? Or into a medium with a higher index of refraction, if you like.

Rambo: That's exactly the right analogy. It tends to be most pronounced when the interface is between about twenty to forty meters per kt to the one-third than at other distances. At very high stresses the shock wave in the saturated and unsaturated materials give about the same velocity, because they're so high up on the stress curve, or up on the compressibility curve, that the velocities

look very similar and you don't get the big velocity differences, usually. If the interface is much farther out, then the distance in which the shock wave has to change its attenuation is much less, and so you don't see quite the effect. But around the twenty to forty meters per kt to the one-third you can really see a pretty good effect from that, on the ground motion. At least that's what the calculations tend to show.

9

Cavities and How They Grow

When a nuclear device is detonated it deposits a very large amount of energy into a rather small volume. That deposition of energy produces a volume of extremely hot, extremely high pressure gases from the surrounding materials. These generate a very strong shock, which begins to move outward from the shot point. That shock is strong enough that as it moves out it vaporizes some rock, melts more rock, plasticallly deforms still more, and finally weakens to a place where only elastic movements of the rock take place.

What is left behind after the passage of the shock is a more or less spherical cavity that contains the radioactive debris from the explosion, and vaporized and melted materials that contain some fraction of the energy released. Fundamental to the understanding of how the containment of nuclear explosions occurs is knowledge about the formation, the growth, and the eventualy decay of the temperature and pressure of the post-shot cavity.

As with so many other things in the field of containment, direct information and data about cavity formation and the conditions in it are extremely difficult to come by. Much of what is believed is derived from measurements at a distance where the instruments can survive the shock passage, from observations on post-shot reentries made through existing or newly mined passages, and from calculations which try to match the data and observations there are and which then hopefully give insight into other phenomena not directly observable.

Cavity Growth

The formation of the underground cavity is an impressive phenomenon to consider. In a tenth of a second or so the rock around the point of detonation of a one kiloton device is moved and altered sufficiently to create a roughly spherical void that is of the order of a hundred feet in diameter. For the Cannikin event, which had a yield of a few megatons, the formation of the cavity took somewhat

longer, perhaps as much as most of a second, but at the end of that time some 20,000,000 tons of rock had been displaced to make a cavity in which the Empire State Building could stand. The relative importance of the various mechanisms that cause the cavity growth and formation is still debatable, although the general outline of what occurs is gennerally agreed upon.

Patch: I think the shock and the gases are not equally important in the growth of the cavity, but I think it's a matter of the timing. In some sense, initially the cavity is driven by the gas pressure inside. That's what launches the shock. But if you look at the calculated pressures inside the cavity, because of the r-cubed effect, the cavity doesn't have to expand very much before the volume goes up tremendously, and the pressure is forced to drop. And so a great deal of the motion of the cavity is really a coasting, momentum driven motion. The fact that the cavity pressures end up at overburden, give or take factors of two, is somewhat fortuitous, because we've done calculations for other, partially decoupled situations, where you don't get anything like overburden pressure in the cavity, depending on how it's decoupled. It just turns out, for the strengths in the rocks we have, and the way things work out, that's kind of where you end up.

An example of where a cavity does not end up at the overburden pressure is an explosion in water, where you can get a tremendous overexpansion, and effectively a very low pressure inside the cavity. It isn't smart enough to realize that the overburden pressure around it is such that it ought to stop, and it keeps on going until it gets to some very low pressure inside, depending on the depth and the yield, and so on. Of course, it then gets smaller, since the outside pressure is higher than that inside. Actually, such a bubble, or cavity oscillates in size, predictably. So, I think it can work out either way.

Carothers: In the very early times after the detonation the pressure of the shock generated must overwhelm any kind of material properties or strengths of the rock.

Rambo: I think that's usually the case in the megabar type of regime. I've heard some people now casting doubt on that, so I did some equational things that relate to the slope of the shock velocity and particle velocity curves. Material properties make a difference

overall, but most rocks tend to look pretty much the same in that high pressure regime. You don't see any big differences; the slopes of the curves in a granite look very much like the slopes of the ones for a weak alluvium. So, there's a tendency to say, "Well, they're all going to be the same." But there are elements, or there are different things out there, that do look different.

Carothers: What's your view of what drives the cavity to its final size?

Rambo: My view is, and I take most of it again from calculations, is that this enormous shockwave that's generated, with a very high gas pressure that sits behind it, gives momentum to the material as the shock is traveling outward. From what I've seen in the best physics that we know, in terms of calculations, is that the cavity pressure then starts to decrease rather rapidly.

Carothers: Well, the rock vapor condenses fairly early.

Rambo: It condenses, but that happens at a later time. Even at very early times, when that rock vapor hasn't even had a chance to condense yet, the cavity pressures are down below where they can have a strong effect on pushing the material outward. What's happening to the ground around the device is that you've imparted a large momentum to it, and so it wants to go out. Then it begins to decouple itself from the cavity pressure behind it, and about all it seems to know is that it has this big momentum, and so it is moving out. As it continues to move out, it's encountering resistive forces, and the peak of the shockwave that's imparting this momentum is beginning to decay rather rapidly. Pretty soon this momentum is fighting the restoring forces of the overburden, and the shear strength of the material, as the cavity wall material trys to get itself into a wider, thinner volume as it expands. Eventually, the material reaches the point where, at maximum cavity radius, the restoring forces which are wanting to push it back are as strong as the final momentum forces that were pushing it out.

Carothers: Nort, the detonation releases an enormous amount of energy into a quite small volume, the shock starts going out, putting a lot of energy into the rock, which then coasts out to some place determined by how strong the rock is. Is that what you think happens?

Rimer: That's containment lore. Basically, the rock doesn't actually coast. I've heard, ever since I came to S-Cubed, the story that you start the walls of the cavity moving, and it doesn't matter how you modeled the cavity pressure. "The cavity just goes and coasts, and keeps going until the rock strength stops it." That's not so. Cavity pressure is an important driver. What's in the cavity, whether it's steam, or the rock is dry, or whatever, is an important driver, and it does control, to some extent, how long the cavity grows. If I were to rate three things of importance to cavity growth, one is the strength of the rock, two is the cavity equation of state, or what's in the cavity. Three is gas porosity, but gas porosity is an order of magnitude less important than strength, for the final cavity size. That's gas porosity, as distinct from water saturated porosity.

Outside the cavity region the details of the rock volumetric equation of state, other than gas porosity crush-up, are relatively unimportant to containment. They're important if you're doing something like trying to determine the hydrodynamic yield. They're important there, but if you're interested in containment based on displacements of the rock, and how much plastic work you do in the rock to form these residual stresses, they're not that important.

Cavity Size, or Radius

A number which is often referred to in discussing containment is the cavity radius. When the term "radius" is used, the implication is that a sphere is being referred to. That is arguably not the right term or implication, since cavities are only approximately spherical, but it is imbedded in the literature and the available data. The quoted radius is generally determined by post-shot drillbacks which are made to retrieve samples of the once molten rock for analysis by the radiochemists. The place where the drill first encounters the radioactive material, if known in space, can be used to determine a distance from where the device was before detonation. If the assumption is made that the cavity grows spherically, with the device as the center, a radius can be defined. Both the assumption that the cavity is spherical, and that the position of the device is the center are suspect, and probably wrong.

The predicted cavity radius is used as one of the means of selecting a appropriate depth of burial. Also, it is generally thought that for a given yield a larger cavity is better for containment since that indicates a weaker rock that allows more cavity expansion, and therefore a lower residual gas pressure in the cavity.

Kunkle: One of the things I have been interested in is cavity sizes. That is, what data do we have that might be able to determine the volume, and define the shape of the cavity. Is it really spherical, or is it perhaps non-spherical? What is its volume, and its actual location. Does it float upward or downward with respect to the shot center, and how is the volume of the crater, if one appears on the surface, connected to the volume of that initial cavity? One of the reasons I've been interested in these things is that they are some of the measurable phenomena of the detonation. You can go out and see a crater in the desert. You can drill back, and find the lower hemisphere of a cavity. These are some of the few things we can actually measure about what happens when a shot goes off.

Many of the other things we would like to know, we just know very poorly. For example, the shape of the rubble column, the chimney, under the ground is largely unknown. We have in the past drilled into a few rubble columns in four and five different places to try to learn something about their shape, but that only tells us about that one, and they may be very individual for all we know. Such things we know little about, but we do know some things fairly well, such as the lower radius of the cavity, which we tag from our rad-chem drill backs.

Carothers: There are three cavities that we know a fair amount about. One is Rainier, where they did an extensive post-shot reentry and drilling program during the moratorium. One is Gnome, which had a standing, partially collapsed cavity, where they reentered and could walk around in it. And one is Salmon, which had a standing cavity, where they could lower a television camera into the cavity and look at it. The Salmon cavity was spherical. It had what could properly be called a radius, and a center. Gnome and Rainier were both flattened on the bottom, with a bigger dimension at the waist than that inferred in the upward direction. Of course, there was surely an instant in time when they were rather spherical.

Kunkle: There must be some era when that was true. It is a rather fortuitous circumstances that we have in the past often shot in quite uniform material. These shots have been located mostly in Area 3, in the Sandpile area, which has a very uniform material. It's hard to conceive of shooting in a more uniform geologic setting.

Carothers: And yet that's an area where there are discrepancies in what you would normally expect the cavity radius to be. Some of those cavities are reported as unusually large.

Kunkle: Yes, there's an area in southern Area 3, in the alluvium, which seems prone to relatively large cavities. But there seems to be a gradation in the mechanical properties of the alluvium in Area 3 as you move from the north to the south, which is up along the drainage toward Yucca Lake. The larger cavity radii may reflect some change in the material. There seems to be a general relationship between the scaled size of the cavity and the material it was shot in. For example, events shot in the alluvium in southern Area 3 have a K-factor, which is a relative measure of cavity size, around the low eighties. Shots in Pahute Mesa, in the very hard lavas, tend to have K-values of 64 or so. And so, we see a range of cavity sizes reflecting the geologic circumstances of the shots.

Carothers: Do you think it is the strength of the material in which the device is fired that is responsible for the variation in scaled cavity sizes?

Kunkle: The strength of the material certainly has an effect. If you look at average numbers, as you move from the soft, fluffy, low density alluviums in southern Area 3, with, say, a density of 1.65, to the medium density alluviums in the center of the valley, which have densities of 1.8 or so, to the higher density alluviums in the north part of the valley which have densities near 1.9 to 2.0, and down into the tuff units, which are perceptibly stronger rock, to the very dense, strong lava units on Pahute Mesa, you see a progression of cavity sizes from larger to smaller as the units increase in their presumed strength.

I say presumed because we don't really measure strength, but one could imagine that those materials are getting stronger. The alluviums are too weak to core. They crumble apart. The stuff that we took out of some of the lavas up on Pahute near the Houston shot

are good tombstone material. I describe them as very competent, very strong, uniform rock. As you move through this progression of rocks the cavities tend to get smaller.

Carothers: The data are scattered, but there is a definite trend?

Kunkle: Yes. Much of the scatter is due to measurement errors. Where actually is the cavity, for example. In the radiochemical drill-backs you have to know where in space, or where in the ground, you actually intercepted the radiation that marks the edge of the cavity in order to back out the so-called cavity radius. The first problem you run into is that this usually isn't a smooth transition from the native rock into the radioactive melt glass. The transition is usually a meter or two wide, with fractures and little pockets of activity mixed in. Turbulent mixing comes to mind, though of course we've never seen that transition layer in that detail.

It's not a smooth, sharp boundary, so one of the uncertainties is where to pick the edge of the cavity to be. That's something which often has a meter, or two meters, of uncertainty. Then there is an uncertainty as you lower a gyro tool into the ground to try to survey in where that spot really is. Those errors build up, and you're left with a sizable error, which increases linearly with the depth of the shot, as to where you actually find that interface, just from the surveying. Much of the spread we see in cavity radii, the K-values, the scaled cavity radii, can be traced directly to our cavity radius measurement errors.

If we look at the shots in Area 3 tuffs, which are fairly deeply buried, the average K-value for those is around 74, 76, plus or minus 8. About two-thirds to a half of that error, somewhere in that neighborhood, comes from cavity radius measurement errors. And so, when you get a discrepancy for a shot, you don't know if you really had a cavity that may have been large in that direction, or if you just happened to get unlucky with the surveying.

For devices detonated in tunnels it is possible to reenter, and if there is sufficient interest, mine back to the boundary of the former cavity and even beyond, into the region where overlying material has fallen in and filled the former void. Then there can be accurate

surveys, visual observations, and photographic documentation. Even so, in the few cases where this has been done, determining a cavity boundary, or volume, has been uncertain.

Patch: A problem we've always had, at least for DNA, has been really tagging the cavity in such a way that you have confidence that you know exactly what the cavity boundary is. In the tunnels we tend to have fairly big perturbations because of stemming columns, and things of that ilk. So, unfortunately, the cavity size is not known very well. It's probably better to talk about the volume, and then small differences are being cubed. In my mind that's a better way to look at it.

An issue which I think is important is the different ways that cavities collapse. Some of them collapse in a rotational mode. That's a shear collapse, if you will, where apparently there's a shear plane that forms behind the molten edge. It's a slope failure, a rotational slope failure. I don't know how far back this shear plane is, but our experience is that the cavity radii tend to be about ten percent greater in the horizontal plane than what you determine by measuring down vertically. Of course, stuff comes down from the top also, and so the exact size of the cavity is a little bit iffy.

Another thing is that, at least to first order, all of the DNA sites we've fired in are wet tuff, and they all are close to the same strength. So, we haven't really been able to say, in terms of cavity growth, or cavity size, how rock strength affects these things. I think that's an important parameter for us when we look at the closures for the DNA experiments.

Maybe I can take a slightly different tangent, that speaks in that general direction from a somewhat different experience base. We've done a lot of work with Carl Smith and the Sandia folks regarding the HE shots in G tunnel. Those shots have ranged from eight pounds up to a ton. The second area which we worked in fairly intensively was with Alex Morris at SRI, with fairly small shots. I think the data, when you look at it, for that range of yields is pretty unequivocal that strength has a very important effect on the cavity size.

And it's strength in a funny way. That is, we have found, with reasonably high confidence, that the response of these earth materials, be they grouts, or be they tuffs, are rate dependent. In particular, they have an effectively higher strength if there are very

high strain rates. That shows up in this data base which spans quite a large range in strain rates, from 3/8ths of a gram charge of HE up to really nuclear size. Over that range we have seen dramatic differences in the scaled sizes of cavities.

I think those are reasonably well controlled observations, because we know, reasonably accurately, what the equation of state for high explosive is, from its initiation all the way out. And we have, for the SRI case, control of the grout material. There is not as much control for the material in Carl Smith's work, except to the extent that it's a homogeneous body of tuff that's relevant to the DNA nuclear sites because the properties are close to those of the rocks they shoot in.

I tend to think of the microphotographs of samples that show this incredible structure, and I tend to think of the movement of the rock as being a very complicated process of grains trying to break cementation, and trying to slide over each other, and doing all kinds of strange things. So, I think of the strength of the rock from a more mechanical point of view. Being a mechanical engineer, I guess I think more that way.

The role, or influence, of the water in the rocks on the growth and size of the cavities is another factor that is not that well understood. Certainly it has an effect. There is general agreement that it weakens the strength of the rock, in some indeterminate way, but how much it affects the growth of the cavity is an open question.

Carothers: John, in calculating cavity sizes, do you think that the principal influence is the strength of the rock itself? How important is the amount of water in the rock?

Rambo: Perhaps we're limited in our calculations in terms of driving pressures from the steam, but I see only a minor difference in the amount of cavity pressure that's generated with say, ten percent water as opposed to twenty-four percent water. The strength of the material makes a big difference. I am much happier with a large cavity, because then I make the assumption that it was fired in fairly weak rock, and the shockwave is attenuated. And from all these biases that come from my calculational background, I see a large cavity as more benign than I do something with a small cavity.

Kunkle: I've looked at models of cavity growth, and if the amount of water in the rock had an appreciable effect on the cavity size you should be able to evaluate the volume percent saturation, and as the amount of water and the volume of water in a given volume of rock increases you should see larger cavities. I have not seen that there is any significant dependence of the K-values, the scaled cavity sizes, on that parameter.

Carothers: When the cavity reaches it's full growth, the belief is that the cavity pressure is determined by the strength of the rock and the overburden pressure.

Rambo: Yes. I think you do have to add the residual stress to the overburden pressure. But the cavity pressure is at least overburden pressure. As far as the water goes, after full growth I don't see a big difference in the cavity pressure, even though I've put more water in the calculation. I do see some differences in the calculations, but not large ones. There is a slight dependence, in some kinds of soils, where if there is a lot of water, the water tends to lubricate it and make the material weaker. Water can make a difference there. That's one effect that can certainly take place. There is a tendency, in a soil-like material, to see that, but it's not strongly connected to the cavity pressure itself.

But I will put in a caveat - - not every rock does that. There was some work done by Bob Terhune, in which he went back into the calculations, and he said, "Look, we see the strength phenomenon difference in the cavity radius, and we see it as to when the residual stress sets up." He decided that it sort of made sense. So, he looked at different areas. He looked at Area 20, and by and large it looked like things set up differently there, in the sense that the cavity radii tend to be smaller than in the valley. In a very hard rhyolite, like the rock the Molbo event was fired in, where the drilling rates were low, there was a small cavity radius. Then you get into something like Baneberry, where they measured a very weak rock, and there was a fairly good size cavity radius. The calculations show the same thing. So, I see tendencies in that direction.

There are still some outliers that I can't explain, and that I don't understand. From time to time you get something that's enormously large, or enormously small, in the relative size of things.

I've seen that kind of thing. Given that, I think there is a trend through all of this that does follow the strength idea. But the data are noisy, very noisy.

Carothers: Nort, more containment lore. The cavity sizes at the Test Site are all about the same, scaled of course, since all the rocks at the Test Site have 15% to 20% water, plus or minus a bit. Would you agree with that?

Rimer: I don't believe that for a minute. I know a lot of people believe that, but I don't believe that for a minute. Most of the cavity measurements are from drillbacks into the lower half of the cavity. They always take the radius measurement from some drillback point to the old shot point. They don't account for cavity buoyancy, and even elastic calculations will show the cavity moving up. An inelastic calculation will show that the cavity may move up two, three, four feet; maybe even several meters for a big shot, depending on how weak the rock is, just because of the presence of the free surface. And for the bigger shots there's stronger material below, so the upper hemisphere of the cavity is going to be quite a bit larger than the smallest dimensions. Calculations have shown that. Of course, nobody knows, because the cavities all collapse.

Carothers: There was one that didn't. That was Salmon. The cavity was reentered, in the sense that they sent down TV cameras, and there was a nice spherical cavity.

Rimer: You're right, but that was not at the Test Site. It was at seven hundred eighty meters, in salt, but not salt all the way to the free surface. And, they reentered nine months later. I spent a lot of time calculating Salmon, and Gnome. It's clear to me that in the nine months until they reentered Salmon that cavity wall creeped in about five meters in radius. I matched all the particle velocity records from that event, and the calculations that matched them require about a 21 meter radius cavity. They measured 16 or 17 meters. I do believe that Salmon creeped in quite a bit. Now, it was buried very deep; if it's less deep, there will be less creep. Evidence from salt mines is that the open drifts want to creep back at you.

Carothers: Another cavity that was reentered was Gnome, also in salt. It was not as uniform a medium, and not as uniform a cavity either.

Rimer: The models that work for Salmon work for Gnome. That was a layered salt, and that may explain the shape.

Higgins: After Rainier, and after we had done other underground shots we found that we always got cavities with a radius of fifty or so W to the 1/3rd feet. We thought, "Ah ha, all rocks are behaving in the same way. It doesn't make any difference what's in them."

And then came some information, first by very circuitous routes, and then directly, that the French shots in granite in North Africa didn't make cavities with a radius of fifty W to the 1/3rd feet. They only made three or four meter cavities, which means a ten or twelve foot radius cavity for a kiloton. Well, that couldn't be, so that informations must be wrong. That was the first reaction.

Then we had a symposium at Davis in 1964; I think it was called the Second Plowshare Symposium. The French sent a very large delegation of physicists who were quite willing to talk about some of the physical effects, as long as they thought it was a one-on-one quid pro quo. They would tell us the cavity radius from some shot, and then they would expect us to reciprocate. Well, the circumstances were such we couldn't do that, so they stopped. But we did get some information before that, and one of the things that was confirmed was that their cavities were grossly different from what we had seen on the Hard Hat shot, which we had fired in granite.

Carothers: How can that be? You had determined that the rock doesn't really make any difference.

Higgins: That's what we thought. That was the first clue, and we were not bright enough to tumble to it soon enough. It should have told us that the conclusion we had come to about the rock didn't make any difference was true because all of the rocks we were looking at were mostly water. Even Gnome, which was shot in salt in '61, was four percent water by weight, so when you put the sodium chloride and the other things into it, that gives a material which is like twenty mole percent water. So, even the driest thing we ever done a shot in was about one quarter water.

What was going on was that the French were firing in the Hoggar massif, which is a block of granite that's like tombstone granite. It doesn't have many cracks, it doesn't have any pores, so

there's almost no water there. It was less than a half of a percent, and it probably was less than a tenth of a percent. So, in their case they really did have a shot in material with no water - - a dry granite.

The United States, and this is an important point, because it affects the arms control talks, the disarmament talks, the treaty negotiations, has never fired an event in any material that isn't dominated by water. The seismic signal, all this business about the geologic differences between the Nevada Test Site and the Soviet Siberian platform, or Novya Zemlya, are trivial compared to the fact that they all have water. Whether it's granite or tuff isn't important; what is different is the transmission path. The French really did several shots in something that wasn't wet, and only they have ever done that.

Carothers: John, have you done work on the Hoggar shots? They are one body of experience of shooting in a very dry, very strong rock, and the cavities there were small compared to the ones we normally see.

Rambo: I've done a little work on that. My understanding is that the Hoggar granite is a rock that is like one unit that has not been fractured. Or if it is, the fractures are much farther apart than they are in the granites we have. When we looked at our granites, the fractures were on the order of a foot or so apart.

In our local NTS geology, if you take a piece of granite and measure it in the laboratory, if it's not fractured, you get a pretty hard rock. And yet, this material, in bulk, is a weak rock, because of the fractures. And, it's certainly not going to be helped any by the shockwave that goes through it. The two sites - - the NTS granite, and the Hoggar granite - - give completely different answers in terms of the cavity radii. If you shoot in something that's less fractured, then you really are starting with a stronger rock, and you get a small cavity radius. Compared to the laboratory data you have to degrade the strength of the rock by almost a factor of ten, because of the rock fracture frequency. There has been some work done in trying to get the strength from the fracture frequency. I did calculations on one of the French shots, and I came up reasonably close to the measured cavity radius.

Carothers: Nort, there's a set of granite data, aside from Pile Driver, which you are probably familiar with, and that's the French tests.

Rimer: Hoggar. Yes.

Carothers: The difference from Pile Driver, as I understand it, is that the Hoggar granite is very dry, and has a very low number of fractures in it.

Rimer: One every three to five feet, compared to one every foot or so in Pile Driver.

Carothers: The other thing about those shots is, presumably, that the cavity sizes were quite small for the yields of the devices.

Rimer: I know. I spent a lot of time on that. Actually, those cavities weren't that much smaller. If you assume that rock is completely dry, you get a cavity radius which is roughly two-thirds the cavity radius of Pile Driver.

Carothers: But that means their volumes were less than one-third of the cavities generally seen at the Test Site.

Rimer: That's true, and there were a couple that were smaller. There are a lot of stories about the in-situ stresses in that mountain, but I can't confirm them. They're not confirmable.

There is some sort of phase reversal that came out of Hoggar, in seismic motion. It can be explained by putting in in-situ shear stresses - - in other words, the vertical stress different from the horizontal stresses, in that rock. Steve Day, who was S-Cubed for many years, and I did a lot of work, and a number of calculations, on that, trying to explain that. We got some good answers, but I'm not totally convinced, because if you accept the answers on the cavity size as being because the material is dry, then the pulse widths would be very much smaller. Therefore the displacements would be much smaller. The displacements that the French have published are fairly consistent with the SRI data. They're further out, and they're a little smaller than Perret's two measurements here, but they're not as small as you would get from assuming a very dry, very strong material. Also, the seismic ground motions aren't that much smaller, if we believe the yields they have given us. So, I'm not convinced of the answer.

Carothers: Let me greatly oversimplify this. There are the people who say, "That rock was very strong, and it is the strength of the rock that really determines how large the cavity can grow." Then there are the people who say, "That rock was very dry, and therefore there was no steam, no gas pressure to push the cavity out."

Rimer: I don't like the cavity pressure argument, because even if you accept that Pile Driver was fairly wet - - at most there was a couple of percent of water in there - - the water would be all in the pores. I'll buy the dry part as increasing the effective strength; I won't buy it on the cavity pressure. There was just not enough water in Pile Driver. Hoggar core samples were sent to Livermore at some time, and the actual intact strength of that granite is very comparable to Pile Driver granite.

Cavity Shape

Presumably the shock wave that is generated by the detonation starts out as a spherical wave, imparting the same amount of energy per kilogram to all of the rock around the device. If the world were homogeneous the rock should move out uniformly and radially, leaving a spherical cavity. How good is that simple picture? Not very, it turns out.

Carothers: DNA has done some tunnel reentries of one kind and another. What can you say about the cavities themselves?

Ristvet: Well, we definitely know they're not spherical. We find that they seem to be fairly symmetrical in the equatorial radius when they're in virgin tuff, but they do snout down the grout filled drifts. The cavities do grow preferentially in the directions of the LOS drift and the bypass drift, which usually are where we have been tagging the radius. Whether that's a function of the mismatch between the strength in the grout and the strength on the tuff, or the high water content of the grouts so they sort of popcorn back in, we don't know. Those are the two leading candidates for an explanation.

We have two events where we probed the bottom of the cavity in the conventional manner. I think the one we did on Hunters Trophy confirms very definitely that the downward growth is less than the equatorial growth. Out the back in the equatorial plane the radius is closer to what the radius is below.

You would think, based on block motion phenomenology, that the in-situ stress field would have some sort of effect on cavity growth and create asymmetries. We don't see it in the data, or it's in the noise. I think our measurements have shown that gravity certainly has an influence on the cavity growth, and the calculations say it should. Where the surface of the ground is does, definitely. We like to use the equatorial cavity radius because that's the one of concern to us, and we can actually walk up and physically put our hands on it. Well, we used to be able to do that until the ES&H of today. The "Low As Reasonably Achievable" requirement makes it very difficult to do a reentry these days.

I'm glad we did the reentries we did when we did them. I think, without a doubt, that the reentries on Misty Rain, and then the subsequent reentries on the shots that worked well - - Middle Note, Mission Cyber, Disko Elm, and Misty Echo - - told us more about how well we were doing at predicting the phenomenology that we were trying to predict for containment than anything else. The Red Hot reentry was invaluable; without it I'm not sure we would have ever done Misty Echo.

Patch: The field folks have done a lot of work to try to look at the shape of the cavity in the vicinity of the stemming column, because that's where we potentially get unusual cavity shapes, because it's not a homogeneous medium. We're trying to put something in the tunnel there that fools the cavity into thinking it's still rock. How successful we've been at that is something we're very interested in.

Carothers: Mr. Patch, DNA has never fired in a homogeneous medium, and you know that.

Patch: Well, yes, that's true. But when we take the tuff out of the mountain and put something else in, it's even less homogeneous than it was. I would say that a lot of our interest in cavity shapes has been with respect to how they've interacted with the stemming. I think that, by and large, we've found that we tend to get preferential cavity growth in the direction of the stemming

column. We do perturb things, and we'd like to understand why that is, and we'd like to know how to perturb them less than we evidently do.

Carothers: John, presumably the cavity grows in a more or less spherical fashion. Or, at least it does in the calculations. Do you think the cavities are spherical?

Rambo: There was a shot called Clymer, which had a large opening above the device. We had three satellite holes with slifers in them, and I could track across those satellite holes and see how far that perturbation went off to the side. It was the first time we had ever actually looked at the shape of the shockwave changing with distance. That became a basis for understanding, or questioning, this idea about a spherical shockwave. It was an actual measurement to base that question on. It was the only time that had ever been done; actually showing the shape of the shockwave.

Carothers: Did those measurements show that the cavity, as it was growing, was not spherical?

Rambo: Yes, but that means that the energy, if it had gone up a line-of-sight pipe for a certain distance, was actually forming its own cavity at that point. Now, I've been biased by calculations I've done in past years where we've shown that things starting in that kind of configuration tend to get relatively spherical with time. But in the early stages, those cavities are not spherical.

Carothers: In the case you're describing I would think of it as looking more like a teardrop.

Rambo: A teardrop, or a bottle shape. Usually these shapes are fairly weak in terms of what stress waves start out from some opening away from the device, and the main body of the stress down below tends to overwhelm them at later times.

Carothers: Bob Brownlee used to be in the business of what LASL called hydro-yield. On Bilby, which was a shot of about 250 kilotons in Yucca Flat, he had three instrument holes. The working point was fairly close to the Paleozoics. He has said that he could see from the signals in those three holes that the cavity was not spherical; it had to have been teardrop shaped to match his data.

The shot point was close to the Paleozoics, so it didn't grow down much, and it tended to grow up more. That kind of a model gave a reasonable fit to his data, but a spherical cavity didn't.

Rambo: I would probably interpret it differently. You do occasionally run into weaker rock, in the tuffs, that tends to move a little bit faster, but not for terribly long. I would say it had to do with the material properties, particularly strength, which can make a big difference to the growth of the cavity. If there is strong rock below, and weaker rocks above, it can grow more in the upward direction.

I believe the material properties could make a big difference, but I don't hold to the idea that the cavity is going to, by its pressure, cause this change in how the growth is going to occur. Some people think the cavity is being driven by cavity pressure at late times, and I don't subscribe to that. I think it's really the strength and material properties of the rocks that can cause a funny shaped cavity. Those same properties can also affect the arrival times of the shock. Some properties may cause early arrivals in the shockwaves, but yet may retard cavity growth. But I certainly can believe a teardrop sort of cavity for a shot near the Paleozoics.

Cavity Pressure

Something which affects leakage through the stemming and the cables, and the possibility of hydrofracturing through the native material is the the cavity pressure, and its variation with time. One body of work, where pressures were measured on high explosive experiments in the tuffs of G tunnel was done by Carl Smith. For nuclear events, Billy Hudson developed a method of measuring the pressure in the fully formed cavity.

Smith: An important thing we could do on the high explosive experiments, which is much more difficult and expensive to do on a nuclear experiment, was to mine back to find out what went wrong with some measurement. We dug back in, recovered all the gauges, saw how good our grout jobs were, and we learned from all those things. For instance, there was a shot where we were trying to measure the cavity pressure. The pressure came down, and settled at about seven thousand psi. We thought that was a wonderful measurement, but when we mined back in we found that the pipe

was plugged. So, we knew the cavity came down to seven thousand psi, and leaked down from there, but the ground shock had jammed the pipe closed, and so we didn't see that.

That was confirmed on subsequent shots of that size where we measured significantly lower cavity pressures. A tamped eight pound shot will generate about eight thousand psi of pressure. As you go to larger and larger sizes of HE the pressures drop significantly. A sixty-four pound shot will develop about forty-six hundred psi. Thousand pound shots only developed about twenty-five hundred psi. We believe that's a rate effect in how the material responds, and how rapidly it responds.

Carothers: And when you go to kilotons?

Smith: You generate just over in-situ pressure.

Carothers: Billy, you have measured pressures in some of the nuclear cavities, have you not?

Hudson: I would claim that our experiments were the first to measure cavity pressure on nuclear shots through any significant fraction of the entire history. In the fairly distant past people tried to measure cavity pressure in conjunction with some other measurement, or some other experiment. As a result it was sort of a catch-as-catch-can measurement. In particular, they tried to measure gas in tubes that were designed to withdraw samples from the cavity. Usually those measurments involved flow from the cavity into the tube they were trying to make the measurement in. Usually that tube plugged. In fact, almost all you had to do was call the system a gas sampling system to be sure nothing came out.

There's more than one problem with that approach. You have a real problem if the tube plugs. Even if it doesn't, if you try to measure the gas pressure at the end of a long tube you have a problem because you're never in thermodynamic equilibrium. If you have a hot gas, maybe with water vapor in it, flowing in one end of a long tube, it condenses and cools, and by the time it gets to the other end the pressure is quite different from what it was at the opening.

We reasoned that the best way to make a pressure measurement would be to always have a very small amount of flow toward the cavity. If you measure the pressure at the source of flow, near your instrumentation package, and the flow is quite small, you could

argue that the flow at the instrumentation package is essentially the same as the flow at the end by the cavity, and the pressures are essentially the same. The tube shouldn't plug if the flow is always toward the cavity, and maybe you could get a pressure measurement that way.

And so, that's what we did. We filled the tube with fluid so we wouldn't have the thermodynamic equilibrium problem you have with a gas. In the first experiments we actually blew the fluid out of the tube, with high pressure, so we knew we had established a flow and we would hopefully stop the cavity growth process from plugging the tube. That worked very well, in that we got some data that at least looked as we expected it to look. We might not have been in direct communication with the cavity, but we were probably fairly close.

We tried the same experiment several times after that. I think we've done it successfully five or six times now. We've varied things a fair amount. For example, we've stopped blowing the fluid out with high pressure gas. That doesn't seem to be necessary, and it slows the response time. Not doing that also makes the experiment a lot less expensive. It costs a lot of money to have high pressure gas systems around, because they can explode and hurt people. If it's a high pressure liquid system, there's not much energy involved, and it's not nearly as much of a safety problem.

The first time we tried was on a DNA shot, and for a reason we don't understand, it didn't work. Probably it was fault motion severing the lines, or something. The first successful one was on a Livermore shot, and after that the DNA people were very anxious to have us try it on another of their shots. Fortunately, that one worked very well. Since then we have had another DNA experiment which looked successful, and three or four Livermore events where the data looked very good.

We've made enough measurements, and we have enough data now that we really think that system works to get cavity pressure. But if that's true, we still don't know why the history from one event to the next seems to vary so widely. So there are still a lot of questions to be answered with regard to cavity pressure.

Carothers: You describe the data as varying widely from shot to shot. What does the pressure history look like? There is the containment lore from the fifties and sixties that the cavity expands

until the cavity pressure is about equal to overburden pressure, and then it gradually decays through various cooling processes. Do you see anything like that?

Hudson: What we think is happening is that there is a sort of a plateau pressure, a constant pressure that is established after cavity growth, which then stays fairly constant for a while, probably due to ablation, mass addition, and so on. The energy per unit volume probably stays constant as long as nothing is leaking out.

Carothers: When you say it stays constant for a while, how long is that? Seconds, minutes, hours?

Hudson: That's one of the things that varies. On some events it's been minutes. On Cornucopia, on the other hand, it was more like hours. The period during which the pressure is more or less constant varies considerably. And, the plateau pressure itself varies considerably. We've seen it both well below and well above what we thought the overburden pressure was. We don't have a model yet.

Carothers: Perhaps the reason you don't have a model is because there is no good model of cavity growth, in the following sense. Cavities that have been reentered are not spherical. They are not the shape that you see on viewgraphs where the predicted cavity has been drawn with a compass. Cavities are lumpy, and some of them are sort of flat, and so on. On Rainier they did a lot of post-shot reentry work, and there was a very lumpy looking cavity. And so was the Gnome cavity. Maybe you don't know what the cavity volume and shape is on the various shots.

Hudson: That may be the answer. The surface to volume ratio may be important. And as you suggest, the contour of the surface may be such that on some events you may have a much greater surface to volume ratio than on others, and consequently you have different cooling phenomena. I don't know.

I think the reason it's so interesting, and puzzling at the same time, is that cavity growth and cavity pressure are the source function for the gas we're trying to contain. Yet for decades we basically have ignored this part of the problem, in terms of modeling. We've made very little progress. We have very little new information, because we've made only feeble attempts to get new information concerning cavity growth and cavity pressure.

Carothers: Well, cavities are different, and perhaps that's why your results are different from shot to shot. How different? Shape, almost certainly. As you said, surface to volume ratio. And then there are people who say that as the cavity is growing the material is moving out radially, and stretching tangentially, the pressure is high, and during that time many hydrofractures are driven out from the cavity. They don't extend a long way, a couple of cavity radii or so at most. But that exposes a large surface of cold rock, and that cools down the cavity, dropping the pressure. A person coming from that point of view might say that the rocks in different places have different fracture susceptibilities, and so, different energy loss histories. And therefore, different pressure histories.

Hudson: That might very well be.

Cavity Temperature

The temperature of the cavity starts at a very high value, a million degrees or so, but it drops very rapidly as the cavity expands. The only real information on the temperature and its time history is derived from examining the detonation products that are separated at different times from the main body of the material in the cavity.

Higgins: At the very high temperatures very near the explosion the transport of energy is very rapid. In other words, after the shock has gone by the particle velocities are high enough that there is rapid communication of temperature and pressure between the center of the expanding gas and its more outward regions. That goes on for some fair part of the first part of the cavity growth. So, the temperature in the cavity gas goes down to some temperature that is considerably less than the electron volt, or the ten thousand degrees, that many of the calculators are fond of putting on their pressure versus time charts.

I feel that's a misleading kind of calculation. All of the evidence from the cavity radiochemistry - - from the fractionation of the various radiochemical species in recovered products - - points to the fact that the temperature in the cavity, by the time the rebound occurs, which is, let's say, in the time between milliseconds

and seconds, has decreased to the point where it's not much above the melting point of the rock. It's certainly below the point where there's any rock vapor left.

That's important, because it fixes the maximum threat. The dynamic phase is going on as the shock wave passes out and leaves this hot stuff behind. The rebound comes back, and that happens within a few hundred milliseconds. Because of the rapid exchange of energy in the cavity up to that time, things have cooled until it's pretty much in equilibrium; the energy is distributed throughout everything that's within that cavity radius. My argument has been that the initial temperature, for calculations, can't be much different than the vaporization temperature of the rock. If it were higher more rock would vaporize until it did reach the temperature of vaporization.

Carothers: That seems to be a reasonable argument.

Higgins: But it's hard for people who do one-dimensional calculations to accept, because the inside zone in their calculations is always at ten electron volts, which is a hundred thousand degrees, after the cavity expands. The reason it doesn't stay that way is that anything that is at ten electron volts is very reactive. It's going to go out and heat up the next thing that's nine electron volts, or one electron volt, and that time is short compared to a few hundred milliseconds.

The difficulty with this whole discussion, from a physics standpoint, is that energy transport in this region of, say, two-tenths of an electron volt, or three-tenths of an electron volt, is something no one wants to deal with. The times, the opacities, the reactions that are going on as things recombine, and you get ionized states, and sometimes molecules that are two or three electrons deficient are all things that are not easily calculable. In fact, they are pretty much, as a general rule, unknown. So, nobody wants to calculate it because nobody likes to work on a problem that doesn't have a nice solution. Scientists don't like non-solutions.

I believe that all of the evidence points to the fact that by two hundred milliseconds, or even one hundred more likely, the cavity has cooled to about two thousand degrees Kelvin. As the cavity is cooling down, the pressure is dropping, and so everything is cool enough that the cavity gases don't have enough pressure to drive fractures.

There is one more phase. The wall of the cavity has a huge temperature gradient in it, and I believe that what happens is that the water in the rockock volatilizes and pops pieces of the rockback into the cavity, causing further cooling down. The pop-back is where the water that is caught in the pores turns to steam and expands. Water going from water at one cubic centimeter per gram goes to twenty cubic centimeters per gram if it goes from three hundred degrees Kelvin to four hundred degrees Kelvin. And that's considerably below the two thousand degrees that might be only a few centimeters away. So, I believe that there is a period of exfoliation that's quite rapid, occurring after the first hundred or so milliseconds, but before a second.

The pressure doesn't change much, because you're adding more mass and more molecules. The temperature goes down because you're taking energy out of the molecules in the cavity and warming the incoming material until it's in equilibrium. The glass has fallen to the bottom of the cavity, together with a lot of rubble, and it's now at about the melting point of the rock, between about eight hundred and a thousand degrees centigrade. And that is the cavity we explore when we go back in and drill or mine.

Sometime a lot later, and it is an unstable thing, the roof of the cavity just falls in. It might even start to fall in when that first pop-off of the water vapor in the water pores occurs. If the rebound has been strong enough so there is an arch formed, then it will stay there for a while, whether an hour or a day, I don't know. If that arch is not very strong, especially in alluvium, I think the blow-off of the popcorn might very well start the chimney. If it's real close to the surface, that's what happens. And that's why we see, in certain other kinds of special circumstances, where you have reflecting surfaces nearby, very early collapse, in a few minutes. Those are the cases where the cavity collapse is initiated by the blow-off of the water in the cavity walls, and those are really dangerous, because the pressure is still fairly high.

Where Does All That Material Go?

Carothers: John, when a detonation occurs, a big cavity forms. What happened to all the material that used to be in the cavity?

Rambo: In our calculations it's displaced outward. We see positive outward displacement. So, you've taken this volume and you've distributed it. If the material has porosity in it, which it usually does, some of the volume is taken up in crushing that out. Although, when I run a completely saturated calculation I still get a cavity of about the same size. The calculations tend to show some positive displacement everywhere. The material has to be either compressed, or move out. I think the outward motion is mostly where it goes, instead of in crushing the material. And, the surface can move up just a little. There are some cases, though, where it looks as though you're moving out material to make the cavity, and then the surface is lower too. So, yes, where does all that material go?

There was a shot we fired, called Carpetbag. From the gauges, the surface was displaced down, compared to where it was before the shot. That's always been a mystery. I tried to deal with that, and I was unable to get the gauges to match the big negative displacement fairly close to the cavity. I didn't see that in the calculations. Another thing that happened on Carpetbag is that the surface kept sinking for many months after the shot. It was quite an interesting phenomenon. I don't think we've seen anything quite like that in other areas.

Carpetbag was below the water table, and so the material was wet. I think the material must have been a matrix which was quite wet and quite weak, and that just the slightest hit from anything would have have let that matrix rearrange, and relieve that whole area.

Carothers: Dan, where do you think all the material that was in the cavity goes? Cavities are pretty large. Even for a kiloton or so you could put this building in the cavity.

Patch: Let me say where it doesn't seem to go. One can easily imagine what you do when you grow the cavity is basically to crush the rock out to some radius. That seems to be a reasonable picture for something that's got a lot of air voids in it; dry alluvium, or something of that sort. But, our experience in wet tuff is that if you go in and take samples post-shot it's very hard to see that you have what I will call a completely compacted region, even quite close to the shot.

Now, we have seen, certainly on some events, where it looks like you're getting this air void back. Preshot you have a one percent air void. Post-shot you get these samples out and you measure them, and they still have one percent air voids. You ask yourself, "How can this be?" It's a very strange thing to have happen. But if you look at the details of the crush curve, you'll see that this one percent air void only takes a little load to crush it out.

Terra Tek has done some beautiful work where they've looked with this technique of injecting metal into the open pore space, and then etching the sample and looking at it with a laser. It's very interesting work, and you can see that what has happened to the rock is that the stresses have generated lots and lots of very fine fractures, so when you take the sample out of the ground there's a tendency for these little fractures to open up a little. It doesn't have to be much to get the one percent back, although that's a different kind of air void. So I think the rock actually does take up some of the cavity volume, but it's darn hard to prove from the data.

I don't think anybody can conclusively say, "Yes, see, this rock used to be one percent air voids, and now it's smashed." But we do have suggestions that there is some cavity growth that is accommodated by the crushing of the material. I think what you basically do is you deform the material around the cavity, and because of the r-cubed effect it turns out once you get a little ways away from the cavity you're talking about a very small amount of deformation over an enormous amount of material. The cavity has a lot of volume, but how much more volume do you have when you go out three or four cavity radii.

Following the Rainier event there were extensive reentry observations made. From observations made in early 1961 Ross Wadman and Bill Richards (UCRL 6586, July 1961, Postshot Geologic Studies of Excavations Below Rainier Ground Zero) made this comment: "Block movement rather than rock compression accounts for the rock displaced from the cavity. The rock moved radially away from ground zero along shock produced shears that, in many cases, were strongly influenced by preshot zones of weakness. The lithologic rock units, below the cavity have been thinned and depressed but not appreciably distorted or mixed.

Rock Melt and Non-Condensable Gases

Carothers: A kiloton of rock melted per kiloton of yield is a number often mentioned, but there are other numbers used sometimes. How much rock does get melted, and how do we know that?

Higgins: Well, the question of how much rock gets melted is an awfully good one, and the how do we know is an also good question. The methodology was, and is, to take a piece of rock that was melted by the detonation, do chemistry on it, and determine what fraction of some chemical species that is unique to the explosion is found in that piece of rock. Then you presume that fraction represents the fraction of the total melted rock, of which you have a little piece.

Carothers: So, if I have a piece of the solidified melt that weighs ten pounds, and I find a millionth of some device-produced isotope in there I say, "There must have been a million times ten pounds of melted rock."

Higgins: Right.

Carothers: Isn't that rather presumptuous of you chemists, to make such a large extrapolation?

Higgins: Well, yes, it is rather presumptuous, but after doing literally hundreds of samples we have found that the answer each one of those hundreds of samples gives is essentially the same. But not always, which is why I said that it is a very good question. It is still an open question. However, there was a time when everyone thought they knew that answer precisely.

Carothers: To know something precisely is to calculate it. An experimentalist never knows anything precisely.

Higgins: It's almost that bad. From the earliest samples there were definitions people tried to follow. There were several kinds of melted rock recovered, and one of them was called "puddle glass." Puddle glass was defined as being a non-vesicular, black, shiny, glassy material.

From the first few hundred samples it was found that the numbers one obtained for puddle glass per kiloton were remarkably

consistent, and constant at about eight hundred tons of puddle glass per kiloton of fission. Or, if you wanted to be more approximate, a kiloton for a kiloton of yield.

The other kinds of glass that were found, which were called variously chimney glass or frothy samples, gave numbers which were more scattered. In the range that I've seen they were from about two hundred tons per kiloton as the very smallest number, up to as much as three thousand tons per kiloton. And I would guess that even larger numbers could be measured if you took a chunk of rock that you could not visually identify as glassy melt, and analyzed it by that same technique.

Carothers: When you talk about determining the amount of melt by taking a very small sample, determining what you believe to be a fraction of some isotope that was produced, and then multiplying that small sample mass by the supposed total amount of that isotope, you're doing the same kind of thing you do with cloud sampling on atmospheric shots. You're making the assumption that things have been homogeneously mixed, and that you've got a representative sample.

Higgins: That's right. And the only proof of whether that is true or not true is from internal tests. One such internal test is to look for a fraction of the fissile material, for example, as compared to the fraction of an external tracer, and look at the variability of one to the other in the same set of samples. If they're widely different, then we could guess that none of the isotopes are representative of the total.

Carothers: The process is similar to what you do in atmospheric cloud sampling, but it must be a harder problem. When you sample a cloud after an atmospheric detonation, you're only trying to determine the bomb fraction; you're not trying to determine the size of the cloud. Here you're trying to determine the size of the cloud, as it were.

Higgins: Yes. But while the numbers weren't published often, we also determined the size of the cloud in atmospheric testing. And, a rather surprising number is that a kiloton of lofted material per kiloton of yield is valid for an atmospheric burst, as long as the fireball touches the ground. That was an astounding discovery.

Carothers: You're beginning to sound as though a kiloton per kiloton is a magical number.

Higgins: Yes, it almost sounds that way.

In the beginning of underground testing we used symmetrically placed tracers in the ground zero room. We put them at the corners of a cube, or perhaps an even more ordered symmetry than that. We put them at points representing the faces and corners of a cubic array, for example. We found there were exceptions to perfect mixing for very small yields. But at about one or two kilotons and above, it really didn't make any difference if we put in six tracers, or one, or four. We got essentially uniform mixing.

The implication of all that is, there is a mixing of vaporized material in the early cavity, while it's growing. The particle velocities are very high, and the particles in the growing cavity make many, many transits across the gaseous region before it stabilizes and starts to condense. If that weren't true, putting a tracer on two sides of the device would give different results in the two directions in the final cavity, and that was never seen on larger yields. On very small yields we did find pronounced asymmetries.

During the 1960's we did experiments where we had open holes below the device, to try to separate the radioactive debris from the center of energy. We even built rather unsophisticated reflectors, and those were successful to a degree. But if you think about it a little bit, if you deflect all these very hot fission products, they are hot enough to interact with their surroundings and cause new gas to be formed, which then mixes back in the direction the fission products came from.

In one experiment we put several hundred grams of U233 in the bottom of a hole which was open to about two hundred feet below the device. In the glassy material that was recovered we looked for just the presence of U233. What we found was as large a fraction of the U233, which was two hundred feet from the device, as of the device material itself. So, the 233 bounced out of the bottom of the hole, back up the hole, and mixed with all the gaseous material pretty uniformly. While we got a big fraction of the total radioactivity directed down the hole, what was in the bottom of the hole mixed very well with those things that were where the device went off.

What was implicit in the results of those experiments was that the glass was a consequence of a multistage set of vaporizations. That is, the initial device energy vaporized material, and the shock wave generated from that vaporized material continued to form more vapor outside of that region. So, simply directing the first vapor down the hole didn't do anything at all about the material that was being generated by the expanding shock wave. Very crudely, what those experiments showed was that the vapor first formed was about seventy or eighty tons per kiloton, and that the additional melting and vaporization made up the other eight hundred or nine hundred that we observed from the total sample later on. The surprise, I think, was that such a small fraction of what was finally melted and vaporized was produced by the device itself. It was about one tenth, or a little less.

Carothers: The cavity is growing, and there's some vapor in there; pretty dense, but vaporized material. You are saying that to the particles it is a thin vapor; the mean free paths are long. It's diffuse enough that the particles can move freely through it.

Higgins: The conclusion is correct, but to think of it as being very thin is probably not correct. A better way to think of it is as an extremely hot region where the particle velocities initially are very high - - like eighty centimeters per microsecond.

Carothers: And everything is highly ionized. Since the atomic scattering cross sections are large compared to nuclear scattering, if you strip the atoms of most of their electrons the mean free paths becomes quite long.

Higgins: That's precisely correct. You have particle velocities approaching many tens of centimeters per microsecond. The vapor density, including atoms and electrons, is grams per cubic centimeter, or some significant fraction of that, but with such high velocities the transit time for any sensible number of meters is not long. The expansion time of the cavity, whether it's from a kiloton or a hundred kilotons, is in the order of a fraction of a second up to, for the very largest yields, a second. So, when the particles are going many centimeters per microsecond you have time for a lot of transits across the cavity, and bouncing around, and scattering, and normalizations of the various regions with each other.

Carothers: If you keep getting consistent numbers from the puddle glass, that also would imply that it doesn't matter much what the original material was; tuff, or alluvium, or basalt, or whatever.

Higgins: Yes. That was the early conclusion, and the early experiments verified that, in a way. Our first experiments in granite, which were Hard Hat and Pile Driver, produced slightly less melt, but not so much so that one would say it was a different mechanism. I believe, in retrospect, and now that we've looked at material from a lot more sites in the tuff and alluvium, that those were spuriously obtained results. It really isn't true in general that the same amount of rock is melted per kiloton at different sites. That was an accident of the composition of the materials. Ted Butkovitch and several other people have more carefully measured some of these same numbers. They add the so-called puddle glass to all of the other glass and ask "How much was heated above some temperature?" The usual temperature they use is a thousand degrees centigrade. And they find that number varies with porosity and water content.

Carothers: The more water, with its high specific heat, the less melt?

Higgins: No, it goes the other way. The more water, the more molten material there is. The reason that's true is that water and almost any silicate rock or compound form eutectics that have melting points that are sharply less than the melting points of the pure rock.

In our initial work we'd always go to the laboratory and carefully dry the samples. Then we would measure all of the things like melting points, and vaporization temperatures, and so on. That turns out to be a gross mistake. We discovered the hard way that when you're dealing with the earth's materials, water is an intrinsic part of the system. To remove it distorts all of the results from that point forward.

There were things that people had worked on for a long time that were changed and amplified by the work at the Test Site. I don't mean that we've been that remarkable in our science in underground testing, but it really wasn't until we began to look at the molten rock formed by nuclear explosions that volcanologists examined their numbers to determine what temperatures existed in the earth to form lava. Prior to the underground tests the

volcanologists did the same thing we did. They dried out their lava samples, and said, "This lava came out of the ground at fifteen hundred centigrade." Then, when they went and looked at the volcano, what was coming out of the ground was coming out at nine hundred degrees centigrade. And they found some lavas, in western Colorado, that indicated six hundred degrees centigrade. How in the world did those volcanos produce that molten rock at six hundred degrees when everybody knows volcanos start out at fifteen hundred?

So, there were elaborate theories about secondary melting producing two or three times more lava than the primary vent produced, and that meant you really had to reduce the measured volcanic flows two, or three, or four-fold, because what you saw really wasn't the amount that came out of the volcano itself. The theory was that what was produced was really a lot less than what you saw, but it was so hot that it melted a lot of other rock. There were a lot of things like that floating around in the literature.

Now, the tuff at the Test Site came out of a volcano. And when it came out, it came out as a solid, even though a pretty hot solid. Some of it came out as a liquid, but not very much. But water condensed into this hot solid almost immediately, and then it melts at around eight hundred or nine hundred degrees centigrade. If you take the water out of it, it melts at fifteen hundred or so. We were extremely puzzled by that until we began to do some experiments at modest pressures, keeping some of the water in it. And lo and behold, the more water that was in it, the lower the melting point. And, of course, the lower the melting point the less energy it takes to heat it to melting.

The point is that the amount of melt is very much dependent on the amount of water that is present - - the more water there is, the more melt, and the less water, the less melt. So, when we said there was the same amount of melt from granite and tuff, we were looking at only that portion of the tuff melt that made puddle glass, and comparing it with the total melt from granite, where all of the melt was essentially puddle glass. They turned out to be very close to the same amount, but that was a fortuitous accident. The total melt from a a detonation in tuff, we now know, varies with water content, and it goes from a low of about a thousand tons per kiloton up to about three thousand. Somewhere in that factor of three, all of the experiments that we've looked at fall.

Carothers: How could it get to be as big as three thousand tons per kiloton?

Higgins: By the inclusion of all of the secondary melted rock. And there's another thing to remember; the rock vapor that's initially produced is much hotter than the vaporization point of the rock. Much hotter. So, a little of that rock vapor can go onto a cold rock and vaporize it too, and still the total is maybe right at the vaporization point. The heat capacity per unit mass of rock vapor is not very different than the heat capacity of the solid rock itself. Slightly less, but very slightly less. So, if you have a gram of rock vapor that's three thousand degrees above the vaporization point, it can very happily vaporize two more grams of rock, and you'll end up with three grams at the vaporization point. That mechanism probably accounts for a lot of the molten material we see post-shot; the secondary vaporization and melting.

Carothers: The material you find in fractures?

Higgins: Yes, or even in the puddle glass. The way we get the occasional sample of the initial rock that's vaporized by the shot is from the material that's frozen out in fractures. When it goes into a fracture it is essentially frozen instantly. Something that gives us information about this comes from the tunnel line-of-sight shots. When the pipe closure fails drastically, a little tiny fraction, a solid angle's worth if you like, of that initial vapor gets directed out a very long distance. We've occasionally, unfortunately, seen that happen. It cools off, and it has so little total energy that it can't cause any more melt. When you work back, it always turns out to be between seventy, eighty, or ninety tons per kiloton. That does kind of prove these speculations that are done from calculations, and thermodynamics, and some other arguments are correct.

Carothers: When you say seventy, or eighty, or ninety tons per kiloton, you mean that's the initial amount of vapor that's produced directly from the device itself?

Higgins: Right. That's the initial rock vapor. Of course, the device doesn't really make much contribution to that mass. In the early days we used to say that we could approximate the device by putting a ton of iron where the device was, and that was a good approximation. If you mix in all of the construction materials and the canister in with the device, that's about a ton. And if you mix

the molecular weights, starting with ninety-two for uranium and one for hydrogen, iron is about right. It's sort of the geometric mean of everything that's around.

Carothers: We have talked about the amount of rock that is melted per kiloton. When that is melted, how much carbon dioxide is produced?

Higgins: Well, that is part, but just part, of the problem of non-condensable gases produced by the explosion. Carbon dioxide is sort of the generic name for the non-condensable gases produced. It's clear there are quite a few of them, and the reason they are important is that when we say that we can contain the explosion, we really mean that we can contain the gases that carry all the various radioactive materials.

First, there are the vaporized rock gases. They condenses really rapidly because they go from vapor to liquid at three thousand centigrade or so, and there's a lot of cold rock around, so those vapors don't go very far. The next least condensable thing is water, and steam can go a little bit further than the rock vapors. When, on the rare occasion the steam gets out it's pretty catastrophic, and it's very spectacular. Those events are very distressing to the containment people. There are tons and tons and tons of steam present in the cavity prior to its condensation to water. If it finds a path out it can carry large numbers of curies, usually on the order of a hundred thousand curies of radioactive material per kiloton, out with it.

That is sort of the last violent level of non-condensable gases. Below that, on a scale of colder and colder condensation, there are carbon dioxide, and methane, and hydrogen, and other permanent gases at room temperature, that are produced by the thermal decomposition of things that surround the explosion. On some of the early tests we observed that the test would be contained, including the rock vapor and the steam, but that on surface collapse, or on a time scale of a few tens of minutes, or hours, following detonation there would be clouds of radioactive gas rolling around the region of the shot. They were invisible, but they carried what turned out to be a large numbers of curies of radioactive gases.

Now, fortunately, in all the cases that were documented, those gases were noble gases, and they are biologically inert. There was great concern at the time that they might contain radioactive iodine, but in spite of intensive efforts no great amount of iodine was ever found in those gases.

Carothers: You'd think there could be. There's the iodine-xenon link.

Higgins: Yes, and there's lots of xenon, but almost no iodine. It's a fortunate fact of the decay sequences that it happens that way.

When we examine those unfortunate experiments, and look for reasons for that radioactive gas, there was first the association of certain areas of the Test Site with that phenomenon. The next step was, what is different about those areas of the Test Site? It was found, number one, that the bad experiments always occurred in the alluvium, and not in the tuff. Number two, they always, almost, occurred in regions where the alluvial material contained large amounts of Paleozoic carbonate gravel. And the worst ones were from Area 5, which means the Frenchman Flat side of the pass when you go out to the CP. There were others, from the far north end of Yucca Valley; Area 2, Area 10, and to a lesser degree, Area 8. In examining those, the presence of carbonate rocks was observed.

The carbonate decomposes at high temperatures, and produces carbon dioxide, which then displaces the gas that's in the pores in the rock, as in a sponge, and pushes that out of the way. As soon as it pushes all of the gas out of the way all the way to the surface, seepage will occur. If there's enough air-filled porosity between the detonation point and the surface, it will push out until it's expanded to atmospheric pressure, and then it'll just stop.

Carothers: Presumably there is some association between how much carbonate rock is present, how much of that rock is melted, and how much carbon dioxide will be formed. Does anybody know that, or do they just estimate it?

Higgins: I'd say that at the present time it's an educated estimate. What has been measured is the temperature at which the carbon dioxide is given off, and that is somewhat less than the melting point of the rock. It's like six hundred and fifty, or seven hundred degrees centigrade.

Carothers: Then I would be right in saying, "Well, if there are eight hundred and seventy tons of melt per kiloton, all the carbonate in those eight hundred and seventy tons is going to be decomposed."

Higgins: Yes. A nitpicker would say it should be a little larger than that, but not very much. The question is, where is the carbonate rock? If you move the working point into a layer that doesn't have any carbonate, you would say there wouldn't be any carbon dioxide generated. That isn't quite consistent with the observations. The reason is that when collapse occurs, if there's carbonate right above the cavity that can fall into the hot cavity, some of that can get decomposed. But, it would be much smaller than if the working point were in that carbonate region. So, those little qualifications notwithstanding, it's the general assumption that the amount melted is the amount interacting to form carbonate.

The other kind of non-condensable gas is that formed chemically by the reaction of metals with water to release hydrogen. It could be the iron or aluminum in the canister, or a lot of other things. One of the more exotic is boron carbide, which can interact with water; one boron carbide can make seven hydrogen molecules. But the chemistry is the same. It has to be in the melt region to be hot enough, and be mixed with water, which is steam under those circumstances. Of course, there's plenty of water around. If you want to approximate the world you take silicon dioxide, plus water on an equal molar basis, and that's pretty good. You're only making second and third order corrections to put in the calcium, and the aluminum, and the carbon, and all the other stuff.

Carothers: Funny, Gary, that with all that silicon around we ended up carbon-based.

Higgins: Isn't it? There is one little fact of nature, however, that says silicon was important. That is that the sense and orientation of the DNA molecule is identical to that of the silica in a glass structure — I learned this fact from Bill Libby. And I maintain that DNA got its pattern by being on a clay particle, and that the first live reproducible virii came from clay. That is, they were organic molecules that got the pattern, and replicated off a piece of clay.

10

Cavity Collapse, Chimneys and Craters

There are many observable things that occur after a detonation takes place, and the cavity has reached its full growth. At some time the roof of the cavity gives way, and the overlying rock falls into the cavity volume. This fall of material sometimes causes the surface to slump and form what is called a crater, although purists call the subsidence that occurs a "sink." A crater is something that is formed when material is ejected from an area, and there are a few true craters on the Test Site, the Sedan crater being the most impressive example. Here sinks and craters will all be called craters, bowing to the overwhelming majority who use that nomenclature.

How and why and when cavities collapse, what the conditions are in the column of displaced material that often, but not always reaches the surface, and the reasons for the shape and sizes of the surface craters is largely unknown. There are some correlations that can be inferred.

Keller: It was in using the data bank I had put together that I discovered the correlation between crater dimensions and yield, and some other things like that. One thing I noticed was that the line-of-sight pipe events always collapsed much faster than the others. And I also discovered that there was a good correlation, if you presume bulking, between the dimensions of the cavities, as best we knew them, and the dimensions of the craters, and the yield.

You would intuitively think there had to be some correlation, except it was popular then, and even now in some peoples minds, to think that compaction was equally as probable as bulking during the chimneying process. Since then the subject has been picked up at Los Alamos by Erik Jones at one time, and Tom Weaver, and Tom Kunkle, so there have been three more resurrections of that subject. Each time there was a larger data bank and better statistical techniques for analyzing it, but nothing new was discovered; it was only refined. One surprising thing was that there was an amazing lack of scatter in the fits to the data.

The thing I always liked about the crater dimensions was that they were the best known features. They used to do very detailed contour mappings of the craters, and so you could really tell exactly what the volume was. And there was a pre-shot and post-shot difference map. Today you don't know that as well because they don't do that before and after comparison.

It turned out that the depth of the crater, not the volume, was the most sensitive characteristic of the crater with regard to the yield. The crater radius was the first order correction to that, and still the volume wasn't. I'm not sure why that's true. Many people tried to relate the crater volume to the yield. They got very poor results, so they were just turned off by the whole concept, and were rather outspoken about how you couldn't tell anything about the event from the crater dimensions.

Carothers: Craters come in a lot of different shapes. There are ones people call post holes, others they call dishes, and there are various other shapes. How can it be that the depth of the crater, which seems to be so variable from area to area for equal yields, tell you anything about the yield?

Keller: Well, let me tell you what the simple relationship was. The first thing I took was a column straight down the middle of the chimney. I took the height of that column before collapse to be from the top of the cavity to the surface, and after collapse to be from the bottom of the cavity to the bottom of the crater. Any difference in that dimension before and after collapse was bulking or compaction of the rocks in the chimney. I expected that there would be convergence of that because of the slumping you see in craters, and also because of the collapse of the cavity into the bottom. And so I expected bulking, and I just plotted that bulking factor, the ratio of those two columns versus the depth of burial. There was a lot of scatter, but it was not nearly as much as I had expected. This bulking factor was very high for low depths of burial and yields, and it asymptotically approached a value of about eighteen percent, as I recall, for high yields and very large depths of burial. That was the first clue that there was reasonable order.

The thing that defeats the argument about there being compaction is that there is a very clean cutoff between events that breach the surface and those that don't. It's a scaled-depth-of-burial cutoff, and it's relatively sharp. If you had compaction sometimes,

it wouldn't be that well defined. Basically you're forced to believe that bulking does occur every time, and that the bulking actually limits how far the chimney will propagate.

Then I tried the crater volumes and that didn't help. I looked at the crater radii to see if there was any correlation there. Now, the depth of the crater will give you a yield but it may be too high or too low. But there seems to be compensations to the extent that if the depth is too great for a particular yield the radius of the crater is too small. And so, there are skinny craters and there are extra wide craters, but I could correct the yield I determined from the depth with the yield from the radius, which is not so well behaved.

The craters that form at some long time after the shot, after there has been a collapse that didn't reach the surface, and the area is presumably stable, have been a threat to personnel that wasn't fully appreciated for some time.

Miller: One time we had something that was almost like what happened up on T-tunnel a few years later, where people got hurt when it collapsed. We had an event in Area 2, and it used to be they didn't fence the GZ, and this hole had no fence. We were doing angle drilling, and we were rigging up the post-shot rig. Part of our equipment were these big blowers to suck air to the cellars in case we had a release from the drilling. This teamster drove up to the location, and he's driving a big rig. He drove all the way to the GZ almost, turned around to get spotted, and as he's returning the ground collapsed. The float with those blowers went into the crater. Fortunately it was not a cookie cutter; it was a saucer shape. Part of the tractor wheels went into the dirt where it cracked, and the tractor couldn't move. This guy jumped out with his hard hat and his lunch pail, and just ran like hell. We took another tractor, or dozer in there, and grabbed hold and pulled the whole thing out. Fortunately nobody was hurt. It was close though, very close. That was the event that caused us to start fencing the ground zero area.

Keller: On the accident on Rainier Mesa with Midas Myth, when they dropped the trailers and some people in the crater as it formed, the same order that I had found earlier for crater formation was relevant to that event. Midas Myth turned out to have one of the smallest scaled depth of burial for events in Rainier Mesa.

Although they'd existed on other events on the Mesa, craters had not been seen, or recognized. They were just so shallow that they were not noticed.

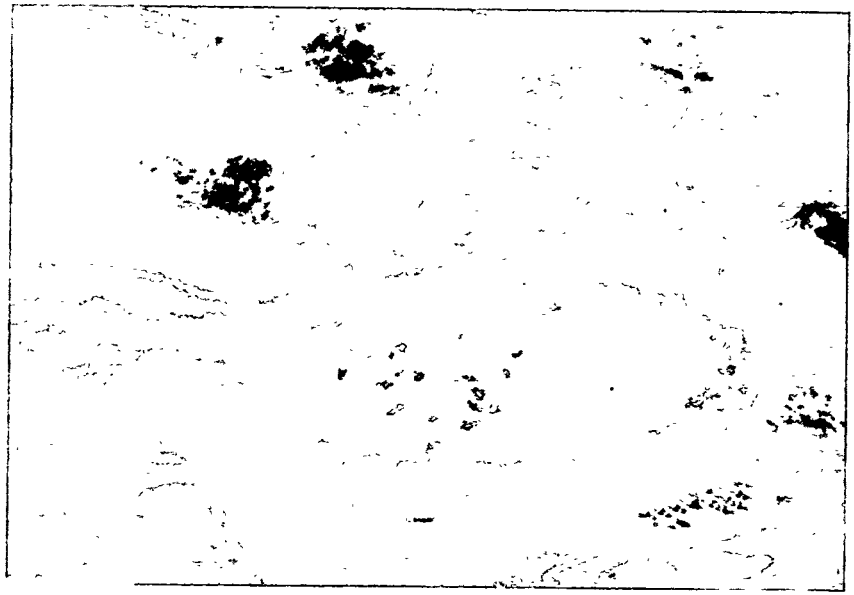
Carothers: Roy, did you ever do any drilling work on Rainier Mesa?

Miller: Oh yeah. There was a shot in a vertical hole there called Wineskin, in U12r, in '69. That had a surface collapse. The fact is, after the collapse they had where they dropped the trailers in the crater, they said there had never been one on Rainier Mesa to collapse to the surface. Ken Oswald took them all up there and showed them that surface collapse.

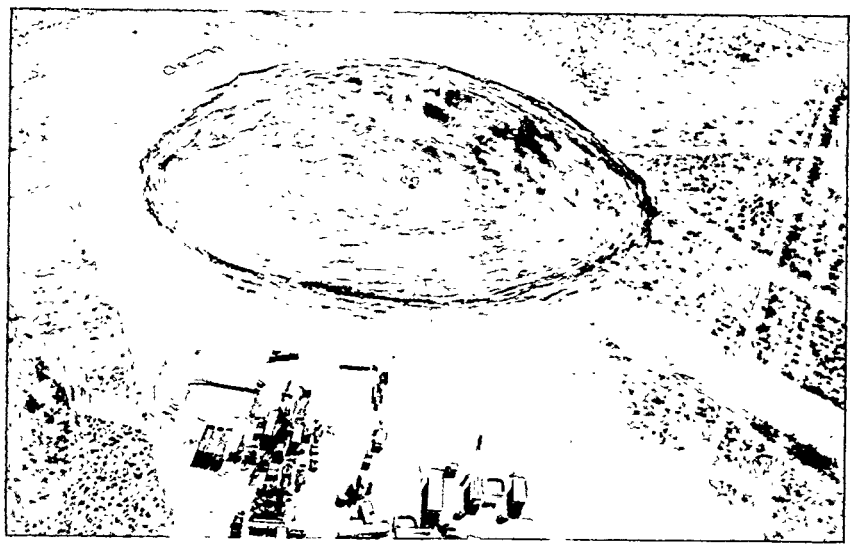
Keller: When you plotted those events that had been shot in Rainier Mesa, and whose chimney heights were known, Midas Myth fit right on a nice curve. The chimney height was just right on the line, and the chimney height would be above the surface, which gives you a crater. At the time, the folks who thought the shots on Rainier Mesa never cratered were not aware of the surveys that had been done, and that showed the shots did crater a little bit. And so it was a matter of not knowing what they didn't know. And one did not normally put trailers at ground zero. It was an unfortunate incident. There is a lot more order to this data than some people are willing to believe, and so within some uncertainty it's a very useful way to look at some things, such as yield, or crater formation.

One thing that's evolved most recently out of that is that Tom Kunkle and I looked at large yields, and we found that some of the crater volumes were larger than that of the cavity inferred from the measured cavity radii. Of course, if bulking is existent in every shot, you can't have a crater volume that's larger than the cavity volume. We looked at that more carefully, and since I believe they all bulk and there's no reason to believe that the large yields ought to compact, the cavity inferred from the crater dimensions is a simple constant times W 1/3rd, in radius.

The conclusion you have to come to is that the radius measured in the downward direction is not characteristic of the cavity volume, and that the cavity volume is larger than that measurement would suggest. And there are good calculational reasons to believe that. There are calculations that have been done that show stress gradients, and the refractions from the surface, tend to allow growth in the upward direction. And so it's in light of that conviction, unless

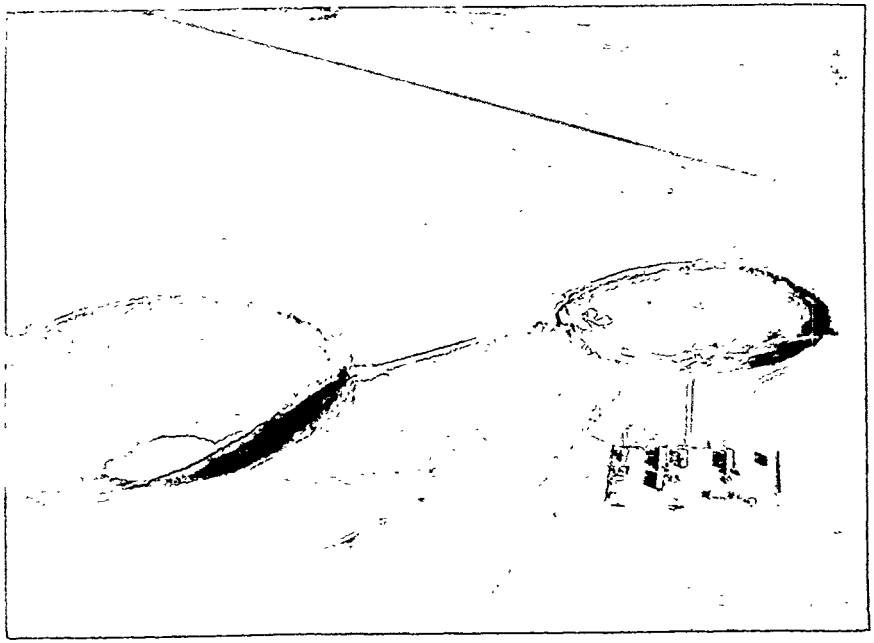


Collapse has just occured, dust cloud from the collapse.

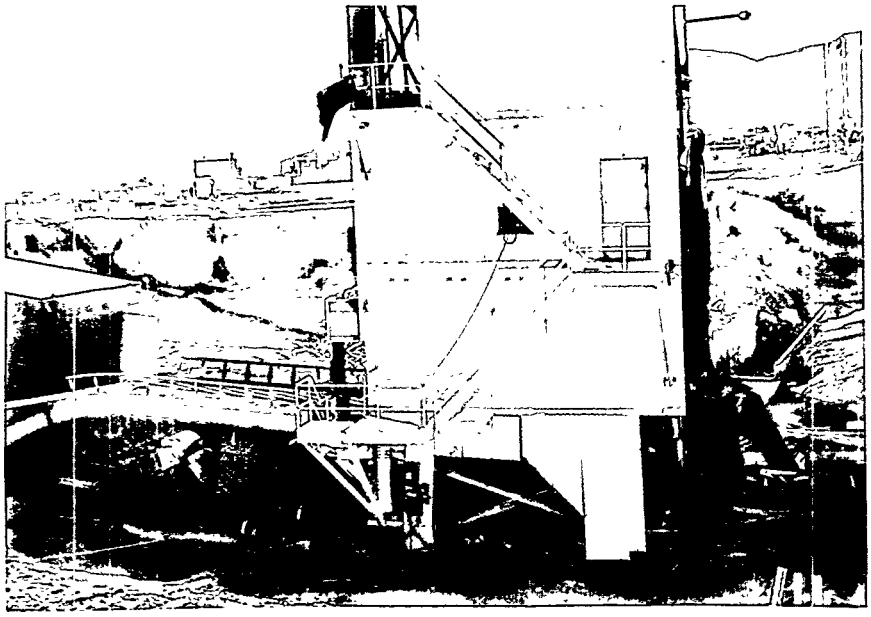


An almost perfectly circular dish-shaped crater.

268b



Hutch (left) and Flax subsidence craters. Two closely spaced events.



Equipment near surface ground zero, after the crater has formed.

you're real near the surface and you get a strong relief wave, and you crater, that cavities really are pretty much a constant times W to the 1/3rd. Now, they are occasionally bigger. They're obviously bigger, as measured, in the southern area of Yucca. Not only is the measured cavity radius larger in that area, the craters are also larger, which supports that correlation.

Carothers: You can look at the crater, and you can do surveys, and you can get the dimensions as accurately as you care to pay for. On the other hand, the cavity radius, if there is such a thing, is in fact poorly known. They drill down, poke into an uncertain spot where the cavity used to be, and somebody picks a radius based on some level of radiation there. The impression I get is that no one believes those reported radii are very accurate. How did you deal with that when you tried to compare the crater depths before and after the shot?

Keller: I presumed that there was uncertainty in the radii, and that the crater volumes were known better than the cavity radii. So, I went back and calculated yields from the crater dimensions, and there were a few yields that were way off. One of them was Bandicoot. Since then they have gone back and re-drilled it, and found that the measured cavity radius is larger than originally reported. There's also reason to believe that the actual yield was substantially higher than the published yield. And so, those kinds of things show up.

There are a couple of events where it's probably worth noticing those differences, and one is Merlin. Merlin is the shot from which the samples were obtained for which all material properties for alluvium are based these days. "Merlin alluvium" — you hear it all the time. Well, those properties are deduced from a presumed yield for Merlin. I've never really pushed it, but I suspect that the Merlin yield as given isn't correct either.

It was like '67 when I first developed this correlation, and Bob Brownlee was excited about it, or seemed to be anyway. He took it to Charles Brown and said, "Hey, here's a way to measure yield." And there were a couple of folks who said, "That's just because we always bury shots at the same scaled depth, and you use depth in there, and that's why you get that." Well, thats's not true. If you just knew the depth you wouldn't get nearly as good a correlation. Another thing Brown said was, "We already have Hydyne, we have

rad chem, and we have seismic; what do we need another yield determination for?" That was his response. Well, it's actually turned out to be more useful than that, but it was interesting to hear the logic that would prevail in those circumstances.

Kunkle: The radius we measure from the radiochemical drill back is developed by taking the point where the bomb went off, and finding the geometric distance to where the drill hole first found radiation. There are a fair number of assumptions there. One is that the effective center of the cavity is the center of of the explosion. Now, there's no reason to believe that should be true. Another assumption is that you have a spherical cavity. Based on these assumptions, you can derive a volume.

Many of our shots, especially those for a successful event, have a scaled depth of burial of perhaps a hundred twenty, a hundred twenty-five meters. Those in the valley collapse to the surface, and make very large, nice surface craters. I've always felt there ought to be a relationship between the size of that surface crater, the volume of the surface crater, and the volume of the cavity. And in fact there is. This leads me to believe that yes, in fact, by and large, that radius we're getting tells us something about an actual radius — that the cavity is more or less spherical, and it's more or less centered on the explosion point.

Carothers: You could make the argument that cavities are perhaps not spherical for a variety of reasons, but except for a certain number of special cases, they're spherical enough. These one or two meter wiggles and protrusions in the cavity wall don't amount to much. We can average those out. And so, while cavities aren't spherical as we draw with our compass . . .

Kunkle: They may be as you draw circles freehand, like many of us draw circles. That has been my impression of how cavity sections may be. Conversely, I think we've seen cases, and we expect to see cases, where the cavities are squashed by the geology. One of the things we seem to see, when shooting over very hard material, in a softer material, or under a very hard layer in a softer layer is that the cavity is either pushed down when it's in the softer material under the hard layer, or squashed on the bottom as it tries to grow down through the hard material. By and large, the little data we have from our drill backs tends to support these models. In the

final stages of cavity growth, the material strength must be determining where that cavity stops. And so, in weaker materials the cavity should be bigger.

Carothers: What comments would you make about craters with respect to cavities?

Kunkle: Well, for events that take place at relatively small scaled depths of burial — a hundred twenty or hundred forty scaled meters — the crater and the depth of burial together are a very good indicator of how big the cavity was underground.

I don't think this is surprising. At least, it wasn't to me. One would envision that the size of the crater ought to be related to the initial size of the cavity and its depth of burial, given the material it's in, of course. And, indeed, this is in fact the case. There is an excellent relationship between the yield of the device, it's depth of burial, and the depth and size of the crater on the surface.

You'd have to improve the rad chem yields for me to do any better in tuff, which is a very uniform material. That was, to me, a rather surprising result. I didn't expect to find such a good regression relationship. Now, in alluvium, it doesn't work as well. There are evidently different types of alluvium we shoot in. More or less, the tuffs in the valley seem, to the bomb, to be tuffs. And of course, geologically there are not large differences between them either. That gives a good ability to deduce actual event yields from observable, unclassified aspects, as we have found.

Carothers: Unclassified aspects perhaps, but I have to know a lot of things. I have to know the relationship exists, and then I have to know that this shot, whose yield I want to know, was fired in the same material and not something else. I do need to know a number of things in order to derive that yield.

Kunkle: Yes. The events I've been most interested in are those involving treaty compliance for a hundred and fifty kiloton limit. And so, the question is, can I verify a hundred and fifty kilotons from unclassified information? After all, I know the depth of burial. I can tell that from the cable lengths, although that could be disguised. But I could find out the depth of the hole, to find a maximum depth of burial. I can tell if there's a surface crater - - that's quite easy to know from a satellite or other overhead photography.

Does this tell me something about yield, if I happen to know it was in a tuff unit in the valley? At a hundred fifty kilotons, if I shoot in the valley, I'm going to be in a tuff unit, because I need that depth of burial to successfully contain the device. Those are all the things I need to know; it is in a tuff unit, the depth of burial, did it make a surface crater. Mostly I need the depth of the crater, which is fairly easy to arrive at from overhead photography. After all, we do it that way now using aerial photographs.

From that information I maintain I can get yields to plus or minus twenty percent, which is about the same as the rad chem people do. And, I don't need to do more than that. It's a very reliable way, at least to me who believes in the process, to verify the yield.

Carothers: And the reason that it works, presumably, is that the tuff in the valley is a fairly uniform material, and the cavities therefore follow a fairly smooth law.

Kunkle: That's right.

Carothers: And as they collapse to the surface, any bulking from shot to shot is very similar. So, since you mostly want to know the depth of the crater, you must feel that's related to the size of the cavity. In a sense you're using the crater to infer a radius for the cavity. That's what gives you the yield in this uniform material, in which you've fired enough shots that you have calibrated it, in a sense. Is that sort of it?

Kunkle: That's it.

Carothers: Well, if I went to some different place, like Pahute Mesa, where this rock layer is hard and that layer is soft, and this pillow of lava is here but was not there, it might be much more difficult.

Kunkle: It's more difficult, but actually the relationship works fairly well on Pahute Mesa, adjusted for the Mesa because there are harder rocks and smaller cavities, and less frequency of cratering up there. But when I adjust for those things, that is, do a Pahute regression, it works quite well there.

Where it doesn't work well is in the alluvium. I have to know more about the type of alluvium the shot is in. We certainly see a larger range of densities, and water contents, and gas porosities in

the alluviums than we do through the other geologic testing units. Of course, if one knows that such a relationship exists, we have published enough declassified event yields to calibrate the relationship.

Let me bring up a side issue. In studying the underground phenomenology from nuclear detonations, I kept coming across this group of shots that were just odd. Nothing looked quite like it should. It was interesting group. Then I found out what they were. If you find a particular kind of device, you throw it out of your analysis, because people don't know very well what the yield was. It just became clear to me that those yields have big uncertainties.

And so, one of the things that occasionally comes up in looking at a proposed shot site is that the neighboring experience may include some things that look pretty wild. There's a crater there where there shouldn't have been one, or there isn't one where there should have been, or this K value looks very strange. But when you actually look at them, there are these particular devices, and I usually just tend to ignore them. And there are some that are so odd that I just, when they come up, gently dismiss them as much as I can. Very odd things happened on Alva and Marvel. But of course, you'd certainly expect them to behave differently than other shots.

Carothers: There's another class of shots which ought to effect the cavity, and thereby the crater. Those are the vertical pipe shots. Generally they collapsed rather quickly compared to the other shots. Do they follow your curves?

Kunkle: We really didn't do enough of those. There are a half a dozen or so, and they're scattered about. They're not a very uniform group.

As far as collapse times go, I've never been able to predict them, so I can't say what effect the pipes had on them. One of the last of these we did was Huron King. It was done the summer I showed up here. Everyone was very happy that it took fifty-nine minutes to collapse, because that demonstrated that the pipe must have had really no effect on it. I suppose that's a good demonstration, but we had a lot of downhole diagnostics that demonstrated it better. So, I became acquainted with that argument rather early on.

Collapse times are interesting, and they're interesting because we can't predict them. For some shots in alluvium in the valley there are some sort of general rules that allow you to tell if it is going to collapse in one hour or ten hours, but really no more exactly than that.

For shots in the valley tuffs, where we can predict if it will collapse to the surface, and the general size of the surface crater, and a lot of things about the shot, we can not even get a handle on collapse times. When the cavity collapses seems nearly a random process. Certainly if it happened in a minute or two, that would be very unusual, and in fact, we haven't seen that. Anywhere from one to ten hours, well, okay. It's been interesting that we just can't do much better than that.

Carothers: Do you think the water that is present plays a strong role in the collapse?

Kunkle: By itself, the water, either by weight percent or by volume percent in the cavity region, that we actually measure, seems to play no particular role in determining collapse time. It must be the interaction of the water with the rock. We have shot in relatively dry sites of four or five or six percent, up to relatively wet sites in the high twenties. So there's some variation there, but not as much as you'd like to have for an experiment to see any effects. But we often see, comparing different shots, factors of ten difference in collapse times. We see markedly different collapse times from similar sites with presumably very comparable amounts of water. I believe that the water, or steam if you like, must play a role in the collapse, but there's probably enough of it always present to do whatever it's going to do.

Carothers: Perhaps that's the point. At the Test Site you rarely shoot in an area with very diferent kinds of rocks.

Kunkle: The collapse times, I found, go hand in hand with another conundrum I have, which is predicting ground motions. Ground motions display a range of characteristics which are understandable, but not predictable.

For example, a class of shots that has been studied a lot is the shots in the valley in the tuff units. Usually they have fairly high design yields. The ground motions fall on log log plots in a very nice and uniform way when you plot them for maximum velocity and

distance. But they're not the same from shot to shot. On some shots the velocity falls off very rapidly, and they have correspondingly very high motions toward ground zero. On other shots the velocity falls off very slowly, or relatively more slowly, with distance, but they don't have much velocity at surface ground zero. It's almost as if there's an energy conservation that the amount of energy under the curve is staying the same, but it's distribution around surface ground zero can be very different.

The puzzling thing is that we can't predict for any particular shot which of these behaviors is going to show. The motion will fall on a well defined curve, but we can't tell in advance what curve that is, and we can't relate the curve to any of the geological aspects. In particular, in our tuff pile location, which is a very uniform section of tuff geology, we have shot very similar shots in very similar settings; same device, same depth of burial, same location in the structure. If you had to try to repeat an event, you can't do any better than that. And they have had completely different ground motions. And, completely different collapse times.

And the collapse times aren't related to the ground motions either, by the way. I thought, ah ha, now I'll have some way to predict collapse times, but no, that didn't pan out. I think that both collapse times and ground motions are sensitive to the detailed properties of the close-in geology, the geology near the event work point.

Carothers: I was about to raise that issue. The shots, you say, were just about as similar as two shots could be, in terms of yield and geologic setting. Perhaps so. Yield, sure. On the other hand, they were probably a thousand of so feet apart. Maybe more. So, they weren't really in the same geologic setting, except in a general way. The details near the working point, the inhomogeneities on the scale of the cavity size, you don't know.

Kunkle: Well, that's true. For some things, like the scaled cavity size, the inhomogeneities don't seem to matter too much in the tuff units. For other things, such as the collapse times or ground motions, it seems to matter very much, and in unpredictable ways. We don't measure enough to be able to link the downhole measurements with what we see happening post-shot.

Carothers: There are people in the containment community who might say, "I could believe that some of the results you see are caused by details of the structure which you guys have never seen. And you've never seen those details because you don't care about them, because they don't affect the containment of your shots. However, you see the effects when you sit down and try to calculate certain things. Some of that detail could be things such as the motion of blocks that distribute the energy in different ways on different shots. Even though the shot points, by your logs and samples, look the same, they're not the same. The blocks aren't the same."

Kunkle: I certainly believe that block motion has an effect. By the way, there is a weak correlation between the joint frequency we see in holes and the collapse times. And the joint frequency tends to increase as you move toward the margins of the valley, from the center, and collapse times decrease as you move out towards the margins of the valley from the center. Maybe if you have more joints the blocks at the keystone are smaller, and then they're not as competent when it comes time to hold the cavity up. Whatever, there does seem to be some correlation.

I think the exact positioning of layers, and the impedance between the various layers in the bedded tuffs plays a part. Calculationally you see this. Calculated ground motions and residual stresses are very sensitive to even small variations in the layer properties you use - - their thicknesses, their positioning. We found this out on an analysis of the Cottage event, for example. The standard model was very sensitive to small variations, and we've seen this in other shots we've tried to calculate. So, the actual details of the geology often seem to matter. Fortunately, not for a lot of the containment aspects.

Carothers: Not for the containment aspects of the kind of events that you do. If your Laboratory said it was necessary to fire an event which had a line-of-sight to the surface, then some of these things could possibly become important. But for the kinds of events that Livermore and Los Alamos do these days, simple emplacement hole shots at a conservative depth of burial, they obviously aren't important to the containment aspects of the shot.

Kunkle: No. Now, we may see some of these effects reflected in some of our containment statistics. What I mean by some of them is the effects of block motions. This is an argument that Carl Keller has made, and I find it quite persuasive. You can imagine our emplacement hole as actually the narrowest of soda straws. It's a pencil line when drawn to actual size on cross sections. And one phenomenon that probably happens on higher yield shots is block motion which is large enough to simply shear off the pencil line. There is then no longer a line running down to the cavity.

You could say that somewhere around fifteen or twenty kilotons we technically get block motions large enough to shear and very effectively block off those stemming columns. That may be a key to containing larger yield events. That's one of the reasons they may be easier to contain; the ground motions, the chaotic block motions close in may tend to slide the earth around and seal off the stemming columns.

So, the cavity sizes and crater dimensions fit nicely in a family. From that you can assume, or infer, what may be happening in the ground. The crater size must be reduced somewhat from the cavity volume, because of bulking of the earth materials. You can work out a bulking factor by calculating the cavity size and the crater volume, and looking at the difference in volumes. Now, we can work out a bulking factor in the tuff that is seven or eight percent, but we can't a-priori know that. I can't work that out from the mechanical models, or the physical measurements on the tuff itself. It's something we simply observe.

And then there's the ground motion. We observe the ground motions and they're understandable. That is, when a new shot is done, you look at the ground motion data. It's understandable in the context of the other shots, but it's not predictable in advance.

Carothers: When you talk about bulking factors, there's the question of how the collapse occurs. Is it a rain of little pebbles, or a massive chunk of material that moves down as a unit. What do you think it is? Or what evidence is there for it being one or the other?

Kunkle: The only evidence I'm aware of is from drill backs and reentries on Rainier Mesa. There are downhole movies, and holes drilled in chimneys, which show perceptible large gaps between the various blocks, most of the way down.

But this is, of course, a limited set of experience. We occasionally will drill through the edge of a chimney, or collapsed area, during the post-shot operation. You commonly lose circulation when you reach that region. That indicates you've reached some kind of fractured area, but we know little of the mechanical properties of the chimney material, such as the rubble sizes, and the spacing between the pieces.

Weart: We did some measurements during the Marshmallow reentry to see how large the cavity was, how far out it had grown. To do that we mined in until we intercepted the edge. We followed certain bedding planes that existed in the tuff, and all of a sudden we came to an area where, although it was still perfectly solid rock, it was disrupted. As we continued to mine in it was clear that what we were now in was a jumble of tuff, and it was not characteristic of the the material we had been following.

Carothers: You couldn't tell from the mining itself that you had entered the cavity region? You were still mining in solid, competent rock?

Weart: Yes. It required no additional support over and above what we had used out in the rest of the drift. It was tightly compacted material. I think a lot of people have the picture that when the cavity collapses there is a rain of rocks of various sizes, and there is a pile of unconsolidated material that makes up the chimney.

That was not our experience at the working point depth. Right at the cavity boundary it was tightly compacted material. We could find evidence as we mined in of fractures that had developed outside this cavity radius. They had had molten material injected in them. It was usually radioactive, but not necessarily so. There wasn't any indication at all of radioactivity at the boundary of the native rock and the cavity.

We did contact the cavity on more than one radius. I don't recall if we mined straight through. We might have. We did try to determine the radius on the horizontal plane, and it wasn't perfectly spherical. And subsequent shots, like Gum Drop, were not perfectly spherical either.

Flangas: When we reentered the Pile Driver cavity, up until we hit the cavity wall there was nothing to indicate there was anything beyond. There was a clean interface, and within a matter of inches we were into the cavity. Now, we've had others where we see the ground get more and more fractured, and more and more ravelly, fifteen, twenty feet away from the cavity wall. That's in the tuffs more. But we've seen them both ways. Ground is not homogeneous, it's not consistent.

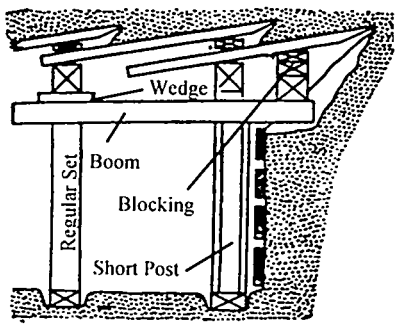
Carothers: It sure took us a long time to learn that. Why didn't you explain that to us sooner?

Flangas: Nobody asked me. My job was digging them, not figuring them out.

As far as chimneys go, on Rainier we drove a raise up to get about a hundred feet above the shot horizon, and that's where we ran into the material that was just powdered. It was just totally disagreggated. It was like working through flour. Then we used a technique they call spiling, in order to get that drift across. We wanted to drift across the cavity and get directly over the ground zero.

Carothers: What's spiling?

Flangas: Spiling. Spiling is a roof support system that is used in very loose or blocky ground. Pointed (chisel shaped) wood (4" x 6") or sometimes metal beams are angled and driven upward and outward over the leading set with the back end braced over the preceding set. This cantilever bracing supports an incompetant roof ahead of the last set and keeps the miners safely under cover while advancing the heading. So, as we spiled across there we noticed that material was totally disaggregatted. And from my experience in block caving, that was a block caver's dream. We could have pulled rock out of there from now on. There's a lot of material in that chimney.



Carothers: Tom, there has been the picture some people have presented of a continual process of decrepitation going on before cavity collapse. There's the heat in the cavity, it heats up a layer of

rock, turns the water it contains to steam, which then blows off that layer of rock, and so on. So, the cavity walls are continually flaking off.

Kunkle: I would expect such pieces of rock to be quite small compared to the major blocks that would fall in during cavity collapse. And, by and large, the cooling that occurs is from the energy that is transported into the rock to make it hot. The conditions near the wall of an underground cavity, following a nuclear explosion, must be quite suitable for steam vapor explosions to occur. The rock and the water in the cavity are under a large pressure. The water in the rock can now be superheated, probably, to appreciable temperatures before it will flash to steam. At those high temperatures, the flash of the superheated water to vapor can have an energy release comparable to a good high explosive. But the energy has already gone into it, by thermal conductivity, and so it is already hot steam and water and rock, being added back to the cavity. The work's already been done, other than the mechanical work, which is soaked up.

But I think those pieces must be small. You're looking at an average size of a pocket, even the big ones maybe, of a few millimeters across. So, you can imagine a little droplet of high explosive detonating just inside the wall, and scaling off some small amount of rock. I think this is unlikely to contribute to the major collapse.

Now, there has been a school of thought that believes that the cavity pressure is related to the collapse time. The model seems to be that of an impermeable membrane, which allows you to push against the rock. I've thought that a mechanism that may be more important in determining when cavities collapse than the steam pressure inside the cavity is the stress in the rock around the cavity. If our calculational models are to be believed, we often reach stress states in the rock immediately surrounding the cavity of compressive stress; the residual stress we like to talk about. That's also expected to dissipate, as water moves out of pores and relieves that, and collapse times may be more related to the migration of the water out of the combined pore spaces than to the actual pressure decay in the cavity. The two may go hand-in-hand, but we know very little about any of these mechanisms.

Carothers: When the cavity does collapse, whatever gases there are in the cavity have to go somewhere. The steam can be condensed by the cold material that falls in. There is presumably only a small volume of other gases, so there ought to be a low pressure in any volume that remains. You might think there would be flow from the surface down into the chimney until that volume is filled up.

Kunkle: That's indeed seen in apical voids. It's not unusual, in a subsurface collapse that extends a fair distance up the hole, to have a containment diagnostic package survive in the stemming above. That package comes into communication, through the stemming materials, with the apical void. For example, on the Barnwell event, some of the upper pressure transducers showed a declining pressure soon after the major collapse, as they came into communication with the reduced pressure in the apical void.

On Rivoli there was a measurement just under the topmost plug which showed the pressure decreasing, presumably as it came into equilibration with pressure in the apical void at the top of the rubble column. So this, indeed, does seem to occur.

I have heard rumors through the years, but I've never seen written documentation, that when you drill back into standing cavities, they have sub-atmospheric pressure in them. I'm pretty sure we've had, at Los Alamos, events in Yucca Flat where we've drilled back into standing cavities where there were pressures below atmospheric. They subsequently collapsed. To my knowledge there are no standing cavities at the Nevada Test Site.

Brownlee: Los Alamos had at least three, and I don't mean that there might not have been four, shots in which we did a very low yield in saturated tuff. For us that's unusual, because low yields would normally be done in alluvium. These happened to be in saturated tuff.

One time the guys came to me terribly excited because they'd had this low yield in saturated tuff, and they said, "When we drilled back, we hit the cavity, and the fans that do the ventilating were running backwards. All the air was going into the shaft; all of a sudden the cavity was just sucking air. How could that possibly be?" So I said, "The next time that happens, make sure you estimate how

much air goes in." We had three shots for which we measured the flow of air into those cavities, and what we found, of course, was that the amount of air that went in was the volume of the cavity.

So, we had a standing cavity with a vacuum. What you immediately deduce is that the cavity was small, and in tuff, so it stood. It didn't fall in. But it was sealed off, and this told us a lot about gas flow through tuff, and how things could seal.

Carothers: Subsurface collapses, by definition, go part of the way to the surface, and when they stop, there seems always to be an apical void above the chimney material. If that void is at low pressure, the flow will be downward from the surface to fill up this big vacuum chamber. That is a mechanism which would tend to militate against any release of gases that might have gotten up that high. Do you believe that's possible, John?

Rambo: I like that idea. We saw that happen on Barnwell, certainly. The pressure dropped, and you could see that on the downhole gauges. The subsurface collapse tended to draw a vacuum, and we didn't see any radiation get above where it was measured at the stemming platform, which was very high in the hole, about four hundred meters up. And so, I think that downward flow certainly does happen.

There were a set of experiments carried out by Ed Peterson, of S-Cubed, sponsored by DNA, which had to do with whether there was any containment threat if a shot site was situated close to the chimney of a previous event. Data was sought as to whether or not there might be flow of gas through the old chimney to the surface.

Peterson: At the time we did the chimney pressuriztion measurements there were a couple of things that were coming up. One was that they were going to shoot Hybla Gold near a nuclear chimney, and they were worried about, if they got gases from the shot into the chimney, would they then leak up to the surface very rapidly. If you place events reasonably close together, and if you get rapid gas flow into an old chimney, could those gases end up going up to the surface rapidly? That was the motivation. It was a pretty much a containment-type question.

Carothers: Wouldn't it be reasonable for me to ask, "Why would you be concerned about gases going up an old collapse chimney? After all, there was a shot there, the chimney formed, and gases didn't go up it from the original shot. Why would it do that from another shot?

Peterson: That is an extremely legitimate question. And it is probably correct that if there were another shot, and it didn't collapse, then there would be all the steam in the cavity, and there would be a horrendous drive because of the steam pushing all the noncondensables Then you could make the argument, just as you did, that the steam is going to condense in the old chimney, because the first one didn't leak either. What you say is true.

I don't know all the motivations for those measurements, but I think we are now in a world in which not everybody who looks at the problem understands all the details of what goes on. So, if you do a test and measure something, and say, "Okay, we did the test and measured it. And so, now that we've measured it, we sort of know what happens," it makes it much more believable to a large portion of the community.

Carothers: I believe that. What kinds of things did you do?

Peterson: The thing we did on those tests was, we injected air slightly above the working point level through a drill hole. They drilled a slant hole going up at fifteen degrees, from one of the underground drifts, and came into the chimney some fifty to a hundred feet above the working point. Through this hole we injected air, plus a tracer such as sulfur hexafluoride. Then we measured the pressure through a drill hole that was drilled from the surface down to the top of the chimney. We also measured the pressure in another drill hole that came in horizontally. That one went in near where the working point originally was. In all three of those holes we could measure pressure, and tracer gas concentration. We also, on the surface, put out three circular arrays so we could take air samples every thirty degrees around the surface ground zero. Those we could analyze for the tracers.

Basically we maintained a constant flow rate, and looked at the pressure response as a function of time. On most of our chimney tests I believe we were flowing gas in at about three thousand cubic

feet per minute. It was between one and three thousand, somewhere around that. Eventually, after twenty hours or so, we could build up the pressure in the chimney to maybe three, four, five psi.

We put in numbers of millions of cubic feet of gas. You can model it, and we found we could model it very well. From the model we could calculate what would happen if we let the pressure decay, and built it up again, and so forth. So, we got to the point where we thought we could understand reasonably well the conditions in the chimney. We did three chimneys, and I think we did seven tests on those three chimneys, which were from Dining Car, Ming Blade, and Mighty Epic.

I believe some of the motivation for using the Mighty Epic chimney was because Diablo Hawk was going to be done in that general vicinity. I think we verified, if nothing else, that gas doesn't come up to the surface from those chimneys.

Carothers: Is that because, although the chimney may have a lot of cracks, and the gas goes up to the top of the chimney, there is then some amount of material from the top of the chimney to the surface of the Mesa, and that's what's really keeping the gas in?

Peterson: Yes, one can make that argument. I believe it was on Dining Car where, when we did our first test, we actually detected gases up on the Mesa at positions that were probably on the order of two or three hundred feet from the surface ground zero. Subsequently, after the USGS came out and looked at it, they found a region there that was fractured. The fractures went down at about a thirty degree angle, and would intersect the uncased bore hole that went down into the top of the chimney. Subsequently that bore hole was cased, and we did another test. Nothing came up to the surface.

So, in that case we really made the right guess — the material above the chimney was what kept the gas in. I can't remember the exact numbers, but we probably put in two to four million cubic feet of gas, and our guess is that at the most, even when we detected it, maybe less than a hundred cubic feet had come out on the Mesa. The tracers are very sensitive, to one part to ten to the twelfth.

If we had been testing over a chimney that was in alluvium, where you wouldn't necessarily get the flow through the fractures, we would then have put some type of a tarp on the surface, and collected the gas under it. That way. if it does ooze up over a large

region, you can still pick it up. I think that what we showed was that there was no gross flow. These slight oozings — I don't think one can tell. But I think the amounts would be so small that it would be almost impossible to detect, no matter what it was that was oozing up at that rate.

Carothers: The conclusion that I would arrive at is that indeed I can safely detonate a device quite close to an old chimney, because it is no more of a flow path than the new chimney that's going to form.

Peterson: I think that's true. If you're looking purely at the fluid flow aspects of it, what you say it true.

Carothers: How else should I look at it?

Peterson: Well, because DNA has a line-of-sight, and ground shock closures, and things like that on the tunnel events, if you do put another shot too close to an old chimney, you may affect the ground motion in a manner that might adversely affect some other part of the system.

Carothers: You're implying that the properties of the chimney, of this material which has fallen in, are different from the surrounding materials, and so you can't treat it as similar to, or the same as the rest of medium?

Peterson: That's true. It may be a perturbation to the ground motion. But I think from the fluid flow and leakage point of view what you say it very true.

Carothers: Do you think that would be true in alluvium as well?

Peterson: I think so. I see no reason why it wouldn't be. On Pahute, Livermore has done shots where they get some collapse, or partial collapse, and there are little fractures that ooze small amounts of activity from atmospheric pumping. But they are very small amounts. The thing is, you can count anything, Jim. It's like our sulfer hexafluoride — there are just molecules that came out. With the measurement capabilities that people have today you can measure far below anything the EPA says is significant for anything, or that anyone else says is significant. You can measure molecules of anything, like our tracer. And there are a lot of molecules. It's

true that Caesar's last breath is still floating around, and every breath you draw in should have a molecule or two of Caesar's last breath. One can mathematically show it.

Carothers: Carl, the Test Site, including the tunnels, is used as a two dimensional grid, as far as siting events goes, and there are some arbitrary rules about how far from an old chimney a new event should be located. Eventually, perhaps, for various reasons, people could be forced locate events closer than those rules would allow. My impression is that nobody really knows very much about what the properties of the chimneys are, and so they stay away from them because they don't know.

Keller: That's right.

Carothers: Do you think that could become an issue in the future?

Keller: I think that if there were a few measurements of chimney permeabilities, and measurements outside those same chimneys, to develop real data on what the relative permeability is, inside versus outside, then you could be much more quantitative about how close you could get. The gas flow codes we have now would easily handle that problem. There are some kinds of sitings that are already all right. You can shoot, certainly, well underneath. I don't see anything wrong with dropping one chimney into another.

Carothers: No, I don't either. No one has done it though.

Keller: No. Well, they've gotten close. But I think in that case you don't have to be very quantitative to convince yourself it's all right.

The thing that I think is most compelling for the measurement of permeabilities in the chimneys is the CO2 question. As they site in different areas, and they encounter higher CO2 contents, they will have to be more explicit about what is an acceptable level. The standard five percent that has been the threshold of concern is based on an analysis of seeps, which occurred all over the site and includes events like Diagonal Line and a bunch of Livermore shots. There's a whole area which Livermore uses that has a high CO2 content. It's also fairly well cemented. It's very important that the threshold of concern for CO2 is very medium dependent. If you're shooting in a material where the chimney is not significantly different from the

native material in permeability, you can go to very high levels of CO2. And in fact, some events were shot in carbonate rock; Nash, and Bourbon, and Handcar.

Carothers: Nash also leaked.

Keller: Yes, but Bourbon didn't, and so you wonder why. Well, Bourbon was deep enough. Seeps depend on the path, and if it's long enough, it can even be fairly permeable. Jack House paid for some work on the relationship of CO2 and medium properties to leaks, and that will be very useful for him if permeability measurements are made. Now, as I have said, you can infer permeabilities from the leak arrival times, but that assumes you know what the CO2 generation is, and that's kind of a flaky number. There are the arguments about whether cuttings or sidewall samples give you a good number, and how you should average, and so on. So one doesn't know the inventory very well.

Carothers: Russ, you have said that there are indications that things other than the simple movement of gases from the detonation through the chimney toward the surface go on in the chimney after the shot.

Duff: When the early Plowshare activity in S-Cubed came along, I had an opportunity on Gasbuggy to look at the chemistry of a nuclear chimney. We had extensive measurements of gas composition over time, after the shot. Chuck Smith, at Livermore, did measurements not only on the composition of the gas - - carbon dioxide and air and methane and ethane, and so forth - - he also looked at HD, HT, H2, HTO. So, we had not only chemistry, we had isotopic chemistry. I tried to develop for El Paso Natural Gas, who were the commercial partner, a model which would explain all of those measurements in a consistent fashion. I think we did, and it is a very different model from what the Laboratory developed.

One thing that came out of it was the postulate that during collapse some of the hot rock was elevated, or at least not flooded by the condensate. So, over a period of six months there was a continuing series of reactions at these hot rock surfaces between the various chemical species. There must have been hot spots in the chimney, and by hot I mean six, seven, eight hundred degrees Kelvin, that lasted for six months.

Carothers: That's not the conventional wisdom.

Duff: Of course it is not. But you look at all the chemical evidence, and ask, "How can you explain that?" Well, I could explain it by a series of assumptions, and continuing reactions were required. So far as I know nobody else has tried to explain why the chemistry changed over six months. But it did. That was my first effort to apply concepts of equilibrium chemistry to the nuclear explosion environment.

In the DNA program there have been a number of places where chemical concerns might be important. We have long seen explosive gases in the tunnel after the shot. Where do they come from?

Carothers: Joe LaComb recently said they were finding hydrogen during their reentry, but it's clean, so it doesn't come from the cavity.

Duff: Well, I haven't thought it came from the cavity for a long time. I've been promoting for four or five years the idea that DNA was seeing the effects of reactions between grout and metal, making hydrogen. Since the grout, in particular superlean grout, is made with desert fines, there is carbonate in it. There have been a lot of chemical calculations which have been done, and reported, which can explain the presence of a lot of carbon monoxide, and little carbon dioxide. In the cavities we're dealing with there should be a lot of carbon dioxide. The stuff that shows up in the tunnels is carbon monoxide, and right there is evidence that it is not cavity gas.

There is some radioactivity in these gases, and I think that represents fission products that get into the very early prompt flow. They get mixed into the stemming, and then are purged out of the stemming by late-time reactions which make hydrogen and carbon monoxide, which then seep into the tunnel complex. That was behind my suggestions a couple of years ago of putting some manganese dioxide into the system to try to control the late-time reactions.

Carothers: I recall that Livermore put manganese dioxide around the device canister on a few shots in the sixties.

Duff: Jade is one. It was done in a radiochemical context. They were trying to modify the oxidation states of certain fission product oxides so the radiochemical collection process would be

better. Before that work came to any particular fruition, as I understand it other chemical techniques were developed and it was dropped.

I've been talking to Joe LaComb and various other people about chemical related activities. Bob Bass was receptive, and he got Sandia to make some gas sampling systems. They have been fielded on a couple of events now. I am professionally gratified to hear Joe LaComb make comments as he did at a recent CEP meeting, saying that maybe, in fact, chemistry is important. I've been saying for a long time now, "Chemistry is a perfectly good branch of physics. There's information there, let's extract it." So, I think there is an avenue of potential advance which I look forward to DNA exploring.

Carothers: The only chemistry I ever hear about at the CEP concerns how many tons of carbonate rock will be affected per kiloton, or some brief mention of the iron in the canister, and how much hydrogen will be produced from that.

Duff: I know. I know. Some four years ago I got hold of a suite of gas sampling data from Livermore, and tried to see what it told us about iron reactions, and how much rock was able to give up carbon dioxide, and so forth. It was surprising data, because there were shots that were right, in the sense that they had big amounts of iron around, they were in tuff, and you'd expect under those circumstances to be a lot of hydrogen, and indeed there was. There were other cases where there was a minimal amount of iron, the shot was in alluvium, with relatively high amounts of carbonates, where you'd expect carbon dioxide to dominate and it did. But there were also cases where the reverse was true, There were cases where where you'd expect lots of carbon dioxide and instead you got lots of hydrogen. Or you expected lots of hydrogen and you got lots of carbon dioxide.

Another problem, which is long standing, was shown in Gasbuggy, but it is also true in all of the Livermore gas sampling. Why is there so much ethane and propane found in the gas after a shot?

Carothers: Now Russell, there aren't any hydrocarbons at the Nevada Test Site. There is tuff, and clay, and lavas, and such like, but there isn't any ethane or propane. You might expect to find that in a gas field, but certainly not in Nevada.

Duff: There's hydrogen and there's carbon dioxide at NTS. And at high temperatures these react, and you get methane, a detectable and measurable amount, like one percent. And, if you look at Chuck Smith's gas sampling data there is ethane and propane found and reported. In equilibrium you expect that hydrocarbon series to be down about five orders of magnitude as you go through each step. The mystery to me is that the observation is one order of magnitude between methane, ethane, and propane. One order of magnitude for each step, and we calculate five or six.

Carothers: At five orders of magnitude per step I would think it would be very difficult to see propane, and perhaps you might not even see the ethane.

Duff: That's right, but we do see them. Now, I don't have the foggiest idea what the implication or importance of that is, but it is a mystery which has been around since Gasbuggy. I firmly believe that when we see something that is a surprise, we have a chance to learn something we didn't know. When we see what we expected to see, we haven't learned anything new. And so, it's in this context that I want to understand that mystery. Not because I think it's going to be better than sliced bread, or somehow take care of the national debt; it's not that kind of important. But I think there may be something about the phenomenology which is hidden, at the present time, in that particular observation. So, as a guy who is interested more in the scientific aspects of things than in meeting the schedule, I am intrigued. And, I think there may be something of value there.

We have a situation in the gas sampling area, which I think is fortuitous. We are getting data, and we've been able to pretty much make sense of it. For instance, on Mission Cyber we were able to say, from gas samples, that in the chimney the cavity gas was seventy-three percent hydrogen and twenty-seven percent carbon dioxide, with a little bit of other stuff. We've got three measurements at different times, and we get essentially the same answer each time. That's not really a profound thing, but it allows us to investigate the whys. What temperatures, what pressures would give rise to that answer? I wish this had happened a decade ago so I'd have some professional time to try to do something with it. It will be the next generation who gets to exploit it, and I hope there is somebody who wants to champion that kind of work, because I think there is an opportunity for major success there.

11

The Residual Stress Cage

What is important in the containment of an underground nuclear explosion? Certainly the depth at which the explosion takes place is crucial. Obviously a detonation on the surface of ground will release the products of the explosion to the atmosphere. A detonation taking place miles underground would certainly be expected to be completely contained, barring some man-made feature which would provide a path to the surface. "Deeper is better." The lithostatic stress, which is always there, works to prevent the formation of any openings through which high pressure gases might escape, and as the weight of the overburden becomes greater the energy released can no longer lift the overlying material as far, and so on.

However, great depths of burial create difficult and very expensive problems to solve. What is a depth of burial at which the containment of the detonation products confidently can be expected, but which is no greater than required for that confidence? For the moment we will put aside consideration of the man-made features such as line-of-site pipes, cables, stemming columns, and other such things.

There are three principal phenomena, aside from the lithostatic stress and the overburden weight that are thought to play important roles in the containment of the detonation products of a nuclear explosion. The importance of any of these mechanisms, or whether any one of them is important at all, or possibly even exists at all in the context of containment has been the subject of extended debate. Certainly they exist, but when they occur and to what degree they influence a particular event is a matter more of opinion than of demonstrable fact. Nonetheless, detonations are contained, regardless of the minimal understanding of these mechanisms.

One is what in the earliest days of underground testing was called the "mystical magical membrane," and is variously referred to today as the "residual stress," the "stress cage," or the "containment cage." It comes about, in theory, when the rock materials that have been pushed out by the passage of the shock wave, and compressed, move back toward the cavity and set up a region around the cavity

where the hoop stresses in the rock are greater than the cavity pressure. Hence, gases in the cavity cannot be forced through that region.

Another postulated mechanism is hydrofracturing, or cracking, of the rock near the cavity by the gases which are at high pressures in the cavity. Such a crack exposes additional cold surfaces, and speeds the cooling of the cavity material, reducing the high pressures that might force materials toward the surface. Hence, they could reduce the flow from the cavity, and be beneficial to containment. On the other hand, such fractures would seem to provide paths for flow of gases toward the surface, or perhaps to some plane of weakness such as fault. As such they could be a threat to the containment of the event.

The third, thought to be sometimes important in tunnel events where a line-of-sight pipe is used, is block motion. This refers to fact that upon tunnel reentries very large blocks of rock have been observed to have moved many feet. Such motion could conceivably be good for containment by moving a very thick block of material across the tunnel, effectively sealing it. Or, it could be bad by destroying or interfering with the action of the mechanical closure hadware typically used on line-of-sight shots.

There is, of course, the possibility that all three of these things might occur in various degrees on every detonation, either reinforcing or interfering with each other in the containment of the gases. In a similar way, it is difficult to confine the discussion of peoples' opinions about why shots contain to just one of these mechanisms. This chapter will consider principally residual stress, the next hydrofractures, and the one following that, block motion.

Carothers: In the earliest days of the underground program there were people who said, "I don't understand why every shot doesn't hydrofract to the surface and vent. Why do they stay there? Everything is diverging, everything is being pulled apart, there is this high pressure gas, and it should hydrofract to the surface very quickly. But it doesn't do that." There were other people who said, "Well, there is some sort of mystical magical membrane that keeps it from doing that. There has to be, because otherwise, you're right, you couldn't contain an underground shot."

Higgins: Just right. And that argument is correct, and all of the descriptions of what that mystical magical membrane was were there. We just didn't really stop to look. There were clues about the residual stress that we found on Rainier. When we went back and examined the sandbags that had been in the stemming around Rainier, we found that the sand, which was just loose tuff that had been shoveled out of the tunnel and put into cloth bags, was now so hard that we had to use pick axes to remove them. The sand was as tight and as solid as the original tuff. Surprising, we thought, but we ignored the clue.

That compaction, we said, was due to the passage of the shock wave. But when we tried to compact materials with plane shocks in the laboratory, we didn't get that. So we said, "I wonder why that is," and ignored the clue that the rebound recompaction was an important part of the containment process. People used to refer to something they called the "mystical magical membrane." Well, it has a real basis in physics, but by using that term we tended to dismiss it as a part of the overall process. That's where the physics should have included the business of rebound, and what we now refer to as the containment cage.

Roland Herbst gave a long talk about this along about 1960 or 1961. He remarked about the fact, and we reduced the argument to the plane wave case, that following the passage of a shock there was reverse motion, or rebound, in the direction from which the shock had come. So, you had not described everything when you talked, in a shock tube, about the passage of the shock wave itself. I said, "You mean the shock rebounds from the other end." And he said, "No, no, no. Make the tube infinitely long. After the shock passes, a little while later the material will go back the other way. There will be a rebound. That's because the material now knows there was a shock wave." We argued about this, and he convinced me that yes, if there was an initial pressure, or an initial number of atoms per cubic centimeter, there would be rebound without any reflection. Knowing that there is a rebound, what we then should have said was that after a period of time the material comes back and recompresses. It's the physical nature of the approximately spherical cavity that makes it persist. It's simply the recompaction of the rock, which is considerable.

Bob Brownlee has a series of photographs he's put together from the atmospheric test series. In many of the early atmospheric tests we had smoke rockets that were fired prior to the shot to leave a curtain of tracers in the atmosphere, so we could watch the air shock from the atmospheric burst, and calculate its dispersion and strength and so forth. We were looking at some of those photographs one day and Bob said, "Watch the smoke trail go by." We were looking at a long view of some bunkers, and the smoke rocket trail went by from left to right, and he said, "Now, watch it come back." And I said, "Recompaction." We had all of the physics in front of our eyes way back in the 1952, 1953 period from the atmospheric tests, because the air does the same thing. When the shock wave goes by, that's not the end; it comes back again. And that's the recompaction in the air.

I think we saw these things, and we didn't think about the importance of them, or that they really were clues to something far broader than we had constructed a concept for.

The point I'm trying to make is that the rebound is a necessary part of the shock expansion, and one that we ignore because of our calculational mind set. We run calculational problems in an artificial one-dimensional framework, which is okay; we can put even a boundary out there, and it sort of works for most things. Except, it doesn't properly tell us the rest of the story. What happens after the shock wave is gone? For a long time we were happy if we could run a one-dimensional computer simulation of a nuclear explosion out to ten microseconds. That made the cavity start to grow, and all these things start to happen, and the shock wave was gone out of the problem. But we didn't ask what happened after that.

Rimer: I was amazed when I came to S-Cubed that people were talking about this "mystical magical membrane," when, to a civil engineer, there was nothing mystical or magical about it at all. The residual stress concept for metals, structures, and concrete is a very well known and well established concept in civil engineering.

Carothers: What kinds of things bring that about? Certainly not a shock wave.

Rimer: Plastic failure, under a non-uniform stress distribution. Say you take a column and press on it. That's a uniform stress distribution; it doesn't introduce residual stresses. But if you take

a beam and put a load on it, you introduce compression on the top, tension on the bottom, and so you get a nonuniform stress distribution through the beam. Or, the torsion of a cylinder. If you load it into the plastic regime, the outside fibers get loaded higher, and they go plastic first. When you take the load off, stresses get locked in. That's a well known concept in civil engineering.

Carothers: Well, we didn't have any civil engineers considering this problem. All we had were physicists and calculator types.

Rimer: That's right.

Broyles: I don't remember who really came up with the actual idea of the stress cage. It was based on some calculations, but it was fairly nebulous. When you look back at it, it's so simple that a high school physics student can understand it. When you deform something classically, and stretch it out elastically, it rebounds, and is going to have a residual stress.

Carothers: That wasn't appreciated by people for a long time.

Broyles: No, and we at Sandia didn't either. And it's not at all clear yet under what conditions, particularly in alluvium, are you going to get how much of a stress cage, or how consistently, or regularly. I think it's quite clear-cut that in tuffaceous materials you regularly get a stress cage, and that there's creep, and that it decays. And that you can cause perturbations in it, and get yourself in trouble with things like line-of-sight pipes sticking through it.

We got started, and Wendell Weart got started, worrying about hydrofracing as a way of breaking out of the cavity. He started trying to understand how you could have calculations which said you had several times overburden pressure in the cavity, and not have the stuff get out of the cavity. We then developed, and did the first in-situ measurements, using high explosives, that really demonstrated the containment stress cage, I don't claim that Sandia invented the idea of the stress cage, but I think we really pursued it, and proved it in a real environment, even though we were devoting most of our efforts to the line-of-sight shots.

Bass: I believe I have seen firm evidence of the existence of a residual stress situation, in some situations in the field - - but in a homogeneous rock. Years and years ago we did two experiments at Sandia. A fellow named Lynn Tyler did a residual stress experiment

called Puff and Tuff. I did all the calculations on that thing, and I'm very proud of Puff and Tuff. It was a beautiful experiment. We fired a 256 pound charge, which had two pipes looking at it. One came down the tunnel we used to put the charge in. We put a funnel on the front of it, where it went to the HE. That was calculated to keep the pipe open, so the gas would come down, and then be there available to crack the formation. It is very important that you put the funnel on; otherwise the hydrodynamics will close off the pipe right away, and you get no gases in it. The tunnel was stemmed, of course.

When we were first designing the experiment, that was the only pipe we planned. Al Church, of the firing group, was sitting in on the meeting on firing the HE, and he said, "Why don't you just drill a hole on beyond the charge, and have one that is in the tuff, not in the stemming?" So, after we excavated the place for the charge, we drilled a hole on into the tuff. It was six inches in diameter, and we put a transite pipe in it and forgot about it. And again we put this funnel on. Thank God Allen suggested that pipe, because that one worked, and the one in the stemming didn't work at all.

So we fired the shot. The HE gases went down the pipe in the tuff, right away, and delivered enough pressure at the end to crack the rock. We know it got down there very quickly because we had a pressure gauge at the end of the pipe in the stemming to find out when the gas got down there, and it got down there like a bat out of hell. We had calculated where the residual stress should be, and when we went back in there was no cracking at all for one cavity radius beyond the original cavity. Then all of a sudden we have a vertical crack that goes up and down as far as you can see, with black detonation products all through it. But there's absolutely no crack where we calculated the existence of a residual stress field. Now, I think that is very good evidence.

There was one thing that was bad about the experiment. That was, we did change the stresses in the tunnel by the excavation. In the same place where this residual stress field would be, we had a modified stress state due to the excavation. This always has to be considered. There's some creep that will take place, and there will be some differences. But indeed, you could go back in there and for

a full cavity radius there was no crack at all. We used the Alpine Miner when we went back in, and we stopped it every six or eight inches, and did a complete map of the area.

Now, something very interesting happened with the pipe in the stemming. We closed that pipe in the stemming. That stemming was supposedly GSRM - - rock matching stemming grout. Now, you know as well as I do, it doesn't match at all. Indeed, we closed the pipe in the stemming; we did not close the pipe in the tuff. I think that this happens should be known in the containment community.

On Carl Smith's high explosive experiments we do see some stress records that look good, and there may be an indication of residual stress on those. I believe I've seen residual stress twice. Once was on Puff and Tuff; the other was a precursor to Puff and Tuff. That was a five pound cylindrical HE charge. It was the first thing that Lynn Tyler did.

After the shot Lynn got a very bright guy to go in and dig it out. This guy had nothing better to do, and he went in there with a dental pick, a tiny chisel, and a paint brush, and dug it out like an archeologist. He found a cavity, a nice little cavity, elongated because of the cylindrical charge. Of course it was small, and the material was a nice smooth, very homogeneous, weak tuff, with no cracks in it at all. Then he found a region that didn't look like the same stuff at all. It had absolutely no structure to it. He did find some little cracks too; right on the edge of the cavity he found some circumferential cracks. Then he got into this region of absolute mush. He went into this region, which was about the same size as the cavity. Then he went to the edge of that material, and he found circumferential cracks all the way around, and radial cracks running all over hell. Now, I claim that is a stress cage. And, unfortunately, I have just given you the best write up known to man. It has never been documented, and I cannot get the man who did it to do that.

Carothers: Carl, your gauges can survive for a little while in a ten kilobar regime. It would seem it would be fairly straightforward for you to make measurements at, say, one kilobar.

Smith: One kilobar is pretty close to the crossover point, where things last forever.

Carothers: Then you should be able to make measurements which would address the question of whether there is actually such a thing as a residual stress cage, or whether it is a figment of the calculator's imagination. Have you done any work on that?

Smith: That has been a prime question for ten years.

Carothers: If it's still a question, you must not have gotten the answer.

Smith: On the HE shots, at a kilobar and below, we have long-term measurements that do show the existence of the residual stress cage, very clearly and unequivocally. These are from both the stress gauges that show a long-term offset, and also from the motion gauges, which are integrated accelerometers, that show the rebound. They show you the material coming back in, and when you look at the calculations you will see that motion is what sets up the residual stress cage. That's really quite clear from the HE tests, which go from eight pounds up to two thousand pounds. In addition, we have been quite successful in measuring cavity pressures on most of those HE shots.

Carothers: If your measurements clearly show the existence of a region of long-term higher stress around your shots, why are there still arguments about whether or not there is what is called a residual stress cage, which presumably is the principal mechanism which causes nuclear events to be contained?

Smith: I suspect that nowadays everyone sort of believes there is residual stress, because it's been talked about and thought about for so long. But, good valid measurements on the nuclear scale are very hard to come by, and I think this is related to the inhomogenieties of the field. On the HE scale we were doing experiments in very selected areas, and we very carefully explored the geology ahead of time to make sure we had a good uniform material. We took a lot of samples to have it characterized, and so we had a lot of data for the calculators to play with. It was an almost homogeneous bed to work in, without fractures and faults, or major discontinuities. But when you go to the nuclear scale, you are encompassing all those geologic problems. The argument may be more now, "What are the departures from a homogeneous region, and how do these departures affect the residual stress?" Maybe these departures are sufficient to negate it to some extent, or in some regions.

While I was doing hydrofrac work I was also involved with the measurements on the DNA nuclear shots. Occasionally Don Eilers would talk Bob Bass into making measurements on some of his vertical shots, and so there were about a half dozen vertical shots, including some LLL shots, where we did some measurements.

Carothers: When you're working in the tunnels you're always working in the tuffs. Were the vertical shots deep enough that they were in the tuffs too?

Smith: Most of them were, but there was one I remember that was in the alluvium. That was U10be, one of the Livermore shots. It was a low yield thing, and we got some fairly nice measurements on that. It was the early days of the gypsum concrete plugs, and there were two stress measurements in one of those plugs; one at the top, and one near the bottom. They saw a little over half a kilobar, and after the dynamic phase they came down and showed about a hundred bar offset. We were recording the signals on a tape deck, which would run out of tape at about eight minutes. But, at about seven minutes these signals, which had been decaying, got down to zero stress level. So, those are a couple of measurements in alluvium that suggest there was a residual stress field loading that stemming plug. And so there are these bits and pieces of measurements on nuclear shots which say, "Yes, there's a residual stress."

Carothers: If you have a shock that's moving out in an infinite medium, after the shock has passed the material moves back a bit, doesn't it?

Rambo: Yes. I see that in the calculations. I think that's part of the fundamental process. There's material outside of the plastic region which responds in an elastic way. The wave runs through and pushes things out, and that whole elastic area outside of the plastic region tends to want to come back in an elastic type rebound. Even the plastic area does some of that.

So, for a brief period, we see in calculations, and it certainly is up for endless discussion, that there is a rebound. The data that we look at, in terms of velocity data, tends to show that also; the overdriven system wants to come back a little bit, to flow back, or to compress around the cavity. In the calculations we tend to see that kind of motion. We think that's the source of our residual stress field, and that's the source of what helps us in containment. That's

without respect to any reflections from layers, or the surface. Those tend to come, usually, after the rebound for a lot of events, but of course there are some that come earlier due to where layers are.

We see this motion in the velocity gauges that are put around many of the shots. You see the peak wave, and then the velocity starts to come back. If you integrate those, in many cases you get the motion of the material coming back, either to where it was or maybe not quite as far, depending on where you are. There are many cases where it comes back all the way, but there are a few cases where it doesn't.

The surface of the ground is a free surface, so the stress at the surface is, in the calculations, always zero. So, there is a reflection back, and it runs back down toward the shot. Spall is the occurrence of the doubling of the particle velocities at the surface. They're traveling twice as fast as the particles do from down below, and so the groumd tends to break apart. You see a rise at the surface that, if you have a sharp wave front, will go twice as fast as the particles do down below. And so this sends a signal that is releasing the stress.

Carothers: The shock goes out as a compressional wave, and is reflected back as a tension wave? It tends to pull the residual stress cage apart, in a sense?

Rambo: That's exactly right. Bob Terhune was worried about this tension, or rarefaction wave coming back from the surface on some particular shots. He thought he was able to see, in the calculations, some shots that had difficulties because high velocities brought this rarefaction wave back before the residual stress had time to set up. In the calculations, and I'm not sure I can answer exactly why in all cases, but if the rarefaction gets back before or during the time of the setup of the residual stress, it doesn't behave as well, at least in the calculations, and it may not set up right. And that may be a detriment to containment.

Hudson: I would say that the idea of a residual stress field as the key to containment is little more than a myth.

Carothers: You have attempted to make measurements of the residual stress field on some nuclear shots, haven't you?

Hudson I have. Not very successfully. I have one set of data on a low yield event, where the stress in the ground, in the vicinity of the deepest plug, which turned out to be about where the residual

stress field was expected, peaked at about a kilobar, or a little less, which was about where it was supposed to. It then fell rapidly to an almost steady state level at perhaps a fourth of a kilobar, which was kind of what was expected, or predicted for residual stress. I even published this, not too widely, but within the community. The data were criticized because there was no way we could demonstrate that the gauge had not been significantly affected by the strain in the medium.

This whole subject is called the inclusion problem. If you're in a stress regime where the ground behaves as a fluid, you don't have a problem, and you can probably make very good stress measurements. That boundary is probably at three or four kilobars. Above three or four kilobars almost everything acts, in the ground anyway, like a fluid. So, if you can measure the pressure, you probably know what the stress is. When you get below, say, one kilobar, then you're trying to make a measurement in a material which doesn't necessarily expand again after it is compressed. The result is that you can have a residual strain - - residual compression, residual expansion, what have you - - that continues to make the gauge feel like it's in a higher or lower stress field, when it really may not be. So, I sort of gave up on making residual stress measurements. They're almost imponderable.

Carothers: There was somebody who said that he could not think of any kind of stress gauge that you can make that isn't sensitive to strain.

Hudson: These stress gauges I'm talking about were designed so they could be corrected for strain. Some materials are much more sensitive to strain than others, and some are much more sensitive to stress than they are to strain. So, by using the right combination of materials you can subtract out the strain. But it's still hard to convince everyone that you've properly accounted for the problem.

App: I don't believe we know as much about the residual stress as we once thought we did. The people who have looked at the stress cage more closely than anybody have been the DNA. They have better control, because they're able to mine back, and they can use more gauging at working point level than we can. I've looked at the data that Carl Smith and Bob Bass have been collecting.

We've been using that data, and it's interesting that they cannot consistently see a residual stress in their stress measurements. Now, it may be an instrumentation problem, or it may be that the residual stress really is absent, or at least different than the way we model it. I don't know which. Calculations certainly show the formation of a residual stress field. There's no doubt about that. But that doesn't mean it actually exists in nature.

Carothers: There are people who might say something like the following: "The physics is right. The codes are right. And if you lived in a uniform, homogeneous world, and you calculated what was going to happen, you would see a residual stress cage, and it would be there. But you don't live in such a world."

App: Well, the codes are pretty good at looking at the potential effects of layering, and non-homogenieties. One suspicion is that material that has been shocked, has been worked, has been strained, and has had tremendous pore pressures built up due to trapped water, is a completely different material than it started out as. It loses its strength, and cannot support a residual stress field.

Some of the theoretical models predict no stress cage. The physics in the effective stress models would suggest that, out at least to some range, you have zero strength in the material. Now, the material has to have some residual shear strength in order to have a residual stress field. It has to be able to support deviatoric stress, or stress differences, in order to have a stress field of the type we're referring to, where the stress tangential to the cavity is higher than any other stress component. If the shear strength goes to zero, you can't have a residual stress field. There has to be some residual strength in that rock. Now, the question is, does that material have essentially no residual shear strength?

Russ Duff, of S-Cubed, has questioned the role of the residual stress as the principal agent of containment. As he expresses it, it is not the physics used in developing the calculational codes, but the presumptions upon which they are based that should be called into question.

Duff: The important observation, to me, in the Rainier reentry, was that the explosion developed a large quasi-spherical cavity with a reasonably well defined lower boundary. This lower boundary was surrounded by roughly a meter of plastically deformed rock, which was fractured at more or less regular intervals. But, outside of this meter or so, the statements are that the rock displacement seems to be dominated by generalized block motions, by motions that occurred along faults, bedding planes, joints; weaknesses in the rock of one sort or another. Now, that observation was made, and was well documented - - there are photographs, there are sketches, there are the clear words.

We can set that next to the comments that have been often made by Joe LaComb and others, that inside of something like two cavity radii you really can't make sense out of the displacements. Things move around in an unpredictable way. For instance, on Tom-Midnight Zephyr, which was a relatively low yield shot fired in Area 12, there was a reentry hole drilled from the tunnel back towards Tom through a region of displaced tuff. If you look at the configuration, and you expand the cavity, displace the rock as the naive picture would displace that rock, the reentry hole, RE #1, would pass from the tunnel to the working point through displaced tuff.

What was observed? Rubber, steel, electric cable, grout, tuff; little bits and pieces of all kinds of things. There was not spherical displacement, or quasi-spherical displacement. This is an example in the relatively recent history of the same thing that was pointed out concerning the displacements that were seen at Rainier. Now Rainier was very much simpler, being a shot with no line of sight, and no stemming in the way tunnels are currently stemmed.

The community has known this now for thirty years, and I feel that we haven't drawn the obvious conclusion from it. The conclusion is that our first-order model of what happens after an explosion, which is based on the assumption that a one-dimensional spherical picture is an acceptable, a correct first approximation to what goes on, is simply not correct. As we do more complex things, as we worry about layering, or as we do line-of-sight experiments in tunnels, or things of that sort, then we go to axi-symmetric calculations. We try to treat the wave reflections from interfaces, we look at the collapse of tunnels, and the interactions with line-of-

sight pipes, and things of that kind. This is all based on an extension of our belief that the first-order approximation of one-dimensional spherical motion is at least a place to start.

Out of this basic assumption comes our concept of the residual stress field. We say the explosion occurs, the cavity forms, the rock is forced out, there is plastic distortion. There is then elastic rebound, which compresses the rock, builds up a residual stress field, and "Voila!" We have the intellectual explanation for the "mystical magical membrane" that people used to talk about before the 1973 or 1974 time frame, when the residual stress concept was widely taken to be the basis for containment.

Carothers: Would it be fair to say that this assumption of a spherically symmetric cavity growth is based on the idea that the amount of deposited energy is so large, is deposited so fast, and the shocks that develop are so strong that within that region you're talking about it doesn't really matter what's there? That it overwhelms the material properties, and it doesn't matter whether it's tuff or alluvium or granite or whatever? Is that the basis of this approximation, do you think?

Duff: Well, that may be the basis of it, and that is what was observed at Rainier, but that approximation seems to apply only for one meter past the cavity boundary- - not for the region over which we think the residual stress field sets up and is effective.

Carothers: Which you take to be between one and two cavity radii?

Duff: Yes. So, I think what we have done, and I'm saying DNA now because DNA is the only testing organization which has made a practice of trying to measure rock properties and strengths in detail, is we've taken cores of the rock, and we have protected that core as well as we can. We have then sent it to the laboratory, primarily to Terra Tek, and they have developed good and presumably reliable techniques to measure the mechanical properties of that rock. And we have used those measured properties as input to material models, which then go into the code, and the continuum mechanics calculational procedures then give us predictions of stresses, velocities, displacements, and ultimately, residual stresses; all the observables and calculated parameters of interest.

I believe, however, that if nature tells us that the displacement for a major part of the overall phenomenon that we're looking at is not quasi-one dimensional, but is governed by the motion of more or less arbitrary blocks of rock, the predictions we get from a one-dimensional model may not be correct.

Now, I want to qualify that in the following sense. The explosion of a nuclear device does give rise to a very large energy release, and it gives rise to very high pressures. These pressures are going to send shock waves out, the shock waves are going to make material motions to generate particle displacements, particle velocities, and they will compress rock just as the one-dimensional argument says. But, if a material is free, or if a material chooses to deform in a non-radial way by slipping along joints or faults or bedding planes, then the overall response will be, or may be, intrinsically different than what we have accepted as intellectually satisfying. In other words, I'm arguing that the residual stress concept, which comes out of the one-dimensional simple picture, may be one of those constructs which seems consistent with the understanding, which is intellectually very satisfying, which meets the needs of the community, and which is flat wrong.

Carothers: I thought DNA people had made post-shot measurements in the tunnels, and that they had found evidence of residual stresses.

Duff: They have not found residual stress. The DNA efforts to measure residual stresses have come in two areas, basically. One of them has been reentry hydrofracs. They will decide that they're going to run a reentry tunnel between the work drift and the main drift on a particular shot. Usually before DNA runs a tunnel they do an exploratory boring to make sure that there's nothing ahead of them that would cause some particular concern. So, they'll have a drill hole that goes from some place near the end of stemming to the cavity boundary, or the cavity vicinity. After they've finished the reconnaissance in that hole they sometimes will hydrofrac it. They set a pair of packers in two places and pump in, let's say, blue dyed water. Then as they mine back, when they reenter this area they can see that the blue fractures go some direction, and some distance. From this they can get the directions of the fractures, and from the measurements of the hydrofracing pressures, they get an idea of the stress states that existed at the time.

Some of the experiments have shown directions of fracturing which are consistent with the expectation, or prediction, of a residual stress field. Inside of a particular radius the fractures are perpendicular to the hole, and outside they are parallel with the hole, or vice versa. But the magnitudes have, I think, routinely been comparable to or less than the magnitude of pressure required to break the rock before there was a shot. So, there is only at most a very small stress increase, but sometimes there is evidence that the directions are right.

Also there have been some efforts to install hydrofracing instruments. Typically this is a hose, or a pipe of some sort, at the end of which they put what has been described as a rebar nest. That is a whole bunch of rebars welded together, jammed in the end of a hole and grouted in. One can then hydrofrac this area with red dye, measuring the pressures. After the shot, and hopefully very soon after the shot, one will pump in blue dye and try to frac the rock again. Then when you reenter you compare the directions of the red fractures with the directions of the blue fractures. And you compare the pressure measurements as indications of the stress states. I don't think these techniques have worked very well - - the pressure lines break when the shot is fired, or something happens to the equipment.

There is a third system which is described as the zero moving parts system. This is equipment developed by Terra Tek, in which there is a high pressure vessel connected to a scratch gauge which indicates pressure. When the ground shock comes along, this high pressure vessel is opened, and a colored fluid is injected into the rock. The scratch gauge indicates the pressure history in the fluid. No electronics, no moving parts except the fluid runs out, and that's it.

That has provided data from at least one experiment. The evidence from the one case where it did work, that I heard about, is surprising because the indicated stress, at basically zero time and immediately after, was lower than pre-shot.

Carothers: Well, we know that can't be so.

Duff: No, I don't know we know that can't be so. The measurement is not consistent with the expectation of a residual stress, but you can argue that well, after all this was only the first time the equipment apparently worked. Maybe it didn't work,

maybe there was some bug somewhere - - so try it again. Maybe they have tried it again; I don't know. I think that when it comes to measuring residual stresses in a nuclear environment, we haven't done it. There are a lot of technical reasons why it's hard to do.

In the nuclear case, the early cases, when there were indications of low stresses, people said, "We didn't get around to reentering and drilling this hole and doing the hydrofrac until three, four, five, six months after the shot. Maybe the stress has just leaked away. But it must have been there earlier." Some of the other experiments, like the zero moving parts measurement by Terra Tek, suggest maybe there isn't any in the first place.

They have found some evidence that the directions of fractures are what one would expect based on the predictions, but they haven't found strong stress fields. Now, one can argue, "Oh, they have decayed away." That might be true.

Carothers: There were tests done at SRI - - small amounts of HE detonated in concrete blocks - - and residual stress fields were found.

Duff: Those were the grout-spheres tests at SRI. I think in that case we may have been misled by experiments which were modeling a real world, but the models were too good, in a sense. The grouts as poured were sufficiently homogeneous that the assumptions of the one-dimensional model were in fact reasonably valid for those experiments.

The measurement technique which was used in those tests consisted of circumferential copper wires cast into grout spheres. The sphere was then placed in a magnetic field, such that as the cavity was formed, and as the grout moved radially outward, the wires cut the magnetic field and generated a voltage; this voltage was proportional to the velocity of the wire. The diagnostics worked, and that in itself tells us the motion was reasonably uniform. It was not dominated by block displacements, which would have sheared the wires. That is a major diagnostic problem in the nuclear area; it's very difficult to get cable survival, which is why it has been difficult to get cavity pressures or cavity gas samples on a routine basis. The conclusion I've come to is that we have measured residual stresses in the grout spheres experiments, where

we're dealing with a homogeneous, well-behaved material. And they seem to be strong. But they go away quite quickly, through some diffusion or creep process.

I think any time that nature responds as the one-dimensional calculations suggest that it should respond, we will in fact get all of the results of the one-dimensional calculations - - the residual stress field and all the other things that go with it. My point is we that have had, in the books, the results of the very careful work that Livermore had the opportunity, and the skill, to do on Rainier. And all of us have heard Joe LaComb and others talk about the difficulties of understanding displacements within a couple of cavity radii of an explosion. I don't think we have drawn the appropriate conclusion from the information we have. And that conclusion, as far as I'm concerned, is that the assumptions we've made about how the world is going to respond do not lead to the way the world does respond. Therefore, the conclusions that we draw from our assumed response prediction may not be correct.

I think there is some residual stress field, because there is some plastic distortion. There is an elastic rebound, but I doubt if the residual stress field is of the magnitude that we predict, is in the locations that we predict, or that it sets up at the time that we predict. It's some result of the distortions and the displacements which actually occur, but not those that we assume based on the simple one-dimensional models.

Carothers: Let's see if you would agree with this. The calculations are fine, and they predict the right phenomenology, but for a world we don't have. If we're going to believe, or base our actions on this kind of a model we could be wrong. You might go on further and say that there are a few cases where we have been wrong for reasons that we have not yet explained, and the model does not give an explanation.

Duff: Precisely. I think that's well stated.

Let's look at some other bits of evidence. Cavity radius. I'm not talking about whether the cavity is oblate, or prolate, or spherized. We have a constant factor, called the K-factor, that is used in every presentation as a measure of the expected cavity size. And we find that 70 is a remarkably good empirical scaling constant for cavity size at NTS.

Carothers: Well, plus or minus twenty percent.

Duff: There is some spread. From eighty to sixty would get ninety percent of the cavities. Now, if you were to go to the person doing the calculations and say, "I have this rock. It is a lava from Area 19, and it is a pretty good basaltic material. We took it over to Terra Tek, and they said it was hard, tough, strong. Okay, Mr. Calculator put that into your code and tell me what the cavity dimension is going to be."

While he's doing that, somebody from Los Alamos brings in a core taken from the Sandpile alluvium. And with some effort Terra Tek will, in fact, come up with a strength for that. You give that to Mr. Calculator and say, "Tell me how big the cavity should be."

Carothers: About the same size?

Duff: No way.

Carothers: Well, that's what we see.

Duff: Sure. But that's not what we calculate.

Carothers: Well, that's Mr. Calculator's fault, isn't it?

Duff: Is it, Jim? Is it his fault, or is it the fact that the containment community, of which I am one, and my hand is up as guilty, has had it's head in the proverbial sand, like an ostrich, and has been ignoring the data?

My point is, we can't calculate even something so simple. The concepts that we think apply, namely that the material properties as measured in the laboratory, and fed into the material models that we want to use, give the right answers, don't. They don't give answers which are in good agreement with our observations. There are two things we can do about that. One of them is we could say we didn't calculate it right. Another one is, we could wonder if our model is wrong. Maybe we're not thinking about the problem right. What I'm suggesting for consideration here is that we're not thinking about it right.

And I have a piece of evidence. Let's consider Pile Driver. That was an experiment done in granite. The strength of that granite, measured in the usual Terra Tek or Livermore manner, I think turned out to be eighty kilobars. It is an extremely strong, competent rock. You put that into a code like TENSOR at Livermore, or TOODY, or STAR at Pac Tech, or CRAM here, or

SKIPPER here, and you get a very small cavity radius. And you get a number of other observables related to stresses and velocities. You get certain predictions. Then you ask, "What is the data?" The data is quite different.

Norton Rimer is one person who has had reasonable success trying to fit a material model to the Pile Driver experience, from first principles. He started with an explosion in a rock whose properties he defined, and made sure that he got the particle velocities and the stresses that were measured. In order to do that he had to use what he called an effective stress model. In other words, he said, "The strength of the rock is not even to a first approximation what Terra Tek measured." Its strength is related to the fluid pressures which you generate in the little fractures. The point is, it was the inhomogeneities in the rock, and not the rock itself, which were central to an effective description. Effective means we had a model which at least agreed with the observations. The straightforward calculation that we would make the way DNA, or Los Alamos, or Livermore ordinarily treats the problem doesn't come close. The code is probably okay; that's just F = ma, usually. And if one has done his job right on certain test problems you can believe that F = ma, and the code is computing that.

But I want to emphasize this point again in connection with the cavity radius observations. I think we are dealing with a situation where the response of the ground to the explosion is dominated by interface slipping characteristics. And, the interface characteristics are likely to be quite different from the apparent characteristics of intact rock. It is not inconceivable to me that the interfaces in hard rock can slip more or less as easily as interfaces can in alluvium. This leads me to question the prediction, the expectation, of a residual stress which comes from simple continuum mechanics codes. There the intrinsic assumption is that material points which start out close together will end up close together.

This assumption leads to a whole bunch of conclusions, residual stress being one of them. If the essential phenomena are governed by motions which don't satisfy the fundamental continuum mechanics assumption, then I don't think that as technical people we are justified in expecting the predictions of continuum mechanics to apply.

What this leads me to is a real question of whether the very convenient, very comfortable, appealing, residual stress concept, which we've all talked about for the last eighteen years, is more than a crutch; more than a construct which is convenient, but which may be quite irrelevant to our real problem. Now, I don't know that the conventional wisdom is wrong. I am saying there's a body of evidence that leads me to question it.

For the last several years there has been a damage failure surface which goes into the DNA calculations. A rock is assumed to be damaged by the shock process, and its strength after shock passage is less than it was before.

Carothers: How damaged unspecified, but damaged in some way?

Duff: Yes. If you take a rock to Terra Tek and you squeeze it, release it, and then you squeeze it again, it will show less strength than it showed the first time. It has been damaged in some way. We have modeled that kind of effect. The models that are used by the DNA community at the present time relate weakness to stress level. In other words, if you stress a rock to four kilobars, its strength is reduced by, say, thirty percent. If you go to six kilobars, it's forty percent. A stress related damage criterion is used in the code, and that fits the experimental data that comes out of the laboratory. It doesn't fit the experimental data which you would derive from core recovered after a shot.

That core is weaker than would be expected, on the basis of the existing damage models. Norton Rimer and Bill Proffer have been doing some material modeling work, and Norton has looked at a different way of describing damage. Instead of using a stress related criteria, he's using a strain related criteria. If you distort rock five percent, to make up some numbers, say the strength goes down ten percent. If you distort it twenty percent, the strength comes down more. He has developed a model, which is very preliminary, in which the model parameters chosen for the calculations were fitted to give the same results along a laboratory uniaxial strain load to four kilobars, and a biaxial strain unload as in the earlier damage models.

In other words, he and Bill treat the Terra Tek data in the same way. However, the two models give grossly different results on laboratory paths to peak stresses to eight kilobars. The newer strain

dependent model has the additional feature of approximating laboratory test data on post-shot damaged samples, whereas the earlier models did not. All of the parameters for the calculations consist of a single set of shear-strain parameters, and a range of damaged strengths varying from mush, for close-in, highly strained material - - which is consistent with the measurements - - to approximately one-half the virgin strengths. The results of the calculations show a later rebound, longer duration of rebound, and a residual stress state which Norton characterizes as marginal for preventing cavity gases from moving significant distances from the cavity. The calculated residual stress field has lower peaks at considerably greater ranges, and in fact, there are multiple residual stress peaks that come out of these calculations.

The residual stress concept, as we've thought about it, is based on relatively simple models of material response. Either the material is just strong - - it's elastic-plastic material, and does things as an elastic-plastic material does - - or it is a material which degrades in its performance it a particular way based on the stress levels reached. And, we have gone from these calculations to an intellectual construct, which gives us a framework in which to evaluate containment. Norton is saying, "If I look at exactly the same laboratory data in a different way, and certainly there is no a-priori basis for saying a stress criterion is better than a strain criterion for describing the onset of damage, I get qualitatively different answers."

Carothers: Tom, for years people have lived with the residual stress cage concept as a measure of goodness, if you like, when calculations are presented. I have had difficulty finding anyone who would say there was good experimental evidence for this residual stress, this "containment cage," in the field.

Kunkle: I have discussed this with Fred App at some length, and he is one of the principal modelers of residual stress fields around nuclear events. Indeed, he would very much like to have a stress profile, or a pressure sensor record to work with. The trouble is that the stress cage occurs in regions of intense groundshock; scaled ranges of maybe twenty scaled meters, and we don't have equipment that normally survives there. Livermore has fielded some experiments in an attempt to look for the residual stress, and I don't believe they've ever had a gauge survive and return

unambiguous pressure measurements that could be interpreted in terms of residual stress. So, it's a theoretical concept that we've never been able to validate, but we don't have, to my knowledge, any experimental data that would say it's incorrect. A major factor in containment research throughout the underground test program is what is the nature of the so-called "magic membrane" that keeps all the gas inside the cavity, or nearby the cavity.

Carothers: John, did your early SOC calculations show a residual stress field around the cavity?

Rambo: Our calculations did show that rebound phase, but because it was a spherical calculation it was constantly bouncing. The wave would go up to the surface and come back down, and then go back up again. But, by and large you could see some differences in residual stress if you had different strengths in there. So, it was kind of good enough to roughly characterize those things, and if you did have a big reflection coming in from the surface, or the edge of a layer that was close in, which was also spherical, sometimes that would make a difference in what you saw, even in a spherical sense.

And we thought, "Well, you know, it's kind of conservative because these reflections come back rather strongly, and if you can survive it as a sphere, then maybe you can survive it in a real situation where the layers are flat and not reflecting quite so strongly." That was the logic behind how we started in that area, and we did do a lot of calculations which we got up in front of the CEP and presented, showing these things.

Carothers: There are people who say there is no experimental evidence that we have, that shows a rebound and a stress cage on an actual shot. Maybe you do get stress fields over here, but they might be bigger than you calculate, and over there they might be smaller, or non-existent, because of the various beds, and layers, and faults, and blocks, and so on. Could you comment on that?

Rambo: You said we've never measured a residual stress, and I say, "Well, is that because we haven't been able to measure it effectively, or is it that the measurements that did take place didn't show anything?"

Carothers: Well, rarely do you look. When you do, the instruments don't survive. Or it's been a long time later, and that stress field has decayed. It is, in fact, a very difficult experimental problem.

Rambo: There was some data from Orkney, a Livermore shot up in area 10. This event did have gauges that would supposedly measure the hoop stress and the radial stress, in two different locations, and the instruments survived. In fact, you could probably run them today if you wanted to. I ran a 1-D calculation to see if I got anything that looked like what they measured. The calculation that I did, going through my normal procedure of guessing things about the material properties, showed residual stress. The gauges also showed what looked like a residual stress, but not to the degree I calculated it. The timing was about right, but the magnitude of it seemed to be less than I calculated. I think that what's happening out in the real world is that there may not be as much residual stress as I calculate.

You can get into arguments about, "Well, was that real data, or are there other things that went on?" That argument goes for almost everything we've measured in the field. My point is, maybe what I'm doing isn't completely erroneous. Over the years I've come to put a lot of faith in the shear strength in my models, as being part of what takes place in terms of this rebound, and how good it is and how good it isn't. In looking at a lot of the logs, where I've tried to divine the shear strength from looking at the velocity logs, I get a feel that the shear strength varys all over the place. It's one of those things that comes and goes, and comes and goes. You can look at density logs and they don't look the same as what we might be experiencing terms of shear strength.

What I think is out there is not homogeneous, and I agree with that completely. I think that there are areas where the residual stress may look a lot better than in other areas. It may have a lot to do with why you get cavities that are not spherical, and why you may go in one direction, even horizontally, or off to one particular side, and you don't see the things that you see in a calculation. And that's because of the limited amount of information I have, to do what I have to do in terms of averaging properties and organizing the materials. I'm looking for generic effects when I do these things, and weaknesses. But I have to also say that there are some cases

where we've modeled a generic weakness, and we may have seen the same thing in the field. I say, "may", because the statistics are very poor.

There are things like Baneberry, which we modeled, that didn't show residual stress. There was a lot of evidence that it didn't have anything like that. For instance, it leaked out of the ground. More recently there was the Barnwell event, which looked calculationally like it had residual stress problems. And after the shot there was radiation high in the stemming. There was Nash, which I did run some calculations on and compared to the Bourbon event. Nash looked worse than Bourbon, and Nash leaked but Bourbon contained. That is probably the only evidence of things actually having happened that I calculated.

The statistics are very poor. There have been cases where I've calculated things that showed residual stress, and they leaked, or had some difficulties. And there have been some cases where I did a calculation which showed that didn't have any residual stress, and they contained just fine. But there's one thread that seems to wander through these calculations of residual stress, although the statistics, as I said earlier, are terrible. That is, there's usually something else wrong with the event besides the residual stress. On Baneberry there was lots clay and lots of water. On Barnwell there was also quite a bit of water. On Nash there was a lot of CO2, a non-condensable gas. Those things may play a factor. If you know you haven't got any residual stress, it may be a secondary thing that is really important. To draw a conclusion out of three or four events like that is a very poor style, but nevertheless in this business, I keep looking for a thread.

Carothers: Russ Duff has said that the calculations are not wrong, but the world in which you work is not the kind of world that the calculations calculate. That's the business of the inhomogeneities, the layers of different rocks, the three dimensionality, possibly block motions. If you only had the right kind of world, the calculations would be just fine, but you're applying them to a world that doesn't exist.

Rambo: I would like to temper that comment a bit. There are some areas where the non-homogeneities are more apparent than others. Take the tunnels, where you're in stronger rock, and there are lots of fracture planes. They have indeed seen motion along

these planes, and the calculators that I talk with say, "We just can't model that sort of thing yet. Or maybe we will never be able to model that kind of thing." Those fracture planes may play a strong role in what eventually ends up as the non-residual stress, or the residual stress being taken away. But as you get down to the Flat, the differences in the strength are not quite as different. In the Flat we're talking about more of a soil type of material, but still there are those areas that have hard rocks and porous materials.

My experience is in looking at drilling rates. In the Flat, drilling tends to go fairly quickly through most of the tuffs - - not all of them, but most of them. I get a different impression from that than what I see up on the Mesa, in looking at the strengths that are measured in the tunnels. It's just a bias that I've picked up over the years, in looking at, and becoming more aware of what's happening in the tunnels. A calculator tends to look at things a little bit differently, because he's looking for, or trying to divine, properties that have to do with containment, or those he thinks have to do with containment.

Another answer to this question about residual stress is that many of the people who say there isn't anything such as residual stress are talking about shots in the tunnels. That's the discussion that seems to be going on now. One of the things that has come through this whole business is that, in the lore, low yield events have more trouble containing than high yield events. And, the people in the tunnels are always shooting in a subkiloton to maybe less than two kilotons range, for the most part. They have done ten kilotons shots, but the low yield events seem to be showing most of the residual stress problems. Or, most of the events where they've leaked radioactivity have been in the low yield range.

To a first degree I try to put layers in the model at different strengths, but there may be things that we don't know are there, or cracks, or the strength properties we may think are all one strength may not be. My argument is that you see more of this kind of thing in the tunnels than you do out in the Flat. My feeling is you ought to see it where you have relatively high strength rock with cracks, and with lots of weakness around the shot point. Those things are going to move, and they do move; in the tunnels they can see that they have.

Although we can hit those kinds of things occasionally in the Flat, I believe we're in more of a soil-like material where the difference in strengths between the material and the fracture zones is less. So, the block motion is not going to be quite so strong.

Carothers: How long do you think the residual stress stays there?

Rambo: In looking at Billy Hudson's cavity pressure measurements, that pressure seems to decay rather quickly for a half minute or a minute. Then it seems to decay very slowly. I'm saying you can only have cavity pressure if there's something there to hold it, so I'm making an association between the cavity pressure that's sitting there, and some sort of residual stress that holds it in. Your question hasn't got an easy answer to it.

Carothers: What mechanism would you hypothesize that would allow or cause a relaxation of the residual stress?

Rambo: I think there could be constant readjusting. First of all, the cavity pressure is likely to decay away because there are cracks and porosity for the gases to go through. As this happens I think the pressure against the cavity walls becomes less, and the materials start to rearrange themselves in terms of stress fields. You hear this in the geophone record as a constant rumbling that's goes on after the shot, before collapse takes place. I think the cooling can even bring some of the cavity gases into condensing to the point where the cavity is at less than atmospheric pressure, and that has certainly been noticed on some shots.

I think this relieving mechanism is just the normal part of the collapse process that's taking place. I don't understand it very well; I can understand how you can get pressure decaying, and causing some of that. What happens after that is just mysterious in my mind, because I've never heard any explanation of it. It has to do with things like what's the strength of various blocks, and this, that, and the other thing. It's the mysterious part of this business, that we have no knowledge of, that sometimes has a lot to do with the success or failure of a shot. That was Agrini and Riola. There are mechanisms out there that have nothing to do with residual stress or what's in the cavity, and that is the risk factor which we can't do much about that goes along with a shot.

Carothers: Mr Rimer, from things you have said, I take it that you believe in the residual stress field.

Rimer: I believe in it for relatively homogeneous materials. The problem is, on nuclear events, we have never successfully measured residual compressive hoop stresses. There are one or two measurements where we put the gauge side-on, and we've gotten records that last a long time. There are funny things that I've seen when you compare those records to a radial stress record at the same location.

On small scale experiments, like the SRI grout spheres, we've actually seen the effects of residual stress. There was a tube in those spheres that was to connect to the cavity after the explosion, so we could hydrofracture from the cavity. Well, once there was a break in the tube, and instead of going all the way to the cavity, it broke somewhere in between. When we pumped in that dyed fluid, it went all around the cavity, right where the dip in the residual stress field was supposed to be. It didn't go into the cavity. It found the easiest path, and that's where it went.

On HE tests Carl Smith has measured very long time stresses. Unfortunately, these are the radial ones, the ones that don't matter too much. We need the hoop sresses. It is a strong containment diagnostics goal of DNA to try to measure these residual stresses.

Bass: You're not liable to see residual stress show up on a radial stress gauge, and that's where all the measurements are made to try to find it. You can see it on a hoop stress gauge if the hoop stress gauge lasts long enough. Those measurements have not been very successful, and they are too far out.

Carothers: A criticism I have heard of those measurements is, "Convince me that you're measuring stress and not strain."

Bass: I won't argue that point at all. Especially when you get down to the range where you can make the measurement. When you get below the shear strength of the tuff, which is three-tenths of a kilobar, I don't know what's going on, and I don't know what we're measuring. I think we're measuring the pressure component, rather than a stress component when we get below three-tenths of a kilobar. And we've got a lot of information saying that's the case, because the curve bends off the wrong way when you make those measurements. This falloff steepens when you get below three-

tenths of a kilobar. In my compilations of data, which are used sort of as the bible of what ground shock exists where, I say don't draw the line below twice the strength of material, because we don't know what we're doing in that region. We just flat don't know.

Rimer: On Misty Echo and Mission Ghost we, preshot, hydrofractured the rock to get the in-situ stresses. Then post-shot we hydrofractured it again. Observations in G tunnel by Carl Smith on HE events showed that the directions of the minimum stresses are oriented differently post-shot than they were pre-shot. That change remains for many months; the magnitudes of the stresses don't remain, but the directions do. We found a change in direction on Mission Ghost. The magnitudes though, where we predict two hundred bars, they were sixty bars, but they were in the right spot. The directions were changed, and the largest changes we measured with the post-shot hydrofractures were near where the largest residual stresses were supposed to be.

Carothers: People have talked about the residual stress as unloading, or relaxing, either due to migration of water out of the pores, or due to creep, but they don't talk about very long time scales. Certainly not months.

Rimer: Minutes. We have tried to calculate this. Pac Tech has used the standard creep model, with data from lab tests at Terra Tek on tuffs. We've tried that way, and we've also tried with a pore fluid migration model, with detailed effective stress concepts. It's difficult; we can make those codes do almost anything, because we haven't tied down the material properties, the models of the rock, especially after a ground shock has passed through. We can't give, from those calculations, a precise time frame for it, but I would say it's minutes. Because we don't know how to tie the calculations down, on every event we're still trying to measure residual stress. But I would say that stress field relaxes in minutes.

Carothers: It's hard to believe that in minutes there would be enough fluid migration to do very much. The permeability is rather low, the pressure gradients aren't all that high, and the fluid has to move a fair distance. The residual stress field, if it does exist, isn't as thin as a foot.

Rimer: No. It depends on the yield of the device, but it's in the range of many meters. But that's another thing we don't know - - how far the fluid has to move to relieve these stresses.

Carothers: Dan, let me make a summary statement that I think represents what a number of people have said about residual stress calculations. People who calculate shocks going out, and so on, are using the right physics, and their codes are okay, and the calculations are fine. However, when they do that they're always assuming a homogeneous medium. When you look at the grout sphere experiments, or the work that Carl Smith did - - Carl searched around in G tunnel for homogeneous blocks in which to do his experiments - - those are homogeneous media. Unfortunately, the world isn't like that. There are layers, and cracks, and fractures, and so you don't actually know what the material properties are, on the scale that you're going to be calculating.

Patch: Sure. One of the problems is basically what you're referring to, which is the geostructure - - fractures, and bedding planes, and all that stuff.

It would be surprising if you didn't run into some perturbation of this so-called stress field that's formed around the cavity. One of the problems that we certainly have is that we don't have a direct measurement of it. We've been trying and trying to get a direct measure of the residual stress field - - what the stress state is, after the shot is over, for a real shot in a real medium, in more than one place. That is certainly a very high priority goal in the DNA containment diagnostic program.

Carothers: And that's a measurement that is very hard to do.

Patch: Yes, very hard to do. The second thing that has given us a great deal of concern is the time dependence of this stress state. We think we know when it sets up. We're pretty uncertain, unfortunately, what its actual magnitude is, and surely don't know when it goes away. There seems to be a body of evidence that suggests it can go away pretty darn quickly.

Terra Tek did some work back probably in the mid-eighties, trying to simulate creep for loaded tuffs. It was an outgrowth of these questions and issues that came out of the SRI program. When SRI fired these little shots, and then subsequently fractured them, it made a difference when they fractured the cavity. If they did it

very quickly, they found very high fracture resistances. It took measured pressures as high as five or six thousand psi in trying to break out of those little explosively formed cavities. That seemed to be pretty strong evidence of a residual stress field, since the spheres would only hold about fifteen hundred if you just fractured a natural cavity. But if you waited, the fracture pressure that the cavity could hold dropped with time, and it dropped very quickly. A matter of half a minute made a difference - - it might bring it from six thousand down to two thousand or so.

Carothers: What do you think causes that to happen?

Patch: There are two schools of thought. One is that it's basically the pore fluid migrating down the pressure gradient. Conceptually, oversimplifying, it carries the stress with it. The fluid flows, and it's under the highest pressure where the stress is the highest, and it goes away, relieving the stress. And that kind of mechanism scales. The bigger the shot the longer the time it takes; it all scales as the cube root of the yield.

The other possibility is that it's creep, or a stress relaxation mechanism of a semi-classical type; a material that is loaded has a stress difference on it, and it tries to flow in a quasi-plastic kind of way. That, in some sense, is a point property, and it's independent on the size of the medium. And so, these two mechanisms, in terms of their time dependence, are very different. The implication is that for a nuclear shot, if the stress field were flowing out as a pore fluid effect, it would take a very long period of time, because you're trying to migrate fluid down what is a shallow gradient in terms of psi per foot, and you have to move a lot of water. The other mechanism is independent of that. It just tries to equilibrate stress differences. Each little microelement of the material, if you will, is unhappy and readjusts it's grains, or whatever it wants to do to accommodate that.

I have been more of the creep mechanism school, myself. The reason, as much as any is that some folks who are smarter than I took a look at what would happen if you took an stressed material, and had the pore fluid flow out of it. Unfortunately, the stress is not like colored dye, and the psi's don't flow with the fluid. What happens is that the material tends to transfer the load; part of it comes out

with the fluid, and part of it is taken up by the matrix of the material. And then the issue was, does it take up a lot of it, or a little of it. My recollection is that it didn't really cancel out very well.

Ristvet: If we believe some of our recent DNA data, yes, we have residual stress, but it's very small. I think some of the measurements we made on the last three events kind of suggest that yes, the residual stress is there, but the magnitude is less than the cavity pressure. What's interesting is we are now calculating those small numbers using a discrete element code that allows certain block motions to occur.

We're getting almost to the point where we can make some measurements. We're finally getting smart enough about how to make the measurements, after twenty some events where we failed. And everybody knew what the problem was; it's called cable survivability. So we went out and made the hardest cables we could, and I give credit to SRI, and in part to Carl Keller who modified SRI's design, and then to Sandia who even made it better. What they have developed is this wire rope wrapped cable. In the tuff or alluvium I think it will work just great, because it can cut through the medium, in a sense, because it is so rigid in comparison, and yet it can protect the soft conductors inside.

Carothers: Norton Rimer has said that as far as he was concerned the best location for testing a device was in a weak rock. If you have a strong rock, like granite, you will get a small cavity, high pressure, and a lot of tensile fractures. He said he liked a nice soft, forgiving rock.

App: Same here. I believe that. Our current models of the ashflow tuffs at the Nevada Test Site suggest that you get a stronger residual stress field in them than in other rocks. For example, you don't get a lot of tensile failure. The failure is predominately shear failure; the material is not physically pulling apart. Also there is a lot of rebound for the formation of a residual stress field. Calculationally, the residual stress is stronger than you get for a weaker material like alluvium, or for a denser material like welded tuff or lava. I think what Norton said is right.

Lava is strong in shear, and it is always jointed. You're not going to find many rocks that are not jointed. The shear strength might be quite high, but the effective tensile strength is zero; during

the outward cavity growth the cracks open up. During rebound they close down again, but during that hysteresis period when the cracks are opening and closing the mechanism isn't there to create a residual stress field, because residual stress formation depends on shear failure.

When the material is failing in shear, as soon as the rebound starts you immediately start forming the compressive, elastic stresses that comprise the residual stress field. So, there's a very basic phenomenological difference between a strong rock and what I will call a medium strength rock such as ashflow tuff. On the other hand, when you go to a very weak rock, like a Baneberry clay, there's not enough strength to support any kind of shear, or residual stress.

If you make a plot of calculated peak residual stress versus strength of the rock, it starts out very low, increases with increasing strength, hits a peak, and then decreases with increasing strength. The way the models are currently set up, it appears that the ashflow tuffs are almost ideal for the formation of a strong residual stress field. The fact that the alluvium is very weak doesn't matter that much because the water table is below it, so it's dry, and there is a lot of volume to take up the gases, even if it doesn't form much of a residual stress cage.

12

Hydrofractures

As discussed in the preceding chapter, what might be called the conventional view, and the conventional calculations assume a homogeneous medium,. Energy is deposited in that medium, and there is a spherical shockwave that goes out. The properties of the medium lead to a rebound of the material, and to the formation around the cavity of a stressed region which is called the residual stress cage, or containment cage. The stress in the rocks in that region is high enough that the pressure in the cavity cannot drive gas or fractures through it. In this view, the residual stress is an important phenomenon in containing the gases produced by the explosion.

There is another view, which might be expressed as follows: there are pieces of evidence which are hard to reconcile with the conventional model. There might or might not be a stress cage, but as a matter of fact, such a concept could be a wrong road. The principal mechanism that accounts for containment could be the release of cavity pressure through fractures driven from the cavity. Because of the nature of the material the fractures don't propagate far enough to reach the surface, although they might through preexisting weaknesses such as fractures or cracks. Perhaps the leading proponent of this view was Russell Duff.

Duff: There is a very considerable body of evidence about containment mechanisms that has been around for a long time, and I don't think our community has responded to that evidence in a responsible scientific fashion, in that the response has not been as true to the scientific method as we might like to think. There is an alternative containment concept to the residual stress cage concept, and that's the work of Griffith and Nilsen on fracture-related containment mechanisms.

Carothers: At the CEP you have talked about calculations which indicated fractures go out very quickly, but there's so much cooling to the walls, and so much pressure needed to drive them, that at most they only go a hundred meters or so. In this picture of containment, as I understand it, the hypothetical stress cage has

little to do with it. There are fractures, and as a matter of fact, the more fractures there are the better it is, because they lead to cooling, and to a decrease in the pressure in the cavity.

Duff: I can provide a piece of pretty good evidence to support the fracture argument. Let's talk about Red Hot. This was an event which occurred in a hemispherical cavity. The yield was relatively small. We have calculated the expected cavity expansion from this event, and it's about three or four meters. What's observed is roughly one meter.

When you have a twenty-three meter start and then you go one more, or you go three or four more, that is a big relative volume difference. From 23 meters to 24 meters is a little bit of expansion, like 12 or 13 percent in volume. From 23 meters to 27 or 28 meters is a lot of expansion, like 60 to 80 percent. What mechanism can make the cavity not expand? Well, one obvious thing is that the pressure went away. When would the pressure have to go away to make the cavity expansion only be one meter instead of three or four? The answer is five or ten milliseconds. Now, that is so fast that whatever happened did so inside of any time frame in which residual stress fields would be set up; that would be more like a hundred millisecond time frame.

So, how can nature get rid of the pressure from an explosion in ten milliseconds? Nilsen looked at this problem, and looked at the fracture system that you might expect from such an explosion in such a cavity. He used his code called FAST, which is a calculating system which is related in many ways to analytic treatments. He came up with an answer that it would require fractures from the cavity at roughly three meter intervals to dump the pressure.

We reentered Red Hot, and it happened that the reentry drift intersected a fracture; you can see it in the floor of the reentry drift. It goes out about fifteen or twenty feet from the cavity boundary and stops, so it wasn't driven for a very long time, but it was driven quite energetically. It is a very narrow crack for the last few feet, but it is quite a large fracture at the cavity boundary. There is a grapefruit sized hunk of rock in this glass-filled fracture, and that rock came from some place far away. So, there was at least one fracture on Red Hot. It didn't go very far, probably because the pressure didn't last very long. Let's say the pressure didn't last very long because there was a system of fractures, lots of fractures.

Nilsen said you could kill the pressure if you had fractures every three meters. So, Joe LaComb drilled a hole parallel to the flat face of the cavity, and I believe he encountered fourteen fractures along the length of this hole. On average they were three meters apart. So, I think that there is a net of at least circumstantial evidence which says Red Hot was contained because a whole system of fractures developed and they dumped the pressure on a very fast time scale.

Ristvet: There was an another hypothesis, which was that the crater threw a lot of cold debris into the cavity. When we looked at the crater through the drilling, with the TV cameras, it was almost exactly as S-Cubed and myself had predicted. I did it empirically, and S-Cubed did it calculationally. The throwout was very small, because the high pressures in the cavity just didn't let anything get thrown out. You have to have extremely high ejection velocities to move through that overpressure.

That also says something else about the timing, which helped validate the calculations too. Those high pressures lasted for only a few tens of miliseconds, and then they dropped very, very fast. That was probably during the time those short, stubby fractures formed.

Now, we did see, on Red Hot reentry, two steam type hydrofracs, the kind with no glass, or very little glass associated with them. They went up above the Deep Well access drift to the base of the vitric. They follow the in-situ stress field perfectly. Those two are not well explained. They had to occur at a very early time, while the pressure was still up, and probably the other fracs were still forming. And maybe they continued to grow during the dynamic phases of the tunnel and cavity growth.

Carothers: I have heard that on Red Hot there is a big fracture that extends a long way, and is wide and open.

Ristvet: Yes, that's also in the Deep Well access drift, where we saw these two steam-type fractures. Those were observed during the actual reentry when Bill Vollendorf and probably Mel Merrit, because he was the scientific director, or whatever the title was in those days, on the shot, went back in there. And yes, they could see this big opening in the top of the Deep Well access drift, filled with glass. However, the ones we actually mined up to were very

wide, a foot wide or so, but they didn't go anywhere. They only went three, four, five meters from the cavity. I think the viscosity of that glass just plugs those things up real quick.

Smith: Well, in addition to those short fractures, there is that fracture that goes over the top of the drift that went over to Deep Well. And this is a fracture with radioactivity in it. My predecessor had them drill down, and my impression is that they traced it down about thirty feet. When I got into the program I was still curious about it, and we drilled a bunch more holes up. It goes up over a hundred feet from where the tunnel intersects it; we drilled holes through it, and ran radiation probes through it. So, in addition to all those short fractures there is this additional one, and I think people tend to forget about that fracture. They concentrate on what was found in the DNA work, when they were looking at all the phenomenology of decoupled and coupled shots.

Carothers: Gary, there were samples of glass found in the fractures that occured on Ranier. Could you tell, from the radiochemistry, when those fractures occured?

Higgins: That fracuring was going on within the first two hundred milliseconds, because the material found in them was from the cavity itself, like copper, and uranium. Uranium is one of those elements that, if it has the slightest opportunity, is going to recombine with any oxygen present. It had done that, but it had done so locally, sufficient to create F-centers, where it had stripped away electrons and made a little electron-deficient well around it. We could see that by x-ray diffraction; we could see islands around the uranium where it had become uranium oxide at the expense of all of its good neighbors. It had arrived as a metal, or it wouldn't have done that, and that record would not have been in the glass. That glass is bright red, instead of being black, because all of the F-centers are color-reactive. The bright red color is because the uranium has made some of the silicon dioxide into silicon monoxide, which only exists as a gas, or in a glass as a dissolved gas.

Carothers: You say this fracture occurred during the first two hundred milliseconds. Does that mean it occurred before the rebound, and perhaps the rebound shut it off?

Higgins: That's correct. I think there's good evidence, from the chemistry, and also now in the calculations to indicate that. During cavity growth, if the cavity gases are at a high enough pressure, and they are, fractures will occur. The mystical magical membrane idea occurred because we knew the pressure was high enough to hydrofract, but it didn't. Well, there's now evidence that it does hydrofract, and part of the normal rebound process is pinching those cracks off. I think that in many materials, like in alluvium, that is also a transient phenomenon, that there is another outgoing relaxation wave. However, that's a sonic wave, and takes many, many milliseconds. The stress cage builds up, shuts the fractures off, and then the stresses relax. By then the pressure and temperature have gone down to where they are essentially in equilibrium with their surroundings.

Carothers: Things have to happen in sequence on a pretty fast time scale to keep you out of trouble in the scenario you describe.

Higgins: Yes, that's exactly right. I believe that pretty fast time scale means some of our mysterious failures are cases where that sequence was just a little out of step.

Carothers: From the evidence from Rainier, seeing the fractures and so on, wouldn't one be led to think that hydrofractures could occur on all shots?"

Higgins: Yes.

Carothers: Now, there are shots which don't release enough energy to form much of a stress cage, if any. Why don't they hydrofract to the surface?

Higgins: I think they do hydrofract, and what contains them is primarily cooling in the fractures. They don't have enough energy to form a stress cage, and they also don't have enough energy to drive a fracture; it takes a lot of energy to do that. You can blow material into the front of the crack, but to get it very far down the crack is really very difficult. People who have tried to calculate hydrofracture from a theoretical point of view are always astounded at how difficult it is to drive a hydrofracture. To initiate a fracture is very easy. To drive it any considerable distance is a very hard thing to do.

Carothers: Particularly when there are losses into the walls, and where you have cooling so you have liquid at the tip of the crack, which you're trying to push on from the back.

Higgins: Yes. Absolutely. The first thing condensation does, and I think is the most important thing, although I haven't convinced anyone of it, is to cause the tip of the fracture to cease being a discontinuity, and become a rounded hemispherical circle. And that happens fairly fast if you try to drive a fracture with a condensable liquid. I've often thought it might be fun to try to simulate such things with a liquid metal driving a fracture into a cold, solid metal. It's not likely to go very far, because you're going to get a wad pretty fast.

Keller: When I was at DNA I funded S-Cubed to build a hydrogen-oxygen torch as a very well controlled high pressure, high temperature steam source, to use in experiments to validate the condensable flow codes; the hydrofrac codes. Some experiments were done in sand-filled pipes to check the porous flow, and some were done in drill holes in G tunnel and P tunnel. The first two in G tunnel worked very well. The last experiment in P tunnel was like the Perils of Pauline. They had trouble, and finally it was a lot of effort which didn't produce very good data. But the first couple of experiments have been used numerous times as proof of the models.

Peterson: I and another fellow, and a few other people here, put together a steam generator that burned hydrogen and oxygen. With that we did some fracture tests in the very impermeable tuffs in G tunnel. On the tests we had a bore hole that was drilled in from the tunnel. What I call the test region, where the steam was being injected, was a four-inch diameter hole eighteen inches long. We injected hydrogen and oxygen and burned it in that little section of the hole. And we also injected water, which turned to steam, to get the right steam conditions. We were trying to get a steam source that had characteristics similar to what we thought was in the cavity.

To do that we were running about a thousand degree Centigrade steam, and I believe we were running pressures of seven or eight hundred psi. We could adjust the steam generator to give whatever we thought we needed for the source conditions. The energy was tremendous that we were putting in there; we were

dumping like one or two megawatts into that little hole. To run for about two minutes required twenty-four big cylinders of hydrogen and twelve cylinders of oxygen.

We looked at the steam flow, and the fracture propagation. The main attempt was to try to calibrate the KRAK code, and validate it. So, we looked at steam fracturing from that source, and steam flow, and steam condensation. We had numbers of drill holes that had been drilled in at various distances from the source hole, and we looked at the fracture tip propagation across those bore holes, and looked at the pressure rise, and so forth and so on. It was to get a better idea of fracturing, to see whether the models really do calculate steam fracturing correctly.

Carothers: When you hydrofracture something you take some water, or steam, or whatever. You pressurize it. There's a little discontinuity in the rock, and the rock cracks. The fluid moves down the crack, transmits the pressure, and the crack extends. That's my view of hydrofracture.

Peterson: I don't think it's any different than mine. I think it's been interesting over the last five years to see what we've learned in terms of fracturing. If we look at fracturing from a cavity, and we take a standard tamped shot, the only time, in most cases, that it looks like you can get any fracture from this cavity is during the time that the cavity is actually growing.

That's the only time that the stress fields are set up in a manner which allows the pressure in the fracture to be greater than the confining pressure. If the confining pressure around the fracture is greater than the pressure inside, the fracture just closes back up. It won't grow. While the cavity growth is continuing, the shockwave is moving out further, and the shock is way ahead of the cavity. Sometimes you can see that you can get these fractures that will grow a little bit. They don't go very far and they don't last very long in time. And then when the stress fields change, they are again closed right up. So the most you see when you go back into one of these events is one of these gas seams that people will talk about once in a while. They saw a little, thin seam that had some radioactivity in it. Even our calculations, at least the ones that I have seen, never indicate that once the cavity is formed that you can fracture out of it any more. If our calculations are right, you just can't because the pressure in the cavity is too low by that time.

Carothers: When the cavity is expanding the material at the boundary has to be moving apart and so that makes it easier for something to keep pushing it apart, because it's stretching, in a way.

Peterson: Yes. And when it stops stretching, and stops that outward velocity you can look at it crudely as the momentum just squeezes it back together. And the reflection of the stress wave from a long way away comes back too, and just squeezes it all back shut.

Carothers: There are pictures taken during the Rainier reentry showing thick seams of dark material, which were glass from the cavity, or material from the cavity, that flowed out in the rock a long way.

Peterson: I don't know what you term a long way. If you're talking like one cavity radius outside the cavity, to me that isn't very far at all. Something like that does not disagree with the analyses that we have done, and is not surprising, and I don't think is unusual. I think one could expect it.

Carothers: There was also the supposition on Rainier that this could be attributed to a separation in bedding planes, rather than a fracture of the native rock.

Peterson: I think that's true, but if I go back and put on a calculator's hat, I don't think it's fair to distinguish the fact that the fracture went along a bedding plane. If the stresses are set up so you can grow this fracture, it's obviously going to pick the easiest place to go. If there's a fault line that's aimed in the right direction, it will go that way. It likes to take the easiest path. So, that's where one would expect to see them. I don't think it's going to start off through the middle of a big bed of rock all by itself, if it could take the planes in one of the interfaces and go along that direction.

I think the interesting thing from the work that's been done on fracturing is that it has allowed us to at least think, at the present, that we understand why Red Hot contained, and didn't just blow everything out of that tunnel.

There was a plug formed in the tunnel, and that plug was moving out fairly rapidly. If you go back and do basic back of the envelope analysis, if you did have a classical cavity pressure history back in Red Hot, that plug should never have stopped. It should never have even wanted to slow down. The analyses, now that we

can do fracture calculations, show that if you detonate something in a cavity like Red Hot, you grow multitudes of fractures. Just multitudes of them. There's no reason that the world around it doesn't want to fracture.

Red Hot was in a pre-formed cavity, and as a result there wasn't much plastic deformation. There weren't big stresses built up in the material around the cavity. The cavity pressure is extremely large compared to the stresses surrounding it. And so it likes to fract, just as they do these massive hydrofracs in the oil field. It's analogous. So you get multitudes of these fractures, and the harder you drive these fractures the more of them you get. When you really drive the rock hard, as on Red Hot, you get a tremendous number of them that are formed.

So, you get a lot of surface area, and as you get a lot of surface area, then you get a lot of cooling. And so you quench the pressure really fast. Of course, that quenches the fractures, and then they all just sort of dribble out and quit. Yet the cavity pressure has gone down tremendously to the point that it isn't really a containment problem. I think that's what our fracturing modeling is telling us. In the reentry on Red Hot, over the last few years, they've found many of these types of fractures that have been driven from that cavity. So, the model may even be correct. I think the fracturing work has been a very good thing to have done, and has given us another handle on why things contain.

Duff: The leakage, the almost disaster, which was associated with Red Hot was related not to a long, high driving cavity pressure, but to a very poor stemming plan. It was stemmed by a wall of sandbags, and that wall of sandbags acted as the wadding in a shotgun. It was put in motion by the pressure, and proceeded to knock out the succeeding closure systems, one after the other. It came to rest twelve hundred feet down the drift, and we were just lucky.

Carothers: John, arguments have been made that hydrofractures from the growing cavity are at least part of the reason shots contain. Do you place any credence in that model?

Rambo: I certainly place some credence in it. I think the hydrofractures don't go all that far because of the cracks there are in the rocks. So, they tend to cool down, and not go too far. But

that puts, I think, a little more responsibility on us to think about what other pathways are available for the gases to go some place. I think hydrofractures are part of it.

Carothers: The pressure acts everywhere. There's a bedding plane, go that way. There's a fault, go that way. You can't take account of that in your calculations, can you?

Rambo: No, the late time phenomenon is not accounted for. When we do run into to a residual stress problem that we want to look at further, we take our material properties down to S-Cubed. They can run calculations that do the dynamics, and accounts for hydrofracture where the gas is allowed to flow out in some worst case scenario, like a single hydrofracture. How far is that going to go, for example. That's what we did on Barnwell, and they did calculate a fracture that went something like a hundred, or a hundred and fifty meters. That was a couple of cavity radii or so. Hydrofractures don't seem to go further than that, at least in the calculations.

Carothers: Norton, you've done a lot of calculational work on hydrofractures. Tell me something about hydrofracturing.

Rimer: I hope I'm telling you people at the CEP that there are limitations on what we know. Therefore we make assumptions, which we consider conservative, and that's a funny word to use, since we try to overestimate, and to do things in a direction to overestimate the length of a fracture. For example, we assume that the rock has no fracture toughness, no strength in tension at all. If it has strength in tension, the fracture will be slightly shorter. We assume we get one single fracture. If there are multiples, the driving pressure will go down faster, and the fractures will be shorter. We do the worst-case calculations, and if those are acceptable, if they give short fractures that aren't going to threaten things, we're very happy.

We don't know the actual details of a lot of this. For instance, we don't know how to calculate the initiation of a fracture; a fracture initiates at a point of weakness. How could we possibly know where in a cavity that's going to happen, especially after the cavity has expanded a factor of a hundred in volume, or forty in volume, depending on which shot we're talking about? We can't possibly know that, so we assume it initiates.

Another thing is that it is difficult to make clear what we mean by a hydrofracture. The classic hydrofracture is one where the gas breaks the rock, and pours out through that fracture. That's not what we believe happens. We believe there are preexisting, or shot formed, planes of weakness - - bedding planes, faults, all of which may be closed pre-shot - - which are the likely places where something will open, and the radiation may come out along those planes. It's not breaking new things, in general. I don't think, in a tamped event in tuff, that we've ever really seen a hydrofracture, in the classic definition of a hydrofracture. What we've seen are radioactive seams which we've encountered on mining for a new event, two cavity radii away. We've seen radiation. The Geiger counter registers something, otherwise, you wouldn't have noticed it. You look more closely, and you see gray, altered tuff, which looks like it encountered some steam. And, it's invariably on some bedding plane.

It's the steam in the cavity that's the fracturing gas, and that alters the tuff. There are other phenomena with steam, and we do consider them. With all the models we presume the fracture initiates, and presume only a single fracture. We can model multiple fractures, and we have done that successfully for the Junior Jade HE experiments. Then there are a lot of degrees of modeling that we employ in our fracture calculations.

The first thing we do is model a fracture where we assume cavity pressure is right at the tip of the fracture as it expands. And we only limit the speed at which it can grow by solid mechanics considerations; fractures cannot go faster than half the Raleigh speed - - half of the shear wave speed, roughly. We allow it to go into any zone in the code. That gives us the most likely direction for the hydrofracture. The next thing we do is take that direction, and we presume a single fracture goes along that path. We insist that only those cells that are along that path are allowed to fracture. Now what do we do as to how the material in the fracture behaves? We can assume it's steam, and allow it to condense, allow it to seep into the wall, allow heat conduction into the wall. Or, we can remove those assumptions, and make the fracture longer. We try all sorts of different assumptions, to see where it gets us.

We include all these assumptions, or we don't include them to give degrees of conservatism. And one of those assumptions is that steam is in the fracture, and either it can or it cannot condense. We

calculate the temperature of the gas. We have all that capability. Most recently Bob Nilson has put in a different approach to the fluid flow in the fracture. He's doing a finite difference approach now, which allows us to put in inertial effects. So, we're doing all those detailed models. We're not maybe having the nth degree of precision; for instance, we're modeling the steam as condensable and not based on temperature. We're not putting a good equation of state of steam in the code. We could, but why slow down the calculation?

Carothers: When you say that you get the direction that the fracture might go, what determines that? Do you put in an estimate of the in-situ stress field?

Rimer: Within the two-dimensional limitations of the code we put in in-situ stress fields. The vertical stress is rho*g*h; the weight of the material above it. We put that in exactly in all of our calculations, to the extent that the grid of the code is in equilibrium. If we run it a million cycles, without the bomb, nothing is going to move. That we had to do for the geophysics calculations, because they're very late time. For the horizontal stresses, the two stress components have to be equal, due to the two-dimensionality, otherwise you get horizontal motion. But they don't have to be equal to the vertical stress. And we've done calculations with those stresses equal to the minimum stress measured pre-shot in the ground.

Carothers: In the tunnels you have the opportunity to get in-situ stress measurements; directions, magnitudes, and so forth?

Rimer: Yes. It's an interesting phenomenon. It's really 3-D. One of the minimum stresses is the horizontal stress. The other principal stress, horizontally, is usually as large as the vertical stress, so we can't model that in the code, but it's conservative to model that one as a minimum also, because the lower the stress, particularly for a decoupled shot, the more likely a fracture path will exist. For a tamped shot those stresses don't mean diddly, because you get a good residual stress field.

Carothers: When the tip of the fracture is growing, what does the tip look like? Does it have a radius, or is it a mathematical point, or what? How do you put that in the code?

Rimer: The mathematicians who do this like to have it be a mathematical point. We allow it to propagate through a cell at a given speed. It's a simplification. The more important thing is, what is the pressure distribution of the gas along the fracture. If you're driving a fracture through a strong residual stress, cavity pressure may have to go all the way to the tip before you can open up the fracture further. In a decoupled event, we calculate a distribution of pressure, fluid pressure, along the fracture, and sometimes the tip is opened by tensile failure, the actual tensile stresses in the rocks surrounding the fracture at the tip. And, you may not have any gas at the tip, but you're still prying open the fracture. There are a number of analytical solutions, theoretical solutions, that Bob Nilson has tried FAST against, and we've run the full code against simplified cases to see if we match the mathematical solutions, and we do.

Carothers: You mentioned tensile failure. That would imply to me you were driving steam, or water into the fracture, and that the fracture was opening ahead of where the slug of water was.

Rimer: That's right. It's being pried open, particularly if the material around it has not failed. If it's elastic you have this strong rock just being pried open.

Smith: The calculators talk about the tip of the fracture being out in front of the water, and indeed we found that in our hydrofrac work in G tunnel. We would hydrofrac with dyed water, and then go back and chase the fractures, the dyed marks. We would sometimes find that the dye would quit, but there would be a fracture in front of it, and so indeed it looked as though pushing that water in was prying open the rock. There were sections out in front of the dyed fluid, the water, that had fractured before the water had gotten to it. Of course, the calculators were delighted when we found that phenomenology, because they think they had predicted it.

Rimer: If the material around the tip is plastic, then you don't have a lever action, so you can't pry anything open. It's the actual conditions in the rock that really matter. For some situations, the actual plastic failure is very important.

Carothers: If you're doing your calculations with a two dimensional code, isn't that a form of built-in conservatism? The world is really three dimensional, and so many effects vary as r-cubed, but you're taking account of them as r-squared.

Rimer: That's a very good point. I'd say yes and no. It's not that things go as r-cubed - - we have the spherical attenuation in the two dimensional code. The problem is the shape of the fracture. If the fracture is horizontal, the axis of symmetry of the 2-D code makes it a disc. That may or may not be bad. However, if the fracture is up at an angle, like we showed as the most likely path for Misty Echo, it makes the fracture be a cone, and that's not the same volume for the fracture.

The worst case may be if you just had a strip that fractured, like toward the Baneberry fault. We always felt that one of these days we were going to get back to Baneberry, and model it assuming the fracture is not as a complete disc or cone, but just as a little piece in a particular direction. That would deplete the cavity pressure less. The time when that fracture came out, which was minutes, may be very sensitive to the amount the fracture depletes the cavity pressure.

Ristvet: You've seen many a calculation presented at the CEP where the peak of the stress field was about one and a half cavity radii out beyond the cavity wall. Now we think it's out a little beyond two cavity radii with the damage models that have come into being. It was always comforting to see that two or three times cavity pressure, so you could say, "Ah, there's no way it can hydrofrac out of there." Well, there are some cases where the residual stress would probably be very small, so I've had a number of hydrofrac calculations done at S-Cubed. It turns out that it's very hard to hydrofrac even if you don't have any residual stress.

We used to model everything as one hydrofrac, and maybe the only time we ever have seen one single major hydrofrac out of a cavity was perhaps Baneberry where there was a very preferential pathway. There was a clay loaded fault, which I would not want to have passing through my cavity, especially one oriented such that the cavity grew up into it and didn't really displace it through radial shear. I think that would be a very scary situation, even if we don't create residual stress for all the other reasons that have been talked about.

Kunkel: We can begin to plot the frequency of fractures at some distance from work points by other bore holes we have drilled. In the valley testing areas we have lots of holes, and we very infrequently come across radiation from a previous shot in another hole. When we do it is always the object of much curiosity. Certainly we're not commonly getting fracturing at large distances away from our shots, large distances being half a depth of burial.

Carothers: Byron, on your reentries, aside from Red Hot, do you see physical evidence of hydrofractures? Have you come across something where you said, "Yes sir, that's a hydrofrac?"

Ristvet: Yes, but it's very rare. The ones we have found have been solitary ones, maybe two, typically along bedding planes or pre-existing faults, and they've extended to a couple of cavity radii. They may actually occur during the dynamic growth phase, when the material is in tension basically, and you can have radial shear.

Usually those are very interesting, because we don't see any glass. What we've always seen is altered tuff. It sort of looks like gray portland cement. We've taken tuff, and when you hit it with a steam torch, or even a regular torch, you get this gray powdery material. The zeolites want to go to feldspar, so you're creating these micro-crystaline feldspars, and so these seams are easy to spot. The USGS, back in the old TEP days of the sixties, when we were first getting into this underground thing, were looking at all this stuff. And I believe Gary Higgins did similar experiments.

Some of these seams don't have any radioactivity in them. Some of them have slight amounts, which are probably the daughter products of some of the early-time gaseous precursors that got out of there. We've never re-entered soon enough to know what the smoking gun really was, because all the lanthanum-barium stuff has decayed away, so you really don't know what gases were down there. You can only sort of guess.

Carothers: You make hydrofracing sound much less of a containment threat than some people have feared.

Ristvet: I think as long as you have a coupled event, where you don't start off with a big air-filled room, hydrofracing is not a serious threat. And we've never seen any evidence of hydrofrac around any of our low yield events in big cavities, in which the pressures are, after a few miliseconds, typically three to four times what the

pressures are in a tamped event. I think Mr. Hudson's measurements, and calculations, of those pressures are pretty close. Again, the calculations say we should have some short stubby fractures. We've mined right up to the cavities, mined right into them, and we had experiments on Minnie Jade to try to detect if they ever occurred and get a timing on them, and we never saw any.

We drilled back over Minnie Jade really specifically looking, because Minnie Jade was the first of those low yield cavity shots. The equilibrium pressure in those cavities is between five and six thousand psi, which is more than enough to highly overwhelm the tuff, and there's no residual stress whatsoever. Those cavities are steam filled. and why they didn't hydrofrac is difficult to say, because even the codes, as best as we can model things, say we should have some close to meter long hydrofracs.

Maybe we do have hydrofracs of a few centimeters. I suspect that is the mechanism, because our cavities have almost always cooled faster than the calculations done by S-Cubed, using simple decay models, predict . When we plug in the empirical kind of data, we can usually predict them doggone close. I think we do drive those higher pressure gases, at least partially, into the pores, and that's a pretty effective cooling mechanism, because the pore water is only seventy degrees Farenheit.

Smith: I did some hydrofac work in G tunnel, which evolved into airfrac. We were driving fractures with air, and again it was to look at the steam hydrofrac problem. We did it with air rather than steam, because then there is one less variable to play with.

But, G tunnel kind of trickled down because they ran into money problems, and there was also this new wave of the future with ES&H, and all the increasing regulations. It turned out that the air we had been breathing for years was not adequate. And the electrical facilities were old. They would have had to upgrade all those things, and the cost to do that would have been very, very high. To drill a new shaft for air ventilation was prohibitively expensive. A lot of those old tunnels were in pretty sad shape, so they were virtually abandoned.

It was costing about 1.2 million a year to keep that tunnel open, but there was other work in there which paid part of that. There was work for the waste disposal folks, and there was some interesting work on gas stimulation which was paid for by private

money from the Gas Research Institute of Chicago. That work was related to the things they do to hydrofrac gas-bearing formations. What they had in G tunnel was 1500 feet of overburden, where they could do the experiments, and then mine back into the areas and look at the results. So, they were able to test a lot of assumptions about stimulating wells with hydrofracs.

There was one experiment they did that was hydrofracing from the surface, 1500 feet above. They did the standard industry practice of colored sands, and walnut shells, and all the usual stuff. Then they started drilling holes, trying to find this fracture that was supposed to propagate five or six hundred feet. They eventually mined back and found out it had propagated no more than twenty or thirty feet from where it started. It got into a region of massive fractures and just stopped.

Carothers: As you know, people at S-Cubed have been doing calculational work on hydrofractures; how they're formed, how they propagate, and so on. Apparently they have come to the conclusion that such fractures don't propagate very far - - perhaps one or two cavity radii. Perhaps that's because you simply can't, in a sense, pump them enough. You can't keep delivering the necessary fluids and the necessary pressures to keep them going.

Smith: We discovered that experimentally. No way could we get big enough air compressors to drive those things. The harder you drive a fracture, the more the aperture opens up.

We did a whole series of shots prior to Misty Echo, called Junior Jade. That was a series of eight pound shots, where we varied the size of the air cavity around an eight pound charge. We were looking at what point do you begin to create fractures. If the shot is tightly coupled presumably it will set up the residual stress, and there won't be fractures. At some point, if the cavity is large enough, you won't set up any residual stress, and there will be fractures.

All told we did about five of these shots, and on the one that was tightly coupled, the cavity indeed grew, and we measured the cavity pressure. We also measured the volume of these cavities with a volumetric technique before and after the shot, and then we mined back into them. On the tightly coupled one, we ended up with a cavity which had grown to two or three times the original volume.

On the next step, with a larger initial cavity, fractures were driven out. The beauty of working with HE in this soft rock is that all the fractures are stained with the HE detonation products, which are basically carbon, and so the black fractures just stand out like gang busters. There were many fractures radiating out from this cavity, and then, out about ten feet one of the fractures turned. We knew from our old in-situ hydrofrac measurements that it went in the direction of the in-situ stress. The fracture always opens up against the minimum in-situ stress.

Also, the big, massive hydrofrac out of Red Hot, that goes over to the Deep Well cavity, is tilted over. On all the hydrofrac work we had done, the fractures were all vertical, and so I asked myself, "Why is that fracture tilted over? Surely, it's in-situ stress that controls that thing." Then we started doing some more hydrofrac work a little bit closer to the portal, and there all the fractures tilted over.

As you play with that, you discover that there is a topographic effect. As you move out from underneath the cap of the mesa, you're seeing the sloping surface of the front of the mesa. And, when you go around a bend the fracture also turns, and it's tilted. Both the azimuth and the inclination of the fracture is affected by the the topographic surface. When you get down underneath the cap of the mesa, all the fractures become vertical. So that answered that question.

So, when the fractures got far enough away from the cavities, they turned, because they're controlled by the in-situ stress. On an HE scale we were able to show that phenomenology of driving fractures, and actually look at them. With those five shots, going from fully tamped to decoupled, we could say that in-situ stress was controlling there. But, we still don't understand the answer to this: when the HE goes off, how does the shot know whether there's going to be an in-situ stress field and not be able to drive fractures, versus it's decoupled and can drive fractures? One thinks of the residual stress phenomena as something happening later on, and containing the fractures, but it looks like these fractures grew as part of the dynamic process, because the fractures grew, and the cavity didn't expand. All that pressure was lost out into the fractures.

Until that time we always thought of fractures leaving the cavities because there was no residual stress field in a partially decoupled shot. The fractures grew in response to the cavity pressure being higher than the residual stress. But it turns out that it's part of the dynamic process, right at the start. The calculations say that the residual stress field sets up when the material rebounds, and that's fairly late.

Almost invariably when we mined back we would not run into any fractures on a fully tamped, fully grouted shot. First you would start hitting softer material, and there was a very distinct boundary between this material and the rock that hadn't been altered, or damaged. You could tell it with a geology pick. Then you hit the cavity. Now, occasionally we would find a black-filled fracture. And occasionally on DNA shots they will run into a radioactive fracture, but it's not the common experience.

Carothers: It is only fairly recently that people have begun to say that while there is residual stress, it isn't necessarily as large as calculated, or as uniform, or doesn't last very long, and the basic mechanism is hydrofracturing which reduces the cavity pressure by absorbing a lot of energy.

Hudson: I can't argue with that. I think a much more believable scenario than the residual stress scenario is having high pressure fluid flowing out of the cavity in fractures. It probably happens all the time. If these fractures are generally distributed, let's say in all directions, then probably it's a good thing. The gas is just distributed evenly in all directions through a large volume, the pressure falls, and it doesn't get to the surface. That may be what happens every time you fire an event. On the other hand, every event may be different. On some events the gas may be bottled up, and they're the ones you should worry about. On other events it may escape quickly, and you shouldn't worry at all. So maybe the really big residual stress field is a bad thing to have, because it keeps things bottled up. We don't know.

Bass: Carl Smith did a bunch of shots in G tunnel called Junior Jade. He wanted to look at cracking out of the cavity. Joe LaComb sponsored it, and it was a very interesting bunch of work. It falls in with some of the Sandia work on how do you gas frac tuff, and things like that. And the answer is, of course, that you gas frac, or you fracture a well with a propellant, not with an explosive. You want

a slow burning propellant to do this work. Well, Junior Jade was very interesting in this respect because as he changed the size of the cavity you have no cracking, and then you have cracking.

It really threw a real mess into the hands of all the DNA calculational people, because they were not calculating cavity size right, or anything else. Calculating cavity size is almost impossible. You've got to have the right material model, you've got to have the right damage model, and nobody's got it.

Carothers: Dan, you can take cores and squash them, and so on, but that core isn't necessarily representative of a block the size of this room, or this building, which may have one or more fractures running through it. Therefore, while rebound is certainly real, it may be more faith than anything else when you say, "I ran some calculations, and I got a good residual stress field, so this shot is okay." So, there seems to be a body of opinion that an important mechanism for containment is that there are lots of fractures that grow while the cavity is growing and the material at the walls is stretching. They don't go very far, but there are a lot of them, and that dumps a lot of energy, so the cavity pressure goes down, and that's what really happens. What are your comments about that?

Patch: I don't think there's anywhere near sufficient volume or time available to get rid of a significant amount of the cavity gas, or the energy that's in the cavity that way. It's conceivable that in a decoupled cavity shot, or a partially decoupled cavity shot like Red Hot, fractures can have a significant influence on the cavity state, although I've always been a little bit bothered by that. I don't see any way, on the average tamped shot, that you can grow crack volumes that are significant fractions of the total cavity volume, so it's hard to see how they can influence the conditions in the cavity.

Carothers: Then my question is, "Why don't all shots vent?" Something has to stop fractures which could grow to the surface.

Patch: Yes, something has to do it. The cavity pressures are known, and measured, to be higher than the kind of pressures it takes to hydrofracture the media. We've done many hydrofracture tests in the tuffs, and the minimum fracture pressures are 300 to 700 psi - - they're not that big. Now the opposing school could say, "Well, that's okay, because there's a lot of molten rock around, and you're just plugging up those cracks with molten rock." So, there

are many facets to the argument, and they confuse, or add ammunition to either camp. I think there is plenty of evidence that the geostructure certainly perturbs the stress state locally, because we have data from the many reentries that DNA has done. And it's not unusual to come across a radioactive seam within roughly two cavity radii, or thereabouts.

Carothers: That's not a very long fracture.

Patch: No, it's not long. And the seams generally are not that hot, in the radioactive sense. You get some detectable amount of activity, but you don't get high readings. My impression is that they're not that frequent either; you don't run into a gigantic network, or a whole nest of these things. There will be one or two, or maybe three, on a reentry that are potentially bothersome when you get in close enough.

13

Block Motion

In the post-shot reentries that DNA has done in the tunnels it has been observed that large blocks of rock have moved and been displaced as a result of the shot. On the emplacement hole detonations there is no reentry other than post-shot drilling to recover samples for radiochemical analysis, so the fact or effect of such block motions is not known for those events. What effect such motions have on postulated containment mechanisms such as the residual stress field, or on such phenomena such as cavity growth or size, is a matter of conjecture. Before the device is detonated it is not possible to say which, if any, block might move, or how much it might move. The question, however, is an important one for persons designing a line-of-sight pipe with various closure mechanisms which are to protect the samples that are to be exposed. It is possible that motions of the rocks could damage the sample protection hardware, and cause the loss of much of the data and equipment that typically is used on the effects shots in the tnnels.

Carothers: One of the things people have seen on post-shot tunnel reentries in Rainier Mesa is block motion. Now, when people talk about block motion, are they talking about blocks the size of this building, or the size of this desk?

Orkild: It depends. A block can be a piece of rock between two cracks; two joints, or two faults. A crack is just an break. "Joint" is a generic term referring to how the crack was formed; a joint is generally formed by cooling, and normally by definition is a crack that has no motion on it. A fault has had movement. So, depending on the spacing of the joints and faults, blocks can have sizes from little cubes to the size of buildings. And, if you move one block you have to move the other blocks.

The Rainier unit itself, called Rainier Mesa tuff, is a series of blocks. Erosion has been going on long enough that the cooling joints have opened up, and those blocks are just sitting there, basically held together by gravity. When something happens, those blocks do move among themselves. As you go deeper into the Mesa,

I think the cracks are smaller, but you still have a series of blocks. And, as you go deeper, gravity is holding them together better and better, until your eye might not be able to detect them as blocks.

When you detonate a nuclear device, some of those blocks move around a little. This one might move a lot easier than that one, this other one might not move at all. We only know what we see in the reentry drifts, but we do see that. When you go back into the tunnel you can observe, and see that this block slid up over that block x-number of inches. Blocks do move, and you wonder why that bed down there stayed there, and this bed up here moved. Then you look and say, "Ah, here's a nice clay zone that this bed can slide on. It can move along that much easier than the one below can move along that gravel bed below it. That's much more difficult." So, blocks do move with respect to each other. We have seen up to a number of feet of motion.

Carothers: The picture I've gotten from what you've said is that we could look at Rainier Mesa as a large piece of material that has a lot of more or less vertical joints and faults, and a number of more or less horizontal layers, which were laid down at different times. And so, in a way it's a fairly loose pile of stuff, on a very big scale.

Orkild: That's correct, on a very large scale. Now, the Marshmallow site, in Area 16, was essentially completely shattered, broken, and cracked. When they mined into it, it was just sitting there as a mass of rocks, held there by gravity, and it was slowly creeping down the hill. Each time it got bumped, it jiggled a little bit and settled back again. The cracks readjusted, and the gases would seep out here and there. Many, many years from now Rainier Mesa will be like that - - essentially a pile of rubble. The blocks are getting smaller and smaller as time goes on.

Ristvet: Block motion is interesting to me is because I got involved with it when I was first at DNA. That was in relation to survivability of underground structures, from both a defensive and a strategic aspect. The big question was, at what stress levels do these motions occur? I said, "Well, it's really more of a displacement level than a stress level."

There's two kinds of block motions in a gross sense, and one has a subset. There's shock-induced block motion, where you're driving it with the displacements of the cavity. There's also shock-triggered block motions, and we've seen a little of that at the Test Site. The high yield shots that were done up on Pahute Mesa triggered a lot of aftershock activity, which results from built-in strains along the pre-existing tectonic discontinuities and faults. All we have ever seen in Rainier Mesa, in all the tunnel events, has been the shock-induced kind of motion.

Now, there are two types of shock-induced block motion. There are the motions that occur along already existing discontinuities, usually bedding planes with some sort of material along them that has very low shear strength. It's usually a very thin layer of montmorillonite clay, typically forty or fifty percent or so. Those motions are well documented. Typically they occur out to between two and three cavity radii. It's rare to see them out beyond that, but they have occurred out to as far as six cavity radii. But, those motions are very small.

We've also seen motion on faults. It's interesting because the faults move, if they're lubricated, but they also seem to be very affected by the in-situ stress field. At the Test Site the faults that strike northwest don't move, but the faults that strike northeast do move. Those happen to be oriented properly with respect to the minimum and maximum in-situ stresses, which are almost horizontal, and ninety degrees to each other at the Test Site. One is equal to the overburden, and the other is significantly less - - two, three, four hundred psi less, and that's because of the crustal extension going on.

The other kind of block motion is when you get in very close to the cavity, and I don't think this kind extends more than about half a cavity radius from the edge of the cavity. Again this is in the tuffs, in the tunnels, and only in the horizontal equitorial plane. This kind of motion stops very close-in, and that's not where the residual stress field is. You see lots of schlickensided faces - - shears - - and they are almost always either perfectly radial to the cavity, or perfectly tangential and they're quite frequent. This is from observations.

Carothers: How much motion do you see? A few inches, a few feet?

Ristvet: Anything from a few millimeters up to . . . probably some of the largest motions we have seen, which were on Diablo Hawk, were motions of thirteen or fourteen feet, on a bedding plane. There was also a fault that was in part related to that bedding plane motion which moved, and totally cut off the drift. There was about six and a half feet of horizontal motion, and two feet of vertical, and that essentially cut the drift off.

Carothers: It would seem that the likelyhood of getting such motion would depend on the way the drift was oriented in the stress field.

Ristvet: Very true.

Carothers: Do you pay any attention to that?

Ristvet: Well, yes and no. As far as siting an event, it doesn't seem to make a lot of difference. In the case of the group of experiments from Miner's Iron through Mighty Oak, it did make a difference because it really reduced the potential for the kinds of block motion that would help keep stemming in. It's interesting that on the events where we have had good block motion, where it's been oriented such that the residual stress field and the faults crossing the drifts would probably move, we've always had very good containment. And certainly Misty Rain was not oriented properly, even though we did see one very major block motion, which was along a pre-existing fault.

Carothers: If you think it's good, then it would seem you could turn the drift a little and have it the way you think would enhance this block motion.

Ristvet: Yes, we could, but if we did that we'd run out of real estate very quickly. It's a desirable secondary feature, I think. Of course, on Misty Rain, it was almost an undesirable feature. The only two faults in Misty Rain that were mapped, that crossed the drift, were the two that moved. And one caught the TAPS, which then didn't close. We modified the TAPS after that experience to give us more clearance, so if it ever happened again the door would probably come down and seal. What happened is, the shroud is very thin metal, whereas the rest is very thick. Now, the movement was very small. It was less than an inch, but it was enough to buckle the

metal, which caught the door, just barely. When we went in there, even though the door looked very secure, one did not want to go underneath it without putting a little bracing there.

Carothers: You also talk about residual stress, and it might be that if you do get such motion, it's going to inhibit or decrease the formation of the residual stress.

Ristvet: What it does is, it spreads it out over a bigger area, or a bigger volume. Consequently the peak is greatly degraded, and allows the relaxation to take place a lot faster, because you're having rock creep occuring along these planes as the residual stress is trying to set up. What I'm talking about is not new, and the modelers who work with the continuum models have been very aware that is probably what real life is like. We've just always felt it comforting when we thought these motions didn't degrade it as much as perhaps it does.

Bass: We have noticed these random motions; indeed, these disordered motions occur. There's no question about that, but I don't believe they're controling.

Carl Smith had a very interesting experience on one event. He and I put in a thing called a SCEMS - - a Self Contained Environmental Measurement System. Sandia has been doing them off and on for years and years. You put in this very strong unit, and then go back and recover it after the shot. And hope it has worked. Actually it has worked on some occasions. Right now it's a dead issue; it should never be fielded again. The last time it cost a quarter of a million dollars, and the data return was absolutely zero.

Carl did get some data on an event not too long ago. He had one of these units up at five kilobars, and that was the closest we thought we could go. In order to make the measurements Carl put some cables out from it, to gauges maybe twenty feet in front of it. We also put gauges in the body of the machine, so when those cables got broken we would still get something. I had designed these SCEMS in the past, and in an attempt to make it move with the surrounding rocks we put big fins around it to tie it to the mountain. That works, and they do tie it to the mountain. The accelerometers on-board and off-board did show the same thing. And when you integrate them they showed the same thing, within limits.

When Carl went back in, the guys who did the reentry were very careful about it and took a lot of good pictures, and you can see this chaotic motion of the type Russ Duff talks about. Here sits the SCEMS, and there sits the outboard gauge. Between the gauge and the SCEMS the cable does the damnedest didos you've ever seen. It's moved three feet this way, and two feet that way, and everything else. And the motion had cut the cable in various places. That rock does not just move radially out, in detail, but the general motion is outward.

Carl has looked at permanent displacements for eight or ten events, and put them all together, and has gotten a very nice curve out of it. Even up in the kilobar regime, and these would be up to five and eight kilobars, which is about as close as you can get back in and measure and have any accuracy, outward motion is absolutely a straight function. Inside there's terrific chaos, but that doesn't necessarily destroy the possibility of a stress cage.

Smith: There aren't any easy answers about block motion. The questions are all research problems.

We did field, about three shots ago, one of the so-called SCEMS units - - Self Contained whatever. You can't make the cables survive as close in as the gauges were, so you have this self contained recording unit. Then, you dig back, recover it, and read out the recording. There was about twenty feet separation between with the gauge and the recorder. And, there was a big fault that went through the space where they were separated. On the reentry we found that the fault had moved, but a foot this side of the hole with the cable in it there was another hole, and that hole was intact. That fault moved six or eight inches, and it was a massive fault that extended for numerous feet, but the movement didn't extend in one direction at all, because it didn't cut the other hole.

That makes you think, "Yes, these big fractures occur, and move at least six inches." But if you look at them on a global extent, they just don't extend anywhere. You've got all this massive block movement, but when you go and look at that fracture very carefully, and look at the other evidence, you discover that there are just numerous of these short fractures. Now, when you mine back and see what looks like massive block movement, it may be a whole

series of short fractures where each of them may have moved six or eight inches. But, I don't think those fractures extend for tens of feet. As I said, I think it's a research problem.

Bass: We also now have some data about when those blocks move. We had never had a timing of when blocks moved until Misty Echo. On Misty Echo I got a lucky break. I found a place to put instrumentation on a fault that Dean Townsend absolutely promised me would move. And, it was out at the tenth kilobar regime. So, being at a tenth kilobar I could get cables to last. I had three-axis accelerometers on each side of that fault, and we watched it move, and we know when it moved. And we know that it moved contemporaneously with the peak particle velocity. It moved right away. So, I think block motions are occuring during the peak of the particle velocity, which I think is a helpful thing. That's before the stress cage is formed. That's important.

For a long time people thought blocks or faults moved in seconds. But on Misty Echo they moved right at the peak particle velocity, and a funny thing happened to these blocks. They were sitting there, side by side. In radial motion outward, they moved together. In horizontal motion they moved together. In vertical motion they didn't. The one farthest from the device rose up over the other block, which went out and down. That lasted about for six hundred milliseconds, and then they moved off together. The bigger block behind became the controlling block, and started moving down. This is well documented.

The motion lasted a second, and we ended up saying it moved seven centimeters, that there should be a seven centimeter vertical displacement at that point. That was at one second. We said, "Okay, that's interesting. That should be interesting for seismic source mechanisms, and a few things like that." We asked Joe LaComb to go back in and verify this by reentry. He came back and said that there was no motion at all. What happened was that the shotcrete didn't break. I said, "Damn it, there was motion. Go back and look again." Joe listened to me, thank God, and he sent F&S back in again to knock the shotcrete off. I said it moved seven centimeters - - it had moved five. I think that's a fantastic bit of data, as to when it moved, and how much it moved.

The question about if there is all this motion, what does it do to the stress cage - - I think the answer is that the motion takes place before the stress cage is formed. The stress cage forms on rebound.

Carothers: You said the motion you measured took place during a period of like a second.

Bass: But that was way, way out. That was long, slow stuff. The blocks were still moving way, way away from the working point. But that's a good point. You've caught me in a problem there, but what we measured was a long way from the working point. And those blocks were moving together at that time.

Carothers: The stress cage sets up, if there is such a thing, presumably in less than a second. So, if all these blocks are going to do all this moving around before that stress field sets up, they have to do it in less than a second.

Bass: All the close in ones that affect containment. I think they are all pretty well calmed down by then.

Duff: On the reentry of Misty Rain they drove a tunnel between the initial line-of-sight tunnel and the work tunnel. It was roughly six meters to the side of the main tunnel. They observed nine faults, which were not recognized pre-shot, over a range of some twenty meters or so. They didn't get very close to the cavity boundary, but there were new sources of displacement even that far out.

Jenkins: In order to get a feel for the spacing of faults all you have to do is look at the outcrops surrounding Yucca Flat. You can see that the density of faulting is much greater than we show in the cross sections. I think that holds pretty well throughout the tuff units, especially the stronger ones, like those buried under the alluvium, for instance.

A number of very small movements along the faults would give the impression that the blocks are shifting. And they do, but on a scale that's difficult to illustrate. In other words, instead of making very tiny lines on the cross section, you put in the dip of the unit, and the boundary of what you think will be the major faults.

Carothers: So, if I want to talk about very small fractures, faults if you will, I will find them every couple of meters in the Test Site? That's typical of basin-range geology?

Jenkins: Yes, it is.

Carothers: It is hard for me to visualize what happens when a block of material the size of this building moves a foot or so. Where does the material go that used to be where the block moved to?

Jenkins: Well, along faults, especially rotational faults, you have a lot of problems with conservation of material. It's awfully hard to do. The material goes some place, and we never seem to know where that is. But, we can see the fact that the block has moved. It was here, and now it's down there. Or over there. It's terribly difficult to draw an accurate cross section because of this very fact. Whenever you start pulling the world apart, something goes wrong such that you lose part of the material that was in there.

Duff: We did a fairly careful job of trying to measure the displacement of an interface on Mighty Epic. This was an event where the Paleozoic rock was coming up underneath the working point, in one direction away from the line-of-sight tunnel, at right angles to it. The interface got within seventy to ninety meters of a horizontal tunnel that was perpendicular to the line-of-sight tunnel. A fairly elaborate experimental program was undertaken to try to measure the displacement of the interface that was predicted to occur.

The Paleozoic, being hard, strong rock would not move, the tuffs would move over the top of it, and one should see a sliding along this interface. Such sliding would represent a potential threat to underground deeply buried assets of one sort or another, such as a deeply buried command post, or missile silo, for example. So, they wanted to know, could it be predicted? This elaborate measurement program was undertaken, and indeed the expected displacement occurred. The only trouble was, it didn't occur at the interface we were looking at. It occurred at a weakness in the tuff, some distance above the interface. There was a weakness there that we hadn't known about. That is an example of a weakness that was exercised in a particularly dramatic way. Motions of a meter or two

occurred. We were able to find it after the shot, but we didn't find it before the shot. We went back and looked at pre-shot records, and cores, and we were unable to identify it.

I think that there are probably a very large number of other displacements that occur that we never recognize because we don't know what was there before the shot. We do relatively little looking close-in to an explosion. The Laboratories never, or almost never, do, and DNA is restricted in its efforts by money, and time, and difficulty, and all the other things that really do apply in the real world.

Carothers: You were talking about the world being inhomogeneous.

Duff: Intrinsically inhomogeneous.

Carothers: Let me offer a thought. The world is inhomogeneous on any scale that you care to use to look at it. If you want to start with a scale of a few thousand miles or so, there's space, and then there's atmosphere, and then there's dirt. If you want to go to an atomic scale, there is silicon, and carbon, and oxygen. On a somewhat larger scale there are molecules, then grains of minerals, and then you to get pebbles, and cobbles, and on and on. How that affects your predictions, it seems to me, is a question that can only be answered if you tell me the wavelength of the phenomena you're concerned with. Would you comment on that?

Duff: I think that's a very crucial point, and one that does indeed need discussion. I think the scale of the disturbance that we're concerned with in a nuclear test is, or can be, characterized by one of the characteristic dimensions of the test. Let's call that one the cavity radius.

Carothers: That would seem to be a reasonable dimension to choose.

Duff: Yes. Therefore, I think inhomogeneities that occur on scales that are of that order of magnitude can influence the phenomenology. And my point is that the modeling that we have done, largely that DNA has done, is based on measurements of pieces of rock core which are measured in centimeters. Whereas, we know from reentry observations that there are non-uniform motions that are occurring on dimensions of meters or tens of

meters. I think this points up a disconnect, an intellectual disconnect, between the phenomena we are concerned with and the data that we're using to try to describe it.

If we find that motions are dominated by what happens at faults, interfaces, bedding planes - - non-uniformities of one sort or another, as was pointed out by Livermore in the Rainier work in 1960 or so - - then we are remiss in basing our study of phenomenology on the response of homogeneous material, measured on the scale of centimeters.

Carothers: Dan, if you're going to think about loads on hardware, and plugs, and so on, what about the observed fact that large blocks of rock move? How do you take account of that?

Patch: I think dealing with block motion before the fact is almost an exercise in futility. The reason I say that is because you can predict, based on a number of rules of thumb, and empirical evidence, and some modeling too, kind of the region in which you would expect block motion and maybe make a guess as to what the amplitude is going to be. And you might be relatively close, if you're lucky. But you can't actually say, "This block is going to move. This one, not that one, and this is how far it's going to move." Our experience is that sometimes a very minor feature will move a lot, and a very major feature won't move at all. To figure out exactly how this is going to play out, pre-shot, is not in the cards.

Carothers: I believe that. Apparently there was block motion on Misty Rain, and it severed the pipe. Some people have said, "That was pretty lucky, because if that hadn't happened it might have behaved like Mighty Oak." Is that true?

Patch: I know there are a number of very smart people who believe that very strongly. And I don't. Part of my feeling on block motion is that it perforce comes relatively late. I wouldn't disagree with folks who say it gets started right away, but it's a cumulative thing, and it has to occur on time scales that are comparable to the cavity growth scales. So, you really get these substantial offsets late in the dynamic motion. I don't know any other way it can happen.

Carothers: Presumably large amounts of material are moving. If you're concerned with the survival of hardware, that postulated mechanism would be a concern, and something you would have to think about. What you do about it, I don't know.

Patch: We perhaps have been lucky, but the only instance I can think of where block motion apparently affected the closure was on Midas Myth, where there seemed to be some kind of offset motion that torqued the housing on the TAPS and kept the door from closing all the way. But by and large, because block motion is, let me say, pervasive, we've been fairly lucky in not having something go right through our pieces of hardware.

On the other hand, we have an amazing propensity on these low yield shots, purely by the luck of the draw, to put the FAC right behind a fault. On almost every one of those shots, maybe not the last couple, but certainly there was a string of about three or four at least, where there was a fairly major fault right in front of the FAC itself. Indeed, on one of them, I think Diamond Beach, it actually cut right through the nose of the FAC if you drew the plane. They have not threatened the survival of those closures; that is, the closures have all survived post-shot.

Now, such motions did cause a whole lot of unusual local motion in the stemming itself, in the vicinity of the FAC, on Diamond Beech in particular, where grout was extruded out and around it. There were some strange things that were difficult to figure out. So I guess I would say block motion hasn't seemed to pose a real threat to the closure hardware, but there are certainly cases where it has severed the tunnel, where there has been almost a full offset. It's made grout go strange places you wouldn't predict pre-shot very well.

Carothers: Here you are, scratching your head, and you're calculating, and you're doing the best job you can. And lurking somewhere over in that mountain is this big block, maybe. Or maybe not. Maybe it's going to move. Maybe not. Maybe it's going to move an inch, maybe it's going to move ten feet. What's it going to do to the hardware? Basically you have no mechanism to deal with that, or I can't imagine how you could.

Patch: I think the way we deal with that is probably pragmatically, and that is to say that we don't want a design that depends on the survival of any one feature. And so we're willing to take our chances. Generally when these blocks move it's the whole mountain that moves, and trying to resist it somehow by building an extra strong structure is, I think, not very likely to be successful.

Carothers: Personally, I have a hard time conceptuallizing this block motion. Presumably this block, which is the size of this room, or this building, moves. Something had to get out of the way.

Patch: You've put your finger on a problem that I have all the time. That's right; it doesn't have a void to move into. Simplistically, if you take a box of sugar cubes and start trying to move them around, if you start trying to grow a cavity in the middle of a box of sugar cubes, very strange things happen, unless they can deform in some way.

Peterson: Some people have stated that the reason we've had problems with some of the events, Mighty Oak being the worst, was the fact that we went to larger pipe tapers. They have postulated that once we went to the larger pipe tapers, the only reason we've had containment is because we've had very fortuitous block motion. That block motion has served to sever the LOS drift, and prevent things from leaking. Now, there's quite a bit of evidence on some shots that we have had block motion. For example, it looks as though block motion cut the drift on Misty Rain. People speculate that if it hadn't cut it quite as much as it did, Misty Rain would have looked like Mighty Oak.

There are clearly identifiable instances where a block of material has moved. Misty Rain is one example, and I think it's true on most of the events. It is documented on numbers of events. There was some on Mighty Oak as well, but you can then always argue that it wasn't enough.

Carothers: You could also argue that was what caused the problem.

Peterson: Well, that's the next point I was getting to. I can go to the other extreme of looking at, say, what is called the "tired mountain," which I think is maybe more properly said as shock conditioning occurs out to a larger radius then we can measure by going in and doing sonic measurements, or accoustic measurements,

or seismic measurements. Or, than we can determine from doing material properties tests. I think Russ Duff speculates it is because of this that one can say there is enhanced block motion. In other words, if you have more and more events in a place, you sort of jiggle the joints, and it allows them to slip easier, and that can enhance the block motion. If you enhance the block motion, then there's no reason for a residual stress field, as we think we get when we do our standard one or two dimensional calculations, to form.

Carothers: Wouldn't it depend on how big the blocks are?

Peterson: That's true. But you don't know that such a stress field forms anyway, and there's a possibility that it doesn't. And of course, if you don't develop the stresses so you really squeeze the tunnel shut, and form a stemming plug as we calculate, then of course you need the block motion to cut the tunnel.

Carothers: Well, I think there's unequivocal evidence, on a number of tunnel events, that the tunnel after the shot was smaller than it was to begin with. And it's not that it's been sheared, it's just smaller. That would seem to me to imply that there has been a considerable stress in those materials.

Peterson: Absolutely. I agree with what you say. What I was trying to say was that if we follow the block motion argument to some extreme, if you get much of it I think it could lower the stresses in certain regions. It might enhance them in certain other regions. If you happen to have an event in which you get block motion that lowers the stresses along the stemming column, then you may not set up a stemming plug. If you do not set up the stemming plug, and you still don't wish it to leak, then you better hope the block motion was enough so it severed the tunnel. Somehow you have to have something that stops the cavity gas from leaking out.

If you get block motion to the extent that you do not get good formation of a stemming plug, then you probably need the block motion in order to stop the leakage. It's a Catch 22, which is the point I was trying to make. If I follow the argument to the extreme, it's almost to the point that if you get significant block motion, then you probably need the block motion in order to prevent leakage.

Or, you could look at the other extreme - - if you don't get the block motion, then you probably set up a stress field similar to what we calculate, and then you don't need the block motion. Which of

these is right, or whether it's a combination of the two, and those are the ones that really get you in trouble, the ones that fall in the middle, I don't know.

Carothers: It's hard for the layman to imagine how very large blocks of rock, perhaps as large as this building, move around in the earth. If such a thing happens, then lots of other blocks must be moving too.

Peterson: Yes. I am not an expert on block motion, but DNA has a fairly large program that studies block motion for some of their work. They have done a lot of studies, and so it's a very well documented phenomenon. If you go back to the Rainier reports, one of the things discussed in those reports is that it wasn't just a uniform expansion of the material. There really were very large blocks of material that moved relative to one another.

Some of the data indicate that the motion comes somewhat late in terms of some of the time scales we talk about. It takes time for a very large block of material to move. We're not talking small things. They are very, very big pieces.

Carothers: Dimensions of hundreds of feet, possibly.

Peterson: Easily. So they don't move instantly. DNA has much information, and inside about two cavity radii it's very difficult to understand what's going on. One of the reasons is because things just don't move radially out. You can't count on everything to move radially out from the source.

Carothers: Or to put it another way, you cannot count on calculations based on the assumption that the earth is a homogeneous material. Which is what you do in one dimensional calculations.

Peterson: That's true. We can put in layers in some of our two dimensional calculations, but in general we don't know enough of what is there. We might know about one fault, and maybe we could put it in a calculation, but maybe there are others there that we don't know about, that are maybe just as important. So you really don't know how to put the structure in a calculation. It's difficult to do if you know it, and if you don't know it, it just gets that much more difficult.

I don't mean to imply by this that I believe it's either the block motion that's made the changes we have seen, or that it's the increase in pipe taper that's made those changes. I have found both arguments interesting, because the increased pipe taper one says, "You had to have block motion in order to get containment on the recent shots." The larger damage region argument says, "We're developing block motions because we were continually shaking the ground in the region where we do the shots." If you follow it to the next level, you can say, "If you have block motion, then you need block motion to get containment." But you could follow it back the other way and say, "If I don't have block motion, then things might work the way they always have sometime in the past."

Carothers: There is another set of detonations; those which occur in Yucca Flat. No line-of-sight, no tunnel. There's just the emplacement hole and its stemming. I don't understand how the block motion argument might apply to those shots. Does block motion occur only because the tunnel is there? Suppose there were no tunnel.

Peterson: I don't believe that the tunnel has anything at all to do with the block motion, or very, very little to do with it. I think it's the motion that occurs as a result of the natural discontinuities in the ground before the shot. I think the block motions generally occur independent of whether that little tunnel is or is not there. I don't think the tunnel causes block motion.

In Yucca Flat, when a device is detonated in the tuffs, I think blocks probably do move there also, but in a stemmed hole I don't believe it necessarily bothers you at all.

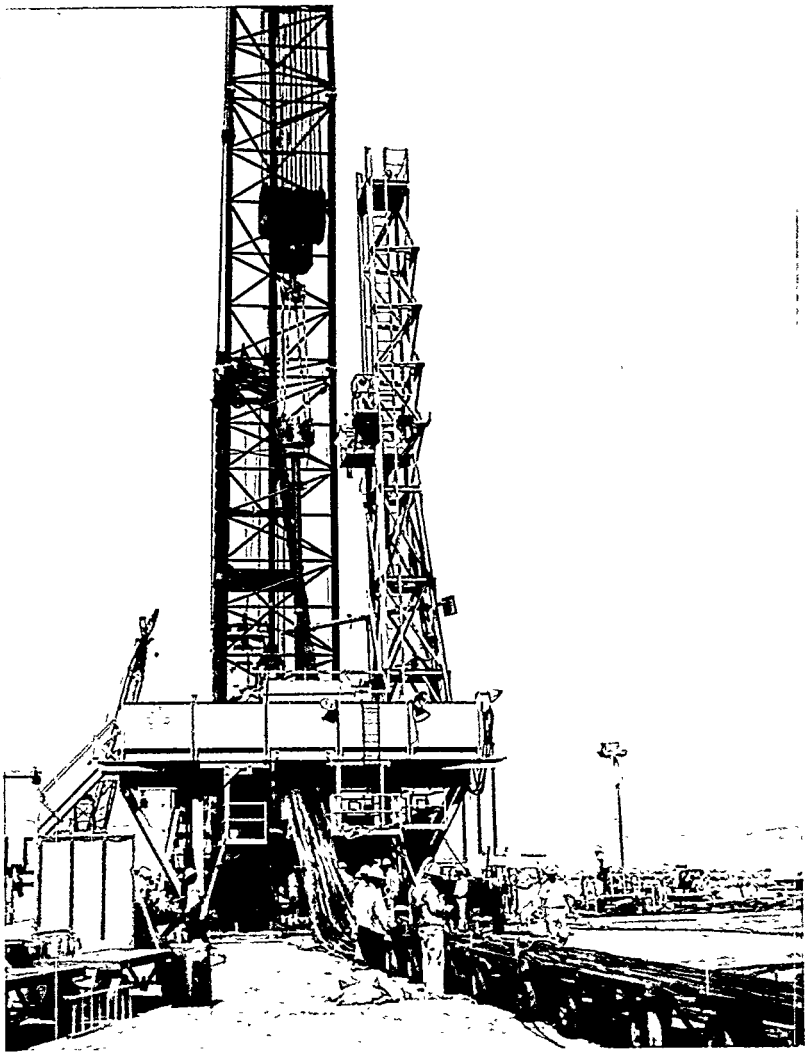
Carothers: Well, the evidence is that it doesn't. Of course, in emplacement holes all there is in the first few hundred feet is a bunch of gravel and a few plugs.

Peterson: Yes. And so they'll never see it, or it doesn't really matter to them at all. I believe it's something that we in containment need to think about, however. I personally don't know what the answer is.

Carothers: Let me disagree with you. The evidence in the Flat is that whether it occurs or doesn't occur is of no concern. The concern, really, is on the part of the DNA people who could to lose

their experiments and samples. It does not seem to be a containment concern, at least for stemmed emplacement holes that do not have a line-of-sight pipe.

Peterson: You are absolutely correct. Since I work for DNA, I think of close-in containment as being extremely important. In terms of release to the atmosphere, I don't think it is a containment issue at all.



Drill rig with stabbing tower on right side.

14

Depths of Burial, Drilling

Probably the most important factor in the containment of an underground nuclear detonation is the depth at which it is buried. It is fairly certain that a device of any yield detonated at the center of the earth would not release any activity to the surface. Conversly, a device of however small a yield, detonated on the surface, would obviously release radioactivity into the atmosphere. So, somewhere between these reducto ad absurdum limits there is a depth for a given yield which will surely prevent a release of radioactivity to the atmosphere. Given sufficient depth, and proper stemming of the necessary emplacement hole, all the considerations of cavity formation, residual stress cages, material properties, calculational models, geologic setting, and so forth become irrelevant.

Like most other statements of obvious, simple solutions to complex problems, the one above is essentially useless in the face of the real-life constraints that exist in dealing with the problem. The first and most immediate constraint is usually money, and in the preparations for an underground detonation how and where the device is placed determines a large fraction of what the eventual cost will be. Drilling six, eight, ten foot diameter holes is not an inexpensive activity, and the cost per foot of depth increases as the hole gets deeper.

As noted in the section on hydrology, the Test Site is one of the few place in the world where the water table is as deep as 500 meters, but devices with a yield above about sixty to seventy kilotons must be emplaced deeper than that. Below that depth the hole will fill with water. To keep that water away from the device and the equipment that is emplaced, the hole must be cased with a liner that will be water-tight; a costly procedure.

Expensive electrical cables that carry the firing signals to the device, the necessary power to the diagnostic equipment, and ones used to return the data from the detectors must run from the surface to the bottom of the hole. For these and other reasons there is a

substantial financial incentive to fire the device at the minimum depth, which will obviously depend on the yield, required for successful containment.

Higgins: Starting in about the year after Rainier, 1958, we started the Plowshare program. Plowshare, as it was then envisioned, was going to include a lot of things, like stimulation of gas wells, and excavation; the nonmilitary applications of nuclear explosives. The questions raised by those applications extended beyond just the cavity puddle and the radiochemical analysis of the samples from the explosion. They went into things like, "Well, how far do the fractures extend? Or there are any fractures?" We knew by then there were some. "Where is the heat, and how much of it is available to recover?" And, "Suppose that, instead of shooting the shot in tuff at the Test Site, we fired it in salt. Wouldn't all the steam stay in then? Salt is impermeable, plastic, and solid. Won't all the steam stay in the bubble and be ready to be recovered?"

So, starting in 1958, the Plowshare program put a lot of effort into trying to answer questions like that. They were important questions, and we didn't have answers for them. We began to be concerned about effects other than just the rad chem sampling. Being quite naive in some respects, one of the things we thought was that it would be a good idea to try a series of Rainier-like explosions. These would be a few kilotons at most, and they would be in a lot of different kinds of materials, to see in what way the properties of the medium influenced the effects that we observed.

These were to be pure science shots. We designed a set which included a shot in granite, a shot in as pure salt as we could find, a shot in some kind of carbonate rock, which at that time we called limestone. I believe that early on we also talked about a shot in basalt, as opposed to tuff, which really isn't much like any other rock in the world. However, it turns out that there really is a lot of tuff, so it's not as irrelevant as we thought at one time. Being mostly not earth scientists, we thought that the world really had a lot more granite, and salt, and sandstone than anything else. But it turns out that four-fifths or so of the world is basalt. Volcanics really are the commonest kind of rock, and the so the Test Site isn't an unusual geologic place in that sense.

The first one we proposed was Gnome, in salt, and it was carried out, in salt, near Carlsbad, New Mexico on December 10, 1961. Before Gnome was fired we had designed other shots; the granite shot, and the one in sandstone, and various others. Hard Hat was originally the medium-effects test in granite. The granite existed at NTS, and so why not do the shot there? So it got designed at about the same time that Gnome got designed.

Excavation was always part of the grand plan of Plowshare. Explosive excavation is not at all new. It was, in fact, the preferred method for excavation in swamps, and some other types of terrain, as early as the mid-nineteenth century. The French, particularly, did a lot of work developing high explosive excavation, and scaling laws, and theories having to do with explosive engineering. There was, and is, extensive literature on the subject, but it all dates from before 1900, and so a lot of modern engineers aren't familiar with it.

Carothers: Why wasn't there some material from after 1920, say?

Higgins: Well, technology developed, and the efficiency of modern machines superseded explosive excavation from an economic point of view. When the competitor was a team of mules and a scraper, after Nobel's development of dynamite explosives excavation was much cheaper. By 1900 that was about the end of it however, because engines and machines got to be very good. Now it's almost to a point where you can move hard rock with machines easier than you can blast it to break it.

Going back to 1955, there was a surface detonation called Teapot Ess. It was not part of the Plowshare program, but it was an underground explosion, deep enough so the fireball would be contained, but not the debris. As I recall it was buried at some tens of feet, and it was about a kiloton. The purpose of that was to understand the effects as a potential antitank weapon, and to confirm the old French scaling curves for producing craters. Would the scaling laws developed with dynamite work with a nuclear yield, or asking the question the other way around, was the nuclear energy as useful as the high explosive energy? There was quite a school of thought that said, "A nuclear kiloton really isn't as big as a thousand tons of TNT."

Well, the Teapot Ess explosion proved that the nuclear energy was as efficient. To the degree one can determine from measuring the size of the crater, it was just about as good as high explosives. The people in the Plowshare program, starting in a few years later, began to scale things and said, "All right, if a kiloton works as well as a thousand tons of TNT, then how about a megaton?" And they began to realize that things like a Panama Canal could be excavated with explosions in the megaton and submegaton range, placed at depths of 600 feet or so.

Carothers: I presume the original argument would be, "The chemical explosive produces a lot of gas, so there's a push, or pressure, from this gas which lifts and throws out material. The nuclear explosive doesn't do that, so it won't be as effective or as efficient in moving the dirt."

Higgins: That was the argument. That first test, the pre-Plowshare program test, was not definitive in that particular, but the crater was about the right size. The issue still was not settled, but it looked as though the vaporized rock did the same amount of work as if it had been a permanent gas. That was important from a containment point of view, because that meant the vaporized material contained a lot of the energy. There was a good mixing, at least until most of the energy was in the gaseous material, and there wasn't a lot of radiant energy left behind.

So, the Plowshare cratering program people proposed a series of shots, like Teapot ESS, at a number of depths to confirm the scaling curves, and to examine this business of the gas coupling at deeper depths. I think the scaled depth of Teapot ESS was about 60 feet. The optimum scaled depth of burst for cratering is about 120, and so Teapot ESS was at about half the optimum depth. The gas becomes more important as the detonation point gets deeper, and the argument was that as you approached a scaled depth of 100 or 120 for a nuclear source the gas acceleration phase, or the gas coupling, wouldn't be very effective. So, one of the objectives of the early Plowshare cratering program was to confirm the scaling curves.

First we confirmed that the old scaling curves that had been published by the French in 1870's were valid. And it turns out they're very precise, and they were valid for both TNT and nuclear explosives. When we used a thousand calorie per gram high

explosive like TNT, we got the same results that the French had. And we found that the effect of wet rock or dry sand was not all that pronounced. There was a little difference, but all these curves existed. By confirming one or two of them we found that we could use all of the curves.

The real issue was how far up in yield could you go, because it's obvious, if you think about it, that in a gravity field there is an upper limit to the size of crater you can make. If you tried to do half the world, it would obviously all fall back, because it's going to fall back into the same world. It might be oriented differently, but there's going to be no crater at all. What flies up one place will fall back somewhere else.

The largest explosion we did was the Sedan event on July 6, 1962. It was 100 kilotons or so, at a depth of about 630 feet. That was the optimum depth from the old scaling curves. Lo and behold! It scaled just as if it had been high explosives. It produced a 300 foot deep crater that was essentially 350 feet in radius, and was very close, or exactly on, the high explosive curves. That verified the scaling curves from 1 gram to 100 kilotons, which is 10 to the 8th grams.

The point, for containment, is that 100 kilotons at the optimum scaled depth of burst produced the right scaled dimensions for the crater. We also looked at the craters from the Pacific surface shots, and those large yields at the surface produced craters also of the right scaled dimensions. The inference was that when the explosive was contained, and it produced no crater, the same logic should apply. In other words, an explosion should be completely contained at the same scaled depth of burst, whether the explosion was a gram or 10 grams or 100 kilotons or even a megaton.

In the absence of gravity, in a perfectly elastic medium, the effects of energy at a point decreases as the radius cubed. But when you put gravity in, and say the explosion is going to be contained in this constant force field, things change. If you include gravity, the containment depth doesn't scale as the yield to the 1/3. Empirically it was found that it wasn't 1/3, but more like 1/3.4. The scaled containment depth, on that basis, was 220 feet. A couple of high explosive tests were fired at that depth. One was in

basalt, a hard dry rock that was thought to be representative of a portion of the hard ridge that separates the Atlantic and Pacific, and therefore was relevant to the Panama Canal issue.

The Sulky experiment was conducted at a depth which should have just barely produced a crater. And, it barely produced a crater. It did what it was supposed to do, at a little less than a half a kiloton. So, it appeared that the logic worked. What was missing was that for containment of high explosives, or the nuclear explosive, that doesn't include any of the gases. While there was no crater produced, for the 220 scaled depth essentially all of the gases went through cracks and came out into the atmosphere. None of the solid material did, but from today's containment of nuclear explosives point of view that would not be adequate. It would not adequate from the U.S. point of view, I should say. There's a difference between the Soviet view and the U.S. view on what containment is.

So, the 220 scaling law is useful only as saying, "Well, that limit we know is too shallow." It certainly establishes a lower limit to the depth of burst for containment, and in that sense it probably is useful. If you apply it to four megatons or so, as you would for Cannikin at Amchitka, that lower limit turns out to be a little over a 1,000 feet, instead of 6,000 feet. Well, there's a great deal of difference in cost between drilling a hole 1,000 feet deep and one 6,000 feet deep. That last mile, so to speak, really costs you.

Carothers: We were willing to go the extra mile.

Higgins: Yes, in spite of the evidence.

But those experiments established how shallow one might go and not release prompt debris. I don't think anyone would have tried it. But it does point out that as you go to larger and larger explosions, the price of complete containment, in the sense that we are now doing it, is quite high. I'm convinced, and I think others who have looked at it are, that we could, if we were ever do a megaton test again, bury it half as deep as we have done in the past, with complete safety.

Carothers: You've been an exponent of that for some time. I have a comment. As the yield goes up to a megaton, or even to ten kilotons, you're burying the explosive at a depth where I doubt there has ever been a large chemical explosion done. So, you're extrapolating these curves, and the implicit presumption is that the

earth in which you are doing this explosion is a homogeneous earth, so what happens near the surface is the same thing that will happen at depth when there are layers of different materials which have dips, and faults, and cracks.

Higgins: That is a complex issue, and there isn't a simple answer. The criticisms and the concerns early on in all of the underground and containment programs were that these fissures and faults and irregularities and uncertainties in the earth would really dominate the observed effects. In fact, as data began to accumulate, what was found was that the wavelength, or the size of the stress wave, and the size of these irregularities were different. Things as small as faults offsets, and voids, and changes of material properties apparently don't interact with the stress wave from the explosion because it is spread out more in space and time than they can involve. The stress wave just doesn't see them; it just wraps around the irregularities.

While it was a very real concern, the early data have been confirmed in a large variety of cases. Faults and fissures and irregularities become important only in very special circumstances. They can be important, but they have to be supplemented by other irregularities that make the stress wave itself, and the pressure field, irregular in such a way that they reinforce each other. I think Baneberry is probably the best example of a lot of such effects occurring simultaneously, and I think most people agree that kind of interaction was involved in the Baneberry failure. I don't think everybody agrees as to which of those things was most important.

Carothers: The importance of irregularities should vary depending on the yield. In other words, if I am on the scaling curves, burying something at the proper depth, it would seem that if I detonate a gram or so I might be greatly influenced by some irregularities in the medium. Now, the earth, like nuclear cross sections, doesn't scale — the earth is just there. If I want to detonate a gram, or ten grams, things as small as the particle size of the medium might be very important to me. If I want to detonate a megaton, particle size is probably completely insignificant. Put another way, they are very small compared to the wavelength of the stress wave.

Higgins: Exactly right. And it's one of the mistakes that can occur if you try to do scaled models of tests at the one gram scale. You have to be very careful to scale all of the particle sizes, and other features, along with the size of the explosive.

When we have a nuclear explosion the wavelengths from the explosion are in the hundred meter range, as far as the bulk of the growth is concerned. After all, the cavity grows from the size of the explosion, which is a meter or so, up to a hundred meters or so. Those things that are a lot smaller than a meter, or a hundred meters, aren't going to make a lot of difference. If you had a hundred meter sized hole, I think there's no doubt that the explosion would find it and go out. A one meter size hole, it's questionable. A tenth of a meter size hole is so small that its not going to make any difference. This is my opinion, and I think it's been shown in a couple of cases. It's not going to make any difference no matter what's in the hole, including nothing. We've done tests many times with ten centimeter size pipes. We worry about them because we worry how big is too big, but the evidence is that they don't make a lot of difference.

Carothers: Well, there are some people who might take exception to your statement. You said, "If you had a hole which was a tenth of a meter in diameter, it doesn't make any difference what's in that hole, including nothing." There was a period of time when you chemists drilled holes, not quite that small, but still much smaller than a meter, near events, and filled them with various things at various times, including drilling mud, nitromethane, and starch.

Some of those holes stayed open. Take Eel, for example. There were two small holes near the emplacement hole. One was filled with drilling mud. the other with nitromethane. The mud, the cables, and anything else that was in them blew out, and the cavity did its best push all the gas out them. How does that square with your statement that it doesn't matter what's in the hole?

Higgins: It does matter what's in it. I made an imprecise, and also unconsidered statement. You can contrive to keep a tenth of a meter hole open, but it takes some special efforts. To keep such a size hole open isn't easy.

Drilling

Regardless of the depth that is chosen as the appropriate one for a planned experiment, a hole must be drilled so the device and the associated experimental hardware can be emplaced. The drilling of the hole is not a containment issue in itself, but on more than one occasion what the drillers were able to do has modified the planned containment or experiment design. Information from the drilling processs, should it reach the containment scientist, can sometimes provide valuable insights as to the properties of the medium through which the hole has been drilled. The characteristics of the hole, such as its diameter and straightness constrain how the various data colection experiments can be designed.

In 1961, when the moratorium ended, Livermore did their first few shots in tunnels, with little success as far as containment was concerned. Los Alamos always used drill holes, and their experience was somewhat better. One of the concerns about the use of drill holes was that they weren't big enough to allow much in the way of diagnostic measurements. During the first few years the holes were 36 inches, or 48 inches in diameter. While large compared to holes that were drilled for things such as oil exploration and production, they were a very small diameter laboratory space in which to place the diagnostic equipment needed to collect data about the performance of the nuclear device.

Miller: By the time I got to the Test Site the common size holes were 36 or 48 inches, and they were doing them in one pass. In the very beginning they would drill a small hole, similar to what they used to do in the oil fields, then open them up with what we called a hole opener, or hole enlarger.

Carothers: The people who were trying to make the measurements always wanted a bigger hole — four, six, eight, ten feet in diameter. Who developed what you might call "big hole drilling?" Did we do that, or was that a commercial development?

Miller: The evolution came from people at the Test Site. The Laboratory would give the requirements to the then AEC, and they, of course, had drilling contractor. Holmes and Narver had the drilling before I came out to the Site. When I came to work out there

I worked as an engineer for Fenix and Sisson, and they did the design work for whatever was required, in conjunction with REECO. I think the answer, probably, depends on who you talk to.

I think that initially it was probably entirely the A&E, Fenix and Sisson, and it kind of evolved to more REECO doing it, mainly because of personalities. It would depend on who was given the job. We had some really fantastic people out there. One with F&S was named Art Hodge. I'm one of the few people on earth who could get along with him, because I wouldn't take off him. He was a mean one, but he was smarter then anybody I'd ever known. He was that type of guy. REECO had a guy by the name of Sim Crews, who was a petroleum engineer. Between the two of them, reluctantly sometimes, because F&S and REECo were always at each others throats, similar to Los Alamos and Livermore, is how these things developed. The prime mover, of course, was the Laboratories — give us a bigger hole, give it to us quicker and cheaper.

Carothers: Where did they go to get eight foot diameter drill bits? Nobody in the world used them, did they?

Miller: That's not really so. The mining industry used them a lot, for what they called raise drilling. They mine in straight, drill a hole down, in a drift, and then run a drill pipe in there. It's pretty simple to drill out a twelve and a quarter inch hole in a drift. Then they put a bit on the top, and drill up, and all the cuttings fall out into the drift. That is called raise drilling. Then they haul the cuttings out like they would in a regular mining operation. When you start at the surface you have to remove the surface stuff that you drill through, and that's the really difficult part. Raise drilling is just one thing they use big bits for.

Carothers: What people have said is, "Well, it was really at the Test Site where we developed big hole drilling. That had not been done before."

Miller: That's not so. There was a guy with Robin Bits, which sells cutters. Fantastic guy, an engineer. I heard him give a talk, and he quoted four different localities where they have drilled big holes, and how they progressed differently. There was the way we did it, there was a guy from Canada who drilled some big holes, and there was a guy in Wyoming, and somebody in Tennessee who did something with coal mines.

Carothers: Was this so they wouldn't have to mine a shaft?

Miller: That's what they were for. It was cheaper to drill than it was to mine a shaft with people. Everybody thinks that big holes were only for shooting nuclear bombs in, but in Chicago, for instance, they have a sewer system under the city, and they drilled big holes down into places to put machinery and pumps down. In fact, this guy with Robin, a lot of his experience came from the Chicago area. And, of course, most of the other experience has come from the mining industry.

So, it wasn't all that hard to get the tools if somebody wanted to go to a six foot hole instead of a four, or an eight instead of six. There were people in the business, and there was always somebody who wanted to make some money, of course. We didn't go from four foot to eight foot. It wasn't that drastic a jump. It went from normal size drilling in the oil fields — for instance, the biggest hole I was ever on before I came out to the Test Site was a 20 inch hole. Then here we went to 36 inch. Of course, you drill a 48 inch hole, and you put a 36 inch ID casing in. Then it went from a 48 to a 64, to a 72, to an 86, to a 96. It just went a little at a time.

The biggest bit at the Test Site was for a 142 inch hole we drilled, but we didn't drill that one on the Test Site. We bought the two bits for one of our programs, and the only time they were used was on the oil shale deal up at Piceance Creek, which was done by a private contractor. The waste disposal project down in Carlsbad used a 140 inch bit body that they extended out to 142 inch, which is pretty simple to do. You just make the outside cutters, the gate cutters, about an inch bigger on a radius, so you have a 142 inch gauge hole. Those were two holes. Livermore never did drill a 140 inch hole at the Site.

I believe LASL drilled about a 144 inch hole to 300 feet for some experiment about the time I came out here. But it was not a common size hole. Of course, we had the underreamers for a while, too.

Carothers: My recollection is that we never had a lot of luck with underreamers.

Miller: Oh, we did. It was difficult to do, but we did several underreamed holes. I think probably about the last one we did was an underream up in Area 2, and that reamer is still there. They never did get it out of the hole.

Carothers: I remember that. Fred Beane was the Test Director. He came and said that this thing was like an umbrella — you put it down when it is folded up, then it would open up. Then you'd turn it, and it would make a big hole down there. So, one day he came in to see me, and said, "We can't get it closed. Must be a rock in it, or something. Can't get it out of the hole" What really happened?

Miller: Well, it started off in a hole in Area 2. The requirement was for 40 feet of 144 inch diameter hole at the bottom. So, we drilled a nice 60 inch hole and set a complete string of 48 inch casing. We put that in, and ran the underreamer in. It ended up that there was a square hole at the bottom.

What happened was that 40 feet a is pretty long section to underream, and the underreamer got to oscillating. When it did, the arms, because it was pneumatic pressure that held them open, started doing their deal as they rotated, and it just amplified it as they went down. Each cut it made, it spiralled. I didn't know how I was going to explain that hole to people.

Those underreamers were very expensive. It took a big piston and a lot of air pressure to force those arms open, and we were using the drill pipe as a conduit to pressure those arms. We would drill the hole, and enough extra hole, which we called a rat hole, so the cuttings would just fall in there. We'd let them fall. If there got to be too many, we'd pull the underreamer out, go back in, and clean out the rat hole.

The one we lost was not because of a rock. I didn't know it until after we shot it off, but the guy who built it, another one of those exceptional engineers, who worked for an outfit in LA, and did a lot of things for us told me that the specs originally called for T-1 steel on the arms. Somebody in DOE saw the cost of it, and changed it to some other kind of steel that wasn't as strong. This was an underreamer with sixteen foot arms, and when those arms opened, they bent. If it had been stronger steel it would probably have been all right. The arms folded up into a grove, but they bent a little bit. So, they wouldn't go back into the grooves, and that is what happened.

Carothers: I remember Fred Beane coming in and saying, "Can't get the underreamer out. Can't get it closed. Schedule, and all this, and all that." And I remember saying, "Well, shoot it off."

Miller: One of the hardest things I ever had to do was shoot that off, but I shot it off. That was a half a million dollar tool. That was terrible. I've made a lot of hairy decisions, but I'm the one who called Fred Beane, and then he probably called you. I said, "We're not going to get this thing out of here. You might as well make your head up to that. He said, "Well, what are you going to do?" I said, "There's only two things to do, and that's abandon the hole, or, shoot the damn thing off." And it was cheaper to shoot it off than to lose the hole.

Carothers: Well, the cost of what's done for containment is something people in containment get hassled about every now and then — all those cable gas blocks, all those logs that you have to run in the hole, all this, all that. Then people eventually come to their senses, and say, "Well, compared to the cost of the hole, all those things don't cost very much." If you say, "I've got to have the hole," then adding the cost of the rest of this stuff is no big deal.

Miller: The fact is that all the things we did in the holes for containment didn't amount to all that much cost.

Of course, the straight and plumb hole requirement was really a challenge for us, but that had nothing to do with containment; that had to do with diagnostics.

Carothers: Yes. Once upon a time we wanted to do some measurements where the emplacement pipe was to be straight, and just hang down in the hole like a plumbbob. So, we said we wanted a straight hole, and you gave us a straight hole. You said, "That hole is so straight you can look from the top down to the bottom, and you'll find that the bottom is only off about two inches from the top." We said, "Yeah, but our pipe won't hang in that hole, because it's slanted." And then somebody, probably you, said, "Oh, you want a plumb hole. Why didn't you say so. You just said straight."

Miller: Yeah. I remember that hole. It was U2v. In fact, I was working for Fenix and Sisson when that requirement came for that first straight hole. You said straight, and assumed plumb.

Carothers: Well, of course.

Miller: We drilled a twenty-two hundred foot hole that had a seven foot displacement at the bottom, and it was a line-of-sight. So, "What's your problem?" "Well, we meant straight down." I said, "Well, that's a different story." Anyway, we got so we could do that.

Another thing that happened with drilling, and it happened on that event that leaked - - Riola. We drilled into that thing to take pictures of the old emplacement hole, and missed it the first time. And that brought up something people ought to know, and we know it at the Test Site now.

Carothers: It was only a couple of hundred feet down.

Miller: It was more than that, but you're right, it wasn't very far down. Anyway, we missed this eight foot target down there. We did everything we could think of to do it right. Here was everybody out there, including the containment people who wanted to take a picture of what was down there, and we missed it.

Then I did something that I very seldom ever did. I'd been up damn near four days straight, and I said, "To hell with it." The whipstocker, the directional drilling engineer - - Robert Thompson had the contract out there - - said, "We hit it." I said, "You couldn't have. It's an empty hole." He said, "Well, you've got to believe your figures." I said, "Not if they're wrong." Anyway, he got mad. He'd been up a long time too. We had a little screaming match, and I told the driller to pick up.

Then I said, "I want to set that Dyna Drill at 160 degrees left, and we're going to drill until we hit that thing." So, the directional drilling engineer got mad and said, "To hell with you. I'm leaving." I said, "Bye." Anyway, I picked it up and started drilling. I could do the directional drilling work myself.

The partner of the guy who got mad at me came out and said, "What are you doing?" I told him what I was doing. He said, "You tell me exactly what you want to do, and I'll do it for you. You don't have to do it. You're paying me to do it." I said, "Fine." So I picked up and he drilled it, and I was sitting there looking at that weight indicator. The whipstocker was in telling a guy in the doghouse, "We'll be here until Christmas, and we won't get that thing." It wasn't thirty seconds later it fell in. Hit it dead center. I thought I was basing this on knowledge, but it was just pure unadulterated luck. Well, you've got to depend on something, sometimes.

What people should remember is that the reason we missed it was because we were using the magnetic declination of sixteen degrees. We'd been using that for a long time, and we had known we had missed some targets before. But, we never had the type of surveying we had on this one. Usually it wasn't all that accurate. Anyway, we got to checking back, and I called H&N that night and said, "What is the declination we should be using?" "Sixteen degrees." And I said, "Yeah." Well, you go back to when the NTS first started, and all the charts out there, all the quads, say sixteen degrees east declination, but in real fine print it says, "Varying easterly three minutes per year."

If you stop and figure it out, over those years it had changed a degree and a half, and everybody was still using sixteen degrees. Three minutes a year is nothing. But over twenty years, you ended up with a degree. Anyway, based on that we started taking a magnetic declination at each location. And it actually varied a little bit across the Test Site. That shows how you can get in a rut.

Carothers: When you first came to the Site both Livermore and Los Alamos were using holes that were cased all the way down. How do you do that?

Miller: Well, this is similar to the oil fields. The only difference is that in the oil fields the pipes screw together, just like the pipe you use to emplace the device. Same kind of pipe. It screws together, and it goes pretty fast. When you get to the bigger diameters, you have to weld each section together. Those sections are 30 to 40 feet long. The string is supported by a strongback, and the next joint is picked up by what we call elevators, and put in and welded. Then you pick the whole thing up and lower it down so you can put on the next joint, and so on until you finally get to the bottom. It took a lot of time, and my time, which I was paid for, but I thought it was ridiculous.

Now, when you get to holes that go below the water table you have to do that if you want a dry hole. You do the same thing of welding a string of casing together. Of course, the bottom piece has a plate welded across the bottom, and so as the string goes down the water supports it to some extent. Actually, you put water in it to get it down, but after it's cemented in you bail that out.

Carothers: After a few years somebody at Livermore said, "We don't need to case them." How did you feel about that? Did you think that made any sense?

Miller: I can remember when that happened, and I thought that was great. I thought we wasted so much money out there it was sickening to me. And I still believe that. Not casing the holes saved the Test Program so much money.

Carothers: Many millions. But the argument always was that you had to case them, because otherwise you might be lowering a device downhole, and the sides of the hole might slough in, or something like that. Did you believe that?

Miller: Oh yeah.

Carothers: Well, then why in the world did you think we should not case them?

Miller: Well, as I remember it, there was a lot of discussion that went on about it. I didn't really think cave-ins were going to happen, because we could repair the hole when we were drillling it, and we did. A bunch of them caved in ahead of time, and we repaired them. The ones that were really bad we cased. But, holes do slough. Fortunately, one never has yet with the device in there. But I'll tell you, being out there on a downhole and hearing rocks falling in is a little discouraging. It happened all the time.

Carothers: How did people get up the nerve to try it?

Miller: I don't know who originally did, but I think it was not people, but a person — Charley Williams. He'd just become Test Director. I was still working for Fenix and Sisson, and Walt Johnson called me up and said, "How about an uncased hole? Do you think it'll stay open?" I said, "It depends on where you put it." I was all for it. Casing was a time consuming experience.

I'm not sure whether the first one was 10d or 10w. In fact, I think the first uncased hole, and the hole with the first device put down on a pipe, was the same hole, on the Test Site. I think that at Hattiesburg they might have emplaced the device with a drilling rig, and Rex was another one they did.

Carothers: Well, uncased holes have been used successfully many, many times.

Miller: Yes. But a lot of those holes sloughed ahead of time, and we'd repair them by cementing up the sloughing zone and drilling back. You would never have used them without doing that

Carothers: It was a long time before Los Alamos started to use uncased holes.

Miller: Oh yes. They were dead set against it, and I never understood it, especially on Pahute Mesa where you have essentially competent ground. There are very few caving zones on Pahute Mesa. Some, but not like Yucca Flats.

Scolman: Our going to uncased holes was largely based on Livermore's success in shooting in uncased holes. It saved a lot of money, but our field engineering group was dragged into that particular regime kicking and screaming. For a long time the argument was, "Well, Area 3 alluvium is not like Area 9 or Area 2 alluvium."

Carothers: "It's loose, it's unconsolidated, and it's going to fall in."

Scolman: Yes, and there's something to be said for that. It is indeed different. But it turns out that yes, you can drill it, and the hole will stand. On the other hand, if you will look at many of the so-called Los Alamos uncased holes, they're uncased for a pretty small fraction of their total depth. We tend to run an intermediate casing, as we call it, in many cases through the alluvial layer, all the way, and then we case when we get below the water table. So there's a relatively short section that is uncased.

Carothers: Certainly holes do slough. A hole is drilled to some total depth, and when checked sometime later it's ten or twenty meters shallower. I don't think anybody knows whether that material fell in pebble by pebble or as a hunk of stuff. That would make a difference if you were half way downhole when it decided to slough.

Scolman: Test Directors worry about such things. You're probably aware of one that we had slough immediately after drilling, which came all the way to the surface. Luckily, it did not go up the hole, and so the surface depression was actually to one side of the drill rig. It did slough all the way to the surface. It was in Area 4, and it was within the last five years.

Miller: There were two of them that did that. The first one was an uncased hole that was drilled to like a thousand feet. They were getting ready to use it, and went over there, and there's a doggone collapse crater. There's the emplacement hole, and right next to it is the collapsed area. The thing caved in, all the way to the surface.

The one they don't like to talk about is the one that occurred with the drilling rig on it. Everybody tried to keep that quiet, because if certain safety people heard about it, who knows what they would have done. What happened was it collapsed underneath the rig, under part of the sub-base, while they were drilling. They hauled trucks in there with gravel; several truckloads; I never did find out how many. They filled it back up, and gently moved the rig off, and abandoned the hole.

The result of that was a meeting just between the drillers; there wasn't anybody else involved in it. I was in some of the meetings. What can we do about it? And I won't mention any names, but one LASL guy said that they were thinking about putting an expanded metal mat all over the location, so if it happened again the roughnecks wouldn't fall in it.

Then Fred Huckabee, who is an old driller - - he used to be a tool-pusher on one of our post-shot rigs - - he looked at me, and sort of made a face, and he said, "I'll tell you what. I used to roughneck, Miller used to roughneck, and I think he feels the same way. If my driller brought me out to a rig and it had this expanded metal all over everything I'd have to ask him what it was for. And he'd tell me, 'In case the ground opens up, that's to keep you from falling in it.'" He says, "I wouldn't have worked another minute for that driller. I would have left." And Huckabee really got mad. He was serious, and he said, "We don't want to start any crap like that, because that tells you that it's unsafe to do what you're doing. You're putting a safety net like for somebody from the Circus Circus - - in case he misses his grip he's going to fall in the safety net. You don't want to do that with a drilling rig."

So there were two events where that happened in the LASL area, and after those things happened, if they had an emplacement hole, and had a shot nearby, they would fill the thing all the way back up with stemming material. Shoot the shot, and go back and de-stem the hole. Suck the stuff out. Like re-drilling it, essentially.

Carothers: Didn't they do it with something like a big vacuum cleaner?

Miller: Yeah, but it takes a drilling rig. It was a design by this guy Art Hodge, for the Snubber event LASL had, where they were going to de-stem this sand stemming in the shaft and reenter it. We used it in Area 7 during the accelerated program when the stemming slumped and tore the cables loose. We went out there and worked all night, and used the same string to de-stem it so they could get down to repair the cables. So, that's the reason LASL did that. They didn't want to lose any more holes. They figured the one that occurred without the drilling rig on it was caused by a nearby shot.

Carothers: This doesn't have to do with drilling, but I'll bet you were involved in it. There were a couple of occasions where we had cable breaks downhole, and we built cages and put people downhole to fix them. Do you recall those?

Miller: Oh yeah. I guess about the worst one was Jorum. It was uncased, but they didn't have to put people down on that one. On Jorum, all the device and diagnostics was in like a submarine, because the shot was in an uncased hole below the water table. The stemming material from the device up to the top of the water was these real beautiful, round beach pebbles - - rounded so they wouldn't abrade the cables. But, they tore the cables loose, and broke the tape and the kellum grips anyway. They saw that with the TV. That was the first time I learned what a tremmi pipe was. We ran a string of pipe in, to the water table, and did the rest of the stemming into the water through that pipe.

I went down one hole, on Flax. Tubing fell in on that one, and it was an uncased hole. They had pre-run it to put in some CTE plugs. The stemming loads pulled one of the strings of tubing out of the bracket, and it made a God-awful mess down there. The top of that fish was about eight feet below the conductor pipe, and it was parted in two more places down below. We designed a fishing tool to go in and grab the fish that was across the hole, and an arm that would go out and grab the other one.

For the top one they sent Joe Dehart and I down. All I had to do was to latch these elevators on to the pipe, and it was sticking straight up, but it took three days to write the safety notes to send us down there. On the safety note it said, "Under no circumstances

will people be lowered below the conductor pipe." I read that and said, "Can't do it. The top of the fish is eight foot below the conductor pipe." "Well, we know that, but we won't get this approved unless we say that." When I said, "Well, I don't understand," they said, "Well, that's just to satisfy all the safety people, and the powers that be." Everybody involved in it knew we had to do it.

So, we went down below the surface conductor, and I latched onto that fish. We had sound powered phones to the surface, and Joe Dehart, who was a big ironworker superintendent, said, "Hold on there. Take off those phones." So we took the phones off. He said, "You see down there?" And I said, "Yeah, it's about sixteen hundred feet to the stemming." He said, "It took three days to write this damn safety order." I said, "So? What about it?" He said, "I'll send one of my ironworkers up in a bosuns chair on the jib of a 4600 crane a hundred feet, and I don't have to have a safety order. If he falls out of it and hits the ground, what's going to happen to him?" I said, "He dies, probably." And he said, "What happens to us if we fall out of here and fall sixteen hundred feet?" I said, "We die." He said, "What's the difference?" I said, "That's easy, Joe. They can produce your ironworker's body. It's going to be difficult to get our bodies. That's the only difference. The only difference." He said, "Put your phones on. Let's go up."

Carothers: As I remember, there was a man who fell into one of the holes up on Pahute, all the way.

Miller: Only to the water table.

Carothers: Well, that's a pretty high dive.

Miller: That's the only person I've ever known to fall into an emplacement hole. A laborer fell into a rat hole where we had put part of the drilling gear in, and it got stuck. They just lowered a rope and pulled him out. I think he was down about twenty feet. Scared the dickens out of him.

Carothers: People have said, "Well, we'd never do Baneberry again. We won't do that. The drilling history all by itself would alert us." I remember that there was lot of work and cementing and drilling and trying to get that hole down to depth. Could you tell me what went on there?

Miller: That was U8d. Well, up there in that area there is a clay zone, apparently. The geologists tell me that when water, which is the fluid we use, wets it, it starts caving in. For a month or so - - maybe not that long, but it seemed like a long time - - we would drill a little bit, and it would fall in. And we'd go and put a cement plug in, the worst way you could put a cement plug. You'd like the hole to cave in cleanly, and then go and cement through the zone from the bottom up. You can get an excellent job that way. But when you can't get it cleaned out, you have to get a little bit going from the top down, and I don't know how many times we did it, but several times. What they finally did was, I think, they raised the working point on account of our difficulty in drilling.

Carothers: We raised it forty feet. I'm the one who did that. My Test Director, Fred Beane, would come in and say, "Well, they had another collapse. But, they cemented it up, and they're going to drill it out." The next day it was, "Well, it fell in again." And it went on and on. Finally I said, "Fred, how deep is that hole now?" He said however deep it was, and I said, "You know, that's deep enough. That meets the overburden criterion. It's not what we said we wanted, but it's good enough. If you quit messing around with that hole, do you think we could use it?" He said, "Well, I think so." So I sent out a TWX, and we took out just one joint of pipe. That's where the forty feet came from. I've always wondered if we'd had that extra forty feet if it would have held just a little bit longer, and maybe it wouldn't have come out. I don't know.

Miller: Let me tell you something else that happened there that I never will forget. During that process I used to go to Livermore every Monday morning; they had regular Monday meetings during that time. After the decision was made to raise the working point I was in Fred Beane's office. After I'd leave that meeting I'd go to his office, because I really worked for him, in a way. Ralph Chase and Fred Beane and I were sitting there, just talking, and Billy Hudson and Cliff Olsen came in there, and they were really upset about raising the working point. They said, "We're going to recommend against it, and we're going to put it in writing." Fred came about half out of his chair, and he said, "You go ahead, and I'll say 'NO' in writing." They turned red and walked down the hall. When Baneberry went up in the sky I kept thinking about that.

The fact is I recommended we abandon that hole sometime before all that. Not on account of I was afraid it was going to vent, but because of the drilling problems. It was costing a hell of a lot of money. It was terrible.

Carothers: Raising the working point wasn't one of the smartest thing I ever did, probably. But I was the AD for Test then, and somebody had to say what to do.

Miller: Well, it's your fault then, whatever you do.

Carothers: Yeah, that's right.

Miller: Well, it was my fault too, because I couldn't drill it deep enough. We could have got it deeper, but we wouldn't have got it shot before Christmas. A lot of the times that seemed like the controlling factor; it was getting to be too close to Christmas.

Carothers: We didn't want to have people down there over that time. They want to come home too.

Miller: Well, there were a lot of shots over the years that had happened the week before Christmas, and people forget that the post-shot drillers always worked through Christmas. Nobody ever thought about that.

Carothers: That's true. Was post-shot drilling your bailiwick too?

Miller: Yes.

Carothers: Now, in the early days we'd shoot the shot, and it would collapse, usually. If it did they'd bulldoze a road down into the crater, move a rig down to the bottom of the crater, and they'd drill straight down.

Miller: That was pre-Cambrian time. That was before me. I wouldn't have liked that.

Carothers: What's wrong with that?

Miller: Well, the worst drilling conditions a drilling engineer can dream up in his wildest nightmares exists down in a chimney. When you go back in from outside of the chimney, most of your drilling is essentially in undisturbed ground. You don't get to the chimney until you get to the chimney edge. And normally, fortunately, most times you have enough overburden pressure to help you pack the ground so it doesn't slough in. Not always, but

most of the time, you have very little trouble. But if you start at the top of the chimney and drill through the chimney all the way down, it's just horrible conditions. Back in those days I would not have done it. I would have quit. They didn't even use blowout preventers.

Carothers: What do you need those for?

Miller: Well, if you like to breath radioactive gas, I guess no reason. I've reviewed lots of histories of when they did things like that, and there were all kinds of problems. To investigate a chimney for a containment scientist would be no problem, because we'd probably do it six months or a year after the event. But doing post-shot drilling rapidly to get fast-time samples for the radiochemist is a different thing. I'm not talking about the drilling. The drilling problems are going to remain. I'm talking about the radiological problems.

Carothers: One of the things that interests people in the containment world is, what is the condition of the rocks in the chimney. They don't think about it in terms of drilling; they think about it in terms of shooting another shot pretty close by. You said that if you start to drill down from the top, you've got probably a lot of loose, broken rock. You lose circulation. I can understand that at the very top of the chimney, but as you get down a ways isn't that rock pretty well consolidated?

Miller: No. I don't think so. I don't have that much experience drilling in the chimney, so some of these things are what I believe. If it collapsed in one big plug, all at once, instantaneously, naturally you probably wouldn't have that much difference. But if it did the slow caving thing, until it finally built up to the surface, it would be different.

When they drilled back right after the shot, I don't know how you could have learned anything about the chimney, the way they pumped tremendous volumes of mud in the hole to try to get the cuttings away, and contain the radioactive gases. I don't see how a person could get any knowledge from any of those holes.

Carothers: People have told me that they have mined back in the tunnels, and that there was at least one time when they mined right through where the working point was, and you couldn't tell when you hit the chimney. It was just competent rock all the way through.

Miller: They have actually mined back to GZ. But you can tell. The one I did you could see. I went up there with Walt Nervik and Ken Oswald, because they wanted to get some radiochemical samples. They actually went up to the wall with a pick, and got the radioactive glass. You could tell where the cavity edge was, because this cavity had formed, and the rubble had come in there. There was a definite difference from one of the tunnel beds tuff into that rubble zone, at least on the one I saw. There was a difference.

Carothers: When you do a drillback, how do you know when you hit the chimney?

Miller: Well, there's several things. I always felt very comfortable out there on a post-shot drilling rig until we got close to the chimney, then I always was sure things were going to start happening. There were some of them where we would drill into the chimney edge, drill fifty feet, and get stuck. When we get to what we call the chimney rubble, it's rock that's being pressed together by the overburden, and when we put drilling fluid in there, things happen down there. Sometimes things pretty bad.

One morning in Area 20 we were drilling along, and we were into the chimney. About six o'clock in the morning I thought a truck had run into the trailer I was in. I ran outside, and everybody was running toward GZ, because they thought it had collapsed. Anyway, I started to go over there, and I couldn't see any dust. The driller said, "Don't go up there, come up here." We never did get the drill pipe out of there. We'd had an underground collapse and the pipe was stuck at the chimney edge, the theoretical chimney edge. We always figured the cavity radius had gone straight up, because we had no other thing to go on. That's where the pipe was stuck, and that's where we shot it off. That happened several times. Things happen down there in that chimney that don't happen before you get to it, and they're all bad for drillers.

Carothers: I have heard that one of the reasons they went away from drilling straight down, to the angle drilling, was that there was a shot which had not collapsed, and the geophones were quiet. So they moved a rig in, they were drilling, and all of a sudden the drill stem droped about sixty feet. Have you heard that story?

Miller: Yes. I know what hole it was, and I know the guy that was there. What happened was, there was no collapse, but they moved in two rigs forty feet from the GZ, one on each side. They really crammed the rigs in together in those days. They used two because usually one of them never got to total depth. Even down in the crater a lot of times they would use two rigs because it increased your chances for success.

Anyway, they set the surface casing at about eighty feet on both rigs, but one rig broke down. They drilled with the other one to, I'd say, thirty feet below the surface casing and the tools just fell in the hole. Well, everybody says, "It's fixing to collapse. It could collapse." So, everybody evacuated the rig. The rigs were still sitting there. It was Tiny Carroll who said, "I want volunteers to go in there and tear those rigs down." Now, who is going to tear a rig down except the roughnecks? There's nobody else qualified. So the roughnecks went in and tore the rigs down and hauled them out.

That's one of the reasons they went to angle rigs, but the main reason was that you can drill from the side and be in undisturbed rock most of the time. You stay out of the chimney, so it was quicker, easier, and had more chance of success. You didn't have to build the road down in the crater. And we could preset the surface casing and have that all done ahead of time, which you couldn't do in the crater. Angle drilling was just like a discovered America for post-shot drillers. It was that kind of a step forward.



Drill bit.



Drill rigs at the bottom of a subsidence crater, drilling to obtain samples for radiochemical analysis.

15

Emplacement Holes
Stemming, Plugs, And Cable Blocks

Let us suppose a location has been selected for an event. It is far enough from permanent installations such as roads and power substaions that the ground shock won't cause damage. In consultation with the USGS, information about the geologic setting is examined to insure there are no anomalous features which might compromise containment. A hole is drilled to a depth appropriate for the yield of the device, and logs are run to confirm that the formation is as expected. The device and the diagnostic instruments are lowered into the hole together with their attached cables. Since this is a simple event, there is no line-of-sight pipe extending from near the device part way or completely to the surface. In this idealized scenario none of the many things that can occur to make life difficult for the field people and the containment people have happened. Everything so far looks good.

Except . . . a hole perhaps eight feet in diameter and several hundred feet deep has been put into the geologic medium that appears to be well suited to contain the projected detonation and its radioactive by-products. And, some tons of metal and other materials have been placed at the bottom of the hole. There are perhaps a hundred or so cables that carry diagnostic data and firing signals running from the working point to the surface. Now the problem is to make the emplacement hole and cables no easier a path to the surface for gases than the the undisturbed medium. The hole has to be filled with something, and filled in such a way that the cables are not damaged or broken. Loss of data due to a broken diagnostic cable is not a trivial matter, but it can usually be tolerated, and perhaps the desired information obtained on a subsequent event. Loss of the cables that carry the firing signals to the device is quite another matter, and creates a very serious problem. And that has happened due to poor stemming methods and badly chosen materials.

The cables themselves, individually and collectively, are a problem, even if undamaged. It has been demonstrated many times that gas entering the broken and open end of a coaxial or multi-conductor cable can, under modest driving pressures, travel inside the outer insulating jacket of the cable for hundreds of feet. Cables are round, and bundling together a hundred or so round cylinders about an inch in diameter leaves many open channels for gas flow. Many of the small seepages that were reported on the events in the sixties occurred through the cables and cable bundles.

Finally, when the stemming material is emplaced, you want it to stay there after the shot has been fired. Where could it go? Into the cavity, of course, and it has happened that stemming material has fallen from the emplacement hole into a cavity that did not collapse for some time. Or, it could fall into the apical void that typically forms at the top of a collapse that does not reach the surface, leaving an open path for gas transport.

Faced with such problems in emplacement hole stemming, Los Alamos and Livermore have taken different approaches to solving them.

Carothers: Bob, how did the Los Alamos stemming plan evolve? You started with just pea gravel and cal-seal.

Brownlee: Well, remember we started out in 1957 only trying to cut the fallout down. We saw the efficacy of plugs, because if we put in a plug somewhere, that did a pretty doggone good job. One of the tests we did was to have just a plug half way down in the pipe — nothing else. Then we did one with a plug that sat just a little ways above the bomb. Same kind of plug, but it did a much better job. It really cut the stuff down. We said, "Well, that makes sense. If you hold it in, it's going to blow a bigger hole because it can't go up, and it will get rid of more energy right there in place. And that's a good idea." That's how we got started.

Then we came to '61, and we said, "All right, we want to put a plug down low." And we had discussions about the stuff that was coming out. "How is it coming out? How are we measuring it?" Well, we were not measuring it so we could distinguish between whether it was coming out through the stemming or whether it was coming out of the cables. We didn't really know. So we went with hand-held meters to a cable, and it was hot. Well, it was coming out of the cables, and it was coming out of the stemming too.

So we put some cal-seal on top of the hole. What if we put some cal-seal lower down, and kept the gas down lower? "Well, the gas is in the casing of the emplacement hole, and it doesn't matter where you stop it. Besides, you have the ground shock, which will just break the cal-seal loose from the side of the casing and the gas will come on up anyway." It wouldn't do that if the casing had a good, clean, dry wall, and wasn't all covered with rust. So we did some experiments, not at the Test Site, of pouring grout in iron casings that had not been cleaned out, and ones that were cleaned out. We did this all very slowly — when I tell the story it sounds more logical than it really was, but these questions kept being addressed.

We finally decided to try some fines, some finer grained gravel. Okay, some fines. What if you just gathered up some surface material and dumped it down there? Would that do a better job of holding the gas down? I think I came across the philosophy very early that the farther down the hole you can keep stuff, the better off you are. So, instead of putting cal-seal at the surface, let's put it down in the hole. Well, the moment we started talking about pouring cal-seal downhole, the J-6 engineers had massive hemorrhages. "That can't be done. Impossible. And besides the cables will have leaks, they'll get water in them, and we won't get any data."

Okay, let's get away from the cal-seal. Let's just put in some fines material as a plug, and see if that helps. Yes, it seems to. How many of these fines plugs do you have to put in? It all depends on the shot. Now, this business of "it all depends on the shot" means that you have to tell the engineers in the field to do something different each time, and we all know that they rarely have the mental capacity for that. Therefore, what we need to do is have a standard stemming plan. If the yield is big this thing works, and if the yield is small that thing works. And you can always pour a little cal-seal on top.

Then you can order your stemming by the foot, and they understand that. You just say, "However deep the hole, just start by putting in a fines layer, and every so many feet, put in another one." Then they don't have to think. You don't have to give them a magic formula for each shot, and they just have this one thing to do.

And then you go out and discover they're cheating ! They're not really putting in the layers at the places that you said they should. So you read them the riot act, and they say, "But why? It doesn't matter where they are." Well, that's sort of right, but you say, "We've got to know where they are anyway." And so, there evolved, finally, LASL Standard 5. We said, "Okay. You do it that way, and we'll watch to see that you do it that way. No more of this discussion of 'Why can't it be different? Why can't it be random?' You do it the way we said." We evolved to that, and it worked fine.

So, the reason why our Los Alamos Standard 5 stemming plan had such a perseverance was because we never happened to challenge it in a way that required us to make any change. And therefore it lasts to this day.

But, if you look very closely, you'll find that we have the standard plan, but you'll also find it's modified here and there. This thing has actually been moved a little bit up, and the spacing is a little different, for instance. If you look into it you'll discover that it's not quite as standard as everybody thinks.

I think I have to say that the LASL Standard 5 stemming plan was notably successful for our shots, which tended to be pretty much the same, in the sense that there was a time when we did relatively simple things. Livermore was doing exotic things, and so they never knew quite what was going to happen, but we always knew what was going to happen. The yield was not going to be more than this much, and we could be pretty sure of that. We did things that even if they failed, they didn't fail wrong, they failed safe. Therefore, our stemming plan handled the things we were doing adequately, and I think it's fair to say that is true.

As we got better the fines became different kinds of fines. The coarse became different kinds of coarse. But, as we got the ability to calculate these things, we discovered those fines were awfully good. No matter how they were shaken up with ground shock they still bonded as tightly to the casings as ever, or to whatever, because we did a lot of shots with casings. We discovered that by the time the gas had gotten around very many of those layers the pressure in the cavity had fallen, and it was all over. We had lots and lots of those layers, not just three or four.

There's one more thing that we did different from Livermore in the early times, which was all to our advantage. We allowed the stemming to breathe for a long time; we'd pour some stemming in and we'd let it sit while any trapped air came out, and pour some more stemming in. It was very slow. We did this as much for convenience as for understanding what was going to happen, I think.

The Livermore attitude was, "We're going to shoot tomorrow, we're behind schedule, and so we will just dump in all the sand." Livermore frequently worked behind the power curve. It's just that simple; they were behind, and you could always catch up all the time in the stemming process. Whereas, we had a schedule where, literally, we were usually ready a week or two early. So, you could take all the time you wanted. You stem, then you go down to the Steak House and have dinner, and come back tomorrow to stem some more. This allowed our stemming to solidify, and all the breathing was gone. On the other hand, Livermore started having collapses of stemming. The stemming would suddenly slump, and it would tear the cables off, and it got expensive. The only reason they changed is because it got expensive.

However, I felt that it was important to containment, and we started arguing that you needed to get the air out of the stemming; you've got to let those fines compact and you've got to let them settle. So, finally, the rate of stemming became a containment issue.

Hindsight says we had to stem slowly whether we knew it or not, because we didn't dare not let those fines take time. As a result, we never had any slumps of stemming. Finally, the argument was that the reason we stemmed slowly was so we wouldn't have slumps, but that's the engineers' argument. From the containment point of view we were arguing you are stemming slowly because it has something to do with containment, not just slumps. But a slump, if it broke the cables, was very expensive.

Then we did a shaft, and now we had a great huge opening; twenty feet by twenty feet. Now you have slumps no matter how slowly you stem; there's a bubble there, it works it's way up, and the stuff slumps. So, we got caught on one of the shafts where we had a slump which tore some cables. It turned out that we were stemming so slowly we could go down and repair it right there, so it wasn't very expensive. You just put people down there with their

soldering irons and their pliers. In a shaft you can get to it, but it's troublesome, because if you're stemming you've got the bomb down there.

Carothers: Was it as surprising to you as it was to me, Tom, that you could not pour sand down a rat hole, as it were?. And a very big rat hole.

Scolman: Yes, and I think it surprised the people who poured it down. We found out the hard way that it was, indeed, possible to bridge certainly a four foot diameter hole, and probably a hole of any diameter you want, and have the stemming fall in later. So the thing that started first off was, "Okay, what is it that we can really fill a hole with?" And so we came up with the requirements, for example, of dry material and material of a certain size.

The notion of alternating coarse and fine layers came before my time; it was in existence when I got there. My belief is that was done so one could say with confidence that the permeability of the stemming column was lower than the permeability of the surrounding medium. Remember we were mostly in cased holes in those days. I think the ability to emplace the material was as important a part of the criteria as the permeability. Whether that was so or not I don't know, because as I said, that was folklore that was there when I came.

Keller: When I came to Los Alamos in 1966 the only interesting events were the line-of-sight events. We barely considered the rest of them. Charles Brown used to talk a lot about the quality of the grout job behind the casing, but that was about the main concern for the normal emplacement hole event. The line-of-sight pipes involved the only challenging containment problems, as far as I saw them.

Carothers: The Nuclear Test Ban Treaty had been signed in 1963. A fair fraction of the events that were fired during the mid to late sixties, of whatever nature, released some amount of activity. Some of the amounts were pretty small, but maybe a quarter, or maybe a third of the events recorded some amount of leakage at the surface. Was that considered acceptable? Why wasn't Charles Brown worried about those?

Keller: Well, you look back now and it seems cavalier, but at the time, while any leak was disliked, the seepage of noble gases wasn't considered a major failure. The concerns were mainly that there would be so much flow up the line-of-sight pipe that you'd have a major fallout problem from the venting. The next level of concern was that you'd have enough radiation leakage from the event to fog the photographic film in the recording trailers. Below that, it was just an operational nuisance to have a leak. Hot cables were pretty common.

But there was, even before Baneberry, a deliberate attempt to limit the leakage to nothing. Then, as now, J-6 stemmed the holes. And the question was whether or not the stemming would work well enough. The LASL Standard 5 was the stemming plan for all the shots, and it had been developed early on. It was developed partly to avoid slumping; that's the reason the coarse materials are in there. The coarse material is terrible stemming, if you consider gas flow, but it doesn't slump and that was why they used so much of it. Then they put in the fines layers, in moderation, to get some impedance to gas flow.

The last event I worked on before Baneberry was Manzanas, and that was the first event where Los Alamos used coal-tar epoxy plugs. That was the hated, messy stuff that Livermore had dreamed up, and J-6, Rae Blossom and company, were not the least bit interested in being caught using a Livermore material. The whole idea was abhorrent. So, I ran into a lot of resistance in trying to design a stemming plan when I requested coal-tar epoxy plugs on Manzanas. And yet, it was pretty clear that the stemming plan for Manzanas would be better if it had some impermeable plugs in it instead of just coarse and fines. So, they finally relented, and it was used.

Carothers: Jack, Livermore and Los Alamos have always had different stemming plans. Do you know why that is so?

House: I guess I would have to sum it up by saying Livermore has been far more adventuresome in looking at different types of stemming material and stemming plans, and to some degree I think that is an artifact of Livermore having dedicated engineers, who are paid to go out and look for new and different, and perhaps better,

ways to do things. Los Alamos has never had the engineering resources to address questions of that nature, and be adventuresome. I hate to cry poor, but this is really, to some degree, the case.

When I joined the containment business, the stemming plan was pretty simple. It was the LASL 5, with alternating layers of coarse and fines, with the coarse lifts always being about four to five times longer than the fines material. They had begun using an additional type containment feature that was known as coal-tar epoxy, or CTE. It was awful stuff - - ultimately deemed unsuitable for use by humans. The plan was pretty much defined, and we used that basic plan with little, if any, alteration.

Now, that stemming plan works, and there is an element of, "If it aint broke don't fix it." The other thing about the LASL 5 basic stemming plan, which features the alternating layers of coarse and fines material, is that we are very fond of the apparent attenuation properties of the three meter thick lifts of fines that exist in our holes, as far as slowing the gas down as it tries to find its way toward the surface. Recently Livermore has chosen to use long lifts of sanded gypsum concrete, and that seems to work fine for them.

Carothers: As I recall, before Baneberry one of the things that was done about leaking cables was to go back in, cut the cables, stuff the ends into the surface casing, and pour cal-seal on them.

Olsen: That was SOP for a long time. We started using gas blocks not long before Baneberry; there were two or three events before Baneberry where we had gas blocks. Those were for multiconductor cables, which were pretty leaky. At the time coaxs also leaked. We were looking at how to gas block coaxs, which you could do, but you had to use bulkhead connectors, which the experimenters didn't like. We ended up manufacturing gas blocked cable, to avoid as much as possible putting something discrete in the line.

We were also looking at cable fanouts before Baneberry. The event which sort of tripped the whole thing on cable fanouts was Pod. We had some downhole motion-diagnostics, and part of the accelerometer and velocity gauge is a thermister to measure the temperature, because the damping is temperature sensitive. We saw, way up the hole, inside the cable bundle where this package was buried, that the temperature went up to that of steam, give or take a little. After a little thought it became obvious that we had nice conduits in the cable bundle, which let the gas go straight up.

Carothers: What was the origin of the plugs? Were they material to seal the cable bundle?

Olsen: The first plugs were on line-of-sight shots. We had some plugs on line-of-sight shots where the plug was more a matter of structure than containment. Usually those holes were cased, and the plugs were there, most commonly, to tie the pipe to the casing, to control the response to the ground motion. Los Alamos was doing a number of line-of-sight shots too, and they started to look into using plugs of some sort. I think they had a concrete plug on Finfoot.

Then, because of the diagnostics we had, we slowly began to realize these things were also stopping gas that was coming up the stemming. We were looking at various things in stemming columns, putting in radiation and pressure transducers. For stemming we put fines in, and coarse in, and sometimes NTS dirt. Sometimes in the sixties you would fill the hole with anything you could get a bucket loader into.

We tried cement, and on Plaid we had a polymer plug. That stuff, which was a sloppy, milky mixture, set up into a plug that was kind of the consistency of a tough gum eraser. I think it was probably one of the best plug materials we've ever had. We'd still be using it, but it was so damnably expensive, even then. But it was great, because it didn't fracture, and it was really tough. It wasn't structural per se, but it did tie things together, and it stopped flow around closures, which was the thing that we had in mind.

Carothers: There was a shot on Pahute Mesa where somebody, who shall be nameless, had a concrete plug poured, and somebody else forgot about exotherms, and so the cables got hot, softened and shorted out.

Olsen: Ah yes. On Greeley and Duryea both we had those problems.

Carothers: People learn slowly, don't they? One might wonder why it took two times to get people's attention. It's strange to look back on, and you wonder, "How could such a stupid mistake be made once, and how could it possibly be made twice?"

Olsen: I agree with you, but I think it was part of the lack of a detailed overview. There were people doing their own little thing, and it never occurred to the mechanical, or civil engineers that the

cables could have a problem. And it never occurred to the electrical engineers that these guys who had been dumping stemming into holes for years would come up with something to screw up the cables. And we did not have an overview that looked at these interactions. I'm not sure that we still have that to quite the extent that we should.

Carothers: Was that concrete plug for containment?

Olsen: The early things that went on, on Pahute, were kind of funny, because some of the stemming things that went on were done by engineers, who almost tried to second-guess containment. We didn't design it. Some event engineer would say, "Well, containment is probably going to want a plug, so I'll throw in a plug." So we'd see the plan, and there would be the plug. So, okay. Not knowing that it should be somewhere else, or whatever, we thought that was great, that we were finally getting some respect, and they were putting in a plug. On Pahute, where we had never had any experience with problems, and because there were no long lines of sight, or anything like that, we didn't really look at the stemming very carefully.

Carothers: Something that Livermore started to use routinely was one or more solid plugs in the stemming column, which would support the stemming above them, and also be an impediment to gas flow. Why did you think plugs were necessary, and start to use the coal-tar epoxy mix?

Olsen: In retrospect that particular thing probably got its rudiments from Scroll. After we looked at the results for a while, we realized that we had a plug there, but the cement was in the wrong place, and the stemming all ran into the cavity. If we had put it in the right place we could have put in a lot less, and it would have done a better job.

Hudson: I guess we really started worrying about plugs as containment features in Area 2, where we had subsurface subsidences, followed by, perhaps, the displacement of gas to near the surface, through the chimney. We weren't quite sure how the gas got to the surface, if it did. But, if it did, it seemed to occur in several stages. During some of the earlier stages we appeared to have radioactive gas going up the stemming column rather easily.

And so, we argued that putting in plugs to better block the flow of gas was a good idea. Those were more for a gas block, I guess, than they were for a stemming platform.

I don't remember just when it was we decided that we needed a stemming platform. It was primarily driven by the idea of a subsurface subsidence, where a significant amount of gas would be displaced perhaps halfway to the surface, after which we might have some, or all of the stemming fall into the void at the top of the subsidence. Any kind of stemming fall would eliminate the impedance between that pocket of gas and the surface. We wanted to avoid that. Riola was a perfect example of where we needed a stemming platform, and we had one that didn't work.

Carothers: I remember one occasion, and there were probably others, before Baneberry, where there was a stemming fall. The device went unexpectedly low yield. It was buried quite deep, and only went about a kiloton. That left a standing cavity into which all the stemming fell, leaving an open hole to the surface. So, I believe stemming falls do occur. But again, the LASL argument is, "Well, the fines bridge, and we never lose stemming."

Hudson: After starting to use instrumentation in the past few years to monitor the performance of their stemming, they have seen gas halfway up the stemming column, on some events, in a fairly short period of time. They've also actually observed their stemming column falling into the void above a chimney. Now, they will argue that they expect the stemming to bridge; they expect the stemming not to fall. But you may remember a CEP meeting where I asked Wendee Brunish if they thought they could depend on that. She said, "No, but we don't really need it anyway." So, they've lost confidence in their stemming as being a dependable bridging mechanism.

Carothers: The Livermore stemming has been criticized in the last year or two on the grounds that most of the stemming is just gravel, with a few plugs of gypsum concretein it. How did you arrive at that kind of design? Was it based on measurements you'd made?

Hudson: I think the current design is driven more by the philosophy of "good enough is good enough" than by measurements. The only place you really block the emplacement hole is where you're blocking the cable bundle and the cables themselves.

So, long stretches of low permeability stemming, where you don't do anything about the cable bundle, is not effective anyway. Los Alamos, on the other hand, has reasoned that stretches of coarse material will give the gases a chance to get out into the overburden. So, maybe a mix of low permeability and high permeability stemming is a good idea — that was their argument.

The current Livermore plan is sort of a blend of Livermore and Los Alamos philosophy. The only benefit that the coarse can possibly have, from a containment point of view, is to allow the gas to expand and come into contact with the porous medium around it. And that may be good. If you have a continuous cable bundle, surrounded by low permeability material, it will certainly be a much better conduit than a cable bundle surrounded by a very porous and permeable material. In either case we've always felt that the only real block to the flow of gas is a location where you block everything across the hole, including the cable bundle.

Carothers: You put the gas blocks there, the fanouts there, the impermeable plug there, and that's presumably where the gas will stop. Now, it has seemed strange to some people, me included, who visualize the process as the device going off, the cavity forming, and maybe a lot of noncondensable gases in the cavity which move out through the medium, which has some kind of permeability. This gas should move out more or less spherically, somewhat like a bubble, and when it comes to the plug, which is perhaps forty or fifty square feet in area, the surface area of that bubble is thousands of square feet. So, the gas just flows on around the plug, so what's the use of the plug?

Hudson: I think the plugs in the stemming column can only be effective at quite early times. Certainly what's in the stemming column can't stop what's going on outside the stemming column, and so you're right there. Sooner or later the gas is going to travel as a bubble, and this is probably what happens on Pahute Mesa, where you have late-time breathing. Even though we block it in the emplacement hole, there are enough fractures to allow the gas to expand until it finally intersects other fractures that reach the surface, after many hours, or days.

Carothers: Late-time seepage out of the cracks on Pahute Mesa could be due to the fact that most of the material covering the Mesa is the Rainier Mesa member, which was laid down while it was hot.

As it cooled, it cracked. It's several hundred feet thick, and the cracks are not necessarily through going, but gases can move from one to the other. In that context, that rock is almost not there to prevent the very slow seepage of gas. It provides overburden, but not gas blocking. Does that seem to be a reasonable scenario to you?

Hudson: Statistically it certainly seems sound. A study that was done did strongly suggests that there is a correlation. When this Rainier unit is exposed to the surface, certainly well over half the time you end up with some late-time seepage, or breathing as it is called. Whereas, when you don't have that member exposed, you don't have that seepage. There are other circumstances from time to time also, like rad chem drilling, which are hard to sort out. On the Barnwell event there may or may not have been a very late-time seep without the post-shot hole, but there is evidence that the post-shot hole was involved in a flow of gas toward the surface. So, it's maybe not as simple as the statistics imply. The fact that we don't see these late-time seeps on Pahute when we don't have this material exposed at the surface is an indicator that there's probably something to the theory.

Peterson: Something that we did for Livermore was a program on atmospheric pumping and why gases come out of underground shots at long times after the shot. I think it has given them a little different picture of why gases come out of chimneys.

The history of it goes back a long way. When we did the DNA chimney pressurization experiments, and we looked at the tracer coming up to, say, the top of the chimney and detected it, one can imagine that when you put gas into a chimney, the gas you're putting in is expanding as in a balloon. If that were the case, if you sampled at the top of the chimney you'd see no tracer for a while, but eventually, when the balloon got up to where you were sampling, you'd see the concentration that you were putting in.

Well, obviously this doesn't happen, because Mother Earth isn't uniform. If we look at the results we got from the DNA chimneys, we started detecting the tracer maybe a factor of ten earlier in time than you would expect if were expanding like a balloon. If we should have seen it in forty hours, we'd see it in four hours at the top of the chimney when we had hardly any pressure up there. In thinking about that for a while, it became somewhat obvious that the theory that one gets gases out of chimneys just by

simple atmospheric pumping — in other words, atmospheric highs and lows — is not the right picture. That's a driver, but that's not why it comes out. In order to get the gas to come out you need nonuniformities in the material, and it sort of bootstraps its way out.

I talked to Livermore about it because I knew they were interested in it many years ago. They thought we were crazy, or whatever, but more recently they became somewhat more interested. So we set up an experiment with a sand column, in a plexiglass tube, about four inches in diameter, maybe six feet high. We had a void region at the top that represented the atmosphere, and a void region at the bottom that would represent a cavity, for example. The sand represented the alluvium, and you can go through the equations and scale things so you get the relative volumes almost correct.

The hypothesis was that if it's just atmospheric pumping in a uniform medium, as you think of alluvium, you would get gas out. I really didn't think you would. So we did a number of experiments. In one of them we set it up with a uniform sand column, and we put gas with a tracer in the bottom chamber that represented the cavity. In another one we put gas with a tracer in the bottom, but we put a pump on the top that would vary the pressure, as atmospheric cycles do. Because this is all scaled one can do a lot of cycles in a fairly short time, and we ran it for four or five thousand cycles. It was equivalent to a thousand years of atmospheric pumping. Well, sure enough, when we monitored the tracer up in the top volume, the tracer concentration in the one that wasn't pumped turned out to be exactly the same as the one that was pumped. The pumping made absolutely no difference whatsoever.

The pumping is a driver, but you need some nonuniformities. So, we made a second sand column in which we put one permeability of sand in an outer annulus, and a different permeability of sand in the center. We used a very thin aluminum pipe in the column so we could fill the center with one sand, and the outside annulus with another sand. We did two columns that way. We left the pipe in one of those columns, but pulled the pipe out of the other one so the two sands could talk to one another. Well, in the one where the two sands could talk to one another the tracer came up very fast. The column where the pipe was still in acted just like the one that was absolutely uniform.

So, the whole thing on this pumping business is that you need the atmospheric pumping, but it is the degree of nonuniformity that exists that makes it work. It is the small fractures, or nonuniformities in permeability, that determine how fast the atmosphere can pump these gases out. When it is nonuniform, some gas flows up in the fast flow paths, and as it does that it diffuses out to the side. When the atmospheric pressure changes it can't push all the stuff that's diffused out to the side back down. And so, on the next atmospheric low, a little more moves up and diffuses out, and tends to stay there during the next high. If you have a lot of these nonuniformities, then the gas can move up quite a bit faster than if you have a fairly uniform medium. That's a containment thing that we have looked at and studied, and I think we found some interesting answers.

We're still doing some work on it. There are a bunch of models, and part of the work we're doing is to look at some of those experimental results. There is some data, and we're trying to look at it to see whether we can characterize what the formation looks like, and why it has done what you see that it has done. You can say, "Gee, it would be nice to learn all these things, and then somebody could go out and drill one drill hole, and they would know whether that's the perfect place to do a test or not." I think we're a long way from that. But I think you have to learn these things, and get an understanding of what's happening, even to be able to make the judgment as to whether you're ever going to be able to do something like that or not.

Carothers: The things that have been done in the field since Baneberry have essentially eliminated the seeps and leaks through the stemming and the cables that had happened on both Laboratories' shots fairly often before then. What did Los Alamos do about the cables, for instance?

Scolman: We had been gas blocking multi-conductors before Baneberry. After that we started gas blocking coaxial cables. And, we pushed for the development of continuously gas blocked cables. As I've often told people, anytime you break a cable you're asking for trouble. For example, when you look for trouble with wiring in your house, or car, you don't go to the middle of an existing run of wire; you look at the connectors.

Carothers: Did you ever do cable fanouts before Baneberry?

Scolman: I don't think so. They were initially a pain until we, and I think in this case until Livermore, figured some simpler ways to do it. We did, for a while, have big three-dimensional cages where the cables were physically separated from each other. They were a pain to put down. And most of the time when we found we had a cable problem going downhole, it was either where we had put a gas block in, which involved breaking a cable and putting in a physical connector, or going through a fanout. We made a point that whenever we were going downhole, when we had gone by cable gas blocks, which in general meant a fanout in the same area, before we went any further we required a complete cable check. We didn't want to put the device downhole and then find out we had to bring it back later. And we did have trouble doing those things. We also did an awful lot of experimentation to try to do things that were probably not possible to do. One of the early requirements was that our cable gas blocks and our plugs in the casing should be able to handle five hundred psi. We found pretty soon that probably was not possible.

The plugs were the problem. The cable gas blocks you can make that good, but you can't make the plugs that good. The other problem, of course, is that if you're going to tell somebody that a downhole plug is good for five hundred pounds, you better be able to test it in place. That's pretty tough to do, unless you put a pipe down, and force a whole hell of a lot of air down there to start with. So, that requirement on the plugs went away, but we did a lot of work trying to do things like that.

Carothers: My impression is that Los Alamos came to the use of plugs somewhat reluctantly. Why was that, aside from the fact that they're expensive, messy, and a pain to emplace?

Scolman: I'd agree that it was reluctantly. We didn't really think they were necessary. We had a body of experience that said fines plugs were very effective. When you say 'plugs', generally what you really mean are 'stemming platforms.'

Carothers: They're called a couple of different things depending on who's talking about them. And I suspect the people in the field who were emplacing them called them a lot of things we needn't mention.

Scolman: To my knowledge, at least in my time, I don't believe Los Alamos ever claimed one of their plugs was a stemming platform.

Carothers: Well, Tom, I have been on the CEP for many years, and I have seen the Los Alamos presenters perform various interesting verbal contortions to avoid calling them stemming platforms.

Scolman: They were directed not to do so. We did not believe that the material being used for these plugs was the kind of physical material that one could count on to stop a stemming fall. In other words, coal-tar epoxy is not a strong material. It's a little bit like asphalt.

There was some pressure for us to follow the Livermore lead and call the plugs stemming platforms, as guards against a loss of stemming. Our field engineering group got their backs up and said, "Look, we're not going to tell you that it's a stemming platform unless we do something to engineer it to be a stemming platform. For example, put in a reinforced cage and some high strength concrete." We did, by the way, for quite a while, put a concrete layer under the lowest plug to protect the coal-tar expoxy from heat. That was a bit of a push toward saying, "Okay, it is indeed a stemming platform." At least there was protection from hot gases being there right at that surface. But we were - - reluctant is too mild a word - - not going to buy in to the coal-tar epoxy plugs as stemming platforms.

Carothers: Well, you were ultimately proven to be right. There was Riola, the coal-tar epoxy stemming platform was challeneged, failed, and all the stemming fell out. That led to different kinds of plugs. LANL now is using two-part epoxy, aren't they?

Scolman: Yes. I think that was driven as much as anything by the toxicity of the coal-tar epoxy. Plus the fact that I think it's a little cheaper. I've forgotten the numbers, but some appreciable fraction of the cost of the stemming material was made up by the coal-tar epoxy plugs. And frankly, I've always considered them chicken fat - - just something that makes you feel better. Particularly the lower ones.

Something that has always bothered me is that I think if Los Alamos, in particular, can be faulted in any way for the containment regime we've gotten ourselves into, it would be because the things we do were designed for cased holes. We now use them almost exclusively in uncased holes, and many of the things that are done don't make very damn much sense in an uncased hole. Including using impervious plugs in a pervious medium.

Carothers: Billy, the Livermore plugs are supposed to support the stemming in the event that the stemming is lost beneath the plug. Will they do that? How do you know?

Hudson: We advertise that the top two plugs, the plugs that are forty or so feet thick, are stemming platforms. We believe, based both on calculations and experiments, that any of our gypsum plugs would act as a stemming platform, even if they were only twenty feet thick. We have never seen, based on our measurements, a gypsum plug fail as a stemming platform, even though it's been as thin as twenty feet. But then, we've only had them challenged a small number of times. The twenty foot plugs, I think, have only been challenged twice.

Carothers: You mean by challenged that there has been a loss of stemming below the plug, the plug stayed there and the stemming above the plug stayed there, so they worked?

Hudson: Yes.

Carothers: The coal-tar plugs were emplaced by pouring the gravel and the coal-tar in at the top of the hole at the same time, but seperately. One of the criticisms of that process was that you really didn't know what kind of plug was formed when those materials reached the bottom and presumably mixed.

Hudson: That's why we now mix the material before we put it downhole. Instead of just letting it dribble down the side of the hole we now put it in through a pipe until it's within about fifty feet of its final resting place. I like to describe the process we were using in the past as like throwing gravel and cement over the top of your house, hoping to get a patio in your backyard.

Kunkle: Brian Travis tried to model the heating and cooling of coal-tar epoxy plugs. In Los Alamos holes this was the material we emplaced as rigid plugs, at the time. CTE, as we knew it by it's initials.

Carothers: Over a couple of Los Alamos engineers' dead bodies, probably.

Kunkle: Well, that material could certainly make you dead if you came into contact with too much of it. I was astounded when I first got here to learn they would actually use this stuff in any field setting.

A thing I recall from graduate school is watching a colleague, Dave Lolley in the Physiology Department, who studied rats and rat problems. One of the rat problems he would cause is skin cancer, which he would cause by simply painting coal-tar onto the skin of the rat. After a few weeks, the rat had skin cancer. So, I was sort of shocked to find we were using coal-tar in rather large amounts at the Nevada Test Site.

At any rate, one of the calculations that I was watching Brian Travis do was the expected heating of the plugs - - the rise in temperature due to the exotherm as the epoxy set, and the subsequent decline in temperature - - as indicated by the thermisters in the plugs. They didn't make any sense. This must have been in July, August, September, 1980.

We could make no heads or tails of those downhole temperature measurements. A tentative conclusion we reached was that the coal-tar and the gravel that was put into it - - it was a coal-tar concrete - - must not have been well mixed together. There must have been some plugs where one side was mostly coal-tar, and over on the other side it was mostly gravel.

Each plug was different, and none of them behaved as they should. That was a puzzlement to us. And then, it must have been September, October, the Livermore Riola event seeped a tiny amount of gas to the surface. That caused quite a stir, because one of the coal-tar epoxy plugs had failed to hold the stemming material that was above it. The plug simply wasn't there. Reentry observations showed that, indeed, it was probably never there. That is, the stuff had been put in the ground but it had never formed into a monolithic uniform plug.

Brownlee detailed me at that time to go study coal-tar epoxy plugs, and the problems that were plaguing them. I dutifully took on this assignment, and together with Billy Hudson we formed a little outfit we called the Stemming Plans and Stemming Modification group, otherwise known as SPASM. We investigated coal-tar epoxy plugs, how the Laboratories were emplacing them and using them, and how well they might be performing. This involved pouring plugs, full-scale plugs, in a hundred foot deep hole we had, and pulling them back up. Then we broke them apart to see what was in them. And we found that, as the calculations had suggested, those downhole plugs were miserable.

They were not what they were planned to be, but they had properties similar to those you might have inferred from simple downhole diagnostics; the temperature records. They were not uniformly mixed. They were segregated; sometimes into layers of nearly pure coal tar epoxy, sometimes layers of gravel, and that type of behavior could have been inferred, and partially was inferred from the temperature records. There were diagnostics that coal-tar epoxy had been put down the hole, and that it was reacting, because it had generated heat.

Carothers: It had generated heat, the temperature had risen, and then had started down. Therefore it was setting up, and becoming a rigid plug. I suspect that the people who were making those measurements used the temperature records only to say, "Well, see the temperature has come down and the plug is now cured. Therefore, we can shoot."

Kunkle: That's right.

We began talking about the the replacement of coal-tar epoxy with an alternate material. We, Los Alamos and Livermore, finally settled on a water-based epoxy, Celanese by brand name. We both put TPE, two-part epoxy, plugs in for a while.

This episode of switching from coal-tar epoxy to two-part epoxy involved a lot of, "Well, let's look back through the records and see what's actually happened on our past events." This was a time to, quite literally, review all of our post-Baneberry underground nuclear tests for how they were stemmed, what downhole diagnostics were put in and what those diagnostics might have seen. The thermistors were the in-situ diagnostics in the plugs and there were sometimes something about plug performance. There were

also radiation and pressure monitors, RAMS units, instruments to measure surface accelerations, occasionally some downhole accelerations - - that kind of stuff. We went through, shot by shot, reviewing our history of Los Alamos stemming. That got me pretty familiar with what we had done in the past, and why, and what problems had been encountered.

Carothers: Those coal-tar plugs had been used for many years, more than ten, at least.

Kunkle: We first put them in just before Baneberry, on Manzanas. And before the Baneberry event we had a design for a shot which would have some in it, but that shot was fielded after the Baneberry stand down.

Carothers: The lesson to be learned from coal-tar plugs is that probably during that ten or so year period all of those plugs had been very poorly mixed. And that was okay because nobody knew it. People would say to the Panel, "We have a stemming plan like this, and we have these coal-tar plugs. They have been used successfully on x-teen events." Then one day one got challenged. And it failed. Until a feature has actually been challenged and survives the challenge, a statement like, "Well, we're going to put in gypsum concrete. We have used that successfully ten times," doesn't mean very much.

Kunkle: In the pre-Baneberry era, the shots were without plugs. We saw many small releases, but they were acceptable under the guidelines at the time. They didn't seem to much bother anybody, and it was fairly well understood, by at least a few people, where they were coming from. That was flow in cable bundles and such.

The coal-tar epoxy was introduced to stop the flow in the cable bundles. That was it's real purpose for us at Los Alamos; at least that's why we started using it. And it seems to have worked pretty well at that, even if it didn't ever set up into a real plug. When we did introduce the coal-tar epoxy plugs, and used them routinely after the Baneberry stand-down, along with the cable gas blocks, the small releases we'd seen near surface ground zero stopped. And so the coal-tar epoxy plugs were quite satisfactory from some standpoints, but they were not structurally competent to serve as stemming platforms.

House: TPE is not the ES&H problem that CTE was in terms of handling, and it is a much more suitable plug because it does, in fact, become a rigid plug. There was a confirmed suspicion that coal tar might never get hard and set up, and could, in the event of a stemming fall below a plug, perhaps drain away. And in one confirmed instance, it did. We have seen physical evidence in terms of pictures provided to the Panel of just that happening. And that event in and of itself really spurred conversion to some other type of plug material.

TPE is, unfortunately, a far more expensive, in terms of pour per linear foot, than Livermore's sanded gypsum concrete. But for some reason, that I won't attempt to address, our field operations people have been not particularly receptive to making a move to sanded gypsum concrete. I think, cost notwithstanding, and if I remember the Chairman's sermon, delivered more than once, cost is not to be considered a factor in containment design, we at Los Alamos favor the TPE because of its properties. Albeit, we are in a process now of reducing the number of TPE plugs, and replacing one of them with a grout mix designated as HPNS-5, which means Husky Pup Neat Slurry, which seems to have a lot of reasonable properties, and is far easier to emplace at great depth.

Carothers: How do you emplace the two-part epoxy plugs?

House: Two-part epoxy is emplaced in a very simple fashion. It is pre-mixed at the surface, in a specially configured, or specially insulated, transit mix truck. The two-part epoxy is called, by the Celanese Corporation, Part A and Part B. Three-eights inch pea gravel aggregate is added to it. It goes through a mixing process and comes out of the truck, down a chute, and free falls down the hole. At one time we attempted, I believe on the Trebbiano event, to emplace a plug at 990 feet using a tremmi pipe. I think the field engineering folks had a six inch tremmi pipe to pour the stuff down, and it didn't go down very well at all. And so it was concluded that trying to emplace it through a pipe was unsuitable, and we have continued with the free fall method.

Carothers: Do you have any concern that there might be some separation of the gravel and the epoxy?

House: As you watch it come out of the chute, out of the transit mix truck, it's easy to see that it is well mixed, and the epoxy has enough adhesive properties to pretty well entrain the gravel in it as it goes down hole. We're talking about 3/8 inch aggregate, which is pretty small. Also, although admittedly this isn't a free fall sample, we do take five gallon buckets right out of the end of the chute, and go test it. But of course, that doesn't tell you what it is like when it gets to the bottom of the hole.

We have done experiments in abandoned or unusable emplacement holes, where we have poured plugs at, say, 120 feet, and then gone down and cored them, and done some sampling. At least at that kind of a depth, which is essentially equivalent to the standard location of the top TPE plug, we find them to be pretty well mixed, far more so than the old coal-tar plugs.

Carothers: Livermore has gone through a series of stemming plan changes. Why didn't Livermore observe that LASL has never had a seep since Baneberry, think their stemming plan must be pretty good, and use the same design?

Hudson: I think if we could be sure we had the same sort of working point medium, which we probably do if we put our shots in tuff, it probably would be perfectly okay. The alluvium in the Los Alamos areas has been described as "more forgiving" in that their alluvium is less cemented than most of the Livermore alluvium. In fact, they have difficulty in drilling a large diameter hole in their alluvium, which means that it isn't cemented as much; it doesn't hang together.

Consequently, It's always been a question, a puzzle, why they have historically had better luck than Livermore. Their overburden material won't support open fractures like the Livermore overburden will, and I suspect that's the main reason. In practice I don't think it has always really been that much better. Prior to Baneberry their release rate wasn't much different from ours. Since Baneberry we've had two seeps, and they've had zero. What sort of statistics are those?

There's another reason for the changes we have made. We've always paid a lot more attention to the performance of our stemming plans than Los Alamos has to theirs, by using downhole monitors to see what goes on. When we saw that radiation was

getting higher in the hole than we liked, we tried to make changes to stop it. Almost all of the time those threats - - when we had radiation higher in the hole than we wanted - - would not have led to a release. We were only concerned that they were an indication of something, maybe, worse to come.

Los Alamos, on the other hand, has for the most part ignored the performance of their stemming plans. It's only recently that they've started fielding very many downhole radiation detectors, for example. So, I suspect that they were fat, dumb, and happy, while we were trying to fix things that weren't all that important.

Carothers: Well, if I were to speak on the side of Los Alamos I could say, "We monitor the performance of our stemming plans with the ground zero radiation monitors, and the stemming works just fine."

Hudson: And I can't argue with that. If you're only concerned with yes or no, as opposed to how and how well containment was achieved, the statistics are such that you can't argue with them.

Rambo: We're the only ones who do a full stemming column calculation for the vertical shots. We include the stemming column in the calculation. For many years all we did was the outside world, but now we put in the plugs. We have material properties for the coarse material, and when it was sand we had that, and for a while we were putting in the two-part epoxy plugs.

Carothers: Might that be because Los Alamos could say, "Why do calculations? We never have any trouble with our stemming plan."

Rambo: That was true until recently. But you can say that about just about anything in containment. Recently they had a shot where gases got quite a ways up the hole because some of the stemming fell out. Before that they didn't have that kind of problem. They thought that the fines layers were going to compress strongly, because they showed in one of the tunnel shots that the fines material does turn into something pretty hard. That's a sellable argument. We ran with fines layers for a while too, in the residual stress area, but we had some leaks past them. So we went to sanded gypsum plugs, thinking they might be even better material, and we've still had some leaks past those plugs. It didn't seem to make any difference whether we had one or the other.

Carothers: It has always seemed a little surprising that a plug would matter. In an uncased hole, when gases come to a plug why don't they simply go around it? They can go into the native material as well.

Rambo: Sure. And I imagine they do in many cases. The difficulty is that once they get into the stemming, then you're relying on man-made items to stop them before they get up to the surface.

Carothers: Livermore uses a few long gypsum plugs in a column of gravel. That gravel has probably a permeability of a hundred Darcies or more. Los Alamos uses many alternating layers of gravel and fines, rather than a lot of gravel and a few plugs.

Rambo: And what do you hear when you talk about that in our containment group? You hear things like, "Gee, it costs a lot of money to put in those fines layers."

Carothers: Of course it does. Almost anything you do is more expensive than just dumping in gravel. Putting in sanded gypsum plugs isn't free, however.

When I see a drawing of the stemming plan at the CEP, the vertical and horizontal scales are different, so it appears that the hole is rather short, and pretty big in diameter. The plugs appear to be thinner than their diameter. Now, if you showed me the stemming plan with equal vertical and horizontal scales, there would be a long, very thin emplacement hole with a few long, thin plugs in it. Looking at that kind of representation, the plug appears to be just a small irregularity in the ground.

Rambo: Yes, just another rock. It is amazing, but there have been many times when they've measured pressure below it, or radiation below it, and not measured anything above it.

I think it also helps to put a plug in the so-called residual stress field, because you've compressed all this material, and flow may indeed stop there. The cavity pressures that they've measured seem to be decay and reach a plateau where they sit for quite a while. That suggests to me that there is leakage, but not at a horrendous rate, but of course shots are different in this regard. The pressure in the Cornucopia cavity, which was fired in a fairly weak material,

sat there for a number of hours before it finally decayed all the way. It was down around twenty bars, which is fairly low, but it was still there for a fairly long time.

It's enough to say there's something there that isn't letting all the cavity gases go out immediately. There hasn't been enough data to put the whole story together yet, but there may be something there. If we could see more data, perhaps we could see that in a weaker material there is something which happens, or doesn't happen, so the gases are held in for a while.

There were shots, like Roquefort and Coso, where calculations showed them close to the margin, and they had radiation high in the stemming column. I think one of the failures in this business is that when we have radiation up the stemming column, very seldom is anything ever done post-shot to look at why that happened. And without ever looking at that you're doomed to keep repeating it. You never learn anything unless you stop and take stock, and say, "Why don't we learn something about this?" The constant statement is, "Well, it contained." But by how much? And what did you learn from that? That part of the process is dead.

Carothers: I can't remember any significant post-shot exploration in the past few years.

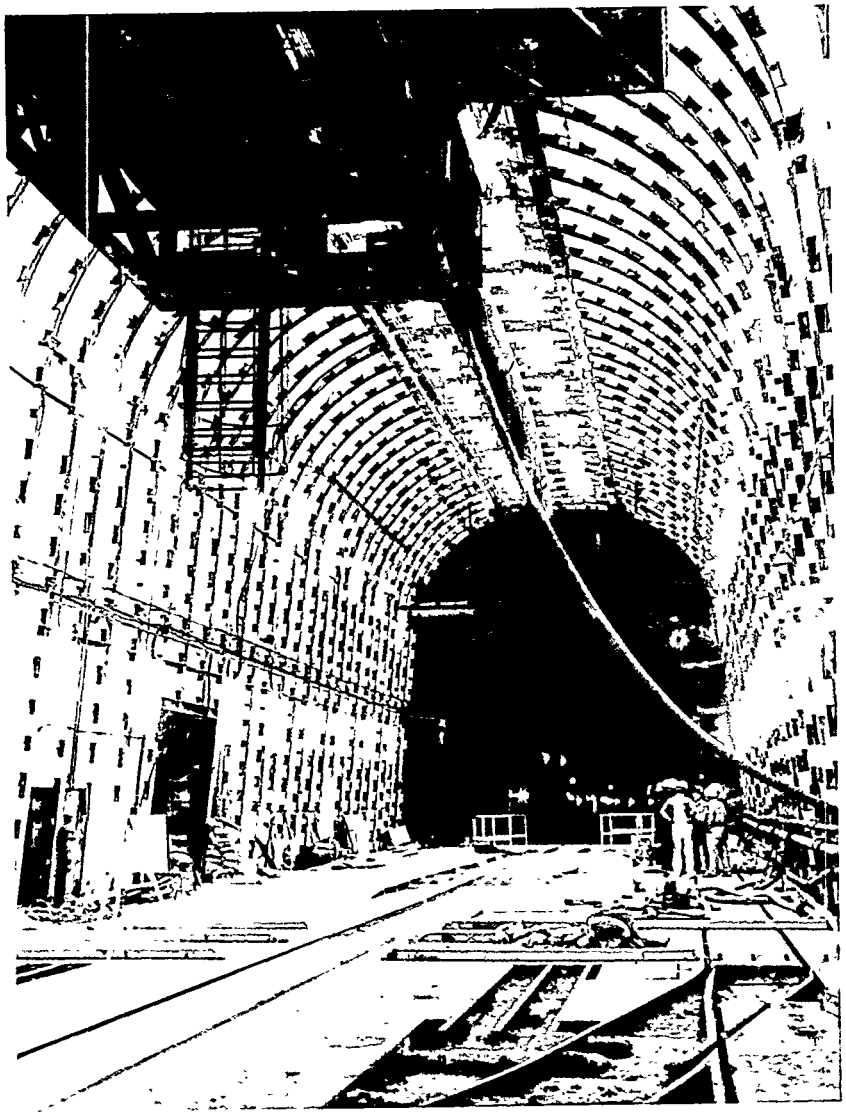
Rambo: That's right. Anyway, there is this realm of calculations that shows things on the margin sometimes. I'm not sure it was totally residual stress, but I looked at Roquefort after I had presented it to the CEP, and I said, "Look, there are some weaknesses around the top of the cavity." I told the containment scientist, "It looks to me like you could get something into the stemming column. Even though there's residual stress in the outside world there isn't enough residual stress to close across this coarse material that we're using for stemming. That's hard rock with lots of permeability. How much residual stress does it take to close that off? I don't know."

That's one of the key issues that we don't really think about very carefully, and it's part of the difficulty in interpreting the calculations. I told them, "I see you've put your two plugs in some very weak areas in the hole. If you do get gases up there, it's liable to go past the first two plugs." Well, that's exactly what happened. And at that point I quit doing that because I was ahead of the game, and it'll probably never happen again.

What I'm learning in this process is that maybe I shouldn't be quite so positive about having a residual stress and that means we're not going to get anything up the stemming column. Of course, we've had a lot of successes, and that's why there's such limited experience. The failures are really where you do most of your learning.

Carothers: Billy, in summary, why should there be two different stemming plans? I could say, "One is better than the other, so you should use the better one." Or, "They're both equally good, in which case you should use the cheaper one."

Hudson: It really is not terribly rational to have two entirely different stemming plans. I think we are getting closer together. Maybe the people in the containment programs at Livermore and Los Alamos are a little more rational today than they were in the past. But one wonders, "Why have we persisted so long in doing things differently, almost for the sake of doing things differently?" I think we probably should adopt similar stemming plans, and similar ways of blocking cable bundles. Parts of each are probably better than parts of the other. Parts of each are less expensive than parts of the other. Why not develop a compromise which is as good as either one, and costs less than either?



Experiment stations in tunnels can be quite large. The basic limitation is cost, not the mining technology.

16

Tunnels and Line-of-Sight Pipes

The Livermore people did the first several tunnel events, starting with Rainier in 1957. One of the problems that concerned the diagnostic physicists as the movement to underground detonations began was that they would not be able to, on an underground detonation, get the data that they were accustomed to getting on atmospheric shots. Fast camera records for the determination of the device yield from the growth of the fireball, for example, seemed to be out of the question. Or, how could there be multiple lines of sight, looking at the reactions in different parts of the device? And so on.

Brownlee: The guys who measured things, whether they were the radiochemists, or the physicists measuring reaction rates, or looking at neutrons or x-rays or gammas or whatever, felt that they obviously needed to test in the atmosphere. So, when we got ready to go underground - - were forced to go underground from their point of view - - there was the hand-wringing, the weeping in the streets, the swearing, because, "We can't make our measurements any more. We can't learn what we need to learn about the bomb." Therefore, if they had to go underground they wanted, always, a pipe that looked at the bomb and gave them a solid angle that was as big as the one where they used to stand for an atmospheric shot. And this pipe had to be open all the way. That's what they wanted.

To some, a tunnel seemed to offer the best way, underground, to provide such access to the device. In principle a tunnel could be as big in cross section as someone was willing to pay for. Further, the device itself, and the associated firing equipment, could be brought in and the device made ready for firing in a way very similar to the way it was done on the atmospheric shots. Since there was personnel access to detector stations until very near shot time, alignments could be made and checked, a failed detector could be replaced, vacuum leaks could be repaired, and so on. All of these

things were difficult or impossible when the device and all the experimental equipment had to be lowered down a relatively small-diameter emplacement hole.

On the other hand, if there was to be no release of radioactive materials to the atmosphere, somehow the opening leading to the device had to be closed after the desired information was obtained. The experience was mixed. Rainier had released no radioactive material. Nor had Logan, which had a line-of-sight pipe used to allow samples to be exposed to the device output. Neptune and Blanca had vented. Both of those could be attributed to an insufficient amount of material over the detonation. So, it seemed that an underground detonation with a pipe of some size to allow the radiation from the explosion to reach diagnostic detectors, or to irradiate samples could certainly be done and the detonation contained. However, as later tunnel events showed, containment of the radioactive products was not as simple as it had first seemed, nor was it easy to assure the protection of the samples.

But first, to do an experiment in a tunnel the tunnel had to be mined. Bill Flangas was the mining superintendent at the Test Site for many years.

Carothers: I have asked people why they picked Rainier Mesa for the first underground tunnel shot, and about the only answers I have gotten is that it was there, and it was good minable rock. What do they mean by "good minable rock?"

Flangas: Well, it's a rock that's in the neighborhood of a couple of thousand psi in compressive strength, and so it's easy to mine. In the tuffs in Rainier it's easy to drill out a face, and once you've drilled it you didn't even have to use full strength dynamite. We were using 25 to 30 percent compared to the usual 50 and 60 we use in hard rock. It's the kind of material that has to be supported, but it's easily supported. In those days we were using wooden sets, and then we went to steel sets, and used some rock bolts. Then we went to wire mesh and shotcrete, which is a mixture of cement and water. It's a modern version of gunnite. The products come out of the nozzle, where they are plastered up against the wall. It's gotten refined to the point where getting six and seven thousand psi strength with shotcrete is pretty routine.

And in tuff you can use the Alpine Miner, which is a machine that's like a tractor. It's got a boom, and on the end of the boom is a rotating cylinder, which has carbide bits on it. This boom articulates up and down, and back and forth. As the cylinder rotates it just grinds the rock away. It works very well in soft rock, like tuff. It wouldn't touch granite.

During the Hardtack II operation, from September 12 to October 31, 1958, seven devices were detonated in tunnels in Rainier Mesa. Neptune, Logan, and Blanca were mentioned in Chapter 1. Mercury (slight yield), Mars (13 tons), Tamalpais (72 tons), and Evans (55 tons), were all events with very low yields, but even so all but Mercury released some radioactivity. Following Tamalpais, fired on October 8, 1958, there was an noteworthy incident related to the gaseous by-products of a detonation, which were not, in a sense, contained.

Flangas: Tamalpias was where we had the infamous hydrogen explosion. When we shot Tamalpias, because of the short lived products, some of the early readings in the tunnel were up there in the 10,000 R range. And so the consensus was, "Okay, this tunnel is gone." And we still had not fired Evans.

We had been working seven days a week, twenty-four hours a day, and I never left that tunnel day or night. Most of the time I was sleeping on my desk. By the time we shot Tamalpais some of us were flat wore out. So, once they start reading those kind of numbers it looked like the ball game was over as far as that tunnel went, and I went home. I got home about nine or ten o'clock that night, and I was still asleep at two o'clock the next afternoon when a call came through that said to hurry on back. The readings were down to 300 or 400 mR, and they were anxious to get started again. By the time I got back up there it was like four o'clock. The Livermore honchos were there, and some of my troops had been assembled and they were there.

I asked the question, "What have we got." They said, "It looks like the highest exposure right now is like 400 mR." We could stand that for reentry. And then, of course, my next question was about explosive mixtures. I was assured that there was no explosive mixture. What had really happened is that due to the inexperience

of both the Lab people and others, the meters they had in those days got saturated, and so they were reading zero, when in fact the place was loaded with hydrogen.

I went into the tunnel and I went back several hundred feet. The hair was standing up on my head, because I knew there was something wrong, but I couldn't put a finger on it. So, I came back out, and I repeated the question. "How are we in terms of an explosive mixture, or are there are any other gases, or any exotic gases I don't know anything about?" And again I was assured. "Quit worrying about it. You do not have an explosive mixture."

I went back in the tunnel. We were doing some preliminary work to get started, because it was important to get ventilation established so we could clear the tunnel out so we could proceed. I came back out again, was reassured again. As I ruled out every possibility, it occurred to me to wonder if my antennae weren't geared to an oxygen deficiency. One of the things copper miners fear the worst is oxygen deficiency, and in those days, in a copper mine, under Nevada state law, you had to provide every miner with a candle. The way you checked for oxygen deficiency was with a candle, because a candle goes out at 16% oxygen, or thereabouts.

Carothers: You can also check for hydrogen that way.

Flangas: Oh boy, can you. So, anyway, I lit the candle, and I went all the way back in the tunnel. I was holding it just about chest level, and it was burning, so that ruled out oxygen deficiency. The rad-safe superintendent had climbed up on a sandbag plug, which was at about the 700 station; - 700 feet from the portal. And he says, "Hey Flangas, hand me that candle." So, I handed him the candle. Well, being a light gas, and without that environment having been disturbed, the hydrogen had accumulated along the top of the tunnel. He was up in that atmosphere, and Lordy, Lordy. I was standing in the middle of the drift, at the 700 station, and he was up at the top of that sandbag plug. He said, when we talked to him a couple of days later, that he saw a flame that just went down to the 1200 station, where the other door was, and he was fascinated by the sight. I was standing right on the track there, and the next thing I knew I was head over heels, and when I picked myself up, I was at the 350 foot station.

I have no idea . . . it was . . . just everything was in motion. We had laid plywood along that entire tunnel to protect the cables. That plywood was shredded to sawdust, to small fragments. There was a six inch steel door at the 350 foot station, and fortunately one of my shifters laid the track across there. We had to pull the track out to close the door, so when we opened the door, we put the track back in. That six inch door folded over that track into a U.

Carothers: Bill, with all that going on, how come you're sitting here today?

Flangas: I have never been able to figure that out. I came out of that thing without a scratch. I think if you tried it a million times you'd have a million dead miners and never succeed in duplicating that.

Carothers: What about the guy who was up on the sandbag plug?

Flangas: Fortunately, what happened to him is that when it went off the concussion knocked him down to the base of the plug, and when the explosion took place, it blew over him. Now, in that melee I turned around to look for him. My miner's lamp was shattered, and the place was just a bedlam. So, I looked for him for about a millisecond, and then I decided, "What the hell, it's every man for himself, and I'm getting out of here."

There were another four or five people in a side drift, and they escaped the blast. It went right past them. After all of this settled down we kind of found one another in the dark there. We finally retrieved this fellow by the name of Wilcox, and he was out colder than a wedge, at the base of the plug. When the blast door folded over it left a hole just barely big enough for a person to squeeze through. We accounted for everybody and got them out. The people on the outside were pretty excited. They thought everybody in that tunnel was dead, and that was a pretty good presumption at that time. So they called the ambulances and doctors, and there was a lot of commotion. It was a very unique experience.

Carothers: There was another case where somebody turned on the power at the portal, and caused an explosion.

Flangas: That was the same incident. Once we got everybody out, and things settled down, we put a gate with a four-inch wire mesh across the portal. We just took some two by fours and made

a gate to keep anybody from inadvertently walking into the tunnel. Then somebody said, "Well, let's turn on the lights and see what it looks like." So, they turned on the lights, and that was the second explosion. I was gone by then, but they tell me that wooden gate we put at the portal, with a four-inch mesh, sailed some three or four hundred feet away. So, those two incidents took place in the same tunnel within a couple of hours of each other.

We learned a hard lesson there. As a result of those incidents, in a very short time reentry became a very formal, tightly controlled process. That was a long time before the rest of the Test Site became procedurized. In fact, I think I'm the person responsible for developing and calling for the first formal mine rescue training. I had a vested interest, because I was leading a lot of those reentry teams. From there on it became a very sophisticated process, and it remains so to this day. No shortcuts, and no hurry up and do something unless you've ruled out all the possibilities. Subsequent to that there has never been another incident of that type.

Carothers: There had been, in 1957, the Rainier shot. After the moratorium started there was extensive reentry work. Did you have anything have anything to do with that reentry?

Flangas: I had a lot to do with Rainier. Once I came here and worked for a few days at E tunnel, I was sent up to take over B tunnel. B tunnel was the one that had the Rainier shot, and at that time they were making some efforts to dig a little incline down towards the original ground zero. But Livermore had a couple events that they needed to fire prior to the moratorium, and there was just one hellacious effort to get them off.

After the moratorium started, and things settled down, we started mining back to recover the initial ground zero, and we did. The only radioactivity that couldn't be handled was just as you entered the cavity, where the melt was up against the wall. What we did was, we just put some lead plates up where we crossed that threshold. Past that you got into a relatively radioactively cool area.

The real problem on that was the ground temperatures were still in the neighborhood of 160 to 170 degrees. We were drilling and blasting, and the manufacturer of the dynamite wouldn't guarantee the product beyond 180 degrees. And we were dealing with at least 160 degrees. So, we would drill the holes for the dynamite, then we would cool them with water, and then put three

or four people in there loading. We could load it out in about one minute flat, under the circumstances, and wire it. So, we felt fairly secure, even though the manufacturer would only guarantee the dynamite up to 180 degrees. We knew that the manufacturers give themselves a little wiggle room.

Every time we exposed a fresh face, because there was a lot of humidity there, there was just a tremendous amount of steam, and visibility was bad. And then there was this business of really pushing on the loading and shooting. The miners took all that in good stride, and we knew that it was significant work. There was always a great degree of excitement with this business, and I guess that's what kept us here. It was a unique operation.

We did that during those moratorium years. Later on it was decided, I guess when things began to get shaky with the Soviets, to prepare a couple of three test beds in the event they were needed, so we dug a couple more sites up there at B tunnel, and we were putting one down at E tunnel also. Then there were three tunnels, called I, J, and K, which, if I remember right, came right after the Russians broke the moratorium. We built up tremendously during that period.

Carothers: Only two of those were used. That area was abandoned after Platte and Des Moines vented. Gene Pelsor said, "The reason they're behaving like that is because the rocks are different." Us somewhat naive physics types said, "Rocks? Different? What's different about a rock?" Anyway, after Des Moines and Platt Livermore got a little wary of the tunnel business, and began to move more and more to drill holes. I think the last tunnel shot they did was Yuba, in 1963.

Flangas: That's about right. That was, again, up in B tunnel.

Carothers: Did you do any work on things like Hard Hat, or Pile Driver? They were in granite.

Flangas: Yes, they were in granite. I quarterbacked both of those. Granite is a much different medium than tuff. The granite there is about ten, twelve, fourteen thousand psi. It takes different things and ways to mine it. In the tuff we were drilling with a rotary drill with a wing tip on it, and we could drill out the holes for the

dynamite in a round in fifteen or twenty minutes. In the granite it took a hour and a half to two hours to drill out a round. Generally we would drill ten foot holes and try to pull nine feet a round.

There were a number of fracture patterns there, and there were a couple of major faults there too. But generally speaking, the fracture patterns were very tight, and the material stood up very well. But there were a series of hairline fractures, in a regular sequence.

Pile Driver was an extraordinarily big, complicated, expensive event. It took some three, three and a half years to prepare and execute that. There was a shaft, and a drift at the bottom. I think it was Walsh that sunk the original shaft down to about 800 feet. That first event, Hard Hat, took place there. Then I wound up making the reentry on Hard Hat. That was my piece of that action.

Carter Broyles was the longtime head of the Sandia effort in underground test and containment:

Carothers: Came the moratorium in 1958 with the balloon with the bomb hanging on it as time ran out. What did you do during those three years of the moratorium?

Broyles: Designed Marshmallow.

Carothers: For three years?

Broyles: Almost. We designed and built it once, in E tunnel. Then when we went back to testing we started all over again. I did a few other things during that time. I finished writing reports from the above ground tests, but I did spend a lot of time on Marshmallow. In fact, for the next I don't know how many years, along with Wendell Weart, who was the Containment Director for DNA or its predecessors, I was the Scientific Director for DNA's effects tests. I was the Scientific Director for Marshmallow, in '62, and then for Midi Mist, in '67. That job doesn't exist at DNA now, but in that job I took the overall responsibility for not only the engineering design of the tests, but for the experimental designs of the tests as well.

Olen Nance, a consultant, was my containment expert, along with Jack Welch, for Marshmallow. It was Olen who designed the hook, the side drift, on Marshmallow, which was supposed to close the tunnel off for sure.

Carothers: That was an experiment which was designed to get effects information, in an underground environment. Logan was the first event of that type, but Marshmallow was somewhat different. You must have spent a lot of time thinking about sample protection.

Broyles: We did. That was really the first horizontal line-of-sight (HLOS) containment design problem that we faced. Marshmallow, in a way, was the most severe test we've ever had, because it had two line-of-sight pipes. One looked directly at the bomb, and the other looked into a holhraum. So, we were stemming and trying to close two pipes, one above the other, both of which were pretty good size.

The original tunnel stemming concept started out with stemming, then voids, then more stemming; the general concept was to be non-symmetric to be sure we didn't generate jets, or a continuous flow down the pipe. That design disappeared, and was replaced by others, some of which may or may not have been better. The whole community was developing a calculational capability, so people's understanding of what you could and couldn't, and ought and ought not to do for containment developed partly as people developed the tools for calculating what might be expected. Bill Grasberger had some input into those calculations, even though he was mainly the bomb designer for the initial source.

Olen's original idea was really a follow-on from the buttonhook design of Rainier, which was designed to push from the side and slam the tunnel shut. His design was a cheaper, maybe more economical way to go. Instead of the buttonhook, it was simply a side drift at an angle. It was was designed to store energy, so it was lined in order to slow down the diffusion of the energy, and so produce a stronger ground shock. It wasn't very many shots later when people decided that the hook wasn't all that useful. You could get just as much by the ground shock squeezing the tunnel down.

There were two sets of doors on Marshmallow. They were simply big, steel doors mounted like the prow of a ship, They were covered with sheets of HE, and slammed shut as a V-shaped thing. They were really debris stoppers and were not designed to contain gases. That's what we had on Marshmallow. So, it was really the ground shock that did any containment that occurred.

Marshmallow, while it didn't contain perfectly, didn't really damage the outside world very much, as did some other underground tests. If you go back and look at Marshmallow, it had essentially every measurement of every type we've ever done on an test with a source like that, including piping out a line-of-sight, and moving the camera bunker underground. We reentered, and the cameras were recovered. The cameras were in a protected bunker, which had a positive overpressure from tanks of nitrogen. It was just like things we've been doing ever since. The film was exposed to a few R, but it was given special development, and they actually recovered images.

Weart: One of the first things I got involved in when I came to Sandia was to reenter an event called Marshmallow, which was a tunnel shot that was conducted in Area 16, in 1962. It was a shot with a long line-of-sight pipe, in a tunnel. It was conducted for experimental purposes, rather than for developing a device, and was considered to be a relatively successful event. At that time there had been only a small amount of experience with tunnel shots, and particularly with pipe shots in a tunnel.

Being a geologist, and with my background, I provided the technical direction for that reentry. People had a desire to continue this type of testing, but they realized that they understood very little about what phenomena, what mechanisms actually determined whether or not you could prevent the radioactivity from coming down the tunnel or down the pipe, and out to an area where it would cause you great difficulty with the recovery of your experiments. So, they thought maybe we could learn something by mining back in to the first several hundred feet from the detonation point. We wanted to see if we could reconstruct from what we observed there what may have gone on. We did develop some ideas and concepts which were used on subsequent pipe shots, but we really didn't have a good understanding. It was all very empirical in those days.

Mostly the kind of thing we did on Marshmallow was to collect samples of the material we had used to fill portions of the tunnel. On that particular event the stemming was just sandbags, and in fact, the tunnel wasn't completely filled. There were individual stemmed sections with long air gaps in between. We took samples from those plugs to see to what density they had been compacted by the ground shock. Even in the void areas, where there was no

stemming, the tunnel was now full of the surrounding tuff, which had been injected into these void regions. And it was tightly compacted, as was the stemming material. To me the most impressive thing was to go back in to where the pipe had been, and see the complete and utter disruption of any continuity of the pipe. There were just massive pieces of steel, almost unrecognizable if you hadn't known what they were ahead of time.

As I recall, the area of fairly intense radioactivity was separated from the place where the tunnel was not collapsed, and was open, by a relatively short distance. It wasn't a long interval; there wasn't a massive plug of a hundred feet or more. It was a relatively short distance, and it led one to think that we may have come close to a situation where we wouldn't have contained this event very well at all. It pointed out that we really ought to understand what was going on.

Broyles: When Sandia got into the underground business a few years later, the doors were recognized as one of the big shortcomings for experiment protection, because we saw lots of projectiles in those days. They would come down the pipes and penetrate the doors. We had a distribution on those doors; everything from gaping holes down to craters with embedded particles. We carried out an extensive survey, and we talked to all the astrophysicists we could find who were experts on moon craters and asteroid impacts, trying to figure out velocities and energies, and so on. We ended up deciding we had things from fractions of grams to hunks, flying from very low velocities up to ten or twenty kilometers per second.

From the things we saw, we were satisfied that a lot of them, probably not all of them, were pieces of the front end of the pipe, or something up quite close. It also appeared that some of it, probably not the high velocity stuff, was grout being thrown down the pipe. Even in those early days that was recognized as very likely the stuff coming later in time. The early pieces were were mostly from the pipe walls, or closures, or the baffles. Most of the early shots had baffles, which were somewhat like a collimator, or a heavy baffle that you put in a muffler. They were a four-inch thick ring that stuck three or four inches into the pipe. After one or two tries it was decided they kept the pipe open more than they shut it down. They blew the pipe up, so it didn't get closed very well.

All of those things influenced people's thinking about what and how to design the close-in stemming to prevent not only late time leaks and containment failures, but to try to minimize the early time stuff that might damage the experiments.

There was always an argument from the very beginning; did you do more good by stopping the stuff, or by letting it go. And there were a lot of arguments that went on about whether you could choose an optimum place to put a muffler. If you placed it in close enough to where the ground shock closed it, maybe you wouldn't interfere with the ground shock closing the pipe. But, if you got it in that close, the cavity would expand and collapse it, and maybe it wouldn't matter. Those kind of arguments went on, and people did some crude calculations. But very quickly the community decided that ground shock wasn't really the way to guarantee, for these horizontal line-of-sights, that the world was protected. And they decided they needed more protection for the experiments than just the ground shock.

So, by the late sixties, on Cypress, we put in the first double sliding doors. That was a Sandia innovation for Cypress. They slid closed sideways as a backup to the ground shock pipe closure, but they also were put in as an early time protection against the high velocity debris, to protect the experiments from that. All of those were originally designed simply as debris stoppers. Later, people thought they could save money by combining that with some kind of gas seal.

As people developed calculational capabilities and equations of state to try to make intelligent calculations, the spaces in the tunnel where there was air between the stemming regions were replaced with some compressible solid material. If you look at the earlier shots, they would have a hundred feet of this, then fifty feet of air, then a hundred feet of that. Then the air got replaced with weak grout, with asymmetrical voids on one side of the pipe so the ground shock would shear things off and close it up.

As time went on, most of the detailed worrying was really about sample protection, because they found protection for the outside world had been taken over by the overburden plugs. After a time everybody recognized that you could design a plug that just

by brute force could contain a complete leak. I think it was after Camphor that DNA really went, in the early seventies, to more or less the current designs.

Carothers: There was a period of a few years when Sandia sponsored their own events underground; there was Cypress, and then Camphor?

Broyles: Cypress and Camphor were the only two, in '69 and '71. Baneberry was near Christmas 1970, so Camphor got delayed until June 1971. It was originally scheduled for right after Baneberry. Those were the only two horizontal line-of-sight experiments, in tunnels, that we did. Before that we sponsored a couple of the vertical line-of-sight shots. Derringer was the first one, and that was, in a way, a different kind of thing. There was a drift at the bottom of the hole, where the experiments were, and there was no line-of-sight to the surface.

I really had nothing to do with that; I was doing high altitude work at the time. Wendell was involved with the containment design, and Bob Statler, I think, was the Test Director for Derringer. The experiments were the exposure of components, and subsystems, and the systems down the line-of-sight. It didn't really contain, in the sense of protecting the experiments; they ended up not being protected enough. Really, essentially not at all. But, the emphasis was more on getting the real-time measurements out. If we could have recovered the samples it would have been a bonus, but that clearly wasn't as important as the other measurements.

Parallel to that there has been the continued evolution of the calculational capability, and as I see it, more and more willingness to believe the calculations of the ground motion and the ground-shock induced motion.

Carothers: Sandia was involved with tunnel events for some years. What was your participation in that work?

Weart: I was involved as a sort of containment design consultant for DNA on many of their shots. I'm not sure I remember the exact sequence anymore, but Gum Drop was a early shot after Marshmallow, and then there were a number of DNA tunnel shots with line-of-sight pipes. Sandia initiated some experiments of their own which required line-of-sight pipes in tunnels;

Cypress, and Camphor. In addition to the tunnels, I worked on the containment design for some of the vertical LOS pipes like Diluted Waters.

Carothers: What were the things that you were trying to address on those early effects shots, as part of the containment?

Weart: Everyone was concerned about the energy flow down the pipes, and how to make sure that did not interact in such a way that it kept the pipe open, rather than letting the ground shock squeeze the pipe closed. We did have some codes that were used to do those kinds of calculations, but they were, I'm afraid, a fairly simplistic look at things. It was as much as anything a matter of timing the closures, rather than any sophisticated effort to minimize or mitigate the flow. It was a matter of how quickly could you get something in the way.

We viewed it as a three part sequence. Very close, within fifty feet of the detonation point, we tried to rely on the energy of the bomb to do the work for us. Then a little further out, but where the line-of-sight would allow it, there were fast acting, high explosive driven systems. And still further out, slower, larger aperture mechanical systems, pneumatically driven. We tried to calculate the times when significant energy pulses might arrive down the pipe so we could try to intercept them. The hope was that we could, if not completely stop them, at least slow them down until what we always regarded as the main mechanism, the ground shock itself, would have a chance to outrace the energy in the pipe and squeeze it off.

Carothers: You said that on Marshmallow there was sandbag stemming. What did you use on Gum Drop?

Weart: I think it was still sand. The early shots all used alternating sand plugs. At first we used sandbags; later we went to sand blown in. But this was not continuous - - there were voids designed to be in the stemming. That came out of some early ideas that Olen Nance had. His concept was to create an interval where the ground shock would not be moving in smoothly and uniformly through a sand-stemmed area. Rather, when it reached the void in the stemmed interval it would implode the wall, create a lot of turbulence, and disrupt the pipe in a more discontinuous way than the more continuous collapse in the stemmed areas. There were observations in some of the early reentries, like Marshmallow and

Gum Drop, which seemed to support this; in the areas where there was no stemming there was much more complete disruption of the line-of-sight pipes than in areas where the stemming was continuous. In the continuously stemmed areas the pipe was squeezed more uniformly, which would leave a tightly squeezed mass of steel, but with little paths through which gases could migrate, and perhaps eventually erode the material to make much larger paths.

And those early designs seemed to work. Whether it was what we did, or just because we were lucky, the early shots were successful; if they had been utter disasters we probably wouldn't have kept on doing it that way. Logan worked well. Marshmallow did have a little seepage out, but not a massive failure; the experiments weren't severely compromised, or anything like that, and Gum Drop, in 1965, worked very well. So, people thought they knew all they needed to know.

But it wasn't too long before we found out that even though you did things exactly the same way, the results weren't always exactly the same. We continued to apply the same techniques we had used for closing the line-of-sight pipe and for stemming the drift itself, but as we began to have more and more of these events, many of them were severe failures. High temperatures and intense radioactivity would get out beyond the stemmed area, beyond the mechanical seals, out to the experiments themselves. And occasionally, even though we would put in things we called gas-seal doors, they were circumvented and some radioactivity was released into the atmosphere. When these kind of events started to occur, people started to wonder, "If the old techniques happened to work all right, what could be different? What can we do to maximize our chance of success, since we obviously aren't optimum."

Carothers: One of the things you could have pointed out to them, Wendell, as a geophysicist, is that the earth is not a nice, homogeneous medium. One place is not like another place, even a rather close by other place.

Weart: That's right. And as we went along, I think that fact took on a great deal of significance to us. We had relied upon the ground shock to provide closure, but we really hadn't tried to optimize that ground shock by finding regions where the seismic velocity would be high, and where we could maintain high pressures from the ground shock out to greater distances. We knew at that

time, from a variety of sources, that you do have higher velocities in some parts of the rocks than in others. For instance, the addition of moisture will change the velocity, and will change the coupling of the energy.

We knew that we would like the ground shock to eventually outdistance the energy within the LOS, and sometimes people envisioned the gaps in the stemming as ways of dissipating the energy in the LOS, and of slowing it down. Later, people built things into the LOS, like mufflers, or enlarged zones, to do the same thing. But the effort was on trying to slow down that energy in the LOS rather than to utilize favorable geology to speed up the ground shock.

Carothers: We're talking about the sixties, or early seventies. What tools, or techniques did you have then to investigate geologic, or geophysical properties? Were there tools available if people had wanted to look at the details of the geologic medium?

Weart: Yes. If there had been sufficient impetus to do it, I think we could have, for instance, determined the seismic velocity in the tuff in the tunnels. The tools were not as easily applied as the ones we have today, but there were techniques for doing it. Those things weren't really applied to containment design in the early days because we really didn't understand in detail what was causing the closure. Because we had some early successes, we just said, "It's working, so we won't worry about it."

As time went on, the experimenters began to impose greater demands. They wanted bigger apertures, which meant bigger pipes that took longer to close, and were harder to close. And they wanted to move experiments in closer and closer. Sometimes these things were in conflict with being able to do the things you'd really like to do to assure the best prospects for containment. I think, in fact, in talking about the Sandia events, Cypress and Camphor, that was one of the biggest changes between those two designs. There was a much larger line-of-sight pipe on Camphor, and an experiment station very close-in which we tried to protect with a massive concrete structure, to hold it open for an interval. And that interval turned out to be an important interval from the standpoint of ground shock closure.

It is interesting to compare Wendell Weart's remarks about the early tunnel shots - - "So, people thought they knew all they needed to know. But it wasn't too long before we found out that even though you did things exactly the same way, the results weren't always exactly the same. We continued to apply the same techniques we had used for closing the line-of-sight pipe and for stemming the drift itself, but as we began to have more and more of these events, many of them were severe failures. High temperatures and intense radioactivity would get out beyond the stemmed area, beyond the mechanical seals, out to the experiments themselves." - - with those of Ed Peterson about events that occured some two decades later.

Peterson: It seems to me that things behave differently now than they did in, say, the Dining Car era in the mid-seventies. There are very small changes in design, but we have seen very large changes in performance. Mighty Oak was the largest, and Misty Rain was pretty large. Huron Landing was somewhat smaller, Miner's Iron was a little bit smaller, and so forth. Yet the design changes were small. If somebody just came up and told me, "This is how we're changing it," I'd say, "It's no big deal. We only guessed at the first one, so how can ten percent kill you?"

But, it appears to, and so, given the science that we all learned in school, one has to ask the question, "Why?" and that is very, very difficult to answer. To me it is as if you plot something versus time, and you were going along flat, and then you see the curve continue to rise as far as things you don't like to see. It hasn't been necessarily a step change, as you would see at a disconinuity; I think it has been a gradual change. But I don't know why the gradual change occurred. We were going flat for so long. And it isn't apparent to me what changes occur with what small design modifications. To go to the extreme, you can talk about the "tired mountain," which would explain things by saying that the structure is just degenerating with time because you've done more and more shots. I am not convinced at all that is what it is.

The DOD sponsored a variey of effects shots after the moratorium, beginning with Hard Hat in 1962. There were cratering events, vertical line-of-sight shots, and Small Boy, the last atmospheric detonation to be conducted at the Test Site. By 1965 the focus was more and more on tunnel events.

Flangas: DNA came into the picture in the middle sixties. They came into the picture with Hard Hat. That was theirs. A few years later they came up to Rainier and made a reconnaissance. I had dug the original N tunnel, and I had dug the original P tunnel, both for Livermore. And, both of them were abandoned. I think I dug N tunnel in about 1963, and then after I finished N tunnel, I went to P tunnel, and took it back about a thousand feet. Then, lo and behold, one day they said, "We're not going to use them." So, we boarded them up, and they were left that way for at least two or three years.

I think it was about 1966 that a colonel came out looking for either to dig himself a tunnel, or find a tunnel, and he wound up in contact with me. I said to him, "I don't know who owns this tunnel, but there is a tunnel that has never been used. It is in a delightful location, and it's a good tunnel." So, I took him into N tunnel. That suited their needs, and whatever arrangements they made between the AEC and the Lab, and the DOD resulted in them taking that over.

The first tunnel events in Rainier Mesa were containment failures. Lacking the base of scientists and engineers that existed at Los Alamos and Livermore, the DOD people doing the events turned to Sandia and the few contractors who could help with the problems of containment and protection of the experiments placed in the tunnels.

LaComb: At first it was General Atomics people, and ultimately those people became S-Cubed, who were saying that there should be this particular kind of grout here, and that one there. On Door Mist (8/31/67), and after Door Mist, they were asking for very high strength grout, which wasn't a good thing to do. Door Mist was not real successful.

Carothers: What led you to focus on the high strength grout as the problem?

LaComb: The reentry observations indicated that, to a degree, we did have a solid tunnel plug, but the leak path went out fourteen feet into the tuff, around the plug, and back into the tunnel. We're not sure what drove that, but we felt we'd have been better off if we could have kept it in the tunnel rather than forcing it out of the tunnel.

Midi Mist, in June of '67, was done with rock-matching grout. That is a misnomer, because that grout is intentionally designed not to match the rock. It's called rock-matching grout, but we've set criteria where it should have a compressional velocity lower than the tuff, it should have a strength lower than the surrounding tuff, and it should have a density which matched the rock as closely as possible, but hopefully not higher. What we wanted to do, in theory, was to make the ground shock going out from the zero room go slower in the tunnel than it was in the rocks, so the shock was driving in on the tunnel, and slamming the pipe in the tunnel closed. It's probably not a bad theory.

Carothers: Dan, to what extent do you get involved in specifying the kinds of grouts, or over what length there should be rock matching grout, or superlean grout, or whatever?

Patch: We like to think we play a fairly important role in that. We certainly have looked at the effects of changing the lengths of the grouts, and we've made recommendations based on what we've seen in the calculations as to whether a grout should be stronger or weaker. We have tried to work with the folks at the Waterways Experiment Station as closely as we can to understand how they formulate grouts. We don't do the formulation in the sense that we don't say how much of what to put into something, because we would be way over our heads there. In a way we don't really work directly with WES in terms of formulations. We'll talk to Byron Ristvet, or Joe LaComb, and say, "For this kind of geometry we think we need a stronger grout in this particular section, because it will help relieve loads," or whatever the criteria and reasons are.

Then, it's been Joe primarily who has had the most direct role in the grout formulation area. He'll go talk to the WES folks and say, "These crazy calculators want something that will do these strange things. What can you guys do?" They'll think about it, and they're very, very good at knowing how all these ingredients interact with each other. One of the problems we do have is that we're looking at how these materials respond at many kilobars, and the formulators are civil engineers and concrete engineers who tend to think about how bridges would react, and what one would do to make a pedestal stronger, or whatever. They bring a much more engineering structural point of view, and we really have to try hard to overcome that different point of view.

Carothers: Each time there's a presentation of a DNA shot at the CEP it seems that the boundaries of the different grouts, and the placement of the hardware are different. This run of grout is a little longer, but not much, and that one is a little shorter, but not much. There seems to be a lot of fine tuning.

Patch: There is a lot of fine-tuning, and I think there's two reasons for that. One of the things that's going on there is in some sense operational. For instance, people may want to put bulkheads at certain places, but because the tunnel has some change in it, it's undesirable to do that from a construction point of view. Things tend to move around for that reason. Again, folks may want to move something a significant distance, so they will call up and say, "We were planning to put the superlean out to X range, but it would be really nice if we could make it five or six feet longer. Do you think this is a problem?" And we'll either say, "That couldn't possibly make any difference," or we'll say, "Well, we don't know. We'd better look at that, because we think it's a little long right now." We run into things like that, where people have wanted to make things a little longer, and we thought were kind of on the long side already, or vice versa.

Carothers: Joe, what kind of consideration was given to the front end of the pipe, where the energy began to get into the pipe?

LaComb: Actually, I'm not sure the front end for Double Play (6/15/66) was ever calculated, although Noyer kept wanting to go back and do it. I think the General Atomic folks tried to calculate Door Mist, and Chuck Dismukes came into the picture then. I think it was about the Midi Mist, Door Mist time frame that the front end calculations started coming in.

Carothers: Were you putting overburden plugs on all of these shots?

LaComb: Yes. In those days it was called a blast plug. I guess there's a difference.

Carothers: Perhaps it represents your expectations, you might say.

LaComb: We were quite successful for a little while. We never did really come up with a good explanation why we had Mint Leaf (5/5/70). It was another gross failure, and it had another leak over the top of the far-out TAPS. It's interesting that the cross sectional

area of that path was the same as the one on Door Mist. That's why Ed Peterson uses eleven square feet when he calculates a leak from the cavity.

Hudson Moon (5/26/70) was not as bad in the tunnel as Double Play or Door Mist was. Door Mist was a step beyond Hudson Moon. Hudson Moon had all the lagging charred, and it didn't have it's strength, but it was still in place rather than being completely gone. The DBS, the debris barrier system, which we had added to the pipe string to be a barrier, did a good job of protecting the samples that were in the test chamber. They got more of a soak temperature than anything straight down the pipe. We were pretty lucky there, because the leak path went outside the pipe. The pipe was closed off by the debris barrier system, so it was kind of a cocoon for the samples.

The Hudson Moon rock samples had been very soft, and they had a very high gas-filled porosity. At the time we tested them, we said they'd sat at the portal during an extremely cold spell and they'd frozen. So, we wrote the physical property tests off because the rocks had frozen. After the test, when we went back into the tunnel and started to investigate it, we came to the conclusion that maybe the measurements were right. They might be real. So, then we went back and started digging further into the Door Mist physical property data. We found that there also was a lot of gas-filled voids there. And the longitudinal velocity in both tunnels was low. Then we started doing some calculations, and we found there is a significant difference in the ground shock attenuation between one percent and five percent gas-filled porosity. So, we then attributed the Hudson Moon failure to the gas-filled porosity.

Duff: Bob Bjork did a series of 1-D calculations of ground shock propagation, and he rather dramatically showed the influence of air-filled porosity on shock wave attenuation. These calculations were based on naively simple material models, but they showed us that if you compare the attenuation for one or two percent air voids with zero air voids, there is quite a difference. If you go to five percent air voids, you get a little more attenuation. If you go to fifteen percent air voids, a little more attenuation. It's the first few percent that makes the big difference. So in the context of the Hudson Moon failure, we hypothesized that what we had there was a

relatively dry medium, such that the ground shock, which had been expected to squeeze the tunnel and develop a stemming plug, simply died. It got too weak too soon.

Then we did a pair of thousand pound HE shots in two media. One was in a fairly saturated medium, and the other was in a fairly dry, Hudson Moon-type medium. And, indeed, they confirmed the validity of the prediction. That has influenced DNA's thinking about appropriate material properties ever since.

Peterson: After the Hudson Moon leak, one of the things that was recognized to be different about Hudson Moon was that it had a high gas-void content. With a material with a high gas-void content, the ground shock damps out fairly quickly, and so one doesn't get the closure that one would expect for an event in a low gas-void material. So, this was pinpointed as one of the reasons for Hudson Moon.

And, this has been the philosophy for a long time - - you do not want a high air-void material. Let me give you two contrasting things, which show why our lack of understanding bothers people like me. There are people who say, and they may be right, that one of the reasons, or at least one of the contributors to the Mighty Oak situation is that it was shot in a material with a very low air-void content. As a result, the ground shock was too strong, and it drove the stemming too hard. So, it drove it right through the closures. You can keep going on with happened from there.

If you look at Mission Cyber, the response you saw was that the peak stress versus range was low. There's a lot of evidence that it didn't come from having too low an air void, but the response on Mission Cyber was similar to what you'd calculate if you just put a lot of air void in the material. It wasn't there, but the response looks similar. And on Mission Cyber that worked great. It didn't crunch anything, and everything was just perfect. So people say, "Well, you know, maybe some of this air void is really okay."

So, you can go from the extreme of people thinking there was too much air void to the point where they think there was too little, and and now maybe a little bit more is better. The history has gone back and forth, and I'm not sure what the answer is.

LaComb: Misty North (5/2/72) was where we first said we would test the overburden plug, and we said we would pressure test the gas seal door. That's where the gas seal plug came into being, We called it the hasty plug for years, because we couldn't get the gas seal door to seal. The concrete had enough permeability that there was always a leak. Finally I said we'd put in another plug. I walked down the tunnel, looking through the lagging, and said, "Put it right here." Three days and twenty hours later I was watching the concrete go into the forms. We could have shot at any time, because the area was so full of people it couldn't have leaked. That's where the first gas seal plug came into being. We leak checked it, and we pressurized between the plug and the door.

It was also the first time we used cable gas blocks We had a block of concrete that was the world's most expensive. There were over a thousand cable gas blocks in it, and the cost was well over a million dollars, and those were big dollars.

Carothers: This was the first time you had done cable gas blocks?

LaComb: Well, we'd been fooling around with cable gas blocking for about two years. We weren't very sophisticated, but we knew how to do it if we had to. We didn't have to go out and start inventing the wheel. And we still use the same technique today. We don't have quite as crude an installation, but it's basically the same. Now we use the bulkhead connectors, but we don't use a board any more because we put them in Vistinex. And we angle them so if there is a leak it will just go out and come up some different conduit.

Carothers: If you had cable holes to the mesa surface you also had to think about stemming those, and the cables in them to keep gases from getting out.

LaComb: The cable holes were stemmed, but we never claimed they were gas tight. We had to go to some extremes to take care of them, because we had to do it a little differently in every tunnel. On N tunnel we put a top hat on the top of the hole, with bulkhead connectors. We squeezed grout and sand down in the hole to get rid of the boundary leaks.. As I recall, in P tunnel we put the bulkhead connectors at the bottom of the hole. In T tunnel we blocked the cables coming out of the downhole cable alcove, and put plugs in the access drifts to the cable alcove. Each tunnel was

unique in its configuration. We could have said, "Well, we're just going to drill these holes out, clean them out, and put in new cables and do it right." Or you can try to save your investment, which is what we did.

Carothers: You obviously wanted to reuse the tunnels. On shots where you had leaks into the tunnel to what extent could you go back and use them again?

LaComb: Well, for Double Play, once we ventilated the tunnel inside the gas seal plug, we really lost very little, except inside the overburden plug. The leak was minimal. We did have to go through and spray the lagging to tie down the dust that was generated when the grout was scoured out. Other than that we pretty much had the run of the tunnel within a couple of months. Door Mist, we lost everything inside the overburden plug, but outside the overburden plug, because the nature of the leak was just a seep, the tunnel cleaned up very well. And, so did Hudson Moon. That was the advantage of having the blast plug in the experimental drift; we were attenuating that release up-front, close-in. Of course, there we were providing any release with a very small volume to dump into, so you could expect the pressures to be high. On Hudson Moon we saw 700 psi on the front of the overburden plug. But at the same time, the advantage of that was that it did save the rest of tunnel complex.

On Mighty Oak, where we had the plugs way out, we lost just about all of the T tunnel complex. If we ever reuse some of those openings, it will be a bunch of years. Outside the drift protection plug though, we have full use of the tunnel for Mission Ghost, Anything inside of there is lost. All the Diamond Skulls workings, the Mint Leaf Workings, the Midas Myth workings - - all of that is lost.

Carothers: You folks in DNA are have a real need to protect the experiments, and you've done a lot of different research projects. Have they all been for better ways to protect the experiments?

LaComb: I think you're oversimplifying to a degree, because one of the drivers for our low yield test program was real estate, and facilities reuse for the economics. A low yield test - - two, or one kilotons, and we're hoping for a half a kiloton - - doesn't have near as much ground shock associated with it. So you spend a lot less

dollars hardening, a lot less dollars shock mounting. You use up, for a half kiloton, compared to ten kilotons, only a fraction of the real estate. Real estate in Rainier Mesa is disappearing, so that's been a big driver in the development of the low yield test bed. Of course, the need for the coupling experiments, like Misty Echo, Mill Yard, and Mini Jade , and the stigma associated with Red Hot have also driven our program. We've got to work the program so we're able somehow to do that kind of test. So, a lot of research has been driven by the need to understand the phenomenology of the events.

Carothers: Bruce, what drove your line-of-sight diameters, which affects the pipe taper and its length? Was it the size of the hardware that people brought for exposure, or did it happen the other way; "We're going to have a shot, it's going to have an exposure area this big, and what have you got?"

Wheeler: I think there was some of both in the early seventies. Primarily it was driven by military system requirements, the Defense Department stuff. The size of, and the number of test chambers was driven by the number of experiments there were; the need for space. The need for sheltons was the way we quantified it. A shelton was a calorie per square centimeter, and was a unit of barter. Many times experiments were not approved to be on a test because there wasn't any room. That could be a reason, and another reason could be the experimenter hadn't done his homework well enough. But space was always at a premium. However big the exposure space was, it was always fully subscribed, as all the DNA tests have been. We used to talk about having a physics event about every third or fourth shot to let the experimental physicists and experimenters play with it, and do phenomenology, and physics.

Carothers: They still would have wanted a lot of space on the next shot.

Wheeler: True. And, we never would have gotten a shot like that funded, because we didn't have any system driving it.

Carothers: Were you getting participation from all three services?

Wheeler: Pretty much, yes. As I recall, the Army participated the least, probably because they didn't have systems other than the Spartan and the Sprint. They didn't happen to have a system that required that kind of testing. The Air Force was always there with

ICBM missile parts and materials. The Navy was there because of their Polaris program, and there was always a lot of phenomenology and materials effects experiments by contractors. And of course Sandia, who developed components for weapons, was a big participant.

Carothers: Carl, you came to DNA in 1974. Perhaps this was an issue that arose before you got there, and had reached some conclusion. That was the question of what kind of tuff should you shoot in. What should the porosity be, for example. Also there had been a lot of fussing around with various kinds of grouts such as superlean, and rock-matching, and so on.

Keller: Yes, before I got to DNA they had already concluded that the rock needed to be saturated to give you the strongest possible ground shock to the greatest range, and that the stemming had to be as weak as necessary to allow closure of the LOS pipe as far as possible. Now, those generalizations eventually led to, I believe, some serious stemming failures. It was true that they tried to get the longest stemmed tunnel by maximizing the ground shock and minimizing the grout strength.

The trouble with that concept was that you also, by reducing the grout strength, suffered a lot of relief with grout extrusion into this large pipe volume. So, it had no confining stress, and therefore no strength, and it just had a ballistic trajectory. That was first dramatically demonstrated on Hybla Fair, which was David Oakley's attempt to push the state of the art. They overshot quite a bit. That was a seventy-six foot long LOS pipe; it diverged to something like five feet at the end, and there were no closures in it. The hope was that there would be a ground shock stemming closure of the whole pipe.

We did a parameter study with calculational models at Pac Tech after that shot, and found that there's a very strong correlation between the pipe taper and the amount of extrusion that you suffer. According to the calculations, if you doubled the pipe taper you started to see a small effect. If you went to four times the normal pipe taper, you had a very dramatic effect. At five times the normal pipe taper you just lost it completely. That was a calculational parameter study that was done as part of the design of the low-yield test concept.

Hybla Fair was premature, and there was talk about how it might have actually killed the low-yield test concept, because it had blown out so badly into the tunnel. But in fact, a couple of years thereafter we dared to offer to pursue that, and we were allowed to when funding was available. The intermediate tests were scaled model tests done by Sandia. They put in the low-yield test design and the Hybla Fair design side-by-side, and drove them with high explosives. Those were scaled models which showed that Hybla Fair failed, but the low-yield test concept didn't. The low-yield test concept went all that way, and that was the only test design I know of that ever evolved all the way from calculations, up through scale-model tests, finally to nuclear proof-tests, and then to a follow-up nuclear test.

After the FAC, the Fast Acting Closure, was developed by Sandia we were sure we could go to double the normal pipe taper easily, because the scaled models that were tested were at that taper. Midnight Zephyr tested out that concept, and the proof of that design was Diamond Ace, which had just a short part of the pipe. Diamond Beech finally tested the whole thing. And then, just about at that time Misty Rain and Mighty Oak occurred. The low-yield test concept was then the only concept left in which DNA had any faith. That test concept has been used many times since then. So, that was an application of our calculational models, and our experimental program, all the way from the smallest charges on up through nuclear scale.

The low-yield concept was designed from scratch, whereas the traditional LOS designs were developed in the field. The early tests had difficulties, and the designs evolved very timidly. Of course, they were all tested on the nuclear scale, where you didn't dare fail, and so the standard HLOS design came about through timid evolution in the field. And they started long before the calculational models could treat the whole problem. Eventually the standard design, as the product of that timid evolution, was proven not to have the margin of safety that we'd become to believe.

Carothers: Well, DNA had been pretty successful with those line-of-sight experiments for a few years. There were a series of events where they worked.

Keller: Yes. There were some puzzles as to why the variations occurred that did occur. It was never clear, at that time, what were cause and effect situations. When we did HE tests at Physics International where we were imploding pipes, we found that there was a fairly strong variation in the standard unperturbed pipe in those geometries. You had to have a major reduction of the flow in the pipe before you could depend on it. I believe the analogy with the nuclear experience is valid, because there we saw also variations that we couldn't explain.

In fact, there were a few wagers. I remember that Dan Patch bet two six-packs that Diablo Hawk would have a much more docile behavior than Mighty Epic. Well, it shot out the doors and Mighty Epic didn't. It wasn't bad though, and it was still well contained. The next event was Misty Rain, and on that one, because the doors were in closer, the pipe taper was larger, and there were a few other things like that, Dan was sure that it was going to be a lot worse than Diablo Hawk. And he lost again, because the doors held. Things like that were really puzzling.

Weart: In the days when I was involved, almost all of the experiments, while they were emplaced underground, were recorded on the surface, or outside at the portal. But as time went on, more and more of the recording and the data acquisition began to take place within the tunnel itself. Faster recording times were desired, and there were cost efficiencies, and so forth. That made an even greater premium on not letting any release out into the part of the tunnel where the equipment was.

Carothers: When did you leave the containment business, and go on to other things?

Weart: My last involvement was probably in the '74, '75 time frame.

Carothers: By then a lot of things had been done to try to insure that there was no release of radioactive material. What changes were made after Baneberry?

Weart: Well, there were two kinds of changes. One involved the engineered hardware - - building more massive, faster acting closures, trying to get things across the line-of-sight pipe as quickly

as you could. Sandia has done a lot in terms of building big, fast-acting closures for the DNA shots, for large diameter lines-of-sight. They have also done some HE closure work.

We also did a lot of work, along with DNA, in trying to insure that the last line of defense, the overburden plugs, the gas-seal doors, really would provide effective seals against high temperature gases. We did a lot of work with Chuck Gulick, who worked for Sandia, and we also worked with Waterways Experiment Station to try and design cements which, for instance, were expansive, and which would form a more positive seal against the rock. We did quite a lot of work in that area.

The other advance that I think was made was in being able to better understand and calculate the behavior of the various interacting energy streams, such as the ground shock, and the pipe energy. There was also a major effort instrumenting those events to try to confirm whether or not our calculations were representing reality.

Carothers: I think that's an area where the tunnel events have had an advantage, in that there is access. My impression is that there was always a fair amount of instrumentation in the tunnels, looking at the tunnel behavior and the medium behavior.

Weart: There were certainly advantages in the kind of things you could do. The geometry afforded you a way of assuring that the instruments lasted long enough to get the data out, because you didn't have to be right in the drill hole along the line-of-sight pipe. So, ground motion measurements, free-field motions, energy flow down the pipe using things like slifers, were much easier to do in tunnel shots. We did try to do those things in the vertical LOS shots, but it just wasn't as easy or as certain.

Smith: I had been involved in the DNA shots to the extent that I would design the stress gauges for Bass, and help field them. C. Wayne Cook did the recording of the data, and Bass would reduce the data. All through those years I had my fingers in measurements on DNA shots; principally the free-field stuff. Bass did the work on the pipe, the pipe flow, and I was never involved in that. So, when Bass retired, and G tunnel closed I just moved into the free-field portion of his work, and Tom Bergstress took over the pipe flow work. Of course, Bass is still the Grand Master of all that sort of

work. And so my work for the last few years has been more or less on the DNA shots, the free-field measurements of stresses and motions.

The original driver for that work in the intermediate regime was, "What sort of stresses do we have loading these containment structures?" That has broadened, now that those things are fairly well known, into a number of things. One of them is failure diagnostics. In case something happens, what measurements do we have that would let us go back and assess what actually happened? What was the pressure and temperature in a certain portion of the pipe when the thing blew out?

The other sort of measurements are for trying to understand what happens around the FAC. In other words, what are the stresses and pressures in front of the FAC, and what is the interaction of the ground shock with the stemming and the rock right around it. So, the attempt is to measure those things, and to try to get a good enough understanding of them so you get a good feel for why that system works, and works fairly well. It's something that has evolved, and it now seems to be a good system, but the community still doesn't think it has a good feel for what the forces are that load the FAC after the bomb goes.

DNA still has problems with gases that come trickling out into the drift complex; there are late time leaks in there, and they would very much like to know how long there are residual stresses loading that portion of the stemming. So, it's the interaction of those things that some of those measurements are used for now.

Carothers: Wendell, was this type of information useful to you? Was there a clear enough understanding of what was going on that you could say, "Look, see what's happening here? It shows us this, and so we should make this change."

Weart: Well, we clearly used it. Some of it was more immediately useful than others. Ground shock data, for instance, was fairly easy to interpret, and it told us about how far out we could expect high enough stresses to really squeeze down LOS systems, and things of that sort. And we had shock velocities, which were easily obtained, and easily used. Energy in the pipe? Not quite as easy to interpret, because of the difficulties of getting measurements that weren't ambiguous.

The easiest measurements to get were times of arrival, and those you could usually get. But the relative magnitude of those energies in the pipe compared to the ground shock energy was more a matter of an active imagination than actual factual interpretation, early on. But it was useful. It was useful in the sense that we could tell in some pipes that the energy was just far outdistancing the ground shock, and therefore we ought to try and do something to slow it down, and minimize it. While we couldn't get a good handle on the energy levels, we could get a good handle on times of arrival, and that led to lots of schemes to try and do things within the pipe structure itself to slow this energy down ; things like mufflers, baffles, helixes, and so forth.

Carothers: You mentioned Baneberry as the event which brought to everyone's attention the importance of the details of the geology around the working point. But you know, Wendell, if I wanted to be a cynic I could say, "You guys didn't learn anything in the six months between Baneberry and when you started to shoot tunnel shots again, so what was different? You didn't have any new knowledge. All you had was somebody pointing his finger at you and saying, 'You better not !'" So what happened?

Weart: Well, I think there was a concerted effort on the part of DNA to locate their tunnel events in tuff which had a high sonic velocity. And so there was an effort to select locations which would be on the favorable side of that particular aspect. Areas which clearly had high gas-filled porosity, which might lead to, or at least were often associated with, lower velocities were avoided. And since this had always been one of the factors that had been primarily responsible, that was a step in the right direction.

There were changes in the backfill, things like the specially designed grouts which would transmit the shock well, but which had weak strengths so they would flow easily, and not resist the closure of the pipe. There were changes like that which were made in that time frame, after Baneberry. I don't know that there was any one event when all of these things came to be applied at the same time. It was sort of an evolution.

I think it's clearly true that was when the major changes came. And there were changes in how the line-of-sight pipe itself was designed, but it was never clear, at least to me, what role those changes played in the successes.

Carothers: Do you think the better containment was principally due to the attention to the geology and stemming, or do you think it was the the fast closures, and the valves, and so on?

Weart: I tend to think it's not the engineering features that makes a containment success. In my view, if you need those things, in part you've really failed. They may succeed in providing protection for the experiments, so from the DNA standpoint they're essential, and I guess there have been instances where they have made the difference in a successful experiment. I really can't judge what improvements in later years have done; I'm just not familiar with what has gone on recently.

Carothers: Well, there were changes that were made following Baneberry that really improved things. If you compare the two or three years after Baneberry with the two or three years before, there's a striking difference, both in the tunnels and with the events in the drilled holes.

Weart: Yes, and I think those successes were probably not due, in large part, to the mechanical hardware, from what I can recall. When you had a success you would go back in, and you would find that significant amounts of radioactivity, of molten material didn't reach those features. If significant amounts of energy did reach the features, they often weren't successful. So, you really need to do your containment, and I would say ninety percent of it, before you get to those features.

Carothers: And that you do with the energy of the device itself, and to use that energy properly you select your geology properly.

Weart: That's right.

Carothers: Why didn't we understand that in sixties? Was it that it wasn't important enough?

Weart: I think it's human nature. We had had a couple of successes, and so we said, "It's working, why change anything? We know enough."

Carothers: Well, you can always blame it on the management. You might go and say, "We really should understand this better," and get the response, "Why should I spend money on that, Wendell? You're doing fine. Keep up the good work."

Weart: Well, it's funny. People did continue to support measurements. We always had active measurement programs on those tunnel shots, even though things seemed to have worked okay. So, people were trying to learn a little more. It may have been in part fear that, because we did understand so little, we were reluctant to make a change that we thought might be right, but maybe it wasn't. We didn't have the understanding to say that. It looks so obvious today to say that yes, this is going to have advantageous aspects. In those days there were people who probably argued strong ground shocks are bad. We just had not examined the phenomenon enough to have a good enough understanding to take a chance on something that was quite different. When it became clear that the old ways weren't good enough, then nobody minded taking the chances.

Carothers: Byron, the DNA, for the last twenty or more years, has sponsored a variety of containment related experiments, calculations, and measurements. More so, I think, than either Los Alamos or Livermore.

Ristvet: Yes. It had to do with there being a different philosophy. The Labs, since they got out of the vertical LOS business, learned how to do what they wanted to do without bringing the pipe to the surface And they also turned over, in some cases, the re-entry vehicle testing they used to do on some of those vertical shots, to DNA. Their concerns were different, and they were much less concerned with sample protection. DNA's research program has been driven by experiment protection and equipment protection, and also trying to preserve the tunnel complex, because that's a valuable resource.

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Pipe Closure Hardware

An integral part of the sample protection and containment design of line-of-sight pipes has been the installation of various massive pieces of hardware, designed to impede or stop the flow of material down the pipe after the detonation. Sandia has done extensive engineering and test work in the development of the various closure devices.

Wheeler: The first of what we called an auxiliary closure was prototyped and built by Sandia for DNA. We called them auxiliary closures because we took the ground shock to be the main pipe closure mechanism.

That was about 1972, after the DNA fast-door blew up, and didn't work when it was tested.. Lockheed Shipyard, in Seattle, was building a big steel contraption to close off the line-of-sight very rapidly. It was a big housing with two opposing doors on parallel tracks. They first obscured the line-of-sight, and then closed flat and sealed the whole area, the whole aperture. They drove it explosively, to get the closure time they wanted. I don't know whether somebody miscalculated, or whether they didn't understand what they were using, but as I recall they used something in excess of forty pounds of bulls-eye pistol powder to try to close these doors. When they tested it, it wasn't surrounded by concrete, or the earth, or anything else, and it just blew all to hell. That was the death of that program.

At that time Sandia came along and said, "We can provide you with doors that will do almost everything you want done." And they did. And in a number of ways Sandia has continued to be a great contributor to the horizontal tests, particularly in the closure mechanisms.

Carothers: Did they receive DNA funds for that, or was that something they did within their own Laboratory?

Wheeler: I think the first that was built they did within their own Laboratory, and they asked DNA if they could install it on the event to test it. That was a significant thing, because it allowed us

to get away from the old explosively-driven debris barrier system — a high explosive machine which created a lot of shrapnel, and sometimes tore up a lot of the experiments. Certainly the explosive products didn't help the line-of-sight any. So, those fast gates were a significant contribution that Sandia made.

Broyles: We designed the basic concept of the sliding doors for Cypress, in '68, and repeated it for Camphor, and continued the development effort on those things until the mid-seventies. We then concluded it wasn't likely we were going to go back to that, because Sandia wasn't sponsoring more shots. We had essentially disbanded that group when DNA came, with a letter from their top person, asking us to please use our unique capabilities to support their program. So, we reactivated the group, and have been essentially designing the hardware for DNA tests ever since, and continuing to make improved versions of that hardware. Jerry Kennedy's department has had that responsibility.

Carothers: I've always thought those various closures were very impressive things. So much moves so fast.

Broyles: Yes. And you should remember that those designs from the beginning were to be debris stoppers. Any absolute late-time containment of gases was a benefit. Somewhere along the way somebody decided that instead of having this big TAPS (Tunnel and Pipe Seal), which we still have for the DNA tests, you could save money, millions of dollars, if you could really make the second closure a gas seal. So that led to redesigning to incorporate a positive gas seal in that closure. Several of those, called the Gas Seal Auxiliary Closure, or GSAC, have been fielded, but they still encounter new problems each time.

People still don't have a very scientific basis for what the strength of those sliding doors should be. Some number like fifteen thousand psi was sort of the static containment pressure strength that they came up with. It was more maybe from the fact that that's what you could build, but you could make some arguments that led to numbers of that order. The real thing was to get a lot of mass.

Now there's a big effort going on to improve that design. That door is a twelve-inch thick forging, hollowed out for weight. Essentially you have a bridge truss for strength, and a certain

thickness to stop projectiles. All of those designs were still envisioned as backups for the primary closure, which was still to be the ground shock.

On the newer test designs, where instead of just those fast gates, there is the HE closure, the FAC, or Fast Acting Closure, which is a much more substantial block, much closer in. I think that has much more direct influence on the containment per se than the other hardware.

Bass: I'm very proud of the FAC, because I was one of the two designers of it. That was a perfect marriage between experiment and calculation. I did the theoretical calculation work - - the two dimensional calculations - - on the FAC. At the same time Paul Cooper did high explosive simulations at tenth scale. We operated absolutely separately, except we started from the same principles, and we had certain ground rules to go by. We compared our results on a Christmas Eve afternoon. We both went home and thought we had a Christmas present, because they had cut into the plug left by the latest simulation firing, and every single place that the calculations had predicted a failure in the spool, they were shown in the explosive test. You could see every crack, every single rebound, any spallation was duplicated. Everything was exactly the same between the calculations and the experiment. We immediately dropped scale model testing and went to full scale test. We estimated to DOE that we saved one to two million dollars by this jump.

One thing it did cause us to do was to turn the detonation point around because we saw we had a weak point. DNA wanted it detonated on the working point end, and we said, "No, because you're putting a very weak structure there, and you're spalling things back at the bulkhead end, so where's the stopper?" So we turned around and detonated on the portal end, coming forward, and then used that as a basis to allow us to make an ogive front end. This was all done calculationally and experimentally in parallel, and I considered that my greatest triumph in calculations. You can do marvelous things with hydro codes if you're lucky.

Keller: The FAC, the fast acting closure, the thirty-inch HE machine, was developed as part of that low-yield test design. The concept was that you would not try to close the pipe where it was so large, because once you closed it, if the grout didn't come to rest, or wasn't confined, it just flowed on down the LOS pipe and you lost

it. The concept was to build the big end of the pipe so strong that you couldn't lose it — a hardened pipe section is what it was called. And, near the working point where the pipe was small, you put in a relatively strong grout and swaged it with the very high ground shock that you had that close-in. So, you developed a short, high quality closure, that plugged the LOS pipe which closed in a millisecond. That served as an absolute plug, so you could not extrude the grout through that hole. And so, as long as the HE machine was closed, and the hardened pipe structure was intact, you had a competent system.

Sandia did the scale model tests. We specified what geometry we wanted, and they built and fielded the scale model tests for the low-yield test concept. They also built our MAC's and the FAC according to our specifications. They did probably a hundred half-scale and fifth-scale HE tests, during the evolution of the FAC. If we'd had to pay the full price of those, at a contractor, it would have added a lot to our budget.

Carothers: Dan, do you get involved in location of the big mechanical closures? Do you do calculations of the stresses you expect them to see?

Patch: Oh yes. That's a very important part of what we're doing. In a way that's almost the central part. Another aspect of that is we really think a lot about what an appropriate piece of hardware is, and where should it go in the pipe string. Sometimes we run into a situation where we really need to have a closure, and it's up to us, working with Joe LaComb and Byron Ristvet to say, "This pipe string is not going to be safe unless we have a closure here, here, and here." If we don't have a closure that will fit at those places, then we either have to take one off the shelf, move it till it fits, and then see if it can stand the loads there, and it may not. If that's the case, then we really try to be closely involved in saying, "These are the performance criteria that we need for new closures."

We've talked with Sandia for many years about their closure design program. For example, this Fast Acting Closure that we see all the time on the low yield shots; we really were the ones that said such a device was needed, and kind of ballparked what the specs ought to be. Sandia folks thought about how they would go about making such a thing, and did the engineering analysis, which was a substantial job. We did the 2-D design calculations, so when they

said, "We need a spool that's about so thick," we took a look at their design and said, "Yeah, you're going to have to put so much HE on the outside, because it's going to close on this kind of a time scale." They took that information and went to small scale, and tuned it up and made it work. They carried the lion's share, but we worked back and forth interactively on what was needed, how it worked, and how to really build the thing.

Carothers: When they wanted bigger pipe tapers, they had to move the hardware in closer because the opening in the doors had a certain diameter, and you had to move the system forward to where it fit the pipe.

Patch: Mechanically, that's what you have to do, but if you do that the risk to the hardware goes up almost exponentially as you move in, depending on what the threat is.

Carothers: But they did do that, because they were going to bigger pipe tapers.

Patch: They did do that, but now they've moved things back. But it's different hardware too, with this Fast Acting Closure machine, which is very different than the gate closures, in some respects at least. It closes in a millisecond, which is a factor of thirty times faster than the gates. That's not so germane to it's survival, but it's just one big slug of material that gets in the way, as opposed to the gates, which are more of a diaphragm configuration. Sandia has done a lot of work in the last couple of years to really bring up the strength of those gate doors. Of course, they've worked on that for many, many years, but I think what they've done recently is going in the right direction.

Carothers: It seems that the hardware now is going in the direction of brute forcing the problem. They're trying to make the hardware so strong that it will survive whatever it sees, much like the overburden plugs.

Patch: Yes. But there's a lot of finesse that may not be obvious, and that comes in getting this big, brutal piece of hardware in the way without giving up the timing. One can easily put more stuff in the way, but it's not so easy to get it in the way on the right time scale. These machines are fairly sophisticated in the design. They're going to the very limits of the materials.

Carothers: When you talk about the timing, are you talking about the material coming down the pipe, or are you talking about the collapse of the pipe.

Patch: We're really talking about the collapse of the pipe. The gates are too slow to catch the front end of stuff that comes down the pipe. They may be able to catch the back part of it. In cases where we've apparently had too much pipe flow you can see it interact with the doors, in terms of slowing them down. So, they're catching the back part of the flow, and that's the more threatening part, in my mind, because it seems to be more massive, more capable of really loading things. The first stuff that comes down is, I think, a pretty faint wisp. It's very energetic material, but it's very low density. I suspect it dissipates and plates itself out, literally, inside the pipe as it goes down the pipe.

Kennedy: The debris barrier system had gates that set parallel to the walls of the pipe, inside the pipe, so they were curved. They were explosively driven to close. They had interlocking fingers, but sometimes they just went on through instead of locking. They didn't work very well, and sometimes they made shrapnel that damaged the experiments. I'm not sure who designed them — whether it was Lockheed, or DNA in conjunction with Lockheed.

DNA also uses a closure that was designed by Lockheed that is called the TAPS, the tunnel and pipe seal, which is supposed to be a late time gas seal. This is a great big toilet seat cover like thing, where the cover is latched up, and at zero time is dropped by gravity to slam closed. It's very slow; it takes of the order of a second to close, so any fast debris is long gone before it latches. Sometimes it hasn't latched and sealed because some of the debris which had gotten there was deposited on the seat, or it didn't fall all the way down.

One closure we developed and used on Cypress was an HE driven vertical closure. The gate was put up above the line of sight. Being that it was explosive driven, it came down like a guillotine at a pretty high speed, and seated at the bottom. I don't remember the exact closure time. We built and tested that at Oak Ridge. It was a huge, massive gate. It was an immense monster of a thing.

About that time, the group then working for Howard Viney started designing these gates that were driven horizontally so they overlaid each other. They were fast acting gates, HE driven. Then

they decided that you could do that more safely by driving them with high pressure gas, rather than HE. You could regulate the pressure, it had lots of safety features, and you didn't have to have quantities of explosives around. That design was all Sandia's, and we paid for a lot of it ourselves, because it was for our own test, Camphor. DNA was very interested in those gates, and we started providing them for their tests too. They started kicking in funding to help our level of design effort for those closures.

Those gates have continued to be developed to this day. Each of the doors in current years is about a foot thick, and weighs about five thousand pounds, even with all of the holes that are drilled in them to lighten them up, while you try to maintain structural strength. These gates come in various sizes, but they are usually designed for either a 60 or 72 inch diameter pipe. They obscure the line of sight in about 17 milliseconds.

Carothers: That's a thing that has always impressed me. Here are these big, massive pieces of hardware, and they work as fast as a camera shutter.

Kennedy: John Weydert, who was one of our great designers of these things loved to say, "If you stood 20 feet or so on the other side of the door, and you aimed your 45 at me and pulled the trigger, and I pulled the trigger on the doors at the same time, I'd be safe." The doors would close before the bullet got there. And he also likened the problem of stopping them to taking a Cadillac at a hundred and fifty miles an hour and trying to stop it in about six inches without damaging it. Starting them was a lot easier than stopping them, it turned out. It was a real problem, absorbing all that energy, and decelerating those things, and making them stop where you wanted them to instead of either going on to China, or rebounding. Either way is bad. That was really the hard part of the design, absorbing that energy, and having them stop in closed position. So, 17 milliseconds is when they overlap, and it's around 30 milliseconds for a complete closure.

Carothers: And you had those on Camphor?

Kennedy: Yes. That was about the first time they were used.

The thing in the history of the development of the fast closures that always stood out to me was the fact that we did provide those for DNA. They did help fund them. We jointly funded a lot of the

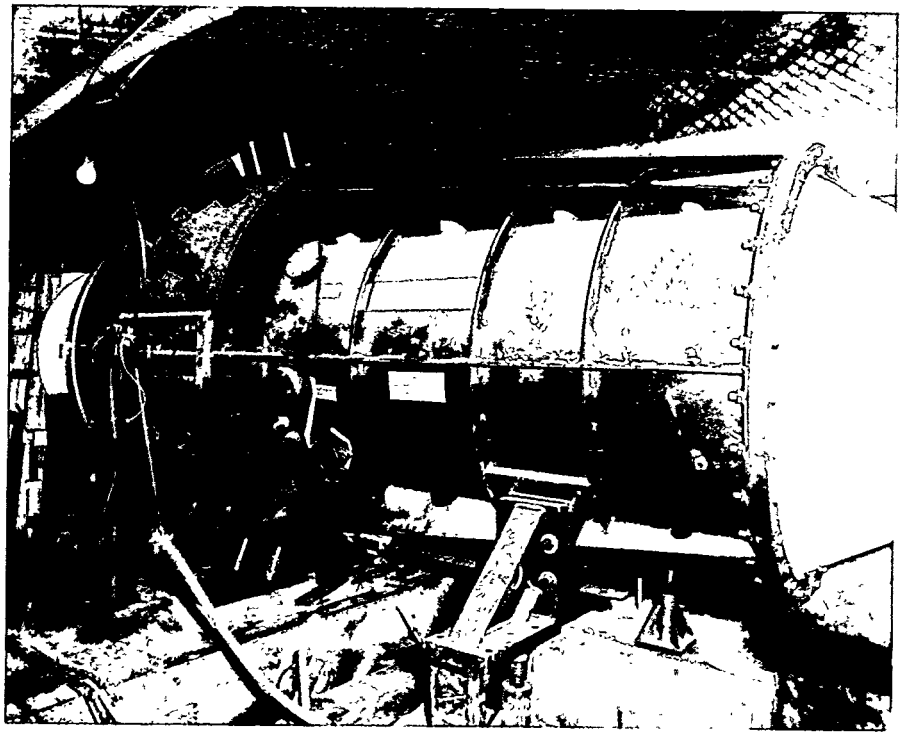
development work, because we felt for a long time that we might still have a need for them. But sometime after Camphor, a couple of years, during the early seventies, was hard times for the Laboratories.

Carothers: There were. We had layoffs in the early seventies.

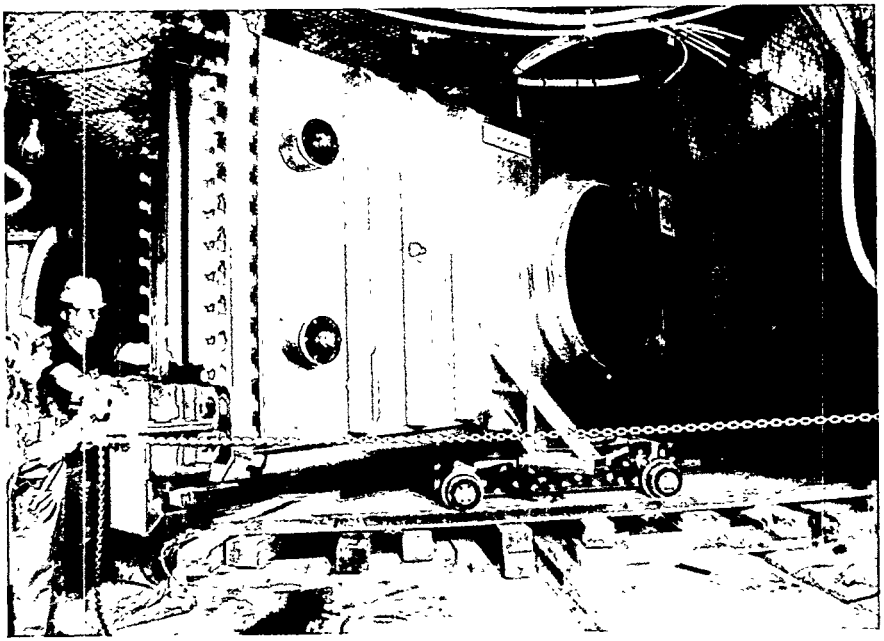
Kennedy: Yes. So there was a lot of pulling in of the horns. One of those was to say, "Well, we're not going to fund to develop fast closures any more, because we don't think we're going to use them anymore. If DNA wants to do that, they ought to take care of it." We were under contract with them to provide some closures through some shot that I don't now remember. I had the duty to go back East to tell DNA that we were going to get out of this business. We would honor our commitments through this particular event, and we would see to the fielding of that hardware, and so forth, but we were giving them this warning. In the future they would have to see to having that done by somebody else. They said, "But we want you to do that. What should we do about that?" And I said, "If I were you, I would get the highest person I could get in this place to talk to the highest person he could talk to at my place, and tell him that they would really like for us not to quit doing this work, and make the argument." And, in fact, that's exactly what they did.

Carothers: There were also some things that were used which were called HE machines. Did you people at Sandia do those designs, and tests, also?

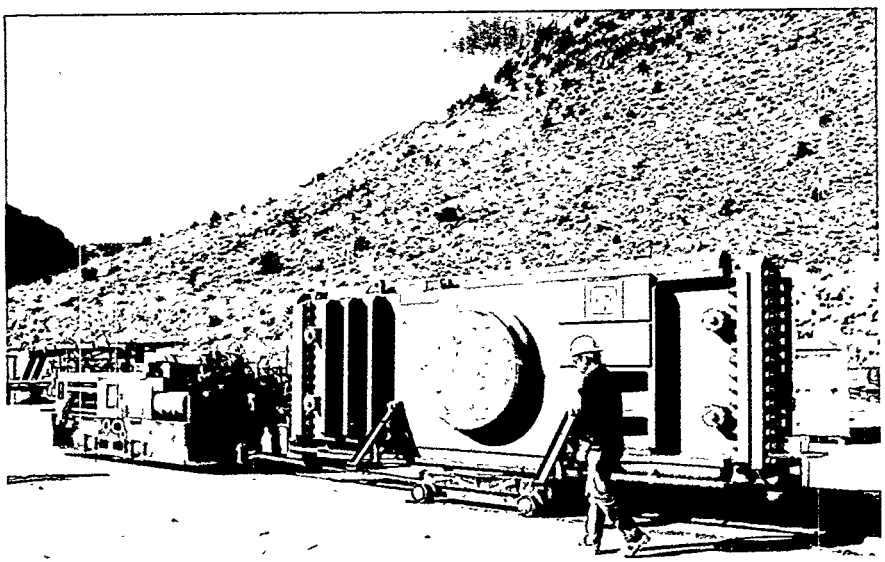
Kennedy: Yes. We early on had so-called HE machines. On Camphor we called them dimple machines. They were in on a close-in section of the line-of-sight. We put like a shaped, or platter charge on the side wall of the pipe. We started with one, and then put one at 90 degrees a little further down, and another one at 90 more degrees, so when they went off they just made the pipe go criss-cross to obstruct the line of sight. All they were supposed to do was to make a mess, and delay any hyper-velocity flow that might want to come down there and get the other hardware. They were just supposed to cause a delay, and a temporary obstruction. It wasn't containment. Nobody even pretended that they thought those things could do that, but they ought to slow down the flow.



Fast Acting Closure or FAC.



Modified Auxiliary Closure or MAC.



In more recent years there was a concerted effort here, funded by DNA in large part, to develop these fast acting closures - - the FAC's. They are a great big spool of aluminum and lead and steel which is HE driven. The HE is carefully designed to close the line-of-sight at the point where it's about thirty inches in diameter, and to close it in about a millisecond.

Carothers: Basically it implodes the pipe?

Kennedy: Yes, there is a cylindrical implosion of a big, thick-walled section of the pipe. It is not just a standard section of the line-of-sight pipe. It is an especially designed spool of aluminum, principally, driven by about four hundred pounds of high explosives. It implodes this spool, and causes a four or five foot length of solid copper and aluminum to be in the line of sight. It just makes a solid plug.

Ristvet: The various auxilary closures have evolved very carefully. They are related to sample protection, and there's a lot of engineering that has gone into them. That's been a unique thing that Sandia has done for DNA over the years, and done very well. The FAC is just an extension of the Livermore HE machine design, but done in a manner that reduced jetting significantly, and improved things which the Livermore designs were not too good at. Giving credit where credit is due, Olden Burchet and Harold Walling and all the rest of the crew at Sandia have been a great group to work with over the years. John Weydert also worked well with metallurgists, and the explosives people, because they were all in the same group. And Jerry Kennedy held that group together for years. It was just an excellent mix of people that had a very 'can do' attitude. We would set the criteria, and the basic criteria was to catch the pipe flow.

Incidently, Sandia told Carl Keller that those doors wouldn't handle the stress loads from the grout as you moved them in. Where we used to have them on Diablo Hawk and Mighty Epic and those shots was really about as close in as you could get and still have only a little less than a factor of one and a half engineering design safety in the doors. It's interesting that they were able to develop a reinforced door that actually almost doubled the effective strength of the doors, the flexure strength. We tested that on Distant Zenith, and it worked fine.

Carothers: Those doors have been driven with high pressure gas. I have always thought that was a dangerous procedure. A fifteen, twenty thousand psi gas system is a scary thing.

Ristvet: What's really scary is having a leak in the system, and not being able to shut the doors.

I think the highest pressure we ever used was eighteen thousand one hundred psi. We have had very good engineering people from Sandia, who were experts in high pressure gas systems. The gas systems were always assembled and tested beforehand. The tunnel was evacuated for that area, except for the two Sandia people who would test the system after it was installed. Then, before it was pressurized again it would be fully grouted in. So, except right at the compressor, which was in a secure, shielded area, there was no potential for harm to people except for the two or so that would be working right on the system. That's the same way you would do a high pressure experiment in the laboratory. You try to minimize those dangers, but you're absolutely correct. They are there.

Carothers: Why didn't you drive the doors with propellents?

Ristvet: I always wanted to. We have had a program going with people in Olden Burchett's group at Sandia. It turns out water-gel explosives work better than propellents. And they're much more reliable than gas systems, in a sense, and you don't have the exposure of people to high pressure gases and things like that.

I think if we had some of the explosives folks that we now have at Sandia involved in the early days of development we probably would have used a fast propellent, or a slow explosive. I emphasize slow. You want the generation of gas to be a little faster than a propellent, but not as fast as an RDX or PETN explosive. There are explosives that are used for various metal-forming applications that would be the right mix to use. We actually got as far as doing scaled tests at Sandia.

Carothers: This was going to be on the next shot?

Ristvet: Yes, it would have definitely been on one door, on Mighty Uncle.

Bass: Now, there's a containment rule — a sample protection containment rule. If the doors hold for a hundred plus milliseconds you've got no problems. After that it doesn't hurt you if they let go.

Half the MAC doors have been taken out in the history of the test program. All of them except Mighty Oak and Misty Rain went out close to a hundred milliseconds. We know that from data. We've had light beams going across the pipe, we've had pressure gauges back there, and it's been my job for years to unsnarl all that garbage.

Dan Patch has called this flow of stemming that hits the doors core flow, and Joe goes through the ceiling because nobody really knows what core flow is. This core flow is tempered by the doors lasting that long. Then, when it takes out that door it has lost enough energy that by the time it gets to the GSAC it won't take it out. The GSAC has approximately 8000 psi strength. The MAC had approximately 10000 psi strength — if it closed. If it is not closed, all bets are off. Then it's just two cantilevered hunks of iron. It's got some strength, but we can't even estimate what it is.

There is a new MAC now, called the STAC, Stemming Anchor Closure. It has been developed as a result of a small working group of me, Dan Patch, and Ed Peterson, where we designed a new closure to prevent the Mighty Oak problem. The main change for this closure is that the doors have steel front and back plates on them, and it can hold probably three kilobars.

Carothers: If it's closed.

Bass: If it's closed. But we can get it closed, because we can drive it explosively. There's no reason not to drive that with propellant. The MAC and GSAC are driven with helium. Originally they were driven with nitrogen, but they changed to helium to get more specific energy. But it turns out that the primacord that blew the tanks that held the gas was providing over half the energy. So really, we were ddoing a lot of the driving with primacord all the time.

Carothers: If it were my tunnel, I would look very dubious at you coming in and wanting to put fifteen or twenty thousand psi gas bottles in there.

Bass: It's the most dangerous part of the test program. It's much more dangerous than the FAC sitting there with four hundred pounds or so of TNT in it. We're going to drive them explosively in the future, and propellants are pretty safe to handle.

Well, I should say that some work has been done, but the Tiger Team visit to Sandia stopped this last event from having propellant drive. The laboratories were closed, for ES&H purposes, or that work would have been done.

18

Pipe Flow

With the resumption of testing in 1961 some events with a horizontal, and some with a vertical line-of-sight were conducted, principally for effects experiments of one kind or another. Here the need for a calculational capability to design an opening that would allow the desired radiation to reach the samples to be exposed, while simultaneously containing the radioactive materials and protecting the samples, quickly became apparent.

Over the approximately eight years that vertical line-of-sight events were conducted before Baneberry some four out of five released activity. Some releases were small, and confined to the Test Site; many were detected off-site.

Carothers: Who designed the pipe string on the Livermore vertical line-of-sight events? Who said what the front-end should be, or what kind of closure hardware there should be?

Hudson: I think the design was somewhat, I shouldn't say happenstance, but it wasn't engineered or designed on the basis of a lot of information. It was more or less a farmer's approach to a problem.

Carothers: "Let's put a big valve in about there."

Hudson: That's right. "Let's close that pipe with high explosives." And those ideas were good, but they didn't know where to put these things, by and large. So they decided putting them in close must be better than farther away. "Let's stop that monster as far down as we can." As a result, most of the early closures were blown right out of the pipe, like a bullet through a gun, because they were in a region where the energy density was too great. As they were moved farther away, they worked better. And that's probably when people realized that, "Hey, maybe there is enough of a basis for science and engineering here that we ought to have a containment group."

We've revisited those old designs several times. I don't recall, at the moment, what our findings were, other than a big recollection that the primary problems of those events was that they tried to stop things too close to the source.

Keller: At Los Alamos I looked very hard at the LOS pipe flow measurements that were available, and they were terrible flow measurements. They put the gauges on the pipe, fired the shot, and the gauges gave you gibberish or they went off the air. There was no really serious effort to measure flows in pipes. You would discover after a couple of efforts, which took a couple of years, that the gauges they were using were heat sensitive, and so the declining pressures you saw at strange times was because you were heating the gauge. And a lot of the gauges were shock sensitive, and they screwed them into the pipe wall, so when the pipe wall was racked by the ground shock it would warp the gauge body and you'd get this funny stuff. It was really kind of discouraging how long, how very long, it was before pipe flow measurements ever became reliable. It was ten years later. I think the first really good set of pipe flow measurements were on Diablo Hawk, which was in '78. Ten years later.

Carothers: Did you also do pipe flow calculations?

Keller: No. We did do the design of the front-end on all the Los Alamos events. The first ones were actually designed with 1-D codes. I calculated lots of slices to determine what the probable 2-D behavior was. The current design is different in aspect ratio, and so forth, but it was really started about the time of Door Mist. It evolved from that point to the larger reverse cones, the longer, more slender front-end cones, and things like that. But it evolved rather slowly.

After I had been at the Lab about two years, Chick Keller joined up; that must have been about '68 or so. Chick's job was to do front-end calculations in two dimensions, and he came in just as eager as he could be to really do them right. He calculated the designs in two dimensions, and after a couple of years he expressed a lot of frustration because the designs that were developed with the 1-D codes were relatively optimum. There was almost nothing he could offer that would be a major improvement. The phase velocity and everything had been determined with 1D slices, and so the 2-D codes only confirmed the 1-D code designs.

And the concept was resilient. Because of our level of ignorance we wanted it to be resilient. We didn't want it to be sensitive to device performance or anything else. There were some changes in things, but generally speaking it was just a very slow evolution of those designs. The biggest changes were dictated by experimental conditions like the aperture that was used on Cowles. It was huge compared to the norm, so that necessitated a different design, but it was not driven by any real revelation from 2-D calculations.

In '69 to '70 I was designing Manzanas and Cowles and Yerba There was also Snubber in there, and I designed the front-end of Snubber. Ajo was a test for that front-end design, and it worked fine. But there's more to a containment design than the front-end, as we found out on Snubber, and as DNA has found out recently.

Carothers: In those days it seemed as though at every CEP meeting I went to there was an interminable series of viewgraphs made from computer plots, with someone saying, "Well, here you have such and such, and now you see . . ."

Keller: Yes, energy ahead of ground shock, and all that. Marshall Berman, Chick Keller, Jose Cortez - - all those guys at that time were calculating front-ends mightily. There were a lot of front-end calculations. One interesting thing about all those pipe flow calculations, and front-end calculations, was that there was not a realization in those days that most of the energy flowing up the pipe was actually generated by the ground shock collapse of the pipe. It was thought that it came through the front-end. You could aggravate circumstances by a poor front-end design, but a good design certainly never got rid of the ground shock generation of jetted material.

I went through all of that stuff before the CEP presentation of Huron King, more thoroughly than I had ever done it before, and I was surprised to find that asymmetric designs were fairly popular in the days of Eagle, and Finfoot, and Tee, and Backswing - - the vertical line-of-sight shots. There were a number of things they were doing wrong in those days, and they didn't realize it. One was that they put the HE machine three meters above the saviour.

Carothers: What was the saviour?

Duff: That was an asymmetric pipe closure system. It was a big, massive C-shaped steel pipe with ribs on it, like gear teeth, so it was non-uniform. The fourth side was closed by a relatively thin, flat plate. The idea was that the flat plate would jam in much faster than the other walls would. It was the kind of thing which has been talked about subsequently on a number of occasions, but in the DNA program we use axially symmetric things, largely because we can calculate them.

Keller: I'm sure something they didn't appreciate at that time was that the source region extends out as far as the full cavity region - - out to the six kilobar range. And so they would put everything in the first third of the source region but nothing thereafter.

Harry Reynolds wrote a paper on the apparent success or failure of HE machines. His conclusion was that you had to be outside of the cavity radius. He didn't know why that had to be the range, but you had to be outside of the cavity range for an HE machine to be very effective. They had placed the HE machines from just a few meters above the can to farther and farther out, and they never seemed to do much until they got out to a certain distance. I read that with amusement, because about a year before we'd done experiments at Physics International which showed that the ground shock implosion of the pipe generated a magnificent jet when you were in the six kilobar range. That happens to be a little bit beyond the cavity radius. And so this paper that was written by Harry Reynolds had all this wisdom in it, which was supported later on when we discovered what was really going on. His conclusions were right, but he was a bit baffled by why they were true.

And there was an external helix on one of the Livermore events, but I didn't know that. I also had put a helix on the outside of the pipe on Cowles. Now I know that the external helix on Cowles was ineffective. I'm sure of that, because when I went to DNA we started doing experiments of that kind. Those tests we later did at Physics International showed that an external helix worked fine for an HE imploded pipe, but it didn't work at all for a ground shock imploded pipe. There was absolutely no effect from some of the strongest asymmetries on the outside of the pipe.

Duff: Probably the most relevant thing that I did of a containment nature while I was at Livermore was on Alva and Backswing, where we diagnosed the performance of the front-end hardware. That's something that hasn't been done since. Interestingly enough, we got an indication of the pressure in the iron of the saviour, and it was somewhere between 100 and 500 kilobars. We could tell by the velocity of the shock wave that was involved. And the velocity of the jet coming up the pipe was two centimeters per microsecond in the closure itself, and that number is the same as DNA is getting these days.

The first events to use a horizontal line-of-sight in tunnels for weapons effects studies were Logan (1958), Marshmallow (1962), and Gumdrop (1965). It was in 1966 that the DNA began an extensive series of effects shots in tunnels in Rainier Mesa. Most of these used a horizontal vacuum pipe that diverged from a few inches in diameter near the device to several feet in diameter at the far end, which might be as much as a thousand feet away from the source. Stations for various exposures levels and experiments could be located along the pipe. Some experiments recorded data as the radiation from the device struck the detectors, and were finished within microseconds. Others involved the reentry and recovery of exposed samples and components, and their success depended on being protected from ground shock, damage by projectiles, high pressures and temperatures, and contamination by device debris.

The design of the line-of-sight system thus involves letting the prompt radiation from the device into the pipe, and then closing it in such a way that other material does not flow down the pipe and damage or destroy the experiments being done. Further, the tunnel complex should be protected, principally from radioactive contamination, so it can be used for future experiments. And, extensive amounts of recording instrumentation and equipment, whose loss would be quite costly, are usually located in the tunnel. Finally, there is to be no release of radioactive material to the atmosphere. The proper design of the line-of-sight system is crucial to the accomplishment of all these purposes, except possibly the last. It has been demonstrated that massive concrete plugs placed in the tunnel can, if properly designed and installed, prevent release of radioactive gases even if there is direct and open communication between the cavity and the tunnel complex.

The first design problem is to allow the prompt radiation from the device to enter the pipe, and then to close the close-in portion of the pipe, called the front-end, to prevent device debris from entering.

Carothers: Chuck, might it be fair to say the way front-ends are today is largely due to you?

Dismukes: Well, front-end design has been my primary occupation since about 1965, but I think that would be giving me a little too much credit. Of course, we have to take the blame when they don't work; they're not all successes. If I'm going to take some of the blame, I certainly want some of the credit for the ones that worked.

Carothers: Seems fair.

Dismukes: We haven't been totally successful. And, that's a major question; when things don't work right, what went wrong? That's something I don't think we know the answer to.

Carothers: When you talk about front-ends, how far along the pipe does the front-end extend? When does it stop being the front-end?

Dismukes: That is also an issue, and it has varied over the years. It was often a time frame of a hundred microseconds or so where we would try to describe the phenomena. At that point the shock might have propagated on the order of a meter outside the zero room into the stemming. As the designs evolved we came up with this thickened pipe wall, a heavy-walled pipe which is sometimes called a reverse cone, or extension. That's grown in length over the years, and now it's out to a few meters. We try to carry the calculations and analysis out to where the shock has reached that range, or beyond. Typically we've looked out to half a millisecond to a millisecond.

Carothers: What factors do you try to include, and what do you try to do out to that half millisecond or so?

Dismukes: The basic concept is simple. We're talking about line-of-sight events, primarily for x-ray experiments, but they don't have to be. We're viewing a portion of the output of the device through a small pipe. We want to maintain this view until the device has put out its prompt radiation, and it has had time to go through

the system. That's typically ten nanoseconds or less. It's very quick, so we don't need the pipe to stay open very long. Then, at that point we want to close that pipe as rapidly as possible, to prevent device debris and radiation we aren't interested in from coming down the pipe.

The basic concept is to create a a sequential set of valves, or closures. In order to close things fast you have to vaporize them, get them very hot. And they won't stay around forever because they're basically just a dense gas. So, we try to follow that with a little denser gas, and a little denser, and eventually some liquid, and eventually solid material which we hope will survive and begin to form the permanent closure of the pipe. The whole system is designed to produce a continuous, but only semipermanent closure of gradually increasing integrity. It's length times density, and the cooler the better, but it's hard to get material in quickly and keep it cool. Most of our systems, at least the way we calculate them, come apart after we've closed them, but over a long period of time. By then we hope to have created the ground shock closure of the pipe further out.

Carothers: What's the purpose of this reverse cone that wasn't there originally, then gradually got there, and got longer, and as I recall changed material a couple of times?

Dismukes: In the early days we essentially just had a pipe sticking into a box and we worried about getting the front of that pipe closed. There was stemming basically up to what we call the portal end of the box, and the pipe was just there. We noticed that when the shock propagated into the medium, the stemming tended to provide a low density path between the hot zero room and the pipe. The place where we were trying to form a plug in the pipe was just very hot, and of a low density. Because the shock was strong and the stemming was of insufficient density to really resist it, we might have a good plug at the front of the pipe, but this could be bypassed, leaving the remaining pipe open, potentially, to the zero room.

Carothers: The pipe was being closed basically with the pipe material and some grout?

Dismukes: Even worse, in the process of closing components closer to the bomb we were producing a fair amount of energetic flow, which we call plasma, which was jetting up the pipe. This stuff

was interacting with the pipe before the ground shock got there, and blowing it out significantly. So, it was a lot larger when the ground shock did arrive, and that made it harder to close. That whole process led to some very tenuous looking curtains of material between the open LOS and the hot zero room. We noticed right away that we ought to try to do something about that. Initially we just put in a couple of feet of thick-walled iron pipe. That helped, so we decided to make it longer, because we noticed that just beyond the end of that thick walled section, again the pipe was exploding.

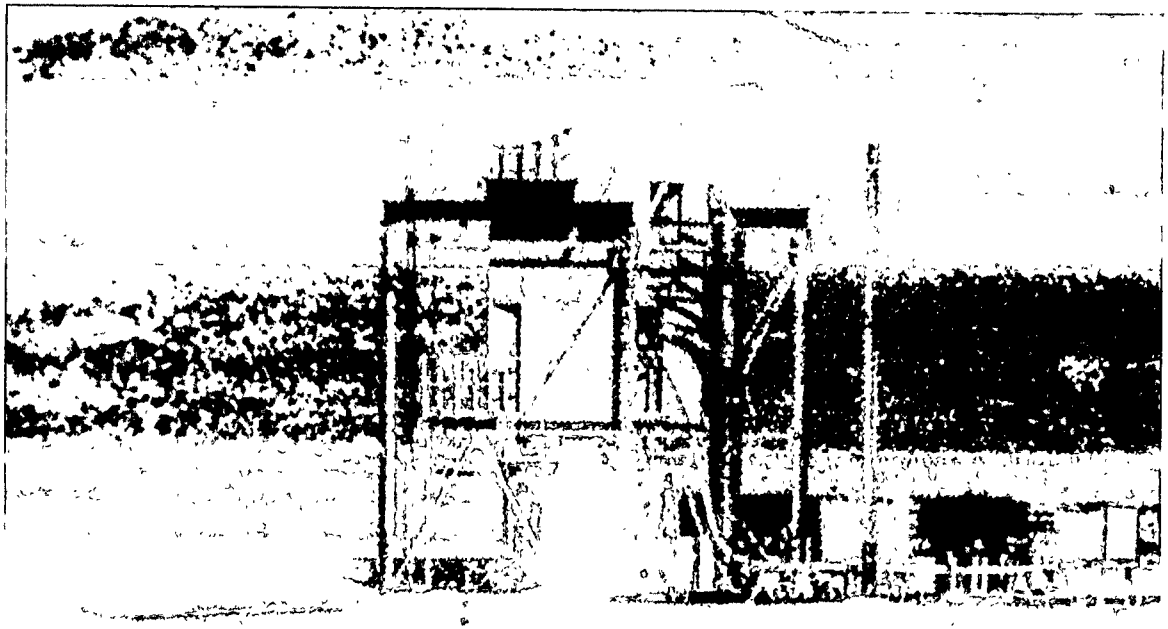
The impedance of the metal was enough to inhibit the explosion of the pipe, so it didn't get as large before the ground shock arrived. That's the basic concept. Also, when you do close it, the material comes in more gently because of its higher density. So, it doesn't get as hot, and it doesn't produce as much of a problem downstream. We started doing a lot of numerical experiments on the computer, and we noticed that higher density was better. And what's the highest density around? Uranium, or something like that, and there was lots of that stuff available. Now, it took a number of years to reach that state. We went to ten foot long reverse cones of steel and we used those for quite a time.

Carothers: Wasn't there a time when they were lead?

Dismukes: No, they have never been lead. We talked about that, and we were a little nervous about lead. It has such a low heat of melt we were worried about squirting a lot of lead up the pipe. And, the density isn't that much higher than steel, so it wouldn't be a big difference.

Somewhere along there, say 1975, we started experimenting with really high density, on the computer. Uranium was much more beneficial. The energy in the pipe wasn't sufficient to open the pipe up. And when the ground shock pushed it back in it was relatively cool.

Carothers: Let me see if I have the concept. The uranium is sitting here. The material that starts coming from the zero room heats it very rapidly and starts to push it out. You would like it to just sit there until the ground shock gets there and starts to move it in.



December 12, 1963, Eagle event. Line of sight to the surface.
The white blur at the bottom of the tower is the emergence of the first flow from the pipe.



Eagle event, a small fraction of a second after the previous picture.

Dismukes: Right. And, the stuff in the pipe that gets there doesn't have a high Mach number. In other words, there's a lot of thermal energy as well as kinetic energy. So, it creates at least many tens of kilobars of pressure in there. That's enough to start trying to make the pipe bigger; to push the pipe out before the ground shock has really had time to get there. The ground shock at first tries to stop it from moving out, and then it tries to push it back in. Of course, the bigger the pipe has become, and the more energy there is in it, the more work the ground shock does on it, and adds even more energy which, potentially, can go up the pipe.

Now, that's one thing everybody always assumes is bad for containment. It might not be. Letting all that energy go up the pipe doesn't necessarily cause a problem, except it can sure attack other things like the mechanical closures. In principle it could even get to the experiment station and do damage to the experiments. But if we've done our job there won't be device debris in it, and so it's not radioactive. So, if we could let all this fast stuff go roaring up the pipe and just got lost, and out of the way, maybe we could close the pipe even better.

Carothers: You are recapitulating a line of thought that occurred in Livermore following the event called Eagle. It was the one that produced a fireball at the surface. We were a little surprised to see that.

Dismukes: I'll bet.

Carothers: I had people who did things like fireball yields, and I said, "How much energy was there?" They came up with a number which was kind of off the wall, not applying fireball yield rules, of course, but HE fireballs, you might say. They said, "About two hundred pounds."

Dismukes: I would have guessed it might have been a few tons. Either way, it's not really very much energy, but it looked spectacular. You wouldn't want to be standing there.

Carothers: No. It sure blew up the tower that had the instruments on it. Anyway, "Bang." There was the fireball, and all these pieces of the tower flying around. Then there was a quiescent period. Not very long. Maybe a couple of seconds. Up to that point there was no radioactivity.

Dismukes: That fireball wasn't radioactive? It was just some hot gas?

Carothers: Yes. Following that, after this short period, there was steam or smoke, and it was quite radioactive. And so, doing the kind of deep thinking that we used to do in those days we said, "Well, there wasn't any activity there in the beginning. Why don't we just let that go by. Then, if we had some big valves, and if we could close them in half a second, or a second, we could just shut it off." That's where ball valves on the vertical line-of-sight pipe got invented. Of course, we discovered they could be taken out one way or another, but that's another story. But that's what you reminded me of when you said, "Maybe it's good to let this go by; it's not radioactive. Then we will close things off."

Dismukes: It's not guaranteed to be bad for containment per se.

Carothers: No, but DNA has always had two problems. The more severe one is to preserve the experiments, to protect the samples. I think of the tower on Eagle when you say, "Let it go by." As far as the problem of a release to the atmosphere goes, if you folks succeed in protecting the experiments, containment is virtually assured.

Dismukes: That's almost axiomatic, you would think. But I don't know if that is really true. And that's because we've had some strange results where things looked good for awhile, and then later they didn't.

Carothers: Anyway, you now have put a lot of high-Z, high-density stuff around the pipe, and hopefully it stays relatively cool.

Dismukes: Right. And so we're really accomplishing two things. We're keeping the pipe, which we have to close later, from expanding. And we're also putting somewhat cooler, higher density material into the pipe. The problem is that if you make this reverse cone longer, you have to run the calculations longer in time to see what happens after you get to the end of it, and we haven't explored that region enough.

Carothers: Computers are getting faster.

Dismukes: Yes. But the modeling issues are significant. Where we think the real problems are is later in time than where we're now talking about. It's in the few milliseconds regime where no one is really dealing with what appears to me to be the most likely source of containment problems.

Carothers: What do you think that source is?

Dismukes: It's that we still have gas, a lot of hot gas, in the pipe, which is trying to expand the pipe, and we have the ground shock coming along. The interaction of those, and treating the flow in the pipe, modeling it well, is difficult. We don't know how to do that right now. That area is the one that looks to us like the biggest problem -- the influence of the pipe flow on the ground shock and the stemming plug formation.

We ran a calculation, one we know isn't a good calculation, out to ten milliseconds on one of our standard designs, and things didn't happen the way we expected. We were kind of shocked. We didn't form a good plug after we got past the end of the reverse cone, or extension. There was quite a long period where we had very little material in the pipe. And then finally a plug started to form. We knew that we weren't treating the physics very well, but that was scary.

And some of our reentry observations are scary. We see occasionally, "core flow." Joe hates the words, but that's what I want to call it. That's where, when you mine back in, down the main drift, you see a well defined stream of grout which you can identify because it's a different color. And that grout came from relatively close-in. How did it get through, if we're forming a plug? It clearly couldn't have been moving, or I don't think it could have been moving, as fast as the ground shock. If it wasn't, then why didn't the ground shock cause the pipe to be closed ahead of it? That stream didn't get through there afterwards, because it is a well defined stream, and it wouldn't maintain that kind of definition. That implies we had a low impedance path through what we would like to think was a plug, and this stuff was being extruded through it.

We've seen that more than once, and that's very frightening. It suggests to us that this plug doesn't always have the integrity we would like to think it does. The other disturbing thing relating to the extensions is that, although we don't understand why it's

happening, there is some empirical evidence that we've had more problems since we've used the long extensions than when we had the short ones. You know, we had a sequence of events when we decided we understood everything.

Carothers: That's right. There was a time when you might well have thought that.

Dismukes: And those pipes had iron reverse cones which were somewhat shorter. Then we got smart, and things appeared to really work well for a while, but we've had a lot of problems since then. Other things, many, many other things have changed, like pipe taper. A number of people have studied this very hard, and there's no finger to point at one thing and say, "That's what did it."

Carothers: I believe that. At the presentations to the Panel there are often lots of viewgraphs which are designed to show that the upcoming shot looks very much like previous ones. "Well, see, here's a whole bunch of other shots, showing where the various stemming grouts are and how long they are. And see, this one looks very much like them, because the grout sections are pretty much the same. And so, this one is good." And I sit there, and I think, "I'll bet there's probably two hundred and seventeen other things that are different, ranging from different manufacturers of the cables, to a different method of mining, to a different tuff, to who knows what."

Dismukes: Sure, and that's one thing I think we are really fighting, and there's no way to beat it; you can never repeat a test. The medium you're shooting in has to be different, either because it has been shaken by another test, or just because the earth is not homogeneous.

Carothers: How would you summarize the state of the thinking today about the front-end, including the reverse cone?. Do people feel fairly satisfied with it?

Dismukes: I always hesitate to answer those questions because I feel it's very tempting to be self-serving and say they're wonderful, and there's clearly no problem, because we designed them and we think they work.

On the other hand, there are people who are concerned about it. I'm somewhat concerned about the reverse cone, because of the number of seeps we've had since we started using these longer,

heavier ones. We had that long sequence of events with no apparent problems, where we decided we clearly understood everything. I don't think we did, and the one problem that I have with those shots is that we missed a beautiful opportunity when we didn't reenter them. We didn't look at them, so we don't know what they did in close. So, we don't know what a good shot looks like, or we haven't seen very many.

We're just amazed every time we do look because there's always something different that we don't like to see. That's somewhat disturbing, but clearly there wasn't a lot of radioactivity seeping into the tunnel on those shots, that's for sure. You have to be concerned that we've done something bad making the changes that we have. The calculations say that we've reduced the pipe flow significantly, and I think the measurements of pipe flow tend to support that, at least qualitatively. As far as earlier time frames, in the front-end, I feel that that's not a problem.

Carothers: That's something that, in principle, ought to be calculable with codes you really believe, on time scales you understand.

Dismukes: But you won't get a universal agreement on that. As long as we keep doing the type of devices and yields that we have been doing, we shouldn't have to worry about it. But if we suddenly go to very low yields, where the energy is very limited, I'm not sure we would know what to expect. We try to design these things to be far from the edge of a phenomenological cliff. So, if the energy changes a factor of two it doesn't really matter, and the system doesn't respond in a non-linear way. We try to operate in what I think is a fairly comfortable regime, where we can afford to be wrong by quite a bit, and still have things basically work right.

If we start going to, for instance, very low yield devices, it's a whole new ball game, because the containment, in the normal sense, can't be achieved with the ground shock. Then the game is to make sure you don't wreck one of the key elements further down the pipe because the front-end didn't work right. There would be, maybe, more threat of having to deal with jets of material out of the device itself, and that's something we've only tried to deal with once, and that was not a success. That was on the Hybla Fair event. We clearly

didn't design that one very well. Apparently the back of the test chamber got blown out by what came up the pipe. For the kinds of devices DNA is currently using I'm pretty comfortable.

Carothers: Really? How about the reverse cone?

Dismukes: Well, I'm a little nervous about that, because we've seen some things that don't make me feel good. The Bermuda Triangle of containment to me is the few meters beyond the reverse cone, and maybe it includes the end of the reverse cone. That's the time frame of a few milliseconds. Nobody's dealing with that problem, for a lot of reasons - - partly for the reasons you already reviewed. You have this little thin pipe in there, stretching over many meters in length, and there don't seem to be enough zones, even with the current fast computers, to really deal with that. Plus, there's a combination of high shear flow in the pipe, where you have very hot gases shearing against the pipe while it's blowing out. And, you also need to treat the strength of the material fairly carefully, because the shocks are getting down under a hundred kilobars. Plus there are some weak shocks of a few kilobars out of the pipe. So, you need to treat strength carefully. You need a Lagrangian code for that, but a Lagrangian code can't handle the shear in the pipe. We need a different approach, I think.

Peterson: Carl Keller thought for years that the most damaging thing coming down to the closures was the pipe flow. He thought that even to the extent that one might be able to remove some of the closures if you could eliminate the pipe flow. Subsequently we reduced the pipe flow a lot, and the reduction of pipe flow correlates better with increasing bad experience than with anything else. I don't understand why. It may be, for example, that the flow we measure is not the damaging one. And so, we reduced the part we can measure, but we may have increased the one which we can't measure. It seems that for almost any technical detail you can bring up there is evidence on both sides; there's contradictory evidence as to which way you ought to go in changing it.

Carothers: Dan, when you talk about late-time calculations, what is late time for you? When does it begin?

Patch: Late time for us was, and this goes back into the history of before I joined the program, times beginning something like a millisecond after the detonation, and in principle it goes on essen-

tially forever, until nobody is interested anymore. In actual practice, late time calculationally has been from about a millisecond to about a second. That's kind of the time span, because that's a critical time range for containment.

Some of the things that we have done were empirical, to look at data from a number of shots, and try to understand the time scale they failed on and why they failed. Some of the things we did were to look at fracture and fracture processes.

When I was at S-Cubed I was working very closely under the wing of Jim Barthel. Jim was doing 1 1/2 D pipe flow calculations with the FLIP code. So, I concentrated pretty much, for the three years I was S-Cubed, on this energetic pipe-flow code, which was subsequently used for Hybla Gold, and the nuclear shock tube studies. That was my primary area of interest. In the last six months to a year I had branched out, and was looking at ground motion from the data base point of view. It was the old issue of HE-nuclear equivalence. What could we determine from the data base that exists for the number of HE shots that were done, and there were quite a few HE shots done in the early seventies, versus what the data base for the nuclear tests had?

Carothers: When you talk about ground motion, do you mean close-in motion? You don't care about the surface motion, or seismic signals, for instance, do you?

Patch: We really did not look at the surface or seismic motions at all. We were much more interested in the stress range out to about a kilobar, where the materials transition out of the plastic regime. The strength is very important there; the materials are plastic, but the strength modeling is important. In a way, the late-time containment, almost, was the regime in which the motions were strength dominated from a calculational point of view, at that time.

Carothers: People talk about the energy release being so large that it overwhelms the strength of the rocks, so the kind of rocks don't matter, close in.

Patch: Our primary interest was when that wasn't true any more. We were interested further out in time, further out in distance. That statement is sort of true for some small time period, and in some small regime. What we do is greatly simplify the details

of the zero room and the front-end. Then we start at zero time and basically let it grow in a more or less spherical way. We may model some shapes, but we don't do a detailed analysis of the hardware and its effects on the ground shock. So, we start at zero time, and we kind of sluff through the early part. Right now we're really trying to fill the gap between the classic early-time calculations and the late-time work we've done. We're trying to do a better job there. What I would say is that what we would like to do is start the calculations with the details of the zero room environment, with the most important mechanical details, but probably not with the sophisticated treatments that are done so well in trying to set the timing of all the hardware.

Carothers: Do you also look at the material transport down the pipe?

Patch: We have not done much with that. That's an area we are just starting to work in. We have relied on S-Cubed to give us a definition of that pipe environment. We've put that into the calculations, as best we can do it, as a boundary condition.

Carothers: You look at the cavity growth, the shock that moves out, and basically you try to tell people what the stemming is going to do, and what the loads on the closure hardware are going to be?

Patch: Yes. We try not only to tell them what is going to happen, but hopefully we're a little more proactive. We not only look to see what the problems might be, but to say, "You really need a smaller tunnel in this region, and you ought to take this out a little farther," and so on. So, in a way we attempt to tune the geometry of the test bed, depending on the medium properties and the objectives of the test. The basic geometry of each test has, in some sense, experimental constraints, such as the length of the pipe, the taper angle, the yield limits, where it's fielded, and lots of other things. At the lowest order we attempt to identify what are the undesirable features of the test bed, and try to figure out how they can best be mitigated. I think I would be overstating our role if we said we were really fine-tuning the designs. We try to, but it's a very complicated world out there. Calculators always have to guard against the idea that they're doing something real, that they know what they're doing, but we're doing the best that we can.

Carothers: What's the origin of the material that makes up the pipe flow?

Patch: I think it comes from the pipe region just beyond the reverse cone. The reverse cone keeps the pipe from expanding, and I think that a real contributor to the flow is the fact that when the ground shock comes along, the pipe is not what you think it is. It's some new shape, which is a lot bigger. I think the reverse cone is probably pretty effective in keeping that expansion from being as bad as it would be without it. But the reverse cone is kind of tapering down; it's quite a long distance in physical space, but it's probably coming down a little too soon. We're still getting many kilobars of stress off the end onto a bare pipe, which can't handle that. Of course, that pipe is blowing up, so that stress actually may be applied to the tuff in a way, because the pipe, I suspect, fractures. It can only expand maybe five percent for mild steel, and then it's going to begin to shatter. So, I think that region is the source of the really serious part of the flow.

Carothers: This serious part of the flow; do you think it's pipe material, or some of the grout, or both?

Patch: I think it's both. I suspect there's a fair amount of steam that's generated from the very strong collapse forces. They're very convergent, so that tends to act like a shaped charge. I think that these collapse forces are generating very high pressures on a relatively limited amount of material.

Carothers: Why don't you make the pipe square?

Patch: That question has been asked many times, and it's a question that's never been satisfactorily answered. I personally don't think it would make very much difference. We've fought this battle back and forth, and I don't know that anybody has shed any real light on how important the pipe being circular is. I've heard it argued that because seemingly very similar shots have quite different flow, therefore if we get just perfect convergence, for whatever reason, we're in a much more serious regime than if things are slightly off. And I've heard it argued the other way, that the flow can't really be that sensitive to the shape, and there are other factors that are causing these differences. I would really like to know the answer.

Carothers: I remember on the CEP, for meeting after meeting people would talk about jets down the pipe. Why were there jets? Well, when you make a bazooka shell you try to make a jet, and the way to make it is you make a nice cone of HE which you light at the apex. So, you have a conical thing that slaps shut, and out comes a jet. And so there was always the thought, "Why do you make the pipes so symmetrical?"

Dismukes: It's certainly cheaper to do it that way, rather than to make them oblong or oval. Not a lot though. There is one basic argument why circular is better than the other cross sections. You get the maximum exposure area from the device for the minimum volume of open pipe. But I think that may be a second order effect. In fact, the symmetry in the production of jets may be something we should try to avoid, but we've always been very conservative, maybe to a fault, in not wanting to try things we didn't think we could calculate.

Carothers: Are the pipes and the closures symmetric because that's what you can calculate, or did you build symmetrically oriented codes because that's the way things were built?"

Dismukes: I believe that primarily it's the first thing you said. We tend to build things we think we can calculate. But Carl Keller did bring the helical insert into the system. Clearly we don't know how to calculate that.

Carothers: And there have been things called mufflers.

Dismukes: And those did appear to have at least some beneficial effects. I never understood how they worked, but they sure did something. There's no question about that. We used them for quite a while. They've sort of fallen out of favor though.

Carothers: The helix was put inside the pipe. One could put it on the outside of the pipe.

Dismukes: Well, we've looked at that, a little bit. We were convinced that you ought to be able to achieve some benefit by asymmetrically loading the pipe from the outside. However, that was never demonstrated. It's difficult to calculate, but we did try to do some calculations. There were also some HE experiments done to investigate the helix effect. They tried some cases of loading the outside asymmetrically, and couldn't see that it did

anything, so that sort of fell out of favor. There has to be something there, but we've never gotten serious about doing something about it.

Keller: We did embark on quite an experimental series, studying the formation and the attenuation of jets in the LOS pipes. That really was an extension of that original debate about where the energy in the pipe was coming from. I had decided that the jetting process was ideal if you had a nice symmetric geometry. And, there were all kinds of extra precautions taken in the design of things, like shells, that were to produce jets. I thought, therefore, that there was probably a way to discourage them. So, I had some calculations done at S-Cubed, and some experiments done at Physics International, and people are still looking at those results.

We did a series of experiments in which we imploded pipes with high explosives; just a cylinder surrounded by high explosives, detonated at one end. It gave a jet out the end of that pipe that was just awesome. With a two inch pipe, with about a half inch of nitromethane on the outside, you could generate a jet that would punch a two inch hole through six inches of solid aluminum. It was really impressive. The PI people just couldn't believe it the first time they tried it - - the damage they did to the target they put out there. They put out a two inch target the first time, and the jet went through it like soft butter. So, they put in a six inch target, and it went through that. They were very impressed.

Well, we tried a helix on the outside, and it worked beautifully; it just completely eliminated the jetting. There was just a speckling on the front of the target. We took some flash x-rays of the pipes, with and without the helix on the outside, and they showed that the helix did perturb the implosion. So we thought, "No problem, we'll just do that on the nuclear tests." We were trying to simulate with this HE implosion of the pipe the ground shock implosion of the pipe.

I don't know remember whether it was by E. T. Morris, from Physics International, or Russ Duff, from S-Cubed, but the question was raised; "Well, maybe the implosion from the ground shock is different than that from HE." And that was certainly a possibility. So we scaled down the HE shots to three-quarter inch tubes, drove

them with HE, and still got the target damage. Then we put in three-quarter inch tubes radiating out from a three hundred pound nitromethane charge in saturated sand, so we generated a ground shock that imploded the pipes. We got awesome target damage.

Then E.T. Morris and I were sitting in the SRI cloakroom waiting for a DNA meeting, and we were depicting the geometries on ten pipes that we were going to put around the next nitromethane sphere. We tried changing the nature of the jet by lining the pipe. One pipe we lined with paper, sort of carbon-like, one pipe we lined with glass, one pipe we lined with polyethylene. And we thought, "Well, that ought to really change the nature of the jet." We were thinking we could perhaps modify the damage by modifying the material that was in the jet.

We also used a very heavy walled pipe that was wrapped with lead. It was a really heavy walled pipe because the calculations had shown that a heavy walled pipe was more effective even than the assymmetry. These were S-Cubed calculations, and that configuration had shown the lowest jetting, in those calculations, for years. I heard that heavy walled pipes were better long before I ever got to DNA. But there was some worry about trying them on a shot, because we thought that if we really changed the form of the energy from a gas flowing down the pipe to a cannon ball, that the cannon ball might do more damage to the doors than the gas flow. So, there was some reluctance to try it. But since we were just doing these HE experiments we could try it without any risk, so we put in a heavy walled pipe.

We had one left and we were asking ourselves, "What shall we try now? What's the variation?" We decided that maybe the plastic lined pipe would show the biggest difference, and if we made that with an asymmetric liner, it would even be better. And so our tenth pipe had a plastic helix on the inside. The helix was only about fourteen mils thick, and the pipe wall was about twelve mils thick. So, it was like heavy scotch tape that we sealed to the pipe.

They fired the shot, and E.T. called me up and said, "You won't believe the results." And he went through the pipes. We had a couple of normal pipes on the shot, standard pipes with nothing in them. They put big holes in the target. All the pipes with the liners - - paper, plastic, and glass - - put bigger holes in the target. The heavy walled pipe put the biggest hole in the target. The heavy wall not only did not attenuate the jet, it made it the worst of all, directly

contrary to the lore. And the pipe with the plastic helix made no crater at all. We couldn't believe that plastic helix made such a difference. There was another pipe which had a lead helix on the outside to simulate the assymmetry we had used with the HE so successfully. It made a big hole in the target.

So, of all those things we tried, nothing worked except the internal helix of plastic. We thought, "Well, maybe there was a mistake in the experiment. Maybe a mouse crawled in that pipe and just blocked it off, or something." We couldn't believe that helix could be that effective. So we tried it again. This time we put in a steel helixe, a lead helixe, a plastic helix, and we tried some of the other pipes again. We had twenty pipes around the sphere this time. We fired that, and sure enough, all the internal helixes were just miraculous in their attenuation of the jet. At the time we didn't know whether we were reducing the source, or whether we were attenuating it. Eventually we learned that we were attenuating the flow. The helix has no effect on the source.

We tried many things of that kind, and it was a fascinating program, because we were studying all this parameter space experimentally. Things that would take weeks to calculate, you could just try. With twenty pipes we could try anything, and in a very short time.

I told Don Eilers at Los Alamos about the results, and he was really excited about them. He decided he'd try it on a nuclear test, so he put in a pipe with an internal helix, and one without on the Flora event, in 1980. He instrumented them to measure the penetrations of steel plates at the end of the pipes. And he found that he got a major reduction in the number of plates penetrated with the internal helix. Los Alamos has used it ever since, and so has Livermore, to reduce the flow of energy in the some of the longer diagnostic pipes.

Bass: I think the best thing we could do to help ourselves would be to put the helix back in the pipe.

Carothers: Why was it taken out?

Bass: Every event that has had a helix has seeped more than Joe (LaComb) wants. Now why? Why does he say it leaks? Because you are dissipating energy quite close in, into the stemming, more than you were without the helix. You're also getting in the

stemming where gas can go around your facility. That's the only leak that you're liable to see through geologic features, is Joe's point. As far as I'm concerned, all the DNA work I've done, or have been connected with where there was a leak, has leaked in the line-of-sight pipe. Except, where the leaks came from a region where there was a helix. There have been little leaks there. Otherwise, it has come right down the tunnel. I don't think we have ever, or at least very rarely, leaked through the formation. I think we leak through man-made facilities.

Carothers: How about the the experiments Carl Keller described which he had done to look at pipe flow? These were the HE experiments with a dozen pipes with a helix of this kind and that kind in them, and they were all on the same shot.

Bass: The trouble is, the helix on those HE shots is not the helix on a nuclear event. The helix works in an entirely different manner in a nuclear event than it does with HE. Carl said the helix worked late. In fact, it works early. If the helix works, the pressure outward on the pipe has to be increased. What we find on events where we had a helix, the pressure at 50 meters, which on DNA events is where there is a muffler section, the pressure out of the pipe is down by an order of magnitude if there is a helix. That says any reduction has to have occurred earlier. Either that or we don't have any idea how a helix works. Maybe we don't know how a helix works.

We do know that on one event the pressures went up inside the muffler section when a helix was used, and that had never happened before. That says we added disorder to the flow. We had pressures higher coming out of the muffler than we had going into the muffler, and we had pressure measurements inside the muffler which were high as you go through the muffler, higher than when you went into the muffler. This is very unusual, and it only happened one time. That one time we had a helix up close to the front. So, we added disorder to the flow. Where we didn't have the helix, the muffler didn't seem to do much. Where we had the helix the muffler did one hell of a lot. Which says to me we had all kinds of things going on. This has been discounted completely, and nobody has paid any attention to it.

Peterson: At about the time that Pac Tech split off from S-Cubed Norton Rimer and and myself started working on what we termed the late-time containment issues. Those are the cavity growth, the cavity conditions, leaks to the tunnel complexes, the ground motion, the thermodynamic and fluid flow process -- the very slow processes that you see.

Carothers: When you say the late-time, where do you pick that up?

Peterson: I suppose about ten milliseconds. All of these times are relative, but compared to the times of the explosion they are slow.

Carothers: I read somewhere once, and I think about it occasionally when I think about time scales for containment, that if you wanted to build an accurate scale model of the solar system, you would have to make it not much smaller than the system is. There are very large distances, and if you try to scale them to a reasonable size, then some of the smaller things, like much of the asteroid belt, vanish. They get so little you can't see them.

In a similar way, when you think about the time scale of containment processes, which goes from small fractions of a microsecond out to perhaps a few hours, how can you possibly scale this to where everything fits? It's got to be in chunks, in a way.

Peterson: That is correct, and of course, that is one of the real difficulties in looking at it, because we do, being people, tend to split things into problems we can digest. But when we do that, we lose the coupling effects between them, which can be very important. You arbitrarily split on what you think you can understand, and that has nothing to do with what might be the most important. So, if you look at the way we evaluate containment, we do have it split into the time scales of various effects. We will look at cavity growth, we will look at ground motion, we will look at pipe flow, we will look at leakage, and things like that, but they're all very, very coupled. And one of the things I think we've fallen short on is to look at the coupling effects between these things. In other words, pipe flow is coupled very closely to ground motion and cavity growth.

Duff: The early efforts recognized that the problem of flow in the line-of-sight pipe, plasma flow, is a very complex problem and very hard to calculate. It's complex because the hydrodynamics that we are dealing with is obscure. We don't really know the source of this axisymmetric jet. We don't know whether it is a jet of material which has been strongly irradiated, vaporized, modified, melted, whatever, and then subsequently is closed off under ground shock. We don't know the detailed nature of that closure. Is it truly an axisymmetric thing, or just due to the nature of the non-uniformities in the real world is it something less? I'm sure those non-uniformities influence the initial conditions. Nevertheless, we made an effort from day one to try to develop a numerical capability to allow us to calculate the flow of the material in the pipe, and the interaction of that flow with the pipe wall. That involves ablation, material entrainment, and all of the processes that get involved. It is a complex problem, and I'm not sure we ever did it very well.

We were also acutely aware of the aspect ratio of the problem we were dealing with. A line-of-sight pipe is a thousand feet long; it starts a few inches in diameter, and ends up a few feet in diameter, order of magnitude. If you look at the numerical zoning requirements for such a geometry, it is a horrendous problem.

Carothers: Well, the zones just have to be little at one end and big on the other.

Duff: Sure. And things don't work well. The codes don't work well if the aspect ratio is more than about three to one. This was in the early seventies; we had no Crays. We were working on a link to a Univac machine that existed somewhere else. Because of the aspect ratio problem we developed what was known as the UNION code that tried to couple three calculations. One was a flow of the plasma, created as well as we could do it. It was inside a cylindrical envelope which was a 2-D calculation of the stemming motion as a result of the ground shock as it propagated out. This was buried inside a 1-D code. The UNION code was to put boundary conditions between these various things. That was coming along.

Jerry Kent was my assistant in those very early days, and I had passed the contract over to him to run. Bjork was one of the major project physicists. These guys became aware of what we now talk

of as residual stress, and they used that awareness as the basis of a pitch to DNA to fund a separate company. DNA went along, and Pacifica Technology -- Pac Tech -- was formed.

Now, the point of this is not to bemoan the fact that part of my staff took off and formed their own company. The main difficulty from my point of view, and I think from the containment point of view, was that the intellectual enterprise of treating the phenomenology from a millisecond to infinity was broken right in the middle. A new interface was installed. We had the job of trying to define the initial conditions. That was Dismukes, who was doing some aspects of the very early time steps. Then Pac Tech had the responsibility to do ground shock calculations, not only in a one-dimensional sense, but in the sense of studying the LOS collapse, the jetting phenomena, and all that may be happening in the pipe. And then we were supposed to worry about what happens after that. Well, interfaces are awkward. They are awkward in the best of circumstances. They are particularly awkward in a competitive environment.

Rimer: The pipe flow aspects have always tended to be called late-time. They're motions that Mike Higginbotham computes for Chuck, and that we put in our pipe flow code, or at least we used to do that. Jim Barthel used to do that work. All that information, like the pipe flow, goes down to Pac Tech for the stemming motion calculations. Meanwhile, we are looking at free-field ground motion, model development, and then the later time aspects, like hydrofracture, porous flow, creep.

Carothers: Do you generate the input for the codes that Pac Tech uses for the stemming motion calculations?

Rimer: Yes, except the pipe flow has not proved to be very important. You can either include it or not, and you get the same stemming ground motion, for whatever reason. Now, what neither of us model is how that pipe flow affects the properties of the grout. The pipe expands, blows up, and none of us have attempted to model, because we don't know how to model, what that does to that grout material

There's a lot of overlap between Pac Tech and us. They do a lot of just traditional, straightforward calculations of each event. At the same time, I'm calculating ground motions. Here I'm talking

about the underground free-field motions around the tunnel, and in the tuff away from the tunnel. What are the proper models for the behavior of the rock? What causes the rebound? What causes the residual stress development? Why does the rock hydrofracture or not hydrofracture? How do we develop models to match ground motion data? I'm the guy who's supposed to develop the new stuff, do the innovations in ground motion modeling, etcetera.

Carothers: Byron, after Mighty Oak DNA made a number of changes in the design of tunnel test beds. The last few DNA events seemed to perform well. What changes were made?

Ristvet: Well, first off, the devices are lower yield, and so the driving forces on the stemming column are a lot less. And we've moved our closures out in scaled range a lot further, so the stemming anchor does not get challenged. Those came about after Mighty Oak, when we took a total relook at Middle Note, which was in 1987. That was the first of the low yield type of design that now has become our standard. We found out how to modify the radiation environment so we could use one source to provide all the various kinds of radiation environments. That's done with shims and filters.

Again, things sometimes appear in the containment world to be cyclic. Compare the following discussion with Byron Ristvet with the words of Billy Hudson and Carl Keller at the beginning of this Chapter.

Carothers: I could translate what you've said to mean that the basic cause of at least some of the problems was that the closures were too close.

Ristvet: Basically that's correct, and that happened in a couple of ways. We wanted to get to a standardized design. The reason for that was so we only ordered a standard section one and section two of the pipe, which are the sections from the working point all the way out to the end of stemming. If we could standardize that we would save a couple of million bucks every time we went out and bought pipe. Of course, that was for the old higher yield shots.

Well, everybody wanted everything possible. Number one, they wanted a large aperture so they could vary the exposure area if it was needed, depending on the source. We didn't use one source in those days, we used different ones on different shots, and some

had larger areas to look at compared to other sources. Apertures could vary anywhere up to seven or eight inches. In order to accommodate a seven or eight inch aperture you have to have a pretty good size bore.

Then we got the idea we could save a lot of real estate and money if we made the pipe shorter. So, the way we do that to get the same exposure area at a shorter range is to increase the beam taper, which increases the pipe taper. And so now we had increased our cross sectional area of the pipe, especially up in the front-end region, significantly. In closure technology in those days we were still using the sliding gates, the Modified Auxilliary Closure, or MAC. Because of the metals involved you can't make those any bigger than about six feet in diameter, and get them to close fast enough. Six feet is about as big as you can get an aluminum billet that's forged, and that has the strength that you would like.

So now we had to move things in significantly closer. Then we had them at a range where the grout stagnation pressures were far exceeding the door strengths. In addition, in the process of trying to perhaps eliminate pipe flow, we were actually making pipe flow worse. On both Misty Rain and Mighty Oak we had reverted back to using iron extensions rather than using the high density tungsten or uranium extensions. One of the reasons we did that is that Carl Keller had felt we were getting a fair amount of yield out of the uranium from the fast neutrons getting up the pipe. To me that never explained why we saw plutonium against the MAC, and even down further on Huron Landing.

Carothers: That's the way you make plutonium -- neutrons and U238.

Ristvet: Well, I know you can make it that way in a reactor, but it wasn't that kind of plutonium. There were some concerns about that, so Carl went back to the iron extensions. And of course, on Misty Rain the pipe flow jumped up again, and it shot the doors out with the pipe flow - - at least the first one, and probably the second one too from the evidence out in the test chamber. We had sort of a coating of iron, with a coating of aluminum, followed by a coating of grout, on every surface that faced the working point. Not only that, there was pretty good cratering back on the bulkheads and other things, because block motion prevented the TAPS from coming down. That is about as far out as we've ever seen significant

block motion, which is rather interesting. Anyway, it was not pipe taper alone, I don't think. It was not bore size. It was the fact that all of those things came together, and we brought those closures in so close that they were no longer effective stemming anchors.

We did a vertical shot called Huron King, and I and Jim Barthel, of S-Cubed, looked at all the old history. It became rather obvious that the HE machines in those days, because they were in so close, became shrapnel against the rest of the closures. That's why we moved our HE machine out to a similar stress range as we use for the FAC today. It was in part that experience that led us to put the FAC where we do. I really think we could go back to those larger pipe tapers, maybe 0.24 inches per foot, 24 inches per hundred feet, and be okay, if we were in a normal zealotized tuff.

If we have the FAC out at roughly a kilobar, because we know it can withstand two or two and a half kilobars so we've got a good factor of two safety, we have confidence in the ability of it to act as a stemming anchor, and not let the stemming go down the LOS pipe. I think my greatest concern in our current low yield design is the failure of the FAC to fire. Even though ground shock will close it, I don't know how much grout will have been shoved through it at that time, and whether the cavity pressure will be sufficiently far down so the stemming won't continue to hydrofrac and erode as it did on Mighty Oak.

Peterson: There have been a number of observations on some events that haven't performed just as we'd like that I find very interesting. You can talk about pipe taper, and sort of the bad performance. Or the performance wasn't as good once we went to the bigger pipe taper. That's true. We also went to a longer extension, and the performance wasn't as good after we went to the longer extension. Some of the shots worked all right, but they didn't all work really good.

People say, for example, that on Misty Rain and Mighty Oak, because we went to a bigger pipe taper we moved our first closures in much nearer the working point. If you really look at that, it isn't much nearer. It's really a very small distance, and the change in the dimensions don't even compare to changes that were made in some previous events.

I believe it was Dido Queen where, from a scaled view, we were in much, much closer, and it worked fine. We went to Diablo Hawk, where the first containment structure was much further out. The containment was okay, but the door got penetrated by the grout flow. Various people pick various different things to explain these things. People have also picked on material properties; there wasn't quite enough air void, or it was a little bit more saturated, and all that.

I guess the one point I would like to make is that if you go back and look at all of these things, and really compare all the previous experience, you can always find one, two, or three shots that worked fine given any of these things. And so, I know I don't understand it, and it's confusing. It makes you go over to what DNA is using now, which is a really strong stemming bulkhead. Given the fact that we don't seem to know very well what happens, or why it happens, maybe we should build something that should stop anything in the tunnel.

Carothers: It's another overburden plug, in concept. It's to hold whatever can get there.

Peterson: Yes. It should hold the most extreme conditions that we've measured so far. It might not be fancy, but one would hope it would work. I think when we want to get fancy we should understand all the things we know, and have seen. When that will happen, I don't know.

When the concept of the "residual stress" came up, people calculated it and could say, "Oh, I can see now why the gas stays in the cavity." One of the things that bothered Carl Keller was that now we understood the residual stress field, and the containment cage, we didn't want a hole to go through it.

That seems reasonable, but one of the things that has always puzzled me is that one of the first designs, on Dining Car in 1975, seemed to work fine. There was a pipe with a certain taper, and the closures were at a certain place. They had rock-matching grout out to a certain distance, and a superlean grout out further, and that shot worked. Now, the peak of the residual stress field on Dining Car was in the area in the tunnel where there was superlean grout, which is very weak. The rock-matching grout stopped inside of that.

At Pac Tech they started looking at these stemming plug formation concepts in more detail, They did a number of calculations, and it appeared that if you made the rock-matching grout column longer, and the superlean grout column shorter, you would set up a better residual stress field across the tunnel.

There is nothing wrong with that concept whatsoever, but that was the time when we started to go with longer reverse cones to lower pipe flow, and made a few other changes. We also started seeing these slight bits of gas seeping into the tunnel complex. Well, who knows? So, the Mighty Oak design went back to Dining Car. It had a rock-matching grout length back to what it was on Dining Car, and a superlean grout length back to Dining Car too. Well, obviously that wasn't the answer.

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Codes and Calculations

The development of computer codes for the calculation of underground effects resulting from the detonation of a nuclear device began concurrently with the first underground events. Bob Brownlee has described his work on the Bernillilo event, fired in 1958. At Livermore there was the Rainier tunnel event in 1957, where the principal objective was to contain the device debris, and the tunnel events for device development in Hardtack II in 1958. Logan, also in 1958, was the first tunnel experiment to use a line-of-sight pipe for effects experiments, and it contained well despite the almost complete lack of knowledge about how the detonation would, or could be contained.

The Plowshare program, which envisaged various civilian applications of underground explosions in a variety of earth materials, needed the capability to predict many of the phenomena that today are considered important to the containment world, principally those associated with the response of the earth to the energy release of the device. The device development events, fired in emplacement holes had considerably simpler calculational requirements. The appropriate depth of burial for the yield was really all that was thought to be necessary, and for that empirical rules seemed to suffice.

Higgins: By Hardtack Gene Pelsor's calculations had advanced, and John Nuckolls had developed a code called UNEC — the Underground Nuclear Explosion Code. It was later renamed SOC when John went to one of the device design groups. It was a simple one dimensional plastic-elastic code with a Von Meses solid equation of state, which doesn't allow much fracturing. But, it did do a very nice job, with the right adjustable coefficients, of reproducing what was going on in the Tunnel Bed tuffs. So, from shot to shot we could use that, and see that the shockwave pressure was generating a seal in the tunnel. What was wrong with it was, the equation of state of the material in the real world is not a Von Meses solid. It's brittle.

Carothers: Or, maybe you could say that it is a pile of rocks.

Higgins: It's a pile of rocks with cracks and holes. So, the rebound, that part which is really the important part of the calculation of containment, wasn't calculated. But the tunnel closing, and the pipes closing; all that was calculated very well. Our misunderstanding of containment was that we thought once the material was at a density of three, it was going to stay a density of three, and therefore we didn't have to worry about it any more. End of problem.

And that really was the end of the problem, in a way, because we could calculate out to maybe a 100 microseconds, if we really devoted everything we had to it. And that was only in one dimension. Peak pressures were calculated quite well, but that's about all. Rise times and decay curves were not calculated at all.

Rambo: I think some of the very first calculations at Livermore that had to do with containment were calculations done for the Benham event. Benham was a high yield shot with some kind of a satellite hole that was of concern. That was probably one of the very first sets of containment calculations.

Carothers: That's rather late in time if you consider that the Partial Test Ban Treaty was signed in 1963. Benham was in December of 1968.

Rambo: That's right, but we didn't have the material properties to do that kind of work, and they didn't do any logging. The only times they would do special cases of logging was on Plowshare related events. On those shots they would go in and do the best job they could to log the hole, even though the technology wasn't really very good.

The K Division people, the Plowshare group, were starting to do calculations. They had a code which was called SOC, which was a 1-D code, that they had started with. Seymore Sack and George Maenchen did some of the TENSOR work that was done. Then there was a kind of a split there. K Division took up some of those codes, and put strength models in them. Ted Cherry was the one who did most of the strength models. He did TENSOR and I'm sure he did some of the work that went into SOC.

Carothers: The original impetus for doing calculations on underground shots came from the Plowshare cratering program, but the names you mentioned are those of device designers.

Rambo: Yes, those device designers did the original SOC code, for the Plowshare people.

Carothers: SOC is a 1-D code, so that means you spherize the world around the bomb. What's a code like that good for? The world is not one dimensional.

Rambo: No, it's not one dimensional, but in the early days we didn't have a 2-D code available. So, by default we took the 1-D code and said, "Well, it seems to predict ground motion reasonably well, at least for the outgoing peaks, before the reflections take place." We never did any SOC calculations, or darn few, that related to containment until after Baneberry. There were calculations done for Plowshare. Then the 2-D calculations came along, and they were put together for the cratering shots. They were much better.

Ted Cherry, in the early days, would try to match field data with SOC. There was a lot of battling going on as to what was really in the code, and how much truth there was to the SOC code. It was under a lot of stress. People were not confident of what was happening. There were problems with matching the data, but that's what you have to do in these calculations. You take a first try at it, and then after the fact, you see what you can learn from it. What you learn from looking at the real data is what there is about your model that is wrong, or maybe you find out that the code was just plain wrong. It was a good feedback loop. When we did fail, we learned more than when we didn't fail, but we still had a lot of mysteries that we were not able to solve.

Calculations played a different part, in those earlier times at Livermore. People like Bob Terhune would walk over to our people and say, "Look, you can't shoot this. Our calculations indicate this, that, and the other thing." It might have been some private theory of his own. There were a couple of places we avoided because of that, and went to other sites. We wouldn't do that today.

Scolman: I am not aware, but I'm not sure I would have been aware, of us ever, ever deciding that a hole was not suitable for an event in the days before Baneberry, other than one time. Carl Keller, who was in the containment business for Los Alamos in those days, became concerned over an event in Area 4. I don't remember the name, but it was a reasonably high yield shot which was to be fired fairly close to the basement rocks, to the dolomite. Carl was convinced that we would generate enough CO2 that it could not be contained in the overlying rock. And he heckled us sufficiently that we finally moved the event to another location.

App: In 1971 Bob Brownlee hired me, Tom Cook, and Tom Bennion to start up the calculational effort for containment. We weren't interested in developing our own codes, not at all. We wanted to get something that somebody else had, and if we had to convert it for our use, fine. Tom Cook and I drew straws to see who would concentrate on which codes. We went by Labs, and Tom got Livermore. Tom got the 1-D SOC code from Livermore, and the 2-D TENSOR code as well. I got the WONDY and TOODY codes from Sandia.

We evaluated and benchmarked the four codes, to determine which would be the most appropriate for us. We chose Livermore's SOC, and Sandia's TOODY. We also used WONDY to a limited extent. Things evolved from there. SOC went by the wayside after a number of years because, although it had a lot of containment lore behind it, and was an excellent code, it was written in a language called LRLTRAN. When the Cray machines arrived, LRLTRAN was not implemented, so SOC no longer worked. Actually, Charles Snell, who now works here at Los Alamos, but who did work at Livermore, has it running again, on our machines. He liked it, and he converted it from LRLTRAN into standard FORTRAN.

The TOODY code has actually been our mainstay. It's what I've primarily used for modeling purposes, with a lot of modifications to fit our particular needs. It's now more of a special purpose code for ground shock modeling than it was at Sandia. We're in the process of benchmarking other, newer codes against it, but I haven't found any that are a substantial improvement. It has archaic coding. It has twenty year old architecture, based on the CDC 6600 system. It's hard sometimes to part with old friends. I know the innards of it; another reason for not wanting to part with it.

Carothers: Billy, when you entered the containment business in 1968 Livermore was no longer doing tunnel events, but vertical line-of-sight shots were being done from time to time. Were there any people in the Laboratory doing theoretical or calculational work on containment related problems? Things such as flow in the pipes, or ground shock closure of the pipes?

Hudson: I wasn't aware of any pipe closure calculations. I think they were starting to do them, but I have the feeling that was really in its infancy. There had been a very little bit done in terms of using the same codes that are used to design bombs to predict how a pipe would behave and close. All that started at very nearly the same time as the containment group was formed. We started then a program of code development for pipe behavior, in concert with the folks at S-Cubed, and also in concert with some folks from Los Alamos. For several years there was a fair amount of effort expended on code development, and in trying to describe how pipes really close.

Olsen: The codes that were available in the beginning were not very good for that type of thing. They were basically derived from the device codes. The early containment calculations were essentially all on front-end things, and at that time the device codes were used for that. We didn't really have anything beyond that, except for some engineering codes that looked at loadings on pipes, and how hard will you hit a valve assembly and will it hold up to 40 g's of acceleration, and that kind of thing. We did a lot by the seat of our pants.

There wasn't really any way to caculate pipe flow. We pretty much had to go in and make measurements to see what regime we were looking at. We tried to do some calculations on pipe flow, but in my opinion there never was any really usable code for that. The closest was a code called PUFFL. If you diddled enough of the many parameters in it you could get it to match things, but as a predictive capability it was pretty close to useless. It was sort of one step better than back of the envelope calculations. For example, if you knew what the burst strength of a pipe was, you could sort of say that if you put that pressure in the bottom of the pipe section, and the pipe opens up, you couldn't transmit more than that to the top of the pipe, because the pipe would open up and dump the flow into the medium. That's the kind of arguments we used.

We would do things like put accelerometers on valve housings to find out what kind of input the valve was seeing, and how it responded. We measured things like that, because we didn't have much in the way of a design or predictive capability for the dynamic environments, especially where there were multiple loadings on different time scales. For instance, on a valve there's a shock running up the steel pipe at one velocity, then ground shock, with a different wave shape, at a slower velocity. And somewhere in there is a loading from flow in the pipe hitting the closures. So, there is this multiple loading on things, and we didn't have any first principles way of attacking that. We did it empirically, which is why there was the emphasis on diagnostics in the early days.

Keller: On Monero and some other events, lo and behold, the radiation monitors in the holes showed the gas was going by the coal-tar plugs. This absolute seal in the casing was not there. And the pressures that were measured that were driving gas by those coal tar plugs were modest; forty-five psi or so. It was at that time that it was clear we needed a code to evaluate gas flow, because gas flow is a big deal in stemming, and in containment in general.

And so Al Davis wrote a 1-D gas flow code based on Darcy's equations. In a period of a couple of months we had that 1-D gas flow code. I said to Al, "But we need to evaluate uncased holes, so we need a 2-D code." He said, "Oh, it will take a year to write a 2-D gas-flow code." And I said, "Come on, Al. I just saw what you did for 1-D. We can do it in a month." He said, "Never. Never." So I wrote the equations, and gave them to John Stewart. John Stewart programmed them, I put in all the input-output statements, corrected the errors in the original program, and John debugged it. In one month we had an operating 2-D gas flow code. It was called JACTS, John and Carls TDC.

With that we were able to evaluate the differences between the cased holes and uncased holes. I took the Monero results, where we had like five pressure measurements in the cased hole, with the top two above the coal-tar plugs. And I took the driving conditions at the bottom as the boundary condition, and calculated a 1-D gas flow up the hole. From that I deduced the kind of permeability you had to have in the plugs in order to to get those volumes and pressures of gas above them.

Then I hypothesized that the casing was perforated. To do that I just removed some of the no-flow boundaries on that casing and let the gas flow out into the medium. And with that I proved to everyone's satisfaction that the amount of gas that you actually released out of this uncased hole was trivial, and yet there was an enormous drop in the gas pressure that was driving against the stemming column. So, there wasn't a containment argument any more about why uncased holes weren't appropriate, and Los Alamos folded on the issue of uncased holes. Another concern was that the holes wouldn't be stable enough, and would fall in during the device emplacement, but Livermore had already proven that that wasn't a big concern at all.

Carothers: The code you wrote must have been for noncondensable gases.

Keller: Right. The JACTS code is for noncondensable flow, and you can still use it for lots of things, but the next thing that was clearly needed was a steam-flow code. You need to treat the cavity gas with a condensable-flow code because the cavity gas was thought to be mainly steam, and that's not the same as the noncondensable gas that the codes calculated. There were all these arguments in the TEP about what the ramifications were of the condensable nature of steam.

It took me a year to write the KRAK code. I had never written a computer code before in my life, and the KRAK code took three thousand cards or so. It was a monster compared to the JACTS code. So, I had to begin to be really organized in my programming. And, I had to learn all the thermodynamics of steam, because KRAK included a full flow of condensation of steam in two dimensions. It did not assume local thermodynamic equilibrium. It treated the difference between the fluid temperature and the rock temperature, and the heat exchange between them. It was an explicit finite difference code, so it was easy to add to or change.

When I finally got it written I did a calculation which showed that condensable flow from the cavity to the walls got nowhere. Steam did flow into the wall, and it actually got in quite a ways, very quickly. Then it condensed and clogged up the pore space with the condensate. That throttled subsequent flow, and from there it just crept along as it pushed this slug of water on ahead. And that slug

got longer and longer as condensation continued. So, condensable porous flow from a cavity was not a containment concern. It wasn't even a concern in the stemming, generally speaking.

KRAK was slow, because it was very detailed. The idea was that while it would be so detailed that it would be too slow to ever be very useful, you would teach yourself with the code what approximations were appropriate, and then you could relax to a more useful speed in a simplified version. It still runs, with the same full-blown modeling, I guess.

Carothers: Well, the machines get faster.

Keller: Yes, the machines got faster, but it was still slow. It was dreadfully slow. Brian Travis took it over after I left the Lab. He worked for me one summer, the last summer I was at Los Alamos, and so finally a professional programmer got his hands on it, and he speeded it up a lot. He also gave me the idea of using an implicit solution for the crack flow. Al Davis was sort of the chief physicist consultant on the KRAK code, and Al's attitude generally was, "Well, it's going to be real hard to do that." Mine was, "Come on, Al. Let's do it. Tell me what the physics is and we'll do it." So, we got along very well. Al kept me correct with the physics, and I got him to hurry.

The next thing that was obviously needed then was a calculation of the greater threat, and that was that threat which had been witnessed in Bandicoot, Pike, and Baneberry, where a fissure propagated from the cavity to the surface. In other words, a hydraulic fracture. So I added the hydrofrac option to the KRAK code, and it's still being used.

Kunkle: When I showed up here in April of 1980, to begin work at Los Alamos, my security clearance was still not through being issued, but it was only a month before it was. I started work down at the G Division headquarters in White Rock, working on the KRAK code, which is a multi-phase, multimedia, steam-driven hydrofracture code. Al Davis and Brian Travis were working on KRAK at that time. Carl Keller had initiated this code back in 1974, with a code called JACTS, but he had left to go to DNA field command, to lead their containment effort.

We were trying to develop that code into an actual working code. At the time I first started it would simply not run calculations. Integrals would not converge, derivatives would blowup; there were the normal programming type of problems. I spent most of the summer of 1980 working with Brian Travis, running problems just to get an answer. We were trying to develop the physics involved in the KRAK code so we could get answers we thought might be right.

How codes and calculations are used today varies from organization to organization. And, the importance of the results of the calculations varies as well. The Livermore and Los Alamos events in stemmed emplacement holes seem to require little more than empirical rules to select a depth of burial. The DNA tunnel events involve the interaction of many of the phenomena produced by the detonation, and extensive calculations are done on how the experimental hardware, including the line-of-sight pipe, will be affected. The results of the caculations done often cause changes in a particular design.

App: We normally don't run calculations for every event, although that's really not a bad idea just to keep in practice, or to see if certain things pop up that are unrealistic in the calculation, or that might be suggestive of a problem we didn't anticipate. But normally we do not work in that mode. Usually it's a specific problem. For example, Dahlhart. We had a nearby pipe, a fish, that was stuck in a nearby exploratory hole. We didn't really know the condition of the pipe, and we were having a difficult time finding out what the condition was. The worry was that it was open, and passing through the region we regard as the residual stress field it could provide a path for cavity gas to get high into the geologic section. We performed some normal ground shock calculations, and used the shock levels to determine whether or not the pipe would be closed off, and how far it would be displaced.

That's an example of how we used the code on a specific event. We don't use calculations for absolute predictions — in fact, I don't even like that word in association with calculations. I prefer to use them as an analysis tool, as part of the overall analysis.

Carothers: If the containment scientist says, "I want these calculations run for this event," how would that be done at Los Alamos?

House: As Fred said, typically we don't do calculations on the events. There's no burning need for them unless we have some peculiar geometry in terms of emplacement. Or, a situation where we might want to look at the effect of the structural situation, such as a fault, or scarps, and so forth and so on. The containment scientist will, if necessary, call for calculational work to be done on whatever particular aspect he or she deems necessary. That's usually done hand in glove with the phenomenologist.

So, we do not do calculations routinely. We have a situation that's a little different from Livermore's. I believe that Lawrence Livermore Laboratory's John Rambo is the designated, and dedicated containment calculation guy. I remember John telling me once, "Well, I take a look at everything." And if he thinks something needs to be done, he may contact the containment scientist, or vice versa. In our Laboratory we don't have either a designated, or dedicated person. We have people who are supported by the containment program in the calculational venue, and who are required to be responsive to needs, but they are on a call-out basis. In some cases the phenomenologist will do the calculational work if it's in that person's particular area of expertise. Tom Kunkle, for instance, runs our KRAK code. Wendee Brunish runs a code called TOODY.

Rambo: Today, calculations are kind of nice to use to get things through the CEP, but nobody wants to look at the negative side of them. Nobody wants to say, "Look, we're going to have to move the site," or do this, that, or the other because we have a calculation that doesn't look quite right. Fred App, from Los Alamos, says, "Well, we never use calculations any more to decide about a shot. If it's negative we don't say we're going to make any big changes."

Sometimes I see problems in the calculations that I don't necessarily bring up, because the system has sort of bypassed them at this point. Calculations don't mean much today. The containment scientist can elect or not elect to look at calculations, if he so desires. He can say, "I don't need any calculations. I think past experience is fine." And so even though I may have a different idea on that,

it doesn't matter, and it can stop right there. So, I see the potential for going past a bad one - - one that may show an indicator, a blip, in a calculation as a potential problem. That never even gets discussed.

The calculator has his own view of things. What I've discovered over the years is that minor differences, or changes in certain properties around the cavity, and certain positions of layers, can make a big difference in a calculation.

Carothers: I have talked to various DNA people who say, "Well, you know, one of the things that's really kind of baffling is we have made what appear to be small changes in our designs, and we get big differences in things." As Ed Peterson put it, "I cannot understand it in the science that I learned, because if someone came to me and said, 'We're going to make a ten percent change in this,' I would say, 'Well, we only guessed at the first one, so what can ten percent do on the next one?'"

Rambo: Yes. I see sensitivities also. The difficulty in answering the question has to do with which problem you are talking about. There are so many changes you can make, and I'm not sure which ones do make a difference. Let me give you an example. Take Galena, which I presented to the CEP. The approach to it was, from the people who look at geology and look at material properties, "Oh, it looks like everything else we've presented before. We've got all these different Grouse Canyon layers that we've shot next to." But when I ran the calculation on it, and by the way I did it on my own, I said, "I think you people may have some problems. I think we ought to look at it." The containment scientist didn't want to do that.

Carothers: It's like doing a test with a weapon from the stockpile. If it works we won't have learned anything, because it's supposed to work, and if it doesn't work They will know that, and that's terrible. Similarly, your calculations will show the site is all right, which we already know, or it won't look all right, and then you'll give us trouble.

Rambo: You've put your finger on exactly what I go through sometimes. That's the biggest issue about calculations - - I really am not independent of the total system. And so I have this anchor around me, that you might call wanting to know the truth.

And that brings up the importance of the CEP, because that is the last decision making process. It isn't the last, but it's close to the last decision making process that takes place. As we continue with this process it's going to be harder and harder to move to a different hole, if that was something that should be done, because the money situation is probably going to get worse. I think that puts even more responsibility on the CEP to make good judgments on these kinds of things.

Carothers: Well, the CEP assumes good faith on the part of the Laboratories. Part of that assumption of good faith is that the sponsor has looked, seriously and honestly, at the the problems which might be associated with the shot that is being brought forward. And, they are going to bring those to the CEP meeting and discuss them, and why they believe any such problems have been satisfactorily resolved. If they know of a question, and they do not bring it to the CEP for consideration, they are willfully subverting the process.

Rambo: Well, there seems to be this idea that if you run a calculation there's something wrong with the site. And that almost stops the process occasionally. It's hard to get away with running a calculation on something, because the containment scientist is afraid. "Did you run calculations on this?" "Well, yes." "Why did you do that? What was wrong with the site that you ran calculations on it?"

The CEP does have the power, however, to demand a calculation, if they know enough ahead of time. If you have somebody or some people on the Panel who say, "This doesn't look right, and we want to know more about it or we won't pass on it," the calculations would be done.

Carothers: It's my understanding that these days there's a fair degree of collaboration between the Los Alamos and Livermore containment people. Does it ever happen that Los Alamos would say, "You know, you really ought to calculate this and see what it says." Does that happen?

Rambo: Yes and no. This usually takes place in communications before the CEP, in which we send each other questions. That has happened occasionally. But, it's never happened that we've demanded calculations from them. I have submitted a question and

said, "Have you run calculations on this, that, and the other?" maybe twice. But our side never demands any calculations from Los Alamos, or at least it doesn't seem as though we have, and we have very rarely if ever run a calculation on one of their sites.

Conversely, Los Alamos has run calculations on our sites several times. I don't know why this imbalance exists, but I think it's the perception of calculations from different sides of the fence. It's as though some of the management on our side is saying, "Well, calculations don't mean much. They're useful to sell to the CEP." When we run up against a negative calculation we're maybe a little more leery, but we're still saying the calculations by themselves don't make a big difference.

It's good to be aware of that, because the people making the decision at the CEP are much more savvy about this process than they used to be in the old days. I think they're able to say, "Well, this is a negative calculation, but there are other factors that take place that have to do with containment." I think that has certainly changed over the years, but we're still living with the remnants of the ideas that if you show any bad calculation to the Panel, they may give us a B or C. I think the Panel is a little more savvy in being able to make intelligent judgments.

Carothers: When you talk about calculations, do you do them for events of the various yields? Do you, for instance, do calculations on the low yield events?

Rambo: Sure. Galena was an example of that. The yield was not high. The layers went from full saturation up to this Grouse Canyon layer that was enormously porous. That to me was a flag that said, "Look, this is at the extreme, and you ought to run a calculation." Eventually I did, somewhat on my own, and that was presented to the Panel. If I had just laid back and not done anything, that calculation would not have been done.

What was kind of interesting about it was that it was different from everything else I've seen in terms of Grouse Canyon related calculations. There was a tremendous change in the attenuation rate. And that produced this focusing effect that I talked about -- the flattening of the out-going wave. For a normal residual stress you like things to go out spherically and come back spherically so it tends to close everything up in a spherical sense. When everything runs up as a plane wave, and comes back that way from the topside,

you don't get a big residual stress. The max cred calculation on Galena didn't show a residual stress. Nobody knew that at the CEP because it wasn't asked. The design yield showed a very weak ten, twenty bar stress along the stemming column.

Carothers: Refering to the Panel again, I think they can make intelligent judgments if they have the information upon which to base those judgments. And that's the point that concerns me. If they don't have all the information, such as not being informed of your calculations on Galena, how can they make an informed judgment?

Rambo: Well, you have to remember that certainly our models for looking at containment calculations leave some things to be desired. I see some of that after looking at comparisons between real data and the calculations. I'm not trying to sell calculations necessarily, because I know that there are calculations that may be misleading, and that some of them have been misleading. But in spite of that, we don't do too bad a job.

There's always been a tendency for there to be higher rise times in our calculations, compared to the measurements. It's like the ground is a bigger absorber than we calculate. But I think in terms of residual stress we tend to capture some of that, and yet there are plenty of arguments that say, "Gee, we don't think there's any residual stress in any of the shots." The DNA people are starting to say those kind of things.

The calculators tend to believe the reason the high yield shots contain is because there's a well established residual stress. You don't see anything going through cables, the man-made phenomenon of the hole is very small, and the residual stress is very thick compared to the pressure in the cavity.

Now, in the calculations you always seem to generate, for the same strength rock, the same cavity pressure. On a low yield shot there is less protective distance than there is on a high yield shot. When you get to low yield shots the man-made phenomenon become large with respect to other things, and that's been one of the ideas behind why low yield shots don't have as good a history as the high yield ones.

But there is Riola and Agrini. Looking at the geology of those two shots, there was just nothing there to calculate. There weren't any nearby layers, and calculations would not have done any good because they would only have shown a nice residual stress field.

Carothers: If you look at those two events, Riola I put down as an engineering failure. There were plugs which were supposed to be stemming platforms, but the one that was called on to actually do that failed, and was abraded away by the stemming which fell past it, so it didn't do the job it was supposed to do. On Agrini there was a strange, very deep crater which must depend on details of the geology that we'll probably never know.

Rambo: That's right. It's as though there are three aspects to this; there's the cavity gas, and there's the residual stress problem. But the third thing that can happen is a strange collapse, or some geological path that takes material to the surface. And, number three can bypass number two. If there is an unusual collapse, it may not matter whether there was a residual stress there in the first place, or what the calculations showed.

Duff: If we are to make progress in some of these containment issues, I think we need to think about how we calculate things. In the containment community we have a world view which assumes that a one dimensional, spherical expansion is the proper view of an explosion. That's where it all starts.

A zero-order approximation is a 1-D calculation. If you want to know about the effects of in-situ stress, or lithographic stress, you go to a 2-D calculation and put in gravity. And what do you know? You get a slightly different result.

So, we start with a one dimensional world. I'm suggesting that perhaps ground motion, for instance, in the DNA context, is governed by the scale of a fault or bedding plane type displacement, which is large compared to the cell size of a computation. Or certainly large compared to the size of the core we're going to squeeze in a press, but small compared to the final cavity dimensions. And it may be that the one dimensional approximation in this case isn't even a good zero-order approximation to what's going on.

I don't know what that means. I certainly don't know how to calculate it, or how to think about doing such a calculation. And that's something that has been thrown at me every time I make this

kind of an argument; that we really ought to open our minds to think about something beyond where we've been before. They say, "Well, gosh, we don't know how to calculate it." If that's the limit of our world, and the limit of our world view, we're sure not going to change that view. And we may not learn the truth.

Carothers: May I rephrase what I think you said? You're saying that the world is inhomogeneous on a scale which is large compared to your computational meshes, but small compared to the region in which the phenomena important to containment take place.

Duff: Perhaps. I have to emphasize the perhaps in all this. I've made a point in my career, and I've tried to make the point here that I was interested in trying to develop an understanding of what was going on, more than concentrating on getting the next shot off, or trying to meet a schedule. It's in this context of trying to understand something that I think the containment community, and I as a significant member of that community, have failed.

And that part of the failure has been in not recognizing the lessons that were learned from Rainier. One reason for me making this point was that within the last few years I went back and reread the Livermore reports on Rainier. They are kind of an interesting thing to look at. I commend them to the Panel. That event was extensively, and very carefully reentered and studied as an example of an underground test.

There is a statement in the reports that the cavity is reasonably well formed, and well defined. It's pretty spherical. And for a meter or so outside of the cavity the rock seems to be plastically deformed, and moves in pretty much a 1-D sense. Beyond that, ground motion is dominated by slips on faults, on bedding planes, and on fractures. Within a meter or so of the cavity ! That is, to me, evidence in the books, from the first shot, that says perhaps the one dimensional world view is not the proper world view.

Carothers: Good out to a cavity radius plus a meter, maybe?

Duff: Yes. But if you have a stemming column which goes out a little further, you may not get the right answers. But it is very hard to get real data, and actual observations. And we deal with the very real personal attitudes of the people in the loop. If you think you know what's going on, and it sort of works, and there's nothing

dramatic that makes you change your view, you say, "Gee, that must be the way it is." It has been shown time after time that we don't know that's the way it is. As witness to that look at Mighty Oak, or Mission Cyber and Disko Elm.

I have criticized continuum-mechanics based codes as inappropriate because the basic assumption that points that start out close together stay close together during the motion is not, apparently, what is observed in the field. Therefore, I have recommended that effort be directed towards the development of a three-dimensional discrete element calculational technique.

Discrete-element is basically a two-dimensional calculational procedure put together some fifteen or twenty years ago by a man named Peter Cundall. He's now at the University of Wisconsin. The technique, basically, imagines that you have interacting blocks of rock, which are are defined preshot, unfortunately. Unfortunately for our case, because we don't know what's in the earth in any great detail. But for civil engineering applications, for which he developed this technique, it's adequate.

In such a calculation there are predefined blocks of material, which in the first approximation are rigid, so these blocks can interact only through interfaces, where there are frictional forces that are defined. There have been extensions to the theory to allow elastic type distortions to occur also. The beauty of the technique is that one block is free to do whatever the forces that are at play ask it to do. It can change its neighbors however it wants to. It is not restricted to the continuum-mechanics assumption that there are proximity relationships that are maintained, nor is it restricted by the slip-line constraints that have sometimes been put into 2-D continuum-mechanics codes Those, I think, are always restricted to one class of boundaries which can slip. The J lines can slip, or the K lines can slip, but not both of them. In the discrete element codes both can.

Let me give you an example of problems I have seen calculated, on personal computers by the way. We'll take a hopper containing an arbitrary array of defined objects; square blocks, round blocks, triangular blocks, you name it. They're sitting in the hopper under gravity. Remove a diaphragm at the bottom of the hopper. The blocks are allowed to flow out under gravity, and they start sliding down. One will fall out, and another one will fall out, and they will

tumble, and fall, and they will pile up and do what they do in complete freedom from the constraints of usual continuum-mechanics calculational procedures.

Now, this calculational technique has been known, as I said, for fifteen or twenty years. There has been work on it at Livermore, and there has been work on it, supported by DNA, through Waterways Experiment Station. I don't know that it has been generalized to 3-D, but my recommendation is that a serious effort be made to try to bring up a practical and effective three-dimensional discrete element technique.

Carothers: My comment is that the development of calculational tools and codes within the Laboratories has been dominated, in the past, by the device designers. People were, and are, intensely interested in what happens inside a device when you fire it. That's where the the big kids play. That's where the interest, the money, and the effort has been, and so they have developed very sophisticated and elaborate ways to make those calculations. And in a device there are no blocks moving around, or hydrofractures, and they are usually symmetrical about some axis. If you're just a little guy who comes along and wants to do some calculations concerning dirt and rocks that maybe crack or jiggle around, you're probably not going to get a lot of money to do that. And so, maybe you adapt some of those techniques that have already been developed to your problem. That might be one of the reasons that has led to the widespread use of the continuum-mechanics techniques you describe.

Duff: I believe that.

Carothers: Chuck, inherent in the efforts to model, or calculate, the phenomenology of an underground detonation, particularly where you have a line-of-sight pipe, there is an enormous range of time scales. Things important occur from fractions of a microsecond to more than many minutes. Similarly, spatially, you have a thin piece of iron which is maybe going to do something and interact with things, and then you have, if you want to believe in block motion, a piece of rock, probably bigger this building, moving around.

Dismukes: You're giving good reasons why the modeling is not as simple as one would like.

Carothers: How do you deal with those things?

Dismukes: Not very well.

Carothers: You deal with it well enough to be successful some of the time.

Dismukes: Apparently. Or we're successful in spite of our ignorance. That's always possible. It could be that just doing everything on the back of an envelope would work every bit as well. It doesn't give you a lot of confidence that you know the effect of parametric changes in the design. We do a lot of that by the way -- not thinking the code is giving us the right answer, but that it will tell us what the influence of design changes are. Hopefully it will suggest what's good and bad when we start changing things.

Carothers: Without saying how good, or how bad.

Dismukes: Exactly. And that's the primary use of the codes, I think. I don't think any of us are deluded to the point where we think we're predicting exactly what happens.

Carothers: Where do the codes come from? Do you develop them?

Dismukes: It's a mixture. We've obtained some from the Labs. One of our early-time codes is really very similar to Livermore's CORONET.

Carothers: Your codes are all basically two dimensional, aren't they? Which came first, the codes or the pipe?

Dismukes: The codes. The codes were designed to deal with nuclear devices, which really were symmetric. The codes came first, because I think we were reluctant to experiment with non-symmetric test configurations because we didn't know how to calculate them.

Primarily the codes are two dimensional. Certain radiation problems you might do one dimensionally if you wanted. You could put in a little more sophistication because you could put better transport in for some things. In principle you can do that in 2-D also, but it gets expensive. The problem with 2-D codes is that they assume the world is axially symmetric. So, you get perfect symmetry, typically around the axis of the line-of-sight pipe. That would suggest that you probably get more jetting and energy flow in calculations than you do in the real world, because nothing is

perfectly symmetric. Certainly the stemming and the placement of the pipe in the tunnel is not axially symmetric; it's only quasi-axially symmetric.

The pipe is symmetric, but it's not located symmetrically. The tunnel itself is not mined symmetrically, and the pipe is not placed exactly in the center of the tunnel. There are support structures, and possibly experiment stations on one side and not on the other, and generally that's ignored. The codes are basically two dimensional codes that calculate cylindrical, axially symmetric kinds of things, and there's no dependence on the axial angle.

Broyles: I still find myself concerned about the role of calculations and the lack of what I think of as an appreciation of the limitations of 2-D calculations versus a real 3-D world. There are a number of people on the CEP, and other places also, who still think of 2-D calculations as the ultimate calculation, not recognizing what to me is a very serious limitation of 2-D calculations. That is the fact that in the 2-D expansion, instead of a 3-D one, you can, particularly in complex geometry, produce a calculation which can scare you to death, when there is no reason in the real world to be scared. In a 3-D world things go as 1 over R cubed when you talk about reflections and things like that, instead as 1 over R squared. I suppose that's a minor point, because it may cost you money and effort unnecessarily, but if you're aiming to be conservative it's at least in the right direction.

People can certainly do better 2-D calculations today than they could fifteen years ago. They have also learned to do parameter studies with 2-D calculations in a much better way, and apply them better. I think an area that can be exploited more is that we do have 3-D calculational capabilities now, particularly for late time, slow, ground motion calculations. In many cases those can be the important response, particularly for the tunnel collapse, the upheavals, the fault motions.

Bass: With the thought in mind that block motion could affect the residual stress field, through our DNA Containment Advisory Team working group we asked that a certain problem be run, and that we be aprised of the results of it. We wanted a discrete element code used, and S-Cubed was tasked to do it. We asked them to put

a fault in, and see what that does to the stress cage. And indeed, those calculations show that if the fault is in the wrong place, it kills the stress cage.

Now, you don't need a discrete element code to do that. Any finite difference code will tell you the same thing. But the finite difference code will only tell you about one fault. You can't put in several faults. With these discrete elements codes you can put in numerous faults, and address an area. We have recommended, and I think someday somebody will do something about it, that if a DNA test area shows multiple faults in what we call the stress cage region that should be addressed with a discrete element code. I think we need to do this.

Certainly a finite element code can go 3-D a hell of a lot easier than a finite difference code. Sandia has some going; certainly a lot of people have some going, but they are not being used, as far as I can see. We're really falling behind with what we should be doing with 3-D codes. We have the capability now to run those codes. People can still think, and with the capabilities we have now we should be running 3-D problems, and people aren't. All of the 3-D calculations have just kind of died.

Patch: Even a 1-D code is useful in a lot of material property studies. It's fast, and it does some of those sorts of things pretty well. Almost all of our serious work is done in 2-D. We do limited work in 3-D, and we're prepared to do it. It's just that one has to be sure that there is something that is truly 3-D, and that the 3-D effects are so important that you can either give up on the zoning resolution that you can achieve in 2-D, or you bite the bullet and pay the cost of doing a 3-D simulation. There are instances when one does that, but they're relatively rare.

I think the most effective way to use 3-D calculations is to home in on limited features. For example, you could ask yourself, "Does it really matter that the tunnel has a horseshoe shape as opposed to a circular shape that the 2-D code forces it into?" In addition to that, DNA typically offsets the line-of-sight pipe. They put it on one side of the tunnel so people can walk on the other side because it makes it nicer to work. So now you have a funny shaped thing with the line-of-sight pipe set off in one corner. Is that important?

The way you do that in 3-D is not to mock up everything from the zero room to infinity, but you take a section of it and look to see whether it behaves in a sensible fashion. You can make a lot of progress in 3-D as long you restrict yourself to a particular question. What you often times need in 3-D is to run a benchmark in 1-D or 2-D, so you can say it's 10% more, or it's 100% more, or 0.2% more. Many times, using a 3-D code, you get an answer, but you don't have any scale, so you have to invent your measurement tool to go with it.

We're currently working on getting a 3-D axial, and quasi-axial symmetric code in which things can vary with the azimuth. At that point I think we can begin to investigate some of the things we see with more confidence.

Carothers: You'd be able to do something like having the wall thickness vary?

Dismukes: Yes. Or you could even have a cross section be slightly asymmetrical. The problem with that kind of code is that if you get to large distances you don't have good angular resolution, but maybe you don't need it there. You can put good angular resolution where you need it, near the pipe. I have high hopes that one day we'll be able to do something with that code. It's just about to come on line.

The usual event in an emplacement hole doesn't have the complexities of a line-of-sight event in the tunnels. So, as Fred App and John Rambo have pointed out, calculations of the phenomenology following such a detonation are not regarded as very important in the planning of an event. And so, development of better calculational tools is not an very high priority project at the Laboratories. Part of the reason for that is that one could argue better codes would have to have better input data, which is not availabe, nor likely to become available.

App: I think the important thing is that we are in a very data-limited environment. There's no doubt about that. And that's the reason we're not predictive. We're not constrained by enough data. We're data limited. I think we have pretty strong analysis capabilities, but we have to always couch it as, "These are the limits of how this rock might really behave." Or, "These are the limits we might have on the phenomenology." We are not truly predictive.

Carothers: How useful would a 3-D code be?

App: I don't think a whole lot more useful to us. There may be some special circumstances where we might want to know what the effect of a fault is, or something like that. I think that 95% of the cases, even if we had a beautiful 3-D code, would probably still be done in 2-D. The world is, to a first approximation, with layering and all, two dimensional. We still do a lot of useful one dimensional calculations, and the world certainly isn't one dimensional.

Now, there are specific cases you might want to look at with a 3-D code. You might want to look at a three dimensional geometry to assess the assumptions made in a two dimensional case. For example, how might a sloping layer affect the results, and in what way? But even then I think we normally would use a two dimensional code for the main part of the study. I think that's how we would approach it. A 3-D calculation is an order of magnitude more expensive to do, and more complicated besides.

And again, in 3-D we are still data limited. How are we going to know how things vary in three dimensions any better than we do in two? The real problem lies with the material properties. I think that's the real issue; the material response.

Carothers: Dan, how do you get the numbers, or the information to build your material models?

Patch: That comes about in a lot of different ways, and hopefully each way adds a little bit of information, so the composite of all those little bits of information makes the model, with its strengths and its failings. The codes, because we've spent a lot of time perfecting what I'll call numerical techniques, by and large do an extremely good job of solving the equations of motion, and doing all the things they should do when they have the right kind of zoning. But they only do what the material model is. So the material model in these calculations is far and away the most important factor, or unknown, or uncertainty in the calculations.

We are frequently accused of just taking the post-shot data and tuning the calculations until they give the best agreement, and then using that model until something new comes in, then tuning again. While I can't speak for others, I don't think that's an accurate description of what I do, and I don't believe it's an accurate description of what a number of other people do either. We try very

hard to make a model based on information that comes from tests on the rock itself, and not from some kind of global empirical data base. And so, it is always personally bothersome to me when people say, "Oh yeah, they just tune their models. They only reason their models do what they do is because they've adjusted all these knobs." That, I think, is a very unfair characterization of what we do. That's not been our approach.

Certainly we compare the results of the calculations to what happens. It wouldn't make sense if we didn't. That's just a basic way of verifying that the calculations are doing the right thing, and often times they don't. Certainly there are sometimes minor features, sometimes major features we're not happy with. I think the appropriate question then to ask yourself is not which knob do I have to turn to get this thing to come out right, but to ask yourself which piece of physics is missing from the numerical models, or if it seems like one knob has been turned the wrong direction, how can that be? It is not to simply say, "Well, if I adjust this part of the model everything is wonderful," and go on. To me that's not science at all, and the implication that we do that I find very bothersome. So, our approach, at least philosophically, has been to verify the calculations, and when we have a problem to use that as a red flag to ask what's missing, and what do we need to measure, and then try to devise some way of getting this new material property we haven't had before. And we're not always successful in doing that. We certainly have our mysteries.

There are a number of models, and each has its drawbacks, and each has its uncertainties. I suppose, to some extent, one rejects those models which give features that just don't seem to be consistent with what the experience in the field is, and if someone wants to say that's how we're tuning the calculations, by winnowing through alternative kinds of models, in that sense I suppose we are tuning the models by rejecting those that seem to be non-physical.

Carothers: I don't know what else you can do. I find it hard to think that you could sit down and from first principles derive a model that would describe what goes on in this very complicated material under rather unusual circumstances.

Patch: If there were something else I could think of, I'd do it.

Rambo: Over the years of doing these things I've tried to bring some ideas together about how calculations play some role in some shots, and on other shots they really don't play any significant role at all. I was looking at what happened after the shot. What's the report card look like? How well did we do; did the calculations mean anything at all?

There's a group of shots that we've done calculations on where we were trying to deal with issues that were of interest for a number of reasons, where people felt there were containment issues that needed to be calculated, and the calculations did matter. We've run these calculations on a lot of events, and they looked okay. And at least in looking at the post-shot analyses where we're looking at radiation, we didn't see anything. So in a sense the calculations have been helpful to kind of get things through the CEP, and maybe there's a connection between the calculations and the fact that we didn't see anything.

Then there's the area that I call kind of worrisome. Worrisome areas are like Roquefort and Coso. Roquefort was probably in the thirty-some kiloton category and had radioactivity that went past the bottom two plugs, and we'd run calculations and presented them to the CEP. It bothers me to run these things and say, "Well, there's still a residual stress even though there's this hard layer that runs through it and there's a lot of perturbation, and there's this Grouse Canyon layer that's close by. It bothers me, to some extent, to run these things and go down to the CEP and say, "Gee, I didn't see anything calculationally." And then afterwards, think maybe there was something I should have talked about.

As a postscript to this chapter, and a case study of the current role of calculations on an emplacement hole event is the Barnwell event. Barnwell, with a yield in the 20 to 150 kiloton range, was fired in Area 20, on 12/08/89. John Rambo was the Livermore containment scientist.

Carothers: John, I know you were quite concerned about the containment of Barnwell. And, let me say that if anyone were to look carefully at the post-shot data on Barnwell, they might conclude that your apprehensions were well founded. There are people who think Barnwell came very close to being a containment failure.

Rambo: I guess I'm one of those people, even though I didn't say it publicly. Barnwell. Well, in the beginning we didn't suspect there were any problems whatsoever. It was down toward the southern end Area 20, in 20az, in new territory, so to speak.

Carothers: It had a yield in a range where we've never had a problem.

Rambo: Or never had seen a problem. Sometimes we haven't looked.

Carothers: We've never had an escape of material in that yield range.

Rambo: That's right. And we didn't on Barnwell either. But that's what we're really discussing. In the beginning I decided to do calculations for an issue that had to do with the CEP, and that was a possible low scaled depth of burial. There was uncertainty in what the maximum credible yield was. We couldn't lower the device any deeper in the hole because the hole was crooked and we'd get alignment problems. So, as the containment scientist I was stuck with a depth of six hundred meters, and I was going to run calculations for scaled depth of burial purposes.

There, the CEP played an important role for the wrong reasons. The whole thing was serendipitous. It was really that way. The material properties, for up on the Pahute, came right in the center to about everything we've looked at in past experience. And the logs were straight as a board. We didn't see any reflections for two hundred meters. It just looked marvelous.

So, I didn't see any problem with the shot. The one thing I did see was, "Gee, the drilling rates for that last two hundred meters look kind of slow." I hadn't seen anything that looked quite like that, but we've had experiences in hard layers here and there and all over the place, and the geology for the Molbo event was like that for quite a ways, and there was no problem there. And so I just tossed it off. "There's no problem with this shot."

Another factor that was important here was that to exercise our ability to measure core samples we went in and took some core samples and measured them in the laboratory. But as a test, before we got the answers back, they said, "John Rambo, we want you to make an estimate of what you think the strength is of this rock." Well, I looked at one version, which was the Butkovich model, which

gives you default values of strength, and I looked at that. I looked at a nearby shot called Hardin, where we had samples that were measured, and I looked at the Hardin cavity radius, from which I could back-calculate strengths. I looked a little bit at the drilling rates, because I had some ideas about what they tell you. Well, I increased the strength quite a bit from the default values that you would get from Butkovich's model, which takes average properties. I came up with a value that was about half of what the measured values were from the laboratory measurements. That rock was much stronger than I'd estimated.

Later on I had some DNA calculators estimate it, and they said, "Yeah, that's about what we would have estimated too," meaning their estimates would have looked like my estimates.We looked at the inside of the hole with the movie log, and it looked like it was uniform stuff all the way up. We didn't see any cracks, we didn't see anything but uniform material. But, above that layer there was a layer with a lot of gas porosity. We took samples out of that layer, and you could break them apart in your hand.

So I ran the first calculation. A big shockwave goes up, traveling in almost fully saturated material up to this two hundred meter level in hard rock. Then it comes to this layer of very weak rock. Lo and behold ! An enormous reflection comes back. In the calculation, just as the rock tries to hit rebound, or to set up the residual stress field, the reflection caused motion which unloaded the residual stress field around the cavity. I just didn't expect that. This calculation looked bad. It had three hundred bars of cavity pressure, and almost nothing outside of it. I had never seen a calculation show something as bad as that.

So, the next object was what we could do to try to save this shot. They said, "Nothing is going to happen." I said, "Well, I'm not sure you could even contain the gases getting up into the stemming column on this." So they said, "Oh, all right," and went back and measured down below the shot, where we thought there was weaker rock, because the drilling rates were higher. And sure enough it was weaker. I put this new model into the calculation, and yes, that helped reduce the cavity pressure in the calculation about to the point where it was equal or about the same level as the residual stress. That made me a little happier, except that when you went to lower yields the effect of this weak material below the shot point started to go away.

So, I knew all this, and I went down to the CEP, prepared to present it to the Panel, if necessary. I thought we ought to be able to contain this thing because it was right on the edge, and besides, nobody on the Panel believes in calculations. I came down with about six notebooks full of viewgraphs to present in case somebody wanted to get into the subject. The rest is history. Dr. Brownlee said, "Calculations don't make any difference." Fred App was starting to get interested in the subject, but he was sort of swept away by Brownlee's strong statement. I was sitting there with my mouth open, thinking, "Boy, did I get through this one easy."

But I was still quite worried about what might happen, because in my calculational experience I had never seen anything quite like it. On the other side of the slate, we had all the experience of high yield events that had never shown any problems. I even went over and calculated a nearby event, called Lockney. I didn't have any measurements, but I tried to back-calculate from the cavity radius and the drilling rates. It didn't look good either, but it was very, very sensitive to minor changes in strength. That was what was interesting about this whole thing. We didn't appreciate how, with high strength you can get these enormous changes in residual stress for slightly different properties. It comes and goes with very minor differences in the strength, and it can be catastrophic if you hit the right combination of timing and of reflections.

As I said, Lockney showed poor residual stress also. But it contained. Lockney had something like five percent water content, and Fred App has calculated other events where he says, "You know, the calculations look pretty bad up there on Pahute, but there's still this very low water content that they're shooting in." And so, I thought at the time that maybe they were right. Maybe containment calculations don't make any difference on high yield shots. Maybe you can shoot anywhere, in any material, and who cares. So that did affect my thinking on Barnwell.

So, I was concerned already, and then about ten minutes before shot time the device physicist came up, and remember that this thing gets worse as you go to lower yields, and he said, "By the way, I just did another calculation. You'll be pleased to know the yield has gone down." It wasn't more than about twenty minutes later that I saw all this radiation going up through the stemming column, up to the last plug. I think we came very close on Barnwell, and the calculations certainly pointed in that direction.

20

Current Practice

Over the years the Laboratories have developed certain practices for the conduct of nuclear operations at the Test Site, including those which relate to containment. After all the theorizing, the designing, the calculating and the planning has been done it is the people in the field who do those things that make the reality of a nuclear event.

From the earliest days of nuclear test work it was recognized that a field operation was a very complex undertaking. Leaving aside the many organizations that were involved in planning, building, providing, and operating the necessary support functions, there was the need to coordinate the activities of the Laboratory people themselves. The Test Directors were the people who had the ultimate responsibility to see that the plans for a particular event were carried out. They served as the authority, at the Site, for the work that was to be done, and that would be done for an event sponsored by their organization. There were several interfaces to be managed; those between the Test Site management, the various support contractors, the Laboratory management, and a multitude of Laboratory people, each with a strong interest in having their experiment taken care of first. Meeting the containment requirements was just another part of the job. How these things are done has changed over the years.

The responsibility for the direction of the Livermore field program is shared by two Test Directors.

Carothers: What does a Livermore Test Director do?

Page: That's a big question. I consider the Test Director, foremost, to be an operations manager for a large field project. A big part of the responsibility has to do with safety.

Carothers: Does it include the things related to containment?

Page: That's a part of it, but the real focus on that belongs to the containment group. But since the Test Director is the man responsible, on the spot, he essentially owns all of those aspects, to first order, from nuclear safety to containment to industrial safety.

Roth: You pick up an event somewhere in its definition stage, and actual production stage. When the event becomes active in the field, the Test Director becomes the lead man in charge of it at that point in time. He picks up that responsibility from a project physicist, who shepherds it from its inception to the point where it's going to the field. I concern myself, first of all, with getting the fielding done, making sure the facilities are available for the canisters and experiments that have to be fielded, coordinating the craft support to carry that out, determining the safety and security requirements of classified gear in the field.

Carothers: Let me start with industrial safety. When do you become responsible for that? I would think REECO, for instance, would do that.

Page: There a couple of aspects to that, but the Test Director assumes ES&H coordination responsibility from the DOE for the shot site at a certain time. It's a formal transition of responsibility. Up until that time NVO had assigned that to REECO, and so REECO had that responsibility. When that transition happens, then the Laboratory gets it, and the Test Director is the person who assumes that responsibility. What that means is that he is responsible for the coordination of the activities at the shot site to assure that they're done safely, that all the independent contractors know what's going on, and that they know what the other people are doing. He has the responsibility to make sure there is a well-coordinated operation. Of course, each contractor is responsible to assure that their people know their jobs, and that they do them safely. But the contractors take their direction from the Laboratory, and then they apply their methods to get the job done.

The craft support is all through the contractors. REECO provides the crafts we need. For security, we call heavily on Wackenhut to do the guard duty we require. We determine the requirements and we lay those requirements on those people, and they, hopefully, carry them out, and we oversee that they are carried out to our specifications.

As things progress I become very busy in overseeing the emplacement of the canister in the hole - - the handling of the cables, the operation of the cranes, all the necessary activities that go along with the carrying out of the event. Also, I oversee the stemming and containment requirements, making sure that the materials that are put in directly around the canister and around the bomb itself meet the required specifications, and that the various plugs, and the gas blocks are properly installed. There are a myriad of details like that.

Roth: The legality of it is that the Test Director is in charge, but of course, it's a cooperative effort with a lot people involved, and you listen to what they have to say.

Carothers: Let us say the device has been delivered. Inside that fence is the Laboratory's area, your area, isn't it?

Roth: That's right. You're talking about a safety and security issue now, but at a point in time, which I normally define as a significant Laboratory presence and activity, that's when I legally take responsibility for that site from DOE. Basically that's when the diagnostic canister first comes out to the site and gets installed in the tower, and significant work and activity goes on in finalizing the experiments. That's inside the perimeter fence, of course.

That is not normally the time when security is on the site. Security is usually not established until significant classified material comes on the site. In some cases there won't be any classified material in the tower where they're installing diagnostic equipment, depending on what kind of event it is. So, it could be as late as a day or two before the full power dry runs before we establish security on the site. And full power is typically a few days before device delivery. But from that point in time we have a secured site, where you have guards on a twenty-four hour basis, making sure only authorized people are allowed access to the site.

Carothers: You said that you legally take over the responsibility for that area inside that perimeter fence. That means you have the responsibility for the actions of the contractors' people?

Roth: We interface with those people through a group at the Test Site which used to be called the Emgineering and Construction group, but that has been recently changed to C&DE; Construction and Drilling Engineering. They actually do the interfacing with the

contractors. They give the requirements for the number of carpenters, and wiremen, and so forth that will be needed on a particular day. They interface with the craft people, with REECO, on a day to day basis. And they essentially report to me, from the standpoint of getting instructions about when we need the tower up, or what we need there, or where we need a work station, and so on. And, they implement those instructions. So, I don't deal directly with the crafts, but they are reacting to my requirements.

I'm responsible for safety, and security, ultimately. That again is a delegated effort; I can't be in every location at every point in time. You have to depend on a lot of people to uphold those requirements. But ultimately it rests on me.

Carothers: Usually only when something goes wrong. Then it's suddenly, "Well, Bernie's the guy in charge. Go see him."

Roth: That's right. They never say that when everything is going smoothly. When something goes wrong, everybody's willing to admit I'm responsible.

Carothers: Do you get involved in the site selection?

Page: No. Only to the extent that the site meets the needs of the field operation, and will allow us to do the experiment we want to do. It has to be the right depth, it has to be the right diameter, it has to have enough room for the trailer park. Ground motion is a big issue, and you don't want the hole located where there could be damage to some facility.

Roth: Somebody says, "We have a device here of X yield, and we need a hole to accommodate it." That falls into the containment area, and they say, "Oh yeah, we have holes A, B, C, D. Then they look at the yield, and the device, and determine the depth of burial that's required, and they say, "Well, this is the hole it should go in." If there are unique requirements for some reason we may suggest differently, but basically that's how it happens.

I don't say where a new hole should be drilled. The geology people and the Test Site people make that decision. But we have kept a running cognizance of what holes are available, and as a drill rig becomes available we might say, "We need another high yield hole on Pahute someplace, so give us a high yield hole." Then we coordinate that with other activities to see that we're not a half mile

away from another high yield event that could go off in the same time period. But the specific location and coordinates are not my choice. That's the geologists.

Carothers: Jack, as the Los Alamos Containment Program Manager, what interaction do you have with the J-6 field operations people? Do they work for you, or are they a separate organization?

House: They are separate and apart. First of all, they are in a different division. Although containment is in the Environmental and Earth Sciences Division, we work for J Division. I consider Jay Norman, the J Division Leader and Program Director for Test, to be my technical boss. Field operations, J-6, are people we work hand in hand with from the very first definition of an event, when we have to go pick a hole.

Carothers:; Who selects the site for a new emplacement hole?

House: I do that. I and a colleague in J-6 work hand in glove on the site selection; where are we going to drill the hole, and how deep are we going to drill it, and so on. I may have picked a set of coordinates on the NTS map that, when the field operations folks actually go out with the surveyors to drive a stake, they find is in an arroyo, or is near a power line, or what have you. So, there is a lot of interaction with the J-6 people. Those guys do not work for us; we work together. They also take our containment criteria and develop a relatively standard and basic stemming plan for each and every event.

Carothers: Jack, you always have the same stemming plan.

House: Well yes, more or less. It's got the same basic ingredients. It's got alternating layers of coarse and fines material. And it's got a grout plug here, and two TPE plugs there, but the locations of those are specified by the containment scientist, and his or her event team. J-6 merely translates their requirements into a blue-line drawing, which ultimately goes to the field for execution.

Among other differences in the way Livermore and Los Alamos conduct their field operations is the manner in which they lower the device and diagnostics hardware down hole. Los Alamos uses wire-rope harnesses, Livermore uses drill pipe. The origins of the difference seem to be lost in the past.

House: If there are valid reasons for the difference, I am not aware of them. I do understand that Livermore is able, on drill pipe, to put a much heavier package down hole than Los Alamos can, even on a four wire-rope harness. We started in the early days just using two wire-rope harnesses. And, as the diagnostic packages got larger, and longer, and heavier, obviously the capability to lower larger packages became necessary, and they added more wire-ropes. There are now two, three, and four rope configurations they use, depending on the size and weight of the package. But as far as how the difference between the two Laboratories as to drill pipe versus wire-rope came about, I don't have the vaguest idea.

In the Test Operations Review Team activities, which has since turned into a effort that is known as the Joint Test Organization, which has the aim of combining Livermore and Los Alamos resources at the Test Site, there has been a consideration of using one system or the other. Interestingly enough, long and hard as it has been studied, I think the ultimate resolution was, "Well, Los Alamos will stay with wire-rope harnesses unless we get a package that is just absolutely too big, and then we'll do it using the Livermore system." So, it's still unresolved.

Carothers: Perhaps you know, Bernie. Livermore emplaces the device and diagnostic hardware using drill pipe. Los Alamos uses wire-rope. Why is there a difference? I'm sure you think drill pipe is better. Is it really, or is it just another difference between practices of the Laboratories?

Roth: Those preferences were developed before I became really established in the program. I remember seeing one or two of our events put down on flat wire rope. That was still in the developmental, or experimental stage at that time. Before I got fully on board that was put aside and everything was done on drill pipe after that.

So, I grew up with drill pipe. One reason for it that I'm aware of is that drill pipe has a much higher weight capacity for putting down a package than a wire-rope set. It's been developed over the years to where we can put a million or more pounds down hole, and we have done that on a few occasions. The one event that comes to mind was Flax, and if I remember the number right we were looking at a 940,000 to 960,000 pound load. Los Alamos has

gone from one cable to two cables to four cables, but I think even their four cable capacity does not equal our heavier drill pipe capacity.

Page: I can't answer the question of why we first started using drill pipe, but the reason we like using it today is that drill pipe offers a heavy load carrying capability. We believe the joining method is reliable, and it's something we can test. And, we've had good luck with it.

Carothers: What do you mean when you say it's something you can test? Do you pull test all those joints?

Page: Yes we do. Of course, then we have to unmake them.

Roth: There's a very strict quality control program involved in all of that. The pipes are first of all threaded and inspected, and then pull tested to some 125 or 150% of what the working load is expected to be. They are then very carefully maintained from that point on to see that they aren't damaged in any way, even to the extent of seeing that somebody doesn't sabotage one of them. They're brought to the event site, put into an enclosed area, and maintained there until they're used. The threading operation itself has a quality control on it, in that the pipe joint makeup has to fit within certain tolerances. The threaded joints are marked with a small diamond, so they have to thread up into some portion of that diamond. Going too far or too short is not acceptable. So, we have very good assurance when we go to lift that load that joint is going to be good, and that pipe is going to be good. And it's special metal. It's not necessarily old D-36 steel; it's API pipe.

Carothers: Do you have to use a drill rig to put the device down?

Page: No. You can use a drill rig, but the emplacement machine can be a crane, a sub-base, and a stabbing tower. The sub-base is a working platform that allows us to tie off the load when we let go of it with the crane. The process works pretty well, and it's reasonably fast.

Roth: The crane actually holds the load, and lowers it pipe section by pipe section. And we use ancillary cranes that feed the pipe up to the stabbing tower. The drillers thread it in, the main

crane picks up the load, releases it from the grips, and lowers it down. People underneath the sub-base tie on the cables and put on the experiments that go on the pipe.

Carothers: Once it's in place, you have to fill the center of that pipe.

Roth: Yes, but that's relatively easy. We just grout it up.

There is a stemming plan for the hole that we adhere to that's defined, and reviewed, and accepted prior to the time we actually carry it out. That involves perhaps a half dozen different types of material. Boron rich material might be emplaced around the device itself, for neutron shielding. Above that, depending on what the diagnostic requirements are, we have overton sand, or perhaps magnetite, perhaps sometimes a mix for neutron shielding. Once above the canister, generally it winds down to a sand, gravel, and eventually a plug configuration.

Gas Blocks and Fanouts

Carothers: Who at Los Alamos designs cable fanouts and cable gas blocks?

House: The field engineering folks do that, and then they bring the design to the containment group for review. We have specs, and both Los Alamos and Livermore use the same specs for field installed, or discrete, gas blocks. While the two Laboratories' field installed gas blocks are of slightly different design, they are the same end product, in essence, in what they are designed to do, and the pressures they are designed to meet, and so forth. But the containment program does not design the gas blocks. They endorse the specifications, such as the need to have a 125 psi gas block for this particular function, and so on.

The fiber optic cables are supposed to be continually gas blocked, and if they don't meet the pressure test that's done on each and every cable, then you've got to strip the coating back to the fibers and discretely gas block them.

Carothers: Let's say you have a reel of fiber optic cable. You cut off ten feet don't you, and test that? What if it doesn't meet that test?

House: Then you don't use that reel, or you put a discrete gas block in the run. You put the blocks in at the standard locations where you have designated gas blocks for the multi-conductor cable. In our particular geometries there are typically three places, one in each of the rigid plugs, where gas blocks are placed.

Carothers: What's your experience with the fiber optic cables? Do many of them fail your pressure test?

House: It's probably about thirty percent that fail, that leak enough so they don't meet the specs. They are supposed to come from the factory, by design, as continually gas blocked fiber optic cables. But, when they sit in the Nevada desert sun, or lie out in a cable way before they've been terminated, there's a degradation that takes place. It in many cases causes the cable not to pass the test, and then you've got to go in and discretely gas block them.

The fiber optic cable is a very small diameter cable — maybe a 1/2 inch outside diameter, which of course includes the sheath and the protective jacket, and so on. By the time you get down to the potential flow path for gas up one of those cables, it's very small. It's hard to envision gas being driven very far up one of those fiber optic cables, but we gas block them because that's the way we do it. Conservatism is perhaps our most important product.

Carothers: Well, coax cables used to leak gases to the surface. Gases were forced a long way through them - - a thousand feet or more. You look at the cable, and you wonder how you could possibly push gas through it, but there is plenty of evidence that it happens.

House: The factory gas-blocked coax works very, very well. I don't have any numbers in my head about failure rate, but it is very low. Coax is good stuff. In terms of our field, or discrete, gas blocks that are installed in the multi-conductor cables, both Laboratories' cable gas blocks work very well. They're not a problem.

Carothers: Who makes the discrete gas blocks Livermore uses?

Roth: They're made on site. That process was developed over the years. The weather coating is stripped off and the outer jacket it cut down to the electrical conductors. That section of the cable is placed in a plastic mold, and an epoxy material is pumped into that mold from one end, and out the other. That epoxy material hardens and encapsulates the conductors and the shielding material.

Page: There are specifications as to how it's done, what the materials are, and what the criteria are for a good gas block. That process is managed by the construction engineering people. The containment people specify where they go, and have the responsibility for seeing that they're in the right place with respect to the formation and the location of the plugs.

Carothers: If you look at the containment history, before Baneberry lots of the shots seeped material through the cables, or through the stemming. Since Baneberry, that just doesn't happen anymore. I think that is a tribute to the people in the field who concern themselves with the stemming, and the cables, and the gas blocks, and so on. People from the Laboratories come to the CEP and say, "Well, we're going to use these gas blocks and this stemming," and the CEP people say, "Oh, fine, that's good."

Making those statements good really depends on somebody out there in the dust and the gravel and the sun, or the rain and the wind doing that stuff right. And the record is that they haven't missed once, on lots and lots of shots, and on thousands of cables. A whole bunch of hot, dusty, sweaty, or maybe wet, cold people deserve a pat on the back for that.

Roth: Yes. For a number of years we did that discrete gas blocking right out in the cable ways, in whatever the weather was, and built tents over the stations. In the present day, as much as possible we try to do that back in the cable yard, under a more controlled environment, and with better conditions. What that means is pre-cutting cables, and pre-locating those gas blocks so they fall in the plugs in the right places, and that works out very well. That alleviates some of the labor involved in discrete gas blocking, but it's still not a trivial kind of task. As much as possible we try to do it away from the shot area, but there are still occasionally late-time requirements where it has to be done out in the field.

Carothers: Byron, "out in the field" for DNA is in a tunnel. What's your experience with leaks from cables? Is it an easier problem?

Ristvet: I like to point with pride that, with the exception of Diamond Fortune, which I predicted would probably seep into the tunnel through the medium at late times, we've not had one atom into the tunnel on anything I designed. That's in part because I changed our gas blocking schemes on the cables. I think the cables were allowing gas to get a long way down the stemming column. With a low yield you just don't smash the cables hard enough to prevent them from being a pathway. We know we get communication through the stemming itself to the FAC. And then we have all the cables wide open, because when the FAC detonates, it just cut all those cables. We saw that on reentry. So now all the multiconductor firing cables are sitting there wide open, and they go all the way back to the TAPS area and near the end of stemming. And you know how it is with radioactive gas; if there's any possible pathway, it will find it.

Carothers: A thing that is a little surprising is to calculate the volume of that radioactive gas that's bothering you so much. It's a few cubic centimeters, or even less.

Ristvet: I'll give you a good example. On Disko Elm we had to describe to the Admiral, the Secretary of Energy himself, that we did not have a major containment failure. We saw activity that came down via the cables, then back into the LOS pipe on the wrong side of the gas blocks. How much was it? It was four curies, maximum, of zenon and a little bit of krypton 85. It was almost all zenon, and the volume turned out to be nine microliters. That is a very small amount.

Carothers: Aren't you proud of those people who develop the monitoring instruments? They sure do a good job, don't they?

Ristvet: They are fantastic. They have to use cyrogenic traps to actually collect it, and pump millions of cubic meters of air through the traps, but they can get it.

Carothers: And they can measure how much there is.

Ristvet: That's exactly correct. And every time they measure a little bit better the standard goes down.

Disko Elm was the last time we saw anything flow down the pipe, and that's when we realized — in fact I caught it in the middle of Distant Zenith — that we weren't separating our cables like we used to. We were using predominately Livermore devices, and Livermore likes to use this four conductor Number 2 for the firing cables. That is an unbelievable leaker, because not only does the jacket have lots of holes in it, but it is a stranded cable. It's a great power cable, and of course, that's exactly what it's used for — for charging up the x-units. But we tested it, and I think the permeability was two or three darcies over a hundred foot length. So, you could imagine it's just a conduit. But, it works real well once you separate the strands. You do that and you cut it down at least into the millidarcy range. You don't even have to take the insulation off.

Carothers: There used to be some people at the Livermore Laboratory who were very touchy about their firing cables, because they had some experiences they didn't like very much. I'm surprised they let you mess with their firing cables.

Ristvet: Well, I talked it over at length with Mr. Ray Peabody et al, who do Livermore's firing, and Ray and Mike Bockas stood and watched every step that was done. And they were there even when we did the same thing on other shots in the same way. When we did it on the Los Alamos device, Everett Holmes and crew stood there and just watched everything that was done, and assured themselves that everything would be okay.

Carothers: It's called attention to detail. Joe LaComb would have smiled and nodded approvingly.

Ristvet: That's certainly right. I can understand the sensitivity. I can remember one DNA shot where we were down to the last set of firing cables because we had a little water getting into the RTV boxes. And the thought of retrieving a live nuclear device on a reentry does not appeal to me. We've thought about it many times though, and we actually have a contingency plan for such.

Plugs

Page: Was coal-tar epoxy the first material Livermore used for plugs?

Carothers: They used concrete plugs on a few shots, but they weren't very enthusiastic about those after they lost the cables on Duryea because somebody forgot about the exotherm when the concrete set up. The cable insulation softened, or melted, and all the cables shorted out. Including the firing cables. It's actually quite embarrassing not to be able to communicate with the device. A lot of people get very upset about that.

Page: That would be a Test Director's nightmare.

Roth: Emplacing the coal-tar epoxy mix was an attempt to solve the exotherm problem you can have with concrete, and still get a rapidly setting up plug. And, it was an attempt to get a tighter seal. All those kinds of things drove the development of that material.

Carothers: I've never talked to a person who liked coal-tar epoxy plugs.

Roth: They were smelly, they were carcinogenic, and they were messy. If you got some of that stuff on you, you couldn't get it off. It was gooey, sloppy stuff that ruined your clothing, and it was difficult to put in place, but for years we did that.

Page: It was miserable stuff. It was just terrible stuff to deal with, to be in direct contact with. It was put together in transit trucks, and it was difficult to control the mix. The coal-tar was just dumped in the hole, along with the gavel, and you were never certain where the coal-tar and the gravel ended up. We made some of those plugs in surface casings, and when we pulled them out, cut them apart and looked at them, the uniformity through the plug never did look good to me. I think they just depended on the fact that there was a lot of it there to give something that was going to do the job. I think we did ourselves a big favor when we got rid of that.

Carothers: There were several components to those plugs; the coal-tar, the epoxy, the hardener, and all that had to be mixed together.

Page: That's right. In fact, we usually had a chemist, Phil Fleming, be there when we were putting those plugs in. That's more precision work than you ought to have in the field. Another thing that people have said is that the coal-tar was a carcinogenic substance, and people working with it were required to wear protective clothing - - lab coats and gloves and boots.

Roth: When the gravel and the coal-tar got down there, there was a tendency for the gravel to settle out, and maybe the coal-tar epoxy flowed a little bit. Hopefully it flowed into the interstices of the gravel, but maybe the gravel built up preferentially on one side of the hole. We couldn't know that, but the plugs were thick enough that we thought we had adequate containment.

Carothers: After the coal-tar epoxy plugs Livermore went to two-part epoxy plugs for a while. Los Alamos still uses them. What did you think about those plugs? Why did you give them up?

Page: I don't remember much about that stuff, but I don't think it was a whole lot different from the coal-tar, myself. Take the requirements on quality control. Here we had two different products that came in from different vendors. Both had to be stored properly, and we built a special facility for them. You always worried about running out of one or the other material at a bad time. And, it had to be blended properly, and it had to get to the hole in a timely manner, because it came from the mixing plant, near the shaker plant, which is a ways away. It might have been a little better product than the CTE in terms of uniformity in the kind of plug it produced, but it still was a difficult thing to work with.

Roth: So, a few years ago we went to the sanded gypsum plugs. That's a cement, sand, and gypsum mixture that has good qualities with respect to expansion or shrinkage. It's mixed on the surface, so we know it's a homogeneous mixture, and when it gets down in the hole it flows very well. Its qualities are such that it can be emplaced without an exotherm that is higher than the cables or experiments close to it can stand, and in special circumstances we can mix it with chilled water. A big attribute of the sanded gypsum for a Test Director is that we don't have to wait for it to set up. We

can put it in the hole, and within a half hour to forty-five minutes it's hard, and we're ready to continue stemming. By the time you get the pipe extracted and the equipment cleaned up it's hard, and we can continue the stemming operation. From a cost standpoint it's a fairly expensive material, but so was the coal-tar epoxy.

Carothers: What makes it expensive?

Roth: I'm not sure. Perhaps the gypsum. The equipment to mix it and pump it not commonly used. It's not a transit mix truck. It's a batch mixing operation where they pneumatically blow the gypsum into a mixture of water and sand, and tumble that. Eventually it gets pumped out, over to the hole and down a tremmi pipe. We've had cameras down there, and it comes squirting out quite violently down at the bottom. It's a good material, but it is expensive compared to concrete.

Page: It seems to form a nice product, and when it's set it's got a strength of about 3000 psi. And we think it's fairly compliant when it's hit with high ground motion.

The Role of the Containment Groups

Carothers: Jack, how much authority does your containment team have with respect to their event?

House: When I assign the containment scientist the responsibility for an event it also includes a team of - - and it may be a mix, or one person might be wearing two hats - - typically a geologist, a geophysicist, and a phenomenologist. If you take the Icecap event, for example, Nancy Marusak was the containment scientist, and she was also the geologist. Mark Mathews was the geophysicist, and Tom Kunkle did the phenomenology work. That was the event team for that particular activity.

Once the containment scientist has the assignment, she and her team have the responsibility, and the authority, to do the event design. Now I, as part of the team as a sort of ex-officio member, have the purview to look over their shoulders, as it were. When we go to a peer review of the containment design, the principals in the

containment program at our Laboratory that we consider as primarily the containment scientists, and the two CEP members, have every right and privilege to take pot shots at it and pick it apart.

Carothers: Can the containment scientist specify what logs she wants? Can she have them rerun if she doesn't like the quality of the ones she gets?

House: You betcha. She also negotiates if necessary with the field operations, the J-6 guys, if they want to reposition a plug so it fits a particular harness connection scheme; they work that out together. The event team is pretty much autonomous; they certainly have the responsibility and the authority to get or take what is needed to successfully design and/or complete the event.

Carothers: Do they specify the locations of the plugs and the plug materials?

House: They do locate the plugs. The plug material, if we are considering the rigid plugs, would be the grout and the two-part epoxy. For instance, again considering Icecap, we had three rigid plugs. One of them was HPNS-5 grout, or Husky Pup Neat Slurry, and two of them were two-part epoxy. We worked hand in glove with the field operations people, J-6, in getting this new to us HPNS-5 mix. It was designed for Los Alamos by the Waterways Experiment Station folks, who are the grout experts.

Carothers: What else does the containment scientist have to do?

House: Well, containment is his or her total responsibility. Once the site is selected, then next thing we have to produce is what we call the containment criteria memo. That defines the plug locations, the types of plugs and material, and of course the working point depth, or depths if it happens to be a multiple, where the radiation and pressure monitors, typically known as RAMS, will go, and how many there will be. The only thing the containment scientist does not specify with regard to the down hole stemming plan is the amount of magnetite. That is defined by the experimenter. We, so to speak, take it from there.

We have recently been required to, essentially, develop stemming plans for underneath the device. We at Los Alamos in particular have had holes that were deep enough to require that. There was one in Area 3 for a shot called Laredo, which was deep

enough that it actually intersected the Paleozoic rocks. The environmental folks have come on the scene and said, "Gee, you've got to do something about that. You have a potentially preferential path for contamination to go down hole." We said, "My gosh. We've just thought about stuff going up. We don't care if it goes down hole, do we?" "Well, you better start thinking about that, because we care about it. And, we carry a pretty big club, us folks at environmental restoration." Or the Earthworms, as they are so fondly known. So, we have specifications for the downwards stemming now.

Carothers: If the Livermore containment people wanted some logs run, would they go through you?

Roth: Not normally. They have their own support at the Test Site, and they pretty much determine what's required to carry out the containment plan; what information is required to present to the CEP. They would go directly and say they need a gamma log, for example.

Carothers: These logging requirements occur certainly well in advance of when the device gets there, don't they?

Roth: Oh yes. It may be as much as a year in advance. That information is accumulated and analyzed by the containment scientist. It is documented, and eventually there is a report, or an input document, that is presented to the CEP for their review.

Carothers: When you start to put the system down hole, who supervises that?

Page: Well, the Test Director owns that operation. He has a project group that works on accomplishing it. The device systems engineer has primary responsibility for the early part of the emplacement - - getting the device package prepared, moved to the hole, and inserted. The Test Director's right hand operational guy is again a construction engineer, because he's the interface with the contractors. We always have a plan as to how we're going to do the work, and the implementation of that plan is generally managed between those two engineers, with the Test Director serving in an oversight role.

Carothers: Once upon a time, and I don't mean this in a derogatory way to your colleagues at Los Alamos, they were putting a device down hole and they didn't put in a cable fanout that was called for. How can you forget a fanout? That's a big thing, and it takes some time to do during the down hole operation.

Page: What can I say? It happens. Lack of attention to detail, poor criteria, whatever. You hope you have enough checks and balances so things like that don't happen. We depend on Raytheon, for example, to keep tabs of everything that happens and everything that goes into the hole. They're generally successful, but if they have a bad design drawing, and the requirement is somehow missed on that drawing, they would miss it. We're supposed to have enough checks and balances so those things don't happen.

There are a lot of things like that, that can keep a Test Director awake at night. There are a lot of things to worry about, because those operations are complex operations.

Carothers: Okay, the device and the diagnostics packages are down hole. Now you have to do the stemming. Who does the stemming? Who says, "Okay, the gravel goes here, and there is where the plugs go," and all that?

Page: The containment program people have the responsibility for designing a competent stemming plan. But, you're right, somebody has to do it, and that's an interesting situation, in a sense. I think the containment group has the philosophy, and I think they have had this for a long time, that they need to maintain a presence at the hole during that operation to assure that the job has been done right. Now, there's been a lot of discussion that it is a field operation, and the construction engineer can do that job just fine. I could argue that one either way, but in my opinion the way that it is done these days is through oversight by the containment engineering group. The actual operation is directed by the construction engineer, but the presence of the containment engineer is the element that assures that the containment packages are installed properly. That's the way I see it.

There is another element that supports doing the stemming right. That is the Raytheon Services Nevada role. Their job is inspection and verification. They're given a very detailed design package that includes all of the specifications for all of the features

that are supposed to go into the hole. There is a gravel specification, there's moisture criteria. There are a lot of elevation features they keep track of, such as where the fanouts are, where the gas blocks are, where the bottom gas block is, where the top gas block is in each fanout, where the elevations stop when you change materials, where the bottom of the plug is, where the top of the plug is. All those features are called out. Many of them are measured at the hole, and RSN rigorously tracks all that information as it's established. They essentially establish an as-built data package for the hole. We depend on that quite a bit for establishing our confidence, once the thing is done, that we have a competent containment package.

Carothers: When a hole is stemmed, how do you know the stemming that's supposed to be in the hole is actually in there?

Roth: Well, first of all, there's a material balance on the stemming that is determined. We weigh it, or volumetricaly measure it.

Carothers: Bernie, you don't volumetricaly measure it. You weigh it.

Roth: Okay. We weigh it. You're right. But we know what the weight per unit volume is, and so from that point we get a volumetric quantity. The entire depth of the hole is volumetrically characterized ahead of time. So, within a given area wherever a given plug is supposed to fit, or a given section of sand, or gravel, or whatever we can calculate from the logging information what volume of material fits in there. Then weighing that volume of material across our weightometer instruments at the top of the hole can pretty well determine what we put into the hole.

Carothers: How do you know the volume of the hole?

Roth: We have a down hole logging system that uses a laser to bounce a beam off the hole wall, and records the distance to the wall. Caliper logs were used up until a few years ago, and they still are as a rough guide. But we now have an instrument that goes down hole, bounces a laser beam off the adjacent surface, picks up the reflected beam, and determines what the distance is. That beam rotates in a full circle as the instrument is very slowly lowered or raised in the hole, so you get a very shallow helix measurement that determines the volume to much closer than one percent. So, we really know what the volume of the hole is.

Carothers: One of the things that came as a surprise to engineers, physicists, whoever, in the 1961, '62 time frame was how hard it was to pour stemming material down the hole and not have it bridge. It seemed incredible that you could have a four, or six, or eight foot diameter hole and the material would bridge in it. How could that be? But it did, and when the stemming slumped it sometimes broke the cables. Do you ever have any difficulty of that sort these days?

Roth: I have heard of those kinds of problems, but since I've been the Test Director I have had neither sloughing or bridging problems. Those were problems early on that people were surprised about. I think that maybe the moisture contents of the sand or gravel would let it build up on pipe strings, so it would tend to bridge. That concern was still present as late as, I think, 1978. The Test Director at that time said we could not fill the emplacement pipe with grout by pouring it in the top. It would never make it to the bottom.

Carothers: I could believe that.

Roth: I had a hard time believing it. The pipe was 9 &5/8 drill stem with an 8&1/2 inch ID, or whatever that dimension is.

Carothers: Don't you grout that emplacement pipe from the bottom up? That is, pump the grout down through a pipe near the bottom and force it up the pipe?

Roth: No. But yes, we did that for years, but not anymore. My concern was a safety concern. We were stabbing a tremmi pipe down the emplacement pipe just to do that fill operation. First of all, it was time consuming. Second of all, if one or more lengths of that tremmi pipe ever got loose, it had a rifle barrel right down to the top of the canister. I could see a real catastrophe occurring, and that was an ongoing concern, especially watching some of the crafts handling those tremmi pipes. It never happened, but it was a concern to me. These days we just put the concrete, mixed with a bentonite solution, into the top of the hole and let it free fall.

Carothers: How do you know it's full?

Roth: Again, by material balance we know it's full. The inside of a pipe is readily calculable, and there's not much question about how much volume is involved. Once it is full we put a bull plug on

top of it as a precaution. That probably isn't necessary, but it gives everybody a warm fuzzy feeling. That's what's being done today with respect to filling the emplacement pipe.

Carothers: How about knowing that the stemming was emplaced as it was designed to be?

Page: We make every effort to install it just as designed, because if it meets the criteria it makes everybody's life a lot simpler. Then you don't have to deal with deviations, and they can be a real problem. There's a lot of motivation to put the stemming in just as the stemming plan specifies.

Carothers: I do believe that. So, the hole is stemmed, and the plugs are in. At that point your job is about done isn't it?

Page: Getting close. There's another couple of days of worrying about final dry runs, and analyzing the containment records and the containment plan. One of the final jobs of the Test Director is to present the as-built stemming plan to the Test Controller's panel. That's done on D minus 1.

Carothers: Yes, and that's when an event called Galena came to a halt. As I recall, there was considerable to-do over the possibility that there was a thirty foot or so void in the stemming on Galena. How could that be, Jim? Why couldn't you convince people that wasn't the case?

Page: Well, I was the Test Director for Galena, and we had a number of different kinds of information that we had to try and interpret. We had stemming switches, we had a measure of the quantity of material we put in the hole, and we had strain gauges on the pipe at the surface, and above and below the canister. So, there was a lot of different intelligence, and when it was all analyzed through a rational process, you could arrive at some conclusions.

We became aware that we had a problem over the couple of days that we were stemming one part of the hole. We had strain gauge readings that changed over a weekend, after we had passed that point in the stemming. We had other changes that indicated the material was moving around. We alerted the Los Alamos containment community, and gave them the information we had. We told the Test Controller we had this concern, but that we were proceeding to complete the stemming. As people thought about it, and did

their own analyzing, Los Alamos asked for a more formal review of the issue. As that started to come into place we decided we wouldn't proceed until the Panel was notified. The approach was to poll the Panel without pulling them together, but people weren't comfortable with that, and it was decided that wasn't sufficient, so a Panel meeting was called.

That was how it went. There were independent looks at the data. People relying on their own experience, and making their own interpretations, felt there was enough uncertainty that we couldn't go ahead without a formal review. We're still totally satisfied that we did not have a void there.

Carothers: Sure. But the important thing, for the Panel, was you couldn't prove it one way or the other. And so people on the Panel then said, "Well, in that case we have to assume that void is there."

Page: I can't argue with that. I think that's a reasonable attitude. Now, you'd like to be able to say that you have absolute certainty of what's going on a thousand feet underground, but we can't always do that.

Carothers: There was a Panel meeting on a Saturday afternoon in Las Vegas, and after hearing what was presented, the Panel felt the shot could go ahead. So, you fired it, and it performed just fine, as far as the containment aspects were concerned.

Page: It did. Radiation didn't get high in the hole at all.

Carothers: Neither Laboratory has done a line-of-sight shot for a long time. If one were needed it would be like starting all over, wouldn't it?

Page: I don't know where we stand with regards to being able operationally to do one of those, but we recently did re-certify our HE closure design. About four years ago we thought we were going to do a shot like that, and we knew there would be a large line of sight. So we rejuvenated an old technology, where we drew from the old design drawings that we had available, and from the experience of people who had been there. I was one of the people who had been in on the early development of that system back in the late sixties and early seventies. We were able to rebuild the machine, and we did one test, with new people. They were all new

people doing the work, and they demonstrated that it closed very nicely. There was a situation where twenty years had passed, and we had not lost the technology.

Carothers: It gives you to think though.

Page: Oh, you bet it does. But now there's a bridge for another ten years, perhaps. If ten years from now somebody wanted to develop one of those, we have three or four young people who, if they're still at the Lab, could do it then. I'll be long gone, but those people might still be around.

Carothers: One thing that I think has been true at both Laboratories - - I will leave DNA out because they have a different set of problems in that they have to protect millions of dollars worth of samples - - is that there is inherently a kind of conflict of interest between the containment people and the field people. Your job as the Test Director would be easier, and the shot quicker and cheaper to do, if you didn't have to do all the logging, and special stemming, and put in cable gas blocks, and so on.

Scolman: I think one way of looking at it is, going underground, particularly with the containment criteria we've got now, puts a buy-in cost, a base cost on any shot that is so high that what you do on the shot does not appreciably effect the cost of the shot. In other words, the difference in cost between a very minimal test and a very maximal test is certainly not as much as it would have been if it wasn't for the containment.

Carothers: I've heard the argument put the other way - - that the shots are so complicated and expensive today that what you do for containment is only a small part of the cost.

Scolman: In some sense, if what you count as costs for containment is what is necessary to run an event through the CEP, and the additional containment hardware you put in, that may be true.

But, first off, there's the fact that you do, indeed, need to drill holes, which requires the maintenance of a drilling capability both for the emplacement holes and the post-shot sampling.. You do, indeed, need to have plants that generate the kind of stemming material you use. You do, indeed, need to do all the logging and those kind of things.

Then you put in the cost of just maintaining the Test Site — the EPA, the weather service, all of these people who are there regardless of how complex the event is.

Carothers: Yes, but you can't fairly charge that against containment. Those people would be there if you were doing atmospheric shots.

Scolman: Well, that's true.

Carothers: After Baneberry life for you as the Test Director must have changed. You had a lot of other things that you now had to do to prepare a shot, and fire a shot.

Scolman: Yes, of course. The TEP was never a particular problem. One didn't worry about getting shots through the TEP; one worried mightily about getting shots through the CEP. The other thing was that the operational requirements that came after Baneberry were much, much different than they were before. We used to draw a line between Area 4 and 9. If it was a Livermore shot we just cleared above that line. If it was a Los Alamos shot we cleared below that. Now we clear the whole forward area on every shot.

And there was a push made, largely driven by NVO, which said, "Okay, let's get everything out of the forward area that we don't need to have there." The reconfiguration studies that were done really didn't lead to an awful lot other than we moved some things that had been out in the forward area back into Frenchmen Flat. Some of those changes, which in general increased costs, were not necessarily involved directly with containment, but more with how one reacted if you had a containment problem when you fired. One of the things on Baneberry that got people's attention, other than the fact that it vented and got off-site, was the fact that we did, indeed, contaminate some people and some facilities. A lot of changes were made to prevent that from happening again.

Brownlee: There's always been a curse, here at Los Alamos, that I haven't quite known how to fight. It has been a very insidious thing, because down through the years, after Baneberry, we never had another failure. And worse than that, we didn't even have a seep. So there has been the attitude, "Why should we do anything different than we've been doing? We had those experiences, we did these things, and since then we've never had a single problem of any

kind. Why then do we need these people working in containment? Let's just keep doing everything the way we're doing it, and get rid of all of those people."

And that attitude is still around. The idea is that we only need one person now, we don't need five or the six. We don't need any containment research now, because everything is doing all right. It's easy to be logical, but that doesn't win the argument. It's very hard to make an argument that can win against that attitude. Livermore, meanwhile, had two episodes, and that helped, because we'd say, "There are still things we don't know." And then the DNA has had things happen, and that helps, but then the argument is, "Why should we hire people to work on some of those things? Let them do that. It's not any of our affair."

And it's that argument which is the real reason why we had the same stemming plan forever, and we did our plugs the same way forever. We never could win the argument with our local engineers that there needed to be any change. You don't need to do it better if what you're doing is all right. We said, "We can do it better," but that didn't matter.

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Sometimes The Dragon Wins

There have been several events where the containment design has failed, for one reason or another. Some of these, such as Des Moines, Eel, Pike, and Bandicoot have been mentioned in earlier chapters. In the course of the interviews other events were described by people who were personally involved with them. In many cases, even though there may have been extensive post-shot efforts to understand the reason or reasons for a particular failure, often there is not agreement of a definitive cause. What follows is not an attempt to analyze and develop an accepted scenario for these events, nor is it a complete listing of all of the events that have had substantial releases.

There is one point that should be mentioned. Following the detonation of a device in a tunnel, while there may be satisfactoriy containment of all of the radioactive products, there is often an accumulation of gases which make it hazardous to reenter the tunnel. Hydrogen and carbon monoxide in particular form explosive mixtures in air, given suffiently high concentrations. (See the description in Chapter Sixteen of the hydrogen explosions which took place following the detonation of the Tamalpais device.) There may be some level of radioactive gases in the tunnel, none of which have leaked out to the atmosphere due to the efforts made pre-shot to form gas-tight barriers to such leakage.

However, after the detonation reentries must be made to recover the experimental samples and various equipment, and to prepare the tunnel complex for future experiments. At a time determined by the Test Controller, which may be several days after the event, a ventilation system can be activated to replace the air in the tunnel with fresh air. The hydrogen and carbon monoxide and other inert gases can be safely dispersed into the atmosphere. Any radioactive products are passed through filters, and the biologically inert noble gases are released in monitored low level amounts over a period of time. As a result of this tunnel ventilation process detectable amounts of activity may possibly be found on-site.

For example, the Misty Rain and Mighty Oak events both were successfully contained by the definitions in the CEP charter and the Nuclear Test Ban Treaty. No activity found its way to the atmosphere following either event, but there was radioactivity in the tunnel complex itself. During the ventilation process activity was detected on-site, and both events are listed as having a controlled release. This is an operational procedure that is not part of the containment design, and does not indicate a containment failure.

Gnome - - 12/10/61

Weart: In addition to shots like Marshmallow and Gumdrop, another shot that helped me formulate some of my thoughts in the early days was Gnome, in the Carlsbad area. It did have a prompt sampling pipe on it. It also had a tunnel with a line-of-sight pipe down it. It was reentered, and I was on that reentry team. The observations there — the fact that the line-of-sight that went straight up pinched off and nothing came out, even though we were trying to get samples through it, the fact that the line-of-sight pipe that we wanted to seal off quickly may have contributed somewhat to the release, the fact that the buttonhook principle wasn't successful in that particular case, and it didn't seal things off — did contribute to some of my early thinking. And some of the early DNA designs followed that thinking. We went along on that course until we had a problem, and then we had to change things.

Carothers: From your observations on the Gnome reentry, to what would you attribute the leak that occurred? You say the line-of-sight pipe may have contributed.

Weart: Well, Gnome was in a location with a bedded stratigraphy, and the line-of-sight pipe went right along parallel to those beds. The combination of the cavity growth and the line-of-sight pipe energy caused the ground to open up preferentially, all along the bedding planes. And that allowed energy to squirt out of the cavity, and out into the tunnel. Whether it would have happened if the bedding planes hadn't been there, I don't know, but it appears to have been a plane of weakness that allowed separation to occur.

Carothers: On reentry could you see radioactivity, injected material, along those planes?

Weart: Yes. Whether it would have gone on the same path without a bedding plane you don't know.

So, because of that, the design for a subsequent shot there, called Coach, which was never fired, would have avoided this situation by having an incline going up on the buttonhook part. That way you didn't have a plane intersecting both the working point and the tunnel. And in a lot of the rocks that we fired in subsequently, alluvium and the tuffs, we've usually not had a bedding plane problem to worry about.

Higgins: I and three other people reentered the shaft and tunnel, and recovered one of the pieces of experimental hardware about December 20, 1961. I can say with certainty that there was no leakagr down the drift or the line-of-sight pipe. The gas seal door was bypassed in a clay seam that was a foot or so above the top of the tunnel. There was no evidence of anything except steam in the fracture or shaft. Leakage must have come from the cavity after it formed, through that seam, bypassing all the engineered features.

Eagle - - 12/12/63

Brownlee: I approached containment from the point of view of containment of LOS shots, and I saw the whole thing in terms of closing pipes with various kinds of things to keep energy from getting out after they had made their measurements. Now, as I saw it, the Livermore experimentalist had an upper hand to a greater degree than they did here. And that started with Al Graves and Bill Ogle; they were determined to allow me to try to keep things from coming out. At Livermore, it seemed to me, closures were kind of secondary. With Los Alamos, closures were kind of first; you had to do that.

When Eagle came along it was for sure, I thought, going to allow the experimenters to get their information, but there was no way to close the pipe. I was convinced that Eagle was going to leak. Almost by accident I told Al Graves, "I think Eagle will come right out. I don't think it's designed right." We had the treaty then, so that could have been a violation of the treaty if it did that. It bothered Al when I told him I thought it would leak. So, he called

up the Livermore Director, who was then Johnny Foster, and as a result we had a meeting in Las Vegas. That was the summer of '63, and Eagle was fired later that year.

I took a lot of heat, because Livermore was offended that Al had asked them to tell him about the Eagle design. But I came home after the meeting and did some calculations, and I was still convinced that it would come out. So I have to admit I took a certain amount of perverse pleasure when it did come out, because I had been taking a lot of heat.

Now, I am absolutely convinced that Eagle was not fired as designed. Los Alamos people went out and watched them put Eagle downhole. They came back and said, "Here's how that was assembled." I said, "It's not supposed to be that way." And so, I think the amount of energy that came out was more than there should have been.

The difference was this. In the very bottom of the pipe there was to be a series of lead rings. My guys say that all those lead rings were piled right on top of one another, like a lead collimator. I'm absolutely convinced that there was a lead cylinder at the very place where there should not have been a lead cylinder. The ground shock had no way of penetrating that lead in time to close the pipe. It squeezed off, in time, but obviously a lot of stuff went by. Of course, it was an awkward thing because the Livermore guys didn't dare own up that they hadn't done it right, so they assured the system that they had done it exactly as drawn. But the Los Alamos people at the Test Site, who lived there, said, "Those lead baffles were put cheek to jowl." And there were a lot of them, so there was just a big lead cylinder.

We had thought about line-of-sight pipes for some time, but I regarded Eagle as the first modern LOS shot because it was the first LOS shot with the treaty in place. There had been the argument, "Let the energy pass and then close the pipe." Now, that's right, in the sense that the Eagle fireball didn't have any radioactivity in it. Is that all right? I said, "It's not all right if it blows everything apart."

Before Eagle, people were saying, "It won't do that." And I was saying, "There's enough energy that it will. It should do damage." And it did. There was more energy there than I was expecting, but as I said, I think there was a reason for that. But after

Eagle we no longer had the debate about letting energy pass before you closed the pipe. So, the concept of closing everything fast was solidified, reinforced, and became doctrine after Eagle, for us. Eagle was a big experience for us.

I think the Eagle design, if it had been emplaced right, would almost have worked. I think it would have changed history if it had been emplaced right. As it was, it looked as though the design allowed all that energy out, and I don't believe that. But Eagle heavily influenced the next designs.

Double Play - - 06/15/66

LaComb: There was radiation behind the overburden plug within like the first second. The radiation got outside the overburden plug within minutes, but it was a slow release. It wasn't dynamic; it was throttled through about a six inch hole and about a two inch hole. These holes were each about eight feet long, so there was quite a bit of throttling there. And we had a very slight seep through the ventilation valves and the gas-seal door, and a seep up through the cable bundles.

I think we got permission to ventilate about a day or two later, and we pumped gas reading better than a thousand R per hour out of the tunnel complex for better than two days. That was as high as the rams went. And that was where we were reading what was coming out of the tunnel complex. Of course the filters, after the first ten minutes, were a thousand R and stayed there.

We had three what were called DBS boxes, which were supposed to fire closed. When it broke loose and came out, it hit those boxes and the test chamber moved about forty feet towards the portal. We were very fortunate, because the door of the chamber ended up right beside the little tiny car-pass alcove we had. If it hadn't, nobody would have ever been able to get in. Those DBS boxes moved over eighty feet.

Further out there were these huge glass bubbles — just huge bubbles, of glass. They were six feet in diameter. They weren't full round; they were hemispheres, as a rule. I assume, because of the prompt release, they were from molten rock from the cavity. And, because the DBS boxes were slowing everything down there, the melt was stagnating there, more or less, and depositing that glass.

But there was enough gas coming with the glass that it formed those bubbles. The same kind of bubbles were seen in the tunnel on Red Hot. It was the same kind of failure in the same time frame. So, I think that glass must have come from the cavity.

There were also glass stalactites hanging down from the ceiling. That glass, on the rock itself, I'm not sure whether it was where the tunnel had been melted and dripped down in place, or whether it was sprayed on and then dripped down.

It was kind of funny, because when we first reentered that area, it was several R. As we walked forward, into the stemming region, it went down to ten mR. Everything had come out so fast that area was clean.

Door Mist - - 08/31/67

LaComb: On Door Mist, as I recall, radiation started to show up in the tunnel in something like eleven seconds. There were two TAPS - - tunnel and pipe seals - - in the pipe string, and two or three DBS boxes. On reentry we found that the close-in TAPS door had closed down about thirty degrees. It had been caught by something, and looked like that must have been a two foot square chunk of steel, because that door, as strong as it was, was just folded. We had put a pile of sandbags forward of the walkway door in the close-in TAPS; there was about a three foot space between the sandbags and the door. That door had at least a ten inch wide flange embedded in the concrete. We never did find the door that was in the walkway. Apparently the sandbags had gone in motion, and they just took it some place out of this world.

The far-out TAPS had a hole eroded above it which was about a foot to a foot and a half high by eleven feet wide.

Carothers: Joe, you say the walkway door was gone. It had to be in the tunnel somewhere, right?

LaComb: Well, there's so much rubble you don't know where anything is, and the radiation levels are such that a lot of times you've only got five or ten or fifteen minutes to look around. If we had spent any effort looking for that door it would have been called "natural curiosity," and that's not of benefit to the program. I'm sure it was in there someplace, because we didn't see any signs of melt on Door Mist.

When we reentered from the overburden plug, just inside the overburden plug it looked like a prehistoric monster. The steel sets were all in place, the tie rods were still in place, but all the lagging was gone; there wasn't even any ashes around that you could see. All we could see going on down the tunnel was just this string of steel sets. It looked like a skeleton. And the back of the tunnel was just flat.

I was team chief on that reentry. We didn't have any ventilation because the vent lines were down, so we'd take three steps forward, stop, and say, "What's the readings?" "It's like a hundred mR, and about five hundred ppm's CO", and so much, maybe five percent explosive mixture. We'd take three more steps and stop. About this time my face mask had fogged up, and I was trying to use my hands like a windshield wiper. We got about twenty feet forward of the plug and it went to over a thousand parts per million CO, and over ten percent explosive mixture. Our face masks were fogging up so fast we couldn't keep up with it. At that time I said, "This is unsafe, guys. We will go around the other way."

Scroll - - 04/23/68

Olsen: Probably the earliest event where material properties really made a difference to anybody was Scroll, up on Pahute, where they were hunting for a medium that would decouple as much as possible. So, they wanted to know the in-situ density. Well, we found an air-fall tuff of very low density; it was 1.3 to 1.4.

Carothers: It contained?

Olsen: Well, it probably would have if we had plugged the holes properly.

Carothers: What was wrong?

Olsen: Again, it was a lack of appreciation for the time scale of things, and what can happen after the initial bang. We poured some sand down the hole, and we also poured some cement in. Except, we poured the cement in at a location where it would be eaten up by any ordinary subsurface collapse, which is what we got. So, because the only plug we had was eaten up by the subsurface collapse, all the granular stemming drained out, and there was an open hole to the surface, and lo and behold, it started leaking.

In retrospect, some of these things, in fact a lot of these things, you think, "God, why were we so dumb? That's obvious." Well, at the time it wasn't obvious. We didn't appreciate subsurface collapses, we didn't really have any information, any data base, that said that a subsurface collapse was likely to go to six cavity radii, give or take some number. We didn't have that kind of information.

At the time of Scroll we didn't know much about what happened on Pahute Mesa at all. We didn't have much experience with the normal geology up there, the density two, give or take a little, stuff that we see all the time now. Much less did we know about one of these unusual sites that we went hunting for, for Scroll. So, as we started to learn things, like where collapses might go to, we started to put in things that would attack that problem.

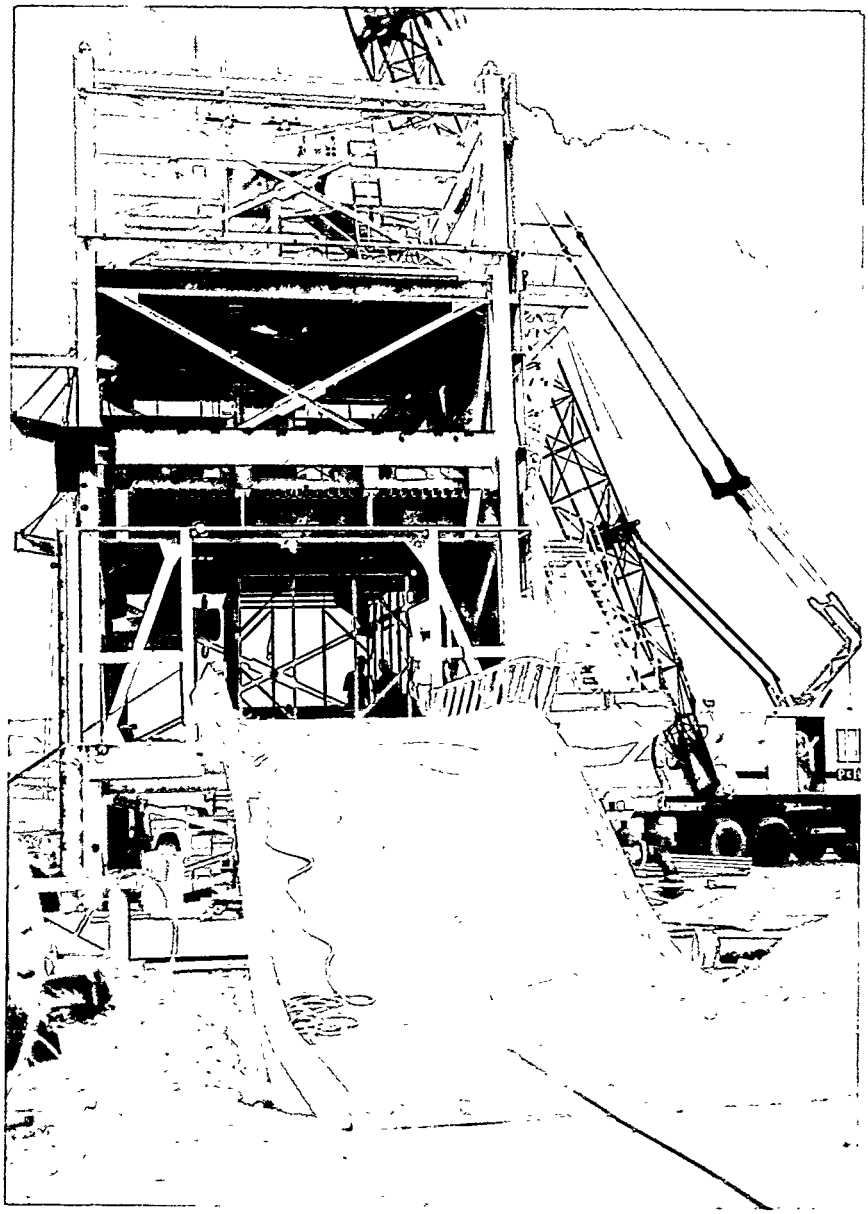
Carothers: Well, Rainier had been fired in 1957, and it had a subsurface collapse. There were extensive post-shot explorations done at the Rainier site during the moratorium, having to do with, among other things the height of the chimney, and so on.

Olsen: That is true. But it wasn't appreciated at the time we did Scroll.

Hupmobile - - 11/18/68

Olsen: Hupmobile was a disaster. It was a vertical line-of-sight shot, and the experimenters wanted collimators in the pipe, because they did not want shine bouncing off the walls, so we putin collimators at almost every pipe joint. This was a fairly largeline-of-sight - - it went up to several feet in diameter at the surface. We had these relatively massive collimator rings, and for ease of installation they had a little, very thin metal lip around the outside. So, they just sat on a pipe joint, and there was virtually no strength in the thing that attached them to the pipe.

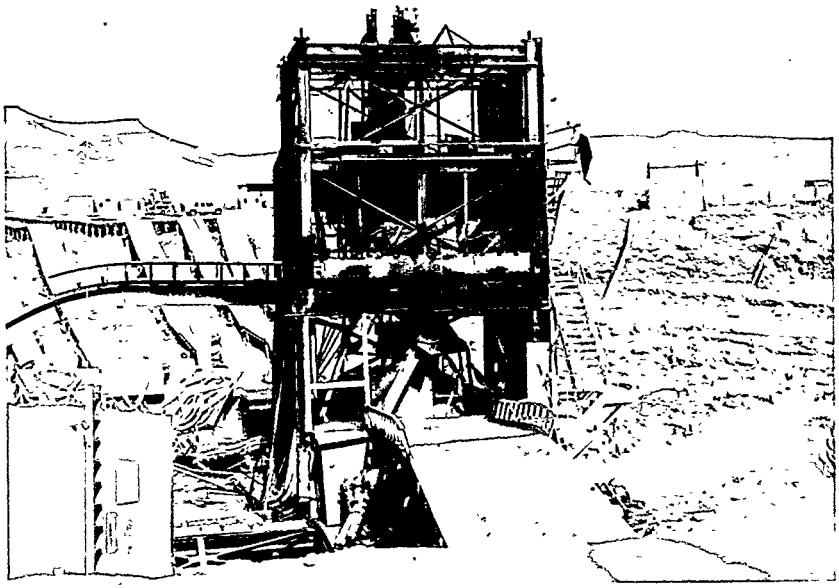
When the flow came along, going upward, and started dumping energy on the downstream side of these things, this little rim of fairly thin metal that was holding them in place gave way. So, these rings went up the pipe, became a tangled mass of stuff at the top, and blocked all of the valves. We recovered, on reentry, something like sixteen hundred pounds of twisted up collimator rings at the top of the line-of-sight pipe. We could even identify which collimator it was that had been torn loose.



Surface structure on Hupmobile, pre-shot.



Equipment package on Hupmobile, post-shot.



Surface structure on Hupmobile, post-shot.

There was a transient ground shock closure at the bottom, and it took like twenty-five seconds or so for the cavity to find the weak spot and erode it enough that it really blew its cork. There was a good sized cloud, but the flow was going through the pipe, so it didn't erode as much dirt and dust as Baneberry. The release was smaller than on Baneberry, but I think it was within an order of magnitude. It was big.

We had a large, several story exposure station at surface ground zero, on top of the pipe, and because of the venting that large exposure station caught fire, and we lost a large share of all the things that were in it. The experimenters didn't like that at all.

Baneberry - - 12/18/70

Weart: I have a little trouble recalling the exact time people started to look for more favorable geology for the tunnel shots. Certainly a marked turning point within the entire community, recognizing the influence of geology and so forth, was with the Baneberry event.

Carothers: You were part of the investigating committee. Looking back on it, what is your view of the understanding that was reached at that time, which may still be the right one?

Weart: As I recall, there were a couple of circumstances which we felt contributed to the Baneberry release. One was the fact that whereas events of this particular yield were normally detonated in alluvium, in unsaturated rock where we had come to expect a certain phenomena, Baneberry was detonated in saturated clay. There was a very high water content, and much more effective coupling of energy into ground motion.

That simply wasn't anticipated. In one simplistic way of looking at it, with that equivalent seismic energy it looked like a much bigger event. And therefore by our criteria, which were empirical, of course, it was underburied. I think the water may have contributed in another sense in that it provided an immense reservoir, a far greater reservoir than usual of not easily condensable gases. That left the cavity at a very high pressure for a very long time. And the third circumstance was a fault, through which the

eventual release occurred, which intersected an interval underground which saw these high pressures. It took a long time; it was three minutes or so before the release started.

So, it may have been that a combination of all of those things were necessary, and that any one of them, by itself, would not have caused trouble. You don't know, of course, in retrospect, but I think all three of those things contributed to the Baneberry release.

Carothers: And that focused attention on the geology.

Weart: Yes, it did. There was the fault, there was the unanticipated degree of saturation, the moisture, and the clay. People thought that if we had been smart enough, and had looked for these things, we might have anticipated that there could be a problem. So, maybe we ought to start looking for those things in the future.

Hudson: The primary problem with Baneberry, we think, had to do with geology. I say we think, because there is still not a complete agreement as to what caused the Baneberry release. And we had not given as much attention to what I should almost call civil engineering, prior to Baneberry, as we have afterwards.

Carothers: What do you mean by civil engineering?

Hudson: Engineering design based on the strengths of the overburden. Behavior of the overburden. We had basically relied on the density of the overburden in the past. And built into that density was all the features that led to successful containment of past events, that we had been ignoring. Such as strength. Clearly, while you may have the proper overburden density in a fluid, it's pretty easy to imagine how some of the device material could get to the surface.

In the case of Baneberry we were almost in a fluid, in that the working point was in a saturated clay zone. We were still operating almost entirely from experience. We didn't really know what to expect from the saturated clay. And, we really didn't know that we were in saturated clay. All we knew was that they were having trouble constructing the hole, a lot of trouble drilling.

But as far as the other parameters were concerned, it appeared to be a good high impedance medium, which would cause the pipe to close, and it seemed to be favorable for containment. At the same time, there were some of us who had questions we would have

liked to have had answered before Baneberry, but I don't think any of us who had questions really had reason for believing it was going to vent. We just had unanswered questions we would like to have had answered before Baneberry. Had we answered all those questions, it's not clear whether we had enough understanding of containment at that time to have avoided Baneberry.

Carothers: It's my understanding that it did not vent through the line-of-sight pipe. That closed off.

Hudson: That's true. I think the pipe had little or nothing to do with the venting on Baneberry. The overburden structure was too weak to contain the event. As a result, as the cavity grew, probably fissures were formed close-in and hot steam entered the fissures, and pushed them outward. And it found the easiest path to the surface and came out through a crack, known now as the Baneberry fault. My personal opinion is that had that fault not been there, it would still have come out through the path of least resistance. It might have been a crack some place that wasn't associated with the fault, but I believe it still would have come out.

The conditions that you needed to not have a hydrofracture out of the cavity just weren't there on Baneberry. You need some strength to keep hydrofractures from occurring, and apparently on Baneberry we didn't have that, so gases came to the surface. Another reason why I think it is related to hydrofracture is because it took three and a half minutes. If it had been something really prompt, associated with the line-of-sight pipe, it probably would have been at the surface in well under a minute. I think we stepped into Baneberry largely due to our ignorance.

Rambo: There were a lot of calculations that were done after Baneberry, using 1-D calculations. They were not successful. In going back and looking at this residual stress field again, those calculations seemed to show a residual stress field. I think the one person who came closest to having some success, using 1-D codes, was Norton Rimer, from S-Cubed. He alluded to weak clay at the shot point as being a possibile reason.

Things kind of got left that way for a number of years. In the meantime, we were developing, with Don Burton, who was the code physicist, much easier ways of working with this 2-D code called TENSOR. For instance, we found that instead of having to zone

everything as constant squares for different layers for different angles, and then try to fit it together, which was almost impossible, we could pull all the zones into a straight line, so we could then put our material models in without having to do it by hand. We could do that with a computer code. And so, what we call constraint lines, in the business I'm in, were put into this code.

As the codes improved, I thought we could go back and do a 2-D calculation of Baneberry. So, we went into the business of assembling this Baneberry calculation. There were certain features that we looked at and said, "Oh my gosh, we ought to put this in, or we ought to put that in." There had been a lot of exploratory work done on Baneberry, after the shot, to pull out properties that hadn't been measured before the shot, because that was not what we did in those days.

One of the things I identified in that calculation was the fact that there was a saturated layer up to a certain surface, and we had to put that in. Above that layer the material becomes very porous. And, we did measure strengths, so we ought to put this weaker material in and a higher strength material around it. The geologists gave us a picture of what it looked like in cross section, and there was a big fault going off to the side, and there was a Paleozoic hard rock scarp off at a certain distance. We put all this together.

Some of the work I had done on the slifer data on Baneberry indicated to me that it was very weak material, down in the working point region. What I had developed over the years was a way of looking back and getting a rough estimate of what the intercept of the particle velocity was, and if it's down close to zero, I made the assumption that it's very weak rock. If there's a high intercept, then maybe it's a stronger rock. Well, Baneberry just went right through zero. It just looked like a fluid. If you were to shoot in a fluid, the intercept of this curve would be down at zero. Baneberry looked like that, when I backed out from the data. I tried to back out some material properties, but we eventually went to a model developed by Ted Butkovich for putting this together. The strength curves we used came from rock measurements in the laboratory.

We ran the first calculation, and essentially it showed the whole thing going belly up, in terms of a residual stress field. It happened on the first try. We were shocked, because we had not had any success with the 1-D calculations, but the 2-D calculations showed this effect right away.

Probably I have a different view point from most people on that shot. There is a layer of saturated tuff above the shot, and above that there is a very porous layer, and so there was this strong wave-flattening effect, what I call a focusing effect, that happened when the shock wave went from the saturated tuff into the porous alluvium. So, we saw what I called a focused event.

That was an important learning point in calculations, I felt - - that you could get this kind of enhanced ground motion. We had looked at Tybo before we had looked at Baneberry, and so when I saw this saturated layer I felt that it was going to cause a lot of things to go on in the calculation that might not normally happen. And indeed, we saw this effect in the Baneberry calculation.

One thing that was interesting about Baneberry was that the fault was right at the edge of this wet, saturated area. There was a pocket of saturation that did not go flat across the fault. It stopped at the fault, according to what the geologists told us. That particular geometry was important to what we saw. The wave going out caused a lot of ground motion going up, along one side of the fault, and when it crossed over, the saturation was different and the motion was less.and that tended to cause a lot of ground motion running along one side of the fault as opposed to the other side of the fault. That was probably an important part of the calculation, in that you saw a lot of motion on the fault.

So you've got motion along the fault, plus an almost plane wave rarefaction that comes back, and you get a lot of tensile failure around the cavity in this weak material. The net result was we just didn't end up with any residual stress, after you put all of this together.

Carothers: To oversimplify. You've described a mechanism where you're not going to get residual stress, and where there will be tensile failures in a weak material. That sound like a situation where you would expect a lot of hydrofractures.

Rambo: Sure. Plus there was a large supply of water to drive that.

Camphor - - 06/29/71

Camphor was a line-of-sight tunnel event, sponsored by Sandia, which was originally scheduled to be fired shortly after Baneberry. It was delayed for some six monthes by the AEC investigation of the Baneberry release, and was the fourth event fired after testing was resumed with the Embudo event on June 16, 1971. In some respects its containment behavior resembled the Mighty Oak event fired some fifteen years later. There was a release of a small amount of gases, where Mighty Oak did not have such a release, but there was extensive damage to all of the equipment and experiments in the tunnel itself, and the loss of essentially all of the tunnel complex due to the fact that there was direct communication from the cavity to the various drifts. Jerry Kennedy, from Sandia, was the Test Director for Camphor.

Kennedy: Cypress worked perfectly well, from a containment viewpoint. It was a storybook test from start to finish. At that time what I think was going on was the DNA events were quite frequent, as compared to now in these later years. So shots were happening numerous times a year, and they were big effects tests, and they were being very successfully contained. Clearly the containment plans were working. So, everybody said, "A piece of cake." I think that was a little of the attitude, but that's not saying people were being slip-shod about it.

Then, roughly two years after Cypress, along came Camphor. The containment and stemming plan changed from Cypress. I'd say it was, maybe, more daring. We were going to use less stemming, because we had convinced ourselves we didn't need as much as on Cypress. These DNA shots had happened, and so it must be okay. You could follow the logic that said it was well designed. We certainly did that. Of course, it did not contain. We didn't have a venting to the outside, but it was a complete disaster inside the tunnel.

We had a couple of big overburden plugs, and after the shot we finally decided that there was a little bit of geology that perhaps we didn't quite understand, around the forward overburden plug, which was at the aft end of the line-of-sight pipe. The other was out at the main gas seal closure. That was a big, keyed-in concrete plug, which was designed to hold overburden pressure, and so on. As

near as we could ever tell, a leak formed around the outside of the close-in plug, through a crack we were unaware of, went around the plug, and then it quickly eroded into the LOS drift, and then the work drift.

The LOS pipe was rolled it into a ball at the forward overburden plug, into a space of about two to three hundred feet long. That was originally over a thousand foot string of pipe. It was all fairly compacted right up against that overburden plug. You just don't really realize from calculations and numbers how much energy there is there, and what it can do to things. You have to see what happens.

The flow didn't go through the gas seal plug in the main pipe drift, at the aft end where the diagnostics were. It went across into the parallel work drift, and then went through the plug over there. That plug had all the cables in it; all the instrumentation cables went through it. The eventual hole, which I walked back through on reentry, was through that area where the cables went through. It was clean as a whistle. It took all those cables out.

The gas seal door was the final thing between us and the great out-of-doors. It was a swing-shut door which you closed on button up. It was just a big steel swinging door with big seals. It was supposed to be speced at a thousand psi and a thousand degrees.

We tried to test that door for leaks. It was all in a big, grouted bulkhead, and we worried about leaks in that thing. It had been there for a long time, because it wasn't a one shot thing. That was there for all time. We worried about leaks in that, and in the course of preparation we had closed that door I don't know how many times. At night on the late shift, when people didn't need to be in and out of tunnel, we closed that thing, sealed it, and then we would pressurize the inside with big blowers and compressors. Then we would check for leaks all over that face. We did it with little squirt guns with soap bubbles. And it leaked. We pressure grouted that plug, and did it I don't know how many times. We thought we were probably wasting a lot of money, because it was a massive effort, but I think it saved our bacon in the end.

Inside that door we measured the temperatures and pressures, and it was pretty clear we had a bad environment right at the door. Later you could see the cables that were inside by that gas seal door, and the insulation was hanging down in festoons. It was really the last barrier, but it held.

Carothers: My recollection is that there was a seepage of a few hundred curies of gas, but there was no venting, and no particulates.

Kennedy: That's right. It took about one minute for it to cut loose. At plus thirty seconds, in the control room, we were patting each other on the back. There had been a perfect, flawless countdown through zero time. Everything turned on the way it was supposed to, and it was ideal technically, from the data standpoint. Everybody was really beginning to feel wonderful.

Then I got a call at plus one minute and they started giving me RAMS readings. "RAMS reading inside the overburden plug is greater than 10,000R." That meant the meter was pegged, and they didn't know what it was. Well, the one by the LOS pipe you would expect to go very high, because it was in a high shine area. And then they said the same thing inside the other overburden plug, in the work drift. And then at the gas seal door. When they said that I had this terrible sinking feeling. That's when we all turned as one and looked at the CCTV picture of the portal, just waiting to see it belch fire, or whatever.

Carothers: And after about plus one minute or maybe two, probably everybody looked at you and said, "Well, you're the Test Director. What are we gonna do, Jerry?"

Kennedy: I remember quite well what happened. I was in the control room, and we had a hot line to the Test Controller's table. A guy handed me that phone, and said, "They want to talk to you," and it was Byron Murphy. He was the Scientific Advisor for the event, and he was sitting down there with Bob Thalgott, who was the Test Controller. He said, "Jerry, I know you're going to be a little busy up there, but do you think you might be able to stop down and see Bob and I?" I said, "Yes sir, I think I can." I remember walking down the hall to the War Room, like I was on stilts — kind of in a shocked feeling. It was a bleak day.

Carothers: You did reenter?

Kennedy: Yes, after a long while. I can't tell you the date right now, but it was many moons later. Of course, right at first it was hotter than heck. The tunnel was hot all the way out to the gas seal door, so the whole tunnel complex was contaminated. To get back

to the drift where the pipe was we mined parallel drifts all the way back, because we couldn't go through the old way - - it was too hot, and too difficult to decon, so we mined new drifts.

Carothers: That wasn't just a gas leak in the tunnel. It sounds as though that tunnel was in direct communication with the cavity, and that there was device debris all over the place.

Kennedy: Oh yes. It was very bad. We parallel mined all the way back in, parallel to the pipe drift itself, and made cross cut entries at interesting points into the drift. In some places we couldn't go still, so we would just put a hole in so we could insert some intrumentation and look around. Some drifts we crossed in the reentry mining we had to stem because they were rather hot areas. There were places where you couldn't stop and look around. In 5 R fields you don't loiter, and we didn't.

Carothers: You had the overburden plugs, and the gas seal door. Did you have any closure hardware on the pipe?

Kennedy: Other than the front end we had a thing called a dimple machine up front, which was to cut off flow in the LOS pipe close in. We had some experiment recovery packages that we hoped we would be able to mine in and pick up and take out, which we did do. Then farther out we had our fast gate, and then one of those gravity fall doors as a backup, the way that DNA did it.

Mighty Oak - - 04/10/86

Carothers: Bob, what are your thoughts about Mighty Oak?

Bass: Mighty Oak. I cannot give an official statement about what happened on Mighty Oak. That's somebody else's province, but I know what happened. I can tell you exactly what happened on Mighty Oak. On Mighty Oak there was too much pipe flow, immediately, and the MAC doors were taken out. We did not have a FAC, the fast acting closure, so those doors were our first line of defense.

The MAC doors came across, and we monitor how those doors move, and where they are. Those doors came together, and they got just about to where they overlapped, and they slowed down. That is the time when the pipe flow gets there. Now, pipe flow is my field. That's where I really have worked on measurements - -

what's going on in the pipe, and in the stemming material around it. We monitor those pressures, and we know when things get down there to the doors. We saw that the pressure got there when the doors were just beginning to overlap. They didn't hit, but just as they obscured the pipe, they stopped. Right at that same time, after this happened, the pressure gauge in front of the GSAC, which is fifty feet further down, picked up pressure.

Now, there are two flows of material to analyze. There is the radiation blowoff, and material from the closure of the reverse cone spool. Both of those produce material running down the pipe. There are approximately ten kilograms of blowoff material in an event like Mighty Oak. It moves at two to three centimeters per microsecond, so it gets down to the MAC doors when they are are still back, but there is hardly any pressure, because it's a very low pressure situation. It's just a little puff of ten kilograms of material. You hardly can see it.

But as the doors close, the second flow comes along, and the second flow is the material injected by the ground shock beyond the reverse cone, closing it down. You have water vapor from the stemming material, you have iron vapor from the pipe, and the pressure at that point is appoximately 200 kilobars. When that goes on axis that takes you to megabars, so you vaporize a little iron and everything else. That material comes down at a half centimeter per microsecond. It gets to those doors in 15 milliseconds, which is exactly when the doors meet.

Okay, something is happening here, and what I firmly believe is, because of an error and a change in the pipe structure up around the reverse cone, they had a much heavier pipe than usual. They had a heavy pipe there to support the helix, and then they took the helix out, but left the heavy pipe in. So, we have a lot of pipe now. On two events, Misty Rain and Mighty Oak, that shrapnel followed the early blowoff flow down, got there just as the doors were coming together, knocked the doors out, and so the second pressure came through. When you get to those doors there's a hole big enough that we had 500 psi against the GSAC, which is the next closure down.

Anyway, we knocked that hole in the pipe. I am firmly convinced that we injected some close-in iron that got down there, and knocked a hole in the doors. With the doors knocked out early, then you had a path for stemming flow. So, when the stemming got

there after thirty to forty milliseconds, a huge amount of stemming went through those front doors, and that took out the next door back, and it kept right on going. We just had a ram running down through there, and it took everything out.

Carothers: Dan, what do you think happened on Mighty Oak?

Patch: Well, I think the focus we had on Mighty Oak was really what we think of as material property problems, both with the site itself, and also interacting with the design. This was one of the designs in a series that used a taper that was larger than had been done in the past. It used stemming that was weaker than had been used, and the weaker sections were brought in closer. Also, if I'm not mistaken, some of the grout formulations were twiddled toward the weaker direction. And, the site itself was highly saturated; there was very little air void.

Carothers: That's supposed to be good.

Patch: It's good if you don't overdrive the stemming, but if you overdrive the stemming, then you can generate a lot of pipe flow, which Bob Bass feels was a very serious problem, because it stalled the doors. One of the things about these gates is, if they're only partially closed, and not fully closed, their strength is very low, because they're not fully supported. My feeling was the doors were knocked out, and there was enough extrusion so the stemming continued to flow.

All of these materials, in comparison even to water, certainly in comparison to air, have a very high modulus. They're very stiff. A tiny amount of flow makes a great deal of stress relief, because the materials are almost incompressible. So, I think there was a low state of stress down the stemming column, and flow started through what was probably a relatively small path down through the stemming. That built up stresses on the TAPS that caused it to fail. The thermal stresses and the pressure loads on the TAPS were such that it couldn't stand the load.

Carothers: In thinking about your small path I am reminded of an interesting tape recording that was made on Camphor. You may have heard it. When they fired Camphor, for whatever reason they had some microphones in the tunnel. For a few seconds it's quiet,

and then there's a little hissing noise that in a few seconds builds up to where it sounds like a train. That opening was eroded from very small to very big very quickly.

Patch: I haven't heard that recording, but we think that's exactly what happened - - that Mighty Oak had a relatively small path, which was capable of supplying a credible amount of gas. There's a fair amount of volume back there, but nothing compared to the volume of the cavity. It wasn't really a nasty flow at first, but once the TAPS let go, and it began to really flow through that path, I think it just cleaned things right out of there. And also I think there's a lot of evidence to suggest that's how Hybla Fair failed also - - that the real failure of the stemming was not a prompt stemming blowout, but from an flow that just eroded the stemming out.

I don't think that Mighty Oak was an impossible test. I think one could successfully design for that site, and I'm not even convinced that one couldn't use that pipe taper, and successfully contain that shot.

Carothers: What would you change?

Patch: Well, that's a fair question. I think one of the things Sandia has done is make the doors on those gates about four times stronger, and they've speeded the gates up significantly. An improvement they've done a lot of work on, and are about ready to field, is to use a propellant, a powder charge, if you will, to drive the doors, as opposed to gas from gas bottles. I think we could speed the doors up enough so we'd have a better chance against them getting stalled in the ways. I think we've got doors that are significantly stronger, like factors of about four. Maybe that's not enough. I don't know.

Ristvet: While I was at S-Cubed I predicted Mighty Oak would be Mighty Oak about six months before the event.

Carothers: What led you to that conclusion?

Ristvet: I happened to be training Dave Bedsun at the time, and doing reentries. That was my only involvement, because my Pacific work really was occupying my time. But I did take the time to come out and help Dave do a reentry, primarily on Misty Rain. Once we got into Misty Rain, which was really the first shot we reentered in the kind of detail you needed to see everything, it became obvious

that the only thing that had been saving us from a containment failure on the previous shots, including Huron Landing and Miner's Iron, was what I call serendipidous block motion. We were shooting the closures out before the stemming even got to them. And, if you didn't have something holding the stemming in, it would go down to the test chamber, and of course, the cavity would follow shortly thereafter.

Misty Rain was just fortuitous. We were a gnat's eyebrow from Mighty Oak on Misty Rain. I said that because of the Mighty Oak geologic setting, the kind of block motion we needed probably would not occur in the LOS drift. There would be block motion on the one fault, but it would occur too late. This was based on the breaking of timing wires, and other studies we had done, so we kind of knew when block motion triggered with respect to ground shock. If Pac Tech's calculations were anywhere near being correct, most of the stemming would be past the fault before the fault would move. My advice to DNA at the time was that it would cost more money to fix Mighty Oak than there was equipment underground. And so my advice was to go ahead and shoot it, and pray that there would be the block motion to keep the stemming in.

Carothers: That must be a characteristic of a particular site or a particular area, because there are a lot of tunnel shots that behaved perfectly well.

Ristvet: Really only N tunnel is where we've seen a lot of block motion, and that's because of the frequency of the faults and fractures. Also the orientation of them is such with respect to the residual stress field that they move easier than they do in P tunnel, say. There we virtually don't have any faults or fractures, and in P tunnel we don't see very much block motion.

Carothers: Ed, is there a reasonable consensus on the reasons for the damage that happened on Mighty Oak? I have heard various opinions expressed.

Peterson: There are a few people in the community who say, and believe, that they understand exactly what happened on Mighty Oak. I think that those people have never been able to convince a reasonable group of other people. And I think if you really had

enough sound scientific evidence to show what happened, everybody would be willing to accept it. People are out looking for the answer.

So, it's interesting. DNA formed the Containment Advisory Team that has looked at Mighty Oak in great detail. I think the people on that committee have tried to look at it very objectively. Everybody has been trying to find an answer, and I think we have been unsuccessful in finding something that we can point to and say, "It's because of this that Mighty Oak did what it did."

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About the Containment Evaluation Panel

The Laboratory or Agency which conducts a nuclear detonation is responsible for the selection of the site, and for the design of any features necessary for containment. The Manager of the DOE Nevada Office is responsible for the safe and proper conduct of the experiment, including the requirement that successful containment be accomplished. The Containment Evaluation Panel serves as an advisory body to the Manager, NVO. It is the responsibility of the Chairman of the Panel to give due consideration to the judgments of the individual Panel members, summarize them, and make a recommendation to the Manager as to whether, from the point of view of the containment design, the event should proceed.

How well and how effectively the Panel has operated is, in some measure, reflected in the fact that there have been only four releases of radioactive material since June of 1971. For these four cases the total amount of material released was quite small - - a total of some 10,000 curies - - and was principally due to the seepage of noble gases from the cavity. A comparison of the post-CEP releases with a few of the major pre-CEP releases, and the total release into the atmosphere for the atmospheric detonations at the NTS is given in Table 1. Recall that for an atmospheric event the total fission fragment inventory is released. For underground events the release is fractionated to some degree by the passage of the material through the earth, the tunnel, the pipe, or whatever the leak path was, and so the comparison numbers should be regarded with that reservation in mind.

TABLE 1

ALL POST-BANEBERRY RELEASES

Event	Date	Release(in Ci)
Camphor	1971	220
Diagonal Line	1971	6,800
Riola	1980	3,100
Agrini	1984	690
	Total	10,810

SOME MAJOR PRE-CEP RELEASES

Event	Date	Release(in Ci)
Platte	1962	1,900,000
Eel	1962	1,900,000
Des Moines	1962	11,000,000
Baneberry	1970	6,700,000
	Total	21,500,000

Release from NTS Atmospheric Tests 1951 - 1963
12,000,000,000Ci

To the extent that the Panel has been successful, or deserving of some credit for the record of containment, that success is based on several things, the most important of which are these:

First: The Manager, NVO, and officials of DOE and its predecessor Agencies have been consistently and strongly committed to the need for successful containment of the events. They have also been consistently supportive of the Panel's activities and recommendations.

The CEP Charter, in Section III - DOE Policies, Paragraph D, has the following words:

Considerations of cost, schedules, and test objectives shall not influence the containment review of any test.

This charge is unusual in its breadth and in the authority it gives to the Panel. Since the formation of the Panel in 1971, every Manager, NVO, and every person in the Headquarters who has headed the Division of Military Applications, or the Office of Military Applications, or the Deputy Assistant Secretary, Military Applications, when asked, has emphasized that it was their intention that this charge be followed by the Panel. No member of any sponsoring organization has ever challenged it, to the Chairman's knowledge, or sought through those channels to modify or overturn a recommendation of the Panel. And, there have been occasions when the Panel's actions have caused considerable costs and schedule delays for a proposed event.

Second: The Members and Alternate Members of the Panel do not serve as representatives of any organization. This is a critical point. They are individuals with experience in the field of underground testing, and knowledge relevant to the containment of underground detonations, who are nominated to serve as independent experts and give their individual judgment concerning the containment aspect of an event.

The Panel members do not vote as to whether an event is expected to be successfully contained, with the majority opinion being the one that necessarily goes forward. The concern of a single member regarding some feature of a containment design has many times been demonstrated to be sufficient to require further review and resolution before the event can continue.

House: I remember one case where Bill Twenhofel, on the Rousanne event, gave it a C! Well, a C is the death knell.

Carothers: I would not send something forward that carried a C. In such a situation I generally suggest that possibly the sponsoring Laboratory might wish to have the opportunity to present further information and explanation before I send my recommendation to the Manager.

House: And boy, did we. And as it turned out, it was a fairly simple matter. There was a site characterization technique we had employed that was a little new to the Panel, and Bill didn't completely understand it. So we journeyed to his lair at the USGS in Denver, and explained to Dr. T what it was we were doing, and what we thought was significant about it, and how it substantiated our structural interpretation. He said, "Oh, I see. I understand that." So, he changed his statement, and we went ahead.

It can be a difficult thing to convince skeptical critics of nuclear test work that the Panel is not some kind of rubber-stamp group, staffed by the sponsoring organizations to give a public facade of responsibility for their activities. Individual integrity has unfortunately been so often shown to be lacking in governmental processes that to claim it for the Panel members is usually met with a raised eyebrow and clearly expressed doubt. Fortunately, the record of the Panel members' activities and actions has been sufficient to convince anyone willing to consider the evidence that the members do, indeed, seriously and honestly review the containment aspects of an event in the full spirit of the Charter.

Third: The sponsoring organizations, and their acceptance of a need for successful containment, are an essential part of the process. Here again, the matter of integrity and honesty is paramount. The Panel fundamentally takes the position that the material presented to them is, in fact, correct within the limits of the Laboratory's and the presenter's knowledge. A mistake may be made, but the assumption is that, if so, it is an honest mistake, and not a lie. A clear example is the number that is given for the maximum credible yield of the device. This is one of the most important factors in determining the depth of burial, and the overall phenomenology of the event. That number as given is accepted by

the Panel as the best that can be given for the particular device, and that uncertainties which might exist in that number are fully accounted for in the containment plan.

In the same way, the Panel accepts as fact that the containment plan as reviewed by the CEP will be implemented in the field, and that the characteristics of the various containment features as built are as they were described to the Panel. The seeps and the leaks that can occur are really prevented by the people in the field who install the cable gas blocks, the cable fanouts, the stemming and plugs, and so on. The Panel relies on the integrity and competence of those people to do the job right, and to describe promptly and accurately any deviations which may occur.

In any organization or Panel that has operated for over twenty years, how it operates and how it might operate in a different manner is a question seriously to be considered. A number of people, CEP members, presenters, observers were asked their opinion of the Panel, and how it operates.

Billy Hudson, LLNL, alternate Panel member:

Hudson: I think that by its very existence the Panel has a strong effect on the way testing is carried out. Knowing that you have to satisfy a Panel of relatively bright people who can ask penetrating questions causes you to look very carefully at your designs. It stimulates attention to detail.

Carothers: At a CEP presentation a person is in a public forum, where the Panel members are going to ask questions. Most people have a certain amount of pride in a situation like that. Not that they're proud of being there, but they don't want to appear stupid in front of everybody.

Hudson: That's right. That's part of it. Another part of it is they don't want to be caught doing something that appears to be stupid after the fact, if indeed there is a failure. So, the CEP is in many ways a public hearing before the fact, only to be brought to light should there be a problem. In that sense I think it has been a very valuable body.

Carothers: What changes would you make in it?

Hudson: It works. Why change it? You know it could be done cheaper, and you know it could be done faster, but you don't know it could be done better. If you said, "Well, gee. That's not a good enough answer. We really should try to do things as efficiently as we can, without sacrifice of quality," then I would say that we could probably make some changes in the CEP. I'm biased though. It's my opinion that phenomenology is the important thing to consider in understanding containment, or affecting containment. Disciplines like geology, for example, are only supplying data for the phenomenologist to think about. In that context then, the role of a geologist, or a hydrologist, should be to say, "Yes, I think you have the right descriptive information," or "No, I don't think you have the right descriptive information." They shouldn't have an opinion about the containment of the event. I would say that in some ways you might have a more effective Panel if it were comprised basically of phenomenologists, and the geologists and hydrologists were cast in the same role as the drilling and cementing people. They would say, "Yes, we agree. You've got the right description," or, "No, we think there's a problem," but not make a statement per se, or categorize.

Evan Jenkins, geologist, USGS, alternate Panel member:

Carothers: How do you feel about how the CEP operates, Evan? What differences would you like to see?

Jenkins: I think the trend towards certain data, and the presentation of only those data is a mistake. In other words, not discussing all the data. I think that our purpose in existing as a Panel is to review all aspects, no matter how benign they might be. And I think it would certainly be beneficial, in a legal sense, should we ever have a problem, to have reviewed all of the data that are available, all that were collected.

Much of the data that has been collected is included as backup data that the Labs have at every meeting, but don't show. For example, the commonly accepted practice is to show the generally east-west cross sections, but not the north-south cross sections. For some events they don't even have a north-south cross section. They should have it and show it. Those cross sections are usually just

horizontal lines, but it's comforting to know that all those lines are horizontal. I think that trend of not showing data could get us into trouble.

As a point of deviation from what I said, I think that the Panel is good enough to recognize points in the document that should be brought up. I hope that we on the geologic side are bright enough to pick up things that should be brought up. I sometimes feel uncomfortable because I certainly don't have expertise in the physics, or the chemistry, or the engineering parts of the presentations.

Carothers: Those people don't have the expertise in geology that you have. That's why there is a Panel.

Jenkins: Well, yes. I have to rely on those other people for these other points. The geology, I think we can handle all right, but I rather hate to sign my name to anything where I haven't seen everything.

Tom Scolman, LANL, former Los Alamos Test Director:

Scolman: Let me say something that I think ought to be done. A great deal of what we have done and do with the CEP, I believe, is to lay down a record that could be examined by whomever. Come back later, and that record offers rational reasons for doing what we've done. It is a record that says, "Yes, indeed, we did look at the proper things. We considered the proper things, and the fact that this thing vented and killed eight thousand sheep in Utah can't really be blamed on our particular community." Frankly, if I were NVO, or if I were even Watkins (Secretary of Energy), I might be inclined to have somebody who could come in with a more or less clean slate, but some scientific appreciation of what we are trying to do, and look and see if we really are doing the right things. Are they defensible? Should we be doing things the way we are, even though some of them were developed for other situations?

Carothers: Well, there are a couple of responses that I'd like to make. One, to take the example of using, as stemming, the coarse and fines layers that were developed for cased holes, in uncased holes. The defense is that they have worked just fine, because LANL has never, on any shot since Baneberry, had seepage on one of their events. So, whether you can justify that stemming design or not, the fact is that it has worked successfully many, many of times.

Scolman: And that's the answer I get every time I bring it up.

Carothers: The other part of my response is that one of the reasons I think the CEP stays the way it is, and does its business way it does is that, like the coarse and fines layers, it has demonstrably solved a problem hundreds of times. Another reason it stays the way it is, is because today it is addressing a political problem as well as a technical problem.

Scolman: That's the point I was making. And I wonder if is it addressing it properly.

Carothers: Well, from the point of view NVO, DASMA, DOE it is. On several occasions I have gone back to Washington for one reason or another; sometimes because there was a worry about the containment of a particular shot, and I am the Chairman of the CEP. I go there and say, "I'd like to tell you about containment." And these are very capable, concerned people who are probably thinking, "If this shot blows out of the ground, there's my career on the line." We go through it, and hopefully they're reassured. Then I say, "You know, we've been in business a long time. The Charter for this Panel basically comes from you, and it says the following '. . . '. Maybe that's appropriate, maybe it's not, in today's world. If you want to change it, certainly that is your prerogative, and we'll do it the way you want to do it." The answer always is, "I don't want to change a thing."

So the Panel stays, and it produces this public record that you're talking about - - we have looked at these various things, we have made no radical departures, our record has been very good, and we stay close to our previous experience.

Suppose you, Tom, decided there was a cheaper, better, but very different kind of stemming, so you changed to that stemming plan. Suppose some leak happened that had nothing at all to do with that, but it happened. You wouldn't be able to justify the change economically, calculationally, theoretically, or however. Somebody would say, "Well, Tom, you had two hundred shots where they didn't leak, and then you changed your stemming."

Scolman: Exactly. No, I agree. It's hard to argue with success.

Carothers: And that's what the people in Washington do not want to do. Nor does the Manager of NVO. I have gone in and offered my resignation to every new Manager. "No, that's fine. We like it the way it is. I don't want to change anything." Actually, I don't think they should.

Scolman: Well, I think the CEP is certainly necessary. I think it's doing good service, and I frankly think, for example, that the chances of us having a Pike-type event, with the CEP, are zero, other than having some designer blow it and get a yield that is perhaps a factor of two or three over design. We might have trouble containing that. On the other hand, I know enough about the design business to think that is pretty damn unlikely these days, so I don't particularly worry about that one.

For a long time I was of the opinion that probably you could come in and present Baneberry over again and get it okayed. I think that's extremely unlikely the way things work these days. Baneberry had enough things against it that you probably couldn't do it.

Carothers: I don't think there's any chance you could get Baneberry approved. The drilling history alone would get it turned down.

There are really two parts to containment. You don't want a venting, and maybe the Panel has helped there. The rest of containment is really the guys in the field, taking care of the seeps and the leaks. Those are really prevented by the guys in the field doing their job right. And, the Panel doesn't really know much about that. The presenter says, "Well, these cables are gas blocked." We say, "Oh, that's good," because cables can leak. But the Panel relies on the integrity and competence of the people in the field. So, maybe the best thing, or the only thing, that the Panel really does is to try to prevent a Baneberry or a Pike.

Scolman: Well, it's interesting, because at least once a year my containment people would come back from the CEP just infuriated, because they felt they had been badly mistreated. That we, Los Alamos, get treated much differently than Livermore does.

Carothers: I don't happen to believe that.

Scolman: Oh, I know that. I take it with a grain of salt. I suspect the same thing happens in Livermore. In fact, Bob Kuckuck has asked me, "How come, why do your containment people pick on my containment people?"

Wendell Weart, geophysicist, Sandia, former Panel member

Carothers: What did you think of the CEP while you were on it? Do you think it provided a useful function, or was it just a bunch of hoops that the people had to jump through?

Weart: I think that in the early days, clearly, it did serve a useful function, because it tended to formalize and focus people's thinking and investigations on areas which experience had shown could be critical. There's probably a lot that went on that wasn't necessary, but it was one of those things that you never know until you examine it. There has to be some formal process for forcing that examination to occur. It's a containment quality assurance program, sort of. And I think that while some of the investigations and things would have proceeded without it, this was a way of making sure that they did, and did in a formalized sense. Everyone knew what was expected, and what kind of information had to be provided. It was more structured than just progress by normal trial and error.

I know there were some instances where one of the Laboratories had to make significant changes - - sometimes in locations, sometimes in designs - - before proceeding. And that is something that clearly would not have been done for that particular event without the CEP.

Bob Brownlee, LANL, Panel member

Carothers: What are your thoughts about the CEP, Bob?

Brownlee: That brings up a point which I think is fair to talk about. I worry a little about the CEP when Jim Carothers, and Gary Higgins, and I are no longer there. I've learned not to trust some of those other guys, because they have not only no memory of the past, which is to be expected, but they really do not have the lessons

of that history either. And therefore, they're capable of just going way off on crazy things, and there needs to be some old hands to balance things there.

We used to not have any turnover on the Panel, but we've had a lot of turnover in recent times. There are some people that you are just not going to educate, but there are a good many others that don't take the time to get educated. And in a while there's not going to be anybody to educate them. When I say that there can be human error, that we're apt to do something really dumb, one of the places where that can emerge is at the CEP.

I've done a thought experiment. Do I think that now, right now today, I could, on my own endeavor - - although I'd like to consult Gary Higgins about it - - design a shot in such a way that the probability of failure was enormously increased, but I could still get it past the CEP without them catching it? Could I get all A's on it? There was a time when I would have thought, "No, I couldn't have." And now I don't think I could either because of Jim, and Gary, and me, and Carl Keller. But if I did just the right things, and conspired with the Chairman, and with Gary, I think I could put through something that would have a very much higher probability of failing than normal, and get straight A's on it. I'll bet you that in five years the ease with which I could do my thought experiment will be greatly increased. And that worries me. Part of it is because the people only go back to '63, and as the years go by they don't even do that.

Joe Hearst, LLNL, observer.

Carothers: You've seen the CEP since the first days, when it was formed. Do you think it does anything useful? Is it a function that once was useful, and now isn't? What are your comments about the CEP as a body, and about what it does.

Hearst: I think, on balance, it's a useful thing to keep the Laboratories honest. Sometimes the Panel does things on the basis of gut feeling, but I think there has to be some sort of reviewing body to uphold standards of some sort. I'm not convinced that the CEP does that as well as it might. I think a great deal of effort is wasted in getting presentations just ever so, and in all the nitpicking - - all the pre-meeting meetings, and all the worry about two decimal

places when you can only measure something to zero decimal places. I think something like the Panel is desirable, but I'm not sure that a lot of what the Panel does is worth the effort needed to make the presentation acceptable.

You might find it interesting to go to a pre-CEP meeting, and listen to the discussions of, "You don't want to say this because it might raise a question," or, "You don't want to say that, because it might inspire someone to ask questions," or, "You don't want to present this information. Keep it as a backup, because it will just lead to a long discussion."

Carothers: No, I have not been to such a meeting. The Panel operates on a presumption that I think is most clearly demonstrated in the question of yield. The Panel takes the given numbers at face value. The belief is that the Laboratory is really telling them the truth about what the design and maximum credible yields are. The fundamental presumption upon which the Panel operates is that the Laboratories will be honest. That shades off into an area with no clear boundary. If everything the Laboratory presents is the truth as they know it, but they don't present everything they know, is that being honest?

Hearst: There is the feeling in these meetings that yes, you should present what you know, but not necessarily all that you know. And you should be very careful about how you work things so you won't get somebody to follow something up and ask questions.

It's like Brownlee saying, "Show me the viewgraphs you haven't shown me. You always make those backup viewgraphs. What are they for?" Those are things they know that they aren't going to tell the Panel, unless they are specifically asked. "Gee, this may make somebody think about differential compaction, so maybe we shouldn't say that sentence. Maybe we should say something different."

John Rambo, LLNL, presenter, observer

Carothers: When did you first start interacting with the CEP?

Rambo: I think I went to my first CEP within a year after Baneberry.

Carothers: What's your view of the CEP? Does it serve a useful function? Was it always, or has it turned into, a political bureaucratic creature, which just serves to validate things in a rubber-stamp way?

Rambo: I think it has changed over the years. I think in the beginning people were honestly frightened of what they didn't know about what causes containment. That led to many ideas, and many discussions about things that may not have pertained to containment. Now it's as though those things have played themselves out over the years.

Years ago somebody who had a personal idea about what containment was all about might have said, "I think this one is a B, or even worse, because I've got my private ideas on containment." When those shots contained, and we went on and on, fewer and fewer ideas were able to live through this whole mish-mash, because the history said, "Look, we're containing, we're containing."

And so, I think this has kind of all evolved down to the place where people have played out their ideas. Things seem to be going pretty well, and we've fired in a number of different kinds of geology, and we can't really sort out any more what's good and what's bad. But the thing that scares me is that every once in awhile I see something in a calculation that scares the hell out of me. But then you go back to the usual things like material properties and things of that sort, and they fall right in the middle. And yet, what I'm seeing in the calculations can be pretty scary at times. So, what do I do? I go to the CEP, in the current frame of things, and things seem like they're just going through like a train running past the station. It's the same old click, "Look at this," click, "Look at that," and the shot passes without any problems.

Byron Ristvet, DNA, Panel member:

Carothers: Is the CEP of any value to the DNA?

Ristvet: Yes. Oh yes. Let me tell you how the CEP helps me, at least. I've always approached the CEP as if I were taking my qualifying orals again for my doctorate. It is a similar type experience, especially in the days of old, when the CEP was little more rigorous, perhaps, in its questioning. But then again, I've thought that maybe it isn't that they're less rigorous than they were,

it's just we're a lot better prepared, and we have convinced ourselves, based on our CEP experience, of what lurks in the minds of the people sitting at that table. Some people talk about, "Well, do you think we can sell that to the CEP?" and I don't approach it that way. I have never thought that way.

I approach it in the manner that the CEP is going to base their judgment primarily on experience, and if we don't have direct experience we have to indirectly derive experience on things. Take the the plugs, for example, the drift protection plugs. I started, when I worked for Carl Keller, actually going out and field checking these things myself, to make sure that they were pretty much like we say the were to the CEP.

Now, I know nobody from the CEP, though they could if they wanted to, could go out and field check what we have built. It just helps me be prepared. I put myself in the position that I'm a CEP Panel member when I'm putting the prospectus together, in that we want to get the Panel to accept the shot, but we also want to assure ourselves. That's why in our vessel concept we proof-test our vessels. And it turns out that our proof-tests at three to five psi above ambient in the tunnels is really a more severe test on the plugs than if we did it at real pressures of perhaps one or two or three hundred psi.

That is because basically we use the Bob Kennedy type keyways, and that is where the plugs seat as you press against them, and as they seat they create a hoop-stress in the rock surrounding the plug. With our pressure grout rings behind those plugs it's impossible for gas to flow around the plug, assuming a fairly impermeable media, which we have in the zealotized tuff. We've done a lot in suggesting the designs, but again, it is these engineering practices, and the attention to detail that is so important.

And that's what's scary in the future, as we lose these people who know what to look for through experience, and who know the tricks of the trade.

Irv Williams, DASMA staff, DOE Washington

Williams: One of the things the CEP has done is try to make sure that the Laboratory people have done their homework. And if they haven't, you know, it's embarrassing to be asked certain questions. I think the CEP is an absolute must, because with a

venting, I think we would go out of business permanently. Another Baneberry, and I think we would be shut out of Nevada. And I don't know any place we could ever go back and test, without a furor, and that includes Amchitka. Therefore I think it behooves us to maintain the integrity and the questioning ability of the CEP to make sure the homework is done by the Laboratories, and that we feel relatively confident that we're not going to have a leak. Without that I think we jeopardize the future of any testing. And potentially the end of the weapons program.

You do need to test, I'm convinced of that. I've been through too many experiments, and too many times we've had people who said, "It's a piece of cake." And then we get a surprise. Some are little, and some are big. We can generally stand the little ones. The big ones make you go back and do your homework. And you can't do it on a computer. You can't do it on a shot table at Site 300, or on a Fermex machine. The only way you can do that experiment is underground.

We have to have the confidence the CEP brings to the Directors (DASMA) here, because they do read the reports, and they do ask questions. Occasionally I have to come back and ask the Panel, "What did you mean?" The words are read, and it's amazing how well they are read by the Directors. The Directors, once they take the job, and they understand the responsibility that goes with it, want to make sure that things are complete, and we try always to make sure it is a complete package.

I've watched the Panel a long, long time, and I've attended meetings where there were some . . . . inspiring discussions, let's say. I think you need to keep inquiring minds in there, and continue to realize that strange things do happen on shots. The people on the Panel need to realize that. That's the big thing, I think. They've got to realize that we get surprises out there. And I feel that maintaining our record is crucial.

Carothers: The CEP Charter contains an unusual sentence, which says that in considering the containment design the Panel shall give no weight, pay no attention, to money, schedule, or data acquisition. That's an unusual charge that the Panel has.

Williams: Yes, and it was intended at the time to say, "We know people will cut corners. We want to make this so corners aren't cut and there aren't incidents and accidents as a result of that." It was meant to give a strong hand to the Panel. That's also why they insisted on the independence of the Panel members.

I have felt comfortable with the way the Panel has operated, and the fact that it remains inquisitive. I would encourage them to keep the Laboratory people on their toes in doing their work, because we all have a tendency to think we're old hands, and dismiss things. Try to make sure that the young bloods coming up are inquisitive, and very serious about their endeavors, so they really fully categorize the experiments. I think the life of the program, from the technical side, rests on our ability to assure containment.

Carothers: I think the people on the Panel, and in the Laboratories believe that too. But an attitude can develop in the Laboratories that the object of the CEP meeting is "to get this thing through." Rather than, "Let's go talk about it together, and see if there's something we missed." That worries me.

Williams: That worries me too. I think there should automatically be full disclosure to the Panel, because, what you might consider to be inconsequential, someone else can consider to be very serious. I feel that to be responsible they should have full disclosure, and do it in descriptive terms, so you can communicate with people back here, so we all understand it.

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Thoughts, Opinions, Concerns

There are many uncertainties and ambiguities that surround the subject of containment. Persons working in the field are certainly aware of them, particularly in the areas of their own expertise. Still, they have been called on many times to pass judgment on things such as the acceptability of a proposed event location, or the possible effect on containment of a particular experimental configuration. Calculations can sometimes offer guidance. Past experience is useful, but not infallable. Ultimately it is the opinions and beliefs of the people involved that weigh heavily in the decisions that are made.

What follows is a collection of some of those opinions and thoughts held by various of the people who have have been quoted in the previous chapters.

Cliff Olsen

Olsen: I think one of the problems in containment is something I had to learn, and I think I learned it slowly. In school, and I think it's almost reinforced in graduate school, you focus closely on something. You have to look at something in great detail, and you tend to lose sight of the fact that there's something else close by. You look at the mechanism of a particular reaction, and you isolate it, and you figure that all out.

In the containment world the scenario is always changing; the environment, and the mechanism concerned, is always changing. For example, if you design a collimator having in mind only what it does to the x-ray flux, and you forget that something else is going to happen after the x-rays are long gone, you can get in real trouble. So, you have to constantly keep thinking about what is going to happen next. Where do we go from here? You have to keep looking at different mechanisms all the time, and how they keep interacting. You can't just say, "Okay, that thing did it's job. Now I can forget it." For instance, on the early shots there were people who would design an x-ray experiment, and they would install it, and forget all

about it. Eventually we learned that you can't do that. You have to look at all the pieces, and you have to look at how they behave promptly, and intermediately, and later on, and maybe even after collapse, when the guy who designed it couldn't care less what it's doing.

I think that was one of the hardest lessons. We had to learn to look through the entire time span of the test, which meant from the time of lighting the HE on the primary to possibly way after it collapsed, and we had to appreciate how everything was going to behave through that whole time period, which is ten decades or more, because a lot of it was uncontrollable. And we just didn't do that in the beginning. We did things that worked fine for part of that time span, but were dumb for a different part.

It was on Umber where a particular thing that became obvious was that you had to concern yourself with things that happen as late as collapse, and that you better be careful about how you engineer stuff to survive collapse. Los Alamos had a line-of-sight pipe with a bunch of valves going off to various things at the surface, and when collapse occurred a couple of those valves sheared off. So, it just started leaking, and there was nothing they could do about it. And it leaked quite a bit.

Bob Bass

Carothers: Bob, what do you think is the fundamental mechanism that leads to containment?

Bass: Mass.

Carothers: Billy Hudson, years ago said that he believed a foot of overburden was more effective for containment than a foot of printouts.

Bass: I think that's probably true. The question of the right overburden has often worried me in Rainier Mesa. We're always firing in the same part of Rainier Mesa, but occasionally there's been a reason why we wanted to pull one out closer to the portal. Then somebody says, "We've got the same amount of overburden, so it's okay." But I don't know that it's as good overburden when you get out towards the portal. It's more of a chopped up mess there. It's got more stringers through it, it's got more damage from erosion.

I don't know that I would trust the same amount of overburden there as I would way back in that mountain. I think you need a competent, solid mass to contain a shot.

Paul Orkild

Orkild: I look at the structure first, then the rock type, and then the water. And then at the stemming. Sometimes stemming, to me, is the all important factor if the geologic media is benign. Stemming is very important.

One of the things that I rely heavily on is past experience. I think predictions about containment depends largely on judgment developed from past experience. I believe that's very, very true. If we didn't have the past experience of the people who are on the Panel, I think that it would be much more difficult. I think that what's going to happen, when you get a new, younger generation, is that they'll struggle.

Carothers: No, they'll have this book.

Orkild: Oh, that's right.

Carothers: "What did Paul Orkild say about this situation?"

Orkild: Oh God!

Russ Duff

Duff: I guess as far as containment is concerned, I would summarize my understanding of it by saying, "I don't." I have been working in aspects of containment-related science since the early sixties, and I've been running the DNA late-time containment contract at S-Cubed for the better part of twenty years. In that period of time I've become very aware of the extreme complexity of the issues of containment. Containment is complex because the phenomenology involved in the explosion includes not only shock physics, but coupled to it are many other processes - - thermal conduction, chemical reactions, diffusion, condensation, and so on - - which occur simultaneously at extreme conditions. And they occur in modified media, and those media aren't well known even before they were modified.

Those phenomena are extremely complex, and our knowledge base is so limited, and our diagnostics are so incomplete that not only do we not know very much, but we're not learning at a significant rate either. I think that in the containment world we're dealing with a situation where a lot of people don't realize how ignorant they are. If it aint broke, don't fix it is an attitude which is unassailable in many respects, from an engineering point of view.

Carothers: Or a bureaucratic point of view. One of the things I've always felt hampered the achieving of a better understanding of containment is the fact that the present system is demonstrably successful - - really remarkably successful considering how little people know. And so, anyone quite reasonably could say, "Why in the world should I spend any money on that stuff? You guys are doing great."

Rambo: That's a very strong argument, and that's what I hear all the time. You have to convince somebody you need to know something, for a dollar value, and that's where the nebulous part of this decision making comes in. What more do you need to know? Until you have a problem, you'll never know that you needed to know it.

Duff: I would make an alternative argument. We know that Haymaker is the only event that has leaked in the 60 kiloton or so range. We have shot I don't know how many events that have yields higher than that. Even in the days before Baneberry, without all the things we do these days, there never has been a leak from events in that yield range, no matter what was done or not done.

So, using that as an example, I respond to your statement, "We have a successful program, so why spend money," by arguing, "We have a successful program which is wasting a lot of money in a lot of respects. If we better understood what was going on we might save a bundle." It might well be that if we understood what was going on we could bury events at, say, just to make up some numbers, a scaled depth of burial of 80 meters, with an attendant savings of hundreds of thousands of dollars in cables, and drilling, and stemming, and time. I can't prove any of that, and that's the problem, but nobody can prove it's wrong either. We can't really make a risk-benefit analysis and show that if you put out this much

money, you'll save that much. We can't do it because we don't know what the answer is, or even what direction the search should take.

And we deal with a management system, a real world environment, where containment is often a necessary evil. You, Jim, have called it a reluctant science, because it is a drain on important resources; time, money, and thought. And therefore, the Laboratories have been very conservative in their designs. They have done very little in the way of what I would call containment research. Their containment programs have been largely minimalist programs. They do whatever is required to get the job done, but no more. No more. The science of containment has not, to me, appeared to be a matter of much concern to the Laboratories.

Now, I understand that, but as a scientist who has lived and worked at both Los Alamos and Livermore, and who has fond memories of those days, I am frustrated, and have long been frustrated, by the propensity to rely so heavily on experience. And by the fact that so little is done that is aimed at trying to understand what's really going on. There has been relatively little research and analysis through the years by the people who are doing most of the work, and progress has been relatively slow.

DNA, on the other hand, has at least had a long-term, consistent program aimed at trying to understand what is going on in some areas. Even there, however, there are very strong elements of conservatism, and very strong parochial views, and play pens of one sort and another. The economic, administrative, and political constraints which have influenced the DNA effort are very real, and they are constricting to the research aspects.

As a result, after spending a very fair fraction of my technical career as a containment specialist, I can't claim to understand what's going on very well. I think I have a broader understanding of aspects of the phenomenology than many of the people who work in the program, but that's only a comparative statement, not an absolute statement in any way.

Bill Twenhofel

Carothers: Bill, when you look at a proposed event, what do you look with regards to containment. What do you think is important?

Twenhofel: I look to see whether there's anything about this new shot that differs from previous experience, with emphasis on geology of course. Are there any flags that come up that say, "This location is different."

An active fault nearby that would move a lot would concern me. And big acoustic interfaces, like the Paleozoics. We don't have a lot of experience shooting near the Paleozoics. Obviously, high carbonate content is a culprit. To summarize what geologic factors should be looked at; faults, acoustic interfaces, carbonate content, clay content, and anything that is not within experience.

Tom Kunkle

Kunkle: Why is it a hundred twenty scaled meters keeps a shot in the ground? We have had shots vent. Only a few times, but shots buried at eighty scaled meters have, on occasion, vented. Even ones at larger scaled depths have. There is certainly historical precedent. Baneberry, for example, a shot that was buried at what we considered a conservative scaled depth of burial was able to push gas to the surface. So it is possible for shots today to do that.

That's a point that we, in modern times, tend easily to forget. We have such confidence in our calculations and our history that we tend to forget that it really is possible that shots buried at a hundred and ten, or a hundred and twenty, or a hundred and thirty scaled meters, or absolute depths of four hundred meters, could vent to the surface. There's some reason that they stay in the ground, and we think we understand that partly, but we don't have any good corroboration. And so it's possible for me to get worried about events, even very large, very deeply buried events.

Bill Flangas

Flangas: We tend to think of one kt as just a little shot, but one kt is a fearful amount of explosion. If you convert that to boxes of dynamite, you realize what a great amount of energy you've got there. You do that under a variety of conditions, and a variety of ground conditions — sometimes saturated with water, sometimes not, sometimes perched water tables, sometimes a pattern of fractures that may or may not lead into the ground zero, so there are a lot of variables. Sometimes they react differently, but in one lifetime I think the testing community has just done an extraordinarily good job of dealing with violent explosions, and controlling them.

Again, when you're dealing with a dynamic force this big, after you've called your best shot, there are still surprises. And they will continue to be there. I think though, between all of us, we have certainly minimized them. I've seen published numbers of the shots that have been done, and it's perfectly obvious we could not have done, in one generation, our generation, that many hundreds of atmospheric events to achieve the reliability of the weapons we have today. I can just not imagine us having shot hundreds of atmospheric shots.

Bob Bass

Bass: I'll tell you where money ought to be spent, when it is, if it ever is. I'm effectively quoting Billy Hudson's ideas on this. I think it's important that containment not rule the experiments. I think there has been a tendency in recent years for containment to be the driving feature. "You can't do that, because it isn't a good containment idea." Billy says, "No. Tell us what you need to do, and we'll figure how to do it."

Carothers: That's exactly right. I know that the Laboratories don't present some things to the CEP. They say,"Well, we will just get hassled about this, so we won't do it." That's wrong, because the CEP might say, "You ought to calculate this," or "You ought to do that, and I'd feel more comfortable, but it can be done."

Bass: Yep, "This is the rule, and this is what we follow." I say, "Experimenters, come. Propose your experiment. There's a way to do it." And if somebody comes up with a reason to do something, we will find a way to do it.

Norton Rimer

Rimer: For containment, clearly absolute depth helps. There's an example that's important that I don't think has ever been brought up at the CEP. For example, if we ever shoot an event in granite, we need a totally different depth of burial criteria to avoid seeps. I did a number of calculations, probably fifteen years ago, for various reasons, about shooting an event in granite. I think the containment depth I came up with was at least 150 meters times the yield to the one-third.

Carothers: By the existing criteria, that would be very conservative.

Rimer: Well, I don't think it would be very conservative at all for granite. I'd be happy with 180, but you know what drilling costs are. It's another medium, and for releasing gases it's a different ball game.

The stronger the rock is, the more it's likely to have tensile failure. That's a funny thing to say, but it's a question of equilibrium at the end. It's the question of continuity of radial stress, which is a boundary condition. The amount that the radial stress can differ from the hoop stress depends on the strength of the medium, so a stronger medium can have hoop stresses much lower in compression than a weak material like alluvium or tuff. A hard rhyolite is the closest thing to a granite that we shoot in at the Test Site, but it's not near as strong — it doesn't have near as high a wave speed. The hardest rhyolite I've seen, the seismic velocity is 4200, 4400 meters a second, and you can get a shear modulus out of that. Granite is 5500 meters a second.

I calculated Pile Driver ad infinitum. Tensile failure occurred from the surface down to below the Pile Driver cavity, in those calculations. Then we calculated deeper shots, on a scaled basis, and even then I got fractures down to the cavity. It was only when I got to higher than 150 scaled meters depth that there was a small - - twenty, thirty meters - - zone of unfractured rock above the cavity. Now, the porosity in some of those fractures was very small;

ten to the minus three. On the other hand, I didn't assume there were joints down there, so even 150 meters scaled depth I'm not all that happy with, for late time seeps.

That's based on tensile failure calculations that we did for different yields and different depths. I think we did 100kt at 1000 meters, 20kt at 1000 meters, 20kt at Piledriver depth, which was 460 meters. We did a number of calculations. We didn't do the whole parameter space, and of course, the models were not as good back then. We've improved some of the things in our description since.

Bob Brownlee

Brownlee: I really think that we have reduced the probabilities of venting so low that what we're apt to get caught up on is something trivial. That's what I think. I am convinced that nowadays the probability, by the time we get a shot reviewed and down hole, of it venting is very low for most of our shots. Of course, it's not the same for all shots, so when we do a certain kind of shot, the probability could be much higher. Now, I have argued you ought to react differently depending upon what the circumstances are. You can take the view, and I understand it, that it's good to always look for the worse case and plan your activities accordingly. My response to that is, "Yes, but that communicates the wrong idea to people."

My feeling is that on the average shot now, if it cannot be compared to any previous failure, then we have to postulate something brand new to have it fail. And we have been testing long enough with a variety of different kinds of things that something brand new is highly improbable. Our luck has been that if it is likely to have happened, it would have happened to us. We would have given it the opportunity to happen already.

Carothers: Would you say, "That's true as long as you confine yourself to the Nevada Test Site."

Brownlee: Oh, yes. That's implicit. Notice what I said. "If you can't compare it with any failure we've had." That means at the Nevada Test Site. If I go to a brand new area, in a brand new medium, I now have nothing to compare to. I have to assume, therefore, that I have to start over.

When we talk about containment, I've always lived in fear of some perfectly simple thing that everybody knows is important doesn't happen to get done. Once in a while I know that I annoy the dickens out of people here, because I ask, finally, "Did you get the stemming in the hole?" What I'm really asking is, "Have you looked at the things everybody takes for granted?" And they hate that question. They just hate it. But I still think that we've got a chance one day of buying the farm for the most indefensible, grossest, error.

Carothers: Duane Sewell would thoroughly agree with you because, when he was at Livermore, he really was quite concerned with safety. An often used expression of his was, "I'm concerned about ten year-itis." You put some new people on a job, or project where something bad could really happen. They didn't know what they were doing, so they worried, and they worked hard, and they learned, and after a time they got to where they were, in fact, experts. And they did this risky business all the time.

Brownlee: And ten years later?

Carothers: Ten years later, of course, "This is a piece of cake." But it's not a nicer piece of cake. It's no less a hazard than when they first looked at it.

Brownlee: And it may be more of a hazard, because in the meantime they've changed a cable, and they've changed the firing set, and they've changed something else. And you've also probably changed out the person who did it, and who remembers?

Byron Ristvet

Ristvet: Let me emphasize we have two definitions of containment at DNA. One is in the classic CEP sense. At one time our containment experience with horizontal line-of-sight shots wasn't much better than the vertical experience of the Laboratories. Today, with regards to the CEP kind of containment, I have very little concern about uncontrolled leakage to the atmosphere from a DNA event. That is especially true now with the lower yield events. For low yields our tunnel volumes are huge, so any threats against the plugs are rather small, and it really makes that part pretty straightforward. Especially because we proof test everything, and we do spend a lot of time on attention to detail.

Carothers: The DNA people really work very hard to protect the samples. If they achieve that, any release is very unlikely.

Ristvet: That's exactly correct. Sample protection is our other definition of containment, and that is very important to us. We have spent a lot of effort trying to understand how to be as confident of that as we are about a release to the atmosphere, but there can still be surprises there.

Ed Peterson

Peterson: Let me tell you what I think our design philosophy for the line-of-sight events has been very recently, and in which I really believe. I think that in a containment design you have to make the first closure, the one that's closest to the working point, sufficiently strong so it can act as a bulkhead to the stemming. You have to know that closure works, and no matter what pressures you get in that stemming for whatever reason, you won't extrude the stemming out through that bulkhead. In the low yield case this has so far worked satisfactorily with the FAC. I suspect in the standard yield shots we have from now on the first closure will be a real heavy-duty closure that can do that. DNA has been designing one like that. So that's sort of number one.

I think the second thing you have to do is, once you understand within your error bars what the conditions of the formation are where you're working, and you know your yield so you know where to place that closure, you then make your line-of-sight pipe so it fits the closure at that place. In other words, you don't move your closure just to accommodate a bigger pipe taper.

I think those two things are basic. Make sure that first closure can act as a stemming bulkhead so you can't extrude your stemming out, and then make sure you position that closure correctly, and make your line-of-sight pipe taper accordingly. I think those are the two major design features. All the other stuff is nice, and all the other stuff you should probably do, but those are the ones I think will save you if something goes wrong.

Of course, you have to go into things you consider sort of QA, such as making sure your tunnel diameters are right, making sure that the grouts are in per their design characteristics, and on and on like that. We aren't to the point where we want to throw any safety margin away.

Carothers: To oversimplify, "Don't get too sophisticated. You've got to have some strength close in to handle things. If you have that, it will make up for a lot of what you don't know."

Peterson: I think that's true. I think we do a lot of, call them good analyses or sophisticated or difficult analyses, and I think they've been very good in that they have given us a way of thinking about things. In other words, they give us some idea of how things may be occurring, and what parameters may be important, and which ones may affect you. But I don't think that at this point you can consider them predictive type analyses you can base a design on. I think that since you don't want it to leak, you'll want to look at the things that will serve as a brute-force type of containment.

One of the calculations we do routinely is to look at the conditions at both the overburden plug and the gas-seal plug. We've looked at them compared to events where stuff has gotten into the tunnel, and we have not as yet measured anything in the tunnel that is worse than our worse case prediction. Exactly why I don't know, but we haven't. And I think those plugs are very important as far as backup goes. Everybody wants to design the tunnel stemming right so nothing gets into the tunnel complex, for obvious reasons. But I don't think you could ever guarantee that something won't happen.

It is a little frustrating, and discouraging sometimes, to look at what you've done, and realize that you cannot really model containment as such. I suppose one would like to, but I don't know how soon that will be possible.

Carothers: Well, there are people who believe you can build an expert system and just punch in the parameters of the shot, and it will tell you what to do.

Peterson: I think those are only the people who don't understand. My picture of the expert system may be different than yours. I see the expert system as being able to provide you with some idea of things that have gone on before, and some idea as to why they've gone on. In other words, if you come up with a

particular problem you might be able to access your expert system and put in that problem, and then you might be able to call up what Joe LaComb says one should do. But I think it's going to be meaningless unless you also get the Joe LaComb type person to tell you why he thinks that is why you should do it. If you don't get the understanding behind it, I think just having the facts are worthless. So I think the expert system might give you some aid in being able to learn how to think about it, or at least know what previous people have thought about it.

Carothers: There's more to knowledge than facts. I have a little poster my daughter Margaret once gave me. It has a picture of three apples. One is green, and one is yellow, and one is red, and it says, "Time ripens all things. No one is born wise." And so sometimes it is worthwhile to talk to a person who has had time enough to get a certain amount of wisdom. It's often easier to find facts.

Peterson: That is true. People like Gary Higgins and Russ Duff and Bob Brownlee and Joe LaComb have an insight, from having been around the program for so many years, that other people just do not have. And it will eventually get lost. You can't learn from them all the things that they know.

I think when I first came to S-Cubed people sort of believed they understood containment. Now, there were always disagreements within the community as to what we understood, but one cannot really argue against success. I think it has become apparent, in the last seven tests, say, even though most of them have worked extremely well, that some of the things we thought we understood we really don't understand well at all. And I think everyone, or nearly everyone, in the community is beginning to believe that. I think that belief is also necessary in order for people to go forward, and so I think that has been a benefit in gaining understanding.

If you get down to the more technical detail of things, I think we have hurt ourselves by compartmentalizing things. There have been various efforts over the years not to do that, but for whatever million reasons, that is the way it has come out. We have divided things up on time scales, and divided things up between work groups. As an example, we look at pipe flow, and we look at cavity growth and cavity conditions, we look at ground motions, we look at stemming plug formations, and we look at late-time leakage. All

of those things are very important, and they all ought to be looked at. And the capability to look at each one of these needed to be developed. But when you break them up in order to develop them, I think you lose sight of the fact that you've only broken them up so you could look at them individually and develop some type of model. You lose sight of the fact that they are interactive, and you forget to look at the interactive part. I personally believe, in terms of the modeling and the understanding, that is the next direction that one has to go.

In other words, if I understand material properties perfectly, I'm not sure I'm going to be able to calculate containment anyway, because I don't know how material properties interact with all of these other things. So, I see that as the thing that really has to be addressed. I have no idea how to do it. Everyone has ideas, but it's nothing trivial, so one shouldn't look at it and say people over the last fifteen years have neglected it, or something like that. It's an extremely difficult thing to do. I'm not sure how one can do it, but I think you have to look at it.

Another thing is that I think we don't even know how to proceed on some of the problems from the physics standpoint. It isn't that you don't have an expert; you don't even know what you should be expert in. Jim, you're very familiar with it, you've sat through all of these things for years. You know, for example that even on something like a Mighty Oak, the leakage doesn't come until on the order of seconds or minutes. Our calculations stop at less than a second. If we have a stemming column that "fails" enough to let something leak, maybe it has another half a percent porosity compared to one that works perfectly. You don't even know exactly what physics to start building in, or how to do it. So, I don't even know how to interact with a neighbor who's doing a different calculation. I don't even know what kind of an expert I ought to go talk to. It's just that there are very fundamental questions that are hard to get an answer to. I don't know the answers. We've learned a lot, but I'm not sure that we understand containment. We know a lot more about it than we did, but I don't think we really understand it.

Carter Broyles

Broyles: I think we at Sandia still take seriously the charge we got when we went back to testing after Baneberry, which was that each of the three Labs was charged with an aggressive, active R&D program for containment. And I've used that to justify our programm. A lot of people say Sandia doesn't sponsor tests any more, so why should it waste its time? It seems to me that we have served a useful purpose as an independent group, without an ax to grind, a lot of times. Perhaps it's useful to have that third party there at the CEP, and other places.

Carothers: It is. And, your people have produced a lot of very useful data.

Broyles: Well, we certainly have had a better record than a lot of other organizations. We've had a lot more continuity and devotion, but you can get into all sorts of philosophical arguments having nothing to do with containment about what produces good results. I still hold, as a personal belief, that if you have the total responsibility for the program, as well as the measurements, you're going to come out on the whole with better results. It's not that you've got better people, but you don't have the artificial divisions where things tend to fall through the cracks that you have if you have six different contractors doing different parts of the job, and then trying to have what is essentially a contract monitor put it all together.

Something I've seen over the years, probably more in the last five than in the early days, is a more cooperative, not only attitude, but effort on the part of all of the players toward working together, sharing their capabilities. I think DNA, and LASL, and Livermore working together, reinforcing each other, has contributed a lot more now than it did in the early days of Baneberry and prior to that.

But everybody has, Sandia just as much as anybody else, the feeeling that if we didn't do it we can't trust it. When you've got the responsibility - - that's something that a lot of people in the system have never faced. It's like the General who's developing the Minuteman, or the Admiral who's developing the Trident. When his neck is on the line, and he has to guarantee something, that's one thing. If you sit down and ask for a scientific judgment, that's another thing. What you demand in proof, I think, is justifiably

different in the two cases. I can be scientifically very certain that something is true, but am I willing to bet the nation's security on it? That's different, and the proof I'm going to ask for is going to be different. I can recognize that, when I sit back and try to be objective. There are a lot of people not connected with the test business who don't really understand that, because they've never been in those kinds of positions.

Tom Scolman

Scolman: Frankly, my biggest concern about containment is that the CEP, over the years, has evolved into some kind of ritual raindance, which forces us to do things not because they make a hell of a lot of sense, but because it's what we've always done. Unfortunately, while we at one time had an organization called a Containment Research Committee, one really can't do research on containment, because you're not allowed to do an experiment that pushes you beyond the known containment boundaries. So, we are more or less forced to do things the way we've always done them before. Take one of the points that I referred to earlier; the fact that the containment scheme that Los Alamos uses, at least, was largely designed in the days when all holes were cased, and I think many of the things we do don't really make an awful lot of sense, or are completely justifiable in the days when a majority of our shots are done in uncased holes.

For another example, I think there's a great deal more to the containment business than depth of burial, which always comes from the same scaled depth. That assumes you're shooting in a known, homogeneous media, and you never do. I argue, for example, that with the faulting that exists at the Nevada Test Site we have probably, without knowing it, fired in almost any configuration you could have managed with respect to a fault. And yet we sometimes reject shot locations because of proximity to faults. We worry about reflections from hard layers, and yet we can't find those hard layers when we do seismic work. We know the layers are there, but do they matter?

Carothers: What you're saying is seismic work uses acoustic reflections and you can't see those layers. So, how can the shock wave from the shot see them?

Scolman: I've asked that question several times and haven't had any answer yet.

Local geology is important. There are blocks, joints, faults, little ones, big ones. I think what you come back to is the fact that you cannot calculate in the detail that would be necessary, for a number of reasons. The thing you really fall back on is previous experience. And that drives you into doing things that, while they may not be completely justifiable in a theoretical sense, at least they've worked, and it's hard to go away from them.

Carl Keller

Keller: At DNA I think we had a different concept of what the future held than the Laboratories did. We had the time to develop test concepts, and the presumption was that we were going to keep on testing, and that we would need these things. The Laboratory people tended to be in a reactive situation where, if they were going to spend anything on research, it had to be identified as necessary to do a particular shot. And that shot almost never was more than a year or so away, and so all the work had to be done at least six months before the shot. So, what was done was generally only in reaction to a unique geologic circumstance, or a unique test geometry.

Bruce Wheeler

Carothers: To what extent do you think the containment requirements, which were severe, had an impact on the programs you were trying to accomodate? Did they really constraint you?

Wheeler: I don't think the containment requirements had a great impact in terms of how long it took to get the test ready - - to build it, and get ready to go. They added some cost, but it wasn't a lot in terms of the overall cost. Back in Misty North times, that was a twenty-five million dollar shot. Diamond Skulls was thirty-two million. Those two shots today would probably be a hundred million each.

So, whatever incremental cost you could attribute to the increased containment concerns had to be a small percentage. So, I never looked at containment as something that got in our way; rather I looked at it as something that if we did it right would help the program continue.

Billy Hudson

Carothers: Billy, it has been my impression that you are not a strong believer in the residual stress field as a basic, or the basic, mechanism for the containment of a shot. Comments?

Hudson: So there's residual stress. We may always have residual stress of some sort, but is residual stress the key to containment? I can imagine residual stress in a medium comprised of marbles, but marbles wouldn't be a very good container for high pressure gas. Cracks can open, the ground can shift, rocks can shift around. At a quarter of kilobar or so, which is sort of where the residual stress regime is, you wouldn't expect these openings to be smashed shut again. So it's not clear that residual stress can affect containment in the first place, even if it is there.

An interesting puzzle is Baneberry. We didn't talk very much about residual stress before Baneberry, if we did at all. The Baneberry release didn't begin until something like three and a half minutes after the shot. It's hard to tie that time into the models that have been proposed. Most of the models would show failure at much earlier times.

I think containment is a combination of hydrofractures, leakage into porous storage areas, residual stress fields which prevent continuing hydrofractures, good stemming plugs. It's all of that. We know that the failure of a stemming column can cause a release, but probably there's not nearly as critical a relationship as far as the residual stress, or the hydrofractures are concerned.

Carothers: What about the difference in the containment between the hundred kiloton shots and the one kiloton shots?

Hudson: There was a perceived difference that the big shots didn't leak, the little ones did. But as we started to take measurements, as we began to get some data down hole in the stemming column on the higher yield events, we discovered their behavior, at least in the stemming column, was much more like the low yield

events than we had suspected. At one time the data seemed to indicate that if events were of higher yield than between ten and twenty kilotons, gas just didn't get out of the cavity. But then we started making measurements in the stemming column on events with yields in those ranges, and we discovered that gas got out of the cavity just about as often, and went as high, as it did on low yield events. So, high and low yield events may not be as different as we once thought. It may be a matter of depth more than yield. If you bury them deep enough, even though the yield is a lot higher, they may be more likely to contain. We really don't understand the difference, but the phenomenology is not as different as we once thought it was.

Carothers: There is an argument about the observed lack of releases from high yield shots, advanced by Gary Higgins. He says, "Well, that's easy to understand, because you guys are using the wrong scaling law. The containment depth really doesn't go as as the yield to the 1/3 power. Because there's the gravity field it really goes as the yield to the 1/3.4 power, properly. There's no difference at one kiloton, but the higher the yield, the more conservative you're being if you use an exponent of 1/3 instead of 1/3.4. You could have shot Cannikin at 4000 feet, rather than 6000, perfectly safely, using the right scaling."

Hudson: The scaling laws, it seems to me, only concern prompt venting, not the seepages. With regard to seepages, I don't think the bomb knows how deep it is. It just tries, however it can, to find it's way to the surface. In the dynamic case there are all sorts of things going on. There is spall. If it's deep enough spall is not a problem. You have large fractures formed radial to the cavity. Clearly, if it's deep enough none of those are going to get close to the surface. As far as the dynamic features are concerned, Gary Higgins may be absolutely right. It could very well be that the scaling rules we use really don't apply. Unfortunately we don't understand these relationships well enough to argue convincingly that we should bury higher yield shots at shallower scaled depths.

Carothers: Well, after Baneberry there wasn't any testing for about six months. Prior to that time, about a third of the shots released activity, sometimes a lot, sometimes a little. After Baneberry, that pretty much stopped. What happened? I don't

think you learned anything new in those six months, but all the leakages stopped, with the exception of four events over twenty years. What do you think accounts for that?

Hudson: One cause was that we adopted a minimum depth of burial. Statistically, for events sited in alluvium before that time, approximately twice as many events involved a release if they were buried shallower than 500 feet, as those events buried deeper than 600 feet. And so, one of the things we did was to adopt a minimum depth of burial. What that did was to avoid some of the higher carbonate content alluvium near the surface.

Even before Baneberry we had adopted the practice of putting cable gas blocks on all cables. I think that was just shortly before Baneberry. The combination of those two acts - - putting in the gas blocks, and increasing the depth of burial - - I think was primarily responsible for eliminating most of those releases.

Right after Baneberry we did quite a few things that we later stopped doing, because we didn't need them. For example, when we had experiments in the emplacement pipe we had sections of the pipe that were malleable. We thought that would help the ground shock closure. These soft pipe sections were fairly expensive. We never did show whether they helped or didn't help, and after a while we decided we didn't need them. We did a lot of things right after Baneberry. Everything we could think of, almost, became a viable suggestion as a solution to some problem.

Carothers: The minimum depth of burial of 600 feet has carried on to today. There are people who occasionally grumble about that when they do a twenty ton shot. Do you think it's really needed for shots like that?

Hudson: The answer is, "Of course not. It's not always needed." The problem is, you never know exactly what the yield is going to be. You never know for sure when you're going to need that depth. If the maximum credible yield is really twenty tons, you probably don't need the 600 feet. Then you have to decide what you do need, and why the shot is going to be contained as well at a shallower depth. After a while people would probably decide that it was easier and cheaper just to use 600 feet.

Actually, it's questionable whether we should be shooting in alluvium at all. You will notice that there have been very few shots in alluvium since Agrini. Agrini was a shot in alluvium, and there was a release through a strange subsidence crater. The crater was something like 200 feet deep; very deep compared to its diameter. So there was probably much less rubble to filter the gas and debris before they got to the surface than on a normal shot.

After intense study of the Agrini event, we decided the only thing we could have done that would really have guaranteed that we didn't have that late time release would have been to avoid the noncondensable gas, which is primarily the carbon dioxide released from the carbonate minerals in the cavity region. While no one made a public statement about it, for several years we did not fire events in alluvium.

Statistics suggested that it you stayed at carbonate contents below 5% it was unlikely that you would have a late time release problem. Above 5% you're much more likely to. We had the Riola event, which was a case where a plug failed, and we had a late time release. The carbonate content for Riola was only about 2 1/2%, so people tended to ignore the carbonate problem, and focus on the plug that failed. Then Agrini came along, where we had a late time release with a strange subsidence crater; and the carbonate content was 2.54%.

I argued that in both cases we might very well have had a release without the strange occurrences associated with those events, and that if we wanted to avoid that sort of release we should stay out of the alluvium. Often we really can't tell what the carbonate content is. We make measurements, but they're not representative, and it could be that the carbonate content at either of those two sites was high enough to cause a release.

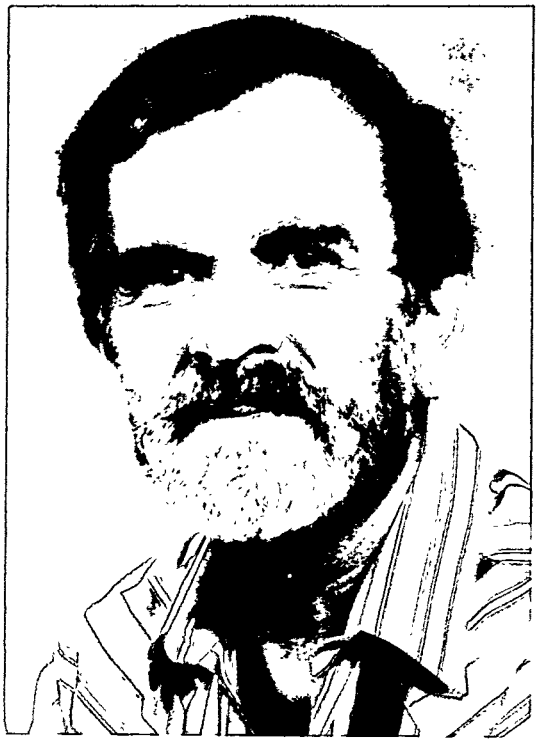
In tuff we've only had one event, as far as I know, in the history of testing, where there was a late time release, and no one really understands why it happened on that shot. I talked to Larry McKague about that, and he suggested that perhaps there was a pocket of stream gravel, in the vicinity of the working point, that could have given rise to that release. If you throw that one out as maybe being a weird geometry, there just isn't enough carbonate in tuffaceous material to be a problem. But you can always have it in alluvium.

Billy Hudson's closing words perhaps make a suitable summary and ending for this book.

I guess the upshot of all that is, we still don't really understand containment very well.

APPENDIX

The people who made this book possible. Somethings about them in their words.



Fred App
LANL — Alternate Panel Member

I went to school at Penn State, and my degree is in geophysics. I graduated from there in 1959. From there I went into the oil patch with Continental Oil Company, and spent six years in exploration geophysics, mostly with seismographs. It was mostly field work, but there was some analysis. About the first four years were field work, and the last two were mostly in the office.

In the field we did a standard type of reflection geophone seismic survey to determine the structural configuration of the strata. One way of doing that is to put the energy source, dynamite, down a hundred foot deep hole. You have several holes spaced some distance apart, depending on what kind of a survey it is, and then there is a surface geophone layout to pick up the signals. We have done them at the Test Site.

There are various types of surveys that are made; there are explosion surveys, and vibroseis surveys. The vibroseis sends out a sweep of signal frequencies in about six or seven seconds, and no frequency repeats itself in the sweep. So, it's a unique wave form that goes out, and they cross correlate what comes back with the sweeps, and you end up with your actual time history recording. The vibroseis system was invented by Conoco, and at the time I was working with them nobody else was licensed to use the system. Only Conoco had it.

There were two reasons why I left Conoco. One, I simply got tired of that particular line of work. I wanted to move into hard rock geophysics, that I thought would be more interesting. The other reason was that in order to be successful, and really advance with the company, you would, almost by definition, end up in Houston. That was the headquarters, and was not an end point I desired to be at.

Another option was Ponca City, Oklahoma, which was better. It's north of Oklahoma City. Of course, if you look at a frequency chart for tornados, you'll see a contour closure that takes in Wichita to the north, and Oklahoma City to the south. And Ponca City is right in the middle. But it's a nice place.

So, for those reasons I decided I wanted to try something different, and for a short while I was with Anaconda, in Butte, Montana. That was a mistake. It was copper mining, in deep mines. In Montana I was working below sea level, an indication of how deep the mines are. One mine was 6000 feet deep. It took a while to get down, and to get back up.

As far as I was concerned, that whole operation was very dangerous. The company itself was not very safety conscious. There were many ladders with rungs missing, and that sort of thing. They had No Smoking signs right at the shaft, and of course everybody would be smoking — and that was the only way out. I left primarily because of the safety problems.

I returned to my wife's home town in North Dakota. I had quit Anaconda without having another job lined up. I started reading the classified ads in the papers, and applied for and got a job with Control Data. Control Data at that time was a booming outfit, and the reason they were booming was because places like Los Alamos and Livermore were buying their 6600 at that time.

At that time Control Data was flush with cash, but they were shy of programmers. So they decided to try an experiment. They decided to take applicants from everywhere — one person might be an art major, just out of school. Another person might be an electrical engineer who had been in the business for ten or fifteen years. In one case they took a seismic explorationist, namely me. I believe there were about 35 in the group. We were brought in to Minneapolis, but they did not bring our families because it was quite intensive training; days, nights, and weekends. You had enough time to sleep and that was it. A second reason reason for excluding families was because if you failed the course, you were not hired. I successfully completed the course, and became a permanent employee of Control Data. I stayed with them for five years. However, all along I knew I did not want to remain in a large city, so I continued searching for employment.

In 1971 I read an ad in the Minneapolis Tribune, offering jobs at Los Alamos, with talk about the beautiful mountains, and skiing, and hunting, and all that sort of thing. These jobs were for C Division, which is the computer division. I applied, and they invited me down. I talked to two C groups. In the meantime Bob Brownlee happened to see my resume, and he asked to interview me as well. After I had interviewed the three groups I had no doubt about my first choice. The way Bob described the containment work, and what was involved, appealed to me.



Bob Bass
Sandia - - Shock Physics

I'm a physicist, so to speak. I went to school in Missouri, which is near Kansas City, in my grade school days and high school. My high school background is rather mixed-up, and strange, and messy - - screwed up by the war.

Missouri had a very strange, and little known situation. It was patterned after the University of Chicago, where you can start to college whenever you're ready, and whenever you can pass the entrance exams. There was not even a limit, at that time, on how many hours of high school credits you had to have. So, I and about four or five other people in my high school class decided we had had enough of high school We decided, "Hey, we've had enough of this. We know everything. Let's go to college."

So, I started in college, at the age of fifteen, at a place called Central Missouri State College, which is now Central Missouri State University. By the time my high school class graduated I was well along as junior in college, mainly because there was this Navy V-12 program at the school, so it was on a trimester basis. So, you could get sixty hours in one year, and I did. By the time my high school class graduated I think I had about seventy hours of college credit. And I had no problems with that at all. It was easy, duck-soup easy. I was also helped by the fact that I was six feet seven inches tall at that time, and weighed about 220 pounds.

I stayed there for one year plus, and then went to the University of Missouri. I started out in chemical engineering, or something like that. I had studied more chemistry than anything else, but it was all physical chemistry. Then the draft came along, and I ended up in the Navy, at first.

I went to San Diego, and went through part of boot camp there, and then they discovered I was too tall to be in the Navy. The war was over, so they said, "Out." I said, "Fine. I'll go." So, I went back to Central Missouri State, and graduated right away. Over all, I think I finished in two and a half years. The degree turned out to be a double major in chemistry and physics, with some background in economics, of all things. And what are you going to do with that?

So, I went looking around a little for a few months, doing nothing, and I ended up going to graduate school at the University of Missouri, in physics. I had discovered that chemistry was a nice, interesting field, with some very nice people as professors. Some of the most entertaining people I have ever known were organic chemists. But, I didn't feel too comfortable with all of that, so I ended up at the University of Missouri, in physics. I fiddled along at Missouri, not being the greatest student in the world, to be honest about it all. But I was chugging along, and then came Vietnam.

At that time there was no such thing as an educational deferment, and so I was draft bait, and I ended up in the Army. I ended up in Fort Hood, Texas. Just about that time they were beginning to think there should be educational deferments, and they had begun selecting out people with some educational background. They were sending these guys back through the Pentagon for

assignment, as enlisted men, all over the United States. I ended up at Fort Myer, in Washington, and at that time they were getting ready to do a thing called Operation Windstorm.

Operation Windstorm was an underground shot, scheduled for Amchitka in 1951. It was to be a cratering event. So, they had all these plans going forward, and the Signal Corps had a major project to measure the residual contamination from a cratering burst. They contracted this job out to the National Bureau of Standards, and lucky me, I got to go to the National Bureau of Standards, in Washington, as a civilian guest worker. The Army sent me there, on travel status, and I spent two years on travel status for the Army, working at the Bureau of Standards.

Then it became obvious that Amchitka was the wrong place to be using as a test site, because, for one thing, the Russians were listening in on it all the time. At the NBS we had built a huge system, to be used on Amchitka, to measure residual contamination. Everything was in waterproof packaging. It was to be installed in prefabed underground concrete structures that had been built by the Navy up in Seattle, or somewhere. We were all done; we were ready to go. The Navy was ready to start shipping these prefab structures to Amchitka. And what did they do? They turned around and shipped them all to Nevada, and this became instrumentation on Jangle ESS. So, we had all these waterproof concrete bunkers out in the desert.

The detectors were all underground in these structures, and they jumped up out of the ground after the blast wave went by; there were elevators to raise them up. There were 121 channels, and we recorded 121 channels of perfect data. Of course, we had two years to get ready. And in those days we had an unlimited amount of money, and we had the whole backing of the National Bureau of Standards to get good data. I have never been associated with something like that before or since.

When I got out I went back to finish my doctorate. Then I made the greatest mistake of my life. I left the Signal Corps in July, drove back to Missouri, drove back to the campus of the University of Missouri, and went to visit friends in the veteran's housing area. I looked at the poverty those guys were living in, and I said, "I can't do this. There's no way." I was making 850 dollars a month in

1953. That was pretty good income. I was single. When I was living in Nevada we were getting expenses the whole time. I was very rich. It was more money than I knew what to do with.

Looking at the campus environment, I couldn't do it. I said, "I'm not going to do that. That's not for me, right now. I'm making too much money." So, I went to work at a radio station, and fiddled around. At the time, though, I had met some people from Sandia, working at the Test Site. I liked what I saw, I liked what they did, and I said to myself, "The Department of Defense is on the outer edge of all this. I'd rather be in the middle." So, I knew people from Sandia, and that's where my formal education stopped for a while. I decided to capitalize on what I had done through the years, and keep on making money. But I went to work at Sandia for 500 dollars a month, so I took a big cut.



Robert Brownlee
Los Alamos — Panel Member

I did my undergraduate work at Sterling College, which is a small four-year college in central Kansas. When I was an undergraduate I couldn't decide whether to major in math or physics, so I majored in both of them. I got my degree, but meanwhile a war had intervened. Then I went to the University of Kansas, where I got my master's degree. After that I decided, "I think I'll just go into astronomy." It turned out that all my training was not immediately applicable to astronomy, so I had to go back and pick up all the undergraduate courses in astronomy. I then went to Indiana University, where I got my Ph.D. from Indiana University in 1955. I got my degree in astronomy and astrophysics.

Carothers: Well, that's an impractical, but interesting branch of physics.

Precisely what my father said. When I graduated the astronomy world was a closed system. The heads of the astronomy departments in these several schools decided, in some dark closet, two or three times in the course of the school year, which of their students they would graduate, and which they would flunk out. What they did was match the graduates with the openings they were going to have the following year. So, when you graduated, the head of your department whispered in your ear, "You should apply for that job over there. You'll have a good chance of getting that one, but don't bother applying for that other one, because you don't have a chance." I think the year I got my Ph.D. there were four of us in the U.S., because that was all the openings there were. But the year I graduated I had two job offers, which was twice as many as you were supposed to have.

It came as a great shock to my father. "Why would anybody pay you to know this stuff, which does not contribute to the growing of any food of which I'm aware?" As you can see, I grew up on the farm. That roots you in a tradition that allows you to see pretty clearly, and detect pretty quickly, city slickers and charlatans of various kinds who always think farmers are, after all, dumb or they wouldn't be farming. Some of the wisest people I've ever met have been sitting out there on the farm. Why and how do they get wise - - not just knowledgeable, but wise? Well, I'll tell you. They sit plowing. You can plow one field for weeks where I grew up, and you do something with your mind during that time. You can't sleep, but you can think. And you have time to sort out a lot of things. I think all farmers are philosophers.

I think I was about five when I asked my father what made the sun shine. He said, "Nobody knows." Well, I was greatly shocked, and I can remember saying, "But Uncle Mason would know." And he said, "No, Uncle Mason doesn't know either, because nobody knows what makes the sun shine." I was in awe that here was something that no one knew.

It turns out that was almost identically the time that Hans Bethe first figured it out. He used to come to Los Alamos regularly every year, and he still does occasionally. Sometimes I would work with him on something, and when I would sit in the room with him I would think, "Here is <u>the man</u>, the very first man in the history of the world who understood what makes the sun shine, and he's right here in this room." As a matter of fact, I still feel that kind of awe.

Because you see, to me that was vastly more important than how much wheat we were going to get that summer. But, of course, I was living on the wheat, so that was important too.

I regarded that question, "What makes the sun shine?" as just awesome. My dad didn't realize that would change the history of the world. I didn't realize it either, but I came along at just the right time. When I got my degree in '55, I took the job that nobody counted on me getting. That was the one at Los Alamos. The other offer was for the vacancy in the Astronomy Department in Nashville, Tennessee. That was the job that had been programmed for me to get.

Carothers: Were these professors aware that Los Alamos was interested in astronomers, or willing to hire them, or was this a surprise?

Brownlee: They were aware of it, and unalterably opposed. It turned out that a colleague also came to Los Alamos, and he and I were ostracized by the astronomical community for some years because we had gone to Los Alamos against all the programming we had. We were slated for these other jobs.

Carothers: What led a nice boy like you to fall in with this bunch in New Mexico? You had an offer for a reputable job in Tennessee.

Brownlee: Yes, but after I had done my thesis work on W Ursa Majoris I had done a solar model, a model of the sun. I had worked it out for one moment in time. Here is a model of what the sun was - - never mind that it's evolving one minute every minute. This is what it was, static. I did that the last year, and I was very intrigued by that. There were a number of questions we couldn't answer; things we just didn't know. Los Alamos was at that time the only place in the world that I knew about where you could get your hands on the center of a star, and have a chance to make observations on it. And one of the things they at Los Alamos wanted to do was to measure the opacity of materials in fireballs.

Now, that was exactly the kind of information I needed for models of the sun, or for stars in general. It seemed a great oddity, even to me - - of course, I was influenced by my father - - that somebody would pay me to do an experiment on a fireball which gave me exactly the information I needed for stars, which were hopelessly out of reach. So, it seemed to me to be a very clever

thing to trick them into paying me to help do experiments in fireballs. I didn't tell them that the real reason I was interested in fireballs was because I wanted to understand something about opacities - - which of course they didn't know anything about - - because I was interested in stars, and wasn't really interested in bombs.

Carothers: And you didn't realize they were tricking you into studying opacities, which they needed to know for calculations about their bombs.

Brownlee: I learned very quickly what they were doing, but that was fine. It was parallel to what I wanted to do. And so, the answer to your question is, they, at Los Alamos, were paying me to do something I could do nowhere else in the world; namely, get my hands on a real, honest-to-goodness stellar center.

And, not only did they pay me, they gave me vast sums of money to do experiments. In 1956 we did the experiment called Lacrosse. It was at Enewetak, forty kilotons, and we had forty lines of sight, trying to measure the opacity of uranium, plus a lot of other things. The opacity of aluminum, for instance, is very relevant to models; there's lots of aluminum in the universe. So, this experiment was forty lines of sight, forty kilotons, forty million dollars. A fellow astronomer and I did that experiment. We were just given the job, and nobody told us how much money we had. It was just, "Do the experiment." When we got all through it had cost forty million, but we didn't know that.

We got the opacity of uranium very nicely, but the number laid around for some years until they finally got to the point where they could use the real number for the opacity of uranium as measured by experiment, and calculate things that had happened to them in the past. We got the numbers, but they weren't used in weapon design for years, because any time they put them in their codes nothing came out right. So, you know the decision - - throw out the truth. I want to say that's the first time I really recognized what charlatans bomb designers were, but it's not true; I had sensed it earlier.



Carter Broyles
SNL - Panel Member

Broyles: I was in the Army from 1942 to 1945. After that I went to the University of Chattanooga, got my BS in 1948, and went from there to Vanderbilt, where I got my Ph.D. in Physics in 1952.

I came to Sandia in 1952, in the Weapons Effects Department. My first work with nuciear effects work was on Upshot-Knothole in '55. From there I did various things, such as being the supervisor of the Nuclear Burst Experiments Division, starting in 1957. I spent some time on high altitude physics work, and managed the High Altitude Physics Department starting in 1967.

When Marshmallow came along in 1962, I was the Scientific Director, and I did that job, although they changed the name to Scientific Advisor, on Midi Mist and Hudson Seal. Also on Cypress

and Camphor, the or Sandia shots. In 1972 I became the Director of Field Engineering. This organization was responsible for conducting Sandia's underground nuclear test program, and was also responsible for the the operation of the Tonapah Test Range. We also supported programs like oil shale retorting, coal gasification, and radioactive waste disposal programs.

I retired from Sandia in 1989, and I now have a position as grandfather, babysitter, and general handyman.



Robert Campbell
Los Alamos — Test Director

I spent World War II as a civilian, and as a commissioned type for the Navy at the Naval Ordnance Laboratory. It started out with mine location schemes, and ended up with a mine testing station on the Bay of Fundy, west of Halifax. It was a nice place for that. It had a 55 foot tide; you'd go along in the aircraft and drop stuff in the water at high tide, and come by a few hours later with your vehicle and drive right up to the things you'd dropped.

After World War II I stayed with NOL for roughly a year, and that year was spent in closing up the station. I was the officer in charge of the place at the end. Some of my friends had already made the jump to Los Alamos. So, I learned of the place, and I made an assumption which turned out to be incorrect. I was chafing, like a lot of us did, with the rules, regulations, customs, traditions, of the United States Navy. And the black shoe, brown shoe type of thing.

The AEC had been formed just a few months before, and I figured that never in my lifetime could anything starting as new as the AEC, and this Laboratory was kind of new, ever get as hidebound, dogmatic, and bureaucratic as the United States Navy.

Well, I was wrong. What I found out rather quickly was that for most of the things that you go through in Naval Regs, something had happened somewhere, maybe years ago, but there was a reason for it being that way. I very quickly found, in this new organization, that they didn't have that history, but they still needed rules, so they made them up. And a lot of times there was no reason for doing it. Somebody just thought, "We'll do it this way." This place was much more awkward than the Navy.

I was married when I came here, and my wife and I arrived in Los Alamos on the 3rd of July, 1947. I guess the decision had been made that this was going to be more or less a permanent place by the time we got here, but the funds hadn't caught up with the decision yet. We had to have a place to live, and all that was available were the wartime four-foot modular - - because that's the way a sheet of plywood comes - - structures of one sort or another. We were assigned to a little house down on Canyon Road. Two little bedrooms, a little living room with an oil stove in the middle of it to heat the place, a little kitchen, a little bathroom - - no tub, just a shower. I think, but I'm not sure, that building was about 32 feet square. It was rather crowded.

I came here without a specific, "that's going to be your job", type of thing. At the time it was awfully hard to get anybody to say they'd come to Los Alamos, so they were taking almost any warm body and making what they could of the people when they arrived. I ended up in a place called R Site. These were people who were doing hydrodynamic testing, as it's called today. I had the fun job of trying to get two metal jets to collide in front of a spectroscope to see what the ionization was.

After a few years at R Site, I don't know whether my feet got itching or I could see there wasn't a hell of a lot of future for me, I jumped. I got into the radiochemistry business for Greenhouse in 1951. Someone, and I think it was dear Edward (Teller), dropped an idea that it would be interesting to know what a fireball looked like as a function of time, from the inside. One of the games that

was thought of was to make a vessel which you would put out there, engulf it in the fireball, and then close it at various and sundry times. So, we were going to get grab samples inside the fireball.

The concept was to take a cylinder, hold that in the flow, and on each end have some sort of valve or gate that would close quickly, and so on. To do that we made ourselves some gate valves that were to operate one time only. They were powder driven, about an inch and a quarter thick, maybe four inches, five inches wide, in a body about ten inches long. The gate went sliding across the opening and jammed into a tapered seat, because they were not to bounce.

The shot was like ten kilotons on a three-hundred foot tower; the collectors were out about fifty feet, so they were engulfed in the fireball. We had some that were through-pipes, set horizontally about six feet above the ground, and we had another variety that was flush mounted. That was a tube closed on one end, with a valve on top, and we took whatever got jammed in. We had five of each kind on the event.

We went in and got the things out very quickly after the shot, mucking about at the bottom of the tower a day or two after the shot. And we did manage to get them out, but there were no samples. They were clean. The part of it we didn't get right was that we didn't get any flow through the damn things. They had sort of a funnel type opening in a teardrop shaped casting, but there was no flow, because it stagnated in the throat of the thing.

Of course, we weren't asked to repeat that experiment. So, I jumped out again. A guy named John C. Clark had more or less watched the criteria, and construction requirements, and every other damn thing for the early phases of Greenhouse. But Jack was pulled out of that when the need for Ranger came along. He was given the problem, essentially, of setting up the Ranger operation.

Ranger was in January 1951, and I was at Enewetak, setting up the rad-chem samplers when Ranger was being conducted in Nevada. So, I missed Ranger, because I was already in the field on Greenhouse. Anyhow, they needed some sucker to start this construction business, gather up the criteria, get it to the A&E, get it back, get it approved, and all that sort of jazz. So I took that over in August 1951, and that's how I got into what became the Test Director business.

Rod Carroll
USGS — Geophysics

Carroll: I have a master's degree in mining and a bachelor's degree in electrical engineering. I have a background in mining, and I worked in geophysics in a private concern in the East. And, I worked in mining in Arizona.

Carothers: So you're really a miner?

Carroll: Well, I got out of that business very rapidly. I took a look around and said, "I'm not a glorified ditch digger." I prefered a little more of what I thought were intellectual challenges. Mining is a sad profession today. One of the country's tragedies today is to travel the old copper belt from Bisbee up through Ajo, all the way north to Montana, in Butte, and see the deterioration of an industry. It's much more devastated than the steel industry in this country.

I thought I needed a broader contact with earth science. I had worked in Mississippi, and I had worked on the Mississippi River, and in the Virgin Islands, and I worked here and there, It was very interesting work for a young man, but I suddenly realized I wasn't getting any intellectual stimulation from the people in the group.

I had a good friend in the Survey who had been a professor of mine in Missouri, and he called me up when I was in the Virgin Islands. He also called me at my home in New York when I came back, and asked if I wished to join the Survey. I said I certainly did. He was in, at that time, what was called the Special Projects Branch. It was the initiation of the Branch. So, I joined the GS in 1961, Labor Day of 1961. I got off the plane in Denver, and there was snow on the ground.

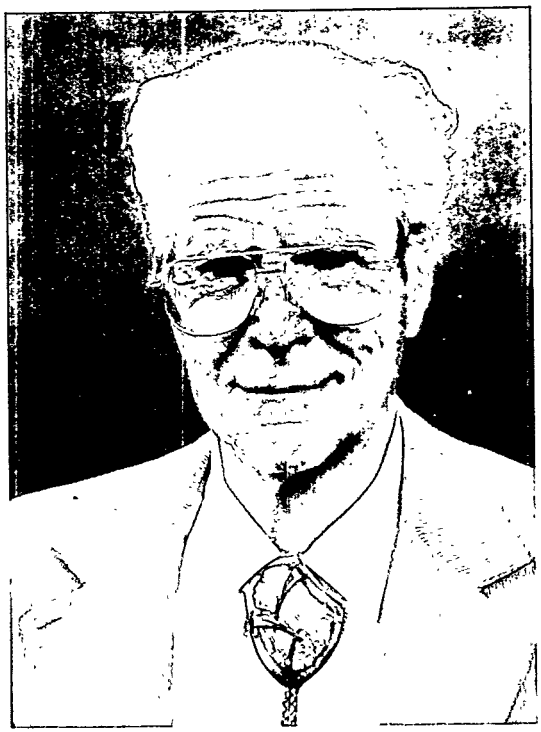


Chuck Dismukes
S-Cubed - - Codes and Calculations

I got my doctorate from UCLA in theoretical nuclear physics. Then I went to work for Ted Taylor at General Atomics on something called the Orion project, which was nuclear space propulsion. The idea in Orion was to expel small nuclear explosions out the back, and use the expanding gases to push a big plate, which was coupled to a spaceship with shock absorbers. It was designed to direct as much of the momentum as possible directly at the ship.

That's how I cut my teeth in learning about calculating radiation coupled hydrodynamics in two dimensions, and got familiar with the codes, which are really the basis for the codes we're still using in the underground test business.

I was working in related areas at General Atomics when S-Cubed was formed as a new company, as a spin-off from General Atomics. Actually, I was one of the founders, although that's probably an exaggeration of my role in the whole thing. I joined them in 1967, about five months after they were officially formed.



Russ Duff
S-Cubed - - Panel Member

I went to the University of Michigan. I was fortunate enoug as things turned out, to have been chosen for the Navy's V-12 program in 1944, and assigned to the University of Michigan as part of an officer training program. My military "training" started at the University of Michigan on July 1st, 1944. I was a V-12 for a year, then I transferred to NROTC, and I graduated in '47 with an undergraduate degree in engineering physics, and that was the extent of my Navy training and military service. In 1947 the Navy was busily demobilizing, and what they did not need most was a green ensign going to the fleet. So, they asked if I would please accept assignment to the reserves. I graciously accepted their offer, and went back to school in September.

I arranged to do a thesis in solid state physics. By this time I had married, and we had one child, with another on the way. There was the small matter of beans for the table. I had the GI Bll, ninety dollars a month, but it was not enough to support a family. There was an opportunity to work in the shock-tube laboratory for Otto LaPorte. He was a German physicist who had been involved in solving the mystery of the iron spectrum - - the LaPorte selection rules. It turned out that not only could I work on shock tubes and get paid for it, but he was also perfectly happy for me to do thesis work there. So, due to a pure accident of economics, I became a hydrodynamicist, sort of, instead of a solid state physicist. Everything seems to come from these minor beginnings.

My thesis subject was the use of real gases in a shock tube. All early shock tube work was basically with air, or an ideal gas. I began to look at the possibility of using gases with different indices of refractions, and specific heat ratios. These things have been investigated much more carefully in the years since, but we had very limited instrumentation at that time. This was the dark ages - - the earth hadn't yet quite cooled.

Carothers: Well, it had cooled, but the dinosaurs had not yet appeared, except in some Departments where they had a few dinosaur-like professors.

Exactly. I finished my thesis in early '51. I applied to three places, and I had three job offers. They were Sandia, in Albuquerque, Armor Research Foundation, in Chicago, and Los Alamos. The Sandia folks paid the most, and Los Alamos paid the least. I went to Los Alamos, because Los Alamos had attracted a large fraction of the graduating class from Michigan for several years. It was an interesting place to go, there were interesting things to do, and I wanted to do them.

For the first five years at Los Alamos I was assigned to the GMX division office, with the interesting title of Research Coordinator. That was a job that had no authority and no responsibility, but it paid, and it was fun. My job was to try to help the various researchers who were scattered throughout the groups of the division, and to suggest things that they might do that would be a little more relevant to the Laboratory mission than what they were doing.

This assignment as Research Coordinator went on for about five years, and I began to suggest the desirability of a little more order in the research activity of the division. In response they suggested I move to GMX 7, and put together a small section doing shock tube and gas detonation research. I did, and so we had a group of six to ten people working there doing truiy fundamental research on shock and detonation physics.

We had all of the support of a major Laboratory, had the freedom to do anything we wanted to do, but the Laboratory really didn't care whether we did it or we didn't. What we were doing was actually irrelevant to the Laboratory's work, but they were willing to support us. I came to realize that if my whole group ceased to exist, fell off the face of the earth, or whatever, nobody in the Laboratory would know or care until the following Friday when the secretary called to ask what to put on the time cards.

In about 1961 I had an opportunity to go to Washington and spend a year on a sabbatical with the Institute for Defense Analysis. I took that opportunity, and was concerned with the early stages of the arming of the South Vietnamese. And also with aspects of the Defender program, which was an ABM system. And also with some problems associated with very large yield explosions. The Russians had recently fired a 60 or 70 megaton device. It was an interesting year.

I came back to Los Alamos in '62 with some hope that there would have been some reconsideration. There hadn't. Johnny Foster, at Livermore, made a pitch to me. Why didn't I come there and set up an equation of state group in the Physics Department, working with Ted Merkle? Johnny can be a very persuasive salesman when he wants to be, and I was sold.

I remember that he made an interesting comment to me. He said, "You know, Los Alamos can beat Livermore at anything it wants to do, anytime it wants to do it. But it never will, because they cannot marshal their resources. They will not put them together, they will not overcome their internal inertia, to do that." That was something that struck a responsive cord in me, because I had been frustrated by the inefficient use of resources at LASL.

So, I came to Livermore, working for Ted Merkle. Ted died shortly thereafter, and my activities were taken up in the Physics Department with a thing called S Division, under Teller. Our job in

S was to look at theoretical and experimental equation of state problems. We did a fair bit of work which was in direct support of the Laboratory, and maintained a pretty active research activity also.

We also did some diagnostic work in the field, and I was impressed, and I said so at the time, by how little the diagnostic people knew about things other than what they were immediately concerned with. They didn't seem to care, and that was always a frustration and an annoyance to me.

Well, after five years I again got the itch. I said, "Look, I came here to do a particular job. That job seems to be going very well. Okay, now what? What's the next challenge?" They said, "Hey, you're doing real well. We really like what you're doing. Keep it up." The same words I had heard at Los Alamos.

Then Mac Walsh, who had been a friend and an associate at Los Alamos, called me from General Atomics and said, "We are setting up a new company. It's called Systems, Science, and Software, and we sure would like you to think about joining us." So, I came down and met with Mac, and Bert Freeman, and a number of other people that I'd known for a number of years, and was intrigued. It turns out I was the first employee of S-Cubed who didn't come from General Atomics.

Paul Fenske
Desert Research Institute - - Panel Member

My family left there when I was four, and moved to Milwaukee, Wisconsin, where I attended grade school. We then moved to Albert, South Dakota, where I attended high school. Then I got drafted. I was eighteen the May before I graduated in 1943, and in a small town like that there were not many guys who were free, because a lot of the young people had farm labor deferments. So, I got out of high school, I knew the draft board was looming over my shoulder, and I didn't know what to do.

I was kind of wandering around not doing anything, and my mother, who was a tough lady said, "Well, you know, the School of Mines starts in two weeks, down in Rapid City. Why don't you go down there for the summer?" And so, the next thing I knew she put me in the car, with my little suitcase in my hand, and drove me to Selby, which was the county seat. That was also on the road which connected Bismark and Pierre — Pierre being the capitol of South Dakota and Bismark being the capitol of North Dakota. There was a bus there, called the Jackrabbit Line, which ran down to Pierre over this washboard gravel road. I got down to Pierre, and I had to wait until two or three in the morning for the Chicago Northwestern train to come through Pierre. I took that to Rapid City, took my little suitcase, walked to the School of Mines, got registered, found a place to stay, and there I was. It was the first time I had been away from home. But you know — if you can't swim, throw you in the water, and you learn how. I registered there as a physics major, and I got drafted out of there into the ASTP program. I first got sent down to Fort Benning, Georgia, which we used to call the Benning School for Boys.

Carothers: I went through the parachute school at Fort Benning, and I sure didn't think that was a school for boys.

Fenske: Well, the ASTP program was different. It was fairly rigorous infantry draining. One of the reasons for it was that we had a bunch of non-coms who weren't going to school, and they thought this was a good time to take it out on all these college guys. We were there about three months, and then the Army abandoned the ASTP program. So, what to do with these guys? Well, they were in the

infantry school, so put them on a train and ship them out to the infantry. And so I was in the infantry in Camp Van Doren, Mississippi, which was the hell-hole of the South.

Carothers: Paul, that's what everybody says about wherever they were.

Fenske: Yeah? Well, that's because they weren't at Camp Van Doren. Camp Van Doren was it. I had some difficulty there where I had to go to the hospital. While I was in the hospital they shipped my outfit, the 63rd Infantry Division, over to Germany. When I got out of the hospital I went to the Corps of Engineers, where I became a construction equipment mechanic. From there they sent me to the Mariannas Islands, where I fixed bulldozers and things like that. When I was growing up in Wisconsin and South Dakota I never realized that you didn't have to be cold in the winter. So, I was on Guam, and Saipan, and then I had enough points to come home in '46.

I went back to school, and ultimately graduated with a degree in Geological Engineering. I got out of school in 1950, and so did everybody else. The job market for engineers was not good; there weren't really any positions available. I had some feelers from some iron mining companies, but I didn't know about the iron mining business. I had some more GI bill left, and I had a brother-in-law who was going to the University of Michigan. So, I went to visit my sister, who was in Ann Arbor. I thought it was a pretty neat place, so I decided to go back to school, to the University of Michigan.

I finished a masters in geology there, and then I was hired by one of the subsidiaries of Mobil Oil Company, the Magnolia Petroleum Company. At the time I was hired the Williston Basin in North Dakota had just had a discovery well drilled, and so they sent me out to do exploration there. I went out and ran around the Badlands of North Dakota for a couple of summers. Magnolia really didn't have any operation there; they drilled a few wildcats, but they didn't have any production up there, and so they sort of bowed out of the thing, and transferred me to Midland, Texas.

There, I just did a lot of well-site geology. I lost track of the number of wells I shepherded down to paying production zones after I got to 150 or so. From there I went to a small independent in 1956, and I worked for them for about three years. Then in

1959 the oil industry was going to pot. You could import Arab oil, and have it for a dollar a barrel, delivered on the dock. To produce oil in West Texas cost us a minimum of two and a half a barrel. It looked to me that the oil industry was going to pot; the Arab oil was too cheap.

And so, I went back to school. I was kind of planning that anyway, and I could see the oil industry was going down, and it was getting to where it wasn't much fun anymore either. I went to the University of Colorado and got a Ph.D. in Geology there, in the summer of '63. Working for that small company I had done fairly well, so I had enough money for three and a half years at the University of Colorado. Of course, at the end of that time I was flat busted.

Then I borrowed some money, and went to Idaho State University, and taught there. So, I was at Idaho State for a couple of years, and it just didn't seem like they were going to do anything for me, in the sense of increasing my pay, which was 6300 dollars a year.

Carothers: Now Paul, academicians by and large, are not highly paid people, but they get the advantages of the collegial atmosphere, the inspiration from the students - - they get all of those things, and all of those things are tax free.

Well, Idaho State wasn't all that great that way either.

So then I went to Hazelton Nuclear Science Corporation in Palo Alto, in 1965, and I got associated with DOE projects. The company was Hazelton, then it became Isotopes, then it became Teledyne Isotopes, and every time it changed names it went further down the drain. But I had been associated with DOE projects, and at that time they had a panel of consultants. George Maxey was on it, and I had gotten pretty well acquainted with Maxey at that time. He kept telling me I should come over to DRI. And, when it seemed that Isotopes was running out of gas, I just went ahead and went over the mountain. George Maxey was on the Panel when I came to work for DRI in the latter part of August, 1971, and shortly after I was attending Panel meetings. I wasn't a member or alternate; I had no official status with the Panel. Two or three months later I became Maxey's alternate. Then, in 1976, Maxey died, and I was made a member of the Panel.



Bill Flangas
REECO - - Mining Superintendent

I was born and raised in this state, in Nevada I went to the Macky School of Mines, in Reno, Nevada, and I'm a graduate mining engineer. So, I've really stuck to Nevada, except when I was in the service. The U. S. Navy doesn't operate in Nevada. My kids asked me, "What did you do in the war?" I said, "I painted." They said, "What did you do when you weren't painting?" and I said, "I thought about painting." i was on a destroyer. it was great duty. In fact, I asked for destroyer duty.

Harry Truman deprived me of my first invasion when he dropped the bomb in August, 1945. My relatively short naval career (1945-1946) was a great learning experience, and I had the honor of participating in the early occupation of Japan in the Fall of 1945.

After the war, and after I got out of school, I worked for Kennecott Copper in an underground copper mine. Then, early in 1958 I got a couple of calls suggesting that there was some work to be done down here at the Nevada Test Site. My name had come up through Mr. Reynolds, who was the owner-manager of Reynolds Electric, who at that time was in New Mexico. He had been hobnobbing in Rotary, or one of those clubs, with the people from one of the Kennecott operations in New Mexico. He mentioned that he was looking for a mining engineer.

It was through that trail I got contacted. I was asked two or three times to come down to the Site and take a look at what was going on. My answer was that I didn't want to get involved in any radioactive work. Then time went on, and a couple of months later I got another call. They said, "Look, without making any commitments, will you come down? We're starting a tunnel, and we just want you to spend a couple of days to help us get started, and you're free to leave." So, I agreed to come down and take a look.

Reynolds was a construction company, and they were trying to dig a tunnel with construction people. Obviously that didn't make sense. I walked into E-tunnel, and they had managed to dig it in two or three hundred feet. I don't how they got there. When I walked in there it was just painfully obvious that they needed miners. So, the question was put to me, "Do you know where there are some miners?" Obviously I did. So, I made a number of calls, and started rounding up some miners, and started putting that force together.

I came down here, to the Test Site, in May of '58 when the Livermore Lab was digging E-tunnel, and they had a little activity going in B-tunnel. It was at the time when they were first considering taking the program underground. In the climate of the times, there was just a great deal of anxiety on the Test Site, even for those of us not connected with the nuclear business, over the confrontation with the Soviets. It just became immediately apparent. And so, I agreed to stay a few days and get that thing started. And, by the time I got it started I got caught up in the excitement, and here I am, thirty-six years later.

From myvery first days I grasped the national significance and felt the dynamics of the NTS. I have had the good fortune to have been a participant and member of this highly skilled and disciplined

cadre of scientific, professional, technical, government, and craft personnel that in my opinion has no equal anywhere in the nation. Although each of was individually focused in his own field of responsibility in a rather complicated organizational structure, objectives were very well met. This mission oriented and schedule driven, "can-do" teams's outstanding successes were significant factors in the outcome of the cold war. I am both grateful and proud to have been involved.



Joe Hearst
LLNL - - Logging

I started out at Reed College, and I chose Reed college because it had a combined program with MIT. My father was a businessman, and he wanted me to take the MIT course in business and engineering administration. With a five year program I could go to a liberal arts school first. Reed was the only liberal arts school that had this arrangement with MIT, where you'd get a degree from each school, that did not have compulsory chapel. I therefore chose Reed. I later learned it was one of the finest liberal arts schools in the country, but that was not a consideration.

Reed was small; there were a thousand people, something like that. Then I went to MIT, took my degree in business and engineering administration, and decided I didn't like it.

From there I took a masters in physics at Boston University. When I finished my masters it turned out I was the best graduate student in their physics department. The other guy wasn't quite as good. They recommended I go on for a Ph.D., which I subsequently did, at Northwestern. And at Northwestern, which had a mediocre physics department, I just barely squeaked through my qualifying exam. I did a thesis in nuclear physics, and my big recollection at Northwestern was, when I finished my Ph.D. there was some sort of party to celebrate my degree. And there was a recorded message from the department chairman, who couldn't be there. He said, "Joe, I want you to remember that a second-rate physicist can be a first-rate anything else." And so here I am.

I interviewed several places, and at that time the requirements for being hired at the Lab were a Ph.D. in physics and vital signs. Be vertical, breathe, have a heartbeat, and that was it. This was just after Sputnik in 1959. And I did learn that the size of the offer I got was inversely proportional to the amount of time I spent interviewing. I went to Oak Ridge for two days, and they didn't give me an offer. I went to Los Alamos for one day, and I forget what happened. I came here for one day or less, and they offered me $800 a month. At Boeing, where I never went at all, they just phoned me and offered me more.

One reason I came here was, I saw this beautiful green valley. I also went to Hanford. At Hanford they gave me a series of slides of the area, and I came back and showed them to my wife. That was the end of that; she said, "No !" I came here in something like December, or maybe in the early spring. It was beautiful and green, and I thought it was that way all the time. Nobody bothered to tell me. So, when we drove out here and all the hills were brown, I couldn't figure out what was going on. I said, "Well, when we get to the Livermore valley it will be beautiful."

I ended up in B Division, and I first worked for a year doing experimental physics, and I really liked that. I was designing ways of doing photography, designing ways of doing pins, things like that.

Then I was put into this bomb design business, and for a while I designed bombs, and I found that pretty boring. Those were the days where you would make a bomb design, more or less by hand, and you would then do some code calculations to see what the result was. Every morning I would go in, and this was before Cal Comp, and hand-plot the results of the calculations. Foster would come in every now and then and look over my shoulder at some of the plots, even though he was an Associate Director then. But my current leader dictated every aspect of what we did; the colors in which we plotted the scales, everything. He was the one person I've ever worked for at the Lab whom I detested working for. He was a little dictator and I had no freedom whatever.

I did write some codes to simplify my job and automate some of the things I was doing. Writing codes was fun, and I enjoyed that. I came in one day, knowing nothing about programming, and went to the guy who was in charge of the programming, and I said, "What's this thing called FORTRAN?" He said, "Take this manual." I took the manual home for the weekend and came back and wrote a program. Nowadays there are courses in this, and I found you could learn it in a weekend.

Carothers: Well, Joe, just remember, a second-rate physicist can be a first-rate anything else.

Hearst: Right. Anyhow, I got unhappy with bomb design, and didn't do very well at it. When the moratorium ended we got into a rush, crash program. Eighteen day turnaround with designs - - from hydro shot to hydro shot was eighteen days. I had to do the calculations, do the ramrodding, make sure the parts were put together correctly, all that sort of thing, and then design the next one. That was okay. I think if I hadn't been working for the guy I was working for I might have enjoyed it, but he was such a tyrant that it was really not fun. And so I helped design another device for awhile, then I went back to doing experimental work.

We were trying to do a series of experiments to look at the face of a pit, as it imploded, and trying to see what was going on, in detail. We were doing very fast photography. This was full time Site 300 work, and I enjoyed trying to make what were very high quality measurements, for the time. That was fun. It was optical, with the fastest shutters you could get. We had our own little bunker, and I also still did my own code work development, which I liked.

I liked doing these experiments, but as you may know, I'm irreverent and like to tease people. And one of the people I liked to tease was the guy who became the division leader. So, when he became B Division Leader, and I insisted on staying in experimental work rather than going back to bomb design, I was asked to leave B Division. I went to K Division, after going the interview route again, doing exactly the same thing that I had been fired from B Division for doing, except now we were trying to develop experimental methods to analyze the effects of shocks on rocks.



Dick Heckman
LLNL — Chemical Engineering

My stepfather was a regular in the Marine Corps, and I'm a Marine brat. There was the war, and my high school time period was during World War II, so we moved around a lot. I think I probably attended some fourteen high schools. I spent a spring semester at Mount Diablo Union High School in 1943, and my first interaction with the Livermore site would have to be in late April or early May in 1943. My stepfather said, "I'm going out to the Air Station. Would you like to take the afternoon off?" Well, any high school kid would, so I came out to Livermore, to the Air Station.

I did my lower division work at San Diego State, with the idea of transferring up to UC Berkeley. I had a very fine chemistry prof at my high school in Santa Barbara. Work with him convinced me I wanted to be a chemical engineer, and I knew Berkeley had a good chem engineering school.

I started in Berkeley was in '48, and graduated in June of 1950. My principal professors in the Chem Engineering Department were Donald Hanson and Ted Vermuelen. I had decided to take a job up at Hanford to work in the 200 process area, the old Purex plant. When Vermuelen discovered I was interested in going into the nuclear energy field, he said, "Gee, we've got some really interesting things up on the Hill." He made me an offer, and so I came up on the Hill, at Berkeley.

I came to work in July, the 5th or 6th, in 1950. I had to laugh looking at my Q clearance number. I suddenly realized I got my clearance before I reached my twenty-first birthday. So, I've had a Q clearance all of my, quote, adult life.

I basically worked under Vermuelen, did my undergraduate work, and research project under him, and then went to work as his chief staff guy on the Lab portion of the old Materials Testing Accelerator, the MTA project, out at Livermore. Standard Oil had been approached about setting up an operating company, California Research and Development, for this big accelerator project. My first assignment was to act as a liaison between the Standard Oil subsidiary guys, California Research and Development people, and the Laboratory.

Then, I had an interesting thing happen. On the annual evaluation, Vermuelen called me in and said, "You've done good work, and should you wish to stay here at the Laboratory, there's no problem. However, being an engineer, your future really lies with this engineering organization. And I've already called up your new bosses and arranged for your interview." This was in July of '51.

So, I quit the Lab, and transferred over to CR&D. I continued to finish up some of the cyclotron irradiation experiments in Berkeley. Then I actually came out to this site, and my office was in what was called Building 13, the old administration building. I went to work for Bill Browning, in the radiation damage area.

I'd gotten married in December of '50, and we moved to Livermore, into a house here, in December of '51. We came out in August to look around, and at that point Livermore was still the original one square mile. The Jensen tract had not been annexed by the city. It was still outside of the city limits, but they were in the process of the annexation. Those houses were more money than we could afford. I mean, they were actually asking ten thousand five

hundred for those houses over there. That was just way too much money. There were none of the flat-top duplexes to rent then, so we had our choice of three houses in town. And so we bought over on north K street, behind the Eagles Hall. Harold Moore was in the process of building one, and when we looked at the house, and agreed to buy it there were just some foundations there. Harold finished that house, and we moved in December. When we moved here there was definitely a lot of open space in the town.

On the MTA project, very early on it became clear that one of the problems in the target area would be radiation damage. And so, Vermuelen had directed my career off towards radiation damage work. We went through a whole series of projects, but by the spring of '53 it became very clear that CR&D was not going to make it. It was just scuttlebutt, but it was very clear. I guess for me, in looking back, the real time was when we realized there was going to be a confrontation between the CR&D group and E. O. Lawrence, about who was really directing things.

Well, of course, there was no question about that in Lawrence's mind. The CR&D president, Fred something — I forget his name — went off to Washington, left on a Monday. He was going off to do battle at the AEC headquarters, and so I called up some of my buddies in Berkeley and said, "Hey, this is going on. What do you guys hear?" They laughed, and said, "It's all settled. E. O. left for Washington on Friday, he came back Sunday, and it's all settled."

Don Hanson was getting involved in a lot of materials stuff over on the Whitney project. I went to talk to him in May, and he said, "Yeah, we've got a place for you, so if you want to come over, fine." Well, I went to talk to my boss in CR&D, and my boss at CR&D told me, "Hey, you're top of the line. The company will fold before you go." So, it was very interesting when he called me in about a month and a half later and said, "I've got some bad news for you. I've got to lay you off." So, I jumped the fence then and came to work for Don Hanson, here at the Lab in September of '53.

Carothers: That must have been very convenient. You didn't have to move. You didn't have to sell your house. You just went in this gate instead of the other gate.

Heckman: It was more than just convenient, because believe me, the guys who couldn't find jobs in the area were stuck with making house payments, in some cases for three or four years, because in a sense there was literally nothing out here, in Livermore.



Gary Higgins
LLNL - - Panel Member

I grew up on a farm.

We had eight people in the eighth grade. There was one teacher. No janitor. We hauled our water from the farm next door in a bucket, and of course the big boys had to do that, and put the wood in the furnace and get it started in the morning. And then I went to a big school, the unified high school. In Hartington there were about four hundred students, and I think there were forty or fifty of them that made it through senior year. Then, since I was only seventeen, and not subject to the draft yet, I started college.

I started at Macalester College in 1944. The war was on and I was not eighteen yet. My dad, who had lost an arm in the first World War, said, "No way are you going to go in until you are old enough. I'm not going to sign". I started out as if I were going to

attend a full year, but when March of '45 came I went around to all the profs and I said, "I'm going to have to leave and go into the service pretty quick." Most of them said, "Okay, your mid-term averages are up, don't worry about taking the final. I'll give you the grade, so you just stick it out until your birthday," which was May 19th, "gets here. If you get your call to go into the service, whenever it is, I'll give you a grade and you won't get an incomplete."

I was discharged in the late summer of '46, early enough to be able to register for school again in the fall. I missed twelve months of school. I graduated in 1949 with majors in chemistry and physics, and a minors in mathematics, German, and English literature, so I was not really anything.

That fall I entered the Department of Chemistry at U. C. Berkeley. I was awarded a PhD in June of '52 after we had discovered elements 99 and 100 in the debris recovered from the Ivy-Mike nuclear test. I went directly to work for California Research and Development, which was a subsidiary of Standard Oil. but I found very quickly I was not suited for work for Standard Oil. I terminated in November 1952, and restarted at the Laboratory, then UCRL, as a radiochemist. I worked on nuclear explosion phenomenology from 1958 until 1983, when I retired from active programmatic work.



Jack House
Containment Project Manager, LANL

My family came to New Mexico when I was nine, and my parents owned a ranch over in the mountains about twenty miles west of Los Alamos from 1946 until 1968. So, I essentially grew up in the neighborhood here, you might say.

I went to the University of New Mexico, in Albuquerque, where I got a bachelors degree in geology with basically a civil engineering minor. UNM had set up a joint program with the Geology and Civil Engineering Departments, and I took that program. The subjects do to some degree fit together.

In the summer of 1966 I started working for Los Alamos, at the Nevada Test Site, out in Jackass Flats, as part of the Rover nuclear rocket engine program. The group I worked with was designated as J-9, and we ran the R MAD building, where we did the

assembly and disassembly, remotely, of the nuclear rocket engines in the Rover program. We lived in Las Vegas, and rode the bus 92 miles each way, each day out to the site.

After about a a year, not liking the bus ride or living in Las Vegas very much, I started seeking opportunities back in Los Alamos. An opportunity became available, and I relocated to Los Alamos in 1967, still with the J-9 group, but doing engineering things back here for the Rover program. Then, in early 1970, Bill Ogle, who was then the J-Division leader, decided to get out of the Rover program support activities entirely. So he disbanded J-9, as we knew it then, and a number of us were sent scurrying looking for other employment.

I didn't get reassigned, I had to go hunt up another job. And so, I went to talk to my old friend Walt Wolff, who was the deputy group leader of J-8, which did timing and firing. He said, "Yeah, I can use you." So, in March of 1970 I went to work for J-8, and became very well acquainted with the Nevada Test Site weapons work, working with the timing and firing folks. I never actually heard anything about containment until that December morning in 1970 when Baneberry vented.



Billy Hudson
LLNL - - Alternate Panel Member

I got the idea that I wanted to be a physicist because I wanted to understand things. Why this, why that? When I was just a little boy I asked these questions. Why? No one seemed to know very many answers. Unfortunately, early in my career I realized that physicists don't know the answers either, but by that time I was too far along to turn back.

I grew up in Kansas, probably thirty or forty miles from where Bob Brownlee grew up. We lived on relatively small farms, moving from one farm to another when I was in high school, until I got into college. But it was pretty much in the same general area around Salina, Kansas, where I was born.

Carothers: Well, Brownlee, as you know, believes in old farmers. He thinks they're the best kind of people you can have on something like the Panel.

Hudson: I think there's a reason for that. On the farm, as a rule, you're too far from the hardware store to run and get a part if something breaks. So, you make sure you have plenty of baling wire and a pair of pliers. It's amazing what you can do with baling wire and pliers.

When you get to be a physicist, I think in many ways you continue doing the same thing. You don't use pliers anymore, and you don't have the same kind of wire, but basically it's solving problems the same way. Maybe that's why Bob likes the idea of a farmer in containment, because many of the problems are of the type which more closely resemble farm problems than big-science problems.

After high school, I went to Bethany College, which is a small Lutheran church school. It turned out that it was less than one mile from where I lived, and so it was the obvious place to go to school. In those days the tuition was relatively low, and I went to college for about the same cost as I went to high school. I graduated from there in what seems to me to be relatively recent times. That was in 1958.

I then went to Kansas State until January 1966. I was basically a mix of teaching assistant and research associate, so I don't think I went to school more than about half time.

My first thesis advisor was Bob McFarland, who worked at the Livermore in the summer time, as part of the precursor to the fusion program. He came back to Kansas State each fall with such glowing reports of how great it was out here at the Lab that I think most of his students came out here. There were six or eight students who were in school at that time, and they all came out here.

So, I went to work for the Lab in 1966, and we moved to Livermore. My clearance came along, and I was then invited to interview many people at the Laboratory, which was a procedure that it's really too bad had to go by the wayside a number of years ago.

Carothers: That was an interesting process. The idea was that you were hired to be a part of the physics staff, and as such you would find an appropriate place in the Laboratory after you got your

clearance and could go talk to people in all the different areas. That's very different from, "We have this job, and do you want this job, and do you fit this job?"

Hudson: Yes. I talked to the people in almost every type of work at the lab, including John Nuckolls, who was a group leader at the time. I was especially interested in what he was doing, and I went back a second time to talk to him, but I couldn't quite swallow the idea of doing experiments on a computer. I was just a little bit too much experimentally inclined. That just didn't seem like physics to me. I've always enjoyed working with my hands. I didn't realize it at the time, but it was always going to be somebody else's hands.

I considered several different places, but I homed in fairly quickly on the Test Program, for a couple of reasons. For one thing, it sounded as though they were doing what I considered bona fide experimental work. It was more similar to what I had done in my own little laboratory as a graduate student. And, at the time, they gave me the feeling that they wanted me more, because I was interested in experimental work, than some of the other areas did. It seemed like a good fit, and I joined L Division. I think in retrospect it was the best choice by a fair amount.



Evan Jenkins
USGS - - Alternate Panel Member

I went to the University of Colorado for my Bachelor's although I came from Nebraska. My grandfather and my great uncle were in the oil business in West Virginia, and we went back there the last time when I was in high school. The geology in the Appalachians is much more visible than it is around Omaha, and I think that's where I got interested. The geology around Omaha, Nebraska, is obscure. There's just a lot of junk there. It raises good corn, but to a rock geologist that geology is junk. So, I came out here to Colorado where, obviously, there's much more geology exposed than even in the Appalachians. That was 1949.

I spent four and a half years at the University of Colorado, then I went into the Army. After that I went to the University of Texas for a master's degree, under Steve Clabaugh. He was, and is, a fantastic man. I graduated in 1959, and then I went to work for an

oil service company in Houston for a year or so. They supplied companies with drilling fluids, and the technology that goes along with it.

Then Dub Swadely, a good friend of mine with whom I did my thesis at the University of Texas, phoned me from Kentucky. He was with the USGS, and he said, "Hey, we're hiring." So, I joined the USGS in Kentucky, on the joint mapping project, doing the whole state. At that time, when we finally finished, it was the most thoroughly geologically mapped state in the country. And I suppose that still holds, because of the money problems that have developed since then.

I spent five years there, and my project chief in Kentucky thought, "Well, you better get around and meet the Survey a little bit." So, I came to the the central region here in Denver, and the Nevada Test Site, and I really haven't gotten around to meet much of the Survey since. So, since 1966 I have put my roots down on the Test Site.



Gerry Johnson
LLL - - Test Director

I grew up in the Northwest, in Washington state,
While attending high school I happened to be one of those troublesome students, but I was a good one. I had no trouble with the courses, and I had time to spare, which I wasted by causing other people problems. But when I was finishing up in high school the superintendent said, "Gerry, what you have to do is go to college. Go right straight through and get a Ph.D. in physics."

My first question was, "What is a Ph.D.?" They didn't teach physics in high school there, but he knew I was interested in scientific subjects. So, he volunteered one year to give me a lab course, as a student of one, in physics. We had a little laboratory, did little simple experiments, but it went very well. That was all the physics I had before leaving high school.

In 1933 I enrolled in the State Normal School in Cheney, Washington, and then entered Pullman as a junior. In those days no

one had any money, especially me. Many of us worked our way through by doing odd jobs, and in my senior year I received a teaching assistantship. I completed my undergraduate work in 1937, and then they gave me a a post-graduate teaching assistantship; a half-time job. I stayed on two years, did a little laboratory research, and received a masters degree in 1939.

From there I went to Berkeley, and it was while I was doing my graduate work the war broke out. I'd been guided by a statistical mechanics and kinetic theory professor, Paul Anderson, at Pullman, to work for Leonard Loeb, which I did. And, if you worked for Leonard Loeb, the story was that as a graduate student you always knocked on his door before entering. As soon as the door opened you were advised to say, "Goddamn the Radiation Laboratory." Then you were permitted to enter. Loeb had no association with the Lab, and in fact, he had developed a lot of resentment between himself and the Lab. It was just a personality problem within the Department.

Loeb was involved with the degaussing of ships, and he was a reserve Commander or Captain, in the Navy. In the beginning none of us took the war seriously. We were all anti-war, and Over the Hill In October, if anybody were to try to draft us. But when France fell, 1940, we suddenly realized that there was going to be a war, and we would be involved.

About that time, the summer of 1940, there were three of us under Loeb, who advised us, "You fellows ought to take commissions in the Volunteer Research Reserve," which was a Navy unit. We allowed as how that might be a good thing, and so we took our correspondence courses in Navy regulations, and ordnance, and gunnery, and they commissioned all three of us. Towards the end of 1940 Professor Loeb went on active duty, so there went my thesis advisor. Soon after he reported, Loeb called me up and asked how soon I could come on active duty.

I was well along on my research and had one prelim to go, an oral, to qualify for a Ph.D., so I replied, "I'd like to take my last oral before coming. I think I could be ready around the first of February. Any time after that I'll be prepared to join you." Well, I passed that oral; I suppose not with distinction, but I did.

At that stage of my life the Navy looked like a great adventure. We had a different feeling at that time, after the war started, but

prior to the war we were no different than any other young people. I put my thesis on the shelf, and went on active duty in late February or early March, 1941. I was assigned to the Naval Proving Ground, which is south of Washington, on the Potomac. At that time it was essentially a test range for experimental and acceptance tests of armor and armor piercing projectiles, and for various other kinds of ordnance, like mines. I became involved in armor and armor penetration, which I continued for five years.

Specialists, like myself, in various technical areas, were sent to various places, and essentially locked up for five years. We missed the war, so to speak. They wouldn't let us enter combat areas. They had the attitude that they shouldn't expose technical people to combat, because of World War I experience. They usually referred to Moseley being killed at Gallipoli.

I thought it was a mistake at the time, and I still think it was a mistake, because we didn't get a feel for the war. We were just there, and problems would come in for us to work on. It's not the same as getting associated with a combat operation and defining the problems yourself. All of us kept trying to get out, at one time or another, to get involved in something else, but they just wouldn't let us. And the work we were doing was fairly pedestrian after we got the experimental facilities and programs set up and going. After the first two years it was nothing but routine. Shoot this bullet at this armor, and make the measurements.

I was in Washington until '46. Then I went back to Berkeley and finished my thesis. I got my degree, and I concluded, "Now what I want to do is get a teaching job and let ivy grow all over me."

So I did that. I heard of a teaching job at Pullman. I got hold of my old friend Anderson, my former professor, and said, "I'm looking around. I want a teaching job." He said, "Can you teach physical metallurgy?" I replied, "Of course." I thought, "I can certainly teach the theory because I've had physics of solids, physical chemistry, and thermodynamics." But what was more to the point, they wanted it to include a laboratory course in which metallographic specimens were prepared. That is an art. I'd never done anything like that. But I didn't tell them that, and I took the job and went to work. I had a tough time polishing and etching specimens, but I finally succeeded in getting some pictures. I felt sorry for those students, but they were patient with me.

I really enjoyed teaching, and the students, and I was learning. But then, after about two and a half years, I realized that here I was teaching these people, or trying to, and I hadn't really done anything in physics. I had no experience, except that little bit of doing a thesis, and reading books and passing prelims - - I had no substantial research experience. And I guess I was a little bored. Pullman is pretty isolated after you've been any place else.

So, I went to the head of the department one day, and I said, "This is not what I want to do. I don't know enough to teach. I want to do some research for a while." And I followed that up at the Brookhaven Laboratory, where I finally got a research assignment in 1949.

Then the Korean war broke out, and I volunteered to go back on active duty again. I went to the Special Weapons Project in Washington, and there I started to work on nuclear weapons. At the end of that, which was two years, I returned to civilian life and joined the Atomic Energy Commission, as a special assistant to Tom Johnson, the Director of Research of the Atomic Energy Commission. There I worked on controlled fusion, using the same propaganda lines we use today. First you show a picture of the rolling waves in the ocean . . . "Think of that as gasoline, give us some money, and we'll have it for you in twenty years." So, we should have had it on line by 1970. We didn't quite make that.

While at the AEC, because I had the necessary weapons clearances, I read the progress reports of Los Alamos and Livermore, which described the nuclear weapons development programs. I thought, "Well, maybe one of those places would be an interesting place to work." I concluded that the Livermore reports were more imaginative. It was just that they were better writers, I guess, but the way it came out to me it looked to be more exciting and more interesting work. So I decided I wanted to go Livermore.

I was told that a man named Herb York, whoever he was, was running the Laboratory, so I wrote him a letter, and said, "Look, I've decided I want to work for you. What do I have to do, to do it?" Not too much later I got a response from him, and an interwiew was arranged. I didn't know what they wanted, or what they wanted to know, but it turned out that they finally hired me. There were about four hundred people at the Lab then, give or take a hundred. Everybody knew everybody.



Carl Keller
Panel Member

I was a reactor physicist by training. I had been working at the Connecticut Advanced Nuclear Engineering Laboratory, on the Snap 50 reactor. They decided to close that down, and Pratt-Whitney was going to make a jet engine expert of me. So I sent out my resume, and I interviewed at Oak Ridge, Argonne, and Los Alamos. And I accepted the lowest offer, which was from Los Alamos. It required that I take a job, not with the people I interviewed at Los Alamos for the full day, but with the people I interviewed for maybe a half an hour before Bob Brownlee had to run off and catch a plane. And I had to change from the reactor physicist business to the containment business as Bob Brownlee's assistant. That was in 1966.

Actually, my first interest was in living in New Mexico. And I had decided that the reactor business was declining. The big companies were taking over most of the reactor research, and the government was doing less and less. I had decided not to try really hard to stay in reactor physics and reactor design, and I took the job for the variety.

The nice thing about the reactor physics background was that I had the nuclear physics I needed. In the reactor business I had done neutron transport calculations, and other radiation transport calculations. So, my background was in radiation transport. I had not done any hydrodynamics calculations before, so the job was initially highly instructive. Actually, in the containment field it has always been that way. I was learning more than I was doing for many years.



Joe Kennedy
Sandia — Tunnel Closure Mechanisms

I came to Sandia in 1963, March of 1963. I had wended my way through graduate school, like everybody else, I guess. I worked for a time with the Lockheed Missile and Space Division in Palo Alto. They paid for my masters degree in physics, at Berkeley. Then I went on to work for a Ph.D. in Physics at Lehigh University. I had never been east of the Mississippi before that. My training there was in solid state physics, but I never practiced solid state physics, except for the first years.

Carothers: Well, Jerry, those tunnels are pretty solid state.

Kennedy: Condensed matter they call it now. That's far more sophisticated than solid state.

I came directly to Sandia out of graduate school in 1963. My wife is a physicist also; we were graduate students together, and she said, "Well, it will be a nice place to stop for a year or so, before we get back to California." And I said, "Right." And so we came here, and we never quite did get away.

I came here, like a good many fresh Ph.D. students, into research. That was frequently kind of an entry place for the new Ph.D. at Sandia I came into a research group which did explosive driven, high pressure physics. So I sat and wrote papers in that for about the first five or six years that I was here.

Then some number of friends of mine kind of jumped over into field test, full scale field test, and it was kind of right upstairs, in the same building, and I got interested in it. And I had gotten tired of writing papers, and wondering if anybody ever read them. Then a friend who had gone to field test said, "We actually do stuff." That appealed to me a lot, so I went there, and stayed there the rest of my career here. The very first event I worked on was called Diesel Train. It was a DNA event, and that was my introduction to tunnel events.



Tom Kunkle
Los Alamos - - Panel Member

I was an undergraduate at the University of Arizona, and I attended graduate school at the University of Hawaii. I chose Hawaii because I was an astronomy major, and at the time the Mauna Kea observatory was being built, and it had more square inches of glass on the summit of the mountain than you could find anywhere else.

Carothers: It seems a bit strange. There have been several people at Los Alamos in the containment business who originally were astonomers or astrophyicists. Here are people who've been looking out at the infinite heavens, and now they're looking down in the ground.

Well, there are some elements in common. In both cases you have to deal remotely with your subject. There's very little opportunity to learn directly the effects or the nature of what you're dealing with. In one case, the underground nuclear tests are inaccessible because of their extreme depths of burial; in the other, the stars and other astronomical objects are inaccessible because of their distance. So both use remote sensing.

In Hawaii I studied galaxies — the structure of galaxies, and especially the material between the stars, the obscuring dust and gas. My specialty is dust between the stars.

Carothers: Well, there you are. Now I see the connection with the Test Site. There is a lot of dust there. When did you finish your degree?

Well, I have two Ph.D.'s. I finished one in 1978 and one in 1979. They're two very different fields. I became something else, as it were, nearly out of necessity. Having arrived in Hawaii to go to graduate school in the fall of 1973, I discovered that there had been an election the preceding year. The only precinct in the state that voted against the incumbent governor, Mr. Burns, was the university precinct. The university budget had suffered mightily since that 1972 election, and there was no money for us, the graduate students.

That left six of us, myself and five others, who had graduated to go into astronomy, looking for employment to keep body and soul together. I started doing statistics for a group over in the College of Medicine. That group was interested in bubble nucleation in supersaturated liquids and fluids. They were motivated by an interest in diving medicine — the decompression problems which are believed to be caused by the formation of bubbles in the tissues, in the fluids of the body.

They were doing some very interesting lab experiments, but they hadn't the least idea how to analyze them, or write up the results. I had a fairly good idea how you might go about analyzing and writing up the results, and I found I could learn how to handle the glassware almost as good as the other medical students. And so, within a year or two I was spending a lot of time doing that. It became a regular hobby for me. That just progressed for a while, and by and by I finished with a Ph.D. in diving medicine, or medical physics as it's really known.

I still needed a job, and that was a problem. Many of us - - many of us being the graduate students of the university - - were discussing at the time about what will we be, now that we're grown-up. There were, it seemed, two opportunities; one for university research, and one for employment in the government, or government-sponsored functions and laboratories.

Very few universities seemed to offer actual jobs. The academic posts were transient, short term, not very well paid, and without benefits. I considered a position at Washington University in St. Louis, which would have been involved Fabre Perot spectroscopy of various stellar objects. That would have been very interesting, and probably could have been slowly developed into a more secure faculty-type position, but it was short term. It would have been up to me to try to develop it into something, and, gee, it would have paid much less than the auto workers in the same city. So, it didn't seem like too good an employment opportunity.

I also discussed a science research fellowship at the Science Research Consulate in Great Britain - - Edinborough, in this case. I very seriously considered taking that position, which would have offered me halftime a year in Hawaii at the National Infrared Telescope - - the British Infrared Telescope, as it is known over here — and then the other halftime in Edinborough, reducing the data. That would have been quite an acceptable position. I very seriously considered that.

But, I had replied to an ad which Eric Jones, who was then the J-9 group leader, had run in Science magazine. He was looking for someone to work in weapons effects at Los Alamos. I replied to Eric's ad, and he had me come out and talk to him, and I liked the position quite a bit. It involved a lot of theory and computations, and statistical analysis of data bases. It was an interesting subject to me, and the group was staffed largely with people I could get along with quite well. There were physicists, astronomers, geologists, and people I had already grown to know somewhat at the University of Hawaii. And so I elected to take that position.

I interviewed here in August of 1979, and accepted the position the following month. I showed up on April 13, 1980, I believe it was, for employment.



Joe Lacomb
DNA - - Panel Member

My family was in construction, so we moved a lot. I went to high school numerous places — Mesa, Arizona; Gold Hill, Oregon; Boulder, Montana, and a number of places in northern New York. I graduated from West Seattle.

After that I went to the School of Mines, at the University of Montana. I was married, I had two children, and I was number one on the draft list in Jefferson county. They called me up and said, "Would you like to sign up for your ROTC deferment?" I said, "That sounds like a reasonably good idea." So then I was stuck doing ROTC until I got a commission. I got out of school in '55, as a mining engineer.

Then I was in the Air Force, stationed at Alstrom, in Great Falls, Montana, as a KC97 pilot, doing air refueling. When I got out of the Air Force, I spent three years in business for myself, operating a silver mine. We did pretty good for a while, but the problem was that in the winter time the snow is fairly deep, and getting from town to the mine at the Continental Divide was interesting at times. You only get about six months of productive time per year. And, you starve the rest of the year.

After that I went to work for what was then called Porter, Urqhart, McQuery, and O'Brien. Porter, Urqhart, and McQuery are all renowned civil engineers. O'Brien was a young partner. They had the contract for doing the site exploration for the Minuteman. I started with them up around Great Falls, and we did two locations in North Dakota, then went to Missouri, and Lubbock, Texas.

Finding the sites is like trying to site one of our tests, to a degree. You have certain criteria. It can only be so close to a school. Believe it or not, you can only be so close to a cemetery. And you can only be so close to a town. And there is certain topography you would prefer to have. You would like to try to get in on a good blacktop road, if possible. You try to take all that into account. And you could only have the sites within five miles of each other. So, you tried find a place to cluster eleven sites - - ten silos and one living quarters. First, you did a map study, and tried to locate these sites in an area on the map, then you went out and drove around and relocated them to fit what you found in the field.

Land use was another thing you tried to pay attention to. You didn't want to pick a site in the middle of some guy's million dollar orchard. You tried to pick fields. In North Dakota, most of the time we were siting in the center of wheat fields. In Oklahoma we were in cotton fields all the time.

After they were sited we went back and drilled to a hundred and thirty feet. We took undisturbed samples every ten feet, and took penetration samples every alternating ten feet, so we had something every five feet. We provided that to the designers of the silos for their structural design. Every site was drilled; any site that made water had a pump test run on it. It was interesting work. That's where I first got involved in soils and foundation work.

In Montana a lot of the silos were semi-dug. They were bucket augured because most of that was soil. When they got to where they had rock, they were mined. They were excavated by drill blasting and typical shaft sinking methods. North Dakota was mostly glacial fill with big chunks of shale. I mean big boulders - - I couldn't believe their size. Some of them had fifty to seventy-five foot dimensions. Of course you can't see them. You just know that boulder was there because of the drill pattern you'd put in. Glaciers are pretty big, and they move big rocks.

My wife had moved from Montana to Vegas because my parents were here. When I got through my last job with the sites, I was sick of it. I was working seven days a week, twelve hours a day, and that gets old after a while. So I just said, "I'm going home," and I came to Vegas. I was here a week, and they wanted me to go down to Vandenburg to drill some holes down there. I went down there for two weeks, which lasted ten, and came back here.

The Nevada Testing Labs advertised for a soils engineer. I went down and applied, got the job, and started working for them. I was with them for two years.

From that I went up to Reno and managed a lab in Reno for about a year and a half. Then they were changing hands, and I decided to get out. So I was leaving, and I went around and talked to my clients. I said, "I'm going going to be leaving, and this is where I'm going to be. If there's anything that I've left undone, pick up the phone and call me." I was really proud; I got fourteen job offers in one day.

I got offered a job to be a project engineer on the remodeling of Harrah's Club up in Reno, and I took that. My goal was to become a project manager for big construction jobs like the Mirage that's being built here - - places I could work for two or three years on a big program, and then maybe go goof off for a year.

Then I got a call from Ken O'Brien saying he had the contract with what was then DASA, in Albuquerque, and they needed a mining engineer. I thought, "You know, as long as I've been out of college, I've never worked as a mining engineer. I've worked in a mine for myself, chased drill rigs, done a lot of other things, but I've never been a mining engineer." So I said, "I'll take it." I went to Albuquerque, and got there in September of '65. There was the

contract, but they didn't know what they wanted us to do. I used to go berserk - - I'd go down the hall, door to door, trying to find work, something to do, something to get involved in.

Then they needed a test group engineer for an event called Double Play, but Jack Noyer had said he'd never have a &@**%! contractor as a test group engineer. Then he changed his mind, and said, "Well, have him go do it." So I came out to the Test Site in mid-December '65, as test group engineer on a tunnel test in Area 16.

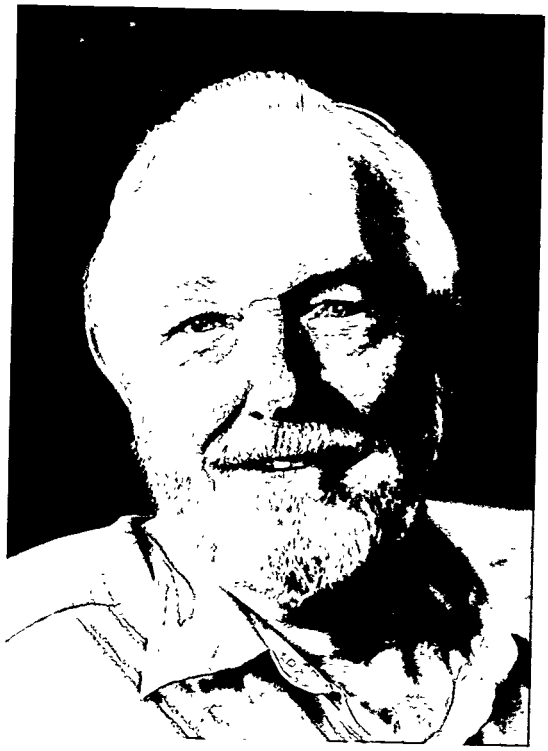


Roy Miller
LLNL - - Drilling Superintendent

I have a BS in petroleum engineering, so I guess I'm a petroleum engineer. There's several different fields of petroleum engineering, and I happen to, for the most part, be interested in the drilling phase of it.

I worked for El Paso Natural Gas Company, in Farmington, New Mexico, when I got out of college. For a short period of time I worked in the Division Office in Salt Lake City, in a pipeline department dealing with gasoline plants, and compressor stations, and pipelines, and that sort of stuff. I worked for them for eight years before I came to the Test Site. I couldn't wait to get back to the drilling fields. So, in 1965 I went to work for Fenix and Sisson. I worked for them until August, 1966, a very short period of time, and then I went to work for the Lab.

It was surprising to me how much the hole drilling on the Test Site was adapted from the oil fields. The holes just got bigger is all; same equipment, same people.



Cliff Olsen
LLNL - - Panel Member

I went to high school in Sacramento. I'm a native Californian, born in Placerville. The family wandered around Northern California. During the war we lived in Berkeley. In 1945 we moved to Sacramento, and I stayed there. I went to high school in Sacramento. UC Davis was just down the street, and so I ended up getting both my bachelors and Ph.D. at Davis.

My degree is from the University of California, at Davis, and I'm a physical chemist. I worked for Charlie Nash, who is still there as one of the gray-haired types now. I was his first Ph.D. student, or his first Ph.D. student who got a Ph.D.. He had just gotten out of UCLA, and he had done work with Bill McMillan. I did my work on exploding wires. You might ask, "What does that have to do with

chemistry?" All I can say is that a lot of people wondered that. From that work I got a fair background in what, at that time, was high-speed electronic diagnostic techniques.

I got aimed here originally because Charlie Nash had a consulting contract here, looking at exploding wires, and high speed switching, and thing like that. The obvious connection is that such things have something to do with detonators, and so forth. And so, he had a little bit of money, and lo and behold, starting about 1958, Livermore funded my graduate research. They gave us a nice high speed capacitor bank, and some very nice oscilloscopes, which would now be considered something for the Smithsonian.

So, it seemed logical to come down here and look around, and they said, "Why don't you apply for a job?" So I did, and they took me. I came to Livermore in 1961, and in only a couple of months got my Q-clearance. These days that's absolutely amazing.

I ended up in N Division for a couple of years, before N Division folded up. I worked for a while on samples of fissionable materials and other things that we put in the Kukla and Fran reactors, which were prompt burst reactors. One of the primary things we were looking at was vulnerability, at that time.

With Kukla, which was a bare sphere, you could just put little things in it. Fran was a little bigger, and was cylindrical, with a cylindrical opening where you could put in a two dimensional sample. The 2-D samples were a little more of challenge for the calculators. We would instrument those, stuff them into the reactor, and expose them to a radiation burst, which was primarily neutrons.

Then, in '64, when N Division started to go the way of the dodo bird I left, and a guy named Jim Carothers offered me a job in L-Division. And, I took it. I started off as a reaction history physicist on Club, and on Fade and Links I did the reaction history. Then I moved on to project physicist, starting with Plaid, which was a line-of-sight shot, but by the time Plaid was finally fired I was no longer the project physicist - - I was in containment by the time it leaked.

Paul Orkild
USGS - - Panel Member

I guess the way I got interested in geology was that I just happened to be looking at rocks one day when I was a wee one and decided that was something I'd like to do. And, later I decided it was a lot better than working on construction, pouring rocks into forms. I figured it was better to pick up the rocks and describe them.

I went to school at the University of Illinois, from 1946 to 1952. I was one of the lucky ones who went through ROTC officers school. But, after they ruined my hearing with a bazooka they decided they didn't need me. One of the classical demonstrations for young officers was to show how a bazooka worked, in the classroom. The sergeant demonstrating the bazooka held it up and said, "This is how you fire it." It went off, and it went out right through the wall. Luckily, it missed everybody. But now I wear hearing aids in both ears, and the whole class of 36 people were hard of hearing after that, I think. It was very interesting, but I decided right then and there that was not the place for me. It made for a short career.

I stayed in school and finally graduated. After doing graduate work in '52, I finally got very hungry, and the USGS had a very lucrative offer, so I went to work. I joined the USGS to work in Alaska, but I never saw Alaska. I ended up working on the Colorado plateau looking for uranium. Those were the days when they thought all the uranium was in the Belgian Congo and up in Canada, and the US didn't have any.

There was an award program for prospectors. There wasn't anything like that for us, even though they used our maps. One of our jobs was to produce photo-geologic maps of the Colorado plateau, which we were doing. The Survey didn't make any money selling those, but the blueprint companies that sold them made fortunes, literally. And the guys who bought the maps and found uranium, they made fortunes. They bought the maps for seventy-five cents. It cost us probably ten thousand dollars to make them.

At that time we were working in what we called the photo-geology section, in Washington, from 1952 to 1956. Photo-geology is where you analyze aerial pictures that were taken of various areas, and make geologic maps based on looking at them, and inspecting them with stereoscopes, and so on. You infer the kind of rocks there are by looking at a picture, the various tones and colors. And being very clever, of course.

We used colored photographs, which were very primitive at that time, but they were useful, and black and white photos. Then we would go out into the field, and field check what we were looking at so we'd have a data base to work from in identifying the various rock units. It was a very interesting approach. Many of the old time field geologists thought it was heresy that we could look at a photo and make a geologic map.

Anyway, in 1958 I got involved the mapping of the Test Site, where they wanted to do the west part, using photo-geology mapping. Then they formed the Special Project Branch for Test Site work, and it's still here today.



Jim Page
LLNL - - Test Director

My first exposure to the Lab was as a summer employee, back in the summer of 1961. I came into Mechanical Engineering and spent three months working on projects in the high pressure laboratory. Then I went back to school and finished up my Masters degree in 1962, at Cornell.

After that I came back into Mechanical Engineering, in what was then Device Division, and went to work on some of the very early stuff that was being done in weapons control. I spent a couple of years working there, and then I went back into device work, and did about a year and a half of auxiliary systems work. Then the Department decided to form an engineering division that would pull all the test work together, into something called NTED — the Nuclear Test Engineering Division. I joined that division the day it was formed, and was in the containment group under Palmer House.

I left that engineering group in 1972, when I took a one year assignment at Oak Ridge, in Y-12, in their engineering organization back there. I did a number of things there. I worked in their special orders group, which was the group that deals with customers like the Laboratory. I worked in their engineering organization for a while. It's an facilities type engineering group that worries about the type of equipment, and where they put it, and how it operates. I got a good look at how the whole outfit works. There must have been a half dozen people from here who went there on an assignment like that, and a half dozen people from the other parts of the complex who came here. I found it to be a very interesting year.

When I came back I spent about seven years doing device engineering for events. Then I got involved in the W-79 as the the project engineer. It was in Phase 4, so it was mostly a production engineering job. From the W-79 work I went back to NTED as the deputy division leader, and I spent about eight years doing that, which, of course, had a heavy focus on the engineering that was done for the Test Program.

I left that job and went over to the Test Program, working in the field operations activities, doing planning and some of the management of elements of the program. From that assignment it just sort of transitioned into a Test Director assignment.



Dan Patch
Pacifica Technology - - Codes, Calculations

I got a bachelor's and master's degree in mechanical engineering at the University of Minnesota. I started in '61, and got done with that in '67. Nobody told me you got a master's degree automatically if you went through a Ph.D. program. U of M was an old timey school, and they had a five year engineering program. I got into a fast track program that said we could get out in four years if we would be good scouts and promise to stick around for a couple of more, and that's kind of what I did.

Then I came to California to go to school at the University of California, San Diego in the AMES Department, which was Applied Mechanics and Engineering Sciences. It had originally been the Aerospace Department, but the aerospace industry went kaphooy in about the middle sixties, so they kept the same letters, but changed the name of the department.

I came to San Diego because I wanted to get out of the snow, and because my advisor said that there was a new engineering school out here; they hadn't graduated a complete class yet when I came out. I think they had been in operation about three years. It was hard to tell what kind of a reputation they had, but the UC system had a good reputation, and they had some very fine faculty members. They had recruited good faculty, so I thought, "What the heck. I'd really like to see what the West Coast looks like, and give this a try."

It took a long time, but I got a Ph.D. in Engineering Physics. That seemed to be a broad enough title to cover all sins. It took five years, plus I stayed on a little longer as a post-doc because my advisor took his sabbatical, and he needed somebody to keep track of his grad students. So I stuck around for an extra nine months.

I knew, through a number of strange connections, some of the people who worked at Science, Systems, and Software. I had known some of these people for several years. It seemed like a nice bunch of people, and an interesting place to work. I thought it would be really nice if I could get into S-Cubed, but I sent resumes out all over, to General Atomics, the Navy, and out of town to various places. Interestingly enough, one of the places I sent my application to was SAIC, at the time. The two places were very comparable. They spun off from GA at about the same time, and they were both about the same size, but because I knew the S-Cubed people, and I had kind of an inkling of what the corporate culture was like I thought it would be nice if I was offered a job there. Well, I was.

I would guess that S-Cubed was about a hundred and fifty people at that time. I interviewed Chuck Dismukes, and Jerry Kent. Jerry was the late-time containment guy, and Chuck was what Chuck was, and still is, of course. Jerry offered me a job, and I didn't quite know what I was getting into, but it sure sounded like what I was looking for. I've never really looked back from there, in a way. I worked for Jerry for two years, and then Jerry left S-Cubed, with a couple of other people - - Bob Bjork and Mike Giddings, and a little later Bob Allen. Those four guys left and formed Pacifica Technology as a little bitty company. After they had thrashed around for a year or so they were in need of some help, because they were doing pretty well. Jerry had continued on with part of the containment work, part of it. We really in some sense split it with S-Cubed at the time.



Ed Peterson
S-Cubed - - Panel Member

I have a bachelor's and master's degree in Mechanical Engineering from the University of Washington. I worked at Boeing for a while after I had a bachelor's degree, mostly on airframes. After I received a master's degree I worked for Ford Aerospace in Newport Beach, not a long time but a few years, on rocket engines and things like that. I interacted with numbers of peopie who had doctor's degrees, and my personal view was that a Ph.D. was sort of a union card that let you do some of the more interesting work that you get locked out of if you don't have one. They don't pick people to do work because they're smart, and good. The Ph.D. is sort of a union card, and that's the basic reason I went back to schooi. It's sort of the circumstances of iife. It was probably worth it. Who knows, but it was interesting.

So, I have a Ph.D. in Engineering from UC Berkeley. I received that in 1968. Following that I taught at the University of Minnesota for four years. In the sixties there weren't enough Ph.D.s to go around, but by 1970 or '71 the market was glutted. For example, at the University of Minnesota we had lost maybe half our students, and there were a half dozen assistant professors. It didn't take too much foresight to see the writing on the wall.

I had worked in Newport Beach, which is sixty miles up the road from here. Now, nothing against Minnesota - - it's very nice, the people are very nice, and all that, but it is not nearly as warm, and they aren't near nice beaches. So, I was looking around for some place between the Mexican border and Newport Beach, and missed it by five miles.

A fellow named Gary Schneyer, whom I had gone through graduate school with, had by pure chance found S-Cubed. I happened to talk to him, came here and interviewed. My bachelor's and master's degrees are in Mechanical Engineering, but the Ph.D. is in Engineering. In going through Berkeley in the department I did, one takes a major, which for me was fluid mechanics, and two minors. Mine were physics and mathematics, so it wasn't really disassociated from the type of things they do here. They made an offer, and I decided to go to work here. The company was very small at that time. So, I came here in 1972. And the principal reason was because it was San Diego. It may not be a good reason, but that was the reason I did. The person who really hired me was Chuck Dismukes, and the people here were interested in front ends at the time, and plasma flow in the pipes. It was really a fluid mechanics type problem that they were most interested in.

There was another person here, who didn't hire me, that I ended up working with some in aerodynamics. He was doing truck aerodynamics and things like that, and I had done some work in aerodynamics. If you look at trucks today, you will see these new aerodynamic trucks. The one that's put out by Kenworth now is almost identical to one that we designed for Freightliner about ten years ago. The new trucks have the whole front end, including the fenders, the cab top, and everything designed as a complete aerodynamic unit. In the very new ones the aerodynamics goes all the way down to the bumpers, and along the sides. I ended up doing a reasonable amount of work on that. All engineering problems

from many standpoints are the same. They're all a little different, but they all have a lot of similarities. The work on the trucks was very technical, and a lot of fun.

A lot of the people that are in containment really only work in one area, but there are others of us that have done other things.



John Rambo
LLNL - - Codes, Calulations

I graduated from the University of Portland in June of 1963, and a slight depression was going on at that time. I had been looking for a job for about six months when some interviewers from the Nevada Test Site came to Portland. So I went down, and they were looking for some technical people. I said, "I'm a physicist, but I certainly would be willing to do most anything. I really would like a job, and I'm interested in working for the Laboratory." They said they were looking for a physicist, they just hadn't advertized in the newspaper. I continued to write them letters that I was still interested, and at the same time I was also possibly going to hire on at Bremerton, with the Naval shipyard.

It was rather odd. I had an interview at Bremerton that was really quite extensive. I was really put to the carpet, technically, and there were a great deal of questions from the Navy people. I

really felt uptight during the whole interview. About that time I got to go down to the Nevada Test Site, for an interview down there. They showed me around the Test Site, took me up to CP-1, and as we were driving back one of the physicists, Bill King, the head of Health and Safety, said, "You know all about radiation and that sort of thing?" I said, "Yes," and that was about the extent of the interview.

I proceeded to be very interested in joining the Laboratory in Nevada, and I was hired on by John Ellis, who was then in charge of a small group developing, as a group, how to measure slifer yields for the nuclear test program. I came to work in November of 1963. I lived in Las Vegas, and worked at the Test Site for five years.

I came in as the physicist who would analyze the slifer data, and then proceed to write reports telling people how the devices went in terms of yield. Things were quite different during those days. Some of my first visits out in the field involved looking at how the engineering construction people, Joe Snyder and Dick Hunter, sat in a small trailer and directed the entire operation from that trailer. We were shooting a shot every week or so at that time. That's something that I doubt we could do today. It was rather phenomenal to see how they would get all this activity going just from that one trailer. People would show up, and they would tell them where they were to go. They were on the net a lot of the time, and it was just that very small operation that was doing the whole thing.



Norton Rimer
S-Cubed - - Codes, Calculation

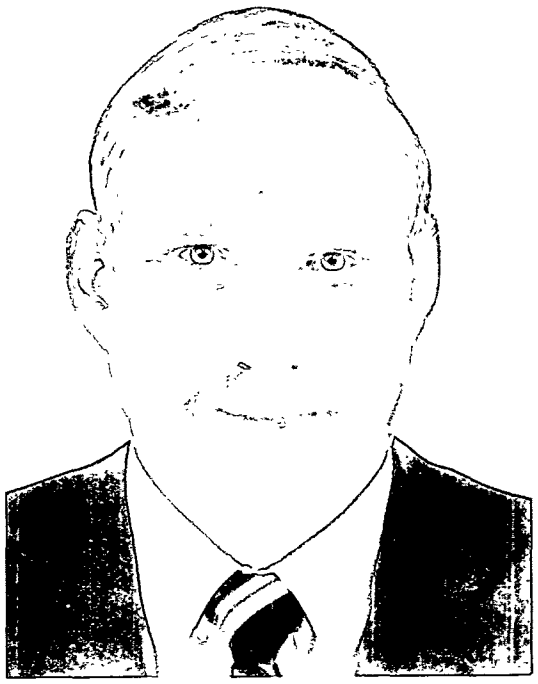
I got my undergraduate, masters, and Ph.D. degrees at City College of New York. I started as a civil engineer, then obtained a masters in hydraulics, and a Ph.D. in plasma physics. From hydraulics to plasma physics was a real switch. Most of the people were doing experimental theses. I was more interested in the computational aspects, coming from fluid mechanics, where I was doing computational fluid mechanics. That change to plasma physics meant taking a lot of new courses, a lot of physics department courses that I hadn't taken.

I finally turned in the document for my degree in 1972, and I came here, to S-Cubed in 1973. Actually, I had been teaching at the University since 1967. I was in no hurry to get out of there, because I was interested in teaching. I loved college teaching, but it was recession time. I must have applied to 200 universities,

including every one in Hawaii and Florida - - I'm a beach person. I think I got about ten or fifteen "no" responses, and two interviews, one of which accepted me. That was a junior college, and I wasn't very interested in that.

When I got here Jerry Kent had just taken over Russ Duff's late-time containment contract. He needed help out here, so he called me up, and I came out for an interview, and they hired me. Partially it was to work for him, and partially to work on plasma physics. I spent about five years writing some of the plasma physics codes that they used then. I was working part-time on containment in those days. Nine months after I was hired Jerry left and formed Pac Tech. Four or five months before that he asked me to go with him, but I like this company. I felt I had a lot to learn from the people here, and I decided to stay.

So, I've been here since September '73. But I'm leaving S-Cubed as an employee right after I come back from vacation. I'm retiring, but I'll be a consultant; I have a half-time commitment. So, I guess I won't get my twenty year watch. I'll stick to the business at least as long as the people I can work with stay around. If someone strange comes in that's difficult to deal with, I probably will just cut out completely.



Byron Ristvet
DNA - - Panel Member

Undergraduate school was at the University of Puget Sound, where I got a Bacheior of Science in Geology, with minors in chemistry, physics, and aerospace studies.

I always liked rocks, and I had an aunt and uncle who were avid gemologists. They got me interested in it. And I've always been an outdoorsy person. I used to like to go out camping, roughing it, and all that. I still do it occasionally, but I've gotten to where a motel is roughing it. In 1969 and 1970 I was a geologist with the Keivel Mining Group, which is Canada's largest Canadian-owned mining firm. I was an exploration geologist the first summer, and an exploration geology manager the next summer. I guess working a couple of summers in remote northern Canada kind of gets you out of the camping experience.

They were long summers and we made good money. There I was in Canada, with a permanent work visa, which I still own, and I still have a Canadian social security card. The Vietnam war was raging, and it was hard to come back. I originally wanted to go to the University of Calgary, since I was on an educational delay from active duty in the Air Force. They were all worried, and said, "You can't go to Canada. You might not come back." Nobody knew I already had a permanent work visa.

Then I went to Northwestern University for graduate school and received a Ph.D. in geology, with the emphasis on low-temperature aqueous geochemistry. I left Northwestern in 1973. I was prematurely called to active duty by the Air Force, so I did not have my thesis even started, as far as the writing. In fact, until the day I left to drive to Kirtland via my home in Tacoma, I was doing lab work. That was about an eight month premature extraction from the University. I went to Kirtland to what was then the Air Force Weapons Lab. I was originally to go there to do environmental chemistry work, which was waste water problems. I got to Kirtland, and in a few days time, three days exactly after I in-processed, I was on a plane to Enewetak, where I got involved with trying to understand the Pacific nuclear craters.

Off and on, that took until 1985 to finally resolve, with many, many trips and about seven hundred days out there. I think the longest trip I took was nine to nine and a half weeks. I think we did a very good job out there, in understanding that these craters really were small. It was all these late-time liquifaction related processes that made them become so large and shallow.

My first visit to the Test Site was in 1974, where I assisted in emptying ejecta collection pans on the pre-Mine Throw event, which was a hundred and twenty ton nitro-methane shot out on Yucca Lake. It was a cratering shot, and the ejecta collection pans were to collect whatever came down where they were.

I was with the Air Force Weapons Lab at that time. I really got in on the original Enewetak project when it was a DNA funded project to look at all the explorations of the craters. I was also involved with DNA on the Minuteman upgrade program, and the silo upgrade program, and a number of other programs. We were working very closely with the shock physics folks, and to some extent the test folks. The characterization of the islands started in

1977, when I was still on active duty, and I was involved as a technical advisor there. It was in 1977 that I decided I really didn't want to stay in the Air Force on active duty, but I continued on as a reservist, even until today.

I was looking then for a job, and originally I had planned to go to an oil company, a research and development organization. I had completed my Ph.D. while on active duty. I was seriously looking at joining the Chevron Research Corporation, but a few things changed my mind right at the last minute. They had to do a little bit with salary and the cost of living in southern California, and the fact that my wife was pregnant, and she had a good job in Albuquerque, and her family is in Albuquerque, and there was this geophysicist job open over at DNA.

So, then I was a civilian employee at DNA. I continued on as a reservist at the Weapons Lab, primarily doing environmental impact analysis, which I still do today. I started in October 1977, and I worked at DNA as the geologist-geophysicist for six years. Then I left DNA to go to S-Cubed, and the purpose for that was so I could be the technical director of the Pacific Enewetak Atoll Crater Exploration. That was finally the realization of what we had wanted to do, which was to drill the craters, which we did very, very successfully.

It was funny. If I wanted to do that, even though it was a DNA sponsored program, I had to leave DNA because my duties in the underground test program would have prohibited me from devoting full time to a program that was very near and dear to my heart at the time. And so I went to S-Cubed with the intention of probably coming back to DNA as a government employee. I was at S-Cubed a little over five years, and then I returned to DNA in 1988, as the chief of the containment technical division.



Bernie Roth
LLNL — Test Director

I'm a mechanical engineer. I graduated from San Diego State College in 1959, and stayed in that general area for five or six years. I came out of the aerospace industry, where I had spent eight years at several different jobs for several different aerospace companies. I had three jobs in San Diego, and then one in Connecticut. I worked on the Atlas program in San Diego for General Dynamics, in the astronautics division. I had two different jobs there. I also had a job at Ryan Aeronautical for a short period of time in San Diego. That was in anticipation of a contract that never developed. The custom of the time, and maybe is still, is that there is feast and famine. You're hired and fired at will in the aerospace industry. Then my last aerospace job was with United Aircraft in the Hamilton Standard Division in Windsor Locks, Connecticut.

After those seven or eight years in aerospace I decided that it was too transient a life, and I wanted to look for something a little more secure. The other part of that story was that I didn't want to live on the East Coast. I had been there for a couple of years, and decided that I'd like to go back to the West Coast.

And so, how did I get here? One September or October weekend there was an advertisement in the local paper that the Livermore Laboratory in Livermore, California was interviewing for all sorts of people. I had already decided that I was going to leave United Aircraft, and had talked to people like Lockheed and so on. So I thought, "Gee, What is this outfit?" And I decided I would go down and at least talk to them.

So, I proceeded with the interview process, and was invited out for an interview at Livermore. That progressed through the various administrative requirements to a job that I started, I believe, on the 20th of June, in 1967. I got hired into what at that time was the Nuclear Test Engineering Division.

I was almost immediately assigned to an event called Hupmobile. All this was new to me, and I didn't know what to expect. I think it was six or seven months before that event was fired. At the time I was very new to the Laboratory, and that was my first test. I didn't realize how complicated that shot was at the time. I just thought they did that all the time. Then I just went on from there to one event after another, in the capacity of what was then called, and is presently called, the diagnostic engineer.

Things just progressed from there. I spent probably four or five years as a diagnostic engineer, and then a position became available in the readiness group. That program ended about two years after I became associated with it.

I jumped from there to the laser program for a couple of years. But, I guess the Test Program had become ingrained enough in my interests that I decided I liked it better back in the Test Program. And so, I came back into the diagnostic group, and took a position as a group leader, which happened to be available. We fielded a number of events, I advanced to section leader of the entire diagnostics section in NTED. I went from there to become a device systems engineer, which job I had for seven or eight years, and then became a Test Director, which is what I am now.



Tom Scolman
LANL - - Test Director

I came direct to Los Alamos in 1956, after getting a Ph.D. in experimental physics at the University of Minnesota. I came to Los Alamos for several reasons. One, I had several friends I had been with in graduate school who had come to Los Alamos, and they were very high on Los Alamos, not only as a place to work but as a place to live. I'm a small town boy. I wasn't particularly anxious to go to a large city, and so I found Los Alamos very appealing, and the work was challenging and interesting.

When I came to work I went to work for the weapons division, which in those days was responsible for the engineering design and production of weapons, both for stockpile and for testing. I worked with a group that was largely responsible for interfacing between designers and engineers, and my involvement with Test was through the fact that this particular group had the responsibility of monitor-

ing and certifying the gas handling for test devices. With this I was involved with the Hardtack operations, both in the Pacific and later when we came back to Nevada, although I was not part of the test organization, per se.

It wasn't a bad life, out in the Pacific, if you didn't mind being away from where you lived for a while. That certainly was the most negative side of it. I think in many ways it was harder on the families back here than it was on the participants in the field. It turns out the group I was in was not engaged in the construction in the field, so as a result we did not have to go out and spend six months in the field for every operation, as much of J Division, the test division, did in those days. For example, on Hardtack Phase I, if I remember right, I spent probably not more than like six weeks at Enewetak.

We did, as some people remember, then come back and do Hardtack Phase II, which was very different. I remember one time where we were out arming a device, preparing it to go up on a balloon so it could be fired at dawn. While we were out arming, three shots were fired within probably five miles of where we were.

Carothers: I've talked with people at Livermore, and the things they have said about that operation are hard to believe these days. Bob Petrie said that they once went out, got the carpenter foreman, and said, "We want a tower. How high can you make it by tomorrow night? Can you make it about this high, and about that wide? And, we'll need some steps." And Walt Arnold told me, "I remember carrying a device up those stairs." I said, "Aw, come on." Do you believe that, Tom?

Scolman: Yes I do. I never carried a device up the stairs, but I did carry one on my lap, in the backseat of a sedan, out to the zero point.

It must have been the fall of '62 that I got into J Division. I had become closely acquainted with Bob Campbell during our involvement in the operations, and I said, "Is there anything in J Division that might be interesting?" He suggested that I look at their timing and firing group, which was J-8. It wasn't really in line with my background, but it was sufficiently interesting, and had some involvement with the field activity. I enjoyed the testing business. I like to go out and do things, and the test people do things.

I worked in the timing and firing organization until about '65 or '66. Then we started branching out, doing things other than tests at the Nevada Test Site. We got involved with some of the Plowshare operations. We got involved with the first shots on Amchitka, and then there was a need for another Test Director. Initially, Bill Ogle asked me to come to the division office and work with Bob Campbell and Bob Newman, as a Test Director. What was supposed to be initially a one year assignment turned out to be the rest of my career at the Laboratory.



Carl Smith
SNL - - Shock Physics

My family were mechanical engineers, and there seemed to be, at first, the typical role of following my father and older brother. But it turned out that there was a physics course in high school, with a very good teacher who steered me in that direction.

I started at a little college in Indiana called Earlham. Then I went to Washington University in St. Louis for a year. It turned out that Brown had a big program in acoustics, with people like Robert Byer, and Robert Bruce Lindsey. After a year at Washington University I decided, "Hey, I'm real hot about acoustics, and the field of ultrasonics." And so, I transferred after one year of graduate school at Washington to Brown University, in physics. There I did my thesis on finite amplitude acoustics - - underwater water waves and finite amplitude effects. I got my degree in 1966.

I went to Stanford Research Institute in 1966, and was there for almost ten years. SRI was still associated with the University when I started. For many years there had been a loose federation with Stanford, but the students rabble-roused at Stanford in terms of making the University pay more attention to what SRI was doing in some of their defense related work. The upshot of all their rabble rousing was that the two institutions were cut apart. That happened while I was there.

There were student protestors outside and stuff like that. I was reminded at that time of Emerson and Thoreau, years ago. Thoreau was thrown in jail for civil disobedience, and Emerson came to see him. Emerson said, "What are you doing in there, Henry?" and Henry Thoreau said, "What are you doing outside?"

The separation didn't really make any difference to the people at SRI. The work didn't change. The place had been on its own for a number of years, and was very much entrenched in what it was doing. It was a minor name change as far as the way the place operated. Our work continued, and the place ran very much as it had before. I stayed there until the end of 1975, and then I went to Sandia, and started there in January of '76.

Actually, I started doing for Sandia exactly what I had been doing for SRI, but it was a job with far more attractive opportunities to advance.

Bill Twenhofel
USGS - - Panel Member

I went to school at the University of Wisconsin, in Madison, Wisconsin, and my father was a professor of geology there. And so, of course, I had to take the beginning geology course, and I just sort of followed in my dad's footsteps. I got a bachelor's degree in 1940, with a major in geology and a minor in mining engineering. I went to graduate school at Madison for one year, and then I went to graduate school at the University of California at Berkeley.

When Pearl Harbor occurred I was at Berkeley, and I left graduate school and went to work for the U.S. Geological Survey in Washington DC. Shortly thereafter I was drafted, and I entered the Navy. I went to work at the Naval Research Laboratory in Washington DC, doing research on the growth of artificial crystals for sonar. I worked there until the war was over, and then went back to graduate school at Madison. After another year there I had met all the requirements except the thesis, and I went back to work for the Geological Survey.

I finally got the thesis done. It was on the geology of the Alaska-Juneau gold mine, in Juneau, Alaska. The Alaska-Juneau gold mine is unique. It had, at the time it was operating, the lowest grade ore of any mine in the world, and it still made a profit. So, I got my Ph.D., in geology, from the University of Wisconsin in 1952.

When I went to work for the Geological Survey in the early part of the war, and before I was drafted I was assigned to the Alaskan work. Then, later, after the war, and while I was in school I went up to Alaska to do field work in geology every summer. After I left Madison with all my requirements for my degree except the thesis, I was transferred by the Survey to Juneau, Alaska, and lived there year round, and worked there. I loved it. For a young fellow, Alaska is a great place, and Juneau was a great little town. It was just wonderful. You feel isolated a little bit, but the hunting, the fishing, and the outdoor recreation was just great. So I lived in Alaska for a time, and then I was transferred from Juneau in 1952.

I went to Denver, Colorado, with the Geological Survey again. I was assigned as the assistant group leader to a group studying the uranium deposits of the United States. The particular assignment

of the group I was in was to make estimates of the reserves of uranium in the United States, and in the rest of the world. I was not involved in the rest of the world, only in the United States.

It was a lot of guess work, but we took reports from mining companies, or from government work. There was a lot of government work, AEC work. You take the reports, and you construct conceptual geologic models in your mind of how deposits were formed, and therefore something about their size.

Before 1952, about 1950, the only known uranium ore bodies in the United States were the yellow and orange oxidized uranium minerals that are oxidized because of the surface processes. With drilling they discovered the primary uranium ore, which is not oxidized, and that led to some big discoveries in the Colorado plateau. I was involved with that until about 1956, when the underground test program began out here. I then got assigned to the Geological Survey group that supported the AEC at the Test Site.



Wendell Weart
SNL - - Panel Member

My undergraduate school was Cornell College, not to be confused with Cornell University. I got my undergraduate degree in '53, and then worked for about three years at the Ballistic Research Laboratories at the Aberdeen Proving Grounds, in Maryland.

Then I went back to get my degree from the University of Wisconsin. I was really interested in geology, but as I went along I felt a desire to get a little more into the hard physics of the thing, rather than the interpretive aspects that geology mostly involves. So, I just gradually migrated into geophysics.

I became associated with Sandia in a fortuitous way. I had never heard of Sandia Laboratories, and one day I got a letter in the mail saying, "We have just visited with your professor at the

University of Wisconsin, who says you are in the process of completing your degree. We'd be interested in sponsoring that if you'd be willing to come to work for us." So I started looking around to see who is this "Sandia Laboratories." The part about sponsoring my work sounded great, because their offer was a lot better than the teaching assistantships that were offered in those days.

So, I joined Sandia in August 1959. It was, I think, primarily because Sandia was trying to address some of the problems of a possible test ban treaty that was being much debated at that time, and they needed a seismologist and geophysicist. At that time we were a rare breed.

I got my Ph.D. in 1961, from the University of Wisconsin. They didn't grant a degree in a specialty, so the degree was in geophysics, and I did my thesis in the area of seismology. That was back in the days when there were very few universities that had separate geophysics degree programs. It was about to change greatly because of the Vela Uniform program, and all the studies that went on in conjunction with trying to understand the seismic effects of underground detonations.

So I did join Sandia, primarily to do seismologically oriented work. But one of the first things I got involved in when I went to Sandia, which eventually led to my containment related duties, was to reenter an event called Marshmallow, which was a tunnel shot that had been conducted in Area 16, in 1962. It was a shot with a long line-of-sight pipe, in a tunnel. It was conducted for experimental purposes, rather than for developing a device, and was considered to be a relatively successful event. There had been only a small amount of experience with tunnel shots, and particularly with pipe shots in a tunnel.

It was fired about six months after the Gnome event, which incidentally was, and I find this hard to believe, only about eight miles from where I have spent the last fifteen years of my life working on a project, trying to find a suitable means of disposing of radioactive waste.



Bruce Wheeler
USAF/DNA - - Test Operations

In 1951 I was here, in Albuquerque, being trained to take care of the nuclear weapons the Air Force had. After graduating from the assembly course I volunteered for, and was accepted in, what they called the nuclear officer course. So, I got to go to Los Alamos and train there, and that was a lot of fun.

So, I was a second lieutenant when they put me in the nuclear business, and I stayed in it virtually the rest of my thirty years in the Air Force. And it was good to me; I got promoted, and I got some interesting assignments, like DNA.

I became involved with DNA and the test work through Ted Jones, who was the Director of Test. That was late in 1971, and I had just been promoted to full colonel. I came to work here in Albuquerque, and served as the head of operations. That meant I

was involved in the details of construction, the entire test bed, the experiment package of the whole facility, and all the aspects of it. That included the calculations, the predictions, and the whole nine yards. At that time it seemed to me that people were being very careful, and a very worried. They were very desirous of putting together a shot that wouldn't do anything untoward.

There were things changing even as I came there. At that time there was not a well-founded, formal, well-managed research program to try to understand more about the containment of these tests, and I thought we needed that. One of the things I tried to encourage, and did encourage after I became the boss, was to go back and look at successfully contained tests; to mine back and see how things had worked right. As I perceived it, the only time the DNA dug back in to see what happened was when something went wrong. I thought there was a void there that ought to be filled with some understanding of the phenomenology of a successfully contained test. We routinely planned to use our contingency fund on every test for reentry mining, if there was any left, and usually there was some.

That job in DNA, when I became Director of Test, was the best job I ever had. I wouldn't trade that for anything. It was a field operation, and I could get the hell out of the office. Somebody asked me, "Why do you spend so much time out there in the tunnels?" I said, "That's where I go to regain my sanity." I enjoyed that kind of work, being part of putting something together, even though we blew it up afterwards.



Irv Williams
DOE/DASMA Staff

I did my undergraduate work at the University of New Hampshire. I joined the Air Force in 1950. I got into the ordnance business, and from there was put into the nuclear weapons business. In my early days I was a bomb commander on the old B-45's. I was non-rated, but assigned to a crew as a weapon commander for the B-45's, in 1952.

Then I went to Albuquerque for bomb-commander training. I had been trained as an engineer, and had a lot of ordnance, armament, fire control, radar work, and so forth, with the Air Force. And so they flipped a coin, and this unit, which was the first tactical bomber unit that was equipped with nuclear weapons, won me. I stayed with them, and went to England for three years with that group. We were at Sculthorpe, which is about fifteen miles from Sandringham, up in the Wash beyond Norich. Norich is in

Northrop County, and it's quite near the ocean, where England juts out into the North Sea. There is a big bay area, which is called the Wash. We operated there for three years, from a British base that the United States had used during World War II. We went in there, rehabilitated it, and operated out of that for three years. That place was really damp, wet, and rainy, and cold. The North Sea is very cold. It never gets much above about 34 degrees.

Then I came back and went to school at the Air Force Institute of Technology, at Wright Patterson. I was in a course called Air Ordnance Engineering, and as a result of that I was picked up, and zinged out to Kirtland to go back into the weapons program. This was after I came out of graduate school.

After three years at Albuquerque, which was a wonderful assignment, doing nuclear weapons work for the B-58 Hustler, and going through command and staff school, I was surprised by my next assignment, which was to Livermore. It was out of the blue. I had asked to go to the West Coast, and I got a letter sending me to Livermore. I was there assigned to the Defense Nuclear Agency's predecessor. I first came to the AEC, I would say, when I first went to Livermore. That was in 1961.

I worked with the engineers and the chemists in explosives, for B Division at Site 300. I kept track of every test design as it grew up during those early days. I followed all of them all the way through, and I did that for a good part of three years. I also spent time down in the plutonium building with Bill Ramsey, and with Gus Dorough in explosives. And occasionally I got to the Test Site.

I was at Livermore from '61 to '64, and I was there before we resumed testing. I was in the office with Marv Martin when the alert came to move and do a test. I don't know who called with those instructions, for sure, but I know people moved, and they went in all directions that afternoon. Immediately, after a short council, things started to move immediately.

So, I was there at the beginning of the resumption of testing. I was able to follow through the full three years, and follow the preparations for Dominic, the Pacific operation. I also did some work with the Laboratory people at Travis, and I spent several times there with the Hotspot team, with Marv Martin. I had a very good introduction to the program. I wasn't part of a design or device team, but I followed the designs and all the work in the Laboratory.

On a few occasions I did help with a little assembly work at the Laboratory, and I worked down at the Test Site with some disassemblies, with Ken Beckman and other fellows from W Division. I got to know a lot of people because of the opportunities I was given, working with Marv, to work with the Laboratory people. It was a way to really learn about the program. It was a tremendous experience.

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