

~Impactful Times~

Memories of Shock Wave Research at Sandia National Laboratories

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Sandia National Laboratories is a multi-program laboratory managed and operated by Sandia Corporation, a wholly owned subsidiary of Lockheed Martin Corporation, for the U.S. Department of Energy's National Nuclear Security Administration under contract DE-AC04-94AL85000.

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(Chapters in red are included samples)

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Chapter I

Introduction

Chapter I

Introduction

1.1 Sandia's Roots

The origin of Sandia National Laboratories began with World War II and the Manhattan Project.¹ Prior to the United States entering the war, the U.S. Army leased land then known as Oxnard Field on the desert outskirts of Albuquerque, New Mexico, to refuel and service Army and Navy aircraft in transit. In January 1941 construction began on the Albuquerque Army Air Base, leading to the establishment near the end of the year of the 'Bombardier School-Army Advanced Flying School.' Shortly afterwards the base was renamed Kirtland Field, after the Army military pilot Colonel Roy S. Kirtland and, in mid 1942, the Army acquired the installation. During the war, Kirtland Field expanded and served as a major Army Air Corps training installation.

General Leslie Groves and Dr. Robert Oppenheimer co-directed the Manhattan Project.² Two major components of the Manhattan Project were Project Trinity and Project Alberta. Project Trinity prepared for the detonation, on July 16, 1945, of the first atomic bomb (nicknamed 'Fat Man') at the Trinity Site in New Mexico. Project Alberta involved the assembly, testing, arming, and delivery of the first airborne atomic weapon (nicknamed 'Little Boy') to Hiroshima on August 6, 1945.

During the Manhattan Project, engineering activities at Project Y or Site Y,³ the high-mesa hideaway in the Jemez Mountains northwest of Santa Fe, were carried out by 'Z Division.' Z Division was named for the division head, Jerrold Zacharias, a professor from the Radiation Laboratory at the Massachusetts Institute of Technology. The division was conceived as an ordnance design, testing, and assembly arm of the atomic bomb program. In July 1945 the site of this forerunner of Sandia National Laboratories was established at Oxnard Field to handle weapons development, testing, and bomb assembly.

Near the end of the war, Groves and Oppenheimer faced the challenge of turning the atomic bomb effort into an operation to produce and maintain the Nation's stockpile and develop peaceful uses of nuclear energy. Space at Site Y was then at a premium. Moreover, the members of Z Division needed to work closely with the military, and the remoteness of the site made transport of weapons components difficult. In addition, to encourage personnel to stay after the end of the war, Groves and Oppenheimer decided to focus what became the Los Alamos Scientific Laboratory on weapons design and to relocate the weapons production and assembly elsewhere. In the several months before July 16, Oppenheimer began looking for a location to continue that weapons engineering work – especially its non-nuclear aspects. Kirtland Field had served the transportation needs for Projects Trinity and Alberta; hence, the decision to relocate Z Division there permanently. Oxnard Field was therefore transferred from the Army

¹ For additional details about Sandia's roots and early years, see the first externally published volume of the history of Sandia, by Necah Stewart Furman, *Sandia National Laboratories: The Postwar Decade* (University of New Mexico Press, Albuquerque, 1990). That 858-page treatise includes extensive notes and a substantial bibliography.

² *Los Alamos National Laboratory: A Proud Past, an Exciting Future* appeared in 1995 as a special issue of *Dateline Los Alamos*, a monthly publication of the Public Affairs Office of Los Alamos National Laboratory. That 50-year Trinity anniversary issue provides an interesting retrospective with many recollections and photographs.

³ During the war, the term 'Los Alamos' was not used to ensure secrecy. The earliest official reference to Los Alamos Scientific Laboratory, according to Alan Carr, the Los Alamos National Laboratory historian, was in mid October 1945. The name was changed to Los Alamos National Laboratory on January 1, 1981.

Air Corps to the U.S. Army Service Forces Chief of Engineering District and assigned to the Manhattan Engineer District of the War Department.

By the close of the war, Z Division had begun to consolidate weapons assembly at the Albuquerque site. By 1946, the site was known as 'Sandia Base,' after the nearby Sandia Mountains.⁴ **Figure 1-1** is an early photograph of Kirtland Field and Sandia Base. From 1947 to 1971 Sandia Base was the principal nuclear weapons installation of the U.S. Department of Defense (DoD). Over that period, Sandia Base and its subsidiary, Manzano Base, conducted atomic weapons research that included development, design, testing, and training initiated by the Manhattan Project. Fabrication, assembly, and storage of weapons were also performed at Sandia Base. In 1971 Sandia Base was merged into Kirtland Air Force Base.



Figure 1-1. Kirtland Army Air Base is in the foreground with Sandia Base in the background (1945). The Sandia mountains are visible in the far left background.

By April 1, 1948, Z Division had grown to about 500 people and was renamed Sandia Laboratory. By mid 1948 it grew to around 1000 employees. On May 13, 1949, President Harry S. Truman sent a terse letter to Leroy Wilson, the president of the American Telephone and Telegraph Company (AT&T).

I am informed that the Atomic Energy Commission intends to ask that the Bell Telephone Laboratories accept under contract the direction of the Sandia Laboratory at Albuquerque, New Mexico. This operation, which is a vital segment of the atomic weapons program, is of extreme importance and urgency in the national defense, and should have the best possible technical direction.... In my opinion, you have here an opportunity to render an exceptional service in the national interest....

On November 1, 1949, a no-fee contract was established with AT&T to manage Sandia Laboratory through Sandia Corporation, an AT&T subsidiary. In 1979 the United States Congress designated Sandia as a national laboratory. The AT&T contract to operate Sandia remained in effect until October 1993. At present, Sandia National Laboratories⁵ (SNL) is managed and operated by Sandia Corporation, a wholly owned subsidiary of Lockheed Martin Corporation.

⁴ These mountains were named for the Spanish word for watermelon because, as viewed from the west, they turn that color at sunset in the winter.

⁵ Sandia operates laboratories and testing facilities in Albuquerque, New Mexico, in Livermore, California, at the Tonopah Test Range in Nevada, and at the Kauai Test Facility in Hawaii. Sandia also has offices in Carlsbad, New Mexico (the Waste Isolation Pilot Plant), in Mercury, Nevada (to support the Nevada National Security Site), in Amarillo, Texas (the Weapons Evaluation Test Laboratory), and a program office in Washington, D.C.

1.2 Science and Engineering⁶

Sandia's urgent task after the war was to build a stockpile of nuclear weapons. The early nuclear weapons were carried by subsonic aircraft, although the structural and environmental requirements did not differ substantially from conventional weapons. Hence, the existing engineering procedures and materials for system design were sufficient, but nuclear weapons had stringent reliability and safety requirements. The engineering groups at Sandia actively developed innovative safety concepts and conducted extensive tests for operational reliability during this period. Parachutes to permit delayed delivery, weak-link and strong-link safety systems, and reliable barometric and contact fuzing were among the new subsystems that had to operate in all environments.

The advent of supersonic and exoatmospheric missile delivery systems imposed more demands. Addressing these issues required understanding the effects of adverse environments on components and subsystems. A major issue was the effect of radiation-produced shocks during a nuclear burst. The high-energy x-ray radiation often caused material ablation, and the ensuing shock waves could cause major damage. The resulting stress waves propagated through materials and often produced component or subsystem failure, which could preclude weapon reentry and operation. Resolving this issue required knowledge of the stress levels produced by the radiation pulse and the tensile failure strength (referred to as 'spallation strength') of a broad range of materials. Knowledge of mechanical response, such as elastic-plastic behavior, did not exist at the stress and loading rates (usually referred to as 'strain rates') attained. Extrapolation of material response from low strain rates and stresses using the highest loading rates available at the time (typically provided by Hopkinson bars⁷) was not reliable. The extensive shock wave research at Los Alamos during and after the Manhattan Project provided information on the high-pressure equation of state (EOS) properties, but essentially no information on mechanical properties in the tens of kbars⁸ range and at loading times of less than 1 microsecond.

The response of materials subjected to impacts associated with delivery velocities and defensive countermeasures was also important. For reliable operation, the subsystem or component had to retain its integrity for several microseconds after impact with the ground or other structures, or after activation of contact and other weapon fuzes, to prevent deformation and destruction. Hence, this issue concerned how large amplitude stress waves propagated through many materials.

Weapons systems were also becoming more complex as component miniaturization evolved. Furthermore, operation of a weapon was sequential, with each component operating fully before the next one was started. Hence, size and timing were critical to weapon delivery. Coupled with miniaturization, another critical factor was the energy to power components such as gyros, triggers, and radar. All required power, either electrically or through mechanical actuation. Weapons would usually be stored for long periods and then have to function without failure. On-board small explosive devices were attractive since they could be stored and then detonated to produce significant power on demand.

The energy released by explosives can cause damage to adjacent components and subsystems from propagation of the stress waves generated by the detonation. How the stress waves were generated and spread through the weapons system had to be determined. A basic understanding of solid material response at a few kbars and at high loading rates for engineering models did not exist.

⁶ Much of the material in this section was summarized from the recollections of C.D. Lundergan, B.M. Butcher, A.J. Chabai, and R.P. Reed in Appendix A.

⁷ Hopkinson bars provided data at loading stresses of a few kbars and strain rates up to about 1000/s.

⁸ 1 kbar = 1000 atmospheres or 14,500 pounds per square inch (psi). The pressure units of kbar and Mbar (1 Mbar = 1000 kbars) are used throughout the text, although some figures show pressure in giga pascals or GPa (1 GPa = 10 kbars = 0.01 Mbar).

The myriad issues facing weapon designers and the need to understand material properties prompted shock wave research on the (i) dynamic response of mixtures, composites, and polymers in explosive power supplies to predict stress limits and times; (ii) electromechanical effects and operational limits from shock loading piezoelectric and ferroelectric (FE) materials; (iii) energetic materials for detonators and explosive power supplies; (iv) pulsed radiation effects on weapon structures and components; (v) ground shock effects from nuclear explosions in geological media and the effects on above- and below-ground structures; (vi) compaction behavior of porous materials for shock cushioning of components and subsystems; and (vii) time-resolved gauges to quantify material response to pulsed radiation.

By the mid 1950s, Sandia management decided to establish a research program modeled after AT&T Bell Laboratories, to support these engineering applications. An immediate outcome was formation of the Physical Sciences Department and the Physical Research Department in the Research Directorate managed by Stuart C. Hight. By 1957 each department had 30-40 researchers in several scientific disciplines. The Physical Sciences Department under Richard Claassen was engaged in fundamental research, and the Physical Research Department under George Hansche was engaged in applied research. Over the years, both departments were to make many significant contributions. Other departments in the Research Directorate included the technical areas of weapon effects, mathematics, and aerodynamics.⁹

Two visionary individuals profoundly influenced the early and subsequent development of shock wave research at Sandia. One was Frank W. Neilson and the other was C. Donald Lundergan. Neilson initiated a research effort in the mid 1950s to understand the response of FE ceramics to shock loading; that effort resulted in the ground-breaking development of time-resolved stress gauges.¹⁰ As a division supervisor, Neilson recruited Orval Jones from the California Institute of Technology. Jones then played a major role in the development and application of shock wave research at Sandia.

On the engineering side Charles Bild, Director of Materials and Process Development, also encouraged and strongly supported shock wave research for weapons component and system development. At the request of Leon Smith, manager of the Electrical Systems Department in that directorate, Lundergan was asked in 1957 to perform projectile impact experiments with an air-driven gun on contact fuzes under development. This work laid the groundwork for a new program to measure the dynamic response of materials under precisely controlled impact conditions (see Chapter III). Lundergan recognized the critical importance of the emerging shock wave technology to Sandia's mission and proposed a comprehensive plan for a new department focused on all aspects of the technology (theoretical modeling, computational capability, and experimental research). In 1959 he recruited Lynn Barker who, along with Roy (Red) Hollenbach, has had a profound influence on the development and application of experimental shock wave techniques.¹¹

A third technical area at Sandia in which shock wave technology has played an important role is geological materials. Since its formation, Sandia has participated with the other two national security laboratories, Los Alamos National Laboratory (LANL) and Lawrence Livermore National Laboratory (LLNL),¹² in field testing nuclear weapons. Aboveground testing was common until the early 1960s, when the United States, Britain, and the Soviet Union signed a treaty on October 10, 1963 banning nuclear tests in the atmosphere, oceans, and space. Sandia continued to participate in underground tests at the Nevada Test Site¹³ (NTS) led by its two sister laboratories. The quartz gauge developed by Neilson

⁹ These details are from a private discussion with Orval Jones in 2012.

¹⁰ This information is from Bob Graham's comments commemorating George Samara's work at Sandia (George Samara Memorial Symposium, Sandia National Laboratories, on May 18, 2007, which are included in Appendix A.

¹¹ Lynn Barker took a leave of absence to study for a Ph.D. in physics at Cornell University in 1961 but returned to Sandia in 1962 after his wife became very ill.

¹² The laboratories have been known by those names since 1981. These acronyms are used in the rest of the text.

¹³ The Nevada Test Site was renamed the Nevada National Security Site (NNSS) in August 2010.

and his team was especially useful in quantifying the threat from x-ray deposition in weapons components.

Several other Sandians made significant contributions to the national defense posture in the early years, including Bob Bass, Carter Broyles, Ronald Carlson, Albert Chabai, Hunter DeVault, Doris Hankins, Bill Perrett, Carl T. Smith, and Luke Vortman. An important contribution was a new gauge technique to estimate the yield of nuclear weapons. In the early 1960s Bass and Chabai adapted the SLIFER¹⁴ technique, developed at LLNL, to obtain continuous shock wave attenuation data in geological materials (*e.g.*, volcanic tuff, granite, desert alluvium, and salt). Bass and Chabai discovered a universal power-law relationship for shock position versus arrival time as a function of yield that was independent of the geological materials. Bob Brownlee of Los Alamos used the Sandia SLIFER technique and this relationship on many nuclear events at NTS since it could provide yield estimates within an hour after the detonation of a device.

With the end of the Cold War and the Comprehensive Test Ban Treaty (CTBT), all underground nuclear testing was discontinued. Since then the National Nuclear Security Agency (NNSA) has relied mainly on the Stockpile Stewardship Program (SSP) to sustain and assess the safety, security, and effectiveness of the stockpile through advanced computing and development of complex models based on laboratory experiments, without the use of underground nuclear tests. An extraordinary set of science, technology, and engineering facilities supports the SSP. At Sandia, the Z pulsed power facility and the Shock Thermodynamics and Applied Research (STAR) facility, in particular, support shock physics studies for materials of interest. These contributions are discussed in more detail in Chapters VI and VII.

1.3 Building Capability in Shock Wave Research

The decision to initiate a shock wave research program at Sandia was instrumental in the rapid development of capabilities to address a wide range of weapons science and basic science problems. In the mid 1950s a small ongoing effort involving shock waves was directed toward understanding how the electrical output of FE crystals responded to dynamic impact, such as shock loading by small explosive charges. The motivation for that research was the need to develop various components, including explosively-actuated power supplies, to fire the nuclear weapon.

1.3.1 Advances in Experimental, Diagnostic, and Modeling Capabilities

The pioneering team of Frank Neilson, Bill Benedick, and Bob Graham began by investigating the electrical output of FE ceramics subjected to shock loading. Barium titanate was one of the first materials studied to understand how loading conditions, such as peak stress and loading times, generated the electrical output. Understanding the coupled mechanical and electrical response required experiments with carefully controlled loading conditions. Precision diagnostics were also needed to measure the input stress and electrical time histories, and controlled shock wave techniques and instrumentation were just beginning to become available in the mid 1950s.

The simplest configuration to produce well-controlled shock compression is planar loading of a thin disk. In this configuration, a planar shock wave propagates through the disk and is detected when it arrives at the back surface, thereby determining the shock velocity. In general, measurements are made of the shock velocity and either the input shock pressure or particle velocity produced by the shock

¹⁴ SLIFER is an acronym for Shorted Location Indicator by Frequency of Electrical Resonance.

wave.¹⁵ These two quantities can then be applied in the equations for planar shock motion to determine the pressure and density, or the specific volume (inverse density), produced in a sample. By performing experiments at different initial shock pressures, a locus of pressure-density points is obtained, which is referred to as the Hugoniot curve for the material. This curve is used to develop material models to describe the dynamic response for various applications.

Facilities for laboratory shock wave experiments were extremely limited in the 1950s. The first experiments at Sandia were conducted with the high explosive (HE) loading technique developed during the Manhattan Project. Explosive shock wave generators, known as plane wave lenses, were used to generate the high shock pressures. Similar techniques, but scaled to lower pressures, were used by Neilson's team. Accurate diagnostics, mainly time-of-arrival shorting pins that had been developed to detect the arrival of the shock wave at the back surface of a sample, were also initially used. In addition, the shock or mechanical response and the electrical output of FE materials were determined to quantify the coupled electromechanical behavior.

The HE experiments were conducted outdoors at a remote site in Tech Area III, about five miles south of the main Tech Area I. In the mid 1960s this effort evolved into a major thrust within the Physical Research Department that focused on the physics and chemistry of shock compression for a broad range of materials. The development and application of this work are discussed in more detail in Chapters III, IV, and V.

In parallel with shock wave research on FE materials, a similar program began in another organization to understand the response of contact fuzes and other weapons components and systems. This experimental effort under Lundergan began a few years after the explosives work on FEs and at first focused on the mechanical behavior and engineering aspects of component response. Along with Lundergan, Barker and Hollenbach were the principal participants in the effort, which began around 1957.

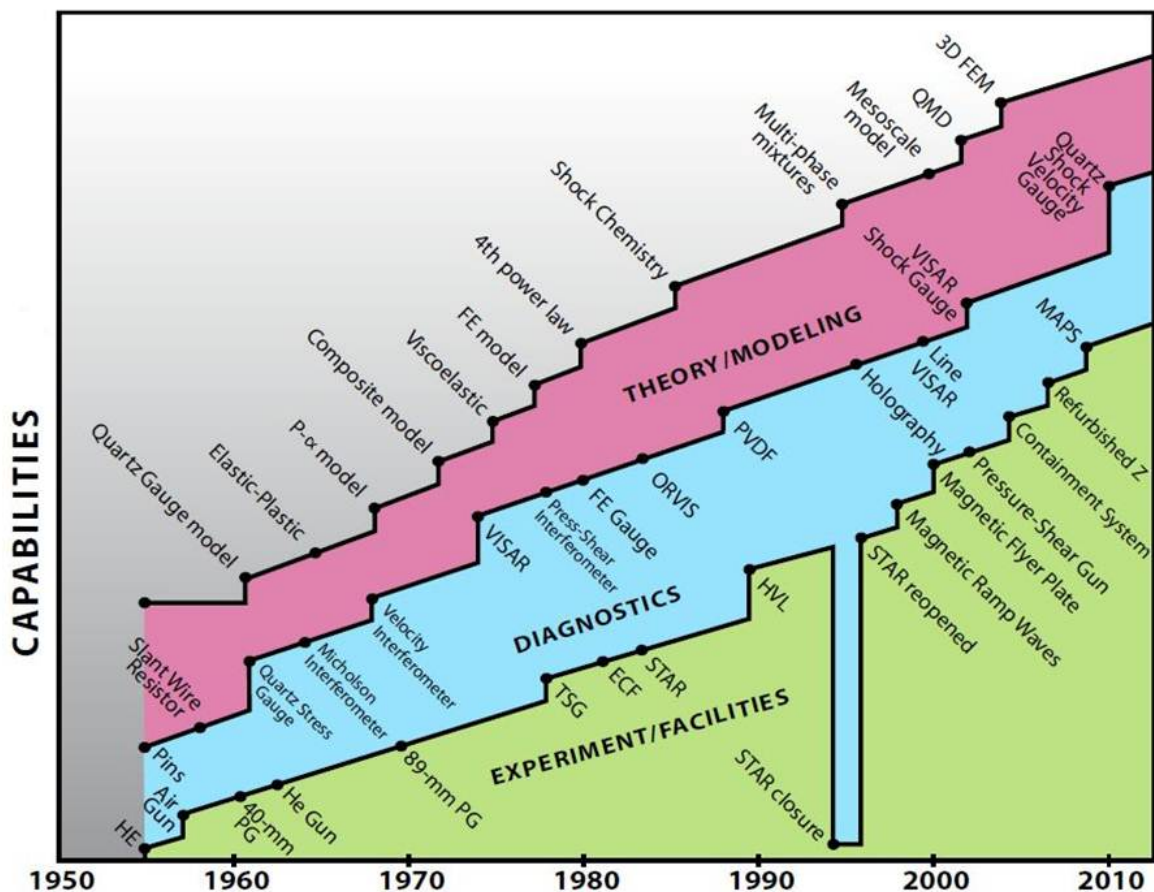
In place of explosive charges, Lundergan took a different approach around 1958 that had a lasting influence on shock wave research, not only at Sandia, but in the entire shock wave community. He used a 100-mm-bore compressed air gun to accelerate a projectile to ~ 0.3 km/s and, upon impact, to produce planar shock waves in a flat-disk target. The air gun was a 'hand-me-down' that had been used to test the effects of impacts on weapons components. Precise perpendicular impact with minimal 'tilt' was achieved by accurate boring and polishing of the gun bore and designing projectiles with flat, normal impact plates. In addition, the use of compressed air as an accelerant rather than explosives avoided several operational restrictions, such as the location of the experiment and safety regulations associated with explosives. Later this gun was upgraded to use helium gas, which increased the impact velocity, and was moved to more comfortable quarters in Tech Area I.

Lundergan's initial air-gun launcher marked the start of a family of precision guns for material property studies and also prompted their development at other institutions, as described in Chapter III. **Figure 1-2** summarizes the development of gun capabilities at Sandia. In 1961 Bob Graham developed a 40-mm-bore powder gun for increased velocity (Graham, 1961). Somewhat later Graham developed a helium gun to launch projectiles to ~ 1 km/s, as described in Chapter III. This was a major achievement in shock wave experiments at the time. In 1969 Darrell Munson and Ray Reed implemented an 89-mm-bore powder gun that doubled the launch velocities to over 2 km/s, providing impact pressures of several hundred kbars. Bob May then achieved a major advance in launcher capability with the construction of a

¹⁵ A longitudinal stress state is produced in a solid for planar loading. In a fluid, this is the pressure. In a solid, the difference between the two states is two-thirds of the yield stress of the solid, which is generally small, so stress and pressure are similar in most cases. Stress and pressure are used interchangeably throughout this document unless it is necessary to distinguish between the two in some discussions.

two-stage light gas gun, which tripled the velocity to over 7 km/s and gave impact pressures of 3-4 Mbars. Around 1990 Lalit Chhabildas invented a three-stage launcher, the Hypervelocity Launcher (HVL), which more than doubled the velocity to 16 km/s, as described in Chapter V (Chhabildas *et al.*, 1990).

Along with the rapid advances in facilities, diagnostics for precision measurements advanced quickly. The initial instrumentation for measuring shock wave properties consisted primarily of electrical shorting pins of different heights on the back surface of a shocked sample. Shorting of the pins determined the displacement versus time history produced by a shock wave and thus the shock velocity (Smith and Barker, 1962). This simple technique was not optimal for determining the structure of the shock wave. To provide more detail about the shock structure, Barker and Hollenbach devoted intense efforts to measuring the free surface displacement continuously. Both a slant-wire resistor (Barker, 1961a, 1961b) and displacement interferometer techniques (Barker and Hollenbach, 1964) were developed, as illustrated in Figure 1-2. These developments are discussed in Chapter III.



Key:

ECF = Explosives Component Facility
 FE= ferroelectric
 FEM = ferroelectric model
 HE = high explosive
 HVL = HyperVelocity Launcher
 MAPS = Magnetically Applied Pressure Shear
 PG = powder gun

ORVIS = Optically Recorded Velocity Interferometer System
 P- α model = model for distention of porous material
 PVDF = polyvinylidene di-fluoride
 QMD = quantum molecular dynamics
 STAR = Shock Thermodynamics Applied Research
 TSG = Two-Stage light gas Gun
 VISAR = Velocity Interferometer System for Any Reflector

Figure 1-2. Development of shock wave capabilities at Sandia. The large drop in experimental capabilities in 1994 resulted from the management decision to dismantle the STAR facility and eliminate all associated experimental activities.

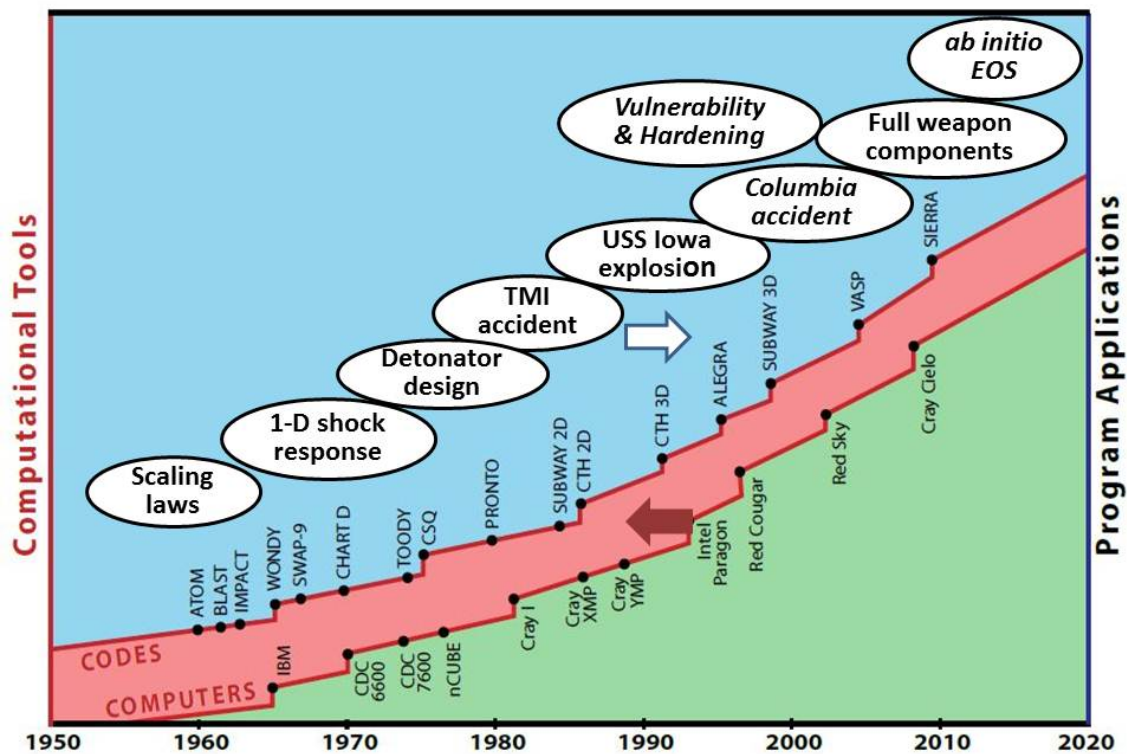
Two ground-breaking innovations in the 1960s discussed in Chapter III were the X-cut quartz gauge and an interferometer technique to measure the particle velocity directly that is produced by a propagating shock wave (Barker, 1968). Neilson, Benedick, and Graham's quartz gauge advance (Neilson *et al.*, 1960, 1962; Graham, 1961, 1975) provided a continuous measure of shock pressure (also referred to as stress here) versus time (*i.e.*, a time-resolved shock profile). A game-changing advance in instrumentation was the 1972 invention of the VISAR (Velocity Interferometer System for Any Reflector), which allowed detailed shock wave studies of any material (Barker and Hollenbach, 1972). These innovations impacted shock wave research worldwide. Their applications include dynamic elastic yielding, dynamic compressive and tensile strengths, determination of phase changes produced by shock compression and the associated transformation kinetics, initiation and growth of energetic reactions, optical properties under shock compression, and effects of non-homogeneous material properties on planar shock compression. Other major advances followed the VISAR, including ORVIS (Optically Recorded Velocity Interferometer System), the piezoelectric polymer PVDF (polyvinylidene di-fluoride), the Line VISAR, dynamic holography for measuring non-uniform material motions, and the use of X-cut quartz as a shock velocity-time profile gauge, rather than as a stress-time profile gauge, at pressures exceeding 3 Mbars to measure shock velocity directly (Knudson and Desjarlais, 2009). These developments are discussed further, for example, in Chapter VII.

Dynamic material models to predict the response for materials of interest to the Sandia weapons program (*e.g.*, metals, viscoelastic materials, geological materials, mixtures and composites, ferroelectric and piezoelectric materials, and explosives) were not available in the mid 1950s and early 1960s. The rapid advance in experimental capabilities led to new models to understand the fundamental deformation mechanisms and to predict how a material or component would respond to shock loading. The modeling deficiency was explicitly addressed in the 1960s, beginning with Neilson's three-zone model of the electrical response of shocked piezoelectrics (Neilson and Benedick, 1961). That model had a major impact on the development of materials for shock wave gauges. Other models developed in this period were elastic-plastic models for metals, composite models for mixtures and laminated materials, models for energetic materials, and models for dynamic failure and fragmentation. Moreover, in the 2000s *ab initio* models, such as quantum molecular dynamics (QMD), began to predict very high pressure EOS response. Figure 1-2 above summarizes the progress in developing a theoretical understanding of shocked materials. That progress required integration of three activities: experimental advances, diagnostic development, and advances in theoretical modeling, as discussed in subsequent chapters.

1.3.2 Advances and Applications of Computational Capabilities

The development of realistic models for shock wave response allowed detailed analysis and prediction of the performance of weapon components and systems during dynamic loading. However, this step required advanced computer codes to simulate configurations in three dimensions and computers with sufficient speed and memory to perform enormous simulations. Fortunately, both tools were developed in the same time frame as the shock wave capabilities in Figure 1-2, thereby allowing increasingly larger and more realistic simulations. **Figure 1-3** summarizes the major advances in codes and computer capabilities from the 1950s to the present.

In the 1950s, and to some extent the 1960s, the computer capabilities and simulations for modeling shock wave propagation had not advanced to the point where complex problems – such as crater formation from an explosive blast, underground shock propagation and interaction with a structure, or coupled radiation-shock response – could be simulated in realistic geometries. Many of these problems had to be addressed with a variety of scaling laws. With the rapid advances in simulation codes and more powerful computers, the situation began to change in the 1960s.

**Key:**

ALEGRA = Arbitrary Lagrangian Eulerian General Research Applications

CDC = Control Data Corporation

EOS = equation of state

IBM = International Business Machines

TMI = Three Mile Island

1-D = one-dimensional

Figure 1-3. Hydrodynamic computer codes and main frame computers used for shock wave simulations.

During this period, mainframe International Business Machines (IBM) computers were available, although they were limited by speed and memory, which restricted their use to simple problems such as the design and analysis of shock wave experiments or one-dimensional (1-D) approximations of weapon components. In addition, the material models and the codes (usually referred to as ‘hydrodynamics codes’ but sometimes as hydrocodes) were not available. In the mid 1960s the only codes to simulate shock wave problems were the 1-D code WONDY, a finite-difference Lagrangian code, and SWAP-9, a characteristics-type code that tracked shock waves.

From the early 1950s to the early 1970s the capability of mainframe computers grew a thousand fold, consistent with Moore’s law,¹⁶ which predicted a doubling of computing capability every 18 months. The Control Data Corporation (CDC) machines introduced in the early 1970s provided a major advance in computing power to develop the next-generation hydrodynamics codes, namely the two-dimensional (2-D) codes TOODY and CSQ, as identified in Figure 1-3. Sandia purchased a CDC 6600, considered to be the first supercomputer, around 1970. By 1976, two CDC 6600-class computers were available. Later, a CDC 7600-class computer was acquired, which was about ten times faster than the CDC-6600 and had 512 kbytes of memory. With these computers, TOODY and CSQ solved increasingly complex problems, such as the hypervelocity impact of a nylon ball onto a steel plate or the analysis of the concrete rupture during the Three Mile Island (TMI) accident in 1979. The purchase of a Cray 1, with 8

¹⁶ What became known as Moore’s law was published in the April 19, 1965 issue of the monthly magazine *Electronics*, by Intel co-founder Gordon Moore.

Mbytes of memory, in the early 1980s significantly increased the capability to perform large shock wave problems.¹⁷ The next large computer at Sandia was the Cray XMP, followed by a Cray YMP in the late 1980s. These machines had the capability to store the very large databases required for large numerical simulations.

The Cray YMP was followed by the massively parallel processing (MPP) computers in use today. The first MPP computer at Sandia was the Intel Paragon, which figured prominently in solving several large shock wave problems. The first production MPP computer for solving shock wave problems was the Advanced Simulation and Computing Initiative (ASCI) Red machine. Sandia researchers now routinely use the Cray Cielo supercomputer at LANL for the largest shock wave calculations. Cielo is 10 million times faster than the Cray 1.¹⁸

The enormous increase in computing power over the last several decades has had a profound impact on the complexity, size, and type of shock wave problems that can be analyzed. The top part of Figure 1-3 highlights a few of the many applications possible because of the increased capabilities. In the 1950s and early 1960s, scaling solutions were often used to predict the effects of complex shock wave propagation in geological media or the damage to structures by large explosions. In the mid-to-late 1960s, WONDY and SWAP-9 began to be used for 1-D shock response simulations, including the design and analysis of experiments and some weapon applications. Porous material crush up was of particular interest to the weapons program and could be predicted with WONDY, as discussed in Chapter IV.

In the 1970s, CSQ and TOODY simulated several important weapons problems as well as problems of national import, including the TMI nuclear reactor accident (as discussed in Chapter IV); in the late 1980s, as discussed in Chapter V, the CTH code was used to analyze the turret explosion aboard the USS Iowa. The combined development in the early 1990s of three-dimensional (3-D) hydrodynamics codes, such as CTH, and the advent of fast MPP computing enabled a greater range and complexity of applications that could be addressed. Notable examples include analysis of the Shoemaker-Levy comet impact on Jupiter and missile intercept lethality, both of which are discussed in Chapter VI. The Jupiter impact simulation provided estimates of crater size and impact flash, allowing observers on the Earth to interpret the astronomical observations. In the 2000s, these capabilities determined that the cause of the Columbia shuttle disaster was foam debris from the fuel tank impacting the orbiter wing.

Code development efforts continued into the late 1990s, resulting in increased capabilities to describe complex material phenomena, including the 3-D SUBWAY code to simulate the electromechanical response of weapon components (as discussed in Chapter VII). The 3-D magnetohydrodynamics code ALEGRA¹⁹ further expanded the types of phenomena that could be studied (see Chapter VI). The *ab initio* calculations of material properties that are now possible are discussed in Chapter VII. Several other model development efforts and examples of simulations are discussed in these chapters. *The remarkable upshot is the enormous increase in the complexity of problems that are solvable through the concerted efforts of shock wave research, hydrodynamics code development, and increased computing capability.*

To accomplish these major advances, the number of Sandia staff involved in shock wave research grew rapidly in the 1960s. By the end of the 1950s, about a dozen staff were associated with that research. By the end of the 1960s, about four dozen researchers were involved. Hiring of shock wave researchers continued into the 1970s and beyond, but at a slower rate. In the mid-to-late 1970s a number of researchers left the Physics of Solids Research and Solid Dynamics Research Departments, primarily

¹⁷ An interesting aside is that the 2011 Smart Phone is equivalent in computer power to the early 1980s Cray 1.

¹⁸ The fastest computer now available, LLNL's Sequoia, is about a thousand times faster than that predicted by Moore's law (Bob Schmitt, Sandia National Laboratories, 2013).

¹⁹ ALEGRA is the acronym for the Arbitrary Lagrangian Eulerian General Research Applications code.

because of other growing programs at Sandia, such as the Energy Initiative in the U.S. Department of Energy (DOE). A resurgence of hiring in shock wave research by the Pulsed Power Sciences Center occurred in the early 2000s with the discovery of magnetic loading, which considerably expanded the material response regions accessible through shock compression. This new area of shock physics is discussed in Chapter VII.

1.4 Chapter Organization

The following chapters are organized by decade. Each highlights the major developments and activities during its decade. Chapter II, “Origins,” describes the programmatic needs at Sandia that led to the emerging shock wave effort in the 1950s. Chapter III, “Explosive Growth,” describes the increase of shock wave capabilities in the 1960s. Chapter IV, “New Opportunities,” emphasizes the wealth of opportunities that emerged in the 1970s and the computational tools that were developed concurrently and applied to important problems. Chapter V, “Heady Times,” discusses the breadth of applications that used these capabilities in the 1980s. Chapter VI, “Black Monday,” describes the near elimination and resurgence of Sandia’s experimental shock wave capabilities and the impact of computational advances on the ability to solve complex shock wave problems. Chapter VII, “A New Millennium,” discusses the techniques and initiatives that have reenergized shock wave research at Sandia. The final chapter, “Looking to the Future,” discusses possible directions for future shock wave research at Sandia. Appendix A contains the individual recollections of about 40 shock wave researchers at Sandia; these provide personal addenda to the chapters and selected quotes from the recollections are included in the chapters themselves. An extensive set of publications is included in Appendix B for those interested in considering in more detail the shock wave research conducted at Sandia National Laboratories in the last 60 years.

Chapter V

The 1980s – Heady Times

Chapter V

The 1980s – Hedy Times

5.1 Background

The previous two decades of shock wave research at Sandia led to (i) advances in experimental techniques, (ii) measurements of dynamic material response for a wide range of materials, (iii) state-of-the-art material models, and (iv) a family of 1-D and 2-D computer codes that could simulate materials used in weapon components and subsystems with considerable accuracy. However, during the 1980s full 3-D code capabilities were needed for higher fidelity simulations.

In the 1980s two major events had a major influence on shock wave activities in the following years. First, President Ronald Reagan began the Strategic Defense Initiative (SDI) in 1983. This forward looking, ambitious program proposed ground- and space-based assets to protect the United States from attack by nuclear ballistic missiles. SDI, popularly dubbed “Star Wars,” was widely criticized as being unrealistic, even unscientific. Claims were also made that it would destabilize fragile international agreements based on the concept of mutually assured destruction (MAD) and re-ignite an offensive arms race. In spite of these objections, the program proceeded and led to new initiatives in most major research organizations.

To accomplish the research goals of SDI, several innovative technologies were proposed by the three DOE national security laboratories (at that time termed the “national weapons laboratories”), many universities, and industrial institutions. By the mid 1980s, considerable funding was flowing from the SDI Innovative Sciences and Technology Office, mostly in the area of applied research. Although none of the proposed concepts was ever fielded in practical applications, much of the basic technology developed through SDI laid the groundwork for innovations that have continued to influence the Nation’s research programs to this day in high-energy physics, supercomputing, advanced materials, and other critical disciplines.

In 1987 the American Physical Society concluded that a Star Wars-type shield was not feasible at the time and at least ten more years of research would be needed to even determine if such a shield could ever be practically applied. In 1993, the research emphasis shifted from national missile defense to theater missile defense and the scope changed from global to focused regional coverage. However, SDI and the new Theater Missile Defense (TMD) program had a significant influence on Sandia’s shock wave programs.

Sandia began several strategic efforts as part of the original SDI program. One was a new program to develop capabilities for launching gram-sized projectiles to velocities exceeding 15 km/s. Such velocities were about a factor of two higher than possible with existing two-stage light gas gun facilities. The experimental shock wave group, then managed by Jim Asay, a first-level manager in Lee Davison’s Solid Dynamics Research Department, participated in this effort and actually exceeded the velocity goal with one of the two approaches that were pursued.

The other major development was the start of massively-parallel computing at the three national security laboratories. Development of this ground-breaking technology, which employed several thousand parallel processors instead of the single processor used in standard computers, was one of the stated goals of SDI. The intent was to exceed computational speeds by many orders of magnitude over conventional capabilities. Under Director Ed Barsis, Sandia played an early role in initiating massively-

parallel computing and, for a short while in the 1990s, possessed the fastest supercomputer in the world. Massively-parallel computing ultimately had a major impact on the ability to perform realistic numerical simulations of dynamic events for both weapons and other scientific applications.

The major advances in shock wave research during this period are discussed in the following sections of this chapter.

- High-Pressure Material Strength (Section 5.2)
- Three-Stage Railgun for the Strategic Defense Initiative (Section 5.3)
- Metallization of Hydrogen on the Two-Stage Gun (Section 5.4)
- Graded-Density Impactor or ‘Pillow’ for Soft Impact (Section 5.5)
- The Sandia HyperVelocity Launcher (Section 5.6)
- Generalized Fragmentation Theories (Section 5.7)
- Laser windows for Mbar Profile Studies (Section 5.8)
- Shock-Induced Solid-State Chemistry (Section 5.9)
- Piezoelectric Polymers for the Bauer Shock Gauge (Section 5.10)
- High Fidelity Ferroelectric Models (Section 5.11)
- CTH – a Robust 3-D Hydrodynamics Code (Section 5.12)
- Turret Explosion on Board the USS Iowa (Section 5.13)
- People and Places of the 1980s (Section 5.14)

5.2 High-Pressure Material Strength

A major goal of shock wave research is to characterize the mechanical and physical properties of materials fully over a stress range of interest so that continuum models can be developed for accurate simulations. Material strength, which is usually described through the yield strength dependence on strain rate, stress, and temperature, is particularly important. The initial yield strength is easily determined, in most cases, by measuring the elastic-plastic wave structure at low peak shock stresses (typically less than 100 kbars), where the elastic response is observable. A number of stress gauges and interferometer techniques have been developed to provide this information. However, the material strength *after* shock compression to high stresses (typically greater than 100 kbars) is difficult to measure with any existing techniques today. Approaches such as piezoelectric gauges, interferometer techniques, and pressure-shear techniques have been used at Sandia whereas lateral-longitudinal gauges have not usually been successful at these stress levels in metals, especially at stresses of several Mbars. Strength information at such ultra-high stresses is needed to address a variety of weapons issues and for other scientific applications.

Asay and Lipkin (1978) first proposed a wave profile technique to extract strength information through a combination of unloading and reloading from a given shock state. The technique was demonstrated at shock stresses of about 30 kbars on aluminum and the measured results agreed well with other techniques. In the early 1980s the technique was extended to stresses of a couple hundred kbars on aluminum and several other materials using the powder gun at the STAR facility (Asay and Chhabildas, 1981; Chhabildas and Asay, 1981; Chhabildas *et al.*, 1981; Chhabildas *et al.*, 1982) and later to stresses over a Mbar in tantalum and tungsten using the two-stage light gas gun; Chhabildas and Barker, 1988; Chhabildas *et al.*, 1988).

After demonstrating aluminum strength measurements to several hundred kbars, Chhabildas and Asay were anxious to extend the capability to Mbar pressures for a broad range of materials. Unfortunately, a strong programmatic need did not exist at Sandia, and discretionary funding was extremely limited. Fortunately, LLNL came to the rescue. Dan Steinberg at LLNL had been developing constitutive models for the weapons program, which needed data on material strength for a wide variety of materials and to

pressures of several hundred kbars. The shock wave group under William J. (Bill) Nellis at LLNL did not have time-resolved interferometry at that time or the technical expertise in wave profile analysis to embark on an extensive program, nor were they interested in pursuing such work. Because of the pressing weapons requirements, Steinberg approached Asay and Chhabildas to perform the experiments. One of the materials was beryllium, which because of its toxicity could not be studied without major modifications to the gun facility, so Asay and Chhabildas initially resisted taking on this task. However, LLNL persisted and elevated their request to Sandia's upper management. Chhabildas recalls the events at the time.

The strength work was especially appreciated by Dan Steinberg from LLNL and he used it to develop [a] strength model now referred to as [the] Guinan-Steinberg model for use with hydrodynamic codes. Dan wanted us to measure strength in the shocked state in beryllium! When the initial request came directly to us by Hal Graboske and Dan Steinberg from LLNL we were not too thrilled and we declined the invitation because of all the precautions that we had to take due to the toxic nature of beryllium. The LLNL folks then approached our upper management directly and we had no choice but to oblige.

The project ended up extending over several years and provided much needed financial support, which helped maintain the experimental capabilities and also elevated Sandia's stature for high-pressure strength work. Moreover, the strength of several materials was determined with the wave profile technique, and the results were used by Dan Steinberg and his colleagues at LLNL (Steinberg, 1996) in the 1970s and 1980s to develop high-pressure strength models that are still in use (Asay *et al.*, 2009). Research on high-pressure strength continues to be pursued actively at both STAR and Z. Recently, the technique was extended to over 2 Mbars for ramp loading of tantalum on Z by Brown *et al.* (2014). The novel new results in **Figure 5-1** represent the most accurate measurements of high-pressure strength made with any technique.

One observation from several strength experiments, particularly for hard, brittle materials, was that the shear stress states inferred from the wave profile measurements did not conform with the expectations of elastic-plastic theory, which has been a commonly used model for dynamic loading at low stresses. Several explanations have been forwarded for this apparent anomaly. One hypothesis by Dennis Grady was especially attractive, although it has still not been confirmed experimentally. This hypothesis is based on many shock recovery experiments in quartz from the Russian literature in which localized deformation is observed in bands. At extremely high stresses and correspondingly high strain rates, Grady suggested that shock deformation deposits the dissipative energy of the shock wave in small regions that are typically less than a micron in width, but less than the width of the shock. Hence, these localized regions are initially extremely hot during the rise in shock stress but rapidly cool after the peak steady state is reached. The outcome would thus be an initially softened state because of thermal trapping and highly localized temperatures, followed by strength recovery as the localized hot regions cool. The thermal trapping effect is a result of the interplay between the rise time of the shock, where dissipation occurs, and the size of the local plastic region that is being produced.

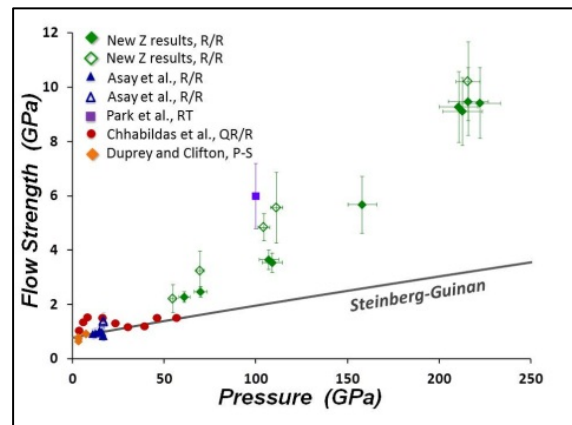


Figure 5-1. Flow strength versus shock pressure for quasi-isentropic compression of tantalum. The new Z results were obtained for two different Ta preparations. The mean pressure is estimated from the measured longitudinal stress. The solid and open diamonds represent two types of Ta; R/R refers to ramp loading followed by release; QR refers to quasi-ramp loading; R-T is the Rayleigh-Taylor method; and P-S is pressure-shear loading (Brown *et al.*, 2014).

Preliminary estimates of the width of the localized regions are less than a micron at low stresses (Grady and Asay, 1982), with the width decreasing rapidly with increasing shock stress. Swegle and Grady (1986) developed a numerical model based on this idea for use in simulations of shock and reshock wave propagation in aluminum. Swegle and Grady's calculations supported the possibility of the shock-induced softening effect, although several of their assumptions have not been validated experimentally. Shock-induced softening is also thought to occur in brittle materials, such as sapphire, and to show major deviations from the elastic plastic model upon initial shocking (Reinhart *et al.*, 2002). Other mechanisms for the apparent shock softening and recovery include irreversible dislocation motion (Johnson, 1993; Winey *et al.*, 2012) and heterogeneous deformation (Asay and Lipkin, 1978).

5.3 Three-Stage Railgun for the Strategic Defense Initiative

Of the several research programs begun in response to SDI, the Solid Dynamics Research Department under Lee Davison received funding for two major efforts. One was to participate with LANL on a strategic program to develop a hypervelocity launcher that ultimately could produce sufficiently high velocities to intercept and destroy ballistic missiles aimed at the U.S. LANL pursued an explosively-driven technique to accomplish this goal. At Sandia, Jim Asay proposed the use of a two-stage gun to inject projectiles at velocities up to 7 km/s into a third-stage electromagnetic launcher, often referred to as a railgun, which would further accelerate the projectile to 15 km/s. Conceptual feasibility was the first step in the research to produce even higher velocities.

Railgun technology was well established in the U.S. and elsewhere at the time, although velocities were limited to a little over 5 km/s (Hawke *et al.*, 1991a). This limitation was thought to arise from the ablation of the conducting copper rails used in the bore of the gun that produced the propulsive magnetic field. It was believed that, during initial magnetic acceleration in the breech, the heated copper rails would vaporize on their surfaces and ablate into the barrel, causing a significant increase in the mass of accelerated material. The thinking in the railgun community was that, if the projectile initially started at a high enough velocity in the railgun breech, there would not be sufficient time for ablation to occur. Thermal calculations indicated that a minimum injection velocity of 5 km/s was necessary to achieve this condition. To achieve that minimum velocity, a two-stage gun was needed. However, developing the technology for interfacing two quite different concepts, a gas dynamic two-stage light gas gun and an electromagnetic railgun, was a daunting task. **Figure 5-2** shows the concept for the three-stage railgun launcher.

Asay assembled a team of several Sandia staff that included Clint Hall, Carl Konrad, Jim Ang, Lynn Barker, Karl Schuler, Gerry Wellman, and Tim Trucano, all of whom had made significant contributions to shock wave research, as well as two LLNL researchers, Ron Hawke and Allen Susoeff, who were well-known experts in this technology from a prior railgun program at LLNL. The project was initiated in the mid 1980s without having any hardware on hand. The team designed, purchased, and implemented a two-stage light gas gun injector, the railgun, the 3-MJ power supply for the railgun, and the vast array of diagnostics and instrumentation to monitor and record the overall performance. Many obstacles were overcome and a three-stage railgun became operational at the STAR facility in about two years.

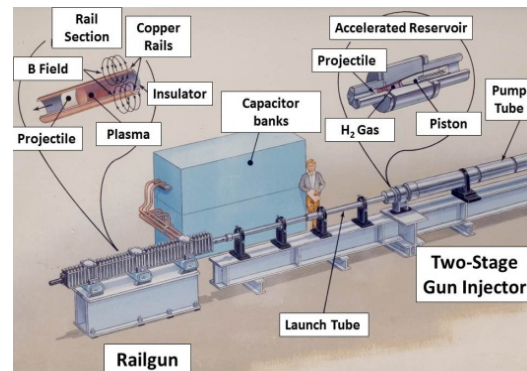


Figure 5-2. Three-stage railgun. The two-stage gun injects a projectile into the railgun at about 6 km/s. The railgun further accelerates the projectile to 7.5 km/s.

One of the major challenges was how to establish a current-conducting armature for a hydrogen-pressurized projectile entering the railgun section at 6 km/s. The challenge was like striking a match in a hurricane made completely of hydrogen gas (a good insulator). Clint Hall's recollection provides an amusing perspective of this daunting problem:

[Jim Asay] gave me a higher profile project...my first REAL one at STAR. I was to be the Mechanical Lead for the two-stage gas gun injected railgun (STARFIRE) [and]...simply couple a hydrogen filled gun with a delicate projectile that doesn't like joints and moves at about 6 km/[s] to a stationary, composite bore hunk that had to be absolutely pristine to avoid pre-arc formation, having a bore that was ... the same diameter most of the time, needed ... a 10^{-3} [torr] vacuum, no projectile blow-by... and measure the projectile velocity right before the railgun section.... [W]e got the two stage injector up and operational, modified a tank from the STAR "boneyard" ... as the impact chamber, and figured out how to make and align joints that actually passed [hypervelocity] projectiles through them instead of turning them into debris clouds. The problem left was the projectile blow-by. Without solving this problem, in-bore velocity measurements could not be made, and the railgun section would pre-arc on every firing. I sketched it [a solution] out and explained it to Lynn Barker, who said "Sounds reasonable."...[I]t worked [the] first time out, and [it] became one of the enabling technologies.... Another projectile innovation that led to a repeatable, self-arcng projectile included the silver epoxy loaded fuse...(that) went into a patent ... entitled "Hybrid Armature Projectile."

About two years after starting that project, the three-stage railgun was operational and was fired routinely to evaluate its performance. The highest velocity achieved was 7.5 km/s for an injected projectile at 6 km/s.

Several other staff played a major role in making this complex system work. Gerry Wellman and Karl Schuler performed detailed numerical analysis of the plasma armature used for the projectile acceleration in the railgun (Wellman and Schuler, 1988). Tim Trucano did numerical simulations to evaluate and minimize projectile gouging (Barker *et al.*, 1988, 1989), and Jim Ang participated in the projectile design and fabrication (Ang *et al.*, 1993). Lynn Barker identified mechanisms of pitting caused by sliding metal contact (Barker *et al.*, 1988) that was minimized through projectile design. Although this unique launcher (Hawke *et al.*, 1991a, 1991b) did not achieve the intended goal of 15 km/s, it did obtain a record velocity for railguns and resulted in the IEEE Peter Mark Medal for Outstanding Contribution to Electromagnetic Launcher Technology awarded to Ron Hawke. The project was discontinued in the late 1980s because of the low incremental velocity attained for the system. By that time, an alternative approach to achieving ultra-high velocities was showing promise and proved to be successful, resulting in the Sandia HyperVelocity Launcher, which is also known as the three-stage light-gas gun.

5.4 Metallization of Hydrogen on the Two-Stage Gun

The enabling technology that eventually led to the success of the HVL was the by-product of a project to make metallic hydrogen using the two-stage light gas gun. After leaving Sandia in 1974 to join Terra Tek Corporation in Salt Lake City, Lynn Barker returned in 1981 as a research staff member and joined Jim Asay's experimental division. Barker initiated two important projects that required the development of techniques to produce shockless (ramp) loading to Mbar stresses. This persistent and personal goal stemmed from Barker's early work in the late 1960s to generate ramp waves with fused silica buffers.

Barker's two projects used vastly different approaches for achieving shockless loading. One was a Laboratory Directed Research and Development (LDRD) project to compress a solid hydrogen sample, sandwiched between two tungsten plates, using a high-velocity tungsten impactor in a barrel filled with

helium to prevent shock loading. The other was to develop a standard graded-density impactor for use on the two-stage light gas guns, which would produce a gradual increase in stress with time up to Mbar pressures upon impact with a target, unlike the abrupt increase obtained by shock loading. Both projects were extremely challenging and would seed technologies critical not only for developing the HVL, but for later research efforts using magnetic loading on Z to achieve projectile velocities much higher than became possible with the HVL.

The goal of the first project was to metallize hydrogen at Mbar pressures as predicted by several solid-state theories. Previous LLNL experiments using cylindrical magnetic compression suggested hydrogen metallization would occur at about 2 Mbars, but these results were questionable. Barker's approach was to start with a solid hydrogen sample at 4.2 K, confined between two tungsten disks in a barrel extension to the two-stage gun. The two-stage gun barrel was filled with helium gas, which gradually compressed the hydrogen to Mbar pressures as the projectile accelerated down the barrel.

However, several obstacles had to be overcome first. One was to develop a cryogenic system for producing hydrogen at a few degrees Kelvin and then compress it gradually to Mbar pressures. A VISAR monitoring the rear tungsten disk determined the pressure history, and four fast MeV x-ray machines determined the compressed density from the separation between the tungsten disks. The thermodynamic measurements of pressure and density would allow a comparison with theory. Following measurements of the pressure-volume response, additional experiments were to be conducted to measure conductivity and determine if the sample became metallic during compression (Barker *et al.*, 1986).

The first task, to implement a cryogenic system and provide a target fixture to hold the sample, required a couple of years to accomplish. Barker developed a unique cryogenic system in which the solid hydrogen sample was prepared in a helium cryostat that Jack Wise had developed. The target fixture containing the hydrogen sample was located immediately outside the gun chamber and was rapidly inserted before the solid sample was warmed up. It was then isentropically compressed to high pressure by the projectile in the gas-filled launch tube of the gun. As Asay says in his recollection,

Lynn conceived of a novel technique that prepared the liquid sample in a short barrel extension made of tungsten that would become part of the two-stage barrel just before firing. The fixture was initially placed in a cryostat located immediately outside the impact chamber, cooled to a temperature necessary to solidify hydrogen, and [was] then rapidly insert[ed] and align[ed] ... to the gun barrel just before impact, kind of like a soft ball pitch.... Lynn called this contraption a "transient insertion mechanism" (TIM), and [he] dubbed it the "Tiny TIM" ... because it was huge. I don't know if anyone ever told him it looked like a classical Rube Goldberg contraption, although it did. However, it worked.

The second part of the project was simple in concept but difficult to implement. The goal was to compress the hydrogen sample continuously to a few Mbars. This was accomplished by filling the barrel of the two-stage gun with helium that was continuously compressed with a high-velocity projectile containing a tungsten facing plate. In early experiments, x-ray photographs showed that the front tungsten plate in the sample fixture broke into several fragments as the helium pressure increased. Although several attempts were made to eliminate this problem, they were not successful, and the final objective of making metallic hydrogen on the two-stage gun was not accomplished. However, an unexpected, but important, spin-off, which involved the shockless acceleration of thin plates to ultrahigh velocities, was successful and strongly influenced future shock wave programs at Sandia.

5.5 Graded-Density Impactor or ‘Pillow’ for Soft Impact

The other project that Barker started upon his return involved a new approach for producing ‘shockless,’ usually referred to as ‘ramp,’ loading of samples in a plate impact experiment. This was a continuation of his long-standing goal to develop techniques for shockless compression of materials to high pressure. In 1970 he and Red Hollenbach had published a paper that showed ramp loading could be accomplished by shock loading of fused silica. The ramp wave could then be used as input to samples bonded to it. However, fused silica was limited to peak stresses of about 35 kbars, and attempts to use this concept with other ‘glass type’ materials were not entirely successful because of the difficulties in reproducibility. The goal of several researchers in the shock wave community was to extend the peak stress achievable with ramp loading to Mbar stresses.

Barker’s new approach to shockless loading was to vary the density of the impacting plate continuously from a low initial density at the impact surface to a high density approaching that of tantalum at the back surface. This would produce a small initial shock in an impacted sample, followed by a gradual increase in pressure as stress waves reflected from the higher density regions in the impactor. Calculations showed that peak ramp loading stresses of several Mbars could be achieved. A major advantage of ramp loading is the resulting low temperatures that can be achieved. Shock loading to Mbar pressures typically produces temperatures of a few thousand degrees Kelvin in metals. In contrast, the temperature states obtained during ramp loading remain very low compared to the shock-induced temperatures. Thermodynamic states produced by ramp waves are nearly isentropic and hence the experiment is referred to as ‘ICE,’ for Isentropic Compression Experiment. This was another appropriate acronym conceived by Barker. **Figure 5-3** graphically illustrates the huge difference in temperatures obtained by shock versus ramp loading of LiF.

Barker, Carl Konrad, and later Doug Scott pursued an unconventional approach for making the graded-density impactor, which is described in Barker’s papers as a ‘pillow.’ He used this term to emphasize that ramp loading produces continuous, or ‘soft,’ stress loading, versus the abrupt, or ‘hard,’ change in stress obtained through shock loading. Barker and Scott prepared a suspension of several fine micron-sized powders with different densities in an alcohol solution and then deposited the powder on a flat plate through sedimentation. Sedimentation of a fluid suspension follows Stokes’ law, creating a difference in sedimentation rates as a function of density, and thus allows higher density particles to settle before lower density ones. The density distribution in the final sediment (Barker, 1984) could be pressed and heated into disks to use as impact plates on the two-stage gun. Through painstaking efforts, the process was perfected and several pillows were tested to demonstrate the capability for producing ramp waves to over 2.5 Mbars. The technique was used to measure the high-pressure quasi-isentropic response of several metals, including the tungsten in the hydrogen metallization experiments (Barker, 1984; Barker *et al.*, 1986; Chhabildas and Barker, 1988; Chhabildas *et al.*, 1988). Although this technique was successful in producing and measuring quasi-isentropic response to Mbar pressures, the small variations in density during manufacture precluded the capability to attain fully reproducible pillows. The difficulty in quality control, combined with Barker’s retirement in 1990, precluded widespread use of graded-density impactors for ICE studies at that time. However, the concepts for shockless acceleration of thin plates established in this research set the direction for approaches that are now used routinely.

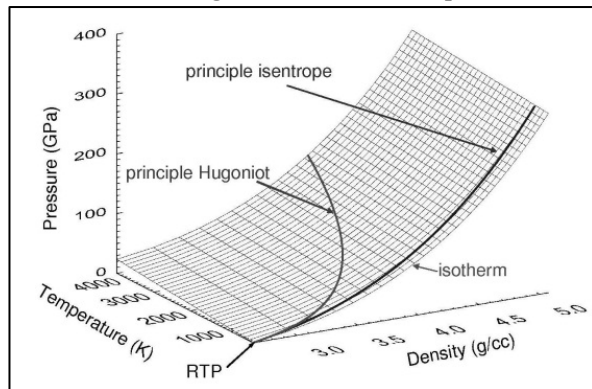


Figure 5-3. Temperature states achieved in LiF by shock (Hugoniot) and ramp (near isentropic) compression. RTP stands for reference temperature and pressure.

5.6 The Sandia HyperVelocity Launcher

Near the end of the hydrogen project, Lalit Chhabildas and Lynn Barker initiated a research effort to understand the basic mechanisms controlling breakup of the tungsten plate in the hydrogen fixture. To determine the effects of material properties such as yield or spall strength systematically, Chhabildas performed well controlled experiments on different plate materials with the same mass, using the same peak drive pressures, and measured the ‘degree of fragmentation’ of each (Barker and Chhabildas, 1990). The figure of merit used to identify better candidate materials for resisting fragmentation was the average fragment size after breakup. The results were rather surprising. It was generally thought that the yield or spall strength would impede instabilities that cause plate breakup. However, Barker and Chhabildas found that a single material property did not apply for estimating fragment size or the number of fragments for gas-driven plates. Tungsten possessed the highest yield strength and yet was the least resistant to fragmentation and easily broke up during acceleration. All other materials tested also fragmented during Mbar pressure gas loading and displayed unique fragment distributions. Nevertheless, a strong correlation existed between fragment size and the spall strength divided by plate density, which Chhabildas referred to as the ‘specific strength’ (Chhabildas *et al.*, 1990a). That figure of merit identified a very good flyer plate material, namely a titanium alloy, because of its high spall strength and low density. As Chhabildas recalls this exciting development,

I observed a correlation between the mean fragment sizes [sic, size] for each material to its specific spall strength (ratio of spall strength to its density). To verify this empirical relation I selected a titanium alloy – Ti-6Al-4V [–] as a candidate material to yield the least number of fragments. To my amazement we were able to launch this titanium alloy intact!

That surprising result was the beginning of the Sandia HyperVelocity Launcher. Shock wave researchers had made persistent attempts, dating back to the 1960s, to launch projectiles to velocities exceeding 10 km/s (the definition of ‘hypervelocity’) for military and scientific applications. Several approaches, including the railgun project, had not been successful. However, the understanding of plate fragmentation gained through the gas acceleration studies was a key breakthrough. The approach taken by Chhabildas and his colleagues involved transferring momentum from a slower moving, heavy two-stage light-gas gun projectile to a lighter flyer plate. The nominal plate sizes and masses required for the flyer plate indicated that Mbar pressures were needed to launch the projectile over less than a microsecond time scale of engagement. Direct impact would have shock melted and possibly vaporized the flyer plate.

A *shockless* interaction between the projectile and flyer plate was therefore required. The pillow technology developed by Barker was initially used for this purpose, although Chhabildas later found that a carefully designed, multi-layered graded-density impactor was easier to manufacture, reproduce, and characterize and worked about as well. In addition, a short barrel assembly holding the final flyer plate had to be coupled to the muzzle of the two-stage gun so that the second-stage projectile did not have any free flight to develop ‘extreme’ tilt with the third-stage flyer plate. The third-stage barrel was made of tungsten with an inner bore matching the diameter of the launch barrel of the two-stage gun, similar to that used in the hydrogen

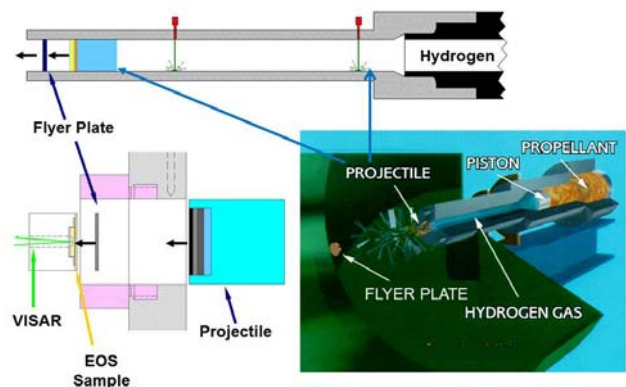


Figure 5-4. Three-stage gun concept (HVL) for launching thin flyer plates. The second stage projectile is launched to 7 km/s maximum velocity. A layered-density impact plate produces nearly smooth loading of the flyer plate to maximum pressures of 2-3 Mbars.

metallization project. A thin flyer plate was precisely mounted in the tungsten barrel and separated a few millimeters from the end of the two-stage gun barrel. The approach used the shockless impact of a thin flyer plate by a heavier, multi-layered projectile facing that traveled at up to 7 km/s. Upon impact, the lighter plate was shocklessly accelerated to a higher velocity, much like a heavy ball hitting a lighter one. The modified three-stage gun configuration that was produced is referred to as the Sandia HyperVelocity Launcher, or HVL, and is schematically illustrated in **Figure 5-4**.

Myriad engineering details had to be resolved before launching a flyer plate to hypervelocities. Clint Hall and Bill Reinhart aggressively pursued these issues. Hall comments:

Before too long, the projectile manufacturing... [and] repeatability, alignment fixturing, plate fabrication techniques, and two-stage performance had all improved. The project was eventually able to launch flat, intact plates capable of reasonably precise EOS measurements.

Making the second-stage impactors was also challenging. Originally, a simple, two-layered impactor was used, but it did not produce good shockless loading. Barker's graded-density impactors produced a much smoother acceleration of the third-stage flyer plate. Through extensive modeling and testing, a properly designed multi-layered impactor was easier to fabricate and worked very well. Reinhart remembers:

These impactors started as simple as PMMA²⁰/copper, to the Barker 'pillow' (fabricated by spraying metallurgical powders and using sedimentation techniques so a smooth variation in shock impedance occurred), [to] the Chhabildas 'plywood' (PMMA/Al/Ti/Cu), to the final configuration [that is] still in use[, consisting] of TPX^{® 21}/Mg/Al/Ti/Cu/Ta.

Extensive 2-D simulations were performed to identify the configurations for the multi-layered impactor that would produce minimal shock formation and 2-D effects arising during the impact. This was necessary because DNA (the Defense Nuclear Agency) at the time wanted to launch 'chunky' projectiles. Understanding these effects was especially important as the launch velocity increased from the initial launch of chunky projectiles at 12 km/s to over 16 km/s with refinements in the technique. Tim Trucano performed the 2-D simulations critical to optimizing the chunky design (Trucano and Chhabildas, 1993, 1995). He states:

I feel to this day that the best computational prediction I ever made was my work ... on the completed gun. [S]imulating the interior ballistics of the performance of the third stage, and the transition of the operation from second to third stage, basically exercised all of the computational capabilities ... Our prediction of the final velocity of the projectile leaving the third stage was within a few percent of the observation, and the [chunky] shape of the formed projectile was very close to [that in the] experiment.

This remarkable achievement was the epitome of Lundergan's strategic objective of coupling experiment, modeling, and computing to achieve a goal not attainable through each independently.

The development of the HVL was a breakthrough in experimentally accessing new regimes of material response that had profound impact for both weapons and other scientific applications. In relatively short order, flyer plate velocities of 10 km/s (Chhabildas *et al.*, 1990, 1991a, 1992a, 1992, 2003) were increased to 12 km/s (Chhabildas *et al.*, 1993; Chhabildas, 1993) and then to velocities exceeding 16 km/s (Chhabildas, 1995). **Figure 5-5** shows a photo of Lalit Chhabildas holding the third-stage extension of the HVL. As Chhabildas mentions, "We were successful in launching aluminum, titanium, magnesium, and tantalum to velocities exceeding 10 km/s. In particular we were able to launch a 0.5[-]mm[-thick] titanium plate to over 12 km/s."

²⁰ PMMA is the acronym for polymethyl methacrylate.

²¹ TPX is the brand name for polymethylpentene (PMP), a thermoplastic polymer.

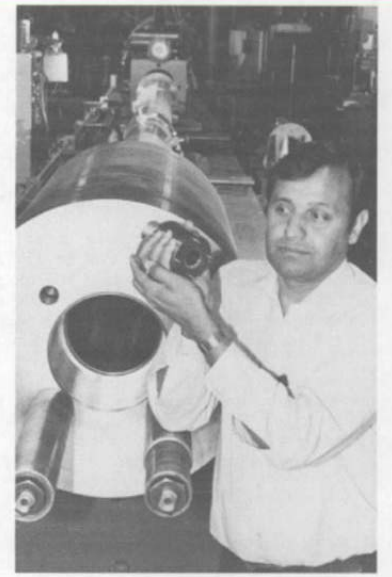


Figure 5-5. Lalit Chhabildas holding the third-stage extension that achieved plate velocities exceeding 10 km/s.

For the first time, the HVL allowed scientific study of highly expanded states of matter that had usually been produced by shock loading and unloading. These thermodynamic states are often referred to as 'warm dense matter,' which is important in a number of astronomical applications and is presently an area of intense scientific interest within the NNSA Stockpile Stewardship Program. Tim Trucano succinctly describes the paradigm shift:

Because of the conjunction of funding, program drivers, research opportunities and talent accumulated in the 1980s, the Sandia shock wave effort pursued every aspect of hypervelocity impact, including: how to achieve it; how to characterize it; how to simulate it, both physically (experimentally) and computationally; and how to assess its consequences. This involved [the] development of experimental capabilities, ... theoretical understanding, ... computational capabilities and specific applications, none of which had really existed prior to the 1980s. As part of this effort, Sandia became closely integrated with a revived national and international community in hypervelocity impact physics.

Early experiments on shock-induced vaporization had been performed with the two-stage gun (Asay *et al.*, 1988; Trucano *et al.*, 1988), but the materials that could be partially vaporized were limited to low melting point ones such as lead and cadmium, and only a small fraction of the material was actually vaporized. The HVL provided an opportunity to extend the suite of engineering and scientific materials for vaporization studies and to probe even full vaporization of zinc impacted at 5 Mbars and released (Brannon and Chhabildas, 1995). Additional examples are given in the references (Wise *et al.*, 1992; Chhabildas *et al.*, 2006).

The HVL also played a role in attracting significant Work for Others funding for shock wave research. As Chhabildas says,

We were asked to evaluate debris shield designs by Johnson Space Center and Marshall Space Flight Center.... DNA even supported a basic research project to investigate the kinetics of shock-induced vaporization in zinc and to determine the role of projectile shape on the evolution of debris generated upon impact on a thin plate.

The studies for the National Aeronautics and Space Administration (NASA) provided the first experimental data for evaluating a debris shield design known as the Whipple Bumper Shields at impact velocities greater than 10 km/s. NASA also initiated HVL experiments to simulate the effects of orbital debris known as space 'junk' (Lawrence, 1990; Chhabildas, 1991b; Ang *et al.*, 1991; Lawrence *et al.*, 1992; Lawrence *et al.*, 1995; Lawrence *et al.*, 2006).

In addition, as a spin-off of the original SDI program, the Theater Missile Defense program originated a multi-year program to use the HVL to evaluate projectile lethality at velocities greater than 10 km/s in hit-to-kill missions (Ang *et al.*, 1992a, 1992b). Other government organizations that operated conventional two-stage guns also pursued assistance from Sandia in upgrading their facilities with this capability, as Chhabildas notes:

This technology was transitioned to [the] Naval Research Laboratory, and AEDC [the U.S. Air Force's] Arnold [Engineering Development Center]... at Cmdr. John Connell, DNA's request. These are large facilities that evaluated lethality effects on space structures.... The impressive

thing ... was we were launching plates for over a millisecond over tens of meters free flight, taking X-ray pictures, and hitting a target less than a meter from its intended trajectory. At AEDC we were able to launch a 50-mm titanium plate [of] 7.6 gm to 9.2 km/s. DNA wanted us to develop a technology to launch chunky projectiles instead of plates. We leveraged this project with an internal LDRD which allowed us to enhance our capabilities to 16 km/s.

The launcher also provided a new experimental capability for benchmarking hydrodynamics code predictions of hypervelocity penetration, impact flash, and debris cloud generation (Ang, 1990; Chhabildas *et al.*, 1993; Chhabildas *et al.*, 1995; Ang *et al.*, 1993, 1996; Chhabildas *et al.*, 2006; Lawrence *et al.*, 2006). The HVL continues to be used for a variety of weapons and other scientific applications to the present time.

5.7 Generalized Fragmentation Theories

In the late 1970s, Grady, Kipp, Schuler, and others developed experimental techniques to characterize the dynamic properties of geological materials. Specific issues concerned fracture strengths under dynamic loading and how such materials, and oil shale in particular, would rubbleize under tensile loading. Grady, Kipp, and Schuler noted that, in explosive experiments such as shown in **Figure 5-6**, fragment sizes varied with distance from the explosive. As Kipp says,

In our department, extensive static and dynamic testing of oil shale samples was pursued, and Dennis Grady and I embarked on a dynamic fragmentation study that would eventually expand to many other materials and a close collaboration over many years.... Not surprising[ly], the fragments nearest the explosive were very fine ...and graded out to much larger ones as the distance from the charge increased. Such a large disparity in size was not going to be good for contemplated in-situ mining... [and] retorting, and work by others was pursued to reduce the rates of loading, and thus reduce the formation of fine [fragment]s.

Since the explosive pressure, and thus the rate of loading (strain rate), depends on peak stress, higher loading strain rates close to the charge meant smaller fragment size, thereby suggesting a strong correlation of fragment size with strain rate.

Analytic models did not exist at that time to describe the relation between strain rate and fragment size. However, from these and other studies, Grady developed a generalized energy- and momentum-based model that effectively predicted the fragment sizes for specifically applied strain rates. The model was based on conserving kinetic energy and momentum in an expanding center of mass system. Since the total energy of the system, such as an expanding shell, is not available for fragmentation, the energy is separated into the center of mass energy and the local kinetic energy the fragments that are created. The local kinetic energy includes the surface energy of newly created fragments and the local kinetic energy of the fragments. This provides a quantitative expression among the fragment size, the surface energy, and the expansion strain rate. Using fundamental material properties

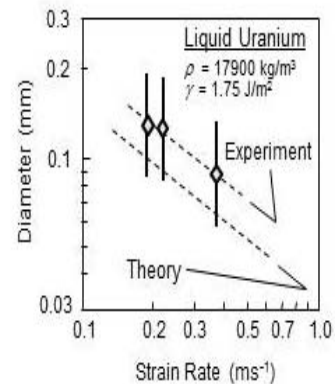


Figure 5-6. Photo of explosively-driven cylinder fragmentation and estimated average fragment size from three tests (Grady, 1982).

such as tensile strength, the rate of expansion, i.e., the strain rate, and the strain to failure, this generalized model was remarkably accurate in predicting average fragment masses in several different applications (Grady, 1981a, 1982a, 1982b; 1990). Grady's fragmentation model was implemented into 2-D and 3-D hydrodynamics codes (Kipp, 1982; Trucano *et al.*, 1980) by coupling the model to output routines from code simulation events. This approach allowed the determination of fragment size in a large number of system applications through computer simulations. An example is shown in Figure 5-6, which compares the average fragment sizes predicted from the model with experimental results.

In addition to experiments using explosives, Grady developed a new experimental technique using expanding metal rings to relate fragment sizes systematically to the applied tensile loading rate (Forrestal, Grady, and Schuler, 1978; Grady *et al.*, 1984). These experiments established the fundamental relation between fragment size and strain rate and accelerated development of the fragmentation model for metals. As Kipp notes,

A natural extension of the strain rate dependence of fragmentation was to metals. The first effort was directed towards ring expansion (done electromagnetically ...[by] Dennis Grady and Dave Benson), and ...develop[ing] models to represent the fragment formation. I set up the equivalent one-dimensional expansion of the ring in WONDY with a uniform velocity gradient to simulate the ring conditions, and randomly inserted initiation points with random initiation thresholds; the fracture model was set up to dissipate the proper energy during stretch, and the results agreed very well with the experimental data – even with large numbers of initiation points, there was only enough energy to bring a few to completion... and obtain the small number of fragments of random sizes observed in experiments. Dennis published a seminal paper on fragmentation that formed the basis of our thinking over the next many years for ductile, brittle, and liquid fragmentation. With the help of Jeff Swegle, the model was implemented into CTH as a monitoring tool.... [T]hat is, when the fragments were defined to have formed, material was not agglomerated into actual fragments; instead, the totals could be summarized in bin-charts, and distributions obtained as a post-processing option. We had found that if we took the average fragment size from the simulations... and imposed an exponential distribution on those averages that the actual distribution from an event could be reasonably represented. It became a powerful tool, and I used it extensively [during] the remainder of my career at Sandia on a wide variety of fragmentation-formation problems, ranging from fine liquid droplets to explosive casing expansion. The dependence on strain rate explained clearly why “pipe-bomb” like explosive devices (characteristic of most fragmenting rounds)... broke into strips, since the axial strain rate was typically much smaller than the circumferential strain rate (large strain rates give smaller average dimensions than small strain rates). Spherical devices, on the other hand, with a centrally-initiated spherical explosive charge inside a thin external metal shell, would lead to nominally square fragments (uniform strain rates in all circumferential directions) – a result confirmed by radiographs.

Grady's fully developed model was published in 1981 (Grady, 1981). Earlier studies provided substantial information for developing the model (Grady and Hollenbach, 1976, 1977; Grady and Kipp, 1979; Grady *et al.*, 1978). Later papers dealt with numerical aspects (Kipp, Grady, and Swegle, 1993; Grady and Kipp, 1995) and were used to validate the analytic model.

Surprisingly, the dynamic fragmentation model was of greater applicability than originally intended. In a specific application of scientific interest, Holian and Grady (1988) used molecular dynamics (MD) simulations to study microscopic fragmentation of condensed matter. In the MD simulations, matter expanded adiabatically, leading to fragmentation into distinct clusters of several atoms. A Lennard-Jones pair potential was used to provide an attractive well for atomic interactions. The simulations established that the homogeneous distribution of cluster masses was an exponential distribution as observed and that the average cluster mass could be reasonably well predicted by the Grady fragmentation model. The

atomistic simulations provided considerable insight into the nature of fragment distributions and also provided reasonably good agreement of average fragment mass with estimates from the fragmentation model.

Grady and his colleagues also used the model to estimate the distribution of galaxies resulting from the ‘Big Bang.’ To make these estimates, they made several simplifying assumptions about gravitational forces and uniformity at the moment of the Big Bang to determine how matter fragmented into galaxies later in time. It was assumed that the universe expanded uniformly. The observed luminosity of galaxies in the universe was used to estimate their effective masses and to compare with the calculations. **Figure 5-7** shows the agreement between the fragmentation theory and the existing distribution of galaxies, assuming that the galactic luminosity is proportional to absolute mass (Brown *et al.*, 1983). The results illustrate the potential of the theory for estimating fragmentation on a very large scale, but not knowing the cause of the effective fracture energy (that is, gravity, dark energy, or dark mass) limits the impact of this comparison.

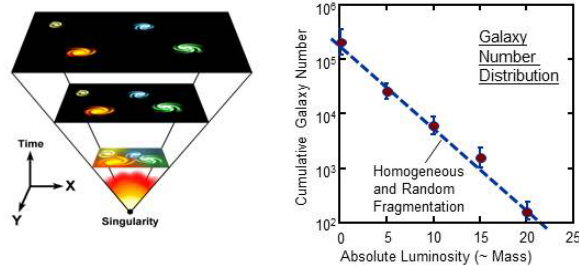


Figure 5-7. Fragmentation on a large scale, i.e., the ‘Big Bang’ (Brown *et al.*, 1983).

An even more impressive example of the fragmentation model’s power was provided at atomic scales. In this case, Grady used the model to estimate the fragments (atoms) created by the collision of 70-MeV nuclei of carbon and silver. The number of different atoms created in the collision, as illustrated in **Figure 5-8**, was measured with a photographic emulsion. A quantitative measure of the atomic

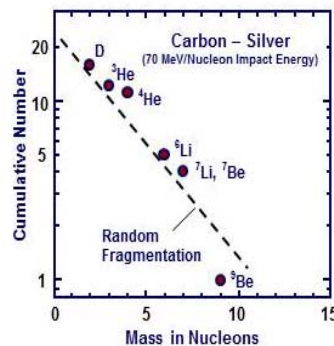
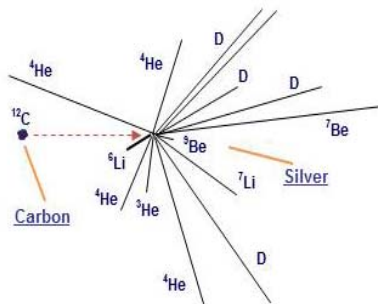


Figure 5-8. Fragmentation in the small, i.e., the cumulative isotopic number distribution for a 70-MeV collision of carbon and silver nuclei.

fragments was obtained by using the nuclear binding energy as the resisting fracture energy. The results shown in the figure illustrate the extremely good agreement with the experimental results (Grady, 2010b). The above examples illustrate the extreme range in scale for applicability of the fragmentation model. These examples and the theoretical molecular dynamics simulations establish the credence of the model for predicting the breakup of oil shale, the destruction of armor, and the distribution of galaxies.

5.8 Laser Windows for Mbar Profile Studies

In the 1980s, a major objective of laser interferometry was to determine near *in-situ* particle velocity profiles for arbitrary loading at impact stresses of 1 Mbar or more. These pressures were dictated by the weapons programs at the other national security laboratories and the scientific goals for studying material properties in the vicinity of shock-induced melting. This required the identification of laser windows that would remain transparent to these stress levels and possess a good impedance match to samples of interest.

The seminal work of Barker and Hollenbach in 1970 that determined the optical properties of fused silica, PMMA, and sapphire was the only available information at the time, albeit limited to low pressures. In the mid 1970s, Larry Lee performed a scoping study of laser window materials through several experiments on sapphire, LiF, and PMMA to explore the peak stresses for transparency (Lee, 1976); in sapphire the peak stresses were about 0.5 Mbar using the powder gun. However, the results were not designed to analyze refractive index changes. In the late 1970s, Chhabildas (Chhabildas and Asay, 1979) demonstrated the use of PMMA windows to about 220 kbars. In the early 1980s, Jack Wise was tasked with investigating several laser window candidates for VISAR use at Mbar pressures. At stresses exceeding several hundred kbars, most candidate window materials are compressed to completely plastic states of stress, making the task of identifying good materials quite challenging. Several materials were explored, including fluids (Wise, 1984). Single crystal sapphire was not a good window because of elastic yielding near 200 kbars. Wise and Chhabildas (1986) discovered that single crystal LiF (<100> orientation) worked well up to 1.2 Mbar, even though the window yielded elastically at a few kbars. They did a careful study of LiF, which illustrated a linear dependence of the refractive index with density to over 1 Mbar. Hayes later showed that a linear dependence ensures that the constant correction found for LiF laser windows can also be used for arbitrary waves (Hayes, 2001).

Furnish and Chhabildas (1999) further found that LiF could be used to attain shock wave stresses of about 1.8 Mbars in LiF, although shock-induced absorption of the laser light produced a time-dependent loss of transparency. The transparency loss results from the closure of the electronic band gap caused by shock temperature increases. This assumption is supported in recent studies by Fratanduono *et al.* (2011), who found that LiF, when compressed by a ramp wave to 8 Mbars, does not lose transparency, apparently because of the lower temperature. Furnish and Chhabildas also found a linear dependence of refractive index for LiF on density. Setchell further demonstrated the linear dependence of the refractive index with density for Z-cut single crystal sapphire below its yield point during shock loading and release (Setchell, 2002). LiF continues to be the most versatile transparent window material for dynamic shock interferometer studies and is usable to pressures of at least 8 Mbars in ramp wave experiments. Similarly, PMMA was also found to be transparent for use as a window material at stresses up to 40 GPa under ramp loading (Anderson *et al.*, 1998).

5.9 Shock-Induced Solid-State Chemistry

Most shock wave research prior to the mid 1980s explicitly addressed mechanical and physical processes occurring during shock compression. In 1979 Lee Davison and Bob Graham published a comprehensive review of the shock wave research being conducted worldwide (Davison and Graham, 1979). Graham noted the attempts to understand chemical processes occurring under shock compression at several laboratories in the Soviet Union. Based on this finding, Graham initiated a broader evaluation of the literature and, as Bruno Morosin states:

He was astonished to find a well-developed scientific effort reporting chemical changes and reactions. This was in contrast to the prevalent opinion of most U[.]S[.] experts,...[who] were in agreement that chemistry could not occur on the time scale of shock compression in normally inert solids.

That finding led to the formation of a major effort at Sandia focused on the topic of shock-induced solid-state chemistry, which was to become known as ‘shock chemistry.’

A core group of researchers was formed with diverse technical backgrounds to address different technical issues related to shock chemistry. This group included Bob Graham for shock and explosive physics, Bruno Morosin for x-ray diffraction and general material properties, Gene Venturini for electron

spin resonance and other magnetic properties, Marty Carr for electron diffraction, Ed Beauchamp for ceramics and sintering effects, and Dave Webb for numerical simulations. Several other Sandians participated based on specific materials and technical issues.

The initial research efforts were concentrated on several ceramics, including titania, alumina, and zirconia, as well as on iron pyrite, to see whether shock modification resulted in reproducible chemical effects. The research was later broadened to other materials. A broad range of technical issues were addressed to identify chemical effects responsible for the many physical and chemical changes observed in these materials during shock compression (Graham *et al.*, 1983). Graham led the effort to understand the interplay between shock compression and chemical reactions. Several Sandia publications on shock chemistry were published beginning in 1984 that covered a decade and involved a large number of investigations.

One of the early goals of the shock chemistry program was to design shock recovery fixtures that produced well defined pressure and temperature states in shocked powder samples to ensure the reproducibility of results (Graham and Webb, 1984, 1986). After perfecting the design of the recovery fixtures, several powders of various materials and mixtures were studied using explosive loading with different-sized fixtures to control loading conditions. The size and configuration of the sample fixture introduced considerable local interactions and temperature variations in the shocked sample. Following shock loading and recovery, the compacted materials were extensively examined for chemical changes (Graham *et al.*, 1983, 1986; Dodson and Boslough, 1990). X-ray diffraction was a major diagnostic used to assess the physical changes, as Morosin states:

The initial three diagnostic tools were conventional x-ray patterns as well as [the] determination of line broadening of diffraction peaks, the intensity and shape of ...[the electron paramagnetic resonance] signatures, and the determination of dislocation densities of transmission electron microscope images. Consistent and reproducible diagnostic results were obtained from the tiny, but carefully... separated, samples unloaded from various locations of the capsule as well as between different loading configurations when merged with values given by Webb's two dimensional numerical calculations for sample pressure and temperature.

A large number of experiments were conducted that systematically addressed several thermo-mechanical and chemical issues. A variety of material powders, including organic, inorganic, and geophysical minerals and particle morphologies, were shock loaded and recovered (Hellman *et al.*, 1984; Graham *et al.*, 1986; Boslough *et al.*, 1986a; Smugeresky *et al.*, 1988; Graham, 1988; Graham, 1989). This was followed by different physical and chemical analyses on the post-shocked powders. Specific investigations included x-ray diffraction to determine morphology (Morosin and Graham, 1984; Morosin *et al.*, 1986), changes in optical properties (Boslough and Venturini, 1986), electron microscopy to determine electronic states (Graham and Carr, 1986; Myers *et al.*, 1986), magnetic effects and microwave loss measurements to address dissolution effects (Venturini *et al.*, 1986; Venturini *et al.*, 1988; Cygan *et al.*, 1989), nuclear magnetic resonance spectroscopy (Cygan and Boslough, 1994), and exothermic energy release (Hammeter *et al.*, 1984). These extensive and coordinated experiments and diagnostics were critical and addressed many issues related to shock-induced chemistry, including chemical reactivity during shock loading (Setchell, 1986; Morosin *et al.*, 1988), the ability to synthesize metal powders, such as nickel aluminum alloys (Hammeter *et al.*, 1988), and the synthesis of specific solid phases (Morosin, 1984, 1988; Graham, 1985; Graham *et al.*, 1985; Boslough and Graham, 1985; Venturini *et al.*, 1986; Simonson *et al.*, 1986; Horie *et al.*, 1986a, 1986b; Graham and Morosin, 1988; Graham, 1988; Boslough, 1989, 1991, 1992; Morosin *et al.*, 1992). Numerical simulations were also integrated with the experimental research to define thermodynamic states and mesoscale loading processes responsible for chemical changes. Specific examples include the modeling of Ni-Al powders (Taylor *et al.*, 1988) and the modeling of chemical processes (Horie and Kipp, 1988). Both explosive loading and projectile

impact were used for these experiments. **Figure 5-9** shows a photo of Bob Graham and Bruno Morosin preparing for a gas gun experiment.

In his recollections, Morosin captures the rapid progress in the program over the following years:

[By 1987] this core group had completed and presented eight short papers at the ... [Fifth Biennial Meeting of the American Physical Society Topical Group on Shock Compression of Condensed Matter], dominating the shock chemistry area, and these were subsequent[ly] included in the ...Proceedings. These [papers] included shock-induced reactions of specific inorganic compounds, defect formation and resulting effects, such as enhanced catalytic action, the initial evaluation [of] line broadening of x-ray diffraction patterns, and enhanced sintering characteristics, all resulting from shock loading of selected single phase inorganic compounds as well as a more complicated clay, nontronite, with its implication for evolution of planetary surfaces, namely that of Mars.... Bob Graham... was able to conceptually arrive at his chemical reaction model for shock chemistry, a process involving turbulent local mixing, possible massive defect generation, [and] a short, rapid transit pulsed temperature excursion [–] all caused by the transit pressure pulse.



Figure 5-9. Bob Graham (left) and Bruno Morosin ready a gas gun experiment.

The extensive, careful, and well-coordinated set of experiments conducted over a decade through the shock chemistry program contributed significantly to the understanding of thermomechanical, physical, and chemical processes occurring during the shock compression of organic and inorganic powders. Morosin emphasizes that

One must recall that this was the era of President Reagan, the perceived Soviet threat[,] and the ‘Star Wars’ initiative. Out of the shock chemistry effort came the [sic: concept] of a radically different, long life, inert battery which could be activated upon impact for possible fusing capabilities, essentially, a shock wave-activated battery.

5.10 Piezoelectric Polymers for the Bauer Shock Gauge

Electrical effects in shocked polymers and elastomers were identified in early shock wave experiments (Benedick *et al.*, 1965; Graham, 1979b, 1980), resulting in the use of polymers as shock wave detectors (Champion and Benedick, 1968). Later, in the 1970s, a polymer, polyvinylidene difluoride, was found to be piezoelectric, first by Japanese researchers and then by a French scientist, Francois Bauer. As a graduate student, Bauer had conducted careful and systematic research on PVDF at the Franco-Allemand Institut de Recherches de Saint-Louis and developed stringent material controls and experimental configurations for its use as a shock wave gauge. This novel work became known to Sandia scientists at an international conference. As Ray Reed recalls:

At an American Physical Society symposium in Albuquerque, we first met a French scientist, Francois Bauer, who presented a paper reporting on his current thesis work with a new Japanese-discovered thin film piezoelectric polymer material, polyvinylidene di-fluoride, PVDF. Bauer was familiar with the work of Graham and Benedick on X-cut crystalline Quartz. Benedick spoke French. Bauer discussed his thesis claim of the reproducible dynamic properties by his special processing of PVDF. He convinced a skeptical Graham to test the claimed (but implausible) superior material properties using Sandia’s precision gas gun facility. Graham’s gas gun test results for PVDF were convincing. Graham convinced (equally-skeptical) me, as

well. From that encounter, formal symbiotic relations developed between Sandia, Ktech, and the Franco-Allemand (French-German) Institut de Recherches de Saint-Louis, France (ISL).

This early work set the standard for accurate shock wave measurements using the PVDF stress gauge and was adopted at Sandia and elsewhere as the standardized ‘Bauer PVDF shock sensor’ (Lee *et al.*, 1986; Graham, 1988; Reed and Greenwoll, 1989; Bauer *et al.*, 1992).

The PVDF shock sensor is a 25-micron-thick piezoelectric polymer with a well characterized shock response for loading under uniaxial shock compression to nominal stress levels to ~ 250 kbars, although it has been used to higher stresses. The charge release during shock loading is accurately measured in the ‘current mode’ with a precision, high-frequency, current-viewing resistor shunted across the electrodes. The time derivative of piezoelectric charge release is measured as a voltage drop across a precisely known resistance, providing a direct measure of the rate of stress loading induced in the gauge. Numerical integration of this measurement over time gives the charge-time profile; the as-recorded stress rate measurement provides unusual sensitivity to shock reflections through the 25-micron-thick sensor and electrical insulation layers. The charge-time profile is related to stress-time from the characterized charge-stress relationship; numerical integration then gives velocity-time and displacement-time profiles.

When used in the current mode, the signal produced is proportional to the differential particle velocity across the gauge, or the rate of change of velocity with time (the acceleration). Since particle acceleration is a direct quantity in the differential momentum equation for wave motion, use of the gauge enhances the accuracy of obtaining stress-strain relations for loading by a stress wave. Integration of the acceleration profile provides the particle velocity profile normally measured in interferometer experiments. The ability to measure acceleration directly is a major advance for shock wave instrumentation.

Several workshops were held from 1990-1995 to present critical findings on the performance of this sensor. The gauge has found use in a variety of applications, including measurement of radiation-induced loading (Lee *et al.*, 1990) and study of energetic detonation (Moore *et al.*, 1989; Moore and Graham, 1990). PVDF-based accelerometers have also been characterized, with the most thorough work believed to be by Togami, Forrestal, *et al.* in the mid 1990s.

The PVDF gauge has found significant use in a variety of applications, including characterization of radiation loading and the non-energetic response of energetic material response (Graham *et al.*, 1988; Lee *et al.*, 1986, 1988, 1990, 1992; Bauer *et al.*, 1992; Lee, 1990, 1992). Because of phase changes that are prevalent in polymers at high stresses, PVDF is mostly useful for shock stresses of less than 200 kbars. It has proven valuable in several applications, particularly the design of materials for weapons applications.

5.11 High Fidelity Ferroelectric Models

Fundamental understanding of shock-induced ferroelectric response began with the work of Neilson and Benedick in the 1950s and 1960s and rapidly progressed over the next two decades. A better fundamental understanding ensued, and the results were integrated into models for use in simulations. Key researchers involved in this long and extensive experimental effort included Bill Benedick, Bob Graham, Peter Lysne, Frank Neilson, and Bob Setchell, although many others also participated.

To bring the large amount of experimental and theoretical data together into practical models for many components, Steve Montgomery was hired in 1975 by an exploratory firing systems development group to continue work on the analysis of explosively-driven ferroelectric current sources. Montgomery spent considerable effort in quickly learning shock wave physics and in identifying the key issues to be resolved for component applications. As he recalls,

While waiting on my security clearance, I was introduced to the pioneering work conducted at Sandia on shock compression of dielectrics by reading reports and review articles. I also became familiar with the basics of shock wave compression by reading review articles and a set of class notes prepared by Dennis Hayes.

Montgomery further recalls some of the early developments at the time:

Frank [Neilson] had proposed using the displacement current generated by rapid changes of electric polarization in materials under shock compression as a power source a few years prior to 1960. Research groups at Sandia conducted extensive experimental studies on a number of different materials, using explosive loading and gas guns, to study this idea in the following decade. For example, the nonlinear piezoelectric response in ...[X-cut] quartz crystals was used as a stress gauge for materials loaded in shock compression, and the depolarization of polycrystalline ferroelectric ceramic under shock compression was exploited to develop a variety of shock wave activated power sources. Applied research, with the goal of improving understanding of material polarization changes in ferroelectric ceramic, continued with Pete Lysne conducting experimental studies and developing simple models for electrical response, and Pete Chen developing more detailed coupled electromechanical constitutive models that could be used in numerical simulations. I was asked to work with Lysne and Chen to gain an understanding of what they were doing and to develop application models. Chen and Lee Davison had developed a one-dimensional response model for coupled mechanical and electrical response when the direction of material polarization and material motion ...[was] identical. This special configuration was called axial mode operation. Jeff Lawrence and Davison built a numerical model for this special case in the one-dimensional wave propagation code WONDY. My group was interested in what was called the normal mode configuration where the direction of the material polarization in the material was perpendicular to the direction of material motion. I converted the axial mode response model in WONDY to a normal mode response model and then applied the model to develop a complex device of interest to my group. The device relied on ... a previously established explosive loading device to generate the desired shock compression for depolarization of ferroelectric ceramic. The culmination of this work was an ‘admiral’s test’ of the device that was first tested with our vice-president in attendance. To my great relief (and the three levels of management between me and the vice-president), the electric output from the device was reasonably close to the result I predicted, and development engineers took over the project. After the demonstration of the ferroelectric device I was asked to take primary responsibility for the firing system to be used in the Pershing II earth penetrator.

However, when this project was cancelled due to an international arms limitation agreement, Montgomery became involved in other Sandia activities.

Over the next few years, Montgomery was engaged in various shock application problems, particularly through a short stint in Walt Herrmann’s Solid Dynamics Research group. However, he returned to a focus on ferroelectrics in the mid-to-late 1980s. As he further recalls,

I returned to the study of polarization changes due to shock compression in ferroelectric ceramics in 1986. The groups at Sandia developing neutron generators were managed by Dennis Hayes, and he wanted to supplement the prototype testing approach with the use of simulations to understand issues encountered during development. At the time, a systematic study of mechanical and dielectric properties of the ferroelectric ceramic materials used [in the] ferroelectric current generator did not exist. Chhabildas initiated a study of [simultaneous electrical and mechanical] ceramic response under shock compression. I investigated methods to permit multi-dimensional simulations of the components. A result of my investigation was the development of a simulation framework realized in the code SUBWAY. I developed SUBWAY in

collaboration with Pat Chavez....Computational capability was advancing and three-dimensional simulations were now feasible with the availability of CDC,... [Cray],... [Digital Equipment Corporation], and ...[Sun Microsystems, Inc.] systems at Sandia. I decided to develop an integrated three-dimensional finite element framework for SUBWAY and teamed with Lee Taylor to use the explicit finite element code PRONTO to replace the function of TOODY. Early three-dimensional simulations of the device provided mixed results. Some aspects, particularly the ability to simulate the electric field in the ceramic, were well received by the development group. However, while the electric output from the simulations was similar to [the] measured output, there was room for improvement. My interest in the ferroelectric ceramic work and application modeling motivated a transfer to the neutron generator group in the summer of 1992. Along with supporting several neutron generator development programs, I coordinated work to improve simulation capability by developing a fundamental understanding of ferroelectric ceramics and encapsulation materials under conditions of operation.

Until his retirement in 2011, Montgomery improved and refined the ferroelectric model to the point where a variety of complex, but controlled, experiments could be accurately modeled. These included quasi-static experiments by Dave Zeuch to understand the dependence of domain switching on crystal orientation, gun experiments by Bob Setchell, Lalit Chhabildas, Mike Furnish, and Mark Anderson to understand coupled electromechanical response, controlled 2-D and 3-D experiments by Bob Graham and Bob Setchell to understand effects of complex loading, and ramp loading experiments by Jack Wise to further examine loading path dependence.

The results of this extensive experimental work provided the basic data for improvement of the material response models developed in collaboration among Montgomery, Rebecca Brannon, and Joshua Robbins. The result of this all-encompassing program was that a compressive ferroelectric model was generated that reliably predicted component response for use in extremely large computer simulations. As Montgomery notes, “*I was routinely conducting simulations ...on massively parallel computers using around 5 million elements, and several component simulations used element numbers ranging in the ...[tens] of millions.*” The noteworthy accomplishments of this group of researchers, dating back to the 1950s, emphatically demonstrate the culmination of extensive experimental testing, fundamental model development validated through modern 1-D to 3-D experiments, and the advent of massively parallel computing, which is yet another example of Lundergan’s strategic vision coming to fruition.

5.12 CTH - a Robust 3-D Hydrodynamics Code

CTH became a principal hydrodynamics code for complex shock wave problems at Sandia and elsewhere in the early 1990s. Development of the code benefitted from continuing management support over several years and the exceptional technical expertise of the staff who worked on it. CTH was the brain child of Sam Thompson who had a long history of developing codes for shock wave applications. Thompson joined Sandia in 1966; his early work involved development of analytic equations of state with Dave McCloskey. Toward the end of the 1960s, he joined Walt Hermann’s growing Solid Dynamics Research Department. The first code for shock wave problems that he developed was the 1-D Lagrangian CHARTD (1969). CHARTD was used for a variety of shock wave and radiation problems during this period. In the 1970s, Thompson made several improvements to CHARTD and also developed 2-D Eulerian hydrodynamics codes, resulting in several versions of CSQ (Thompson, 1973, 1974, 1976; Thompson and Lauson, 1972, 1974; 1977). In 1980 Thompson was promoted to supervisor of the Thermal Hydraulic Analysis Division. This left a void in the development of multi-dimensional hydrodynamics codes in Herrmann’s group, especially for a robust 3-D computer code to handle the increasing demand for realistic simulations for weapons problems and other scientific applications. In addition, support for the 2-D code CSQ waned, and there was no serious effort on 3-D code development.

In 1982 Orval Jones was promoted to the Vice President responsible for field engineering, weapons analysis, development testing, and fabrication laboratories. Walt Herrmann was promoted to replace him as Director of Engineering Sciences. In turn, Herrmann promoted Lee Davison, then a supervisor in the Physics of Solids Research Department, to fill his previous position as manager of the Solid Dynamics Research Department. One of Davison's insightful decisions was to entice Sam Thompson back into the Solid Dynamics Research Department to manage code development efforts. As Tim Trucano recalls in his recollections,

Lee convinced Sam Thompson to leave the nuclear reactor safety code development area... and return to revive CSQ and begin to develop a serious 3-D code capability [for] the code that would be called CTH. When Sam returned he brought ...Mike McGlaun back as well. I ended up working very closely with Sam and Mike during the rest of the decade because it was their computational tools that enabled me to pursue most of the computational work.

Mike McGlaun, who had originally worked on cratering studies for Al Chabai, remembers his new assignment under Thompson: “I came back to the shock physics group in 1986 to design and build CTH. We decided to develop a tightly integrated suite that modeled three-dimensional, two-dimensional, and one-dimensional problems.”

In late 1988, Davison changed jobs from the Engineering Dynamics Department to the Engineering Sciences Department, also under Herrmann. Dennis Hayes became manager of the Solid Dynamics Research Department and, during his one-year tenure, he promoted a robust code development effort. As Hayes recalls,

In 1988-89 I moved to the shock physics department. An important change was taking place in the computing world. Parallel computing had arrived and we staffed it to implement shock codes and made good progress. Another related effort was to transform CTH from a vertically integrated project to a full-fledged software engineering project, with benefits that exist to this day.

Rapid progress was made on 3-D CTH development under Hayes' leadership. In addition to CTH, he implemented a new code development project with James Peery, initially as project leader, to build a 3-D magnetohydrodynamics (MHD) code in partnership with the Pulsed Power Sciences Center. This code was to be the workhorse for analyzing high temperature plasma states being studied in that Center. The code was initially called RHALE for Robust Hydrodynamic Arbitrary Lagrangian Eulerian, but it was later changed to ALEGRA for Arbitrary Lagrangian Eulerian for General Research Applications. As it turned out, in the 2000s this code had a major influence on high energy density research being conducted on Z and it greatly influenced the transformational effect of shock wave research being developed on that pulsed power accelerator.

After Hayes left the Solid Dynamics Research Department in 1989 for an assignment in Washington, Jim Asay was promoted to manager of the Solid Dynamics Research Department. Phil Stanton was promoted to replace Asay as manager of the experimental division. Sam Thompson also left the Department at that time, returning to research on reactor safety, and Mike McGlaun was promoted to supervisor of the code development branch. Paul Yarrington managed the code applications branch in the Department, which was primarily engaged in modeling geological materials. At the time, the Defense Advanced Research Projects Agency (DARPA) had just initiated a multi-lab competition to develop a robust 3-D hydrodynamics code intended for use by all DoD laboratories and contractors to maintain the best modeling capability for DoD problems and would provide additional external support. The management team of the Solid Dynamics Research Department made a strategic decision to participate in this competition with a team of nine technical staff members led by Steve Rottler. As McGlaun vividly recalls,

The Department of Defense (DoD) wanted improved shock modeling capability for their laboratories and contractors. DARPA invested in shock codes at LANL, LLNL and SNL. DARPA funded us [Sandia] to send CTH to key DoD users... [and to] add features they needed. The users were very happy with CTH.... In a year, DARPA was heavily investing in CTH. DARPA was a great partner who helped mature CTH and helped add features important to the DoD. Our external user community exploded (no pun intended). By 1995, CTH had over 130 external users. DARPA's support was politically important inside Sandia because their funding augmented NNSA funding and they had a reputation for developing world-class technology.

Trucano also remembers:

I believe Sam's first hire upon his return was Steve Rottler, who quickly developed an important role on the LANL-Sandia SDI project.... [He] became the first formal PI for CTH late in the decade, leading CTH to be the code selected by DARPA to become a new generation hydrocode for DoD; and [he] eventually became the Chief Engineer (VP) for nuclear weapons at Sandia [and, in 2013[,] the Vice President of Sandia's California site].

As a result of the dedicated and talented efforts of the CTH team, CTH was selected as the code of choice for DoD applications and has enjoyed considerable external funding, continuing to the present. After Rottler's promotion to first-level management in 1990, Gene Hertel took over the project with management skills that have elevated the code to its continuing prominent status in the community. As Hertel states,

CTH is the most widely used hydrocode in the United States, and has been the most used code on the Department of Defense high performance computers as long as they have been keeping track. CTH is used extensively at Sandia and Los Alamos to simulate a wide variety of phenomena.

5.13 Turret Explosion on Board the USS Iowa²²

A large explosion occurred in the middle gun turret of the USS Iowa Battleship on April 19, 1989, resulting in major damage to the ship and considerable loss of life (Schwoebel, 1999). The USS Iowa was the fourth battleship in the United States Navy to be named for the 29th State and the only ship of her class to serve in the Atlantic Ocean during World War II. The explosion occurred in the center gun room, killing 47 crewmen and severely damaging the gun turret. **Figure 5-10** shows a photograph of the aftermath of the explosion captured by an on-board ship camera.

Two major investigations of the accident were undertaken, one by the Navy and the other by the General Accounting Office (GAO), aided by Sandia. These investigations produced conflicting conclusions. The Navy's investigation concluded that one of the gun crew members had deliberately caused it. Sandia's investigation focused on determining a probable cause for the explosion and concluded that it occurred when the gun was being



Figure 5-10. Turret explosion on board the USS Iowa Battleship. The center gun on the near turret is shown lying on the deck after the accident.

²² M. R. Baer, Sandia National Laboratories, contributed most of the material in this section (private communication, 2013).

loaded. A team of researchers led by Director Dick Schwoebel worked on this investigation full time for two years. In 1999, Schwoebel published a book, titled *Explosion Aboard the Iowa*, on the Sandia investigation of the explosion (Schwoebel, 1999).

Figure 5-11 shows a cross-section schematic of the 16-inch gun that exploded, which contained five powder bags of nitrocellulose propellant that were accidentally over-rammed into its breech. Each powder bag consisted of uniform layers of cylindrical perforated propellant pellets and a ‘trim’ layer that was randomly placed at the end of each bag to equalize the bag’s weight. Several computer analyses and small-scale tests were conducted to identify possible causes for the accident. Karl Schuler performed analytic analyses that showed the powder bags had been rammed 24 inches, three inches further than the 21 inches claimed by the Navy. Furthermore, the Sandia study concluded that the propellant train had been subjected to an over-rammed impact load. Ken Gwinn conducted structural analysis of the impacted propellant train, which suggested that high stress localization occurred for the hypothesized loading; this scenario led to breakage of the propellant pellets in a direction transverse to the pellet perforations. To validate this possibility, Paul Cooper conducted 450 drop tests using small bags of D-846 powder. The tests demonstrated that the trim layer would often ignite when compressed at high speed. His investigations confirmed that, for over-rammed impact loads, the propellant pellets experienced brittle fracture and energy localization that generated hot fragments sufficient to cause ignition.

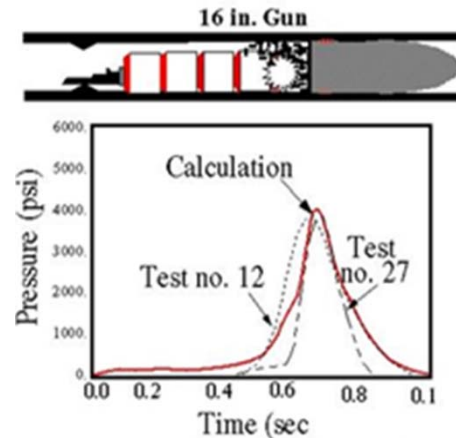


Figure 5-11. The top schematic illustrates the location of five powder bags accidentally over-rammed into the breech. Each bag consists of uniform layers of propellant pellets and a ‘trim’ layer to equalize weight. Interior ballistic calculations established that the explosion likely initiated at the trim layer in the forward bag.

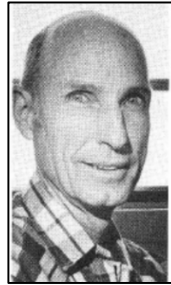
Mel Baer performed interior ballistics analyses of the subsequent combustion, which are also illustrated in Figure 5-11. At the time that these calculations were initiated, CTH was not configured to treat multiphase flow – that is, a mixture of propellant gas and unreacted material occurring in the breech during ignition. Baer implemented this capability in CTH to model the combustion process and examined the resulting dynamic loading on individual propellant pellets. His analysis also confirmed that the trim layer was the most vulnerable component and established that the explosion most likely occurred in the vicinity of the first (most forward) powder bag. Later tests at full-scale in open breech guns confirmed that, under these conditions, low-velocity rammed loading could, indeed, trigger a violent combustion event.

Based on these initial results, the Navy asked Sandia to participate in full-scale tests with an open-breech gun. These tests established conclusively that the propellant train could be ignited by an over-rammed impact. In July 1991, the team of Sandia investigators presented their final findings to Navy admirals; shortly thereafter the Navy apologized to the families of the crewmen initially implicated in the Navy investigation. However, some skeptics have still not fully accepted Sandia’s conclusions, even though the original Navy scenario has long been disproved and many have now accepted Sandia’s assessment as the most likely cause for the accident.

5.14 People and Places of the 1980s



**Tim Trucano, “end of tracks”,
waiting for the train in Russia**



**Lynn Barker, Distinguished
Member of Technical Staff**



**Carl Konrad (left) and Dave Cox at
powder gun chamber**



**John Martinez evicting unapproved
visitor to poster session at STAR**



**Bob Graham, Distinguished
Member of Technical Staff**



**Jim Asay, Department
Manager**



**Lalit Chhabildas (right), 1983
Aeroballistics Range Association
meeting**



**Mel Baer, Distinguished
Member of Technical
Staff**



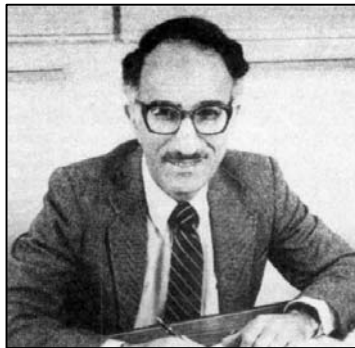
**Pete Lysne,
Distinguished Member
of Technical Staff**



**Jace Nunziato (left) and Sam
Thompson (upper right),
Supervisor**



**Clint Hall (left) explains second stage of the
3-stage railgun**



**George Samara, election to
National Academy of Engineering**



**Karl Schuler (left) and Paul Cooper
preparing a drop test for Iowa
explosion**



Anita Renlund (left), Wayne Trott (3rd from left) at Explosive Components Facility



Al Chabai, Supervisor



Bob Graham (left) and Ron Jacobson



Herb Sutherland,
Distinguished Member
of Technical Staff



Dennis Grady at STAR, American
Physical Society Fellow



Doug Bloomquist,
Supervisor



Darrell Munson,
Distinguished Member
of Technical Staff



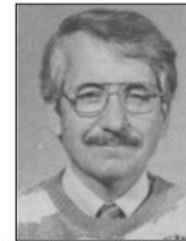
Larry Bertholf,
Department Manager



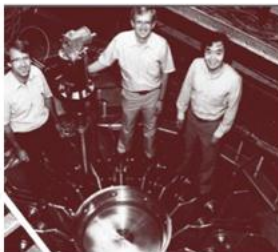
Karl Schuler Distinguished
Member of Technical Staff



Orval Jones, Vice-
President



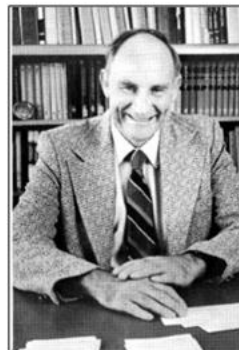
Doug Drunheller,
Distinguished Member
of Technical Staff



Keith Matzen, Rick Spielman,
Warren Hsing at Proto II



Ed Basis, Director



Walt Herrmann, Director



Ray Reed, Distinguished
Member of Technical
Staff



Jim Kennedy, 1980

Chapter VII

The 2000s – A New Millennium

Chapter VII

The 2000s – A New Millennium

7.1 Background

After the turn of the century, dramatic changes occurred in experimental and theoretical shock wave research at Sandia. In the 1950s and 1960s, computational capabilities to design and interpret shock wave experiments were extremely limited. Use of slide rules and small desktop calculators were common. Dynamic phenomenological models to describe the shock compression of complex materials such as composites and porous materials were just beginning to be developed. In addition, instruments to measure the fine details of shock compression and dynamic material response were limited. During those decades, the innovation and intuition of experimentalists and modelers were crucial in advancing shock wave research in spite of these constraints. In the 1960s, the pioneering developments in loading and diagnostic technology were instrumental in advancing knowledge in the later decades. This was especially true for high pressure applications until the mid 1970s, when time-resolved gauges were developed for routine use. At Sandia, Bob Graham and his team concentrated on developing the piezoelectric gauge, which is also known as the quartz gauge (Graham, 1961a, 1961b; Neilson *et al.*, 1962, 1975; Graham and Ingram, 1968; Graham and Reed, 1978). Meanwhile, Lynn Barker and his team concentrated on developing optical interferometric gauges and, in particular, the Velocity Interferometer System for Any Reflector (or VISAR), which grew out of a wide-angle version of the Michelson interferometer (Barker and Hollenbach, 1964, 1965, 1972; Barker, 2000). Considerable progress was made in understanding dynamic compression processes and, amongst researchers, there was an underlying excitement in developing new capabilities and solving complex dynamic materials problems using the new gauges.

By 1990 many early shock wave researchers who had made seminal contributions had left Sandia or transferred to other organizations not directly involved in that research. Of those remaining, a large number retired in the 1990s and early 2000s; only a few now remain from the early days. Some who left in the 1990s and later include: Lynn Barker and Jace Nunziato, 1990; Peter Chen, Lee Davison, Walt Herrmann, and Orval Jones, 1993; Al Chabai, Charlie Daniel, Bob May, Ray Reed, and Sam Thompson, 1994; Gerry Kerley, 1995; Dennis Grady, Bob Graham, Dennis Hayes, Peter Lysne, Karl Schuler, Carl Smith, Phil Stanton, and Al Stevens, 1996; Barry Butcher and Bruno Morosin, 1997; Dennis Mitchell and Steve Passman, 1999; Dick Rohde, 2000; Jim Asay, 2002; Darrell Munson and Jeff Swegle, 2003; Doug Drumheller and Larry Pope, 2004; Jeff Lawrence and Herb Sutherland, 2005; Bob Hardy and Mike McGlaun, 2006; Lalit Chhabildas, 2007; Bob Setchell, 2008; Mel Baer and Wayne Trott, 2010; Marlin Kipp, Steve Montgomery, and Paul Yarrington, 2011. The loss of these key researchers left voids in certain technical areas, but Sandia had the foresight to hire extremely talented people in the late 1990s and early 2000s, thereby sustaining Sandia's reputation as a premier shock wave research organization.

The new researchers who joined in the late 1990s and later included Scott Alexander, Justin Brown, Kyle Cochrane, Jean-Paul Davis, Mike Desjarlais, Dan Dolan, Heath Hanshaw, Marcus Knudson, Rudy Magyar, Thomas Mattsson, Seth Root, Chris Seagle, Luke Shulenburg, and Tracy Vogler. Each of these highly-qualified individuals brought new skills and approaches that have resulted in a richly diverse shock wave program.

Personnel losses had an adverse effect on maintenance and operation of the shock wave facilities constructed in the previous decades. With Bob Graham's retirement in 1996, Bob Setchell was the remaining experimentalist from George Samara's original department involved in shock wave research in

the late 1990s. Setchell continued to use the Helium Gun Facility built by Graham in the 1960s for research on a variety of topics, including the refractive index of sapphire under shock compression (Setchell, 2002), the mechanical, shock-induced depoling properties, and the microstructural effects of shock-compressed ferroelectrics (Setchell *et al.*, 2000; Setchell, 2003, 2005, 2006; Setchell *et al.*, 2007). Setchell's work in the 2000s led to refinements in the material models for FE ceramics during this period (Montgomery *et al.*, 2002, 2004, 2008). After Setchell retired in 2008, the Helium Gun Facility discontinued operations for all practical purposes and ownership was transferred to the STAR facility.

Mel Baer and Wayne Trott had established a coordinated research program to investigate mesoscale mechanisms of shock initiation and growth in energetic materials using a Line VISAR. When they retired in 2010, this innovative research effort suffered a serious loss but is being continued by Marcia Cooper and other researchers. Today, energetic material research continues at the Explosive Components Facility (ECF), and to some extent at STAR, with an increased focus on weapons applications.

In spite of the decision to dismantle STAR after a last scheduled experiment in May 1994, the facility was resurrected in the late 1990s through the tremendous dedicated efforts of several individuals, mainly Lalit Chhabildas and Bill Reinhart. STAR continues to operate today under Reinhart's supervision, with assistance from John Martinez and Tom Thornhill. Many new techniques and technologies continued to be developed once the facility was reopened. In particular, techniques were developed to launch particles on the three-stage gun to over 19 km/s (Thornhill, 2006). A diverse range of novel techniques continues to be developed at the facility, including multi-dimensional experiments to validate computer codes (Chhabildas *et al.*, 2000, 2003, 2006), the effects of microstructure on spallation (Chhabildas *et al.*, 2002; Vogler and Asay, 2004; Furnish and Reinhart, 2006; Furnish *et al.*, 2007; Vogler *et al.*, 2008b), and pressure-shear measurement techniques (Vogler *et al.*, 2012). Other experimental studies at STAR include the evaluation of 2-D effects of shock loading (Reinhart *et al.*, 2002), well-controlled expanding tube fragmentation (Vogler *et al.*, 2003), hypervelocity penetration (Reinhart *et al.*, 2004), dynamic response of concrete (Hall *et al.*, 1999), shock-induced vaporization (Chhabildas *et al.*, 2006), shock compaction of ceramics, mixtures, and powders (Vogler *et al.*, 2003, 2007a, 2007b), high-pressure material strength of glass (Alexander *et al.*, 2008), phase transitions and strength in ceramics (Reinhart and Chhabildas, 2003; Vogler *et al.*, 2004b, 2006; Vogler and Chhabildas, 2006), and shock kill of biological agents (Vogler *et al.*, 2005). Time-resolved spectroscopy was also introduced over a spectral range from visible to infrared to the terahertz regime to monitor hypervelocity impact-generated debris (Reinhart *et al.*, 2004; Thornhill *et al.*, 2006; Wanke *et al.*, 2007). Management of STAR continued under Lalit Chhabildas until 2007, then under Clint Hall until 2010, and is at present under Gordon Leifeste. As of this writing, the facility is well funded and continues to produce valuable data for a variety of internal and external organizations.

The most significant change in experimental shock physics at Sandia occurred in the late 1990s with the development of the magnetic drive on Z.²³ This fortuitous event happened through a combination of circumstances. These included (i) the NNSA Stockpile Stewardship Program initiated through the Defense Authorization Act of 1994, which provided a focus on shock wave research at the extremes of pressure and temperature, (ii) the breakthrough in z-pinch technology by Tom Sanford in 1995 (Sanford *et al.*, 1995; Sweeney, 2002), which increased the power output of plasma sources by several orders of

²³ Z is a powerful pulsed power accelerator at Sandia that began its life in October 1996 by producing intense x rays from imploding z pinches following a three-month modification of the then 11-year-old PBFA II (Particle Beam Fusion Accelerator II). Since June 1997, Z has been used for experimental studies in shock physics, weapons effects, radiation transport, inertial confinement fusion, dynamic materials, and the structure and evolution of planets and astrophysical objects. At the end of July 2006, Z was taken off line for a complete refurbishment to replace outdated components, enhance reliability and precision, allow pulse-shape flexibility, increase diagnostic access, and improve high-energy performance. The first shot following this extensive refurbishment was in September 2007.

magnitude, (iii) the foresight of the Pulsed Power Sciences Center management in exploring (with associated risks) new opportunities for materials research using Z, (iv) the early strategic decisions to develop shock wave capabilities critical to the SSP, (v) the coalescence of several experienced individuals in shock physics and pulsed power who stuck through early struggles to find a reliable and accurate source for producing shock and ramp waves with the Z machine, and (vi) the hiring of highly-qualified university graduates who brought new ideas and enthusiasm to the budding shock wave research program in the early 2000s. Without the coalescence of these crucial events, the present program on Z, which is a truly premier capability, would not be a reality.

Another key development was the significant effort to implement MPP at Sandia that began in the 1990s and grew rapidly in the early 2000s through the Advanced Simulation and Computing Campaign. The 3-D MHD code ALEGRA (Haill *et al.*, 2003) benefitted directly from this development and initially focused on pulsed power and z-pinch physics applications. The extremely large problems that could be solved with MPP computers also enabled *ab initio* quantum molecular dynamics (QMD) EOS calculations, initially for low molecular weight elements and then for a broader class of materials. A highly talented theoretician, Mike Desjarlais, and later a team of theoreticians, produced a focal point for a unique capability in the Pulsed Power Sciences Center that allowed integration of QMD theories with the highly accurate shock wave data being obtained on Z by Marcus Knudson, Clint Hall, Dan Dolan, Seth Root, and others. This combined team of theoretical and experimental researchers made a series of breakthroughs in generating accurate EOS data that are still inaccessible on other experimental platforms. The combination of experiment, theory, and computation under a single (second level in management structure) department represented a return to the unified approach advocated by Don Lundergan and implemented by Walt Herrmann in 1966 but which was lost in the restructuring of the early 1990s.

With Asay's retirement in 2002, the materials research at Z continued under the leadership of Chris Deeney, and STAR activities were managed under the Senior Manager, Dillon McDaniel. Lalit Chhabildas was promoted to lead the STAR activities in 2003. After his retirement in 2007 and Deeney's transition to a Washington assignment in 2006, Clint Hall was promoted to department manager. He combined the two shock wave activities at Z and STAR into a single department and initiated several innovative and strategic directions that continue to influence shock wave research at Sandia today. Materials research at Z and STAR was most recently, until mid 2013, under Mark Herrmann, the senior manager of the High Energy Density Science Department, with Dawn Flicker managing the experimental dynamic materials programs on Z from 2011 to 2013, Thomas Mattsson managing the theoretical studies from 2011 to present (with Dawn serving in that position from 2008 to 2011), and Gordon Leifeste managing STAR and Veloce, beginning in 2011. Changes in the management structure of the Pulsed Power Sciences Center in mid 2013 have produced an even stronger emphasis on materials research in the Center, with a new HED²⁴ Material Physics Department being created in June 2013 and Dawn Flicker being promoted to senior manager of that new department in July 2013²⁵.

The general approach to how Sandia shock wave research is conducted differs significantly now from the early days. Early research using explosives and gun launchers was relatively inexpensive (a few thousand dollars per shot in today's dollars) and experiments could be conducted within a few days of being defined. Typically, only a few people, in addition to the researcher, were involved in an experiment. Although this traditional approach continues at ECF and STAR, and at the Veloce facility, which will be discussed later, the bulk of basic shock wave research is now performed on Z. The Z experiments are very expensive (a hundred thousand dollars or more for each shot), and the planning and

²⁴ HED is the acronym for high energy density. effects, radiation transport, inertial confinement fusion, dynamic materials, and the structure and evolution of planets and astrophysical objects.

²⁵ With Dawn Flicker's promotion to senior manager, her position as manager of the experimental dynamic materials program on Z was filled by John Benage, a new hire from the LANL HED program.

scheduling of experiments must begin three to four months in advance. Upwards of 50 team members are involved in operating the Z facility. In addition, the EOS requirements and technical approach for each experiment are now subjected to intense scrutiny by the national HED Council before approval. This puts considerable emphasis on ensuring that proposed experiments meet the programmatic needs of the NNSA SSP or other major programs. Once specific experiments are approved, the preparations usually involve detailed theoretical and computational analyses, and a coordinated team effort. As a specific example in shock wave research, the design of a magnetically-driven flyer plate experiment requires extensive calculations with ALEGRA and circuit codes to define the temporal current profile in order to prevent melting or vaporization of the flyer plate by either current diffusion or the formation of an intense shock.

A large fraction of the Z shot schedule is now dedicated to materials experiments. The unique pressure and temperatures regimes accessible in the Z materials research program have kept it firmly integrated into the national SSP. With the magnetically-induced ramp loading capability available on Z, Mbar-level pressures can be achieved in material samples at low temperatures. Only diamond anvil cells (DACs) can produce similar states, but presently the upper pressure of DACs is 2-3 Mbars for measuring isothermal hydrostatic states, and the samples for DAC are typically a few microns in size, as compared to the mm-sized samples for isentropic compression experiments on Z. Magnetically-launched flyer plates on Z produce highly accurate shock pressures to tens of Mbars with corresponding extreme temperatures in accessing a thermodynamic region known as high energy density physics (HEDP). Furthermore, a combination of shock-ramp (shock loading to an intermediate state, followed by further ramp loading), ramp-shock, or shock-unloading experiments can produce essentially *any* thermodynamic state between these extremes. Although intense lasers like the National Ignition Facility (NIF) can achieve higher shock pressures than Z, the laser experiments do not have the accuracy of magnetically-driven flyer plates for Hugoniot measurements nor can they produce accurate EOS data over the wide range of thermodynamic states accessible by Z (Knudson *et al.*, 2004).

In addition, the high accuracy EOS data provide a critical evaluation of *ab initio* theories and the means to differentiate among various theories. Sandia's shock wave research program has produced a number of firsts, including (i) highly accurate shock Hugoniot data for deuterium to nearly 2 Mbars (Knudson *et al.*, 2003c), which resolved a controversy generated by the shock data on the Nova laser at LLNL (Knudson *et al.*, 2003); (ii) discovery of an ultra-high-pressure triple point of carbon (Knudson and Desjarlais, 2008), which has implications for planetary compositions and masses; and (iii) determination of the ultra-high-pressure EOS of water (Knudson *et al.*, 2012), which has inferences for the masses of giant planets, such as Neptune, Uranus, and the exoplanets.

The major developments in shock wave research in the 2000s that are discussed in this chapter include:

- Development of Shock Wave Capabilities on the Z Machine (Section 7.2)
- Ramp Loading to Multi-Mbars (Section 7.3)
- Magnetically-Driven Hypervelocity Flyer Plates (Section 7.4)
- *Ab Initio* Equation of State Theories (Section 7.5)
- Containment of Toxic Materials (Section 7.6)
- Compact Pulser – Veloce (Section 7.7)
- Effects of Shear Stress on Porous Material Compaction (Section 7.8)
- Magnetically Applied Pressure-Shear (MAPS) Experiments (Section 7.9)
- People and Places of the 2000s (Section 7.10)

7.2 Development of Shock Wave Capabilities on the Z Machine

From the mid 1950s to the early 1990s, the principal methods used for shock wave studies at Sandia and elsewhere involved either plane wave explosive lens or flat plate impact from gun-driven launchers. In the mid 1990s, high-powered lasers were being developed at many laboratories for inertial confinement fusion (ICF) applications. A scientific spin-off of this evolving discipline involved deposition of the laser energy onto a small flat disk a few hundred microns thick or into a gold-plated hohlraum that releases a soft x-ray pulse, typically a few nanoseconds (ns) in duration, that is subsequently deposited on a flat disk. The rapid heating of a surface layer produces an intense shock wave that can be used for EOS studies. However, the shock wave generated in this way is not steady (that is, with a constant pressure amplitude), which seriously complicates EOS applications. Nonetheless, the laser shock technique has become highly refined over the past 20 years, with the result that EOS properties can now be measured using laser-driven shock waves at multi Mbars with EOS uncertainties of a few percent.

At the time lasers were being developed for ICF and EOS applications, fast pulsed power techniques at Sandia were also being pursued for ICF. Fast pulsed power is defined as the ability to deliver large amounts of current (tens of megaamperes) to a load in times of a few hundred ns. This capability was developed progressively through a series of pulsed power systems, extending from SPASTIC and HERMES I in the 1960s to PBFA I and II in the 1990s. In 1996 PBFA II changed directions from a light ion source to a current source for imploding wire-generated plasmas. The converted machine was called the Z accelerator (the Z machine, or simply Z) and generated about 20 MA of current that could be used to implode cylindrical plasma magnetically. Z was used for nearly a decade, beginning in September 1996, to conduct ICF, dynamic material property, weapons physics, radiation transport, and radiation effects science experiments. Then, consistent with Congressional direction, operations of facilities funding at Sandia was designated in 2002 to begin the conceptual design of a refurbishment of Z. The Z Refurbishment Project, which was completed in September 2007, had three objectives: (i) increase the shot capacity, (ii) increase the current level from 20 MA to 26 MA, and (iii) increase the precision of the data. All three objectives benefitted shock wave research; in particular, the current objective created higher pressures for EOS experiments (with peak pressure scaling as the square of the current) and the precision objective included an advanced current pulse shaping capability that led to precise tuning of the ramp and shock loading of material samples.

A byproduct of fast pulsed power led to a revolutionary advance in the ability to perform ultra-high-pressure ramp and shock wave experiments. The Z machine provided a focus for the rebirth of a robust experimental shock wave program at Sandia. The beginnings of this capability began with Don Cook, the Director of Pulsed Power Sciences from 1993-1999. In the summer of 1996, Cook requested that Jim Asay form a shock wave program for high-pressure EOS studies on the Z machine. As Asay remembers:

[My] transition back to shock wave work began one morning during the early summer of 1996 when Don Cook, Director of Pulsed Power Sciences, stopped by my office... [and] said he wanted to develop a shock wave capability ...using the Z Accelerator.... The motivation for this new thrust arose out of program reviews of the Z pulsed energy program that emphasized the need ... for studying ... HEDP in support of the newly established NNSA Stockpile Stewardship Program.

Asay formed a small group initially consisting of Lalit Chhabildas, Clint Hall, and himself. Chhabildas focused on reinstating the capabilities at the STAR facility with Bill Reinhart, while Hall and Asay concentrated on establishing a shock wave capability on Z. Hall and Asay worked together at the beginning of this effort, with Marcus Knudson joining the group in 1998. Over the next five years, the three worked closely as a team, with the support of several staff, to develop shock wave capabilities on Z. Carl Konrad also joined the effort shortly after it started and was instrumental in implementing the

VISAR techniques on Z. After Asay's retirement in 2002, Hall and Knudson, along with Chris Deeney who replaced Asay as manager, considerably expanded the Sandia shock wave program on Z to an internationally-recognized effort.

In October 1996, the principal mode of operation at Z employed a cylindrical array of several hundred thin, high density wires (smaller in diameter than a human hair) that vaporize after intense current is applied. Conducting cylindrical plasma is formed from the vaporized wires along with a circumferential magnetic field that accelerates the plasma toward the center (the z axis, thus the term 'z pinch'). Stagnation of the imploded plasma within a container (referred to as a 'hohlraum' or empty room) produces an even higher temperature plasma that radiates intense x-rays with energies of a few eV (a few tens of thousands degrees) a few ns. This intense x-ray burst can be used to implode a small capsule containing a hydrogen-based fuel mixture for ICF applications, but it also offered the possibility of irradiating a thin cylindrical sample disk placed on the outer hohlraum diameter. The resulting deposition in a thin disk produces a planar shock wave in the sample through ablation of the front surface. Energy deposition calculations indicated that up to 5 Mbar shocks could be produced in most materials, exceeding that possible with a two-stage light gas gun.

Asay, Hall and, later on, Marcus Knudson investigated the z-pinch mode for about two years (in 1997-1999) with the goal of developing a high-quality shock wave capability on Z. As Hall recalls:

We began putting infrastructure in place for optical shock arrival sensors and VISAR systems, but encountered and subsequently overcame a number of obstacles like precision in timing mandated by small sample sizes, changing index of refraction in optical fibers from the inherent radiation, decaying pressure pulses, and non-uniform ablative loading.

These issues were systematically resolved and the first shock wave experiment with a z-pinch source, Z Shot 135, was conducted on October 6, 1997. As the excerpt from the Z Operations home page mentions,

This will be the first EOS shot. We have three secondaries [cylindrical attachments to the primary hohlraum containing the z pinch], shock break out, VISAR, and XRDs²⁶ [for measuring x-ray intensity from the z pinch source].

Asay and Hall, with recommendations from Professor Yogi Gupta at Washington State University, made the strategic decision immediately after the shock wave effort started in late 1996 to focus first on developing cryogenic capabilities to obtain Hugoniot data on liquid deuterium (D₂). This was a very high-risk decision, since a good shock wave source had not yet been demonstrated, and cryogenic capabilities for shock wave experiments at 20 K were not available at Z. This capability had to be developed from scratch and, unbeknownst at that time, would take over two years to accomplish. Furthermore, the considerable shock wave instrumentation (i.e., multiple VISARs and spectrographic equipment) would take about three years to field routinely, and considerable funds would have to be found to implement such precision capabilities, which would be even more difficult without a demonstrated shock wave source. At that time, insufficient funds were available through the Pulsed Power Sciences Center to achieve these objectives.

Fortunately, rapid progress was made on all fronts. Vice President Gerry Yonas and the Deputy Director of the Pulsed Power Sciences Center, Jeff Quintenz, agreed to share the cost of developing the cryogenic system, which became operational for shock wave studies in early 1999 (Hanson *et al.*, 2000). Bill Reinhart at STAR provided a discarded VISAR and loaned another one for use at Z. Mike Furnish and Paul Raglin provided financial support and the use of two multi-point VISARs from the subcritical

²⁶ XRDs are x-ray diodes.

experiments program at the Nevada National Security Site (NNSS), formerly known as the Nevada Test Site (NTS), as well as the hands-on support of three experienced technicians to field these VISARs on Z. After he left Sandia in 1990, Lynn Barker invented and patented²⁷ a multi-beam VISAR (Barker, 2000). A variation of this concept was developed for Z, which significantly contributed to the success of the experiments by allowing highly accurate high-pressure EOS measurements. John Porter donated several of his dedicated shots on Z to test configurations for shock wave experiments and also helped support the cryogenics development. The Z Operations Manager at that time, Johann Seamen, redirected significant operational funds to configure instrumentation on Z to allow VISAR experiments in the intense radiation environment. With these parallel efforts, rapid progress was made and the first shock wave EOS experiments with a z-pinch source were reported externally by Hall at the Shock Compression of Condensed Matter conference in the summer of 1999 (Hall *et al.*, 2000). A working cryogenic system for shock EOS measurements on Z was also reported at the same conference (Hanson *et al.*, 2000).

The z-pinch approach for producing planar shock waves was able to generate intense shock pressures (Hall *et al.*, 2000), but the EOS accuracy was not to the required standards because steady shock waves were not produced by the pulsed radiation source and the spatial uniformity (planarity) was not adequate. Tailoring the energy deposition process showed promise for producing longer duration deposition, less shock attenuation, and better planarity but was discontinued when a purely magnetic drive was discovered in early 1999 that produced gun-quality ramp, and later shock, loading (Asay, 2000; Asay *et al.*, 2000). As Knudson comments about this period,

...[T]he z-pinch load on the Z Accelerator proved to be at least one too many stages of pulse compression. We had felt that the resulting x-ray burst was insufficient as a source for dynamic compression. Rather, we desired a source capable of producing steady shock waves for durations of several 10s of ns. We therefore spent nearly a year investigating the use of low-density, carbon-based foams (such as TPX) as a way of 'conditioning' the radiation that reached a sample, such that the resulting radiation and foam tamper would support a significantly longer, steady shock. These efforts, while somewhat promising, ultimately proved to be inefficient and unsuccessful...

However, the path to producing quality shock waves on Z, which had been pursued for two years with z pinches, was about to take a sudden turn.

7.3 Ramp Loading to Multi-Mbars

The discovery of a high quality magnetic drive on Z was serendipitous. One of the co-developers of z-pinch technology, Rick Spielman, needed a non-invasive diagnostic for measuring current close to the z-pinch source where radiation fields are extremely high. Discussions with Jim Asay and Clint Hall in early 1999 concluded that the VISAR would be a good tool for measuring particle velocity and pressure and, hence, magnetic field and current near the z-pinch source. The pressure measurement could be unfolded to determine the current flowing to the source. The first experiments with this approach were successful (Leon *et al.*, 1999; Asay, 2000) and led to further developments (Hall *et al.*, 2001, 2002) that ultimately enabled highly accurate ramp wave experiments to about 4 Mbars. As Clint Hall remembers,

...[We] placed two small disks (one copper and one iron) into the anode plate on a wire array load and ...attached shielded VISAR diagnostics to the rear of each to measure their respective

²⁷ L. M. Barker, "Multi-Beam VISAR Using Image Coupling from One Optical Fiber Bundle to Another through the VISAR Interferometer," issued by the United States Patent Office, February 9, 1999.

free surface velocities. As it turned out, the Z shot was fired late on a Friday afternoon, with the raw VISAR data saved and delivered to me to be analyzed on the following Monday morning. I made it until Saturday around lunch time before I had to head into work to see what we recorded. I sat down, opened the files and reduced them. When I had done so, I got a very nice trace from the iron, but it had an odd feature I couldn't understand. It just so happened that Lalit [Chhabildas] was in his office working too, so I went down and showed the trace to him. He immediately thought that it might be clearly showing the alpha-epsilon phase transition in the iron, but couldn't remember at what pressure it was supposed to occur, so we jumped into the company truck and headed into Area 1 hoping to find someone that might be there and remember. The best we could do was to find a viewgraph on a poster outside a door (that we just had to 'borrow') showing the iron transition from a gun experiment. Sure enough, they correlated, and the birth of magnetic loading for ramped wave experiments on Z gained credibility.

The first experiment was not optimal in terms of drive uniformity and the use of theoretical design approaches to analyze the ramp waves, but it did demonstrate that a high quality magnetically-driven ramp wave could be produced and improvements followed rapidly.

The basic concept for an isentropic compression experiment (ICE) using magnetic drive is shown on the right side of **Figure 7-1**. Current flowing on or near the surface of a conductor produces a magnetic field that interacts through the Lorentz force, resulting in magnetic pressure on the conductor. The applied magnetic pressure is transmitted via elastic and plastic waves through the conductor and into a sample mounted on the conductor's rear surface. If designed properly, the sample does not experience magnetic or thermal loading and is subjected to only the smooth pressure loading shown in Figure 7-1 that resulted from the initially smooth current-time profile.

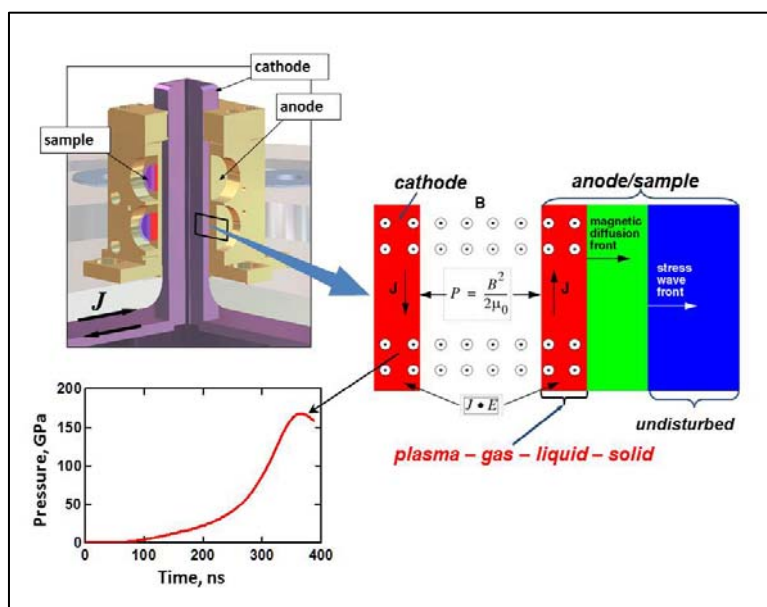


Figure 7-1. Concept for magnetic drive. The left diagram shows the concept for the ICE cube. Pressure history is shown at the bottom left. The magnetic and hydrodynamic pressure increase on the sample is shown in the diagram at the right.

Clint Hall conceived of an insightful innovation, which allowed the simultaneous planar loading of several specimens on a single Z experiment. This configuration consisted of four individual panels of aluminum or copper with diamond-machined, counter-bored holes for mounting specimens, as illustrated in the upper left of Figure 7-1. The panels were bolted together, making a hollow square cube that was mounted on the anode of the current-carrying center section of Z. The cathode was connected to a solid square conductor inserted through the center of the sample (anode) cube and shorted to it at the top. With current flowing on the two surfaces, the magnetic field in the gap (typically about 1 mm) produces magnetic pressure on the inner surface of the sample cube that initiates a stress wave propagating through the thin metal floor of the cube and into the sample, as illustrated on the right of Figure 7-1. Clint called this fixture an 'ICE cube,' which appropriately describes its function. Typical dimensions of the ICE cube are about 30 mm on a side with sample diameters on the order of 10-20 mm diameter and

thicknesses of 1-2 mm. **Figure 7-2** shows Hall setting up an experiment with an aluminum ICE cube. Copper was also used because of its higher conductivity. Typical current rise times in most ramp wave experiments range from 200-300 ns and are shaped in time to minimize shock formation (Hayes *et al.*, 2004; Davis, 2006).

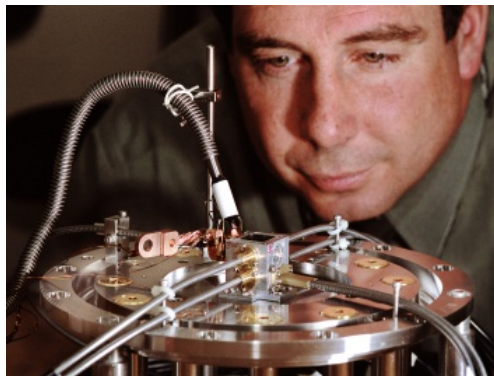


Figure 7-2. Clint Hall setting up a Z experiment on an Al ICE cube. Fiber optic cables transmit and receive VISAR signals for analysis of the ramp- or shock-loaded sample.

The most important feature of magnetically-induced ramp loading is the smoothly increasing current and corresponding pressure with time, as shown in Figure 7-1. This allows a relatively straightforward method for analyzing the evolution of stress waves produced in the sample to deduce pressure-density states achieved by ramp compression (Hayes *et al.*, 2004; Hall *et al.*, 2002; Davis 2005, 2006; Davis and Deeney, 2005; Reisman *et al.*, 2001). Smooth compression is important in many applications, including the detection of phase boundaries (Asay *et al.*, 2000; Reisman *et al.*, 2001; Davis and Hayes, 2002, 2007; Davis and Foiles, 2005), dynamic yield strength measurements (Asay *et al.*, 2000, 2011; Vogler *et al.*, 2009a, 2009b), and acceleration of flyer plates (Hall *et al.*, 2001; Knudson *et al.*, 2003). Another critical development involved the ability to shape the current pulse by firing the 36 modules of Z at different times to obtain

the optimum pressure drive history and delay shock wave formation in the sample (Davis, 2006; Hayes *et al.*, 2004; Lemke *et al.*, 2011). This development significantly increased the sample thicknesses that could be studied.

The use of magnetic loading to produce smoothly increasing Mbar ramp loads was a major breakthrough for materials response studies on Z. Hopkinson bar experiments are routinely used to study material response at strain rates up to about $10^3/s$. Magnetically-induced ramp compression is the first accurate method for extending these kinds of material studies to strain rates ranging from $10^5/s$ – $10^8/s$. In addition, ramp compression is much less dissipative than shock compression, resulting in nearly isentropic thermodynamic states during loading. For liquids that exhibit only bulk response, but even for solids, the plastic dissipation produced by strength effects is relatively small; because of this, the compression process is referred to as ‘quasi-isentropic.’ Since dissipation is small, the thermodynamic states produced by ramp loading permit access to previously inaccessible states of matter at high pressures but low temperatures. **Figure 7-3** illustrates the difference in pressure-density-temperature states between shock and ramp loading of aluminum (Asay and Knudson, 2005).

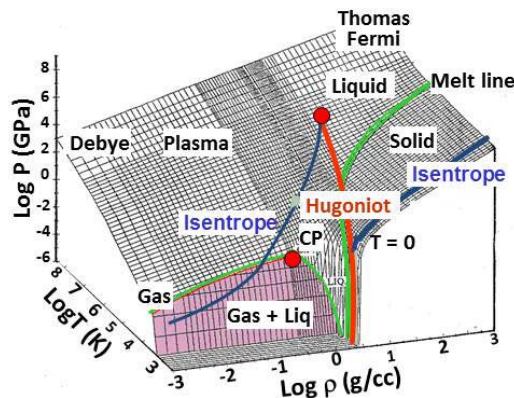


Figure 7-3. Pressure and temperature states achieved in aluminum for shock and ramp loading, which produces near-isentropic states. The combined capability enables the study of new physical regimes, including cool solid states at high pressure, hot states at high pressure, and vapor and plasma at expanded states. CP represents the critical point for aluminum.

The ability to ramp load materials to Mbar pressures had been an unreachable goal of the shock wave community for several decades, beginning with the first isentropic compression experiments (Barker and Hollenbach, 1970). They used fused silica buffers to produce ramp waves that could be used to ramp load samples. Although the graded density impactors developed later in the 1980s by Barker and Scott (Barker, 1984) showed promise for achieving this goal, those impactors had limited reproducibility and

uniformity that precluded their widespread use. The layered-material impactor developed for the HyperVelocity Launcher (Chhabildas *et al.*, 1990) also produced nearly shockless loading and was very reproducible but did not produce smoothly increasing pressure.

Magnetic loading avoids these problems but introduces other issues concerned with current diffusion and the shock up of the stress wave produced in the sample. Current diffusion, as illustrated in Figure 7-1, occurs rapidly. But, fortunately for good conductors such as Al and Cu, the stress wave in the sample outruns the diffusion front so the sample remains at ambient conditions before loading. Shock formation in the sample can be minimized by properly shaping the current-time profile.

Since its inception in the late 1990s, magnetically-induced ramp loading has continued to be perfected and improved through the work of many investigators, including (in alphabetical order) Tommy Ao, Jim Asay, Jean-Paul Davis, Chris Deeney, Mike Furnish, Tom Haill, Clint Hall, Dennis Hayes, Randy Hickman, Marcus Knudson, Ray Lemke, David Reisman (LLNL), Seth Root, Rick Spielman, Art Toor (LLNL), Tracy Vogler, and Jack Wise. The technique has been used to measure the EOS properties of a large number of materials at an accuracy approaching that possible with shock loading. These results have been invaluable for developing material models used for weapon applications and applications such as planetary science. Several of the results obtained with the method are given in Asay, 2000; Asay *et al.*, 2000; Reisman, 2001; Davis *et al.*, 2001, 2002, 2005; Davis, 2006; Davis and Hayes, 2007; Hayes, 2001; Hall *et al.*, 2001, 2002; Hayes *et al.*, 2004. These papers cover a range of applications, including quasi-isentropic compression of materials, ramp compression of laser windows, and studies of phase transitions with ramp waves.

7.4 Magnetically-Driven Hypervelocity Flyer Plates

The next step in generating high quality shock waves on Z involved the use of ramp loading to launch thin flyer plates to velocities far exceeding previous capabilities, including the HVL. This revolutionary advance was enabled by the smooth increase in pressure produced by ramp loading. The pressure history shown in Figure 7-1 either allows a sample to be loaded continuously for EOS studies or, alternatively, allows the conducting panel itself to be launched as a flyer plate to impact a specimen located behind it (not shown). In the flyer-plate mode, Marcus Knudson, Clint Hall, Ray Lemke, and others have demonstrated that flyer plates on the order of 17-mm diameter and up to about 1-mm thick can be launched to velocities as high as 46 km/s (Lemke *et al.*, 2003a, 2003b, 2005a, 2005b, 2011). This is about *five times* higher than possible with conventional two-stage guns and about *3-4 times* higher than with the HVL. Using Al flyer plates, impact pressures as high as 40 Mbars can be produced in high-impedance materials. This velocity range considerably extends the pressures available in the laboratory and enables extremely accurate EOS measurements to multi-Mbar pressures.

Figure 7-4 shows a typical configuration for launching flyer plates on Z.

The following quote from Knudson captures the excitement of this development:

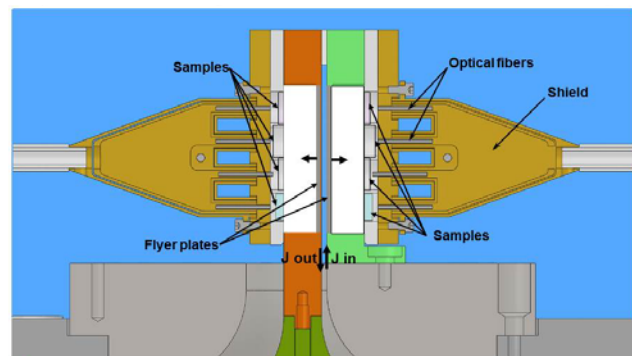


Figure 7-4. Stripline configuration to produce magnetically-driven flyer plates. Multiple flyer plates and samples are used in each experiment. Optical fibers and samples are shielded from magnetic field effects.

The next significant breakthrough occurred during the 1999 American Physical Society meeting of the Division of Plasma Physics in Seattle, WA. Jim Asay, Clint Hall, and I were at the lobby bar of the Westin in Seattle, discussing the magnitude of the impulse these types of experiments delivered to the electrodes. Back of the envelope calculations suggested that the impulse was sufficient to launch relatively thin anode plates, roughly 1 mm in thickness, to significant velocities, well in excess of 10 km/s. That evening I ran some simple hydrocode simulations and the results were extremely encouraging. Upon returning from that meeting, we immediately began planning dedicated experiments on Z to test this new concept. Given that it takes roughly three to four months to design and fabricate hardware for Z experiments it would not be until the spring of 2000 that the first experiments would be performed.

In the meantime we presented the idea of magnetically accelerating flyer plates on Z at the 2000 Garwin review,²⁸ an annual external review of the pulsed power program. I presented our plans on the first day of the two-day review. I'll never forget the response of David Reisman [LLNL], who was attending the meeting to present on the progress of the ICE platform the following day. He had been intrigued with the idea that we had presented, and that evening in his hotel room had run several Mach 2 calculations, including full magneto-hydrodynamics. He... [concluded] that it was a great idea, but unfortunately it would never work. The Mach 2 calculations indicated that no matter how thick the initial electrode, the magnetic field diffusion was so severe that the field would burn through the plate before it could reach a terminal velocity, and thus the plate would be vaporized prior to impact.



Figure 7-5. Marcus Knudson at Z holding a flyer plate that will be launched to velocities in excess of 10 km/s.

A few weeks later we went ...[ahead] with the planned experiments ...I still remember sitting at home on the evening of that first flyer plate experiment analyzing the data, so anxious that my hands were literally shaking as I plotted the breakout time versus distance. Low and behold, despite the Mach 2 simulations, the data suggested that a significant portion of the plate was still at full density upon impact, implying that the diffusion rate in the simulation was too high. This marked the birth of the magnetically accelerated flyer plate platform on Z.

Figure 7-5 shows Marcus holding a flyer plate assembly. Clint Hall also remembers this momentous and ground-breaking development:

Jim, Marcus, and I were sitting at a table between talks [at the Division of Plasma Physics Meeting held in Seattle, WA, 1999], and we began discussing another whole [new] concept. Instead of directly transmitting the ramp loading into the sample of interest, we thought we could instead launch the bottom of the panel as a flyer plate and impact our samples for high pressure Hugoniot data. We did what any self-respecting physicist would do at that point and drew it out on a cocktail napkin, stuffed it in one of our pockets, and went to our next talk! When we got back to SNL, we did indeed retrieve the ‘highly precise scientific sketch,’ did some calculations, performed the experiment, and we exceeded years of HVL development on our first shot with a metallic flyer that was good enough for EOS experiments moving at ~10 km/[sic: s]!

²⁸ The annual external review of Sandia’s Pulsed Power Sciences efforts is sponsored by Sandia and is usually held for 2-1/2 days in May or June. In his recollection, Marcus uses the term ‘Garwin review’ because Richard Garwin was, at that time, the chairman of that annual external review.

Conception and development of the magnetically-launched flyer plate for making extremely accurate shock Hugoniot measurements at ultra-high shock pressures was truly a game-changing event, as the HVL was earlier using conventional two-stage guns. Many of the technologies learned for launching flyer plates with high impulsive loads on the HVL were valuable in achieving similar flyer plate capabilities on Z.

Later *ab initio* calculations by Mike Desjarlais showed that the actual high-pressure electrical conductivity of aluminum was about a factor of two more near the melting point than the values being used in Mach 2, which were based on Lee and More's electrical conductivity model for dense plasmas (Lee and More, 1984); hence, the plate did not melt and vaporize as Reisman had predicted (Desjarlais, 2001; Desjarlais *et al.*, 2002). These theoretical and experimental breakthroughs had a profound impact in the shock wave community by demonstrating new methods for obtaining accurate EOS data at ultra-high shock pressures. Since the first experiments in 2000, Knudson has continually refined the flyer plate technique to increase the velocities well beyond ~ 10 km/s (Hall *et al.*, 2001; Knudson *et al.*, 2003) and to improve the EOS accuracy to equal that of gun-launched projectiles (Knudson and Desjarlais, 2009).

To explore the range of velocities possible with magnetically-driven flyer plates, Ray Lemke performed an extensive investigation using ALEGRA MHD simulations, which established an upper limit of about 46 km/s for the present capability of the refurbished Z (Lemke *et al.*, 2002, 2003a, 2003b, 2003c, 2005a, 2005b, and 2011). His predictions were established experimentally and show that, with sufficiently high currents, flyer plate velocities as high as 65 km/s will be possible. However, this is an upper limit for any magnetically-driven flyer plate because of limitations imposed by current diffusion and the requirement that shocks not form in the flyer plate (Lemke, 2013). Generally, the ALEGRA predictions have accurately reproduced the VISAR measurements, as shown in **Figure 7-6**. Because of the complexity of these experiments, ALEGRA calculations are routinely performed to define the experimental parameters for every flyer plate experiment.

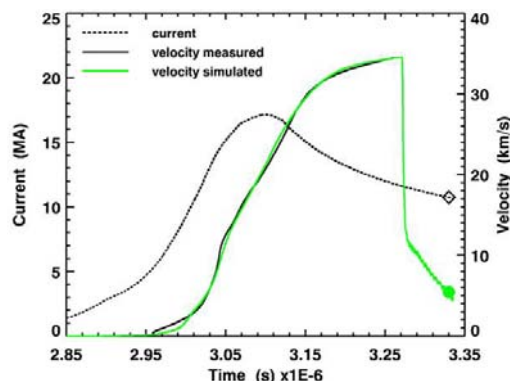


Figure 7-6. Comparison of simulated and experimental flyer plate velocities for a stripline geometry. Plate thickness is 0.9 mm, peak current is about 22 MA, and peak velocity is about 35 km/s Lemke *et al.*, 2003).

Over the next several years, Marcus Knudson championed the use of the magnetic flyer plate technique for studies of several materials of scientific and practical interest with unprecedented accuracy and at pressures far exceeding the capabilities of conventional launchers. The method was used to obtain (i) near-absolute Hugoniot data on aluminum, followed by measurement of the release adiabat into the liquid-vapor region (Knudson *et al.*, 2003d, 2005); (ii) shock loading and partial unloading of beryllium to determine the high-pressure EOS and phase boundaries, including determination of its high pressure yield strength; (iii) the Hugoniot of water at extreme pressures, which is revising conventional concepts of the compositions of giant planets and exoplanets²⁹ (Knudson *et al.*, 2012); (iv) discovery of a high-pressure triple point and a new solid phase of carbon (Knudson *et al.*, 2008); (v) over more than a hundred shock Hugoniot measurements on quartz to over 16 Mbars that have established it as the most accurate standard for high pressure EOS measurements (Knudson and Desjarlais, 2009); and (vi) extensive studies of the high-pressure EOS of liquid deuterium that enabled critical comparison with *ab initio* theories and established the Z data as the accepted high-pressure EOS for hydrogen (Knudson *et al.*, 2001, 2003a, 2003b, 2003c, 2004).

²⁹ For additional discussion, see Section 7.5.

The extensive research on liquid deuterium, beginning in 2000 and continuing through 2013, is a noteworthy accomplishment that established the credibility of the Sandia shock wave program and helped secure Sandia's role in the NNSA national program for HEDP. In the late 1990s, laser shock experiments on Nova at LLNL (Collins *et al.*, 1998) indicated that the deuterium shock Hugoniot was about 50% softer (for a peak compression of about six) at Mbar pressures than predicted by conventional phenomenological models (for a peak compression of about four). With the successful development of the magnetic flyer plate technique in 2000, Knudson obtained shock data on deuterium at similar pressure levels, but with substantially better accuracy (Knudson *et al.*, 2001, 2003a, 2003b, 2003c, 2004). The Z data indicated a peak compression closer to the classical model for the pressure-density relationship, as indicated in **Figure 7-7**. However, investigators involved in the laser effort on Nova steadfastly maintained that their deuterium data were correct; the controversy persisted for several years and involved most of the high-pressure community. To resolve the discrepancy, Knudson's team performed a variety of experiments that included reshock, reverberating wave, and Hugoniot data at higher pressures and temperatures, including temperature measurements by Jim Bailey (Bailey *et al.*, 2009); those experiments supported the stiffer response. In addition, the development of the extremely accurate quartz standard for shock wave research (Knudson *et al.*, 2009) substantially reduced the error bars of the deuterium Hugoniot data, as illustrated by the green data points (Knudson, 2013) in Figure 7-7. The combined data set, and especially the most recent data, provides compelling evidence for the Sandia results, which are now generally accepted by the scientific community. Later laser shock experiments resolved earlier systematic errors so that the Z and laser data now agree within the reported errors for each (Hicks *et al.*, 2009; Knudson and Desjarlais, 2009). Moreover, the improved understanding of the hydrogen EOS obtained over the past decade has broad application to developing models for the interiors of giant planets, astrophysical plasmas, warm dense matter, and inertial confinement fusion.

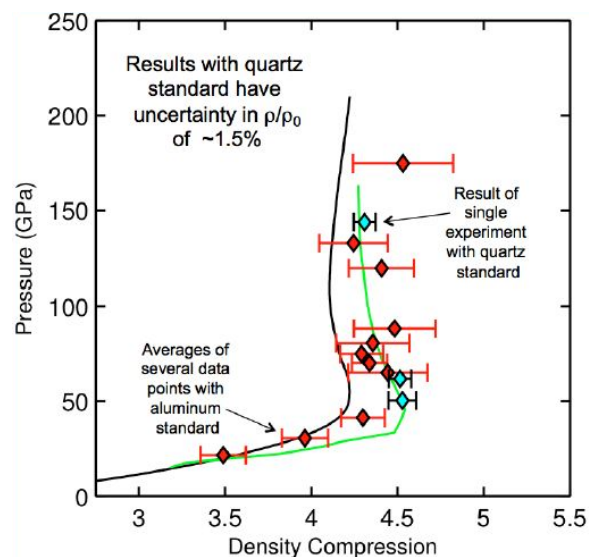


Figure 7-7. Experimental Hugoniot data for liquid deuterium obtained on Z in comparison with classical Sesame calculations (Kerley) and QMD calculations (Desjarlais, 2003). The red Z data represent averages of several experiments. The green Z data points were obtained with an improved quartz diagnostic that significantly reduced the error bars (Knudson, 2013).

The shock wave group at Z had grown rapidly in the decade after the first magnetically-driven experiments in 1999 through the research of a number of newly graduated students, including Scott Alexander, Tommy Ao, Justin Brown, Jean-Paul Davis, Dan Dolan, Marcus Knudson, Seth Root, Chris Seagle, and Tracy Vogler. In addition, several experimenters from the original group under Walt Herrmann in the 1980s rejoined the shock wave program through internal transfers. These included Michael Furnish, Randy Hickman, John Martinez, Bill Reinhart, Tom Thornhill, and Jack Wise. Several researchers from different technical areas in the Pulsed Power Sciences Center also contributed to the program by adding innovative instrumentation and theoretical capabilities for shock wave studies, notably Jim Bailey, who transferred techniques developed for measuring plasma temperatures on Z to the problem of measuring radiation temperatures in shocked deuterium, and Dave Hanson, who developed a novel cryogenic capability for performing shock wave experiments on deuterium and other materials at temperatures of a few degrees Kelvin. Kyle Cochrane, Mike Desjarlais, Heath Hanshaw, Thomas Mattsson, and other recent additions brought strong theoretical skills to the program.

The magnetic loading capability available on Z considerably expands the range of EOS data that can be obtained. Through a combination of shock or ramp loading, followed by unloading, it is now possible to study the full EOS surface for a variety of materials, including expanded states, as shown in Figure 7-3 for aluminum. The behavior of materials at extreme pressures and temperatures referred to as HEDP is a research area of intense interest in the national and international research communities. The study of hot dense matter covers the range of material response at several hundred fold compression and several thousand eV, which is pervasive in supernova, accretion disks, and the interiors of stars. These states are also produced in practical applications such as laser plasmas and z pinches. Shock wave experiments on Z have been especially useful in understanding a variety of HEDP phenomena.

7.5 *Ab Initio* Equation of State Theories

Beginning in the late 1990s when the Nova laser experiments at LLNL on deuterium first started producing shock Hugoniot data at higher pressures than possible with gas guns, and later when Hugoniot data were generated with Z, several theoretical investigators began exploring the use of quantum molecular dynamics for predicting Hugoniot curves. QMD is a powerful tool for predicting EOS properties of low-molecular-weight liquids shocked to a few Mbars, but requires large computational capability. The emerging ultra-high-pressure shock wave data on deuterium provided a unique opportunity for solving these computational-intensive problems with the fast MPP computers that were just becoming available. The thermodynamic regime accessed by shock experiments is referred to as ‘warm dense matter,’ a region where the thermal occupation of excited electronic states is generally non-negligible and ions are strongly coupled.

At Sandia, Mike Desjarlais led the implementation of QMD for EOS applications early in the 2000s. His first use of the theory concerned the conductivity of aluminum for magnetically-driven flyer plate applications. Existing conductivity models in MHD codes being used for these calculations predicted that the high current densities on Z would completely diffuse through and vaporize an aluminum flyer plate before it could be launched to full velocity. However, the initial experiments by Marcus Knudson indicated that these results were erroneous.

To address this issue, Desjarlais performed a series of QMD simulations to determine the conductivity of aluminum over the pressure and temperature regimes accessed during magnetic acceleration. As he states,

...[C]onductivity calculations... [on aluminum] established the power of using QMD simulations to create accurate conductivity models for materials away from ambient. The first application in shock work was to the aluminum conductivity, [which is] needed for modeling the flyer plates. This advance was essential to doing predictive design simulations and as an immediate consequence rapidly advanced the pace of data accumulation by permitting much more accurate pre-shot design.

The QMD simulations established that aluminum was significantly more conductive than predicted by the classical theories and therefore a significant portion of the flyer plate would remain at ambient conditions upon impact. This conclusion explained the successful flyer plate results observed experimentally and the miscalculation produced in previous Mach 2 calculations (Hall *et al.*, 2001; Knudson *et al.*, 2003). Desjarlais’ QMD research on the conductivity of aluminum has had a major influence on the design of all flyer plate experiments on Z by defining the operational limits for flyer plate materials, including the thickness and pulse shaping requirements (Desjarlais, 2001; Desjarlais *et al.*,

2002). This *ab initio* research was followed by additional QMD predictions of the shock loading and unloading response of several other materials.

A specific example highlights the role QMD played in resolving the controversy about the high-pressure shock Hugoniot of liquid deuterium. At the time of the controversy over the experimental differences, Desjarlais initiated a research effort with QMD to predict the Hugoniot of deuterium. Using density functional theory (DFT) and the VASP (for Vienna *ab initio* Simulation Program) code that was developed at the Technical University of Vienna, he developed a set of interaction forces to treat the ions and electrons of deuterium and simulated the prior gas gun data to about 400 kbars (Desjarlais, 2003). Excellent agreement was obtained with the data. Encouraged by this, he extended the calculations to shock pressures of about 1.5 Mbar (Desjarlais, 2003), where the Z and Nova laser data disagreed significantly. The QMD predictions showed good agreement with Knudson's Z data (see Figure 7-7) but serious disagreement with the laser data above 400 kbars. To further support the stiffer EOS response, QMD simulations of experimental recompression states produced by shock reflection were performed, again indicating good agreement and validating the Z results (Desjarlais, 2003). Additional QMD calculations also predicted the shock temperature measurements (Bailey *et al.*, 2009) and the Hugoniot data at higher pressures (Knudson *et al.*, 2003a, 2003b, 2003c). These highly integrated studies provided compelling evidence for the Sandia results and were crucial to resolving the deuterium controversy.

Experimental and theoretical collaborations using *ab initio* theories continued on several other materials, including (i) the multi-Mbar shock loading and unloading of aluminum into the vapor region (Knudson *et al.*, 2003d, 2005), (ii) the high-pressure Hugoniot of quartz (Knudson and Desjarlais, 2009), (iii) the identification of new phases of carbon (Knudson *et al.*, 2008), (iv) the ultra-high compression of water (Knudson *et al.*, 2012), (v) the compaction of porous materials (Cochrane *et al.*, 2013), and (vi) multi-Mbar studies of other fluids and foams (Root *et al.*, 2010, 2012; Mattsson *et al.*, 2010). The quartz experiments are especially noteworthy; extensive magnetic flyer plate data to 16 Mbars have established quartz as one of the most accurate standard laser 'windows' for high-pressure shock wave measurements (Knudson and Desjarlais, 2009). Since most, if not all, laser windows become conductive by shock loading to a few Mbars, the window becomes a *shock velocity* gauge (Knudson and Desjarlais, 2009) rather than a *particle velocity* gauge, as originally intended by Barker (Barker and Hollenbach, 1974). Good agreement between the experiments and the QMD calculations was also obtained on quartz and provided physical insight as to its high-pressure response. As Desjarlais commented, "QMD work [on quartz] provided the explanation for the non-linear U_s - u_p relation through the observation of an extended dissociation regime and the corresponding consequences for the specific heat."

Knudson's shock wave experiments on carbon (diamond) demonstrated that highly accurate Hugoniot data can be used to identify subtle changes in the phase diagram (Knudson and Desjarlais, 2008). Knudson was able to deduce that a new phase of carbon, identified as bc8, existed at high pressure and that a triple point occurred among the diamond phase of carbon, the new phase, and liquid carbon at 8.5 Mbars on the Hugoniot. QMD calculations by Desjarlais predicted these results and, as he mentions,

the subtle changes in U_s - u_p as the Hugoniot traverses the diamond-liquid-bc8 coexistence region were predicted by the QMD calculations and observed in the data and gave convincing evidence for the diamond-liquid-bc8 triple point and the first experimental evidence for bc8 in carbon.

Figure 7-8 shows the phase diagram for carbon resulting from these experimental and theoretical studies.

Recent years have witnessed an explosion in the discovery of exoplanets orbiting other stars. Two categories of these planets include ‘hot Neptunes’ and ‘icy giants.’ The temperature and pressure states existing within this body of exoplanets establish in which group a newly discovered planet fits. It is important to understand the composition of these planets, as well as the EOS of light elements and compounds like water over wide pressure and temperature ranges, in order to study the formation and evolution of the planets in our solar system and other stellar-exoplanet systems. In the past, much of this information came from phenomenological equations of state such as ANEOS and SESAME. However, the growing power of *ab initio* EOS calculations has changed the approach for determining high-pressure properties of materials important to planetary science. A particular example is water. Recent flyer plate data on water to 4.5 Mbars and QMD calculations (Knudson and Desjarlais, 2012) have established a more accurate and much different EOS for water than has been used in the classical models. A notable consequence is that *ab initio* EOS calculations for water predict the core temperatures of Neptune and Uranus are 20% cooler than predicted by earlier models.

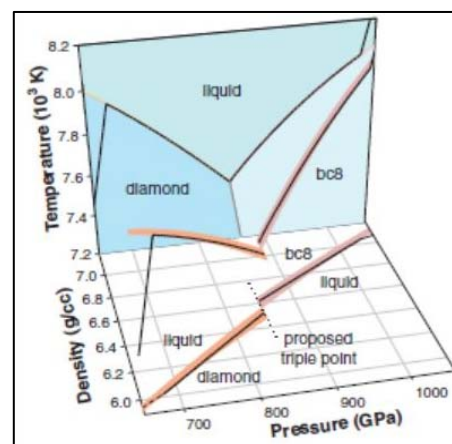


Figure 7-8. Coexistence regions in P-p-T space for carbon. The Hugoniot (black line) enters the diamond phase at ~680 GPa (6.02 g/cc), reaches the diamond-bc8-liquid triple point at ~850 GPa, and exits the bc8-liquid coexistence region (pink band) at ~1040 GPa (7.04 g/cc) (Knudson *et al.*, 2008).

The ICE and shock data obtained on Z and supported with *ab initio* calculations have established the Sandia shock wave program as a major asset to the fundamental science and weapons communities. In his recollections, Jim Asay reproduced a quote from a *Sandia Lab News* article (Vol. 64, No. 4, p.1, Feb. 24, 2012), which highlights this recognition:

Both Congress and DOE have recognized the value of the experiments conducted on Z by Sandia and its partner laboratories, Los Alamos ...and Lawrence Livermore ...in support of NNSA's Stockpile Stewardship Program. The Senate's Subcommittee on Energy and Water Development's 2012 appropriation bill commented that 'The Committee understands that (two Z) experiments (on plutonium) yielded fundamentally new and surprising data about the behavior of plutonium at high pressure and this new data [sic: have] been one of the most valuable contributions to the stockpile stewardship program. The Committee continues to strongly support the weapons physics activities at Sandia's Z facility that are critical to sustaining a safe, secure, and effective nuclear stockpile.' Wrote NNSA Deputy Administrator for Defense Programs Don Cook regarding the joint plutonium work on Z carried out by Sandia and LANL, 'These high-quality data will likely provide new insights and challenge our fundamental understanding.... This accomplishment has been one of our most valuable technical contributions to the stockpile stewardship program.'

As of this writing, the Sandia shock wave group maintains a strong theoretical and experimental program in high pressure EOS research. QMD plays a central role in this extensive effort. Other theoreticians include Thomas Mattsson and Kyle Cochrane, who have extended QMD simulations to other fluids, including xenon and carbon dioxide (Root *et al.*, 2010, 2013), and to polymers (Mattsson *et al.*, 2010). The combined capabilities include a diverse team of researchers for addressing a broad range of material issues in the NNSA Stockpile Stewardship Program. The resolution of the controversy over the high pressure response of deuterium is a prime example.

7.6 Containment of Toxic Materials

The magnetically-driven ramp and shock loading techniques have also been extended to toxic materials, such as beryllium, plutonium, and uranium, which cannot be studied in conventional laboratory environments. Early in the development of the magnetic driver, Jim Asay, Chris Deeney, and Clint Hall proposed that magnetic ramp loading should be a strategic capability of the NNSA SSP for studying toxic materials. Various approaches for achieving this goal were discussed with colleagues at the other national security laboratories. The development of explosively-driven fast pulsed power at NNSS was first discussed with Bob Cauble, Art Toor, and others at LLNL. LLNL expressed no interest in pursuing research on toxic materials at that time (about 2001) and the Sandia interest was dropped. Another approach pursued by Chris Deeney involved the development of a small pulsed power system that could be located in an underground environment in Nevada and dedicated to studying toxic materials. This option was also not well received by the other laboratories and, after considerable discussion, was not pursued.

A third approach was given serious consideration and ultimately led to the development of the only experimental NNSA capability for high-pressure ramp loading of toxic materials. Chris Deeney conceived of an innovative containment system for this application. In this case, the ICE cube is enclosed within a hardened steel container connected to the current source in the center section of Z. The ramp or shock loading experiment is conducted as usual, but immediately after the data are acquired, 36 detonators ringing the outer part of the containment chamber are triggered to close off the ICE cube from the rest of Z. The chamber and the toxic residue can then be transported to a storage site.

Achieving this capability on Z required a daunting effort over several years in the early 2000s, including (i) engineering design of a containment system that had never been used in a high-energy pulsed power environment; (ii) coordination of several internal and external groups involved in the mechanical, electrical, explosive, and pulsed power aspects of the project; (iii) coordination of safety issues with the Environmental, Safety and Health organizations; (iv) partnership with LANL, who prepared the samples and arranged for transportation to and from Sandia; and (v) obtaining the appropriate local and national approvals. **Figure 7-9(a)** is a schematic of the containment system, showing the complex mechanical-electrical-explosive configuration necessary to ensure containment after a ramp loading experiment on a sample. Randy McKee, manager of Pulsed Power Engineering in the Pulsed Power Sciences Center, was instrumental in designing and improving the fixture; he is shown in part b of the figure next to an actual containment chamber in 2013.

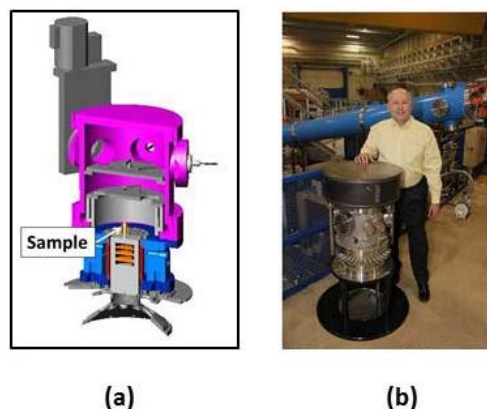


Figure 7-9. Containment system for studying toxic materials on Z. (a) Schematic of the explosively-contained chamber that isolates the sample after an experiment. (b) Randy McKee with a containment chamber to be used on Z.

Several individuals at Sandia and elsewhere thought that this could not be done, at least within a short time. One high-level manager at another laboratory was heard to say “they will never shoot Pu on Z.” However, Deeney obtained approval for the local management controls and proceeded with the project. Initially, he personally organized and managed the various internal and external groups necessary to accomplish the project, since Hall had temporarily left the group. But Deeney was able to entice Hall to rejoin the group, which accelerated development of the project. Hall organized the various working groups into a coordinated team and, over the next three years, accomplished all of the tasks necessary to

perform the first test shot with the containment chamber. This was successful as Hall exclaims in his recollections:

After several years of work, we were ready to test the first full system on Z. We fired the shot, and all real-time indicators suggested that it functioned properly, but the only way to know for sure was the much anticipated helium leak test. We pulled the system from the machine and hooked it up to the detector. Suffice it to say that I did the ‘happy dance’ when there were absolutely no leaks! The system worked as well or better than we had hoped, [in the] first time out. This turned out to be yet another enabling technology that truly provided a future for Z and the SNL shock physics effort, even if we didn’t fully understand it at the time.

With the successful demonstration of the containment system, additional work was necessary to obtain the internal and external authorizations for experimenting with Pu on Z and to develop an inter-lab team effort with Los Alamos to define the appropriate experiments, prepare the samples, transport them and the waste to and from Sandia, and analyze the results. Hall resolved these issues, and the first Pu experiment was conducted with Marcus Knudson as the principal experimenter on May 25, 2006 (Z Shot 1720). Hall recalls this moment:

[The] Pu shots ... were perfectly contained, and they went off without a hitch. Marcus [Knudson] and Dennis Hayes collaborated to make some nice deductions from the analyzed data, all the logistics went perfectly and lo and behold, we had done the seemingly impossible on a shoestring budget! This experiment was tremendously important as it not only provided some new insights into Pu behavior; it had secured SNL’s future in... [DMP, the NNSA Dynamic Materials Properties Campaign] for many years to come.

Two more Pu shots were conducted in 2006 with Knudson as the experimenter on June 14 (Z Shot 1731) and July 19 (Z Shot 1757). The purpose of the three experiments was to obtain previously unavailable information related to pit aging.

The combined strategies to choose the liquid deuterium EOS problem as the first objective in the Z shock wave program and the decision to proceed with the development of a shock wave capability on Z for studying toxic materials have paid countless dividends. The resolution of the deuterium EOS controversy put Sandia and Z on the map in the Defense Programs community. The first ramp loading experiments on Pu solidified Sandia’s position in the Stockpile Stewardship Program and enhanced the funding for shock wave research at Sandia in the coming years. In addition, the containment system is now opening doors into whole new business lines, enabling Sandia to bring in an entirely new customer base with highly recognized national needs. Without that system, the problem set for shock wave studies on Z would still be Defense Programs-centric.

Recognition of the role that the Z facility has played in advancing the understanding of nuclear weapons performance is emphatically demonstrated by a FY 2012³⁰ Senate Energy and Water Development Appropriations Bill,³¹ which recommended additional funding to support nuclear weapons research at Sandia:

Science Campaign.—... The Committee commends Sandia National Laboratory for successfully and safely performing two plutonium experiments at the refurbished Z facility. The Committee understands that these experiments yielded fundamentally new and surprising data about the behavior of plutonium at high pressure and this new data [sic: have] been one of the most

³⁰ The fiscal year is the funding cycle for government agencies and begins on Oct. 1 of each year.

³¹ Senate Report 112-075 (Government Printing Office, 2012), p. 101.

valuable contributions to the stockpile stewardship program. The Committee continues to strongly support the weapons physics activities at Sandia's Z facility that are critical to sustaining a safe, secure, and effective nuclear stockpile.

A lot has happened with respect to experiments with toxic materials on Z, particularly plutonium, since completion of Z's refurbishment at the end of FY 2007 and will continue to happen. Through the end of FY 2013, ten more Pu shots have been conducted since the refurbishment of Z. Moreover, the containment system has become a lot more complex than it was for the first three Pu shots in FY 2006, before Z was refurbished. Containment now includes negative-pressure tenting and other added safety precautions. The Z plutonium experiments continue to yield new, surprising data about the behavior of the material. On May 15, 2013 the current delivered by Z was increased 45 percent, thereby enabling the highest pressure ramp (that is, shockless) compression experiment ever obtained for plutonium. This accomplishment required very precise pulse shaping at 16 MA currents. The new data will reduce the uncertainties in complex simulation codes that are used to assess the safety and performance of the Nation's weapons stockpile.

7.7 Compact Pulser - Veloce

Although Z experiments provide invaluable data at ultra-high ramp pressures (up to 4 Mbars) and shocks (to about 40 Mbars) that are difficult to obtain by other means, the experiments are extremely expensive. Scientific and weapon requirements exist for ramp loading data at more modest pressures (less than 500 kbars). Recognizing this need, Clint Hall initiated a program in 2004 to build a pulsed power generator for ramp and shock studies for low-pressure studies that are especially oriented to the Sandia weapons program. The first pulser, built under contract by a French organization and called Veloce, became operational in 2006. Both shock and ramp loading techniques have been perfected on that machine (shown in **Figure 7-10**), which generates up to 3 MA of current with a rise time of about 450 ns. High quality ramp loading has been achieved on several metals to peak pressures of about 170 kbars with operational costs similar to those for gas gun studies (Ao *et al.*, 2007, 2008; Asay *et al.*, 2008, 2009, 2011). Veloce has also been used to launch flyer plates to velocities of a few km/s, although these velocities can be easily obtained with more conventional techniques.



Figure 7-10. Photograph of the Veloce pulser used to produce magnetic ramp loading to peak pressures of about 200 kbars.

Veloce has also made significant contributions to the shock wave program on Z by providing a low-cost test bed for new experimental approaches before transferring the concept to Z. An example is the stripline configuration to produce magnetic loading, which is illustrated in **Figure 7-11**. The method provides better pressure uniformity across the sample surface and is not subject to misalignment effects, as with the ICE cube used on Z (see Figures 7-1 and 7-2 in Section 7.3). That method is also more efficient for producing higher pressure loading for a given applied current and allows more accurate analysis of rate-dependent materials by measuring the panel velocity history directly opposite the sample. This technique was first developed and perfected on Veloce,

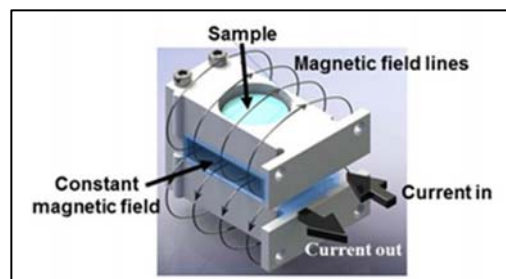


Figure 7-11. Stripline technique developed on Veloce for ICE experiments. Samples are placed opposite each other or the input particle velocity history is measured on one panel and the sample response on the other. This provides a direct measure of the input pressure history to the sample.

starting in 2006, and then transferred to Z with minor modifications. The stripline configuration is commonly used for very high pressure measurements and for achieving the highest flyer plate velocities. The configuration, as implemented on Z, is shown in Figure 7-4 in Section 7.4.

Another example of technology transfer to Z is a unique heating technique developed by Jack Wise on Veloce (Shaw *et al.*, 2012), which allows samples to be preheated to several hundred degrees before ramp loading. This technique was transferred to Z after it was first perfected on Veloce, resulting in substantial savings during development. The high temperature experiment, Z Shot 2189, was conducted on a beryllium sample, heated to 656 degrees C and ramp loaded to high pressure (Wise *et al.*, 2014). Wise also designed a soft recovery fixture and implemented it on Veloce in anticipation of its use on Z (Chantrenne *et al.*, 2010).

7.8 Effects of Shear Stress on Granular Material Compaction

The study of combined longitudinal and shear loading at Sandia began with the work of Lalit Chhabildas and colleagues in the early 1980s (Chhabildas and Swegle, 1980; Chhabildas and Hardy, 1982). The technique used normal impact of a Y-cut quartz crystal to produce combined longitudinal and shear waves that were transmitted to a sample bonded to the crystal rear surface. This technique was effectively used to study several inert and energetic materials (Chhabildas and Swegle, 1982; Chhabildas and Kipp, 1985). The main drawback was that the maximum longitudinal stresses were limited to about 30 kbars, the elastic limit of quartz. Impact techniques were needed to extend the impact stresses to higher values. A slanted, *i.e.*, oblique, impact technique originally developed at Brown University had been developed for this purpose (Duprey and Clifton, 2000). However, it was not until about 2010 that the slanted impact technique was implemented at STAR (Vogler *et al.*, 2011).

To accomplish this, Lundergan's original projectile launcher, referred to as the *precision impact launcher* (Barker, 1961; Smith and Barker, 1962), was transferred to STAR in the 1980s and converted into a slanted impact gun. In this configuration, a straight slot is milled along the interior of the gun barrel and a projectile is fit with a key to ride in the slot during launch. The key prevents the projectile from rotating, thus allowing the impactor, which is mounted at an oblique angle, to strike a sample at the same angle. Upon impact, individual longitudinal and pure shear waves are produced in ratios determined by the oblique angle. This approach allows direct measurement of shear strength at a given longitudinal stress. The technique is referred to as the compression-shear experiment or, more commonly, the pressure-shear (P-S) experiment.

Figure 7-12 shows the P-S configuration. Both the normal and transverse velocities, which can be related to shear strength, are measured with two off-axis VISARs, designated as V1 and V2 in Figure 7-12. Addition and subtraction of the VISAR signals allow determination of the two velocities.

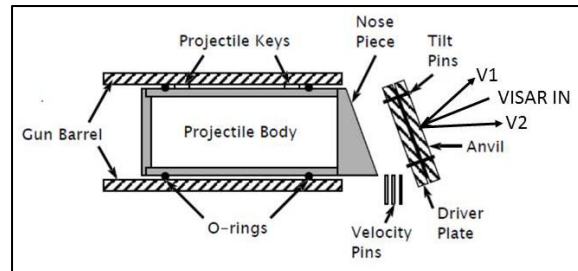


Figure 7-12. Slanted impact technique for producing pressure-shear loading of granular materials. The granular sample is contained between the driver plate and anvil. A normal laser beam is incident on the anvil, and the off-axis VISAR beams, V1 and V2, are used to determine the longitudinal and shear particle velocities.

Tracy Vogler and his colleagues have perfected this technique for use at STAR and have applied it to several investigations of high-pressure shear strength. Vogler has also used it to study the longitudinal shock and shear compaction of granular metals (Vogler *et al.*, 2011). A notable result is that normal shock compaction of tungsten carbide (WC) powder does not change the average granular size for shock

stresses to about 45 kbars because of the high strength of WC. However, if a 2-kbar pure shear stress wave is transmitted through the granular material after longitudinal loading, the particles are pulverized to a size about ten times smaller than that obtained for longitudinal compression alone (Vogler *et al.*, 2011).

7.9 Magnetically Applied Pressure-Shear (MAPS) Experiments

Immediately after the ramp loading technique was demonstrated on Z, there was a concerted effort to develop experimental techniques for obtaining material strength information from unloading wave profiles. Early results were demonstrated on Veloce and then extended to Z (Asay *et al.*, 2008, 2009, 2011; Ao *et al.*, 2009). Justin Brown significantly extended the technique for use on Z, both in terms of peak pressure and accuracy of the strength results (Brown *et al.*, 2013). Figure 5-1 shows the results of his work on the flow strength of tantalum to 2.5 Mbars. An intense effort has been initiated to develop a technique on Z for *direct* measurement of flow strength at high shockless pressures. This effort began in 2007 through an LDRD project led by Scott Alexander that is based on the pressure-shear technique developed for gas gun studies. The basic concept has been applied to magnetic loading techniques.

The technique was first conceived and implemented on the Veloce pulser. In this approach, a quasi-static magnetic field of up to 10 Tesla (100,000 Gauss) is imposed for several milliseconds on a stripline configuration containing the sample before the pulsed current from Veloce is applied. This external field saturates the conducting stripline and the sample. Upon application of the pulsed current at peak quasi-static field, a normal longitudinal stress is induced on the sample assembly from the applied current, as described earlier, and a transverse stress is also produced in the driver panel. The quasi-static external field interacts with the current to produce a transverse motion in the driver plate, which induces a shear stress in the sample if the shear strength of the driver plate (typically molybdenum) exceeds that of the sample and the shear stress is transmitted across the interface. The shear stress transmitted through the sample equals its shear strength at the corresponding longitudinal stress. The sample shear strength can be determined from the measured free surface velocity of an elastic anvil bonded to the sample. Again, this is dependent upon the shear stress being transmitted across the interface between the two.

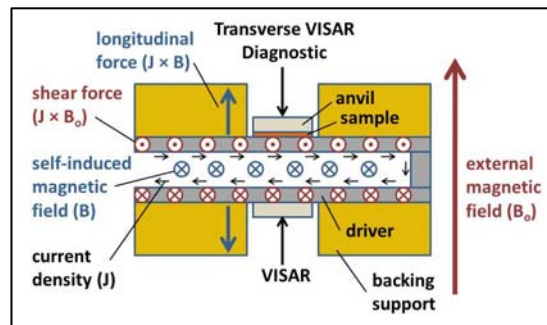


Figure 7-13. The magnetically applied pressure-shear (MAPS) stripline technique for inducing normal longitudinal stress (pressure) and shear loading on a thin sample. Off-axis VISARs are used to determine the longitudinal and transverse particle velocities, as for the slanted impact technique.

Figure 7-13 shows the concept for producing combined longitudinal stress and pure shear. A dual-VISAR system is used to determine both the longitudinal and transverse velocities and thus the shear strength of the material at the applied longitudinal stress. Although development of the concept on Veloce was extremely complex and required many experiments to resolve the pulsed power, instrumentation, and material issues, these problems were resolved and demonstrated in MAPS experiments on aluminum (Alexander *et al.*, 2010). The technique has been transferred to the refurbished Z and is being evaluated for performing P-S experiments on Ta.

7.10 People and Places of the 2000s



Jim Ang, Department Manager, 2011



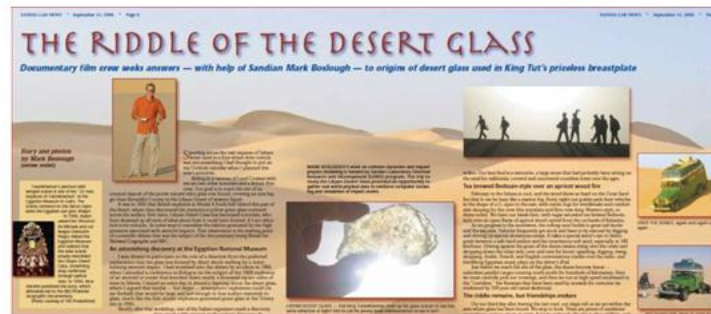
Dillon McDaniel, Vladimir Fortov, Jeff Lawrence (left to right), 2003



Chris Deeney (left) and Don Cook at NTS gun facility, circa 2005



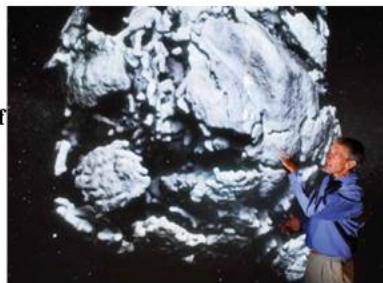
Lalit Chhabildas, Department Manager, 2007



Lab News, 2003 - Mark Boslough seeking meteorite impact-produced desert glass used in King Tut's breast plate



Jim Bailey, Distinguished Member of Technical Staff



Mark Boslough explaining the Tunguska asteroid impact mystery, 2008



George Samara (left) in lab, 2003



Wayne Trott, Distinguished Member of Technical Staff



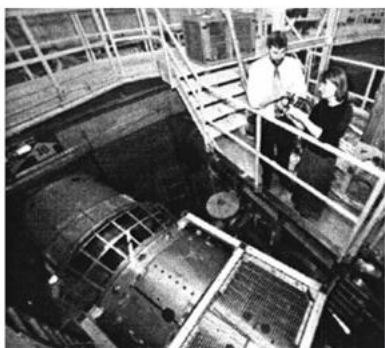
Steve Rottler receiving DoD weapons award, 2004



Bill Proud, Mike Furnish and Linn Furnish (left-right)



Tim Trucano, Senior Scientist, 2006



Chris Deeney (left) and Christine Coverdale at Z Machine



Jim and Pat Asay, 2005



Allen Robinson,
Distinguished Member
of Technical Staff



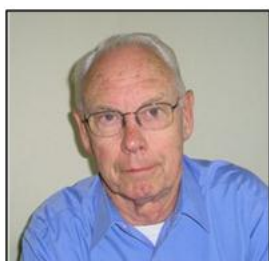
Alita Roach (left) and Bill Deal
(center), 2005



Clint and Juanita Hall, 2005



George Samara, Senior
Scientist, 2004



James N. Johnson, Gig
Harbor, WA, 2010



ALEGRA team members, left to
right, Dan Carroll, Allen Robinson,
Kent Budge and Sue Carroll, 2004



Keith Matzen (back) explaining
Z to a Japanese visitor



Jack Wise (left) and Bill Reinhart, 2013



Jeff Quintenz at Z center section

Appendix A

Personal Recollections (selected samples)

Editors' Note

The individual recollections present a window into the personal experiences of people who participated in the shock wave program at Sandia. We made a strong effort to contact and encourage as many people as possible to participate. Over 80 people were contacted and about 40 provided recollections of their personal experiences. Each contributor was asked to provide a summary of their role in shock wave research at Sandia, bringing out any interesting events or anecdotes that happened along the way. We purposely gave very little guidance on style and format, which is why some recollections are written as a 'stream of consciousness,' while others are more technical and consist of short annotated summaries of major papers by the submitter. The result is a rich and interesting mix that highlights individual personalities, personal struggles, and technical successes. The dates provided for each person represent his or her hire date at Sandia and the date he or she left the shock wave program. If this is different from the retirement date, the retirement date is also noted. Quotations from the individual recollections are liberally used throughout the text to bring out the personal aspects of shock wave research at Sandia and to provide a pointer to further reading of the recollections. In some cases (*e.g.*, George Samara and Walter Herrmann) the recollections of others were used to capture the seminal contributions that these researchers made to the shock wave field. In total, the recollections are a unique glimpse into the shock wave program at Sandia. A short montage of photos is also given at the end of each chapter to associate 'faces' with the recollections. These photos often tell a story in themselves.

**Contributors to
Recollections of Shock Wave Research
at Sandia National Laboratories
(Recollections in red are included samples)**

James A. Ang	
James R. Asay	
Melvin R. Baer	
Lynn M. Barker	
Mark B. Boslough	
Barry M. Butcher	
Albert J. Chabai	
Lalit C. Chhabildas	
Michael J. Desjarlais	
Douglas S. Drumheller	
Michael D. Furnish	
Dennis E. Grady	
Robert A. Graham	
Thomas A. Haill	
Clint A. Hall	A-5
Dennis B. Hayes	A-15
Walter Herrmann	
Eugene S. Hertel, Jr.	
Roy (Red) E. Hollenbach	
James N. Johnson	
Charles H. Karnes	
James E. Kennedy	
Marlin E. Kipp	
Marcus D. Knudson	A-19
Carl H. Konrad	
R. Jeffery Lawrence	
Raymond W. Lemke	
C. Donald Lundergan	
Peter C. Lysne	
J. Michael McGlaun	
Steven T. Montgomery	
Bruno Morosin	A-25
Darrell E. Munson	
Raymond P. Reed	
William D. Reinhart	
George A. Samara	
Karl W. Schuler	A-31
Herbert J. Sutherland	
Samuel L. Thompson	
Wayne M. Trott	
Timothy G. Trucano	
Tracy J. Vogler	
Jack L. Wise	

Clint A. Hall**Clint Hall and Shock Physics.....Who would have thought it??****1984 – Present**

I arrived at Sandia fresh out of New Mexico State University with a Bachelor of Science in Mechanical Engineering Technology. Having interviewed a few months earlier with several organizations at SNL, I had narrowed my choices to two, pulsed power in Area IV and the STAR facility. My choice was made when Jim Asay (Department Manager) and Carl Konrad (STAR facility Supervisor) convinced me that I could become the “go-to person” for all things mechanical after a “small learning curve.” Suffice it to say that ‘small’ is a relative term. When I arrived on site in June of 1984, the first thing I learned was that I actually knew NOTHING at all about what a shock wave was, how they were made, how they were used, or why anyone even cared so much about them. I did know, however, that the STAR had a good machine shop and a welder! What more could a mechanical guy ask for?

The first thing that ‘glazed my eyes over’ was Carl trying to explain a Velocity Interferometer System for Any Reflector (VISAR). I got velocity and then was lost from interferometer on. He started talking about delay legs and interference and etalons, and everything just started to go gray in my mind. Similar things happened when Carl and all the other electronics types out there started talking about digitizers, terminators, signal generators, time marks....good grief, I had to get over to the launchers with their fluid flow, their gas expansions, their debris mitigation, their energy absorbing tanks before I felt like Jim actually hadn’t made a HUGE tactical error in hiring me. Before long, I actually understood the guns and their operations and could kind of set up an experiment on them as long as I had some ‘help’ from the other STAR techs. Maybe it was because I was starting to catch on, or maybe because Jim and Carl wanted someone that thought in terms of steel rather than electrons for diversity sake, but I was eventually named Second Alternate Supervisor and Pressure Advisor at the STAR. Felt pretty good about myself right about then because my career goal of becoming the STAR facility Supervisor when Carl retired was on its way to becoming reality!

After taking over responsibility for the single-stage compressed gas gun (‘airgun’) that nobody really cared about and making some improvements that actually made it more user-friendly, more reliable, and look more professional, Jim apparently decided I couldn’t screw things up too badly and actually gave me a higher profile project...my first REAL one at STAR. I was to be the Mechanical Lead for the two-stage gas gun injected railgun (STARFIRE). Hey, I could handle this right...simply couple a hydrogen-filled gun with a delicate projectile that doesn’t like joints and moves at about 6 km/s to a stationary, composite bore hunk that had to be absolutely pristine to avoid pre-arc formation, having a bore that was kinda the same diameter most of the time, needed to have a 10^{-3} torr vacuum, no projectile blow-by, oh, and measure the projectile velocity right before the railgun section so you could dump the electrical energy at the right time. See what I mean...piece of cake! To make matters even more interesting, collaborate with two guys from LLNL (railgun experts) while doing all this. Well, we got the two-stage injector up and operational, modified a tank from the STAR ‘boneyard’ to work as the impact chamber, and figured out how to make and align joints that actually passed projectiles through them instead of turning them into debris clouds. The problem left was the projectile ‘blow-by.’ Without solving this problem, in-bore velocity measurements could not be made, and the railgun section would pre-arc on every firing. I remember SNL was hosting the Aeroballistic Range Association (ARA) Meeting in Albuquerque around 1988, and I was one of the highly technical guys that got to help put up and take down the chairs each day. As my reward, I got to stay and listen to the talks. I also remember being ‘intently engaged’ in them to the point of my mind wandering to the projectile blow-by problem. And that was when the seal design came to my mind. I sketched it out and explained it to Lynn Barker, who said “Sounds reasonable...” I

figured out how to machine it and made one for a test. Well, to make a long story short, it worked the first time out and became one of the enabling technologies for the STARFIRE project. Another projectile innovation that led to a repeatable, self-arcing projectile included the silver-epoxy-loaded fuse (Jim's idea to use the epoxy that would ablate and create a self-timing, initiating arc) with a HIGHLY sophisticated system of Scotch tape for the mold. All these concepts eventually went into a patent granted in June 1993 entitled "Hybrid Armature Projectile." After several years of effort, however, the best velocity the launcher system was able to achieve was ~7.5 km/s with an injection velocity of ~6 km/s, well below the project goal of 15 km/s. After reviewing the rate of major successes vs. incremental ones, the amount of dollars invested, and prospects for major advances, Jim Asay made the executive decision to shut the project down.

An alternative HyperVelocity Launcher (HVL) technology was being explored at the time utilizing momentum transfer to impart the energy from a 6-km/s, two-stage projectile with a variable density impactor to a lighter stationary plate mounted in an extension at the end of the launch tube. Lalit Chhabildas and Bill Reinhart were working on this, it looked interesting, and the railgun program had just been shut down, so I decided "that project might be fun to work on" since they were already asking for my help anyway. Before too long, the projectile manufacturing and repeatability, alignment fixturing, plate fabrication techniques, and two-stage performance had all improved. The project was eventually able to launch flat, intact plates capable of reasonably precise equation of state (EOS) measurements. One of the things I was most proud of was a method to eliminate the use of Glad[®] sandwich baggies as an integral part of the HVL plate installation into the barrel extension. By actually machining the part to the correct dimension, you could avoid the use of a 'highly-precise and repeatable' flat disk cut from a 'sandwich baggie' to provide the interference fit required to keep the disk in place. Yeah, they needed help!

Two things occurred in 1994 that had a profound effect on the direction of my SNL career. Discussion was taking place at the national level around closing one of the three nuclear weapons laboratories. Having a Mechanical Engineering Technology degree which, though useful, was less marketable than a standard Mechanical Engineering degree, caused me to rethink going back to school part time, in case SNL was the lab that would be closed. I had decided to do so under the SNL University Part Time program when the second event occurred, hereafter referred to as 'Black Monday.' At the time, there were approximately ten Sandia employees and about eight contractors working at the STAR. Jim Asay had been promoted to a Level-2 Manager and was no longer associated with the site. Our Department Manager, Phil Stanton, called out to the STAR and told Carl that he was holding a MANDATORY staff meeting in Area I for all Sandia employees, and there were to be no excuses to miss it. This was in early December, everyone was getting ready for Christmas, and nobody knew what the meeting was about. All the STAR tech folks piled into the van and headed into the area. When we got there, the entire department staff had already assembled in the meeting room, which was now standing room only, and there were plenty of hushed whispers going on. Phil stood up and proceeded to say the following. Effective immediately, the shock physics department was being disbanded due to a decision by the Director Ed Barsis. He told all the STAR techs that we were to do nothing now but concentrate on finding another job at SNL. The contractors would be given a month to be placed elsewhere, and the staff members were to be reassigned throughout the Center. You could have heard a pin drop. I have never seen such shock on a group of people's faces in my life. As people began filing out of the room, Phil told Carl and me to stay. He told us that we were to finish up all the work that the STAR was committed to, and had been paid to do, then officially close and mothball the site. It was a dark day indeed. Carl and I reminded Phil that a minimum of three people were required by approved Safe Operating Procedures to operate the site. He allowed us to keep Bill Reinhart on as a contractor until the work was completed. As people began leaving for other jobs, Bill, Carl, and I decided not to throw in the towel just yet. We kept executing the experiments, but we also began to 'hide' the equipment that would be required to restart the facility, and didn't let people take anything from that 'stash' that were backing up with trucks hauling

Appendix A: Personal Recollections

equipment and supplies away for other efforts at SNL. We also realized that the ONLY way we were going to be able to save the facility was to get Jim back involved. We met with him several times and he agreed to help us find a new SNL sponsor. The Explosives Component Facility (ECF) stepped up to the plate and took ownership using a new 'campaign mode' of operation that reduced facility costs. The STAR was saved, although hanging on by a thread. Carl and I had found other jobs, so Bill and his contract were transferred to the ECF.

Jim continued looking for a more permanent solution and began discussions with Don Cook, the Director of Pulsed Power Sciences and owner of the recently reconfigured Z machine at Sandia. Don had recently commissioned an independent review of his inertial confinement fusion (ICF) program, which funded Z and its operation. In that review, he had been told to develop additional uses for Z other than just capsule implosions. Interestingly, work at LLNL on the Nova laser had been showing some potential application for using high energy light to drive high pressure shock waves, even though the pulse lengths were very short. Don felt that SNL could do something similar with a hohlraum on Z for measuring the dynamic response of materials. Don knew, however, that for a serious program to develop in his Center, Jim would have to head it up, so he was finally able to arm twist Jim into becoming a manager in his Center and begin to build a dynamic materials characterization effort.

Now Jim is a smart man, but so was Don. Don gave Jim just enough money to get him to come over, but not enough to support a department, so Jim had to look for someone that was cheap (he ruled out all the staff members with that one), had some knowledge of conducting material response measurements, and would be willing to take a chance on shock physics at SNL after having just gone through Black Monday recently. Who might Jim have found that had half his time already paid for by the University Part Time Program, thought he (Jim) walked on water, and would be dumb enough to consider this line of work again? Well, it still took a call from Don Cook to assure me that he was serious about this effort before I decided to come back! Anyway, Jim and I formed the new shock physics effort at SNL, and we were having to do it with pulsed power instead of conventional launchers.

At the time all this 'department building' was beginning, Z was in its infancy and going through growing pains. Everyone that worked on Z felt the machine had tremendous potential, there was a teaming attitude, and that atmosphere posed opportunities for Jim's and my shock physics efforts. We began putting infrastructure in place for optical shock arrival sensors and VISAR systems, but encountered and subsequently overcame a number of obstacles like precision in timing mandated by small sample sizes, changing of the index of refraction in optical fibers from the inherent radiation, decaying pressure pulses, and non-uniform ablative loading. As we were solving these issues and becoming recognized in the community, Jim and I began adding folks to help establish this effort, and also began strategizing for the future. I was beginning to understand the research and science of shock physics because Jim and Lalit were teaching it to me as I worked on my Master's Degree at the University of New Mexico, so we both realized the difficulties inherent with ablative loading and knew it was of limited use, but we had no other options at the time for creating those pressures on Z. As Jim and I pondered solutions to the ablative loading problem, I continued working on my thesis, which looked at understanding the dynamic response differences in concrete as a function of aggregate size under Lalit's direction. The resulting publication won the Aeroballistics Range Association Prize for best student paper in 1998 and allowed me to complete the requirements for an M.S. in Mechanical Engineering from the University of New Mexico in 1999 and, with the degree, to be promoted to a Member of the Technical Staff.

Being the 'good citizens' that we were, Jim and I decided to help Rick Spielman (a pulsed power researcher) make a pressure measurement on Z that he felt confident he could accurately convert to current (the measurement he was really after). To do so, Jim and I placed two small disks (one copper and one iron) into the anode plate on a wire array load and then attached shielded VISAR diagnostics to the rear of each to measure their respective free surface velocities. As it turned out, the Z shot was fired

late on a Friday afternoon, with the raw VISAR data saved and delivered to me to be analyzed on the following Monday morning. I made it until Saturday around lunch time before I had to head into work to see what we recorded. I sat down, opened the files, and reduced them. When I had done so, I got a very nice trace from the iron, but it had an odd feature I couldn't understand. It just so happened that Lalit was in his office working too, so I went down and showed the trace to him. He immediately thought that it might be clearly showing the alpha-epsilon phase transition in the iron, but couldn't remember at what pressure it was supposed to occur, so we jumped into the company truck and headed into Area I hoping to find someone that might be there and remember. The best we could do was to find a viewgraph on a poster outside a door (that we just had to 'borrow') showing the iron transition from a gun experiment. Sure enough, they correlated, and the birth of magnetic loading for ramped wave experiments on Z gained credibility.

Jim and I recognized that there were still many issues to be resolved, but this promised to be a gold mine! We began presenting results at conferences in invited talks, contributed talks, and poster sessions to get the word out about what we were doing and where we felt we were headed. We also began taking a careful look at pressure gradients, diffusion, and analytical techniques. With a technology that had so much promise, but so many details to understand before it could become accepted as a routine method for gathering quasi-isentropic material response data, we knew we needed more help. Jim had taken back the STAR facility from the ECF and, with it, Lalit. Because both of them needed help, I was being forced to choose between STAR projects and those on Z by these two task masters. The choice had to be made between working on proven technologies at STAR with a reasonably steady funding stream under Lalit, and unproven, though possibly revolutionary, technologies on Z under Jim that did not have steady funding and might never amount to anything. Once again, I cast my lot, career, future, and ability to feed my family with Jim on Z, a choice that fortunately proved once again to be the right one many, many times from that point on.

As word about the ramp loading began getting out, our funding began to improve, and with it, the opportunity to begin adding staff. Two additions were pivotal to the future of shock physics at Sandia. The first was adding Marcus Knudson, a very promising staff member fresh out of Washington State University (WSU), and the other being Chris Deeney. Chris was a plasma physicist on detail at Headquarters (HQ) when Jim began talking to him about coming back to SNL as a manager of the newly re-acquired STAR group. Even though Chris did not have a materials background, he was very bright, young, politically correct, and looked like he had all the skills to be a good manager and leader. Chris did indeed join our dynamic materials team, and he and I very quickly became rather co-dependent.

Jim knew we needed some theoretical help to understand the magnetohydrodynamics of the ramp loading process and got it through Art Toor and David Reisman from LLNL. We got that help at an SNL meeting with Jim, Art, David, myself, and a few others from our Z pulsed power groups where we were discussing solutions to the non-uniform current loading from radial convergence of the current as it flowed over our samples mounted flush to the lower surface of the anode plate. Two 'eurekas' occurred in this meeting that made ICE loading what it has become today. The first was that the samples could be mounted vertically on the 'current return can,' and the second was that the can could be square and constructed out of panels (my idea). I worked with the target designers to come up with a panel arrangement that would allow diamond-turned surfaces, and self-indexing corners for construction. We tried it out and, *voilà*, it worked the first time out! At last, the birth of another enabling technology that had an enormous impact on shock physics at SNL....my baby, the isentropic compression experiment (ICE) cube:

C. A. Hall, J. R. Asay, *et al.*, "Experimental configuration for isentropic compression of solids using pulsed magnetic loading," *Rev. Sci. Instrum.* **72**, 3587 (2001).

Soon after Marcus had arrived, I was giving an invited talk at the American Physical Society Division of Plasma Physics meeting on the ICE cube and our ramp loading technique. Jim, Marcus, and I were sitting at a table between talks, and we began discussing another whole concept. Instead of transmitting the ramp loading directly into the sample of interest, we thought that we could instead launch the bottom of the panel as a flyer plate and impact our samples to obtain high pressure Hugoniot data. We did what any self-respecting physicists would do at that point and drew a sketch out on a cocktail napkin, stuffed it in one of our pockets, and went to our next talk! When we got back to SNL, we did indeed retrieve the ‘highly precise scientific sketch,’ did some calculations, performed the experiment, and we exceeded years of HVL development on our first shot with a metallic flyer that was good enough for EOS experiments moving at ~10 km/s! My choice to cast my lot with the Z technology when I was forced to choose between Jim and Lalit was quickly being validated. The first paper on the magnetic flyer plate technique was given in:

C. A. Hall, M. D. Knudson, J. R. Asay, *et al.*, “High Velocity Flyer Plate Launch Capability on the Sandia Z Accelerator,” *Intl. J. Impact Eng.* **26**, 275-287 (2001).

With the department, personnel, technology, funding, and future taking shape nicely, the one and only real disagreement that Jim and I ever had took place. After announcing his impending retirement, I asked Jim about his views on my taking over the department as manager. He felt that without a Ph.D., he could not support my doing so. Obviously, I felt otherwise and decided I had to find some other group that would allow me to continue advancing. I had to make a choice between two offers. One at SNL in the Radiation Effects Department, and the other at the Nevada Test Site (NTS) running the Atlas Project, which was to disassemble a Z-class pulsed power machine that had been built at LANL, take it out to the NTS, rebuild it, run it, and make it successful. Ultimately, I decided to stay at SNL and actually ended up working in the same building as when I was in Jim’s group, just three floors up! It was a very difficult time for both Jim and myself. I could not make myself move out of my old office completely, and just kind of kept it ‘as is’ for several months to maintain contact. I think I was having separation anxiety from Jim, who had been such a fixture in my career for so many years!

After several months during which Jim and I didn’t talk much, Chris Deeney came to me and offered me a job in his materials group, promising me that I would be considered if his job opened up after Jim retired. I agreed and came back to the group after only ~6 months time. Chris’s number one priority for me when I returned was to take the struggling containment chamber project and make it successful with minimal funding and a lot of cards stacked against us from both the other labs and our own internal management. This project proved to be a perfect way for me to demonstrate and improve the meager management skills I had at the time. There were three groups designing separate parts of the system, little communication, significant technical challenges, an unfocused testing plan, a requirement that it work perfectly each time it was used, and a lot of skeptics everywhere we looked. Chris, being the good guy that he was, washed his hands of it and threw it squarely into my lap. The sad thing is that I loved it, and I suspect he knew I would feel that way when he handed me the project. The design groups and I had several ‘come to Jesus’ meetings where I took control of each design effort, forced communication, made some hard and unpopular decisions about the testing plan, and we finally started making significant progress. After several years of work, we were ready to test the first full system on Z. We fired the shot, and all real-time indicators suggested that it functioned properly, but the only way to know for sure was the much anticipated helium leak test. We pulled the system from the machine and hooked it up to the detector. Suffice it to say that I did the ‘happy dance’ when there were absolutely no leaks! The system worked as well or better than we had hoped, first time out. This turned out to be yet another enabling technology that truly provided a future for Z and the SNL shock physics effort, even if we didn’t fully understand it at the time.

By now, Jim and I were ‘back to normal’...thank god...and he introduced me to John Taylor, someone that would end up having a HUGE impact on my life and career at SNL. Interestingly, he was

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one of Jim's neighbors and they rode to work together most mornings in a van pool. Well, it turns out John managed a large program at SNL that was highly classified, and Jim became one of his principal investigators for a piece of it having to do with material response. As Jim was getting closer and closer to retirement, he began to pass control of some of his projects off to others. The 'Taylor work' was given to me. All of a sudden, I was forced to get all kinds of security checks, clearances, sign away my first born, and learn how NOT to talk to anyone about what I did on this project. Quickly though, John liked how I managed the part I was given and expanded my role and control to be a much bigger part of his program. I really enjoyed the work, and was becoming exposed to another whole customer set and way of thinking and conducting research. It turned out to be very fast-paced, results oriented, and of immediate impact to the Nation.

The dreaded day had finally arrived: Jim actually retired and moved off to work with Yogi Gupta at WSU. I don't think there really was much of a decision to be made at that time, but thankfully they gave his Senior Manager's job to Chris, who had taken over as Manager of the 'Z department' and Lalit backfilled Chris's job as manager of the 'STAR Department.' All this movement left the manager of the Z department open, and Chris put it out to bid after several months. Once again, it was decision time for me. Did I feel qualified to run this fast-growing technical effort that had so much promise, so many new young staff, and so little Center management support in reality? What if I couldn't do it....what if I failed to provide vision, keep the staff, and/or maintain the funding? This wasn't just characterizing materials using tried and true technologies and analysis methods like would be done at STAR...this was WAY outside my comfort zone. Back in the day, I KNEW I could run the STAR, and I felt pretty strongly that I could run the STAR Department, but this....this was way more 'research-y' with much more at stake. I finally made my decision based on two things. One, that Chris must have felt I could do it because he highly encouraged me to bid on it, and two, that he would be there to "help me" if I looked like I would fail. I finally did bid, and Chris gave me the job.

You have heard the old adage, 'When it rains it pours'? Within three months of my having accepted the Department Manager position for Chris, John Taylor and the HQ Program Technical Advisor Bruce Boughton wanted to meet with me. Turns out that John was taking a new job working in a Vice President's office and didn't feel he would have time to continue running his program at SNL, so he asked me if I would consider taking it over. I truly enjoyed the work that we were doing on this program, the successes we were having at SNL, but I wasn't sure I had time to make both it, and my new Department, successful. I explained all that to them, but they were insistent that "I was the guy" they wanted to take over. How do you turn something like that down.....you don't.....so I accepted the offer. How hard could it be, right? I was about to find out.

The good thing about working for someone that is a REALLY good boss (Jim and Chris both were, but I am referring to Chris in this instance) is that he takes you along to all the meetings, you learn management skills, you have a security blanket, someone to encourage you. There is, however, a down side as well. You aren't the only one that recognizes that. Yes, you guessed it, Chris got a job offer back at HQ to run the Science Campaigns, which had become the major funding source for not only dynamic materials work at SNL but also much of the pulsed power mission.

Fortunately for me, the Shock Physics on Z Department and the activities in the Dynamic Materials Properties (DMP) subprogram were in pretty good shape when Chris left for HQ. As a team, he and I had started down several strategic roads including the ability to do contained hazardous materials on Z, the authorization basis for conducting Pu characterization experiments using that system, solid DMP base funding, the concept for a compact ICE driver, and a reasonably good, diverse staff. Of course, having grown up in this group, I did know about some of the 'issues' that were less apparent and I actually did have some strategic thoughts of my own (imagine that!). Anyway, with a good base, and Chris positioned in the Defense Programs Science office back at HQ, things were looking pretty good for our future in Department 1646 (Dynamic Material Properties).

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Several things still needed some work before they were actual mature capabilities that we could use for problem solving though, so we began working them in parallel as a group. The first of these was shooting Pu on Z. The authorization basis had to be completed before we could do the work, the targets had to be designed, the transportation issues had to be addressed, we needed LANL buy-in, the waste stream..... The list was long. Designing and shooting a material response experiment like we were trained to do was the easy part, it was all this other legal stuff that was the headache! I assigned Jeff Gluth to do nothing but work the ES&H (Environmental, Safety and Health) and transportation issues, Marcus to do the experiment design work with LANL, and I got to do the coordination and the 'dog and pony show' presentations that went on *ad nauseam* after Chris had gone to HQ. To make matters worse, we were going to exercise the truly unique quasi-isentropic loading capability on this material instead of the relatively straightforward Hugoniot approach. So not only were we fighting the ES&H issues, another lab Associate Director saying, and I quote, "They will never shoot Pu on Z," all the folks in the Pulsed Power Sciences Center that didn't want to do these experiments (of which there were many...and vocal ones at that), we were also trying to pull off a home run technically on an incredibly challenging problem in a very visible experiment! Like I had said before, easy, right? Well, to make a long story short, we did shoot two Pu shots in three days with 'the world watching,' they were perfectly contained, and they went off without a hitch. Marcus and Dennis Hayes collaborated to make some nice deductions from the analyzed data, all the logistics went perfectly and lo and behold, we had done the seemingly impossible on a shoestring budget! This experiment was tremendously important as it not only provided some new insights into Pu behavior, it had secured SNL's future in DMP for many years to come.

Chugging along in the background while all this was going on were two other significant projects. Having worked with Randy Hickman in the past on the railgun project, I knew he would be the right guy to get the mini-pulser facility, Dynamic Integrated Compression Experiments (DICE) online. I was able to talk Area IV out of a building that was in dire need of a facelift but was perfect for this application. We were able to convince folks that the million or so dollars that it would take to design and construct this 'mini-pulser' would be well worth the cost when it became useful as a test bed for developing diagnostics and experimental techniques that would eventually be transitioned onto Z. We leveraged the Special Technologies Laboratories in Santa Barbara (funded through some indirect DMP dollars) to design and build us a state-of-the-art, air-delay VISAR system for the low pressure work we planned to accomplish in this facility, used hand-me down digitizers from Z, invested in some streak cameras, and attached an amazingly capable instrumentation room to the pulser. We also designed and built a small, cheap, and cheap to operate, single-stage gas gun for doing complementary shock work to the pulser's ramped compressions.

I needed a programmatic driver to justify the space and investments we were making in DICE and began targeting the SNL mission space of neutron generator (NG) design and production. The ties to this group had faded away over the years, but I felt we needed them so that the Vice Presidents in their line organizations would support our materials work. I also needed a very detail-oriented researcher that already had credibility with the NG community, documented EVERYTHING to the nth degree, and was looking to become the cornerstone for such work in this group. I knew what I had to do....go get Jack Wise and convince him to leave the Geological Mechanics group and come work for me. Well, it took a little 'fancy talking' on my part, but as it turned out he was looking for a new challenge and decided it might not be too bad to work for me. He moved into the group and this fledgling relationship that we as a program so strategically needed began to blossom. Jack was the perfect fit, but the SNL work was not quite enough to keep him busy. As it turned out, one of the problems that we really needed to work out for DMP was the ability to map phase lines using the isentropic compression capability. To do this, the obvious approach is to elevate the initial temperature of the sample to several states, compress the sample, and cross the phase line from these various initial states to get a description of its placement and slope in pressure-temperature space. Easy, right? Well, technically not so difficult for ramp compressions....but VERY hard for ramp compressions to about 600 degrees C on nasty materials inside a containment

chamber with micron-thick glue bonds that maintained perfect integrity at these temperatures. What's life without the occasional difficult challenge....

Everything was going along quite well for the group. We had an amazing team of researchers that all knew their roles, worked very well together, and bought into the strategic department vision. We had developed our own target fabrication and design team (both of which contributed mightily to our successes on Z and DICE), and significant technical advances were coming at a record pace. Our biggest challenge had become getting enough time on Z to try the new ideas Marcus, Dan Dolan, Jack, and Seth Root were conceiving of, while still characterizing the materials we were being asked to do for direct programmatic drivers. Z was getting very difficult to access for our increased needs, and DICE didn't have the pulse control we so desperately needed, nor could it access the pressures we wanted for much of the work, so the logical step was to build a 'mini-Z' at DICE. Such a machine would bridge the gap between the worlds and let us continue being innovative. We began development of such a machine and named it Genesis. It was nearly fully designed, and the infrastructure ready to accept it at DICE when I left the group in 2009.

One of the visions that Jim had, but was never able to fully realize at SNL, was to have all the shock physics activities housed within one department. At this time, there were two separate departments in the Pulsed Power Sciences Center related to the Z efforts and to the STAR group, a dwindling group that operated the 'Building 808 airgun' and the airgun at the ECF. Lalit was retiring, so I had the opportunity to combine those groups into one, which I convinced management to do. By doing so, we had a 'super-group' capable of kbar through multi-megabar pressure regimes and both Hugoniot and quasi-isentropic compressions that were rivaled by no others. In addition, I was able to hire Mark Anderson away from the 808 gun facility to work on my Nuclear Counterterrorism (NCT) Program in another group, so his old group was officially decommissioned. In effect, for the first time that I am aware of, all the shock physics at SNL was under one group, with me as the manager.

They say that success is a two-edged sword, and I have to firmly agree. As it turns out, both the materials group and the NCT Program were growing, becoming more diverse, and each was demanding more of my time. In reality, each was a full time job. To make matters worse, SNL began to demand more and more responsibility, accountability, and formality from its Level-1 managers. In short, the two jobs were becoming too much for me to do alone. The writing was on the wall, and I knew I would soon have to choose between the jobs, just as I had been forced to do years earlier with the STAR and Z career paths. It took a lot of soul searching, I can assure you. How do you walk away from such a functional group where all your strategic objectives over the previous 5-6 years were becoming reality, I was becoming knowledgeable enough to provide realistic technical input to such distinguished researchers as Marcus Knudson, Dennis Grady, and others from LANL and LLNL, and the future looked so bright? It was one of the most difficult decisions of my life, but I knew that the NCT mission was the one that would most benefit from my expertise, whose future was looking bright as well, and was of greater immediate importance to the Nation. Therefore, I chose to maintain leadership of that effort and stepped down as manager of the shock physics group in March 2010.

Although it is at a much different level, and I can't talk about it or publish much anymore, I still maintain an active role in the shock physics community as the national subject matter expert in dynamic material characterization for the NCT Program.

Final thoughts...

When I came to SNL in 1984, I had no idea what a shock wave was, much less that I would have any significant impact on such a field of study during my career. When I look back at the past 25 years in

Appendix A: Personal Recollections

which I was fully engaged in this research, I feel both fortunate and blessed to have had a role in several developments that eventually helped shape the SNL program. My contributions culminated in the opportunity to lead the effort for 6 years, building upon the strong foundation left to me by both Jim Asay and Chris Deeney, yet putting my own mark upon it as well. Just as Jim and Chris must have felt before me, I knew the strength of the group was in the people and that this effort would survive long after my leaving. I was right.

Special Thanks

There are many people that have had positive impacts on my shock physics career at SNL, but I am most indebted to three people. Jim Asay stepped out on a limb by hiring me, then believed in me as a technician and staff member, providing me with many opportunities to succeed that were not only challenging, but stretched my abilities as well. He also taught me most of what I know about dynamic material response and helped form my initial management style through his leadership and example. Chris Deeney believed in my managerial abilities and took risks to his own reputation by providing me with high profile opportunities that could either ‘make or break’ his department and possibly both our careers in this field--the most significant one being his decision to promote me to manager of this highly technical group. Chris also constantly took me to meetings with him where I learned another management style that would eventually become a significant part of my own, including how to mentor and advance others’ careers. Carl Konrad was a patient mentor and great friend, teaching me much of what I learned as a technician, helping me learn how to “just get it done.” He was supportive of many of my technical ideas and approaches, while making it fun to work at STAR. I am very pleased to count all as not only mentors, but personal friends as well. I would also like to thank Dennis Grady, Dennis Hayes, and Lalit Chhabildas as well for helping me to learn shock physics through very patient instruction.

Dennis B. Hayes

1959-1997³²

I just stumbled into this shock wave thing. In late 1959 I was working swing shift in the print shop at Sandia and that allowed attending the University of New Mexico as a freshman during the day. But they closed down the swing shift so I looked at the weekly job postings for union members and found a data reduction clerk opening in Byron Murphy's Division. This Division was involved in explosive cratering as part of the Plowshare Program (Al Chabai, Ron Carlson, Hunter DeVault) and in ground motion studies near underground explosions (Bob Bass, Luke Vortman, Bill Perrett, Dorris Hankins). Although this group was not directly tied to the emerging shock effort at Sandia, these application-oriented missions were two of the drivers for the growth of shock programs.

Members of this group were certainly well versed in shock basics: jump conditions, the Mie-Gruneisen equation of state (EOS), the equations of motion, thermodynamics, and so on and my exposure to those was a daily occurrence. And the staff stretched me out in those disciplines. I was becoming familiar with the 'finger exercises' of shock wave physics with the added benefit of gaining proficiency with the content of several math, mechanics and thermodynamics courses before reaching them in the classroom.

Al Chabai was a skilled user and proponent of dimensional analysis as a tool to organize measurements that did not lend themselves to modeling. For instance, his work on scaling of explosive cratering data gained international recognition and is still in use today. Being a good student of Al's work led me to try some similar analysis of my own circa 1964. This Sandia group, along with their LANL colleagues, had amassed hundreds of time-of-arrival measurements made very close-in to several dozen underground nuclear explosions. But the data remained largely unanalyzed owing to a lack of sophisticated modeling capability. It turns out that each underground test was assigned a yield based on radioactive isotope ratios gathered in post-shot gases. *i.e.*, a rad-chem analysis. Scaling both distance from and time-of-arrival of the shock by the cube root of the rad-chem yield produced a remarkable, low-scatter universal curve time-of-arrival graph that I proudly tacked above my desk. I was just a technician and too naïve to understand the importance of publication. This result soon found its way into the test community's 'public domain' and I was told with some improvement ultimately became one of the U.S.'s primary tools for monitoring compliance of the 1974 Threshold Test Ban Treaty.

In 1965, with my B.S. completed, I joined Al Chabai's newly formed division concerned with x-ray vulnerability of re-entry vehicles that were part of the Nation's nuclear deterrent. I developed the VANDAL x-ray deposition code that found wide use at the lab by component, system, and material organizations and that led to extensive contact with a wide swath of Sandia programs and people. Walt Herrmann had just arrived at Sandia and I worked with him and Pat Holtzhauser to implement an x-ray deposition capability (with rapid heating leading to pressure buildup and thus to shock generation) into his WONDY code. Much of the other work during this time entailed designing experiments for underground tests and analyzing the results. Classified reentry shield concepts for defeating x-ray attacks were successful.

³² Dennis Hayes had a varied career at Sandia involving both technical and management positions. He also had external assignments, including a year as Scientific Advisor at DOE Headquarters in 1990-91 and as President of Lockheed Martin Nevada Technologies in Las Vegas in 1996-1997. He retired from Sandia in 1997 but continued as a consultant to shock wave research at Sandia until 2010.

In the fall of 1969 I was selected for entry into Sandia's Doctoral Study Program and spent three years under the tutelage of Prof. George Duvall at Washington State University (WSU). My topic led to the discovery of metastability during the phase change in KCl and also measuring the rate at which the 2-GPa phase transformation from b1 to the b2 crystal structure occurs. I had the good fortune to work with fellow student Joe Andrews, who had a very clever and exact way to treat phase transformations in shock codes, which I adopted, expanded, and used in a variety of different ways at Sandia and Los Alamos for the next three decades.

Graduation from WSU in 1972 was followed by a 1½ year stint in Walt Hermann's shock wave department where I had the good fortune to work with Jim Asay on shock melting in aluminum, with Jim Johnson and Jim Asay on the multi-phase behavior in bismuth, and with several other people on other projects. My role was to supply modeling capability for the experiments. I twice taught a full semester course on shock wave physics. The students were mid-career Sandians caught in the transition from a one-mission to a multi-mission lab. The course notes, and to a lesser extent the lecture videos, turned out to be quite popular and their use is widespread. I attribute the popularity to the fact that the material is taught at the most elementary level possible. The students can learn the concepts first and add sophistication as needed in their own specialty. The overall experience in Walt's group was a first for me: actual fundamental research at Sandia.

In 1974 I became supervisor of the Explosive Components Division. Most of the work was on scheduled components for nuclear weapons. But we did have a rather sizable reimbursable with the Navy to study porous HNS³³ explosives. The Navy was introducing HNS detonators and mild-detonating fuses on their C-4 missile and desperately needed a more thorough understanding of the behavior of this new explosive. The Explosives Research Department was unwilling to take on this customer-driven work so we built up a capability within the Division. The principals were Dennis Mitchell, who performed front surface impact VISAR measurements on porous HNS that allowed EOS and reaction rates to be measured, and Al Schwarz, who built our own VISAR and made innumerable measurements on component performance that could be compared with model results. Jim Kennedy and Steve Sheffield were valuable team members. I did most of the modeling. The result was a rather complete material description. With the help of Sam Thompson, we introduced reaction kinetics of HNS into CSQ (the 2-D progenitor of CTH) and did numerous numerical studies on detonation corner turning and other two-dimensional phenomena important in component design.

In 1978 I became manager of the Fluid Mechanics Department. We did almost no shock work. The exception was Jace Nunziato, whom I had promoted to supervisor of one of the divisions. He spun his magic by applying his mixture theory to shock initiation of composite explosives and to deflagration-to-detonation transition. I helped some but mostly watched. I did work on one problem (and charged indirect³⁴!). Heterogeneous deformation in the shock front was in vogue and so I developed a simple model that accounted for the competition between heat generation from plasticity in shear bands with thermal diffusion occurring during the finite rise time. The result was that the shear bands in a shock front should number just a few, regardless of the strength and rise time of the shock. However, the holy grail of the 4th power law between stress and strain-rate was elusive and not rendered by this simple analysis.

At a place like Sandia, interesting problems can arise unexpectedly. Immediately after the Three Mile Island (TMI) reactor accident in 1979 I got a call asking for help on the hydrogen release problem. The big question: "Would an internal hydrogen detonation breach the concrete containment structure of the

³³ HNS is hexanitrostilbene is a high explosive with good thermal and vacuum stability that was developed by the Navy in the 1960s.

³⁴ Indirect costs are those incurred to operate Sandia as a whole and that cannot be identified specifically with an individual "direct-funded" project. These indirect costs include, for example, space costs, corporate taxes, laboratory directed research and development, and environment, safety, and health.

reactor?” I assembled a team of Sandia experts. We worked long days and nights and were in constant phone contact with officials on the TMI site. Between Sam Thompson with his ANEOS equation of state and his CSQ 2-D code, Mike Cieslak on material and chemical issues and Walt Murfin, a Sandia expert at damage shocks cause in concrete, we were able to give definitive guidance to the TMI Emergency Management: there was no danger of containment failure. But the larger question went untested as the large volume of hydrogen produced later had insufficient oxygen to be detonable. I have always believed the good marks we made during this effort helped Sandia establish itself in the nuclear reactor safety arena.

In 1982 I became manager of the Ceramic Components Department, whose mission had little to do with shock physics. The sole exception was the responsibility for the design of ferroelectric power supplies that are used in neutron generators. However, adequate models were sorely lacking so our work was strictly empirical in the tradition of much of Sandia’s component development up until that time. This mission was the driver for Steve Montgomery to later do his superb modeling of shock depolarization of ferroelectrics that has changed the design world in this area.

In 1988-1989 I moved to the shock physics department. An important change was taking place in the computing world. Parallel computing had arrived, and we staffed that area to implement shock codes and made good progress. Another related effort was transforming CTH from a vertically integrated project to a full-fledged software engineering project, with benefits that exist to this day.

1990-1991 was spent as the Scientific Advisor to Admiral Mike Barr, the Deputy Assistant Secretary for Military Application at DOE Headquarters in Washington, D.C. I spent 1992-1995 as the Program Manager for the Defense Programs tech base and 1996-97 as President of Lockheed Martin Nevada Technologies in Las Vegas, NV. During this time, I was essentially disconnected from the details of the research in shock physics.

Upon retirement in mid 1997, I entered into consulting contracts with Sandia and Los Alamos. At Los Alamos my main project was to develop a multi-phase EOS with phase change kinetics for plutonium. The results are classified and therefore not published. Another interesting project was to measure twinning kinetics in the alloy U-6wt%Nb and confirm that was the reason for some of the rather bizarre behavior that had been observed in shock experiments. That was done jointly with Rob Hixson and Rusty Gray of LANL.

Most post-retirement effort at Sandia was designing and analyzing ramp wave experiments conducted on Z. This required developing a method to integrate the equations of motion backward in time. The technique maps VISAR records into stress-strain isentropes and has proved quite useful for hyperbolic systems.

Early retirement was almost like a full time job but it tapered off. I billed 1200 hours the first year and essentially zero during 2011 with the drop off roughly linear over that period. Amazingly, the amount of shock-wave-based research during retirement exceeded the aggregate conducted during my regular employment time.

One closing note is that only a small slice of Sandia’s mission space involves applications that have contact with the shock dynamics field. Over the years, this has resulted in constraints on program support and growth within the lab. In spite of this, the shock dynamics effort has maintained strong management support. It is quite remarkable that Sandia has excelled and, in many respects, has been the preeminent organization in this field for more than three decades.

Personally, I am quite grateful for the opportunity to have worked with so many very capable people on a whole variety of interesting and important problems.

Mine has not been a typical career, mixing research with application and management. I liked it that way.

Marcus D. Knudson

1998 - Present

Several events occurred in the late 1990s, as I was finishing up graduate studies in Shock Physics at the Institute for Shock Physics at Washington State University, which ultimately guided my choice to start my career at Sandia. Firstly, LLNL published a paper in **Science** on the landmark experiments that had recently been performed on deuterium at the Nova laser facility. Secondly, somewhat prompted by the success of the dynamic compression work at Nova, Don Cook, who at that time was the Director of the Pulsed Power Sciences Center at Sandia, convinced Jim Asay to evaluate the potential of performing meaningful dynamic compression experiments at the Sandia Z accelerator. Thirdly, DOE established the Science-Based Stockpile Stewardship Program, necessitating national investment in such scientific fields as dynamic compression, which put me in the enviable position of being able to choose a career at any of the three national labs. All three of the labs were attractive for their own reasons. LANL was probably considered the strongest ‘pure’ shock physics effort, with its long history of a solid, gas-gun-based experimental program on metals and energetic materials. LLNL was probably considered the ‘sexy’ shock physics effort, with the emerging laser driven shock program. Sandia was probably the most unorthodox of the three, having recently all but closed down their gas-gun facility and being in the throws of trying to establish a high-energy-density shock physics program at the Z facility. I ultimately chose to join Sandia, to work with an incredible mentor in Jim Asay, and to have the unique opportunity to enter in on the ground floor of what turned out to be an incredibly successful and rewarding endeavor. Needless to say, I have never regretted that decision.

When I first arrived at Sandia, in November 1998, the shock physics program at the Sandia Z accelerator was truly in its infancy. The team consisted of Jim Asay, Clint Hall, myself, and a few technicians. We had at our disposal a single continuous wave (CW) laser and three channels of VISAR for diagnostics. Experiments were being done in a ‘ride-along’ mode; we had to beg and convince other experimentalists in the Inertial Confinement Fusion (ICF) program to allow us to hang secondary hohlraums off the side of the return current cans in their wire-array experiments. In hindsight, this type of effort was doomed from the start. We know now the futility of trying to use the ~5 ns x-ray radiation burst from imploding z-pinches to drive meaningful dynamic compression experiments. But back then we had to learn the hard way.

The Z accelerator was designed to perform pulse compression. Energy is stored in 36 Marx bank capacitors at the perimeter of the ~110-foot diameter by 20-foot-tall tank of transmission oil and water. This energy is released and transmitted to the center of the accelerator via transmission lines with laser-triggered gas switches and self-breaking water switches, each designed to compress the discharge successively to a roughly 100-ns, 10-20 MA current pulse. The wire array is itself a final stage of pulse compression, which uses the current pulse to vaporize a cylindrical array of wires and implode them on axis, producing a roughly 5-ns burst of intense x-ray radiation. This x-ray radiation could then be used to drive large amplitude shock waves ablatively into materials of interest, in much the same way as intense laser radiation. Given the hohlraum temperatures that had been achieved at that time, we had expected to be able to attain approximately 5 Mbar pressures through x-ray ablation.

However, the z-pinch load on the Z accelerator proved to be at least one too many stages of pulse compression. We had felt that the resulting x-ray burst was insufficient as a source for dynamic compression. Rather, we desired a source capable of producing steady shock waves for durations of several tens of ns. We therefore spent nearly a year investigating the use of low-density, carbon-based

foams (such as TPX^{®35}) as a way of ‘conditioning’ the radiation that reached a sample, such that the resulting radiation and foam tamper would support a significantly longer, steady shock. These efforts, while somewhat promising, ultimately proved to be inefficient and were unsuccessful. We were unable to produce any results that warranted publication.

Meanwhile, there was an interest from Rick Spielman, a pulsed power scientist who was very influential in the design of the Z accelerator, to use our VISAR capabilities to try to measure current density near the experimental load. The idea being that at small enough radii, the current density becomes large enough that appreciable conductor motion occurs during the discharge of the pulse that would be detectable using VISAR techniques. Jim Asay and Clint Hall were successful in demonstrating this type of load current measurement. As the temporal profile of the magnetic pressure mimicked that of the current pulse, the resulting electrode velocity exhibited a beautifully smooth acceleration indicative of a smooth ramp compression wave. Jim Asay was quick to identify the significance of this, and the use of pulsed power to perform magnetically driven Isentropic (or quasi-Isentropic) Compression Experiments – more commonly known as ‘ICE’ experiments – was born.

Early on it was recognized that Sandia would need to partner with one of the two weapons physics labs to accelerate the growth of this new dynamic compression capability. A relationship was struck with Art Toor, Bob Cauble, and David Reisman of LLNL. I’ll assume that more detail of this partnership can be found in the recollections of Jim Asay and Clint Hall. I will only summarize that, through this partnership, significant progress was made in developing a modular, coaxial short circuit load for performing ramp compression experiments on the Z accelerator.

The next significant breakthrough occurred during the 1999 American Physical Society meeting of the Division of Plasma Physics in Seattle, WA. Jim Asay, Clint Hall, and I were at the lobby bar of the Westin in Seattle, discussing the magnitude of the impulse these types of experiments delivered to the electrodes. Back of the envelope calculations suggested that the impulse was sufficient to launch relatively thin anode plates, roughly 1 mm in thickness, to significant velocities, well in excess of 10 km/s. That evening I ran some simple hydrocode simulations and the results were extremely encouraging. Upon returning from that meeting we immediately began planning dedicated experiments on Z to test this new concept. Given that it takes roughly three to four months to design and fabricate hardware for Z experiments, the first experiments would not be performed until the spring of 2000.

In the meantime we presented the idea of magnetically accelerating flyer plates on Z at the 2000 Garwin review, an annual external review of Sandia’s pulsed power program. I presented our plans on the first day of the two-day review. I’ll never forget the response of David Reisman, who was attending the meeting to present on the progress of the ICE platform, the following day. He had been intrigued with the idea that we had presented, and that evening in his hotel room had run several Mach 2 calculations, including full magnetohydrodynamics. He came to the conclusion that it was a great idea, but unfortunately it would never work. The Mach 2 calculations indicated that no matter how thick the initial electrode, the magnetic field diffusion was so severe that the field would burn through the plate before it could reach a terminal velocity, and thus the plate would be vaporized prior to impact.

A few weeks later we conducted the planned experiments. Given the fact that we only had a few VISAR channels at the time, we had to rely on shock breakout measurements. These were effectively reflectivity measurements in which a sudden loss of reflectivity occurred upon the arrival of the shock wave at the free surface, to obtain several transit time measurements in a wedged target to infer the shock velocity. I still remember sitting at home on the evening of that first flyer plate experiment analyzing the data, so anxious that my hands were literally shaking as I plotted the breakout time versus distance. Lo and behold, despite the Mach 2 simulations, the data suggested that a significant portion of the plate was

³⁵ TPX is the brand name for polymethylpentene (PMP), a thermoplastic polymer.

still at full density upon impact, implying that the diffusion rate in the simulation was too high. This marked the birth of the magnetically accelerated flyer plate platform on Z.

Concurrently, Mike Desjarlais was beginning to explore the use of the relatively new quantum molecular dynamics (QMD) calculation capability to study equations of state and transport properties of warm dense matter and plasma states. At each time step, QMD uses quantum mechanics to solve for the electron density for a particular ion configuration and then uses classical mechanics to calculate forces on, and displacements of, the ions. This process is repeated for a long enough simulation time that steady states are achieved. Mike Desjarlais pioneered the use of the Kubo-Greenwood formulation with QMD to calculate electrical conductivity in condensed matter systems, beginning with aluminum. Using these techniques he discovered that the widely used Lee-Moore-Desjarlais conductivity model predicted conductivity that was ~20% too low at temperatures just above melt. In simulations of the flyer plates, this resulted in a diffusion rate that was too fast compared to experiment. His revised conductivity model, which was fit to the QMD conductivities, in contrast was found to result in very good agreement between experiment and simulation.

Ray Lemke, who had begun approaching this problem from a simulation perspective using the magnetohydrodynamics (MHD) capability of ALEGRA, had independently identified the deficiency of the available conductivity models through his analysis of the experimental results. He worked with Mike Desjarlais in improving the conductivity models in ALEGRA and began an effort to develop a truly predictive, self-consistent, two-dimensional simulation capability utilizing ALEGRA, which is the workhorse for experimental design and analysis to this day. Integral to this simulation capability is a circuit model for the entire vacuum section of the accelerator, with an element of the circuit being a 2-D ALEGRA simulation. This circuit can be driven by a Thevenin equivalent voltage to represent the voltage at the insulator stack. Such a model enables one to predict the performance of an arbitrary short circuit load accurately, while properly accounting for the effect of load deformation and the increases in inductance on the delivered current to the load. Also integral to this circuit model is inclusion of a Z flow loss, and the eventual short, that occurs at the convolute upstream of the short circuit load.

This design capability has been used to develop a standard coaxial flyer plate load that has been used extensively over the past several years. The rectangular geometry features an offset cathode design that results in the two anode flyer plates reaching different terminal velocities, thereby enabling Hugoniot data to be obtained at two different pressure states on one Z experiment. Furthermore, the roughly 30-mm height of the flyer plate allows for multiple targets (typically up to five) to be fielded on each panel. By varying the cathode thickness and the machine charge voltage, flyer velocities ranging from ~7-32 km/s can be achieved. Later, using the stripline configuration (described below), we were able to launch an aluminum flyer plate to over 45 km/s, which is in excess of 100,000 mph.

The flyer plate technique has proven to be extremely fruitful. Several problems of scientific interest have been addressed. Perhaps the most notable of these is the high-pressure response of liquid deuterium. This was the first set of experimental data to be published using the flyer plate technique and, more significantly, these results resolved the controversy surrounding the high-pressure response of deuterium. Another notable result is the discovery that beryllium remains in the hcp (hexagonal close packed) phase at much higher pressure than previously thought. In fact, based upon the relative magnitude of the longitudinal elastic sound speed, beryllium either melts directly from the hcp phase, or undergoes an hcp-bcc (body centered cubic) transition at a pressure just below that of melt on the Hugoniot. Z flyer plate data also provide evidence for the existence of a diamond-bc8-liquid triple point along the diamond Hugoniot. Quartz has been extensively studied using Z, and it was determined that the Hugoniot response of quartz is stiffer than previously thought. This is quite significant given that quartz has become a widely used standard in multi-Mbar Hugoniot experiments. More recently, the flyer plate technique was used to demonstrate that two different equations of state for water that are commonly used in planetary modeling

are far too compressible. Finally, the flyer plate technique has been used to obtain Hugoniot data on several cryogenic fluids including xenon, krypton, ethane, and krypton/ethane mixtures.

The circuit model for the vacuum section of Z developed by Ray Lemke has also been used extensively in the design of ICE loads. Through an effort led mainly by Jean-Paul Davis, a very robust method of ICE experimental design has been developed that relies on a strong synergy among experiment, theory, and simulation. Briefly, 1-D ALEGRA MHD simulations coupled to the DAKOTA optimization routine are used to determine the anode/cathode arrangement, electrode thickness, sample thickness, and time dependent magnetic field required to produce a desired velocity profile at the rear of a sample of interest. The results of 2-D ALEGRA MHD simulations, which provide the changes in inductance and scale factor relating current density to magnetic field, are then used to convert the magnetic field profile to the desired load current pulse and, after accounting for current loss, to the desired machine current pulse. A transmission line model for the accelerator, called Bertha, is then used to identify a machine configuration (water switch settings, charge voltage, and laser trigger gas switch timings) necessary to produce the desired current. This method has been used time and again to design ramp compression experiments on Z.

Another significant advance for ramp compression experiments was the development of the stripline geometry. In the 2006 timeframe, the year before the upgrade of Z, we were exploring the peak stresses that could be achievable through magnetic compression. To do this we systematically reduced the size of the rectangular cathode stalk and the gaps between the anode and cathode at the load. The resulting load dimensions were a 9x2 mm cathode with a 17x4 mm anode box, leaving 1-mm gaps. It was quickly determined that the utility of this load was going to suffer due to the unrealizable alignment requirements of the cathode stalk within the anode box. Coaxial load geometries could be crudely thought of as two inductors in parallel. Misalignment results in differences in inductance, which in turn result in unequal distribution in power between the two panels. The unequal power distribution results in higher initial current density, and therefore magnetic pressure, in the smaller anode-cathode gap side, which manifests itself as an apparent time shift between the velocity profiles.

We used the roughly year-long shutdown for the upgrade of the Z facility to evaluate the alignment requirements to enable high precision ICE experiments at multi-Mbar stresses. It became apparent early on that it would be next to impossible to achieve the required alignment fidelity at a facility such as Z. Careful analysis by Jean-Paul Davis revealed that the gap between the anode and the cathode would need to be uniform to within roughly 5 microns along the entire length of the load (~30 mm) to ensure magnetic pressure uniformity. At that point we began to consider alternatives. Motivated by the load geometry of Veloce, the small pulser facility at Sandia, we embarked on the stripline design, which in principle can be thought of as a coaxial load with three anode panels removed. What is left is a two-panel arrangement where the cathode panel emerges through an opening in the anode plate and stands opposite the anode panel with a shorting spacer between the top of the anode and cathode panels. This arrangement allows for samples to be mounted directly opposite each other, one on the anode and one on the cathode, with a single anode-cathode gap in between. This ensures that both samples experience the same magnetic field history.

The stripline geometry relieved the alignment requirements, however, it also resulted in a few technical complications that took some time to mitigate. Unlike the coaxial geometry, which fully contains the magnetic field within the enclosed anode structure, the stripline geometry allowed the magnetic field to encompass the area around the panels and samples. As a result, we had to come up with a tractable way of protecting the samples and the fiber optic cables used for the VISAR diagnostics. We protected the sample by completely covering the back of the panels with metal, effectively building a Faraday cage to exclude the magnetic field. The fiber optic problem proved to be more difficult and required several iterations over tens of shots to come up with a robust probe assembly that we refer to as the pitch-fork design. Furthermore, we learned the hard way that even though the fibers are encased in

bicycle brake cable, the cathode probe cable had to be elevated off of the upper anode plate for several feet or the fibers would lose transmission over several hundred nanoseconds during the current pulse.

A second complication related to the unconstrained magnetic field is a current density gradient, and thus pressure gradient, up the length of the stripline panels. Dave Seidel and others assisted us with EMPHASIS calculations of the load geometry to identify and mitigate this problem. The main source of the gradient was the transition from the radial feed of the magnetically insulated transmission lines to the vertical feed at the load. Since the magnetic field is unconstrained in this region, a significant current flow is induced on the sides and back of the cathode panel, with a current density that becomes lower along the length of the stripline. This aspect of the stripline geometry has been mitigated through adopting tapered panels that widen near the short, with the exact taper identified through EMPHASIS calculations. The tapering has been shown through experiment to reduce the gradients significantly in the stripline design.

Both the coaxial and stripline geometries have been used to obtain valuable ramp compression and material strength data on a variety of materials. Perhaps the most notable ramp compression data has been performed on tantalum. During the same year in which Z was re-commissioned, we were tasked with a level-1 milestone to perform a ramp compression experiment on tantalum to 4 Mbars. In a relatively short amount of time we were able to reestablish short circuit experiments on Z, commence experiments using the stripline geometry, resolve the diagnostic issues associated with the stripline, and establish the Bertha model to a high enough fidelity to generate shockless compression of tantalum to 4 Mbars. Another notable study included a fairly complete investigation of the strength of LiF, a very common window material in dynamic material properties experiments, under ramp compression to 1 Mbar. A similar study of the strength of Be under ramp compression was performed to a similar stress level. High stress ramp compression experiments have also been performed on aluminum, copper, gold, lead, and molybdenum.

In addition to the load development, other improvements have been made along the way to advance the magnetic compression capabilities on Z. The most notable of these improvements was the capability to perform high fidelity pulse shaping. Initial experiments utilized what we now refer to as a synchronous long pulse; here, all 36 gas switches are fired synchronously (in fact, in the early days a single laser split 36 ways triggered all of the lines simultaneously) and the water switches are directly shorted. In this configuration the current pulse essentially increases linearly in time over ~200-300 ns. For low stress experiments this is acceptable. However, for higher stress experiments, as well as high velocity flyer plate launch, this linearly increasing current pulse results in shock formation at a relatively short propagation distance. Given that for most materials the sound speed changes most rapidly at low stresses, a concave up current pulse is required to mitigate shock formation and to maximize the propagation distance of the ramp compression wave. This was accomplished, in principle, by replacing the single laser trigger source with 36 individual lasers. In practice, significant improvement was also required to Bertha, the transmission line model used to predict machine performance. Jean-Paul Davis, Pat Corcoran, and others have done a tremendous job in continually improving the Bertha model and extending the pulse shaping extremes to very long and complex pulses. This work has been critical in the advancement of dynamic material property capabilities on Z.

Significant improvement in the diagnostics was also required to maximize the potential of Z for these types of studies. In particular, given that each Z experiment, whether ICE or flyer plate, could utilize up to 20 or more samples, it was critical that the number of VISAR channels be significantly increased. Relatively early on, within the first 2 years, we were able to increase the Sandia-owned VISAR channels from 3 to 14 by converting our two Valyn VISAR systems to multipoint VISARs. To increase the capability further, Jim Asay was able to leverage aboveground experiment (AGEX) capabilities through borrowing VISAR systems intended for Nevada operations. In this way, an additional 14 channels of VISAR and at least one more laser system would be transported to Sandia to support Z experiments during the downtime between subcritical experiments. Later, Jim was able to secure Nevada monies that were allotted to support AGEX experiments to fund members of the Special Technologies Laboratories

(STL), Santa Barbara, CA in the development and fabrication of new-generation, multipoint VISAR systems and extremely fast photodiode detectors. Key to this work was Bruce Marshall, Gerry Stevens, and Terry Davies, among others. This group was able to outfit Sandia with a state-of-the-art 38-channel, sub-200 ps resolution VISAR system that is still in use today.

A second critical advance in the area of diagnostics was the repurposing of a short-pulse (5-10 nanosecond) YAG laser that had been used for holography experiments at the STAR facility. As the number of VISAR channels grew, the CW laser became ineffective. It came to my attention that there was an unused Continuum YAG laser at the STAR facility. We contacted Continuum to see if it was possible to convert this laser to a custom, long-pulse (5-10 microsecond) laser for use as a light source for velocimetry. Continuum was very gracious and offered to repurpose the system for ~\$80k. The system, while it had its shortcomings (*i.e.*, poor stability, low beam quality, *etc.*), increased our radiance at the sample by orders of magnitude, and turned out to be necessary to provide sufficient light to enable us to use the fast photodiode detectors that had been designed by STL. We have since replaced the repurposed laser with a much more stable, higher beam quality Continuum system that has been our workhorse for the past several years.

Today we continue to push the envelope and develop new and exciting platforms to enable a wider variety of experiments in dynamic material properties. Particular areas that are currently in development that I am excited about are being enabled through more extreme pulse shaping. I'll briefly mention two here. First, an ability to produce a double-humped current pulse is paving the way for a shock-ramp platform that will open up the equation of state surface between the principal Hugoniot and the principal isentrope. In particular, one can now begin to design experiments in which the material is first shock melted, and then ramp compressed back into the stability field of the solid, to investigate rapid-resolidification. Second, we have been developing techniques using electroplating and diamond turning technologies to produce multi-layered flyer plates that can be used to perform double shock and shock and release experiments.

Looking back, it is staggering how much progress was made over the past decade. This progress was realized through the integration of various groups with expertise in shock wave physics, pulsed power, high energy density theory, computation and simulation, and optics and diagnostics. In addition to those I specifically identified in these recollections I would also like to acknowledge the following people who have played a key part in the advancement of material dynamics experiments on Z. Hardware designers: Chris Russell, Andy Maurer, Dustin Romero, and Devon Dalton; Diagnosticians: Randy Hickman, Jason Podsednik, Bruce McWaters, Kevin Youngman, Charlie Meyers, Devon Dalton, and Anthony Romero; Scientists: Dave Hanson, Jim Bailey, Greg Dunham, Chris Deeney, Dan Dolan, Seth Root, Chris Seagle, Justin Brown, Scott Alexander, Heath Hanshaw, Matt Martin, Becky Coats, and Mark Savage; Managers: Chris Deeney, Tom Mehlhorn, Dawn Flicker, Thomas Mattson, and Mark Herrmann. These are exciting times, and I look forward to the advances that will be realized in the years to come.

Bruno Morosin

1961-1997

Sandia's Shock-Induced Solid State Chemistry

Bruno came to Sandia in 1961 from the Microwave Department of Hughes Research, Hughes Aircraft as a staff member in the Solid State Physics Department. He was perhaps notable as the only Supervisor being transferred five times between two Departments, one dealing with the fundamental aspects of Solid State Physics while the other dealt with Applied Physics.

In the late 1970s, Robert ("Bob") A. Graham and Lee Davison examined and reviewed the scientific literature dealing with the shock compression of solids. This comprehensive study eventually appeared in Physics Reports **55**, 255-379 (1979) as 'Shock Compression of Solids' by Lee Davison and R. A. Graham. Upon its completion, Bob Graham decided to look in more depth at Adadurov, Goldanski, and other Soviet scientists referenced in the Mendelev Chemical Journal **18**, No.1, 1973, and he was astonished to find a well-developed scientific effort reporting chemical changes and reactions. This was in contrast to the prevalent opinion of most U.S. experts that were in agreement that chemistry could not occur on the time scale of shock compression in normally inert solids. Many of the references were in Russian journals that appeared without English translations. It was a major effort for Bob Graham to acquire the journal articles and have them translated. Pat Newman of the Sandia library staff provided much of the indispensable service needed for translations of the Soviet literature. In 1979, Bob Graham wrote an internal memorandum, "Chemical Activity in Shock-loaded Solids," as well as subsequently going to Washington, D. C., to present a briefing to the National Materials Advisory Board on this well-developed Soviet scientific effort, existing essentially unknown to the remainder of the shock compression community. This triggered a briefing to the then current upper management, consisting of Sandia's President, Morgan Sparks, the Vice President, Albert Narath, and the Director of Solid State Research, John Galt, on this Soviet shock compression-induced chemistry effort; such briefings were extended to other internal and external organizations.

In response, Sandia management decided to transfer Bruno Morosin from his Materials Division position under Jim Schirber's Solid State Department to become the supervisor of the Shock Wave & Explosives Division in the Applied Physics Department under George Samara.

A core group was organized around various scientific disciplines. Within Samara's department, this consisted of Bob Graham for his physics involving shock loading as well as his explosive technology background, Gene Venturini for electron spin resonance and other magnetic properties, and Bruno Morosin for x-ray diffraction and general materials properties. In addition, other individuals at Sandia, such as Marty Carr for electron diffraction expertise related to defects, Ed Beauchamp for ceramics and their sintering properties, and Dave Webb for calculations of the spatial temperatures and pressures profiles due to the shock wave response within the Bob Graham's containment fixtures, were interfaced as experts in their particular fields. This core group was tasked to aid Bob Graham on the evaluation of

the Soviet literature on this subject, coined 'shock chemistry,' for what is essentially shock wave-induced solid state chemistry and/or materials modification, and propose an appropriate Sandia effort.

Initial research efforts were concentrated on several compound ceramics. Titania, alumina, and zirconia were studied for various applications by Ed Beauchamp, Bill Hammatter, and Ron Loehman in order to see if shock modification might offer an alternative or enhanced pathway to the desired effect for those current Sandia projects. Another example was an on-going study of the mineral pyrite, namely iron sulfide, on enhancing coal liquefaction, which appeared to offer some promise. Pyrite was shock loaded and employed in the liquefaction reactors to note if the increase of defect levels might enhance the catalytic properties of the pyrite. Early on, it was established that the shock wave action and its inhomogeneous interaction of compression, relaxation and materials mixing via local transport as well as diffusion, could significantly alter ordinarily tough inert materials by introducing high numbers of defects.

Eventually, various professors and their students from universities were sought for particular capabilities lacking or oversubscribed at Sandia; these included Yuki Horie at (initially) North Carolina University on modeling shock processes, Jim Stewart at the University of Maryland on profile code development, Frank Williams at the University of New Mexico on catalysis, and Don Williamson for Mossbauer studies at the Colorado School of Mines. Pat Newman continued to provide the much needed translations of the Soviet literature as journal papers were uncovered. In addition, the assistance of many unmentioned individuals within Sandia and students at various universities provided the laboratory help required; these individuals became either co-authors or were acknowledged in the published journal papers.

Early in this effort on October 7, 1980, a one-day meeting with a Soviet scientist, Stephanov Batsanov, was organized by Jim Mote of the Denver Research Institute at the University of Denver. Jim Mote was one of the leaders in materials processing with high explosives and had previously been at Sandia in the Applied Physics Department. Besides Bob Graham, Bruno Morosin and Gene Venturini attended from Sandia with the aim of determining the current Soviet effort, if it could be assessed, and possibly determining if some unknown new technology was being developed. The outgoing Batsanov was accompanied by two others, who contributed nothing and did not appear to be scientists. Batsanov's earliest publications appeared in Soviet journals beginning in 1965 with somewhat meager content and what he presented was already well delineated by Bob Graham's review of the Soviet literature.

It should be noted that Davison and Graham's previously mentioned review did include most of the early efforts by non-Soviet scientists of shock-loading effects on materials. The early 1960s graphitic carbon-to-diamond work of Paul DeCarli and J. C. Jamieson, of F. P. Bundy, and eventually of DuPont's diamond grit multi-million dollar synthesis business enterprise using old underground Delaware mines, stemmed from the fundamental work at Los Alamos on controlled compression of condensed matter. Much of the work of the Soviets and other foreign investigators was reviewed in context with the early results of Sandia's program and was described in the Annual Review of Materials Science **16**, 315-341 (1986) by Graham *et al.*

By the following year, this core group had completed and presented eight short papers at the June 21-26 American Physical Society Shock Conference, dominating the shock chemistry area, and these papers

were subsequently included in the conference proceedings. These included shock-induced reactions of specific inorganic compounds, defect formation and the resulting effects, such as enhanced catalytic action, the initial evaluation line broadening of x-ray diffraction patterns, and enhanced sintering characteristics, all of which resulted from shock loading of selected single-phase inorganic compounds as well as a more complicated clay, nontronite, with its implication of evolution for planetary surfaces, namely that of Mars. The major positive step demonstrated that, unlike the Soviet experiments, the Sandia results were very reproducible and consistent with the pressures and temperatures suggested by the preliminary calculations of Dave Webb. As time went on, these three-dimensional calculations were refined and samples were carefully taken from the recovery capsule as a function of location, subjected to various experiments, and shown to be consistent among various experimental shock configurations. In addition, Bob Graham, with the discussions and modeling results of Yuki Horie, was able to arrive at a conceptual chemical reaction model for shock chemistry, a process involving turbulent local mixing, possible massive defect generation, a short, rapid transit pulsed temperature excursion--all of which were caused by the transit pressure pulse.

What proved surprising to several in the Solid State Department were the consistent and reproducible results from these explosively-driven experiments. Shock-modification experiments were carried out with Bob Graham's containment fixtures, named 'Baby Bear' and 'Momma Bear' using standardized geometries and explosive loading arrangements. In these shock compression experiments, the peak pressure and temperature are controlled by the size and shape of the powder sample and the recovery fixture, in addition to the explosive-loading system. In this standardized shock-recovery system, three explosive-loading arrangements resulted in reproducible loading into five sample-fixture arrangements so that peak shock pressures from 4.5 to 27 GPa could be achieved. Then, by varying the density of the powder compact over a typical range from 40 to 65 % solid density, the shock-induced temperature could be varied from 50 to 1000 C. Evidently, by carefully interfacing the components of the experiments, astonishingly reproducible shock modified samples could be achieved. Bruno Morosin was queried numerous times by his colleagues in his former department on the reproducible issue.

Initially, the sample capsules were opened, taking care to avoid contamination, but used post-shock essentially in their entirety. This probably caused some averaging of the sample characteristics. However, as Dave Webb's extensive two-dimensional numerical calculations became more refined, detailed locations of the recovered samples were examined. The intact sample capsules were opened carefully under a binocular microscope in order to separate the compacted sample disk into smaller research samples for various analytical diagnostics; ruptured, but essentially recovered, capsules were treated in a similar fashion, but with some possible areas being discarded, especially if post-shock annealing of chemistry reactions were involved in that specific material. For example, ruptured capsules could possibly have introduced impurities that might easily confuse electron paramagnetic resonance data. Not described was the use of the helium gun facility for some studies requiring peak pressures below the lower threshold of 4.7 GPa for the explosively-loaded system.

The three initial diagnostic tools were conventional x-ray patterns as well as the determination of line broadening of diffraction peaks, the intensity and shape of the electron paramagnetic resonance signatures, and the determination of dislocation densities of the transmission electron microscope images. Consistent and reproducible diagnostic results were obtained from the tiny, but carefully separated samples unloaded from various locations of the capsule as well as between different loading

configurations when merged with values given by Webb's two-dimensional numerical calculations for the sample pressure and temperature.

From the very start, diffraction studies conducted on what were considered tough ceramic material, such as alumina, titania, various carbides and nitrides, yielded broadened diffraction lines more typical of those found for heavily cold-worked metals. Both crystallite or coherence size and residual lattice strain may be determined from such line broadening. Significant differences between the residual strains from ball milling and from shock modification were also apparent in these materials when examined. As expected, such sample modification affected the magnetic properties as well as any other measurement in which the local environment of component ions is important.

The importance of how x-ray diffraction profiles are collected and analyzed was pointed out early in the papers. Long time scans of the diffraction peaks were required for good statistics for the points along the entire profile of the diffraction lines. The early Warren-Averbach analysis employed selective diffraction lines, but as most other peaks were also measured, it was realized that a more complete potential in analysis was present. Two computer codes were developed, XRAYL for producing idealized powder diffraction lines profiles from overlapping powder patterns and CRYSTZ for computing crystallite size and strain from broadening of powder diffraction lines; eventually, with Cam Hubbard at ORNL, documents were prepared with 3 ½ inch computer disks and published for external distribution. Furthermore, our study of lanthanum boride comparing shock modified and various milling procedures, part of which was a graduate student's thesis at the University of Maryland using data collected at National Institute of Standards and Technology (NIST), had the spinoff of establishing the pure material as one of the standards that the scientific community can obtain. Lanthanum boride's simple cubic pattern, large unit cell, and unusually sharp peaks allow excellent standardization of diffraction procedures and/or equipment as well as use as an internal standard. However, the eventual more gigantic step in this area came somewhat backwards, probably from discussions Morosin had with computer code developers at other laboratories. This essentially allows, in crystal structure determinations and refinements, all the diffraction lines contained in the spectra to be treated as part of the least square refinement procedure using parameters for elliptical crystallite size and a strain tensor of the material being analyzed. Hence, the results of these parameter fits usually escape the notice of users of the computer codes since, for the most part, the values are considered as scaling parameters rather than as useful information on the crystallite nature of the substance under study.

One must recall that this was the era of President Reagan, the perceived Soviet threat, and the 'Star Wars' initiative. Out of the shock chemistry effort came the concept of a radically different, long life, inert battery that could be activated upon impact for possible fusing capabilities, that is, essentially a shock wave-activated battery.

Towards the end of 1986, superconductivity was discovered in copper-containing compounds with transition temperatures much higher than those in the metals and alloys employed at that time in various magnetic applications; this resulted in a large shift of research effort at Sandia for many investigators, particularly those with a solid state physics background. The structure and magnetic response were two essential ingredients needing investigation for these new materials; hence, part of the core shock-induced solid state chemistry group suddenly became diluted. The intense era of high-temperature superconducting materials was followed by the discovery of the third form of carbon, coined

‘buckyballs,’ which again cut into the time the core group allocated to shock chemistry. Nevertheless, the core group carried out several intersecting studies, subjecting superconducting ceramic materials to shock loading and published the resulting effects. In addition, studies of MnO, SnO, and revisiting pyrite serve as examples of other studies that were conducted during this time frame.

In these later years, another unfortunate Sandia transition occurred. The implementation of more rigid budgeting resulted in the removal of the previous ease with which a researcher could ‘walk down the hall’ and have a sample quickly that could be subjected to the experimental procedure.

In this reduced budget environment, a few experiments were conducted on materials thought to be important for other projects. The report that ‘red mercury’ was a new explosive material led to much speculation as to its exact nature, from energy release of an isomer transition to the least exotic, and to that of the compound itself because of its low decomposition temperature. Shock loading experiments were performed in 1990 at the Socorro explosive site to avoid the strict materials problems instituted at Sandia by the DOE Secretary O’Leary and the ‘Tiger Teams.’³⁶ Its decomposition under shock loading was demonstrated to be quite energetic and initiated even for the lowest pressure experiments. The catastrophic rupturing of the containment fixtures even at the lowest pressures used suggested that the helium gun facility should be used; however, that was impossible because of the potential for mercury contamination problems. Mitigating the shock impulse via foams, light flyers, *etc.* were discussed, but never undertaken.

As General McArthur stated in Congress upon his return at President Truman’s request, “Old soldiers never die, they just fade away.”

Fading away may appear to be the case for ‘shock chemistry’; however not for Bob Graham. He has continued to observe his surroundings and apply his knowledge of physics, chemistry, and materials science learned in his long career, wherever he was. On October 26-27, 2012, in Socorro at the annual meeting of the Four Corners Section of the American Physical Society, Bob presented a talk entitled “High Temperature and High Pressure Mix of Iron Oxides from the Impact Event at Bee Bluff Crypto-Meteorite Impact Crater of South Texas.” This is one of the latest of his papers on the study of a geological feature that he observed while hiking near his home soon after retiring in Texas and his careful probing of rocks and formations discovered in the immediate area. Bob is currently writing a memoir, “Crystal Heart Beat: Sandia and Bell Labs and Lightning in a Bottle,” built around his long career at Sandia.

The intent of the above write-up is to give a perspective of Sandia’s involvement in the area of the modification and/or solid state reaction of materials subjected to shock wave loading, coined as ‘shock chemistry.’ Those seeking a more complete understanding of the science can review the references where overall general reviews of the Sandia and Soviet efforts may be found. An exception was made concerning the description of Bob Graham’s containment fixtures so that comments on line broadening of diffraction lines can be made more coherent and complete. Progress on line broadening was previously given in individual papers on specific materials as they were published; however, these have not been

³⁶ The term ‘Tiger Teams’ at that time (the early 1990s) was used to describe the teams of experts that investigated each DOE laboratory for environmental, health, and safety concerns.

summarized to any extent. Moreover, the computer codes developed have not been indicated nor has the impact on the general science of crystallography been described.

Karl W. Schuler

1967-1978³⁷

I arrived at Sandia in April of 1967. My background was theoretical continuum mechanics. I had never done any kind of experimentation but wanted to do so. One of my professors at Illinois Institute of Technology (IIT) suggested that, if I wanted to get into experimentation, I should definitely go to one of the national labs because they had lots of equipment ready to use. In contrast, if I went into a university I would spend two or three years trying to get experimental apparatus in order to get a program started. There were also two Sandians at IIT working on their PhDs who had extolled the virtues of Sandia.

I was hired by Don Lundergan, who wanted me to work on wave propagation in polymeric materials. There were two reasons for this: the metal behavior at low shock pressures (<50 kbars) was being very adequately covered but many polymers involved in nuclear weapons had only been studied quasi-statically. Under shock loading their characterization was lacking, particularly for time-dependent effects. Secondly, Lynn Barker was developing the laser interferometer and Plexiglas[®] looked like an ideal low-impedance window material.

Walter Herrmann was our department manager. The department, Org 1140 (Mechanical Behavior Department), was primarily located on the second floor in Building 805. I was given an office that was part of, I guess you call it, a 'bullpen.' When you walked in from the second-floor hallway, you went down a central aisle with cubicles on either side that were about eight-foot square and had five-foot-high walls. On the right side, the first cubicle was occupied by Floyd Tuler; the second by Larry Bertholf; the third by Ray Reed; and at the last one, at the window, was Barry Butcher. On the left side, was a larger cubicle occupied by Sam Key and Zelma Besinger. Then there was a passageway that led into the division supervisor's office area. On the far side of that passageway was my cubicle and next to my cubicle, again at the window, was Lynn Barker's cubicle.

I guess because of both the physical proximity and our joint interest in Plexiglas, I worked closely with Lynn Barker. He was definitely a great mentor to me. I'll always remember that in his cubicle there was a blackboard and along the top of the blackboard was written "I'm perfectly willing to be wrong." Very seldom was Barker wrong.

For the first couple months I remember watching Lynn and Red Hollenbach set up shots in Building 855, which housed the gas gun. I also went out to the Area Y explosive site to watch explosive tests conducted by Darrell Munson. The gun facility in Area Y had not yet been built. I'm not exactly sure when I fired my first shot because the shot dates were typically placed on the front of the shot folders and I no longer have access to them. I do have a record of firing Shot 2110 in September 1967 on Plexiglas that involved the use of the BRACIS (Beam Reflection At Center of Impact Surface) technique. This was a technique to determine the transit time of the shock wave by using a light beam that was totally internally reflected at the center of the impact surface of the transparent target. When the transparent projectile impacted, the light beam would continue on into the projectile and one would observe a decrease in light level that could be interpreted as the shock wave initiation at the center of the impact surface. I'm not sure if this was the first shot to ever use the BRACIS technique, nor am I sure who came up with the idea, although it was probably Lynn. But I do remember that the first time it was tried it was on one of my shots, and Lynn and Red were kind of hovering around in the control room. I don't think they thought it would work, but by golly it did. The usual technique, at the time, to determine when the central impact had occurred, was to have four pins around the periphery of the target carrying different

³⁷ Karl Schuler retired from Sandia in 1996.

voltage levels that would be shorted out as the projectile impacted. The average of these shorting times was the time of central impact. This technique worked well for materials that could be lapped very flat, but we found that trying to lap polymers like Plexiglas flat left a lot to be desired. Without true flat surfaces the uncertainty in the central time of impact was very large.

I'm sure before Shot 2110 I had fired other shots. Some of these investigated the behavior of polyurethane and employed the other instrumentation techniques that the division used. These techniques included quartz gauges, slant-wire resistors, and displacement interferometers.

Using the interferometers, it was possible to resolve the fine structure of the stress waves in Plexiglas. The loading shock typically consisted of a jump to a certain particle velocity followed by a gradual rise to the final particle velocity. By searching the continuum mechanics literature I found papers by Alan Pipkin, who was a friend of my thesis adviser, that described the analysis of stress waves in nonlinear viscoelastic materials and predicted wave profiles similar to those that Lynn and I were observing. Thus, I was able to use Pipkin's work to analyze my experimental data. And by November 1969 I was able to submit a paper about the propagation of steady shock waves in Plexiglas.

The extensions to the work on Plexiglas came fast and furious. First, we did experiments where we backed up the Plexiglas impactor with steel or aluminum. This produced a first shock followed by a second shock and allowed us to extend the range of the data. We also looked at the unloading waves in all these experiments. When the gun facility was completed in Area Y, I decided to extend the pressure range using the three-and-a-half-inch powder gun. In fact, I was the one who fired the first maximum shot on this gun (20 pounds of black powder). I seem to remember getting an impact velocity of 7000 ft/s. The next time somebody tried 20 pounds of powder in this gun, it was Bob Graham and it was his first shot on the powder gun. Unfortunately, the catcher had not been installed correctly in the impact tank. Furthermore, the door of the impact tank had not been properly secured. So, when Bob fired the shot, the rear door of the tank opened and a major disaster ensued. A lot of the sheet metal on the building was removed, along with all the lights and most of the instrumentation in the building. In the wake of this disaster, I took the opportunity to drive our purchasing agent out to the site to show him what had happened. He was always complaining that I was trying to buy plant items using expense money. When he saw the destruction, he agreed that this was a hazardous environment and anything we bought in the way of instrumentation was at risk. In the future I never had any problems submitting purchasing requests through him, using whatever kind of money I had available.

But the real interest was on thin pulse attenuation in these polymeric materials. To look at thin pulse attenuation in a metal, one would take a thin impactor mounted typically on carbon foam. To accelerate thin pieces of polymer seemed very difficult because it was going to be very hard to find something to support a thin impactor. So I decided to turn the experiment around. What I did was put the specimen into the projectile off to one side and, then, at the very center of the projectile I put a prism and another prism behind the specimen; with this configuration, the interferometer beam went down the center line of the gun, made two reflections in the projectile, reflected off the back of the specimen in the projectile, and then went back out to the VISAR. The target consisted of a thin foil that was stretched using an O-ring over a hole in the target plate. Of course, the best foil to use was gold foil. So I found a supplier who would provide a 3-inch-wide, one-thousandths-of-an-inch-thick gold foil. What I didn't know was that every piece of precious material that arrived at Sandia receiving had to be inspected and weighed. The supplier assured me that he would ship my gold foil wrapped on a large diameter cylinder so that it would stay nice and smooth since I needed it to be very flat.

When the shipment finally arrived at our lab, the gold foil looked like a used piece of tinfoil. It had been wadded up in order to put it on the delicate balance to make sure that the fractions of an ounce of gold that I had bought were really there. I called the supplier and told him what had happened and he said no problem, I'll let you know the secret on how to make gold foil flat without a super expensive rolling

mill. He told me to take a piece of the gold foil and put it on an optical flat, then put a drop of oil in the center of the foil, put another optical flat on top, and gently squeeze this sandwich in a press. The oil flows out and flattens and thins the foil. When I finished flattening the foil I took it to the inspection department in the machine shop and had its thickness measured. The experiments worked well because the projectile did not rotate as it was launched. I was able to input pulses from a gold foil less than a few thousandths of an inch thick into various thicknesses of Plexiglas.

My weak point was doing the paperwork at the end. I was *not* a good or fast writer. I always had a cartoon on my office wall depicting an outhouse with the caption “The job's not done till the paper work's finished.” Fortunately, management recognized this and teamed me up with Jace Nunziato, who was a superb writer. Between the two of us we pumped out quite a few papers. I'd do the experiments, reduce the data, make up some figures and tables, and discuss them with Jace. Together we would test published theories with the data and scope out a paper.

Jace could start at eight o'clock in the morning with a pad of yellow paper and a red felt tip pen. At four o'clock in the afternoon he would throw the empty pen into the wastebasket and hand me the yellow pad fully covered with text for overnight proof reading so that it could be given to a secretary the next day. Jace's greatest coup was to show a collection of our papers to the editor of the **Handbook of Physics**, who asked us to write part of a volume. So now on my resume I can claim to be the co-author of a major monograph. After it was printed we got a check from Springer, the publisher, that took forever to cash since it was for Deutschmarks. After it was cashed in dollars, we calculated that each of the four co-authors received a penny per word for our tome. So much for me making it as an author.

But my real problem in writing was the progress reports. Before I had been at Sandia for more than a few weeks I had to write my first progress report. It went something like this: “But Don, I've only been here for eight weeks,” I protested. “Yes, but in the research group we write progress reports. I've asked Lynn to help you write your first reports and I'm sure with his help you'll be done in no time,” replied Don. “But Lynn, I haven't done anything,” I insisted. Lynn replied, “Yes, you have. I know you've been in the library researching the literature. You also came to the Lab and I showed you how to do a shot on the gas gun and how to use the laser interferometer, so you've started your experimental program. Why, we even discussed other experiments you plan to do.” He continued “So, *Theories of Non-Linear Viscoelastic Wave Propagation* could be the title of your first report. For the second, I suggest *Preliminary Experimentation* and, for the third, *An Experimental Study of Shock Wave Propagation in Plexiglas*.”

I went back to my cubicle and began to write. I felt that never had so much been written on so little. This was before the days of the word processors so I had to scrawl everything out on paper. But I had been given three suggestions for quarterly reports which made me feel, after a while, that I might have actually done something in the eight weeks I had been at the Labs.

After two days of scribbling, I had used up a whole pad of paper but my three reports were each less than one sheet of paper. I turned them into Don. Within an hour he came back with a number of suggestions that I quickly incorporated. I resubmitted them again to Don, who sent them up the line to Walter, my department manager. Walter immediately called me into his office and told me that I needed to have a first sentence or first paragraph in the reports to justify my existence.

My existence? He explained to me that, in the research group, we always had to indicate to upper-level management how our research tied in with the Laboratory's objectives and this was best done in the first sentence or paragraph of your report. That way, upper-level management could quickly glean that you were working on the right problems and, if they were interested, they could continue to read what you had done.

Also, he noted that I had one report that was about some kind of theoretical research and another report about my planned experimental program. He suggested that I should combine them and use the opening phrase, "A combined experimental and theoretical program has been started to...". In the future, I remembered this and, in virtually every quarterly report I wrote in later years, I started with some variation of this phrase. Apparently, upper-level management loved studies that combined theory and experiment. A second point Walter made was that, once you came up with your justification sentence or paragraph, you shouldn't change it from report to report, because that would just confuse upper-level management. Back to my cubicle to scrawl some more. When I was finally done, I had bragged profusely in a progress report that met the expectations of my management.

In the Department we only had to write progress reports every six months. But in the middle of 1968 a major reorganization occurred. We were moved into the research directorate and became Department 5160, and I was required to write *quarterly reports*. At about the same time, Don Lundergan decided to step down and Barry Butcher became my division supervisor.

As good as Plexiglas was for doing tests and being able to write journal articles, Plexiglas was not really of interest to the weapons community. Some time in 1968 I was put in a study group to look at the MC 2043 neutron generator. This experience brought me into direct contact with the kind of polymers that were of interest to the weapons community. In particular, loaded polymers were of great interest. These are polymers such as epoxy that are filled with various kinds of materials. Perhaps the most interesting of these was alumina-filled epoxy. I began to do shock wave experiments with this material. The most curious thing about this material was the great disparity between the loading wave velocities and the unloading wave velocity. The loading wave velocities were typically that of a polymeric material, but the release waves or the unloading wave velocity was far higher than you would expect from a polymer. My explanation for this phenomenon was that initially the particles of aluminum oxide, which have a very high wave speed, are separated by the epoxy but, as the loading wave propagates through the material, the particles are brought into contact. Thus, the unloading wave is able to move through from one alumina particle to the next at a relatively high velocity. Specimens were prepared using different amounts of the aluminum filler material, and I began a very extensive series of experiments. I also began reading papers on composite materials and mixtures and doing experiments on polymers loaded with other fillers. One of the most interesting fillers was epoxy loaded with glass microballoons. This material could sustain high hydrostatic pressures until the microballoons collapsed when it would drastically decrease its volume.

As I began to accumulate shock wave data using the gas gun on these materials, I felt it would be desirable to have a triaxial test facility where specimens could be subjected to both a high hydrostatic pressure and a superimposed axial load. So, in the early 1970s, I began to order a pressure vessel and other equipment that could be built up into a hydrostatic triaxial test facility. In 1973, when the Arab oil embargo hit, energy research became very important. In fact, in 1974, Congress reorganized the Atomic Energy Commission into the Energy Research and Development Agency (ERDA). Sandia was instructed not only to continue their support of nuclear weapons but to begin doing energy research. After much discussion, the research group at Sandia decided to investigate the production of liquid fuels from oil shale. The pressure vessel that I had purchased to continue my research on polymeric material and mixtures suddenly became the centerpiece of a rock mechanics research lab devoted to oil shale research. We purchased a very large, stiff load frame to put the pressure vessel in so that triaxial testing on oil shale could be done. I also began a series of experiments to look at the shock response of various grades of oil shale.

The rock mechanics lab was set up at the north end of Building 855. But, by 1977, I had essentially stopped working in shock waves and was heavily involved in rock mechanics. By 1978 we had completed many tests of oil shale in the rock mechanics lab, and I had developed a very compact gauge to monitor the lateral expansion of oil shale samples in the triaxial testing machine. The work that I had done on the

loaded polymers was taken over by Rod Bodie, who ultimately coauthored a paper with me on the alumina-filled epoxies. Although we planned to test the alumina-filled epoxies and other filled epoxies in the triaxial test facility, the first priority was given to testing oil shale and other rock mechanics experiments such as those involved in the nuclear waste depositories in salt and at the Nevada Test Site at Yucca Mountain.

I did, however, continue my involvement with the neutron generator group; when I left the shock wave and rock mechanics groups in 1978 to become a member of the structural analysis group, I continued to support neutron generator standoff testing. In 1984, with the help of a lot of people who were still in the shock wave community, I fielded an instrumentation package for monitoring neutron generator standoff in weapons systems tests. I used the technique in various stockpile and proposed weapons systems from 1984 to 1996. After I retired in 1996, I was kept on as a consultant to continue advising the systems group on neutron generators.

My time in the shock wave group also significantly contributed to my health. In 1967, when Jeff Lawrence and I arrived, we were the only smokers in the department of 30+ people. There was evidence, such as stained fingers and old well-used ashtrays, that many others in the group had been smokers. However, because of the death of a member of the group from lung cancer they had all quit! When Lynn bet me a dime that I could not stop smoking, I stopped. I still have the dime taped to a card inscribed with words "Barker was wrong."

