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KORNBERG, Harry

Bonneville Power Admin. and EPRI
Formerly Hanford Biology Division

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and letters

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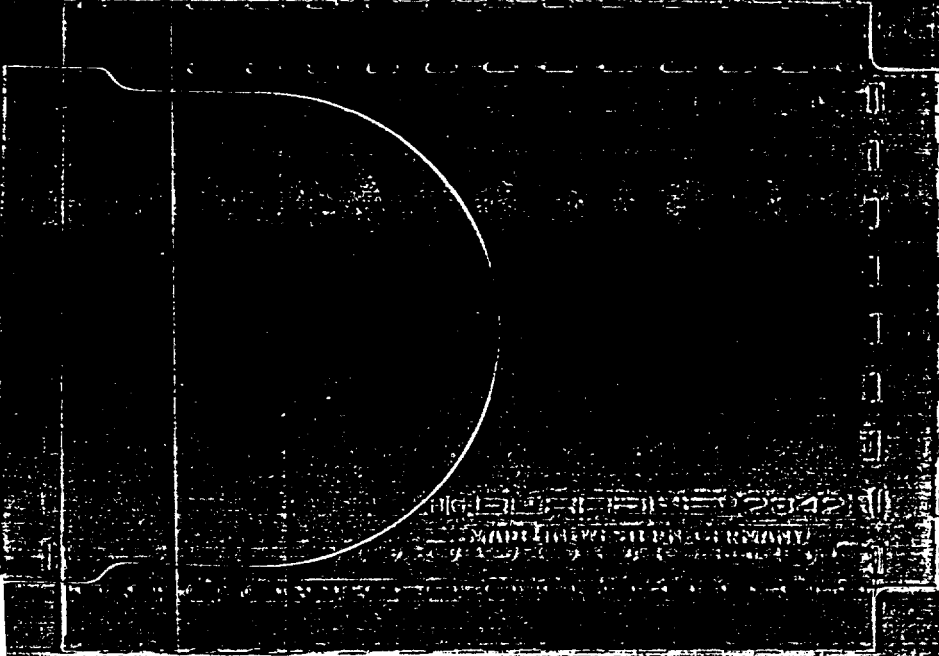
National Radiobiology
Archives Project

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FOLDER

Harry Kornberg



0027062

INTERVIEW WITH HARRY KORNBERG - SYNOPSIS
9-1-82

- The Test Site:

Sheep

G.I.'s

Test personnel - lack of full bioassay analyses for Pu or other internal emitters

- Accident at Hanford - Pu "projectile" in arm of worker

Treated with DTPA, washings collected, etc.

Early wash very effective

- General discussion of chelation therapy - also zirconium - toxicity of the agents

- Origin of Hanford Environmental Health Foundation well before the registries. M.D.'s with General Electric wanted independent arrangement

A bit about early days of registries

- Re/Jim Park's work - coverage in the chapter I gave Harry

Reviewed statements in manuscript

- Re/Hackett and McClanahan

- Tritium model - simple one - not right - led to doing some measurements

Re/Role of tritium

Re/AEC commissioners

- Back to NTS problems - thyroid surveys in Utah

- Contributions to chemical toxicology

Re/An unpublished paper he presented at early meeting of Society of Toxicology - lead toxicology

Kornberg - page 2

Use of short-term tests for long-term predictions

- Naming of outstanding people
- Russell-Grahn work on mutagenicity of Pu and tritium
- Park's idea that bone is really the "critical organ" for inhaled Pu - even insoluble
- Uranium Registry - not uranium miners

Uranium mines

Radon in homes

0027064

INTERVIEW WITH H.A. KORNBERG
His home, Vancouver, WA

September 1, 1982

Re: Utah Sheep

KORNBERG: [Contents of that section were looked at. We were off record on that, N.S.]
The scientist who started this is a guy called Harold Knapp. In a tome that measures a couple of inches thick he essentially claims that we forgot (those of us who were involved in those measurements) to measure the radioactivity in the sheep and in the food that the sheep ate which is an insult and an outrage. We, of course, measured that. We measured the bones. We measured the thyroid. Knapp should know enough that if there is any radioactive fallout eaten it would appear as radioiodine in the thyroid to begin with, and certainly we would have seen it in the radioactivity in the bones if there was any strontium. We measured it soon enough.

STANNARD: What connection does all of this have with the G.I.'s who were exposed at the Nevada Test Site?

KORNBERG: It could have an awful lot of connection, I suppose, because if a judge or jury can be convinced that we perpetrated fraud for the Government, the same thing would apply there. I have no idea how much exposure the G.I.'s got.

STANNARD: The only hole I can see in our monitoring procedures for the personnel around the test site was what I was mentioning the other day. We didn't have a very good bioassay program at NTS. They did nose swabs, but there wasn't much urinalysis, and it's probably too bad they don't have those records.

KORNBERG: They wore film badges. Was that about it?

STANNARD: They wore film badges, and in the so-called safety shots where it was plutonium, we wore protective clothing and full-face oxygen (actually air) breathing equipment. I thought we had gone out of our way to do urine analysis, but when I looked over the bioassay records there in Las Vegas in June, I couldn't find any urine analysis records. There were a lot of things that we set up

to do that never got analysed. They were snowed under with plutonium analyses. In fact, they've still got a lot of Kermit Larson's soil samples stashed away - samples that he took way back when. I don't know if they showed anything or not. [N.B. Not analysed when this was written, N.S.] Kermit finally let loose of them, and they are stored somewhere under the aegis of the Nevada Operations Office of DOE. If that thing goes the way it might go, it could cost the taxpayers tremendous amounts of money.

I feel impelled to say something about it besides just the facts of what was done.

KORNBERG: Yes, I think something has got to be said because you could get some guy like Harold Knapp claiming that even if uranalysis or feces analysis was done - really what happened was that they were inhaling and ingesting insoluble radionuclides that burned the gut and gradually got absorbed and deposited in bone and caused some kind of cancer. One can make any kind of claims, but anything short of whatever is thorough (and I don't know what a thorough analysis would be). Were environmental analyses made? Do you know how much was present in the air?

STANNARD: They were pretty good.

KORNBERG: I think one could then make a lot of inferences about it. Was your urine analysed?

STANNARD: I thought so, but I couldn't find any number. It was the last afternoon of a long meeting in Las Vegas, and the place was getting ready to close. I found so much stuff - they've got thousands of documents there. I ran into this fellow who I think was the guy that took the samples. I said, "Can you look up Stannard on such and such a date?" He looked and found nose counts, but none of the people in the group remember really specifically that we did a full thrust bioassay. I'm sure the same problem exists with the G.I.'s. Film badges won't show what they ingested or inhaled. Unless they did do bioassay of some kind, they are wide open.

KORNBERG: Yes, were any whole body counts made?

STANNARD: I don't know. I'd like to find out. I was going to ask you if you knew of any.

KORNBERG: I'd be surprised if there weren't.

STANNARD: I would too. Good point.

KORNBERG: Phil Cuckor phoned me late one night many years ago. An accident had happened in the Separations Plant, I believe. Some kind of minor explosion occurred in which a projectile of plutonium was imbedded in the muscle of the worker's upper arm. He was taken to the hospital immediately, and DTPA solution was already prepared and being made ready to apply. I requested that the bio-assay laboratory become activated immediately to take aliquot samples of the DTPA wash. We had a surgeon open up the arm, excise as much of the plutonium, and remove it as he could, and then I remember a drip situation was set up in which the DTPA solution was allowed to drip on the wound, and we caught the drippings in some kind of beaker; every 10 cc's or so would go to the lab, and I'd get the numbers back so I could plot it to see when it started leveling off. I didn't like the way the dripping was going, so I changed that. Instead of using a dripper I asked for a fairly large syringe (I think it was about a 20 cc syringe). I used that instead, so I had in addition to the complexing, I was using the force of the solution to wash the stuff out. We kept that going for a number of hours until it looked like the curve was levelling off. This guy ended up with less than the maximum limit of plutonium, but that was one time when we saw it happen when we were able to do something about it fairly soon. We gave him DTPA intravenously too, and that removed some into the urine, but most effective was the early wash. It did help to use the force of a syringe. It was published later.

STANNARD: What's your general feeling about these various efforts at therapy? It sort of started with Jack Schubert.

KORNBERG: Yes, and he started with zirconium, didn't he? Then Harry Foreman came up with EDTA. Then the DTPA came and Vic Smith, in our place, also did a lot with DTPA in animals. Our findings, and I guess those of Harry Foreman also, were that it was ineffective unless administered early - soon after the experimental animal received the plutonium. We followed that procedure in human accidents. We tried to get it out mechanically and then wash it out forcefully with DTPA and then inject it into the wounds.

STANNARD: Would it be safe to say that no lives have ever been saved by these procedures?

KORNBERG: I think it would be absolutely safe to say. Then on the other hand I don't think any lives have been lost by any of the accidents.

STANNARD: The lives weren't saved because they weren't in jeopardy?

KORNBERG: Right. And if they had been in jeopardy would they have been saved? I don't think they would have been saved by the DTPA. They would have been saved by a surgical intervention, I believe.

STANNARD: Do you remember the story on the toxicity of some of those things? I remember one of them had some effects on the middle ear. Maybe that was the zirconium. It was in England.

KORNBERG: Yes, but I can't remember it. It would be reasonable that the college (?) should do that. I don't think anything like that has been experienced with the chelating agents. Do you recall any?

STANNARD: No. I just recall the zirconium. I guess there was quite a lot of work on toxicity of the substances used as the chelating agents. They aren't innocuous. Of course, one had to be careful not to tie up too much calcium. The lung gavage technique (or lavage, I guess it was) got started from what Roger McClellan said - he sort of started it - but I don't know whether he started at Hanford or at Lovelace.

KORNBERG: We tried it at Hanford, and I don't even know if Roger was there at the time. Maybe he was, but somebody got the notion of trying that, and I

thought it was a rather horrible procedure, but later found out that it was not an unreasonable one. It was the most effective of anything that could be done for lung deposits, but it still wasn't effective enough to mean an awful lot. Again, as I dimly recall, it was most effective when used right after the incident.

STANNARD: I'm looking for a chapter that I can make short and sweet, and I thought maybe that would be one.

KORNBERG: That would be a good one.

STANNARD: Victor Smith has spent a whole career on therapy.

KORNBERG: Yeah, he's done all right. [Almost] hopeless to really do something in therapy for internally deposited emitters. There is a whole book out that Norwood and I put together, you recall, on the topic. Although it doesn't summarize what was known as of that date - and I don't think we know a heck of a lot more, but we didn't know much then that was of any great effectiveness.

STANNARD: I did some work in the early days on British antilewisite [BAL] and polonium. The chelating agents didn't work at all with polonium. Of course, polonium is not a bone seeker. It tends to form colloids by hydrolysis. It [BAL] worked; you could reduce the toxicity in rats, and also you could increase the gut absorption, which was an interesting sidelight. I'm a little surprised that British antilewisite wasn't tried in more cases. Maybe the chemistry said it wouldn't work.

KORNBERG: I remember your work. I remember reading of it - a very long time ago. I can't remember if we tried any of it or if we didn't. It seems to me I asked about it. For some reason or another we didn't go through with it. I don't know why. Was it itself toxic, or did the new ones coming along show more promise?

STANNARD: I think the other ones kind of eclipsed it. Of course, it worked awfully well for the original purpose - lewisite, etc., but I guess the complexes with radioactive materials were fewer - only the kind that were sulfhydryl bound.

How did, what is now the Hanford Environmental Foundation, get started?

KORNBERG: There again, Phil Fuqua would be the guy who could give the real story of that. He's a very good friend. Dag Norwood, of course, was the first president of that. As I remember, and this is just to lead you hopefully, Newell, to getting the story straight, when I first got there at Hanford, all of the M.D.'s were employed by General Electric for the Manhattan District, and then the AEC. General Electric, not being in the medical business, didn't particularly like the idea and the practitioners didn't either, so they were given the opportunity to open their own offices, which they gradually did for the most part. There were clinics, and a hospital was formed. In the meantime, there were still some industrial physicians, among them Norwood, Fuqua, and others that I can't recall at the moment, stayed on for a bit, but I think that with the help of General Electric a special foundation was formed for occupational health problems. Then with the addition of Frank Ably, long since retired - an industrial hygienist - an environmental health foundation was formed. I think that's the bare skeleton of it.

STANNARD: What is the connection between that and the uranium registry - or trans-uranium registry?

KORNBERG: The transuranium registry - - - ?

STANNARD: That was not just a Hanford activity.

KORNBERG: No.

STANNARD: I think Dave Brunner had a lot to do with getting it going.

KORNBERG: Yes. That was a very good idea. I was on one of the early advisory committees of that, and . . . a little bit hyper-critical about its lack of progress. Since Canada had something going - quite successfully - and they were way ahead of us, I wanted to see us catch up. I really don't know how strongly HEHF fitted into that transuranium registry. I think they had the Hanford part of it. I think they probably had that better in hand than most others. That's about all I know about that one. Being my usual critical self

I lasted two years on that committee and that was the end of it.

STANNARD: The Environmental Foundation has now taken on uranium, as well as transuranics. And I think it's high time - long past time, because this is the largest single group of exposed individuals we've got.

KORNBERG: Absolutely.

STANNARD: Just about six months ago I had a letter from the fellow who is running it and asked if I would become a member of their Advisory Committee.

KORNBERG: Good.

STANNARD: I'm glad to because I think more subjects are out there, unless the exposures have been really so low that you are never going to measure anything. There are a lot more people and the statistics are a lot better for uranium than with plutonium.

While I was in Los Alamos, I just happened to be there when the 26 - 27 plutonium workers were back. Louis Hempelmann was there - and they were having a great old time - with a reunion. Of course, all of them were pretty well up in years, but they are just a feisty gang. They say, "Look at us - plutonium didn't bother us." They were having a banquet and Louis asked me to stay. I couldn't, I had to go down to Albuquerque. They are still looking at them and still finding nothing, which is really quite surprising. I suppose if you did a calculation of the expectation of seeing one tumor, at the levels that they were at, it still might not be very likely.

KORNBERG: Yes, I guess that's right.

STANNARD: [Has questions re: the manuscript] 1. I thought I had given Jim Park quite a fair shake on what he did - you felt that I didn't quite show the extent of his contributions.

KORNBERG: Was that in the part you sent me? Bill Bair you gave (you did a beautiful job with him, but I thought you hadn't talked yet with Jim Park. But then knowing Jim - even if you talked to the guy - he's the same way.)

STANNARD: We just talked about the results. It was hard to get him to talk

about what his own identification was.

KORNBERG: Yes, it's very difficult to do that.

STANNARD: I guess I hadn't really focused on the fact that he was self-effacing. Maybe if you could find those pages of manuscript, we could see how I might change them a bit to give him better coverage.

KORNBERG: That would be a good thing we could do.

STANNARD: Have you got them handy? (Kornberg - yes). Clark and his bones. I gathered that what I said you thought was not quite right.

KORNBERG: I think that he really contributed some important information here - that doesn't come out (in this area) - deposition of plutonium. Here it is - at the end of the first complete paragraph on page 77, you say, "It is puzzling that the authors do not remark upon this". If you could just send Clark page 77 and say - "is this right - or did you guys later remark upon this"? That's the one that got me.

STANNARD: Now page 81 - we were talking about Sikov and Mahlum and you say, "It seems to me that Mahlum spent a few years in WDC with the AEC." I guess "WDC" is Washington. That I didn't know. Then you say, "is Sikov dead? The end of the paragraph reads that way". Well, he isn't, thank goodness.

KORNBERG: "He has stayed close to the last, with his colleague of many years, D.D. Mahlum, ever since."

STANNARD: Oh, I was just using the expression - meaning "close to the last" - like a shoemaker.

KORNBERG: Oh - my advanced years had me read it another way!

STANNARD. Well, that could be misinterpreted by someone else. I couldn't for the life of me understand your question - Should I say "he stuck to his knitting"?

KORNBERG: Yes, or "nose to the grindstone".

STANNARD: Or, "Sikov never varied. He just stayed with his field." Quite different from some of the folks we've been talking about. He reminds me of a fellow I knew

at Harvard. [Told story of Stacy French and purple sulfur bacteria, N.S.]

KORNBERG: "Jim Park continued after appointment to be Manager of the Inhalation Toxicology Section. He brought the diversification program to new levels that brought additional attention and continued keen interest in the inhalation toxicology work at Hanford." Something like that.

STANNARD: Great. Bill Bair made a big point of having me see a couple of girls that I hadn't focused on. One was McAffey?

KORNBERG: The two I wouldn't have thought would be the two Ann thought of - the Pat Hackett and Bea McClannahan.

STANNARD: Those were the ones. Evidently they had been there at it for years.

KORNBERG: They've been there for years. They came, essentially joined as technicians really, and they turned out to be really super technicians. They arrived about the time I became head of the biology division, back in 1959 or shortly thereafter. It was some years after, I threw them out on special leave; they hung my picture on the wall of their room in Pullman and threw darts at it as they were getting their Ph.D. They were very happy to get that degree. They were that good that I wanted them to get the doctorates, which they did.

STANNARD: Where did they go?

KORNBERG: They went to WSU [Washington State University, Pullman] by their choice. Again they are examples of people who stayed in the background and made great contributions.

STANNARD: Unfortunately, my interview with them was a little bit difficult because I saw them together. They had been bringing me people on the hour - every hour - for four days, and I was really getting kind of tired. I didn't have a counter on the tape recorder I had at that time, so I couldn't push a button to say when one was talking and when the other was talking. I would recognize their voices right away, but the girl who did the typing really had trouble knowing which one it was.

KORNBERG: McClannahan has been doing the chromosome aberration work.

STANNARD: About this model that you worked up for tritium.

KORNBERG: Yes - simple-minded.

STANNARD: Do you have any record of it or what it was?

KORNBERG: Oh, yeah, I remember I figured the thing out, and it looked so neat that I'd been pressured to give a seminar, since I insisted everybody give a seminar, and they said I had to do it too, and so I did. I gave it and I especially remember Dag Norwood and Phil Fuqua came out to hear it (the whole thing was mathematical) and both of them hate mathematics - so it went over like a lead brick with them. All I did was pretend that the lung was a piston inside of a cylinder. The piston went down for the standard man's breath displacing one liter - pulling one liter of tritium contaminated air in - and the inside of the piston was wet with water (water vapor was inside from the body), and I assumed that the piston pulling this liter in, mixed with the water vapor, and then pushed it out - and pushed out whatever was available to push. When I caused this mixing process, to my surprise, it didn't push out very much of the tritium. Most of it had mixed up and stayed there in this simple minded piston and cylinder. Well, that's crazy, and I thought we had better measure it to make sure, and that's when I found out that I was wrong about this piston not being right.

STANNARD: Why? Was the classification still going on, so you didn't know about what Los Alamos was doing?

KORNBERG: Oh yes, I didn't know what Los Alamos was doing then. They didn't know what we were doing. And those that were in the audience were those who had the need to know about tritium.

STANNARD: Ernie Pinson was doing somewhat similar work, but he never got in a chamber to my knowledge. Ernie got his degree at Rochester in water balance. He worked with Ed Adolph. They had one of these balances where you measure insensible perspiration. You sit on it, and keep losing weight just sitting there.

I have made veiled remarks that there was a lot of tritium around Hanford,

but I read it over again just before we came up, and I have put it in such a way that I don't think I have said that they were making the tritium for the hydrogen bomb project. I just said, "while tritium was involved in the fusion process". What I said was - there was tritium manufactured in the process rather than it was being made to go into the process. I wonder if there is any real reason I shouldn't say that the tritium was there to go into the fusion process, because it's known now.

KORNBERG: I presume that it is, too. I presume that it was then. I haven't seen this specifically stated anywhere, but that was the understanding we were given - why everything was kept pretty quiet.

STANNARD: "Pretty quiet"; is it alright the way it is? - if it didn't trouble you at all - you read it. It didn't sound wrong to you?

KORNBERG: No, no it didn't sound wrong.

STANNARD: I wrote it round-about to be sure that I didn't say some of the things that you could have said - simply not to get into what could still be a classified area.

KORNBERG: I can't imagine them being classified. Is somebody looking over this stuff, with that in mind? That occurred to me when I wrote.

STANNARD: A good question. I don't know whether Ray Baalman is....He would not be. Bill Bair is reading it all and is sensitive to the problem.

KORNBERG: You know a guy who would be good would be some real old timer. I could get Bill Burr. I don't know if Bill Lotz would know. I don't think so. I don't think he would quite have a good handle on it. Who was the other one you mentioned that had an M.D. - a good friend of ours from Washington, D.C. - later worked for Dixie Lee Ray?

STANNARD: Bruner.

KORNBERG: Dave Bruner. I think he might be able to help.

STANNARD: Of course, none of them were really biomedical people - except Dixie Lee and Bruner.

KORNBERG: I never did know any. I talked with Willard Libby a couple of times when he was there. Of course, the other one I knew very well was a later commissioner, Wilson Johnson. He had a strong interest in biomedical problems. That's the extent of it.

STANNARD: Bill Neuman was prejudiced against Strauss. He thought Strauss was an S.O.B. Strauss wrote quite an interesting book, and Dave sent me a copy of it. It is called "Men and Decisions". It was about the struggles they had including cooperation with the British. One of the things I am trying to decide is how much I need to say about the milieu for the work that was being done. I'd like to say a little bit.

KORNBERG: Yes. Hanford was looked at as such an outpost of civilization that we really had only occasional contact with Washington.

STANNARD: One of the things I find puzzling is that as far as I can see there was as much work done in studying the human population, around the Nevada test site, as studying the sheep - or a lot more.

KORNBERG: I would hope so.

STANNARD: I got the impression that there were some pretty good people involved in looking at thyroid nodules, etc. Was that so well settled that this is why they don't bring it up? Whereas, with the sheep, there is some room for doubt?

KORNBERG: I certainly wish I could answer that, Newell, but I can't. I haven't followed up on the Utah business at all, once having left Hanford ten years ago. As a matter of fact, earlier I had left radiation biology and went into cross linkage between biology and engineering. It's a key question. That may be addressed in this bulletin - BRH bulletin - but I haven't read it either.

STANNARD: We'll wait and see. Ed Wrenn, or somebody at the meeting in Las Vegas, showed a film of St. George (it wasn't a film - it was a lot of slides) and sort of re-hashed the whole thing.

One of the general questions I wanted to ask you was how you visualize and see how the work in internal emitter toxicology contributed to chemical toxicology. We

keep saying it led the way.

KORNBERG: Yes. I have been interested in that for many years. As a matter of fact, shortly after the formation of the Society of Toxicology (it must have been 15 or 20 years ago), I gave a paper in Williamsburg comparing radiotoxicology with conventional toxicology. It was an armchair exercise in which I tried to see what would happen if we established permissible limits for a few materials present in our environment the same way we establish limits for radioactive materials in our environments. What I picked was strontium-90 - the way strontium-90 permissible limit for body burden and concentration in foods was established, as well as the same for plutonium. I then applied those criteria (such as the concentrations of the radioactive materials) which would cause something to happen (cause a production of tumors, for example, in the case of bone seekers - and lung tumors in the case of inhaled plutonium). I compared that with the amount of lead that would be required. I came up with some very interesting figures. Recall, now, this was in the day before anyone ever thought of the big flap on lead. About all I remember from that paper now is that if we use the maximum permissible limit in food, for lead, it was established the same way we established it for either strontium-90 or plutonium. The amount of food we could eat was, I think, somewhat less than one sandwich per day. It would be exceeding the general population limit, which is 1/10th of the occupational. If you were in occupational exposure you could have somewhere around the equivalent of eight sandwiches a day. It turned out that the intake would not be enough to support life. I could see nothing wrong with the calculation - it was straight forward; it was the truth as I saw it. The one thing that I hadn't anticipated is that in the audience were a large number of toxicologists from the tetra-ethyl lead corporation.A copy of it is probably in the archives, and I'm going to try to find that and send it to you, because that was not published. I intended to publish it, but when I saw the reaction - the very strong reaction from

the lead people, I thought perhaps I was doing them a non-deserved injustice, so I gradually forgot about the thing. I'll look it up.

STANNARD: It would be interesting because now it seems that the points of view are coming closer together. Did you use the linear no threshold idea for the lead? Or a threshold?

KORNBERG: I didn't do anything of that sort. What I did for lead was look through the lead toxicology literature extensively. Essentially I did use a threshold. To find out what concentrations of lead and what amounts of lead could cause something to happen. I think it was some work in England in which it was shown that rather small concentrations of lead led to bone tumors. This work was questioned, but there were other indicators of chronic lead poisoning that I also used. The figures came out pretty much the same way. I did not have to go to a linear kind of extrapolation to get this figure. The data was good enough in the literature.

STANNARD: Of course, now the mutagens and the carcinogens are beginning to be plotted in the same way as we did for radiation. In a way, they are going to have to go down the same road that the radiation toxicology people did. One of the things I would like to try and do in the final chapter or the overview, or whatever, is point out some ways in which all of this work could save the chemical toxicology people from having to do all this over again.

KORNBERG: I think that would be great. For years I was interested in that. That was even before I gave that paper - so many years ago. Nothing at all was being done along those lines then, and I had no idea you had this in mind now, but that would be a beautiful thing to include. It's high time that someone brings that up again. It can lead the way. What it would do to modern society, I don't know, but I just now happened to recall another thing that I think I pointed out in that same paper. If we establish the same kinds of limits for ordinary conventional toxic material in water that we do for plutonium in water, we'd have to stop drinking water. I think I had some elements other than lead, but I know lead was one of the

notables. It would be better to drink plutonium contaminated water, at its maximum permissible limit, than it would be to drink water that we daily drink now.

STANNARD: That's just the sort of thing I would like to be able to quote-chapter and verse. I haven't had too much luck in finding good, solid material that has one foot on each side of the street. The chemical toxicology people do their bit - others do their bit, but one thing that I have kept wondering about is if there is any way they (the chemical toxicology people) could avoid doing these long-term experiments. I'm not sure they are going to try. It's easy to say, "you've got to do the same thing we did" and really study long term. As you know, the toxicology people have only very recently started looking at long term. They usually ran 30 to 60 days and that was it.

KORNBERG: Yes, that's right. This was one of the areas that we attempted to diversify in. In 1965 when General Electric turned over its activities at Hanford to the seven new contractors, one of which was Battelle Memorial Institute for the Hanford Laboratories, the whole idea of many contractors was to diversify the activities of Hanford. The diversification act I went into was to diversify our chemical toxicology methodology into applying it into classical toxicology. It didn't gain an awful lot of support. It was just too different. There are ways now in which it can be done using special short-term tests to get some kind of an idea. What pops to mind right away, of course, is the aims and tests similar to that. I think if we can set up some kind of observed ratios here, I think it would help cut down the length of time. It would give us a ball-park estimate. I'm sure there will be cases where we'll want to go ahead and do some long term tests.

I think we should know enough now about the relationship of some of these shorter-term tests to know what happens in the mammal to set up some kind of reasonable procedure.

STANNARD: Well, it's something I would like to do along with other things. It probably will have to be moderately short. The main area that you hear the most

about is inhalation toxicology. But I think that's mostly on the basis of technology. Inhalation toxicology worked out for radioactive material was way ahead for a long time of the technology for other inhalation toxicology. They were leading the way - probably still do.

KORNBERG: Yes. I don't think it's impossible that we could set up another system to get an idea of what would happen in such cases. I don't know if any work has been done at all using techniques such as for particulates. If it hasn't, I think it should be - both radioactive and non-radioactive. We need some new approaches in this for both to cross the bridge. You are right; I hope we don't have to look forward to the long-term toxicology experiments that we've done in radiation hazard assessment.

STANNARD: Another fairly general question I had was who you would name as the outstanding people that ought to be sure to get some billing of some kind or other in here. Of course, I've named a lot of them. Let's say you were going to do this and didn't know anything of what has been written - just going around the country and over the years.

KORNBERG: Now we're talking about internal emitters? Other than the Hanford people, we'd have to name those who started the very early work on uranium toxicity, which is considered an internal emitter. You've done that, I guess.

Certainly there, and then we'd have to go to the radium people - Rochester people - Robley Evans comes to mind. Names that I haven't used in years. Who are the people at Argonne?

STANNARD: Asher Finkel, Leo Marinelli, Bob Hasterlik.

KORNBERG: Yes, Bob Hasterlik I was trying to recall. There was another - was it a "Bob" working on dogs and cesium, I thought, at Argonne.

STANNARD: Bill Norris.

KORNBERG: Bill Norris, yes! I'm not putting them in any order. It helps me to just think of the laboratories themselves. Oak Ridge, I don't recall having done much

with the internal emitters.

STANNARD: They did mostly health physics for a long time. The biology division was all external radiation.

KORNBERG: I was thinking of the biology research. From there I guess you have to go to Los Alamos and Pinson with his tritium. Certainly Wright Langham. Getting out to California who were the very early workers there? Hamilton? Scott. Axelrod.

STANNARD: Of course, Durbin, a little later. She was a deiner in the beginning. She keeps insisting she did nothing but wash dishes during the war years.¹

KORNBERG: That sounds like Durbin all right.

STANNARD: While we are on Argonne, we didn't mention Miriam Finkel.

KORNBERG: Did Austin Brues get into internal emitters?

STANNARD: Quite a bit.

KORNBERG: Then he should be in there too.

STANNARD: Of course, he had a lot to do with the work on establishing the toxicity ratio between plutonium and radium. That was the whole basis for standards for bone seekers until fairly recently.

KORNBERG: Now that you mention it, I can actually hear his words on that score and how he attacked the notion of threshold - that this is just a convenient term used in medicine for drugs. It's gotten too strong a hold in this business. He should be included by all means.

STANNARD: You would be interested to know, if you don't, that Bill Russell at Oak Ridge has finally started doing genetic work with internal emitters.

KORNBERG: I certainly am interested.

STANNARD: With both tritium and plutonium. He's working with a fellow from Argonne who is well known in the internal emitter field. The Argonne man has been doing the injections, and then they've been using that same big mouse experiment type of approach [specific locus mutations, N.S.]. It's very interesting because the effectiveness

doesn't look to be as high as for tumorigenicity. The REE, if you want to call it that, would be somewhere around 1, at least much lower than 10 or 20 for the genetic effects. He keeps saying "it's just preliminary data - we don't have enough yet - it takes years to work these all out, etc." But it's finally going, and one of the things I've found hard in approaching the genetic effects is why there was no genetic work done for so many years.

KORNBERG: On the internal emitters?

STANNARD: Yes.

KORNBERG: We, of course, wanted to do some at Hanford. The signals that we got (nearly everything we suggested, as I said yesterday, was supported by Washington, but not genetics). I think the reason was that genetics work was so damned expensive, and they had a big genetic program going at Oak Ridge. I would like to have seen us do some.

STANNARD: One of the questions that's coming up now in the NCRP work to develop a risk system, instead of a dose system. We've got five Task Groups of this committee that are looking at the direct evidence from internal emitters for risk, as compared to external radiation. Because, of course, at the present time essentially all you do, except for radium, is use the risk from external radiation and then put in a bunch of factors, calculated dose would that be associated with it. We have what looked like at least two or three cases where the risk from a deposited radionuclide just isn't the same. One of them is iodine-131, which looks to be about anywhere a 10th to a 20th as effective at the same dose of external radiation.

KORNBERG: Is that right? That's fascinating, particularly with these Utah sheep.

STANNARD: Yes - it's something that I don't know how it can get in the report - or if it should. Mostly this has been coming out from the Marshaleese and experimental work. They've even gone to the point of trying to decide that there is an explanation; that the dose rate is much lower with deposited radioiodine, whereas with external x-irradiation usually those were at high dose rates. Then there are some things about localization, too. The iodine is highly localized, and there may be areas that get very low doses and areas that get high doses, so there are areas where recovery can

take place rapidly, whereas with the external radiation it's average dose, etc. Another instance is in liver. That it is mostly from the thorotrast data. One of the people at Utah is chairing this Task Group - insists that the data from thorotrast and from their Beagles, with thorium and even with plutonium, say that liver is more sensitive and the internal emitter more effective per unit of dose than external radiation. Well, that's the other way around.

KORNBERG: That liver is more sensitive from the thorotrast radiation? Well, to what extent does the colloidal property, or chemical property play a role?

STANNARD: Exactly, and that's why they are holding back. They are doing an experiment with something that isn't colloidal. I've forgotten what it is. It's something with, radioactively, the same properties, and they hope the experiment will be finished within a year, so we're holding off on conclusions to see if there could be a chemical contribution. But they are all gung-ho. They say the liver is not a radio-resistant organ, and it should be separately listed in the new ICRP system. Now there is no weighting factor for liver - it's just lumped together with other tissues.

KORNBERG: Now who is saying this? It would seem to me it would be so obvious.

STANNARD: Chuck Mays, Norm Telles, Austin Brues, Glenn Taylor.

KORNBERG: Austin Brues? Chuck Mays doesn't surprise me, but Austin Brues does.

STANNARD: The chairman of the Task Group is Glenn Taylor. They were quite insistent that we can't say for sure, but we want to be prepared. The other area is lung, and the plutonium work with lung, which is mostly the Hanford work. It seems to indicate that plutonium is awfully darn effective in producing lung tumors. You could, if you wanted to fiddle around with the calculations and not use too high an RBE for alphas, say that plutonium just has an unusual ability to produce lung tumors. There are still some people who are talking about chemical toxicity of plutonium.

KORNBERG: That's a hard one to swallow. Have you spoken with Jim Park about the toxicity - the tumorigenicity of plutonium (inhaled plutonium) on bone? Jim thinks

that the real (or he did some years back) long-range target organ for inhaled plutonium is bone.

STANNARD: I haven't talked with Jim about that specifically. This would be even for insolubles?

KORNBERG: Yes. That's the surprising one. That's why it made such an impression on me. This was just in a conversation with him. It would be interesting to see if he still feels that way. I think for plutonium inhalation toxicity you can find none more knowledgeable, first hand, than Jim Park. He's the guy who has been right there. He's been watching those dogs now for three or four dog lives.

STANNARD: Another thing I was going to ask you about - that's the uranium miner story. I didn't send you that chapter. As you know there was a lot of history of the governmental part in Salt Lake City and a lot of accusations that the government didn't do what it should in those days. I gather the miners are getting restive and may be going to start some of the same sort of business as we are seeing in Utah: But it was a different branch of government, of course. It was the Public Health Service, and the Public Health Service insisted that this was a state problem, and mining was within the jurisdiction of the state's assistance program - part of the public health service. I was at NIH during the time that some of this was coming up, and I know how diligently they applied that idea - that the Federal Government had no right entering into certain areas of the state's jurisdiction, and one of them was mining - anything to do with mining. This was one reason that the uranium miner business went so long, even though the Public Health Service was giving it money, etc.

KORNBERG: That's why I was so interested in the statement you made the other day that the uranium miners are now being included in what was previously called the transuranium registry?

STANNARD: Those are uranium workers, not uranium miners.

KORNBERG: Oh, uranium workers - not the miners?

STANNARD: No, not the miners. The miners have been a separate group - and I think they are being handled quite adequately. They are not in the registry.

KORNBERG: You no doubt said uranium workers and I read it as uranium workers of all kinds - whether they worked in mines or not.

STANNARD: I think of the uranium miners as all radon and radon daughter exposure, not uranium.

KORNBERG: I remember the first time, years ago, when Utah came into the uranium miners situation. I was very new to the game, and I asked with considerable shock that aren't the uranium miners covered by the same radiation hazard protection arrangements that the other atomic energy workers are. It was a very naive question to ask, and the guy answered (it may have been young Dave Bruner), "unfortunately, I'm sorry to say, they are not". But this always seemed to me to be a very strange state of affairs that probably goes back to the early history of mining in the United States and jurisdictions.

STANNARD: It goes straight back to the so-called "states rights". (You know when the Public Health Service had many "divisions" - I was in the Division of Industrial Hygiene, and right in the same building were some of the people from the division of State Services.) They bent over backwards to say the Feds should only lend assistance. "We are not going to tell you how to run your shop." If you need help we'll provide it. The AEC provided a lot of help through the Health and Safety Laboratory, in the way of instruments and people, but the jurisdiction stayed with the states for a long, long time. I found that very fascinating. Of course, now they are included. But it wasn't really until the Federal Radiation Council got involved and set up federal levels. They are still bouncing it back and forth. Have you had any contact with this more recent flap about radon in houses and closed spaces?

KORNBERG: No. I just follow the generally available news on this stuff, not being directly concerned with the area, but I think it's altogether correct and justified that this be looked at. It's part of the total radiation picture, but I think people

should understand.

STANNARD: We got headed off a little bit ago on naming the people that should be named.

KORNBERG: Yes. One would want to include the Utah people, too. My Chinese friend there.

STANNARD: Webb Jee.

KORNBERG: Yes, by all means, Webb Jee.

STANNARD: (Answering question) Tom Dougherty.

KORNBERG: Yes, he certainly should be.

STANNARD: I wrote to his widow. She was a hematologist. She wrote back and said, "I am just doing plain, straight hematology, and I'm so far away from it I don't think I can help you any". I read into it that she probably didn't want to go back very much. That's one project that's been really amply documented.

KORNBERG: Yes, that really has been.

STANNARD: Meanwhile, what happened at Oak Ridge in Comar's outfit - that's easy to come by? With Comar gone I've talked with people there.....

KORNBERG: Lotz is the only one you can go to now, I guess. Lotz, or maybe Trum would know - Rust - but those are the three that would be able to piece together what that did.

STANNARD: Well, I know it started with the Alamogordo cattle. I remember visiting, and they had all kinds of metabolism work going on with cows - great big set-ups, much as you had for sheep.

KORNBERG: I always admired it because we had a devil of a time doing it with sheep, and here they had cows.

STANNARD: I got the impression they went into fairly fundamental things, too. It was not all directly applied, or am I wrong?

KORNBERG: Oh, no, they were in fundamental work. I'm trying to recall one of the guys who was there before Comar came, who was doing just that, he was looking at some very fundamental stuff and doing some nice clean work. He left, I think he was

somehow found in disfavor somewhere in administration. He was on some kind of a committee to make recommendations for the future of that project. I remember that came out, but I was impressed by the rather sound fundamental work on the metabolism he was doing. I can't recall his name. But the people who were there were certainly contributors - Comar, of course, in internal emitters; Bernie Trum was there for awhile; John Rust - all of these people, I'm sure you've got information from them.

When we were talking about Oak Ridge, I meant to go then to the University of Tennessee, but forgot it.

Was there any work being done at UCLA on the internal emitters?

STANNARD: Almost all of it in connection with the tests. Kermit Larson and Staff Warren. In fact, Staff got so keyed up over Crossroads that he was involved with a lot of the later tests - even when he was Dean of the Medical School. It seemed to be a kind of an outlet for him.

KORNBERG: Very early in the fallout business, ? and Herb Parker asked me what I thought the major hazard would be from the fallout. He had some numbers that he was provided with from Washington that he gave me. I looked at it and said, "I thought that the major hazard (and this shows how wrong someone like I am can be) would be fogging film - this could get into the film industry". How wrong can anyone get - the public relations part was where the hazard eventually came.

STANNARD: It was the film industry that picked the whole thing up in the first place.

KORNBERG: That's right! That's the one I thought might be adversely affected.

STANNARD: Would you like to talk a little bit about the Utah sheep?

KORNBERG: Yes. I see that Leo did a very nice job there. The part in which he says he specifically undernourished a young lamb in order to test the sensitivity, specifically where he says he created radiation burns to test the sensitivity, I think both are very important. They are parts that I had forgotten about. My only

excuse is that we had a lot of things going on at that same time. In so far as renewing this whole controversy right now, I think it's an outrage; nothing short of an outrage, and an insult. It's an insult to the scientists who participated in the original trial. It's an insult to them that they would be, or could be, influenced. They were not. I think it's an outrage to the judicial system that one in science cannot argue. One can argue in law, but one cannot argue in science. We were arguing that there was no way that the radiation from fallout, or any other aspect of fallout under the conditions that existed in those animals in Utah, could have effected their deaths. What the devil was wrong in trying to convince people that that was a fact - it's beyond me at this time. I see no difference in that than arguing whether someone is innocent or guilty of killing someone - which is being done every day, I guess, in courts.

My opinion of what killed those sheep remains exactly the same - fallout had nothing to do with it. Malnutrition, endemic disease, poorly cared for sheep and people wanting to make the most of the situation and collect from Uncle.

STANNARD: I was a little puzzled on the transcript of Leo's remarks on which way he was leaning (see if I can find the place) - "the ranchers stated that the animals had some beta burns and some whole body radiation - and that they had lost a lot of sheep and quite a few lambs, and the lambs were somewhat smaller than normal. It had been a very bad winter; very dry, they had had some snow, and I suppose they had to dig their way through the snow, so it was very, very bad. The AEC called on the various people who were available on this subject. The people that were looking at it had not had any experience so far as I know with radioiodine or beta radiation and were not all that knowledgeable of sheep. So although they didn't invite us down for this, we were able to obtain data and said that the majority of loss had started as early as April and extended through May." This was 1953.

KORNBERG: The losses started before the test⁷

STANNARD: No, it was just 12 days after the first shot. It extended a few days after the second shot. One of my questions was, do you know who was doing that work there? I know they had veterinarians going in and looking at the thyroid.

KORNBERG: I think it was some Utah state Public Health people, plus local veterinarians that worked for the sheepherders. Neither one of them knew anything, as near as I can recall, about radiation biology. It would be up to Leo to say whether they knew much about sheep.

STANNARD: Then he says, "They sent us some samples from the adult sheep that were taken in early June and were identified by the farmers as showing the same things as some of those who had died. What we did, we had the experiments set up, because as you pointed out earlier the main study started in April 1950 in advanced gestation. In other words, they were in the last couple of months of their pregnancy. It was the same thing as far as exposure time goes, as in the Utah sheep. We looked at these samples that they sent us from these animals to see if we could see Dr. Mark's work, and some others. Anything showing there that looked like radiation and is the thyroid showing anything. What we did was take for each, etc....."

What I am a little puzzled about was whether he's agreeing that they saw nothing in iodine, but they did have beta burns - they did have whole-body radiation - or whether he's saying if they had anything it was not....[N.B. Was clarified later, N.S.]

KORNBERG: The way I read that particular part was that Leo say "Ok, ha, these were sheep that were pretty much on their last legs anyway because they weren't well fed. They were badly nourished. Let's go back and see if sheep that are badly nourished will be more sensitive to what we consider an inconsequential amount of radiation." So he went back and tried to find that out in which he vastly underfed sheep, which he didn't like to do, and then found out that, no, they were not any more sensitive at the highest levels in Utah.

STANNARD: There is another point in here that I find interesting that I think I ought to be sure on. Leo says that they did recover from their under-nutrition, etc. but the beta burns never changed. They just stayed the rest of their lives, but

the shepherders said differently. The beta burns probably healed up, and that's why they didn't see them.

KORNBERG: Right. The only way I can read that is that the Utah sheep have the ability to heal beta burns where any other sheep do not.

STANNARD: He says this probably came to trial in '55 or '56. At the trial they called me in - and there were also cattle at the Nevada test site and horses. He went down there and saw them. They brought John Rust and Bernie Trum. Both Trum and Rust had had extensive experience.....

- End of Interview -

Notes

Harry Kowberg - 9/1/82

Reese fur sheep -

b8 - Who at Yale?

b9 - Off to record become testifying -

b10-11 Story -

-12. Evans interviews -

Stowater effect - Written up anywhere?
Revised frequently but been less
continued -

b13 Feeds of contaminated vegetat
gradual solubilization -
Early 3H work - mostly metal?

Part of military

Plant work -

Stad 231B - Get Rod Res paper by Harry
* mentioned in his letter

221B - re/ Sheep in Utah -
Scene news - late August -

Per center accident - re/ Therapy

b9. 88 - Expand material in
footnote (c) to include

- Jim Parks -

Same as look over re/ Classification

Harry Kambary 9/2/52
Tape 231B continued

231B - Paper at Wallingbury to SOT
Chemical vs Red Top.

Extreme end is interesting
Long range target eye for labels T-1
Gene -

- Talk @ J-1 - Does he still find
that way?

Utah sheep case

US District Court - District of Utah

No C-81-6123C August 1952
C-19-55

Whit - Kambary

C-22-50

"Navy" spot 14.4 kt - lower (3 or 4)

March 29, 1953

"Harry" 32.4 kt - 1st

May 19, 1953

Steep in area 40 mi W of

160 mi ~~at~~ east

University High near
of Ocean =
C-22-50

Facilit for Navy with

Harry with self

Plants and Feb 17, 1953

Invest check June 5, 1953 94 PHS = 42

1/2/8 C

Utah content -

Visit at Hartford

has blank

Utah Range

Final report to AEC Jan 6, 1957

Concerned in by PH S, US Bureau

Atomic Industry, reviewed with

Concerned by Dept of Health of State

Utah

2 related repts. attached

"A comparative study of Hartford & Utah Range sheep"

"Crust study of Experimentally produced Beta leucis & skin lesions

Utah range sheep"

Both by Buxton

Material destroyed for "Fatal Lesions" report

Open meeting - Cedar City, Utah - 7/13/57

Sci investigators, sheep men, other

members of Institute

P. Casan, Trane from Hartford work

A Robert Ulenstein, US Army Vet for M&DL

concluded road was a satisfactory case

2 Robert Thomsen, small animal vet for Van Alams - ditto

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"Congressman Nixon's letter
Fraud?"

HSC had said Nixon letter

1956 adj. files in folder

with ad-concealed folder

BEC presumed to represent

witness to testimony or not

Constitutional knowledge

produced in committee

as data by subpoena

admitted answers

BEC falsified documents

RZ covered up

long paragraph about material

in Bill of Justice because

of fraud

Fraudulent some time

not actually in folder

denial evidence

Original stereo notes were destroyed

but long date to 10 year period

retention of such records

(Bank is hard to see notes even today)

11,770 check given in area

1420 laundry notes = 2970 records

dry spin & sent 1953

"Reason to believe" state report

PHS had serious mapping with a number of repeats. No names
Reasons for "cover-up" -
Functell comments at direction of
general manager.

Early visit to Cedar Lake
USPHS visit - Arthur Wolff
Vet - Inhibition R E Thompson
US Army Vets - J. Ruck, B. ...
Vet. em. - C. Coon

"Hot spots" into fallow pattern

1979 Hendy 1 - R E C did not have
doses @ continuing a few weeks
areas concerned
PHS (Hendry) in disapproval

p 22 detail on changing the maps
Reason - 5R

Inset - 36-103 f
Rog. a back of sheet 2-4m from another
fact was 26 days after exposure
Disagreement on fallow areas
Jensen (Commander) vs Johnson &
W. H. ...

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Ray Austin & Howard Jeff.

Quoted below - dated Oct. 1957

Memorandum said no evidence

There's soft copy for evidence

Undisclosed 1952 report

Thomas Hobbs - U.S.P.H.S. - 2000

Similarities Oct. 1952 report

825

Lab report

Similarities as:

stillborn

low birth weight

weakness

eyes - last of week

" - weakness

N.B. - 7.40 A.M. / day up to 10:00 P.M.

No indication of such doses in the fall of

Big fuss - Quoted below

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