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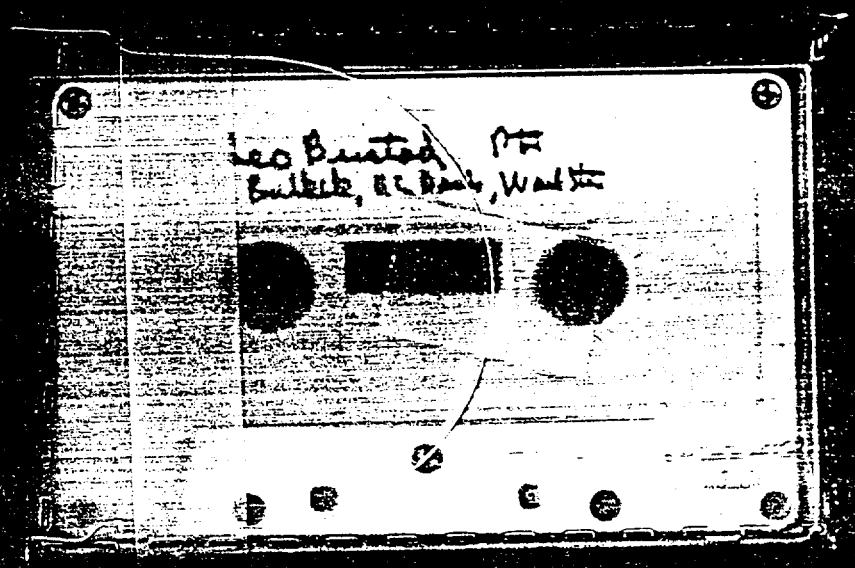

INTERVIEW WITH LEO BUSTAD - SYNOPSIS
9-25-81

- Much anectodal material
- Re/Sheep experiment at Hanford
- Re/Sheep experience at NTS
- Re/Pig experiment at Hanford

  Reasons for using the pig
- Military trained at Rochester
- Early inhalation work
- A bit on personalities
- Re/Working for AEC
- Advisory Committee for Biology and Medicine
- Contributions to other fields
- Public education
- Summer Institutes

Interview with Dr. Leo Bustad
from Washington State College
of Veterinary Medicine of
which he is Dean at Pullman
Washington. Discussion takes
place in Albuquerque at the
Inhalation Toxicology Research
Institue.

September 25, 1981

STANNARD: Leo, would you like to start with specifics on this stuff I sent you or reminisce in a general way first?

BUSTAD: Perhaps reminisce. You might start off with some questions. I have gone through this on the part that I am familiar with or have some first-hand association, and I've made comments. I'll just leave those with you. There might be one or two places that when we finish up I'll go over any questions and point out something.

STANNARD: I did get out the write-up here that gives the purpose, etc. I wasn't sure that you were totally aware of why I am doing this.

BUSTAD: All I'm doing for you was from the outline for this chapter.

STANNARD: I see. You didn't have the outline of the book? [Yes, he did]

BUSTAD. In 1948 I was reviewing the literature on radiation, and I was quite interested in it. I was contacted by Carl Herde from Hanford Laboratory about possible employment there. I guess they were seeking somebody because they were considering extensive studies in sheep. I was reviewing a lot of available literature, and very valuable to me was the Manhattan Series from McGraw-Hill. I went through those, and they were of great help to me to see how far they had gotten. Then I went back to the discovery of isotopes (Fermi) and then the early work of Lawrence and all those people, so I can see somebody going into this field - this is very valuable to have these books. Then there was a handbook on atomic energy (I have now forgotten the author.) [Samuel Glasstone, N.S.]

STANNARD: Alexander . . . . Glasstone?

BUSTAD: Yes, Glasstone. It was very handy. Having known that I joined Hanford on my wedding anniversary right after graduation with my DMV degree, and I already had a masters in not only nutrition - they were then doing a few pilot studies on sheep, and we were

planning for the large study on the sheep. I had to assemble everything pretty much from scratch. We were building the building, and it was pretty well planned when I got there. They started construction when I was there.

It was an interesting summer getting ready for all of this. I also got to know the other groups - like you mentioned Dick Foster (I was closely associated with that), and they were building the ecology building, and Pat Hackett was already there, and Kornberg was already in biochemistry. It was a small group so we were just speculating on a lot of things and getting organized. We were also looking at monitoring because Carl Herde was very interested in environmental monitoring and the significance of the effluents on the environment. I'll never forget - I and others knew about the cormorants that they utilized in Japan for catching fish. We thought maybe we ought to check those out - maybe they could catch some of the fish in the Columbia River for monitoring. So I, with a few others, (there were some cormorant nests across the river - we had an experimental farm across the river). At some risk I did get some young cormorants (I got pretty well chewed up), and we kind of tamed them down, and we tried them out in the river - we couldn't get them to fish well - maybe we didn't work with them long enough. I took him into Dick's outside ponds one night (he doesn't know this, I don't think, until today - I'll have to tell him about this). We put the cormorant in one of his pools. Boy, it caught a trout just about that fast.

STANNARD: One of Dick's favorite trout?

BUSTAD: Yes, one of the small ones. It wasn't an experimental group. So we knew it could catch fish, but they seemed reticent to do it in the river. I suppose we could have worked with them. I guess they gave that up. It was just an accessory thing for me.

STANNARD: One of the things I am shy on is anecdotal material for Hanford and some other places. Some of the places I have visited I know the anecdotal material fairly well. This that you are speaking of is going to be in the first chapter on the work around the sites. It is my impression that that work with the pile effluents with Foster and

Company and group along with the work being done over at the University of Washington was by far the first radioecology as we now know it.

BUSTAD: Yes, it was. They were into it early, and of course the long interest of Loren Donaldson. He was an early consultant; and, of course, the Columbia River as a major fishery was a very critical entity. What with dumping the effluent in there - and you know they established the retention basins to cool off the water for radio activity and perhaps a little bit on temperature because they were concerned not only with radioactivity but increasing the temperature of the river. As I recall at low water flow times maybe 43,000 cubic feet per second (that seems to come to mind)(but it would go up very much higher). However, at low times (and if you have enough reactors) you could raise the temperature and thus you could have some effect on the fish in the river. For example, calamaras is a disease of fish that I think is temperature sensitive. If you have low water flow in late summer and higher temperatures and with this impinging on it, is this going to be a serious problem? We had to consider that. We also had to consider - is this going to affect them migrating, i.e. coming up and establishing their nests. What was the extent of the streaming of this material? Do you release the effluent in the middle of the river - is it enough at the side - and then follow the pattern down to see if there are hot spots in the river as a result of these short-lived radioactive materials. So there was extensive work done on this. I think it was somewhat underrated. There was a fantastic amount of work done on this - a very dedicated group - Jerry Davis worked very hard on this. I haven't seen his name, and he still continued at least up until now [now it's nuclear regulatory commission]. He was a very good ecologist. And Wayne Hanson worked on this with the wildlife because we had this wildlife problem. The Columbia River was an important area where Canadian geese came to nest. Also they had the waste ponds,and the migrating fowl would come in there, so it was very important to know where these animals went. So they did banding because the Canada geese are

flightless for a short time - I think it is late July or early August - so, many of us would help them then run down the geese and put tags on them. We could then follow them and give instructions to the hunters to let us know where they went. We also did this for ducks - and also did a lot of radioanalysis of the ducks. We also had a deer problem there. There were some horses that were running loose on the reservation. You see, the reservation was very large. You could drive for 40 miles and still be inside of it. The horses were a hazard because people would run over them - out on the highway - it was a hazard to the drivers. But the deer were also a problem, and they were multiplying. I'll never forget, I drove into the experimental animal farm where I headquartered, and I met a patrolman; and I did a double take on the patrolman because sitting in the back seat was a deer. Actually sitting up and looking out, just like it was a member of the team. What had happened - it shows the fallacy of trying to tame a wild animal - this deer had been tamed by some patrolmen, and it got to be a real nuisance as it grew up. It was a real problem. It got to be a very mischievous thing. So when it got pretty big, they hauled it out to the other deer, way across the reservation, but it didn't realize it was a deer. It followed them around for awhile, but one day it was running along, and it came on to some people working on the railroad there, and it split off from the group and went over and joined the people! One day they decided they would put it on an island in the river, so they hauled it over to the island and dropped it off and scurried back, and it wasn't very long before the nearest patrol house for one of the reactors - here comes a dripping deer! What really sold its fate was one day it came into the area, got into one of the areas, and the door to the main administrative building was open. --It walked in and stuck its head into one of the secretary's offices, and I guess the secretary just screamed. Maybe it went through a plate glass window, I don't know, but I think from there it was taken a few hundred miles away out in the forest. It was kind of a funny story.

STANNARD: Your remark that that work has been under-appreciated is one reason I want to spend quite a little time on it in that chapter. I am no ecologist, as you know, but I can talk to people and listen, and I think it ought to be together with a lot of other things.

BUSTAD: Yes. I would certainly interview Jerry Davis on it.

STANNARD: I forgot about that. I've seen Dick Foster.

BUSTAD: Have you seen him? Well, it would be well to get his views. Another one I would give is Wayne Hanson. He is a very interesting guy.

STANNARD: I'm running out of time. I've interviewed over a hundred people.

BUSTAD: Because on that whole wildlife story, that's a big issue. Have you got anybody on wildlife?

STANNARD: What I would like to do would be to do this, and then either have somebody else or have them commission me to expand on it for another volume.

BUSTAD: Yes, because that whole wildlife issue - and what went on there - and Vince Schultz, you know he was at Headquarters for a while but is out our way now, and he has written some. Then some people in Colorado. But Wayne Hanson had been in it, and then he was up with the Annecubic Pass in Alaska - for all the cesium stuff on the fall-out and the high levels. They would haul up and count the people - they were the highest concentrations, you know, of anybody, of the people in northern climes. Like the people in northern Sweden - up in Alaska - and fish . . . . .

STANNARD: Oh, yes, because they ate reindeer that ate lichen. . . .

BUSTAD: That's right. It's a very interesting story. Maybe you can look up some of the literature. But then Hanson was also at Los Alamos. He was at Colorado with that group, and he spent a lot of time at Hanford, and he is back at Hanford now. He is a real interesting guy.

STANNARD: Well, I will be getting to Hanford because that's where I guess most of the editorial work will be done. Maybe I should try to see him.

EUSTAD: So you might want to see Wayne Hanson. He is an unusual fellow - I get a big bang out of him.

You see, there is quite a population of wildlife out there, not only water fowl, but there were sage hens and other forms that they were looking at. So that was an interesting era.

Another thing anecdotal was - it was heavily patrolled because we were in a security area. When I first went there, I had to go through five gates to get to the sheep. It changed somewhat with time, but I'll never forget - and it was a very remote area across the Columbia - one day a French national was looking at his map, and he thought this was a good road so he went down, and he came down across the river from all our industries, and his car broke down. He saw all this activity across the river - all secured. The Columbia river is very large - so I don't know if he stripped down - but he caught a log and he swam across the river. Well, needless to say, they saw him coming - they had spotted him before. Being a French national I imagine he was quite a long time explaining that. Of course they were right there to meet him, and drove him off. I don't know whatever happened to him.

Then, we should also tell you the alligator story while we are working on these things. I don't know if you remember that.

STANNARD: Bill Bair has told me. . .

EUSTAD: And how they got away - and we had to report to corporate headquarters periodically about how we were progressing.

STANNARD: It might be worth having your version.

EUSTAD: Some people were very interested in looking at alligators and their radiation resistance, so they got these alligators. As I recall, perhaps only one or two of them escaped. You can see that this would be a very unpopular thing - especially for the swimmers and the boaters, etc., down the Columbia. They were banking on the fact that they probably wouldn't survive because it gets pretty cold. However, they weren't

sure so we had to have people looking. Eventually - there was a hot spring way on down the Columbia across the way - and the last one was found in this hot spring. They came in and got it, and then the person whose land it was on, I think, he put up a complaint. You better check this with him - he complained that they were taking his alligator, or something, but then they went back (I think) and got some more alligators.

STANNARD: They got more?

BUSTAD: I think they did. I think there were two shipments. Anyway, they had another big escape of more of them, and that's when we really got into trouble. That's when some higher administration got ahold of this - and then we said Oh, no! We had to give a report as to our success - a weekly report, I think, on how we were making out finding these alligators.

STANNARD: I remember. That's the story Bill told me - how they had to account for every alligator, period, or else.

BUSTAD: Yes - it wasn't a funny matter. But as far as I know alligators have not established themselves in the Columbia River.

STANNARD: Am I right that the first appreciation that iodine was one of the primary fission products to escape and be distributed around came from the measurements around Hanford?

BUSTAD: Yes. They were certainly aware of it. I think Herb Parker set up a good system of detection. In his Health Instrument Group - he had a group that was running around that whole countryside taking specimens and taking measurements, and iodine was one of the things they found. That's when they conjured up - and that's one of the reasons I ended up there - to address the issue of "what is the significance - the biological significance - of releases of radioiodine " We selected sheep because they were a ruminant, and they were in large population around there, but we felt that they were a ruminant - i.e. grazing animals with great capacity so they would consume more per body weight than anything else, and they would also be representative of other

ruminant animals - other grazing animals - and they were cheaper, and they were available, so that's why we set that up.

STANNARD: I may have been incorrect in the chapter I stated, because sheep were probably the most likely animals to be exposed is one reason they were used. It was more than that.

BUSTAD: Yes, it was more than that because there were a large number of them there, but they were pretty much in feed lots, and there were a lot of cattle around there, but cattle were too expensive. We did eventually get a few dairy cows, but we felt the sheep data was very good.

STANNARD: I was thinking more of the location of all the reactors at that time were where there could be sheep grazing. I'll change that.

BUSTAD: Yes. I've already changed part of it.

STANNARD: Did you find any terrible boo-boos? I hope not.

BUSTAD: No, not very many, just some spelling. . .

STANNARD: Did you find it about at the right level?

BUSTAD: Yes, it was really fun reading. It was fun to recall some of these events. It was in 1949 when I joined them, and we were getting our plans in order, and then a deal was made, since thyroid was critical, of course, because it concentrates radio-iodine more than any other gland concentrates anything in the body. One of the authorities of the world, of course, in the thyroid was Shields Warren. Arrangements were made for me to go back to Harvard in the teaching hospital and spend some time there with Shields Warren's laboratory, which was, of course, a great experience for me. And what I did then (Fall of 1949 - four weeks in his laboratory). Then I was visiting around to many of the big names. I stopped off in Chicago to see Bloom, who worked on that project; I visited the people down at Naval Medical Research Institute in Washington; I went down to Yale to see (can't remember his exact name); and I did a lot of visitation and tried out various things relative to what kind of doses I should use, and it was exceedingly helpful kind of thing. I helped in the

planning of this. Of course, I mentioned the security aspect. Kornberg, of course, was one of the leaders in trying to declassify everything. Anybody in bio-medicine likes to have open literature discussions. So there was a big push on to get this unclassified, and we were successful, by the 1952 Annual Report, to have it unclassified, which was a great thing. We could then go out and tell people about the things we were doing because we felt that it was very critical to inform the public about what we were doing. Kornberg and the rest of us were very interested in doing this; in fact, it turned out well because of another great experience I had. I was asked to summarize some of our data at the International Congress of Veterinary Medicine in Stockholm in 1953. I was very keen about going there and seeing a lot of the institutions over there - to visit Harwell and to visit the Swedes and Norwegians because they had done a lot of work on radiation. So I gave some papers and talked to some very good people on the Continent and also relatives, for the first time, in my parents' native Norway. Since I had the language a bit, it was a very great experience for me, and I took my wife along. It was while I was there that the big Utah sheep incident occurred.

From the shots in the Nevada Test Site in '53 (I think it was especially the March shot - March 24 and May 19 shot)....

STANNARD: Yes, I feel compelled to write that up in some detail and as much depth as I can. I've got those big reports from DBER (I've forgotten the man's name now), and I have a lot of questions about it; but rather than ask you questions, why don't you just tell me what happened [off the record at Bustad's request.*]

-Gap-

BUSTAD: ...When I returned from my travels in talking about our studies, I returned to Hanford in the summer of 1953 and was greeted with the word that there had been serious losses of animals on the Nevada Test Site, especially following two of the nuclear explosions - I think it was the ones on March 24 and May 19, and we didn't have a lot of data. I had never been on the Test Site myself. I was

*[Note - After the legal battle was ended, Bustad could be more open. He then went over what I had written about the sheep and I modified it, N.S.]

familiar with it because we were essentially a laboratory of studies there [NTS-Utah] that we had set out. And, of course, in any test situation there was iodine - radioiodine involved, and they had some measurements on it. They also claimed that the animals had some beta burns and some whole body radiation. They gave us word 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 them - the various people who were knowledgeable on this subject. The people that were there looking at it (on site) had not had any experience, as far as I know, on radioiodine - or on beta radiation - and not all that much knowledge of sheep. So although they didn't invite us down there for this, we were able to obtain data and said that the majority of loss had started as early as early April and extended on through May.

STANNARD: Now this was still what year?

BUSTAD: 1953. So in other words the losses occurred starting about 12 days after the first shot and extended to a few days after the second shot. They sent us some samples from the adult sheep that were taken in early June that were identified by the farmers as showing the same things as some of those who had died. What we did, since we had the experiment set up because as you pointed out earlier, our main study started April 18, 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 effects - and is the thyroid showing anything? What we did was to take, for each one of their thyroids, we went through our whole list of control animals and found the slides that looked just like them and paired them off. We couldn't see any indication of any radioiodine damage on any thyroid they sent us. The bone marrow

didn't show it either. So we questioned this, and the following year (1954) we were brought down to Cedar City, which is the home of many of the sheep farmers. I and other people were brought in to address how we looked at it. I had my slides along to show what kind of experiments we had done. I, too, was very interested in this, not only from the standpoint of what really happened there - and are there some other samples they can give us. They had some readings as to the amounts, and it was a very interesting thing. The ranchers were there, and I tried to tell them what we had done, and then I stayed on, and they finally said to me, "Well, Doc, you know we didn't have nice fine sheep, well fed like you did. If there was radiation involved, it wasn't just radioiodine. They were burned on the skin, they had whole-body radiation, and that was a problem. "These sheep weren't getting very much [food], and most of us didn't supplemental feed them." I said, "You've got a problem." I thought about that, and so I went back home with Lyndon George and others, and we said, "You know they've got a point there". It really hurt me to do this because I got some young sheep (we felt we should start the study immediately - I couldn't breed some sheep then and go and take them through). I took some of our lambs - I starved them - I really fed them very sparsely. We had used plaques to study beta radiation of the skin of sheep, and so we took these plaques; we actually caused radiation lesions (beta burns on their skin.) At the same time we whole-body radiated them with, I think, 50 or 100 R, and we gave them several hundred microcuries of radioiodine. The reason for these doses - I tried to give them doses higher than anything that they had reported to me that occured on the Nevada Test Site. This was known as a triple radiation assault. One of their complaints was - their sheep didn't grow after all of this, so what I did after this was all over and they recovered, I put them on full feed and checked their weight against the controls. They recovered from this, but you could see the beta radiation lesions until they died. If you burn the skin, it stays there. The sheep there they said were probably healed up - this was a month later - well

radiation lesions where you really knock out the skin and all - their mark stays - they are marked for life.

STANNARD: Who was it that made these remarks - just one of the ranchers?

BUSTAD: Yes, the ranchers that I talked to afterwards. They mentioned it, and then I talked to them afterwards. I was interested because one of the issues was the lambs. Then this finally came to trial, as I recall, in '55 or '56; and at the trial they called me in, so it was an interesting experience. There were some problems with it because you see there were also cattle on the Nevada Test Site - probably closer in - there were also horses. I was eventually called down there, and I saw these. They brought in John Rust, Bernie Trum, and I think Lushbaugh (Lushbaugh did beta radiation studies for me). Both Trum and Rust had had extensive experience in radiation, and there was no hesitation on their part or mine to say, these cattle and these horses had beta radiation. The horses showed eye lesions. There was no question - they had it, but we could not detect this in the sheep. So it was a very interesting experience, and I felt very sorry for the sheepmen for they had incurred big losses.

STANNARD: Wasn't there some question too on where the fallout actually went? That is, that the projected path was different from what they said it was?

BUSTAD: I don't know. They are going back now and studying that. They are trying to get some data on it, and they hope to have the results on this sometime in the future.

STANNARD: Gordon Facer in the [old] Division of Military Operations sent me a big box with the maps of the fallout patterns for darn near all the shots. I don't really think, unless I do another volume, that I want to get into it, except to haul out particular ones, so if when you go back you remember the code names of these two shots you are considering I'd appreciate it.

BUSTAD: Yes, it's Harry and Nancy (I'm not sure, but I'll write it down to call you).

STANNARD: O.K., you haven't got anything else to do, of course, when you get home!

[redacted]

BUSTAD: He did some excellent work on this.

STANNARD: I think I have a quotation in here from that work.

BUSTAD: I remember we did tritium work in animals, and I had to wear a suit and a fresh air mask - and somebody was out there pumping the air through your fresh air mask while you're in there dealing with the animals. Fantastic experience.

Coming back to the radioiodine then, we extended this then to pigs because, as you note in there, we felt that some of the metabolic studies might better be done on pigs, but pigs get too big, and they eat you out of house and home, and you can't take one fall out of five, so we became very interested in miniature swine. I knew about the Hormel miniature, but we couldn't get them uncastrated. Thus we couldn't raise them - because they (Hormel) wouldn't release any that were intact for breeding, so that is why we had to develop our own. We wanted them white, so we could see redness. We were interested in de-contamination because the skin of the pig is quite similar to that in people. In the early work, for example, on radiation (erythema doses and all) were done on the pig (young pigs showed about the same as man). The epidermal turn-over time is about the same, so it had some real good reasons. It's a monogastric animal; we thought the GI absorption of a lot of these things might be most appropriately done on them. In nutrition studies, for example, testing infant formulas are best done on pigs. In fact, a lot of laboratories used to use pigs because they could raise a baby pig on the infant formulas for humans. If it would raise a pig, it would certainly raise a child because the pig's requirements were somewhat more stringent. So we felt this had some biological basis. We did develop the miniature swine, and we used some of the early white pigs of the standard breeds to determine the uptake of plutonium, which you mentioned in there. That was, of course, continued for a long time. But we also studied radioiodine, and one of the things we did (and you did it at Rochester) . . . . At one time we had an unusually good cadre in the military of radiation biologists - probably the biggest cadre of well-trained people in

radiobiology were in the military due to training at Rochester, and they had also come to our place. We trained quite a few officers. A lot of them made a lot of contributions through the years. In the early days one of the things, for example, we were interested in is-when is the first uptake of radioiodine in the fetus - fetal uptake in the sheep had never been determined, and it's very comparable to that in people. In this regard, Charlie Barnes, who was the first officer from the airforce to join us, took his training. He was the one who developed it - he was the one who deliniated what is the first uptake, and the pattern of uptake in the fetal lamb. He eventually went on the NASA, and joined NASA and was the officer responsible for the evaluation of radiation in space for the moon shots.

STANNARD: I got to know him on a NASA advisory committee.

BUSTAD: Yes - an interesting guy. We had a lot of people who went through there for their training. It helped them, and it also helped us. It was a very useful thing. I think one of our problems today is we don't have those now.

STANNARD: It's a big mistake too.

BUSTAD: Yes, it really is. We continued the radioiodine study. It's probably one of the longest experiments going. In fact, we initiated the main study April 18, 1950. I left there fifteen years later - went to California - and it was after I got to California that I got either a telephone call or telegram, "Today we fed our last dose of radioiodine". So it was a very long study and very complex.

STANNARD: That was all sheep?

BUSTAD: That was the last of the sheep study.

STANNARD: The swine went a little longer?

BUSTAD: I'm not too sure if it went any longer. We did some of those along with them. We didn't study swine for the lifespan....

STANNARD: The swine were more for plutonium?

BUSTAD: Well, we also did radioiodine studies with swine for quite a while to determine uptakes etc.

STANNARD: As far as the long haul. . . .

BUSTAD: The long haul was with sheep. We also did some beta burns on pigs and kept them on about 13 years. We put plutonium in them - in the skin - and followed them way out.

Then we used these miniature swine. We set up this experiment which you mentioned on strontium-90, and that was an interesting experiment, and it tied into these others. We also did some studies on some of the other radionuclides.

We did quite a bit on skin - on trying to decontaminate skin - because that's always a problem in the radiation industry. People get crammed-up on their skin, so we were testing various agents - we would contaminate it and then try to remove it - and what's the best way to remove it.

STANNARD: I didn't put in much on that. I have a chapter on therapy. I thought I would start it off with some of that before I got into systemic therapy. Before we finish, I want to hear a bit about your recollections of Harry Foreman's work while he was at Hanford.

BUSTAD: Yes, that was interesting. He was very interested in DTPA, and he was one of the leaders in that. He was at Los Alamos. He came up and spent some time with us. I can't help you much more than that. I sort of maintained contact with Harry Foreman for a long time after that.

You remember Hoyt Whipple? He was one of the first people I met and very handy to me because one of our biggest problems in coming there was what was going to tell us what happened to the sheep with radioiodine - critical dimensions of the thyroid. In order to have any kind of dose-effect relationship, you had to know what you had in the thyroid, continuously, because we were feeding them seven days a week for their entire life span. This was going to be the indicator of the dose to the thyroid, and if there was damage, of course, it would be less. So we established what we called the QOD.

The amount of radioiodine - we determined the thyroid - divided by what's in there every day. We found out some very interesting things - that it is cyclic - with season, and it's also the lower dip in the spring when they've had their offspring, and it is drinking a lot of milk. Their thyroid goes down (thyroid of the mother).

But what was really critical is how do you determine the amount of radioiodine in the thyroid with accuracy if you are feeding 0.15 uCi per day or up to 1800 uCi per day, or in one sheep - we gave 280 millicuries in one dose. Now, there was no one instrument, in those days, that would do it. So Hoyt Whipple and his associates (I think Gamertsfelder got into this - also Eisenaucher) helped us develop an instrument system - a series of instrument systems. For the high doses we used an ionization chamber, for some of the lower levels we used 3 GM tubes of a very special nature. You see this was before this scintillation counter. On one, we made a big rack with a tube in a shield in a container - four boxes - facing into the thyroid, and the sheep would stick his head in there and you would put it in there for the very high doses. This was M-1 model (M-1a, b, and c models) of a sheep counter. They weren't shop items. These all had to be manufactured. These are described by Herde. I worked with Herde on this, and he was kind of a honcho working with Hoyt and all these fellows. It was reported in our first annual report - this was for the '50-'51 (the 1951 Annual Report), and it's one chapter (or article) on instrumentation which describes some of this. Without this, we couldn't make it go. . .

STANNARD: Fits in very well, and I don't know as I am going to discuss that in either chapter. I might as well put it in here, so I'll look for that.

BUSTAD: Then we had to make a whole body monitor. We made a whole body monitor for sheep and pigs, and I also made one for a mouse. I took a leave of absence as an NSF postdoctoral to the University of Washington, which eventually led to my getting my Ph.D. I went over with Loren Carlson, and my study was on the radioactivity

in mice, and one of the things I was interested in was plutonium and whole-body monitoring.

One thing I should tell you, it was either 1951 or 1952, I was in charge of the experimental animal farm, and Harry Kornberg decided to reorganize, and he wanted to get something going in inhalation studies. So he said, "Leo, in addition to your other duties, you've got to get something going in inhalation studies". There was a lot of goings-on as to how you would make an exposure chamber for these animals. I felt we had to get something going, so Lou Temple, and we hired a physician named Ralph Wager from Florida (STANNARD: I remember him well), and we said we had to get going. So we started off with intra-tracheal injections.

STANNARD: Well, I'm crediting Ralph Wager with having produced the first lung tumor with plutonium by intra-tracheal instillation to be sure.

BUSTAD: It was he and Lou Temple. They did this working with me. I had enough to do, and my recommendation was that they should have a separate unit for this to do it right, and that's sort of where Bill Bair entered.

STANNARD: I'll always remember that.

BUSTAD: He was very successful. . . . But you see, in order to do this, you can't have this as a part-time activity. I felt very strongly about this. I really felt bad about Ralph Wager - he had a real medical problem - serious medical problem - I think his kidneys gave out. He took his own life. But that got it going. We had to get something going and we did. I'm proud - at least we got something going.

STANNARD: Yes, and they are still at it.

BUSTAD: I think my recommendations to have a separate unit and get somebody that could work full time at it were right. They were, of course, next to us - in fact, we modified space right in our facility at the experimental animal farm for their first work. That's where the exposure facilities were.

STANNARD: Yes. I remember a sort of a barn. One thing I would like to diverge just a moment on because Jack Healy wasn't at Los Alamos [when I visited] was what Healy did primarily at Richland. I find his name every so often, but somehow he doesn't fall in a slot the way some of the others do.

BUSTAD: He was in the biophysics end of things, but we did a lot of work together because he used to "massage" the data. He would get the data to apply it to situations and try to come out with predictions and applications. He would also figure out (was interested in) modeling and compartments. He would say, "You know what we need to do is do this kind of a study, and you take measurements at this time, whether its a pig or a sheep, and find out what's in the urine and the feces". He helped us from the mathematical and biophysical aspect on certain metabolic studies that would help him in his analysis and eventually for computer utilizations. So he was very valuable to us, although Nap [Jack?] says he made a mistake - he blames it onto me - but it was Jack who published the article. He said his co-efficients weren't quite right. I don't know whether that's true.

STANNARD: Jack's quite a nit-picker.

BUSTAD: Yes, but he was very valuable to us.

STANNARD: I got the impression that Kornberg and Parker didn't see eye-to-eye at all times.

BUSTAD: No, there was a lot of conflict.

STANNARD: Was that because Harry was trying to be too academic?

BUSTAD: Their chemistry didn't coincide. Their philosophies were quite a bit different.

STANNARD: Well, I'm not going to write that in the book.

BUSTAD: Herb was quite critical of him . . . (off record)

STANNARD: We want to spend a little time on the Davis days. This would just be general. By the way, do you have pictures in your files of any of the early Hanford installations and people? I have a feeling I could probably get them through Bill Bair's shop.

BUSTAD: Yes, they have a pretty good file there - maybe a few slides - we took quite a few pictures. Glenn Horseman and that gang should have quite a few pictures - pretty good ones, in fact.

They did the story of my life. I'd have to look at that - a lot of those are funny. We had some great times. We had a Christmas party out there - no booze - but we just had a good time - a great time. It was a good esprit de corps.

STANNARD: Yes, this is one thing that took me a long time to appreciate because of Parker. I thought he was so rigid - and he had a very tight ship, and also Hoyt Whipple gave me the impression (off tape).

One thing I wanted to ask was that you saw a lot of the AEC administration days; I saw it at Rochester but in an academic institution. From where I sat in Rochester, it couldn't have been more academic. True, there were lots of times when AEC phoned up on Friday night and wanted an answer on the toxicity of something-or-other by Wednesday of the next week. But on the whole, the AEC was a benign group - and I thought sponsored first class science. Now I've had time to talk to Dave Bruner, Walter Claus, and they look back on their days with AEC as the best part of their lives.

BUSTAD: I think if history truly reflects the truth, I would say the early days of the AEC, and their advisory committees, would have to rate among the best of any government agencies. They didn't have competing in-house research; they were completely committed to these programs; they didn't have a heavy hand; they gave us a lot of freedom, yet they watched it; they were in communication with; they didn't make unreasonable demands; and they would go around and review the programs, and I think they were honest with us in their evaluations.

One of the great experiences of my life was Shields Warren. . . . When they come up with an advisory committee, who was on the advisory committee, Detlev Bronk, Allen Gregg, Shields Warren, Weird - Dean of School of Medicine at Western Reserve and Goodpasture from Vanderbilt and Stakeman, a pathologist from Minnesota. These were all among the leading scientists of the world. They would come down and visit an old sheep-

herder like me at Hanford. They would spend a day and a night - you're out to dinner with them and spend the evening with them. My God, the day and evening I had with Detlev Bronk and Allen Gregg, my golly, people would give their eye-teeth for that. And then to be able to go, as a young sheepherder, into the laboratories of pathology at Deaconess Hospital - I had associations that are still carried on today. It was just a great experience.

STANNARD: Probably the organization of the Joint Committee on Atomic Energy was a very good accident, or arrangement, however it came about. I don't think the Congressional relations would have been quite as easy if it hadn't been for that, and the people that were on that were not peanuts, in general.

BUSTAD: That's right - impressive.

STANNARD: I'm delighted to be able to write about these years because I think they were superb years in our history, and they represent an era that is so different from the pantywaist era that we seem to be in now, where everybody is afraid to say their name for fear of what the lawyers will do. I can see what we were then. I don't know whether we could ever be it again, or not. But what do you think of this awful tendency to lambaste the AEC for the things they didn't know?

BUSTAD: People don't realize the complexity of the problem, and I think the work they got started and the work that was done, the instrumentation developed, has fed into all of biology and has contributed into all of these fields; and if history again is a true reflection, they will look on this as one of the great things that happened and that we did work safely. That we worked as safely as we did in those early years is certainly a reflection of the management. The problem in later times is that they did not carry forward with the kind of stringent demands we made. Boy, if we fouled up, even the slightest amount at Hanford, you would have a hearing that you'd wished you'd never done it - and you would question whether you wished you had been dead, and, boy, you would think twice before you ever did it again (if you had one of these Herb Parker investigating committees). It's like Admiral Rickover, but you see, I'm grateful for

that. If they would have had Herb Parker and our management system at Three-Mile Island, most people would never have heard of Three-Mile-Island, because it would have been operating right.

STANNARD: Quite right.

BUSTAD: We had stringent requirements. Many of the people were so dedicated they were working day and night. We felt we had interest and cooperation from AEC. I thought it was a great arrangement, and I think the amount of material that was produced of quality nature is very substantial. Probably for the money invested I think they would look a long ways for anything better.

STANNARD: Suppose you wanted to write down (I've called it spin-offs, but I'm not going to keep that title) the contributions the program made to basic biomedical science. You can think right away of certain things - I can think of Aser Rothstein's membrane work. He would never have started it if he hadn't been looking for how uranium affected the kidneys. He found that there were enzymes at the surface of the cell - nobody knew that before. Of course, Bill Neuman's whole career in bone biochemistry got started because he got thrown into that. How about some others?

BUSTAD: I think some of the plant work (I think you've already noted it) and some of the people who left our place who went on to carry it on looking at some of the basics for therapy - on the DTPA - and some of the basics. They had to go back to the basics on development of removal systems. I think about California when I went down there in 1965 and was there for about 8 years. When we were there, we were working on radio-strontium, radium, and we were working with Berkeley on the positron emitters in a short-lived isotope. One of the things that was discovered as a result of this - we were looking at some bone cancers, or some of the bones, and we didn't see anything in x-ray, so we gave them fluorine-18, which showed bone blood flow, and, my golly, there was a blank space in this bone (NO, we gave them fluorine-18, and it was a big white area over the bone) so then we turned around and gave them a positron emitter of iron isotopes,

and there was a blank there because there was no bone marrow there. So there was something you couldn't see on an x-ray. That led the people at Berkeley to develop other systems. Instead of short-lived isotopes they went to dyphosphinate, tellurium (into a tellurium dyphosphinate, I think it was) which is now standard operating procedure with the Anger camera - but this was the first time this showed, and it showed up in a strontium study with a tumor.

STANNARD: That's interesting. When I talked with Bill Meyers in Columbus, he felt that nuclear medicine and what I am calling internal emitter research really went parallel ways, and that internal emitter research didn't contribute very much to nuclear medicine. I suspect, in terms of the amount of contribution from the '30s, that's true, but here is a good example of where we joined up with them.

BUSTAD: They were developing and VanDyke and that gang - it was a very exciting time. Marv [Goldman] can probably tell you several more from there. I would have to think some more about this. I wrote an article about the various things we had learned about dogs as a result of our strontium study and the pay-off. I'd have to go back to the article. . . .

STANNARD: If you could find that, I would be very appreciative.

- Gap - [This time was spent in reviewing some of Leo's comments on the manuscripts I sent him, N.S.]

STANNARD: Thank you.

BUSTAD: For friends or enemies. . .

STANNARD: Did I have the foresight to send you a copy of the part of Chapter 8 that concerned Davis?

BUSTAD: Yes, and I commented on that.

STANNARD: You've been through it too.

BUSTAD: Yes. I had a great time at Davis, the eight years I was there. It was an interesting study. It was pretty much set-up when we were there. We started some other things;and before I left,we started to diversify which Marv Goldman carried out in good

form to diversify the laboratory. We wrapped up that study and pretty much went along on the other one. We got heavily into the myeloproliferative diseases as a result of that, and I think it was very useful to get all the "beaglers" together, and we did this during my time there. (In other words to really work together with the people from Utah - the people from Argonne and the people from Lovelace - to periodically get together to compare results and share.) I think that was a very fortunate thing to get them speaking together and more corporate work. We were pleased about that - and then the cooperative thing we did down in Berkeley, and also we did some cooperation with the Army at Lederman General Hospital [too faint to hear]. They had a pretty good Anger camera too.

The other thing we did during my time there - we were concerned with nuclear war, and I was involved with the Princeton Conferences on the Prevention of Nuclear War. It was a great experience for me, but a number of us, Max Knoll from Oak Ridge and John Rust and a lot of us, put out a document (I think it was the Office of Emergency Planning, or maybe it was the National Academy) on looking at the nuclear war effects on agriculture and animals. I don't know if you've seen that document.

STANNARD: I have, but I don't have one.

BUSTAD: I still think a lot of that data is very good. I had an interesting time. The physicians are just now recognizing this problem, that is,physicians in numbers. For example, I had dinner with Robbins, who is president of the Institute of Medicine [too faint to hear] and a dean somewhere.

. . . . .I was able to send him stuff that he was unaware of. A lot of physicians are organizing, you see, on this whole nuclear war effects matter, and some of them are just discovering the terribleness of it. It may have some positive effect because they had a conference, you know, at Arleigh House in Virginia this year, and I

Breschnev's personal physician was there. If they can get enough of the physicians from the leaders of the nations (unfortunately, I don't think Reagan sent his personal physician), they could have a real influence. I would encourage this.

Maybe we can gain. One of the things Parker had me do when I first got there - one of the few things I can do very well - I have some ability with communication, and I had collected all the material I could on Hiroshima and Nagasaki and the effects of whole body radiation, and I gave talks to a lot of groups to show them what happens once radiation occurs and some of the stuff from Hiroshima. Well, we probably ought to do that again, throughout the world. Hopefully it frightened them enough so you think twice before you do such a crazy thing. I don't know.

STANNARD: I don't know, maybe the reason we have the present hysteria on nuclear energy is because of the way it came upon the scene. People don't need any reminding. . . .

BUSTAD: Maybe not, but we also (there again, like you said there are two sides to the coin)- - - and show also all the things that have happened as a result of this. If you look at all of our work on internal emitters, a lot of the techniques we developed are what are utilized now. You know at Oak Ridge they used to bring the people down there for isotope techniques. You talk to a lot of these people that eventually got into it they had been at Oak Ridge or one of these other laboratories for the summer - learned the technique and its limitation and then went to work on it and went from there - labelling things.

STANNARD: Of course, the AEC had its instrument program for small schools.

BUSTAD: Yes.

STANNARD: . . . and high school and college summer teachers - institutes. . .

BUSTAD: I still get people coming up to me now, "you know you talked to me at the University of Washington summer institute." That was a great experience. I just had one guy within the last year, and it must have been twenty years since I was there.

STANNARD: I chaired the committee for AEC that was responsible for the Equipment Grant

program. I had something to do with the summer institute. It was all sort of same gemisch - mostly Chuck Shilling's doing. I don't see too many of them anymore, but then I see the military and how well they've done.

BUSTAD: I will say this about - had the AEC even done more of the summer institutes and had them properly schooled to mention the advantages of peace-time uses of nuclear energy and stress all of that, they would have had their fingers out in the major places, and that's what the teachers would have taught. But instead, we got the other, intelligent people - emotional and berserk almost. They just make some of the wildest statements. People who I regard as very intelligent; there is no basis in fact for what they say.

STANNARD: And they are not interested in the evidence, even though they are very stable in other respects. It's probably a mass psychological phenomenon, but that's another matter.

Leo, you want to get ready to go, and I've taken a lot of your time. I appreciate it very much.

Tape 208 A - Bustad

Act out confidence & see what
Chapter I sent him. - Read Chap 8
His start - Used NNES a lot in early days
also Alaska time
Contacted by Karl Herde re 1 day large study
in sheep - Pretty well planned &
facilities constructed -
Idea of comments to catch fish from
river -
Couldn't catch fish in the river very well
but did fine in Rich Foster's trout
pond (control fish)
Thermal effects at low water flows -
Fish - Calomares
Radiated group - mutually Jerry Davis worked very
hard on thermal effects as well as
radioactivity
Ducks, deer, horses
Tame deer - patrolmen had turned -
* Try to see Jerry Davis and Wayne Hanson -
re/ wildlife - Arctic -

Story of French national
Alligator Story -
Iodine - First appreciation -
Joined the in 1949
Was sent to Harvard to learn about thyroid
from Childs W Moore, also Chicago (Bloom),
McClosky, et al)

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0026787

Bustad - p2

He was on the team to learn about iodine when the 1953 Sheep[illegible] took place

Returned to Hanford in summer 1953 - was told of exposure of animals at NTS and asked to take part in field studies.

Iodine work - When does first uptake of radioiodine occur in pig fetus? About same time (relatively) as in humans. Done by Charlie Barnes, their first military radiobiologist.

Iodine. Main study initiated in 1150

Went over 15 years - after he had gone to UC-Davis. Largest expt. extant.

Did β burns in pigs - 13 years -

Pu in skin - 13-15 years -

Hoyt Whipple helped him a lot (c) dosimetry

Very wide dose range - needed a series of instruments - diff instruments for diff levels -

Recruited '50-'51 by Herde

208B

AEC advisory.

The early days of AEC and the Advisory Committees among the best of any Govt. Agency -

Completely committed to progress

Did not have a heavy hand

Honest in evaluations

Adv. Committee had best scientists in the country.

Take world sheepherder from Hanford out to dinner; Thull

Cragg, Bush, [illegible], Lawr. of Wells, and Morestin, etc.

The years represent a great era in our history

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0026788

Bustad - p3

x/ Criticism of AEC -
People did not realize the complexity of the problem.
That we worked safely in those early years indicated quality of the operation.
"If we had had Herb Parker and his management system at Three Mile Island you would never have heard of Three Mile Island"

Contributions to Basic Biomedical Science
DTPA, chelators in general - basis of removal systems -
Short-lived positron emitters -
Cl - 18 Collab @ Berkeley
circulation in bone - couldn't see on X-ray - showed black space.

McClellan starts in middle of 208B

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