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Synopsis

MED needed an industrial hygienist
his role in developing the H₂ in HEC
lack of consideration of internal control problems in early
phases of workplace programs.

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John B. ...

What is it -
Early hours for Nevada tests - each of
biomedical representatives.
Fallout at Rochester (2nd shift)
H₂SL role in alerting people to street measuring

REPOSITORY PNL
COLLECTION General Human Subjects
BOX No. J. Newell Standard
FOLDER N/A

DBM not interested at the time
Rob of H₂SL in recontacting the radiation work
re) Take John (Bailey) Pae - I never heard of a
followed up as idea
Richard was not really Shields Women's
initiative -

Fellow given job @ New Jersey Health Dept.
Hester Barner - took down from which
There was a break in continuity of the
radiation work.
Suggested I check Mth annual reports to check
on what was done (at what time) in early 50's

No fallout network for early tests
Setup with help of DBM but with
blessing of Pae & military applications,
Full publication as reports of facts & included
particulars - See also report (Archives Env. Health)
Cal 1950 -
John Baughman is Shields Women in attitudes toward
... in environmental matters
... H₂A checked program

EISENBUD: Well, I'm talking now about people who reached maturity in the industrial hygiene field before the war.

STANNARD: Before...

EISENBUD: Before World War II, yes. And then Bernie Wolff who you probably remember, he hired me. He began to realize that these dusts involve special techniques in collecting them. You had to have somebody who understood how to bridge, the engineering ~~of~~ biological gap. I guess Jim ^{Stewart} ~~Stewart~~ may have, ~~stood toward~~ ~~an industrial hygienist~~. He hired me. Jim worked with Bernie. And I set up ^{AASH} ~~Hassle~~. In those days was a constant battle to keep it alive because they didn't understand why the national laboratories couldn't do everything that we could do. And they said the same about Rochester. They wanted you folks to do it. There was a lot that you could do, but you didn't have the practical field experience that I was able to put together with Hanson Blatz and Bill Harris and myself. We trained a whole generation of people that are now the Ed Wrenns out on their own.

STANNARD: Well, I get the impression that there was a little pulling and hauling even during the Manhattan Engineering days when everything was supposedly very practical between places like the Metallurgical laboratory where they still were scientists and wanted to be scientists. The practical problems where they got hauled out and said, "Well, you better go to the ^B ^C Brush Corporation and take some samples." I gather Rochester was more cooperative in that sense than some of the other labs.

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EISENBUD: Oh! Rochester played a major role. Of course, you were doing all the biological work. But nearest thing you had to an industrial hygienist was Sid Laskin who I guess gradually developed a lot of techniques. But certainly in 1945 he didn't have the kind of background that you needed to go in and clean up a plant. This is not, I'm not speaking disparagingly of Sid, it was just that it's a special kind of skill. Now it had some advantages. Sid developed some nice instrumentation. But later on Drinker, Hatch, Silverman, Williams, myself, and then the slightly younger group like the Harry Schultes came in. But that was all in about 1948. I was the first. I went in in '47, and I brought most of the others in as consultants or actually to do the work.

STANNARD: Well, my chief assignment is to describe the research. But in answer to your original question, I don't think it would be a very interesting book if I just talked about the results of the research. I do want to get as much anecdotal, pertinent anecdotal material even semi-political things ^{as} ~~in~~ that I can. Have you read Neal Hines, Proving Ground?

EISENBUD: Yes.

STANNARD: That reads very well. If I can write a book that reads as well I'd be happy.

EISENBUD: I think he's, he's not a scientist.

STANNARD: He's a professional writer.

EISENBUD: Right. Well, you see you've got to understand these things

to explain why it is that... Let's see. For example, when they set up the weapons testing program, the only attention was to external radiation and the criteria for using ^{Continental} the, they finally decided to use the continental test site. It was based on the assumption that nobody would get more than I think it was 25 r in those days. They had no concept that there may be problems due to internal radiation. Of course, and I don't understand why they didn't consider delayed effects either. They should have known about them because they had the radium experience by then.

STANNARD: Well, that was one of the questions I had written down to ask you. Mel Carter gave me a pretty good description of the role the Public Health Service had in off site monitoring at the Nevada test site. The fact that they had somebody on ^{duty} test group that had the veto power. For instance, Howard Anders had veto power over any shot. It wouldn't go if they felt that the radiation hazards somewhere off yonder were going to be unacceptable. On the other hand, just recently I received through ^{Bill} Newman a letter from a law firm that has old secretary Udall in and he's raising hell about how the AEC biomedical people stood back and did nothing while all of these innocent people were being exposed. I'm not sure of my facts ^{over} here but were they really standing back and doing nothing or were they just not aware?

EISENBUD: Well, let me tell you what happened during the first series. First of all,...

STANNARD: The first series in Nevada?

EISENBUD: Yes. First of all, let me say that I met with Udall last week. He came up to see me, and he went ^{from} ~~me~~ to see Shields Warren. So apparently he's making the rounds. He showed me a memo which he promised to send me a copy of, which shocked me. It was the minutes of a meeting held down at Los Alamos ^{which} ~~which~~ they decided to use the Continental test site. They were banting around all these numbers as to what would be permissible and what people would be exposed to. There wasn't anybody from DBM there, and that was either 1949 or 1950. They were already several years old. The only physician there was Jim Cooney. Do you remember Jim Cooney from the Army?

STANNARD: From where?

EISENBUD: He was the radiological physician.

STANNARD: Oh yes.

EISENBUD: And the only other person with a biological orientation was Wright Langham. So there wasn't anybody representing the Public Health Community. And there wasn't anybody in DBM that had a public health perspective. Now when the tests started at Ranger we at HASL didn't even know about it. Which is interesting because after all we were AEC staff and we had probably the best developed field capability for sampling in the world. By that time we had 100 people at HASL and we could measure anything we wanted to in the field. Instead they had alerted the national laboratories to set up two or three gamma recording measurements. Brook ~~Haven~~ ^{haven} had one ~~at Brook Haven~~ and one up in Boston I think and one up in Maine maybe, I'm sure of

that. Now after the second shot there was a fallout at Rochester which you'll recall. Harry Elair called me and told me about it. Told me that he had gotten his information from the Eastman Kodak Company. They had confirmed that the background was up very substantially. I called Tom Shipman who was down at the test site in charge of radiological safety. Tom just poo-pooed it. He said, "How could that be? I've just been out to ground zero and there's no radiation there." Of course it was an air burst you see and it all went up and moved over to Rochester. We at ~~HASL~~ ^{*}, I was just mad. The first thing I did was call Cooper who was in charge of things out at Brook Haven, asked him to send somebody up to read his instruments which is all we had. It was Friday, and he thought it could wait till Monday. And it had snowed. The radioactivity came down in snow. So we called people we knew. We called you folks up at Rochester. We called the ^{Melvin} Allincrot Company down in St. Louis, and ^{Harshorn} at Cleveland. And we called somebody up at Harvard. We had within a couple of hours people out collecting snow, putting it in ice cream containers which were then hand delivered to us in New York the next morning. We boiled them down, did beta counts, which is all you could do in those days. We had a map of that fallout episode. But that's the way it came about.

STANNARD: What year was that?

EISENBUD: Well, it would have been '49 I guess or maybe the winter of '49 -'50, or '50-'51.

STANNARD: That must have been when the test site was just opened.

EISENBUD: The first series! It was the second shot of the first series. *

STANNARD: I see.

EISENBUD: Now, you see, HASL reported to Kelley who was the manager of operations. We reported right to the general manager. If we had to depend on DEM support in those days, they would have just eliminated HASL. They didn't see any need for it. We had, it was quite a struggle. But in your area I think we accomplished quite a bit. For example, it was we I think who got the radium study started again. There's a tape that you ought to get hold of which John ^{Harley} ~~Henry~~ I think had transcribed. It was a meeting with ~~Mark Lynd~~ ^{Matthew} and Hobb and Bob Evans, everybody that was involved in the radium cases. *

STANNARD: That's very interesting. I got the impression that Shields Warren had reached in and saved Bob Evans' program.

EISENBUD: No, that came about in a very peculiar way. First of all, the decision was made, you ought to get that tape and read it, that all of the information had been milked out of the radium cases and that there wasn't anything that needed to be done. And this kind of bothered us a little bit. Several years later, maybe five years later, a very strange character came along who was working for the New Jersey health, ^(M) no, I don't remember where he was working. I got him a job with the New Jersey health department. He came in and wanted to get a Ph.d. in health physics. I was still at AEC but I was part time with Norton. And he said, "Can you think of a *

problem I could work on?" He had completed all his course work. I told him I'd always had the feeling that there was more information to be obtained over at New Jersey. I got him a job with the New Jersey health department. He went around. He got himself a detective from the police department, and he began to locate people.

STANNARD: Who was this?

EISENBUD: Lester Barrer. It turned out to be a disaster. He had some, oh, I guess to be kind, I'd say psychological problems, a little paranoia. He began to ferret out some cases. Now whether Bob Evans was simultaneously getting interested in it again, but there was a lapse of several years where there no measurements made. I don't know, but I always felt the reason that Bob Evans and then later the Argonne got involved was because Lester Barrer began to organize ^{the} thing down at New Jersey and they thought well, they better get into it, too.

STANNARD: That's very different from the story I got both from Bob and Sheilds Warren that there was essentially no break in continuity.

EISENBUD: Oh there was, there was! You look at Bob's record of the number of cases measured. That's the best way to do it. There were years there where I think there might have been none or maybe one or two cases measured.

STANNARD: Well, that was the impression I had originally. And I was surprised to get this feeling that...AEC, if there was an AEC. But this was pre-AEC.

EISENBUD: No, no. This was in the AEC days.

STANNARD: I see. This was early AEC?

EISENBUD: Well, not even that early. It was I think that Evans program essentially died as far as radium was concerned in the early 50's, and didn't get started until about 1956.

Check Evans' takes

STANNARD: I see.

EISENBUD: What you ought to do is look up his annual reports. I suppose that might be hard to do, but you could get a feel for what he did.

Check those out.

STANNARD: Now this fellow in New Jersey was Barrer?

EISENBUD: Barrer. As I say he was a disaster. He caused a lot of trouble. He became paranoid, and had terrible scraps with Bob Evans.

STANNARD: Maybe that's why Bob doesn't say anything about him.

EISENBUD: Well, I could see why. He was very, I think he was a little devious. But he accomplished quite a bit. He showed that using professional investigative techniques, which hadn't been done up to then, was possible to locate the radium dial workers. He used a detective.

STANNARD: As you see, I have a chapter on radium. I wasn't planning on making it very long except to put in the parts played by the establishment between 1940 and the present. I plan to reference these beautiful papers back clear to 1898. I have a story there, but I don't feel that it's incumbent on me with these things so beautifully summarized [Eisenbud] to write a long chapter on radium—except to start the whole thing off with radium because that's what started it, and what's really

still continuing it in many ways.

EISENBUD: Yes. There are a lot of things that... (SKIP)

STANNARD: Now the Udall ^{also} ~~and~~. I just finished writing a very ad hoc response to him in which I said I thought that the problem was more that the ~~biology in medicine~~ the biomedical experts were not listened to. ^{also} ~~And~~ that the information that they had was simply not good enough to prevent the military from predominating and saying that "We've got to have this information for national security." And the only people that could say whether the risks taken were really worthwhile ^{with those that knew what} we learned from the tests in terms of national security. ^{There} ~~There~~ ^{probably aren't very many people like that. In fact, I can't think of any. I had the feeling that he ^[Udall] was putting it all at the door of the biomedical community. And the thing I don't know about and you will is whether AEC did intimidate their biomedical people from above. And whether those of us, you know, some of us at Rochester said, "Hold it. Now, let's not go on with these tests until we know what the story is." And they said, "Oh, we can't wait. We've got to get back. Commissioner Libby hauled Bill Newman and me down to Washington and dressed us out." ^{''} Made his life miserable for him. ^{!!} So we know there was pressure from above. But was AEC consciously misinforming the public in your view? Or were they just not informed ^{and} relatively ignorant in terms of our current knowledge?}

EISENBUD: They didn't go out of their way to get the facts. A professional approach to the problem would have demanded that when they started a test, there'd be a fallout monitoring network.

And there wasn't any. Julian Webb up at Kodak was the first to point out that you could have fallout at great distances from the shot. You know the story.

STANNARD: In general.

EISENBUD: Well, Kodak has its own ^{men?} They traced ^{Contaminator of film} ~~utility~~ ^{back to a} contaminated corn field in Indiana after the Trinity shot. If you're going to be an historian you've got to look at all aspects. You mentioned Bill Libby. Bill Libby was one of two scientific advisors to Nixon during ^{his} campaigning which I think illustrates a certain point of view. Now do you know who the other was? Shields Warren.

STANNARD: Is that so?

EISENBUD: Now Shields Warren was that conservative. The only other person I've ever met that was more conservative politically than Shields was John ^{Buzler} ~~Boyer~~. It's possible to understand why they fought as they did. I don't hold it against them. John and Shields were extremely conservative politically. They were completely dedicated to the likelihood that there was going to be a nuclear war in the early 50's. They believed it. So much so that I believed. And I guess come to think of it we were pretty close to it. If you believed that then there were certain risks that you would expect the public to take. And I go with them that ^{*} far. But why they didn't set up the monitoring network I don't know. We set it up at HASL. We set it up with the blessing of the Division of ^{Military} Applications and against the wishes of the Division of ^{Biology} and ^Medicine. Then if you're going to

put together a team to study these things you'd want a little more strength on the physical side than they had. They had nobody that understood particle technology down there. They asked me to prepare a memorandum which incidentally got published I don't suppose you'd remember, it was probably the first review of the deposition and fate of ~~an~~ inhaled particulates as published in 1951 in the Archives of Environmental Health ^{or} Journal of Industrial Hygiene, I don't remember. But the history of that was interesting. I used to sit in at these meetings. These people knew nothing about dust. You know, even the well-trained biologist knew far less about the physiology of the lung than I did as an engineer. So finally in frustration I put together this memo which they like ^dso much. I think it was Shields that suggested I try to publish it and I did. It was I think the first review of the subject that had been published up to that time which was about 1950 or so. I could send you a reprint of it.

STANNARD: I would love it. As a matter of fact, I'd like to ask if possible if you'd send me a curriculum vitae with a list of publications. And then reprints of what you think I could use because you're one of the folks I want to emphasize and I just didn't ever get a very complete set of reprints from your early work.

EISENBUD: Well, I think we made important contributions. The reason they were important was that they came from a group that were totally unsophisticated by the standards of the Division of Biology and Medicine expected. In other words, DBM wanted every-

body to have a Ph.D. I think the only one that had a Ph.D. in our group was John Harley, and he got it from work in the laboratory which I want to tell you about because there's something you ought to know on that one. You had to work ^{human} in an academic background; most of us came from either industry or health departments. In my case, insurance industry. So we were sort of a different kind of group, but we were the ones that saw the need to do something out in those radium plants. We saw the need to do something in the mines. And were told to stay out. No question about that. We were the ones that stepped in when nobody else was able to get the facts about fallout. Then the group of us that went over to NYU were the ones that saw the importance of iodine, and did the first measurements on humans. You know, exposed to fallout. And did the first blocking experiments. That should have been done. (NEW TAPE) We saw the need for world-wide monitoring. We set that up again not with the cooperation of DBM. But mainly the State Department was concerned. They made ^{their} embassies available, and we had these collectors up on the roofs of the embassies. Then when the big fallout occurred in the Pacific in 1954, there is an interesting background there. DBM did not, well, no, I would say that, the Task Force did not see the need to monitor beyond Eniwetok in Bikini. By that time Shields was out. John ^{Boyer} was much more field oriented because of his background. Rockefeller Foundation, worked in the field as you know for many years. You know, it gives you a different perspective. You see things. You're wearing different spectacles. So he saw the need to expand the

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for earlier

monitoring network. Actually it was John and I that saw the need for an intensive Pacific monitoring program, partly because we knew what had happened down at Jangle which was an underground test in Nevada. There was massive fallout close by, and that was only a fraction of a kiloton. Here they were going up to 10 megatons. I would say at that point which would have been 1950, starting with the Mike shot in 1952, we got complete support from DEM. And eventually ended up working for them. It was very nice after that because John and later ^{Dunne} ~~Dunne~~ got along great. They sort of used us as part of the division of biology and ^{and} medicine which Shields had not done.

STANNARD: Another question I had, Merle, ~~was~~ there was a lot of uranium research done in the laboratory, and you had a lot to do ^{with setting up the field work} ~~with setting~~... What contributions do you think it made to ^{the} setting of standards and the general understanding of uranium health problems? Was it pretty direct? Of course, there was Rochester and Tannenbaum..

EISENBUD: Yes. I think it was important. After all, uranium which was a very rare metal all of a sudden became one of the major ferrous metals almost over night. It was the work of Rochester that called attention to the ^{mexico} - toxicity and emphasized that it was probably the toxicity rather than the radiological hazard that was important. Now where I would fault Rochester, and ^{Frank} ~~Kersch~~ and I have talked about this, and I guess I've talked about it with Harold, too, was for not seeing the need to do as much field work as they did laboratory work. Because while, it was very interesting, the contrast was shown at the '55 Geneva convention

where I think it was Harold that read a paper from Rochester group and I read one from our group. My conclusion which was published almost in that language was that among all the heavy metals uranium was probably the least toxic. And Harold's conclusion based on the experimental work was that it was among most toxic.

STANNARD: Now why the difference?

EISENBUD: Because ^{of} species difference. But the point is we had during the war several hundred men exposed to concentrations that were several hundred times greater than what had been recommended as a result of Bill Neuman's calculations. Now we, Joe Crigley and I, set up a system for getting at tissues when people died. We got six autopsies, or maybe it was five over a relatively short period of time. We published that in 1955. Then at the conference down in Washington a few years ago, Ed Wrenⁿ who was arranging ^{it} asked me to review the history of the uranium production line. I thought that would be a nice thing to do. The last thing I had written was in 1955. So I'd be able to update it particularly with the addition of new cases. I found there were no new cases.

STANNARD: Yes, I remember that.

EISENBUD: Yes, the last one was at Mallincrot^d 20 years before. I think, see the difference between the people that are field oriented as a focal point. Now why you folks could not have seen the importance of confirming your judgments based on animal work by getting a human work from two points of view. First, epidemiological since after all it was known that people were spilling albumen. And men had been

removed from the job because of that. Those should have been worked up. They never were. As time goes on it's even more disappointing that there's been no follow up of the people who are known to have had at least transient albuminuria from uranium hexafluoride exposure back in the...

STANNARD: You're right exactly on the nose of one of the puzzles that has been worrying and concerning me. This concerns NCRP ^{as much as} ~~more~~ ^{of the record} than it does this book writing. Back on the record...

EISENBUD: But your exposures during the war and five years after the war were very high.

STANNARD: Sure.

EISENBUD: Men were excreting a milligram per liter. So those are the ones they ought to concentrate on. One of the problems is that it may be that people like Sid Marks, even Dag Norwood who's been sort of isolated, may not know what went on.

STANNARD: Well, I just wonder, ^{of course} certainly I'll urge Sid Marks to get in touch with you and be sure that he gets whatever he can from you because he may be working in a little bit of a vacuum.

EISENBUD: Well, see, he's working like an epidemiologist. What he's doing is ^{is?} ~~What kind of~~ ^{What} would be a follow up success? Here, what they got to do is just pick out a small group of people. And the list exists. I gave it to Mancuso. I gave him a list of everybody that ever worked in that Mallin^dcro^t plant with our estimate of their accumulative exposures, with their social security numbers and all. And those are the people they ought to go after,

not worry about people working at the National ^NLabSor Los Alamos, Hanford, where even though the exposures may have been over the TLV they ~~still~~.

STANNARD: These are the Mallincrot ^d people?

EISENBUD: Mallincrot and Harsha ^{aw} were the two bad ones, yes.

STANNARD: Another question on uranium: ^uWhy was there so little work done on the higher specific activity ^{uranium} isotopes, 232, 233, 235. There was only two or three experiments that I can find. Was it because nobody thought that the uranium-thorium fuel cycle ~~would be~~ ^{would be} being of any importance?

EISENBUD: Well, I think Rochester wasn't set up for it. You couldn't handle the ^{high} specific activity.

STANNARD: No, no. There was, the stuff was around because Miriam Finkle did an experiment with it. And there were a few others.

EISENBUD: But what were needed were inhalation experiments. I don't Miriam did any inhalation experiments. So ~~that~~ what they needed to do was set up a high level, high specific activity inhalation chamber. Well, everybody was busy. I can understand why that one might ~~be~~ ^{get neglected}.

STANNARD: At that time it ^{was thought to} just didn't seem like a very important problem I guess, or else it would act like radium.

EISENBUD: Where did they start the first plutonium inhalation experiments?

STANNARD: Well, I guess they went more or less simultaneously at Rochester and Battelle, or what was then Hanford. The first serious inhalation experiment, ^{with plutonium} big program really was Bill Bair's job out at Hanford. But ~~Rochester~~ ^{redirection inhalation section} when I had the, ~~well, I guess we got~~ the answer to that one. I don't like to ~~push this because it's much~~.

~~too important~~

EISENBUD: No, it is an important point and I'm not sure I can add to it. You would have thought that when they started the plutonium experiments they would also have done enriched uranium.

STANNARD: Yes. And I think Ed ^{Werner} has now shown that uranium acts more like radium than it does like plutonium which would mean in terms of standard setting that the n-factor if we keep it should be one. This makes a difference of a factor of 5 in the levels if we really follow it through. ~~Very good.~~

Another question I had was: Are there any unknown people that made a lot of contributions whose name ^S should be mentioned but they have kind of dropped out of sight? Bernie Wolff is one I think of.

EISENBUD: Bernie was very, well, just the fact that Bernie saw the need for HASL which is a unique organization. Now it's been copied by other ^S, even the national labs are modelled now after HASL. But he saw the need to bring in, put together a group that was field oriented, act as a bridge between the biologists and the people operating the plants.

STANNARD: Now, you would call this internal emitter toxicology that he was interested in?

EISENBUD: Sure, yes. John Harley deserves a lot of credit.

~~STANNARD: Chester?~~

- ~~EISENBUD:~~ John. You might want to talk to Bill Bale about this and get his opinion. You know one of the most widely quoted documents in this field of radon is the famous Bale memorandum.

STANNARD: I know it.

EISENBUD: I don't know how good Bill's memory is nowadays, but my recollection was that the importance of the radon daughters was identified probably by 1948 or 49 at HASL where we were making measurements in the field. We set up a room so that we could have a ~~control~~ ^{controlled} radon exposure, a chamber. I wanted John Harley to come down from RPI to run our analytical lab. He was a good, Ph.D., he was working ^{on} ~~on~~ ^{it} was in analytical chemistry with emphasis on physical measurements, or physical methods I should say. So we worked it out with Herb Clark who was his professor to go ahead and come down and do some quantitative work on the relative role of radon and the daughter products.

STANNARD: Really?

EISENBUD: And his thesis was just on that. The thesis was published in 1952. John of course had been working on it for a year or two before that. Now Bill came down to visit us and saw what we were doing, and went back and wrote a memorandum. He put some of his own input into it, some independent calculations which were very good. There are two things I don't understand, was why Bill didn't straighten the record out since John was already working on this, and two, why John didn't publish his thesis except a short note which he put in Nucleonics I think about 1952. He didn't do justice to himself. Part of that may be because the Bale memorandum was so being widely quoted he thought there was no need to do it.

STANNARD: And yet that memorandum is not in the open literature.

EISENBUD: Yes. So you might want to ask Bill about the history of that

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memo.

STANNARD: When we went to Atlanta unfortunately Bill was in Rochester at the time.

EISENBUD: I don't know his whereabouts now. Is he in Atlanta?

STANNARD: He's in Atlanta. He's at Georgia Tech.

EISENBUD: I see.

STANNARD: I've been wanting to get Bill to help me with instrumentation because he had so much to do with ^{the} development of some of the early instrumentation. But that's another story.

Well, we've got Bernie Wolff, John Harley - that story is brand new to me.

EISENBUD: It came to mind because before Udall came up to see me he sent me a little package of material and in there was the Bale memorandum. It was the first time I'd read it since he wrote it which is, what, 27, 28 years ago.

STANNARD: Yes, during...

EISENBUD: Yes, 1951. Bill Harris did a lot of good work in gathering industrial hygiene type data which ultimately resulted in methods of control.

STANNARD: One thing I hoped we could discuss a little bit was, I would like to ^{insert} ~~insert~~ some things in this book that would if possible save the chemical toxicologist from having to repeat the mistakes we made in the radiation part ^{1.} or at least having to do all the work over again. Otherwise history will just have to repeat itself. Can you think of some things that ought to be said

no matter what that would be directed largely at how what we did in the radiation field can be taken as lesson number one by the chemical people? They're already adopting the linear ^{the -} threshold business for better or worse.

EISENBUD: To me the important lesson both in chemical toxicology and radiotoxicology is while the animal work is important that fundamentally all it does is tell you about mechanisms. And it suggests which important organs to look for damage in and give you some feel for dose response. But in the radiation field we probably knew enough about radiation effects from epidemiological studies before World War II to be able to manage a program safely. In other words we already had the tenth of a microcurie^{mu} of radon. We had a figure of, what was it, a tenth of an R per day which was suggested by Fiella based on the flimsiest of epidemiological methods. And this for six day week would be 30 R per year. Now if you convert from R to rads and make allowance for the fact that Fiella's experience ~~with~~ was with weak x-ray, 50 - 75 kV. So there ~~was~~ ^{is} more penetration for what we use today, that's probably only a factor of three or four from what we're using now.

STANNARD: Yes. Well in fact, Merle, one of the things that has been impressing me in this whole job is how similar what we accept now is to what came out of the early work. We spend millions of dollars and thousands of man years, and we've got a lot more reliable information in this respect. But in terms of there being major changes in point of view, there are not very many as far as I can see.

EISENBUD: ~~No~~. There are a couple. Of course the big thing in the last ten years is the fact that there's more to the cancer story than just leukemia. For every leukemia there may be four or five other types of hard cancers. No, that's really about it. I think we have a little better picture of the dose response curve at lower levels, and we now know something about dose protraction effects which we didn't know. But you don't need to know these things in order to control them.

STANNARD: Right. I suppose the dose rate factor may be a very important thing.

EISENBUD: That's what I mean by dose protraction, dose rate.

STANNARD: Of course, that's ^{always} ~~already~~ been there with internal emitters, ~~built in business~~. Makes you wonder ^{about} ~~if~~ some of the so-called differences.

EISENBUD: There's one thing I think I've pointed out a few times, things that I've said in public, maybe in writing, that is that it was a remarkable historical coincidence that the tenth of a microcurie of ~~radon~~, radium was set when it was. It didn't have to be. If there wasn't any Bob Evans it probably wouldn't have been. Or it might have been ten years later. You know, the difference between 1940 and 1950 isn't all that important. And Bob, bright guy as he is, he certainly didn't know that the war was coming, that there would be a Manhattan District. At least, I don't think he did, 1939 or 40 when he published that recommendation.

STANNARD: I don't think so.

EISENBUD: So, it came out in 1940. Some of the internal, at least the bone-seekers were pretty well taken care of. Fella had proposed a tenth of an R per day within a few years before that. So the stage was set for safe management of ..*radiation*

H. Aufmann
STANNARD: It was really a remarkable coincidence. I was impressed by something *Louis Howland* Willy Empelem says, "~~It was~~ plutonium had been as soluble as radium we would have been in a terrible mess." Just very fortunate that plutonium was *quite* insoluble and *as* poorly absorbed from the gastrointestinal tract as it is.

EISENBUD: Yes, we would have been, *in trouble* particularly if in other words we had *one* two things; *was* one, if we didn't recognize the fact that radium could produce bone cancer. Now that argument really wasn't settled until the early 1930's. Martland told me, but I have never seen it, I've tried to find it after he died, he had a letter from Madame Curie in which she called him a charlatan for saying that small amounts of radium could produce cancer since the whole world knew that radium cured cancer. So you're right. If they hadn't made that discovery, and it took ^a combination of some rather remarkable people, Bloom, Martland. Bloom is one of the unsung heroes by the way. Do you know his name?

STANNARD: Yes indeed. ~~I had planned to...~~

EISENBUD: He was the fellow that picked it up originally. And then Martland was the next one.

STANNARD: I hadn't planned to say too much about Bloom and those folks because they're so well documented in *the* radium papers.

EISENBUD: So, if it wasn't, ^{if} they didn't know that radium could produce cancer. And if it turned out that plutonium was more soluble, then you could tell what would happen because one kilo of radium killed over a hundred people. And we produced plutonium by the ton.

STANNARD: No one has even turned up with demonstrable damage. Very important point.

Well, did you want to get to this one o'clock session?

EISENBUD: If you'd like to talk for a few more minutes I'd be happy to, or maybe we could get together some other time. What else do you want to cover?

STANNARD: Well, the primary other point I had written down to discuss with you is your view of what's behind the change in public attitude. I've done a lot of reading and even ten years ago we were looking at this whole field as a respectable field, as something that was an honor to be associated with. It had some problems. Now all of a sudden in the view of the public it's terrible. It's very frustrating. But that's really not part of my book except I intend to emphasize at that time the people ~~were~~ had a very different mental attitude than they do now. *

EISENBUD: I don't know why it is. There's a lot of things going on in society that I don't understand nowadays. The papers are very imprecise in what they have to say about these things. And their errors of omission are worse than their errors of *commission* - things they don't say. Now when Udali came up, he's handling two

things. He's handling law suits against the government because people have developed cancer from being exposed to fallout. He's also trying to get, representing the uranium miners, he's trying to get an act from Congress to give the uranium miners a special compensation as they did in the case of black lung disease.

*End of tape - Interview completed because
Merrill had to leave.*