



Oral Histories

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HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

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(a) *Conducted December 20, 1994*

(b) United States Department of Energy
Office of Human Radiation Experiments
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(d) FOREWORD

In December 1993, U.S. Secretary of Energy Hazel R. O'Leary announced her Openness Initiative. As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects. The Office of Human Radiation Experiments coordinated the Department's search for records about these experiments. An enormous volume of historical records has been located. Many of these records were disorganized; often poorly cataloged, if at all; and scattered across the country in holding areas, archives, and records centers.

The Department has produced a roadmap to the large universe of pertinent information: *Human Radiation Experiments: The Department of Energy Roadmap to the Story and the Records* (DOE/EH-0445, February 1995). The collected documents are also accessible through the Internet World Wide Web under <http://www.hss.energy.gov/healthsafety/ohre/>. The passage of time, the state of existing records, and the fact that some decisionmaking processes were never documented in written form, caused the Department to consider other means to supplement the documentary record.

In September 1994, the Office of Human Radiation Experiments, in collaboration with Lawrence Berkeley Laboratory, began an oral history project to fulfill this goal. The project involved interviewing researchers and others with firsthand knowledge of either the human radiation experimentation that occurred during the Cold War or the institutional context in which such experimentation took place. The purpose of this project was to enrich the documentary record, provide missing information, and allow the researchers an opportunity to provide their perspective.

Thirty audiotaped interviews were conducted from September 1994 through January 1995. Interviewees were permitted to review the transcripts of their oral histories. Their comments were incorporated into the final version of the transcript if those comments supplemented, clarified, or corrected the contents of the interviews.

The Department of Energy is grateful to the scientists and researchers who agreed to participate in this project, many of whom were pioneers in the development of nuclear medicine.

(e) DISCLAIMER

The opinions expressed by the interviewee are his own and do not necessarily reflect those of the U.S. Department of Energy. The Department neither endorses nor disagrees with such views. Moreover, the Department of Energy makes no representations as to the accuracy or completeness of the information provided by the interviewee.

1.3 ORAL HISTORY OF DR. JOHN W. GOFMAN, M.D., Ph.D.

Conducted on December 20, 1994 in San Francisco, California, by Loretta Hefner, archivist for the Lawrence Berkeley Laboratory, and Karoline Gourley, a researcher for the Office of Human Radiation Experiments, U.S. Department of Energy (DOE). John W. Gofman was selected for the oral history project because of his research at the University of California, Berkeley, and his biomedical work at the Lawrence Livermore Radiation Laboratory (LLRL). The oral history covers Dr. Gofman's codiscovery of uranium-233, his involvement with isolating the first milligram of plutonium, his work as founder and director of the biomedical program at Lawrence Livermore, and the evolution of his opinions on the effects of radiation on humans.

(a) Short Biography

Dr. Gofman was born in Cleveland, Ohio, on September 21, 1918. He received his B.A. in chemistry from Oberlin College (Oberlin, Ohio) in 1939. He received his Ph.D. in Nuclear/Physical Chemistry from the University of California, Berkeley. He received his M.D. from the School of Medicine, University of California at San Francisco in 1946. He married in 1940 and has one grown child.

Dr. Gofman began his career by working for the Plutonium Project as part of the Manhattan Project at the University of California, Berkeley from 1941 to 1943. During that time, he developed two processes for separating plutonium from the uranium and fission products of irradiated fuel. This work, conducted with Dr. Glenn Seaborg, was the precursor to full-scale plutonium production at the Hanford Nuclear Site in Washington. Between 1947 and 1951, Gofman was a physician in radioisotope therapy at the Donner Clinic, University of California, Berkeley. From 1947 to 1954 Gofman was an Assistant Professor of Medical Physics in the Division of Medical Physics, Department of Physics at the University of California, Berkeley. In 1954 this position turned into a full professorship, and in December 1973 it became a Professorship Emeritus, a position he continues to hold today. He was the medical director for the Lawrence Radiation Laboratory (Livermore) from 1954 to 1957. From 1963 to 1969 he was an Associate Director of the Lawrence Livermore Laboratory and from 1963 to 1966 he was Director/Founder of the Lawrence Radiation Laboratory Division of Biology and Medicine.

Dr. Gofman has published many times on such topics as the following:

- lipoproteins, atherosclerosis, and coronary heart disease,

- ultracentrifugal discovery and analysis of serum lipoproteins,
- the relationship of human chromosomes to cancer,
- the biological and medical effects of ionizing radiation, with particular reference to cancer, leukemia, and genetic diseases, and
- the lung-cancer hazard of plutonium.

(b) Oberlin College; Enrollment in Western Reserve Medical School

HEFNER: Today is December 20, 1994, and Karoline Gourley and Lori Hefner are here with Dr. John Gofman for the purpose of creating Dr. John Gofman's oral history.

GOURLEY: How is [it] that you first became attracted to science as a profession?

GOFMAN: I really was a child of the Depression; I would say my first reaction out of high school was, "It would be nice to be able to get a job, any job." And there *were* no jobs. I spent the summer after graduating from high school trying to get a job, any job, and couldn't.

GOURLEY: What year was that?

GOFMAN: 1935, and I didn't have the ambition to be a scientist or anything else. I just tried to stay alive. I then decided maybe since I'd done well in school in mathematics and science, I'd try to [get] into engineering school. Because as far as my dim vision of science was concerned, engineering and science were the same thing.

Someone from Rensselaer Polytechnic Institute [(New York)] had visited my high school and said that they hadn't had any students from Cleveland, Ohio (where I lived) before and that if the school nominated someone for a scholarship that there was a very good chance of getting it. The math teacher talked to me about it and I thought that was a great opportunity. So I applied for a Rensselaer scholarship and thought no more about it, figuring I was going to get it. [When] anyone asked where I was going to go to school, if I were going to college, [I would answer,] "I'm going to Rensselaer." Except when April came around, I got a letter saying: "They are very happy you've been accepted for class, but we've run out of scholarships above the level where [your] name came in." They could have sent me anything, actually, if the tuition were \$50: I didn't have \$50 to pay for tuition. So that ended my Rensselaer appointment.

Then there was an engineering [school] in Cleveland, Ohio, called Case School of Applied Science.¹ I took the exam for a scholarship at Case and tied for fifth, which was good enough to get me a half scholarship, which I also couldn't afford. I was about to give up, or either go to Ohio State or just not go to college.

Then, somebody suggested that there was a school near Cleveland called Oberlin College, and I said, "That's a music school and I'm not a musician." [He responded,] "No, there's a college there as well as a conservatory." So I went down and talked to the Dean of Admission,

and he said, "You're a little late applying for a scholarship." See, it was already summer, but they were very honest and gave me a first-semester scholarship with arrangements called student aid, [by] which you were eligible for half tuition, if you did well.

So with the scholarship for the first semester and the possibility of student aid, I went there with a combination of board jobs,² stoking furnaces, and cleaning up snow. I managed to get through the first year, and then things got easier and easier. Because I got regular jobs for my board. The Democratic Administration had a thing called the National Youth Administration [(NYA)] and in college you could get a job and work up to 30 hours a month at 50 cents an hour, so that [provided me] \$15 a month, which was a very, very good thing. Roosevelt had put that into that situation. I got one of those NYA jobs, starting my sophomore year.

I had my chemistry the first year. A professor of chemistry named Luke Steiner, a very fine man, a very good chemist, had given me an opportunity to work with him in his lab. Beginning sophomore year, I worked in his lab on his research project, which was the study of the absorption of various vapors on porous gels. I worked with him for 3 years (sophomore, junior, and senior year) and I then decided I ought to be a chemist. But for some strange reason, I can't really explain my senior year in college. I thought about the idea of medicine, especially the idea of doing chemical research in medicine. Dr. Steiner thought that was a good idea. At Christmas vacation, there are a certain number of things that are very irrational in my history. I might as well just tell you.

GOURLEY: Oh, go right ahead.

GOFMAN: I decided I ought to go to medical school. I went over to Western Reserve Medical School in Cleveland and I went to the front office, the dean's office. There was a very nice lady, tall lady, Juliette T. Brown, and I introduced myself and said I would [like to] apply for admission in medical school. And she said that the medical school admissions were closed sometime ago.

GOURLEY: Late again.

GOFMAN: I'm late again. She said, "Why are you applying now?" I said, "Because I just made up my mind this week to apply." Ordinarily, people should throw someone out of their office when they come in and say these things. It just didn't occur to me that it was that brash. She said, "Where do you go to college?" I said, "Oberlin." She said, "what are you majoring in?" I said, "Chemistry." She said, "Aren't you a pre-med?", and I said, "No." I would still have to make up some things to be a pre-med, because I knew I hadn't had courses like Cat Anatomy. It turned out-talk about the quirks of fate-the dean of Western Reserve Medical School, Torald Sollman, was a pharmacologist, and chemistry was his love. She [(Ms. Brown)] said, "How are you doing in chemistry?" I said, "quite well at Oberlin"; I had my record with me. She went off, saw the dean, came back, and said, "Dean Sollman will see you."

I went in to see Dean Sollman, who had a stack of journals about this

high. (*holds his hand, palm down, about two feet off the floor*) He would peer over the stack of journals and he appeared to be about 70 yearsold at the time. He said, "I understand you would like to apply for medical school and that you're a chemistry major." I said "Yes." He said, "Have you taken the Medical Aptitude Test?" I said, "What is that?" So he told me, "Here, I'll give you the test right now," and he did. I handed it in and he promised to let me know. Remember, I didn't have all the requirements to get in.

So about four weeks later they sent me a letter saying I was admitted and that I would have to make up the missing courses, which were Embryology and Cat Anatomy. I couldn't get both in before the fall semester, but I agreed I would dissect a cat in the summertime at Oberlin, and I did. But I did get into the Embryology³ course.

I enrolled in Western Reserve Medical School in the fall. I got along fine in medical school that first year, but I could see I was not going to learn much about chemistry in the Biochemistry Division of the medical school. Western Reserve had a new professor of anatomy. The old professor of anatomy had just died a year before, [and he] was from a Scottish school that always felt they had to terrorize medical students. But the new professor was a great guy, Normand Hoerr. I got to know him.

He had done a lot of research and had a Ph.D. as well as an M.D. In those days, it was pretty exciting doing histochemistry⁴ on tissues. I talked with him once, and said, "You know, I think I'll go back and study some more chemistry before completing my medical education." And he said, "That's a good idea." I went to see the dean who had admitted me and gave him my reasons. He said, "it's a silly idea." He said, "All the chemistry you need is here." But I wasn't convinced of that.

(c) To University of California, Berkeley to Study Physical Chemistry

GOFMAN: So, I went down and talked to Professor Steiner, [whom] I had worked [for] in a job at Oberlin for three [years]. He said, "There is only one place in the world for the kind of chemistry you'd like to study (physical chemistry), and that's Berkeley. There is no other place."

It turned out, just in a quirk, that in the two years before, Oberlin had sent two [graduating] men in a row to Berkeley for chemistry. The college had never sent any before, and none before them had ever applied [for admittance]. Both of them [were] doing well at Berkeley. He [said,] "Maybe with that record you [can] get a teaching assistantship⁵ there." I applied, and I did get a teaching assistantship.

By August 1940, I came [to Berkeley] to be a graduate student in Chemistry. Norman Hoerr had assured me, "I don't care what anyone tells you, if you want to come back [to] Med School after you finish Chemistry, I'll guarantee that you'll be coming back here to Western Reserve." What I had was essentially a leave of absence based on this one man's assurance that I [could] get back in. The dean was not too sympathetic, as I said.

I came out to Berkeley. The dean of the College of Chemistry at Berkeley at that time was Gilbert Newton Lewis, one of the all-time greats in chemistry. [There were] many, many famous things that he did. In fact, he was the father of chemical thermodynamics.⁶ The book that he wrote with Merle Randall, [commonly] called "Lewis and Randall," was the bible of thermodynamics worldwide for several decades. Kenneth Pitzer, who later went to head Physics at AEC⁷ revised the book.

At any rate, I introduced myself to Mabel Kittredge, who was the secretary with the department, and she said, "You can get to see the dean." She gave me an appointment and I went in to see Gilbert Newton Lewis. [He said,] "Some of the graduate students should take a course or two but they don't bother much with courses; get your research started within the next few weeks."

(d) Assisting Seaborg's Research, Discovery of Uranium-233

GOFMAN: I was terrified-"Get your research started." I didn't think I knew anything to get started in research. I figured you'd take courses for at least a year or so. The system at Berkeley-I don't think it's different now-was [that] you went around as a graduate student to see professors to see if they had something that they wanted a new student to work on with them. I finally narrowed it down to seeing William Francis Giauque. Low-temperature thermodynamics-[it] looked like interesting work. The other [choice] was this young guy who was an assistant professor, I think, at the time: Glenn Seaborg.

GOURLEY: Oh, *there's* a name.

GOFMAN: So, I chose to work with Glenn Seaborg. I did get started on my research within a couple of weeks.

GOURLEY: What specific research were you working on with Glenn Seaborg?

GOFMAN: The specific research was the one hole in a series of radioactive nuclides. That was called the " $4n+1$ " if you divide the atomic mass number by four. They had [radionuclide] members with [zero] things added and things with two, three and four added, but no " $4n+1$." Seaborg said, "Maybe we can find out why this is missing." That was [the] year after fission was discovered. Before the discovery of fission, somebody had thought they had seen protactinium-233, which was in that $[4n+1]$ series. When fission was discovered, they no longer knew whether they had a protactinium or didn't, because there was a zirconium nuclide that would have the same chemical properties as protactinium. They weren't sure anymore whether protactinium existed in this one [series] they had thought they made before the discovery of fission.

The first start of the work was to bombard thorium with neutrons that made thorium-233 from thorium-232. It was very short-lived for [an] isotope: 23 [minutes' half-life,⁸ decaying by beta emission to protactinium]. This radioactivity had a 27-day half-life [and] the

properties, either of zirconium or protactinium. Very little was known about the chemistry of element 91 (which [is] protactinium) at that time. Except it was known that it did have some chemical properties similar to zirconium.

I remembered I had gotten as far as Christmas Day in 1940 where I was able to crystallize zirconium oxychloride in a concentrated hydrochloric acid, show[ing] that the radioactivity did not go with the zirconium [but] was left over after I crystallized away the zirconium. Therefore it was protactinium-233. We fully published [this finding] in *Physical Review* with Seaborg, [me,] and Joe Kennedy. [Do] you know Joe Kennedy?

GOURLEY: No.

GOFMAN: Joe Kennedy was one of the most brilliant chemists I've ever met. He [was] working with Seaborg. He was the guy who did all the equipment manufacture for our group. There were no scaling circuits, there were no counters, no nothing. Joe built them, and in fact some commercial companies grew out of some of the things he developed. He was a chemist with golden hands and very brilliant. Ernest Lawrence⁹ knew it, and so when things went a little further Joe split away from our group because Ernest needed him to work on the 234U and 235U separation of the electromagnetic method for the war, the bomb.

GOURLEY: Right.

GOFMAN: Then a little later, Joe was tapped by J. Robert Oppenheimer¹⁰ to be the chief chemist at the Los Alamos Lab. But in the early days, Joe helped get us started. The next step, since protactinium decayed by a beta emission, [was that] there had to be uranium-233 because that's what you get [from] the protactinium [decay] one unit higher on the periodic table. The idea was to look for uranium-233. By then, we knew about fission. There was talk about a possible bomb.

So the question was, "what kind of properties would 233U have?" We didn't know whether it would have a half-life of 5 days or 100,000 years. I started to look for alpha particles¹¹ growing out of the protactinium samples. It was just marginal that there was some alpha [emission] growing out with a very long half-life. It was so marginal we couldn't be sure. We knew we needed a much bigger bombardment of thorium to try to make more.

Summertime came, school was over in May (at that time it started in August; I think it's back to that system now). So, we had no support, no monetary support, it was just little support.

GOURLEY: This was at Berkeley with Seaborg?

GOFMAN: Yes, and I said I sure would like to stay for the summer. [However,] I got married before I came out to California, and with the teaching assistantship and 65 [dollars] a month, it was possible to live, but there was nothing [(no income)] for the summer. Seaborg tried to get me a \$150 [stipend] for the summer, which wasn't available. I did

have 6 weeks that were taken care of because I was a lab assistant in Physical Chemistry. But then the last 6 weeks of summer, there was no support at all, so we went back to Ohio to live with our families. Seaborg has written that up in some memoirs of his own; I can't remember which of the books. He couldn't get \$150 for somebody [who] worked on a program which eventually got labeled a fifty-quadrillion-dollar discovery.

I came back in the fall and all kinds of things were different. By then the Office of Scientific Research and Development was getting more serious and we had money.

GOURLEY: Where did the money come from?

GOFMAN: From the Government and the Office of Scientific Research and Development. That was before the Manhattan Project.

GOURLEY: Okay.

GOFMAN: Seaborg even got some money to hire Ray Stoughton, who had just gotten his Ph.D. a couple years before at Berkeley, to help me with the work. To make a very long story short, work went fine. Because Seaborg was convinced the work was important, they even managed to get me absolved of the teaching duties of being a graduate teaching assistant in the freshman Chemistry class, which had to be two afternoons a week. Then I had a very good break: I only had to read papers for Professor Giauque, who was the other person I wanted to work with. I had taken his course the year before. I really got to understand it the next year when I was the teaching assistant and I had to read the problem sets on the exams. He was a great man.

The work went fine; we did get a big bombardment of thorium, let the protactinium decay, and finally concentrated the material down. We put it all on a plate and watched the alpha particles grow out and showed we had uranium-233 with a half-life of about 150,000 years.

GOURLEY: Now how dangerous is that?

GOFMAN: Uranium-233 in the amount we had? We had four-millionths of a gram. Not very dangerous.

GOURLEY: Okay.

(e) The Manhattan Project

GOFMAN: We proved we had four-millionths of a gram, and by then things had moved along. The Manhattan Project had gotten started. Things even became easier at Berkeley, [with] the Manhattan Project¹² backing and the Army. If you needed something, they could even put a triple-A priority on it and get it off the train going somewhere else. So, the work was enormously facilitated when the Manhattan Project people came [to Berkeley].

GOURLEY: Who came in from the Manhattan Project?

GOFMAN: Harold Fidler, who was later with the Berkeley Rad [(Radiation)] Lab. He was, I think, a Major or Colonel and he was assigned to the Berkeley Project. I got to know Harold, then. I got a lot of help from them in a variety of ways.

One thing, for example: We wanted to know whether uranium-233, which we'd just discovered, would be fissionable. Would it or would it not be like plutonium or like ^{235}U ? We had a small neutron source made with a mixture of polonium and beryllium. It was weak, it was just not enough so we decided we had to have about a gram of radium. That's a curie. (That's dangerous to handle, by the way.) We bought the gram of radium for \$10,000 and mixed [it] with the beryllium that came in a lead block to Berkeley.

By then, Seaborg had gone off to Chicago. Because the vision of the effort under the Manhattan Project was Harold Urey at Columbia. [Urey] was going to try to work out the gaseous diffusion method of separating ^{235}U . Arthur Compton of Chicago [was trying] to figure out whether a reactor would run; that was the Fermi Project. Also, if a reactor did run, could you make enough plutonium? The third thing was Ernest Lawrence's electromagnetic separation at Berkeley. Although I got to know Ernest Lawrence very well later, I did not participate in the Ernest Lawrence Project. I was working with Seaborg.

When I finished the work on ^{233}U , I became the fourth chemist in the world to work with plutonium. Really, they say Seaborg and McMillan were the first two. The guy who really did the only chemistry that was worth talking about before I got in was Arthur Wahl. He was a graduate student one year ahead of me. He knew everything in the world there was to be known about plutonium, and he taught me. And I got started at the same time.

At the same time, I was getting ready to measure whether uranium-233 was fissionable. The radium and beryllium source, which is a strong neutron source, arrived. I had to be able to move that radium source up to a fission chamber and also test it with paraffin surrounding the fission chamber [and] without the paraffin slowing neutrons down, versus the high-speed neutrons being made in the reaction between the radium alpha particles and beryllium.

I was having the shop make me a lead train to move my source up to the fission chamber because it was too dangerous to handle by hand. The shop had a lot of priority jobs and they couldn't do it all at once.

Seaborg came back from Chicago, having gone there to head the plutonium section [of the Metallurgical Laboratory, Manhattan Project] in Chicago. I stayed behind in Berkeley. And he said, "How are the fission measurements going?" I said, "I haven't done them yet." And he said, "You haven't done them yet, with a war going on, you haven't done the fission measurements!" I said, "Glenn, I haven't done them for a very distinct reason: The lead [train] isn't finished." He said, "Don't worry about that, let me show you how to handle it."

He went over to the Old Chemistry Building, that was torn [down] later. We went over [to] where I had the lead block with my gram of radium. He got a stick and tied a string to it: "Just hold it out there and put it in front of the fission chamber and then put it back."

He was there for 5 minutes, but I was going to do that every day to take measurements. That's probably where I got a major share of what dose of radiation [I received] from that operation. A gram of radium is a roentgen¹³ per hour at one meter; when you handle things at a small fraction of the meter, dose goes up as [the inverse] square [of the distance]. So, I got a good [(appreciable)] dose.

But we succeeded, and we proved that uranium-233 was fissionable [with slow and] fast neutrons. Therefore, it was one of three [materials] in the world that you think of making bombs out of, although you can only get it by having thorium, or a mixture of thorium and uranium, irradiated in a reactor. A lot of it has been made since, and bombs have been made out of it, too.

Since I was all set up for the fission measurements, I [measured] uranium-235 and plutonium, too. An interesting thing happened when I made those measurements. Professor Oppenheimer wanted to see the measurements.

GOURLEY: Now, where was Professor Oppenheimer?

GOFMAN: Berkeley.

GOURLEY: He was also there?

GOFMAN: Yes, before Los Alamos. Professor Oppenheimer was looking at the measurements for calculations. [I looked at Oppenheimer's equation, and] I said, "Isn't there a factor of ten to the sixth¹⁴ that is wrong here?" He looked at it, and said, "Yeah, it doesn't matter." He was a remarkable guy.

So, what happened was that Seaborg had gone off to Chicago and I completed the uranium-233 work. There was one episode before Seaborg left that was very interesting.

Graduate students were rather playful. Seaborg's lab, where I worked, was on the third floor of Gilman Hall on the Berkeley Campus. As I told you, Dean Lewis had been the Father of Thermodynamics. Thermodynamics always use big water baths to control temperature of their vessels and operated at a certain fixed temperature. But that era was all over in Berkeley. There [were] big bathtubs out in the halls. In fact, one on top of the other, on the third floor of Gilman Hall.

I don't know what got into us, but Wahl, Spofford English, Bob Duffield, and I were working one Friday night before Seaborg had left for Chicago. We decided we'd stack those bathtubs in Seaborg's office, which is where we [were] working on our counters and things. I don't know [why], it never occurred to us.

We were there the next morning, Saturday morning, working: Art Wahl, Spofford English, and I. The door opened and there is Seaborg with a visitor. And who was the visitor? It was Harold Urey, who headed the New York operation of gaseous diffusion [research].

Seaborg, without cracking a smile or anything, stepped into the bathtubs because he couldn't do anything other than that; there was no room left. Urey stood in another one. Now, these are some famous people, and there was a famous mental exchange.

Urey said, "Glenn, I think we ought to give up this plutonium project." And Seaborg said, "Why do you say that?" Here we are just completely crushed. What a mess we made, and Seaborg, and Harold Urey standing in bathtub[s] in room 203 [of] Gilman Hall. Urey said, "Look, I don't know how long the war will last, but I don't see any possibility that you can learn how to isolate plutonium from that mess we have of fission products and uranium in time for the war effort." So he said, "I think we ought to give it up and just focus on uranium-235," [for] which Ernest Lawrence had one project and he had the other. Fermi's reactor had already run, so it was assured you could make the plutonium.

Seaborg was uncanny in one feature. He had an uncanny knack for being able to see ahead, what would be important and what might not be important. He said to Urey, "Oh that's no problem, Professor Urey, we worked all the techniques out for separating plutonium once it's made."

There we were sitting-remember, Wahl was the only guy in the world who'd worked with plutonium and I was the second one, besides Seaborg and McMillan. We knew damn well what we didn't know. Here he's telling Urey, we have all the techniques worked out! It's the furthest thing from the truth, but I guess he figured we'll work it out.

Then they left. We cleared the room up, and he never said a word about those bathtubs being put in his room. One of the most famous conversations in the whole war in that room with Harold Urey suggesting we stop the plutonium project. Well, we didn't stop the plutonium project, as you know.

GOURLEY: Right.

(f) From Research to Laboratory Production of Plutonium

GOFMAN: Art Wahl taught me everything he'd learned about plutonium and I went on working on plutonium chemistry. By the way, just as an aside: Everything was compartmentalized. The Security Division of the Manhattan Project came to see me and said, "You're not working on uranium-233 anymore, you're working on plutonium." I said, "That's true." They said, "Then you don't have a need to know what's in your own notebooks." I had to give up my notebooks that I'd done on the uranium-233 work. I got them back after the war.

I did work on chemistry of plutonium. The whole thrust was to learn enough about the chemistry of plutonium toward being able to

separate it when Hanford[']s separation facility] would be built. So we worked on a test-tube level to try to do separations.

We thought that plutonium in the higher oxidation state would behave like uranium. There was a compound we knew about [called] sodium uranyl acetate. That's uranium in the "plus 6" oxidation state. The plutonium might behave that way. I tested that and it did behave that way.

If you precipitated sodium uranyl acetate, even if you had just a limited number of atoms of plutonium, the plutonium went with sodium uranyl acetate. Based on that one thing, I worked out a process that would isolate plutonium away from uranium in one step and then get it to go [back] with uranium. I could cycle it back and forth to get rid of the fission products, by having two different oxidation states of plutonium.

On a lab-bench basis with little beakers, it all worked fine and the plutonium came through the process. I wrote it up, and [as a result of my work] there was one possible way for [a separation facility at] Hanford.

We [had] an occasion to use it. Oppenheimer decided with [Manhattan Engineer District Commander General Leslie R.] Groves and [the] military to step up the Los Alamos Lab. He invited all of us at Berkeley to go with him. Joe Kennedy, as I said, became his head chemist. We knew Joe.

I elected to stay in Berkeley [because] they were then so unsure [about] security that anybody that [went] to Los Alamos, [went] with [the] understanding that it's for the rest of the war. You [would] have no communication with the outside. Oppenheimer said that to me; [you] could not even telephone or write. They had to back off of that [idea] quite a lot. They were very worried about security. I decided to stay in Berkeley.

Oppenheimer went down to Los Alamos, and about 2 months [later] (this was late 1942 or early 1943) he contacted me with a note that said he wanted to see me. By then, the faculty member who was responsible for our group, (Bob Connick and I were the group leaders), was Wendell Latimer, who was a superb chemist, an excellent chemist [with] just the right kind [of experience] to work with the inorganic chemistry of elements like plutonium. Oppenheimer said he wanted to see Professor Latimer and me. He and Joe Kennedy came [to Berkeley] and we met in Professor Latimer's office.

Oppenheimer said, "we need a half a milligram of plutonium." I said, "You're going to have grams of it in a half-year to a year from Oak Ridge." He said, "Yes, I know. We're going to have grams of it, but right now we need a half a milligram and there's only a twentieth of a milligram in existence." I said, "Why are you telling us that?" [He responded,] "Because Joe says you make it." I said, "He does? What do you mean? We'd have to bombard uranium on the Berkeley cyclotron to make it, depending [on] reactor [availability]." Professor Oppenheimer said, "Yes, I know. It would take a lot, maybe a ton of

uranium. My chemists have told me."

GOURLEY: So, a ton of uranium to make-

GOFMAN: Uranyl nitrate.

GOURLEY: Oh, uranyl nitrate.

GOFMAN: Uranyl nitrate to try to make a half-milligram of plutonium. I said, "Well, we have [to] bombard it for 6 or 7 weeks." He said, "Yes, I know that. I've already cleared that with Ernest Lawrence." Then there's the other part of it[, I told him]: "I haven't [the] vaguest idea whether this process that I worked out in test tubes and beakers, scaling right up to pounds at a time, will work. All things don't work well as you try to scale up from the lab bench to the manufacture operations." He said, "Well, Joe thinks you can do it." Joe is sitting there. [I'm thinking,] "Thanks a lot!"

Well, we got the ton [of] uranium nitrate stacked around [the] Berkeley cyclotron to capture every neutron that was escaping. Bombarded it for about 6 or 7 weeks. Let it cool a little; I should have let it cool [for] months. We didn't.

Then in room 110, Gilman Hall, we set up big jars and handled 10 pounds of the uranium at a time. With each jar, we took it the first step of our process and [then] the second step.

After about three weeks of around-the-clock work, we had it down to about a quarter-teaspoon of liquid [with] plutonium [in it] and nothing else. We had 1.2 milligrams, and we just needed a half[-milligram].

Joe Kennedy and Oppenheimer came back up. The first thing Oppenheimer said is, "How much did you get?" I said, "You needed a half a milligram." Oppenheimer insisted, "Come on John, I want to know how much did you get?" [I answered,] "You got a milligram and two-tenths." He said, "We'll take a milligram and you can keep the two-tenths to play with it for chemistry." And that's what we did.

At that time, Glen Sheline had joined our group. We wanted someone to work on the microchemistry, on [the] grounds that we might soon have [a] little bit of plutonium to work with. If you work in little capillaries, things like that, you can do chemistry at the very small level.

Within a couple [of] days, after Oppenheimer had taken his milligram with him, which was [a] 20-fold increase in the world supply; by the way, Glen got the remaining two-tenths. He precipitated it. We went through the basis of the whole process, and we could see sodium plutonyl acetate. So we got to see plutonium for the first time. The whole process we had gone through, we had never seen it [(the plutonium)]; we were just tracing it by its radioactivity.

During that whole period, everything moved towards scaling up for Hanford. I used to go back to Chicago every 4 to 6 weeks, where we were transferring information to the du Pont¹⁵ engineers who were going [to] operate Hanford, trying to get them to understand what we

had learned of the chemistry.

By then, I had quite a bit of radiation [exposure] and didn't know too much about it. We were very careless, by the way, in the [way] we handled things. None of us knew a damned thing about it. Glenn Seaborg, who poo-pooed the whole thing-he still does-he's obviously wrong as hell.

GOURLEY: But back then, pretty much nobody knew?

GOFMAN: There was a lot known that I didn't know. I hadn't gone back and looked over everything of that era from the day Roentgen discovered the x ray to 1942. That was a whole era of medicine and radiology that I hadn't looked at. I looked at it hard this last year.

GOURLEY: Okay.

GOFMAN: I talked to Latimer [of University of California at Berkeley (UCB)] and said, "What would you think? I don't want to go to Hanford." I had had a lot of radiation with the work on the radium and a lot of radiation from the plutonium isolation. That was a dirty job, converting that whole ton of uranium down to plutonium. I said, "I think I might like to go back to medical school." The war was still on, 1944, and he said, "If you stay in the project, I'm sure we can arrange an academic appointment for you under the Department of Chemistry." [However, I was interested in] the medical school, and I said, "I'd like to apply for admission to the second-year class." I told them I had the first year at Western Reserve and they admitted me to [the] second-year class. I finished up my medical work at UC [(University of California)] San Francisco [(UCSF)].

By then, I had a lot of capital built up in Berkeley as a result of having been one of the workers in the early days of [the] Rad Lab. I didn't know John Lawrence and I knew Ernest only a little, but I did have capital earned. [I] applied for an academic position. I visited the Mayo Clinic [(Rochester, Minnesota)] [and] talked about a position there. Joe Kennedy and all of the chemists from Los Alamos had gone to Washington University in St. Louis. I called Joe up to ask how they were doing. He said, "Why don't you think of coming here to Washington University?" Arthur Compton had [left] Chicago to become the chancellor at Washington University. I visited there [and] they offered me a very nice position in Radiology, which I thought of doing. But finally the Berkeley assistant professorship came through in John Lawrence's division.

John was Ernest's brother and had come [to Berkeley] to work, when Ernest said maybe there's something in artificial radioactivity that might be of interest [to] medicine. He had come out in, 1937 or 1938, and worked (they created a division for him), essentially in the physics department, which was called the Division of Medical Physics. It was John Lawrence, Joe Hamilton, Hardin Jones, and Cornelius Tobias.¹⁶ They had worked together some during the war. Joe Hamilton had worked on various radioactivities and metabolism of fission products, [including] plutonium.

I joined that department and became an assistant professor. I didn't

have anything to do with radiation except I worked one day a week at John Lawrence's clinic treating people with radioactive phosphorus (leukemia, polycythemia). By the way, I had an [additional] appointment within [the] medical school as a lecturer. But then I got to do less and less at the medical school because, aside from [the] work one day [a week] at John's clinic and the teaching of handling radioactivity in the lab, I had started to work on heart disease. I had some ideas on how you might study cholesterol.

HEFNER: We have so many questions about this area, I don't want to take you off course.

GOFMAN: Please do, just tell me.

HEFNER: I want to talk you about your colleagues, certainly Joe Hamilton, Dr. Tobias, [and] Dr. Jones. There seems to [have] be[en] quite a bit of contention between the Department of Medical Physics at UCB and UCSF-

GOFMAN: Yes.

HEFNER: Why don't I just leave it at those two [topics] and then we'll go into the heart disease. I've got a few questions about that, too.

GOFMAN: I did start working on heart disease. We were able to figure out why [the previous work ended with] bizarre results that happened in the ultracentrifuge, an instrument for studying proteins and lipoproteins.¹⁷ We solved that in 1948 and published [our findings; we] opened the way for [our] discover[y of] the whole sequence of low-density lipoproteins. We worked on coronary disease.

I got the Stouffer Prize in 1972 for the work on heart disease. Last year, I was honored by being [a] guest speaker at the American Heart Association. It's been a long time since I work[ed on] that. But I gave a talk. It took me about 6 weeks to prepare it.

At any rate, about Berkeley: My joint appointment with the medical school was in the Department of Medicine, not the Radiology Department. But I knew-not in great detail-there was bad blood between the Department of Radiology and John Lawrence.

Joe Hamilton was working in Crocker Lab-at that time it was [the] building where the 60-inch cyclotron was-and Joe was working in collaboration with the people in the Department of Radiology. I think something happened very early that made Dr. Stone and the others in radiology very jealous of John Lawrence.

To the extent that I understood [it] at all, it just seemed as though they felt since they were the radiologists of the Bay Area University of California, it all should be in their department. Here was this guy, John Lawrence, off by himself and independent of them. And they really didn't like [that], but Joe Hamilton and John Lawrence were never close to each other.

I had known Joe Hamilton in [the following] way, when I was a

student of Glenn Seaborg: I was a graduate student, and Joe Hamilton had scheduled bombardments on the 60-inch cyclotron. So you needed to get something done, like I needed a bombardment, like 25 pounds of thorium nitrate to do the 233U work, you went to Joe Hamilton and got it scheduled. Now I did some other work on uranium-232-still another nuclide-and Joe had to arrange those bombardments. So, I got to know him only through his being the chief honcho at the Crocker cyclotron.

Joe was a very, very careless guy and you figured if anybody was going to be hurt by radiation, its going to be this guy, because he just didn't seem [to care].

(g) Joe Hamilton's Cavalier Approach to Radiation

HEFNER: Describe a little bit why you say that. What behaviors made you think that?

GOFMAN: If you need a target made or a target handled, he'd bring it back. He shouldn't have been carrying the thing. Just a general handling of things. This guy didn't seem to respect radiation.

HEFNER: Do you know why, do you have any sense why?

GOFMAN: I have a letter from a lady. Did Greg Herken¹⁸ ever show you that letter?

HEFNER: No.

GOFMAN: Do you see him much?

GOURLEY: I see him now and then.

GOFMAN: There's the *Cal Monthly*; you know the magazine. Well, Russell Schoch had called me up and said, "I've done a question-and-answer interview with all the heretics on the Berkeley campus. But I've never done you." So he did, and this lady read the *Cal Monthly* interview and she said, "I was a classmate of Joe Hamilton. This guy was just wild; he would do the craziest things all the time, and we'd just wonder when he was going to blow up the place." I can locate that letter for you sometime, if you would like to have it.

HEFNER: Okay, thank you very much.

GOFMAN: I've forgotten her name, but I have it in an envelope with all my papers concerning that "60 Minutes Australia." She described Joe as an undergraduate who had crazy ideas. Who would do things like inhale some gases that would change his voice?¹⁹ She thought he was strange.

HEFNER: What strikes me is how parallel your academic credentials are to Dr. Hamilton-an M.D. and the love of chemistry.

GOFMAN: I always got along fine with Joe. I think he's gotten a very, very bad

rap in this whole human experimentation [uproar]. All my relations with Joe Hamilton were always cordial. I didn't know what he was doing with the radiology group. And even though he was in Crocker Lab and I'd come to the Donner Lab [at Berkeley], we'd see each other occasionally. I'd known him before in the war years. Always cordial. I think people made him out to be a monster ever since some of this recent [public attention].

HEFNER: So he's not the mad scientist he's portrayed to be?

GOFMAN: Oh, no, no. Joe is just a very simple person. He may have been isolated, but Dorothy Axelrod, who used to date Bob Duffield (who was one of my close friends in chemistry), worked with Joe and she loved to work with him. She didn't just think he was a fun guy.

GOURLEY: [Is] Joe Hamilton the one [who] you hear stories [about] tak[ing] the nuclear drink in front of a class?

GOFMAN: Oh yeah, I think he did drink radioiodine. I've heard those stories. He was teaching a course of applications of radioactivity in biology and medicine in our division. When Joe died of leukemia, I inherited that course and taught it after that.

GOURLEY: Did you do the same thing?

(h) Medical Treatments With Radioactive Phosphorus (32P)

GOFMAN: No, I didn't drink anything. I did work in John Lawrence's clinic, as I told you, and treated people with radioactive phosphorus. That was not human experimentation: John had tried this.

Every person that came to John's clinic was referred by a doctor for radiation treatment, and patients knew what they were getting into. To clarify things for you, I had already [known about these radiation treatments] from having read all of the stuff they'd done.

The treatment of polycythemia [veras]²⁰ was very successful with radioactive phosphorus. The treatment of leukemia was successful only in one form of leukemia, called chronic myelogenous²¹ leukemia. It was the other leukemias that did poorly with radiophosphorus. By the time I got there in 1947, they were no longer treating other leukemias, but the chronic myelogenous leukemia did quite well sometimes.

GOURLEY: Which type of radiation?

GOFMAN: Radioactive phosphorus, 32P. John had one young lady, I remember, who was about 20 years of age. She was in here, I think, [for her] 10th or 11th year of treatment.

There was a woman sent in by a doctor at Berkeley. She had a chronic myelogenous leukemia, and he told her, "You've got 3 to 6 months to live, but if you want to go over to John Lawrence's clinic in Berkeley, they might accept you as a patient to treat you with

radioactive phosphorus. We don't know if it's any good or not, but any rate, it can't hurt you."

John was too busy to see her, so I [saw] this lady, a lovely young lady who had two children about 5 and 7 years of age. She was depressed: [she] had just been told [that] she had 3 to 6 months to live. I told her [that] I'd looked at the records and I'd helped treat some of the people with her disease and some of them have [had] years of good health with radiophosphorus. She'd listen to all of it, we treated her, and she responded very well.

We spent the first year convincing her that she wasn't dead, because she'd been told she had 3 to 6 months to live; it was already a year. She did [so] well the second year [that she] took a trip to Europe with her children. The third year, we had to treat her again, because she had a relapse and she did well with that. But then, at the end of the fourth year, she went into what's called an acute phase of myelogenous leukemia; there was nothing we could do.

So this lady, who had 3 months or 6 months to live, lived 4½ years and watched her children get a little older. I think being able to give someone 4½ years of life when they were pretty sick when they came, was quite an accomplishment. I thought it was worthwhile, working on that.

I did leave that work when my own work on heart disease got very busy. I [had] 50 people working with me in Donner Lab; [a] lot of graduate students. I just couldn't keep up the work on the radiation.

HEFNER: Let me finish out with some things [and] then hit the heart disease.

GOFMAN: Whatever you want.

(i) Conflict Between University of California San Francisco and Berkeley

HEFNER: Did you know about the issue of treating patients over at UCB? UCSF really wanted all the patients treated over at UCSF.

GOFMAN: That's what I think is part of [the] reason for the conflict between John [Lawrence] and Robert Stone-because John had his clinic right in the Donner Laboratory.

HEFNER: He even signed an agreement that no patients would be seen over at UCB, but yet they were.

GOFMAN: Oh, I didn't know that John had signed that agreement. The hell was violated out of that agreement, I could tell you. I got there in 1947; Hardin Jones and I met the summer before, during the year of my internship. He was enthusiastic to get me to come over there. I think I got the appointment because of Hardin Jones. I'd just arrived there at Berkeley in 1947. I talked to John, [who] said, "If you want to work in the clinic, we'd love to have you." So I did. I started out working every Friday, taking care of those patients. I never heard anything about a conflict at all, but we sure as hell were treating patients.

HEFNER: What did you think of Professor Jones?

GOFMAN: I thought very highly of Hardin Jones. He was a great facilitator of work. He's very bright, in the first place. Hardin's a guy who had golden hands in the laboratory. A superb investigator, but Hardin just seemed to enjoy meddling around in other people's work, rather than working himself.

I got him interested in the lipoprotein work we were doing. He did help some, but most[ly] he just wanted to facilitate things. We wrote some papers together; he's a damn smart guy. Totally up-and-up guy; you could trust him with everything. We did a lot of work together and I would say that, for me, Hardin Jones facilitated a hell of a lot of the things I got done, in every way-running interference in the university. And I had to run interference for John in the university, too.

That happened because I [had] some influence in the university, in a peculiar way, that had to do with Ernest Lawrence getting very excited about our heart disease work. He'd bring regents around, so the chairman of the board of regents thought I was the best thing since sliced bread, and things like that.

That group in San Francisco really wanted to kill the Berkeley operation in John Lawrence's hands. It was an ongoing thing. I remember working with Mrs. Hearst, who [was] a regent, to try offset some of these things. I met her through Ernest and John. We cooked up this thing to have a big celebration [for] the 20th anniversary of John's coming to campus, with a lot of fanfare. Mostly to offset this thing from San Francisco.

I was partisan to the Berkeley operation then, although I didn't know any of background of why they [were] jealous, except they were. A lot of crazy things there.

I was awarded the Gold Headed Cane, which was given to a senior medical student with the best promise of being a true physician: we get a cane with a gold head on it. Professor Kerr was the one who instituted this thing at UCSF, and I was a lecturer in the Medical Department.

But talk about jealousy! By 1952 or so, we'd gotten an awful lot of publicity nationally and internationally, in connection with the heart disease work. Professor Kerr called me up and said he wanted to see me. He came over to Berkeley to see me. [I] didn't know what he wanted. He said, "I had to talk to you about your work on the heart disease. You know, I'm the professor of medicine in [the] medical school and you're in my department." I said, "Yes, I know; I'm a lecturer in the Medical Department. My assistant professorship is here at Berkeley." He said, "You never checked with me about publishing your papers."

I said, "Of course not, Professor Kerr: we don't do that here. I don't check with John Lawrence, either, when I publish a paper, and it never has occurred to any of us to check with anybody." "Well," he said, "that's not how we do things." I said, "Well, what do you want to

do? There is an easy solution, Professor: just remove my name from the medical school department affiliation, because I'm not going to check my work with anybody." He said, "Well, we don't have to be that drastic about it."

You know, that's crazy, just absolutely crazy. He should have been very happy we were doing the work. We got a lot of recognition for the University of California. Here he was, this was a totally separate battle from John Lawrence's battle, but he left and we never checked any papers with Professor Kerr.

I did [a] number of collaborative studies with other people in the Department of Medicine and published with them, Felix Kolb, [and] Alex Simon in Psychiatry. So there were these antagonisms, and you know, it was an antagonism that was bred out of this thing. The first-year [curriculum] of the medical school was in Berkeley. The second, third, and fourth years were in San Francisco. Anatomy, Physiology and Biochemistry were all in Berkeley. In the 1950s, there was a debate [about whether] the whole medical school [should] move to Berkeley, or should the first year move to San Francisco. The San Francisco people-I think largely the entrenched doctors in San Francisco-won, and the first year moved to San Francisco. I think they still thought that anything that had to do with humans belonged to them. They were jealous of our work and jealous of John's work separately.

HEFNER: You also worked with Cornelius Tobias, [who] was in that department.

GOFMAN: He was, and I didn't work with him at all.

HEFNER: Okay.

GOFMAN: Tobias and I were always friendly, but he was working on the radiobiological effect. I knew him, but we weren't close. I did a lot of work with Hardin Jones, but practically nothing with Tobias.

HEFNER: Okay, so is there anything else about how that group worked? How were you [a] part of the [Lawrence] Radiation Laboratory?

GOFMAN: When I came there in 1947, I had an assistant professorship in what is called the Division of Medical Physics, a branch in the Physics Department under Professor Raymond Birge. Our appointments were: 50 percent of our salary came from Rad Lab AEC funds and 50 percent was from the university. But it was understood that if anything were ever to happen to the Rad Lab Funds, the university would pick it up. I was in the Rad Lab.

(j) Reflections on Ernest Lawrence

HEFNER: How were you treated by all the physicists? Were you a welcome[d] group?

GOFMAN: Yes, I think we were a very welcomed group. When I did the work on

heart disease, I needed a lot of subjects to get blood, do some studies. Not with radioactivity, but with vitamins, things like that. We were trying to influence by blood lipoproteins. There were all kinds of people in Rad Lab who volunteered to be subjects in our work. We were very enthusiastic.

We received, in the early years, like 1950, one or two invitations. I was invited up the hill to give a talk at the Physics Colloquium. There were about 250 people. There I had to talk on the lipoproteins and heart-[you] get an idea on what the quality of the leadership of that lab was with Ernest Lawrence there.

I figured that physicists would know about the optical system of the ultracentrifuge, and I wasn't about to try and explain it. So, I went over that very lightly. At the end of my lecture, a voice in the back of the room said, "John, I don't understand the optical system." It was Ernest Lawrence. [There were] 200 scientists there and Ernest Lawrence. He was such an unassuming guy. So I went a little bit [over] the equations. He said, "John, I still don't understand, but I'll come down and see you about it." [I thought,] "He's never going to come down and see me about this thing."

About two weeks later, I was sitting in my office about 5:30 in the afternoon doing some work. Ernest Lawrence sticks his head in the door, and said, "I'd like to see that optical system in the ultracentrifuge." We went back, took off the cover, took off all the housing, and went through it from beginning to end. He said, "Oh, of course. How's the work going?" I said, "It's going well, but we sure could use another ultracentrifuge." They then cost \$16,000. He said, "Come up to my office next week."

I did go up; called his secretary for an appointment and I got up there and went in to see Wally Reynolds, who was the business manager in the Rad Lab. Ernest said, "Wally, John needs another ultracentrifuge." Wally said, "It's not [in] the budget; you don't have the funds for it." Ernest said again, "John needs another ultracentrifuge, Wally. Get it." And I got it.

Ernest Lawrence, if he thought you were sincere and were doing worthwhile work, it didn't matter whether it was high-energy physics, low-energy physics, or medicine. If you were working in his lab and he thought you were doing something useful, there was nothing too good for you in way of facilitation of your work. Anything I didn't accomplish on my work was nobody's fault at Rad Lab but myself. Because I just have to say that with Ernest Lawrence's backing, I just had the royal carpet laid out for me to do work.

John Francis Neylan was the chairman of the board of regents, and Ernest brought him down. Ernest loved to tell the story about the heart disease and exactly what everything meant. He'd give a better lecture on it than I could. We studied John Francis Neylan's blood, and Neylan would then bring other visitors down to see. Berkeley Rad Lab in those days, being a part of it, especially with Ernest, was fantastic.

(k) Heart Disease Studies

HEFNER: Let's talk about the heart disease studies. How did that occur? How did that grow out of your research?

GOFMAN: Well, it grew out of this: I came back to Berkeley. As a young assistant professor, you're expected to start some research. In the first six months, I didn't have any good ideas about cancer which I thought I might work on. But I had one idea about heart disease [and] cholesterol, which was poo-pooed at the time [as] just a bunch of nonsense.

Didn't seem like nonsense to me. I felt maybe the reason why it just [had] such a bad name is that people didn't have the technology to study *how* blood transports cholesterol. So I decided [to] look at *how* the blood transports cholesterol.

There were two avenues that were then in existence. One is a result of the war years and the blood fractionation to get blood products for the military. At Harvard, they used what's called the low salt ethanol methods of isolating fractions of the various proteins-albumin, globulin. They showed that some of the cholesterol was carried in certain of these fractions.

At the same time, a physical chemist, an associate of The Svedberg-the great Swedish physical chemist who invented the ultracentrifuge, Kai Pedersen-had written the monograph, in 1946 or early 1947, on the ultracentrifugation of serum. [He wrote,] "Serum is a nonideal thing to study because there's some unstable molecules in the serum and you can get any result you want from the ultracentrifuge." His whole thesis was, "Don't try to do it with blood."

But we had just acquired at Donner, through the way of facilitating the study of large molecules, the second ultracentrifuge that was built in America. Melvin Calvin got the first one; we got the second one. We decided what we could find out, we at least ought to see if we got the same results as Kai Pedersen. And we did.

It looked crazy as hell. It seems as though if you just breathed on the serum, you'd get a different answer. One thing in *our* ultracentrifuge diagrams: it didn't look [as though it] was just a problem of unstable molecules. We got what's called a "dip" below the baseline. We never should have gotten a dip below the baseline. Frank Lindgren and I puzzled and puzzled over this.

I think Frank was the one who finally had an idea. We tested that there might be something of low density in the solution that could move either way, depending on whether you were in the solution that had the proteins in it or the solution free of proteins. It all opened up because we were able to explain everything about the so-called unstable molecules of Pedersen. There *were* no unstable molecules. It was just that you were dealing with something [with a] density close to the density of [the] liquid. Depending on slight changes in sodium chloride or sugar concentration, those things would get crazy patterns. But all the craziness disappeared.

(l) AEC Support for Heart Disease Studies

GOURLEY: Was AEC interested in this?

GOFMAN: Not terribly; they weren't. The AEC did support my work.

GOURLEY: How did that work?

GOFMAN: Why did the AEC support it?

GOURLEY: Yes.

GOFMAN: AEC supported Donner Lab as an entity. So, I share[d] in that support. But actually, as a matter of fact, with an expansion of that work, Shields Warren, who was then head of the AEC's Division of Biology and Medicine, suggested that we get [the American] Heart Institute[s] support. And I did. A lot of money from the Heart Institute and a number of private grants, too. Because [of] the AEC, we were doing some things with tracers and the study and lipoproteins, but they didn't regard it as mainline AEC work. That's why I did get the additional support from the Heart Institute. But the initial support was because we were an AEC Lab.

GOURLEY: Right.

HEFNER: So there really is no close connection between the blood lipids and your radiation sickness study?

GOFMAN: There were some. Two of my graduate students (one of them is an adjunct professor now), Tom Hayes and John Hewitt, [were] doing studies on lipoproteins in connection with fatal irradiation. Saw some very interesting effects of the lipoproteins that were predictive of whether animals would live or die with radiation. There was a big AEC interest in [that] aspect of the work.

GOURLEY: So, did you work with them on that?

GOFMAN: Yes; they were graduate students of mine but that wasn't my main line. My main line was on the heart disease aspects. We were able to show [that] lipoprotein levels were [predictive of] heart disease.

(m) Heparin and Lipoprotein Research With Human Subjects

HEFNER: Is there any connection with this heart research with heparin²² treatment tested at that time?

GOFMAN: Yes. One of my graduate students, Dean Graham, had noted there was a paper by Paul Hahn from the University of Tennessee that showed that [when] he gave dogs heparin injections, their blood might have been cloudy from fatty globules, [but it] cleared up [after injection]. We had this elegant technique by then. We could study the lipoproteins in this ultracentrifuge.

Some of us took some heparin by injection. Dean studied the blood and he just cleared out some of the lipoproteins with the heparin infusion. Then we tried to do that in a test tube, and that didn't work. But if the person [was injected with] the heparin, [and we] drew the blood, then that blood would cause [test-tube] alterations in the lipoproteins. Bernard Shore, a graduate student of mine, finally proved that the effect of heparin was to release into the bloodstream an enzyme which is called lipoprotein lipase.

We thought that the heparin story [was of] obvious importan[ce] [for] the whole question of management of lipoprotein. Endothelial cells,²³ [under] heparin, [activated release of] the lipoprotein lipase from the endothelial cells. We were hopeful of really doing some management of the whole problem of abnormal lipoproteins with the heparin, but it was not that easy. I probably injected myself 150 times [during] heparin experiments we did on ourselves.

HEFNER: Why don't you comment about that, too? Here we are in [the] context of this interview [about] human radiation experiments, and you've drawn a pretty stark contrast of today's human use standards versus then. Why don't you [comment on the] contrast?

GOFMAN: Max Biggs, who later came back to Livermore and took over the Medical Department for me, was doing a Ph.D. thesis [(dissertation)]. He had his medical degree [from Harvard and did some work on] people with these abnormalities of lipoproteins. He did do some tracer work with tritium-labeled cholesterol in those patients.

I can't remember whether Biggs had to go through any committee at all. I don't think we did at that time. But we did an awful lot of experiments on ourselves. Hardin Jones and I, we were interested in what happened acutely after a fatty meal; we were interested in what happened for 24 and 48 hours after giving yourself a heparin injection. We did an awful [lot of] experimentation on ourselves. Max Biggs' work did involve giving some tracer to some of these patients with lipoprotein disorders. Donald Rosenthal did some of that, too. Those were [the] only things where we were using tritium as a label for cholesterol.

HEFNER: We found some information about patients at Stockton, [California] and Napa, [California].

GOFMAN: We worked a lot with the people in Psychiatry. How that happened was that Mary Lasker, who died last year, of the Lasker Foundation, helped me a great deal with my work. She once said, "If all this work of yours on heart disease is correct," (she didn't doubt it at all), "shouldn't it apply to stroke and cerebral arteriosclerosis²⁴?" I said, "Well, Mary, it should, [but we have only] checked this out on the heart disease." We studied hundreds of people with heart disease. That didn't involve injecting them; we got blood from them. I said, "I don't know the answer about cerebral disease." She said, "Well, couldn't you find out?" I said, "Yeah, we can start doing some studies on the correlation between heart disease and brain disease." [She said,] "Well, what will it cost?" I said, "About \$75,000, maybe put in about \$25,000 a year for 3 years." I wanted the program; she sent me a check for doing the work. That was Mary.

To do the whole story, I contacted Alex Simon, who is a professor of psychiatry at UC here in San Francisco-he's a wonderful guy; and Nathan Malamud, who worked on cerebral pathology. He [(Malamud)] was also in Psychiatry. [I] went over and talked to him. One of the studies we arranged to do was to get tissues from heart and brain of consecutive deaths. They get people who died in the mental hospitals. I don't know what they had to get, in the way of permissions, to take these tissues. I just don't know, but they arranged that and we did some studies that were published in the *American Journal of Cardiology*. Nathan Malamud and Wei Young, a Ph.D. student of mine, and I, published a series of three articles in the *American Journal of Cardiology* on the relationship of cerebral and coronary arteriosclerosis.

Out of that, we were wondering whether people who had strokes in the mental hospital would show anything. We did some studies of the blood and some of the people in Stockton and Napa got interested in the possibilities [that] these lipoproteins might be involved in mental disorders. We [did this] through Alex Simon's contacts in those places. We arranged studies in both Stockton and Napa State Hospital-[no radiation studies].

GOURLEY: What were the studies? What did they involve?

GOFMAN: [They] involved the study of lipoproteins in their blood. Never any radioactivity involved at all. No tracer studies at all.

HEFNER: How about any research with Langley Porter [Clinic in San Francisco]? Were you involved in any research [there]?

GOFMAN: [Just pathology studies on] a hundred hearts and brains. It's a classic study. I think it's the best study that had been done at that time. I don't think that anyone has done any better studies since of the correlation of the amount of arteriosclerosis in one of 16 cuts of heart arteries with each other. Then the correlation [of the] cerebral arterial system of one cerebral vessel with the other, [and] then the intercorrelation between the cerebral and the coronary. In answer to Mary [Lasker]'s question, "Does this apply?": We concluded that there were two things: one is a lot of interrelationship between this disease in the brain and the heart; and, [two,] there is also a lot of independence, meaning that there is a local factor in the vessels that partially determines [what] happens, as well as the general factor such as lipoproteins. The group that I contacted to do that was Nathan Malamud, the pathologist, and Alex Simon, who were professors of psychiatry at Langley Porter.

HEFNER: Do you have a sense of informed consent at that time, how it would be structured?

GOFMAN: You mean to just get the arteries and the brain sections [from deceased persons]?

HEFNER: Yeah.

GOFMAN: I have no idea. My only thing was, Alex Simon was a guy who was a doer and Nathan Malamud, a shy guy, but a superb pathologist. They said, "Yes, we can get the hearts and the brains." They did. Wei Young, the Ph.D. biophysicist working with me, he did all the sectioning and staining of the tissues and all the measurements on those hearts and brains, result[ing] in that series of three papers. I never asked anybody for permission. I didn't know of any-

HEFNER: -any use for internal review board?

GOFMAN: No, I don't remember anything about it at all. I just said, "Alex, can [you] do this?" He said, "Sure, we'll get the hearts and brains." And they did. I haven't the vaguest idea whether they had to go [to] anybody. It [is not] the way things are done today-I can tell you that.

HEFNER: That's true. It is good to contrast the two, to see where we've come.

GOFMAN: The things I did with a lot of people, we were a referral center for the people with these bizarre blood lipoprotein patterns from all over the world. Sometimes, for some of them, I wanted to know whether they would alter their diet.

Bill Donalds was the head of the Cowell Hospital, the head physician; he was a good friend of John's and Ernest's. I wanted to know a lot of things about diet and lipoproteins. I went over to see Bill Donald and said, "If I can get some cooperation from your dietitian, we could [do] some interesting studies on arteriosclerosis problems." Bill said, "Sure."

He introduced me to Virginia Dobbin. They set up a diet table and I had between four and eight people eating lunch and dinner at the Cowell Hospital. Virginia did all the menus. I would tell her we would like to have a high-cholesterol diet or a low-cholesterol diet, a high-fat diet or a low-fat diet, or a high-animal-fat diet or a low-animal-fat diet. Alex Nichols-at that time (he's a professor in the Division of Medical Physics) [he] was a graduate student of mine who got his Ph.D. with me-co-handled that whole diet study.

We did do a lot of human experimentation in this sense. We had both some students and some of these people referred from around the world. We would have them on one diet or another and we'd study their blood every week-[of course, all these people knew these were *experimental* studies.] And we didn't get any permission from anybody to do it but they never got any radioactivity.

We had that diet table running at Cowell Hospital for a few years. We had excellent cooperation from Virginia Dobbin, and my wife, and Hardin's wife and Tom Lyon's wife. Tom was the cardiologist in San Jose who worked with us, providing us clinical material. The [wives] wrote a book on the low-fat, low-cholesterol diet in 1951.

HEFNER: The staff wanted me to ask you about that.

GOFMAN: You want to see it? I have it here and it's been revised a few times. It's still selling. Let's see, from '51; that's 43 years. That's a long time

for a book. At least they get some royalties every year under that book. When Alex and I made some major discoveries about carbohydrate and various fats of the diet, he and I and Virginia Dobbin wrote a book on dietary prevention and treatment of heart disease which was nowhere near as successful as the low-fat, low-cholesterol diet. But it's a damn good book.

I don't know if I know anything more about the John Lawrence thing. There were some underhanded things, actually some efforts to try to destroy the division, which was the reason I went to see Mrs. Hearst. I asked her to set up [an event] honoring John Lawrence, and she did. [The] regents sponsored [a] dinner and made a big to-do about the Donner Laboratories and all the good things that were done there. It was just a kind of slap in face to the medical center, which was not being very nice.

HEFNER: Was there anybody else trying to destroy-

GOFMAN: In Berkeley?

HEFNER: Yes, in Berkeley.

GOFMAN: Yes, that was a lot later. Some of the biologists there didn't particular[ly] like the Division of Medical Physics. In the reorganiz[ation], it was shifted [to the] molecular and cell biology thing. The Division of Medical Physics became a department, then was abolished in the biological reorganization.

I think there was a lot of arrogance on [the part of] some of the people in molecular biology. They [thought they] were the greatest thing since the wheel and everything else didn't matter. I think they considered some of the goings-on as to why Berkeley Rad Lab [should] not work on heart disease. Just offshoots of our work and it's gotten to be rather well accepted. But I think that to some of the molecular biologists-this is before they grew up-they regarded this as too practical, and therefore of no interest in a great campus with fundamental science. You get stupidity on campuses sometimes-[that] has no equal and sometimes equals most of the stupidity elsewhere. Nothing secret: I'll tell them that, too.

HEFNER: On the off chance, do you know anything about experiments conducted by Will Siri or others on the San Francisco 49ers, the whole-blood-volume study?

GOFMAN: I do know this. Will Siri was at the Lab when I joined the Lab in 1947. He's an extremely bright guy. He wrote the first book on applications of tracers²⁵ in biology. He was working for his Ph..D. and I was on his Ph.D. committee. He could not take an [oral] examination: he simply blocked completely. We talked to him and said, "Look, maybe we [can] do it in writing." He refused. He never got the Ph..D degree; he was helping John Lawrence with all their work with radioactive iron and iron metabolism. That's all I know about.

John was so interested in polycythemia vera, the disease that he successfully treated with radiophosphorus. It's a disease of too many

red blood cells. They were interested in that and altitude effects [in other] continents. I think Will went with John Lawrence down to South America in connection with some of that. I never knew details, except that I had a very high respect for Siri, and I still do.

I tried like hell to make some progress to get him to get his Ph.D. He just could not stand examinations. Having written the first book on radioactivity and applications of artificial radioactivity, you'd ask him a question on his exam and he couldn't answer. The guy obviously was one of the world experts. That's all I knew about him.

(n) Radiophosphorus Therapy for Polycythemia Vera

HEFNER: Given what you know, your social and political sensitivities, are any experiments from that 1950 era-the radioactive iron, the treatment of breast cancer, radiophosphorus-are there any of those experiments, [which you] say to yourself, "That was a little in the gray area"?

GOFMAN: Well, let's take the radiophosphorus first. I made some radioactive nuclides of yttrium [that] we tested in John's clinic. Nobody who came to that clinic, to my knowledge, came as an experimental subject without knowing. These were people who had a serious disease, knew just what was being done, and wanted it.

You may have heard some criticisms of John Lawrence's radiophosphorus therapy. Some people said he killed people; I think that's unfair and false. I [will] tell you what the situation was. The disorder that was John Lawrence's great success was polycythemia vera, which he treated with radiophosphorus.

Treating those people [with] radiation was *not* a new idea. Radiation had been used to treat polycythemia for decades before John Lawrence came on this scene. But they used either radium or x rays. Now that's just a fact. So, John thought, "Well, if these people are making too many red cells and radiophosphorus goes to the bone marrow and the spleen," (organs involved in the making red cells), "maybe that will work better [than] or at least as well as external x rays or radium." That's the history of it.

So if anybody says that John Lawrence introduced radiation in the treatment of polycythemia vera, that's a falsehood-an out-and-out lie. John Lawrence was giving radiophosphorus and it turned out to be a very good way of managing these people who would otherwise be treated elsewhere with x rays or radium.

Now, what was worrisome, however, with anyone [treated with either] radium, x rays, or John Lawrence's ^{32}P , was th[at] some of the people with polycythemia vera after x years, where x could mean 5 or 10 or 15 years, went into a new phase of their disease where they became leukemic. Some of the critics of radiation treatment of polycythemia vera said radioactivity *made* them become leukemic.

When I came to Berkeley and worked in John's clinic for those first couple of years, with us in the clinic was a young doctor by the name of Robert Rosenthal. Robert Rosenthal's father, Nathan Rosenthal,

was one of the great hematologists²⁶ of this country in New York. When Robert was with us there, Nathan Rosenthal visited occasionally. One afternoon, I remember, John called us all together and we were going to talk about polycythemia vera and the conversion to leukemia and this whole question of whether the radioactivity was causing the leukemia. Nathan Rosenthal, by that point as one of the world's leading hematologists with about 40 years' experience, said, "You know, I've treated polycythemia vera for over 40 years" (or some number like that!). He said, "It doesn't matter what you do." One of the treatments at the time was venapuncturing with repeated bloodletting.²⁷ "Whatever you treat them with," he said, "eventually, if they don't have a stroke from the polycythemia vera, if you can control that, [if you] make them live longer without a stroke by cutting the red cells, they'll all end up with leukemia. It's got not a damn thing to do with the treatment."

John felt very much relieved by that. There are still people who say that the radiophosphorus caused the leukemia. I don't know whether a decent study has yet been done to ascertain whether they're [right] or not. It's just as clear as crystal in my mind [and I am] amazed, that he said, "They'll all end up with leukemia no matter what the treatment."

(o) Pre-1945 Medical Use of High-Dosage Radiation

GOURLEY: What about some of the other tracers?

GOFMAN: The iodine or iron?

GOURLEY: Yes.

GOFMAN: Let me say that I've gotten a lot clearer in my mind on this since February of this past year, when this symposium at the AAAS²⁸ came up. Nobody seemed to know that radiation was the cause of breast cancer. So, I've gone back and looked.

The '40s are *not* the interesting period with respect to human experimentation. Human experimentation started back in the '10s and the '00s of this century with Roentgen's discovery. Every disease known to man became subject to treatment with x rays or radium.

I can tell you this because I've been in the dungeon [of the UCSF library], where all the pre-1960 volumes are, day after day of this year and went through page by page in the *American Journal of Roentgenology*.

I wanted to know the flavor of the times. I wanted to know what the radiologists were saying to each other in their meetings. I went through some 40 years of journals page by page. You know I can find things out by getting a bibliography but I wanted to see what they were saying in their actual papers.

You name the disease; name any disease. You want to know whether asthma was treated with radiation? I'll show you the papers of Dr. Eugene Leddy from the Mayo Clinic in the '20s [and '30s].

We've treated 200 people in the Mayo clinic with x rays. And then they decided to modify it. They treated another 250, and then the final study: "We treated 1,000 people with bronchial asthma with x rays." You think every one of those people had a consent form? That was therapy.

X rays were-there was a radiology department in the Mayo Clinic. They did diagnostic and therapeutic work. Now in the very earliest years after Roentgen's discovery of the x ray and Curie's discovery of radium, both got into medicine very quickly. It looked promising; [but it was not] limited [to] the therapy of cancer, with both x rays and radium. That was only part of the story. Every disease you can think of, there is a paper. Let me just go through some of them.

GOURLEY: All right.

GOFMAN: There was a disorder [that] for a couple of hundred of years worried the hell out of people. That disorder [is] what we call today SIDS- Sudden Infant Death Syndrome. Nobody knew what was causing the SIDS. I have over there, on the shelf in the library, a 1914 issue of Gwathmey's *Anesthesia*. The worry was that there [were] some kids that have an operation, tonsillectomy or something, and they would die when the anesthetic was given; or, everything was going [fine] and they would just suddenly die.

Over a hundred years back [from today], somebody [thought] this must [be] due to something with [the] thymus gland. It was a mysterious gland that no one knew what it did. We don't know too much today, but more [than we knew then]. There was this idea that the large thymus might be the basis of a [disease] called status thymicolymphaticus. What status thymicolymphaticus did was, first of all, some of these children [had] trouble [because] of obstructed breathing. They were said to have a crowlike respiration and they died. Gwathmey's book indicated that they were complaining [about] anesthesia.²⁹

It was a very scary thing if you were a mother and had a baby that was having some respiratory difficulties and you heard about this thymus [problem]. You'd worry about it.

In 1911, a man by the name of Sidney Lange, a physician, a radiologist, in Cincinnati gave [a] paper, saying [he] had a lady bring in a child. She'd had two children before die of this Sudden Infant Death Syndrome. This child was getting blue and having trouble breathing, and he said, "I irradiated the thymus gland." The child did fine, and so I did a second, and a third, and a fourth.

[The Lange work] didn't cause too much change in the first few years. But then it got picked up. The answer to Sudden Infant Death Syndrome was to check the thymus with x rays to see if it is enlarged. You can see the shadow: it's right underneath the breast bone. If there was any indication it's enlarged, [the] treatment was x rays.

Now how much x ray did you think they gave? You talk about tracers, you know tracer experiments, you give somebody a fraction of [a]

rad; *they* gave these kids 200 to 400 rads.

It didn't start with the bomb; it started in 1911 when Sidney Lange gave that paper. *And it became the rage.* You, as a surgeon who had operated on a child without first checking whether he had [an] enlarged thymus, can face a malpractice suit. So, there were just thousands of children tested.

Some people said, "Hey, look, why do we wait until these kids' tonsils [are] taken out? Why don't we do something better?" Here's what they did.

A man by the name of Sam Donaldson at Ann Arbor, [Michigan], a professor of Radiology, took consecutively 2,000 babies born in the nursery, [and] studied them when they were less than a week old to see if their thymus was enlarged. If their thymus was enlarged, they said, "Why wait until they come in with troubled breathing? Give them the x ray right now." They did.

Two thousand [infants] were studied and 5 percent of them, who they said had enlarged thymus, [were] treated [with x ray]. They weren't to be outdone, because Conti and Paten, at the University of Pittsburgh, constructed a series of 7,400 consecutive children with the exact same thing being done.

So, you're talking about a few human radiation experiments done in the 1940s, when experiments [that] would make you hair stand on end were already completed in the '20s! Not on a few people, not on 18 people with plutonium, but 2,000 children who were not even out of the nursery! The only thing you had to do to get treated with radiation was to be born alive. They didn't treat you if you were born dead. If you were born alive, you got treated.

Mosher at the Massachusetts General [Hospital]-that's like the mecca in medicine-I've already talked to you about Leddy at the Mayo Clinic, which is pretty hot stuff, and Massachusetts General is the mecca of medicine. In 1925, Mosher talked about the kids that had to have tonsillectomy and adenoidectomy. Every one of those kids [who] came to the Massachusetts General or the Massachusetts Eye and Ear Infirmary, for possible surgery of their tonsils, had their thymus studied. No operations were done if their thymus was enlarged. They then had to get 200 to 400 rads to their thymus. He reported on 5,000 children, proudly announcing they hadn't had any of them die under anesthesia.

So this became the rage in medicine for something like 45 to 50 years. The atomic era was near the end of that; they were pretty much over by 1960. In fact, in 1948, just after I'd been at USC³⁰ Med Center-the Pathology professor, one of them, Jim Reinhardt and Jesse Carr-Jesse Carr, the coroner of San Francisco and professor of Pathology-wrote an indignant paper in *Archives of Pediatrics* in 1948, scathingly criticizing the people who said that this thymus enlargement was a myth. "I know, I see these autopsies, I am [a] coroner," [he wrote]. He used to lecture in Pathology.

Here was this thing that went on for 50 years. This business of

human experimentation in the '40s and '50s, forget it. The big period of human experimentation was the '10s, '20s, and '30s. [The human experimentation after 1940 couldn't compare.]

Now why did I get interested in that, in particular? [Because] in fact, in irradiating these thymuses, they couldn't keep from irradiating breasts, and these kids are developing breast cancer now.

Breast cancer, if you want to know the real story, it's not a disorder [in which] you should look for what happened to you two years ago. If you want to know about breast cancer, look back 10, 20, 30, 40, and possibly 50 years ago [or more]. Because it's proved beyond [a] doubt, that those people who got irradiated then, 10, 20, 30, 40, 50 years later show up with clinical breast cancer. The most sensitive infants are irradiated in utero.³¹

Now I don't know, but I'll bet you between the Mayo Clinic, New York University, [and] the dermatologists throughout this country, [they] were the biggest users of x rays and radium. Now let me put it in the vernacular: There [is not any] disease that they didn't treat—none: eczema, psoriasis, lichen planus, warts, boils, carbuncles.

Interestingly enough, they really thought they were getting good results. On this thymus thing, I can show you paper after paper: "I've treated 5,000 of these children with 90 percent success." I can show you famous people saying, "If you don't get a result from treating thymus, you've got the wrong diagnosis because all of the cases I treat respond." Yes, all this appeared later.

So, the dermatologists all were going crazy. George MacKee, a professor of Dermatology in 1921, said, that the most valuable agent in dermatology for treatment is radium and x ray: "We are successfully treating 82 separate diseases with x rays and radium."

How many of those people do you think [gave] him formal consent? You got 82 different diseases, you'd think they didn't know about these. As manna from heaven, they had to experiment on people. They write up on papers on thousands of patients, 82 different diseases treated by dermatologist[s].

My brother-in-law, Jim McGinley, was the head of dermatology at Kaiser, San Francisco; he just retired. You'll see his name on some of the publications that we did on heart disease work. He got some of the special people with lesions, like heart lesions, but the lesions are out on their skin, called xanthomatosis.³² He said the last people at Kaiser, about 1960, put away their x-ray machines; but [un]til 1960, they were treating people with x rays regularly.

There isn't a disease they didn't try. Can you imagine asthma? A thousand patients at the Mayo Clinic treated for asthma! [At] Mass. General, 5,000 treated checked for [enlarged] thymus. That's where it all started.

And what happened in the human experimentation period, these people who treated all these patients made a cardinal mistake in

radiology. What was their cardinal mistake? I don't look at them and fault them individually for anything they did. I don't fault Eugene Leddy for treating a thousand patients with radiation. I might, in Leddy's shoes at that time, have done exactly the same thing. But a cardinal mistake, and it's being made today, by the way, but for a much more "malignant" reason today[, was made].

People thought if there was harm to be seen from a new agent, you surely ought to see it in 30 or 60 or 90 days; a year was like an eternity. The entire radiation community's thinking was, "massive doses." Radiologists were losing fingers, radiologists were developing cancer of the skin and dying of it. They refer to them in the journals as the pioneers who gave their life for their technology. Big dose-nobody argued about it. But 200 to 400 rads, *which today we think of [as] mountainous*, big doses that they didn't think were harmful, for a crazy reason.

They didn't have very good ways of measuring radiation at that time. So they used [what] was called the erythema skin test. If you take an area of the skin and irradiate with x rays, keep increasing the milliamps at a time [with] the x-ray beams on. Finally, you'll get to a situation where a week after the radiation you get a reddening of the skin, called erythema. That's the medical term for reddening. They said, "Look, below that dose, there's certainly not much to worry about." That's 300 roentgens. [That] is what's required to give you an erythema.

So it got into the mindset of the whole group of people worldwide, that doses like 100 rads, 200 rads, they're not going to do anything. And besides, if you haven't seen something [with]in a year, what are you worried about? I could show you in paper after paper from the finest institutions in this country or abroad, stating, "If it doesn't really work in every patient, we can [at least] say there will be no harm." Two hundred to 400 [rads]! And so the idea that the harm would come 10, 20, 30, 40, 50 years later, simply was *never* thought of.

This is an illustration of what we've called disaster creep. Scientists and physicians *never* in the early part of this century, *never* thought of the possibility that what they had to look out for was something 40 years down the line. Properly, people get pretty damn excited if the most sensitive [to radiation] are kiddies that are less than a year old. At 40 years old, they don't regard themselves as proper candidates to die. A 40-year-later cancer is a serious matter.

(p) Attitudes Toward Radiation in AEC's Biological & Medical Program

GOFMAN: But this was lost on people who came in to run the Atomic Energy Commission Biology and Medicine Program after the passage of McMahon's Atomic Energy Act. They brought in the whole troops from radiology from all over this country. These people all had this mindset that 200 to 400 [rads] of x ray or gamma rays can't hurt you. Poo-pooed it. Let me illustrate it for you. I don't know your community, but you've heard of the shoe-store fluoroscope, I'm sure. Did you ever see one?

GOURLEY: No, I haven't.

GOFMAN: Too young. The shoe-store fluoroscope: I know when I was kid in the '30s, I visited the shoe store and got fluoroscoped. The first scientific paper on the shoe-store fluoroscope was written in 1949. Why was it then? Because every goddamn hamlet in the United States, anywhere, had a fluoroscope in the shoe store. And nobody studied [it], nobody had the vaguest idea of what kind of dose you got to feet or anywhere else.

And so in the *New England Journal of Medicine*, back-to-back in 1949, are two papers: one, [Dr. Williams] on measurements he did on the fluoroscope in [about] a dozen shoe stores; and, the second paper was by Louis Hempelmann, and Louis, as you know, came up from the Rochester³³ group of radiology. Louis Hempelmann said, "Well, we really don't know much about 200 R but we really [should] probably restrict the use of the fluoroscope." Here, they've been in every hamlet for 20 years, at least.

They had great solutions for how to handle this problem: Put a sign on the shoe-store fluoroscope, "Do not do more than 3 examinations per day nor 12 per year to you, as a customer." What did you have to do to look into your feet, the bones in your feet? You press the button. That was the only control of this thing. So, 20 years after these things had been all over the country [they comment on safety]. Why [the long delay]? Because they didn't think any of these things mattered.

And they're the people who came in to lead the atomic energy scene. Shields Warren was doing pathology, Robert Stone, radiologist, Stafford Warren, radiologist, Stafford became a dean at UCLA. He's one of the early publishers on various methods of doing pelvimetry and other examinations when he was a radiologist.

I have to tell one thing: Stafford Warren was Robert's Stone's right hand in the Manhattan Project Medical Division. When we did that job for Oppenheimer of isolating that one milligram of plutonium from uranium, Stafford Warren announced he was coming to inspect our operations there and in Gilman Hall. He and a couple of others from the Biology and Medicine Project of the Manhattan Project came.

Here we were getting irradiated with lead in front of these big vats to try to give us some shielding. We were using up chemicals like crazy to process a ton of uranium nitrate. We had to use a lot of sodium acetate and sodium nitrate and [it] came in 5-pound cardboard casks. We emptied out a cardboard cask; we'd set it over the corner of the room.

Stafford Warren's report on our operation to the Manhattan Project [included] nothing about radiation hazard. They said, "They have these boxes stacked in one corner of room. Somebody could have one of these boxes fall on them." It would be like a cardboard hatbox falling; could not hurt you. That was his report! But that's a separate little vignette. So, of the whole cast of characters, Eugene Saenger, who's gotten a hell of a lot of bad rap.

GOURLEY: Why do you think he's gotten a bad rap?

GOFMAN: I think he deserved a bad rap. But most of them don't deserve a bad rap. I don't think Joe Hamilton was really an evil person. I don't [think] Louis Hempelmann was an evil person at all. In fact, he participated in some very good studies of [dose] reconstruction [for] some of the people who got early doses to the thymus gland and thyroid, [and then developed] cancer. Later, breast cancer occurring.

Now, there is a peculiarity about this; it's interesting how peoples' minds can be compartmentalized. Let me illustrate that. When 1946 broke on the scene, we had the Atomic Energy Commission set up [and in operation January 1, 1947]. (I like Shields Warren by the way, I thought very highly of him.) A question comes up, "Well, what are you saying, are you telling me that no radiation was harmful?" Yes, they did know that it was harmful, but in a crazy way.

In the 1920s, there were these women who were tipping their brushes [with their tongues], painting the [watch] dials [with radium], and they got most horrible bone necrosis and cancer of the bones (sarcomas). Harrison Martland wrote that up in 1928, so everybody in the medical world knew about this. Moreover, in two places in Europe where they mined silver and other metals also rich in uranium-one was in Germany and one was in Czechoslovakia-there had been a disease known for 300 years called mountain sickness. Turned out that in 1879, it was discovered that mountain sickness was lung cancer. In the 1920s and '30s, it was pretty well settled that the alpha particles from radon and the daughters³⁴ of radon were the cause of that lung cancer.

The interesting thing was [that] somehow, all these radiologists didn't relate the external use of the gamma rays and x rays. The gamma rays from radium and the x rays to these internal things caused by alpha particles. [It was] all radiation, but somehow they separated [one from the other in their minds]. So, I can understand, although it was part of a terrible misuse of the technology, doing whatever you want to do on anything new and not thinking about the long-term consequences. Somehow they just never related these two things.

You had the whole scene dominated by the people who'd come up through radiology. You know, if somebody in Tennessee gave somebody something, some iron experiments or calcium experiments, I can see these people saying, "Hey look, what are you making a fuss about, we used to give people 200 rad from the thymus [in] the chest."

I think if Ruth Faden³⁵ doesn't understand that, she's not going to understand the whole story. The story doesn't start in 1945. What started in 1945, however, was a different thing. That was an arrogance. I haven't got a good thing to say about the Atomic Energy Commission, at all. But for very distinct reasons, I don't want to go into that and not answer the questions you have in your mind.

GOURLEY: I don't know whether we should continue chronologically or-

GOFMAN: I was just going to say, I am not [an] antibomber, so I'm in trouble.

The Department of Energy officials hate me, the nuclear industry hates me, and a good segment of the disarmament [movement], [be]cause I'm not a disarmer.

People talked, "What's Gofman's hidden agenda?" The trouble is, I don't have one. The last thing is that my hidden agenda is to get rid of nuclear weapons. Because I'm a bigger supporter of nuclear weapons than anybody in the Department of Energy, that I know. I really think they're important. They just think that they're a continuation of bureaucracy. But I just wanted to [say] that you'll hear bad things about me, such as, "Gofman thinks nuclear deterrence is important." And I do. I don't favor giving up our bombs. It's a real mixed-up picture.

(q) Establishing Livermore Laboratory's Division of Biology and Medicine (1962)

GOURLEY: Should we follow through with that or go to Livermore? Let's get on to Livermore.³⁶ What brought you to Livermore and to DBM (Division of Biology and Medicine) there?

GOFMAN: Well, in 1953, after the Livermore Lab was established, Ernest Lawrence called me up to his office and said, "Jack, you're the only person [in the] Rad Lab family that is both a chemist and a physician. I'm afraid that the 100 or so guys [who] have gone out from Berkeley to Livermore; Herb York and others who set it up, are going to hurt themselves."

I don't know if you know about it [but] Ernest was an absolute bear on safety in the lab. He believed in it. He said, "If you go out there a couple days a week, as my personal representative, nose around in everything. If you don't like what you see, tell them how to change it, and there's no use going to anybody else because you're speaking for Ernest Lawrence." So I said, "Sure, I'll go there."

GOURLEY: What sorts of things were you looking for?

GOFMAN: Mishandling of hot materials from Nevada without adequate protection. Not wearing their film badges while doing things. All violations of radiation safety, and there were many.

So, starting in late '53 or early '54, I went out to Livermore a couple of days a week. In order to hang my hat, I decided I'd organize the industrial medical department. I became the medical director for Livermore. Industrial medicine, not research.

So, all the new examinations, new hires, people leaving, or whatever, going out to the Pacific for a test, all the exams before and after, all came through me. But that was just a place to hang my hat. What I'd do was go around to [see] what the physicists were doing, or the chemists and somebody was handling some hot materials without their film badge. I'd say, "Where is your film badge?" and they'd say, "Over in the drawer in the other building." And I'd say, "Why?" and they'd say, "I can't get the job done if I'm wearing the film badge; I'll get overexposed." So I had to raise hell with them.

At any rate, in the course of that, there are [a] lot of questions that came up among the weaponers. I got to know them.

GOURLEY: What were some of the questions?

GOFMAN: Oh, you know, people were criticizing them about the carbon-14 and about tritium³⁷ and that they were releasing [during] the test. Was it really this bad? And so I would help them with calculations, [including] John Foster who was in the Weapons Division. So I got to know all the weaponers, [such as] Herb York-

GOURLEY: Teller?³⁸

GOFMAN: Oh sure, I knew him too. I knew him a lot more later. But that wasn't my research function at Livermore.

I brought Max Biggs, who had gotten his Ph.D. with me and he was Assistant Director of Medicine there. I did it [un]til 1957, at which time Max took over completely and I didn't go out to Livermore anymore.

Then things moved up to 1962, John Foster had become the Director of Livermore Lab and I knew him from those earlier days [when] I helped him. He called me up one day, and said, "Will you come out here to see me?" So I did. "We have a peculiar request. We got a request from the Atomic Energy Commission to see if [we] would set up a Biomedical Research Division here at Livermore."

He said, "What do you think of the idea?" I said, "Biomedical division at Livermore?" I said, "The AEC's got 18 or 19 Labs already; why a biomedical division at Livermore?" He said, "Well, look, they don't say it, but I know what the reason is." We had this huge series of tests in Nevada. By the way, when Khrushchev broke the voluntary moratorium on testing, [President] John Kennedy gave the Labs an order: "Put on a spectacular show." That was just one. They did.

But the trouble was that the milk network was by then, in '62, much more in place, and Utah was getting clobbered with radioiodine. Of course, it became a real flap in Utah, and of course the Federal Radiation Council solved it by announcing that the safe level of radioiodine in milk was three times higher than they thought. That just took care of it! John [Foster] said that the commissioners were just getting flack thrown at them from all over and they're on the hot seat.

GOURLEY: Where was the flack coming from?

GOFMAN: Utah downwinders. So I said, "They're getting a lot of flack from Utah. What in the world has that got to do with setting up a biomedical division at Livermore?"

He said, "Some of the commissioners feel that if we, who are making the bombs and setting them off in Nevada, had some biomedical advice, maybe they wouldn't get caught flatfooted with things like the scandal in Utah."

I said, "But Johnny, what are you going to do if you start the biomedical division and you set off bombs in Utah? Could a biomedical division prevent fallout from coming down?" I said, "It's crazy, you can't do it."

He said, "Well, aren't there some things you can do?" and we talked about [it].

I agreed that one of the things that could be done would be to examine all the new bombs and look at them. If you got iron in this position, it gets a hell of a lot of neutrons in radiation. Couldn't it be better if you substituted aluminum or something like that? You might thereby cut down the amount of fallout that would occur in some of these bomb tests. I said, "[A] biomedical division isn't going to stop the problem of fallout, except in maybe making it less."

So, Johnny said, "Do you think fallout is important?" I said, "Yes, the cancer and genetic effects of the fallout are very important. Aside from heart disease, it's the most important problem in medicine, as I look at things."

GOURLEY: You are basing that opinion, then, on what?

GOFMAN: The fallout?

GOURLEY: Yes.

GOFMAN: There was enough evidence to be very worried about the fallout. Linus Pauling³⁹ had made calculations that indicated that we might have caused a lot of potential cancers and genetic defects for what had already been done in worldwide testing. In 1956 and '58, Alice Stewart had written on the fact that just about half a rad to a rad [received by] children in utero was enough to give a big excess of 50 percent [increased risk] of cancer in the first 10 years of life. So a lot of things [were] coming up. So, I said, "Johnny, the way I live my life is, I decided to work on big medical problems. As I don't understand anything other than [that], if I'm not working on heart disease then I want to work on cancer and genetic injuries-because those are the big things that kill people by the millions prematurely."

GOURLEY: Yes.

(r) "Jack, all we want is the truth."

GOFMAN: "So," he said, "you think fallout [is] important?" I said, "Yes, it's important." He said, "How would you like to come out here and build this biomedical lab?" And I said, "No way, no way." I said, "I don't trust the Atomic Energy Commission. Look at what they did to Linus Pauling, look at [the] criticism they leveled at Alice Stewart."

GOURLEY: Even then?

GOFMAN: Yes, even then. I said, "I don't trust them." Foster said, "Do you trust *me*?" I said, "I've known you now for quite a while, Johnny."

He said, "I'm never going to Washington like Harold Brown did." He said, "I'm going to stay here at this Lab because I think that's where the most action is." So he says, "I can tell you one thing: if you came out here, you'd have my absolute backing." I said, "That's nice to have."

He said, "Do you trust the regents in the University of California?" I said, "Johnny, I have very good relations with the regents in the University of California. I have very staunch friends, I do trust them."

He said, "Do you know Clark Kerr?" I said, "Of course I know Clark; he's the president of the University." I said, "I have a lot of respect for Clark."

He said, "Supposing I could get you a letter from the regents and from Clark Kerr that if you came out to head this thing and if you were ever unhappy about it, you can go back full-time to your professorship, no questions asked." [I said,] "Well, I certainly wouldn't give up [my] professorship, but I could cut my time down, but I don't think it's for me. I'm happy at Berkeley; I've just gotten through the years of having 50 or 60 people working with me. I'm back in the Lab working myself, physically working. Not administering a group of 60. Why would I want to do that?"

He said, "Well, fallout's important. How would you like to work on problems like that? Build your own staff: all the people you want to get, bring in. You'll have the best facilities in the world." -Livermore does indeed have the best facilities. "They'll build you a building," [Foster said,] and indeed they did. He said, "You don't have to worry about your budget. The money is just about automatic." (Maybe not so now.)

I said, "I don't think so, Johnny." But I did bring a few guys who had gotten their Ph.D.'s with me, to talk with him.

He said, "Will you do me a favor? Would you write up a protocol of what it would be like, if it were going to be done, even though you don't want to commit yourself [or] have anything to do with it?" So I did that. I thought about [it] and there were some really attractive features. A three-and-a-half-million-dollar budget each year, [a] new building, and not having to worry about grant applications over and over. So, what [can] I say, somewhere along the line, I had a lapse of cerebration. I said, "I will do it."

We had to then go into Washington to sign the papers. Now at that point, Glenn Seaborg, my former mentor for my Ph.D., was chairman of the [Atomic Energy] Commission [and] had been there since [President John] (Jack) Kennedy was inaugurated. Theos Thompson was a commissioner; I've forgotten the names of two others. Jim Ramey was a commissioner; he was not there that day.

Wally Reynolds and John Foster and I went in. Seaborg had Chuck Dunham, by then the head of Biology and Medicine, having replaced Shields Warren, who had retired, and some others in the room. We were supposed to draw up the papers and sign the papers to

establish that I was to become head of this new Biomedical Division and an associate director of the Livermore Lab. There are 10 associate directors.

I said, "I would like to make a statement." I hadn't talked to Johnny about it at all. Glenn Seaborg said, "Go ahead." [I said,] "I would like to say I don't really give a damn about the Atomic Energy Commission's programs. I care about the public health. And so, what I want you know is, you're asking me to set up a division to consider the health effects of atomic bomb tests, uses in nuclear war, nuclear power, peaceful uses of explosives. We'll investigate these problems, but you're not going to get me to be silent and use the secrecy stamp to keep something from surfacing that I think the public ought to know."

So I said, "having said that, I think you should think twice about whether I'm the right person to head this program." I made [it] very clear exactly how I feel about it.

Glenn Seaborg said in memorable words, "Jack, all we want is the truth." If I'd ever seen the opposite of reality, this was it.

So we signed the papers; everything was hunky-dory. We got the budget; I brought out about 35 senior people from around the country. They had either gotten their degrees with me or I knew [them]. We built a division with 125 to 150 people in the whole division-lots of engineers who were working on fallout and the weapons testing. I made an agreement with John Foster that I would only have to be the head of the department for two years because then I wanted to get back in[to] the lab. That [agreement] was honored. I was head of the department for two years but I remained as an associate director of the whole Livermore Lab after that. Everything went fine.

(s) Livermore Biomedical Department's Work on Fallout and Plowshare (1963-65)

GOURLEY: Could you describe some of the work and some of the programs?

GOFMAN: We had a number of people trying to find out things about the whole distribution of radioactivity in people and animals. Arthur Tamplin headed a part of our project, which was called [the] Information Division, pulling together all the world literature on this.

Another part of our program was to try to work steadily on unmasking the evidence concerning human radiation effects and try to build some generalized ideas of what the health effects of radiation were. I worked on that with Art [Tamplin].

Bernard Shore headed the Experimental Division, where there were all kinds of studies being done about radioisotope uptake in animals. We had an animal farm there at Livermore, as a matter of fact.

So, everything was dedicated [for the Lab to know] at the cellular level about metabolism of radionuclides [and] about radiation effects

[on] the analytical level, taking all the information from Hiroshima, Nagasaki, from studies of women who had TB [(tuberculosis)] and later developed breast cancer. Alice Stewart's studies. Court-Brown and Doll were publishing on the people who had been treated with massive doses of x ray for a disease ankylosing spondylitis.⁴⁰ It's a form of arthritis of the spine. X rays helped that disease, but they were getting cancers and that was being published. So on the back burner, Tamplin and I were trying to pull all this stuff together.

GOURLEY: What was on the front burner?

GOFMAN: Front burner were all the things out at [the] Nevada Test Site [and] the animal data [from] the work in the lab. Everything went fine. One of the first projects, [was] to investigate, as an offshoot of Sedan, whether we should dig a Panama Canal with 300 one-megaton hydrogen bombs. That was Edward Teller's favorite baby. We went out there in '63 and we were doing a lot of consideration.

GOURLEY: The Sedan Shot was July-?

GOFMAN: About '62, I think.

GOURLEY: Okay.

GOFMAN: So we were doing an evaluation of Plowshare⁴¹ explosives, and in particular, the idea of the Panama Canal. The Directors' meetings were every Thursday morning and the 10 of us got together with John Foster-who then, by the way, went off to Washington in spite of fact that he was never going to [go to] Washington.

Michael May became the Director of the Lab. I had very good relations with Mike May. Everything was fine.

Within three months of being at Livermore, I got a call from Chuck Dunham in Washington. He said, "Can you come in next Tuesday?" I said, "What's up[, Chuck]?" He said, "I can't discuss it on the phone, but would you come in?" I said, "Yes."

I went to Washington. Five others and [I] were there. None of us knew what Chuck wanted. He said, "We have one hell of a problem. We got this guy Harold Knapp in our department here in Washington Biology and Medicine. Harold Knapp has done some calculations about Utah. He calculates out that the doses some of those people got from radioiodine seem like a hundred times what we said anybody could get." Dunham said, "He wants to publish it." So we said, "So?"

He said, "Look, if Harold Knapp publishes that, all of us at AEC are going to [be] made out to be liars. We cannot tolerate that." We said, "What do you want us to do?" [Dunham said,] "I want you to talk to this guy and see if you can change his mind about publishing it."

Here I [had been] three months before, Chuck Dunham was sitting in back of the room, listening to me say, "I'm not going to be manipulated." And here he is with a request that we help him

manipulate somebody else!

Harold Knapp came in the room and Dunham left. Boy! Knapp was just bristling. He told the six of us in the group to go to hell! We said, "Hey, calm down. Dunham just asked us to look over your work." When he had calmed down, we just checked his work. That wasn't so unusual for a group to look at a specific thing.

He was very sound. I think [there were] a couple of minor points that someone suggested; he agreed it [was] a good change. [We told him,] "Go ahead and help your publisher report it." So, Knapp left. [Dunham] checked on us, came back in, and we said, "Well, Chuck, we can't find any reason why Harold Knapp shouldn't publish his paper." He was just beside himself. He said, "We're going to be a bunch of liars!" We said, "The AEC will weather this; they've weathered all kinds of storms before. Then, they'll weather this and it won't hurt a thing and just go ahead." So he did.

The sky didn't fall on the AEC. I think these bureaucracies, nothing ever affects them. But here is the first time, three months after I'd gotten there, they're asking me [to] help a cover-up. But that all died down. Knapp published his report.

Then in about 1965, I decided that I ought to talk at the Directors' meeting on the Panama Canal. I said, "The conclusion of our Biomedical Division is: The idea of digging the Panama Canal with hydrogen bombs is biological insanity."

Edward Teller was unhappy but nobody else said a word about it. I got a little flack later in the Lab with hearing rumors that "Gofman was the enemy within," because the Lab was dedicated to the Plowshare program.

GOURLEY: Now what about Project Chariot? There were some biological studies associated with that, too?

GOFMAN: I don't remember anything that we had to do with [that]. There were these tests in Alaska; I just don't remember details about that. But we had that test in Colorado breaking up some natural gas formations underground with nuclear bombs. There was a lot of radioactivity [that] came out of those and I didn't think those were a good idea at all.

But most of that died with one thing, namely when the nuclear nonproliferation treaty was signed. All Plowshare activities had to cease because they figured the other signers would say, "Look, you say that's for peaceful nuclear explosions, but how do [we] know that it's not for weapons tests?" So they quit all the projects associated with Plowshare. Plowshare just died in 1965-66, as far as I remember anything about it. We certainly didn't hear any more about the Canal.

GOURLEY: I wasn't sure that some of treaties just got rid of aboveground testing-

GOFMAN: The underground stuff, that was a separate treaty. The [Limited] Test Ban Treaty, signed in 1963, got rid of the aboveground testing, and

permitted the underground.

But you couldn't test underground on a Plowshare program; that was just a way of covering up the weapons testing. That was a little later. So we weathered the Plowshare thing.

(t) The Controversy Over Nuclear-Armed Antiballistic Missiles (1969)

GOFMAN: They built us the building. Glenn [Seaborg] came out for the dedication of the building and I did get back in the lab after two years [and started] working on chromosomes in cancer and radiation. Everything went fine until the antiballistic missile treaty was being considered in the Senate.⁴²

A guy by the name of Ernest Sternglass had done some calculations and was cited in *Esquire* in a article entitled "The Death of All Babies." His estimate was that 400,000 children were going be hurt with genetic disease as a result of the weapons program. The Washington office of AEC sent out Sternglass's paper to me and the directors of other installations. I gave it to Tamplin to look at. Tamplin looked at it and wrote something he was going send in to the *Bulletin of the Atomic Scientist*. We forwarded that to Washington, and in response [the AEC] said, "Let's have a response of your view."

What Tamplin did was he calculated that the maximum number would be more like 4,000, not 400,000. So we sent that into Washington. The next thing, I heard was from Mike May. He said, "Jack, I don't know what's going on, but Washington's very unhappy with Tamplin's report on the Sternglass thing." So I said, "I'd think they'd love it because he's just saving their skin. Four thousand is the hazard, not 400,000, not the death of all babies."

He [suggested that I] call John Totter up and find out what the hell [was] going on. So I said, "Sure." I called John Totter, the head of [AEC's Division of] Biology and Medicine at that time; the head of Biology and Medicine because I put him there.

GOURLEY: How did that work?

GOFMAN: It worked this way. Spoff English and I were graduate students together in Chemistry. Both of us worked with Seaborg. Spoff had elected to give up his assistant professorship in Berkeley Chemistry and had gone back in the [Atomic Energy] Commission with Seaborg and had moved up to a higher position. He called me up one day: "Chuck Dunham's moving over to the National Academy of Medicine; whom shall we choose for the head of Biology and Medicine?" I said, "John Totter is your man," and he was appointed. That's how it worked.

He later said some nasty things about me, and probably doesn't know that it was because of my recommendation that he became the head of Biology and Medicine.

So I called up John Totter. He and Spoff English were on the phone. Yeah, Spoff, I knew very well; we were graduate students together. I

said, "[I] understand you're unhappy about Tamplin's report on the Sternglass issue." They said, "Oh no, we're not unhappy about that. But we think that the *Bulletin of Atomic Scientists* is not the place to put it. It ought to be in some more restricted genetics journal."

I said, "Gee whiz, this is a public fanfare; what could make more sense than to put [the work] in the *Bulletin of Atomic Scientists*? The public can see it." "No, no, we think it ought to be in the genetics journal." And I blew up and I said, "Look, John and Spoff, what you want is a whitewash, and you can go to hell." I said that. It's not the most politic thing to say, but you know, that was so damn blatant that I just couldn't take it.

Then I saw Mike May. "Did you talk to John Totter?" [he asked,] and I said, "Yes." He said, "How did it turn out?" [I said,] "I told John Totter to go to hell," and it was awful, nothing else was said. That blew over, too.

GOURLEY: So, now all of this was around 1969.

GOFMAN: Yes, around 1969. Then two things happened in '69. One, I got an invitation from the Institute for Electrical and Electronics Engineers to give one of the plenary addresses at their annual meeting, which was to be on nuclear matters and it was to be in San Francisco. One of my engineers got me the invitation. I said, "Sure, I'll do it."

Another thing happened: There was going to be a symposium on nuclear power. The AAAS was going to hold it and whoever was the chairman of that thing asked if I'd give a talk there. I said, "Well, that's on nuclear power; Tamplin is far more versed in the details of the hazard there. Why don't you extend the invitation to Tamplin?" So they said they would, and they did.

Then I gave my talk to the Institute for Electrical and Electronics Engineers. By the way, whatever you heard about *l'enfant terrible*, like myself, the paper [I] gave there, the most conciliatory, modest, soft-pedaled paper, where we suggested there was a danger [of] something [like] 16 or 32 thousand deaths per year, if everybody got the allowable dose.

(u) Ethical Responsibility to Prove Technology Is Safe

GOURLEY: Going back a little bit, I found a speech that you had given in '57 to the Public Relations Society of America. The quote that I pulled out of it said, "There [is] no proven harmful effect of radiation due to testing...."

GOFMAN: That's right. Have you ever seen a little book that I wrote called "Irevvy and Irrelevant," an illustrative view of nuclear power?

GOURLEY: No, I haven't.

GOFMAN: Remind me to give you a copy before we break up today.

GOURLEY: Okay.

GOFMAN: In that book, [which] was written in '79, I said, in this talk about things that are done in violation of Nuremberg principles, [that] I thought I was a good candidate for [the] Nuremberg trials.

GOURLEY: Oh, really?

GOFMAN: I suggested that on these grounds: I said in the mid-'50s I had been such an enthusiast for technical development that I resented anybody who wanted to stand in the way of technical development until they had proved there was something bad about it. That talk was during that period, and soon thereafter I gave a lot of thought to it. My God, that's the worst possible position I can imagine! I said [that] I thought that giving that talk and the position I took-that you don't interfere with technology, unless you can prove the opposite-it was a good basis for having a Nuremberg trial.

GOURLEY: Do you think a lot of scientists [during] the '70s or even through today, still have the view-

GOFMAN: Absolutely, absolutely. They virtually think that it's the public duty to prove they're being harmed-not their duty to prove it's safe. And I think just the opposite. But I [have] thought the opposite from late '57, after that, but you're absolutely right about that talk. It was the most senseless position to have that I can imagine. That's why I wrote about it in 1979, as the basis for a Nuremberg trial. I've given talks on that subject, too. The place where I've been stupid; it's just really amazing.

GOURLEY: Are there any other places where you've been stupid?

GOFMAN: Probably, probably. I can't imagine all of them but there must be some others. You know, there was no big flack about that. The regents were worried about that and I did talk to the board of regents. I said, "Look, I don't believe a case has been made." You don't really realize that things were not coming along very fast at Hiroshima [and] Nagasaki. Leukemia had come along; they were already pretty sure in the '50s about the excess of leukemia.

(v) Linking Radiation to Breast Cancer (1965)

GOFMAN: But do you know that by 1965, not a word had come out of the Atomic Bomb Casualty Commission on breast cancer, which we now know is [the] most sensitive [tissue] to radiation[-induced cancer]?

A doctor in Nova Scotia by the name of Ian McKenzie published a paper in the *British Journal of Cancer*, saying, "I had a lady come into my office with a breast cancer. [It] looked to me as though she'd had a lot of radiation on the skin over the breast cancer. I asked her about it [and] she didn't know of any radiation."

[It] turned out [that] the lady had been in a TB sanatorium 15 or 20 years before. The then-leading therapy of tuberculosis was to inject

air into the space between the chest wall and the lung. That's called pneumothorax, air in the thorax. The idea was to rest the lung. It was an extremely important technique because people who didn't get their lung at rest where the parts of the lung were already eaten up by TB, even though they didn't seem sick, continued to spew out tubercosid,⁴³ but when they had that rest of the lung, the two parts came together and they stopped spewing out tubercosid and went home instead of going to the graveyard. So the treatment by pneumothorax was the leading therapy for TB. There were people who looked like they were going to do fine before that, and went on to die, because it wasn't available to them before about '27 or '28.

So she had this pneumothorax treatment and had been fluoroscoped⁴⁴ 200 times. She never thought she'd had radiation; she'd been fluoroscoped 200 times!

GOURLEY: On her feet?

GOFMAN: No, on her chest, because they wanted to see if there was still air left from the previous injection and do you need a new one. So they were just checking these people by fluoroscopy, and that's where she got a hell of a big dose of radiation. So McKenzie went to the sanatorium records, pulled out the records of about 800 women-it was about 570 who hadn't had the treatment and a few hundred that had-and showed that there was about a 15- or 20-fold excess of breast cancer.

As a result of that, in '65, the people at Hiroshima/Nagasaki, said "Well, McKenzie [found] this, we ought to find something here." Then they looked and they published that they were having breast cancers in Hiroshima/Nagasaki from the radiation.

GOURLEY: Now, was this data gathered by DBM at Livermore as well?

GOFMAN: You mean the Japanese data. That came directly out of ABCC [(Atomic Bomb Casualty Commission)]. We were looking at things like that. So I'm trying to point out to you everything wasn't too well known at that time.

GOURLEY: People didn't always know where to look?

GOFMAN: I don't know why they didn't, they weren't up to speed on looking [at] that thing out of Japan. But they corrected it. After that, they looked pretty hard at the breast cancer and they [did] some nice work on breast cancer in Japan.

(w) Conflict With the AEC on Low-Level Effects of Radiation (1969)

GOFMAN: At any rate, I was coming up to '69 and the talk that I gave. Tamplin had the invitation to this nuclear power thing. I'd given the talk at the IEEE, as I say, an extremely conciliatory talk: not a wild, raving, manic thing at all, which I'm capable of doing.

Anyway, Mike May calls me over and says, "Jack, the AEC is upset

about your talk before the IEEE." I said, "Why? [It was] such a reasonable talk." He said, "No, no, it's not what you said. It's the fact that you didn't notify them in advance of giving it and they get flack from newspaper people and so forth." So he said, "Would it be agreeable with you, whenever you or Tamplin or somebody is going to give one of the papers on the health effects of radiation," (which was our mission; just doing my job), "would you consider sending a copy to the AEC in advance?" I said, "Sure, that's fine." I said, "They're not going to censor it?" He said, "Who would stand for that?"

The next paper up was Tamplin's for that nuclear power symposium. We gave a copy to Mike May and sent a copy off to Washington.

A couple of days later Tamplin walked into my office. Threw down this paper on my desk. He says, "Look at this!" I looked at it. Everything was lined out what he wanted to say-the only thing left were the prepositions and conjunctions. I said, "[Did] this come back from Washington?" He said, "Hell, no! This is Roger Batzel," who's Mike May's right hand. He told me that if I want to, go ahead and give the original talk at this meeting, [but] I can only go as a private citizen, not as a member of the Livermore Lab. I cannot use Laboratory secretaries to type anything and I must pay my own expenses for the travel. Ordinarily, the Lab loved it when we would go give a talk somewhere; it's publicity for the Lab.

GOURLEY: Especially if one said there were no harmful proven effects.

GOFMAN: Right. So all these things that [we] can't announce, even being a member of Livermore. I just couldn't believe it. I couldn't believe it. I called up Mike, and I believe that session he came over to see. I said, "Mike, what in world's going on? I agreed to have this stuff we are doing seen by the AEC in advance. But," I said, "censorship like this!"

[Mike said,] "Jack, why don't you be realistic?" That was the first time Mike had ever said anything like that to me. You know what I told him? You know, when I told Totter to go to hell about that Sternglass thing, he didn't say a word about it. He said, "Why don't you be realistic: you just can't put out stuff like this."

I said, "I'm very realistic, Mike. If this is going to stand, I'll tell you what I'm going to do this afternoon. I'm going to call up the guy who's organizing the [AAAS nuclear power] symposium and I'm going to tell him that Tamplin can't come to the meeting. The reason he can't come is the Livermore Lab is a scientific whorehouse. He's being censored by the Livermore Lab."

Mike said, "Jack, you know we've known each other a long time." He says, "Why don't you go home and sleep on it and we'll talk tomorrow." I said, "Well, I'm telling you what I'm going to do, Mike." He said, "Yeah I know, but just sleep on it."

The next day we got together and I had already called the guy [from the AAAS nuclear power symposium] and told him exactly what I told Mike that I was going to tell him. He was very upset because he was going to be the chairman of this meeting. He didn't want to have to

read my letter to the assembled meeting saying that Livermore Lab is a scientific whorehouse.

So Mike said, "You really did that?" "Yeah, but it's just what I said I would do, Mike." He stormed out and we never talked for about 9 months after that. Well then, the rest is sort of history.

(x) Testifying Before Congress on Radiation Effects

GOFMAN: Senator [Edmund] Muskie [(D., Minnesota)] was holding some hearings on the underground uses of nuclear energy. His aide in Washington had asked if I'd come and testify. He didn't know about this whole paper I'd given, so I essentially upgraded the thing Tamplin and I had done and went back to testify before Muskie's committee. Senator Mike Gravel was there from Alaska and he turned out to be a real friend. Muskie was very friendly. But then it was pretty sure we'd better call that number 13,000 cancer deaths, not 16,000. We'd been wavering before that.

Ed Bowser was the Secretary on the Joint Committee on Atomic Energy. This is also very important for you two to know. The Joint Committee on Atomic Energy was as aristocratic as you can get. Ed Bowser came into Muskie's hearing room and he said, "Can you come over to the Joint Committee Headquarters? The Chairman wants to see you." That's Chet Holifield, [U.S.] Representative from California. The Chairmanship of the Joint Committee on Atomic Energy alternated: one session [it would be a] Senator, next would be a House Representative. They went back and forth. Holifield was the chairman.

So I said, "Sure, I can come over," and said, "Tamplin is in town with me." "Oh, bring him along, by all means." So we went over to the Headquarters and went through secret passages in the Congressional Building. They were up there in the Green Room; all very secret. There were a number of people from the Joint Committee staff there. I remember one guy by the name of Dr. Graham; he was friendly.

Chet Holifield and Craig Hosmer of the Joint Committee came in and Holifield turned to me and said, "Just what the hell do you two think you're doing, getting all those little old ladies in tennis shoes up in arms about our atomic energy program?" I said, "I don't think we're doing that Mr. Holifield. We were doing our job." This guy Graham said, "Mr. Holifield, these are two of our distinguished scientists from the Livermore Lab."

Holifield said, "I don't give a damn who they are: They are hurting the atomic energy program." He said, "listen, I've been told that if we gave everybody in this country one hundred times the dose that's allowed, nobody would be hurt." I said, "Well, Mr. Holifield, that doesn't agree with anything we've learned about this question. That sounds like a horrible dose. Where did you hear this?" He said, "The Atomic Energy Commissioners told me that."

I said, "Well, Mr. Holifield, I'll look into it. I'm surprised, but that

doesn't square with our findings." He said, "That's what they told me." Then he turned to me and said, "There are people like you who have tried to hurt the Atomic Energy Commission program before. We got them, and we'll get you." He didn't mean to kill us, but he meant they could take care of our reputation. That's a long story.

HEFNER: This is a Congress person?

GOFMAN: Yes, the Chairman of the Joint Committee and the Representative in the House of Representatives-from California, no less.

HEFNER: Threatening you?

GOFMAN: Yes. We went back on the airplane, [and] I said to Tamplin, "Where the hell do you think the commissioners got this stuff? Is Chet Holifield telling it straight that he was told that 100 times the dose wouldn't hurt anybody." He said, "I don't know." So we went through everything we could and we found one thing that could be the basis of it.

Namely [that] Robley Evens at MIT was continuing to [study] the dial painters. He had published stuff saying he saw no harm down under a thousand rads-not a rad, under a thousand rads. The commissioners were obviously referring to Robley Evans [and] the dial painters [from the 1920s].

There were plenty of things wrong with this thousand-rad safe threshold. [(My later studies show there were many allusions to 500 or more rads being "safe"-in addition to Robley Evans'.)] But there was that basis [and others], and they did misinform Holifield. By then, newspaper people were getting interested in the whole thing. CBS [television news] decided to have [a] week-long set of five morning sessions on radiation hazards and we were on five of them, and commissioners were on [some of] them.

What we had said in our paper was, "We ought to think of cutting the allowable dose tenfold," and the AEC said this was awful. The [AEC] said, "We're never going to give people even one millirad, let alone a hundred seventy millirads." I said, "Then you got no problem; we're suggesting cutting it to 17." Then they would turn around and say, "We don't know if that's enough of a cushion." They didn't make any sense at all.

Everybody who was anybody realized the Atomic Energy Commissioners were getting their feet in deeper and deeper in this whole controversy. Glenn Seaborg has written a book recently, [in] which he says exactly that. I'll tell you about that in a little bit.

That's when things really started happening. We were on these TV programs. CBS morning show, lots of newspaper articles and the *Saturday Evening Post*, and somebody asked John Totter what he thought of our work. He said, "It's ludicrous, just nothing correct about it at all."

GOURLEY: I read criticisms: John Gofman's sloppy work, bad statistics. What you do have to say about these things?

GOFMAN:

[What I have to say is that whatever rubbish you are reading is undocumented bull_ _ _ .] It just became a war. It just became a war as far as they were concerned. They were going to destroy us.

A couple of interesting things happened. I wrote a letter to Glenn Seaborg. I said, "Glenn, you[ve] got some rotten apples in that barrel. Your staff attacks on us are going to hurt you. It's going to hurt the atomic energy program. It's going to hurt us. It's just going to discredit everything." I said, "I think you ought to do something about it. We are doing exactly the job we were assigned to do."

He wrote back and said, "Look, the way we do things is, we don't reach down into the departments. You're going to have to solve this with John Totter." There was no solving it with John Totter. He was continuing to attack us, as were others in the Commission. Glenn did not attack publicly; this came later.

I heard back from the Joint Committee on Atomic Energy. Bowser called me up and said, "The Chairman is inviting you to a hearing. We're going to discuss your work." That was the plan. The standard plan for destroying you was to hold a hearing where people from all over the Commission come in and address the issues that you are raising.

I just realized we'd better have a lot more ammunition. Art Tamplin and I worked our butts off and we did about 14 separate papers. They're referred to as the "GT" [(Gofman-Tamplin)] documents and they were all eventually published in the *Congressional Record*.

So, we had 178 pages of testimony. I said to Bowser, "Tell the Chairman I need about three hours to testify." He said, "Three hours, we've never given anyone over an hour!" So I said, "I think I need three hours; I have a lot to say." He said, "Well, I'll talk to the Chairman." He comes back, calls me up, and says, "You have one hour. The Chairman says that if there's more that you have to talk about, we'll schedule some more hearings."

So we had 178 pages of scientific stuff and I took it over to the Information Division at Livermore Lab. They nearly had a conniption fit. They had heard all the flack about this. Roger Batzel came running over to see me. We've always maintained an open dialog in spite of everything. He said, "What's going on here? Why do you need this 178 pages of stuff and you want 250 copies?" I said, "Yes, Roger. Chet Holifield has invited me to speak at a hearing of the Joint Committee," and I said, "If you don't want to do it, I'll call Holifield's office and tell him the Lab has decided not to permit me to prepare this material for you, Mr. Holifield." He said, "Oh no, no, don't do that. We'll do it." So I got the 250 [copies], of which I sent 100 to scientists around the country, thinking it might be a good idea to have a copy out in some other people's hands.

We went in and I presented the thing. I thought they were going to just tear it apart. Holifield said, "Well, you submitted so much material. We haven't had time to go over it. We'll call you back. Do nothing until you hear from me." So we never heard again from Holifield.

(y) Gofman and Tamplin Ostracized

GOFMAN: But there was an important thing that had happened in the interim between the day we first met Holifield and when we went back to Washington to present this stuff at this hearing. Even though I had given up on my official appointments at Livermore and I was just in the lab; I was very happy doing research.

GOURLEY: What was your research based on at that point?

GOFMAN: Chromosome studies. During the period where I had been head of the department and Associate Director of the Lab, I had mornings open to anyone who wanted to come into my office: had all kinds of problems, needed another technician or their wife was sick, they needed this, or [one] needed that, or they wanted to talk about their research. It was Grand Central Station [un]til noon. Twelve o'clock noon, I went into the lab to work and I never would see people. I wanted to work.

During those several weeks, both Tamplin and I were working until 11, 12, or 1 in the morning every night to try to get these papers ready for the Joint Committee. I noticed the most interesting thing during those weeks. Nobody ever came into my office again, nobody. From Grand Central Station to a desert. Nobody needed to see me at all.

So, I just worked in the lab. I worked on this preparation. But on two occasions in the evening, two different scientists stuck their head in my office. I can paraphrase only what they said, not exactly: "Look John, I looked over your calculations on this whole flack about radiation. I agree with you. I don't see anything wrong with your calculations." I'd said, "Great. Tamplin and I have a lot to do. How about you doing this or that on some other part of it?" And the answer from them was essentially this, "Look, you're a professor in the University, you don't have anything to worry about. If I help you, they'll slice my throat."

I said (to myself), "Look, this is a slave empire. If you never find radiation harmful, [or if] you can find huge doses harmful, nobody worries you. That doesn't worry [the] Commission. They can see that. But start to find that low doses are harmful and they're going to fight you every step of the way. They don't give two hoots in hell that it kills millions of people or billions. They're going to fight to preserve the empire. The bureaucratic empire and the bureaucrats cannot tolerate radiation to be harmful."

(z) Benefits of Radiation Therapy and Ethics

GOURLEY: Now, one thing confuses me terribly about all this, and I'm not a scientist and I'm new on this: You, yourself, said that [there are] medical benefits in certain cases and certain specific cancers and that sort of thing. How does the line fall between where it can be therapeutic, [and] where it's harmful?

GOFMAN: Line falls at one point. I have no difficulty with radiation therapy being beneficial in certain situations. I have no difficulty with diagnostic radiation, finding something important out [from] a diagnosis that can [save] a person's life.

GOURLEY: Which diagnosis are you speaking of here?

GOFMAN: You can talk about the possibility of pneumonia that's not appreciated or some mass in the abdomen or something like a cardiac lesion. *I have never in my life said people should not have an x ray.* I have never argued against radiation therapy. I talked to you earlier about some places where I participated in radiation therapy and I know people were benefited.

GOURLEY: Right.

GOFMAN: That is a world apart from what your problem is in this whole thing. Where I stand on it is, you voluntarily, you accept a risk for a benefit to you or your child or your mother or father if you discussed it with them. That's not what I'm talking about. It's when somebody says you shall be allowed to get x units of radiation as a member of the public without any benefit to you: "Society will benefit."

That's immoral, it's illegal, and it's being done every day. I just think it's just illegal and it's a violation of the Declaration of Independence. It's a violation of Constitutional rights and none of the medical ethicists are saying a goddamn thing about. I'm very critical of medical ethicists for that.

I've written some things down for you on the fact as a polluter or potential polluter, and radiation is one pollutant. If you say that your pollutant is safe when you know it is unsafe someone can get hurt, and thereby you try to get your pollution accepted at some level, you are guilty of random premeditated murder. That's a crime. If you say you don't know whether it's safe, then you are guilty of a different crime, that's a Nuremberg crime, experimenting on people. So as a polluter you've got to come clean. There is no basis for saying it's safe when you know it isn't. That's a lie and a fraud and a crime.

GOURLEY: Now, what about some of these spas and stuff?

GOFMAN: There are spas where you can go breathe radon and you can go get yourself a good case of lung cancer. There are many people who believe in them and go to them. *Stopping people from being nutty is not my function in life.*

GOURLEY: So you don't think there's a therapeutic benefit there?

GOFMAN: There may be; I don't think so. Let me put it this way. I was telling you a little earlier about these doctors writing papers [stating that they] treated 500 cases with 92 percent success. *Now people say that disease never existed!* This thymus thing is not believed to have existed. I can pull out paper after paper of leading institutions where doctors are saying I had success in one hundred to five thousand patients. What the hell are they talking about? Just like your people

who go to spas, I think.

So, I don't know what to say about it other than-did you ever see David Copperfield, the illusionist who could make a train disappear? Well, I think there are a lot of David Copperfields around in the world having illusions.

But my dividing line in answering your questions is: *What you do [to] yourself is your business*. You chose to take a risk; that's okay with me. You should be told the truth about what the risk is. I don't think it would be fair of me to tell you, "Hey, look, Karoline, you go ahead and have this radiation treatment, it will never hurt anybody." That's false; that's terrible on my part. But if I tell you what the hazard is, or we don't know the hazard-there may be *no* harm, there *may* be. If you then want to do it, I don't believe it's anybody's business.

The only place where I deviate from that is that if you harm yourself and then have children and can pass that harm onto your children; that's unfair. I don't have any difficulty with people doing hazardous things. There are, after all, astronauts. Nobody's going [to] say being an astronaut is a safe job. Yet, they do it, and I think that's their privilege.

(aa) Concern Over Low-Dosage Harm; Public Acceptance of Nuclear Energy

GOURLEY: How much dosage do you think you've accumulated?

GOFMAN: About 100 rads. People would say, "How come you're living?" If you read any of my papers, then you'd know why. Even at a 100 R it causes a lot of harm and a lot of cancers. More people would escape the harm than would get harmed on a statistical basis.

But I consider myself pretty lucky. Two guys there at Rad Lab, Joe Hamilton and Bert Low-Beer, weren't so lucky. They were both guys who took a lot of radiation and both died of an early leukemia. So, I feel that every decade I pass is just amazing to me. I didn't expect to live.

I consider the whole approach on permissible doses of the poison is illegal, dishonest, and I can tell you the proof of my position is really what the vast majority of the public believes is this. The vast majority of the public does not know I exist. But I can tell you that proof that the vast majority of the public would agree with me. It's the fact that AEC and ERDA⁴⁵ and the Department of Energy are desperately frightened by anybody knowing that there is no safe dose of radiation. *Because why are they are frightened? Because they know damn well you cannot sell poison to the public.*

GOURLEY: Now-

GOFMAN: Just tell people, "But we're going to give your children radiation. Some of your children are going to have a defective heart and some of them are going to come out with only part of their brain. But think nothing of it, our atomic power program is great." You sell that;

where?

GOURLEY: Now you've been quoted on a lot of things: "There's no safe dose."

GOFMAN: [You bet I have, and I am correct]. There is no safe way.

GOURLEY: What about [natural] background [radiation]?

GOFMAN: What about it? I've talked about background a lot. Background is roughly one-tenth of a roentgen per year of external background, leaving aside radon. It is my opinion, background is causing just as many cancers and genetic injuries as I've calculated for *any* man-made radiation. Look, the Lord did not say, "I'm going to set in a certain amount of background that is okay and make it safe." I know of nothing that came down from the mountain that guarantees that safety. I believe background radiation is just as harmful as any other kind of radiation.

By the way, I'd like to address another point. What about genetic repair? We do indeed have repair systems. DNA repair, chromosome repair, those are real things that do operate. *John Gofman never said repair doesn't operate.* What I've done in four chapters of my 1990 book (a copy of which I'd be happy to give you), in chapters 18, 19, 20, and 21, I finally was able to do a thing I've been thinking about for 20 years, namely to test the idea of whether it's possible for there to be a safe dose.

I concluded, by nuclear track analysis through cells, that cancer has been produced down as low as one track through a cell, [one] radiation track. Karoline, there is no lower dose than one track. Either [you] have a track going through a cell or you don't. There's no little bit of it. Since cancer has been produced with one track, there is not any safe dose and it can't be.

People said, "What about repair?" Great. The only difficulty about repair is, a lot of the damage that radiation does is repaired. But there's a certain [amount] of damage *not repaired*. A certain amount of the damage is unrepairable by the mechanisms we have and a certain amount of the damage is *misrepaired*.

All [of] the cancers and all the genetic injuries perceived are due to those three residual things: the unrepaired, the unrepairable, and the misrepaired injury. I wouldn't be surprised if something like 90 percent of damage is repaired and we [are] darn lucky to have repair systems.

Background doesn't phase me: *There is no safe dose.*

GOURLEY: You think it would surprise some of your critics to hear you say that the background doesn't phase you?

GOFMAN: You might say the argument doesn't phase me. I didn't mean that background is all right. No, it doesn't phase me to have you say, "What about background?"

GOURLEY: I'm just learning.

GOFMAN: It's a very good question. I've been asked it many times. I can show you in an extended thing how it is so easily possible to get the wrong answer on questions like this. Even well-meaning people can get the wrong answer. A certain number of times if you set the experiment up, you're guaranteed to get the wrong answer. So it doesn't surprise me that there are a lot of sincere people out there who really believe I'm wrong.

GOURLEY: For example?

GOFMAN: Sincere people who really believe I'm wrong? I really don't know any of them.

GOURLEY: Are there any particular judges?

(bb) Attempt to Discredit Gofman's Testimony in Johnston Versus U.S.

GOFMAN: I'll tell you something. First of all, William Douglas, former Justice of the U.S. Supreme Court, wrote me some very complimentary letters about my work. He was a great fan of my work; thought I was right on track.

I was a witness in the Karen Silkwood case. Judge Frank Theis was the judge. It was a jury trial. As you know, we won that case. Judge Theis said some very nice things about me. I was back in Wichita, which is his home. He took me out to lunch. Then we had a talk about things like that.

Then Judge Jenkins in Utah, the downwinder trial, said some very nice things about me. There's Judge Patrick Kelly in Wichita, who said some extremely *unnice* things about me. I'll tell you something about that case.

GOURLEY: I'd love to hear what you have to say about that case.

GOFMAN: I have a lot to say about that case. There's going to be some developments I hope within the next year. I can only say that at this point.

GOURLEY: Can you give us a hint?

GOFMAN: Yes, I'm going to have something from some major judges of the Federal Court say something about that.

HEFNER & GOURLEY: Oh, good.

GOFMAN: But there's a very important thing about, Judge Kelly's [decision] in *Johnston vs. the U.S.* It is so scary that you might think you're living in Adolf Hitler's Nazi Germany, not in the United States. Let me tell you what that is.

I was in [to see] Karl Morgan,⁴⁶ the eminent physicist, the man that I just think so highly of. We were both witnesses in that case. I'm sure you know the scathing condemnation that Judge Kelly provided both Morgan and me. I think Earl Johnston, I think he took off on *him*, too.

Any rate, I testified and I went back to San Francisco and about four weeks later, Ken Peterson, the lawyer, called me up and said, "John, [when] you were being sworn in and examined, we used the fact that there's a plaque which you said was given to you by the Atomic Energy Commission for the discovery of 233U [and] its fissionability and that it's on the door of the room where you did the work." I said, "Yes, what about it Ken?" (I have Glenn Seaborg's book showing a picture of the plaque. If you've never seen it I'll show you the book in the next five minutes.)

Peterson asked, "you think that it could be that the plaque's been taken down?" I said, "Ken, are you crazy? That plaque is not going to come down until that building comes down." I said, "It's sitting just 10 feet away from the plaque given to Seaborg. That plaque has to be there."

"I didn't think we'd be calling you to talk about this, but I think we'd better." Peterson said, "You're pretty sure the plaque is there?"

I said, "Of course it's there Ken. If you want me to I'll go over and take a picture of it," (which I did by the way)-"What makes you think the plaque isn't there?" He's in Wichita and he thinks my plaque isn't there anymore.

So he said, "Well, they brought up Jacob Frabikant, member of the BEIR⁴⁷ committee." Jacob Frabikant is from Berkeley.

HEFNER: He passed away about two years ago.

GOFMAN: Frabikant?

HEFNER: Jacob Frabikant.

GOFMAN: I didn't know that.

HEFNER: Yes, we'll talk about that later.

GOFMAN: I didn't know that. Anyway, though I don't speak about the dead, I'll have to. Peterson said they brought Jacob Frabikant in, and this is the conversation, paraphrased:

"Dr. Frabikant, are you on the Berkeley Campus?" He said, "Yes." "Have you ever gone by Gilman Hall?" He said, "Yes." "Have you ever gone into Gilman Hall?" He said, "Yes." He said, "Have you ever been on the third floor of Gilman Hall?" Imagine this, Jacob Frabikant had about as much business on the third floor of Gilman Hall as I had on the moon shot. He said, "Did you ever see a plaque on any of the doors on the third floor of Gilman Hall?" He said, "Yes." "What did the plaque say?" "It said, 'For the discovery of plutonium.'" "No other plaque there?" He said, "Not that I saw." "Thank you."

So here somebody came in, a professor at Berkeley, who had made a liar out of me. There is no plaque there [is, in effect, what Frabrikant had said in testimony.] So I said, "Well, Ken, this is serious. I'll go over."

O'Connor and I went over, [and] took a picture of a calendar and the plaque on the wall. I'm going to get that picture of the plaque, you ought to see it.

The reason I said you [Karoline] should really worry about this is where you're living [Washington, DC]. Glenn Seaborg [was] the Chairman of the Atomic Energy Commission, who wrote a press release for AEC in 1968 saying what a great discovery the 233U work was and that they were awarding this bronze plaque to the discoverers for this [work]. The U.S. Department of Energy and the Justice Department, trying a case in [Wichita], act as though they don't know that a different branch of government has announced this "fifty quadrillion dollar" discovery. There is your plaque in Glenn Seaborg's book. Fourteen years after this book, they frame up this thing in Wichita, and *you* asked *me* to comment on Judge Kelly and the whole shebang?

Sure, the plaque's on the door. It's not coming down until the building comes down. Then they'll put it somewhere else in the Chemistry Department. That's the kind of tricks they were up to. Judge Kelly's decision doesn't phase me. But god, they spent a lot of public funds sending out thousands of copies of Judge Kelly's decision. Where did you get a copy?

GOURLEY: From the law books.

GOFMAN: They sent it out all over the country. I'll tell you about that sort of thing. In Judge Jenkins' courtroom in the Utah case, there was no jury, because the trial was against the U.S. government. Anything against the U.S. government, you don't get a jury. Judge Theis in the Karen Silkwood case, there was a jury. Judge Theis and Judge Jenkins both are just elegant men. If Theis hadn't had a jury in the Karen Silkwood case, I think we would have won based on Judge Theis. But we did have a jury.

But when the case came up of *Johnston vs. U.S.*, what you may not know from the law book, there were 19 civilian defendants, companies, in addition to the Government. Based upon my deposition and Karl Morgan's deposition, they[, the private companies,] plunked down a million nine hundred thousand dollars, not to go to trial. Did you know that? A million nine hundred thousand dollars, [be]cause they thought the evidence was that good against them. When Ken Peterson called, it was the day before they were going to go to trial, but they all settled except the U.S. government.

Ken said, "Well, now let's try it against the U.S. government." I said, "Ken, are you crazy? You just got a million nine hundred thousand dollars put in your lap. Anytime you go to court with a judge and no jury, you're taking an awful big chance. It happens occasionally, you'll come out all right, but don't count on it. I'd love to be out of this case,

now that you're alone against the government, because you lose the jury. If one of those civilian defendants had stayed in, we would have had a jury in Wichita. Patrick Kelly wouldn't be able to [do] what he did. But once you don't have anybody but the Government, no jury."

[Ken] said, "I feel I have to do this for my clients. They think we ought to go after the Government and we [have] Judge Kelly's decision, including a frame-up using Frabrikant as their foil."

(cc) The Need for Cultural Change at the Department of Energy

GOFMAN: I thought a lot about things like that, the Government. A lot about the slave empire and the fact that this has got to be changed. When I was talking to [Energy Secretary] Hazel O'Leary, much of those two hours that we talked, I said you [need to] change that culture, so that scientists are not afraid to speak out. All they have to do is to have a Gofman and a Tamplin and 10 years later a Mancuso and Alice Stewart and 5, 10 years later a Greg Wilkinson, and that's enough to keep all the other slaves in line. You have got to change that culture, which is bad for the country."

She said, "I'm listening to you." I think she has spoken out a lot about dissidents and about whistleblowers. Hazel's been right on it.

This year I thought about it in January and February. I called up Glenn Seaborg and I said, "Glenn, I think you ought to issue a public apology to me." He said, "What are you talking about?" I said, "I think that to go on with the Atomic Energy Labs, DOE having scientists feel and act as though they are in a slave empire-beautiful facilities, but no freedom to really speak up-is a bad thing for science. It's a bad thing for humanity and it can lead to the wrong answers, getting credence on health effects that could eventually hurt millions, hundreds of millions or billions of people. You've established an excellent record as a scientist, but as a human you don't come across very well."

Then he said, "Well, why do you say that, Jack?" "Because you've allowed this thing to go on in your tenure at the AEC," [I replied]. We talked about it and I'll come back to that. "Well," he said, "What do you want?" I said, "I just want an apology from you and Mike May and Roger Batzel for the fact that you didn't back us when we did our job. If all the people working in the Department of Energy Labs saw you do this, it could change the culture inside those Labs concerning being afraid to speak out."

He said, "Well, that makes some sense; did you talk to Roger Batzel or Mike May?" I said, "No, I started with you, figuring that if you didn't do it, they wouldn't. I'll talk to Roger Batzel next." He said, "Fine; as a matter of fact the AAAS meets here in a couple of weeks. We can call a press conference to do it." He was telling me we could call a press conference for this.

So I called up Roger Batzel and I told him the same thing and why I wanted it. I said, "It doesn't mean anything to me personally, but it could have a salutary effect." He said, had I talked to Mike May? I

said, "No, I haven't." He said, "Well, I'll talk to Mike May." They were supposed to get back to me.

About two days later, Glenn Seaborg called and he said, "Roger and Mike won't do it." I said, "What about you?" and he said, "No, I can't do it by myself." I said, "Well, it's just pretty bad, Glenn, you really need it. You really need to do something for humanity." He said, "Look, Jack, I treated you very fairly." I said, "You did when I worked with you on uranium-233. I did all the work; you never questioned my honesty, you signed your name on those papers. When I did the work on 233U and plutonium, you were quite happy to take some of the credit for that. You never worried about it. But when I did the job you asked me to do, you didn't back me." He said, "I did back you." I said, "Really, you should know what the other Commissioners wanted to do to you." He said, "I opted for lesser sanctions."

At that point on the telephone, I blew up. I said, "Goddamn it, Glenn, you opted for lesser sanctions? I did the damn job you asked me to do, the job I said I would do, and you opted for lesser sanctions when you should have [been] praising me?" I said, "You don't really live in the real world, Glenn; you live in Ronald Reagan's world." He said, "What do you mean by that?" I said, "There was a world as it really was and there was a world as Ronald Reagan would like it to be." I said, "You're the same way: there's the world as it really is and the world that Glenn Seaborg would like it to be!" He said, "That's very harsh, Jack." He sent me a copy of his new book: "To John Gofman with my esteem and affection"-a whole chapter about darkening clouds and the trouble we caused!

GOURLEY: What sort of marks would you give it for accuracy?

GOFMAN: Not good. I called him back and said, "You've got some mistakes in it." It wasn't accurate, but it's not bad. In a lot of ways, he did say that we presented our case well. He just thought that [it] was okay [that] he opted for lesser sanctions. You know what the sanctions were, don't you?

(dd) AEC Responds With Sanctions to Gofman's Public Dissent (1972)

GOURLEY: Tell us in your own words.

GOFMAN: Well, actually, in 1970, during most of these TV battles and battles in public meetings and [by] 1972, they had already taken away Tamplin's 13 people. When they were asked by the press, they said he didn't want them anymore. A total fabrication, total fabrication.

In 1972, Roger Batzel came to see me. He said, "Jack, I have something I [have] to tell you." "What's that, Roger?" "Last year in '71, the Atomic Energy Commission came to us and said we should take away your research funds." [The] \$250,000 a year [that] I was spending.

GOURLEY: This was on the chromosome work?

GOFMAN: Right. He said, "We told him that we disagree with your position on nuclear power and hazards calculations, what you're doing in that. We think your chromosome work is very good and we were not going to take away your money." He said, "They went away and they've come back now and said, "Take away John Gofman's \$250,000 or if you don't do that we will just delete \$250,000 from the Lab budget and you can lose 13 other people." So I said, "Roger, that won't fly."

I made a mistake: I should have taken it up at the academic senate. I should have made it a hell of a big issue with the University. I didn't. I don't know why I didn't, because I think we could have really blown the thing wide open at that point.

[Instead,] I said, "Look, nobody's going to lose their job because of me. I [will] see if I can get the money from somewhere else. If I get the money from, say, the National Cancer Institute, can I take all the equipment?" (I had an awful lot of gear that I was using, high-powered gear.) "Can I take that into Berkeley with me, when I go back to my full-time professorship?" He said, "Sure, that's no problem."

So I went into Washington and got to see Frank Rauscher, who [was] the head of the National Cancer Institute at that time. One of my former students, who was an Associate [Director] of the National Cancer Institute, arranged the meeting. I asked Rauscher, "You know about my conflict with the AEC?" He said, "Yes." I described the [chromosome] program and he said, "That's exactly the kind of program we need. We have some work going on at Yale on breast cancer, but this would fit in very well, if you want to do the chromosome part of it." I said, "I'd love to." He said, "What would it cost us?" "About \$250,000 a year is what I need to do the work." He said, "I'm very optimistic but I have to just let you know in a few weeks." I left and saw Roger Batzel. [I told Roger], Rauscher was pretty optimistic and he said, "Fine; let's see what happens."

Four or six weeks passed and I heard nothing. I just wrote a very brief note to Rauscher saying there's no hurry and nothing desperate that needs to be done, but I [would] like to know how things are coming on that possibility. He didn't answer.

I got a letter from a third-[echelon] deputy, saying "Thank you very much for your inquiry. The work you're suggesting is not in the mainline interest of the National Cancer Institute, but if you ever have any other ideas, please let us know." What obviously happened was Rauscher must have talked to some people about this possible grant and I think they probably said, "Hey, this guy is giving the Atomic Energy Commission fits, what do we need him for?" So I never got the grant.

When I got that letter from that deputy, I told Roger Batzel and dissolved my program. We did [it] that day. People were reassigned to other things.

They didn't want to fire me from the Lab; [it was] just something they didn't want to get caught doing. So Roger said, "Well, you know you're welcome to stay." I said, "That's very nice, but there's not

much reason for me to stay without my research program." I'd come all the way out here to Livermore-for what?

I said, "I need about six months to clear things up and then I'll go back to Berkeley full-time." In fact, they found me some nice rooms in Building 90 on the hill: "You don't even need to stay the full six months; we'll set you up in Berkeley for that period." Then I moved down to Donner after that.

(ee) Return to Berkeley

HEFNER: Did you come back to LBL [Lawrence Berkeley Laboratory]?

GOFMAN: Building 90 for those four or five of those six months.

HEFNER: How did the rest of the community treat you-say, the old scientists and the regents?

GOFMAN: I never brought it up with the regents. I think I made a mistake not to bring it up with the academic senate and the regents. I think I would have gotten a very fair hearing from them. I don't know why I didn't do that. LBL, I have a lot of friends at LBL. Ed McMillan and I did not end up friendly. Ernest Lawrence and I were very good friends.

But Ed was angry with me because in the days when Charlie Schwartz was raising all kinds of hell, during that whole thing about the university reconstitution, they wanted to hold an outdoor rally and speech at LBL on the hill. Charlie invited me. I said, "Sure, I'll talk." Ed called and said, "You can't talk." I said, "Ed, you're just behaving like an ass, I'm going to talk." He said, "you just shouldn't. I forbid it." I said, "It doesn't matter." I did talk, but we never got past that. I like his wife, Elsie.

HEFNER: Why did Dr. McMillan say that? That's pretty contrary to his own politics?

GOFMAN: I don't know; I honestly don't know. I couldn't believe Edward McMillan. I knew him quite well. I just couldn't believe Ed McMillan was telling me that. John Lawrence cooled off considerably toward me.

HEFNER: He took a turn to the right during the free speech movement.

GOFMAN: Yes. John never was outright unfriendly to me after the controversy, but he was cool.

HEFNER: How about Hardin Jones?

GOFMAN: We stayed friends. I didn't see Hardin too much. He didn't agree-

HEFNER: He also took a turn to the right.

GOFMAN: Yes. Hardin was so wrong on some of those drug things. We were quite friendly to the end. I've seen Helen since then. [Andrew] Tobias

and I were never very close. Tobias tends to be defensive of the atomic energy [establishment]: "It never hurt anybody." Tobias's coworkers like Graime Welch is a great admirer of my work. The Donner people, of course, are good friends of mine.

HEFNER: Eleanor Blakley?

GOFMAN: I don't know her. Alex Nichols is a very close friend of mine. I don't know how many people on the hill-I don't think that most of them even know about the controversial years, do you?

HEFNER: Yes, you think they do? By all means.

GOFMAN: Really?

HEFNER: Everybody knows.

GOFMAN: I didn't know that.

HEFNER: Yes.

GOURLEY: Back at Headquarters even, there's boxes and boxes of stuff.

HEFNER: Oh, yeah.

GOFMAN: Oh really?

HEFNER: It's a ton of written stuff.

GOURLEY: I have with me the memorandum when they were evaluating your chromosome program.

GOFMAN: They sent it out to their committee that [conducted the] evaluat[ion]. They also had an Inspector General's report on whether we were harassed. Did you know about that?

GOURLEY: No.

GOFMAN: The Inspector General of the AEC said he could find no evidence of harassment.

(ff) Reflections on Career Decisions

HEFNER: Given all of that, and given the subsequent years, would you do it all again? Would you take any different turns?

GOFMAN: That's a good question. By the way, I want to tell you one little thing. When the 50th anniversary of the plutonium [discovery rolled around], I got an invitation and I was a little delayed in replying. Glenn [Seaborg] called me up and said, "we didn't get your reply that you're coming. You are coming, aren't you?" I said, "Oh yes, I'm coming." "Well, we want you to sit at the head table." I did, and I gave a talk. It was like [old times].

GOURLEY: When was this?

GOFMAN: Two years ago. Two or three. The 50th anniversary of plutonium. The Chemical Potentials, the Chemistry Department has a picture of Arthur Wahl, Seaborg, and me standing in room 307, where we all did the work. So, you know, it's like nothing ever happened.

In answer to your question, Lori: There were so many accidents of life, like Juliette T. Brown [not] throwing me out of her office at Western Reserve University. It turns, [she] could have said, "No, no chance you're getting into med school," and I might have forgotten the whole thing and never have gone that route. There was deciding in the first year of med school to try to come out to California and study chemistry. Then this miscerebration in taking the Livermore assignment. You mean, would I do the argument again? The work at Livermore itself?

HEFNER: Yes, was a it a misstep for you to take that job? Sounds like your intuition already told you, despite all these reassurances?

GOFMAN: Yes, I think there was enough to say if you're going to have to count on those reassurances, don't do it. I think I'd probably, on that ground, would not have. I thought [it was] a little more than just a chancy decision.

HEFNER: It's also thrown you into international and national-

GOFMAN: This disrepute?

HEFNER: Into the whole controversy about these-

GOFMAN: Let me say this, Lori. I don't mean about [this] taping. This is just how I feel about these things as a person. I tend to try to evaluate my life, whether it's been worthwhile or not, and somehow it makes a difference to me whether I think it's been worthwhile. I feel very proud of the lipoprotein work. It was good work; we [were] castigated for that work, in case you didn't know it.

HEFNER: I didn't know it.

GOFMAN: All kinds of criticism by others in the field, largely jealousy.

HEFNER: Because that work is still continuing.

GOFMAN: People like Don Fredericks, the ex-head of the National Institute[s of Health], wrote me up in one of the issues of *Circulation* [April 1993, Supplement], like a breath of fresh air. *I get fantastic praise for that work from very highly placed people.* I'm a fair-haired boy in the heart disease thing. I'm proud of that work; it's good work.

About the current controversies, I take it very seriously. I feel I made my contribution to pay my way, so to speak, as an individual with the lipoprotein heart disease work. I don't think I have to apologize to anybody. My son said, "Well, do you think you did anything in your lifetime that was worth anything?" I said, "Yes, I do."

(gg) The Controversy Over Low-Dosage Harm

GOFMAN: Having gotten to know about this problem [of no harmless level of radiation], I take it very seriously, because I believe how this controversy settles out [is important] and I'm not optimistic. I'm a little more optimistic because of [journalist] Eileen Welsome and [Energy Secretary] Hazel O'Leary. [But] I think the chances are pretty good that the deceptive position that radiation isn't harmful, may win out by default, because so much money goes into it.

If it wins out by default, the textbooks will be wrong. Once you can get the database altered so the textbooks are wrong, there is no way you'll recover that. An army of Einsteins will not be able to fix it. So the textbooks will say, "[In] 1992 to 1994, it was proved that low doses of radiation don't hurt you."

That will open the floodgates toward, "Don't worry about the waste disposal, because even if everybody gets 10 millirads or a 100 millirads a year, it won't hurt anybody." What I see there is that millions, tens of millions, or hundreds of millions of people are going to suffer. There are going to be a lot of extra cancers. There are going to be a lot of genetically deformed children.

I consider that a personal tragedy that should [not] be allowed to happen without trying to counter it. I feel, though I don't feel too optimistic, it's essential to try to do something to prevent that from winning out. But I'm not optimistic. I would almost say I was never really ready to throw in the towel and just go away.

HEFNER: You and Alice Stewart have some similar concepts, some similar concerns. Have you corresponded, talked with Dr. Stewart?

GOFMAN: Yes, we're good friends. I talked to her the last time, about two months ago, when she was at a symposium at Spokane. I was one of the invited speakers. Apparently when they originally planned the symposium, the Hanford Health Information Network thought they were not going to have any money to have people from overseas come. They didn't invite Dr. Alice Stewart [from the United Kingdom]. I had an invitation to fill about three slots. I insisted [that] I show my CNN tape for an hour and 10 minutes. Bea Kelleigh arranged everything, it was all set.

We were talking one day, [and] she said, "Good news: We got permission to spend the money to get someone from overseas. We're going to invite Alice Stewart." I said, "Bea, how are you going to invite Alice Stewart when you've got the program filled?" "We checked with Alice and she'll be happy to talk to people in the hall." I said, "Bea, that's nonsense, you cannot do that. You don't invite Alice Stewart to come to a major meeting and not have her talk." So I said, "You got to do something about it, and I'd like to know about it." She didn't have an answer.

Five days later, I called her up and said, "Since you haven't figured out a solution, I have. I'm going to give my speaking openings to Alice Stewart and you can still show my videotape." That's how Alice

Stewart came to that meeting. I talked with her on the phone after that. I didn't go to the meeting myself but my videotape was shown. Apparently it's quite popular, my videotape.

No, Alice is a very fine person and have a lot of respect for her. I disagree with her on some of the technical points, but technical disagreements are standard. She's a good person; she's taken a lot of flack from the establishment. There are many people who don't believe her work today. I think that thing on the children in utero is correct.

The Atomic Bomb Casualty Commission originally said they didn't see this excess leukemia or cancer in the children in the first 10 years of life that were expected from Alice Stewart's work. When I wrote *Radiation and Human Health*⁴⁸ in 1981, I analyzed why they might not see it. I didn't think that the absence of the finding of Hiroshima/Nagasaki disproved her work.

The interesting thing is that in the last few years Yoshimoto in the Atomic Bomb Casualty Commission, or what's called RERF⁴⁹ now, has analyzed the kids that were in utero and have a big excess of adult cancers. They are showing, even though they disagreed with the early effect of Alice Stewart, that 30 and 40 years later they're seeing a big excess. The children radiated in utero are as sensitive as the next group to them, the 0- to 9-year-olds, and maybe more sensitive.

Alice Stewart made a very big difference. See, Alice Stewart's work on those children in utero in '56 and the more definitive paper in '58, was the thing that broke the back of this. I was telling you [that] from 1910 to 1945, people [were] saying "200 rads, 400 rads won't hurt anybody." Then she comes out and says one rad will give a 50 percent increase [risk] in cancer and leukemia. This is just a world of difference in thinking.

I'm pretty hard-nosed; I don't like work that I don't respect [even though it] makes my point in spades. I consider [that] it detracts from the truth and it hurts everybody. There are lot of people who have claimed things that just aren't so. I don't admire [that]. That's not Alice Stewart.

HEFNER: Who's following behind you and Alice Stewart, the next wave bringing up these issues?

GOFMAN: I wish I knew that. I know some. There's David Rush at Tufts, who is very good. He just wrote me a letter. He wrote the book with Jack Geiger, called *Dead Reckoning*, on all the things that are wrong with the Department of Energy studies of health effects. David is 60 years old and he's thinking of whether he should stay at Tufts in his professorship or maybe try to do something and try to pull together whatever information can come out of the ex-Soviet Union. There is Boris Gusev, I think he's in his sixties, in the Soviet Union. He's the one that I was telling you that the United Methodist Ministry is trying to help get his material over here translated. There are not too many: Steve Wing, [University of] North Carolina, published a paper on the Oak Ridge people; he looks pretty good.

HEFNER: He used to be at Argonne,⁵⁰ didn't he?

GOFMAN: Wing? Was he? I didn't know that.

HEFNER: I think he might have.

GOFMAN: I suspect there are a number of people that I don't know, that don't all communicate with me. I get some nice letters from some people around the world.

HEFNER: Have there been politicians or certain Congress people or certain social action groups that have been more supportive of you?

GOFMAN: Mike Gravel, who was a Senator from Alaska at the time; he later lost his seat. He made a hell of a difference, [be]cause he got a lot of our things in the *Congressional Record*. He was friendly.

GOURLEY: There's a special problem with the things above the Arctic Circle, isn't there? Radiation levels.

GOFMAN: In what way?

GOURLEY: Elevated levels?

GOFMAN: Gee, I don't even know.

GOURLEY: Okay.

GOFMAN: Part of my ignorance file. Howard Metzenbaum [(D., Ohio)] in the Senate has been a great friend of mine. We were high school classmates together. He has asked me many times, "What can I do in the Senate to help?" I don't like to have Howard do things for me that are based on our friendship, but I have asked sometimes to put something I've written into the *Congressional Record*. A way of getting it in. He'll usually write something about me. He's retiring now, but he's been there, and been willing to help. That's about the extent of my Congressional favors. [From] the U.S. Supreme [Court, there was] William Douglas; but he died, unfortunately. He was [a] very good [friend].

HEFNER: Any social action groups and environmental groups?

GOFMAN: I have very good working relations with the Natural Resources Defense Council; Tom Cochran there. I told you I prepared that [Belarus] manual, which later became that book. EDF,⁵¹ I don't know. Henry Kendall at the Union of Concerned Scientists [and I] were very friendly and I think he would always say nice things about me. I have some friends in Germany; some in Russia. I got this letter in yesterday, some comments of people who got a hold of the book. One geneticist said, "That's the most beautiful piece of scientific work I've had my hands on."

HEFNER: This was your Chernobyl book?

GOFMAN: I didn't think it was *that* good! I get letters from some people who say

they've followed or they've read what I've written. I think I have more friends out there that I don't know well. But I have a lot of people who hate my guts. I know that.

HEFNER: All over the controversy?

GOFMAN: Yes. I don't have any quarrel with atomic power other than I don't believe it's consistent with health. I don't have a hidden agenda, except I have an agenda about health: It matters.

(hh) Skepticism About the Value of Formal Arms-Control Agreements

GOURLEY: Now, your belief is that it's important to have nuclear weapons, but they have a purpose and its deterrence is important?

GOFMAN: Yes.

GOURLEY: Is testing important [for] maintaining the stockpile?

GOFMAN: That's debated. Personally, I don't see anything wrong with testing under the ground. It's just not that big [of] a danger of hurting anything. I think the people who are making a big to-do about it: "We must stop, we must get a comprehensive test ban."

Let me say something about comprehensive test bans: I do not believe in treaties. People of goodwill don't need treaties and people of ill will never abide by treaties. So they just don't need to have them. We don't [have] a treaty with Canada aside from NAFTA,⁵² but we don't have [a real] treaty. We won't bomb each other.

Secondly, anybody who thinks you can fight a nuclear war and win, is insane from the word go. If a nuclear war is fought, then deterrence has failed. I think this: It's a dangerous world and there are people of ill will out there. All the people of ill will did not disappear in World War II. I think that any society that has any basis of decency and humanity [had] just better stay on the technological edge or they're going to be overrun by the thugs, tyrants and murderers of this world. I don't want to see that happen.

I don't want to see the United States go down. It's got a lot of goddamned difficulties, such as the people who would engineer that little fiasco in Wichita, setting Jacob Frabrikant up with that. Worried the hell out me that the Justice Department participated in that. But still in all, if you can think of any other place in the world that even comes close to the freedoms that we do have-I'd like to see this place get better and not go down the tubes. I think there's a danger. I consider the people who are too avid in the disarmament movement are going to kill us.

GOURLEY: Do you think that some of your critics, the folks who say, "Oh, Gofman with this radiation 'no safe level,'" do you think they would be shocked to hear you say that the dangers from a few underground nuclear tests are worth it?

GOFMAN: I don't know, they might.

GOURLEY: I would think that would be the sort of thing that would silence a lot of your critics.

GOFMAN: My view would be, I'd say all these things publicly and I think the case I would make is not that the underground tests are not harmful. They *are* harmful, a little will leak out. A small number of people will get hurt. I would want to tell the American people if I were king, "I'm leveling with you, this is something we need to do to preserve the freedoms we have. Don't you agree that we need to do this?"

But I wouldn't be running a movie out in downwind St. George, Utah, saying go out and come out and see the cloud, no harm, and all that. I think that was where the big mistake was. The arrogance of the DOE and the Atomic Energy Commission: "We know best, we don't have to tell you, we'll hide it and we'll lie to you." That's all I object to. With respect to the safe dose, which they're trying to sell, I consider that a war against humanity. They're conducting a war against humanity and somebody's got to fight them. I don't have an objection. I don't like the comprehensive test ban treaty. You know Helen Caldicott?

HEFNER & GOURLEY: Yes.

GOFMAN: Helen Caldicott's very famous worldwide. She's an antibomb activist. She was saying, "We've got to have a treaty to ban the cruise missile, because pretty soon they won't be able to detect them at all." If you won't be able to detect them at all, what the hell is the sense of a treaty? I don't understand the disarmament movement. But they don't like me, the disarmament movement.

(ii) Motivation During the Manhattan Project

GOURLEY: In your life, you've accepted quite a level of risk, as far as radiation is concerned.

GOFMAN: I was pretty happy to do that. I would do that again, because in my lifetime (you don't know about it, Karoline), [I would do] anything that would help defeat Hitler and [the] Japanese warlords. I'd be happy to take a lot of radiation or whatever it took. You could have come into our lab in Gilman in 1942 to '44 at midnight or two in the morning. The chances were better than even you'd find us all there. We didn't have to be there; that was just how we felt about things.

I've had people from NHKTV, that's big Japanese TV. They have several different branches-Tokyo NHK, Hiroshima NHK; we were on both of them. I remember the last one. They said, "Don't you feel badly that you worked on the bomb, thinking of all the horrendous things that it did at Hiroshima/Nagasaki?" I said, "Well, you know, it shouldn't have happened. I think you should have thought of it when you bombed Pearl Harbor. If we hadn't had Pearl Harbor, we wouldn't have [had] the Philippine Death March and we wouldn't

have [had] Guadalcanal and we wouldn't have had Iwo Jima and we wouldn't [have] firebomb[ed] Tokyo and Hiroshima; it would not have been bombed. You should have thought of that before you bombed Pearl Harbor."

(jj) Ethics and Human Radiation Experiments

GOURLEY: (*presents a document*) This is from the Joint Committee, because we were talking about Dr. Batzel. It's a letter that you wrote commenting on some of the things that were being said. The criticism of you was that the AEC staff said that your interpretations were not based on experimental work of your own.

GOFMAN: That's when I told them, "Look, you want me to go out and bomb my own Hiroshima?"

GOURLEY: Exactly.

GOFMAN: Can you imagine somebody saying in a field like the health effects of radiation, where it all depends on what's happening out there, it should be your work?

GOURLEY: Yes, after you said, "Go bomb your Hiroshima," you said, "Go and irradiate children, infants in utero and TB patients." Now, in light of some of the things that came out about the radioactive oatmeal [given to] the kids and all this other stuff was going on-

GOFMAN: Yes. I consider the things, like the injections of plutonium, immoral. You'll get a copy [of that videotape]. I don't have an extra copy. I can show it to you today if you want to see it. I'll see that you get a copy of the thing I did on "60 Minutes Australia." It was a matter of very, very deep personal significance.

The 18 people who got the plutonium were chosen because they were believed not [to] have a long life expectancy. I have for a long time, as a physician, known that the dumbest thing that a doctor can do is to decide the life expectancy of someone else. The Eileen Welsome story shows you that some of those people lived 25 years. It was a personal experience which is on that tape, and I'll tell it to you briefly.

Helen [(my wife)] was an intern in pediatrics at the time. This young boy was brought over from Australia to become one of the 18. Any child that was brought into UC Hospital had to go through the pediatrics admission service. Helen worked this child up on his admission physical. She didn't remember it, but they showed it to me and some of the documents that came out from Hazel's [(DOE Secretary O'Leary's)] office. I was working with the "60 Minutes Australia" group.

That was 1946 when this child was here. I must have been marching around the halls when that kid was in that hospital, because I was in my senior clerkship and getting ready to start my internship.

In a year after that child was here at UC, my son was born. Helen didn't do well after the delivery. He was a little premature and he came along okay. She got sick; it just was a low-grade fever and nothing else showing. Finally, after a couple of months, she had some tenderness on one side of the abdomen. I went back in to see her ob/gyn [physician], Dr. Overstreet, [who] thought she ought to be operated on; she might have an ovarian abscess. He did operate on her. She had an ovarian abscess and they spilled some of the abscess material.

When she got out of surgery, her temperature skyrocketed. She was very sick. That was in 1947. She was in the hospital and they analyzed the organisms that came out of the ovarian abscess. It turned out it was a called *bacteroides funduliformis*; it's an organism that's common in us; almost never pathogenetic. It's in the bowel, almost never hurts anybody [unless] you get an infection with it.

Professor Kerr, the one I told you later criticized me for doing things without getting permission, was brought into her case because she was going downhill very rapidly. He called Chester Keefer, who had been the expert during World War II on U.S. soldiers with infectious diseases. He told him about it and [asked] what we should do. He said, "Penicillin; 500,000 units is [the] only thing we know. They're 95 to 98 percent fatal." She had already had the 500,000 units of penicillin and it didn't work.

Henry Brainerd, [the] infectious disease head man, had no idea-I had no idea, just watching [her] going downhill. She had gone down to about 80 pounds, her temperature was about 40[C; 104 F] and it was just a matter of time. *She was an ideal candidate for a plutonium injection. Here is a woman who is about to die. [Has] maybe a couple of weeks [to] live. You can get all your data. In that year, one of the 18 was injected in that hospital. This was an ideal candidate for plutonium injections; a patient with a limited life expectancy.*

GOURLEY: Did you know of the plutonium injections at the time?

GOFMAN: No, I had no idea. But I'm just telling you that.

GOURLEY: Right.

GOFMAN: I have a moral principle on this thing. We were just watching her die. Jack Frenkel was a resident in Pathology. He recently retired from [the University of] Kansas. He was a Pathology professor, came up to me and said, "I checked Helen's organism in the lab: 500,000 units of penicillin won't touch her." He said, "5 million might."

Now you have to understand in those days, a million units of penicillin was regarded as astronomical. [He said he thought] 5 million might help. We quickly went to Henry Brainerd and Professor Kerr and [told them about] this testing done on her organism. They both agreed, "Let's get the 5 million units." Jack said that might [not] touch her. I said, "What about 10 million?" and they agreed to that. *Nobody [received] 5 million in that hospital. I don't think anybody had had a million. But Professor Kerr and Henry Brainerd agreed that Helen ought to be tried on 10 million units a day.*

I promptly went down to [the] pharmacy and tried to arrange [it]. They said 10 million units a day, we don't have 10 million units in the hospital! That was a little discouraging. Professor Kerr, to his everlasting credit, called Dr. Robert Cutter, [who] was the head of Cutter Lab, one of the big manufacturers of penicillin. Dr. Robert Cutter gave us a gift of 100 million units of penicillin: 100 million, enough for 10 days. I went over to Berkeley to pick it up. We came back.

The penicillin wasn't that pure in those days. To get 10 million units into [a single injection] you had to combine a lot of [vials] into it; it was a fairly big mass. I finally put some procaine in it to try to deaden the pain. Helen didn't have much buttocks left to put an injection in[to]; [her] temperature was about 40 degrees [Celsius]; she was just out of it, sick. Gave her the first 10 million units that day. By the next morning, her temperature was near normal. By the tenth day, she went home. She's better. She didn't die from the bacteroides. *One guy, Jack Frenkel, turned medicine on its head. He took a fatal disease and on his own initiative tried something that nobody suggested, and it worked.*

GOURLEY: So it was a human experiment that worked?

GOFMAN: It was a human experiment that worked. There was a guy that said, "Listen, if you're so goddamn smart, don't go playing God and telling people that they don't have much [time] to live and give them plutonium injections when something can turn that around in three days." Maybe they [have] a lifetime to live. In that same period, the one who's daughter was Elmerine, what was his name?

HEFNER: Elmer Allen, Cal-3?

GOFMAN: Cal-2 was the little boy from Australia (Simeon Shaw) and the program was called "The Betrayal of Simeon Shaw." It seems that [Helen] could have been one of [the] people given the injection, because she didn't have any lifespan left. So, it reminded me of how immoral it would be for somebody to give her an injection of plutonium before Jack Frenkel turned things on its head.

GOURLEY: You know the players who were involved with that. You know Joe Hamilton, Dr. Stone, you know Low-Beer, you know probably Dr. Bellamy.

GOFMAN: Bellamy I didn't know, I don't think, but I sure knew all the others.

HEFNER: You weren't there and so this is an unfair question, but how could they do it?

GOFMAN: Well, I think what I said earlier [about] Low-Beer and Stone coming out of an era of 30 years or so, [where] they just didn't think about the hazard of plutonium. They should have thought about it because they knew [about] radon. The idea of putting those people in the positions where they were the favorite children of the era. Automatic checkbook, great prestige, "We're saving the world," "We can't be

wrong." You know what I think, Lori.

One of the things you'll see in situations like that is at one level down or two levels down from the top, people almost try to think what they would like to have at the top. Do all these things that they think the bosses would want.

I think maybe they could have thought, "Well, look, we're going to prove that radiation isn't that harmful." There was almost a mantra, "Radiation can't be that harmful, radiation isn't that harmful." They're still singing the mantra today.

Egen O'Connor and I discussed that a great deal. The issue of what it takes for people to live with themselves; we think about that a lot. She believes that if you don't have a rationalization for yourself, that they cannot do these things. They [have] to have a rationalization. I sometimes think that some of them are evil anyway. They don't need a rationalization. But she thinks they do.

Sometimes it's pretty hard to see that rationalization. I don't know, I think that maybe, Joe was just a simple guy. He sure wasn't a malevolent person. Joe Hamilton, just wasn't. Stone, I guess I have to say Stone and Bert Low-Beer really didn't think they were harming anybody. I think they were buying the short-life-expectancy thing. I don't think Louis Hempelmann is malevolent. I didn't know Stafford Warren well. Surely, Shields Warren was not malevolent in character, a very fine man.

I think that overblown idea [about their] importance almost negated having to think: "We couldn't be wrong, we're just doing the things that are needed." I hear of people talking: "It was because of the Cold War." I listened in on one of the [Congressional] hearings where Markey was saying, "Elmerine, you should think of your father as a hero in the Cold War." I groaned when I say this, "What the hell is Markey talking about? The Cold War didn't require any of this stuff." That's baloney, just real baloney. I don't think it required anything. I think it just gave these people a checkbook and they gave them a little wooden block with a rubber piece put on [it] that said "SECRET," and they could just stamp a thing, and the whole world was precluded from seeing what was on your piece of paper. Think of that power! I think people do crazy things when they're obsessed with power.

HEFNER: So the whole rationale that we did it to understand worker safety?

GOFMAN: Lori, let me tell you about worker safety. [Do] you know the story about Eisenbud and the uranium miners?⁵³

HEFNER: (*nods*) About the uranium miners; go ahead and tell me though, please?

GOFMAN: Well, in 1947, the AEC was getting off the ground; Merrill Eisenbud was a young health physicist, working in the New York operations office of AEC. He and his boss, a fellow by the name of Wolff, decided to look over the situation in [the] uranium mines in the West. They came out here and found that the mines were not that [safe-not

ventilated adequately]. Nobody had informed either the miners or the mine operators of what the probable consequences would be. Now that's 20 years after the dial painters, so we knew what alpha particles could do. It's 10 and 20 years [later]; both a decade of the '20s and '30s where they knew about the European mine workers. So the idea of what alpha particles could do was well known. Merrill Eisenbud comes back from the trip and he writes a memo to the AEC saying-

GOURLEY: This was in what year?

GOFMAN: 1947: "If we don't do something about those mines, we're going to have a lung cancer epidemic. It's going to be larger than they had in Europe." Their answer was to move Merrill Eisenbud into a different division. The Washington office took over handling it. They never informed the miners nor the mine operators. We did have the lung cancer epidemic. [And you ask if they cared about worker safety?]

HEFNER: We also paid them a differential to make sure they went into the mines.

GOFMAN: Did we? I didn't know about that.

HEFNER: We paid them extra.

GOFMAN: I think that to say that an organization that knowingly was sending some men to their deaths and you can tell me they cared about a worker's safety?

GOURLEY: But now earlier you said, [in] 1945 with the war going on, you would gladly assume that risk for what you did. Now, Eisenbud, his big thing was Project Sunshine,⁵⁴ right?

GOFMAN: He may very well have been the big wheel [in Sunshine, I don't know]. He's not an opponent of atomic energy. He probably doesn't like me.

GOURLEY: I was wondering if there was some overlap between his studies on strontium and the fallout and the work done at Livermore?

GOFMAN: Which work at Livermore?

GOURLEY: The Biology and Medicine-

GOFMAN: That I headed? That whole thing on Project Sunshine was like a decade before us.

GOURLEY: So did you review that?

GOFMAN: Yes, we reviewed those things. That was part of the story. It wasn't a big deal, but we did review it. There were no good studies out of Project Sunshine of people being exposed to various levels [for] following the curve of effects. That's what was needed. You had Project Sunshine [that] showed everybody was getting strontium into their teeth and bones, but nobody had a study showing they were

being hurt. Hiroshima gives us [a] summary of people [exposed] at various doses and you find a dose response that's positive. That's pretty definitive. Sunshine didn't do that.

HEFNER: That covers it for me. Is there anything you would like to add?

GOFMAN: Well, I gave you those sheets.

HEFNER: We will place them in the office.

GOFMAN: You got the sheets?

HEFNER: We got the sheets and I'll try to get the transcript, the one tape you talked to us about.

GOFMAN: Let's see, there's one that I have. [I] just talked to CNN's video and it has 10 minutes of the interview which lasted three days. They used three minutes, but the hour are the tapes of "60 Minutes Australia." I need to get some more copies, I don't have any [extras]. Well, I'll get a copy, that will be an hour talking about the morality of the plutonium injections and I'll get you a copy. The story of Helen [Gofman] and Jack Frenkel is on that tape. Tapes that are like those, had a lightbulb blow up in the middle of things. Just was strictly uncut, which that is, too. I want to get you a copy of "Irrevy" to wherever you are; you can read about my Nuremberg trial [idea].

GOURLEY: I'd like to get a copy of that.

(kk) Message From John Gofman

Supplement to the Oral History of John W. Goiman
March 20, 1995
An Overview in Retrospect of the "1945 + Human Radiation Experiments"

It is my opinion based upon some major studies I have accomplished in the past year that it is a grave mistake to consider "human radiation experiments" as a phenomenon peculiar to the advent of large-scale atomic energy.

In fact, the really significant events were in 1895 (Roentgen's discovery of the X-Ray), and 1898 (the Curie's discovery of radium). The true era of massive human radiation experimentation began very shortly after Roentgen's work, and by the 1940-1945 period, all the features were in place that ASSURED we would have precisely what has been found to have been the case in the post-1945 period. But there really was nothing special about the human experiments beginning after 1945.

Two Major Facts of Life Which Must Be Conceded Here

1. Humans in recent decades (last couple of hundred years) operate on the technological imperative. Whatever is discovered must be applied immediately. There has been no thought, until recently, about DISASTER CREEP which can occur as a result of looking only at the short span of time for consequences of exposure to new technologies.

2. A special example of disaster creep is the inordinately long latent period before the full flowering of cancers following exposure to carcinogens such as ionizing radiation. The time is clearly at least 50 years and it may really be 60 or more years.

THE RESULT: The bulk of the cancers from x-radiation and radium gamma rays simply were not seen, partly because of the long latency and partly because the idea that long-term follow-up was essential was clearly dismissed in the half-century after the Roentgen discovery.

THE FALSE CONCLUSION; Doses of 200, 400, 600, and even over 1000 Roentgens of exposure to partial body radiation were erroneously exonerated as cancer producers. Millions of cancers were set in motion in the populations receiving ionizing radiation in the half-century before the A-bomb.

And this set the stage for all the events recently receiving notice. How?

Radiation below 500 to 1000 roentgens of exposure was ridiculed as being of no consequence by failure to look at the follow-up of persons exposed.

When the post-Hiroshima era resulted in the massive Atomic Energy Bureaucracy, with all the biases built-in from 50 years of having missed the boat concerning cancer production, WHO WAS PUT IN CHARGE OF THE PROGRAM ON HEALTH EFFECTS? THE VERY PEOPLE WHO HAD A TOTAL BIAS IN FAVOR OF "No Problem from Low-Dose Radiation." Although there should have been more thoughtfulness over the uranium miners and the dial painters, somehow the idea became accepted that beta particles and electromagnetic radiation simply had shown themselves not to be a worry. Alpha particles, grudgingly yes.

Not that these people were correct. THEY WERE NOT. But I am describing the atmosphere in which these individuals came to be the dominant forces in setting up the post-war era of biology and medicine of irradiation. The bias was overwhelming, and with their short-sighted look at the problem, it seemed as though they really believed there was no harm.

That was the EARLY phase post-war. But once the bureaucracy was set up and the movers and shakers were told, "No problem with health issues," the door was opened wide for all sorts of proposals from nuclear power, massive uses of radionuclides in medicine and elsewhere, and even all the "Plowshare" ideas.

This set up a new phase. Once the biologists had told the high moguls there was no problem with health effects, all kinds of wheels were in motion and from there on out, the biomedical people had to try to have biology conform to their erroneous view of what the real truth was.

And all hell would break loose if the moguls had been embarrassed by the poor biological guidance from an inept biomedical community. And that community, seeing this golden goose of unlimited funds for research and grants, simply was not in any mood to say, "Go Slow," or that our prior guidance was wrong.

We are now slowly coming off that erroneous mountain --- but because so much prestige and so much funding have gone into the enterprise, the easiest path is denial that any problem exists at doses of a few rads. After all these same people just a couple of decades earlier were telling the Congress and the public that 500 to 1000

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rads were "Safe" exposures. I have recently found even more evidence that this was the prevailing view at the bureaucratic top.

There is a fundamental rule that exposing persons to a potential poison, with an assurance of safety when that cannot be assured, is fraudulent. At the very least, this constitutes human experimentation, with its Nuremberg connotations. Such experimentation is commonplace today, with so-called safe standards being set for "tolerance" doses. The idea of safe doses was much, much more in error for the 50 year period before the atomic bomb.

Now we can go into the Oral History, but I think failure to appreciate the 50 years before the a-bomb completely confuses the persons looking into the ethics of so-called "human experimentation." The outcome WAS CRADLED long before the post-bomb period, and was an inevitable expectation.

End of Prologue

I have felt these conclusions needed to be here. They have resulted from an in-depth year-long investigation of the extent to which ionizing radiation, primarily medical x-rays and radium gamma rays, accounts for the current level of breast-cancers. We estimate that 75% of all breast-cancers were and are induced primarily by medical irradiation. Most of that was in the horrendous use of fluoroscopy and the equally questionable uses of radiation in the therapy of benign diseases — from dermatologists to rheumatologists. There is some REAL human experimentation.

John W. Gofman, M.D., Ph.D.

March 20, 1995