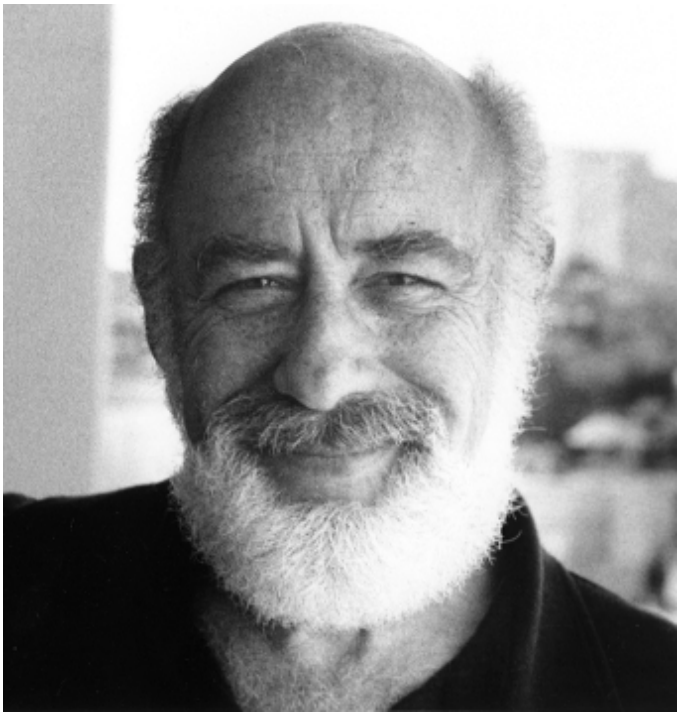

Dr. John Gofman

NUCLEAR WITNESSES, INSIDERS SPEAK OUT: JOHN W. GOFMAN, MEDICAL PHYSICIST

"Licensing a nuclear power plant is in my view, licensing random premeditated murder. First of all, when you license a plant, you know what you're doing—so it's premeditated. You can't say, "I didn't know." Second, the evidence on radiation-producing cancer is beyond doubt. I've worked fifteen years on it [as of 1982], and so have many others. It is not a question any more: radiation produces cancer, and the evidence is good all the way down to the lowest doses."

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The following is chapter 4 from the 1982 book *Nuclear Witnesses, Insiders Speak Out* and is an interview with Dr. John Gofman detailing his personal experiences and knowledge regarding the nuclear establishment. Dr. Gofman is a Professor Emeritus at the University of California, Berkeley (Ph.D. in nuclear-physical chemistry and an M.D.) who was the first Director of the Biomedical Research Division of the Lawrence Livermore Laboratory from 1963-65 and one of nine Associate Directors at the Lab from 1963-1969. He was involved in the Manhattan Project and is a co-discoverer of Uranium-232, Plutonium-232, Uranium-233, and Plutonium-233, and of slow and fast neutron fissionability of Uranium-233. He also was a co-inventor of the uranyl acetate and columbium oxide processes for plutonium separation. He has taught in the radioisotope and radiobiology fields from the 1950s at least up into the 1980s, and has done research in radiochemistry, macromolecules, lipoproteins, coronary heart disease, arteriosclerosis, trace element determination, x-ray spectroscopy, chromosomes and cancer and radiation hazards. Starting in 1969 he began to challenge the AEC claim that there was a "safe threshold" of radiation below which no adverse health effects could be detected.

Chapter 4 of *Nuclear Witnesses* outlines Gofman's career history and how, over time, he came to understand the true dangers of artificial, man-made radioactive matter and how the government, and the people in charge of the nuclear industry, were suppressing the facts of this danger from humankind. This book provides a wealth of information about the radioactive contamination of Mother Earth by the nuclear industry since the 1940s.

Quoting the book's author Leslie Freeman, "It is the premise of this book that if the American people knew the truth about radiation there would be no nuclear issue." The myth is that there is a "safe threshold" of exposure to radioactive material, a permissible does below which no health effects can be detected. Who's interests are being served here? Who benefits? Certainly *not* people being dosed!

> "My particular combination of scientific credentials is very handy in the nuclear controversies, but advanced degrees confer no special expertise in either common sense or morality. That's why many laymen are better qualified to judge nuclear power that are the so-called experts." Gofman has achieved the singular distinction of being branded "beyond the pale of reasonable communication" by the nuclear power industry.
> —from *IRREVY, An Irreverent, Illustrated View of Nuclear Power*, 1979, by Dr. John Gofman.

— ratitor

Gofman sits back. It is the attempt to deceive the public that makes him so angry. His reaction was the same when he learned how the Atomic Energy Commission was deceiving the public about the effects of low-level radiation. When the AEC tried to censor his findings about radiation-induced cancers, Gofman reached his turning point. To him, censorship is "the descent of darkness.". . .

Then I started hearing that there were a lot of people from the electric utility industry who were insulting us and our work. They were saying our cancer calculations from radiation were ridiculous, that they were poorly based scientifically, that there was plenty of evidence that we were wrong. Things like that. So I wondered what was going on there. At that point—January 1970—I hadn't said anything about nuclear power itself. In fact, I hadn't even thought about it. It was stupid not to have thought about it. I just wondered, Why is the electric utility industry attacking us?

I began to look at all the ads that I had just cursorily seen in "Newsweek" and "Time" and "Life," two-page spreads from the utilities, talking about their wonderful nuclear power program. And it was all going to be done "safely," because they were never going to give radiation above the safe threshold.

And I realized that the entire nuclear power program was based on a fraud—namely, that there was a "safe" amount of radiation, a permissible dose that wouldn't hurt anybody. . . .

"Someone from the AEC came to my house last weekend," he said. "He lives near me. And he said, `We need you to help destroy Gofman and Tamplin.' And I told him you'd sent me a copy of your paper, and I didn't necessarily agree with every number you'd put in, but I didn't have any major difficulties with it either. It looked like sound science. And—you won't believe this—but do you know what he said to me? He said, `I don't care whether Gofman and Tamplin are *right* or not, scientifically. It's necessary to destroy them. The reason is,' he said, `by the time those people get the cancer and the leukemia, you'll be retired and I'll be retired, so what the hell difference does it make *right now*? We need our nuclear power program, and unless we destroy Gofman and Tamplin, the nuclear power program is in real hazard from what they say.'. . .

. . . in 1972 the National Academy of Sciences published a report called the BEIR Report—Biological Effects of Ionizing Radiation—a long, thick report, in which they walked around the problem as best they could, and finally concluded that we were too high between four and ten times. But if you read the fine print, they were admitting that we might just be right.[22]

When that came out, everybody realized that the AEC was not worth a damn. By then the AEC had gotten themselves into another flap. Henry Kendall and Dan Ford of the Union of Concerned Scientists showed that the AEC didn't know whether the Emergency Core Cooling System would ever work or wouldn't.[23] The Emergency Core Cooling System was the last barrier of safety in a major nuclear accident. This further damaged the credibility of the AEC.

Those two events—the conflict with Ford and Kendall and the conflict with us—finally led them to realize they could no longer use the words "Atomic Energy Commission," and so the government abolished the AEC.

"We are now solving the problem," they said. "We'll create two new agencies—ERDA (Energy Research and Development Agency) and NRC (Nuclear Regulatory Commission)."

ERDA was supposed to promote the development of atomic energy, and NRC was supposed to concern itself with public safety. The idea was that it was the promotion of nuclear energy that made the AEC's safety work so poor. The new NRC was only supposed to involve itself in safety—no promotion.

Which turned out to be one of the greatest lies in history. . . .

I had made one mistake. If the Department of Energy or the AEC gives you money on a sensitive subject, they don't mean for you to take the job seriously. They need you—with your scientific prestige—so they can point to you. "We have so and so studying the problem." Studying the problem is marvelous. But if you want the money and the continued support, you should go fishing or play golf. My mistake was I discovered something. . . .

Gofman decided to take an early retirement at the age of fifty-five, so he gave up his position at the University in 1975 and became professor emeritus. Although no longer engaged in active teaching, Gofman did not give up research. In the next years he discovered that plutonium was even more hazardous than he had thought. "Plutonium is so hazardous that if you had a fully developed nuclear economy with breeder reactors fueled with plutonium, and you managed to contain the plutonium 99.99 percent perfectly, it would still cause somewhere between 140,000 and 500,000 extra lung-cancer fatalities each year.". .

The requirement for controlling plutonium in a nuclear economy built on breeder reactors would be to lose no more than one millionth or ten millionth of all the plutonium that is handled into the environment where it could get to people. Which brings up a fundamental thing in nuclear energy—there are some engineers, scientists, who are not merely fraudulent sycophants of the system. They're really out of touch with reality.

I was once on an airplane with a strong pronuclear engineer. I said, "I've done some new work on plutonium. I think it's a lot more toxic than had been thought before. At what toxicity would you give up nuclear power?"

He said, "What are you talking about?"

"If I told you that you had to control your plutonium losses at all steps along the way—burps, spills, puffs, accidents, leaks, everything—that you can't afford to lose even a millionth of it, would that cause you to give up nuclear power?"

"Oh, I understand your point now, John," he said. "Now, you tell me—we look to biologists like you to tell us how well we need to do. If you say I've got to control it to one part in ten million, we'll do it. If you say it's got to be one in a billion or ten billion we'll do it. You tell us

what we have to engineer for, and we'll do it."

I said, "My friend, you've lost touch with reality completely. I've worked in chemistry laboratories all my life, and to think you can control plutonium to one in a million is absolutely absurd. If you were a patient of mine who came in to see me, I'd refer you to a psychiatrist."

"Well, John, engineering is my field. And we believe we can do anything that's needed."

Engineers do believe that. That's the arrogance of engineers—they think they can do anything. Now their mistakes catch up with them, as you see from the DC-10s and the Tacoma Narrows Bridge that fell down, and the Teton Dam and the most recent episode, Three Mile Island—where the unthinkable, the impossible, did happen.

Nuclear Power: A Simple Question

Many people think nuclear power is so complicated it requires discussion at a high level of technicality. That's pure nonsense. Because the issue is simple and straightforward.

There are only two things about nuclear power that you need to know. One, why do you want nuclear power? So you can boil water. That's all it does. It boils water. And any way of boiling water will give you steam to turn turbines. That's the useful part.

The other thing to know is, it creates a mountain of radioactivity, and I mean a *mountain:* astronomical quantities of strontium-90 and cesium-137 and plutonium—toxic substances that will last—strontium-90 and cesium for 300 to 600 years, plutonium for 250,000 to 500,000 years—and still be deadly toxic. And the whole thing about nuclear power is this simple: can you or can't you keep it all contained? If you can't, then you're creating a human disaster.

You not only need to control it from the public, you also need to control it from the workers. Because the dose that federal regulations allow workers to get is sufficient to create a genetic hazard to the whole human species. You see, those workers are allowed to procreate, and if you damage their genes by radiation, and they intermarry with the rest of the population, for genetic purposes it's just the same as if you irradiate the population directly.[27]

So I find nuclear power this simple: do you believe they're going to do the miracle of containment that they predict? The answer is they're not going to accomplish it. It's outside the realm of human prospects.

You don't need to discuss each valve and each transportation cask and each burial site. The point is, if you lose a little bit of it—a terribly little bit of it—you're going to contaminate the earth, and people are going to suffer for thousands of generations. You have two choices: either you believe that engineers are going to achieve a perfection that's never been achieved, and you go ahead; or you believe with common sense that such a containment is never going to be achieved, and you give it up.

If people really understood how simple a problem it is—that they've got to accomplish a miracle—no puffs like Three Mile Island—can't afford those puffs of radioactivity, or the

squirts and the spills that they always tell you won't harm the public—if people understood that, they'd say, "This is ridiculous. You don't create this astronomical quantity of garbage and pray that somehow a miracle will happen to contain it. You just don't do such stupid things!"

Licensing a nuclear power plant is in my view, licensing random premeditated murder. First of all, when you license a plant, you know what you're doing—so it's premeditated. You can't say, "I didn't know." Second, the evidence on radiation-producing cancer is beyond doubt. I've worked fifteen years on it, and so have many others. It is not a question any more: radiation produces cancer, and the evidence is good all the way down to the lowest doses.

The only way you could license nuclear power plants and not have murder is if you could guarantee perfect containment. But they admit that they're not going to contain it perfectly. They allow workers to get irradiated, and they have an allowable dose for the population.[28] So in essence I can figure out from their allowable amounts how many they are willing to kill per year.

I view this as a disgrace, as a public health disgrace. The idea of anyone saying that it's all right to murder so many in exchange for profits from electricity—or what they call "benefits" from electricity—the idea that it's all right to do that is a new advance in depravity, particularly since it will affect future generations.

You must decide what your views are on this: is it all right to murder people knowingly? If so, why do you worry about homicide? But if you say, "The number won't be too large. We might only kill fifty thousand—and that's like automobiles"—is that all right? . . .

People like myself and a lot of the atomic energy scientists in the late fifties deserve Nuremberg trials. At Nuremberg we said those who participate in human experimentation are committing a crime. Scientists like myself who said in 1957, "Maybe Linus Pauling is right about radiation causing cancer, but we don't really know, and therefore we shouldn't stop progress," were saying in essence that it's all right to experiment. Since we don't know, let's go ahead. So we were experimenting on humans, weren't we? But once you know that your nuclear power plants are going to release radioactivity and kill a certain number of people, you are no longer committing the crime of experimentation—you are committing a higher crime. Scientists who support these nuclear plants—*knowing* the effects of radiation—don't deserve trials for experimentation; they deserve trials for murder. . . .

. . . The only solution is, you must stop *all* efforts to develop first-strike force solutions everywhere—whether they be nuclear or other—and move toward a more just society.

Even if you made an agreement to abolish all nuclear weapons, but you left established power structure in the U.S. and the USSR, they'd go on to research mind control or some chemical or biological thing. My view is, there exists a group of people in the world that have a disease. I call it the "power disease." They want to rule and control other people. They are a more important plague than cancer, pneumonia, bubonic plague, tuberculosis, and heart disease put together. They can only think how to obliterate, control, and use each other. They use people as nothing more than instruments to cast aside when they don't need them any more. There are fifty million people a year being consumed in a nutritional holocaust around the world; nobody

gives a damn about starvation. If fifty million white Westerners were dying, affluent Western society would worry, but as long as it's fifty million Third World people dying every year, it doesn't matter.

In my opinion, what we need is to move toward being nauseated by people who want to be at the top, in power. Can you think of anything more ridiculous than that the Chinese, Russian, and American people let their governments play with superlethal toys and subject all of us to these hazards? The solution is not to replace one leader with another or to have more government. Society has to reorganize itself. The structure we have now is, the sicker you are socially, the more likely it is that you'll come out at the top of the heap.

* * * * * * * * *

Author's Note

Two things happened that led me to write this book. First, a doctor tried to convince me to take radioactive iodine for an overactive thyroid. I refused. Several months later John Gofman told me I was very fortunate. The radioactive iodine, he explained, would have increased the chance of my getting cancer by more than 100 percent.

The other thing that led me to write this book was the accident at Three Mile Island. Coincidentally, my thyroid condition had been diagnosed the same week that Three Mile Island vented radioactive gases into the atmosphere. I read everything I could lay my hands on, groping for the truth behind the evasive reports published by the Nuclear Regulatory Commission. I finally read verbatim transcripts of the Commissioners' meeting held the day after the accident. The words these men said to each other stunned me. They had no idea what was happening and no idea how to stop it. And meanwhile they were issuing reassuring reports to the public.

I wanted the truth. For the first time I felt my survival was at stake—nuclear power was not an abstract issue: it was a matter of life and death. I started to talk to people—scientists, doctors, nuclear workers.

I interviewed twenty-four people who have worked with or around nuclear materials. In nineteen cases I traveled to the person's home or place of work. Most interviews took between two and four hours and were followed up by phone interviews. I taped the in-person and telephone interviews and listened to them several times, taking notes. I then selected and transcribed those which I felt contained the clearest and most important information and were also the most fascinating as narratives. These were the transcripts from which I worked for the chapters of this book.

A word about the editing I did. In every case I tried to maintain the exact words, the exact flavor of the speech, and the exact meaning intended by the speaker. I have cut out sections that were redundant, irrelevant, unnecessary, or confusing. The repetitive "you know" or "like I said" was eliminated when it seemed too distracting—appropriate perhaps in conversation but not on the

page.

Each chapter was returned to the narrator in draft form for comments, accuracy, and approval. In some cases a name was changed to protect an informant, an expression was changed, a statistic was corrected.

The final version of each chapter was then written—including an introductory section, footnotes, and a bibliography of sources relevant to the chapter. Each narrator was also asked for a photograph to include with his or her chapter.

The question that I asked initially in each interview was about personal background. This was followed by a series of questions about what experiences the person had which made him or her change or develop a point of view on nuclear power. I did not merely listen. When I did not understand, I asked questions. When I did not believe something, I said so. I asked for proof, for reasons, for the thoughts and feelings which made people act the way they did. I asked them to describe experiences in such a way that I could see what they saw and hear what people said and did. They described specific hearings and meetings. Again and again I asked to be told what went through their minds as they experienced the things they told me about. It was these personal moments that most brought me into their lives and that I have attempted to bring to the reader.

It is the premise of this book that if the American people knew the truth about radiation there would be no nuclear issue. The information speaks for itself. In this book people who have had direct personal experience with the nuclear establishment speak about what they learned. They did not necessarily start out as proponents or opponents of nuclear power; they are people who have in common a genuine respect for hard work. In almost every case they found their integrity as workers threatened by involvement with the nuclear establishment. When they mentioned that something was done sloppily, that some regulation was being violated, that something was dangerous, their concerns were ignored, trivialized, rationalized, or twisted. Some, unable to work under such conditions and feeling their sense of decency outraged and their survival in jeopardy, began to speak publicly. Then they found out what they were up against: it wasn't just their boss, it wasn't just their boss's boss: it was the union, the utility company, the military-industrial complex that were insisting on the myth that nuclear power was "safe." No one was permitted to challenge this myth and retain credibility. Nuclear energy existed for the "benefit" of the people and nuclear weapons were necessary for "national security."

The stories in this book are evidence that even in the face of intimidation, people still believe their own experience matters and that other people matter. They are concerned about the lives of their children and the continuation of the species. These people know that when people hear the truth, they listen.

CHAPTER 4
John W. Gofman, Medical Physicist

A cool, crisp morning, late in August 1979. From inside a meticulously furnished living room in the quaint house, built high on a hill overlooking the city of San Francisco, you can see the city orange and white, glittering in the distance.

John Gofman sits across from me on a wooden bench-sofa built into the corner of the living room. He lights a pipe and crosses his legs. On the verge of sixty, he is surprisingly youthful. His oval-shaped face is framed with a thick snow-white beard. His skin is ruddy and smooth, his eyes quick, piercingly alive.

As usual, I begin the interview by explaining what led me to write this book. I tell him about discovering I had an overactive thyroid and the thyroid specialist who recommended radioactive iodine as a cure. Gofman's eyes narrow. He leans forward. "Did you take the radio-iodine?" I shake my head no and explain, "I was afraid of it."

"Let me tell you what that would have done to you," he says. His voice rises in anger as he explains that the dose the specialist said he wanted to give me would have increased my chances of developing cancer by "50 to 100 percent—which is a massive increase!" Gofman sits back and relights his pipe. Then he continues, warming to the subject: "The logical question is: if what I say is true, then how come the medical profession doesn't know it? Well, there are many reasons, some of which don't even surface. For example, hundreds of thousands, perhaps a few million people have been given radio-iodine treatments already. Think of how hard it is for the physician to think that his profession can have endangered the lives of five hundred thousand to a million people. So psychologically he has a wall that says, `No, this cannot be harmful. I personally have not seen a single cancer from it.' Which of course is a ridiculous way to look at it.

"The Public Health Service sponsored a follow-up study of some 30,000 people who had received radio-iodine. Came to the conclusion that it didn't appear that cancer was seriously increased. Absolutely rotten, miserable, stupid, unscientific study. Published in a quality medical journal—but that didn't in any way prevent it from being all those things—unscientific, miserable, and stupid. What was wrong with that study? First of all, we know that very few cancers surface before ten years after the radiation. Then they get more and more frequent. In the study the average person was followed up only nine years. In other words, they were studying the people in the period when

you don't *expect cancers to occur!*

"Also, the number of radiation-induced cancers goes up in proportion to how frequent that particular cancer type is anyway. Breast cancer is 20 percent of all cancer in women. So after you have treated women of twenty-five or so with radio-iodine, you should look in the fifty-year age bracket, when breast cancer becomes a common disease. So the whole damn study, averaging nine years of follow-up, is at the wrong time and is giving a false impression of security that's going to kill more and more people.

"The epidemic of doctor-induced cancer from radio-iodine is ahead of us yet!

"You would think that medicine would have become wiser from the experience with asbestos, with vinyl chloride, with radiation. But they don't seem to learn from such experience. They seem to think that radio-iodine is something special. The next thing will be radio-strontium is something special. Then plutonium is special.

"I'll sit here and confidently say into your recorder—and if you hold the tape for another ten years, I will still be confirmed. I don't say many things positively. A lot of things I'll tell you I don't know—we're uncertain, more work needs to be done. But on this one I don't put any of those qualifiers in. It is going to occur. The dose to the body from radio-iodine at therapeutic levels is such that it's going to produce many, many cancers. Then it's going to be: `Oh, we must not use radio-iodine any more. At the time we did it, it was the best medical practice.'

"See, that's the out. If the whole profession was idiotic in a given time and agreed to the idiot position, that's regarded as the `best medical practice of the time.' That's the story."

Gofman sits back. It is the attempt to deceive the public that makes him so angry. His reaction was the same when he learned how the Atomic Energy Commission was deceiving the public about the effects of low-level radiation. When the AEC tried to censor his findings about radiation-induced cancers, Gofman reached his turning point. To him, censorship is "the descent of darkness."

"I'm not interested in being a crusader," Gofman says, "but somebody had to say something about this issue, so why not me?"

The Beginning: Uranium-233

Born in Ohio, John Gofman grew up in Cleveland and attended Oberlin College, with a major in chemistry. He thought he might like to do medical research, so in his junior and senior years he took courses to qualify him for medical school. After graduating from Oberlin 1939 with an A.B. in chemistry, Gofman entered Western Reserve University Medical School. Although he enjoyed learning medicine and did quite well his first year there, he realized he was not getting the sound scientific background in physical sciences that he would need for medical research. In 1940

Gofman took a leave of absence from medical school and enrolled at the University of California at Berkeley as a Ph.D. candidate in chemistry. "The first thing you did when you came to Berkeley as a Ph.D. candidate was to choose a research field. I looked around and there was a young professor there by the name of Glenn Seaborg, who was working in artificial radioactivity."[1] Glenn Seaborg was the scientist who discovered plutonium,[2] the man-made radioactive element that would be used five years later in the atomic bomb dropped on Nagasaki (9 August 1945).

I thought, probably all kinds of biochemical problems in medicine are going to be solved by the application of radioactive tracers.[3] How better could I prepare myself for a future medical career than to work on a problem involving artificial radioactivity?

So I elected to work with Glenn Seaborg. He assigned me a problem—there was a possibility from thorium you might be able to make a substance called uranium-233, provided it existed, and we didn't know whether it would exist or not.

He said, "Why don't you see if you can find out whether it exists or not?"

It was just an interesting problem in nuclear physical chemistry—an unknown part of a whole systematics of the heavy elements. So I started to look, and the work went quite well, and in about a year and a half I had discovered uranium-233.

We used the Berkeley cyclotron—an accelerator machine—to develop very high energy particles, and from this to develop neutrons with which we could bombard natural thorium. By a complex series of chemical steps I was able to isolate and prove the existence of uranium-233 at a time when I had four one-millionths of a gram. This was not an amount I ever saw—you traced it around by its alpha particle radioactivity. So all the chemistry I was doing, I could never see the material I was working with; I was only tracing it. I had to measure the amount I had by its radioactivity—instead of a scale that uses gravity, you're using radioactivity to weigh things.

By then, things had shaped up to the point that it appeared possible America would enter the war and that the discovery of nuclear fission might mean that nuclear bombs were possible. Scientists in this country voluntarily stopped talking about their work in public. It was an informal agreement.

It was possible that uranium-233, which I had discovered, might be one of the substances used to make a bomb. It depended on whether it fissioned more easily or less easily than plutonium, which had been discovered by Seaborg, or than uranium-235, which exists naturally. These were the three candidates to make a bomb, and certain physics measurements on the fissionability would determine which was the best.

So I started to work on trying to find out if uranium-233 was fissionable, and I proved that it was,

using what's called both slow- and fast- moving neutrons. In fact, I proved that it was even better in many respects than plutonium for this purpose.[4] All that was connected with my Ph.D. thesis which I finished in 1942.[5]

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1. *artificial radioactivity:* radioactivity was discovered in uranium in 1896 by Becquerel. All substances that are found naturally and are radioactive are "naturally radioactive." When man bombards an element to convert it to a new radioactive element, as Madame Curie's daughter did in the mid 1930s, this new radioactive element is referred to as "artificially radioactive."

2. *Glenn Seaborg* received the Nobel Prize for chemistry in 1951 and became chairman of the Atomic Energy Commission in 1961. He remained in that position for ten years.

3. *radioactive tracer:* use of a radioactive substance to trace the behavior of an element in the body. Radioactive potassium-40, for example, traces the movement of potassium through the body.

4. U-233 can be made from natural thorium. Thorium does not chain react by itself. Another element that makes the thorium chain react was necessary—uranium-235 or plutonium. Then the thorium continued to chain react and could produce U-233. However, at the time there was not enough U-235 or plutonium around to use for converting the thorium to U-233.

5. Ph.D. in Nuclear/Physical Chemistry from the University of California at Berkeley, 1943. Dissertation: The discovery of Pa-232, U-232, Pa-233, and U-233. The slow and fast neutron fissionability of U-233.

The Manhattan Project: Building the A-Bomb

I was all in favor of making a bomb. And I want you to know that I have no guilt about it. I would do it again, and for this reason: as I appraised the situation at that time, there was not for a long time in history any worse aberration of human conduct and human monstrosity than the Nazi regime in Germany. And the idea of an atomic bomb that could win the war against Germany was highly attractive to me. While nothing required me to work more than eight hours a day, I spent at least sixteen in the average day on the bomb project. I was very highly motivated simply because I thought it was important to win the war against Germany.

By this time the Manhattan Project had started, and the government was backing it. They hadn't backed any of our work before. We were working for peanuts in terms of money. Seaborg's group became one of the integral parts of the bomb project, and then Seaborg left to go to Chicago to the headquarters where the Fermi reactor—the first one—had run. They were definitely going to go ahead and attempt to make a bomb out of plutonium.

I stayed behind in Berkeley and became the leader of the residual Berkeley group that Seaborg had had before. Seaborg and a fellow by the name of Arthur Wahl were the first two people in the world to work with plutonium, and I became the third.

In order to make a bomb out of plutonium, we had to learn a hell of a lot of chemistry of plutonium, at a time when practically no plutonium was available. We had never even seen it. We were tracing its radioactivity around by its alpha radioactivity.

But we learned quite a bit about the chemistry of plutonium in the year that followed. About that time, J. Robert Oppenheimer[6] took a large group down to form the Los Alamos Laboratory in New Mexico, which was to be a secret isolated lab, to go on with the bomb work. The other labs—in Berkeley, Chicago, and Columbia—were feeders to that project.

Very shortly thereafter, Oppenheimer came up to see me and said, "We have a very desperate problem. We need to have half a milligram of plutonium."

That was something like ten times what had ever been available before.

"You're going to have grams of it in a year," I said, "when the Oak Ridge reactor runs. Why do you need half a milligram now when you're going to have two thousand times that in a year?"

"We need that measurement," Oppenheimer said. "We need it badly because it will alter the whole way the Project goes."

"Well, what do you want?"

"Well," he said, "I talked to Ernest Lawrence"—who was head of the Lawrence Laboratories—"and he has agreed to give up the cyclotron for as long as it will take to have you make some plutonium. We figured out," he said, "that you could make half a milligram if we bombarded a ton of uranium for maybe a month or two."

So after a few hours of thinking about it I finally agreed to do it, to place a ton of uranium nitrate—that's two thousand pounds—and then go through an intricate and complicated series of steps to purify the plutonium from all that uranium. We were going to make half a milligram, less than a needle in a haystack.

It was a big, dirty job, and dangerous, because uranium gets hot as a firecracker with radioactivity from all the fission products that accumulate—all the strontium-90 and all the cesium-137 and the radio-iodine, and everything else. I didn't know enough to have good sense, but I knew that it was dangerous.

To make a long story short, we bombarded the uranium night and day for six or seven weeks. I set up a small factory and built it on the Berkeley campus. In three weeks we isolated what turned out to be not half a milligram, but 1.2 milligrams of plutonium. Pure. In about a quarter of a teaspoon of liquid, out of this ton. I gave it to the Los Alamos Lab.

So I was the first chemist in the world to isolate milligram quantities of plutonium, and the third chemist in the world to work with it. We knew nothing of its biological problems.

I got a good radiation dose in doing that work. I feel that since that time, with each year that's passed, I consider myself among the lucky, because some of the people who worked closely with me in the Lawrence Radiation Lab died quite prematurely of leukemia and cancer. I'm still at a very high risk, compared to other people because of the dose I got. I probably got a hundred, hundred and fifty rems in all my work. That's a lot of radiation. And damn stupid, but nobody was thinking about biology and medicine at that point. We were thinking of the war. So we did it.

For the next few years Gofman continued working to develop processes for separating plutonium. "It was already clear that we were going to have big reactors running at Hanford, Washington, to try to make enough pounds of plutonium to make a bomb, and they'd need to be able to separate it." The process Gofman had worked out in Berkeley to separate one milligram of plutonium was a candidate process. After working intensively on the project, Gofman decided in 1944 that he was no longer needed. "I felt that from here on out it was strictly engineering work. We didn't know if the war would last one year or ten. I didn't want to do engineering work—not that I was against the bomb or anything—I just felt the project didn't need my kind of talent any more.

Gofman applied to the second-year class at the University of California Medical School and was accepted in their accelerated program. He was still a medical student when the bombs were dropped on Hiroshima and Nagasaki. "When I heard the announcement of the explosion of an atomic bomb, I knew they'd completed the project. That was my only reaction." He finished medical school in 1946 and did his internship in internal medicine at the University of California Hospital in San Francisco. Then in 1947 he was offered an assistant professorship at the University of California, Berkeley, which he accepted.

Gofman remained in that position, teaching and doing research from 1948 to about 1961. He made a number of major discoveries working with cholesterol and lipoproteins.[7] By 1954 he had moved up to a full professorship and had become internationally known as a result of numerous publications on coronary heart disease. Then something happened which altered the course of things for him.

Early in the 1950s a controversial decision had been made to set up a second weapons laboratory in the United States.[8] The first weapons laboratory was at Los Alamos, New Mexico, where the atomic bomb had first been designed and tested. The second, the Lawrence Livermore National Lab, was set up at Livermore, fifty miles east of the University of California at Berkeley under the aegis of the University's Lawrence Radiation Laboratory, of which Gofman was a member. Much of Gofman's funding at the Lawrence Radiation Lab came from the Atomic Energy Commission, although at the time Gofman was not doing any radiation work himself. With the decision to set up

a new weapons laboratory, there were two parts to the Lawrence Lab—one at Berkeley and one at Livermore.

Ernest Lawrence called me in one day. We were good personal friends. "I'm worried about the guys out at Livermore," he said. "I think they may do some things to harm themselves. You're the only person who knows the chemistry and the medicine and the lab structure. Could you do me a favor and go out there a day or two a week and just roam around and see what the hell they're doing, and see that they do it safely? If you don't like anything they're doing, you can tell them that your word is my word, that either they change, or they can leave the lab."

So I decided to do it.

While I was out there—to have something to do between times of roaming around—I organized a Medical Department at the Livermore Lab. It was then a lab of about fifteen hundred people. It's now about seven thousand. I organized the Medical Department and served as the medical director. But I was there only a day or two a week. The rest of the time I was in Berkeley teaching.

In the course of my wandering around I got to know all the weaponeers who were working there. I worked with them, helped them with some of their calculations on health effects and problems of nuclear war, and so forth. They were making bombs, new bombs, hydrogen bombs, designing all the bombs within the nuclear subs, for missiles and so forth.

I stayed out there until, one day, in 1957, I thought, I've done this long enough. Besides, one of my former students, Dr. Max Biggs, had come back as assistant director of the Medical Department. It was time for me to go back to Berkeley, to teach and also return to my research. By about 1960 I decided that, although there was still a lot left to do in heart disease, the excitement of my early discoveries, the night and day work, wasn't there any more. I'm not very good at dotting I's and crossing T's. If it's not something really new and unknown, it's not something I want to do.

By then, two of my students were on the faculty and were doing very nice work. So I said, "I'm going to get out of the heart disease work totally. You take over." They did, and they're still there, doing fine work. I shifted my major emphasis to the study of trace elements in biology and worked hard on that from about 1959 to 1962.

In 1962 I got a call from John Foster, who was by then the director of the Lawrence Livermore Lab.

He said, "I'd like to have you come out." I'd met with him and worked with him during the years that I'd been at Livermore. He said, "We had a very interesting approach from the Atomic Energy Commission. They're on the hot seat because of this 1960s series of tests which clobbered the Utah milkshed[9] with radio-iodine. And they've been getting a lot of flak. They think that maybe if we

had a biology group working with the weaponeers at Livermore, such things could be averted in some way—like you'd advise us not to do this or to do this differently."

And I said, "So?"

He said, "They're willing to set up something very nice—like a biology and medicine lab at Livermore, with a very adequate budget, starting at three to three and a half million dollars a year. You know, we've got the best computer facilities in the country. We've got engineering talent coming out of our ears, and electronic and mechanical engineering. So you'd have support. What do you think of coming out here and setting that up?"

"That's crazy," I said. "I'm perfectly happy in Berkeley. I've got my research. I'm up to my neck in my trace element research. I've gone down from having to supervise fifty people in my heart disease project to where I now have three people working with me. And it's just the way I like to work. I can be in the lab, and I don't have to think about administrative details. And now you're telling me to come out and head a division and be back in the administrative field. I'll be out of the lab—"

"Oh, no, no, you won't be out of the lab. Just organize it. And after a year or two you can get back in the lab full time, but under circumstances that are much better than you'd ever have."

"Well, I can tell you one thing," I said. "I wouldn't consider giving up my professorship to take this thing, because I don't trust the Atomic Energy Commission."

He didn't seem surprised at that.

I said, "I don't think they really want to know the hazards of radiation. I think it's important to know, but I don't think *they* want to know."[10]

I kicked around the idea of going back to Livermore for a while. Sometimes you have a lapse of cerebration, and in one of those weaker moments I finally agreed that I would go to Livermore and do that job, because Johnny Foster said, "Listen, the AEC can't fight the University of California, the Regents, and this lab. And I can tell you one thing, if they try to prevent you from telling the truth about what you find about radiation, we'll back you and the Regents will back you, and they'll just have to eat it."

Well, those were nice words. I didn't completely believe them. But the Regents wrote me. The president of the university wrote me a letter of terms, stating that if for whatever reason I was unhappy about the Livermore set-up, or the AEC's behavior, I could return full time to my teaching with no further explanation.

So I cut my teaching down to 10 percent, and took two posts at Livermore—one as head of a new

bio-medical division, the exact mission of which was to calculate and do the experimentation needed to evaluate the health effects of radiation and radionuclide release from weapons testing, nuclear war, radioactivity in medicine, nuclear power, etc.—all of the atomic energy programs. And I was given a three million dollar budget to start. I pulled in ultimately about thirty-five scientists—some who'd worked with me before at the university, some from outside—and finally built up a division which was one hundred and fifty people total, with engineers, technicians, and so forth, including the thirty-five senior scientists. I also became an associate director of the entire laboratory. There were nine associate directors and a director. Anything in biology or medicine was my general area. As an associate director, once a week I was at directors' meetings that concerned all lab matters. So I was involved in the bomb testing and everything else.

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6. *J. Robert Oppenheimer:* a nuclear physicist involved in the Manhattan Project, and selected to head the Los Alamos Scientific Laboratory to lead the race for the atomic bomb. A respected scientist, he was later accused, in 1954, of being a Soviet agent.

7. *lipoproteins:* all the fatty materials in the body that are not soluable in water, such as fat and cholesterol, are not transported by themselves in the blood, but combine with certain proteins. These proteins are called lipoproteins.

8. J. Robert Oppenheimer opposed this second weapons laboratory. Edward Teller, a nuclear physicist involved in the Manhattan Project and credited with being "the father of the H-bomb," strongly supported the new lab. Teller won.

9. *milkshed:* an area where dairy cattle graze and provide milk. The 1961-1962 series of atomic tests resulted in radioactive fallout being carried to Utah. Between 1958 and 1961 Eisenhower and Khrushchev had informally stopped testing. Testing resumed in 1961 and was finally banned in the atmosphere in 1963.

10. The Atomic Energy Commission "had tried to ridicule Linus Pauling's calculations about strontium-90 and carbon-14 in the late fifties—for which Pauling got the Nobel Peace Prize. They said his calculations were wrong. I even got caught up in that mythology—thinking that Pauling might be wrong about the low-radiation doses causing all these diseases. I took the wrong position in 1957 on Pauling's work, saying, `Since we don't know the answer for sure, we should not impede progress'"—John Gofman.

A Visit to the Washington Office of the Atomic Energy Commission

A couple of disturbing things happened. Within a few weeks after I'd gone out to Livermore, I had a call from an Atomic Energy Commission official, who said, "You've got to come into Washington next week."

"What for?"

"I can't tell you over the telephone."

“Sure, I’ll come.”

I got there. There were five other guys from AEC-supported labs around the country assembled in a room, and this AEC official.

“The reason I called you together,” he said, “is we have a problem. We’ve got a man in the bio-medical division in the Washington AEC office by the name of Dr. Harold Knapp who has made some calculations of the true dose that the people of Utah got from the radio-iodine from the bomb tests in 1962. And he says that the doses were something like one hundred times higher than we’ve publicly announced.”

So this group of six people, of which I was one, said, “What do you want us to do?”

“We must stop that publication,” he said. “If we don’t stop that publication, the credibility of the AEC will just disappear, because it will be stated that we’ve been lying.”[11]

I said, “Well, what can we do? What do you want us to do? If Knapp has that evidence, then he ought to publish it.”

“We can’t afford to have him publish that evidence,” he said.

“But if it’s right, we can’t stop him. It’s not our job to stop him.”

He said, “Well, will you do this? Talk to him. Look at the data, and see if you can convince him that it would be better not to publish it.”

So he brought Knapp in the room and he left. Knapp was surly, and properly so. Because here was a guy that did a straightforward scientific job, and he had this evidence, and he wanted to write it up.

And he said to the group, “What’s wrong with what I’ve done?”

We hadn’t even seen his data yet.

He gave us his data and said, “Do you think I’m too high? Or do you think I’m right? Or too low?”

We looked at the data, and as a matter of fact, there were a few minor technical questions the people had to ask him, and then we concluded that the guy had a very good scientific story and it ought to be published. So we told Knapp he could leave, and the AEC person came back in.

“Did you get anywhere?” he asked.

“Yeah,” we said, “we think Knapp ought to publish his data and you face the music.”

He was very disappointed. But since the committee wasn't going to do anything—this is a matter of record now—do anything to help the AEC try to suppress scientific truth, Knapp did publish. And the sky didn't fall. Unfortunately, in this society it takes a hell of a lot more than revealing some awful things for the sky to fall.

But it taught me something that was very, very different from what Glenn Seaborg had told me. (By now my former professor was chairman of the Atomic Energy Commission.) When we had signed the contract for the Livermore work, I told him, "You know, Glenn, you ought to think twice about my being the head of this thing. Because I don't really give a damn about the AEC programs, and if our research shows that certain things are hazardous, we're going to say so. And so why don't you think twice about me taking this job?"

"Oh, Jack," he said, "all we want is the truth." And here within a matter of a few weeks one of his chief men at the AEC is asking us to help suppress the truth. So I came back to the lab and I told Johnny Foster, "Well, the first encounter with Washington was to help with a coverup."

And he said, "Well, how did you handle it?"

"We told them to go to hell."

He said, "That's fine. That's fine."

So there was no further flap from that. But it taught me something about the Washington office—that they would lie, coverup, minimize hazards. My worst suspicions were confirmed.

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11. "When I told them in '62 how high the dosage levels were, the deputy director of the Division of Operational Safety had this pitch: `Well, look, we've told these people all along that it's safe and we can't change our story now, we'll be in trouble.' And I told him, `Well, I know you guys have been telling them that, but I haven't, and I'm supposed to be studying fallout. So don't tell me what answers I have to get"—Dr. Harold Knapp, former member of AEC fallout studies branch. In Anne Fadiman, "The Downwind People: A Thousand Americans Sue for Damage Brought on by Atomic Fallout," *Life,* June 1980, p. 39.

Plowshare: A Minor Disagreement[12]

There was a project called "Plowshare"—peaceful uses of nuclear bombs. Big project. They wanted to dig a new Panama Canal with 315 megatons of hydrogen bombs. The current Panama Canal is not too good for large ships, and they were thinking of digging a deeper canal. They were going to implant hydrogen bombs and blow a big hole in the ground. Two places were being considered—Panama and Colombia—and negotiations were under way with those countries. They would place more bombs and blow them up, and finally dig this whole trench with bombs. But all

the radioactivity would spew into the atmosphere and over the countryside.

One of my first assignments was to figure the biological hazard of that, and I concluded by 1965 that the project was biological insanity. Just an awful thing for the biosphere. Kill a lot of people from radiation, from cancer eventually. Project Plowshare. Turning our swords into plowshares.

Even with the fragmentary knowledge we had then, I opposed the project—which did not earn me a lot of favor with the Atomic Energy Commission. They thought I was being obstructionist. But my objections didn't stop it at all.

What stopped it were U.S. efforts to negotiate a test-ban treaty. And the ability to have a test-ban treaty with ongoing shots for so-called peaceful nuclear explosives could always be shots that were really for military purposes. So they elected to stop that project temporarily. It was really nothing to do with the biological hazard that made them quit. It was because of these political negotiations to keep other countries who didn't yet have bombs from developing them. As though the ones that do have them can be trusted for anything.

In any case, in 1965 the bio-medical division got known in the lab as "the enemy within" because we opposed things like Plowshare. But it was really fairly good-natured. In no way did it interfere with my status in the lab. I did give up the headship of the department after two and a half years. I appointed one of my junior associates as chairman of the division so I could go back into the lab. I had a new project by then, on cancer and chromosomes and radiation. It was an area I was very interested in, and a new one for me.

Things went quietly until 1969.

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12. See Chapter 3, pp. 68-69 [the section from the chapter on Ernest J. Sternglass entitled "A Note About the Plowshare Program" —ratitor].

Sternglass Challenges the AEC

By 1969 Johnny Foster had gone on to head the Defense Department Research and Engineering, under McNamara, secretary of defense, and he was no longer head of the lab.

That year a man by the name of Dr. Ernest Sternglass, who had been studying infant mortality, published some papers saying that something on the order of four hundred thousand children might have died from radioactive fallout from the bomb testing. And "Esquire" published an article called "The Death of all Children" based on Sternglass's work.[13]

The AEC was desperately worried about this because they were just then trying to get the

antiballistic missile through Congress, and they thought if Sternglass's work was accepted, it might kill the ABM in the Senate. So they sent Sternglass's paper to all the labs. I got it, looked at it quickly, and wasn't sure what to make of it.

But Arthur Tamplin, one of my colleagues, was much more into that thing than I was. And I said to him, "Art, would you look at this?"

He came back about three weeks later and said, "I think Sternglass is wrong. His interpretation of that curve is not right."[14]

I'll say today—ten years later—the new evidence coming out suggests to me that Sternglass may have been right. But Tamplin's argument seemed good to me at the time. I felt he should write it up as a report. And he did, as an article to be published in the "Bulletin of the Atomic Scientists," stating that he thought Sternglass was wrong, and since people had raised the question, he estimated how many deaths had been caused by fallout. His estimate was four thousand, not four hundred thousand.

At Livermore, Tamplin became the "hero of the lab." He had countered this man who was saying that something was going to hurt the ABM program, which the lab was heavily involved in. So Tamplin was an absolute hero, even to someone like Edward Teller, who all through that period was also an associate director of the Livermore Lab.[15]

Tamplin wrote the paper and submitted it through the lab, to tell Washington what he thought of the Sternglass thing.

I saw one of the top lab officials with whom I got along very well, and he said, "Something's wrong. I don't know what's going on, but Washington AEC has called me up. They're very disturbed about Tamplin's paper and don't want him to publish it the way it is."

"*Disturbed* about Tamplin's paper!" I said. "He's the hero of the day. He saved their neck on the ABM program. What in the world can they be disturbed about?"

"Look, Jack," he said, "I don't know what they're disturbed about. It's not my area. Would you do me a favor and call this fellow at the AEC?"

So I called the AEC and told Arthur Tamplin, "You better be on the other line. Just in case. It's your work they're concerned about."

On the phone I said, "What's on your mind about Tamplin's study?"

"Oh," the AEC official said, "we like Tamplin's study."

I said, “Gee, I heard you were terribly disturbed about Tamplin.”

“No, no. We like Tamplin’s study. *Very* well.”

“So what’s the problem?” I said.

“Well, Tamplin has proved that Sternglass is wrong, and that four hundred thousand children did not die from the fallout. But he’s decided to put in that paper that four thousand did die. And we think that his refutation of Sternglass ought to be in one article—like the *Bulletin of the Atomic Scientists,* which is widely read—and that his four thousand estimate ought to be in a much more sophisticated journal.”

“Well,” I said, “I’ve talked to Arthur about this, and he says that doesn’t make sense, because if you publish an article saying Sternglass is wrong, the first thing anyone will ask you is what do you think the *right* number is?”

“No, the two things are just separate,” he said.

Arthur Tamplin was on the phone. I said, “Art, I don’t think it makes sense.”

“No, it doesn’t make sense to me.”

I said, “What in the world is the sense in separating these two things?”

And this AEC fellow said, “Well, one ought to be in a scientific journal. “

I said, “What you’re fundamentally asking for is a whitewash. And for my money, you can go to hell.”

That’s where we ended the conversation.

So I saw my friend at the lab, and he said, “Did you call Washington?”

And I said, “Yeah.”

“What was it?”

“They wanted a whitewash of Tamplin’s four thousand number.” I explained it to him.

He said, “What did you tell them?”

“I told them to go to hell.”

And he said, “Fine.”

That was April 1969. And I never heard a word more about it. Tamplin published that paper.

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13. See Chapter 3, p. 61 [the end of the "Westinghouse: 1952 - 1967" section of the chapter on Ernest J. Sternglass —ratitor].

14. The curve showed that infant mortality in the United States had been steadily declining, and that the decline had leveled off right after the atomic bomb testing. Dr. Sternglass's interpretation of this leveling off was that it was a result of more babies dying from radioactive fallout from atmospheric bomb testing. (See Sternglass, *Low-Level Radiation*.)

15. "In truth, Edward Teller ran the Livermore Lab, but for public purposes he liked it better to be known as only an associate director"—John Gofman.

The Harassment Starts: Low-Level Radiation

During the 1950s and 1960s the, Atomic Energy Commission maintained there was a “safe threshold” of radiation below which no health effects could be detected. This so-called safe threshold provided the justification for exposing American servicemen to atomic bomb tests, for permitting workers in nuclear plants to receive a yearly dose of radiation, and for operating nuclear power plants which released radioactivity to the environment and exposed the general population even during normal operation. But in the 1960s evidence began to come in from around the world—from the atomic bomb survivors,[16] from some people in Britain who had received medical radiation[17]—with estimates of the numbers of cancers occurring per unit of radiation. Gofman and Tamplin assembled these figures and concluded that there was no evidence for the AEC’s so-called safe threshold of radiation. In fact, they estimated that the cancer risk of radiation was roughly twenty times as bad as the most pessimistic estimate previously made.

When Gofman was invited to be a featured speaker at the Institute for Electrical, Electronic Engineers meeting (IEEE) in October 1969, he and Tamplin decided to present a paper on the true effects of radiation “So we gave this paper,[18] and said two things. One, there would be twenty times as many cancers per unit of radiation as anyone had predicted before, and two, we could find no evidence of a safe amount of radiation—you should assume it’s proportional to dose all the way up and down the dose scale.” The paper did not attract much public attention, only a small article in the “San Francisco Chronicle” and nothing in the national press. Senator Muskie was holding hearings on nuclear energy at that time[19] and invited Gofman to address the Senate Committee on Public Works. Muskie did not know about the paper given before the IEEE but invited Gofman because of his position as associate director of the Lawrence Livermore Laboratory. Gofman gave an amplification of the paper he and Tamplin had presented at the IEEE meeting entitled, “Federal Radiation Guidelines Protection or Disaster?” This was picked up by the Washington press.

Within two weeks I began to hear all kinds of nasty rumblings that we were ridiculous, we were incompetent.

Here I'd been getting a budget of three to three and a half million dollars a year for seven years, and suddenly I'm hearing rumors out of Washington that my work is incompetent. That wasn't a criticism of me. That was a criticism of them. If they give someone three million a year for seven years and in two weeks they suddenly decide he's incompetent, what's wrong with them for seven years?

It was obviously related to what we'd said.

A guy from Newhouse News Service phoned me and said, "I have a statement from a high official on the Atomic Energy Commission, and I asked him about your cancer calculations, and he said that you don't care about cancer at all. All you're trying to do is undermine the national defense."

I said "Me, undermine the national defense?"

He said, "What do you have to say about that?"

"Nothing."

"You're not going to deny it?"

I said, "Do you think I would lower myself to deny a statement like that?"

He said, "You wouldn't be considering a lawsuit for libel if I publish that statement?"

"What I consider doing is my business," I said. "You're a journalist. You've got a story. If you'd like to publish that story, you go ahead and you take your chances, but I'm not going to tell you whether I have in mind a libel suit or anything else. You just do what you want with it."

"You're not going to deny the story?"

"No. I'm not even going to comment on something that low."

He never published the story.

The next thing we experienced was this. I'd had an invitation about four months before, to come and give a talk in late December '69. It was to be a symposium on nuclear power and all the questions about it. And I'd said to the person inviting me, "You know, the kinds of things you want from me are much better handled by Arthur Tamplin, because that's been the area he's worked on.

Instead of me, could he give the talk?"

"Oh, that's just fine," they said. "We wanted to be sure to have one of your representatives there."

So he was scheduled to give the talk on December 28.

Well, this friend of mine at the lab asked to talk to me right after the Muskie hearing. And he said, "Jack, I have a problem. The AEC has contacted me, and they're very disturbed about your IEEE talk and your Muskie testimony."

"What are they disturbed about? I've sent them the paper, sent it out to a hundred scientists. If they're disturbed, they can tell me what's wrong with it."

"No, no. They're not saying that," he said. "What they're saying is that it's just embarrassing to them to have these things given at a meeting and then in testimony before they've had a chance to review it. If you would just in the future do me one favor, send them your papers—your testimony—before you give it, I think the whole problem would be solved. They just don't want to be caught unawares."

"Well," I said, "that's very reasonable. Sometimes we have a scientific paper ready, sometimes we don't, to give it to them three weeks in advance or so. But we'll try."

I talked to Arthur Tamplin. He said, "Sure, what do I care. They can have it."

His paper was about a month from delivery before the American Association for the Advancement of Science. So I said, "Would you give me a copy of it for the lab to send to the AEC so they can scan it?"

So he did. Three days later Tamplin came into my office mad as hell, and threw this thing down on my desk.

Apparently someone in the lab had done some editing on it, and the editing was such that all that was left was the prepositions and conjunctions. All the meat was gone. This hadn't even gone to Washington. It was our own laboratory that had censored it! My own colleagues who were going to protect us from censorship.

I went over to my friend and said, "What the hell is going on? When you asked me if we'd give the papers to the AEC in advance, I told you I wouldn't tolerate any censorship. And you said, `Jack, do you think I would tolerate any censorship?'"

He said, "Jack, be realistic."

“I’m very realistic,” I said. “We’re just not going to tolerate any form of censorship.”

“You’re overwrought.”

“Listen, you know what I’m going to do? I’m calling up the guy from that meeting from the American Association. I’m going to tell him what has been told to Tamplin—that if he gives the paper unaltered, he cannot say he’s a member of the Livermore Lab, he must pay his own travel expenses, and cannot use a lab secretary to type the paper.”

That’s what the lab had told him!

I said, “I’m going to call the AAAS[20] and tell them I’ll send a letter instead of Tamplin going to the meeting. In the letter I’m going to say that the Livermore Laboratory is a scientific whorehouse and anything coming out of the Livermore Lab is not to be trusted.”

“Jack, you’re just excited,” he said. “Go home. Think it over. Let’s talk tomorrow.”

I said, “I’m really very cool, but if you want to talk about it tomorrow, that’s okay. You know what I’m going to do.”

The next day he came over to my office. “Well, did you get some sleep and think it over?”

“Sure, I got some sleep, and I’ve thought it over. And I also took care of what I told you I would.”

He said, “What do you mean?”

“I called the guy from AAAS and told him what I was going to do, that I was going to submit this letter to be read before the assembled public meeting, that the Livermore Lab is a scientific whorehouse and practices censorship.”

He turned all colors and just stormed out of my office.

Well, the upshot was the lab backed off on virtually everything—Tamplin could have lab funds and so forth. A couple of minor modifications to the paper, which Tamplin agreed to and they removed the censorship. So my statement was never read and Tamplin did go to the meeting.

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16. See Chapter 2, p. 38 ["Author's Note" section from the chapter on Rosalie Bertell] and Chapter 3, p. 62 [first half of "The Decision to Leave Westinghouse" from the chapter on Ernest Sternglass —ratitor].

17. Dr. Alice Stewart's epidemiological study showed that pregnant women receiving diagnostic X-rays had children with a higher risk of leukemia and cancer. See Alice Stewart, Josefine Webb, and David Hewitt, "A Survey of Childhood Malignancies," *British Medical Journal* (1958): 1495-1508, and Alice Stewart and George Kneale, "Radiation Dose Effects in Relation to Obstetric X-rays and Childhood Cancers," *Lancet* (197O):

1185-1188.

18. Gofman, "Low Dose Radiation, Chromosomes and Cancer," presented at the Institute for Electrical Electronic Engineers (IEEE) Nuclear Science Symposium, San Francisco, 29 October 1969, pp. 640-652.

19. Gofman, "Federal Radiation Council Guidelines for Radiation Exposure of the Population at Large: Protection or Disaster" presented to the Senate Committee on Public Works, 18 November 1969, in *Environmental Effects of Producing Electric Power,* pp. 695-706.

20. [20] *AAAS:* American Association for the Advancement of Science.

The Decision to Fight: January 1970

Gofman had resigned from his position as associate director of the laboratory six months prior to this episode with Tamplin, although he remained in the Livermore Laboratory as a research associate. "The resignation of my associate directorship had nothing to do with politics. I just thought it was time to go back to teaching."

Gofman was now teaching part-time at Berkeley and spending half of his time at Livermore doing research. In January 197O he learned that Tamplin had been stripped of twelve of his thirteen staff people.

I went back to my friend at the lab, and said, "You son of a bitch! What you're doing is so obviously just harassment to please the Atomic Energy Commission. I didn't think you could stoop this low."

"Jack, it's not that," he said. "Tamplin didn't want those people."

"Don't tell me Tamplin didn't want those people. I know what Tamplin wants. And he didn't want to lose any of them. He's got a lot of work to do, and so do I on the radiation hazard question. You've looked at our calculations. What the hell are you harassing Tamplin for?"

"It's not harassment," he said. "It's just that the laboratory budget was cut."

"The laboratory budget was cut 5 percent and Tamplin was cut 95 percent. That doesn't make any sense."

But it stuck. I wasn't able to undo it. I wrote a letter of complaint to Glenn Seaborg, and he said, "I can't interfere with lab management." Which was bullshit too.

Then I started hearing that there were a lot of people from the electric utility industry who were insulting us and our work. They were saying our cancer calculations from radiation were

ridiculous, that they were poorly based scientifically, that there was plenty of evidence that we were wrong. Things like that. So I wondered what was going on there. At that point—January 1970—I hadn't said anything about nuclear power itself. In fact, I hadn't even thought about it. It was stupid not to have thought about it. I just wondered, Why is the electric utility industry attacking us?

I began to look at all the ads that I had just cursorily seen in "Newsweek" and "Time" and "Life," two-page spreads from the utilities, talking about their wonderful nuclear power program. And it was all going to be done "safely," because they were never going to give radiation above the safe threshold.

And I realized that the entire nuclear power program was based on a fraud—namely, that there was a "safe" amount of radiation, a permissible dose that wouldn't hurt anybody. I talked to Art Tamplin. "They have to destroy us, Art. Because they can't live with our argument that there's no safe threshold."

He said, "Yeah, I gathered that."

"So," I said, "we have a couple of choices. We can back off, which I'm not interested in doing and you're not interested in doing, or we can leave the lab and I go back to my professorship and you get a job elsewhere, or we can fight them. My choice is to fight them."

He said, "I agree."

Congress Hears the Evidence

The system used to discredit scientists like us is usually to call you before the Joint Committee on Atomic Energy—it's a Congressional committee—and they let you present your evidence, and then they get all their lackey scientists, the ones who are heavily supported, to come in and say why you're wrong.

So I got the call just like I expected to from the Joint Committee. Would I come in on January 18, 1970 to testify?

I said, "Art, just as expected, they're ready to slice our throats at a Congressional hearing. We've got a lot more evidence that's sort of undigested than we had when you gave your paper and we gave the one at the Muskie hearings."

In about three weeks we wrote fourteen scientific papers. I'd never done anything like that in my life. And we learned new things. Stuff was falling together. We took on the radium workers. We took some data on breast cancer. There was a whole study of radium workers and their deaths. A guy at MIT had said they wouldn't get cancer below the safe threshold. We pointed out his papers

were wrong. There were the uranium miners, who were getting lung cancer. And we analysed that and showed how it also supported the idea that there was no safe dose. We studied the dog data. Studies were being done at the Utah laboratory and sponsored by the AEC—they were irradiating dogs and studying how many cancers appeared. We took a whole bunch of new human and animal data and wrote fourteen additional papers that buttressed our position, that indicated, as a matter of fact, that we'd underestimated the hazard of radiation when we'd given the Muskie testimony.

We were going to take all this as evidence before the Joint Committee. But I wanted to be sure that our material got out to about a hundred key scientists in the country in case the AEC tried to prevent us access via the journals.

—That's always something you have to worry about. The journals can easily not publish what you want to say. It's a simple technique. If the journals have editors and staffs supported by an industry or government agency, you can be blocked from getting your things published.

So to be sure that people knew what we were saying, we sent our material around to about a hundred separate scientists to let them know what we were doing.

I went to the lab and said, "I want 400 copies Xeroxed." We had put together 178 pages.

The dwarves who occupy such positions of course immediately ran to the master and said, "Gofman wants 400 copies of this! Do we have to do it?"

And so he came to me. "What's this 400 copies of 178 pages?"

"Well, the chairman of the Joint Committee on Atomic Energy has requested that we testify. We need 200 copies to send them, and I need 200 copies for other distribution. If you prefer, I'll call up Mr. Holifield, the chairman of the Joint Committee, and tell him the laboratory even wants to censor things from Congress."

"Oh, no, no. Don't do that!" he said. "We'll do the papers. I just wanted to know what you needed them for."

So we shipped off our 200 copies to the Joint Committee. Their purpose was, of course, to distribute the papers to the people that they were going to get to come in and attack us.

January 28 was the day. I presented the evidence based on these fourteen additional papers.

At the end of the testimony, Mr. Holifield said, "Now I certainly appreciate your presenting this material, Dr. Gofman. You realize that with 178 pages of testimony we haven't had all the time it would take to digest it in detail, but we'll invite you back sometime."

They didn't have any answers. Their people were just caught flat-footed, and meanwhile we'd gotten things out to a lot of people—a much stronger story. Their little escapade failed.

One of the guys we had mailed the papers to called me up. He was in the Public Health Service, in a division separate from AEC. It was on a weekend.

"I've got something disturbing to tell you," he said, "but if I tell you and you ever want to use it legally, I'll deny that I told you."

"That sounds like terribly useful information," I said. "I can't use it, but you think I ought to know it. Well, go ahead."

"Someone from the AEC came to my house last weekend," he said. "He lives near me. And he said, `We need you to help destroy Gofman and Tamplin.' And I told him you'd sent me a copy of your paper, and I didn't necessarily agree with every number you'd put in, but I didn't have any major difficulties with it either. It looked like sound science. And—you won't believe this—but do you know what he said to me? He said, `I don't care whether Gofman and Tamplin are *right* or not, scientifically. It's necessary to destroy them. The reason is,' he said, `by the time those people get the cancer and the leukemia, you'll be retired and I'll be retired, so what the hell difference does it make *right now*? We need our nuclear power program, and unless we destroy Gofman and Tamplin, the nuclear power program is in real hazard from what they say.' And I told him no. I refused. I just want you to know if you ever mention this, I'll deny it. I'll deny that I ever told you this, and I'll deny that he said it to me."

"Well," I said, "it's nice to know. We realized that we were in a war to the death, and that there was no honor, no honesty in the whole thing, but that's the way it is. You're not going to stand behind what you found out. That's okay with me too."

Abolishing the Atomic Energy Commission

By now I was convinced that nuclear power was absurd and fraudulent, that there was no safe level of radiation. Tamplin and I were writing and giving talks against nuclear power. In June 1970 I gave testimony at the Pennsylvania state legislature, recommending that all new construction of nuclear power plants cease—at least for five years—till the whole problem was sorted out. Our stock at the Livermore Lab was zero.

But we couldn't get them to fire us. They wouldn't do that. If they fired us, it would be an admission that they couldn't tolerate the truth. We put out more and more reports that were scientifically damaging to the atomic energy program. Meanwhile our Muskie testimony[21] had gotten very wide notice in the press, and Ralph Nader had entered the action and was asking Muskie what he was going to do about this testimony if it was so damaging to the nuclear power program. Muskie contacted Robert Finch, secretary of HEW, and said, "What are you going to do

about this study of Gofman and Tamplin's?"

So Finch went to the National Academy of Sciences and said, "I call for a study of whether Gofman and Tamplin are right," and awarded the National Academy three million dollars to do a study. Some sixty scientists were invited to participate.

At no time did the National Academy of Sciences invite either Tamplin or me to be on this committee or contact us—from 1970 to today. But in 1972 the National Academy of Sciences published a report called the BEIR Report—Biological Effects of Ionizing Radiation—a long, thick report, in which they walked around the problem as best they could, and finally concluded that we were too high between four and ten times. But if you read the fine print, they were admitting that we might just be right.[22]

When that came out, everybody realized that the AEC was not worth a damn. By then the AEC had gotten themselves into another flap. Henry Kendall and Dan Ford of the Union of Concerned Scientists showed that the AEC didn't know whether the Emergency Core Cooling System would ever work or wouldn't.[23] The Emergency Core Cooling System was the last barrier of safety in a major nuclear accident. This further damaged the credibility of the AEC.

Those two events—the conflict with Ford and Kendall and the conflict with us—finally led them to realize they could no longer use the words "Atomic Energy Commission," and so the government abolished the AEC.

"We are now solving the problem," they said. "We'll create two new agencies—ERDA (Energy Research and Development Agency) and NRC (Nuclear Regulatory Commission)."

ERDA was supposed to promote the development of atomic energy, and NRC was supposed to concern itself with public safety. The idea was that it was the promotion of nuclear energy that made the AEC's safety work so poor. The new NRC was only supposed to involve itself in safety—no promotion.

Which turned out to be one of the greatest lies in history.

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21. Hearings on nuclear energy were held in 1969 by Senator Muskie. Dr. Gofman delivered a paper entitled, "Federal Radiation Guidelines: Protection or Disaster?" which was an amplification of the talk given at the IEEE meeting three weeks before.

22. National Academy of Sciences, "The Effects on Populations of Exposure to Low Levels of Ionizing Radiation," report of the Advisory Committee on the Biological Effects of Radiation (BEIR Report), November 1972.

23. Daniel F. Ford and Henry W. Kendall, *An Assessment of the Emergency Core Cooling Systems Rulemaking Hearings* (San Francisco: Friends of the Earth/Union of Concerned Scientists, 1974.)

Resigning from Lawrence Livermore

Meanwhile I continued my work on cancer and chromosomes in the Livermore Laboratory. We continued to put out reports on the radiation hazard problem. In 1972 one of the people at the lab came to me.

"We have a problem," he said. "Now you may not believe this, John, but last year the AEC came to me and said, `We need to take Gofman's money away that he has for his cancer chromosome work' [which was $250,000 a year] and we told the AEC that while we disagreed with your position on nuclear power, we thought your cancer chromosome work was first-class science, and we were not going to remove your funds. And they let it go. But this time they've come back and said, `If you don't remove Gofman's funds, then we will remove $250,000 from the lab budget. You can fire other people if you insist on keeping Gofman's program.' So what do you want us to do?"

"Under no circumstances can anybody lose their funding because of my problem," I said. "I'll tell you what I'll do. I'll go back to the National Cancer Institute and see if I can get $250,000 to move my program to Berkeley with my professorship, and then I'll resign from the lab if I can."

So I went and saw the head of the National Cancer Institute. We talked about three hours.

I said, "You know all about the conflict with AEC?"

"I know all about the conflict," he said. "We like your program. We need it. It might take me three or four weeks to arrange it, but I think I can get you the money."

So I went back and told the lab it looked good.

Three or four weeks passed, and I didn't hear anything. Six weeks passed, and I didn't hear. So I dropped the head of the National Cancer Institute a note. I didn't want to press him because those things can take longer.

Then the strangest thing happened. I got back a letter from one of his third-echelon deputies, saying, "Thank you very much for your inquiry. Your work on cancer and chromosomes is not a mainline interest of the National Cancer Institute. We cannot fund it under any circumstances. But don't be discouraged about further applications at some later time on some other programs to the National Cancer Institute. Sincerely yours."

So I realized what must have happened. The head of NCI had probably talked to some other people in the government and gotten the word back. "This guy has just created nothing but havoc for the AEC, and now you're going to take him on to do the same thing for the National Cancer Institute?

You need to support Gofman like a hole in the head!"

I went to my contact in the lab and said, "I've failed. I know of no other source to accumulate $250,000 a year. So tomorrow I'll let all the people know that the program has ended. You can reassign them to other work."

As long as they weren't working with me, it was fine with the AEC. So the AEC won. They managed to destroy my cancer research program.

He said, "What are you going to do personally?"

"Well, I have a few more things I'd like to write up," I said. "But let's figure about six months, and then I'll resign from the lab."

"You know," he said, "you don't have to resign."

"Yeah, I know. But that's what I choose to do, and I'll go back to Berkeley full time, without the research. I'd like to keep my secretary and one assistant for the six months."

"Oh," he said, "that's just fine." It was really funny—he said, "Gee, you're driving out here fifty miles a day. Couldn't we make the last six months a little more comfortable for you? You know, we could get you space in the Berkeley division of the Lawrence Lab and you wouldn't have to drive out here."

"Well, that's very nice," I said. "As a matter of fact, I'll take you up on it."

So they arranged space for me in one of the buildings of the Berkeley Lawrence Lab, and I spent the last six months there, except for my teaching, which I was already doing half time.

And on February 1, 1973 I resigned formally and became a full-time professor.

I had made one mistake. If the Department of Energy or the AEC gives you money on a sensitive subject, they don't mean for you to take the job seriously. They need you—with your scientific prestige—so they can point to you. "We have so and so studying the problem." Studying the problem is marvelous. But if you want the money and the continued support, you should go fishing or play golf. My mistake was I discovered something.

After Resigning from Lawrence Livermore

Tamplin stayed on about another year and a half in the lab as, in his words, a "non-person." He

had no staff, he worked alone. Then he joined the Natural Resources Defense Council as a senior scientist.

When I got back to my own lab in Berkeley, I thought, Well, the National Cancer Institute wouldn't give me the $250,000, but surely I can get a small grant to continue the kind of calculations we've been making on cancer, particularly since in a major symposium at Berkeley, Dr. David Levin got up and added further shock to the AEC by stating—and it's on record in that publication—We in the National Cancer Institute have checked out the Gofman calculations by a totally separate method and have come up with the same answers.

So I applied for a grant from NCI for $30,000 to continue my calculations on cancer and radiation. It was a good application. I figured, Gee, a $30,000 grant they're not going to refuse me. I got a letter back from them saying the grant's refused on the basis that this sort of work is better done by a committee than by an individual. It was a revelation to me.

It seemed to me that I must be on a list of "enemies of the state." I never saw a list, but you know it was the Nixon administration, and Richard Nixon was said to have such a list, so I concluded that very likely I couldn't get any money from federal sources at all.

"The AEC Made a Mistake Not to Get Rid of Me"

It's a hazardous occupation, you ought to understand, to take the position that we ought to cancel the whole nuclear power program. It would probably have been wise for them to get rid of us—physically—in the early seventies.

Today I don't think it matters what they do to us because hundreds of thousands of people know about nuclear energy and its deficiencies. But at that time it was a very small group of people, and Tamplin and I were among the leading individuals giving the AEC trouble. Physically eliminating us from the scene would have been a useful thing to do. I don't know why they didn't. Of course there's always a hazard—you don't want to make martyrs. But you can have people have accidents on the highways and things like that. I sometimes wondered when I started my car whether it was going to explode. . . .

Funny thing—when the fire occurred in 1973, at the height of the period when we were really giving nuclear energy the most trouble, my friends said, "I heard the AEC burned your house down."

So I said, "That's crazy. It couldn't have been the AEC. Listen, the house next *door* caught on fire, and ours caught fire from *it*."

They said, "Do you think they'd start a fire *in* your house?"[24]

24. Gofman's sister-in-law was in his house. She was the only one there at the time. She got out safely. The house burned to the ground. Gofman says, "I have no evidence that the AEC had anything whatsoever to do with it."

Researching Plutonium: The Cancer Hazard

After leaving the Livermore Laboratory and finding that he could not get government funding for his research, Gofman was not sure what to do with his life. "Personally I am not cut out for the social scene. You know, I'm most comfortable in a laboratory, working with instruments and materials, and not seeing people. I don't like going to public things. People can change, but if you've been doing something you like to do for something like thirty years, to try to develop a new format of things you prefer is difficult."

Gofman decided to take an early retirement at the age of fifty-five, so he gave up his position at the University in 1975 and became professor emeritus. Although no longer engaged in active teaching, Gofman did not give up research. In the next years he discovered that plutonium was even more hazardous than he had thought. "Plutonium is so hazardous that if you had a fully developed nuclear economy with breeder reactors fueled with plutonium, and you managed to contain the plutonium 99.99 percent perfectly, it would still cause somewhere between 140,000 and 500,000 extra lung-cancer fatalities each year."

There are no commercial breeder reactors operating at this time (1981) in the United States. However, breeder reactors are planned, are even now prefabricated, waiting in storage for a go-ahead on construction. The Clinch River reactor, for example, is a fast breeder proposed for a site in Oak Ridge, Tennessee. All the components have been built by Westinghouse and are now stockpiled in warehouses. Every year the U.S. Congress appropriates millions of dollars for the Clinch River project and advanced breeder technology research.

Breeder reactors have a plutonium core surrounded by a blanket of U-238, a nonfissionable isotope of uranium. When the plutonium fissions, it gives off fast neutrons that hit the U-238 atoms, converting the uranium blanket to plutonium. Thus the breeder reactor produces, or "breeds," more plutonium than it starts with. Plutonium is the ingredient essential for producing nuclear weapons.

As the nuclear industry readies itself for full-scale breeder development, reports on the carcinogenic nature of uranium are suddenly receiving widespread coverage in the press. The "San Francisco Chronicle," for example, reports that the "breeder reactor would likely reduce the number of occupational deaths associated with the nuclear industry, since it largely operates on

plutonium and thus would reduce the need for the uranium that fuels existing atomic power plants."[25] It also quotes a U.S. official as stating that "the dominant factor by at least a factor of 100 in real fatalities is in uranium mining." There is a cruel irony in admitting the danger of uranium mining only to seduce the American public into accepting an even more treacherous plutonium technology.

Breeder reactors will lead to a nightmare "epidemic of lung cancer in this country,"[26] and widespread weapons proliferation. In themselves, breeder reactors are extremely dangerous. They use liquid sodium as coolant, which ignites and violently explodes on contact with air. The plutonium fuel, if ignited, can produce a nuclear explosion equivalent to an atomic bomb, which would rupture the reactor's containment building and release enough deadly radioactivity to kill millions of people.

The requirement for controlling plutonium in a nuclear economy built on breeder reactors would be to lose no more than one millionth or ten millionth of all the plutonium that is handled into the environment where it could get to people. Which brings up a fundamental thing in nuclear energy —there are some engineers, scientists, who are not merely fraudulent sycophants of the system. They're really out of touch with reality.

I was once on an airplane with a strong pronuclear engineer. I said, "I've done some new work on plutonium. I think it's a lot more toxic than had been thought before. At what toxicity would you give up nuclear power?"

He said, "What are you talking about?"

"If I told you that you had to control your plutonium losses at all steps along the way—burps, spills, puffs, accidents, leaks, everything—that you can't afford to lose even a millionth of it, would that cause you to give up nuclear power?"

"Oh, I understand your point now, John," he said. "Now, you tell me—we look to biologists like you to tell us how well we need to do. If you say I've got to control it to one part in ten million, we'll do it. If you say it's got to be one in a billion or ten billion we'll do it. You tell us what we have to engineer for, and we'll do it."

I said, "My friend, you've lost touch with reality completely. I've worked in chemistry laboratories all my life, and to think you can control plutonium to one in a million is absolutely absurd. If you were a patient of mine who came in to see me, I'd refer you to a psychiatrist."

"Well, John, engineering is my field. And we believe we can do anything that's needed."

Engineers do believe that. That's the arrogance of engineers—they think they can do anything.

Now their mistakes catch up with them, as you see from the DC-10s and the Tacoma Narrows Bridge that fell down, and the Teton Dam and the most recent episode, Three Mile Island—where the unthinkable, the impossible, did happen.

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25. "Fast-Breeder Reactor Backed—Jolt for U.S.," *San Francisco Chronicle,* 26 February 1980, p. 5. The uranium hazard arises from the exposure to radon gases in the uranium mines.

26. John W. Gofman, *"Irrevy," An Irreverent, Illustrated View of Nuclear Power* (San Francisco: Committee for Nuclear Responsibility. 1979), p. 105.

Nuclear Power: A Simple Question

Many people think nuclear power is so complicated it requires discussion at a high level of technicality. That's pure nonsense. Because the issue is simple and straightforward.

There are only two things about nuclear power that you need to know. One, why do you want nuclear power? So you can boil water. That's all it does. It boils water. And any way of boiling water will give you steam to turn turbines. That's the useful part.

The other thing to know is, it creates a mountain of radioactivity, and I mean a *mountain:* astronomical quantities of strontium-90 and cesium-137 and plutonium—toxic substances that will last—strontium-90 and cesium for 300 to 600 years, plutonium for 250,000 to 500,000 years—and still be deadly toxic. And the whole thing about nuclear power is this simple: can you or can't you keep it all contained? If you can't, then you're creating a human disaster.

You not only need to control it from the public, you also need to control it from the workers. Because the dose that federal regulations allow workers to get is sufficient to create a genetic hazard to the whole human species. You see, those workers are allowed to procreate, and if you damage their genes by radiation, and they intermarry with the rest of the population, for genetic purposes it's just the same as if you irradiate the population directly.[27]

So I find nuclear power this simple: do you believe they're going to do the miracle of containment that they predict? The answer is they're not going to accomplish it. It's outside the realm of human prospects.

You don't need to discuss each valve and each transportation cask and each burial site. The point is, if you lose a little bit of it—a terribly little bit of it—you're going to contaminate the earth, and people are going to suffer for thousands of generations. You have two choices: either you believe that engineers are going to achieve a perfection that's never been achieved, and you go ahead; or you believe with common sense that such a containment is never going to be achieved, and you

give it up.

If people really understood how simple a problem it is—that they've got to accomplish a miracle—no puffs like Three Mile Island—can't afford those puffs of radioactivity, or the squirts and the spills that they always tell you won't harm the public—if people understood that, they'd say, "This is ridiculous. You don't create this astronomical quantity of garbage and pray that somehow a miracle will happen to contain it. You just don't do such stupid things!"

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27. See Chapter 2, p. 45 [second half of the "After Leaving the New York State Department of Health" section of the chapter on Dr. Rosalie Bertell —ratitor].

Licensing Murder

Licensing a nuclear power plant is in my view, licensing random premeditated murder. First of all, when you license a plant, you know what you're doing—so it's premeditated. You can't say, "I didn't know." Second, the evidence on radiation-producing cancer is beyond doubt. I've worked fifteen years on it, and so have many others. It is not a question any more: radiation produces cancer, and the evidence is good all the way down to the lowest doses.

The only way you could license nuclear power plants and not have murder is if you could guarantee perfect containment. But they admit that they're not going to contain it perfectly. They allow workers to get irradiated, and they have an allowable dose for the population.[28] So in essence I can figure out from their allowable amounts how many they are willing to kill per year.

I view this as a disgrace, as a public health disgrace. The idea of anyone saying that it's all right to murder so many in exchange for profits from electricity—or what they call "benefits" from electricity—the idea that it's all right to do that is a new advance in depravity, particularly since it will affect future generations.

You must decide what your views are on this: is it all right to murder people knowingly? If so, why do you worry about homicide? But if you say, "The number won't be too large. We might only kill fifty thousand—and that's like automobiles"—is that all right?

People have told me they agree with my calculations. One of the associate directors at Livermore actually said to me, "Jack, you have a right to calculate that thirty-two thousand people would die from the standards we have in force. What I don't understand is why you think thirty-two thousand a year is too many."

"Look," I said, "if I didn't think thirty-two thousand were too many I'd give up my medical

diploma saying I didn't deserve it."

He didn't understand that.

People like myself and a lot of the atomic energy scientists in the late fifties deserve Nuremberg trials. At Nuremberg we said those who participate in human experimentation are committing a crime. Scientists like myself who said in 1957, "Maybe Linus Pauling is right about radiation causing cancer, but we don't really know, and therefore we shouldn't stop progress," were saying in essence that it's all right to experiment. Since we don't know, let's go ahead. So we were experimenting on humans, weren't we? But once you know that your nuclear power plants are going to release radioactivity and kill a certain number of people, you are no longer committing the crime of experimentation—you are committing a higher crime. Scientists who support these nuclear plants—*knowing* the effects of radiation—don't deserve trials for experimentation; they deserve trials for murder.

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28. See Chapter 2, p. 31 ["Radiation Standards for Workers and the Public" section of the chapter on Dr. Rosalie Bertell —ratitor] on health effects of current allowable radiation doses.

First Strike Capability: "The Power Disease"

In the six years that I was on the Board of Directors at the bomb laboratory, I became more and more worried about nuclear weapons. One day, after a couple of years at Livermore, I said at a lab meeting, "Do you know, every week we get together and talk about the next bomb thing. This whole business of trying to solve any problems with nuclear weapons is ridiculous. We ought to be having our discussions about the sociopolitical aspects of missilery and nuclear weapons, not just about bomb design."

I was told, "You're wrong, John."

"What do you mean, I'm wrong?"

"Look, we're scientists. Our job is to design the best bombs we can. It's for statesmen and the politicians to figure out what ought to be done about it."

That bothered me a great deal. My thoughts were these: everybody thinks nuclear weapons are a way of deterring war. That's not true. Nuclear weapons are going to *lead* to war. I'll tell you why. If you're a weaponeer in the United States, what do you have to think about? Since you don't know the facts, you must assume that the Soviet weaponeers are trying to get in a position where they can either hand you an ultimatum or bomb you out of existence if they think they have what's

called a "first strike capability"—namely, the ability to bomb you out of existence without you retaliating. The only reason there hasn't been a nuclear war yet is that both sides realize that they haven't been in that technical position of being able to get away with it without too severe losses on their side.

Now some people say, "You don't need to worry. If one side gets a first-strike capability, the other side will get it too." That's not true. Scientific and technological advances are such that one side might get there six months early or a year earlier. Then they would be in a position to say, "Now we have the other side so they can't retaliate."

What would happen under those circumstances? Suppose the United States made the breakthrough.[29] "We've just figured out a way we can destroy the Soviet Union and not get any significant damage in return. We can either not use it, or we can use it."

Now why should we *not* use it? You say to yourself, "What if the tables were turned and *they* were the ones that reached this point? Would they also not use it?" I think there's a high chance that if one side gets that advantage, they'll use it. The only solution is, you must stop *all* efforts to develop first-strike force solutions everywhere—whether they be nuclear or other—and move toward a more just society.

Even if you made an agreement to abolish all nuclear weapons, but you left established power structure in the U.S. and the USSR, they'd go on to research mind control or some chemical or biological thing. My view is, there exists a group of people in the world that have a disease. I call it the "power disease." They want to rule and control other people. They are a more important plague than cancer, pneumonia, bubonic plague, tuberculosis, and heart disease put together. They can only think how to obliterate, control, and use each other. They use people as nothing more than instruments to cast aside when they don't need them any more. There are fifty million people a year being consumed in a nutritional holocaust around the world; nobody gives a damn about starvation. If fifty million white Westerners were dying, affluent Western society would worry, but as long as it's fifty million Third World people dying every year, it doesn't matter.

In my opinion, what we need is to move toward being nauseated by people who want to be at the top, in power. Can you think of anything more ridiculous than that the Chinese, Russian, and American people let their governments play with superlethal toys and subject all of us to these hazards? The solution is not to replace one leader with another or to have more government. Society has to reorganize itself. The structure we have now is, the sicker you are socially, the more likely it is that you'll come out at the top of the heap.

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29. The U.S. has more than thirty thousand nuclear bombs in its weapons arsenal at the present time and is capable of destroying the Soviet Union dozens of times over. The deployment of a whole new generation of

weapons—cruise missiles, the MX missile, and Trident missiles—suggests that the policy of "deterrence" is really a cover for the development of a "first-strike capability"—that the U.S. is actually planning for a nuclear war. See Robert Thaxton, "Directive Fifty-nine: Carter's New `Deterrence' Doctrine Moves Us Closer to the Holocaust," *Progressive,* October 1980, pp. 36-37.

BIBLIOGRAPHY

Fadiman, Anne. "The Downwind People: A Thousand Americans Sue for Damage Brought on by Atomic Fallout." *Life,* June 1980, pp. 32-40.

"Fast-Breeder Reactor Backed—Jolt for U.S." *San Francisco Chronicle,* 26 February 1980.

Ford, Daniel F., and Henry F. Kendall. *An Assessment of the Emergency Core Cooling Systems Rulemaking Hearing*. San Francisco: Friends of the Earth/Union of Concerned Scientists, 1974.

Gerber, C. R.; R. Hamburger; and E. W. S. Hull. *Plowshare*. Washington, D.C: Atomic Energy Commission, Understanding the Atom Series, 1967.

Gofman, John W. "The Cancer and Leukemia Consequence of Medical X-Rays." *Osteopathic Annuals,* November 1975.

_____. "The Cancer Hazard from Inhaled Plutonium." CNR Report 1975-IR. San Francisco: Committee for Nuclear Responsibility (CNR), 14 May 1975.

_____. "Estimated Production of Human Lung Cancers by Plutonium from Worldwide Test Fallout." San Francisco: CNR, 10 July 1975.

_____. "Federal Radiation Council Guidelines for Radiation Exposure of the Population at Large—Protection or Disaster?" Testimony presented to the Senate Committee on Public Works, 18 November 1969. In *Environmental Effects of Producing Electric Power.* Hearings before the Joint Committee on Atomic Energy (JCAE), 91st Congress. Part I, October-November 1969.

_____. "The Fission-Product Equivalence between Nuclear Reactors and Nuclear Weapons." *Senate Congressional Record, Proceedings and Debates of the 92nd Congress, 1st Session.* Vol. 117. Washington, D.C., 8 July 1971.

_____. *"Irrevy," An Irreverent, Illustrated View of Nuclear Power*. San Francisco: CNR, 1979.

_____. "Low Dose Radiation, Chromosomes, and Cancer." Presented at the Institute for Electrical, Electronic Engineers (IEEE) Nuclear Science Symposium. San Francisco, 29 October

1969.

_____. "The Question of Radiation Causation of Cancer in Hanford Workers." *Health Physics* 37 (1979): 617-639.

Gofman, John W., and Arthur R. Tamplin: "Epidemiologic Studies of Carcinogenesis by Ionizing Radiation." In *Proceedings of the Sixth Berkeley Symposium on Mathematical Statistics and Probability,* edited by J. Neyman. Berkeley: University of California Press, 1971.

_____. *Poisoned Power: The Case Against Nuclear Power Plants*. Emmaus, Pennsylvania: Rodale Press, 1971.

_____. *Population Control through Nuclear Pollution*. Chicago: Nelson-Hall Company, 1971.

Mayo, Anna. "John Gofman: An American Dissenter." *Village Voice,* 7 May 1979.

National Academy of Sciences. "The Effects on Populations of Exposure to Low Levels of Ionizing Radiation." Report of the Advisory Committee on the Biological Effects of Ionizing Radiation (BEIR Report), November 1972.

Pauling, Linus. *No More War!*. New York: Dodd, Mead & Company, 1958.

Seaborg, Glenn T., and William R. Corliss. *Man and Atom: Building a New World through Nuclear Technology*. New York: E. P. Dutton Company, 1971.

Segi, M.; M. Kurihara; and T. Matsuyama. *Cancer Mortality in Japan, 1899-1962*. Sendai, Japan: Department of Public Health, Tohoku University School of Medicine, 1965.

Shutdown! Nuclear Power on Trial. [Testimony of Dr. John W. Gofman and Dr. Ernest J. Sternglass.] Summertown, Tennessee: The Book Publishing Company, 1979.

Sternglass, Ernest J. "The Death of All Children." *Esquire,* September 1969.

_____. *Low-Level Radiation*. New York: Ballantine Books, 1972.

Stewart, Alice; Josefine Webb; and David Hewitt. "A Survey of Childhood Malignancies." *British Medical Journal* (June 1958): 1495-1508.

Stewart, Alice, and George W. Kneale. "Radiation Dose Effects in Relation to Obstetric X-Rays and Childhood Cancers." *Lancet* (June 1970): 1185-1188.

Teller, Edward, et al. *Constructive Uses of Nuclear Explosives*. New York: McGraw-Hill, 1968.

Thaxton, Robert. “Directive Fifty-Nine: Carter’s New `Deterrence’ Doctrine Moves Us Closer to the Holocaust.” *Progressive,* October 1980, pp. 36-37.

Underground Uses of Nuclear Energy. Hearings before the Subcommittee on Air and Water Pollution of the Committee on Public Works of the U.S Senate, 91st Congress, 1st Session, on S.3042, Part I, 18-20 November 1969. Includes testimony of John W. Gofman: “A Proposal for at Least a Ten-Fold Reduction in the Federal Radiation Council Guidelines for Radiation Exposure to the Population-at-Large: Supportive Evidence.” Presented to the JCAE, 28 January 1970; “IRCP [International Committee on Radiation Protection] Publication 14 vs. the Gofman-Tamplin Report.” Supplement to Gofman testimony, 28 January 1970.

Underground Uses of Nuclear Energy. Hearings before the Subcommittee on Air and Water Pollution of the Committee on Public Works of the U.S. Senate, 91st Congress, 2nd Session, on S. 3042, Part II, 5 August 1970. Includes testimony of John W. Gofman: “16,000 Cancer Deaths from FRC Guideline Radiation (Gofman-Tamplin) vs. 160 Cancer Deaths from FRC Guideline Radiation (Dr. John Storer): A Refutation of the Storer Analysis” Testimony to the JCAE, 9 February 1970.

The Hopi believe this is the Fourth World. There were seven worlds created at the beginning. The first three were each destroyed in turn because the humans inhabiting them had diverged too far from their original sacred path of connectedness with and respect for all life on Mother Earth. Their prophecies (see *Book of the Hopi* by Frank Waters) describe the possibility of such a destruction of the Fourth World (in the forms of uranium mining, the existence of powerlines, and the atomic bomb):

If we dig precious things from the land, we will invite disaster.

Near the Day of Purification, there will be cobwebs
spun back and forth in the sky.

A container of ashes might one day be thrown from the sky,
which could burn the land and boil the oceans.

KOYAANISQATSI

ko.yan.nis.qatsi (from the Hopi Language) **n.** **1.** crazy life. **2.** life in turmoil. **3.** life out of balance. **4.** life disintegrating. **5.** a state of life that calls for another way of living.