



DOE Openness: Human Radiation Experiments: Roadmap to the Project



# Oral Histories

- HOME
- ROADMAP
- WHAT'S NEW
- SEARCH HREX
- MULTIMEDIA
- RELATED SITES
- FEEDBACK

**Oral Histories**    DOE/EH-0455

# HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

## *Oral History of John W. Healy*



***Conducted November 28, 1994***

**United States Department of Energy  
Office of Human Radiation Experiments  
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## CONTENTS

Health Physicist  
William J. Bair,  
Ph.D.

Biochemist Waldo  
E. Cohn, Ph.D.

Dr. Patricia  
Wallace Durbin,  
Ph.D.

Merril Eisenbud

Dr. Nadine  
Foreman, M.D.

Radiologist  
Hymen L. Friedell,  
M.D., Ph.D.

Health Physicist  
Carl C.  
Gamertsfelder,  
Ph.D.

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

Radiation  
Biologist Marvin  
Goldman, Ph.D.

Julie Langham  
Grilly

John W. Healy

Hematologist Karl  
F. Hubner, M.D.

Oral History of  
Radiologist Henry  
I. Kohn, M.D.,  
Ph.D.

Medical Physicist

- Foreword
- Short Biography
- Early Career with Du Pont (1942 to 1944)
- Arrival at Hanford (1944)
- Early Environmental Monitoring Efforts (1946)
- Developing Precise Calibration Standards
- Operation Green Run
- Monitoring Successfully Detects the Soviets' Entry Into the Nuclear Age

Katherine L. Lathrop and Physician Paul V. Harper  
Pathologist Clarence Lushbaugh, M.D.  
Health Physicist Constantine J. Maletskos, Ph.D.

Unknown Health Hazards From Fallout  
Monitoring Livestock Exposure  
Monitoring Salmon in the Columbia River  
Discovering Unaccounted-For Release Hazards  
More About the Green Run  
Unexpected Change in the Weather Pattern  
U.S. Air Force Involvement in the Green Run  
Other Accidental Releases

Radiologist Earl R. Miller, M.D.

## FOREWORD

Health Physicist Karl Z. Morgan, Ph.D.

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 As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects.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

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Physiologist Nello Pace, Ph.D.

Cell Biologist Don Francis Petersen, Ph.D.

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

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

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

Donner Lab  
Administrator  
Baird G. Whaley

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

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## DISCLAIMER

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

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## ORAL HISTORY OF JOHN W. HEALY

*Conducted on November 28, 1994 in Los Alamos, New Mexico by Dr. Darrell Fisher, a health physicist from Battelle, Pacific Northwest Laboratory, and Marisa Caputo, Oral History Team Leader and Special Assistant from the Office of Human Radiation Experiments (OHRE), U.S. Department of Energy (DOE).*

*John W. Healy was selected for the oral history project because of his participation in the Green Run, the 1949 intentional radioiodine release at Hanford, Washington. The oral history covers Mr. Healy's early career at the Du Pont Company, environmental monitoring at Hanford, the Green Run, and the Hanford accidental ruthenium release in the 1950s.*

### Short Biography

Mr. John Healy, more commonly known as Jack, was born in Corry, Pennsylvania on May 9, 1920. He graduated as a Chemical Engineer from Pennsylvania State College (now Pennsylvania State University) in May 1942. Mr. Healy first went to work spinning and testing new fibers for the Pioneering Research Group, Rayon Division, Du Pont Company in Buffalo, New York. In the fall of 1944, Mr. Healy began a job in radiation protection at Hanford, Washington. Within several weeks of his arrival at Hanford, Mr. Healy found himself in the Special Studies Group, responsible for creating new means of radiation measurement and environmental monitoring. Mr. Healy remained at Hanford when General Electric acquired the managing and operating contract in 1946. Later positions held by Mr. Healy include the following two:

- 1960 to 1968-Technological Hazards Consultant, General Electric, New York
- 1968 to 1985-Staff Member, radiation hazards work, Los Alamos National Laboratory, Los Alamos, New Mexico.

### **Early Career with Du Pont (1942 to 1944)**

**CAPUTO:** Today is November 28, 1994. My name is Marisa Caputo. I'm with the Office of Human Radiation Experiments at the Department of Energy. With me is Dr. Darrell Fisher, from Battelle, Pacific Northwest Laboratories and Dr. [George] Voelz,<sup>1</sup> from Los Alamos National Laboratory, and we're interviewing Jack Healy. We're hoping to discuss Mr. Healy's work at Hanford and, specifically, the Green Run, and any other intentional releases he might have knowledge about. Could you give us a brief synopsis of your background and how you ended up at Hanford?

**HEALY:**

I graduated from Pennsylvania State College-now University-on, I believe, May 9, 1942 as a Chemical Engineer. This odd date was because the graduation was accelerated about a month so that graduates could join the new wartime effort. I went to work for the Du Pont Company in the Pioneering Research Group of their Rayon Division in Buffalo, New York. I was involved in spinning and testing new fibers with a two-cell hot-air spinning outfit. One fiber that I made extensive tests on was a substitute for kapok, a natural fiber used in life vests and other applications where buoyancy was important, but had been unavailable for some time because of the war. This product was called "bubble yarn" because it was a rayon monofilament spun in the normal manner except that a burst of air was inserted into the middle of the filament just before it entered the bath that would solidify the yarn. The bubbles could be made fairly large or small and could be spaced as desired. While they were successful, another substitute for kapok was found. I was told that by the end of the war, Du Pont had over a million dollars' worth of the material in storage.

About October of 1943, I was asked to go

Wilmington<sup>2</sup> and interview for a new job. At Wilmington, the head of the technical program-I believe his name was Dr. Thigpen-announced that the company was producing a material for a new atomic bomb and described the process to me. He also indicated that there were new hazards involved with ionizing radiation and radioactive materials. My job, if I accepted, would be in a group devoted to protecting people from these new hazards. I would initially go to a pilot plant near Knoxville, [Tennessee], at a town called Oak Ridge, for training, and within some months would move to Hanford in Washington State in time for operations. I was also cautioned that this information was secret and that I should tell no one-not even my wife. I went home in a bit of a daze because of my interest in science fiction. This seemed to be the ultimate in science fiction. I talked the moves over with my wife and finally agreed to take on the task.

On February 1 of 1944, I went to Oak Ridge. There were eventually 12 of us for training, under the leadership of Herbert Parker. The training consisted of morning lectures, mostly on the physics of radiation and radiation dosimetry, conducted by Dr. Karl Z. Morgan, a cosmic ray physicist. In the afternoons, we spread out through the plant and performed actual radiation protection work. My wife was due to have a baby in June-about the time we were slated to go to Hanford. I requested a delay of a few months so that the baby would be better able to make the trip. Herb Parker did not like the delay, but he finally agreed.

### **Arrival at Hanford (1944)**

**HEALY:** I finally went to Hanford in September or October of 1944. My original job was in environmental monitoring; we called it "site survey." When I arrived, I found they had discovered that the graphite used as a neutron moderator<sup>3</sup> in the reactors was storing energy, causing it to swell. It was unknown how this energy could be released, or how fast. If a mechanism causing it to be released rapidly was present, then there was the possibility of a major explosion. As a result, Herb Parker told the management that [in case of an

accident], he would have a car in the vicinity of the reactors at all times. Since the site survey crew consisted of two people-myself and Phil Lindvig-we divided the time for the next few weeks, and I spent the remaining time writing emergency plans while half-asleep.

A few weeks after my arrival, I was asked to meet with Phil Church, a meteorologist from the University of Washington, who had spent the last few years releasing smoke in the vicinity of the separation [plants] and measuring the amount of dilution in the atmosphere. This research served as the basis for a method to control dissolving times to periods when the atmosphere could best receive the gases. Dr. Church wanted me to spend time making measurements to check on these methods. I told him that I agreed with the importance of his proposal, but we only had two people [to perform the research, our] available instruments were not adequate enough to make quantitative measurements of the cloud, and, finally, our primary job was to protect people. It turned out that at Hanford, Dr. Church was a powerful person, and the next day I found I had a new job in a group called Special Studies. It consisted of two people: my new boss, Jane Hall, who later became Associate Director at Los Alamos, and myself. Leona Marshall, from the Technical Division, also worked closely with Jane. They were wonderful people and very helpful to a chemical engineer, who found himself [without a] physics background. Is that enough background?<sup>4</sup>

**CAPUTO:** Yes, that's great.

**FISHER:** This is interesting. We would like you to continue.

**HEALY:** My first job for Jane Hall, believe it or not, was to look for mesons<sup>5</sup> in the B reactor. This seems strange nowadays, but there was not that much known of fission at that time. It had only been a few months since they started the B reactor. It was supposed to run at 100 or 200 megawatts, [but] it reached one megawatt and just petered out because of fission product [poisons]. We really didn't know what went on completely in the fission process.

think Jane felt that this was a good opportunity to

at least look for mesons. I carried about four tons of lead to the third floor of the B reactor and set up a shielded coincidence counter.<sup>6</sup> I then took the leisurely little trip, at 35 miles an hour,<sup>7</sup> to B reactor every week to retrieve the charts and analyze them. We never did find anything, and finally the experiment was shut down. The only good part of it was that somebody else had to carry that four tons of lead down.

Du Pont was set up so that the Technical Division analyzed all samples in their Chemistry Department. If you had any samples to be analyzed, you would submit them to Technical, and they would do the analyses. The only problem was: they were swamped, and radioactive decay waits for no man! No schedule! We were taking samples from the reactor water-cooling basins to determine the quantities and types of radioactive materials being released to the river. The main [radioisotopes] were manganese-56, with a half-life of 15 minutes; sodium-24, with a 14.8-hour half-life; and even P[hosphorous]-32, with a 14-day half-life.<sup>8</sup> A delay of one month in getting a sample analyzed was rather serious. It became one of my jobs to set up a crude analytic technique. I had a lab in the 700 Area where the Site Survey Group would bring samples from the reactor. There, they would be evaporated on a watch glass and measured in a beta counter.<sup>9</sup>

### **Early Environmental Monitoring Efforts (1946)**

**HEALY:** At this time, the method for identifying radionuclides was to run a decay curve. That is, a number of measurements were made at different times, from which the half-lives of the radionuclides could be estimated. From a table of known half-lives, the radioisotope could then be identified. This got to be quite a tedious chore, since there was only one person working on it [(the analysis)]. [To speed this process along], I started working on a gadget to do the evaporation directly on the watch glass.<sup>10</sup> The gadget consisted of a rod, balanced on a fulcrum, with a basket that held a watch glass on one end and an adjustable weight on the other end. I put the watch glass in a holder and adjusted the weight so that when the

watch glass was full, it would tilt down on to a hot plate. When the water evaporated, the watch glass would then lift off [the hot plate]. I used a siphon to drip water on to the watch glass; overnight, it would evaporate a liter of water. I finally adjusted the siphon by grinding a valve in the delivery end so that the watch glass, when dry, would lift up and hit the valve to open it. Water would [then] flow out, and the watch glass would drop back to the hot plate. I had about 20 of these [arranged] around the room. It was really quite spectacular to see them bobbing up and down as you turned the lights off.<sup>11</sup>

That [project] led to the formation of our own laboratory. Not only for the water<sup>12</sup> but for all environmental samples. One of the problems was analyzing vegetation. I worked first to try to separate the iodine from the vegetation, but had very little luck. It wasn't until about 1950 or '52 that Bob Thorburn and Lyle Schwendiman devised a system for doing this. [Before that system was developed], we packed the sample, cut it up on a watch glass, and then slid it in a beta counter to measure. This was a crude method, but at least it was reproducible to a point. I could go on all afternoon about this.

**FISHER:** This is good, Jack. You developed some techniques for primarily which radionuclides of interest?

**HEALY:** Primarily on the vegetation; it was iodine.

**FISHER:** This would be about 1947?

**HEALY:** I think we started about 1946. The measurements made in '46 have been discarded; I haven't seen them since. I think by '47 we had a pretty good system in place.

**FISHER:** What is your recollection of your findings? Were you able to measure releases of iodine in and around the reactors?

**HEALY:** Not the reactors, the separation plants.<sup>13</sup>

**FISHER:** The separation plants. Okay.



## Developing Precise Calibration Standards

**HEALY:** Yes, with no problem. After all, during the war and the year or so thereafter, we put out about 100,000 curies,<sup>14</sup> I believe. Maybe it was 200,000; I don't know the numbers. But I *will* say, as a result of that experience, one of our first jobs, when we got some decent facilities, was to calibrate our measuring equipment. We started with radium D, which disintegrates to E, F, and so on, one of which is a beta emitter<sup>15</sup> of 1.07 MeV [(million electron-volts)]<sup>16</sup>-I think somewhere in there. You probably know this better than I do; I haven't thought about this for years. We could measure the alpha emission<sup>17</sup> on an alpha counter, 52 percent geometry,<sup>18</sup> due to backscatter. This [measurement] told us the number of disintegrations per minute of the mixture at equilibrium. We then used this [rate] to calibrate our beta counters, but this was only for the specific energy of the beta emitted. Lyle Schwendiman did this work. He did such a magnificent job, that by 1949 to '50, or in there somewhere, we had correction factors for self-absorption, for the diameter of the sample, and for all other factors needed for all samples.

Remember, there were no national standards then. The only way we could express numbers in fundamental units was to calibrate our counters. This worked well. After the [National] Bureau of Standards<sup>19</sup> put out a carbon-14 sample, Lyle obtained one for cross-checking. Carbon-14 emits a 0.14-megaelectron-volt beta-a very low energy. Measuring them on micro window counters, with [a] window thicknesses of three to five milligrams per square centimeter, is very difficult.

One day, to my surprise, I came to work and found a copy of a letter that Lyle had written to the Bureau of Standards telling them that their standard was ten percent in error. Or was it two percent? I don't remember. At any rate, I thought, "Oh, my God, here we are trying to tell the Bureau of Standards how to do their business." About two weeks later we got a letter back from the Bureau of Standards acknowledging that Lyle was absolutely right.

**FISHER:** The separations process was engineered to contain activity.

**HEALY:** Well...yes, to contain activity in solutions.

**FISHER:** In general, but it wasn't always successful. There were always problems with release of gaseous products, and the releases from Hanford were really of two kinds: normal operating releases, and the more infamous intentional releases to measure the transport of radionuclides in wind currents.

### **Operation Green Run**

**HEALY:** As far as I know there was only one intentional release, and that was the Green Run.

**FISHER:** Can you tell us more about that experience and your involvement in it?

**HEALY:** I don't know exactly where to start. I've been thinking about it since your phone call the other day.

**CAPUTO:** What was the motivation behind Green Run?

**HEALY:** As I understand it, the decision was made at the top.

**CAPUTO:** Do you know who made the decision?

**HEALY:** No, I did not. But I suspect it had to go through the Atomic Energy Commission, through GE Company, and probably through the Pentagon. My involvement in it was very simple: I prepared the plans for it and worked with the lieutenant who was out there-Lieutenant Harlen.

**FISHER:** Do you remember his first name?

### **Monitoring Successfully Detects the Soviets' Entry Into the Nuclear Age**

**HEALY:** No, I don't. He was always "Lieutenant Harlen" or "Lieutenant." Maybe I should start back a little further. I believe it was in '59, we had some equipment on top of Rattlesnake Mountain.

**CAPUTO:** In '59 or '49?

**HEALY:** '49-pardon me: '59, '69; they're all the same. We had some air sampling equipment on top of Rattlesnake Mountain. It was a very special thing; we didn't normally have it there. It was powered by a portable generator. We got some air samples that gave quite positive [radioactive] results. I took the data home and analyzed it over the weekend. I went into Herb Parker[*'s office*] the next morning and said, "Herb, I thought you were supposed to be notified if there was a bomb blast." He said, "I am, by God!" I said, "Look!" and gave him the data that showed the decay curves for a typical blast. I had it pretty well dated-about three or four hours from the date finally announced. It turned out to be the first Russian bomb. As far as I know, we were the only ones in the country that had gotten any [radioactive fallout] debris from it. We developed a great admiration for the efficiency of the Air Force, because they descended that night [to investigate our findings].

**CAPUTO:** How long did it take to figure out that was the source of the radioactivity?

**HEALY:** You could plot the decay curves and look at them.

**CAPUTO:** So it was that day [that the bomb had exploded]?

**HEALY:** The activity went down to about T to the minus 1.2 power.<sup>20</sup> So I extrapolated back to what was about zero. The Air Force personnel descended that night and by the following evening my data, my samples, everything else had disappeared [because the Air Force team had confiscated the data and samples]. The chief scientist [(for the Air Force)] was Bill Early, who I believe was a geologist. The reason I bring this up is because I believe this was the same outfit that later showed up to carry out their part of the Green Run. They had a C-47 [airplane]<sup>21</sup> equipped with some instruments, and their job was to fly around and make measurements-I believe mostly the noble gases.<sup>22</sup> But at any rate, on the Green Run, I was told that we were going to do it and planned for it.

**CAPUTO:** Who told you?

**HEALY:** Herb Parker. We set up our schedules and even tried a novel method of sampling [smoke]stack gases on the Green Run. I had tried it before on radon, and it worked well. Most noble gases have a fair solubility in oils, so we ran the gas through a sample of olive oil until it was saturated and then got the gas out and measured it. It worked very well. We had often planned to use it for other sampling, but we never got around to it.

**FISHER:** Herb Parker was a stickler for keeping exposures as low as reasonably achievable.<sup>23</sup> Was he opposed to the Green Run?

**HEALY:** As far as I know, not before the release. I don't know.

**FISHER:** Do you remember any conversations with him about it?

### **Unknown Health Hazards From Fallout**

**HEALY:** No, I don't. After all, as I said before, we had put out one to two hundred thousand curies of iodine only a few years before. Iodine was considered, at that time, to be less of a hazard than other radionuclides. It was serious, but not as dangerous as some of the others. For one thing, most thyroid cancers can be treated. As I remember, about that time, the limit for a dose to the thyroid was about twice that of [a dose to] the other organs: 30 rem<sup>24</sup> per year instead of 15. And the biggest problem, as it turned out in the long run, was that Herb and I did not see the milk-to-man pathway. Although, I had inklings that Herb later did begin to see the problem.

**FISHER:** It wasn't known at that time?

**HEALY:** No. That surfaced at the time of the Windscale accident,<sup>25</sup> which was about 1958. The British set up limits that I thought were inordinately small until I saw the derivation of it.<sup>26</sup>

**FISHER:** It was the [food chain] concentration [factors] that were important [for radioiodine].

**HEALY:** The real thing was that it was a pathway from the

air to the ground. We knew that the iodine deposited very heavily on the ground. We knew that from our own experience. There was also the fact that A.C. Chamberlain in England had done some experiments releasing iodine on a cricket pitch and found that it deposited very heavily on the grasses. In fact, when I went over to England for the Windscale review, A.C. greeted me like a long-lost brother, because everybody else doubted his experiments, but our experience backed his findings very closely. From the grass, the iodine went to the cow and then to milk, which was then consumed by the most sensitive portion of the population: the children. Now, I'm sure that if Herb had realized this pathway existed, he would have put up a fight as only Herb Parker could.

**FISHER:** That's an interesting commentary on the Green Run that I haven't heard expressed previously. I appreciate your bringing that part up. Was it known what the curie level of iodine would be at that time, during that run?

**HEALY:** Yes, we had calculated that. The only thing that went wrong, but I didn't know it at the time, was that they had loaded the dissolver with 16-day cooling material.<sup>27</sup>

**FISHER:** Instead of?

**HEALY:** Twenty, that was our goal, to go for twenty days. It would be about three or four thousand curies of iodine, seven to eight [thousand curies] of krypton.<sup>28</sup> Krypton was no problem. You could measure it in the air if you tried, but it never turned out to be much of a problem to people because it didn't deposit on the ground; it just blew away. We could measure it, but that's about it. Iodine turned out to be the main problem. I'll repeat that if I hadn't been so ignorant, and had caught on to the air-grass-cow pathway, I think the Green Run would never have taken place. We had a Biology Group by that time. They were busy feeding iodine to sheep. But no one who really was in the business caught on.

**FISHER:** That's a very interesting commentary.

## Monitoring Livestock Exposure

**HEALY:** You remember that when the AEC<sup>29</sup> was formed, the Biology Laboratory at Hanford went into operation, and one of their main enterprises was studying iodine in sheep. They had a sheep farm, and they administered iodine to find the level of damage. One of Parker's big concerns was with free-ranging animals, not so much with the people, because the animals roaming around could get very high doses to their thyroids from eating the vegetation. Again, [this was] another offshoot that misled us toward the final goal [from realizing the hazards of iodine-131 in man].

**FISHER:** Where were these experiments set up with the grazing animals?

**HEALY:** Out at 100F Area. That's where the Biology Laboratory was set up.

**FISHER:** Kornberg was involved in that?

**HEALY:** Harry Kornberg was in charge of the biology. It was set up at F primarily because there was an empty water-treatment plant there that had been adapted for biology use. Secondly, Dick Foster's fish laboratory, which was in-place well before the project started, was [located] at 100F. There was a focal point for it [(the Biology Laboratory)]. We got a million and a half dollars from the AEC for biology and a million and a half for the physical sciences to establish facilities. For the physical sciences, we decided to wait until facilities could be established in the 300 Area.

**FISHER:** About what year was this?

**HEALY:** '57 to '58. Kornberg decided that he needed room earlier in order to get the Biology Group established. So he went with the 100F solution.

**FISHER:** What was the main concern: weapons testing, fallout, or releases from processing?

**HEALY:** Releases from processing. Parker's vision for setting up the laboratory was that the nuclear industry would have [public safety and worker exposure] problems in the future. Hanford already

faced many of those problems. Our goal was to look at these problems and come up with solutions.

### **Monitoring Salmon in the Columbia River**

**FISHER:** Was the fish experiment to look for uptake of fission products from Columbia River water?

**HEALY:** (*Healy chuckles*) Strange you should ask that. No. They were more concerned about the temperature in the river, and the [river temperature's] effect on fish. Plus, somebody-I guess it was Laraine Donaldson-said that if they harmed a single salmon in the Columbia River, they would never hear the end of it. Salmon is a very important topic in the Pacific Northwest. They set up the Fisheries Laboratory, about the same time they started to build the plants, in order to get a background and start experimentation. Dick Foster was a fishery student who had his Ph.D. from the University of Washington, and he was in charge. Laraine Donaldson was guru.

I snickered at your comment because I was thinking about the first time we started to look for radioactive materials in the fish during the war. Dick Foster and Sim Cantrel brought some fish to the laboratory and taught me how to dissect them, so I could measure the radioactivity in their organs. A little later Dick dropped a big stringer of fish in my sink and said, "Here's some more." I picked out the biggest one and started to cut it open. I prowled through that fish for half an hour. Finally, I took my gloves off and went over and called Dick and said, "Dick, I've been looking through this fish for half an hour, and I can't find its stomach." Dick said, "Oh, what kind of fish is it?" "It's a sucker." "A sucker doesn't have a stomach." You learn something new every day. That was the start of the program for measuring the radioactivity in fish.

**FISHER:** What were you finding in those days with regard to radioactivity in fish?

**HEALY:** Mostly P32. The Pacific Northwest is rather low in phosphates. The uptake in the fish will depend

upon the character of the stream it's in. [However, the fish we] studied took up phosphorous strongly. Phosphorous was a relatively low component in the [reactor] water going into the river, but it was a major component in the fish.

**FISHER:** What was the source of the P32? Was it activation of sulfur in the water?

**HEALY:** I don't know whether it was activation of sulfur or [activation of] phosphorous.

**FISHER:** I would think it would have been based on activation of sulfur.

**HEALY:** That would require a fast neutron reaction, wouldn't it? Neutrons in the reactor are pretty well slowed down.

**FISHER:** Yes, you're right.

### **Discovering Unaccounted-For Release Hazards**

**HEALY:** Most of the activation products were just neutron captured. I'm saying simply, most [of the radionuclides] were from neutron captures. I don't know about that one [(the P32 in the fish)]. The problem was that they calculated the amount [of radiation] coming out of the reactors according to the composition and volume of water and the neutron flux. [These radiation levels were] really quite low. [However, the actual] amount coming out was about a hundred times greater, because there were two types of film that appeared to form on the tubes. [This film] held the stuff up and let it irradiate longer before releasing it back to the river. This is the problem they never discovered in the power reactors, although I tried to tell them what was going to happen. They said, "Oh, no, our water is pure."

### **More About the Green Run**

**FISHER:** Do you have any further comments on the Green Run operation?

**HEALY:** Yes, let's get back to that. That's what we came



for, after all. Lieutenant Harlen and the DC-3 or C-47, whichever you wish to call it, showed up a few months before the Green Run was due to take place and made a number of tours over the area. I took one with them and vowed, "Never again!" I don't like flying airplanes that size and at a level where you have to climb to get above a 200-foot stack. The turbulence was terrible.

We set our plans for the run, had the crews ready, and put on some special stack-monitoring equipment, including one for collecting iodine, which never worked. There was a sag in the line that would collect any condensate [that formed] and all the iodine was in that condensate. We found that out afterwards by analyzing the condensate. We had to postpone the run several nights because of weather conditions. We finally got a reasonable forecast from one of the meteorologists, Don Jene, so we agreed that we'd go with it.

**CAPUTO:** Who's "we"?

**HEALY:** Me. I called them up and told them to start the dissolver. This was about ten o'clock at night.

**CAPUTO:** On December 12th?

**HEALY:** I don't remember what date it was; sorry. This is one caution I meant to put in at the start of this interview: This all occurred 40 to 50 years ago, and I've found, from experience, that my memory, shall we say, is fallible. For example, on the first finding of the iodine in the vegetation, I gave my version to Carl Gamertsfelder<sup>30</sup> once and Carl jumped all over me [for having so many facts wrong]. Thinking about it, he was absolutely right.

### **Unexpected Change in the Weather Pattern**

**HEALY:** At first, everything went fine. The wind was blowing from the south to the north, which was relatively uninhabited. The inversion held well. That was one of our criteria for dissolving. But the problem was that about two o'clock in the morning, we got a dead calm, and then the wind

reversed direction 180 degrees. Generally, the noble gases come out<sup>31</sup> first and the iodine comes out a bit later. About two o'clock in the morning, we had the calm; and then the wind, instead of blowing to the north, began to blow down the river over Richland, Pasco, and Kennewick, [Washington]. This, I believe, was the reason for the high [iodine] levels that we got on the Green Run. I later calculated, using Sutton's equations- which are passé, to say the least- the concentrations in Richland, Kennewick, and Pasco. There were mostly within a factor of two of the ones that occurred. Nearly all [were] within a factor of ten, [a prediction accuracy] which I thought was really good. Incidentally, you've seen the report we wrote on it?

**CAPUTO:** Yes.

**HEALY:** You're one up on me, you know more about it than I do. That thing and all the papers were classified Red Label.

**CAPUTO:** Part of it is still classified by the Air Force.

**HEALY:** I haven't seen the report since.

**FISHER:** You designed the monitoring program for the Air Force, or did they come in with their own?

**HEALY:** They came in with their own.

**FISHER:** And with their own people?

**HEALY:** Yes. As a matter of fact, I tried to get Harlen out of bed at two o'clock in the morning to get the plane out to see what was going on, but he said, "I'll wait until morning."

**FISHER:** Was there just one Air Force plane making measurements, or were there several?

**HEALY:** As far I know, there was one. Now, there were special pi-ball<sup>32</sup> runs made by the weather balloons from all of the Air Force facilities in the Northwest, so we had a pretty good handle on the winds.

**FISHER:** Did you report to Herb Parker the next day on the

results of this?

**HEALY:** I didn't have to: Every manager in the place was calling Herb Parker! I couldn't go back for two weeks to my office in the 200 Area.

**FISHER:** Because?

**HEALY:** Because the iodine had deposited on the ground. People would go out and back in, step on the shoe and foot counters, and set them off. That disrupted their operations very badly. I was very unpopular, believe me. Herb was not much more popular.

### **U.S. Air Force Involvement in the Green Run**

**CAPUTO:** What role did the Air Force play in the decision to finally go ahead with the release? Were they pressuring you to do it?

**HEALY:** No.

**CAPUTO:** No pressure?

**HEALY:** No, no pressure. I was concerned that if we left this stuff in the dissolver too long it would not be usable.

**FISHER:** For what reason?

**HEALY:** Because the iodine would decay out.

**FISHER:** In-place?

**HEALY:** We had delayed it two days already. I didn't know they'd put in 16-day stuff, and it was still 18 days.

**FISHER:** Was the Air Force able to obtain the information they wanted from this experiment?

**HEALY:** That I don't know.

**FISHER:** Was there any follow-up on the release by you, or by Herb Parker, in terms of the Air Force meeting its objective? Was there any kind of feedback mechanism, or did they simply take their information and data and that was the end of it?

- HEALY:** As far as I know, that was the end of it.
- CAPUTO:** In the report, which I guess you haven't seen, it's Herb Parker saying he would never do it again.
- HEALY:** I've heard that.
- CAPUTO:** I was wondering if you knew the basis for that: Was it because of the human exposure, or were there other reasons behind it?
- HEALY:** I think the concentration in this small area, because of the change in the wind and the deposition, was much higher than would reasonably have been expected. For example, my calculations showed that we managed to contaminate an area about twice the size of the Windscale accident, with about one-fifth of the amount of iodine released. It was a very unfortunate [shift in] weather pattern.
- CAPUTO:** *(turning to Fisher)* Any more questions?
- FISHER:** Jack, it's fascinating to hear you say this and for you to give this story. We appreciate the opportunity to have you put this down. The oral history program supplements the written record, which is not complete. Before we turn off the tape recorder, is there anything else that you would like to add that might be of interest to future investigators?
- HEALY:** I can't think of much. I'll probably get in bed tonight and think of all sorts of things.
- FISHER:** It's very interesting; we appreciate it.
- CAPUTO:** Thank you very much.

*NOTE: The interview initially ended here. It was resumed after Mr. Healy remembered another topic he wanted to address: an accidental release.*

### **Other Accidental Releases**

- FISHER:** Tell us about the accidental ruthenium<sup>33</sup> release at Hanford.

**HEALY:** The people in the operations?

**FISHER:** First of all, what year was this? Do you remember?

**HEALY:** It was in the '50s. The people in the operations had a scrubber on the line to remove the ruthenium. They had oxidized it. Ruthenium becomes volatile when oxidized. Then they put a scrubber on to move it. We were working on a monitor for that stack. I believe it was the B Plant where we used a scrubber and a counter to measure the emissions. Since I don't like logarithmic charts [because their nonlinear scale is difficult for many people to understand], and we wanted to cover the full range, from background to emergency situations, we rigged the recorder with a micro switch so that, when it hit the top of the chart, it would reduce the sensitivity by a factor of ten. We were testing this and, unbeknownst to us, the operations people liked this instrument so much, that they ran a wire from it over to the control room.

This night, the scrubber went dry. So what did the recorder do? It started about the middle of the chart; went up, back, steady, and returned right back to about the same place. The operations people assumed that a mishap in the equipment had occurred, and kept going. [Consequently], we put out 260 curies of ruthenium. Fortunately, it was a nice, clear night. Weather conditions were neutral with a good wind of about 20 to 25 miles an hour, so it just left a streak of ruthenium right up the mountains to the north.

The bad part of that occurred later. About a year later, we started getting particles—you may have heard of those hot<sup>34</sup> particles—around the Hanford reservation.<sup>35</sup> This was the paper where Parker published that you could lie out naked in your yard for twenty-four hours a day and never get a burn.<sup>36</sup> I was off on special studies at the time, but I was called in to try to make sense out of what had gone on. Eventually, we analyzed the particles, and found they were ammonium nitrate and the active material was ruthenium-106, which is a long-lived isotope. We speculated that the material had deposited with the ammonium nitrate on the inner lining of the stack and then flaked off. They took care of this by washing down the stacks.

Coincidence plays a big role in this. For example, when we started the K Reactor up, they had a test in which all 2,000 tubes had a little gadget put on to stop the flow. After the test was over-and this was presumably a pressure test on 2,000 tubes-they had a man go around and remove each of these stoppers and another man check that they were removed. Then they started up. They had a pressure sensor on each tube. Guess which tube the pressure sensor failed on? The one that still had the plug in it. So they burned out one tube in the reactor. Talk about coincidence! That one put a little bit [of radioisotopes] out, but not a lot. It was mostly noble gases [that were released in the ruthenium accident].

**VOELZ:** Did they burn out the sensor?

**HEALY:** No, they burned out the whole tube.

**VOELZ:** Yes, but why did the sensor fail?

**HEALY:** I don't know why it could be. But it was a flow sensor, so I doubt it was [sensor failure that caused the tube to fail]. What were these [other intentional] releases [of radioiodine at Hanford you mentioned earlier]?

**CAPUTO:** There were two intentional releases in the 1960s, not a whole lot of radioiodine. They [(researchers)] actually purposely put people in the pathway and then measured it and their exposure.

**FISHER:** There were some field experiments.

**HEALY:** This was John Honestead?

**FISHER:** Possibly. I know Joe Soldat was involved with some others. The idea was to determine human thyroid uptake from environmental releases, and so a couple of Hanford scientists stood out in the field and inhaled air for a half-hour or so.

**HEALY:** They should have talked to me. When we replaced Bob Thorburn from under the stack in the Green Run [in Washington State], you could read him with a Geiger counter from here [in New Mexico].

**FISHER:** I'm glad we had these additional comments from you. Thank you very much.