

CHEMICAL HERITAGE FOUNDATION

ALFRED O. C. NIER

Transcript of Interviews
Conducted by

Michael A. Grayson and Thomas Krick

at

University of Minnesota
Minneapolis, Minnesota

on

7, 8, 9, and 10 April 1989

(With Subsequent Corrections and Additions)

ACKNOWLEDGMENT

This oral history is one in a series initiated by the Chemical Heritage Foundation on behalf of the American Society for Mass Spectrometry. The series documents the personal perspectives of individuals related to the advancement of mass spectrometric instrumentation, and records the human dimensions of the growth of mass spectrometry in academic, industrial, and governmental laboratories during the twentieth century.

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Alfred O. C. Nier

Alfred O. C. Nier

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ALFRED O. C. NIER

1911 Born in St. Paul, Minnesota on 28 May

Education

1931 B.S.E.E., Electrical Engineering, University of Minnesota
1933 M.S.E.E., Physics (under Henry Hartig), University of Minnesota,
1936 Ph.D., Physics (under John Tate), University of Minnesota

Professional Experience

1936-1938 Harvard University, Cambridge, Massachusetts
Postdoctoral Fellow under Kenneth T. Bainbridge

University of Minnesota, Physics Department
1938-1940 Assistant Professor of Physics
1940-1944 Associate Professor of Physics
1944-1966 Professor of Physics
1953-1965 Chair
1966-1980 Regents Professor of Physics
1980-1994 Regents Professor of Physics, Emeritus

1943-1945 Kellex Corporation, New York
Scientist

Selected Honors

1950 Elected to National Academy of Sciences
1953 Elected to American Philosophical Society
1956 Arthur L. Day Medal, Geological Society of America
1959 Elected as Foreign Scientific Member of the Max-Planck Institute for
Chemistry
1960 Pittsburgh Spectroscopy Award
1965-1966 National Lecturer, Sigma Xi
1971 Atomic Energy Commission Award for Contributions to Development
and Use of Atomic Energy
1977 NASA Medal for Exceptional Scientific Achievement
1980 Elected to American Academy of Arts and Sciences
1980 Elected to Royal Swedish Academy of Science

- 1980 Honorary Doctor of Science, University of Minnesota
- 1981 Distinguished Service Award, University of Minnesota Chapter, Sigma
Xi
- 1982 Elected to Minnesota Inventors Hall of Fame
- 1984 Victor Goldschmidt Medal of the Geochemistry Society
- 1985 Field and Franklin Award for Outstanding Achievement in Mass
Spectrometry, American Chemical Society
- 1985 Thomson Medal, International Mass Spectrometry Conference, Swansea,
Wales
- 1992 William Bowie Medal of the American Geophysical Union

ABSTRACT

Alfred O. C. Nier was born in Minnesota in 1911 to parents who had emigrated from Germany. After a brief dispute over his name, Nier was baptized Alfred instead of Hans, since his mother believed Hans sounded too German. However, his two middle initials proved problematic during World War II when the government was researching publications by Alfred O. Nier and Alfred O. C. Nier for security clearances. Consequently, the majority of Nier's publications are without his second middle initial.

Having been interested in radios during high school, Nier decided to study electrical engineering when he enrolled at the University of Minnesota in 1927. When he graduated in 1931 he pursued engineering jobs; however, few firms were hiring due to the Depression. Luckily, during his undergraduate career Nier had been involved in physics research with his mechanics professor Henry A. Erikson. This physics experience led him to a research position and teaching assistantship with University of Minnesota professor Henry Hartig. Nier earned a master's degree in electrical engineering, though most of his research experience was in physics; he began his doctoral research at a time when quantum mechanics and x-rays were burgeoning fields of study. After much deliberation Nier chose to work with John Tate, head of the physics department and editor of the *Physical Review*. Subsequently, Tate assigned Nier to work on mass spectrometry. In the mid-1930s Nier built his first mass spectrometer and quickly obtained the first spectrum of benzene, though he never published it. Instead his first publication was in *Review of Scientific Instruments* in 1935 on feedback control for magnets.

Nier spent the majority of his doctoral research obtaining a precise understanding of how mass spectrometers worked and how he could improve the instruments to enhance his isotopic abundance studies. It was in the area of isotopic abundance where Nier encountered his scientific hurdle: a nuclear physics controversy over the mass abundance of potassium-40.

After completing his Ph.D. in 1936, Nier was awarded a National Research Council Fellowship. He elected to work with Kenneth T. Bainbridge at Harvard University. After working for General Electric over the summer, Nier began his work on 180° mass spectrometers in the fall. Fortunately, Bainbridge, who had excellent funding despite the Depression, had been able to build a large electromagnet over the summer. By December Nier completed a mercury spectrum and, through stabilizing the power supply and maximizing the accelerating potential, was on his way to establishing more precise isotopic abundances than the ones F. W. Aston produced in 1915. While at Harvard, Nier was introduced to geochronology and geochemistry through studying the atomic weight of common lead and uranium-lead.

Nier returned to the University of Minnesota after completing his postdoctoral research in 1938 instead of staying on as an instructor at Harvard or becoming a researcher at Westinghouse. Despite teaching a heavy course load Nier was able to begin building a magnet for his mass spectrometer and a thermal diffusion column to provide carbon-13 for stable isotope tracer studies. However, he had a diverse range of projects to complete on his 180° mass spectrometer with the help of students and his machinist R. B. Thorness.

In the fall of 1939 Nier became involved in work related to uranium-235 and UF_6/UBr_4 (Nier refers to UF_6 in the interview but references UBr_4 in some publications). Nier, with E.T. Booth, J.R. Dunning, and A.V. Grosse, demonstrated conclusively via mass spectrometry that uranium-235 was the isotope that underwent slow neutron fission. As his research group at Minnesota was the only one capable of analyzing uranium he was ordered to begin separating

uranium-235 on his 180° mass spectrometer. After Pearl Harbor and the official entry of the United States into World War II, Nier and his research team worked under the command of Harold C. Urey as part of the Manhattan Project. Nier's mass spectrometry expertise would prove invaluable to the war effort; Nier initially built four instruments for isotope analyses and ten instruments specifically for hydrogen-deuterium analyses. Nier taught many how to use and build these machines and allowed General Electric to produce his mass spectrometers. One such instrument that GE built was the Nier designed leak detector for the K-25 diffusion plant in Oak Ridge, Tennessee. Nier worked with the Kellogg Corporation to support gaseous diffusion processes to make line recorders, which were mass spectrometers monitoring the process stream.

After World War II, Nier returned to the University of Minnesota where he remained as a professor. Nier's post-war mass spectrometry research touched on many areas including electrical detection, atmospheric studies and mass spectrometers for rockets, geochemistry, and precise masses. Nier participated in the upper atmosphere Aerobee flights throughout the 1960s, the Viking Project in the 1970s, and the Pioneer Venus project. During this atmospheric work Nier became friends and a collaborator with Klaus Biemann.

Throughout his oral history Nier discusses his many publications, the instrument details of many mass spectrometers, his awards, and his interesting career. Nier explained that his short attention span and unique education in physics and electrical engineering allowed him to capitalize on the new field of mass spectrometry when the country needed his expertise most.

INTERVIEWER

Michael A. Grayson retired from the Mass Spectrometry Research Resource at Washington University in St Louis in 2006. He received his B.S. degree in physics from St. Louis University in 1963 and his M.S. in physics from the University of Missouri at Rolla in 1965. He is the author of over forty-five papers in the scientific literature dealing with mass spectrometry. Before joining the Research Resource, he was a staff scientist at McDonnell Douglas Research Laboratory. While completing his undergraduate and graduate education, he worked at Monsanto Company in St. Louis, where he learned the art and science of mass spectrometry under O. P. Tanner. Grayson is a member of the American Society for Mass Spectrometry [ASMS], and currently is the Archivist for that Society. He has served many different positions within ASMS. He has served on the Board of Trustees of CHF and is currently a member of CHF's Heritage Council. He continues to pursue his interest in the history of mass spectrometry by recording oral histories, assisting in the collection of papers, researching the early history of the field, and preparing posters recounting historic developments in the field.

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INTERVIEWEE: Alfred O. C. Nier

INTERVIEWER: Michael A. Grayson

LOCATION: University of Minnesota
Minneapolis, Minnesota

DATE: 7 April 1989

NIER: [...] When I got your note, I did a little looking around to see where things were, and I got out a number of things, reprints, that were, sort of, significant, some of which you may want to take.

GRAYSON: Yes, any copies of reprints that we can have would be great.

NIER: Yes, and I have some photographs. I don't know whether I have duplicates of these. I could lend them to you or let you know which they are and have copies made.

GRAYSON: Well, I have arrangements in St. Louis [Missouri] for making copies of photographs.

NIER: Okay, there may be a few here that I don't want to give up.

GRAYSON: I understand.

NIER: So, we'll have to negotiate that; whatever's most convenient for everybody. Also, since you raised the question about early meetings, were you acquainted with this 1951 conference held at the Bureau of Standards [Symposium on Mass Spectroscopy, National Bureau of Standards, 1951]?

GRAYSON: No.

NIER: That's the first meeting on mass spectrometry I ever attended. [The 1951 National Bureau of Standards meeting]

GRAYSON: Oh really?

NIER: I have a picture of the people at the meeting here, there was a group picture taken at the steps. John Hipple arranged that. He was the guy who had been at Westinghouse with Ed Condon and then went on to the Bureau of Standards. He was head of the section there; then he went to North American-Phillips, and was at Penn State [University] for a while.

GRAYSON: But you say Hipple was at Westinghouse at the time?

NIER: Yes.

GRAYSON: Okay.

NIER: Well, no, he moved to the Bureau of Standards.

GRAYSON: I see.

NIER: He had been at Westinghouse, and then Ed Condon became Director of the Bureau of Standards, and brought Hipple with him. Condon had been director of research for Westinghouse. But Hipple arranged this conference. It was a very nice group picture. I bet, there aren't many of the groups in existence any more.

GRAYSON: Is it in the book anywhere?

NIER: I don't think the picture's there, but I have an eight-by-ten of it. It's a very good one.

GRAYSON: That would be very excellent to go with the interview.

NIER: As a historical thing.

GRAYSON: Yes, and to identify the individuals that were there.

NIER: I have a standing list of these people. There were a lot. There were a hundred or so people. It was a very nice conference.

GRAYSON: This work is referenced in your publication list, is that correct?

NIER: Yes, because I gave a talk there. Maybe two talks.

GRAYSON: Yes, I believe there are two. Okay.

NIER: It's referenced there. I thought you ought to know about that.

GRAYSON: Yeah, that is an excellent point. Well, how do you want to proceed here?

NIER: Well, you're the one in control! [laughter]

GRAYSON: I have a number of questions...[laughter]

NIER: Well, why don't you go ahead, and then we'll have to pick up loose ends, maybe.

GRAYSON: I understand.

NIER: Well, now, tell me what's going to happen so we can see if that'll help us decide how you're going to do it.

GRAYSON: Okay. The immediate plan with this recording is to have it transcribed. And then, of course, you'll get a chance to edit...

NIER: Or censor...

GRAYSON: Yes, censor it, yes. I'm presently negotiating to arrange for the Society [American Society for Mass Spectrometry] to become a partial affiliate with the National

Foundation for the History of Chemistry [NFHC, now the Chemical Heritage Foundation], and I feel that eventually at some time or another, hopefully soon, this material will then be archived with the NFHC. And, so doing, thus make it available, essentially to scholars and people doing history of science work. So, that is the plan, and I believe, indeed, that's what will happen. It's a matter of timing as to when it would happen.

NIER: So, it should be reasonably well-organized.

GRAYSON: Well yes, it should be, on the other hand, I think the important thing here is to explore a lot of little nuggets that you don't get an opportunity to explore in some of the review papers.

NIER: Since nowadays, you put these things on word processors, you can move things around.

GRAYSON: Yes; the way these usually work is the transcripts are abstracted in terms of titles of subjects, and so if a particular tape is of interest, that could be found out fairly rapidly. So, rather than worrying too much about the organization, we might just kind of free run...

NIER: Alright.

GRAYSON: ...because if we spend a whole lot of time on how it's organized we may end up missing the point. Well, what if we start with the very first question I have here; how old are you now?

NIER: I'll be seventy-eight in May [of 1989].

GRAYSON: That's in a month. Then, can you recall your initial interest in the areas of science; just when, how soon, how early in your life did that actually become an interest?

NIER: Well, that's hard to answer because I don't think there was any really definite time. The only thing I could point to is that I had done well in arithmetic in grade school. In high school, I was interested in the science courses. I took physics and chemistry there, and I took all the math that was available. And also, I took shop courses and drawing courses, which at that time were things that were kind of standard for people. I entered high school and you had to choose what kind of curriculum you were in. I went into the college preparatory curriculum, because my parents felt that I should go on to college and university.

GRAYSON: That was not common at that time, was it, for a young person to go on to college?

NIER: No, I'm trying to think. One time, I was wondering how many of my graduating class in high school, went on to higher education. [I graduated in 1927.] It turned out to be more than I thought it was after I looked at what had happened to some of the people. But I suppose a third, something like that. But of course, as may be pointed out in some of the things I've written already, my parents were immigrants. My mother had come as a ten-year-old, with her family from Germany, and my father came as a teenager. They met, because he lived in a rooming house next door to where they lived in St. Paul [Minnesota]. Father had gone to a trade school—he was a machinist by trade. My mother, I don't think had much formal education. I would say the equivalent of grade school probably was all that she had. But they were, like immigrant families, very interested that their kids should get an education, and this was characteristic. This is just a standard thing, if you look at the whole record. My wife came from a Norwegian family in upper Minnesota, and the same kind of attitude prevailed among these people, too.

Keep in mind, my mother came in 1880s and my father came in 1890s. So this was sort of the opportunity. I was always steered in that direction. In high school, that was the time when radio was a coming thing. Keep in mind, this was the middle 1920s and people built their own radio sets, and I had friends who did this sort of thing. You could buy parts and tubes and crystals and whatnot, and so I got into that kind of thing. By the time I graduated from high school in 1927, it was understood that I would go into electrical engineering, which I did, and I came here to the University [of Minnesota]. Unfortunately my folks really didn't have much money, I carried newspapers when I was in high school to help some, and I rode the streetcars dutifully, everyday, back and forth from the other side of St. Paul over to here and back. I knew every bump on the streetcar tracks. I continued for four years here, and graduated in electrical engineering. That, sort of, seemed to be the direction I was going to go.

GRAYSON: Well, then, I gather that mass spectrometry interrupted that somewhere along the way.

NIER: Well, I want to give you another thing I wrote. I received an honorary degree here in 1980, and I wrote a blurb which I hadn't realized I'd written. It told a little bit about historical things. I made a copy of it, which you may want as part of the interview.

GRAYSON: Excellent, yes.

NIER: But, the story's a kind of an interesting one, because it shows you how you never know what's going to happen. All engineering students took physics in their sophomore year. That was the standard thing, at that time. This department was organized such that one person taught mechanics, another taught heat, another taught electricity, and so on...this was the style of many places at that time. Anyhow, the guy who taught the mechanics course was also the head of the department. His name was Henry [A.] Erikson, and he was one of these very tall, dignified sort of persons. I was in this class of probably two hundred students or something like this, and in those days, they kept track of your attendance in class, and if you missed classes too often, you failed the course. This was the standard thing that went on then. So, for these big classes we had numbered seats in this big auditorium, and they had somebody going around five minutes after the class started and seeing which seats were vacant. The instructor knew where everybody sat. Well, after the third quiz just before the class, this very dignified professor came up the middle of the room—I sat a few seats from the aisle—and said he'd like to see me. He didn't ask what my name was or anything else, he just came and said he'd like to see me. Well, then he turned around and went back. He asked me at my convenience to come to his office. I didn't know what this was all about, and, the people around me began to buzz. They wondered if I'd been caught cheating or something like that. But, anyhow, I went to his office, and made sure he was talking to the right person. And what had happened was I had gotten hundred's on the first three tests in the physics course, so he raised the question, "Had I considered becoming a physicist?" I said, "Well, no, I really hadn't." But he said I really ought to consider it, and what's more, he gave me a job, working as an assistant, doing some experiments. So this was my real introduction to physics as such.

GRAYSON: This gentleman's name?

NIER: Henry Erikson.

GRAYSON: Erikson.

NIER: It's spelled with an E- [the Norwegian spelling] R-I-K-S-O-N

GRAYSON: Okay.

NIER: He did this for a lot of young people. It was really an introduction to science on a hands on sort of basis. For an undergraduate, it was quite a break. Besides, I got fifty cents an hour, which was the going rate then, which was high in 1935.

GRAYSON: That was high. I didn't earn that when I started working.

NIER: So, I did this and in the last summer I was an undergraduate, he got me a job full-time with somebody else, working in X-rays. Professor [Joseph] Valasek. V-A-L-A-S-E-K. By the way, he's the guy who discovered ferro-electricity. Didn't get credit for it for decades, but he's been recognized since. [J. H.] Van Vleck has officially given him credit for this. *The J. H. Van Vleck*. They had a symposium in Valasek's honor some years ago. Valasek's still living—he's about ninety years old. But anyhow, I worked for him full-time in the summertime, and part-time during the school year; so I saw a lot of what went on. Well, Professor Erikson wanted me to continue on as a graduate student in physics, and I said, "Gee, [this was my senior year] I wanted to really get out on the job for a while, and I wasn't thinking of graduate work so I think I'll pass it up." Well this was 1931, and jobs were not available for engineers. So, I estimate, I think about five people out of our graduating class of eighty got jobs.

GRAYSON: But why? Was it because of the Depression then?

NIER: Yes, the Depression, people were being laid off right and left and companies were not hiring. And there weren't many opportunities locally; there was probably a job for one or two electrical engineers at the power company and the telephone company and that was it, there was no industry. Honeywell [International] was still making dampers for stoves and things like that and 3M wasn't interested in scientists. So, the jobs were with Westinghouse, GE, Bell Labs, and so on and they just weren't hiring.

GRAYSON: But I would've thought...you probably graduated with good grades and you were a pretty good student.

NIER: Yes. But I didn't have all the other things they wanted, so...

GRAYSON: What other things were they interested in?

NIER: Well, I mean, that you'd been active in school activities and things like this. They were looking for these management types; leadership assistants, and so on. The research opportunities were very limited so people who had that sort of talent were very limited. Well, it turned out I was rescued, luckily, by a man in electrical engineering who was really outstanding. His name is Henry Hartig, who himself was a physicist and had come back to Minnesota. He had worked at AT&T sometime before he came back on the faculty here in our electrical engineering department. This is, by the way, mentioned in my honorary degree talk. And so, he rescued me and got me a teaching assistantship in the electrical engineering department.

GRAYSON: So your good qualities, your good record was recognized by someone.

NIER: Oh yes, he knew who I was. He saved me when he found out I didn't even have a job when I graduated. I spent two years there and got a master's degree in electrical engineering. But they had so few courses available in engineering departments in those days, that one took the courses in physics and math and so on. So, except for having taken one course in electrical engineering in circuit theory or something like that, I took just standard physics courses. It was just as if I'd stayed in physics as far as my coursework and that's all you did the first two years anyhow. But I did spend two years there, and that was valuable experience. I was always interested in instrumentation. As you can see the thread of engineering runs through this. When I finished up in 1933 with a Master's degree, Professor Erikson again offered me a teaching assistantship. There weren't things like research assistantships in those days. The only avenue was a teaching assistantship. So, he again offered one to me, which I accepted then because I was now on the way. That's the history of that time.

GRAYSON: In these teaching assistantships were you actually teaching courses or doing lab...?

NIER: Lab—teaching labs. I think we had the standard load, which was called a half-time position. A standard load was four two-hour labs a week plus grading the papers that went with these.

GRAYSON: And these typically had twenty or thirty students in each?

NIER: Well, probably less than that in our case, more like fifteen, or sixteen, or something like that. You had so many stations they worked in pairs in many experiments. I think it hasn't changed that much since.

GRAYSON: No.

NIER: So, that was, kind of, the way it was. I did that in electrical engineering and it was very valuable. I taught a lot of different things, a radio lab, and a transient lab where you did experiments with an oscilloscope. This was one that had to be pumped down, crazy thing, never worked right; but that was the one for doing high-speed transient phenomena, stuff like that. That was kind of an interesting experience. Much more interesting than the run-of-the-mill thing. It was quite valuable and the communications lab I taught there was, kind of, fun.

GRAYSON: To a large degree then, you had a foundation in electronic instrumentation that you could carry with you into mass spectrometry.

NIER: Yes. Which most physics majors didn't have, by the way.

GRAYSON: Yes. It turned out to be an extremely valuable background.

NIER: A very valuable background, that's right.

GRAYSON: So, a little fortuitousness there.

NIER: That's right. As I say, you have to be in the right place at the right time.

GRAYSON: So, mass spectrometry. We were trying to get to the point where you first became aware that such an activity existed and was worth pursuing or looking into. When did you really...

NIER: Well, I had been in and out of the physics department for years since I'd worked here as an undergraduate and the electrical engineering department was just across the street. I'd gone back and forth and I was more at home in the physics department actually, than I was in electrical engineering. Because I'd spent so much time here. I knew what the graduate students were doing. It was a very interesting time; this place excelled in the study of the electron impact of gases, thanks to John Tate. He was not head of the physics department. He never wanted to be head of it. He was editor of the *Physical Review* for twenty-four years. He got the job very young and he died quite young—he was only sixty when he died. But, he was the leader of that enterprise which was the largest research enterprise in the department here. A few students went into theoretical physics, which this department pioneered because we had Van Vleck on the faculty at that time. He turned out some of the first students in quantum mechanics in the country. Professor Valasek, in his work in X-rays and other related things had students, but otherwise, it was mainly Tate who had students. And they all worked on something related to electron impact and gases. There was a real break-through at the time. There were several students who were very, very good, they were predecessors of mine.

One that's really outstanding is Walter Bleakney, who was the man who first recognized the importance in electron-impact studies of separating the acceleration of the electrons from the acceleration of the ions. (Figure 1) Keep in mind that quantum mechanics was just coming in. It was the 1920s and there was an interesting thing that people called the critical potential. It

was an interesting subject in the late 1920s. To do this quantitatively, you have to know how fast the electron has to go to make an ion. You see, it isn't just any old energy, you have to have a minimum energy or it doesn't ionize. Well, that wasn't very well understood, and if you did it the way the conventional spectrometers of the 1920s did it, where you accelerated the electrons in one direction and used the same field to accelerate the ions in the opposite direction, you got all mixed up and couldn't determine the potential of the electron when it made the ion, because it wasn't a sharply defined thing. So, Bleakney came up with the idea of having the electrons go at right angles to the ions. I wrote to him a few years ago—he's still living, well up into his eighties—about what the history of that was. He says yes, he was the one who really came across this idea. So, what you do is collimate an electron beam with a magnetic field, so you get a tight pencil of electrons. You can control their speed accurately by sending them through suitable diaphragms—a suitable gun arrangement—so, when they come out, they're going at a very definite speed, very sharply defined. Then you send them into a region where there is a small cross-field, and draw the ions out at right angles. Then you can do tricks with them, accelerate them, and so on.

GRAYSON: So, this is essentially the beginnings of the electron-impact ion source that everybody uses.

NIER: Everybody's used since. It was Bleakney's doing.

GRAYSON: I see.

NIER: And this was about 1929.

GRAYSON: But his primary interest was this very fundamental phenomenon related to the ionization energy for different gases.

NIER: That's right, and he was the one who discovered quantitatively the formation of multiply charged ions. Before that, it was a big mixed-up business. His first study was on mercury, because we had mercury diffusion pumps and you just adjusted the trap temperature to get mercury vapor. He determined the ionization potential for singly charged mercury. He could tune to the mass 200 position and gradually increase the electron energy. You couldn't resolve the isotopes with those early machines, so it was just about 200.

GRAYSON: He was just looking at this lump in the vicinity of mass 200, a single lump.

NIER: Lump, that's right, the single charged mercury, around 200. And you got nothing until the electrons were going 10.4 eV, or something like that. Then you began to see the onset and generally reached a maximum around 100 volts or so. You tuned to the position of doubly charged, like mass-100 m/e, and you didn't get anything till you got to, I don't know what the number was, 35 or 40 eV something like that.

GRAYSON: Yes, noticeably higher...

NIER: Then you turned to a third of 200, and you began to see those at sixty, seventy or eighty or whatever it was, I think he observed up to quintuply charged; that was the first time anybody had done that quantitatively. And this guy was just a graduate student, you understand. Then, the other thing he's responsible for is molecular ionization studies, which everyone's forgotten since. It really was a most amazing time. I was an undergraduate then and coincidentally, Bleakney was my lab instructor. I never knew that I would follow in his footsteps, but he was the teaching assistant in my first course in physics; just a coincidence.

But anyhow, Ed Condon, who was one of the pioneers in what might be called "chemical physics," especially the application of quantum mechanics to chemical problems, was here a year or two on the faculty as a theoretical physicist teaching quantum mechanics. I think he's one of the true pioneers of the middle 1920s, and he came here when Van Vleck left to go to [University of] Wisconsin. And that was when he worked on this problem of what happens when you hit a molecule, like a diatomic molecule, with electrons—what happens to the molecule? He was studying the quantum mechanics of this business, when either he or somebody else, came up with this idea, that if you raised the molecule to a higher energy level that it would fly apart. It doesn't start flying apart, you have to go to the right energy and then it flies apart. And you have excess energy over what would be in the state of the particle, so the thing would fly apart with energy. And so, if you take a neutral molecule, I think they worked with nitrogen, or carbon monoxide and hit it with electrons that were going at fast enough...you raise its energy up to the point where the ion and the neutral fly apart. And so, he pointed out that you ought to be able to observe these kinetic energy ions.

GRAYSON: Now, you said the second thing Bleakney was not known for was this business with Condon and molecular ionization/fragmentation.

NIER: Yes, Bleakney was a graduate student here, and he knew Condon...of course, everybody knew Condon. He was described like an old pair of pajamas: one of the most friendly, wonderful guys that you ever wanted to meet and a wonderful storyteller, just a terrific guy. Anyhow, he knew that Bleakney had this spectrometer that could do these tricks, you see? So, Condon told Bleakney, "Why don't you go downstairs and play with the apparatus, I'll teach your lab this afternoon." Or something like that. Well, Bleakney did essentially that, and sure enough, found the energetics of hydrogen fragmentation.

GRAYSON: So, this represented additional work in molecular ionization, fragmentation and so on as opposed to just the elemental ionization.

NIER: That's right. These were the two important things that Bleakney was involved in. And then he got a National Research Council Fellowship, and went on to Princeton [University]. There were two other guys who followed him shortly who also were very good, P.T. Smith, who very few people ever heard of. He was the most wonderful apparatus-builder, and my hero of those days.

GRAYSON: He was on staff here?

NIER: No, he was a graduate student, at my time. He had done this nice work, and knew how to build instruments nicely.

GRAYSON: When you say, "he knew how to build instruments nicely," can you explain?

NIER: Well, he knew how you fabricate this stuff; making electrodes and so on, and putting them in a vacuum system. He had a real appreciation.

GRAYSON: He had a way of going from an idea on a piece of paper to something that actually worked; that was a talent that some people had.

NIER: Yes. He had this peculiar sort of a talent, and he followed up things that Bleakney had done; measuring total cross-sections, without a spectrometer, just measuring the ions coming off, without trying to separate them by mass. And the work that he did, about 1931...people still, very proudly point out that they checked Smith's values of 1931.¹ That's how good he was. I'm sure that there've been improvements, but in general, these guys did things well.

GRAYSON: Yes

NIER: The other guy, Wally Lozier, L-O-Z-I-E-R also had a National Research Council Fellowship—there weren't many, maybe about two in all of physics and these guys got them

¹ John T. Tate and P.T. Smith, "The Efficiencies of Ionization and Ionization Potentials of Various Gases Under Electron Impact," *Physical Review* 39 (1932): 270–7.

and went off to Princeton. They followed Bleakney to work with him. I knew them all, because I'd been around for a year. So, after they were all gone I was casting around for something to do. I think I told this in my reminiscences about geology or something. I thought the last thing in the world I wanted to do was to work in that field. I thought I would do something else, so I considered various things, I thought of working in microwaves, because at that time, people were talking about the ultra-high frequency things you could send through lenses made out of "pitch" and stuff like that. It sounded like an interesting thing. And they talked about "plasma oscillations;" interesting discharges in gases.

So, I started out and set up the standard discharge tube where you have the striations in it and stuff like that. Tate, was a very systematic guy, and the students had great respect for him because he was so good, so you were sort of afraid of him. He was so busy, he was then instrumental in setting up the American Institute of Physics. He was editor of the *Physical Review*, the *Review of Modern Physics*, and he commuted all of the time to New York [New York] to set these things up. Remember you went by train, and it took two days each way, so, he wasn't around that much, plus his editorship...

GRAYSON: We'll have to continue on another tape...

[END OF AUDIO, FILE 1.1]

GRAYSON: Okay, we're on Side B of Tape 1, interviewing Al Nier. Tom Krick and Mike Grayson are doing it. I meant to say that on Side A of Tape 1, but at any rate, we've got it on Side B, and he was telling us about Tate's activities. [laughter]

NIER: Tate, while he was the advisor for most of the experimental graduate students, he never spent much time with them. In particular, he didn't really spend much time with you unless you were getting results. It was really a rugged existence for people. I was batting around here, trying to work on something that he wasn't particularly interested in, and he would come and see me once in a while, and he'd look at the glow discharge and say "So, what're you going to do with that?" and so on. I wasn't quite sure what I was going to do with it; then finally, onetime he came down, and said "Gee, sounds a little like you're working on something that General Electric did years ago and never bothered to publish."

GRAYSON: Real helpful!

NIER: Real helpful! But, it was helpful. There was a message there. I batted around a little more, and he suggested I might work with a guy by the name of John Williams, who had just come here as a post-doc to act as an assistant to Tate. Tate had prestige, he had money, so he

could hire people full time as post-doc assistants. John Williams, whose whole background was in X-rays, had been with Sam [Samuel K.] Allison at [University of California] Berkeley and then at [University of] Chicago. He came here, and was supposed to work on electron impact, but by that time, the field was really drying up in the sense that it wasn't terribly exciting for physicists to continue in the field. Chemists hadn't quite caught on, and couldn't build instruments, even though it would turn into more of a chemical field; the study of the structure of molecules and so on. Tate had lost interest. He, sort of, had a short interest-span anyhow, because as editor of the *Physical Review* he knew everything that was going on, and nuclear physics was just coming in. He had Williams working with him, and they were going to build up a mass spectrometer, sort of, picking up where P.T. Smith had left off.

Well, Williams barely got here and nuclear physics began to break forth, and people were able to do nuclear disintegrations with relatively low voltages, a few hundred kilovolts. You could perform reactions on the light elements like lithium and boron. We had a 300,000 volt X-ray set here in the department, which really had never been used for anything before. So, Williams was encouraged to go into nuclear physics and of course, he was anxious to do that anyhow. He dropped out of the mass spectrometry entirely, and I was left alone; all on my own.

Well, in the meantime, I'd built an instrument taking advantage of all of the work of my predecessors. All the early instruments that we're talking about were 180 degree mass spectrometers, and you had a solenoid that enclosed the whole instrument. You accelerated electrons along the solenoid, because the magnetic field collimated them and you drew out the ions sideways. You had a long glass tube, which would be on one side of the hole in the solenoid, and a horseshoe-shaped glass tube would come off it, which contained a 180 degree analyzer. Bleakney's instruments were housed entirely in glass, using a four-inch diameter glass tube. (Figure 2) Smith came up with the idea of just having an arm on the side for housing the magnetic ion analyzer, so you didn't need the great big glass tube. You didn't have to have wax ends on it, so you could seal it all up. And I should mention also that the thing that was remarkable here was that they used ultra-high vacuum techniques in the sense that there were no grease joints, no stop-cocks, no nothing else. They used mercury pumps, and so you could pump down, bake the apparatus, and so on. You didn't have all of the impurity problems, because many of the early people who played with electron beams, had water present, and you formed hydrides and had all kinds of stuff that led to confusion in your results.

GRAYSON: So, these were glass-sealed systems?

NIER: Pyrex glass-sealed, the electrical leads were tungsten, which you could seal into the glass.

GRAYSON: So, you had to have some glass-blowing technology?

NIER: That's right. We had a very good glass-blower. But you also had to be able to do some of your own glass-blowing; I became a pretty good glass-blower.

GRAYSON: So, even as we're talking about the early 1930s, you're saying that the vacuum technology that was being used in these experiments was comparable to vacuum technology that was available many, many years later in other parts of the country.

NIER: In most places. Except that the people who manufactured electron-tubes had known this for decades, like GE, Westinghouse, Bell Labs and so on.

GRAYSON: Then, in 1930, electron tubes had been manufactured for how long?

NIER: Ten or fifteen years—1920 thereabouts. The good vacuum technology was standard in factories, in places like General Electric.

GRAYSON: So, in a way, better vacuum practice was being done in industry.

NIER: Industry, but not in the universities.

GRAYSON: Not in academia, I see. That's interesting.

NIER: And very few people in the academia had good vacuums. There wasn't anything mysterious about it; it just was the way it was.

KRICK: They just worked with grease-seals in those days, and just did it that way.

GRAYSON: Yeah.

NIER: So, anyhow, that was the kind of tube I inherited. I didn't work on electron impact on gases. Although I had the first spectrum of benzene that anybody ever had, and I was never encouraged to publish it. (Figures 3, 4)

GRAYSON: Really?

NIER: Mass 78, 77, 76 and then, finally...

GRAYSON: Where is it? [laughter]

NIER: I have it around. I have a copy of it.

GRAYSON: You have it around?! [laughter]

NIER: But it was never published. I made a slide of it once and showed it at a meeting, but that's as far as it ever got. The thing I missed the boat on was the metastables because as you got down in the 20s, and you had some fractional mass numbers. I didn't try to interpret them. It wasn't until later, when Hipple—John Hipple was at Westinghouse—and Condon, who had been interested at the time, published something on the interpretation of these fractional mass peaks—26.3 or thereabouts.

GRAYSON: When did you put benzene in the mass spectrometer? I mean, that would have been what year?

NIER: 1934 or 1935.

GRAYSON: Okay. What prompted you to put benzene in?

NIER: Well, it sounded like fun. The instrument I had was a larger solenoid than people had used before. It was supposed to be part of a big electromagnet to be used with a cloud chamber for some nuclear studies, but the electromagnet was never completed, so there was a solenoid available with a seven-inch hole in it; about so long. It weighed 500 pounds or thereabouts. I have a picture of it, by the way.

GRAYSON: Yes, we can hopefully get a picture or get a copy.

NIER: So, I had a larger magnetic field and a larger radius than had been used before. A five-kilowatt generator was used to power it. I could get resolution up to a hundred or so. I worked with the cadmium isotopes for instance, and so, to work at mass 78 wasn't all that hard. I

looked in the literature of the chemists who had tried to build instruments in the 1920s. They had built instruments and done some work on electron impact, but with their greasy, watery systems, they found all kinds of crazy stuff. Also they didn't have nearly the resolution I had. At that moment, I had the highest resolution mass spectrometer in existence. There was nothing going on in Europe along this line, and there were a few places in America. Princeton and Chicago did some work in the chemistry department. But nobody had as good an instrument as I had with this solenoid that I'd inherited when Williams started out. There was a real break. Again, it shows you what happens to people. One of the problems we had was, you had to run the solenoid, which took a lot of power, five kilowatts, off a motor generator. And motor generators weren't exactly the most stable things; armatures would wander back and forth on the shaft, and the output voltage changes a volt or two in 110, or thereabouts.

GRAYSON: Sounds like mass spectrometry in China today [1989].

NIER: [laughter] Yeah. Right. So, the question is, what to do about it, and it was at this point that my engineering background and connections came in. To me it seemed hopeless to control the output voltage of the generator, because it required stabilizing so much power. So why not be more subtle? It occurred to me that, since the mass that you collect is proportional to the square of the magnetic field divided by the accelerating voltage, why don't you fool the instrument by monitoring the magnetic field, and changing the ion accelerating voltage so that you stay on the peak. I went to talk to Dr. Hartig, who I'd known so well, and who'd been my savior a couple of years before, about this. Could we use a vacuum tube as a magnetron, and put it on the axis of the solenoid, outside of the instrument, pick up the fluctuations, and modulate the high voltage in such a way that if, for example, "B" went up by 1 percent, the accelerating voltage went up by 2 percent. B^2 over V . The result was, the ions didn't know the difference, so the trajectory was stable. I could work with heavy ions where you needed high resolution. It was my first publication, a little note in *RSI [Review of Scientific Instruments]*.²

GRAYSON: Yes, I noticed that.

NIER: As far as I know, only one person elsewhere ever used the idea—someone at Columbia. One of the guys there used the idea but I don't know whether it was successful or not, because they had other instrument troubles. Anyhow, it was the thing that got me started, because it made it possible for me to work with our cumbersome apparatus. The magnetron gave us the stability we needed. To tune up this apparatus you sat it on the side of a peak, and tuned the gain of the magnetron. You moved the position of it, with a slider. You slid it in and out of the magnetic field, to get it so that it would just balance, so that it didn't overdo it or didn't under do it, and you could sit there all day, on the side of a peak.

² Alfred O. Nier, "Device to Compensate for Magnetic Field Fluctuations in a Mass Spectrograph," *Review of Scientific Instruments* 6 (1935): 254.

GRAYSON: What you were really doing was taking the fundamental concepts in electrical engineering such as feedback control...

NIER: That's right. That's what it was. Feedback control.

GRAYSON: And using it in the physics experiment to make the thing work.

NIER: To make it work, that's right. That is why it's so interesting how these things overlap. It isn't quite clear which is which, you see.

GRAYSON: Well then, how does one get a spectrum with this machine?

NIER: Well, we changed the accelerating voltage. We had decade boxes, which was all you had then, and put-and-take boxes, because it was just a potentiometer arrangement. We used a put-and-take box so you didn't change the resistance of the string; you just changed the tap on it, and you did it in a precise way. You sit there, and turn the nobs, and follow the galvanometer spot that you had to look at...

GRAYSON: So, now, we're saying that taking a spectrum represented a day-long proposition?

NIER: Well, hours.

GRAYSON: Hours long?

NIER: Originally. And another great break came. Being born at the right time and being around at the right time helped. The electrometer tube had just been invented. One of the other graduate students had built, for a master's degree, an amplifier using an electrometer tube. My first work was done with a quadrant electrometer, where you have a little piece of aluminum foil shaped like a four-blade outboard motor propeller hanging on a metallized-quartz fiber in a hollow round metal box cut in quadrants, opposite quadrants tied together. A potential difference between the pairs of quadrants sets up an unbalanced force which rotates the little propeller-shaped object, which in turn twists the fiber and attached mirror. You passed a current you wanted to measure through a resistor mounted between quadrant pairs. You couldn't buy commercial high-impedance resistors then, so you took a pencil and a piece of insulating

material, such as fiberboard, and put pencil marks on it. You rubbed the pencil on the insulator, and that was the resistor. If you got the resistance too low, you erased some of the graphite. You adjusted this to get the amount of resistance you wanted. And that was standard in those days. Everybody was doing this in the 1920s and early 1930s. But I came in just at the tail end of the quadrant electrometer era, so I had the privilege, if you want to call it that, of working for only a short time with a quadrant electrometer!

GRAYSON: And so, basically, you adjusted the put-and-take box, as you call it, to move to different portions of the peak, and...

NIER: That's right, and you would go over the whole mass spectrum, dot by dot.

GRAYSON: Okay, then the job of the operator constituted recording the information at each point.

NIER: On a piece of paper.

GRAYSON: Did you stay at a particular spot on the potentiometer for a minute, or did you collect data?

NIER: Well, what one generally did was probably not much different from what one might do today. You went over the whole spectrum to see what it looked like, and then you went back and did peak-stepping.

GRAYSON: Okay. The mass scale was assigned in what way then?

NIER: Yes, inversely proportional to the accelerating voltage.

GRAYSON: So, if you knew the mass of a prominent ion, you could compute the others as well.

NIER: Then you could predict every other one.

GRAYSON: And this was on a machine that had a resolving power, you say, of about a hundred.

NIER: Yes.

GRAYSON: And the peak shape looked like?

NIER: Well, not as sharp as nowadays, but not bad.

GRAYSON: Not bad. Do you have any data from those early experiments?

NIER: Yes, yes.

GRAYSON: Could we get copies of that kind of information?

NIER: Yes. You better write that down so that we remember to do all these things I'm promising you. [laughter]

GRAYSON: So, the benzene...you just put that in as, kind of as a lark.

NIER: Yes, but I said, "Look, my advisor isn't interested in this anymore, so I better get in something to do with nuclear physics." And I said, "Gee, if I can do benzene, I could do such things as argon." Argon-38 had just been discovered, spectroscopically by some people in Holland, if I remember correctly, but the exact amount of argon-38 wasn't known. Argon-36 had been known before that. [Francis William] Aston may have even seen the argon-36 along with argon-40, the most abundant isotope. We had standard gas we bought them from Linde or from Air Products. Sure enough, when we introduce argon, there were three isotopes. without any question. So I made the first measurements on argon-38 that showed the five-to-one ratio from argon-36 to argon-38, I did that in a half an hour one afternoon. (Figure 5)

GRAYSON: This was with the machine that had the feedback control.

NIER: And the stability, so you could work effectively on isotopes.

GRAYSON: Then that represents really the, the discovery of the isotopes of argon; all of them.

NIER: Not quite, they'd been known, but I made the first quantitative measurements on all three.

GRAYSON: So you made accurate abundance determinations of the argon isotopes. I noticed in some of the early papers the references to a "spectrograph" in the title.

NIER: Yes, we were loose, everybody used the word "spectrograph" because that's what Aston had invented. But that was wrong; and later that was corrected. But I used it for a long time, I didn't know any better. And a lot of other people didn't know any better either, but that was corrected some time down the road and instruments which use electrical detection are now called mass spectrometers.

GRAYSON: In essence then, you really never did any work with spectrographs. Could that be said?

NIER: That's correct.

GRAYSON: So, it was always been electrical detection.

NIER: It was always electrical measurement. That is correct.

GRAYSON: Okay. That's interesting. Even in your Mattauch-Herzog instruments, you still were doing electrical detection.

NIER: Always.

GRAYSON: It's occurred to me, and this is just an aside, that you see "spectrometrists" used some times but I haven't found that word in the dictionary. You see "spectroscopists," but I don't know if I've seen "spectrometrists."

NIER: I may have. But it isn't common apparently.

GRAYSON: No, but I think most mass-spec people think of themselves as a “mass spectrometrists” as opposed to a “mass spectroscopist.”

NIER: Yes, of course. But you see, “mass spectroscopy” is broader. It would include either spectrography or spectrometry; so that gives you more prestige in this instance. [laughter]

GRAYSON: Well, now that we have those little nomenclature details taken care of, we do have a promise that you are going to supply us with a copy of your raw data, right?

KRICK: Right, benzene and other point-by-point measurements.

NIER: Right.

GRAYSON: That would be excellent, because I think, as you’re well aware, with instruments today, and computers and so on, so much of this type of activity is just taken for granted and people don’t even think about it.

NIER: Oh, yeah. Well, the strip-chart recorders of those days were very primitive...

GRAYSON: Oh yes!

NIER: Even during the war, when we were building instruments for the Manhattan Project. Electronic chart recorders were not yet available, at least at the time when we needed them. So, we employed the old Leeds & Northrup devices where you had a galvanometer and a needle that went back and forth and something bit down on the needle. They were quite precise. Amazing instruments, by the way, but terribly slow by modern standards.

GRAYSON: I just finished listening to a reminiscence from John [H.] Beynon who described a high-speed recorder they had in England. He said, at that time, which I don’t recall when it was, if the damping was a problem, you had to stay away from it because it spread ink all over the place. [laughter]

NIER: I believe that. That’s right.

GRAYSON: Well, I posed this question to you earlier; if you look at the mass spectrometer as a series of different systems: a sample inlet system and an ionization system, an analyzer, detector, data system and vacuum system, starting back from when you first became involved, each of these systems were kind of fundamental and basic, and today we have tremendous advances in each of these. Why don't we just explore each of them with your early instruments. Like sample inlet. Obviously, when you say "put benzene into this machine", how did you do that? How did you put samples in that early machine?

NIER: Well, we had all-glass systems. And, we didn't use stop-cocks and such. I don't remember exactly, but we had a bottle of benzene—I still remember the little bottle. We got it from somebody in organic chemistry, and it was very pure stuff; that was part of it. I suppose I put a small amount in a glass tube, froze it in there, and then sealed that onto the manifold system. This probably had a u-tube. You could feed mercury into the bottom of a u-tube, a y-shaped one, and you could seal it off that way. And I suppose that's what we probably used in place of stopcocks. The leak system at that time, the standard thing was to send gas through a glass capillary. It was an art form to make the capillaries. You took a piece of 8 millimeter Pyrex tubing, heated it locally with a torch, and then drew out the tubing to a capillary. We worked entirely with Pyrex, so it was easy in that sense. Then you broke off, say, six inches from that part of the tube; and you hoped you'd drawn it at the right rate. If you pulled very fast, you'd make a very small tube. If you wait long enough, till it's just about ready to harden, you can make kind of a heavy walled tube. There was a lot of technique involved. You got pretty good at this. After you broke it off, you would test it by putting pressure behind and putting the end underwater to see if it bubbled. If it bubbled, it probably was too fast. So, you threw it away. Then, if it was too slow, you'd break off some more. So you got pretty good at this. Then you sealed it into a tube, which covered the capillary, so there was a ring-seal connecting it and protecting it. You ended up with a continuous length of, 8-millimeter tubing, with a barrier in it, and the capillary sticking in one part. That was how you admitted the sample into your spectrometer.

We didn't worry much about fractionation, what it did to the sample. You were so happy to get something going, so you didn't worry much about that then. This was all sealed onto the apparatus, you pumped with the mercury pumps. In those days, we couldn't afford liquid nitrogen, except in special experiments. We used dry ice in alcohol or acetone for cooling. We got liquid oxygen in those days from the Air-Reduction people who had a plant over here in southeast Minneapolis [Minnesota], not far from the hotel you're staying in. I used to go over in my own car and buy it by the liter. I would take a quart thermos bottle, and go over there, and they'd fill it for me and I'd bring it back, and you nurse it along. It would last you about 24 hours or so, and I would go back the next day and get another liter if I needed more. The department owned some dewar flasks that would hold a couple liters. They were used for demonstration purposes, in standard-run physics experiments.

GRAYSON: Do you have any feeling for the level of vacuum that was actually achieved?

NIER: Yes, we used a McLeod gauge. We didn't use ionization gauges; some people did. In a McLeod gauge, you trapped a known amount of gas in a bulb and squeezed it up into a capillary where its pressure could be measured using $pV=p'V'$. Pressure was measured beyond the trap, because you didn't want mercury in the system. We had a very good glass-blower. He made these McLeod gauges with a sensitivity such that the first millimeter of the capillary was equivalent to about 2×10^{-6} torr. So you had a 500cc bulb with a capillary that was about a half a millimeter in diameter. And of course, there was the matter of selecting capillaries that were uniform. But, since it was a squared scale, it didn't have to be that accurate anyhow. But we didn't really use that very quantitatively, the amount, it was just an indication that....

GRAYSON: Essentially the vacuum system was usable.

NIER: ...it was good. You could sense when the vacuum was good, because if there was no air in there, the mercury would bang against the end and stick. So you had what you called a "sticking vacuum," meaning that there was very little gas in there. That was like 10^{-7} [torr] or so, and I suppose that's as good as we ever got.

GRAYSON: And so, this would be between the trap and the mercury diffusion pump.

NIER: That's correct, that's right.

GRAYSON: Then basically, with your trap you were pulling out the mercury so you had a quite good vacuum.

NIER: I think we always worked with better than 10^{-6} . But I don't think we ever got down to 10^{-8} or lower. The mercury vapor pressure was enough, first of all, that it would probably be around this range...

GRAYSON: Sure.

NIER: ...and there must've been a little water. But it was certainly orders of magnitude better than what most people were doing then. And, as I say, that was standard technique in this lab.

GRAYSON: Okay. Now, if you were introducing a sample for isotope analysis, there were two problems. One was getting the sample into the gas phase, and the other was the problem of getting it inside the instrument. I guess getting it inside the instrument would be similar to the way you would do for benzene.

NIER: Yes. Well, argon was a cinch.

GRAYSON: Oh, yes.

NIER: You just trapped some of it, that was easy.

GRAYSON: Yes, gases would be somewhat easy. Now, for instance, for some of these other isotopes...?

NIER: Well, that took a little more technique.

GRAYSON: Did you have a chemist assist you in some of these.

NIER: No, not in those days. [laughter] But I stuck to things I learned to work with...you see, the next thing I did after argon was potassium. (Figure 6)

GRAYSON: Okay

NIER: That was interesting, and it really fit into the nuclear physics picture because at the time there was a big argument about which isotope was responsible for the radioactivity of potassium. That'd been argued about for years. The potassium-39 and potassium-41 were well known, and many papers appeared in *Nature* magazine in which people were speculating, is there another isotope? Is there a potassium-40? Is there a potassium-42, or potassium-43, or some other isotope? This was right in 1933 or so, when nuclear physics was just coming in. I said, "Well, this is just made to order, because if I'm going to get attention around here, I need to be in nuclear physics, and here it fits in." [laughter]

So, what I did then, was to start working with potassium. And the way you did that was you got some potassium chloride, and you heated it with calcium filings, and you could make metallic potassium that way. We sealed off the potassium in little tubes with break-offs on them. I did the same thing with rubidium, for instance. That was kind of the standard thing that

one did. Of course, at room temperature, potassium doesn't have much vapor pressure. So you had to run the spectrometer tube hot. But you couldn't run all of it hot—not the collector end—because the glass became, conductive. So, we had to separate the ion gun and the acceleration part from the analyzer part. You only heated that part of the tube, and some potassium would leak through the slit into the analyzer, but not enough to make any difference, and it would fall dead since it was cold there. It didn't bother you. It never got around to the collector itself, and that worked.

GRAYSON: So, basically, the problem of a hot-ion source is what you had to deal with.

NIER: Yes, that's right.

GRAYSON: The fundamental technological problem that we all have with samples even today. You had to devise a way of heating it, keeping it warm.

NIER: Devise a way of heating, correct

GRAYSON: Did you just wrap a heating tape around it?

NIER: Yes, just a heating tape around it. Well, we put a layer of asbestos on it. I should be dead! [laughter] I think the asbestos problem is badly overblown. But anyhow, be that as it may, you got a roll of asbestos, got a piece off the roll, wet it, let it dry on the glass tube, wound a nichrome heater on it, put another layer of asbestos on, and waited until the next morning and it would be dry. Or if you were not patient, you'd start putting electric current in gingerly, and it would dry out more rapidly; sometimes we put some water-glass on as a final coat to hold the asbestos in place.

GRAYSON: So, you didn't even wrap it with heating tape. You made your own heater?

NIER: I don't think heating tape was available in the United States. So we made our own heaters. Besides, it was cheaper!

GRAYSON: You made your own heaters, on the spot.

NIER: That was standard. And sometimes, if you wanted to take them off, you made a piece of aluminum tubing out of this aluminum sheeting, and wrapped it around and put rivets in it. That made a slip-on furnace, a tube-furnace that could be slipped on things.

GRAYSON: Okay, that's the end of this side.

[END OF AUDIO, FILE 1.2]

GRAYSON: [...] That was the primary way of volatilizing a sample?

NIER: Yes. Also, you could put a little oven into the source itself. A little locally-heated oven; that's what I did later, but not here at Minnesota. I did that at Harvard [University] when I worked with elements like calcium, and strontium, uranium, things of this kind. I had a little box that I could heat with a little heater right in the source.

GRAYSON: Kind of like a predecessor to today's solids probes that are heatable, but they were a very, very fundamental kind of thing.

NIER: Yes.

GRAYSON: Well then, what about that mass analyzer? Well we've, kind of, covered that. At this point, the early mass analyzer you worked with was essentially the solenoid design. Was this a derivative of your predecessor's instrument?

NIER: Well, there's P. T. Smith, Bleakney first, then Smith, who had come out with the idea where the analyzer was an arm on the side of the tube.

GRAYSON: And that was essentially the technology at the time?

NIER: Yes. I have a picture of that, my first glass tube.

GRAYSON: He's going to give us a picture or a copy of that picture, right? Make a note of that, Tom. Now, you've got to detect this ion current at the other end of the tube.

NIER: Yes, and I was fortunate. In the midst of all this, the electrometer tube became available and we happened to have one. One of the other graduate students at the time had built this amplifier and so I had the use of it.

GRAYSON: We're starting from a faraday cup?

NIER: Yes, a faraday cup. I think we still used the graphite on a little fiber board for the resistors. But, about that time, you could also get S.S. White resistors. S.S. White was a dental supply place and I've never understood why they made resistors, but you bought a little black thing that was about the size of a fuse, like an automobile fuse. A little object about a quarter of an inch in diameter and an inch and a half long. It had leads molded in. Well, the way resistors have always been made. But this was solid, some black stuff, and I have a feeling...I don't know, it'd be interesting to hunt that down sometime...this was some of the stuff they made dental plates out of. They probably put graphite or some other electrically conducting material in it to make it dark. Somebody discovered that it had resistive properties. See, this was some kind of plastic...and so then, they made these things deliberately by just simply molding some wires in and you bought these things 10^9 ohm, 10^{10} ohm, 10^{11} ohm and so on. And if the resistance was a little lower than you wanted, you would file off or grind off some of it to make it smaller in diameter.

GRAYSON: This was to get the feedback resistance for the electrometer?

NIER: Yes, but there wasn't any feedback in those days. I mean, it was *the* resistor.

GRAYSON: Okay.

NIER: You had the current go through it to get a voltage drop across it and the electrometer, just like any vacuum tube, measures voltage.

GRAYSON: Because you were working with small currents you needed a large resistance.

NIER: Yes, because the bigger the resistance for a given current, the bigger the voltage. Of course, the virtue of the electrometer tube, was that it used very low voltages. They're very well made. I still have some here by the way. Some of the original ones, and...

GRAYSON: Is it possible to swipe one?

NIER: Well...you can look at it.

GRAYSON: Or look at it or take some pictures of it.

NIER: I don't think there are many of those around. And that was the standard that everybody used for measuring small currents. The voltages on the tube...instead of the standard vacuum tube in those days which had like 90 volts or 135 volts or 200 volts on the plate—were 4 volts or 6 volts. So you never ionized any of the gas that was left inside. They were carefully made, under a very good vacuum. They used oxide-coated films, so it didn't get very hot, so you didn't have X-rays or much ionizing ultraviolet. The result was the internal impedance looking into it was very, very high, like 10^{13} ohms or 10^{14} ohms or more; so when you put a resistor across the input, it wasn't shorted by the tube. They used balancing schemes by making a bridge out of the tube. It was a pentode, so it had several electrodes in it and you had a galvanometer between the screen grid and the plate. The galvanometer measured, then, the unbalance of the circuit when current flowed in the resistor between the cathode and input grid.

GRAYSON: So, then, the output of the tube was measured by the galvanometer deflection? Is that the way it was hooked up?

NIER: Yes.

GRAYSON: How did the signal information get from the galvanometer to the recorder.

NIER: Well, the light reflected from the mirror on the galvanometer to a scale. And you read the deflection.

GRAYSON: So, you read the scale off as you went along? And adjusted the put-and-take-box.

NIER: Yes. That's right. And then you took the reading. You waited till the galvanometer got to equilibrium, five seconds or so. You had to have the right damping resistor in the galvanometer so it didn't oscillate. It took time.

GRAYSON: Yes. [laughter]

NIER: Except for the long time it took, you could still make very good measurements. You could also get by without a resistor and just measure the charge build-up in the capacity of the system. You then got a rate-of-drift measurement, and you'd measure the rate. In all of my work at Harvard, where I'd worked on very rare isotopes, I did just that. (Figure 7) I took the resistor off and watched the rate of drift. You could measure currents about 10^{-16} , 10^{-17} amperes. You waited, like, 30 seconds for the spot to cover the scale, and you worked backwards for calibration.

GRAYSON: So, the drift measurement would be kind of like an integrative...

NIER: That's right, an integrative measurement.

GRAYSON: ...integrative measurement, compared to the electrical detection scheme.

NIER: That's right. And that was used by people long before my time; there were people doing ionization measurements with radioactivity. They had been working with that kind of technique for years. I mean, that was nothing new. That was the technology of the times.

GRAYSON: That was standard practice.

NIER: Right. We sometimes forget that individuals were doing some pretty exacting experiments in the 1920s.

GRAYSON: They didn't have the technology we have today, but they were still using what they had to make very good determinations.

NIER: They were doing some very good measurements. Slow...

GRAYSON: That's interesting. As an aside, I talked to a friend of mine once, and we were going to do a little discussion of... [beeper beeping]

NIER: Pill time. I'm sorry. Go ahead.

GRAYSON: Okay, we were going to do a little local course on mass spectrometry for some of the people in St. Louis, and I told them, "I'll bring a spectrum from a magnetic machine, you can bring a spectrum from your quadrupole machine, we can show them the difference." And he said, "I can't get a voltage versus mass signal out of my quadrupole mass spectrometer." The computer takes everything. How can you do that? You have to be able to look at the mass spectrum in reality somewhere along the line but, in modern instrumentation, in a lot of modern instrumentation, it's not available, you *cannot* look at it.

NIER: Black box.

GRAYSON: So, I think the point that you mentioned about this early work is that people certainly knew how their machines were performing every step of the way.

NIER: That's correct.

GRAYSON: Because if they weren't working correctly, you certainly were aware of it.

NIER: You knew it right away.

GRAYSON: What about some of those problems, incorrect performance? Obviously, we're talking about experiments that worked. There were some that didn't work, right? You had problems or difficulties.

NIER: I was very naive when I was here doing this work. One of the interesting things that I ran into was mass discrimination. We didn't worry much about discrimination in the instruments; I guess I didn't know enough. You sensed from things that you knew, that it must be working. You'd put nitrogen in and people had measured the nitrogen-14, nitrogen-15 ratio roughly. So, you knew it worked.

One of the things I encountered, but not really seriously until I went to Harvard after I left here, was how to make sure that the ion source doesn't discriminate. I became pretty good at judging the performance of the source by changing various parameters and seeing how an isotope ratio varied. For instance, what I used as a standard when I was at Harvard, was mercury, because there was always mercury present, because of the mercury vapor pumps. I would compare the ratio of the mercury-204 isotope to the mercury-198. I think if you look at a modern isotope table, the mercury-198 is very close to 50 percent higher than the mercury-204.

So I would look at the mercury, and I would diddle with the ion repeller voltage that you used to push the ions out of this ionizing region and fool around with other things. If you used extreme values, you might get funny ratios; instead of 1.5, you'd get 1.6 or 1.7 or 1.3 or something of that sort. So, I got to be pretty good in estimating when it was working right. Keep in mind, we had no standard isotope mixtures that you could go against. It was just your intuition and experience.

GRAYSON: Well, at the very least, though, you were tuning the instrument to a point where the ion source operation would be standard.

NIER: Standard, yes, and not critical. For instance, you could change some of these parameters so that the values you got didn't change much over a big range, and then it would drop off the end. You worked in the middle of the range. It was always a system of compromising. And this was...experience, I mean you had to learn and it took a lot of feel, you couldn't have a technician come in and take data tomorrow morning. You had to do it yourself.

GRAYSON: Right. So all this data that was acquired with regard to the isotopes...there were a lot of little technique things that were done...

NIER: It was just full of it!

GRAYSON: ...in order to make sure that when you got to the end, and there was an answer, that it was indeed an answer which represented a measure of reality as opposed to just measuring something that came out.

NIER: That's right, that's right...

GRAYSON: You've got a source discrimination problem that you had to deal with. Tuning it so that it wouldn't discriminate between the various isotopes. What other kinds of problems did you have to deal with to ensure that you had an answer that you could put in the literature and not have someone come after you with a...[laughter]..blunderbuss.

NIER: Well, you worried about impurities, of course. If you were talking about rare isotopes, you were always worried about interference. You did tricks like change the electron accelerating voltage. In potassium...see, I found potassium-40 when I was here.³ That's what

³ Alfred O. Nier, "Evidence for the Existence of an Isotope of Potassium of Mass 40," *Physical Review* 48 (1935): 283.

really set me up in business because it was only abundant one part in 8,600 I think, something like that. And that was really very difficult, but I remember we did tricks. When you ionize potassium, it takes... I don't know what the ionization potential is—about 3.8 volts or some number below 5... way, way down. And so, if you ran the electrons slow enough, you'd eliminate most other impurities and interference. You'd do tricks like this. Or if you're dealing with vapors, such as potassium, I'd change the vapor pressure by heating the source or cooling it and seeing if the potassium-40 tracked to 39-41. You did various tricks of that kind, you worried about doubly charged ions interfering or whether you got doubly charged when you wanted them. You did all these kinds of tests. And again, as I say, this represents experience. You got a feel of when things were right.

GRAYSON: Well, you're keeping a very critical eye on what you're doing, every step of the way, to ensure that you're not making assumptions which will get you in trouble later on.

NIER: That's right, that's right.

GRAYSON: And, purity would be a problem. Did you rely on, for sources of these materials say... for instance, potassium, did you rely on a chemical house or did you have special samples made?

NIER: Well, both. But most of the stuff came out of bottles from chemical houses. It wasn't till later that I had lead samples made for me when we were doing the geological work, but that was because you had to make those. But most of these things just came out of bottles. You could do a pretty good job making potassium and rubidium, for instance. When you made it yourself out of the chlorides, with calcium, you automatically got a pretty pure assay on it.

GRAYSON: Because?

NIER: The process itself did it. And then the vapor pressure; other things were not that volatile, so you'd distill the stuff over, and get pretty good stuff.

GRAYSON: There were a number of different things that helped you to get it. And it was, again, another aspect of the whole measurement problem that you took into account as you proceeded to do this work, and to determine these isotopes.

NIER: That's correct, right.

GRAYSON: So, now your recorder in these days was really, the person individually. He wrote down the results.

NIER: With a pencil...and a notebook.

GRAYSON: [laughter] Then you did what—did you plot the data?

NIER: Plot it on graph paper, on 8-1/2 x 11 graph paper, 1/2 inch standard paper. [laughter]

GRAYSON: And took into account all of the multiplication factors?

NIER: You had a shunt on the galvanometer to cut the sensitivity so you could change scales. So, we'd have steps like factors of two and a box that you switched. And generally you made the shunts yourself. You could also buy these, but I think we made our own. We had precision resistors and you bought multiple point switches—and you wired resistors between the points; and that was it.

GRAYSON: So, basically the technology of the vacuum system was there, pretty much to the same level as we have it today. Some of the detection was getting there with the advent of the electrometer tube. In terms of recorders and sample inlets and actually mass analyzers, the technology was kind of not where it is in modern-age, yet. I mean, there was a lot of evolution that had to be done. But fundamentally, the vacuum system and some of the electrical detection was pretty far along.

NIER: Yes, was really quite far along. As I say, I came in just at the right time on that. The interesting thing is that, nobody used inverse feedback amplifiers yet. And the first ones we used were during the war or just at the beginning of the war. They ended up in all of the Manhattan Project instruments which we developed. There had been very few publications on inverse feedback amplifiers.

GRAYSON: What we know today as the operational amplifier.

NIER: The operational amplifier...that's right. Of course it was all vacuum tube stuff in those days so it was sort of tricky the way you wired stuff together. But we started using them in about 1940 for our work. They were never as stable as the plain electrometer, so at first we used

them only for measuring large currents; for example, we used them when we did a multiple collection with the instruments used for analyzing uranium in the Oak Ridge [Tennessee] plants used for isotopic separation.

Actually, we started using feedback amplifiers here in 1940, well more like 1941, when we separated carbon isotopes for biological experiments.⁴ We had to do a lot of analyses, so we built the first system where we could measure the 44 peak, CO₂ with the inverse feedback amplifier and then balance a fraction of the current against the current from the 45 peak. You would have a low impedance output on the feedback amplifier; put a decade box across it and then take a fraction of that and balance it against the 45 peak. Then you used the galvanometer as a null reading device. And that's when we started measuring ratios directly.

GRAYSON: So, that really represented, for the isotopic abundance work, a technological step forward.

NIER: That's right, a real step forward.

GRAYSON: A step of real significance. Because the answer was almost there and basically, it was just a matter of getting some stability.

NIER: There were a few publications on inverse feedback amplifiers at the time when I was at Harvard, from 1936 to 1938. The people there in electrical engineering were very good, and they knew all about inverse feedback systems and I learned a lot about how you build inverse feedback amplifiers. There were two guys by the name of [F. V.] Hunt and [R. W.] Hickman who were faculty members there, who had worked on inverse feedback circuits for high voltage supplies.⁵ They had a publication on it and I learned from them how to build inverse feedback devices.

GRAYSON: Do you feel that your background in double-E [electrical engineering] helped you here again?

NIER: Immensely.

⁴ Byron F. Murphey and Alfred O. Nier, "Variations in the Relative Abundance of the Carbon Isotopes," *Physical Review* 59 (1941): 771.

⁵ R.W. Hickman and F.V. Hunt, "On Electronic Voltage Stabilizers," *Review of Scientific Instruments* 10 (1939): 6-21.

GRAYSON: Because you were able to pick up where they were right away and you could see exactly what it was they were doing.

NIER: I understood right away what they were doing. And there were tricks, like especially in the power supplies because you had high voltages and you had problems isolating things and getting it down to a low voltage. There were all kinds of tricks, so unless you understood circuit theory you couldn't get very far.

GRAYSON: I would gather, then, the dynamic range of these instruments—even the earliest ones you worked with—was really quite good.

NIER: Yes, it was quite good. You see, at Harvard, I had a standard glass galvanometer scale 50 cm long and you could read this to a fraction of a millimeter. The light spot was focused and it had a hairline that projected an image on the scale. You could read that to say 2/10 of a mm. Then your shunt on the galvanometer changed the range a number of times. And if you used drift methods for very small currents, you picked up another factor of 100 or 1,000. So I suppose, we were playing with million to one dynamic ranges. It was cumbersome, but it worked.

GRAYSON: Yes, but essentially, mass spectrometry has inherent in it the need for that type of dynamic range capability.

NIER: That's right. Yes.

GRAYSON: And even though at the time, the technologies were not as advanced as they are today, you were actually making use of the full dynamic range then.

NIER: Yes, that's right.

GRAYSON: You mentioned Harvard a number of times. Maybe we should move on to Harvard for a minute. But first, you were encouraged by Tate to get into a different area. Were you, kind of, kicked into a different area? [laughter] Did he recognize that what you had gotten into was valuable?

NIER: Well, indeed. When I was able to do the argon in one afternoon, and then moved on to potassium and found K-40 [potassium-40], then I was really high on his list.

GRAYSON: So, you moved up from the bottom to the top? [laughter]

NIER: Yes, and there was another fortuitous thing that happened in connection with that, which is most interesting. Tate was editor of the *Physical Review* and we published in the *Physical Review*. I'd made the K-40 discovery in spring of 1935, just about the time he was ready to leave for the summer to go to Columbia where he taught a course in summer school. My paper on K-40—it was a letter and I have copies of this by the way—went into him as editor. So, here he was, my advisor, and also the editor. [laughter]

GRAYSON: That's an interesting situation. [laughter]

NIER: And I was his student. He was very cautious and did not like students making mistakes.

GRAYSON: He had your whole life in his hands. [laughter]

NIER: Well, he accepted the paper as editor, and it was in press. You couldn't stop the presses. As I say, this was an interesting field at the time. There was a guy at the Department of Agriculture, his name was [A.] Keith Brewer, who was really a pretty good physicist. He was working on isotope abundances in the alkali metals and used thermal ionization in his work. You heated up a salt of the element you were interested in and got ions out. Brewer also was working on potassium, which I didn't know at the time. He had a nice instrument, and with thermal ionization you didn't have the impurity problems; at least of the kind we might have. While my letter to the editor was in press, Brewer sent in a manuscript to the *Physical Review* in which he stated that potassium-40 could not exist with an abundance [of] even a tenth as much as I said was there. So, here was my advisor and the editor of the *Physical Review*. [laughter] I don't think he called me, but I think he wrote to me. I may even have the correspondence.

GRAYSON: That would be worth looking for.

NIER: It would be worth looking for, because this was most interesting. But anyhow, I know I was quizzed, "Did I do this, did I do that, am I sure on that?" And I convinced Tate that what I had done was all right. So, he wrote back to Brewer and told him of my work and that this was being published and wouldn't he, Brewer, like to do his work over again to check. Whereupon Brewer did it over again and he got 1 in 8,300 instead of 1 in 8,600.⁶ Well, I only claimed an

⁶ A. Keith Brewer, "The Abundance Ratio of the Isotopes of Potassium in Mineral and Plant Sources," *Journal of the American Chemical Society* 58 (1936): 365-70.

accuracy of 1 percent. So, we agreed. What had Brewer done wrong? He had nice flat-topped peaks. His resolution wasn't as good as mine. He had nice flat-topped peaks, so if you're looking for a little bitty thing in a valley between two big things, between potassium-39, potassium-41 if you have a nice flat-topped peak, all it does is raise the level in the valley. And he didn't see as sharp a bump in there as I saw. So, when receiving Tate's letter, he did something; probably narrowed his slits, and of course, he found K-40. Then, he published a value confirming the value I had turned out. His letter to the editor is in the literature, a few months after mine.

GRAYSON: So, basically, Tate was kind of in a tight spot. Because, he had work from his student, who was very young, that said X...

NIER: Who was very young. [laughter]

GRAYSON: Yes, who was very young, that said X, and here was a fellow...

NIER: Who was experienced, he was an older man. And he had a very good reputation. He had done good work...

GRAYSON: And who had published before in this area...

NIER: He was an authority...

GRAYSON: Was recognized, and so, he must have asked some pretty incisive questions before he sent this back to Brewer and said, "Do it over again."

NIER: Well, this didn't hurt when it was all over with, as you can imagine.

GRAYSON: Yes, definitely. [laughter]

NIER: So that's what really was a decisive factor, I'm sure, in my getting a National Research Council Fellowship. After all, here I was, twenty-four or twenty-three years old and had found a new isotope and it was an important one because of the radioactivity business and so on. I'd demonstrated that an authority in the field had done it wrong, so what more do you want? [laughter] So, that was very helpful.

GRAYSON: I assume Tate had the connections inside the system that would have set your...

NIER: Yes, he was a wheel. He was the editor, and he was on the council of the American Physical Society, and everything else, and on the board of the National Research Council. So, he could write letters and say this is a guy you ought to support, and soon, and so on.

GRAYSON: So, you were going from the low man on the pole, so to speak, to the top of the pole. It was almost fortuitous wasn't it?

NIER: That's right. Well look, I've been retelling you these things that happened. You had to be there to take advantage of the opportunity. It's always true, but, you know, the opportunities were there, that's just the point. That was a very fortunate break because there were only two fellowships in physics, two in chemistry, two in biology. That wasn't very many—in the whole United States. I got offers, then. I had the fellowship, and of course the schools said they'd like to have these people come as post-doc's, it didn't cost them anything except for their lab expenses.

GRAYSON: At this time you were a post-doc?

NIER: Well, I had just gotten my degree in 1936 here. So in the spring of March or thereabouts I got word that I'd been awarded a National Research Council Fellowship, 1,600 dollars the first year and if you were appointed a second year, it would be 1,800 dollars. That was a lot of bucks. So, where to go? That was the next step.

GRAYSON: Okay, then you had, to choose...

NIER: Yes. Bleakney got in touch with Tate or with me and said "Why don't you come here, [to Princeton] and carry on the tradition?" See, two other guys had already gone there and why not me? Meantime, [Kenneth T.] Bainbridge, at Harvard, who was in mass spectrography in the business of measuring precise masses, had built this beautiful double focusing machine, together with a guy by the name of Ed Jordan who had gone to Harvard as a post-doc to work with him. Bainbridge was one of these wonderful people that you wish you could meet all the time because he was very generous and very interested in helping people. He assured me money from their private funds. After all, Harvard had endowments. There was a fund called the [William F.] Milton Fund—named after somebody, some benefactor. I think they offered me,

for my lab 5,000 dollars. Which was a lot of bucks in those days. That'd be like 100,000 dollars or something like that today.

GRAYSON: And it was prestigious institution.

NIER: Right, and free shop-work and the like. They had machinists. So, he offered me that opportunity. And told me about all the wonderful things: I could come there, I could supplement their mass work, I could be the abundance guy. He had a good reputation, he was real first-class scientist. Bleakney on the other hand was also very good. Well, Bleakney said if I came Princeton, I could build a super-duper spectrometer using their cyclotron part time. They were just building a cyclotron. And, I could build a bigger 180 degree spectrometer than anybody. Everybody was thinking in terms of 180 degree deflection in those days. I could have had a radius of curvature of 2 feet or more. Cyclotrons magnets had a diameter of 6 feet or more. The idea was that I could have the magnet part-time.

Well, I thought about the matter a little bit and decided, for a variety of reasons, that I should go to Harvard rather than Princeton. One of them being that to be realistic, to think that I could have the magnet for much time...to have them take all of the guts out of the cyclotron and replace it by the spectrometer at a time when nuclear physics was really hot just didn't seem very realistic. Not that they wouldn't have treated me well, in other ways. But the whole scenario didn't seem very realistic. Also, I thought, maybe instead of everybody from Minnesota going to Princeton, somebody ought to go to Harvard for a change. Tate didn't discourage me. He agreed that was a good idea. This was a very happy thing for me. I think it would have been a more friendly atmosphere at Princeton. I would have felt much more at home there, because Harvard, at the time, had a reputation of being...I wouldn't say "unfriendly...but it was, kind of, New England cold, and they were pretty formal.

[END OF AUDIO, FILE 1.3]

GRAYSON: Okay, we're recording Side B of Tape Two, and Harvard was a cool place.
[laughter]

NIER: Cool weather-wise and cool otherwise. [laughter] Well, I found Bainbridge extremely friendly. The people I met were extremely pleasant. The people were not all New Englanders. True, the atmosphere was a bit formal, but friendly. And I found everybody to be extremely cordial.

GRAYSON: Even then, the Midwest and the Northeast Coast have this difference of...

NIER: Oh yes, there's a different culture.

GRAYSON: Yes.

NIER: But, it turned out a lot of the Harvard people were Midwesterners and Westerners. True, you had a lot of hangers-on from the East that stayed there. One of my very good friends I shared a lab with was working on high-pressure stuff there. It was a big lab, and I had one side of it and he had the other side of it. He was from California, for instance. So, there were people from all over. It was really quite cosmopolitan. And Bainbridge was very helpful.

At the same time, I also got an offer to work at General Electric. They were interested in hiring people who had just gotten degrees for summer jobs and also graduate students. So, I was offered a job at GE, working for the summer before my fellowship started in Fall. I went to Schenectady [New York] for the summer months, and my boss was [Chauncey] Guy Suits, who later became the director of research for General Electric. But he wasn't at that time. He was a promising young man in the lab. I worked on arcs or something like that. He was interested in vacuum arcs, and the techniques I had, of course, fit right in. It wasn't the most productive summer. I don't know if I did him any good, but I certainly learned a lot.

GRAYSON: And, you probably made some connections there that helped you later on, perhaps.

NIER: Some. I got to know him, I got to know [Saul] Dushman.

GRAYSON: Okay, that's a nice guy to know. [laughter]

NIER: A nice guy to know. We became good friends later on. I met everybody else: Albert Hall, who was the associate director, I met [Irving] Langmuir, even [Willis R.] Whitney, who was the pioneer in the lab. So, I met all these people...I'm sure they didn't remember me. But, Dushman did. I met other young people like myself, I got to know people. I knew Fred [Frederick] Seitz, he was one of those working there in the summer also, and stayed on. There were quite a few. John [P.] Blewett, one of Bleakney's students in mass spectrometry, also had a job there, working on something else. So, I got to know other people.

GRAYSON: GE had a number of summer appointments.

NIER: Yes, working in basic science.

GRAYSON: Okay, do you know if they had an ulterior motive.

NIER: This was a recruiting scheme, if that's what you mean. You know, get a line on young people...

GRAYSON: Okay.

NIER: The standard thing that goes on at many places.

GRAYSON: But they had to commit to the concept that these people had valuable talents and assets for the future of General Electric and its products.

NIER: That's right. They wanted to look at the young people, and see, they didn't have to make any commitment.

GRAYSON: Sure.

NIER: It was a neat way to do it. Very good.

GRAYSON: Well, the same thing is done today.

NIER: Oh, it's done everywhere. The government has a lot of post-doc positions at the Bureau of Standards, and at the Naval Research Lab, and places like that, with the same kind of idea where it's a one or two-year thing, except that in my case, it was three months.

NIER: So, that was a good experience.

GRAYSON: And then you went on to Harvard?

NIER: I went to Harvard in the Fall. But, more important than that, before I settled at General Electric, which was around 1 July that summer, the summer of 1936, I went to Harvard to meet Bainbridge. I had never met him. I had an old car then that just barely got along. He and another family from the electrical engineering department at Harvard shared a big house in New Hampshire. This was the standard thing, you rented houses on an abandoned farm or something like that. So, they had me as guest over there for a weekend. I met him very briefly at Harvard, and then he went up to New Hampshire, and invited me to come up there later on in the summer. We sat around for about a half a day, and talked about what I ought to be doing. This was the beginning of July, and I wasn't going to be coming until September. We came up with the design of an electromagnet—this 2-ton electromagnet—which became the basic magnet for the instrument. The decision was, we should make something bigger, get away from the solenoid, and then you'd get a stronger magnetic field by having an electromagnet with a smallish air gap. So, we came up with a magnet that had a two-inch air gap, and I think it was 12-inch diameter poles. Something like that. (Figure 7)

GRAYSON: This was still 180 degree deflection.

NIER: Still 180 degrees, yes. And so we built it, and it had a five-inch radius. That was the original 180 degree tube that fit between the poles of the magnet. This whole thing was sort of designed there, between us. I don't know how much my contribution was and how much Bainbridge's was. He certainly contributed a lot on the magnet design. He knew about how you built magnets.

GRAYSON: The basic idea of a 180 degree deflection instrument, was that in the literature?

NIER: That's what everybody had used. [laughter]

GRAYSON: The idea of putting the ion entrance right at the edge of the magnetic field was already used?

NIER: No, you had the whole thing immersed. And what I had to do was to build an ion source that would squeeze into a tube that would fit into a two-inch air gap. And this is where the modern ion sources came from really. From that everybody else has pretty much copied. You see, the ion sources that I was acquainted with, which Bleakney had used, which P.T. Smith had used, which Wally Lozier had used. You had a long electron accelerating system because it was in a solenoid. The ion sources were five or six inches long. You accelerated the electrons in stages, and you had diaphragms to screen the sections. You did a very careful job of defining the electron beam without having stray fields. That was important if you were going to do electron-impact work quantitatively. But if you were just going to play with isotopes, it

didn't matter. It was a matter of compressing the source enough so that it would fit into the tube. So, we used a 45 or a 48 millimeter tube—48 millimeter, I think it was—which would fit in a two inch air gap, along with an oven around the tube. I still have one of the tubes.

GRAYSON: Well, I'll need to get a look at it.

NIER: You can look at it. I have some old tubes up here.

GRAYSON: I need to get a picture of all these wondrous old things.

NIER: It was a matter of designing the source to fit that gap, which I did.

GRAYSON: What you're saying is that there was a point of departure here, and, you were trying to take all the stuff your predecessors had done and squeeze it into a smaller space?

NIER: Squeeze it into a smaller space.

GRAYSON: Or were you trying to actually redesign the whole concept of the ionization region?

NIER: No, I think the idea which they had was sound. It was a matter of squeezing it, so it would fit in there.

GRAYSON: Okay.

NIER: Without losing too much.

GRAYSON: Okay. So you were still trying to get some of the capability of their design.

NIER: Oh yes, very definitely. And so, you didn't do as good a job of separating the fields which accelerated the electrons and the ions. It didn't work that well. But it was good enough for the isotope work. And that was what I was going to work on, was isotopes, see?

GRAYSON: Now, when was that design was worked out? Over the summer, or when you started working at Harvard? In the summer you had decided on the...

NIER: The magnet, that's right.

GRAYSON: Having established that, then you had to deal with fitting the ion source in the air gap.

NIER: That determined how much space we had.

GRAYSON: Yeah.

NIER: I suppose I thought about it during the summer. And when I came back in September, I started working on it. Because I got there right after Labor Day, or Labor Day weekend, in September, and I had spectra before Christmas.

GRAYSON: Okay, that was 193...?

NIER: 1936.

GRAYSON: 1936.

NIER: So, in three months, I had an instrument running. Now, Bainbridge, in the meantime, once we'd decided on the magnet, had gone back to Cambridge [Massachusetts]—I think he commuted—and had got the magnet started.

GRAYSON: Sure. Because that would be a pretty substantial piece of work.

NIER: Yes and they had a good shop. First-class guy at the head of it. And industry was interested in jobs. Of course, Bainbridge's idea was: you had only the best, you had to have special materials. So, he got hold of Armco iron with low carbon...good magnetic properties; and he contacted whoever it was in Ohio on how these things were made. They cast a big yoke, a big C-shaped yoke, and prepared coils on it. Remember, this was using about five kilowatts.

They had water-cooling tubes; they drew square copper tubing for cooling tubes. Square copper tubing!

GRAYSON: Sounds like a first-class operation. [laughter]

NIER: First class. But, industry was interested in jobs. Boy, I'm telling you there was a Depression, it was in the middle of the Depression.

GRAYSON: So, if you had money, and...

NIER: If you had money, boy, you were king...

GRAYSON: And if you wanted square copper tubing... [laughter]

NIER: You got square copper tubing. [laughter] They didn't mind drawing square copper tubing on special order. So, by the time I got back, in September, the magnet yoke may have even been delivered. It was being machined—they probably machined it at the factory. Certainly, that magnet was running by November.

GRAYSON: Now, the pole faces for that...were they 180 degree?

NIER: Full circular. Everything was symmetrical. Full circular.

GRAYSON: But the ion path was 180 degrees.

NIER: 180 degrees.

GRAYSON: So you had that tube in there.

NIER: And I have a good picture of that instrument, by the way.

GRAYSON: Yes, yes. [laughter] We've got to add that to the list of things to collect. Did you put a glass tube in this machine?

NIER: Glass. It was sort of horseshoe-shaped. The ion source was in a cylindrical part, and attached to that was the 180 degree analyzer, and you brought out an arm at right angles to that; I have a reprint on that.

GRAYSON: The glass-blowers must have loved you guys.

NIER: It was wonderful! And they would do these beautiful ring seals so you could separate the source part from the analyzer part. Now, I did all the glass-blowing on the little stuff. Like, you see, if you wanted to change the source, you cracked the tube off. As I think back, I think it was 45-millimeter tubing. See, it took a little skill to heat 45 millimeter tubing and not have strains in it.

GRAYSON: Yes.

NIER: And you weren't always successful, so you sometimes had problems. But we had a very good glass-blower at Harvard, as we had here.

GRAYSON: Was it pumped by a mercury diffusion pump?

NIER: Yes, all mercury. I had two systems. One for the inlet system and one for the mass spectrometer.

GRAYSON: Did they use liquid nitrogen there or were you still using that carbon dioxide bath?

NIER: Both. But, I think we used liquid nitrogen more frequently.

GRAYSON: It was easier to get there...

NIER: It was easier to get there than here.

GRAYSON: And so, by December, you were actually getting spectra.

NIER: Yes, I got my first spectrum of mercury.

GRAYSON: Did the detection scheme use the faraday cup-electrometer tube?

NIER: Very much like I'd used here. I built the thing up, and they knew about this technique. But I built my own amplifier; I had some help from the shop. But I did all the wiring myself.

GRAYSON: Okay. Could you also do the integrating type of measurement?

NIER: That's right, that's right.

GRAYSON: Charge drift, was that you called it?

NIER: Yes.

GRAYSON: Was there anything in the literature about how that was done, this charge-drift measurement?

NIER: I suppose, if you look back to 1915, or sometime like that...but it was understood. You simply charged up the capacity of the system. And the voltage was equal to the charge divided by the capacity. So, you just built it up.

GRAYSON: When did the first publishable data come off the machine?

NIER: Pretty quick. In the Spring.⁷

GRAYSON: And that was done on?

⁷ Alfred O. Nier, "A Mass Spectrographic Study of the Isotopes of Hg, Xe, Kr, Be, I, As, and Cs," *Physical Review* 52 (1937): 933.

NIER: Well, I played with krypton and xenon, and mercury, about the first thing...so, my first paper involved a number of elements.

GRAYSON: Mercury...you mentioned it before. People looked at it because it was in the vacuum system.

NIER: Yes.

GRAYSON: I guess there was information in the literature on the isotopes of mercury then?

NIER: Not as good as I was able to get.

GRAYSON: That's around 200, and the performance of this machine at Harvard was absolutely better than anything you had before.

NIER: Oh yes, 200 was a lead-pipe cinch. [laughter] No, I worked up in 300, when I was working with uranium and such.

GRAYSON: Okay.

NIER: So, there was no problem. I got another break at Harvard. It was a marvelous place. In addition to the people, they had marvelous facilities. They had a 100,000 volt storage battery, which had been used by a guy by the name of Duane, for precision X-ray measurements. The battery was used to get the voltage accurate for X-ray studies. It consisted of 50,000 little glass cells, each having one or two watt-hours capacity. When Duane completed his research the cells were emptied and dried out. If people needed a low capacity storage battery for research, there was a technician who reactivated the cells by filling them with sulphuric acid. So, I had 2400 volts of battery. It was a little dangerous. [laughter] I got a nice burn on it one time. Got across it...

GRAYSON: Yeah. There was no current limit.

NIER: No current limit...this is how you get electrocuted. So, aside from that hazard...everything went well. [laughter] Most of the time I accelerated ions by at least 1200 volts, and I guess at times, I used up to 2400, but I don't remember...

GRAYSON: So, let's see, for the higher masses, you'd go to the lower accelerating potentials anyway.

NIER: Well, you had to use as high an accelerating potential as possible, because that's what helped your resolution.

GRAYSON: Sure. This was a fixed-field...the magnetic field was fixed?

NIER: Oh, no...electromagnet.

GRAYSON: Okay.

NIER: Then I had a problem with my magnetron scheme; it wouldn't work. I thought it would be a lead-pipe cinch but the stray field outside of the magnet would not track the field in the gap, because of the hysteresis in the iron. So this was when I had to build an amplifier to control the field of the generator itself. It was a DC generator, and they employ a fixed magnetic field, which the armature turns in. One adjusts the output voltage by changing the DC current through the field coils of the generator. So I had to build an amplifier that would control that, using the output voltage of the generator with a feedback system, and that's when I made use of these wonderful people there in the Electrical Engineering Department, Hickman and Hunt, who knew all about feedback amplifiers. I got a lot of good tips from them about how to build it, but I had to build it myself.

GRAYSON: Well, you had the technological know-how.

NIER: That's right; it wasn't easy by the way, because of the hunting of in high gain feedback systems. There's a lot of time-lag in a loop involving a generator. So, feedback is all good and well, but you get into oscillations if you don't do it right. A lot of effort went into perfecting the circuitry. But, it was possible to stabilize the generator well enough, that we got the stability we needed.

GRAYSON: So, what you really were using here was a stabilized power supply.

NIER: DC. That's right.

GRAYSON: As opposed to the method with the solenoid.

NIER: That's right. So, I had to stabilize the output of the generator. The thing that worked to help you, was that the magnet itself had a high inductance, so it didn't respond rapidly. All you needed to do was to make sure that the average of the voltage you pressed on it stayed pretty good. There was still ripple, because you never get rid of that in a DC generator. But we got it down, so it was pretty damn good.

GRAYSON: The whole concept of that magnet and its size and the power required to drive it, obviously, you wanted a magnet with a fairly high magnetic field.

NIER: Yes.

GRAYSON: You just looked at the technology at the time, and decided the parameters you needed to make this particular thing.

NIER: Well, I think once we made the decision to have a five-inch radius of curvature for the instrument; then, it told you what the relationship between the magnetic field and the accelerating voltage had to be. We wanted to use, a couple thousand volts accelerating potential so you knew what the magnetic field had to be. We pushed this up, I don't know, 8,000-10,000 gauss, something like that. It was very good magnet material.

GRAYSON: It was very high for that time.

NIER: Oh yes, very high for that time, that is correct. But that's what pushed the whole thing up to something that was different.

GRAYSON: So, basically, there was a decision to get a certain performance, and then, given that; you had a certain accelerating potential required, and so on, and that's what drove the design.

NIER: That's what did it.

GRAYSON: The earliest work that was published was on xenon and mercury. In the case of mercury, you were measuring the isotopic abundances?

NIER: Well, there were values in the literature—I've forgotten exactly—but I think the only values in the literature were produced by Aston. This was accomplished with a photographic plate as a detector, measuring the density. How this guy ever got anything out of that, I don't know. It was amazing how well he did, you know. But, on rare isotopes, he usually was off. Because, remember, his apparatus had lots of grease and wax joints and water was present. Ions were produced in an electrical discharge. So, Aston missed the boat on rare isotopes generally. He either found things that weren't there, or didn't find them or had other problems. There was some question about his mercury results. The mercury-196 isotope, for instance, is a very rare one, and he was off by a factor of five or thereabouts in the amount—I don't remember exactly, but he was off. I could show there were no other isotopes such as mercury-197, mercury-195, mercury-203, and so on. I was able to establish that beyond any doubt.

GRAYSON: So as late as 1937, even though mercury had been in mass spectrometers probably from the first year, that was probably the first time it was really accurately known what the isotopic abundances were.

NIER: And the amount, that is correct. And those values we got then are still pretty good, you know. The people since then have done it with separated isotopes and precision calibrations, and so on, but if you look at the original values, they're not bad.

GRAYSON: Now, you spent several years at Harvard?

NIER: I was there two years.

GRAYSON: Did this collaboration with Bainbridge work to his satisfaction?

NIER: Oh, he was very happy.

GRAYSON: So, he was pursuing the mass measurement problem?

NIER: Mass measurement, yeah. That's right.

GRAYSON: And you were pursuing the abundance measurements.

NIER: And he'd gotten into other things, also. They were going to build a cyclotron at Harvard, and he'd gotten involved in that. He was very happy. As long as I was getting along, he felt his role was to see to it I could do what I wanted to do. It was an assistant's utopia.

GRAYSON: It was an ideal situation. [laughter]

NIER: [laughter] Really, an ideal situation. A most wonderful person...

GRAYSON: Much better than having your advisor tell you that, "Gee, you did it," and publish it...

NIER: No, and he, he was just wonderful.

GRAYSON: So, what do you feel were the most important things that came out of your stay at Harvard?

NIER: Well, to be sure, determining the isotope abundances was interesting and important, because it was the first time people had done reasonably precise measurements of abundance. Now, I might say, at the very same time, by coincidence, Bleakney, and several students at Princeton were doing similar things, but hadn't moved in the direction that I had. They did some other nice things on isotopes at the same time I was at Harvard. They were working on different elements. There was never any overlap.

GRAYSON: Okay.

NIER: So, I continued with that work. But then the significant thing that happened in that period was the introduction to geochronology. It happens that this was a heck of a good place to be for this. First of all, T. W. [Theodore W.] Richards, who was the atomic-weight chemist of the early part of the century and got the Nobel Prize [Chemistry, 1914] for his work on atomic weights, had a marvelous collection of stuff that he left behind when he retired, or died, or whatever happened to him. And his successor was a guy by the name of Gregory Baxter,

who had all of this material. You have to remember that after the discovery of radioactivity in 1897 or 1898, or whenever this was, people became interested in geochronology when it was realized that uranium and thorium decayed to form lead. So, measuring the atomic weight of lead was important. That became a hot field in 1910 or so. That's when Richards did his work. Chemists showed, that if you had common lead, you got an atomic weight of 207.21. And then, if you had uranium-lead you got a weight near 206, because 206 and 207 came from the decay of uranium. From thorium, you get close to 208. So the chemists were measuring the atomic weight of lead to tell how much common lead impurity was present in these different specimens.

Then there was a guy by the name of Alfred Lane, a geologist, who was retired and lived near Harvard. He had been at Tufts College—it's now called Tufts University—which was in Somerville [Massachusetts]—a suburb next to Cambridge. He was one of the very few persons in this country interested in quantitative geological age measurements. Not just stratigraphy, but numbers.

GRAYSON: This would be in 19...?

NIER: 1936. I was introduced to him, when word got around that I had a mass spectrometer that could work on lead and uranium, and other heavy elements. I'd suddenly made a lot of new friends, you see. Lane was a wonderful guy. He was a funny guy, but wonderful.

GRAYSON: In what way?

NIER: Well, sort of, peculiar. He bustled around, and really had interesting mannerisms. First-class scientist, and a very well-known geologist of his day. He used to come around with a little bag, and he was getting a little absent-minded, and his mind would wander, and he had a little notebook he wrote in. He had very interesting mannerisms. Wonderful guy. And, so, he was very interested in my work, and tried to promote it, getting samples and things for me to work on. So I spent most of my time, or a very large part of it, working on things related to geochronology.

GRAYSON: And Bainbridge felt this was fine?

NIER: Oh, he encouraged this, he thought this was terrific. That was one of the attractions, why he said I should come there; because I had opportunities to enter the field of cosmology.

GRAYSON: Okay. In some cases, a guy like that would try to commandeer your abilities for his own pursuits.

NIER: Oh, no, no. He wanted good science to be done, and he saw that this was good science. Through him, I met Baxter who provided me with very pure samples of lead, and made it into lead iodide for me, which was volatile. You could put it in our instrument, and heat the part which contained the sample. Baxter made wonderfully pure lead iodide for me.

GRAYSON: So, these samples at Harvard had been collected by a fellow who was more interested in the decay scheme between the radiogenic elements.

NIER: Well, in the geochronology thing, in using atomic weights for telling how much impurity was present in common lead.

GRAYSON: Okay, it was the whole business of using isotopes from radiogenic sources for dating. Was that concept pretty well fleshed out at the time?

NIER: Oh, yes. It had been done for years. You measured the amount of uranium and the amount of lead. From the atomic weight of the lead, you tried to decide how pure it was. From alpha-particle counting measurements of uranium and thorium, one knew the rate at which these elements decayed to form isotopes of lead. From the amounts of lead and uranium isotopes in a uranium mineral and a knowledge of the decay rate of the uranium, one can compute the age of the mineral.

GRAYSON: So, your ability, to look at the isotopes of these elements was really opening up the door.

NIER: It was a new dimension.

GRAYSON: It was a tremendous experience.

NIER: Yes. I could do one sample in a day. Actually, the measurements took me an hour, but I had to clean the instrument up because you filled it with lead iodide vapor that had to be baked out of the system between samples. So, I could only do a sample every other day, but the actual analysis took me about an hour. I could do in an hour for which the chemists, in making atomic weight measurements, needed weeks. Everybody was very much interested and supportive.

And Baxter—and this was quite a break for me, again this business of being in the right place at the right time—Baxter became intrigued with mass spectrometry. He was an analytical chemist of the very highest-order in wet chemical methods. But he became interested in the mass spectrometry business that could do the determinations in so short a time. So, he fed me common lead samples that they had accumulated way back even before his time. From T. W. Richards' time. I ran a dozen or more of common lead samples and I found that the isotope abundances varied, although the atomic weights were the same.⁸ Well, his first reaction was, “The mass spectrometer has to be wrong, because after all, we chemists have done this so carefully for so long, with so many people. What could be wrong?” The reasoning didn't take into account that you could have a coincidence: that the variations with isotopic abundances were such that the average weight remained the same. Just a crazy coincidence. I mentioned this in my story in the reprint I sent you.

GRAYSON: Yes.

NIER: On my reminiscences of...I don't know if you have that or not, if you know about it, Tom.

GRAYSON: Yes, I think I sent Tom a copy.

NIER: Okay. At the same time, there was a guy by the name of Arthur Holmes, a very famous geologist, a very good geologist, at the University of Edinburgh. He was interested in quantitative geological measurements. Are we coming to the end [of the tape] here?

GRAYSON: Yes, I think we better stop on this tape, we're about to run out.

[END OF AUDIO, FILE 1.4]

[Nier and Grayson are looking at cartoons taped to the doors of a cabinet in Dr. Nier's office.]

NIER: This one was given to me by a friend who sat in front of me at the football game after Minnesota had lost. [laughter] He thought it was appropriate for me because in the cartoon one character says to another “Anyway, we have a better physics department than they do!”

⁸ Alfred O. Nier, “Variations in the Relative Abundance of the Isotopes of Common Lead from Various Sources,” *Journal of the American Chemical Society* 60 (1938): 1571.

GRAYSON: Speaking of football and mass spectrometry, I was up at the University of Nebraska once, and the person [Michael L. Gross] who runs the mass spec center there takes out an ad in the football program.

NIER: They take out an ad?

GRAYSON: Yes, they have an ad in the football program. How's that for getting PR?

NIER: That's interesting, isn't it...yeah.

GRAYSON: You have to do everything you can.

NIER: Right. There's some wonderful ones. There's some German ones [cartoons] that are wonderful. See, she says to this dog with the sad look. "What is more important—that you should not catch a cold, or that the people shouldn't laugh at you." [laughter] Then, I think this one's wonderful. "This is reality, children. We simply can't switch on another program." The man said as he changed a flat tire on the car. [laughter]

GRAYSON: [laughter] Yes, life is not a TV. When would be a good time for a photo opportunity? Would you want to take a couple of pictures?

NIER: Anytime, whatever you say. You can take some in the lab if you want.

GRAYSON: Well, I wouldn't mind getting some in here. I was just kind of looking at your organization of books and papers.

NIER: Well, it's not very good.

GRAYSON: "Not very good"?! I think it's excellent. It's a lot better than mine! [laughter] Mine is simply a file, and...

NIER: Yes, the things I haven't sorted are over there.

GRAYSON: Yes. Oh, I see some old ASMS [American Society for Mass Spectrometry] bound volumes over there. That's encouraging.

I like that arrangement with some general order to the shelves.

NIER: Yes, that's right.

KRICK: What's great is that they can be archived later on.

GRAYSON: Now, are those your own personal copies of the journals? So, you get to take off all of this on your income tax, right? [laughter] You subscribe to all of these journals. I guess you can't deduct much anymore unless you have a substantial fraction in journal subscriptions.

NIER: I don't even try. I'm trying to stay off the list of people that should be investigated regularly. [phone ringing]

Excuse me.

[recording paused]

GRAYSON: Let's continue with, Tape 3, Side A. Does anybody recall where we stopped? [laughter]

NIER: Oh, yes. We were talking about Holmes.

KRICK: Holmes.

GRAYSON: Okay, Tape 3, Side A and we were talking about Holmes. So we'll pick up with Holmes. In the meantime, I'll be taking some pictures. Do you suppose it'd be okay to close the door. The typewriter is making a lot of noise.

NIER: Yes, I think we should...of course.

GRAYSON: Holmes. And I may just move around and take an odd picture or two while we're chatting, in kind of an informal situation. So, Holmes...

NIER: Yeah. Well, um, let me think for a minute. Holmes had just come out with a paper, and this is mentioned in the article that I wrote for the *Annual Reviews*.⁹ He had just come out with a paper in which he said that the lead ores could not have a magmatic origin.¹⁰ I believe the argument went that they always had the same atomic weight, and therefore, the same isotopic composition. In the magma, the lead would be in contact with thorium and uranium, and you ought to be generating uranium-lead and thorium-lead at the same time. Therefore, the atomic weight of the lead ought to be varying. What he didn't take into account, which nobody took into account, was that you had the crazy coincidence I mentioned earlier.

What happens is that the half-life of thorium is about four times that of U-238 the principal isotope of uranium, and the abundance of thorium, in the earth, is about four times that of uranium. So, you generate about equal amounts of Pb-206 and Pb-208 when the two decay together. You see, if you think of a couple of hour-glasses, the sand gets piled up at the same rate. Okay. The atomic weight of common lead, 207.21, is about halfway between. There's a little bit of 207 also from the decay of U-238. So, you generate not only U-206, but a little bit of U-207, and the U-206 and U-207 balance off the U-208. You can have various amounts of radiogenic lead but you always get about equal amounts of uranium and thorium lead, so the average atomic weight doesn't change. Now, if you made real precise measurements of the atomic weight, you'd find it changed, but within the precision of the chemists atomic weight determinations, it was not measurable.

GRAYSON: So, this is another case where your data is flying in the face of some expertise that's out there. This is the second time now, right?

NIER: That's right.

GRAYSON: So, what happened then?

NIER: Well, when this first happened, Baxter didn't believe me. He gave me unknowns—as I told in this article—as if I were a freshman in analytical chemistry. And after doing three or four of them and coming out with consistent answers he said, “Well, it has to be so.” Then he made the interesting comment, he said, he was glad that he was nearing retirement. [laughter] He

⁹ Alfred O. Nier, “Some Reminiscences of Isotopes, Geochronology, and Mass Spectrometry,” *Annual Reviews of Earth and Planetary Sciences* 9 (1981): 1-17.

¹⁰ Arthur Holmes, “The Origin of Primary Lead Ores,” *Economic Geology* 32 (1937): 763; Arthur Holmes, “The Origin of Primary Lead Ores: Paper II,” *Economic Geology* 33 (1938): 829; and Arthur Holmes, “The Measurement of Geological Times,” *Nature* 135 (1935): 680-3.

was a wonderful guy. He was quite formal, very stiff in a way, but very friendly, and very nice, and had a real sense of humor.

GRAYSON: The competition was getting too tough, huh? [laughter]

NIER: Getting kind of tough, so he was glad he was getting near retirement.

GRAYSON: So, what about Holmes?

NIER: Well, I wrote to Holmes and I described this in the article a little bit. There was a professor at Harvard by the name of Gratton. He was in economic geology, and they'd been teaching their students about the origin of lead ore all this time. And here Holmes, also an authority, comes out with a contradictory idea on the origin of lead ores, and Gratton was just fit to be tied. And there were others the same way. So, Gratton was just delighted that I'd come up with the data which refuted Holmes theory. I wrote to Holmes, and he was delighted, too...or other reasons. And I met him later, as I've mentioned in the article, and we talked about it a little bit, and he said, "Gee, how was he to know that there was this coincidence, that it would be this sort of thing," and so on. He was very friendly to me. We exchanged Christmas cards for years. I visited him in 1954, had a nice afternoon with him and his wife, also a geologist. Very friendly, and we even did analyses of lead samples for him afterwards, so we kept in contact with him for years. He died in the 1960s, so that was the end of that. But it was one of these cases, I wish I'd got to know him before. You know there's so many people like this, that you wish you'd gotten to know, but it was too late.

GRAYSON: But, at least, he was enough of a scientist to realize...

NIER: Oh, yes. Well, he told me...

GRAYSON: ...that even though his work was correct, that there could possibly be another explanation.

NIER: Yes, well, he jumped on the bandwagon then and was very much a fan of the isotope work. As a matter of fact, he was the first one who pointed out, what I should have done a long time ago...Although I'm trying to figure out why I didn't...that you ought to make use of these variations, somehow, to tell you something about the age of the common lead minerals. I did do things like this, starting with the older, the most primordial minerals and so on, but I never took the real step of tying everything together. I don't know why, but I have a feeling that somebody

restrained me. Because it had occurred to me, but they said “Well, you better not stick your neck out that far,” [or] something like that; so I never did. But, Holmes jumped on the bandwagon.

At the time the earth was believed to be two billion years old. And he said, on the basis of these measurements, it ought to be at least three billion years old. Others came up with 3.3 billion years. And later on, when Clare [C.] Patterson did this precision work on meteorites, that’s where the 4.6 billion figure we now use comes from. But it’s making use of the variations in common lead, and working backwards. I was starting to say that, if you take a primordial sample and add to it various amounts of other lead of known age you can work back and figure out how long the addition must’ve been going on. So, that was really the start of the lead isotope field. I was very happy to be involved in it.

The other interesting thing which occurred at that time, of course, was the measurement of the uranium isotopes.¹¹ I had lead samples, both common lead and uranium-lead and thorium lead, which I’d gotten from Baxter and also through Holmes. Not through Holmes...I mean, through Lane. There are actually two uranium series. There’s the U-238, that decays to U-206, and the U-235 that decays to U-207, each at a different rate. So, you have like two hourglasses, running at the same time, see? So, if you measured the isotopes of lead accurately, and you knew the isotopic composition of uranium accurately, you could then determine and compare the ages by the two methods. But one didn’t know the relative abundances of the isotopes of uranium accurately. Aston had observed the isotopes on his photographic plates, and showed that for U-235 there was a little smudge on the plate. But that’s as far as he got. So, people had guessed at the relative abundances of the uranium isotopes, but I think they were off by a factor of three or some amount like that. It was realized then that we could now accurately measure the uranium isotopes. Bainbridge was certainly in on this thing. We’d all talked about it. And Lane, of course, was just delighted at the prospect that I might measure uranium isotopes. So the question is, how do you do it? So I’d looked back and seen what Aston had done, and he had used uranium hexafluoride; so I said “Well, gee, that’s the thing to use.” Well, UF₆ was a rare commodity in 1937.

GRAYSON: You didn’t just go buy that, did you? [laughter]

NIER: You didn’t just buy it by the kiloton as one can now. So, Lane said “We’ll find somebody to make some.” He got a grant from the Geological Society of America, to have somebody make it, but he couldn’t find anybody who was willing to take on the project. He had 500 dollars, but he didn’t find anybody who was either willing or able. Probably not willing. Because there were people who could do it. But it wasn’t easy. So, the money reverted. And the question is “What else should I do?” And then I looked, and I went over to...what is it?

¹¹ Alfred O. Nier, “The Isotopic Constitution of Uranium and the Half-Lives of the Uranium Isotopes I,” *Physical Review* 55 (1939): 150.

“Mellor,” is that the chemistry book, the one that tells you how to make stuff?¹² Used to be kind of a bible, in two volumes. It showed you how to make all kinds of compounds. I went into the chemistry library at Harvard and looked up how to make other compounds of uranium. And what you did was, you took uranium oxide and mixed it with carbon, and pass chlorine over it, doing it at 1000 degrees, or some awful temperature. I was hoping to make some uranium tetrafluoride or bromide, which was volatile at my oven temperatures. See, I’d already had the “oven technique” of putting little ovens in the source, so I could introduce the uranium that way.

Well, I told Baxter about that, and the story I like to tell, which is a bit of an exaggeration, but probably not much, was that he was so offended by the technology that I was describing that, as a good chemist, he decided that he’d better take charge. [laughter] Baxter taught class at Radcliffe College and he often dropped by my lab on his way there. One morning I came in and there on my desk were several sealed quartz tubes containing uranium tetrabromide and uranium tetrachloride. It was very hygroscopic, so you had to rush to get a little bit into the oven and then into the vacuum system. I worked with both compounds and made the first measurements of the uranium isotopic abundances, and that’s where the 139-to-1 came from.

GRAYSON: That is, in 19...?

NIER: 1937.

GRAYSON: And so, the relative abundances of the various isotopes of uranium became known.

NIER: In addition to U-235 I was able to measure U-234, which is in equilibrium with the U-238. Its abundance is only one part in about 17,000.

GRAYSON: Okay. This was done on either the bromide or the chloride or both?

NIER: Both.

GRAYSON: And so, that would’ve required working up around, what? Mass 340, 350?

¹² Joseph William Mellor, *A Comprehensive Treatise on Inorganic and Theoretical Chemistry*, 16 vols. (New York: Longmans, Green and Co, 1922-1937).

NIER: Not in this case, It turned out we got an abundant number of metal ions so we worked with these.

GRAYSON: It probably was simpler to work with the metal, because of the many isotopic peaks from chlorine and bromine.

NIER: The isotopes are both lousy.

GRAYSON: You would have all kinds of other information.

NIER: The spectrum at the uranium position was very clean and I have a reprint of it here for you.

GRAYSON: Oh. Excellent.

NIER: Knowing the U-235/U-238 ratio [was] important, because that added a whole new dimension to age determinations. Also, I could come up with a number that was not known at the time: the ratio of the activities of the uranium and the actinium series. The uranium-series referred to the decay of U-238 going to U-206. The actinium series came from the U-235 going through actinium to U-207. And the number I came up with was 4.6 percent, which is still a good number, fifty years later. The uranium isotope ratio I gave was 139 to 1. I said it was good to 1 percent. The latest accepted value is 137.8. So the early value is still within 1 percent. I was lucky; the uranium measurements were pretty ragged, but I took a lot of readings and averaged them. I think I was a little lucky, in that I came out within 1 percent of the presently accepted value, which is really pretty good when you stop to think of the difficulties with the measurements.

GRAYSON: So, a lot of these little experimental details were particularly critical in doing these measurements, making sure the ion source was not discriminating, and making sure that the samples didn't have impurities that you were getting confused by, and so on. So, you always had a healthy skepticism about what you saw and were willing to look for all other possible explanations and eliminate them: variation of ionization potential, etc...

NIER: As many of those things as you could perform. Of course, with the little oven in there, you see, it was easy to heat or cool it, and you could run different pressures, and see if things tracked.

GRAYSON: So, a common experiment was, then, just to change the amount of sample entering the ion source.

NIER: All the parameters you could lay your hands on, yes.

GRAYSON: And make sure that you got several determinations and do the statistics.

NIER: Yes. It worked out quite well, but again, experience was terribly important in this, because you got a feeling of what flew and what didn't fly. You'd get this feeling.

GRAYSON: Then, would you say that that represents the major thrust of the work that occurred when you were on the NRC Fellowship at Harvard?

NIER: Yes, it turned out to be, in retrospect, to be the main thing that I did at that time. I was pretty busy doing that, by the way. [laughter]

GRAYSON: What other things were you doing at the time?

NIER: Well, the other thing that was interesting. One of my friends was a guy by the name of Earl [A.] Gulbranson, who was an instructor in chemistry up at Tufts. And he was interested in carbon isotope variations in nature, and we collaborated on an experiment. And I always felt I let him down, because I didn't follow up on the work as I should have.

GRAYSON: Because?

NIER: I didn't follow up on the variations we discovered in carbon isotopes. This probably took place in 1938. Things overlapped into 1938, from 1937. We found that natural carbon isotope abundance ratios vary by about 5 percent. In limestones, the C-13/C-12 ratio was 1 in 88 or so. For a piece of wood or other organic compounds it was more like 1 in 93. Gulbranson was a very good chemist. He made the samples, and I analyzed them as CO₂. We even looked at the CO₂ in air. I remember driving down a side road, outside of Cambridge one dark night, with a funnel sticking out to bubble the air through a tube dipped into a solution containing calcium hydroxide. We precipitated the CO₂ to form calcium carbonate. And later Gulbranson would change it to CO₂. This is how we obtained air samples. We studied, about a dozen different sources of carbon. That was the beginning of looking for variations in carbon isotopes.

That was done so long ago, most people today don't realize the first measurements were made over fifty years ago.

GRAYSON: Now, did that work get into the literature then?

NIER: Yes, it was published.¹³

GRAYSON: And this was with, I'm sorry, what was the guy's name?

NIER: Gulbranson, Earl Gulbranson. The paper is in my list of publications.

GRAYSON: Okay. So, that was kind of a seminal work, but you really didn't do any more along that line of study—the C-13 type work.

NIER: Yes. We followed up and did some more of it here, later on, with a graduate student, a chap by the name of Byron Murphey.¹⁴

GRAYSON: Do you have a feeling for the number of publications that came out of your NRC Fellowship time at Harvard? Were there three, four, six, eight?

NIER: Six or eight would be more like it. They're all kind of interesting, in that they all had something that was related to isotopes. It's in the list, you can pick them out easily.

GRAYSON: I guess that was a two-year appointment?

NIER: Yes.

GRAYSON: Bainbridge, I guess, had an opportunity to ask you to stay there if you wanted?

¹³ Alfred O. Nier and Earl A. Gulbransen, "Variations in the Relative Abundance of the Carbon Isotopes," *Journal of the American Chemical Society* 61 (1939): 697.

¹⁴ Byron F. Murphey and Alfred O. Nier, "Variations in the Relative Abundance of the Carbon Isotopes," *Physical Review* 59 (1941): 771; and Alfred O. Nier, Robert W. Thompson and Byron F. Murphey, "The Isotopic Constitution of Lead and the Measurement of Geological Time, III," *Physical Review* 60 (1941): 112.

NIER: Yes.

GRAYSON: Did you have that opportunity to stay at Harvard?

NIER: Yes, yes.

GRAYSON: But you elected to come back to Minnesota?

NIER: Yes.

GRAYSON: Okay, could you kind of explain that one? [laughter]

NIER: [laughter] Well, it was a difficult decision, and it was not based primarily on scientific reasons. My wife was an only child, whose mother had been a widow for many years. My parents were getting quite old. They were almost old enough to be my grandparents. I had a sister who died very young, I was the only one. Neither family had very much money. And the problem of staying there, when I could come back here and help, played a very important part in the decision.

I had an offer of three jobs in 1938. Condon wanted me to come to Westinghouse to the research lab. Harvard said I could stay on as an instructor, but there was a problem. This was when Conant became president, and the tenure situation was very uncertain. So, the young people had a real serious morale problem. There were two retirements here in the department. And, Tate had become Dean of the Liberal Arts College and was anxious that there be some continuity in the work he had started. Also, the people here knew me, and I had done well at Harvard, so, they were anxious for me to come back. I had to choose between these three jobs. I decided against Westinghouse, fond as I was of Condon, whom I liked very much. He was director of research at Westinghouse. Harvard made it attractive, and you never know: Should I have stayed or shouldn't I? It certainly would've been a happy situation. There was no question of that, but I didn't see how we'd ever cope with the problem of the family, with both of us having the problem of parents who needed us. So, I decided that I'd come back here. Financially, the offer was about the same as Harvard. I knew my way around, felt more comfortable, and I felt living here in the Midwest was preferable to living in New England. Anyway, I felt more at home. So, all these things added up, and I made the decision to come here. While my initial appointment was as an untenured assistant professor, the chance for promotion seemed a lot better, because Harvard had been dropping people right and left.

GRAYSON: Really? What was the reason for that?

NIER: Well, there'd been a lot of hangers-on who were on as instructors at Harvard, for whom there was no future. And they just hung around, hoping lightning would strike. In fact, the young people there whom I knew thought I was out of my mind turning down a chance to stay at Harvard. They may have been right, I don't know. But it's just one of those things, and how do you judge? Certainly, professionally, I'd have done very well there, because they had all the support for me. Better than here, as a matter of fact. However, Tate was awfully good about helping when I came back. Bainbridge was always good to me, no matter what. When I left he felt badly, but he let me take my spectrometer tubes with me. Tate, out of his research budget, got money for me to build a magnet like the Harvard one, and so on. So, I had, you know, six in one hand, and half-a-dozen in the other. Teaching loads here were awful, so I had to do a lot more work, whereas at Harvard it would've been much lighter.

GRAYSON: So, tell me a little more about the teaching loads.

NIER: Well, you had two full courses that you had to teach, plus some extra things, and so on, and that took a lot of time. Especially starting out from scratch. The first year was very rugged, very rugged.

GRAYSON: Would these be like introductory physics courses?

NIER: Yes, I taught an intermediate course and an introductory course, but a large class: a couple hundred kids. You give a speech every morning. Four times a week plus a quiz. Plus all the kids bellyaching about the quiz grades and working with the graders.

GRAYSON: And then they want partial credit. [laughter]

NIER: They want partial credit of course, exactly. [laughter]

GRAYSON: We'll have to get a copy of that cartoon, so that those people who hear this tape and look at this transcription will be able to understand "partial credit." [laughter]

NIER: I think that's a marvelous cartoon. (Figure 8)

GRAYSON: So, basically non-scientific or non-professional things drove your choice to come to Minnesota at the time. So you made that decision, and did come here, and essentially, by that time, I guess your reputation was pretty well-established.

NIER: Yes, yes.

GRAYSON: And you didn't have any trouble with Tate coming around giving you left-handed hints about where you were going.

NIER: Oh, no. Everything was wonderful. They did everything they could. I was very happy.

GRAYSON: Okay. So, did you just, kind of, continue along the lines of your work at Harvard?

NIER: Well, I had to set up a new apparatus again. Tate had arranged for the magnet, so we copied the Harvard magnet. In the meantime, they had improved techniques in casting metal, and so on. I had to wind the coils myself. It was "Number 8" wire or some size like that.

GRAYSON: So, how does one do that?

NIER: Well, you learn how. You go to a lathe, and have spools made, great big brass spools. I can show you the magnet. We still have it...it's used in another instrument.

GRAYSON: Yes, we need to take...

NIER: ...and you can get a picture of that, too. And, so with the help of the guy in the shop we wound this magnet.

GRAYSON: This was an enamelled wire?

NIER: No, this was cotton-covered stuff that we had.

GRAYSON: Oh, wow!

NIER: And then we had to paint it with black caulk for making it thermally conducting, sort of like tar. It was a mess! But you did this on the lathe to get a better space factor because the heat conductivity was important. As in the Harvard magnet, we used water cooling tubes to keep the coils from getting too hot.

GRAYSON: Did you say you used square tubing too, or? [laughter]

NIER: No, we didn't use square tube. We had round tube. [laughter] I became an expert in winding coils, which I didn't have to do at Harvard, because they'd had it all done for me. There was a lot of brute work I had to do here just getting started again. But I had an instrument running by Christmas.

GRAYSON: So, you came here, when? At the beginning of the summer? End of the summer?

NIER: In the middle of the summer.

GRAYSON: Middle of the summer.

NIER: I left there about August.

GRAYSON: And brought?

NIER: I had the spectrometer tubes that Bainbridge gave me, and the magnet was being built during the summer. I'd sent the details to Tate, and he had arranged it for me. So, the magnet was delivered in Fall, but the coils had to be made yet, and I had to participate in that, so I spent a lot of time with the machinists in the shop winding coils. Well, it wasn't that bad, but you know, it took time.

GRAYSON: Yes. Well, definitely. It's a time-consuming job. How many miles of copper wire do you suppose you had?

NIER: Well, it was pretty heavy stuff, so it wasn't that many miles, but it was a lot. When you see the coils you'll understand; they weighed 500 pounds apiece or so.

GRAYSON: So, even though you were rebuilding your whole apparatus and instrumentation, you were able to do things in a very short period of time.

NIER: Yes, well I had help, and I got some money for it. Then I had to build a regulator for the generator. The generator here wasn't as good as the one at Harvard. It wasn't as steady. So I had to build this thing all over again, the regulator for the generator. But then, I had some help. I had a grant from our graduate school so I could hire undergraduate students, who were good. They were kids who knew how to build electronics devices. So, I had people building these things for me. By that time, we also could get away from using batteries. We built, I think it was a 1200-volt power supply for accelerating the ions, which was electronically controlled. I had a very good student working on that.

GRAYSON: That kind of electronics technology...were people building high-voltage power supplies at that time, or, was that a common thing to buy those?

NIER: In some limited cases. People had Geiger counters and instruments like that. To be in physics, you had to build devices of that kind. This was different than high current supplies and things like that. By that time, I'd learned how to do it, thanks to my connections with the Harvard electrical engineers.

GRAYSON: So, all that business about feedback was put to good use again.

NIER: Oh, yes. Good use, that's right. See, there's a real advantage to having an engineering background.

GRAYSON: Well, it's one of the things that's becoming obvious to me. If you'd had a straight, classical physics background, a lot of this instrumentation would've never come to pass.

NIER: Never done. Never come to pass.

GRAYSON: Because you would have hit a brick wall. You knew that you needed an XYZ, but you didn't know how you were going to get one, and you didn't know anybody who was going to be able to do it.

NIER: That's right. You didn't even know who to talk to.

GRAYSON: Yes, but with the double-E background, you immediately knew what to do.

NIER: That's right, that's right. It's just terribly important. You can't over-estimate this at all.
[laughter]

GRAYSON: So, a nice combination of fate.

NIER: Yes, yes.

GRAYSON: I think we're about to the end of this tape, so why don't we stop before we go any further.

[END OF AUDIO, FILE 1.5]

[END OF INTERVIEW]

INTERVIEWEE: Alfred O.C. Nier

INTERVIEWER: Michael A. Grayson

LOCATION: University of Minnesota
Minneapolis, Minnesota

DATE: 8 April 1989

GRAYSON: We're starting Side B of Tape 3, and Dr. Nier is talking about an article published in the *Physical Review*, 1935, in which the potassium isotope—potassium-40—was discovered.¹⁵ Now, he's telling us about the lettering on the figure.

NIER: Well, Professor Tate, who was the editor of the *Physical Review*, was accustomed to doctoring up documents that came in that fell short, in one way or another. Often this was the case in the lettering. And especially, when he got foreign things—the Germans especially, I think, were not accustomed to doing their own drawings. They would send in just a rough draft of the drawings, and expect the editor of the journal to do the finished draft. So, he was accustomed to fixing up figures. But he didn't like the lettering I had on my drawings. So, the result was, he did it over. If you look at random at the *Physical Review* of that era—the 1930s when he was active as editor—you'll find many papers in which the lettering will look like that in my article, which appeared in the 1 August 1935 journal. This is all hand-lettering, you understand. We recognize his very distinctive lettering. You saw a lot of that in the journals of that time.

GRAYSON: This was a free-hand lettering that he did?

NIER: Freehand, yes.

GRAYSON: Okay.

NIER: He was very good at it.

GRAYSON: It is very good, and it is clear and legible.

¹⁵ Alfred O. Nier, "Evidence for the Existence of an Isotope of Potassium of Mass 40," *Physical Review* 48 (1935): 283.

NIER: [laughter]

GRAYSON: Okay. Let's see. Where had we ended?

NIER: We were back in Minnesota...just a minute.

GRAYSON: Oh yes, you'd decided to move back to Minnesota.

NIER: Well, I was just getting going, wasn't that it?

GRAYSON: Yes, you'd finally gotten your equipment together again, and were working on it. You had it ready to produce results again. So, at this point, what exactly did you get involved in initially, when you first started again in Minnesota.

NIER: Well, when I came back, I wanted to pursue the work on isotopes, which I'd been working on originally here, but more so at Harvard. And, I had, as I said, two mass spectrometer tubes which I'd built at Harvard, which Bainbridge allowed me to take back with me. And I had additional lead samples which I'd gotten at Harvard also and then some that I got from our geology department. I wanted to look further at variations in common lead. And then I also had samples of uranium lead that had come from somewhere or another, I suspect through Alfred Lane or maybe through Harvard, I don't remember. One of the things we continued was the work on the variations of the lead isotopes. Also at the time, I gave a paper at the American Physical Society Meeting in Washington in April 1939, which would be fifty years ago on the iron and nickel isotopic abundances.¹⁶ I'd gotten my instrument going in time, around Christmas or shortly after, and the abstracts were due in late winter. [laughter] So, I made it.

GRAYSON: The game then, was the same as it is now. When the abstract deadline comes around, that's when the work really gets done.

NIER: Really gets pegged, you see. [laughter] So, I had a paper on that. And that's what I reported on there. Now, there was an interesting story about that paper. The secretary of the American Physical Society at the time was quite an elderly man, who was a little bit out of touch, and was getting a bit senile. So, he didn't know exactly what these different fields were,

¹⁶ Alfred O. Nier, "The Isotopic Constitution of Iron and Chromium," Paper presented at the meeting of the American Physical Society, Washington D.C., April, 1939.

where the papers fit. And, my paper on the isotopes of iron and nickel didn't seem to fit anywhere. But there was a solid-state section on the kind of things solid state people worried about—phase diagrams, and so on, in metals. This was on Saturday afternoon, the last afternoon of the meeting, you understand.

And my paper, which didn't seem to fit in with phase diagrams and stuff like this, was the last paper of the afternoon. So, it turned out that that session had a small group. As things were, there were a few dozen people there when things started. But when my paper came on, everybody walked out, except the chairman, my wife, and somebody else. And I remember the chairman of the session was Ed Condon, who I knew already, as I said, the man who was later going to be the Director of the Bureau of Standards. So, I had this small courtesy group who listened to me tell about the isotopes of iron and nickel at the American Physical Society meeting. So, that was the crazy thing that happened there. However, that was the meeting right after nuclear fission was discovered and where I met [Enrico] Fermi. I knew John Dunning already, who was the man in charge of the Columbia cyclotron, and was interested in nuclear physics, and through him, I met Fermi at that meeting.

GRAYSON: At that meeting?

NIER: At that meeting. That was April of 1939, and fission had just been discovered a few months before. It was one of the things that was talked about a lot at the meeting. And that's when I got acquainted with Fermi. So, that was a positive thing that came out of the meeting.

GRAYSON: Well, my feeling of meetings is that more than half of the meeting may actually be in the contacts you make. The papers that you attend are a small portion of the meeting. [laughter]

NIER: I think more than half. It goes on at lunchtime or in the corridors. Dunning had figured out that, if I just souped up the spectrometer a little bit, I could collect enough separated isotopes of uranium to make possible a determination of the fissionable isotope. He knew how much uranium it would take to detect fission if they bombarded my samples with neutrons. I don't remember the exact conversation, but he pointed out that if I could collect some fraction of a microgram of uranium-235, they ought to be able to verify it was the fissionable nuclide.

Now, it had been predicted by [Niels] Bohr and [John A.] Wheeler, that uranium-235 ought to be the one responsible for the slow neutron fission. But it had never been demonstrated experimentally. That was the whole point of it. So, we went through this calculation. Then, in the meantime...a lot of things were happening. I had gotten interested in isotope separation. In particular, C-13 which seemed to be useful as a tracer for biological purposes.

GRAYSON: When did that start?

NIER: Well, you see, there were several papers published in about 1937 or 1938 in Germany. [Klaus] Clusius and [Gerhard] Dickel, C-L-U-S-I-U-S and D-I-C-K-E-L, published something about the thermal diffusion column.¹⁷ You have a hot wire in a vertical tube and a cold wall around it. If you want to do it in quantity, you'd have to water-cool it to get enough power in. Then, you put a gas in, anything you want to. And what you have between the hot wire and the cold walls is a gradient—a concentration gradient of the isotopes, of the isotopic molecules. It's a phenomenon that you can't explain by elementary kinetic theory. It takes a more sophisticated theory, because it depends upon the law of force between the molecules. The molecules coming in one direction encounter others from the opposite direction with different temperatures and speeds. It's a subtle sort of effect. It'd been discovered many years ago by [Sydney] Chapman and somebody by the name of [David] Enskog, I believe it was, in 1910 or thereabouts. But it had never been put to use, as best as I can determine. And these guys, Clusius and Dickel, pointed out you might use a column like this as a fractionating system. It would repeat itself, and because you'd get a convection flow up and down with the gas moving like a roller-towel and you'd keep concentrating heavier molecules at the bottom of the column in this way. I don't think they enriched isotopes initially, but they separated some gases this way. It seemed natural to use methane gas for separating carbon-12 and carbon-13. It was light, and involved carbon.

So, I built a column, and as a matter of fact, if I remember correctly, it was started before I left, and Jay Buchta, B-U-C-H-T-A, who was head of our department, was awfully good about taking care of everybody. He could never turn down anybody who asked a favor, and he would try to help, and he saw to it that the shop continued working on this in my absence when I went down to the meeting. So, when I came back, a lot of the column was completed. I proceeded, and showed immediately that we could separate carbon isotopes in small—very small—amounts. And then, of course, the column was made longer and longer. We had an empty elevator shaft downstairs. So, my original column was 24 feet long, and we enlarged it to 36 feet, got another floor in, and we had another column in parallel finally, where you circulated between the top and the bottom of one, but that came later. Anyhow, I was producing enriched carbon, and thinking it could be used for nuclear experiments, separate targets for bombardment, and so on. But also, I knew a lot of people in the biological field who were just anxious to get their hands on separated carbon isotopes for tracers. So, I began producing enriched carbon.

GRAYSON: Now, what was the whole concept of doing this? Why did you want to do this enrichment?

¹⁷ Klaus Clusius and Gerhard Dickel, "Neues Verfahren zur Gasentmischung und Isotopentrennung," *Die Naturwissenschaften* 26 (1938): 546.

NIER: Well, it sounded like fun. [laughter] And it seemed like a useful thing to do, I don't know. And, as I say, I knew people in the medical school, I knew people in the biology departments who were interested in the possibility of this, doing tracer work. See, carbon-14 was not yet then available.

GRAYSON: So, you had a kind of inkling in the back of your mind what you would like to do with this if you could get it, if you could enrich it.

NIER: That's right, there'd be uses for it. I might not use it, but somehow or another, I always had these friends who could use it. I belonged to the faculty club, where people from one department used to sit with people from other departments. It was very interdisciplinary. Nowadays, the organic chemists sit at one table, the historians at another, and my physics friends are all sitting at a certain table. They never sit with anybody else. I usually try to sit with other people, but that isn't done much any more. So, I got to know a lot of people in other fields, which is one of the important things about a faculty club, I suppose.

GRAYSON: So, you're saying that, maybe, over a lunch, at one time or another, a problem was posed to you, "Could you do this?" And you thought, well, maybe not, and maybe you thought about it some more, and...

NIER: That's right, there were a lot of connections like that, and I did get a lot of people started on work of this kind, through this sort of informal contact, which is one of the wonderful things you can do in the right environment. So, we were producing C-13 and I had very good friends in chemistry, who were friendly competitors in this, they got into it also. Ivan Taylor, who used to be active, was a very good analytical chemist. He died too young, of cancer some years ago. And another fellow by the name of George Glockler, was interested in this, too, because the physical chemistry of the whole process was an interesting one. So, they built a column in the chemistry building around an open stairwell. They used to have open stairwells.

So, we were friendly competitors in this business. They didn't have as good of machine-shop facilities as I had, so my column worked better. You had to have precision between the center wire and the tube. We didn't use a wire, because we wanted to get more throughput, so we had a tube, a heated stainless-steel tube, with a heater inside of it, about an inch or an inch and a half in diameter. And then, there was a water-cooled brass tube outside, with the spacing about a quarter-inch between the hot and cold walls. But it had to be very precise, because if it wasn't symmetrical, then the thing wouldn't work right; the fractionation wouldn't continue on. If I remember correctly, my colleagues used pipe instead of tubing. I used tubing, which was precision stuff, and had fancy spacers to center the hot tube. A lot of the parts I had to make myself. But the shop gave me wonderful help. So, we built the columns, and were producing carbon. When the plant was expanded in a year or two after that, we were producing routinely 10 percent C-13 from 1 percent. So, you could do some pretty decent tracer experiments.

GRAYSON: Now, did this thing start out evacuated, or did you charge it with gas?

NIER: Well, you started with it evacuated, of course, and then you filled it with methane. We got pure methane, natural gas, from some particular well in California, and they gave it to us free, because it was cheap. We'd get cylinders, and we'd just have to pay the shipping costs.

GRAYSON: And then, once the system's charged, then you'd turn on the heat?

NIER: You'd turn on the heat. At first we did it batchwise, but then later we fed fresh methane continuously into the top of the column, and then took out enriched stuff at the bottom. Actually, you shouldn't feed in at the end in a fractionating column, you feed in someplace in between. So, one end takes out one kind of component and the other takes the other. That's the way you'd run a fractionating system, but I don't think we ever got that efficient. I think we just had fresh methane going past the top, and took out the heavy at the bottom.

GRAYSON: When you were doing this in a step-function charge type thing, how long did it take for the separation?

NIER: Pretty slow. [laughter] The output, I don't remember what it was now, but the output was about a liter of gas a week. This ran at, I think, just about atmospheric pressure,

GRAYSON: That was about 10 times concentrated?

NIER: That was about 10 percent concentration. That's the order of magnitude. And, friends of mine in physiology used it for tracer experiments in metabolism studies, I think, with mice, something like that. And then, I had other friends in botany, who grew radish plants in a bell jar with this, and then they could make sugars, that were labeled, because they let the plants do things for them, you see.

GRAYSON: Were they trying to get to the labeled sugars, or were they trying to...?

NIER: Yes, labels.

GRAYSON: So, there were actually taking your product and reducing it further?

NIER: Making other things out of it, making further things out of it.

GRAYSON: Okay.

NIER: But this was what led to the 60 degree instrument, rather than the 180. It became apparent that not everybody could have a 180 degree instrument that weighed two tons, and took a five kilowatt, stabilized generator to run it. And of course, we had expanded needs. I had graduate students working on different projects and we were pursuing several programs at the same time, and the capacity of the spectrometer wasn't enough to do what we wanted.

GRAYSON: Okay, so now, we're talking about the late 1930s, early 1940s?

NIER: That's correct.

GRAYSON: You had gone to the APS [American Physical Society] meeting, and met Fermi, and shown this iron-nickel work that had been misplaced, and at the same time you were also doing this work with the carbon, both producing it and analyzing it.

NIER: And analyzing things for people. And I had students to help me on the analysis, of course.

GRAYSON: Okay. But all of this work was all being done on one machine?

NIER: One machine. The 180 degree machine, with a two-ton magnet...

GRAYSON: Which was a biggie, okay.

NIER: That's right. Yes, sir.

GRAYSON: So, we're, kind of, poised here in a period where there's a lot happening all at once.

NIER: That's correct. And I was teaching eight hours a week. [laughter]

GRAYSON: [laughter] Of course, by this time, you had the course material down pretty well. [laughter]

NIER: Better. [laughter]

GRAYSON: [laughter] And still giving partial credit. [laughter] So, I did see a copy of the letter from Fermi that he had sent to you. The impression that I got from the letter is that he was kind of giggling you, like "Hey, kid, get to work on my project!" [laughter]

NIER: Well, it's interesting. I never met him again, which was too bad, because I never got to Los Alamos [National Laboratory] until after his death [in 1954]. So, I never saw him. I may have seen him in Chicago [Illinois] after that, but never had a chance to visit with him again, because it would have been interesting to our earlier contact review at that point. But certainly, he was interested in it. And see, they had a very good group there at Columbia. First of all, there was Dunning, and there was Herb [Herbert L.] Anderson, who made a name for himself, as an assistant to Fermi, and the other people around there, so they were very interested in all aspects of the fission problem. And Fermi was never that convinced, you see, about the uranium-235 business. I raised the question with Wheeler a few years ago, and he acted as if they always knew that U-235 was responsible for the fission of uranium. But nobody had ever demonstrated it. So, I don't know. Well, I'm sure they were quite sure of what they were doing, but still it needed demonstration. And that, of course, was the interesting thing. And to demonstrate that you could separate enough that way that you could actually do something with it. That was an important point, you see?

GRAYSON: Okay, I'd like to take a little bit of time then, and just explore that whole experiment. I understand that initially, you finally did get some UF₆, which you hadn't been able to get before. Is that correct?

NIER: That's correct.

GRAYSON: But then, when you did actually try and do the experiment with UF₆, it didn't work.

NIER: It didn't work.

GRAYSON: And what was the problem there?

NIER: Well, I met Fermi in April of 1939. During that summer, I was working away on the diffusion studies. Actually studying the thermal diffusion of gases by having two bulbs at different temperatures, and studying how much the isotopic concentrations varied. This was basic stuff. And that's when I got started on the wedge instrument, the sector instrument, at the same time. And, we were carrying on the lead work at the same time as well as running analyses for people who were using our carbon. So, you see, we had a full situation here. Well, I had been in touch with Dunning, on and off. I don't know if I still have the correspondence. I lost a lot of that after the war, because it got mixed up with classified things which went to Oak Ridge [National Laboratory], so I may have even lost it. Or it may be buried somewhere, or have been thrown away by this time. But anyhow, I certainly was in touch with Dunning. There was a guy by the name of [Aristid von] Grosse—von Grosse, actually—who was a very, very good inorganic chemist, who'd been with Universal Oil Products in Chicago, and gave up his job there, and went to Columbia to work with Dunning on fission after it was discovered. He knew all of the chemists who knew something. And so, they arranged for me to get some UF_6 . Where it came from, I don't know. Maybe the Naval Research Lab was manufacturing it then, because they were going to [use] liquid thermal diffusion as a possible separation method.

Anyhow, I got some UF_6 . And this would have been in the Fall of 1939. I don't know exactly when, now. And just put it in my instrument like any other gas. We tried to see what we could get on targets, placed at the end of the magnetic analyzer. In place of the faraday cup, we just put some little strips next to one another at the location of the normal outlet slit of the instrument. We found that you couldn't tell the difference between the targets I called uranium-235 and uranium-238 because the UF_6 is, sort of, a sticky substance, and it went on everything. And the amount that just diffused around the whole spectrometer tube was just too much. It was a gas at room temperature. And so, you couldn't tell anything.

It was at that point then, and I think this final decision was made over the Christmas holidays, that we decided that we would have to approach it somehow differently. Get rid of the UF_6 and use a relatively non-volatile substance like uranium tetrachloride or bromide, with an oven in the source, and then when the vapor hits a cold surface, it falls dead. It will never get around to the collector in a 180 degree instrument without ionization and separation. So, that was the decision.

GRAYSON: So, basically, you're going back to use the same type of materials...?

NIER: At Harvard. In fact, I could have done the separation at Harvard years before. [laughter] For the final experiment, I built an instrument with a seven-inch radius glass tube.

We covered the inside with Aquadag, the colloidal stuff which you baked on. It made a nice black surface and was a good conductor. The whole source, the electron bombardment system, the little box with the oven, etc were on the end of a stem that went in through a ground joint. And I have pictures of this, by the way. This was the first time I departed from using all-glass systems. We actually used a grease joint to put this in. And the collector, the same way, had a ground taper with the collectors in it, coming in from the other side. Well, we got it going, and were able to soup up the heater in the source and get a higher pressure. Actually, I don't think we had a real arc. But we probably had a glow discharge in the source. I had much bigger ion currents than I'd ever gotten before. Within about a day's running or thereabouts we managed to get enough uranium-238 so you could actually see it on the target. A little smudge.

GRAYSON: So, you could actually see the sample build up as the experiment was running.

NIER: You monitored it on the uranium-238 collector. I had a meter connected to it and measured it with an electrometer.

GRAYSON: Yes.

NIER: So, then you do a calculation. It's so many amperes for so many hours, how many atoms in Avogadro's number and all of this stuff. You can figure out, predict how much uranium-235 should be there. You just add a fifth.

GRAYSON: So, the collection arrangement was essentially two independent collectors?

NIER: Two independent collectors. They were little strips of nichrome or platinum. And the resolution wasn't all that great, but it was good enough. Uranium-235 and uranium-238 are, you know, three mass units apart.

GRAYSON: Yes. So, basically, the machine essentially just ran.

NIER: You had to babysit it, because it would drift off of the peaks. You couldn't just leave it overnight or anything; and you had to keep watching the current because the material would be depleted, so you'd have to soup it up. Or if it got too warm or something...because it wasn't just a straight electron-impact ionization, there was some kind of a discharge there, the pressure was high enough. So, it was, kind of, erratic.

GRAYSON: In order to actually get this thing to go in a length of time that was reasonable, you were really pushing the whole instrument.

NIER: You were pushing everything. When it was run as an analytical instrument an ion current of 10^{-10} amperes would have been a pretty hefty current. This was more like 10^{-9} or 10^{-8} .

GRAYSON: Significant...yes.

NIER: And you began to get a space-charge problem, though, because, the ions weren't all that fast. I remember now, the voltage supply on this machine was 1200 volts. It was the power supply that accelerated the ions. You do the calculation in that range, and you find that the space charge...the repulsion between ions begins to be a problem.

GRAYSON: So, basically, you were right at the edge of where you could work.

NIER: Just about. Oh, and the vacuum really was bad. You see, the decomposition of the compounds gave you icky chlorine and bromine in the machine. The vacuum wasn't as good as it could have been. That's one of the things that limited you, too.

GRAYSON: Then, at the end of the experiment you removed the collector and sent it away?

NIER: That's right. And the stories that were told are really so. On a Friday afternoon, which if I remember correctly, was 1 March 1940, I wrote a letter, and pasted the little targets on the margin of the letter. I went down to the Minneapolis Post Office—in those days it was open twenty-four hours a day, you could mail letters—and sent it “Airmail/Special Delivery” to my friend John Dunning. This was before the days of Scotch tape, and I don't know how I fastened it on, but I think it probably with a piece of a Dennison label. Dennison labels go back, way, way back. The letter still exists, but I don't know its whereabouts. I should have asked, since I kept no copy.

GRAYSON: You didn't take it to the Xerox machine?

NIER: [laughter] I didn't take it to the Xerox machine, that is correct. And since it was a long-hand letter, I wanted to get it off in the afternoon, because there was a plane that night. And, so I mailed it. And Dunning called me, woke me up on Sunday morning. I was still in bed, it was very early Sunday morning, and they had worked all Saturday night, and had

demonstrated that, indeed, the one that I called uranium-235 gave fissions with slow neutrons, and the other one didn't. It was very definite. And that's the story.

Well, then, the letter. I saw Dunning hundreds of times, literally, afterwards. And I never asked him for the letter or a copy of it. There were copying machines existing later on...I can see why he might not have wanted to give up the original letter, but after all, it was my letter. But be that as it may, I never got a copy of it. And, it turned out, he died some years ago, and his wife guards all his stuff jealously. And I've put it up to one of the historians at Columbia to see if they couldn't get their hands on it and get a Xerox for me, at least, but there's no hope. He tried, as he was interested. So, as long as she's living, this is not possible. She was very devoted to John and had some strange notions about the value of something like this. You know how people get.

GRAYSON: Sure.

NIER: So, you just don't know.

GRAYSON: But for the sake of anyone who might want to follow it up at a later time, there's a good chance that...

NIER: It's in his papers, because he saved everything, and I know he had it, because he had that and many other things.*

GRAYSON: Because that would be a quite an interesting document to try to locate some day for posterity.

NIER: Indeed it would, indeed it would.

GRAYSON: Mailed...hand-carried and mailed, probably for less than three cents...well, air, special-delivery, it might have been ten cents.

* In the fall of 1989, a biographical memoir on John Dunning appeared in the National Academy of Sciences Biographical Memoirs. It contained sample of correspondence between Dunning and Nier. Following Mrs. Dunning's death some of Dunning's papers came to John Ray Dunning, Jr. Included was a copy of the 1 March 1940 letter. John, Jr. kindly sent copies of much of the Dunning-Nier correspondence to Nier. See Alfred O.C. Nier papers, collection number 999, University of Minnesota Archives, University of Minnesota - Twin Cities.

NIER: With special delivery it was ten or twenty-five [cents] or something, which I paid myself.

[END OF AUDIO, FILE 2.1]

GRAYSON: ...talking about John Dunning's relationship to the Manhattan Project.

NIER: Well, that was the beginning, you see. John was very interested in this all the time. He was the one that started the ball rolling. He was always interested in the electric power aspects of the problem, and of course, recognized, with the war coming on, that a bomb was a real possibility. But I know he always stressed the power situation and felt that in the long run, you had to have some way of separating U-235—that you would do it in large quantities—to make it practical. He had a very, very good group of young people and some not so young with him there at Columbia. In particular Gene Booth, Eugene [T.] Booth. John was a very imaginative, flamboyant type, a real entrepreneur who might wander off into the wild blue yonder. Although he was very sound in every respect, he was always on the optimistic side of things, whereas Gene Booth was a young, very conservative-type person. This was a wonderful team because Booth would hold Dunning down to practical things, because he could sometimes find flaws in John's extrapolations, and so on. So, they worked together, it was just a marvelous team, wonderful people to work with and I just loved those guys. Gene and I are still very good friends. I haven't seen him for a long time.

Later they pursued the diffusion separation process. I got into the Manhattan Project through them, as I mentioned in this historical article I wrote for the Chemical Society. But I don't know if you want to include that now or if you want to talk about the sector instrument.

GRAYSON: Well, I suppose, probably, while we've started into the nuclear fission-type thing, perhaps we can continue to explore some of the details of that, as it... basically, you're saying, in the 1940s, in early 1940 period, Dunning had a vision of what was coming?

NIER: Yes. Well, I think we've got to talk about the sector instrument first, because that plays an such important part in the Manhattan Project, later on. Coming back to our previous discussion about all the activities we had that required mass spectrometers... it became obvious we couldn't do it all with a single 180 degree machine, so we needed more capacity. I remembered so well that Bainbridge's mass spectrograph had a combination of 127 degree sector electrostatic analyzer followed by a 60 degree magnetic one. That's the way they did their mass analyses. I also remember reading papers of a guy by the name of Bill [William] Stevens, who was I think at the University of Pennsylvania at the time, and somebody by the name of [Michael] Barber in England. They had published papers in the early 1930s about focusing charged particle beams with sector magnets. They pointed out a general theorem if

you have a source of charged particles emanating from a point and send it through a sector magnetic field, if the apex of the sector and the source of the ions and collector are in the same line, then the diverging beam will focus at the collector. The 180 degree case is just a special case where you open up the apex angle to 180 degrees and the straight line is the diameter of the circle. Theoretically, any angle would do. If it wasn't for the fringing fields, you could have a 1 degree sector. It seems like the question is if you're going to do this, what angle should you choose? And I thought about this a little bit, and choose 60 degrees (Figures 9, 10, 11, 12) because the 60 degree deflection worked so well in the Harvard machine of Bainbridge's; and in part because of other considerations. When I thought about other angles, it turns out that it would have taken quite a bit more magnet for 90 degrees. If you stop to think of it, going from 60 to 90 is quite a bit more. Also, if you were going to have a flattened tube that you fit between the poles, it was a lot easier to bend something through 60 degrees, rather than 90. Flattening tubes isn't the easiest thing in the world. Later, we perfected the technique of bending and flattening tubes to any angle but at that point, we didn't know how to do it well.

GRAYSON: So, now you're thinking about making metal flight tubes, at this point?

NIER: Yes, metal flight tubes.

GRAYSON: Even though glass was primarily what had been used up until then?

NIER: Yes, but now, you see, if you wanted to use an electromagnet, you had to have a narrow air gap to exploit it. So, there wasn't room for a glass tube. I never built a sector glass one.

GRAYSON: Okay, so the technology forced you into this.

NIER: But then you had to have glass to metal seals at the source and collector ends. In the original tube, of which I have a photograph, we had a copper tube which went through the analyzer. Five inch lengths of Kovar tubing were silver-soldered to the copper tubing. The source sat on one end, the collector on the other. Glass tubes which encased the source and collector were sealed to the Kovar. Now the presence of the Kovar really bugged things because the Kovar is very magnetic and you had a big piece of Kovar right next to the source. You wanted to use a magnet for collimating the electron beam but the magnet magnetized the Kovar and affected the ion trajectories. This didn't work too well. Subsequently, we changed that and all the tubes we built afterwards we continued the copper tubing all of the way to the source and collector and made the glass to metal seals through Kovar cups which fit over the copper tubing; well away from the source and collector.

GRAYSON: So, you moved the glass-to-metal sealing arrangement as far away from the magnet as possible?

NIER: Yes, it was quite far away; anyhow, you no longer had a problem. The only difficulty you had was working with the Kovar. You had to be careful because it cracked if you didn't anneal it properly. So, there was a lot of little technology we had to learn.

GRAYSON: Yes, It sounds like a mess, to be quite honest with you. [laughter]

NIER: It was. [laughter] But it worked.

GRAYSON: I've done enough of that kind of stuff to not like it at all.

NIER: But that's the way the thing went. We standardized on a 6-inch radius for the magnetic analyzer.

GRAYSON: And so now we've got 6-inch radius, 60 degree sector instruments.

NIER: That is correct.

GRAYSON: And for the magnet for these, you still used an electromagnet.

NIER: Electromagnet. But you could run it off of a couple of automobile storage batteries instead of a motor generator.

GRAYSON: Okay, that's a big change in power requirements.

NIER: That's right.

GRAYSON: Was this because of physical size?

NIER: Yes.

GRAYSON: The magnetic fields weren't as high?

NIER: We didn't go quite as high with the batteries. In the case of higher fields, we had to still use a generator but I had the stabilized generator by then. But for light molecules, such as carbon dioxide we could use batteries. Those were the days of 6 volt automobile storage batteries and I think we had two or four of them.

GRAYSON: What would high mass be for this type of machine?

NIER: Well, at first, we were just shooting to do carbon dioxide, but then we extended the studies to heavier molecules.

GRAYSON: That was still with the battery...

NIER: No, when we went to the heavy elements we had to use a generator.

GRAYSON: Okay, so, somewhere the battery power supply was not adequate, I mean, you had to use something else to get to higher masses.

NIER: That's right.

GRAYSON: It was probably around...

NIER: And I think we maybe even tried having the batteries on and charging them at the same time they were being run, using them as a capacitor, but I don't remember exactly.

GRAYSON: But basically, the whole concept here is that the machine was certainly simpler and more useful.

NIER: And more people could have it.

GRAYSON: Now this prototype machine was in 19...?

NIER: 1940.

GRAYSON: 1940. So, was this being developed simultaneously with the problem of measuring the uranium?

NIER: Yes, in another part of the room.

GRAYSON: How many graduates [students], how many people, did you have working for you at that time?

NIER: Well, I had one or two undergraduate kids working as technicians. They were very good ones because you could choose. Also, I probably had one or two graduate students working with me at the time, so I think the total group, including myself, was around five people.

GRAYSON: You had shop support?

NIER: Shop support, yes. We built a lot of the apparatus ourselves but the shop did the precision machine work. And we had by that time a machinist, who later became head of our shop, who was an utter genius, R. B. Thorness, T-H-O-R-N-E-Double-S, known as “Buddy”. He started working in the University shops when he was just a kid out of school, so the older men there called him “Buddy”. He carried that name until his death. He was a little older than I was and died, unfortunately, some years ago of a heart attack. He had very little formal education. He may have gone to vocational school instead of high school. Maybe he graduated from high school, I don’t know. He had a sense of what was appropriate. When you used a hacksaw to get something done or when you used a grinding wheel to get the surface smooth and precise. He had the sense for what was appropriate and he could design mechanical things for which he didn’t have any formal training—levers and moments of inertia of cross sections, and so on. I would have trusted him more than any mechanical engineer who did this professionally. [laughter] He knew where you made things hefty and where you didn’t. It was very important to take him into your confidence, so that he considered himself part of the project.

This is something that’s missed by some scientists. We are very dependent on people of this kind, and you’d better make them part of the team, if you want to get their cooperation. We would explain the experiments to him; and he might not have fully understood what we were

trying to do, but he got the gist of why it was important, why he should do a good job. As a result he was very, very cooperative. He would come up with new ideas on how to do things. He was very interested in finding better ways of fabricating things. The problem of the Kovar cracking was so frustrating for him, he just about went crazy. It just had to be licked, so he just kept working on it until he perfected the proper technique.

GRAYSON: Now, was he on the staff?

NIER: In the physics department we had three men to help on mechanical things. A machinist who was supposed to do fancy machine work for everybody in the physics department, faculty and graduate students. He got the job when his predecessor retired. His predecessor was pretty good but he had a real personality problem. We couldn't get anything out of him if he came in and felt bad or had a real or imagined problem. He always seemed mad at something. But he was a very good machinist and when he did things, they were good. But Buddy was better and was a nice guy you could work with. Then we had a man who did general work who was a combination glass-blower-photographer, and machinist, whose job was more or less to help graduate students in their problems. He was very versatile and very good. He was a half-time instructor and also taught courses in laboratory arts, as it was called. I got to know him very well and that's where I learned how to blow glass and how to make tungsten-to-pyrex seals and all such things. Then we had a third man who came a little later on, who did carpentry and some machine work. So, that was the whole mechanical staff. But all the fancy things we're talking about were done by Thorness.

GRAYSON: Did he become the machinist when this other fellow left?

NIER: Yes, he became the machinist. He had been in a University shop before that, which happened to be housed in our basement next to our shop.

GRAYSON: Oh, I see.

NIER: So, he just came and changed workbenches.

GRAYSON: Okay. All these people then were employed by the physics department?

NIER: Yes, we had three employees in the physics department then at that time. Actually, he technically was still part of the central shop assigned to us, so it was always a touchy thing. But later the payroll item was transferred to us, but as far as we were concerned, he had always been

ours. During World War II, he came with me when I moved to the Kellogg Corporation in New York. He headed my shop, because I had a development laboratory there. But that's another story.

GRAYSON: Yes. We were picking up on the development of the 60 degree sector machine and the fact that there was obviously a need to make these machines and have them in other places. Anyway what happened to these machines that were made?

NIER: He then built tubes for a few other labs before the war. I don't remember where they went. But he built a few of them, I know.

GRAYSON: Did you have in mind, at the time, increasing the number of analytical instruments available here, or did you have in mind creating...

NIER: We were not interested in going into the business of building instruments but tried to help others. If somebody wanted to have a mass spectrometer tube and would pay for Thorness' time, we would do it. I know we built at least one tube for Harold [C.] Urey, before the Manhattan Project was formalized. We also built one for the people at Princeton, Hugh Taylor, who was a very well-known chemist and also dean of the graduate school. They had built a tube which didn't work very well. They were working on isotope separation problems related to the uranium program. It wasn't called "Manhattan Project" then. It was still under the aegis of the National Defense Research Committee as it was called headed by Vannevar Bush.

For all practical purposes, we were in the war before 7 December 1941. It was fairly obvious. I was going to build a house that summer, and so I knew about the difficulty in trying to get materials. There was a fair amount of activity in the uranium program. Urey saw the need for developing methods for separating isotopes and for analyzing the results. He was in the chemistry department at Columbia then, and Dunning was in the Physics department. Work was supported by the Office of Scientific Research and Development, OSRD, which was the agency that did things for the National Defense Research Committee. I obtained a contract to separate some more uranium-235 by electromagnetic means. This would have been the summer of 1941. We worked very hard on this and got larger quantities of it but not nearly enough to be really useful. We simply didn't have...

GRAYSON: Was this being done on the larger 180 degree machine?

NIER: No, it was done on a 60 degree, which was put aside for the purpose. The 180 degree instrument was being used at that time for uranium isotope analyses. It was the best machine we had and we were monitoring the work of the people who were trying to separate uranium by

various methods. And we had the only instrument in existence that could measure uranium isotope ratios.

GRAYSON: So, this machine was reserved for the real important work.

NIER: That's correct.

GRAYSON: Because these people trying to enrich the fissionable material...this was the only machine in the States...

NIER: In the world!

GRAYSON: Yes, you did it...or no you didn't?

NIER: Yes, yes.

GRAYSON: So, you reserved that exclusively for the...

NIER: Well, almost exclusively, I may have done some lead work at the same time.

GRAYSON: Okay, because it had been demonstrated, it could clearly do it properly.

NIER: And so we didn't tie up the magnet for the electromagnet separation of isotopes. We built a 60 degree instrument for trying to separate more uranium. It didn't work as well as it should have. It wasn't as good as the 180-one that we had at the time. Part of the difficulty was the vacuum problem, I think.

GRAYSON: In that you needed a better vacuum or...?

NIER: That was the main problem.

GRAYSON: Essentially, you could get a pump of...

NIER: Well, I couldn't do as well with the 60 degree instrument. I didn't have a big enough magnet to get a high enough field. We couldn't have energetic enough ions, and if you didn't have energetic enough ions, the space charge bothered you.

GRAYSON: So, it was well-suited for lower mass work.

NIER: Yes.

GRAYSON: But not at all for higher masses.

NIER: Yes, at that time.

GRAYSON: A number of these were built then. One was shipped to Columbia and I guess there were a number of others.

NIER: There was the one that we built for Urey, I know that. There were several others but just where they went I can not say at the moment.

GRAYSON: You obviously used some of them here. Did you have a couple of them here?

NIER: We had one for the analysis of CO₂ and later we had a second 60 degree instrument. We still weren't building instruments on a very systematic scale. In about 1941 or 1942 I also had a contract to try to separate uranium isotopes using thermal diffusion with a column having a hot wire in a cool tube. In the elevator shaft, I remember we had a piece of...I think it was quarter-inch copper tubing 36 feet long. We stretched it to make it straight and put a wire down the center if you can visualize this. The wire was supposed to stay centered. It never worked very well, and it was never pursued. It turns out the method itself wouldn't have been very good, but it was a separation project which I thought would be worth trying.

GRAYSON: So, this would fall into the category of experiments that you tried, but really weren't quite successful?

NIER: They weren't successful, no. And I gave that up then. That was the end of my attempts to separate uranium isotopes. Except in the fall of 1941, when Ernest [O.] Lawrence at [University of California] Berkeley got into the act. He'd been concerned with the radar development. He was one of the wheels that got the Radiation Lab started at MIT [Massachusetts Institute of Technology], and was, you know, high up in advising the government on research. He decided that Berkeley, with their beautiful facilities, ought to get into the uranium program. He visited me, as a matter of fact, in the summer of 1941 and looked at our facilities, and saw how pitiful they were, and must've gone back and said, "Oh my God..." or something like that and they then proceeded to set up the 37-inch cyclotron as an isotope separator. He had many able assistants such as Emilio Segre, for example. I spent two weeks there, overlapped Thanksgiving Day, in the fall of 1941. I came home on 4 December 1941 three days before Pearl Harbor Day. I helped them with the design of the 180 degree separator they installed in the cyclotron magnet. They had big pumps because of the cyclotron as well as high voltage supplies for accelerating ions, etc. They immediately began to get currents that were 100 or 1000 times as big as ours ever were able to get. They continued the development after I left and that led to the thing they called the Calutron. It was essentially a blown-up version of what I had here. It was my last direct association with trying to separate isotopes.

Then Pearl Harbor Day came along. Urey had been beating on the powers-that-be before that, saying "Look, if we're going to work on these things you will have to have decent analytical facilities to measure what you are doing." I remember, he took me in to see Vannevar Bush at one time, pointing out how important it is that the OSRD ought to support instrument development so they could tell what they were doing. They were spending all kinds of money on separation methods, but nobody thought of spending money on the analysis of the product. So selling Vannevar Bush on this, and I don't know what influence he had, whether it was even necessary. The uranium project had been under the direction of Lyman Briggs, who was the director of the Bureau of Standards.

If you want to get some insight on that problem, you'll want to read Richard Rhodes' book on the making of the atomic bomb.¹⁸ That's a worthwhile book owning, by the way, or borrowing from the library because it's a whole history of the atomic business, going all the way back to the discovery of radioactivity, and it's got a lot of the personal notes on the people. It's a very interesting...terribly long, but he discusses this period very frankly and doesn't have very kind words for Briggs, which I could verify. Briggs was a real nice man, a real southern gentleman, but slow moving, and he didn't see that this was all that important. He was completely out of tune on the urgency of the war.

GRAYSON: What was his background? Was he a physicist?

¹⁸ Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon & Schuster, 1986).

NIER: He was a physicist, and apparently, a fairly good one in his younger days. But he was director of The Bureau of Standards, which was a desk job after all, and I don't know what he had done. I'm sure he had done some good science before, at one time.

GRAYSON: But he just really wasn't able to grasp the fundamental concepts?

NIER: And didn't understand the era we were living in. So he didn't push on these things, everything was so slow and I was so mad at him, at one point, because he could have gotten support for me—I could have done something. I was so mad after going to Berkeley and found out how they had taken over on things I could have done just as well months before.

GRAYSON: With just some support.

NIER: Support, you see. But he didn't see the importance of it, you see. But a lot of other people didn't either, so you can't fault him alone. But Rhodes really takes out on him. You've got to read that. It's just worth reading by itself. But anyhow, be that as it may, everything changed when 7 December came along. Among many people, I was invited down to Washington [D.C.] for a high-level meeting either at the beginning of January or late in December—a discussion of what ought to be done. And following that, we had all the money we could ever use for the analytical end. Urey was my boss. I reported to him, because he was in charge of some phase of the isotope separation business. After all, he was a very prestigious guy—a Nobel Prize winner [Chemistry, 1934] for his separation of heavy hydrogen. He was a hard guy to get along with in some ways, in other ways, very easy. He and I were always very good friends and he always respected my opinions and requests. The decision was that we immediately ought to build four mass spectrometers for doing uranium isotope analyses. (Figures 13, 14)

GRAYSON: This came following Pearl Harbor?

NIER: Following Pearl Harbor. So there's no problem. Within weeks, we had a contract for building four 60 degree instruments for uranium analyses. These were to be sent to other parts of the project.

GRAYSON: Which was in addition, or above and beyond, the ones you already had.

NIER: Already had, yes.

GRAYSON: So, you researched the design in 60 degree instruments ahead of time.

NIER: That's right. Even before the push for uranium analyses instruments came along we had built a complete 60 degree instrument for carbon tracer work. We called it the Rockefeller instrument because it was built with some money left over from a grant the Rockefeller Foundation had given the University to build a Van der Graff generator to help on biomedical research presumably to make radioactive isotopes. At the time there was the question of do you do it with a Van der Graff generator or with a cyclotron? The cyclotron was, of course, much better for production of radioactive isotopes but at that time that wasn't established. So we got money here; I think it was 25,000 dollars from the Rockefeller Foundation to build the Van der Graff and the tank outside of our building, which hadn't been used for decades. The money was for performing nuclear physics experiments and producing radioactive isotopes. But that all came to an end pretty much when war time came. There was money left in this budget, so I was able to get some thousands of it for building an instrument exclusively for the isotope analyses—for the carbon work to help the biological people. We built an instrument. I have a picture of that. [laughter]

GRAYSON: We need to get a copy of that. [laughter]

NIER: Yes...it was built exclusively for the work to help my biological colleagues.

GRAYSON: So, even though the money was supposedly for the generator, the fact that it was still going to be used for biomedical work...

NIER: We got permission to divert it for medical applications.

GRAYSON: Okay.

NIER: We had an instrument then, devoted entirely to analyzing C-13. I hired students to run it. We built the instrument...I did a lot of the construction myself, but we had electronics help. Kids who could build electronics boxes. The shop built the tube and other parts requiring skilled machining so it wasn't too bad. We had it operating in the fall, maybe earlier—I don't know the exact date. We had that operating certainly before the end of 1941 on a routine basis. It was there that I came up with the null-detector business, which we never published, because the uranium problem came up at the same time and I thought it was too important to publicize. So, this was quietly put aside and appeared in the uranium instruments during the war, where we used the same trick. The uranium-238 went to a collector which had a slit in it, and uranium-

235 went through the slit to a collector behind. We used the null method of balancing a fraction of the uranium-238 current against the uranium-235 current. And so, we transferred that whole technology to the uranium instruments which were now being built, starting in January of 1942.

GRAYSON: Okay, now you came back from this meeting with the people in Washington, and essentially, was this a planning meeting?

NIER: Yes

GRAYSON: I mean, you had scientists from all...I mean, were they physicists or...

NIER: I don't remember

GRAYSON: But basically you had a fairly large group of prestigious [scientists].

NIER: Some dozens. I was a nobody in that crowd.

GRAYSON: [laughter] That's hard to believe. But anyway, and what was the format? I mean did you just sit around and talk about the problem?

NIER: Well, they talked about a lot of problems. And there was a lot of side conversations because not everybody was let in on everything. For example, they were talking then about uranium reactors for making plutonium.

[END OF AUDIO, FILE 2.2]

GRAYSON: We're starting Side B of Tape 4. There were a number of people, then at this meeting in Washington who knew different things about what was happening.

NIER: Yes. I don't remember the exact connection between my getting the contract to build more machines and that meeting. But certainly, it was related. And of course, from a technical standpoint I dealt entirely with Urey from that point on. We dealt with the administrative office at the Office of Scientific Research and Development on the contractual side. I more or less lost contact with other people then, except for John Dunning.

GRAYSON: Then you came back here and very shortly started working on these machines.

NIER: We began building four of them immediately.

GRAYSON: 60 degree sectors for doing the uranium?

NIER: Uranium, that's right. Also, we still had the only facilities for making uranium analyses so we carried on the analysis. There was centrifuge work going on at [University of] Virginia by Jesse [W.] Beams and his colleagues. There was the work at Columbia—the diffusion studies through membranes. There were other experiments going on in some other places. There was Phil [Philip H.] Abelson, the guy who became editor of *Science*. He was then working at the Department of Terrestrial Magnetism but I think he also had some connection with the [U.S.] Navy in Philadelphia [Pennsylvania]. They set up a liquid thermal diffusion column there using liquid UF₆.

So, there were at least these three programs directly involved with the uranium separation. Quite independent of all this, the development was going on at Berkeley on the Calutron but they used radioactive counting methods so I no longer interacted with them. I never saw any of them again. I knew Lawrence quite well. We were friends, but I just had no contact with them at that time. So, my contacts then were exclusively with either Urey officially, or John Dunning officially and unofficially.

GRAYSON: Now the next generation of 60 degree sector machines, had some modifications to analyze uranium because, as we mentioned earlier, the first generation didn't do as well.

NIER: Well, we had bigger magnets than I had for the original one, the 1940 one. Bigger magnets were employed and we used stabilized power supplies, electronic ones: that time we could build these things; you had to wind very fine wire, miles of it. Remember we used high voltage with vacuum tube regulators which were high voltage, low current devices; unlike the solid state things these days. We could get pretty decent fields. The magnets were heavier, everything was beefed up. But it was still a basic 60 degree machine and we perfected the manufacture of the spectrometer tubes so that the Kovar seals no longer cracked most of the time, and so on.

GRAYSON: Again, your electrical engineering background probably came in handy for applying the latest and greatest electrical techniques.

NIER: Undoubtedly because I kept up with electronic things of the times.

GRAYSON: Okay. Then, now, basically, this was a contract of four instruments?

NIER: Yes.

GRAYSON: And having these four machines, what happened to them?

NIER: I got to know Arthur [H.] Compton, who was at Chicago at the head of what was called the Metallurgical Lab. And I knew all the people who were in charge of things. I knew [J. Robert] Oppenheimer for instance too. The decision was made some place, and I'm sure Compton had a lot to do with it, that they would send me, Mark [G.] Inghram, I-N-G-H-R-A-M, who was a graduate student of [Arthur J.] Dempster's at Chicago who was getting started in mass spectrometry. They would send him to me to help us with our uranium analysis. He and Ed [Edward P.] Ney, N-E-Y, Edward Ney, who was an undergraduate, they did the bulk of the uranium analysis for the country. Inghram, a graduate, a beginning graduate student, and Ney an undergraduate.

GRAYSON: This was with those four instruments?

NIER: Well, with one of them, plus the old 180 degree one. Four weren't built yet. But they were being built. Gradually, by the end of spring, we had built at least two and maybe three of the four. And we were doing analysis then by that time routinely on one of the 60 degree ones, the 180 was no longer used. We worked out the whole manifolding system and everything was more convenient, it was designed to do the job. So, that was the first of the instruments where it was a self-contained package. It had a frame of its own and I have pictures of that too, so you can see that.

Well by the summer of 1942, Ney graduated, but he had been working for me a couple of years and was the best experimental student in the department including all the graduate students. He was just a natural. He's a full professor in our department, a very distinguished guy now. So, the question is what do we do. The decision was to send him to the University of Virginia with two instruments to help on the centrifuging work which looked very promising but wasn't pushed as hard at Virginia as it might have been. They didn't quite push it. Two of the instruments with Mark Inghram went to Columbia, and set up in the physics department as part of that program. So, he was in charge of the mass spectrometers at Columbia and did all the UF₆ analyses at Columbia.

GRAYSON: So, these gentlemen were in on the building of the instruments?

NIER: Of the instruments, yes. They worked part-time building, part-time running analyses. That's right.

GRAYSON: They had a full knowledge of what needed to be done to make it work correctly

NIER: That's right, they knew it from A to Z. It was during this period, sometime in 1942, that the critical experiments were performed at Columbia determining that the diffusion method indeed was promising. I think they had either a 6- or 12-stage system. Their little barriers were little disks about the size of a penny or so they had developed—Dunning was in this with everything he had, plus a very good staff of people. They had even developed pumps for pumping UF₆. These used slyphon bellows for seals. UF₆ is a very corrosive compound. You had to have an all metal system, but not any old metal—it had to be Monel or nickel. They had built pumps that could pump UF₆ up to pressure. They had been working on valves that you could use. You couldn't buy valves that didn't have string packing; I take that back, refrigerator valves didn't have string packing, but they had brass housings which were terrible things for UF₆. You couldn't even buy miniature valves that were any good. At Columbia they had some very good people in the physics group working on uranium separation with Dunning as the leader.

Until the middle of 1942 we made all of their isotope analyses. I wish I still had the telegram which I got from Gene Booth after they had sent us some critical samples. They never told us which samples were critical so as not to prejudice us. I got this wonderful telegram from Gene saying that either I could read minds or we did a good job. It was to tell me that everything was as it was supposed to be. The measurement confirmed that the diffusion method was performing as hoped for and could be developed further. We had built four machines. I sent two to Columbia and two to Virginia.

In the meantime we got a contract for three more. At that point I was going to fold up things here and move to Columbia to help out on their activities, but their machine shop was already booked to capacity and I couldn't live without a machine shop if I was to develop new instruments. What was I to do? So, I stayed in New York for two weeks, I think it was, and finally got frustrated. I talked to Urey and Dunning and said, "Gee, I could do so much more good if you let me go back home again, where I have a machine shop that I can do anything I want and with people to do things. We need more instruments, so why don't you let me go home?" So, I came home. Stayed another whole year. That was a very productive year. We were out of the uranium analysis business then and worked exclusively on the development and construction of new instruments.

GRAYSON: Okay.

NIER: We built three more uranium instruments. We then built ten heavy hydrogen instruments for doing HD [Hydrogen-Deuterium] analysis. These were all glass with magnets mounted inside the glass housing. I now have one which Charlie Stevens discovered at Argonne [National Laboratory]. (Figures 15, 16) It was a leftover tube. They were going to throw it away. It used to be here in my office. I put it in our storeroom. We built these for HD analysis. They were a lot simpler than the uranium instruments. They had permanent magnets, which were small since you were doing hydrogen. (Figures 17, 18, 19) We had only 3- or 4-hundred volts accelerating voltage; for stabilized voltage we used the drop across VR tubes. These were glow discharge tubes that would give you a fairly steady voltage. By that time we had emission regulators—automatic emission regulators—that used the emission current to tell the filament how hot it should get to keep it steady.

There had been something published on an ion-gauge regulator by Louis [N.] Ridenour, a physicist at Princeton, in *The Review of Scientific Instruments* during the late 1930s or early 1940s.¹⁹ We adapted the circuit for use on a mass spectrometer filament. Before that we used a storage battery and a heavy slide wire resistor for controlling the filament current—one of these things that you bought from Central Scientific. Because you had to pass 5 amperes through the filament. You had to use some care in adjusting the current. We had the first stabilized emission regulators on mass spectrometers, and that was standard equipment on all our stuff here. That was the only tricky thing we had in the spectrometer. But anyhow, we built about ten hydrogen instruments. Three went to Trail, British Columbia. You had to take the train or plane to Spokane [Washington] then you took a bus the rest of the way. It was like going to Shangri-La on a little road that hung on the side of a mountain. I remember because I did this a number of times. But, anyhow, I went up there myself and set up three instruments there.

GRAYSON: This was for deuterium determination?

NIER: Deuterium. They had a heavy water plant there. Three instruments went to Morgantown, Virginia where DuPont [E. I. du Pont de Nemours and Company] had a plant. And three more went down to Indiana where somebody had a plant. I don't know if I ever went to that plant, but I did go to Morgantown.

GRAYSON: What was the interest in deuterium?

NIER: Well, they thought heavy water would be part of the reacting business, the bomb business, which turned out not to be. But that was one of the things being pursued.

¹⁹ Louis N. Ridenour, "'Magic Eye' Ionization Gauge," *Review of Scientific Instruments* 12 (1941): 134-6.

GRAYSON: Yes, at that time, anything that looked like it might have a possible application...

NIER: Yes, was being pushed. Like isotopes.

GRAYSON: [laughter] Yeah.

NIER: Then, at the same time there was this other interesting development. I travelled quite often to New York. I went to see how Inghram was getting along, to visit Urey because I was supposed to report to him.

GRAYSON: Were you still being paid by the university

NIER: I was on a regular university appointment but OSRD paid my salary. They reimbursed the University. I just got my regular salary.

GRAYSON: But you were able to use the facilities here.

NIER: Yes, and they may have paid some overhead even. This was before the days that overhead was such an important thing. You don't even use the word overhead anymore, it's "indirect costs," you know. [laughter] On second thought, I think there probably was no overhead. While all of this was going on I was relieved of all teaching.

GRAYSON: I see, what year were you relieved of teaching?

NIER: Starting in the summer of 1942, for sure.

GRAYSON: So, the word was...

NIER: Maybe even earlier than that.

GRAYSON: ...that you were supposed to work on these problems.

NIER: Yes. And my labs, then, had special keys, not everybody could get in...only the people who had business there. In the midst of all this construction, I was going back and forth to New York and I knew Dunning very well because after all, we had helped them on their method. By coincidence, a very close friend of mine, Manson Benedict—whose name became very important later on—was hired by the M. W. Kellogg Company. He was a very, very good chemical engineer and was part of our gang at Harvard. He was a post-doc as I was, but working in geophysics, and subsequently he was hired by the M. W. Kellogg Company as a process engineer. You see, Kellogg was a company that built oil refineries and power plants and other industrial plants. When the decision was made to go the diffusion route as one of the ways for separating uranium, and Kellogg was given the contract, he ended up as the chief process engineer.

He in turn hired another old colleague from Harvard days, a guy by the name of Bob Jacobs, who had shared a lab with me at Harvard. He then was working on high pressure phenomena with Bridgeman. We all knew each other well. He was given The responsibility for getting the big diffusion plant uranium tight. Getting the plant vacuum tight was important because, if the thing leaked, the UF₆ would plug the barriers and that'd be "bye-bye plant."

GRAYSON: I hate to interrupt, but UF₆, you say, would plug all the barriers if there was a leak in the vacuum system. I think we need to expand on that some.

NIER: Well, if water from the atmosphere got in, as was very likely if there was leakage, it would react and form UF₄ which would plug the holes in the barriers.

GRAYSON: Okay, so the important point is that this plant, whatever size and however operative, had to be held under vacuum. There could be no leaks.

NIER: That's right. It ran at below atmospheric pressure, I've forgotten the exact amount. It was 1/5 of an atmosphere or thereabouts. So, air could leak in if there were leaks. Everything had to be tight; the welds, and people didn't know how to make welded joints that were really tight in those days. At least it wasn't of concern for steam applications. A little bit of seeping didn't matter for steam. What is more, because of the corrosion problem, stainless steel, nickel and Monel were used. Those materials were hard to weld. There was a whole lot of technology that had to be learned. And then there were many thousands of valves in the plant, all with welded bellows; because you couldn't use packed valves. These were made out of Monel, with welded bellows in them, and so on. This all had to be vacuum tight.

GRAYSON: Not so much that you had to get that good of a vacuum, it's just that you had to keep any possibility of water out.

NIER: That's correct.

GRAYSON: You could probably get by with a little bit of air in there.

NIER: That's right, yes.

GRAYSON: That wasn't a problem.

NIER: That's right. In fact dry nitrogen was let in. In operation of the plant, UF₆ was forced through porous metal tubes and the pressure dropped. You had to pump it up again for the next stage. They had big centrifugal blowers for pumps and how do you run something with a rotating shaft that's 4 inches in diameter into a vacuum-type system? So, they had seals which they had perfected with carefully honed surfaces which used a film of dry nitrogen as a lubricant. So there was slight leakage of very dry nitrogen into the plant.

GRAYSON: You had to keep the water out.

NIER: You had to keep the water out. Also you worried about other leakages as well... various refrigerants for example. Because of the pumping, the UF₆ got hot so you had to have all kinds of refrigeration equipment to cool the gases. Also because of the size and complexity of the plant you had miles of welded joints. Then, the question came up, "How can you find leaks if you're going to have miles of welded pipes and joints and couplings?" There must have been... maybe a million joints is too many, but the number of joints that had to be welded was way, way up in the tens or hundreds of thousands. So, how do you test all this stuff? Well, you had to find some way to do it. You could use the usual stunt... you could pump down and have an ionization gauge and read the pressure; or you let it stand and watch the pressure rise with a bourdon gauge. Well, these were all far too crude. So, in kicking this around, and I don't know exactly whose idea it was, but in one of many brain-storming sessions the question came up, "Could you use a mass spectrometer as a leak detector?"

GRAYSON: Was this down at Oak Ridge?

NIER: No, this is all up in New York.

GRAYSON: All up in New York, okay.

NIER: I'd never been there, to Oak Ridge at this point. We're talking now, 1942, when I had more time, since we were no longer doing the routine analysis. And, so, when the question came up it appeared feasible to develop an instrument for the purpose. In practice you used the leak detector as a sophisticated ionization gauge. You tuned it to helium. There isn't much helium in the air and you could sniff around with helium. (Figure 20) Well, at the time, we were building the hydrogen instruments, which were for light masses. So, it was simple for us to adopt a hydrogen instrument for the purpose. In a month or so we threw together a portable instrument (Figures 21, 22, 23) that could do analysis. Now this first instrument had a glass tube. We played with it and showed that you could detect very small leaks this way. We built four such portable instruments, is my recollection. We shipped a couple of them to Columbia, where they were doing all the pilot plant testing so they could experiment with the procedure. There were people on the job doing all kinds of pilot plant studies. The decision was that the instruments should go into production. (Figures 24, 25, 26) General Electric received the contract for the construction. This followed shortly after the decision was made to pursue the gas diffusion method of isotope separation on a large scale.

GRAYSON: This was in 1942, or sometime?

NIER: Late, sometime in 1942, yes. Everybody was concerned; how can we help K-25—that was the name of the plant, K-25. How can we help K-25? The feeling was they had to have many hundreds of helium leak detectors. GE had the contract, a blanket contract for instruments of that kind. So, they got the contract for building helium leak detectors. Now before that, they had received the contract for building uranium analysis mass spectrometers. I should have mentioned that, after we built those first ones, one of the seven we built went to GE as a prototype.

GRAYSON: Okay, did you just ship the instrument there or did you supply drawings of the circuits?

NIER: Yes.

GRAYSON: Did you supply mechanical drawings of the components?

NIER: Yes, drawings as well as a prototype instrument.

GRAYSON: Basically, you were in an instrument development research design mode at that time.

NIER: That is correct, developing and building prototypes.

GRAYSON: Prototypes.

NIER: Now this period overlapped my moving to New York. By the summer of 1943 in Minnesota we had built seven uranium instruments, about ten hydrogen instruments, and four leak detectors. We may have built a few other things in addition that I have forgotten about now. A uranium instrument went to GE as a prototype and they went into production on those and built, I don't know how many dozens of them for the project. One helium leak detector went to GE to serve as a prototype for the hundreds to be built. Following this, the question arose, what should I do from here on? We're now to the summer of 1943. I was faced with a dilemma, what would be the best thing for me to do. Lawrence wanted me to come out and help at Berkeley, Oppenheimer called me to ask me to come to Los Alamos which was then being set up. Compton raised the question, "Wouldn't you like to come to Chicago?" Nobody put any real pressure on me, but in talking it over with Urey I thought the most good I could do would be to work for Kellogg, the Kellogg subsidiary, which had the big responsibility for building the Oak Ridge gaseous diffusion plant. There were a lot of potential analytical problems in this plant.

GRAYSON: Was it known at that time that diffusion was the answer?

NIER: Centrifuging had been dropped, they had not gotten far enough by the time a decision had to be made.

GRAYSON: Okay.

NIER: It may have been the superior method, but it hadn't been demonstrated that you could do it, whereas the Columbia people had demonstrated that the diffusion method could be scaled up and produce substantial amounts of concentrated U-234.

GRAYSON: And the idea of electro-magnetic separation...?

NIER: Ernest Lawrence was pushing that full-blast.

GRAYSON: So, he was still pushing that at the time.

NIER: And it was being developed full-blast.

GRAYSON: Would it be fair to say the government had decided to pursue both of those?

NIER: Yes, that is correct.

GRAYSON: But you elected to cast your lot with the diffusion method. I think it's an interesting area, an important area. Your natural background would be, to a certain degree, with the magnetic separation work.

NIER: Yes. But I thought that was hopeless for a big production thing.

GRAYSON: Okay, in your own mind even though you knew that that was a good technique, you didn't think it was going to be able to produce the quantities necessary.

NIER: Certainly not large quantities. But remember, the first bomb was made that way. They got enough for one bomb.

GRAYSON: Yes. So then what you did decide at that time was that your talents and abilities would be better off spent working to support the gaseous diffusion plant.

NIER: Yes, that is correct.

GRAYSON: Even though your own native background lent itself to magnetic separation.

NIER: But the diffusion plant offered a tremendous challenge. There were real problems ahead. [laughter]

GRAYSON: Yes.

NIER: Now this was a very practical kind of thing, you see. I got out of science at that point. We were all out of science. I don't know if it would have been any different if I'd gone to Los Alamos. Maybe I would have worked on exotic things.

GRAYSON: It would have been more scientific?

NIER: Much more scientific. The other people came out of that experience pursuing science because Los Alamos was run like a high-powered university thanks to Oppenheimer.

GRAYSON: Whereas the problems that you were dealing with...

NIER: Were very engineering.

GRAYSON: ...were much more engineering oriented.

NIER: Very much engineering.

GRAYSON: And more mundane? But the end to which they would be applied was effective.

NIER: I think what I did was more important to the effort.

GRAYSON: Okay.

NIER: In reaching the decision you can't forget the personal angle. I was a good friend of John Dunning's. He had been very generous with me in giving me credit for demonstrating the first fission property of U-235. My name was first on the paper announcing the result.²⁰ There were four of us that were on that paper. Also, I knew Manson Benedict of Kellogg who was a very dear friend of mine. I also knew Bob Jacobs. And a very large problem was looming for the diffusion plant. That was how to monitor the performance of the entire plant for all the

²⁰ Alfred O. Nier, E.T. Booth, and J.R. Dunning, "Nuclear Fission of Separated Uranium Isotopes," *Physical Review* 57 (1940): 546.

impurities, all the refrigerants, and every other damn thing that could leak into the process stream. So, it looked like a challenging problem to monitor a whole huge plant like this for all the crazy stuff that can happen to it. Dunning really doesn't get the credit he deserved, because he had the imagination to foresee all the problems that had to be solved to make the plant successful. "Gee why don't you monitor the thing with a mass spectrometer?" I don't know who said this first, but certainly, Dunning was all in favor of it; so that was to be my big assignment when I went to work for Kellex in 1943. I took a leave of absence from the university. I'd been on the university payroll—to be sure paid by the government but on the university payroll. So, in the summer of 1943, I entered the commercial world as an employee of Kellex, it was a subsidiary of M. W. Kellogg set up to build the diffusion plant. So, I moved to New York City.

GRAYSON: This is the second time you went to New York?

NIER: The second time I went to New York. This is now in August of 1943.

GRAYSON: But you also took along your machine-shop man with you didn't you?

NIER: Yes, I had my shop which was needed for further development. I had with me several of the students who had worked with me. But I had lost my best and most experienced people. Both Inghram and Ney were gone. But I had several other people who were coming along, including Charlie [Charles A.] Stephens who is now at Argonne Lab. He's done very well, he was one of my good people then. He worked on the development of the hydrogen instruments that were sent to the various hydrogen plants during the war. He came with me as did [Wallace T.] Wally Leland and Donald [L.] Drukey, two other undergraduate students.

GRAYSON: So, there were a number of people from here that went to Kellex and...

NIER: Including our electronics man who'd developed and built our electronic units at Minnesota. He headed up an electronics shop for me at Kellex and Thorness, headed up the machine shop. We had a nucleus to start with. We got started in the fall of 1943. Nothing happened for a while, it takes time to get started. We didn't have tools and we didn't have this and we didn't have that, so I spent a lot of time in the library at Columbia trying to figure out how to analyze all the things they want us to look for. And I was the last one in the world that wanted to use a large number of mass spectrometers to monitor the plant. I thought of the complexity of trying to keep dozens of these things going all at once. It seemed very formidable. And the project manager...is this the end of it?

GRAYSON: Yes, why don't we stop here, because this is a good place to stop, and we'll pick up on the next tape.

[END OF AUDIO, FILE 2.3]

GRAYSON: We had just left off with the discussion of...this is worse than bridge, nobody can remember who dealt last, but you can remember the play of the last four hands. [laughter]

NIER: Well, as I was saying, I went down to New York, and we were in a building called the Nash Building. It had been the Nash Motor Company's warehouse for cars in New York City. It was just above Columbia University at 133rd and Broadway. Kellex had several floors in the building; and the group at Columbia that pursued the diffusion-related problems were on other floors of the building. So, we saw the Columbia people all the time. But we had several floors of our own there. And I had half of a floor of that building, for our development group. When I first went there, I had time, because I arrived in August and it was some weeks before we could actually get started doing things. I spent a lot of time in the library, trying to figure out ways which could avoid using mass spectrometers.

GRAYSON: You knew too much about the instrument. At that time, running one was more of an art than a science.

NIER: [laughter] That's right. Finally, the Project Manager and the right-hand man to the president of the company entered the act. He was an electrical engineer by background and was intrigued with the idea of a mass spectrometer doing things. He thought it was just wonderful. So, in one of our conversations, I was told him about all the things I was trying to do to avoid using mass spectrometers. I don't know the exact words he used, but the effect was, "Look, we hired you to do something about mass spectrometers, and here you're trying to get away from them!" It was something like that. [laughter]

And so, he became one of my great fans. Al Baker was his name. And I suppose his background was designing power plants for oil refineries and other large installations. But he was very interested in electronic devices. Anyhow, the decision was made at that point that we should analyze the process stream with mass spectrometers. Now, there were conversations between other people, of course. There would be around fifty sections in the large building, which was a huge, U-shaped structure—one-half mile from end to end. There were fifty different sections which could be isolated from one another. I don't remember how many diffusion stages there were in each section; probably dozens of stages. And, you could isolate these different sections, in case something went wrong. There was to be a mass spectrometer in each one of these. (Figure 27) There were, I think it was something like fifty-four locations where we were going to put mass spectrometers, and each one would be backed up by a

duplicate. It was so important that there was a spare instrument. There were thus one hundred and eight mass spectrometers set up there. We called them Line Recorders since they made a continuous on-line analysis of the process stream.

GRAYSON: We knew that mass spectrometers were difficult beasts, so we put two of them there. [laughter]

NIER: Later, after the war, and after they got going, they found that they didn't need nearly that many, but at that time, we didn't know how closely the process stream had to be monitored. The spectrometers were all pumped with glass-mercury pumps. The spectrometer tubes themselves were metal. The instruments only needed to go up to mass 69. Why 69? CF_3 . Because they used a lot of fluorocarbons in the plant. This was the first large scale use of fluorocarbons as refrigerants, lubricants, and whatnot. The CF_3 , just like the CH_3^+ is an important ion. If you have hydrocarbons, you have a CH_3^+ so we'd have CF_3^+ That's all the heavier we had to go really. We didn't attempt to do the uranium on them. In fact, we didn't want the uranium in the instruments. We took the uranium out. When we let the gas in, in fifty-four locations each with its own instrument, we passed the gas over a little pool of mercury, whose temperature was controlled. [laughter] The UF_6 reacted...I won't say violently, but decisively...with the mercury, it fell dead. And the other, the impurities in there, which were inert gases, were not affected and went into the spectrometer. When UF_6 is admitted to an instrument molecules are adsorbed on surfaces. When the electron beam in the source struck a surface, the UF_6 decomposed to UF_4 , an insulator. So, the uranium analysis instruments had to be turned off, taken apart, and the sources cleaned regularly. It was part of the ritual in running them.

At Oak Ridge, you had dozens of uranium isotope instruments in the diffusion plant for analyzing the products of the plant; both in the diffusion plant and in the electromagnetic plant, which was being built in the next valley. You had helium leak detectors all over the place. Some were on the foundry floor at Crane's Plumbing to check valve castings. Others were in factories all over the country to check other products, plus many dozens at Oak Ridge, for checking the plant as it was being built. The Line Recorders were located in fifty-four places in the diffusion plant. The problem of monitoring continuously in so many places was a bit of a project, and the guy who was my immediate boss, fellow by the name of Thomas Abbott, was an engineer from GE. He had been a superintendent at one of their plants, and understood instrumentation of all kinds; he felt we ought to have a central control room. At each Line Recorder we had a strip chart recorder with a slave run off of it in the central control room. (Figure 28) You had fifty-four recorders in a room, around the walls, so at a glance a single operator could see the composition of the process stream at every place throughout the plant. Now, we had to freeze the design on all of this in 1943. This was before the days of electronic recorders. So, the best thing we could do was to buy recorders from Leeds & Northrup which employed galvanometers as sensing units. When the galvanometers got off balance, there was a mechanism which would turn a slide-wire to indicate the balance. Also these were multi-point recorders. So, we could record...I've forgotten whether it was eight or twelve different signals

selected by a commutator. We performed peak-stepping on the spectra, and picked out the things to see. We recorded oxygen, nitrogen, HF CF₃ and a few other ions. And that's the way the plant was monitored continuously. This became very routine, to be sure. There were terrible maintenance problems, but the technicians got pretty good at this thing; and the instruments were actually pretty reliable. Now, there was one interesting incident . . .

GRAYSON: Now, was there an individual responsible for each pair of instruments?

NIER: Yes, there was an instrument department that [Union] Carbide [Corporation] had which maintained and operated the instruments. There were some interesting unpublished stories in connection with that whole operation. One refers to the period when the original leak detectors were put into operation. Since our use of helium leak detectors was a pioneering effort, we said "Well, they're sort of experimental." The people in the instrumentation section of Carbide, looked upon this as a license to try to rebuild the instruments themselves. [laughter] And they had a fiendish time with the maintenance of the leak detectors, because they got pretty rough treatment. They'd lose vacuum, burn out filaments, and all kinds of things. So, you had hot-shots in the instrument department who thought it would help if you replaced the battery-operated amplifiers by electronically-run ones. And you ran into ground loops, and all kinds of electronic problems. We designed the amplifiers to run off of a six volt automobile storage battery and two 45-volt radio-B batteries, because it was felt this ensured reliability. You changed the batteries regularly, and there was no problem. It was a nuisance, but it was reliable. Well, the hot-shots wanted to replace all of this stuff. Then they decided there ought to have been more stages in the feedback amplifiers; and then they ran into problems with the feedback. Things were just going to hell on wheels.

Tom Abbott and I always traveled together, he as a manager-executive-type, and I served as a technical consultant on our visits to Oak Ridge. We had long talks about the maintenance problems and finally decided we would go to the management of Carbide and say they should get rid of the people who headed up the instrument department, and put in someone who would follow rules. There was terrible soul-searching on this, but we persuaded them that the two high-level people responsible, should not be in charge of maintenance of instruments. They should move them to some place where they could do their inventing, and get somebody who was a bit more pedestrian and who would follow instructions and be faithful, and so on. The management of Carbide was just aghast that we'd make a suggestion of this kind. But they followed it and the down-time dropped from 50 percent to three percent, or something like that. The leak detectors were more reliable than your automobile after that. So, that was just one of the little side things that came along.

All of the mass spectrometer instruments worked pretty well. We also developed a number of other instruments. We had chemical work going on in our lab. We had to work closely with GE who built production instruments based on our prototype. This was very interesting. We often disagreed with the GE people, but it was never on a personal basis. We were the best of friends with all of the engineers there, but there were a lot of disagreements.

We would curse each other during the hours of the day, but at the end of the day, they would take us out to dinner and we were all palsy-walsy. Like the lawyers in a court case. They may be on opposite sides, but they go out and have drinks together. So, it was very interesting to see how the people could separate work issues from personal ones. After all, the GE people were accustomed to this; they were accustomed to dealing with customers who had problems, so they knew how to be smooth, and friendly, and so on. We had very good relations with the engineers.

They didn't like a lot of our designs, often for good reasons. In the lab we were getting by with specifications that were marginal. We would use vacuum tubes beyond their ratings, and a lot of similar things. If you had to put the GE label on it, you would not do. So, they were much more conservative in their designs. Some things they sometimes redesigned for the sake of redesigning. But most changes were made for good reasons. So, it worked out very well in the end. We built several instruments that way.

At Kellex, when I went down there, one of the first things we did was make an all-metal version of the leak detector—it had been glass before—and handed them that. And they followed it pretty closely, and it worked very well. And they did a good job on that. One of our responsibilities was to monitor the radioactivity coming out of the stacks of the plant. [laughter]

GRAYSON: You mean, there was an environmental concern?

NIER: Oh, yes, But it may have been more of a security matter, of not letting people know what was coming out. So, we had to monitor the stuff that was coming out of the stacks. So, how did you do this? Well, we were going to use ionization chambers. I knew they were working at Chicago at the Metallurgical Lab with ionization chambers, but we got no cooperation in learning about their designs. This was the compartmentalization business. We had to go back to square one and develop our own ionization chambers, which was probably a good thing. You know the way you do this, you have an of an insulated structure with high voltage on it in a container. Ions are produced by the radioactivity and you measure the ion current. The instrument had to withstand corrosive atmospheres, which I'm sure the Chicago people never had to worry about. So, we had teflon insulators and nickel structures. We started out fresh, without any pre-knowledge of what had been done, and built big chambers, as large as garbage cans. We were trying to measure very low levels of radioactivity—the alpha-particles from UF₆ which might be leaving the stacks was what we were trying to detect. We had those big tanks which had something which looked sort of like a birdcage inside, with a high-voltage on it for collecting ions.

Who was going to build this at GE? Well, it turned out it was the mercury rectifier department that normally built big, power rectifiers for power plants. [laughter] They were low in work at the time, and so it was assigned to them. The man who was in charge was really a top-notch engineer, an older man, who was nearing retirement. Our meetings were interesting because before, we were dealing with mainly these young people, who had to be eager beavers

to prove something or the other. This guy didn't have to prove anything. So, he said, "Tell us, what is it that works right, and what is it you're having problems with, and we'll do what we can to help out." We outlined the status of the thing, and they followed our recommendations exactly, and didn't feel they had to re-invent the wheel. It was a very interesting experience. The devices were very successful because there was no monkeying around.

GRAYSON: I get the impression, quickly going back in time, from 1944 to 1934...you started up essentially three or four, four or five different times, in a way. You started up here with your graduate work, and then you went away to Harvard, and then you came back here, and then you went to Kellex. And each of these moves represented an almost a complete new beginning experimentally.

NIER: That's right. Of course, the mass spectrometer was, sort of, central to the whole thing. And at Kellex, we had various assignments. I had several chemists working who were pursuing other analytical problems along that line. It was an interesting job in the variety of things going on.

GRAYSON: But a reasonable amount of your time was concerned with bringing yourself up to a certain level of performance.

NIER: Oh, yes. I had to learn about ionization chambers for example.

GRAYSON: Right off the bat, you're faced with re-building equipment or spending a reasonable amount of time learning new things.

NIER: Well, we had to develop all kinds of things. One of the devices we developed, long before anybody else had, was for measuring the pressure of a corrosive gas. We could measure the pressure of UF_6 at low pressures, a couple of torr pressure. We had bellows instruments, just like the ones that you buy now. Whatever they're called...they're made by some company who made the pressure gauges with diaphragms that deflect. We had devised a balance gauge with which we could make absolute measurement of pressure. We had a pair of bellows and you had a vacuum in one of the bellows, and sample pressure on the other. We put weights on a scale and in effect "weigh" the pressure. That was part of the development. One of my assistants, Charlie Stevens, whom you may or may not have met worked on measuring low pressure of UF_6 on flow measurements, etc. He is now in the chemistry section at Argonne, and has done a lot of interesting things there. He was one of our undergraduates. He never pursued graduate work, although he was as good as any of our graduate students. He finally ended up at Argonne, in a very responsible job and was very good. At Kellex he worked on all these crazy things.

GRAYSON: The activity associated with that plant represented a consortium of different large companies, General Electric, Kellex, etc.?

NIER: DuPont, Union Carbide, and many instrument companies helped.

GRAYSON: Westinghouse was probably involved as well. You alluded to the fact that in dealing with different companies you ran into different problems. Were some of these companies jealous of the information that they had?

NIER: I don't think so. I think that everybody worked together. They weren't in competition on particular things. They had different assignments. For example, we dealt with the people who made hydraulic instruments. Pneumatic instrumentation is a tremendous field. They'd been using unusual feedback devices before the electronic people had discovered some feedback. You know, it was amazing, the interesting pressure gauges and stuff these guys had developed. What is it? I've forgotten the name of the company...someplace in New York State. And I was amazed to learn about feedback schemes they had: flow meters, precision flow meters, that they knew about long before people applied feedback to electronics. So, there was a lot of interesting things. We dealt with them because we had a lot of pneumatic instruments that we worked with. So, we interacted with a lot of different groups. GE had the prime contract for the electrical instruments for the plant, so that's who we dealt with there. The DuPont people were concerned with some of the plastic, the fluorocarbons and such. It was all different groups.

GRAYSON: Yes. You mentioned teflon. I didn't realize that Teflon was used in this plant, or developed for it.

NIER: Well, Kel-F came out of it. It was called Kel-F because Kellex had something to do with it. And I don't know who they worked with. With DuPont, I suppose.

GRAYSON: Well, DuPont is definitely the fluorinated carbon company.

NIER: Yes, that's right.

GRAYSON: I'd just assumed that teflon had not come about until somewhat later in time.

NIER: Well, maybe the thing you think of is teflon, but there were these compounds that were fore-runners of that, that were being used. I think that the fluoro-carbon business got a real boost as a result of the Manhattan Project, and the need to work with corrosive halogens.

GRAYSON: Because it represented a material that was inert to the corrosive environment.

NIER: That's right.

GRAYSON: And then, of course, for the refrigeration side.

NIER: You had C₈F₁₆, and various compounds of that kind.

GRAYSON: How come Kellogg became involved in the naming of Kel-F? Was that just because the work was done in conjunction with Kellogg?

NIER: They had something to do with the development. I don't know for sure.

GRAYSON: And so, that kind of got put all together. [beeper beeping] This whole thing says that leak detection started—the idea of a helium leak detector—really started in this period. You were very much the father of it.

NIER: Initially, we were the only ones who had leak detectors. What happened was, hundreds of the instruments were distributed among the many vendors of components for the plant. Their existence was supposed to be real secret, but so many people knew about it, it was no longer secret. A lot of the other people got into the act. For instance, the Consolidated [Engineering Corporation] people in Pasadena—Consolidated Electrodynamics, or whatever they call themselves—at the end of the war, started selling leak detectors. GE tried to keep on, after the war, selling them. But neither they nor the Consolidated people had the knowhow or provided the service necessary to employ them. And this led to a couple of guys, who worked for us at Kellogg, a guy by the name of Al Nerken, N-E-R-K-E-N to be exact, and Frank Raible, R-A-I-B-L-E, starting a little company, called Veeco. They sold leak detectors and told people how to use them. It became a very successful company.

GRAYSON: I understand that the University of Minnesota owns or holds the patent on this?²¹

NIER: We had the patent on them, but it was later shown it wasn't worth fighting for. We got royalties for a number of years.

GRAYSON: Oh, you did?

NIER: Yes.

GRAYSON: You actually did get royalties?

NIER: From both Veeco and Consolidated. But then they decided they didn't need to pay us anymore, because it could be established that somebody may have used the principle sometime before. At first it was simpler to pay the royalty...a kind of blackmail in a way. It's a fuzzy-wuzzy area. And, rather than cause trouble, they paid us five percent, or whatever it was. I got a fourth of it, I think, from the University. I got a couple tens of thousands of dollars out of it. And the University got the rest.

GRAYSON: Do you have a copy of that patent anywhere?

NIER: Oh boy. [laughter] Maybe.

GRAYSON: [laughter] Maybe.

NIER: I must have, but I don't have it right here.

GRAYSON: That would be an interesting document. I'm sure we could dig up a copy.

NIER: Yes. But that was one of the things that came out of it. And that was about the only thing that we really got a patent on. The problem was, once we got mixed up with the government, then we no longer had any patent rights. But see, we'd been using the principal well before. But the first actual instruments, the ones with wheels on them and so on, were built

²¹ Alfred O. Nier, "Method and Apparatus for Determining Leaks," U.S. Patent #2,846,199. Issued 25 October 1949.

as part of the government work. As I say, we got a patent, obviously, we got the patent. But it doesn't do you very much good if people really start to contest it.

GRAYSON: Yes. Well, then it becomes a problem with the legal beagles to fight out.

NIER: Yes, it really is. A patent gives you a license to sue somebody.

GRAYSON: Right. Now, I missed that point about whether or not you trained people to run each station where Line Recorders were located.

NIER: Yes. We had people located at each station who knew enough to operate the instruments as "black boxes". Then you had some very good people in the control room. In particular, I remember well one very good young chemical engineer, who was very much on the ball. He was one of the people on the shift. The plant operated twenty-four hours a day, you understand. You had crews on shifts. He was very much on top of the situation. He was very good. We had one incident that isn't in the textbooks where the whole plant was shut down one night in the spring of 1945 through an error. What happened was, that at some important place, something went wrong. A bellows joint or seal broke. A lot of the plant was protected in that you had jackets containing dry nitrogen over practically everything in the plant. But anyhow, something went wrong, and a big leak developed, about halfway through the plant. Well, the instruments in the central control room showed you exactly where it happened. It was just a textbook case. It was the kind of thing you would publish in a textbook to show how the instrument monitoring system worked. And this poor young chemical engineer, screamed bloody murder, telling the powers-that-be "to isolate section such-and-such, this is where the problem is!"

But the people, the old-fashioned engineers who had the ultimate authority thought they knew better how to run plants; they didn't believe him. Their idea was, the way you tell when something goes wrong, was you look at the ammeters that were in the lines that drive the pumps...and if you change the molecular weight of the gas that you're pumping in a centrifugal pump, the load changes on the pump. So, you look at the ammeters and measure the sensitivity of this. Well, the problem never showed up well enough on the ammeters. The result, was the whole plant filled up with air and nitrogen. The whole plant! The spring of 1945. By coincidence, Al Baker, the project manager for Kellex, came to town that morning on the train, and was confronted with the situation. Now, I wasn't in on the details of these operating problems at the plant. I never lived down there. Anyhow, I was just in and out, but I happened to be there too when the accident occurred. [phone ringing]

NIER: I'll take it. Excuse me.

[recording paused]

NIER: Al Baker immediately got to the bottom of the problem and the negligence of the Carbide people who operated the plant. And remember, he was a great fan of the spectrometers. The strip charts off of the recorders showed exactly what had happened. Well, I don't know what happened to those in authority. I don't think any heads rolled, but some were certainly bashed in. After that, the young chemical engineer had a lot more say in how the plant was run. That was one of the things that came out of it. Just that one incident showed why you had to have a sophisticated analysis system.

GRAYSON: Well, the whole idea of instrumenting the plant...

NIER: Yes...

GRAYSON: ...with the mass spectrometers was...

NIER: ...was just this very reason.

GRAYSON: ...was just for that purpose.

NIER: But you see, it was a whole new technology. Keep in mind, this all happened so fast.

GRAYSON: I know, yes.

NIER: And they had all of these old engineers who had been very competent in reading ammeters and the like. But their idea of the way you monitor something was a little out of date. It was a perfectly good way to do things at one time, but it wasn't under those conditions. And they'd never worked with such corrosive stuff before. Luckily, the plant was not damaged, so it resumed production in a few days. That was in spring of 1945, as the war was nearing an end, and they wanted U-235 enriched, and here the plant was shut down. So, it was shut down for a number of days. But luckily, what happens when something like that occurs...you only lose the production during the time it is down. You don't have to build up the concentration gradient in the plant, as you would if you started from scratch. This is like any fractionating system. If you just shut it off, everything picks up where you left off. Within a few days, they had the

plant more or less back again to where it was, and luckily, no permanent damage was done. But that was a very good example, and you won't find this in the textbooks. [laughter]

GRAYSON: Well, that's what we're here for. As you mentioned, a lot happened in a short period of time. Something that occurred to me in looking at this...from 1940 forward, a tremendous amount of work had to be done. From the point where it was known that you had to enrich U-235 until the time when all of this was actually happening. I know you're talking about five years later. When did it really go on line, initially?

NIER: Well, the electromagnetic plant was running ahead of any of these others. That was already producing stuff in 1944. And that started out with normal uranium, so anything you could do to enrich the starting material helped. If you started with double, it was like doubling the size of that plant in the output, because you started at that much of a higher level. So, I'm sure what they were doing was feeding stuff into that plant very early.

GRAYSON: Sort of, piggy-backing between the two plants.

NIER: Piggy-backing...also they had the liquid thermal diffusion plant, which Phil Abelson had been involved in. It was producing material also. Not terribly enriched, but nevertheless of help. I don't remember how enriched the material from this plant got.

[END OF AUDIO, FILE 2.4]

GRAYSON: You were speaking of the thermal diffusion work of Abelson's?

NIER: Yes, I'm sure some of that material went in as feed for the electromagnetic plant, which was called Y-12. That was the code name for it. Also, our gaseous diffusion plant was feeding material, I'm sure. You see, when they got this plant started, they started bit by bit. It wasn't really running full-blast until maybe mid-summer of 1945 or so. Perhaps even as early as late spring. But they certainly had sections running, so they had doubly-enriched or triply or quadruply-enriched stuff, months before. The output of this plant was so huge when it got going, that they could divert large amounts to the electromagnetic plant, and it would never be missed, in effect. This was actually the situation. So, I'm sure a lot of that went into the electromagnetic plant as feed. I don't know the details on that. I'm sure it's no longer a secret. I was not privy to that kind of information, and never bothered to find out.

GRAYSON: As an interesting aside, this morning on the news, I just heard that [Soviet General Secretary] Mikhail Gorbachev has unilaterally decided to cease production of uranium.²²

NIER: Really, I didn't see that. Oh, my gosh!

GRAYSON: He announced it in England, while visiting with the Prime Minister [Margaret Thatcher] and the Queen [Elizabeth II] there.

NIER: Oh boy, how interesting!

GRAYSON: Yes. Well, maybe, we've just completed the cycle in the production of this stuff.

NIER: Well, they've got so damn much of this stuff that it's just awful.

GRAYSON: Yes, that is most definitely true. So, you stayed with this Kellex activity for a number of years...about three, I guess, was it?

NIER: Well, two. A little over two. My job was really done with Kellex in about September. The war had ended. It officially ended about Labor Day.

GRAYSON: That was 1945?

NIER: My job was done. Anything more was too late to affect the war, you see. So, I didn't see any reason for hanging around. People were giving up new efforts. The Kellex engineering effort was shutting down and the plant was turned over to Carbide. Kellex built it, and Carbide was the operator. For years, they had the contract for running it. We worked with the Carbide people, in training them, in helping them, and so on; there was a lot of going back and forth. By now the plant was entirely in their hands. So, I got out in October, 1945. A lot of my guys had already left. The whole operation at the Nash Building was closing down. Everybody was going home, or doing something else. There were lots of jobs around, so people were scrambling everywhere. And I decided to come back to the University at that point, and came back here in the middle of October 1945.

²² Craig R. Whitney, "Gorbachev Plans to Stop Producing Uranium for Arms," *New York Times* 8 April 1989.

GRAYSON: Okay. Before we start on that, I'd like to just explore one idea. In the beginning, when you first were faced with doing the analyses for this plant, you were looking, casting about for some other way to do it, and kind of got put back on the path of mass spectrometry. But, in retrospect, could you say that without mass spectrometry, without that technology that you had provided in your group, that this plant maybe would have been a lot slower coming on stream.

NIER: Well, I think so. The helium leak detector was certainly spectacular. This was orders of magnitude better than other methods for finding leaks. And they had really good guys who applied the technique. My friend, Bob Jacobs, was in charge of that, and, as I said earlier, some of the key people he had were Al Nerken and Frank Raible, who ran the vacuum testing, and of course, trained a lot of people who did a lot of the maintenance and testing. The helium leak detector saved many months, if not years, in getting the plant running. It turns out, the line recorder, which was the big thing we worked on after I went to Kellex, you probably could have gotten by without. You'd have had more incidents like the one that I talked about. But, it was not a life-and-death matter. And of course, the uranium isotope analysis instruments, which we worked on earlier...without them, you wouldn't have known how any of the enrichment plants were behaving. And you had to be able to do this properly. You could have made rough measurements with alpha-particle detectors. The people at Berkeley were doing some of that. You could do rough measurements that way, but you could not do precise ones. The spectrometers really were important in that.

GRAYSON: So, between the helium leak detector technology and the ability to get precise measurements...

NIER: Of the uranium. I think that there was no other way to do it decently.

GRAYSON: It would have been just impossible to do, almost. You're really talking about a significant contribution to that whole effort. And, had you elected to go to Berkeley?

NIER: Someplace else?

GRAYSON: I guess this stuff would've got done eventually.

NIER: Well, it isn't likely it would have been done very well. You know, nobody's indispensable, but there wasn't anybody else in the world who had the experience I had. It was certainly the right place to be, for me to be, because I had the background. Keep in mind, there

weren't many mass spectrometers in the world at that time. We were the only people who could even make measurements of these kinds. True, there were people coming along. Consolidated was manufacturing instruments, 180 degree instruments, which were used in the oil industry. They sold them to big oil companies, where they could do routine analyses of hydrocarbon mixtures in their plants in one percent of the time needed by the old methods of analysis. So, there were spectrometers available. But, the companies that made them...you were supposed to use them in a certain way. It's just like when you buy an instrument now. Unless you use it the way it's made for, it isn't too useful. There weren't many people who had the flexibility that we had, in that if a new problem came up, I said, "Sure, we'll go home and try it." and next week, we'd probably have an answer. And that's the way we lived during that time. There certainly were many clever people who could have done the same thing, but they didn't have the mass spectrometry experience. That was really the unique thing that we had, that other people didn't have, was the combination of experience and ability to develop new instrumentation.

GRAYSON: Okay, I think that's the important point that I wanted to draw out here. This fundamental contribution, which may get overlooked, or may not even be considered to be important by a whole number of other people who are not familiar with this activity, should be brought out. Because of the problem with leaks, and the insensitivity of the leak detection methods of the time and the insensitivity of other methods for measuring isotopes to determine how well the plant was performing...these were fundamental problems that needed a solution for the plant to be successful.

NIER: At that time, it was important. Once you get the plant running, fine, you don't need these sophisticated techniques. To get it started, you need to know what you have. There's really no substitute. And certainly, the uranium mass spectrometers were very important in monitoring the electromagnetic plant as well as the diffusion plant. In the electromagnetic separation plant they used a large number of 180 degree separators. It was a batch process so a large number of analyses were required. A friend of mine, Gus Cameron, Angus Cameron, was in charge of these analyses during the war. He was originally an employee of Eastman Kodak, and was transferred to Tennessee Eastman, which ran the Y-12 facility for the government during the war. He did a superb job. He learned about mass spectrometry at Columbia. I met him there. He learned how to run them from Inghram, who had two of our machines. Soon he was buying dozens of them for the electromagnetic plant in Oak Ridge. You had banks of instruments with people running samples twenty-four hours a day. I think they had twelve instruments in continuous operation. Sometimes the electro-magnetic separation units didn't work right, and you didn't want to mix the product from a malfunctioning unit with another one that was working right. You had all kinds of problems like that. So, the analyses were very important. There is no getting around it.

GRAYSON: Speaking of Columbia at that time, there was a fellow by the name of Vince [Vincent G.] Saltamach? Do you know Vince?

NIER: Yes.

GRAYSON: Could you just give us a little insight as to what he was doing at that time?

NIER: I don't know. I've only met him since. He worked for Ivan Taylor, who was a very good friend of mine. Ivan, by the way, helped out during the war. He was at the Bureau of Standards. He was in our Chemistry department and left Minnesota and went to work at the Bureau of Standards during the war. When we had problems with developing the line recorder, he worked on the problem of taking UF₆ out before it got into the spectrometer. He was a very good chemist and he guided some of these young people that I had. He was a most generous person with his time. He moved to New York to help us out; a wonderful guy and one of my very dear friends.

GRAYSON: Ney and Inghram came through here. What became of them? I did get a chance to look at some of the interview that had been done in 1976. I think you referred to these gentlemen as being very young, responsible people.

NIER: Yes, they were good. [laughter]

GRAYSON: Their responsibilities far outweighed their age. What exactly did they do?

NIER: Well, Inghram was a graduate student at Chicago whose education was interrupted by the war. From 1942 to 1945 he was an employee of Columbia. He was in another building in New York, but I saw him regularly. Following the war, he went back to graduate school and finished up at Chicago, got on the faculty there, and retired last year. A very distinguished person. When the centrifuging business came to an end at Virginia, Ney had more time to pursue graduate work. He did that during this period and got his Ph.D. at Virginia. He was added to the staff there and we lured him away in about 1947. He's been on our faculty ever since. He's now nearing retirement age. And is a very distinguished—the most distinguished person in our department actually. Not everybody agrees on that, but I think I know better.

GRAYSON: So, the responsibility of these young men was devoted to making sure that the analyses were done properly.

NIER: That's correct; and in the development of the instruments. They played an important part in that.

GRAYSON: This problem is important because it was the period when they were still sorting out which of these separation techniques was going to be pursued. They had to provide accurate results.

NIER: That's right. We were the key ones. The samples came to us. And of course, after the summer of 1942, there were instruments at Columbia and Virginia, but until then, the only instruments were here.

GRAYSON: So, with the instruments at the other locations, they were able to do things there.

NIER: That's right.

GRAYSON: After the war ended, you returned to this part of the country. As I recalled from the tape that I viewed, there was a question about your going to Wisconsin at one time. And you had, kind of, gotten talked out of it by Tate. His argument essentially was that he knew what you didn't, perhaps...that you were going to get sucked into the war effort.

NIER: Yes, well, I was at Berkeley during the last two weeks of November 1941 and 4 December, I came back to Minnesota. Now, I always had a terrible tendency to get airsick. Earlier in the Fall, I'd been invited to Wisconsin to give a talk and meet people—the standard visit. You invite people to come and see whether or not you want to offer them a job. The connections there were kind of interesting too. This is related in part to the biology activity. I had provided heavy carbon to a man by the name of Harland [G.] Wood, who is now a biochemist of some note. At the time, he was a post-doc at Iowa State [University], working in the bacteriology department. We had provided him with heavy carbon and did the isotope analysis. He demonstrated a very fundamental thing about how certain bacteria can incorporate inorganic carbon in certain places. It was apparently a real breakthrough. This was known in the biological community. Wisconsin has always had good biochemists, and I think the dean of the Wisconsin Graduate school, Dean [Edwin Broun] Fred, was a bacteriologist by training if I am not mistaken.

The physics people knew me and what I had been doing. So, when there was an opening in the physics department everybody there thought it would be a good idea for me to come down there because it would help their biology program as well as physics. In any event I came back from Berkeley on 4 December, sick as a dog. I got airsick on the trip. It was a DC-3 that went up and down...we had stormy weather. I remember there was a guy on the flight who didn't have a belt on as we came near Salt Lake City [Utah] and he got thrown against the ceiling, and had to be bandaged up. Oh God, I was sick. I came home, and my wife was having the inside

of the house painted! Well, it turns out that the president of the University of Wisconsin was visiting on the campus and he'd said that he wanted to talk to me. I called when I got in, and told him about my flight and that if I had a chance to rest for a little while perhaps we could talk. I hadn't eaten anything for twenty-four hours which didn't help any.

So, I slept for about an hour and ate a bowl of soup. I felt fine, and then I went and interviewed him. Out of that came the offer of a job. But Tate was then head of division six of the National Defense Research Committee which was in charge of anti-submarine warfare. He commuted regularly between New York and Minneapolis. See, he was a wheel in science in this country. He was also dean of the college that we were in, so he was wearing several hats. It was a decisive thing...should I go to Wisconsin which offered me a very attractive job. They had lots of money from the [Wisconsin] Alumni Research Foundation or should I stay here. I really didn't want to leave here, although, Madison [Wisconsin] was not as far as the East and I could still handle my parents' problems.

Also, they had certainly treated me very well here. So I think the clinching thing...in those days people had ethics on this job business and if you accepted a job somewhere, and they held it open for you for a long time, you were on the spot. You were supposed to honor the commitment. And Tate was really a very proper, sort of, person, and he emphasized this. This was on or about 1 January 1942—after Pearl Harbor Day. He said, "Look, you're not going to be doing anything normal for the next few years." It wasn't clear what I was going to do next, but he didn't think I was going to be around here during that time. "If you accept a job to go some place now, five years from now you may not want to go there." He knew the war situation, he knew everything that was going on, he was on top of the whole thing from his involvement with the anti-submarine program. "If you accept a job someplace else now, you will be obligated to take it afterward, but if you stay here, there are no obligations." And I believed him.

GRAYSON: Well, it was a good argument. Shortly after that you ended up leaving here just as he suspected.

NIER: And I came back, picked up where I left off. I was welcomed back and everything. Now there were a lot of people who accepted jobs someplace else during the war and never took them. I can't say that I was solely motivated by the ethical argument but it certainly was a part, and when you add it together with everything else, it was a convincing argument. Tate was so sincere and so honest and so decent. When he said something, you listened. [laughter]

GRAYSON: Like when he said they were doing this research at GE and didn't publish it!

NIER: Yes. [laughter]

GRAYSON: So it was 1945 that you came back to Minnesota.

NIER: I came back in the Fall of 1945.

GRAYSON: And once again you had to start building instruments.

NIER: Yes, and we were in worse shape than we were before. It turned out most other people had contracts with the Atomic Energy Commission and got the instruments that I had built. And we didn't have such a contract. So, we didn't have any instruments. We really had to start from scratch, which was not all bad. But it would have been useful to have some of the components. We picked up where we left off, and I was lucky. I got some money from the Graduate school and I got some money from the Research Corporation which doled out small amounts for people starting out. They were very sympathetic to youngish people. I was still young then! But I had some money to get started and had several very good graduate students at that time. We built an instrument very similar to the uranium instrument. That's when [L. Thomas] Tom Aldrich was a graduate student looking for things to do and we made the first accurate measurements of the He-3/He-4 ratio. (Figure 29) It had been measured at Berkeley with the cyclotron by [Luis] Alvarez and [Robert] Cornog before the war, but they were off by a factor of ten on the relative amounts.²³

GRAYSON: What was the interest in that ratio.

NIER: Well, it was interesting for many reasons. For instance, we showed it varied a great deal in nature. We were able to do that. They had found at Berkeley also that the atmospheric He-3 was about ten times what it was in oil wells. There was this kind of geophysical interest in the ratio. When you have anything that varies that much, it certainly is interesting. Tom Aldrich worked on problems such as that. Initially it was kind of hard to find problems to work on. We didn't have the "in" that people who'd stayed in science had. Inghram had a tremendous advantage because he went back to Chicago which had connections with the Argonne lab. He had access to instruments we had built and was able to start right out. He was still a graduate student, but they had a lot better instrumentation than we had. We then looked for other problems, and the question of the radioactivity of potassium was looked into. It had been shown in the meantime that potassium-40 was the radioactive one. I forgot who, but he used a mass spectrometer to collect the ions with a target at potassium-40 and showed that that was the radioactive isotope. Enough was known about nuclear physics that it decayed into argon and calcium. So, the question was, is this really so? If you have a potassium mineral, does it really have argon in it. So, we worked on that and Tom showed, well sure, if you have

²³ Luis W. Alvarez and Robert Cornog, "He-3 in Helium," *Physical Review* 56 (1939): 379.

potassium in the mineral, you find excess argon-40 in it. And that set the stage for the potassium/argon method of geological dating. We never pursued it beyond that.

GRAYSON: There is an Aldrich chemical house. Is there any relationship between those Aldriches?

NIER: None.

GRAYSON: Okay.

NIER: When he finished here, he went to the physics department at the University of Missouri. Later he got a job at the Department of Terrestrial Magnetism at the Carnegie Foundation in Washington. Merrill [A.] Tuve, the Director, was a very imaginative guy, interested in all kinds of things especially geophysics. He went there working on geophysical problems. He retired a year or two ago.

GRAYSON: There is a period then when you went back to measuring isotope abundances. It seems that there was a switch-over to atomic mass measurement where you are looking at the precise mass. Why did you suddenly make this change?

NIER: The time seemed right for it. We knew how to make electrical measurements. After all, we had a lot of experience. We had built more spectrometers than all the people in the world combined had ever built. All the mass measurements at that time were being done with photographic plates as detectors. They had perfected this very highly—it was beautiful work.

GRAYSON: This was the work that was still a derivative of Bainbridge's work?

NIER: Well, Aston, originally. And then Dempster had an instrument, and later Bainbridge and [E. B.] Jordan had instruments for measuring masses. So, these were some of the mass measurements going on at the time. But somehow or another, the business of putting a photographic plate in a vacuum, adjusting the machine, taking the plate out to see if you were right, and developing the plate, measuring it with a comparator to see where the peaks were and so on; didn't appeal to me! Of course the interest in nuclear physics, and the binding energy of nuclei, the whole question of atomic energy after World War II made it seem like mass determination was a logical thing for us to work on. We decided to go into the precision mass measurement as a part of nuclear physics development of 1945, 1946, 1947. I said, "Gee, there's got to be a better way to do this." The thing that occurred to me was that the people with

photographic plates had to focus ions along a whole focal plane to be useful. But if you had an electrical detection instrument, then you only had to focus at one place. You didn't have to make the compromises the plate people had to make. So, it occurred to me that you ought to be able to soup this business up somehow, taking advantage of the fact that you only had to collect ions at one place.

I had a graduate student by the name of Edgar Johnson, who was the most wonderful, handy guy in manipulating mathematical expressions. He *never* made a mistake. God, I couldn't divide one fraction by another fraction without making a mistake. This guy *never* made a mistake. He was looking for a Master's thesis problem, I said, "Why don't you investigate how we could put together a double-focusing arrangement so we could have energy focusing—velocity focusing—and maybe higher order angle focusing. There ought to be a way to get higher order angle focusing if you only had to focus the ions at one place. So he went to work on this. And within six months or so he came up with all the equations. Remember, this was before the days of computers, even hand calculators. He came up with the equations which demonstrated indeed there was a whole family of geometries that would do this job and do it well.

GRAYSON: Okay. So, he was able to divine that there were a number of different geometries that could be used.

NIER: Yes, well, an infinite number if you wanted to change one degree at a time. So, we went ahead and built an instrument according to his computation. He never participated in that part. He didn't want to go on. He got a Masters degree and he really didn't want to go on in this sort of thing. I got him a job with Harold Urey at Chicago. Urey by that time was at Chicago. So, he worked around the lab there. Urey was always looking for young people to work in the lab as technicians. He must have been bored running mass spectrometers, I can understand this. Johnson wasn't terribly interested in advanced work, which was a tragedy because the guy was bright.

GRAYSON: Its interesting because if you think about the Nier-Johnson geometry and this was work done on a Master's thesis. Probably that affected mass spectrometry more than many, many, many Ph.D. dissertations. It's just a curious fact of life that it worked out that way.

NIER: That's correct. That's correct.

GRAYSON: When it came time to design the instrument . . .

NIER: Well, by that time, he had all the equations ready.

GRAYSON: But the equations were general in form and you would pick the, say for example the magnetic sector radius . . .

NIER: Well, I had a feeling that we ought to stick with the 60 degree magnets of which we had some on hand.

GRAYSON: Okay.

NIER: And instead of the screwy 127 degree electric analyzers where you had to have the exit slit in the field and come out at some other angle we employed a 90 degree analyzer. Johnson worked out the second-order angle aberration for the general sector case. That was a unique contribution which nobody had ever made before. 90 degrees seemed like a good electrostatic analyzer size. The machine shop could build something that was a fourth of a circle. So how do you do it with 90 degrees electric and 60 degrees magnetic?

GRAYSON: So, basically, he had worked out the general case. So, you applied this to your particular angles.

NIER: And he contributed, obviously, to the decision. We came up with an asymmetric geometry because the angular aberrations were not quite equal in the two. But if you had the electric sector analyzer have a different magnification than the magnetic sector analyzer, one could cancel out the angular aberrations. So, you deliberately put aberration in to cancel out another aberration. Well, its just like optical lenses with color compensation. Its the same principle. So we came up with that, and that would have been in about 1949. Johnson had left by that time. Later, he came back to Minnesota Mining and worked on a color printer or some other similar devices. Spent his career there. He's retired now and has moved away. I don't know what's happened to him. I saw him a few times; we were very good friends over the years. But he just didn't want any part of mass spectrometry.

GRAYSON: That's interesting.

NIER: Following the development of a practical instrument, we saw the possibility of doing mass work and we started a systematic program. This would have been about 1949. I had a series of students who worked in this area.

GRAYSON: This was using the Johnson geometry instrument. So, it was a high resolution machine?

NIER: Yes. With the second-order angle focusing we could employ a bigger angle than other people were able to use before. One other thing we put into this original machine; we used a second mass spectrometer tube as a monitor. This is going back to the magnetron at the end of the solenoid concept. What we had was a sector magnet that was big enough that you had a second mass spectrometer tube next to the main one. The second one was just single focusing. You needed double-focusing when you worked with fragment ions because if you were dealing with fragments, you might have kinetic energy ions. You couldn't compare a fragment with the molecular ion and obtain a correct mass measurement. In the monitor mass spectrometer, you had a split collector. If the beam wandered one way or the other, it sent a signal which told the high voltage to correct itself to keep it centered. Since both instruments used a common ion accelerating supply, the trajectories in the double-focusing mass measuring instrument were stabilized.

[END OF AUDIO, FILE 2.5]

GRAYSON: We were talking about a method of regulating the magnetic field in the double-focusing machine.

NIER: The original instruments had a second mass spectrometer tube, which acted as a standard or stabilizer. What was used as a reference was the constancy of the mass that you chose. We used inert gases and so on, which were nice. And you didn't have to have the reference any particular mass, because you could adjust the relative voltages for the two mass spectrometers, so it was easy to find a mass that was appropriate. This arrangement was used in our early measurements, and we used a strip-chart recorder, too. I've forgotten who made the recorder. It had a pen that moved in an arc so that instead of vertical lines across the paper, there were arcs. The graph paper, the strip chart, had curved lines on it. That's the way it was, because these were relatively high-speed recorders that could do the job. We took our measurements right off the paper. You had a pair of peaks at different mass, and you measured from the half-height on the left-side, here, to the half-height here. And then you went to the other side, took the averages of the two, and said, "That must be the center." This gave you the mass difference. The measurement of mass was reduced to a measurement of resistance, because you had changed the accelerating voltage to scan the spectra. So, you had to have precision resistors.

We went to the Bureau of Standards to find out how to get precision resistors. Well, of course, one could go to Leeds & Northrup or a similar company and buy cumbersome things in boxes. This was just as computers were coming in. There was a little company in a loft in New York, near the Medical Center, up on 168th Street. They were upstairs in a warehouse building,

and they made precision resistors there for computers. They cost a fraction of what Leeds & Northrup's charged. What's more, they had special temperature compensators. It was really a small company, but very imaginative. I went there, and talked to the owner of the company. He's one of those enterprising guys who started the business, selling resistors to computer manufacturers. Not that Leeds & Northrup wouldn't have done a good job, but this guy did it cheaper and much more flexible. He wasn't yet in the business where the resistors had to be in special boxes, and stuff like that. You'd just buy resistors from him. So we bought all of our resistors from him. Plus, we had some other precision resistors here. So, we were able to reduce the measurement of mass to a measurement of resistance. So, $\Delta m/m = \Delta R/R$, for the mass measurement.

GRAYSON: Yes.

NIER: And then I had a very fortunate thing happen. A guy by the name of—of all the things—Tom [L.] Collins came as a post-doc. He came from the University of British Columbia. A very, very clever guy. He later was on our faculty for a while. He went on to a research position at Harvard after he left here. He ended up, I think, at the National Accelerator Lab as a top electronics man, if I'm not mistaken. Just awfully good...very, very clever. And, during that period, we worked on a scheme for improving the inverse feedback system we were employing. The problem with inverse feedback is, while it's wonderful, it's not perfect, because it doesn't quite go all the way. You see, you miss the mark by $1/g$, if g is the gain. You don't quite get there, and that wasn't good enough for our measurements, because if g changed, which it is apt to do, your $\Delta m/m$ equals $\Delta R/R$ with the little bugger factor...the bugger factor would change a little bit. This wasn't quite good enough for our measurements. So, I think it was Collins who came up with the idea: "Why don't we have an infinite gain arrangement?" Then the g goes to infinity, and the bugger factor doesn't come in. So, we used the mechanism out of a Brown strip chart recorder. These were electronic recorders which had very high gain amplifiers in them.

GRAYSON: This would have been 19...?

NIER: 1952 or 1953.

GRAYSON: 1950s, okay. We're getting into the 1950s.

NIER: The 1950s now. Maybe even a little later than that...in the early 1950s. In addition to the normal inverse feedback circuitry we used the Brown recorder mechanism to do the final balancing. It was something that you relied on to remove the error signal remaining after the normal feedback circuit had done all it could. So, then we really had $\Delta m/m$ equal to

$\Delta r / r$. We proceeded to make measurements for a period of time that way, and that's the way it went. In the meantime, other people also came along. Another Johnson, Walter Johnson, who was a student of mine, who is now on our faculty came back from GE where he went when he finished up here. He worked on other improvements. And there were other graduate students at the time that I had, a whole series of them: a guy by the name of Tom [T.] Scolman, Karl [S.] Quisenberry, Clayton [F.] Giese, Jay [L.] Benson, and others who were very good at all of this stuff.

In the midst of all this, the multi-channel analyzer became available. You could store a bunch of signals, different things, all at once. We got one of those and made use of it for accumulating data. Lincoln Smith, whose name may or may not mean something to you and mass spectrometry, was interested in the mass business right after the war, also. He had been a graduate student of Bleakney's at Princeton in the 1930s, at the same time I was a graduate student. He'd gone to [University of] Michigan, where he taught for a while, and then came back to Princeton. He was independently wealthy. He was not on the faculty at Princeton but he worked in the Forrestal lab, I think it was called. It was a research lab set on the Princeton campus. He came up with some ideas for some radio frequency mass spectrometers, with very complicated schemes for sending ions around torturous paths. A very clever sort of thing, but terribly cumbersome. He had also been at Brookhaven [National Laboratory] for a while, I guess, and come up with schemes whereby you'd take your mass signal and have it on an oscilloscope screen. You'd have a signal for one peak and one for the other in a mass doublet and would superimpose them by switching techniques; reverse the sign of one of the peaks to get a null signal. He was really the first one to use that scheme; the time had come for us to follow suit. So, we adopted this same thing. You had one signal, and you took the other one, and turned it upside down, and bumped them against each other. If they were exactly matched, you didn't see anything. And if they were a little bit unmatched, you got a funny little wiggle, because it was on one side and it would go like this, and on the other side, it would go like that. You got very skillful at it if your eyes would stand it. Then, that was the way that masses were measured afterwards. You still had the resistance boxes and all of the tricks with this, but you now had a visual scheme, where you looked at the signal on an oscilloscope screen.

GRAYSON: This was the concept of peak-matching?

NIER: Yes, that's right. Now, the Enhancetron entered in, too, because then you could store the data. The Enhancetron was the name of the multi-channel analyzer. You had the multi-channel analyzer take data for you. I was out of it by that time, so Walter Johnson carried on. He had some very good students. If you look at the table of accepted precision masses, the ones that are in the handbooks, you will find they were determined by our students.

GRAYSON: So most of all that work was in the 1950s to 1960s?

NIER: Our early work was done with an instrument having a 6 inch magnet. In the early 1950s we built a larger instrument employing the two ton 180 degree magnet bought for me in 1938. The poles were replaced by 60 degree sectors so we now had a radius of about 18 inches. That was used for a number of years, and they did some very nice work on masses of a lot of elements; the rare-earths, for example. As well as many elements throughout the table.

GRAYSON: So, you weren't an author on a lot of this work.

NIER: Not at all. I'd turned it over to Johnson.

GRAYSON: Okay.

NIER: He'd come back on the faculty, and he and his students carried on.

GRAYSON: Okay, so a great body of detailed information about the accurate masses and a whole series of elements, was done with that equipment here, under Johnson.

NIER: Walter Johnson, not Edgar Johnson.

GRAYSON: Right, yes, Edgar forsook mass spectrometry, much to all of our regrets. So, here you are getting out of mass spectrometry. What happened?

NIER: Well, even before that...remember, I was chairman of the department in those years.

GRAYSON: Okay, well, that's new. I didn't know about the chairmanship.

NIER: I taught classes. I was chairman from 1953 to 1965. Twelve years, and I was acting chairman once before, I think it was thirteen years I was chairman of the department, with a lot of headaches plus teaching classes. Now they relieve chairmen.

GRAYSON: You mean the chairman still had to teach?

NIER: I still had to teach, like everybody else.

GRAYSON: Did you teach the full load, or did they give you some consideration.

NIER: Well, there wasn't much difference. It was a little easier than other people. I only taught a single five-credit course, instead of two three-credit I think.

GRAYSON: I see.

NIER: I finally got it down to a four-credit course, I think it was, for the last couple of years. But I did that for quite a few years.

GRAYSON: Then, with your responsibilities in that regard, did you forsake your research efforts?

NIER: No, I'm a little bit like a three-year-old that has a limited attention span. In 1954, on my first trip to Europe I went to Germany, and I became acquainted with [Josef] Mattauch, we became very good friends. I visited practically every year after that, until his death, I kept up with him.

GRAYSON: Do you have any pictures of him?

NIER: Oh yes.

GRAYSON: Would it be possible to get a copy or arrange to get some?

NIER: Maybe, if I can find it, you can arrange it somehow.

GRAYSON: Okay.

NIER: Yes, they were doing precision mass work, too. And they had various instruments...after all, Mattauch-Herzog, etc.

GRAYSON: Sure.

NIER: So, we were competing with them on the mass work, but he was still hanging on to the photographic business. They had built a super-duper big machine. [Heinrich] Hintenberger, who was his right-hand man, built an instrument which was supposed to have perfect focusing, perfect to the second order. But it had some other serious problems with it, so the machine never really worked.

GRAYSON: Now, is that the Hintenberger of Hintenberger and Koenig that did the whole series of different things.

NIER: Yes. Hintenberger was really good.

GRAYSON: But anyway, the machine didn't really work, though.

NIER: Didn't work, and it never really measured masses. And they were out of it then. So, we did most of the measurements. [Henry E.] Harry Duckworth—at McMaster [University] originally, later he went to Winnipeg, the University of Manitoba—did mass measurements as did Ogata in Japan. Was there anybody else doing it? That was probably it. And those were about the only mass measurements that were made. But you see, what was happening. The shell model of the nucleus was established and by the end of the 1950s, mass measuring didn't look very interesting. I'm sure you could get another decimal point, but there wasn't any real good reason to get another decimal point. If there'd been a tremendous breakthrough in theory, or something, it would be worth getting another decimal point, I guess it would have been worth pursuing. It was getting tough. We got masses out to seven decimal points or thereabouts. But it was getting tough!

GRAYSON: Now, was this all based on a C-12.

NIER: Yes.

GRAYSON: As opposed to...?

NIER: Oxygen-16.

GRAYSON: Were you involved in getting C-12 as the standard?

NIER: Yes, yes.

GRAYSON: Could you give us a little about how that came about?

NIER: Well, there's an article that came out in honor of Beynon's birthday. It is a very good review article that Harry Duckworth and I wrote.²⁴ I have a reprint of that here. There was an issue devoted to that. And we wrote the lead article in this, they put us at the head of the list. Harry is so good at writing, so he wrote most of the article, he corrected all of the things I wrote. [laughter] We communicated between here and Winnipeg. He's retired now, he ended up the president of the University of Winnipeg. He'd always wanted to be president of a University. He was only Vice-President of the University of Manitoba, but he became President at the University of Winnipeg, which was a smaller, private school. But anyhow, Harry's very, very good with writing, just marvelous at it. So, we wrote this together.

I was on the Atomic Weight Commission in the 1950s, late 1950s. The Atomic Weight Commission still exists. They don't do much anymore, but they still exist. And [Edward] Ed Wichers, who was head of the chemistry part of the Bureau of Standards, a very, very good analytical chemist, was chairman of the Commission. I was on it, they wanted to have a mass spectroscopist on it, and several Europeans...Mattauch was on, and some analytical chemists from Europe. I think it was an Englishman and a Norwegian at the time—I've forgotten the exact names, you can look back and see what they were. Anyhow, they were groping around...and this would have been 1955 or 1956, that era...groping with the problem. Actually, several problems. First you had the chemical scale, with the mixture of oxygen as 16. The physical scale, with O-16 and then the oxygen isotopes with varying abundance in nature. So your conversion factor does funny things. So, how can you make this better? Well, the ordinary chemist said "You just define a mixture and call it that, and let it go at that." But Wichers was not satisfied with this. He said we ought to be able to do something better. "Can you find another standard?" Mattauch was interested, and said "It ought to be carbon." Since masses of the elements were made using hydrocarbon fragments for comparison, it made sense to use carbon as a standard rather than oxygen. The matter wasn't really thought through until an international meeting on isotope separation in Amsterdam [Netherlands] in April 1957. My family and I were over there. We brought our American car over, and I had a leave for a quarter plus the summer, so we would have spent five months. I went to the meeting in Amsterdam. We had just arrived in time for it in April, and the Atomic Weight Commission was meeting in Paris [France] later that summer. And, so I saw Mattauch there in Amsterdam. I saw him again in Mainz [Germany], where he was the director of Max Planck Institut für Chemie, and of course, saw him in Paris, later in the summer. I saw him three times that summer, and as I say, we were very close friends. In Amsterdam, we were already talking about the mass standard

²⁴ Alfred O.C. Nier and H.E. Duckworth, "Evolution of the Unified Scale of Atomic Mass, $^{12}\text{C}=12\text{u}$," *International Journal Mass Spectrometry and Ion Processes* 86 (1988): 1-9.

question, since we were on the Commission. It was the problem of what do you do, and Wichers was interested in unifying the scale somehow. Various people made the suggestion that you use flourine, which had only one isotope.

GRAYSON: Selecting a mono-isotopic element certainly has an attraction.

NIER: Fluorine is a poor choice since it is hard to refer to. There was a good reason for choosing oxygen in the first place since it reacted with so many elements. The problem was, do the physicists give up their scale? They already had O-16 equals 16. The chemists had the mixture, and so many atomic weights were based on that. To drop the chemist's scale seemed impossible, from a practical standpoint. So, we had to find some compromise, and this we did. As the result of a conversation which I had with Mattauch—and this is really so—in the bar of the American Hotel in Amsterdam one evening, I pointed out to him “If you chose C-12 equals an even 12, the problem disappears”. By coincidence, the amount that C-12 isotope differed from an integer 12 on the O-16 scale was just about the conversion factor between the physicists' and chemists' scale. So, all you needed to do is have everybody agree on C-12 equals 12. The only thing chemists had to do was to go to their lecture rooms, where you had a chart, a periodic chart on the wall—either painted on the wall, as it usually was in chemistry lecture rooms, or curtains—where it says oxygen equals 16, you just erase that part and write C-12 equals 12.00000. And the chemists don't have to change a damn thing, because the change was so small that it didn't effect any of their procedures that led to all of these combining weights for molecules and so on, which had always been.

For the physicists, it wasn't so convenient. Duckworth and I tell the story in our paper. It's been told in other places, too. By coincidence, the mass measurements, in mass spectrometry, were in a state of flux at the time. There were two separate scales that had sprung up. There was the one done by the mass spectroscopists, in terms of O-16 equals 16. And then there was the one the nuclear physicists had built up based on reaction energies—knowing the energy of alpha particles and beta particles as you went from one element to another. They could build a whole chain of masses based on reaction energies. There was a group at Caltech [California Institute of Technology], who were very active in that. [William Alfred] Willy Fowler was one of them, and there was a young fellow by the name of Ward Whaling, who was in this sort of thing, and they were coming out with precision tables of masses, based on reaction energies. And they didn't agree with the mass spectroscopic ones.

Here we are in 1956. We had a conference in Mainz, a mass spectroscopy conference in honor of Mattauch's sixtieth birthday. And this really came out, because we had the nuclear physicists, who represented their view, and the mass spectroscopists representing their view. It was clear, there was a problem. So, this was all in a state of flux and neither scale was right. But measurements were improving all of the time, and they were coming together. Mattauch had been a Rockefeller fellow. He was an Austrian, you understand...he came from Vienna [Austria]. But in the 1920s, he was, for a couple of years, at Caltech. And he was really a

Texan-American at heart. He was a real flamboyant-type guy, just full of beans. And he knew all of the important people at Caltech from his previous association.

The Atomic Weight Commission met in Paris in August—I think it was 1957 at the time of the IUPAC [International Union of Pure and Applied Chemistry] meeting [Congress and General Assembly]. The possibility of adopting C-12 as a standard came up at the meeting of the Commission. Mattauch, after all, was a wheel. He was the director of one of the Max Planck Institut in Germany, so he was a wheel in Europe. And he knew people who were at the heart of the mass business at Caltech. So he came on a missionary visit to Americas in the spring of 1958, and talked to them about the problem, and they were amenable to something like this. And so, between him and Harry Duckworth, who was kind of the Canadian big wheel on this sort of stuff. He was great on this Commission business and always represented Canada. The problem was worked out. IUPAP, the International Union of Pure and Applied Physics, met, I think, in Canada, about that time. IUPAC, the chemistry group, met someplace else as well. Between these several guys, they sold it to their respective bodies. At that time, you erased oxygen equals 16, and changed it to C-12 equals 12, and it's been that way ever since. So, we no longer can give problems to kids in class on how to convert from one scale to another.

GRAYSON: Yes.

NIER: Except you could give a hypothetical problem: “If you were still a student then, how would you do this?” [laughter] But, I mean, this came about that simply. It was just fortuitous that they were able to work out these problems.

GRAYSON: Yes. It's probably also fortuitous that the one guy, Mattauch, who talked to you, knew the right people...

NIER: That's right. Knew the right people, he knew all the right people, that's correct.

GRAYSON: ...because, without that, I could see a committee forming, and everybody getting bogged down.

NIER: Wichers had a lot of prestige. Ed Wichers, after all, was head of chemistry for the National Bureau of Standards, and he had a lot of prestige. He was nearing the end of his career, he was getting older then—and he was anxious to settle this problem in his lifetime. He was just delighted at the solution. The thing that made me hesitate in the first place, I appreciated that even before, was I said “Gee, the chemists will never accept as a standard, something that you can't put your hands on”. You ought to have pure C-12 in your hands in order to do it. Well, it

didn't really matter, you see; because you knew the relationships well enough. They didn't balk on this, and they were so relieved. As I said, everybody bought it, and that was the end of it.

GRAYSON: Well, I wonder if with today's various machinations, anything like that could have happened.

NIER: Well, there's personalities involved. And, as I say, the people who deserve credit for putting it across were Harry Duckworth in Winnipeg and Mattauch in Germany. I just was a bystander at that point. If there was anything I wasn't interested in, it was to get into that argument.

GRAYSON: So, your measurements were good. It's just that that was an aside to the whole problem of selecting a reference to define the accurate mass of something. Obviously, you have to settle for some standard.

NIER: Yes, and of course, C-12...since we used hydrocarbons for comparison masses for these precision mass...that's where you find something at every mass number. This was very handy, and Mattauch was just delighted with that prospect, you see, because there was a big argument. One of the things we couldn't agree on, was the difference of mass between O-16 and C-12, He-4. That's a doublet that you measured. And there was some disagreement. This way it was actually resolved, both the nuclear measurements and the mass spectroscopic measurements finally came together. But it was fortunate there was this discrepancy at the time, so nobody had an entrenched position.

GRAYSON: Yes.

NIER: This was important.

GRAYSON: You're probably right. I would suspect, that the chemists had the largest entrenched position of any group.

NIER: Yes, you couldn't change that very well.

GRAYSON: And, the insight that you provided.

NIER: And the number of people in physics, who were really interested in the problem of precision masses; it was mainly people like at Caltech and a few other places, who were doing these precision energy measurements to get mass differences and so on, and a handful of mass spectroscopists.

GRAYSON: Well, that work that Johnson was doing...how did he report his results?

NIER: We didn't care. I think at first we reported them both ways.

GRAYSON: I see, okay. Because, it didn't matter, but you had to report it one way or the other, so you just selected both, which probably was maybe the nice way out of the problem.

NIER: Yes at the time...so, you could convert it. It didn't really affect us very much. We found our error. We had a mistake in the measurement of the doublet, and that was part of the discrepancy between the mass spectroscopists and nuclear reaction-based scales.

GRAYSON: We did talk about one or two experiments that didn't work, like, for instance, the one with the UF₆ measurement initially, the one with the thermal column that you were going to try. I'm sure that there were others. Would you care to maybe just talk about some of the more interesting, or less interesting of those that didn't work? [laughter]

NIER: Well, one I remember was when I was a graduate student. I was essentially done, I had my thesis done, essentially a year before I was going to get my degree. And after having found potassium-40, it would have been interesting to try to check into the radioactivity of it. And we had, on the campus, a young instructor, by the name of Donald Hull. Where'd he come from? Well, he was a student of [Willard F.] Bill Libby's in California, and knew all about soft beta-ray counting...such as potassium and stuff. He came here as an instructor, while I was still a graduate student. We thought if we could somehow bugger the isotopic composition of the normal isotopes, we might see which isotope the radioactivity goes with, An experiment had been done by [George de] Hevesy in Denmark years before, where he changed the isotopic composition of potassium by free evaporation. You just evaporate a lot of potassium, and the lighter isotope comes off easier, and finally you're left with a residue enriched in the heavier isotopes. They'd done this and demonstrated—I think the experiment worked—that they get a slight difference in atomic weight. All they could do was measure atomic weight change. Here we were, and we could measure isotopes.

So, I thought, wouldn't it be interesting to do this, and especially since the other guy was an expert on beta-counting. So, a fellow graduate student, a very dear friend of mine, who I had known for years—we were undergraduates together—by the name of Andrew Hustrulid, was

working on something else. And I had free time, so we set up an apparatus where we had what was like a fountain of molten potassium. We had to develop a pump that would pump the potassium, a little iron piston pump. You see potassium doesn't react with iron, so we made the mechanical parts all out of iron. Just like a pump on the farm for water. It would come up and flow over a surface and down, and we had cold walls, so the potassium would stick. So you'd keep it mixed, which was important, and the lighter one would remain close to the surface, so you'd keep evaporating until essentially it disappears. We took what was left, and gave it to the expert in beta counting, and tell him, "Here, measure the activity of this stuff." And you see, we knew the isotopic composition from making isotopic measurements.

[END OF AUDIO, FILE 2.6]

NIER: So, we had produced potassium with altered isotopic composition and Hull was to measure the activity. Well, like everybody else, he had his hands very full. He was a new instructor with a heavy teaching load, and he had trouble getting a lab started. He had other interests, so it was never done. I finished up my degree, and left, and so did Hustrulid.

GRAYSON: And you had this nice sample.

NIER: A nice sample. I still have the residue. I've got a bulb in the cabinet right behind you, I think. I'm sure the safety people don't know about it. A sealed-off bulb of solid potassium that we used—about 250 cc's. I've forgotten, we got about 10 percent enrichment of the K-41 and 5 percent of the K-40 which should have been enough. Our measurements were good enough. The counting measurements were not that easy. You see, these were such soft rays, you had to put the stuff in the counter.

GRAYSON: Oh wow. You had to be right there.

NIER: Yes. But Hull was capable of doing it. You see, he worked with Libby who was the first person to measure radioactivity in tritium, for instance.

GRAYSON: Well, it, kind of, never really got completely finished.

NIER: No, never finished.

GRAYSON: Were there any other kinds of things like that; kind of blind alleys, or skeletons that you care to discuss. [laughter]

NIER: Well, there were lots of them, but I don't recall them all now. There were never a lot of big things that really went wrong. It was usually just sort of a little side thing which wasn't really a failure. It was just never completed. I suppose there were a number of things of that kind that we never finished up. At one time, I had wanted to look for the neutrino, before they found it, and I had some support from the Office of Naval Research. Right after the war, they supported nuclear physics things generally. I had some support, and got a very potent alpha-particle source. The idea was to measure some darn thing or another, measure conservation of momentum, and so on. So, I tried that, and never did very well on it. I never finished up.

GRAYSON: Yes. Like so many things, it was an idea that you explore, and it just doesn't pan out.

NIER: You don't really do anything with it. There were a number of false starts like that. And some of our instruments never worked. You only heard about the ones that worked. But, there were a number of things like that. We, of course, had failures in rocket flights. I got into rocket flights, and had failures there.

GRAYSON: That was a little more spectacular.

NIER: Those are a little more spectacular. I have pictures of some of those rockets, too.

GRAYSON: Did you happen to see that most recent big failure, where this huge rocket took off and did a couple of cartwheels? That was impressive.

NIER: I started flying in about 1960. I decided, "Gee, this is the space age that's coming along, we ought to get into that." The problem of studying the composition of the upper atmosphere was an interesting challenge. So I thought, "Gee, with all of the experience we have, we ought to build mass spectrometers to do this." And so, we built miniature instruments. And I pursued that for quite a long time.

GRAYSON: I'd like to explore that, but we've been at it almost a day. It's getting close to four p.m.. As a matter of fact, I think it is four. Would you want to break now, and maybe call it a day.

NIER: Well, it might be useful to do that, because then we could talk about some of this documentation I have. Maybe I should have a little bit of rest if you're going to go on.

GRAYSON: Sure. Okay, and I would like to explore the business with the other things, and do a lab tour, too.

NIER: Well, we'll pick up tomorrow morning.

GRAYSON: Okay. What would be a good time to start off in the morning?

NIER: Well, nine o'clock?

GRAYSON: Nine o'clock.

NIER: I'm up early, earlier than that, if you wish.

GRAYSON: Well, I had this problem that we would either not get enough material or get too much, and I think I know that we don't have the first problem. I would be willing to meet earlier, but I don't want to wear you out or down. I think we're getting a lot of excellent material.

NIER: Well, there isn't that much more. I mean a little while longer, I think.

GRAYSON: Okay. Well, why don't we just plan on starting at nine.

NIER: Because I think the rocketry is, sort of, interesting, too: Mars. And the various things I've done since.

GRAYSON: Okay, we can start it and then plan for nine in the morning.

NIER: Nine in the morning. I think the building's open. If not, go to one of the front doors, because I think there are classes here, but sometimes they have the side doors locked.

GRAYSON: Okay.

NIER: But if not, I'll look for you, if you can't get in.

GRAYSON: Well, it's a simple walk.

NIER: And come in the front door, fine. I'll make it my business to be here then.

GRAYSON: Okay. Then we'll conclude this part of our activity at this point, and pick up tomorrow.

NIER: Very well.

[END OF AUDIO FILE 2.7]

[END OF INTERVIEW]

INTERVIEWEE: Alfred O.C. Nier

INTERVIEWER: Michael A. Grayson

LOCATION: University of Minnesota
Minneapolis, Minnesota

DATE: 9 April 1989

NIER: [...] Well, do you want me to just list what I have here? [Papers and photos are being examined and discussed in Nier's office.]

GRAYSON: Well, yes, let's just talk about it the way you would as if we were not recording it.

NIER: Well, I have here some reprints, or copies of reprints that I'll give you that are relevant. I have the one on the discovery of potassium-40. And I'll give you the first one. And then, here's a picture that appeared in what is called *The Journal of Applied Physics* of our 1940 instrument. Now, I have better pictures than that. Now let me see what this is here. Then, here's one of my instrument at Harvard, the one that fit in the electromagnet. With this, the first work was done with a number of different elements. On the other side, it tells that.

GRAYSON: Now, was Tate still in charge of the *Physical Review* then?

NIER: Oh yes. Now, let's see how the figures were. [laughter] No, I think we had a lettering set then. Yes, this was done with the lettering set.

GRAYSON: You used a Leroy Lettering Set, right?

NIER: Yes, that was done with that.

GRAYSON: Leroy must have sold a tremendous number of those to physics departments. [laughter]

NIER: They had an engraving machine at Harvard, and they owned a Leroy set, and a guy in the shop on a moonlighting basis would make sets for us. [laughter] And they charged 50 cents

or so for a strip. I have a Leroy set of my own that was home-made. Here's the paper by Edgar Johnson.

GRAYSON: Okay. This is the double-focusing work.

NIER: The double-focusing one, yes.

GRAYSON: Now even though this particular design is used quite a bit in the organic analytical business, you never received anything from that, monetarily.

NIER: No, we never patented that. We should have.

GRAYSON: Yes, I mean in remuneration, in terms of dollars.

NIER: Well, there's another interesting angle on that. Here's a paper that you may not be acquainted with. I was invited to talk to the Bunsen-Gesellschaft in 1954.²⁵ That was the occasion of my being in Germany. I gave a review on isotopic masses and abundances. This is where I presented some of these results. Here is the instrument, with the second spectrometer tube. This was our single-focusing wedge instrument. The significant thing in this whole paper, however, is one sentence in here...which Beynon picked up, "...instrument of this type will undoubtedly prove useful in the field of gas analyses in the future. Molecules having the same mass numbers, but differing in weight by an amount determined only by the difference in binding energy of the nuclear particles, may be clearly resolved as in this example. Extension of the use of this instrument to resolution of heavy hydrocarbons should prove fruitful." [laughter] Beynon read this article.

GRAYSON: Well, it has...proven quite fruitful. [laughter]

NIER: This is where we showed the mass difference between CO₂ and hydrocarbon ions. I talked about the comparison of the scales. See, this was in 1954, before we changed to the C-12 scale. So, that was, kind of, an interesting paper. The history of this, the important thing, is buried there, in that one sentence. [laughter] Here's the paper on common lead, variations in common lead, the first thing we came out with.²⁶ In which we showed that you could consider

²⁵ Alfred O. Nier, "The Determination of Isotopic Masses and Abundances by Mass Spectrometry," *Z. Elektrochem.* 58 (1954): 559; also in *Science* 121 (1955): 737.

²⁶ Alfred O. Nier, "Variations in the Relative Abundances of the Isotopes of Common Lead From Various Sources," *Journal of the American Chemical Society* 60 (1938): 1571.

common lead as a mixture of a primordial, plus varying but equal amounts of uranium and thorium lead. This was the thing, which, as I say, the geologists were gaga about. Here's the original 60 degree instrument. before. And here's the Kovar tube that came all the way up here, and then this is silver soldering on the top to keep it on there.

GRAYSON: I see.

NIER: Here's the magnet, and so on... Here's some uranium lead, from the first lead samples. And then there's the related uranium measurements where the first uranium ratio of 139 to 1 came from. Here's 139 plus or minus one percent. Here's the isotopes of uranium, and the spectrum even shows the isotope U-134, which I gave as having an abundance of 1/17,000 of U-238. They've measured it more accurately since. It's 1 in 16,400 or something like that, but when you consider the difficulty of making the measurement...[laughter] And then, here's the first carbon isotope paper. I worked with Earl Gulbranson on this, and we showed that the 12/13 isotope ratio in nature varied by some 5 percent.

GRAYSON: Okay. That was in 1939. It seems like a large number of these papers are very close together in time.

NIER: Well, it was finishing up work done in 1938 at Harvard.

GRAYSON: I see, okay.

NIER: And then, there's a delay in publication...

GRAYSON: Sure.

NIER: Then, this was this first paper on the device for compensating for magnetic field fluctuations in a solenoid mass spectrometer. It has a diagram of the circuit. A simple circuit is all you need. As far as I know, only one person ever used it. We used it very successfully. Then at one time in about 1950, I built a portable instrument for the people in surgery. And this was before transistors, so the thing was really cumbersome. I have a picture of that thing. The medical school, had an anniversary a few years ago. It was their hundredth anniversary and they remembered this instrument, and they asked me, "Did I by chance have a picture?" And sure enough I did. (Figure 30) So, I lent it to them. This showed how you used this. This appeared in the *Journal of Thoracic Surgery*. You had this connected to people to monitor respiration,

you see. We also have a photograph of the 180 degree mass spectrometer tube used with the instrument. (Figure 31)

There were some other people doing similar things in other places. I think we had a better instrument, but I didn't have time, and none of the medical people knew how the thing worked. I provided them with a guy to help run it, but this is a case where you ought to buy an instrument that's all done. Because, see, they don't have any time or sympathy for the concerns of this sort of thing. So, it was never pursued. Although, it saved one life, at least, that I know of. Something went wrong, and they weren't giving the guy enough oxygen.

GRAYSON: Oh, really?

NIER: I think the machine told them that. So, that's a story there. I have some other things, but those are the ones that I found in particular. Here is something that you don't have. I got an honorary degree from the University in 1980. I had to give a little speech. And that gives some background on things. Repeats some of the things I told you this morning. So, you can have that to put in there.

GRAYSON: Yes.

NIER: You have that?

GRAYSON: "Reminiscences of the isotopic..." Yes, yeah...

NIER: You don't have this, I bet. Did you see this? I got a medal—the [V. M.] Goldschmidt Medal from the Geochemical Society [in 1984]. And Harmon Craig of [Scripps Institute of Oceanography, University of California San Diego] La Jolla [California] gave the introduction, and I gave a little talk, which repeats some of the same stuff that we were talking about here, but you should have that for your file. Harmon Craig was such a character, that just reading this is an interesting aside. Here's something which you may not have. This is the one I mentioned before. Duckworth and me on the unified scale. That came out just recently.

GRAYSON: 1988. That's pretty recent! [laughter]

NIER: Yes. It that reviews that whole history of the standardization on C-12. Probably with more detail than you're interested in.

GRAYSON: Oh, no.

NIER: It's all there. But it tells it very well, and Harry deserves an enormous credit for getting that out. Here's the conference in 1951 at the Bureau of Standards. And here are all the characters that were present. There's a map of all of them. (Figure 32)

GRAYSON: Okay.

NIER: Now, ASTM E-14 probably already existed, but maybe it didn't. There was a group in England, Institute of Petroleum-sponsored thing...this I don't want to lose now.

GRAYSON: Right, we'll have to make some arrangements to ensure its safety.

NIER: I could lend this to you if you want. I did a little research on this a few years ago. There was a meeting of mass spectroscopists in England, who were interested in that sort of thing about 1949. This group then met every three years and this developed into the international meeting. I went to the first few of those. I was not at the original one, but I was at later ones; several of them. I got to meet quite a few of the people. They were mainly English and then it was broadened to include the Continent. At first, it was just a handful of people, it seems like it just snowballed. You'd be interested in this meeting, since you're not an organic chemist. John Hipple organized this.[†] He was then head of something at the Bureau of Standards. And he was so afraid that it would be swamped by organic chemists, with all of their things, that it would be like the E-14 meetings. They deliberately excluded—that's too strong. They didn't deliberately exclude them, but on the other hand, they didn't make much of a point of inviting them. The meeting was by invitation. But they had practically everybody in mass spectroscopy in the world. Leading Japanese people, Ogata, was there. Mattauch was there, [Paul] Ewald, Hintenberger, all of these people were there. I met them for the first time in 1951. [Jaap] Kistemaker, from Holland, who was a wheel there, plus countless others. I don't remember everyone who was there at that meeting. And it was the first time I had ever met other mass spectroscopists, you know. I knew a few but it was the first time that I'd ever met a lot of these people. And I think for a lot of them, it was their first time. Certainly it was the first time I met the Germans.

GRAYSON: Okay. We'll have to work out some mechanism here to get copies of this photo.

[†] The meeting discussed in further detail here is the same one alluded to at the beginning of the interview, the National Bureau of Standards meeting in the early 1950s.

NIER: You ought to know about that meeting, no matter what. I'd be glad to lend you the whole business. But, as I say, I don't want to lose it.

GRAYSON: No, I understand.

NIER: Maybe the thing to do is take it with you.

GRAYSON: I could. I just want to make sure it is safely transmitted. This tape that John Beynon did with Graham Cooks, Beynon tells this horror story where some English scientist was doing some research in the history of mass spectrometry, and Beynon sent him all of his early ASMS booklets in the mail, so he could go through them, organize them, and so on. And the guy never got them. They're somewhere in the English mail. He had all of these fantastic things, that I would like to look at myself—the early ASMS proceedings—but they were lost.

NIER: Well, can you make some arrangements? You know, we could have them done at our photo lab, here, at North Hall.

GRAYSON: Right, I thought about that, with Tom [Krick] being here, that would be most convenient.

NIER: Yes, I was going to say, and you could make some financial arrangements there and have them done.

KRICK: I could get them to you. I could even put them in my budget.

NIER: So, whatever you could do. You and I could work it out. There may be other things like that we could do here.

KRICK: And then they'd be physically located right here.

NIER: Right here, at North Hall, and they do beautiful work over there.

KRICK: They do real good work over there.

NIER: See, they copied this thing. And I've got the stuff that goes with it. Look, isn't this a marvelous thing.

KRICK: Yes. That's unusual.

NIER: With this sheet, you have all of these people in the group portrait identified. (Figure 32)

GRAYSON: Yes. That would be absolutely excellent.

NIER: Here is the foreword. It gives you an introduction. And this is the book; with the index, and so on. You should have all of this. Well, we can xerox this for you later. These are things, I think, you don't want to miss.

GRAYSON: Right.

NIER: I'll keep it now, and you and I can work this thing out and have copies made, if you want to. You can see some of these people here. Here's Bleakney. I've got a current picture of him. I say current...he's now about eighty-five or eighty-six years old, and this was, as I say, in 1950, so that's forty years ago. He was at Princeton. Here's Ed Condon. He was then Director of the National Bureau of Standards. Here's Bainbridge.

GRAYSON: Oh, wow!

NIER: And here I am with these people. A lot of wonderful people in this photograph—good, good photograph. It was done by somebody who was a good photographer. But these were the people who were doing mass spectroscopy or closely related things, at the time. And there are a few chemists involved, there are a few here, but if you read the papers, there's nothing here about fragmentation of molecules or radicals or fragments. [laughter] It was really...well, the title of the meeting was "Mass Spectrometry in Physics Research."

GRAYSON: Sure.

NIER: Well, we should organize this material a little more.

GRAYSON: Yes. I think that the point you make about getting a little break before you go out tonight is good. It's getting a little late, and I don't want to push things.

NIER: There's my first instrument. The one with the solenoid.

GRAYSON: Yes.

NIER: Here are the mercury pumps. The tube is inside here, with the asbestos wrapped around it, heaven forbid!

GRAYSON: Yes.

NIER: There's asbestos up there on the shelf, by the way.

GRAYSON: What's this? Does this say "thermos" on it?

NIER: Yes, it's a thermos bottle. [laughter] We didn't even have pyrex ones, we had ordinary ones. You had to be damn careful. Otherwise, they imploded. Here were the traps for the mercury pumps. The scale is back here. The galvanometer you can't see; it's hidden behind some stuff here. Then, I have a picture, from the Harvard instrument. Now, this is the so-called Rockefeller instrument, that we built specially for the carbon work. Here's the magnet, here's the tube, and that had the first null measurement feature. The galvanometer is hidden behind it...here's the scale, you sat behind it. On the other side, was the console, where you sat. And then, the Harvard instrument is here somewhere...I'll have to check...oh, here's that surgery instrument.

GRAYSON: Yes. That is fascinating.

KRICK: Portable too.

NIER: Here's the first commercial leak detector.

GRAYSON: Now, there is a conference in France, I think, in the Fall. A fellow [Pierre Duval at Alcatel in France] has contacted me, asking me for pictures of this type because he's going to be presenting some material on the history of leak detection. [Duval died before the meeting and the paper was presented by one of his co-workers.]

NIER: Well, whatever you want to do, you're welcome.

GRAYSON: What I'd like to do is tomorrow sit down and go over all of this.

NIER: We better do that. Are you going to be free tomorrow, Tom?

KRICK: Yes.

GRAYSON: We can go through these pictures and make some notes on them.

KRICK: Well, I was thinking you could even take the pictures and xerox them, and then make notes on them.

NIER: I think that would be very good. That's very important. And, if somebody doesn't break down the xerox machine this afternoon, I can do this tomorrow.

KRICK: Yes.

NIER: But unfortunately, the xerox machine, if somebody buggers it, then it won't be available tomorrow. Otherwise it's available, and I have keys to everything.

GRAYSON: Okay.

NIER: Here's the first spectrometer.

KRICK: We could copy them on 11 by 17, so you'd have more room on each one to write.

NIER: Something I wanted to show you is the Harvard machine. But I've got our first leak detector also. Sure, by all means, you're certainly free to have it. Now, let's see. This I got at a symposium.

GRAYSON: There's even some negatives there, I see. Aha! [laughter]

NIER: No, I've misplaced a lot of stuff. There should be one here on the Manhattan District instruments. Here was the Consolidated Instrument tube...sixty degree hydrogen spectrometer. Here is the instrument where the panels were first introduced. There's one with me on it, though, which was really a very good picture.

GRAYSON: So, this is looking at that machine from the front, as opposed to the back, and this represents the scale...

NIER: Yes, the scale is there.

GRAYSON: ...that you would look at. It was all very neat and organized.

NIER: Here was the machine that the first work was done here, in 1938, 1940.

GRAYSON: That's a plywood panel I see.

NIER: Plywood panel, yes. I made that myself. But, we didn't have all that help in those days, you know.

GRAYSON: You've got this nice little convenient business where there's a little cowl over the light, so you can see your controls, when you turn out the lights. [laughter]

NIER: Well, you had to see that electrometer spot. The spot was up on the scale.

GRAYSON: Yes, but you also needed a little light to know which knob to turn. [laughter]

NIER: What to write down, and things like that. You know, I think I have most of the pictures that would be interesting.

GRAYSON: Okay. Well, like I say, we will have tomorrow, and if we're going to be going out this evening, which I think we'd really like to do, I think it would probably be a good idea to wrap it up for today.

NIER: Okay. Now, my wife and I can pick you up at some time which is convenient.

KRICK: Or, if you want me to come with you, I'll pick you up.

NIER: Well, fine, whatever you say. Where do you live?

KRICK: Right next to St. Paul campus.

NIER: Well, so do I.

GRAYSON: Everybody's right here, so maybe we don't have to pick up anybody. Well, it depends on where we want to go.

NIER: Well, where do you want to go? You had thoughts of where you wanted to go?

[END OF AUDIO, FILE 3.1]

[END OF INTERVIEW]

INTERVIEWEE: Alfred O.C. Nier

INTERVIEWER: Michael A. Grayson

LOCATION: University of Minnesota
Minneapolis, Minnesota

DATE: 10 April 1989

GRAYSON: [...] Professor Nier and I are going through some, photographs that we're going to have copied, so that we can...include them with the interview. And this particular picture here is?

NIER: Well, shortly after World War II, I got a grant from the Committee on Growth which was some money put up by the American Cancer Society for promoting research related to cancer. And they'd given the money to the National Research Council, and who in turn, gave the grant to me. And this was to develop a new mass spectrometer which might be useful for tracer work. And, we developed such a machine in 1947 or thereabouts, and then the design was given to the Consolidated Electrodynamics people in Pasadena [California], who proceeded to manufacture these. I don't know how many they made and sold, but it was a number of dozens, as far as I know, and they were used by people in biochemical research, where they used tracer isotopes.

GRAYSON: There are two people in this picture. (Figure 33)

NIER: Yes. Myself, and Harold Washburn, who was the director of research for the company, and responsible for the development of various instruments for the company. And that picture would have been taken in about 1948 or 1949. (Figure 33) It has the name "Consolidated Nier" on the instrument, and by the way, in the British Museum, in South Kensington, there is such an instrument, on display there.

GRAYSON: Oh, really? Excellent. My son is there now. I'll ask him to go by that museum and take a picture of it for me. (Figures 34, 35)

NIER: [laughter] Good. Well, these are details of the instruments. I don't know if we need anything more of this. This is the developmental model. (Figure 36)

GRAYSON: Okay, so basically, it was a 60 degree sector instrument. The ones with some people on them, perhaps those would be more interesting.

NIER: Well, here's the prototype from which the Consolidated people based their instrument. I've forgot who the individual is there, with me. It may have been somebody who was visiting or a Consolidated person.

GRAYSON: Physicists didn't wear suits in the laboratory, did they, in those days?

NIER: Not always. [laughter]

GRAYSON: Why don't we just put it in the copy pile. So, this is essentially the prototype design for that particular instrument?

NIER: Yes; here are the people. There were three of us on it. [laughter]

GRAYSON: This was taken in this building, I guess.

NIER: Yes, it was...in our sub-basement, where the instrument was set up. There are names on the back of this one.

GRAYSON: Oh good!

NIER: Howard Ecker and Ray Hopper. They may have been Consolidated people. (Figure 37)

GRAYSON: This one has more people on it.

NIER: And it shows more of the instrument. The recorder and the flight tube. And you can see the inlet system.

NIER: Now, here's another view of the instrument. And here's a profile view.

GRAYSON: Actually, a profile view might be interesting it shows a lot of detail on the analyzer side.

NIER: Well, why don't you take that one. Here's the negatives. I think that's a nuisance, to go separately through the negatives, isn't it?

GRAYSON: I don't know, if they're in good shape, it's easy to just print from the negatives, if we have them. Oh my, there's more stuff in there.

NIER: These are schematics of the spectrometer.

GRAYSON: Okay. That's a double-focusing machine.

NIER: Let's see what this is. This was when we were doing our meteorite work. Comparing the helium-3 to the helium-4. You see, here we had a multiplier and a collector. It even says helium-3. We took a fraction of the output. It shows it here schematically. When it balanced, we used the recorder to note the difference. (Figure 38)

GRAYSON: So, because the helium-3 was so much less abundant, you used an electron multiplier?

NIER: That's the way we did it.

[END OF AUDIO, FILE 4.1]

GRAYSON: [...] There's so much good material here. So, this was the Kovar problem?

NIER: Here's some console pictures here. But there's one like this with me on it.

GRAYSON: Well, pictures with people on them are worth more than those without anybody.

NIER: Well, I have to look through these prints. [paper shuffling]

GRAYSON: So, you obviously documented the stuff pretty thoroughly during the years...

NIER: It all depends. [laughter] Some things, yes.

GRAYSON: Probably depended on how big a hurry you were in at the time.

NIER: Yes, that's right. That's usually the way it is.

GRAYSON: You have some good negatives there, I think we could certainly get some decent prints.

NIER: Yes, this is the original 60 degree instrument.

GRAYSON: Okay. I'm sure that can be printed up readily.

NIER: Yes, Well, let's see. Here's the console. I don't know if this is important. This was a hydrogen instrument made after the war, with one of my students. It was all metal, and it had multiple collectors for mass-2, 3, and 4.

GRAYSON: I see...

NIER: That's just a little diversion.

GRAYSON: I like the battery there. What's the battery doing?

NIER: Well, at the collectors, you had a suppressor plate, to knock back the secondary electrons. It was just a 22 and a half volt battery.

GRAYSON: You just plugged the battery in.

NIER: To get the voltage on the suppressor plate.

GRAYSON: Well, here's some interesting details of the guts of an instrument.

NIER: Yes.

GRAYSON: Showing all the slits and so on. Is this a dual-slit?

NIER: Apparently yes, there are the two cup-pieces.

GRAYSON: Okay. And the framework.

NIER: Yes, see we did document that well.

GRAYSON: Yes, all kinds of good bits and pieces, there.

NIER: We had a glass pump on this instrument; a little 180 degree instrument. It may have been a version of the portable one for the medical people. This was a spherical joint, so it had a glass ball on the end of the diffusion pump. It was sealed with black wax.

GRAYSON: That was where the diffusion pump was.

NIER: Yes.

GRAYSON: Then was this for water-cooling the little coil?

NIER: Apparently yes, that was the water-cooling.

GRAYSON: What were you trying to keep cool there?

NIER: Well, the wax...see, because you baked the rest of it, and this was to keep the wax from melting.

GRAYSON: Okay, I see. That's an interesting bit of detail. That was on the hydrogen spectrometer?

NIER: Yes.

GRAYSON: It says...what is this? Somebody [Byron F.] Murphey?

NIER: Hydrogen machine...Murphey, from his thesis...Byron Murphey...that was right after the war.

GRAYSON: I see, thesis work plus one *Phys. Rev.* [*Physical Review*] publication.

NIER: Yes, that's right. [laughter] It was published in *Phys. Rev.* By the way, that had in it the leak system which was adopted by the people in Chicago...Harold Urey, and others. I don't know if that shows in the instrument. You could switch back and forth from one gas sample to the other. Well, it doesn't show on this picture. There were little glass valves. Yes, there it is!

GRAYSON: These were magnetically operated?

NIER: Magnetically operated, and you could switch between them. That became standard practice. Here's a good picture.

GRAYSON: Yes, let me take one of those, too.

NIER: One of Urey's people came up to look at all of this, and they adopted it on their instrument, and published on it, and so it became known as the Urey-McKinney leak system, but they saw it here first. We had been using it and published it before them, but it's very seldom that people acknowledge this. That where the idea came from.

GRAYSON: That's the problem with letting people into your laboratory.

NIER: Yes, well, it was published in a different place, so it was in a different context.

GRAYSON: Yes.

NIER: Nobody did anything deliberately; it was just one of those things that happens. I think I refereed a paper once, where I called an attention to somebody on that, and in that particular paper it was corrected. That's the only publication. This was Tom Aldrich, who worked with me, and that was his work there. This would have been in 1946 or 1947.

GRAYSON: Okay, can we go ahead and get a copy of this one also?

NIER: Sure, just write it, "L.T. Aldrich."

NIER: Are you recording this?

GRAYSON: Yes.

NIER: Okay.

GRAYSON: What we'll do is make xerox copies and then we can write on them.

NIER: Right. That's L.T. Aldrich, who did the original work on the abundance of He-3 and He-4 and identified the Ar-40 in potassium minerals.

GRAYSON: Batteries seem to be a common.

NIER: Yes, we always had these 45-volt batteries for various things. Usually, they had a 22 and a half volt tap, and that was convenient to use, as I said, for the suppresser voltage at the collector, to stop secondary electrons.

GRAYSON: Is this, then, the inlet system?

NIER: This would be the inlet system on that, yes.

GRAYSON: Lovely glass work, yes.

NIER: We used stopcocks on a lot of this stuff. That wasn't always true, but it was certainly true then.

GRAYSON: We're looking for a better picture now.

NIER: I'm looking for a better picture, and it exists, I know that.

GRAYSON: We don't want to mess up your files too much here.

NIER: No, well, I'll have to straighten it. Already, they're in pretty bad shape, so it doesn't really matter. [laughter] Alright. Here's something. What do you want to do with these?

GRAYSON: Well, if we can't find a better picture, the one that you mentioned with you on it, why don't we go ahead and use that.

NIER: Okay. I have negatives here. Now what's this...is this?

GRAYSON: That is a negative of...

NIER: That's of the original instrument.

GRAYSON: Yes. If you want to take the negative of this one, that would probably be best. Why don't we do that for now. We'll take the negative, we'll take both negatives, and then, rather than copy from the print, we can go directly from the negatives.

NIER: Okay. But we might as well xerox these, so you know what you're identifying.

GRAYSON: Oh yes, most definitely. Yes, good point.

NIER: Now, let's see what else I have here.

GRAYSON: Have you made arrangements for these papers to be deposited somewhere?

NIER: No.

GRAYSON: Has anyone approached you? [laughter]

NIER: No, no.

GRAYSON: Would you like to make arrangements?

NIER: I wouldn't mind. If they stayed here, nothing will ever happen to them.

GRAYSON: Okay, I would...I'll explore. Obviously, ASMS doesn't have the resources, but the Center for the History of Chemistry might.

NIER: Here's the...hydrogen tube used during the war for the H-2 and H-3. That's a schematic. And it shows the two collectors.

GRAYSON: Yes, okay. I wonder how these would xerox. [laughter]

NIER: Not very good, I don't know.

GRAYSON: We may try it.

NIER: I may have pictures on some of that. But there's a lot of stuff here.

GRAYSON: There's a tremendous number of negatives in this envelope.

NIER: Yeah. I got the photograph...this is the photograph of the tube.

GRAYSON: Oh yes, okay.

NIER: And here's the first helium leak detector.

GRAYSON: Now, when you say "first," how "first" is this one?

NIER: Well, the first one built to be such. This was the prototype whose design was given to General Electric.

GRAYSON: This fellow deserves special attention.

NIER: Here's another negative, another picture of it, but I've got some prints of that.

GRAYSON: Okay. We'll hold on to the neggies, in lieu of that, and if we get the prints, then we can make xeroxes for notes.

NIER: Well, there's several views. See, here's another one of those. There are a few pictures. It's amazing that we did take such pictures, and they were saved.

GRAYSON: Now, were these photographs taken by someone on staff for the University?

NIER: Yes, yes.

GRAYSON: So, you had a facility that you could call on for this kind of work.

NIER: Yes. We had a very good photographer, one of the people who worked in the shop, who did this with various things, and he was a very good photographer. He was an amateur everything. He was just the kind of person you needed.

GRAYSON: He was good at it, huh? [laughter]

NIER: He was good at it. Well, here's another. This was probably a hydrogen tube, the ones used for the deuterium work. See, I've got a number of negatives like that, and I don't know how many of these are pertinent, really.

GRAYSON: Yes. Actually, this would probably go good. If you look at the schematic drawing of the tube and this print, they really go together.

NIER: Yes, yes...they do.

GRAYSON: That tube would probably be worth.

NIER: Here's one of the early seven of those uranium analysis instruments.

GRAYSON: Oh, look at all of the vacuum tubes!

NIER: Yeah, not all of the electronics are there. There was some stuff missing on the right. But it shows the flight tube.

GRAYSON: Is there an oscilloscope there?

NIER: Yeah, that's an oscilloscope. That was part of the kit. When we provided these...see, we made these to ship away and we gave an oscilloscope with them for troubleshooting.

GRAYSON: I see. You knew there was going to be trouble, huh?

NIER: Yes, trouble would be likely. And here's a schematic of the helium leak detector in use.

GRAYSON: Oh, okay.

NIER: Now that I think about it, there may be a publication.

GRAYSON: Do you have a publication on the leak detector in this package?²⁷

NIER: Yes. Now, it should be there. If not...well, it may not be in that package, it may be here yet. I intended it to be...wait a minute. It may be right here. We should xerox that.

GRAYSON: Yes.

NIER: See here? There it is, see?

GRAYSON: Yes. There it is. Most definitely.

NIER: We should xerox that.

GRAYSON: We'll put that in this pile, to be xeroxed.

NIER: Yes...and here's the, the "Line recorder," as it was called.

GRAYSON: Okay. So, these were for the gaseous diffusion plant?

NIER: Correct.

GRAYSON: And this publication actually showed up in [1947], which was some time after the work was done.

NIER: Well, we had to get the manuscript de-classified. So, it was after the war.

GRAYSON: Okay, yes, I assume that almost everything that happened during that period of time was very heavily classified.

²⁷ Alfred O. Nier, C.M. Stevens, A. Hustrulid, and T.A. Abbot, "Mass Spectrometer for Leak Detection," *Journal of Applied Physics* 18 (1947): 30.

NIER: This was an interesting adjustable leak we had, where we had a collet arrangement that you used to adjust the flow.

GRAYSON: I see.

NIER: This gave a rapid time-response, which you needed. Also, it avoided fractionation, because the gas was fed in through a long capillary, and the flow was fast enough so diffusion you wouldn't get back diffusion. Anything that went in had to come out.

GRAYSON: Okay, so we're looking at Figure 3 on page 190 of the article in *Analytical Chemistry* on the gaseous diffusion plant.²⁸ So, the process stream was coming...

NIER: ...coming by here, yes, and you sampled it by having the tube stick in the process stream.

GRAYSON: I see, and by just simply adjusting this valve...

NIER: You adjusted it to a pitch.

GRAYSON: You could control the amount that got in.

NIER: That's right.

GRAYSON: This is similar to the picture that you gave me with some preliminary papers, showing the various gases that are being measured in the process stream.

NIER: That's right, and there was a pulse of gas here. They were always doing things in the plant; I guess it was a nitrogen pulse that went through the plant, and was recorded by this.

GRAYSON: Yes, we'll add that to the "copy" file. So, this was an example of...

²⁸ Alfred O. Nier, T.A. Abbott, J.K. Pickard, W.T. Leland, T.I. Taylor, C.M. Stevens, D.L. Drukey, and G. Goertzel, "Recording Mass Spectrometer for Process Analyses," *Analytic Chemistry* 20 (1948): 188.

NIER: Of a spectrum that was obtained. The mass range went up to singly charged mercury, see? The instrument didn't have high-resolution, deliberately.

GRAYSON: Sure.

NIER: It had a permanent magnet, by the way. It was a 60 degree instrument with a permanent magnet, and I think we had a three-inch radius, something like that, on it. Two or three-inch radius.

GRAYSON: Okay. So, the mercury isotopes appear...kind of...as one broad peak.

NIER: Right. And you've got the doubly-charged mercury ions.

GRAYSON: Yes. There's some CF₃.

NIER: That's because there'd been some fluorocarbons in the process stream.

GRAYSON: Okay. But then, let's see...CO and N₂...you can see a 29-peak as...

NIER: ...and O₂. You can see a little bit of the 29-peak.

GRAYSON: It's just beginning to be resolved there.

NIER: Yes, but we didn't pretend to do isotopes. It was just to do the main gases.

GRAYSON: So, basically you were interested in peaks from 69 on down.

NIER: That's correct. And we deliberately didn't want too good resolution, because we were using peak-stepping. The recorder, remember, switched. We had a multi-point recorder, and switched peaks...you didn't want to slip off the peak.

GRAYSON: So, you didn't normally record the spectrum that we see here.

NIER: That's correct; you jumped from peak to peak with the commutator that was built into the recorder.

GRAYSON: Then, you just sat on each peak for...how long?

NIER: It had to be long enough, because these recorders were slow. So it probably sat six or twelve seconds on each peak, to give the recorder a chance to read it, and then it switched to the next one.

GRAYSON: And then, all of that information was fed...

NIER: Well, from that recorder, there was a slave recorder. The data such as you got from this recorder would be available locally. But the slave recorder printed in the central control room, anything that was on the local one. So, probably the only information that ever got to the central control room was the peak-stepping data. So that's the kind of things you saw in the control room.

GRAYSON: So, the control room was looking at 50 chart recorders, or more, that had this information.

NIER: That's right.

GRAYSON: And this is what the young fellow, when he said "Hey, there's a problem here."

NIER: And they didn't pay any attention to him.

GRAYSON: "Stage number X has got a problem, let's shut down here."

NIER: It told him exactly where it was.

GRAYSON: And so, all of this fantastic amount of money and technology that was created for this exact problem was ignored when they actually needed it.

NIER: Yes, up until that day. But the beauty was that a light component, like air, would just want to go to the top of the plant right away. So, there was very little spill-over in the other direction in the plant. So, you would see that everything looked good, up to a certain point, and then, from there on, you saw this on each of the recorders, and as this pulse worked through, you could just see it going from stage to stage to stage.

GRAYSON: Right on down the whole plant.

NIER: It finally filled the plant, and then backed up all the way. So, this is what they saw that eventful morning.

GRAYSON: Okay. That's excellent we'll get a copy of that paper.

NIER: Is this one I showed you there?

GRAYSON: That looks like probably it is. That's included in the paper. I think that is Figure 5.

NIER: But I was wondering if you have a better copy.

GRAYSON: We could probably take that as a copy.

NIER: Why don't we copy that.

GRAYSON: And then we show it in more detail.

NIER: Now here's the spectrum, too.

GRAYSON: Okay. This is original art for the paper.

NIER: I don't know how interesting all of this is. Well, I have all of that. That's the important thing.

GRAYSON: Yes.

NIER: This is the inlet system on the instrument. Here was the plant stream. Here was the leak, it was just a leak.

GRAYSON: Okay, so this was an overall view?

NIER: Can you see this area?

GRAYSON: Yes.

NIER: There's the little mercury pool, and this was silver-soldered on it, to a thing which dipped into a thermos bottle. And that held the temperature. This was at room temperature. You wanted this warm; and we didn't want the mercury to go beyond a certain point.

GRAYSON: Yes, sure.

NIER: So, you had this as I remember so that only the impurities went on.

GRAYSON: Yes, that's nice.

NIER: I don't know if that's in the paper.

GRAYSON: I didn't see anything that looked quite like that.

NIER: Well, it may be, because you may not have been looking for it.

GRAYSON: To whomever is transcribing this, we apologize for dropping papers all over things. No, this says "more instrumental detail." It doesn't really show that.

NIER: Well, let's look and see. You should have that.

GRAYSON: Yes, this negative, shows...

NIER: That would be an interesting one. It's discussed, but in order to cut the paper down, we didn't use it.

GRAYSON: So, it's the old typical thing, you probably prepared a little more artwork for publication than you actually ended up using. Well, here, that looks like the positive.

NIER: Is it? Well, here, we could xerox.

GRAYSON: Why don't we xerox the positive for sure. Why don't you hold on to that. We may not need photowork on that, because it is xeroxable.

NIER: Here's the General Electric leak detector.

GRAYSON: Excellent.

NIER: And that's the inside.

GRAYSON: Excellent. Oh my, it's got a catalog number.

NIER: Yes. [laughter] That was the original commercial version. They made hundreds of those for the Manhattan Project.

GRAYSON: There is a little Variac down there, on the bottom and more batteries again.

NIER: Yes, probably to adjust the heat on the diffusion pumps. They were so funny. We bought these probably from Distillation Products. This was a water-cooled pump. And then we

had a trap here—a liquid nitrogen trap—to keep the oil from coming over into the analyzer. And here's the tube. This was brass.

GRAYSON: So, you're saying this was an oil diffusion pump?

NIER: Yes, oil diffusion pump.

GRAYSON: So, they got away from mercury in this design.

NIER: Yes. Oh, yes. That was the first one. We had already gotten away from it when we gave them the improved design. See, the glass one was built here.

GRAYSON: Okay, and the one built here had a mercury diffusion pump.

NIER: We went to Kellex, in the summer of 1943. I had Charlie Stevens, who was one of our undergraduates. After graduation he came with me to New York, and spent the war there in the lab. He was one of the people who did a lot of this work. You can see, here's the fore pump for backing the diffusion pump. This is the tube here. And in the original version, the ends were soft-soldered on because of the brass and solder we could not use mercury pumps.

GRAYSON: Okay.

NIER: Filaments often burned out in these instruments because the vacuum wasn't very good.

GRAYSON: True.

NIER: The filaments went in here where you can see, there's an Apiezon black wax seal. There was a tapered fitting so this went into the side so you could replace the filaments readily. And of course, the familiar battery; probably a couple of 45-volt batteries. Notice there was a storage battery here.

GRAYSON: Oh, yes.

NIER: Now I recognize it. This was for the filaments of the tubes for the amplifier.

GRAYSON: Okay, sure. This had to be portable, so you didn't want to plug it into the wall.

NIER: Portable, and we used an inverse feedback amplifier for measuring ion currents, rather than the big electrometer tubes, such as the FP-54, which was also manufactured by GE. We found these little acorn tubes, which had been developed for ultra-high frequency work, were made very well, and had a high input resistance. So, they would serve as electrometer tubes. And they had low filament power as well. And these two 45-volt batteries were the B+ power supply for the amplifier, and then the storage battery was used to heat the filaments.

GRAYSON: Let's suppose we're operating this leak detector...

NIER: Yes...

GRAYSON: ...I guess there's an independent source of helium...

NIER: Yes. That's right, that would be independent.

GRAYSON: ...and this fellow is trying to look for a leak. He observes a number of gauges and knobs.

NIER: Yes.

GRAYSON: What did he do?

NIER: Well, he would tune one of these knobs to make sure he was tuned to the He-4 peak.

GRAYSON: Okay, so you checked out the mass spectrometer initially...

NIER: Yes, initially.

GRAYSON: ...to ensure that it was seeing what you were leak testing with...

NIER: That it was tuned to helium.

GRAYSON: Okay.

NIER: And then, there was a sensitivity switch on the instrument amplifier, so you could cut down the sensitivity. Then as your leak got a smaller and smaller you would turn up to a higher sensitivity. The sensitivity switch was here. This was an ion gauge that was self-regulating.

GRAYSON: So, that was to check out the...

NIER: The vacuum.

GRAYSON: So, when you started it, you had...

NIER: ...to be sure it was pumped down.

GRAYSON: ...to ensure that it was pumped down before you turned on the filament. Did you have a filament protection circuit in it?

NIER: Probably, but they were improved as time went on, with more automatic things put on...

GRAYSON: Sure...

NIER: ...but, the original ones, I'm sure had that much. That the filament wouldn't go on...

GRAYSON: ...wouldn't go on, unless the ion gauge said "yes." [laughter]

NIER: But the early ones were pretty primitive in that sense. We didn't have a lot of safety interlocks and stuff.

GRAYSON: Yes, that's understandable.

NIER: There was such a hurry to get these thing going, that I doubt that we had too many refinements at the time.

GRAYSON: So, then an operator would look at one or the other of these meters...

NIER: Yes, that's right.

GRAYSON: ...to determine whether or not helium was detected.

NIER: Yes. Well, I know, now, what the three meters are. One was to measure the spectrometer filament current and the other was emission. And the third was the helium signal.

GRAYSON: Yes.

NIER: I'm quite sure that's what those meters were.

GRAYSON: So, basically, of the four meters, three of them were to tell you the machine was operating correctly. [laughter]

NIER: Yes, that's correct. And this was the whole ion gauge control.

GRAYSON: Sure.

NIER: Now, I didn't realize we had all that detail. [laughter] Well, the guy who worked on the development when we moved to Kellex was Charlie Stevens, who is now at the Argonne National Lab. By this time it was 1943, and you didn't have much choice of personnel. So all I had left over was undergraduates who were just finishing up. And of course, it was possible to get draft deferment for these people, because they were doing vital work. Although, at one point, they decided this development wasn't important enough, and he was to be drafted, in spite of the fact that he had the sight of only one eye.

GRAYSON: Oh, my.

NIER: But, I managed to get the personnel people at Kellex managed to straighten that out.

GRAYSON: This was Stevens?

NIER: Stevens, yes but that was one of those things that happened. Here, they were depending on this leak detector for the plant, he was the only man I had working on it, and they were going to draft him because somebody thought it wasn't essential.

GRAYSON: Well, that's, bureaucracy.

NIER: Bureaucracy. Yes, bureaucracy at work.

GRAYSON: Suppose that we were going to check a leak with this system, or check the vacuum system for leaks. We would push it up, plug it in; how long would it take before you were ready to operate and check leaks?

NIER: Well, these machines were kept pumped down, certainly, with a fore vacuum. So, it was a matter until the oil warmed up and it got going. So, I suppose, in a half an hour or so, you were ready to start working.

GRAYSON: What type of pump oils were used for these?

NIER: I don't remember. They were not these sophisticated ones that you have now. But they were pretty good oils; standard commercial oils. Distillation Products were the people that we got these things from. Or maybe, one of the Apiezon oils.

GRAYSON: So, once you got away from the glass systems, you preferred to go with oil as a pumping medium.

NIER: Yes. See, we had soft-soldered joints in the instrument...so, we didn't use mercury as a pumping fluid. [laughter] It was certainly better to have an all-metal system; it was much more

rugged. And, there weren't any decent mercury metal pumps available at the time. There was one manufactured by Edwards, but, uh...well, it pumped alright. The trap that came with it had a capacity of about 100 cc's of liquid nitrogen. It was a little finger that went into the side of the thing, sort of like the squirrel who builds a nest in the side of the tree, in a hole in the tree. This was sort of the way the trap was. And the liquid nitrogen lasted like 15 minutes or something like that. [laughter] So, this wasn't practical. And nobody had a real good trap that you'd want with this system. At least, nobody had gone to the trouble of manufacturing one at that time.

GRAYSON: So, let's see. What else, do you want to discuss.

NIER: Well, these were all related to the Manhattan Project.

GRAYSON: I'm just going to quickly riffle through some of these other things.

NIER: You may be missing some items.

GRAYSON: This would probably be for the leak detector.

NIER: Yes. That was the example of using it. Yes, that was the idea.

GRAYSON: We'll just add that to the other pictures, just in case it isn't like anything that we have.

NIER: Now, I think we've gone through most of these negatives. Here are some details from that one paper, so I think that's alright, we have those. Now, this one says, "Historic," but I don't know. [laughter] That could mean anything.

GRAYSON: Well, I think there's probably a lot of history here. It's being made, even as we work.

NIER: These were parts from the various leak detectors. There were a lot of details of the spectrometer and the uranium tubes, too. Here's either the leak detector or the first hydrogen tube. It has only one collector on it.

GRAYSON: Working with the negatives is kind of difficult right now. It's good to have the negatives for making prints.

NIER: Well, I don't really have any good record of this stuff. That's the problem. I mean, this is all I have. See, a lot of this material may have never been printed. Or, they may have been printed, in connection with the report I had to turn in, and so there were no print positives saved.

GRAYSON: This looks like a pair of...

NIER: No, they may have been different views. You see, we had several instruments at the time.

GRAYSON: At least, a pair of those would make another good set since they show the layout of the whole thing.

NIER: Right. Here is a schematic of the gas inlet, and that's the Line Recorder, as we called it.

GRAYSON: And I've got the problem the slide projectionist has at the meeting...which way is up?

NIER: You've got eight choices.

GRAYSON: [laughter] You're intimately familiar with all of them, huh?

NIER: My colleague Ed Ney, when we had visitors we were considering for jobs, used to grade these people for prospective jobs depending on how many times they had to put their slides in to make them come out right side up.

GRAYSON: I'm having a hell of a time with this negative.

NIER: Do you want a magnifying glass?

GRAYSON: Now, this looks like a 60 degree sector.

NIER: Yes. That was the so-called Line Recorder, for monitoring the gas stream in the plant...

GRAYSON: Okay.

NIER: ...because it has that mercury trap, you can recognize it from that trap, for catching the UF₆ as it came in.

GRAYSON: Okay, so, this looks like it's kind of an overall of the two...

NIER: Yes, it's an overall schematic. That may be in that reprint. I don't know.

GRAYSON: Okay, this is it.

NIER: In a sense, it's here.

GRAYSON: Yes, The fundamental parts are here.

NIER: Let's see, this is the option! Wait a minute. It was an alternate arrangement. "The leak," "Pirani gauge," "Flow meter..." Well, here was the cold trap. This is another view.

GRAYSON: Okay, so it is shown there. In the paper.

NIER: Yes, it is.

GRAYSON: That's the problem with these papers. Sometimes stuff gets in, and sometimes it doesn't.

NIER: Well, I don't know what else there is here. There may be more detailed things. I don't know if you want any of that or not.

GRAYSON: A lot of that looks like it's in that paper. I think this is another view of the instrument. Is that correct?

NIER: Yes.

GRAYSON: It shows the other side, or a different angle.

NIER: Yes, a different angle.

[END OF AUDIO, FILE 4.2]

GRAYSON: We're sorting through photographs and records.

NIER: It's now 9:36 a.m.. [laughter]

GRAYSON: We get this down to the last detail here!

NIER: That's the Harvard instrument. That's the first 180 degree one with the tube between the poles of the electromagnet. You can even see the square copper cooling tubing on the outside of the magnet coils. (Figure 7)

GRAYSON: Yes.

NIER: The galvanometer is over here on the right; the scale that you read off is here; the put and take decade box for sweeping the mass spectra is on the table.

GRAYSON: Is this a potentiometer?

NIER: Yes, well this is the put and take box and this is another resistance box for changing something, I don't know what that was for.

GRAYSON: The batteries are ever present.

NIER: The batteries are B batteries. Over here on the far left is a whole bank of amplifier tubes used for controlling the generator field. You had to control an ampere of current or something like that for the DC field and these were... I've forgotten if these were the 6A3's or 6L6's. A whole bunch of these power tubes were used in parallel to control this field which required an ampere, or maybe it was a half an ampere, but that's a lot of current for radio tubes.

GRAYSON: Yes.

NIER: You had like a dozen or so in parallel to do it. But that's the only pictures I have. These are the originals and then I had this printed from it. I may have the negative. I have a slide of that. So that's what you'd probably want because that's a historic instrument. By the way, in 1936 or 1938, Harold Washburn was working for Consolidated Engineering. They made instruments for geophysical prospecting. Herbert Hoover, Jr. was the president of the company.

GRAYSON: Okay, that's how Herbert Hoover, Jr. got involved then.

NIER: Yes.

GRAYSON: Okay, because I'd heard his name in connection with the company but I didn't know in just what way he was involved.

NIER: Washburn, I think, got his degree from Berkeley if I'm not mistaken, and apparently was picked up by Consolidated when he graduated. He worked for them for years you see. But in an article or an interview, he gave once and was published in one of their house organs there, he mentioned how they had seen this instrument and the work we had done with it, and they used this instrument as a pattern for the famous 201 [CEC Model 21-102 mass spectrometer] or whatever the number was.

GRAYSON: 102.

NIER: Yes 102. It had a tube just like the one that we had and so he gave us credit for it. Our instrument was a model for their tube. They had a better magnet, more compact and better designed than ours, but essentially it was the same except their tube was horizontal whereas our

tube was vertical. I've got a picture of their instrument somewhere, I don't remember where now.

GRAYSON: I noticed there's a tendency in a lot of these instruments for the tube to be mounted horizontally.

NIER: You mean vertically?

GRAYSON: Vertically. Sorry about that.

NIER: Yes. Well, I can tell you that, [laughter] in this case, it's probably because the massive magnet was more convenient to set it on edge. But in the case of the wedge-shape instruments, it was to make it more convenient to wheel the magnet out; the magnet was on a little cart and so you could wheel it out and then bake the tube readily. See, because we had gotten away from the designs of the other people who'd worked before, like Dempster and Aston and so on. They had to minimize the size of the air gap, so they made the sides of the pole faces of the magnet part of the tube, part of the housing. Bainbridge and Jordan in their double focusing instrument did the same thing; which meant you couldn't bake the instrument. I grew up with this business where you're supposed to have a good vacuum. So, all of our instruments were made so that you could take everything else away and put an oven, or a rapid heating device on the tube so you could bake it and get rid of the junk (organic contamination). That was a characteristic of practically all the work we did in those days.

GRAYSON: So, like you said, it was a matter of a good vacuum?

NIER: That's correct. And here the gap had to be...I remember distinctly...it was a 2-inch gap. The more I think about it, we used 45 millimeter tubing for the source region housing. The analyzer part was smaller so that one could slip a thin oven over the glass tubing. You could bake the glass tubing even when it's in the electromagnet. We allowed a little bit of space for that. That was the secret to a lot of that work because it got rid of all the water and hydrocarbon and junk.

GRAYSON: But the 180 degree design now, Dempster did some work with 180 degree.

NIER: Yes, well he was the first one who really used it in a spectrometer in about 1918.

GRAYSON: That was in Chicago?

NIER: In Chicago, yes. He heated up metal salts as a source of ions because it was well known in the last century that you'd get ions when you heated them. He was also able to ionize gases. But as I say, all the instruments at that time used the same accelerating voltage for the electrons as for the ions. They're sort of superimposed and therefore you had a lousy spread of ion energies which meant that in a single focusing instrument you'd get very poor resolution.

GRAYSON: Yes, the resolution would be lousy.

NIER: And besides, it wasn't definite what the electron energy was in this style of source. This was why Bleakney's source was so important an innovation.

GRAYSON: So, was there a general disregard for the importance of the electron energy?

NIER: Well, this was evolution. It took time for people to see how important the ionization process was. And vacuum technology in the 1920s wasn't that well developed. The glass mercury diffusion pump was sometimes called the Princeton pump—I think it was developed at Princeton by people there in the 1920s. General Electric certainly was using pumps of that kind before then. It may even have been that the Princeton pump was an offshoot of the General Electric work. They were pumping down radio power tubes for transmission applications before the 1920s, so people were manufacturing vacuum tubes in the late teens. Although in World War I think they had spark transmitters and all kinds of other devices. I think vacuum tubes for radio transmission really came in during World War I, or about that time.

GRAYSON: The problem also applies to light bulbs and incandescent bulbs. Light bulb technology is quite old so did they do some back-filling with an inert atmosphere to avoid oxygen.

NIER: Well, there's a wonderful book on the light bulb. It was put out by Rutgers University. They had a project called the Edison project and there's a wonderful book that covers the period in the late 1880s and the whole history of the development of the light bulb. It tells about Edison's operation there in the East. They had to develop their own vacuum pumps for the purpose and apparently, the way you got a vacuum was to drip mercury down a tube and you'd entrain gas in between the droplets of mercury. But Edison and his people worked on the development of better pumps because this was the secret of light bulb technology. Edison was a great entrepreneur, when he saw something that had to be done, he would jump in. They had to make their own insulated wire for instance for the cables in New York City when they

electrified Pearl Street Station downtown. So, there's a book on that, a marvelous book that's really worth reading about that whole development.

GRAYSON: So, really, the vacuum technology was pretty much there, it's just that it wasn't always being applied by people in mass spectrometry at the time.

NIER: Well, I think people like [Irving] Langmuir for instance at GE had a lot to do with this. This would have been in the early part of the century between 1910 and 1920. I think a lot happened in vacuum technology.

GRAYSON: Okay, so let's see do we have any other pictures we want to take up to xerox before we take a break here.

NIER: I want to find one of the console, I have the negative somewhere. Here is an interesting aside. In 1962, I had a small contract from Jet Propulsion Lab to develop a portable mass spectrometer to take to Mars. And we built a little instrument which you can see in this picture. This was a 60 degree instrument, as you recognize, and the magnet wedge went a lot further for the little sputter ion pump. It was built into the same magnet used for deflecting the ions. It was never flown. There was supposed to be a mission to Mars in about 1965 and I built this prototype for that purpose. I had a small contract for doing that and we built this instrument and it was mounted in an attache case. So this is one of the first really portable instruments.

GRAYSON: Well, it wouldn't hurt to get a copy of that.

NIER: Well, I have the negative too on that. You can certainly have them if you wish. I remember my daughter was a student at...what's the girls school's in Columbia, Missouri?

GRAYSON: Stephens [College]?

NIER: Stephens. She was a student at Stephens from the years from 1961 to 1963 and I have friends at the University of Missouri, which was located in the same town. And I remember giving a lecture at the Physics department I carried a mass spectrometer in an attache case. It had a slight leak in it—it was battery-powered. But because of the leak, it didn't keep quite good enough vacuum, so I had to plug it in. I remember we stayed in a motel someplace in Iowa on the way down and I wasn't sure the pump would start up again but it did and then it wasn't so far anymore to Columbia so it got there all right. So we got to demonstrate this and

they provided an oscilloscope so we could show the traces. I let in air and they could see oxygen and nitrogen on the trace.

GRAYSON: And so this a completely stand alone instrument?

NIER: Yes, it was mounted in an attache case.

GRAYSON: You put it on the lecture desk and started taking spectra.

NIER: You couldn't tell what it was until you opened it up. It had some batteries built into it and of course you had to plug it into house current to run it for a long time. But it had a stand-by battery if I remember correctly for running for a little while, but unfortunately the instrument had a slight leak in it so it was sort of race between us and the vacuum.

GRAYSON: Is that instrument still around someplace?

NIER: Well, we built another version of it here for demonstration purposes and I don't know what's happened to it. That was directly related to this one that we built for Jet Propulsion. At the time we said, "Gee we ought to have something like this of our own here." That didn't have the built in pump; it had a separate pump on it. I remember I was visiting NASA [National Aeronautics and Space Administration] at that time; and John Nagle who was the Associate Administrator for Research. He had been a student here before and I knew him; in fact, I had him in one of my classes. I remember meeting him on the lower floor while I was waiting for the elevator. He just came down and I was showing this instrument to some of my friends. I was trying to get some money in the rocket business at the time; going to see some of his underlings trying to convince them to support our work. I met the boss [Nagle] coming out of the elevator. I remember opening the case up on the ground floor lobby of the NASA building and gave John a demonstration right on the spot. [laughter] And there was a crowd gathered around to see this little instrument in an attache case. This would have been around 1962.

GRAYSON: So you did a little sales job right there!

NIER: Right, a little sales job right on the spot.

GRAYSON: It would be interesting to see if you could take that attache case through airport security today!

NIER: Oh boy! I read in the paper that they are going to ban all electronic devices on airplanes. I think this is a moment of hysteria. But after what happened. [The 21 December 1988 Pan Am Air Flight 103 bombing over Lockerbie, Scotland occurred several months prior to this interview.] This includes tape recorders and all that kind of stuff.

GRAYSON: Well I packed all this stuff up to bring up here and I'm thinking, "Gee whiz, I wonder if I am going to have any trouble with any of this". But it went right on through. But for a while, I was a little bit concerned about carrying this stuff around. I think your right about this hysteria.

NIER: I don't think there is anything more here. [papers shuffling]

GRAYSON: No, I don't think there is anything more here. Do you Tom? [laughter]

NIER: Now here's the classic picture of the first 60 degree instrument—the 1940 machine. That's the first one.

GRAYSON: So there is a little rail here on the floor.

NIER: Yes, these were cheap casters. They were sort of lousy as a matter of fact. The ball bearings were kind of loose; we could have used more expensive ones. But it permitted you to slide this (the magnet) away and you could bake it. And this was an adjustment source magnet. Some people still use these for diddling with the electron beam so as to get more sensitivity. The electrometer tube was either an FP-54 or the Western Electric equivalent. This could be evacuated to cut down noise. We had humidity problems, there was no air conditioning in the labs.

GRAYSON: Where was the collector slit in this instrument?

NIER: Right in here. That would be in the diagram in the 1940 article. And then there is an extra lead which went to the familiar battery for the suppressor to stop the secondary electrons. And this was a little electromagnet used for the magnetic field for the source. Later ones used permanent magnets.

GRAYSON: This was permanently heated?

NIER: No, no. That was just for baking. This oven was left on because it was a nuisance to take off. This was asbestos by the way. [laughter]

GRAYSON: Is this the ‘take-off’ arm for the vacuum?

NIER: Yes.

GRAYSON: This was a singly-pumped vacuum?

NIER: Well, maybe. There may have been a second vacuum system on the rest of the analyzer. That may have been on the inlet line where you let gas in. Maybe it was pumped through here, primarily.

GRAYSON: Looks like maybe that was the primary analyzer pump. Any other of these you want to take up to xerox-graphy.

NIER: Here’s the negative of this.

GRAYSON: Okay, let’s include that in our pile of negatives.

NIER: I’m still missing a picture of me at the console. [paper shuffling]

GRAYSON: Oh yes, we were going to get a copy of the early spectrum of benzene. That would be in a notebook I guess.

NIER: I have it, but I don’t know where it is.

GRAYSON: Okay.

[Nier walks around room getting things from file cabinets. Finds picture of Bleakney.]

NIER: Oh, here's a picture of Bleakney. I wanted to get a picture of him. He was about eighty-two or eighty-three at the time. All he could send me was this. He had no pictures except this one that was taken on a golf course on his eightieth birthday or something. [laughter] Ah! Here's the one I'm looking for.

GRAYSON: Very good. Intrepid scientist at the plywood console. [File cabinets opening and closing in background. Discussion of getting lab notes. Pictures being taken in the office.]

Basically this sample [of potassium] was from the experiment which was started out but never completed.

NIER: We did succeed in enriching a small sample in the potassium-41 isotope. We checked it with the mass spectrometer.

GRAYSON: There is quite a bit there. All ready to use in case someone wants to do an experiment with enriched potassium.

NIER: There is about 250cc there. Oh, wait a minute. You wanted to see an electrometer tube. These are things I didn't know quite what to do with.

GRAYSON: Well, I'm the same way unfortunately.

NIER: Here is the Western Electric equivalent electrometer tube. That's what we always used in those days. We painted Aquadag on there to shield it.

GRAYSON: So, the Aquadag was to keep out electronic noise.

NIER: Well, just to keep charge from building up on the glass.

GRAYSON: Okay.

NIER: This was the input, the important lead. The FP-54 was the General Electric version of this. This is the Western Electric version, which I happen to use all the time. During the war,

General Electric was making these things. These are equivalent; it was a matter of six of one or half dozen of the other. These are several I bought just after the war.

GRAYSON: Were they hard to come by.

NIER: During the war, very difficult. Because they quit manufacturing them because they were not considered important for the war effort. It was only crazy people like us that used them.

GRAYSON: I see.

NIER: They were expensive back in those days. I've forgotten what we paid back in the late 1930s. I think it was like 50 dollars—so, that was real money. I have better version of the early spectrometer tube. This one is a really early flight tube because it isn't flattened as far. This one was probably about 1940. (Figures 39, 40) You see the filament came in the side here; and the gas inlet was here. The deflector end was here. This was copper tubing that went all the way in here and here are the kovar cups that went over the outside.

[END OF AUDIO, FILE 4.3]

NIER: This is an original hydrogen tube. This is the real thing. It's got a little bit of dust on it. It is one of the war-time articles...two collectors.

GRAYSON: Okay, why don't we take that back, too, and dust off the stuff and I'll see if we can get some close-ups.

NIER: Well, what we came in here for was one of my original 180 degree ones from Harvard. It may have been rebuilt here, but its primarily all there. This was on display somewhere. It's all wrapped up. We can take one cover off.

GRAYSON: This is rather elaborately packaged. That would be excellent. (Figures 41, 42, 43)

NIER: Now this one I've never unpacked.

GRAYSON: It looks like it's all packing.

NIER: I may have taken it out and saved the packing. Maybe the tube that we were looking at was the one that was in here.

GRAYSON: Okay.

NIER: I think that was it. See we made a number of those and that was one of the very early 60 degree ones. The original one that's in the photograph was destroyed, a long time ago. I know that was never saved. And these are various versions of things we'd built in the 1960s or so, and didn't put an inventory number on it. (Figure 44) This was a small 60 degree one with a permanent magnet. We gave away a bunch of this stuff to chemistry people that were interested. But this would have been one of the early 1960s instruments, small instruments. Interesting that the inventory people put a number on it.

GRAYSON: Yes, they also have an interesting operation in our organization.

NIER: We must have the same sort of thing. [laughter]

GRAYSON: You know, they came around and put an inventory number on a piano! You know, anything you can remove.

NIER: Yes, right. This is a historic thing here.

GRAYSON: So what we need is a phillips screwdriver, so we can get through.

NIER: Well, we have screwdrivers down in the lab.

GRAYSON: Now, this is a storage room here, I guess?

NIER: Yes, sort of. [laughter] [walking through room] There is too much junk here. Were you at the [1984 ASMS] meeting in San Antonio [Texas]?

GRAYSON: Yes.

NIER: You may remember Harry [J.] Svec had a display there.

GRAYSON: Yes.

NIER: He had some of this stuff too. But I don't know what he's done with it. They were interested in setting up some of these historic items. A guy at Oak Ridge, whose name I don't remember, starts with a "G" or something, I don't remember.

GRAYSON: Glish...Gary Glish?

NIER: Glish, yes. There was a note I saw somewhere asking for historical things and I said, "Gee, I've got these things, are you interested?" He was, sort of, overwhelmed by it. He didn't realize the feedback he'd get on this sort of thing.

GRAYSON: Yes. Well, I inherited his job.

NIER: Oh, I see, okay.

GRAYSON: So I'm, kind of, following up on these things.

NIER: Yes, well, we have the stuff as you can see. Now, you may not know, or maybe you do, my original tube for separating U-235 is at the Smithsonian [Institution]. It's been given to the Smithsonian.

GRAYSON: Okay, this is the one that you said that you had given to them and they had misplaced it.

NIER: That's right.

GRAYSON: Then, what happened?

NIER: Well, what happened, they found it again. And then they mentioned in a letter that we hadn't formally ever given it to them so it didn't really belong to them. They hadn't displayed it, except at an anniversary of something. Well someone else, George Washington University or some such wanted to borrow it and the Smithsonian couldn't lend it out since they didn't formally own it. They couldn't lend out somebody else's things. So I followed up, and sure enough, the regents had approved it, but it had never been mailed to them. Well, I figured I might as well get it back, but I got a letter from the Smithsonian reminding me that it would be so much better off in their hands. Something about the Smithsonian will be here long after I'm gone. [laughter]

GRAYSON: [laughter] It's a real nice thought they put into it.

NIER: Yes.

GRAYSON: Okay, now you were going to dust off some of these things and open up them up.

[recording paused]

NIER: This is one of the hydrogen tubes. This would date back, during war-time.

GRAYSON: Now, putting that together inside the glass envelope must have been a challenge.

NIER: Yes, just like putting a ship in a bottle. Well, here is the tapered seal.

GRAYSON: Ah, okay.

NIER: This was to make it easier to put the electrometer in.

GRAYSON: Okay.

NIER: You just slipped it on.

GRAYSON: So, this seal has nothing to do with the vacuum?

NIER: Nothing to do with the vacuum.

GRAYSON: It's just a way of mounting...

NIER: A way of mounting

GRAYSON: ...the electrometer. So that's an interesting use of the glass vacuum seal.

NIER: Yes, Here's the source. This is the standard source that we built in those days.

GRAYSON: Now, the insulators are just pieces of glass?

NIER: They're matched for size. This was before the glass-ball technology, which the Consolidated people pioneered. We've now adopted it for everything. The filament is missing. The filament would have gone in through this stem and down here, this is the filament.

GRAYSON: Okay.

NIER: So, the filament opens out into here.

GRAYSON: I wonder if I can [photograph] it from the other side.

NIER: The filament should be here. The magnets are little horseshoe magnets, bought from Central Scientific. They were painted red, because that's the way you painted magnets: red. [laughter]

GRAYSON: A magnet is a red thing. There's a series of magnets there, is that correct?

NIER: They're all in parallel, to get enough field.

GRAYSON: Yes.

NIER: Now, the commercial version was exactly of the same construction as the first leak detector. We used one of these, and GE then, had solid blocks of Alnico. They put a rectangular block of Alnico down, which is obviously the thing to do. You wouldn't want to buy these little horseshoe magnets. [laughter]

GRAYSON: So, that is the whole works...

NIER: That's right.

GRAYSON: ...including the magnets, so basically, if you pumped it down and put a filament in it.

NIER: You'd be ready to go. I've forgotten exactly what the field strength was. The power supplies we used were usually 375 volts, which was a good acceleration. And the way you got that was...maybe it was 300 volts...with these VR tubes, fully regulated gas-filled tubes. If you put a resistance in series with them, you could get a constant voltage. So, it was good enough for our purposes, for the required resolution and all that.

GRAYSON: The extra length here, did that serve any purpose?

NIER: No, just to make it easy to seal it up.

GRAYSON: So, you were always dealing with the problem of the glass seals.

NIER: To put it together, you had to drop the pieces through here, and then this was put in at 60 degree to it with a long screwdriver and screwed together. You see, the parts were all put in on three sides and was supported by this. You had to do a pretty good glass-blowing job for making these assemblies.

GRAYSON: Yes.

NIER: But, you see, there was some leeway with these rings and so on. And this would be sealed off last.

GRAYSON: Now, these seals, did you make all of these?

NIER: Our laboratory took care of it.

GRAYSON: These were graded seals?

NIER: Yes, these were uranium-glass.

GRAYSON: Oh, uranium glass. What year would this instrument be?

NIER: 1943. 1942 or 1943.

GRAYSON: Okay.

NIER: Another glass we used was cobalt glass. These left a blue ring. These glass-blowers were sort of artists, they used different combinations of glass, and sometimes they didn't know quite why. We had some great glassblowers. This is a very good graded seal here.

GRAYSON: Kovar came on the scene somewhere in this time period?

NIER: No, it already existed for several years. Because I remember, I became acquainted with it when I worked at GE in 1936. They called their version "Fernico." It was an iron alloy.

GRAYSON: Okay, sort of like an iron-nickel-cobalt alloy—hence the name.

NIER: Yes, the GE version of it. Kovar we bought from a place in Pennsylvania.

GRAYSON: Now, we're looking at this other machine with a graded seal at one end and a copper tube that goes on up...would that be the detector?

NIER: Yes, of course.

GRAYSON: Was this to shield against stray fields, I mean, the use of the copper tube.

NIER: No, it was just part of the analyzer tube.

GRAYSON: Okay.

NIER: The magnet would have been here, so it was just an intermediate place to put it.

GRAYSON: Sure.

NIER: I think it was arbitrarily five inches.

GRAYSON: I see. And this machine would have been what vintage?

NIER: This would have been 1941.

GRAYSON: Okay, so, but earlier than the other one.

NIER: Yes, this was later. Some of these had sophisticated sources. This was a simple one; but it already had the focusing plates in it. The split plates for focusing ions.

GRAYSON: You had a half-plate design.

NIER: Yes. And then, you could steer the beam one side or the other and do focusing that way. I need a clean piece of paper there. [laughter] [Looking around the storeroom for a specific instrument.] It was on loan. A local guy who was a consultant in vacuum technology, borrowed it for a meeting of the American Vacuum Society. They had an anniversary a few

years ago, at a meeting in Boston [Massachusetts] if I'm not mistaken, and I loaned him a tube. They displayed various historic instruments. And that's what the packing in that box that you saw was about.

GRAYSON: Okay. Do you know if there would be any of the earliest leak detectors around? The glass leak detectors?

NIER: I don't think so. I don't know. You see, I never got to have any of that stuff after the war.

GRAYSON: Was that because it was pretty much involved with the Manhattan Project.

NIER: Yes, that was all Manhattan Project. I'm sure somebody got it. We didn't.

GRAYSON: Well, as I mentioned, the fellow from France asked if I could collect some pictures; which we already have now...of the Nier files and photocopies of some of the very important machines. That General Electric box that you showed me, that was what was used at the K-25 plant?

NIER: You mean, the thing on wheels?

GRAYSON: Yes.

NIER: Yes, that's the real thing.

GRAYSON: That was what checked the leaks?

NIER: Indeed. And they made hundreds of the instruments, I don't know how many hundreds, a number like five hundred sort of sticks in my mind. They were used elsewhere in the Manhattan Project too. Of course, immediately after the war, GE sold them.

GRAYSON: Yes.

NIER: Commercial instruments. But they didn't follow up on it. Well, it was too small of an item for a company like GE. You see, there's a place for the smaller instrument companies in this whole business.

GRAYSON: That's, what happened to CEC. They went through a number of owners before they got to DuPont. And, it turned out that, the mass spec business was much too small of an operation for DuPont. And they got rid of it. It's kind of distressing. I find it somewhat interesting, that, in this country, there isn't really a premier...

NIER: That's right, that's right...

GRAYSON: ...high-performance instrument manufacturer of mass-spec instrumentation like you have in England, France and Germany.

NIER: ...and Germany, that's right.

GRAYSON: But, not the United States.

NIER: At one time, I was trying to interest Honeywell in making instruments. But, Honeywell's far too big. It's the same story.

GRAYSON: Yes, it's not a dollar-volume operation.

NIER: That's right.

GRAYSON: Well, I guess that's one of the "cons" of capitalism, I suppose.

NIER: Yes. [laughter]

GRAYSON: [laughter] The guy looks at your product and says, "Gee, that's interesting, but you're not going to make enough money for me. Go away."

NIER: Yes. I don't know if this is worth doing any more now, only two or three pictures,

maybe.

GRAYSON: I was going to shoot a few more.

NIER: Yes. Well, I'd like to take one more look for that box. I may just have peeled open the wrong part of the box.

GRAYSON: Okay. [recording paused] Now, this particular device, is...?

NIER: Well, this is a 180 degree instrument, and is one of two that I brought back from Harvard in 1938. That was used for the original work on lead and all these other isotopes.

GRAYSON: Now, these were made at Harvard?

NIER: Yes. The glass-blowing was at Harvard. The parts inside may have been changed or modified from time to time. But you would recognize this in the photograph of the Harvard instrument. The collector and the electrometer were waxed up. We kept a vacuum in the electrometer case, because of the humidity problem. So, that would be waxed up. That's why the black wax. We painted the inside with Aquadag.

GRAYSON: How would the magnet fit, all around it?

NIER: All around it. This was a 180 degree five-inch radius, flight-tube and the magnet had 12-inch diameter. The magnet extended out beyond here, the magnet pole was larger than this.

GRAYSON: I see.

NIER: Then, the aquadag would reach out beyond the magnet. You could slip aluminum foil over the rest of it, to shield it. These connections were made to the various power supplies. This instrument appears to have a slight modification. This one may have been one modified here during the period, 1930 to 1939, because of the focusing plate arrangement. But the housing was the original housing.

GRAYSON: I see.

NIER: And the copper tube, and that quartz oven, are from the original instrument. The oven was used to bake out the instrument originally. We always baked out the metal parts in those days.

GRAYSON: What's that nice discolored piece, right there.

NIER: Oh, I think it was heated so hot that the copper evaporated and became discolored.

GRAYSON: Oh, my!

NIER: I also had historic samples of glass melted against the copper, where the furnace shorted at some time or the other, and it got too hot. Come morning, the glass had collapsed and was completely around the copper. I had wonderful moldings...

GRAYSON: [laughter] Modern art?

NIER: Modern art, yes.

GRAYSON: Yes.

NIER: But, the bulk of this instrument is from 1937.

GRAYSON: So, these were then shipped to some place recently for some kind of display?

NIER: Well, this may have been on display somewhere. I discovered this up in our lecture-demonstration room, as an example of a mass spectrometer tube.

GRAYSON: Oh.

NIER: A lot of this stuff was just thrown on benches and every lecturer coming in would use it. But then we decided to salvage it.

GRAYSON: Well, particularly since it did some very important work. Something that's not so easy to see from the pictures is that there is a copper tube inside.

NIER: Yes, there's a copper tube. The other thing that's interesting is that you had to allow for the ions to bend as they started. This part is not at right angles to the end. Remember, everything is in the magnetic field, so they start bending right away. In the paper describing this instrument in that first work: mercury, xenon, krypton and so on, I think the angle was 2.8 degrees or some number like that. But this, as you can see, was taken apart quite a few times. And this was mainly my glass-blowing on this part. Usually it was.

GRAYSON: This was to replace the filament.

NIER: Replace filaments, for example.

GRAYSON: Did you find that job to be one that you didn't really like?

NIER: Well, you accepted it as a certain necessary part of the job. But you never thought about it. You just did it and that was it.

GRAYSON: Yes. That's a nice little transport and display case.

NIER: Yes, I may have lent it to somebody around here for something. I don't think this one was ever shipped. But as I say, if you'd arrange to get them into a suitable historic place, I'd be more than willing to give this stuff up. Because, I'm afraid of what's happened to other historic things around here. New young post-docs come in and they've got to find space and they throw the junk out.

GRAYSON: Yes.

NIER: We've thrown away priceless things.

GRAYSON: Well, there is a collection problem, but I'm in contact with someone from the Smithsonian staff, who is interested in collecting some of these things.

NIER: Well, it may be that this would be suitable for the Smithsonian. We may joke about this business, but the Smithsonian *will* be here. [laughter]

GRAYSON: Yeah.

NIER: So, for something like that, it might be...

GRAYSON: Well, I tell you, what really would be a good thing to do if you can, is to write a little bit of a documentary on the instrument and include those papers with the machine. That would preserve the essential importance of the instrument. For instance, this machine here, which did some very fundamental work at Harvard at an important period in your life and in an important period of isotopes could be documented in this way. Basically, their interest is in collecting instruments that have historic significance, rather than just collecting instruments for the sake of collecting instruments.

NIER: Yes.

GRAYSON: As you can well understand, they have a limited amount of space.

NIER: Well, they'd have warehouses full of them!

GRAYSON: Yes, yes. And as you are aware too, just trying to keep track of all that stuff is a big problem. But this particular piece of equipment here, I would think would probably be of particular significance.

NIER: Well, there is the hydrogen tube, the original 180 [degree] one, which was one of these very early ones, and one of these 1960's.

GRAYSON: Well, you know better than anyone which of these would be the most significant pieces of equipment, and if you could document that, then I think there is a good chance of placing these machines in a place where they can be preserved. [A conversation about tools takes place while the case for one of instruments is reassembled. Another package is opened.]

Now, this is a 60 degree sector tube. (Figures 45, 46)

NIER: Yes, it goes back to the period immediately after the war. [...] This may have been one from about 1946.

GRAYSON: Well, it's got the blue cobalt-glass seals. Maybe if we just lay it down here, we can get a better contrast. There we go. Is this stainless steel?

NIER: I don't know if that's stainless or copper.

GRAYSON: I don't know. It looks to me like stainless.

NIER: That had to be done immediately after the war.

GRAYSON: Now, this little business, here... what is that about?

NIER: This is a tubing pressed here, and flattened.

GRAYSON: That's very nice-looking.

NIER: Yes. Well, we got very good at this, our man Thorness, who was head of the shop, devised interesting ways of bending tubing and was very clever. What he did was, he filled the tube with water, and froze it with dry ice, rather than using lead or Woods Metal or something else. This way, he had no trouble cleaning up after the bending. After all, this had to be used in a vacuum afterwards.

GRAYSON: Yes.

NIER: People who make furniture don't have to worry about that. And then you could bend it, and it didn't kink. To flatten it, you melted the ice, and then soldered caps on the end and pumped it up with water again. As you flattened it, the volume became less, so the water became compressed, and you wouldn't form the dimple in the middle, which is what happened normally if you tried to flatten it. It also gives you that figure-eight cross-section.

GRAYSON: Now, you did publish a couple of papers with the results from this instrument.

NIER: We published...I think we called them “notes” on this technology.²⁹ I don’t know if anybody ever made use of them. This way of mounting the filament was used initially, and then when it came to those things that we called “line recorders” in the Manhattan Project, we remodeled the filament mounting. Rather than a stem coming out of the end, we mounted it like this.

GRAYSON: So, to replace the filament, you would break the glass right here.

NIER: Break that off, and pull this out.

GRAYSON: Now, how much did you have to pull out?

NIER: Well, there were some long screws. I think you reached in with long screwdrivers and the whole source would come out. And then you went in with tweezers, to pull the leads off here. It also has the focusing plates in the source, which we did not use before the war.

GRAYSON: These are like half-plates.

NIER: Yes.

GRAYSON: Now, did you publish the half-plate concept?

NIER: In 1947, it was published. But it was used on that carbon instrument, the so-called Rockefeller instrument before the war.

[END OF AUDIO, FILE 4.4]

GRAYSON: The publication of the paper was delayed for some time because of the security situation [due to the Manhattan Project]?

²⁹ Alfred O. Nier, “A Mass Spectrometer for Isotope and Gas Analysis,” *Review of Scientific Instruments* 18 (1947): 398.

NIER: Yes. The concept of the half-plates was developed before we were officially involved in the Manhattan Project.

GRAYSON: The concept of the half-plates is quite common in magnetic sector instruments today.

NIER: Well, everybody adopted it afterwards.

GRAYSON: Each of these plates had a tab welded to it so that you could make electrical connections.

NIER: Yes.

GRAYSON: These details are important, I think, because so much of this business depends on these clever techniques for doing things. Are there any other things that we should be taking pictures of while we are at it.

NIER: I suppose some of the more recent rocket instruments.

[The interview moved to a different laboratory to take pictures of some of the more recent small mass spectrometers.]

NIER: There is a lot of junk here!

GRAYSON: I'm sorry, what was that? [laughter] This wooden arrangement here was a mockup of the real instrument which was used to lay out the thing in design.

NIER: The spectrometer fit in this space. The whole mass spectrometer with all of its electronics went from here to here in a nine-inch rocket. We had two of those in a single rocket.

GRAYSON: So, this box would be full of electronics.

NIER: Yes. I've also got a spectrometer I see here. A whole mass spectrometer. I have to be careful how I pick it up here so I don't cut my fingers. [Nier holding an instrument in his hand.] (Figure 47)

GRAYSON: Now is this an even more miniaturized version?

NIER: Yes, this is the smallest one we ever built. It has a one inch radius. (Figure 48)

GRAYSON: This looks like it has a Mattauch-Herzog geometry.

NIER: Yes.

GRAYSON: Oh my goodness, that is interesting. I've never seen anything quite that small with Mattauch-Herzog geometry.

NIER: The magnet's missing here.

GRAYSON: Yes, obviously.

NIER: But then it had two electron multiplier tubes in it by the way.

GRAYSON: And it looks like it's bakeable.

NIER: Yes. See, here is a little ion pump. It is just a small commercial one, not that good. It was only used for standby while you were waiting to ship it. Then there was a getter. A cartridge went in here with titanium wire so you could flash it just as you took off. You flashed the wire at the initial moment when it opened because there would be a surge of gas. You are still at pretty low altitude, about 90 kilometers, and the pressure would be pretty high, somewhere around ten to the minus four. To get the surge of gas which the little pump couldn't handle you would flash the titanium wire.

GRAYSON: That was to get you started after the instrument was first exposed to the atmosphere it was supposed to sample.

NIER: Of course, it wasn't necessary after the initial opening of the instrument. We got nothing out of that flight because a wire had gotten pinched. At the critical moment, it failed from the vibration and shorted. It was a timer that ran all of the electronics. The whole flight was a disaster. It was our last flight and it was a tragedy. It was just carelessness in the assembly. You don't get a second chance like you do in the lab.

GRAYSON: Oh, I see some stuff from Bendix aerospace down here.

NIER: Here is one of our leak [detectors] that we developed during the war for our uranium work. It was a flattened tube wrapped around a shaft which was driven by a worm gear. (Figure 49) And this was what was used during the early days of the war for adjusting the flow of the uranium hexafluoride. We wanted copper because there was very little reaction, and you could adjust the flow by adjusting the worm drive. And later, people improved these things, using nickel tubes and other alloys.

GRAYSON: How hard was it to flatten the tube and coil it up for one of these?

NIER: It was an art involved in making them, but after you learned the art, it was very easy to make them successfully.

GRAYSON: So when would this have been manufactured or used? In 1940?

NIER: It was in our original instrument that we designed. We were using these in 1941. These valves were made by the Hoke people. They had a diaphragm that was welded around the edges like a refrigerator valve. They were developed for the Manhattan Project originally. I rediscovered this in some interesting correspondence from the war period when some patent question came up a few years ago. (Figure 50) A legal beagle sent me some of the correspondence and I discovered something which I had completely forgotten. I was the allocator for the United States for these valves during some period in 1944 or 1945.

GRAYSON: In other words, if there were any to be given away, you had to approve the use of them.

NIER: I doled these things out ten or twenty at a time. And of course once they got into production, there wasn't a problem anymore. They had to be used in various places and so what often happened, as it does in the case of red tape, they would treat the request for one the same

as a request for a thousand. You turned it over to some bookkeeper. I was given the responsibility for passing on these things, and I discovered there was all of these pieces of paper with my signature on them. I'd say, "Yes, so-and-so can have ten, someone else could have 25 [inch] and so on. We worked with the Hoke people. The Kellex and Hoke people were very cooperative in developing the valve.

GRAYSON: This is once again relating to the fact that you had this corrosive gas that you had to control and meter.

NIER: Yes. They probably had Monel housings, if I remember correctly and probably Inconel diaphragms. These various alloys were very good in this application. Then Crane made a little valve which was sort of equivalent to this which had a little bronze bellows in it. It was smaller than this and it was also a nice little valve. The Hoke people were located in Brooklyn [New York] if I remember correctly. The president of the company was a guy named Teeters; and he was really a hands on guy. If a fancy machine went down, he would put on his overalls and repair it. So he did everything. He was a chemical engineer by training and he had been hired by Mr. Hoke and later became the president of the company.

GRAYSON: I know an older fellow in ASMS by the name of [Richard M.] Teeter, and I wonder if there is any relation.

NIER: Well, maybe it is his son or something. Teeter was older than I was. Teeter became the president of the company during the war. Could be that he is related to your friend.

GRAYSON: Are there any other things that you want to show us while we are here?

NIER: Well, there are a few other instruments here.

GRAYSON: This looks like a 90 degree sector.

NIER: Yes, but it is a double focusing. This even has adjustable slits.

GRAYSON: Pretty high technology stuff!

NIER: It is a 90-90. Here is the source; the 90 degree electric analyzer here; and there is a 90o magnetic analyzer here followed by the collector system here. (Figure 51) And I don't know what this was used for! [laughter] This would have been around 1960.

GRAYSON: The size suggests that it was used for some kind of miniaturization study.

NIER: Yes, it was. And it may have been a prototype for some of our work in preparation for rocket flights. It might have also been used for rare gas studies of meteorites. I'm sure I can find notebooks that refer to it. Notice that you could adjust the width of the slit, and the output of the electric analyzer. It was sophisticated in that sense. We made our own flanges. These were not miniflanges. We did not use ConFlat flanges; we had some other schemes that we used because we were developing our own for our own purpose. They were considerably cheaper than ConFlats, but they were not as good as ConFlats.

I recognize that. This is a miniature Nier-Johnson built in the early 1960s. It was a miniature instrument when I considered the possibility of flying these in rockets using a double focusing geometry. In fact, we flew one of this kind. There is a publication on this.³⁰ I have a reprint I can give you.

GRAYSON: This must have been fun.

NIER: Yes, in those we had leak problems. When they soldered them, they didn't do a very good job. Here was the source magnet. This is a kind of a Rube Goldberg arrangement.

GRAYSON: I don't know. It looks pretty good to me.

NIER: Here is a prototype that led to the Viking [Lander Spacecraft] instrument that went to Mars. So, this is one of the first very miniature ones that was used when we were studying the idea.

GRAYSON: The geometry is a little different.

NIER: Well, this was a double focusing which is a smaller version of the one we just looked at.

³⁰ Alfred O. Nier, "Small General Purpose Double Focusing Mass Spectrometer," *Review of Scientific Instruments* 31 (1960): 1127.

GRAYSON: But there is a reverse angle on the first sector.

NIER: Yes, this is probably a Mattauch-Herzog geometry with just a single collector. That what this was. See, this had the glass balls in it. We had now adopted the glass balls. They raised the price on the pyrex balls, so we changed over to sapphire. All of this instrumentation is from the early 1960s.

GRAYSON: That would have been a single mass Mattauch-Herzog instrument?

NIER: That's correct.

GRAYSON: That explains the physical layout.

NIER: Here is another one. It has the Johnson-Nier geometry. (Figure 52) This may have been used by one of my colleagues for some of the rare gas work on meteorites. We built a number of these at the time. Probably a two-inch radius for the magnetic field.

GRAYSON: This would be the pump here.

NIER: Yes. A standard commercial pump.

GRAYSON: The magnet technology has advanced since the days of five kilowatts and all that.

NIER: Yes.

GRAYSON: Do you have any feeling if your requirements for magnets forced any developments among the magnet materials people?

NIER: I doubt it. I think the thing that forced the magnetic material people was NMR [Nuclear Magnetic Resonance Spectrometry].

GRAYSON: Oh, really. You think that that had a stronger influence.

NIER: They wanted more uniform fields.

GRAYSON: Yes, well a uniform field is extremely important in NMR.

NIER: I have an engineering model of the Viking instrument in the lab.

GRAYSON: This really isn't your lab! I was just looking at all those boxes over there that are interesting.

NIER: Well, I have a lot of reprints and stuff like that. I might have a picture of Mattauch here. I can look real quick. [papers shuffling] Here's my correspondence with Mattauch. The Max Planck Gesellschaft sends out a black bordered announcement when people die. His eighty-first birthday was in 1976. Somebody got me a nice photograph of him, but I can't find it. Here is a nice letter, a long-hand letter.

GRAYSON: Did he write in English?

NIER: Oh yes, he was very good. This was written in 1951. He was in Switzerland before the move. He's talking about these doublets in 1951. Here's a letter from 1948.

GRAYSON: Germany...French Zone, that is an interesting concept [on the letterhead]!

NIER: Yes. There's decades of correspondence here.

GRAYSON: These papers are fascinating.

NIER: If you have any ideas about where to put this stuff. Of course, our University Archives are interested in the papers. The important thing is to know where the papers are for reference. The American Institute of Physics for instance wants to know these things. They aren't trying to collect stuff necessarily. They like to know where things are so that when somebody makes an inquiry, they can refer them to the resource. There is a reprint here I want to give you. See, here is Nier and Roberts on the original double-focusing instrument. Here is a file on medical things when I was dealing with the medical instrument. Here, here is a box that we want to take down.

GRAYSON: Pre-1936 data! Can we look inside that and see just what lurks in pre-1936.

NIER: I didn't use notebooks back then. Everything was on loose leaf paper. You asked about the benzene spectrum. Here it is.

GRAYSON: Can we get a Xerox of that.

NIER: Why not? (Figures 3, 4)

GRAYSON: That's a nice piece of original work.

NIER: Unfortunately there's no date, but it does refer to a page. Page 25 or 24. Here are the readings you took from the galvanometer. 21 April, Easter Sunday, 1935. How do you like that!

GRAYSON: Let's get a Xerox of that too.

NIER: I have a nice print of that spectrum, but heaven knows where it is. I think the thing to do is make a list of these loose ends, and then I can hunt them up. Let's just look a little further here. Here's all my hardware data; some of the early notebooks. There's all kinds of things here. At one time I had a little research project with the Kellogg people to develop a hydrocarbon instrument. This was pre-Consolidated. But I was so busy.

You asked earlier about my failures. Well this is one I never came through on. We never finished it in time. And then Consolidated came out with their instrument. I was ahead of them at the time but with all the things going on, it just never happened. Well, there is a mixture of junk and mixture of valuable things here.

GRAYSON: Ah yes, what is that old story about one man's junk is another man's treasure.

NIER: Yes. Here is a load of periodicals; most of them I've given away.

[Interview moves out of the store room.]

GRAYSON: Here's some mail that you don't want to leave behind. I don't know if it's important or not.

NIER: Oh yes. That's a very important memorandum from the Department Chairman stating that the Department is against changing the semester system to the quarter system. This is a draft for us to comment on. [laughter]

GRAYSON: Business as usual, huh?

NIER: Well, I think it is a crazy idea. There's one other reprint I want to see if I have here. [papers shuffling]

This is an instrument that we are doing some testing on at this time. We admit the gas through a series of capillaries with the idea that this could be used for sampling planetary atmospheres. You let the gas through a series of tandem capillaries with pumps in between; small sputter ion pumps. A molecular beam is formed and you put a flag in the beam and measure the background. In that way you should do a better job distinguishing between the sample and the background in the instrument itself.

GRAYSON: This would be for atmospheric studies?

NIER: Yes, for example for a probe going to Titan. It might be useful also for analytical purposes; because you might be able to do traces of water for example. Actually, it doesn't work quite as well as it should. Here is a standard 20 liter Varian triode pump. You need the triode pump because the competitors diode pumps with slotted cathodes isn't worth a damn for rare gases. They brag about them, but they're no good. We had to develop our own here. This is a miniaturized version of the Varian one which we made ourselves. This one has a large standard magnet, but you can miniaturize this for a flight instrument. We built four of these pumps and put them on different places on the instrument. That pretty well covers this project that we are working on.

GRAYSON: Where is the mass spec. in this whole thing?

NIER: [laughter] The mass spectrometer is right here! It's a two and a half inch instrument.

GRAYSON: Well, all I can see is pumps! A pump here, a pump there...

NIER: Now over here is an instrument that we published on in 1985.³¹ We had money from the National Science Foundation on a cooperative project with the geology department. This instrument is primarily used for doing the ratio of helium-3 to helium-4. This is a Mattauch-Herzog geometry. We built this for geology and they never came through with their end of it, and it's been over here ever since. People lost interest in it, or changed jobs, or whatever...so, I still have it. I'm not anxious to give it up, on the other hand, I'm not trying to keep it. It's safer here. We use it as an auxiliary instrument for testing stuff.

GRAYSON: So, the detection scheme is...?

NIER: The detector is over here and there are multipliers that connect here. They are not connected up now; that's an electrometer. This is an experimental thing that you can put four multipliers on or four faraday cups. It is a multiple-slit instrument.

[END OF AUDIO, FILE 4.5]

NIER: Let me show you the cross-section here: Our standard ion source; some focusing plates, z-plates; a 31 degree electric analyzer; the magnetic analyzer; and three collectors; one for mass two, three and four.

GRAYSON: Okay, now, each of these is still the same fundamental design in terms of geometry.

NIER: It's all Mattauch-Herzog. That's all we do now: Mattauch-Herzog. And this shows three multipliers, but right now there are only two, but that's beside the point. This is the standard thing we're working on these days.

GRAYSON: Is this a scale drawing?

NIER: Yes, this is the size. That's been published in 1985 in *RSI*. (Figures 53, 54, 55, 56, 57) As I say, this was developed for geological work, but it turns out that we never used it that way. We did a little bit to help them but it was never moved to geology. I still have it, they're not

³¹ Alfred O. Nier and D. J. Schlutter, "High Performance Double Focusing Mass Spectrometer," *Review of Scientific Instruments* 56 (1985): 214-9.

crying for it. What would happen, if it went over there, is people would steal components off of it. So, since nobody would take it, it was nobody's baby...

GRAYSON: Sure, I understand.

NIER: This is our own magnet design.

GRAYSON: That's a very nice, clean-looking little design.

NIER: This is the electronics that we built for the job. It's sort of professional-looking, as you see. And it worked pretty well. We can almost resolve helium-3 and HD. We weren't really trying to do it because we have wide slits. But you have a decent sense of the thing. You get a peak looking...well, this is an example of the kind of peak. This is all with multipliers, and so on. Let's look at the other instrument. This is the one all our work has been done on in the last couple of years. These instruments are copies of each other.

The main thing we've been playing with in the last few years is extra-terrestrial materials. My goal was to work on individual particles that they collect in the stratosphere, with U-2 planes that have fly-paper on their wings to collect the stuff.[‡] It's more sophisticated than that, but that's the idea. They're available from NASA's Johnson Space Center in Houston [Texas]. The people at Washington University, Bob Walker, and others have made extensive studies on extraterrestrial particles.

GRAYSON: Yes.

NIER: They have a beautiful program. They have ion probes, X-ray analyzers, etc—every damn thing. I've never visited them.

GRAYSON: You should come down.

NIER: I should. There wasn't time when I came down [to speak to the Midwest Mass Spectrometry Discussion Group]. You know how this is. I should. I know these guys and I should visit them.

[‡] The U-2 plane was used initially for photographic surveillance from high altitudes for years before satellites were placed in orbit. The Russians caught one of our pilots and caused a lot of embarrassment to the Eisenhower Administration. This aircraft was designed specifically to cruise at extremely high altitudes.

GRAYSON: Yes.

NIER: They do a lot of things sort of similar to that. They haven't worked on helium. We're the only ones who have worked on helium. What we do here is take these individual grains that are too small to see. People at Houston [Lyndon B. Johnson Space Center] fold them into a little piece of tantalum foil, under microscopes. Then we put them on a flange, which you can see we have on here. We heat them up, and use an optical pyrometer to see how hot they get.

GRAYSON: Okay.

NIER: Bake the gas out, put it through charcoal traps and then into the spectrometer. We're trying to extend the work to other gases. We've been working just with helium and neon, which you can do with liquid nitrogen and charcoal. You take everything out, except helium and neon, so that goes through and you use the instrument statically.

GRAYSON: And, the interest in the ratio of these gases is...

NIER: Well, the three/four ratio is interesting.

GRAYSON: Okay.

NIER: For instance, is it primordial or has it been bugged by spallation from cosmic rays, or from the solar wind or other impregnating particles. Most of work has actually been done—not with these particles, we just started on that recently—but it's been done on samples from the deep Pacific. They pick these up with a magnetic sled dragged along the ocean floor. Something like 25,000 feet below sea level.

GRAYSON: Okay.

NIER: They pick up these particles that stick to a bunch of magnets put together. They call it a sled. It turns out that these magnetic particles apparently are debris from meteorites or asteroids or something of that sort, or maybe comet dust that settled on the earth, settled in the ocean. The reason they do it in the deep Pacific is if you're anywhere near a continent, you get so much stuff coming down the rivers; rainfall, and such, which buggers it. So, the deep Pacific is the place. We found very interesting variations in the isotopes in this stuff down there.

GRAYSON: Really?!

NIER: We worked with a guy by the name of Don Brownlee from the University of Washington. He's in the astronomy department, but he's a geo-chemist type, and he's got all of this stuff. In fact, he was the first one to work with these stratospheric particles; so now they call them Brownlee particles. They're named for him. He doesn't call them that, but a lot of other people do. He's a real nice guy. We're working with him on that problem; now we are trying to extend the measurements to argon. We've never tackled that before because the liquid nitrogen and charcoal freezes out the argon, but if you have it warmer then you can get at the argon. I think dry-ice and alcohol slush is the thing that people use. I've never done it before, but it runs about minus 100 [degrees]. Argon would come through behind oxygen and nitrogen.

GRAYSON: Yes.

NIER: So, we're just learning the technique of doing this. That's the kind of thing we have here. Most of our effort has been on this. And this guy who works for me, Dennis [J.] Schlutter, just loves computers and automated stuff. He's not the tidiest guy in the world, but most of the time, it works very well.

GRAYSON: Yes.

NIER: So, you leave well enough alone. This isn't the neatest lab in the world...

GRAYSON: Well, I've been equally or less neat, or equally as un-neat. [laughter]

NIER: This is the heart of our work for the last few years. And, oh, in here, we have something we haven't used recently, which is unique in the world. Back when we were flying instruments in rockets, we wanted to check on how the source behaved at high speeds. (Figures 58, 59) You see, with a spectrometer on a rocket or on a satellites, satellites in particular, you're plowing through the air at high speeds, about eight kilometers per second in an earth satellite. And so, the instrument really plows into the atmosphere and the density in the source will be like fifty times what it would be if the air was standing still. You're going so fast.

GRAYSON: Yes.

NIER: So, you want to simulate this characteristic. What you do is hold the spectrometer fixed, and hit it with a fast beam. You can make a fast neutral beam at this end. This was a phenomenon that was not known until the 1940s, after the war, and the people at the University of Toronto, at their Institute of Aerospace Studies studied it. I believe they've got some really first-class kinetic theory people there. We learned about this, in fact, we did the testing at their facility, where we used to send gas at high pressure through a tiny hole. It streams through, of course, and, what it does is: all the degrees of freedom of the stream are biased in a forward direction. You pick up extra velocity that way, a velocity that's characteristic of the average molecular weight of the gas. So, if you have hydrogen, then it's faster. Well, suppose you want to study nitrogen or oxygen. You put the nitrogen or oxygen into the hydrogen, a tenth of a percent or something like that, so that the molecular weight doesn't change much. Then this is swept along with the bulk of the gas, because it's high pressure. Consequently it picks up these velocities. With something that you can heat to a finite temperature, say thorium oxide, something you can heat hotter than Hades, we can get speeds of, like 6 kilometers a second. Remember, at room temperature, the kinetic speed is, like 500 meters per second.

GRAYSON: Yes.

NIER: Or 300 meters, or something like that. So, we can't get up to eight (km/s), but you can get the trend of the curves, if you run it at different speeds. So, this was built for that purpose. And nobody in the world has another one that's as good as this. But it's been standing idle...had a graduate student, John Valentine, who did his thesis with this, and he's been gone for five years. He's now at Air Force-Cambridge, whatever it's called, a geophysical lab of the Air Force. He came back to test an instrument on it this summer. It worked fine, he turned it back on again, it still works, just like when he used it. [laughter] We've not stolen too many components from it!

GRAYSON: Got some rather healthy pumps.

NIER: Yes. And we even have a liquid helium pump on there, and part of another diffusion pump. But we're pretty crowded here. It's a very nice facility. The Viking instrument was tested on this, because that entered at, what...five kilometers a second. Pioneer-Venus instruments, they went into Venus under that project, we tested those and the Pioneer-Venus orbiter was also tested on it, for the people at Goddard [Space Flight Center]. So, it's been used as a testing instrument for several missions.

GRAYSON: Okay, so, do you still have graduate students?

NIER: No. not for nine years, which is terrible. I should have kept on trying to have graduate students, but I thought, “Well, I wanted to be free, and I didn’t want to compete and we didn’t have too many good students around anyhow. [papers shuffling] That’s this instrument, that you saw. In fact, that’s an extra print, if you want it.

GRAYSON: You’re sure that this isn’t a modification.

NIER: Well, I had made it for something, but we had never used it. We were going to throw it away.

GRAYSON: Well, why don’t we throw it in *my* trashcan!

NIER: Now, that will fold to 8-1/2 by 11 if you fold it properly. They had some other paper that was left over that was a different size, but our drawing paper is multiples of 8-1/2 by 11, so you can fold stuff and file it. Now here is another instrument, and some electronics that is the companion of the instrument over there. This one has the magnet in place. This was flown on a rocket. These things have been at the bottom of the Atlantic Ocean. When the rocket came in, it sank...or, wait; this one had been in the water and was cleaned up. Its the standard small instrument, just like the one I showed you in the other room.

GRAYSON: Except with the magnets.

NIER: With the magnets. This is all complete here.

GRAYSON: And then, the source goes where?

NIER: This is the source. And the gas would go in here. See, this would go through the atmosphere, like this.

GRAYSON: Now, there was a paper, with Thorness, about opening these things in flight.³²

³² R.B. Thorness and Alfred O. Nier, “Device for Remote Opening of a Vacuum System,” *Review of Scientific Instruments* 33 (1962): 1005.

NIER: Yes, you could take the cap off. That's standard technique in our lab. We had this covered with an aluminum cap, which you cut off with a cutter mechanism, like a pipe cutter, except that it had twelve little wheels. It was run by squibs to fire it. We got pretty good at that technique. Here is a lab instrument, which I was interested in doing SIMS [Surface Ionization Mass Spectrometry] with. Here you have to deal with the problem of grounding things. You could ground the collector end, and here's an insulator between the source and the collector, so you could do tricks with it. I'm sorry, you had to float the collector end because you wanted to have this grounded. This creates problems all of itself. As you can see, you had fiberglass insulation holding parts. Looks like the magnet is missing on this one.

GRAYSON: Okay, this is still another Nier-Johnson?

NIER: No, Mattauch-Herzog. Everything we do now is that way. Mattauch didn't live long enough to appreciate this. He had been so proud, he was such a nice guy. The fact that I would adopt his design, he would have just been in heaven.

GRAYSON: This uses a gold O-ring seal?

NIER: Yes. That's the way we fastened things together.

GRAYSON: So, you think that Mattauch really...I mean, obviously you were competitive at one time.

NIER: Oh, yes. But we were the dearest of friends. In the accurate mass measurement work, we put him out of business. It was sort of tragic, because he was getting on in years, and he just couldn't compete with our electrical detection system.

GRAYSON: So, it's kind of interesting, in doing what he was doing, which was measuring the masses accurately, the Nier-Johnson geometry took over.

NIER: Yes.

GRAYSON: But then, in your subsequent work, you used the Mattauch-Herzog geometry?

NIER: That's right. You see, the compactness of his design was better. Because, if you want to fly things, where you have to shake the bejesus out of it, you wanted it as nearly like a sphere as possible; from a vibrational standpoint. And you could hardly match anything better than in this geometry, in which everything is sort of close together, you see.

GRAYSON: And the fact, too, that you have a focal plane...

NIER: Yes. In our rocket flights, we made good use of this; especially in the satellite flights. We had three very successful flights on the Atmospheric Explorer satellites, where we had, what we call, an open-source instrument: ambient gas just went in directly. We could measure the atomic oxygen and molecular oxygen separately with our tricks of taking advantage of the speed of the rocket, and then put a retarding potential on. As it turned, of course, there was a tremendous modulation in the ion currents as the satellite rotated. See, you're running away from everything but the fast tail of the Maxwell distribution in one direction, and in the other, everything plows in at one hundred times what it ought to be. So you get modulation factors of a million, or something of this sort. So, it's fun to analyze the data, the computer-nuts just go crazy, they love this. Now, I'm looking for our Viking model, and damn it, I don't see it. Maybe somebody borrowed it. Now, let me think for a minute.

GRAYSON: Just for the sake of the record, I'm going to count...one, two, three, four, five, six, seven, at least seven miniature mass spectrometers here, on the shelf, plus...do you suppose there could be as many as a hundred mass spectrometers laying around? [laughter]

NIER: No, no, no. [laughter] You've seen most of them.

GRAYSON: Well, I don't know, between the various bits and pieces.

NIER: Here's one of the magnets for a small one. That's for either the inch or the inch-and-a-half ones. The ions come in like this. And here's your focal plane. Why don't I have the Viking instrument?

GRAYSON: What's this beastly up here? Is that anything?

NIER: No, this would be a little thing, just so big, fashioned from an aluminum plate. Something must have happened. Maybe I lent it to somebody. Well, here, is something worth looking at. This is our standard inch-and-a-half instrument, such as was flown on the Atmosphere Explorer satellites. We used a lot of these for lab testing and so on, and they're

pretty good for smallish sort of things. This is the standard thing—built-in magnets here made of Alnico. The electron beam goes through a little hole in the Alnico. Our shop has this wonderful machine, one of these EDM [Electrical Discharge Machining] machines, for etching these holes, electrically. So, we can go right through a piece of Alnico, for instance, and do tricks like that.

GRAYSON: So, actually, the ion beam goes through the magnet?

NIER: The electron beam. This is the yoke, a soft-iron yoke around it, which collimates the electrons so they go along the diameter. It's like a two-pole DC generator or motor, where you have two poles, and there's a field across the diameter, and the electron beam is on that diameter.

GRAYSON: Yes.

NIER: Well, I'm still looking for the Viking instrument, which I wanted to show you.

GRAYSON: There's something way in the back there.

NIER: Oh, here it is! It was staring at me. This is the heart of what went to Mars. This is similar. This end is new. [recording paused] This was one of the models of the Viking instrument that went to Mars to measure the atmosphere as it entered on 20 July 1976. A special flange was put on in order to test it for the speed characteristics. This fits our speed thing there.

GRAYSON: So, this was actually tested on that piece of equipment in the other room?

NIER: That's why I happen to have it. Now, the instrument was surrounded by an electronics box, so when the whole thing was done, as it flew, it was a cube about a foot on a side. These things came complete with electronics and everything. This had more or less the same standard ion source, Mattauch-Herzog geometry, and so on.

GRAYSON: What was the magnetic sector radius?

NIER: Inch, one inch. It had an inner radius for measuring hydrogen, but we never got enough data. It wasn't sensitive enough. We got no data that was worth anything on hydrogen.

GRAYSON: Now, what's the smallest radius instrument?

NIER: This is it. The idea of having two collectors was used in the atmospheric studies, as in the other instruments you saw. And this is the one-inch instrument that I showed you before, in the other room, that flew in a rocket. In Viking, you wanted to measure helium and the heavier things. You had nitrogen, oxygen, argon and some other ions you were interested in; and then helium. The abundance of the helium is like a 100,000th of the other things. To measure this at the same time you're measuring the others put an awful strain on the dynamics if a single collector is used. It's easier to have a light-mass collector which is much more sensitive than the others. And that worked out very well on our flights. Especially on the Atmosphere Explorers, where we were doing the satellite work.

GRAYSON: Would you care to put a dollar figure on the value of all of these instruments?

NIER: Well...there's a couple million dollars worth. I was getting grants for development work on these things on the order of 100,000 dollars a year. Sometimes more, and sometimes less, over quite a few years, so that's the kind of money that this represents.

GRAYSON: A nice little, uh, collection. [laughter]

NIER: Yes, well, these are the things that we've taken a little better care of.

GRAYSON: Now, all of the electronics for these instruments, where did they go?

NIER: Well, various things have happened to them. Ones that were flown, they were either wrecked or some of these have been at the bottom of the ocean. They've gotten wet with salt water.

GRAYSON: Okay. So, these have flown and come back.

NIER: Yes, and they've been washed and dried out to get the salt water off. I take it back. Those have not been wet, because they were in a sealed rocket. These are from a rocket that floated when it came in.

GRAYSON: Okay.

NIER: But, since the tops were open, the spectrometer tubes did fill up with water.

GRAYSON: Sure, yes.

NIER: But the electronics never got wet.

GRAYSON: So, when you got through with your mass spectrometer, your experiment ended by the instrument filling up with water?

NIER: Yes. [laughter] Exactly.

GRAYSON: Wasn't even clean water. Supposedly, a friend of mine had the bad experience of coming in and looking inside the window of his ion source, one time, and finding water in there. A cooling line on the diffusion pump had corroded through the pump wall.

NIER: Well, I understand something like that happened to the big instrument they built in [Max-Planck-Institut für Chemie at] Mainz that Hintenberger was involved in. He came up with his idea of having second-order focusing, not only for angle, but also for velocity. They devised a beautiful instrument with weird angles and curves. They built this beautiful thing, but unfortunately, they never got any real data, because they ordered the oil diffusion pumps at a time when stainless steel wasn't available in Germany. The pumps eventually rusted through, so they had real problems.

GRAYSON: They had brass pumps?

NIER: No, they were not brass. These were commercial pumps of regular steel that they bought, and they just didn't stand up for what was needed, and so they had problems. But they had other problems with the geometry of the instrument. As I understand, the z-focusing blew up or something.

GRAYSON: Well, Hintenberger and Koenig published a whole series of geometries...

NIER: Yes, that's correct...

GRAYSON: And, this was one of those geometries that didn't work?

NIER: This was one of those geometries that didn't work. It should have, but there were some other factors, practical things like fringing fields, I think, which didn't work out quite like they had planned.

GRAYSON: In looking back over the years, basically, the Mattauch-Herzog geometry and the Nier-Johnson geometry, seems to have dominated.

NIER: They have survived.

GRAYSON: I know, VG is one of the few companies that's building a multi-sectored, non-Nier-Johnson geometry instrument. And, if I read correctly, I think they took their fundamental design out of the papers that Hintenberger and Koenig had published.

NIER: Very likely. Now, of course, one of the big improvements that was made goes back to a guy by the name of [William G.] Cross, who was a graduate student at Harvard. He published in, about 1950, a geometry which did not pertain specifically to mass spectrometers.³³ If you had a sector instrument, and didn't enter the magnet normally, but you entered at an angle of 27° or so, then, it behaved, as if it were twice as big. And so, you get the dispersion of an instrument that was twice as big. What the people who built that thing, VG and others, didn't tell you, was that the angular aberration went up by a factor of four, whereas the dispersion went up by a factor of two. So, what happened was, instead of the alpha-squared r aberrations that you normally have for a simple sector instrument, they became four alpha-squared r. All in all, the instrument worked very nicely. But, it isn't all great. You can't get something for nothing.

GRAYSON: Yes, well, you get mass range at the cost of resolving power.

³³ William G. Cross, "Two-Directional Focusing of Charged Particles with a Sector-Shaped, Uniform Magnetic Field," *Review of Scientific Instruments* 22 (1951): 717-22.

NIER: True and it may not matter. But if you were interested in fuzzy resolution and so on, and in sensitivity, at the same time, it wasn't all for nothing. That, I think, is the moral of the story. [laughter]

[Interview moves back to Nier's office.]

NIER: There may be some other things to copy, so maybe we should hold off.

GRAYSON: Okay. We've just completed, a tour of a store-room, which has a lot of wonderful old things. [laughter] And a tour of a laboratory, which has a lot of wonderful new things, and we're on Side A of Tape Nine, and I guess what we want to do at this point is to pick up with the discussion where we left off yesterday. We pretty much finished the Manhattan Project. You'd become chairman of the department at about that time.

NIER: Well, we didn't discuss the meteorite work.

GRAYSON: Okay.

NIER: We ended up about 1950, the beginning of the mass work.

GRAYSON: Yes, I think we discussed the accurate mass measurement studies and pretty much completed that. What do Tom's notes show?

KRICK: Oh, a lot of people talking about Bill Libby, and Donald Hull.

NIER: Well, that was pre-war.

KRICK: Oh, yes, I have a page here. Let's see.

GRAYSON: I know that we had discussed that business with regard to the accurate mass measurement. And Walter Johnson had taken over completing a whole bunch of work on very fine, accurate mass determinations of the elements.

NIER: That's right.

GRAYSON: And so, maybe we should just pick it up from that point.

NIER: Just for a moment, you've got a list of all my reprints?

GRAYSON: Yes.

NIER: But, here's an updated list, which has some more things.

GRAYSON: Okay, now we're on number 190.

NIER: There's a couple of things in press right now.

GRAYSON: Okay.

NIER: See, that list has the Duckworth-Nier thing in it, which the other one didn't have, so that brings you up-to-date.

GRAYSON: Yes. So, what follows the accurate mass work.

NIER: Yes. I got out of that. The last time my name appears on something is in 1957 or so.³⁴ In the 1950s, following up on the work which Aldrich and I had done in the late 1940s, on the helium-3 in *Nature* and the Ar-40 it became interesting to participate in the game of looking at rare gases in meteorites. This field was warming up. We were knocked out for a number of years, because they added a wing to the building, and I lost my labs. This goes on regularly. [laughter] You have to live a long time to survive those situations. So, we started working simultaneously, with the mass work in the early 1950s on this problem. Following 1954, when I visited Mainz for the first time, I became acquainted with one of the post-docs, an assistant of Professor Mattauch by the name of Rudolf [H.] Bieri, B-I-E-R-I. He was interested in coming to the United States. His wife was especially interested. And so, they came, and he worked as a post-doc with me for several years. We started work on meteorites at that time, heating things very hot.

³⁴ W.H. Johnson, Jr. and Alfred O. Nier, "Atomic Masses in the Region Xenon to Europium," in *Nuclear Masses and their Determination* (London: Pergamon Press, 1957).

[END OF AUDIO, FILE 4.6]

NIER: We heated the meteorites up to a very high temperature and looked at the evolved gas. Most of the time we used induction heating. This meant you had all kinds of problems, because everything got hot. The walls of the container got hot, and you got glow discharges, and you'd knock stuff off of the walls, and so on. We decided that wasn't such a good idea. So, we went into direct heating, and built little furnaces right in the vacuum, so you could heat it hot enough to melt an iron meteorite, for example, in a crucible.

GRAYSON: So this, really, was just following through with your previous success with isotopic measurement, abundance-type, where you'd used heaters?

NIER: Yes. Except this wasn't in the spectrometer, it was in a manifold outside. Then you purified the gas with titanium filings. There were several other groups. Mainz, I think, had started on that kind of thing. There was a group of people at the Smithsonian Astrophysical lab in connection with Harvard doing similar things. And a guy by the name of Oliver Shaeffer at Stony Brook [University]. So, there were four or five groups, and we got into the act at the same time.

GRAYSON: Now, this is kind of a departure, in some ways, from the previous work. What was your initial interest in this?

NIER: Well, yes and no. It was nuclear physics, and we were being supported by the Office of Naval Research, in the nuclear physics program, and we had better spectrometers than anybody else.

GRAYSON: Okay.

NIER: So it was a logical thing; and it was parallel with the work on atomic masses. It was going on at the same time as the atomic mass work, which I was losing interest in already. And so, Bieri was here, and did those studies for a while. Then came John Hoffman, who is now at the University of Texas in Dallas. He did some very nice work on helium-3 and helium-4 in meteorites. We did depth studies, for instance, where we drilled holes in iron meteorites, and took samples at various depths. You could find out how the isotopic composition of the helium changed. And we did, really, the first decent work of that kind—these depth studies. John did a beautiful job.

GRAYSON: Now, these were stony-iron meteorites?

NIER: No, these were plain-iron ones. And he worked on a meteorite called Carbo. Then we were instrumental in getting the U.S. National Museum to cut up a meteorite called Grant, which has become a classic meteorite. We paid for their sawing it in half and taking a slice out of it, and this slice was cut into bars and distributed to people all over the world. We played a small part in making that particular, very important meteorite available to a lot of people. We ourselves did the helium-3 and helium-4 as a function of depth in these bars. That was followed up by Peter Signer.

I was in Europe in 1957, visiting the University of Bern. My car was acting up—the brakes were defective. It was almost a new car, but the brakes had been bugged by the people in Rome [Italy], who claimed they knew all about brakes on Chrysler cars. So, by the time I arrived in Switzerland from Paris after that meeting of the Atomic Energy Commission that I mentioned earlier, where we had first talked about C-12 had to hit the brakes three times to stop the car. This is how badly out of adjustment they were. So, Professor [Fritz] Houtermans, at [University of] Bern, who was the director of the physics institute and very well known, and interested in cosmology gave me Signer for the day. He was a graduate student, just finishing up, who knew a little bit of English, and he acted as an interpreter between me and the garage people. They had a marvelous Chrysler garage in Bern, so my car was all fixed up that day.

Anyway, I got acquainted with Signer. I spoke some German, and so we communicated, and I found out he was finishing up, and he didn't have a job. So, I said, "Why don't you come and spend some time with us?" So, he was there for a year, two years, three years, four years, or something. We finally put him on the faculty, and he ended up as an associate professor, and would have been promoted shortly to a full professorship, but he decided to go back to Switzerland. He's back in Zurich [Switzerland] now, and has a very important job. A wonderful, wonderful person. He's followed up on the work on the depth effects in iron meteorites. And then expanded it to other types of meteorites later on. But the particular thing he worked on when he was with us was iron meteorites.³⁵ And there's a model, called the Signer-Nier model, which he came up with, on how you figure out why these isotopic variations ought to go with depth in meteorites. He pioneered that.

GRAYSON: Can you sum up the model in a few words?

³⁵ P. Signer and A.O. Nier, "The Distribution of Cosmic-Ray Produced Rare Gases in Iron Meteorites," *Journal of Geophysics Research* 65 (1960): 2947; Peter Signer and Alfred O. Nier, "The Distribution of Rare Gases in Iron Meteorites," *Problems Related to Interplanetary Matter*, National Academy of Science, National Research Council Nuclear Science Series, Report No. 33, Washington, D. C. (1961): 31-44; and Peter Signer and Alfred O.C. Nier, "The Measurement and Interpretation of Rare Gas Concentration in Iron Meteorites," in C.B. Moore, ed., *Researches on Meteorites* (New York: John Wiley and Sons, 1962): 7-35.

NIER: Well, it had to do with spallation reactions. What happens is the very energetic particles in cosmic rays come in and blow up the atoms that are there to form the lighter elements. In iron, you form all the lighter elements: some radioactive, which some people pursued and others stable. In the case of noble gases, you could extract them. You could tell something about how the hardness of the radiation varied as you go down. It'd generate different amounts. You could make all kinds of models to resolve how big the meteorite must have been when it was floating around in space. When you extrapolate out, you could decide how much was lost as it burned up the surface when it came in. People had a lot of fun working backwards from data like that. [laughter]

So that was, sort of, our adventures in that field. About that time, I was so busy. I was chairman of the department—this was in the late 1950s, early 1960s, and Signer more or less took over this sort of thing. Then, when he decided to go back to Switzerland in the early 1960s, we got [Robert] Bob Pepin, who's now on the faculty here. He was a rare-gas man who'd come from John [H.] Reynolds' laboratory at [University of California] Berkeley. First-class guy and he has, sort of, taken over all of these things, and has his own group for that sort of research. I really got out of that business at about the same time.

In 1957 [and] 1958, when people were first flying rockets to the upper atmosphere, there was the International Geophysical Year. It seemed to go on for three years or so, but I guess it was only one year. But anyhow, in connection with that, people flew rockets for all kinds of experiments. The Aerobee-rockets were developed by the people at the Naval Research Laboratory for this. Note that this was before NASA was created. A lot of the Naval Research people went to NASA and became big-shots at NASA. But it was originally the Naval Research Laboratory. It looked like it would be interesting for us to get into the act, because we knew how to build mass spectrometers. So why couldn't we build miniaturized ones to fly with the rockets? That's how I got going on that in the early 1960s.

GRAYSON: So, the study of the meteorites was completed as far as you were concerned?

NIER: Yes, I got out of that entirely.

GRAYSON: But, you were familiar with some of the other problems in that type of activity.

NIER: That's right, and of course, we would come back to that looking at this cosmic dust stuff. So, it's all related, you see.

GRAYSON: You went into the business of looking at the upper atmosphere with mass spectrometers in rockets.

NIER: Yes, sending up rockets with spectrometers on them. As I told you before, I have this short interest-span, like a three-year-old. This is natural for me to move on to something else. This seemed to be a new interesting thing to work on. I was fortunate, and got a grant from NASA for laboratory development work. At the same time, I had some support, well, originally from the National Science Foundation for participating in rocket flights along with the Naval Research people. So, you'll see publications in the list from early 1960s, where it's myself, John [H.] Hoffman, who, in the meantime had gone to the Naval Research Lab before going to Texas, and a guy by the name of Charlie Johnston.³⁶ There is somebody else...oh, gosh, I know him so well. Anyhow, there's a fourth person there, where we published several things on the first rocket flights. We built the mass spectrometers for that purpose. There's a paper published on that, we did turn up some rather nice work on measurements. Then, I had a series of other graduate students, who carried on that research. We then got support from NASA for rocket flights. We had about a dozen Aerobee flights in the period from 1962 through 1970, or thereabouts. We flew things in the upper atmosphere, various instruments. Some were plain 90 degree sector instruments. The first ones were double-focusing, Nier-Johnson, but we gave that up afterwards, because it seemed an unnecessary complication for our purposes.

Then about 1970, or thereabouts, the Viking Project came along. There was a routine announcement which said, "Anybody interested in participating, this is going to Mars, just send in your name." So you applied like applying for membership on the basketball team! Because they were forming teams. I sent in my name, and said, "Yeah, I might be interested in this sort of thing." And so, they not only accepted me, but asked me to be head of what was called the "Entry Science Team." There were about [...] whether it was seven or eight or ten, teams. There was the biology team, the organic analysis, that was headed by Klaus Biemann, who I got acquainted with then, and various groups of that kind; the seismology group, the infrared and ultra-violet team. The "Entry Science Team." consisted of myself, Nelson Spencer from Goddard, who headed the mass spectrometer work there, Bill Hanson at the University of Texas at Dallas, who was an ionosphere-type man—he measured ions in the atmosphere, Mike McElroy, a theorist at Harvard, he was originally at Kitt Peak, and he was the theoretical astronomer of the group, and Al Seiff, S-E-I-double-F, who was an expert at Ames Laboratory on measuring density of atmospheres from high-speed vehicles going through the air. So, he developed temperature and pressure sensors to measure the atmosphere as the lander came into Mars. We measured at the high altitude end of the atmosphere; and Hanson was responsible for measuring the ion composition at high altitude. One interesting aspect of the thing was that we had to tie the mass spectrometer analyses, done at high altitude, to the density measurements, done at the low altitude. We had to tie them together, make the curves match, for a natural transition.

³⁶ Alfred O. Nier, J.H. Hoffman, C.Y. Johnson, and J.C. Holmes, "Neutral Composition of the Atmosphere in the 100- to 200- Kilometer Range," *Journal of Geophysical Research* 69 (1964): 979-89; and Alfred O. Nier, J.H. Hoffman, C.Y. Johnson, and J.C. Holmes, "Neutral Constituents of the Upper Atmosphere: The Minor Peaks Observed in a Mass Spectrometer," *Journal of Geophysical Research* 69 (1964): 4629-36.

GRAYSON: Yes.

NIER: It turns out that one of the things that came out of this matching, was you could determine Avogadro's number, if you hadn't known it. Because you're tying molecular things to bulk things, and Avogadro's number entered in. You could make a rough determination of Avogadro's number from our data, had you not known much better at the time. [laughter] It was an interesting side-light of that.

GRAYSON: The formation of the team for the entry science, were you the person who got to pass on who became team members?

NIER: Well, NASA headquarters selected the people and they formed the teams. There was only one individual who said he wanted to be a chairman or head of a team, one of the teams. A very good man, incidentally, but he wasn't bashful. Everyone else was a little bit reticent, and I said, "Oh, gee, I don't want to do it," but I was asked by NASA if I would head that team, and so I did. That's the way it came out.

GRAYSON: From my own background, I've had a little bit of experience with this type of thing. Was there competition amongst the teams later on for space and time, and power, platform resources, etc, on this entry vehicle, because there's only a finite amount of that stuff.

NIER: Not really. It was an amazing experience. There had to be cost-cutting. There were problems on this, but the teams worked together very well, and there was no competition between the teams. I don't think anyone resented another team getting some extra help, or not being cut as much. Because I think there was a very cooperative spirit in the whole thing. There were about seventy scientists, altogether, when you added it all up, and I thought it was a very cooperative group. As a team leader, we had regular meetings of the steering group, so we met regularly with the top people at NASA, and they had a very good project manager for the Viking Project. Jim Martin was his name. He was really a hard-boiled guy, who didn't make friends easily. I think it was because people kind of hated his guts because he was so tough at times. But he did an extremely good job in managing things, and holding the whole thing together. He consulted with the scientists, and then he made the decisions. He finally said, "I have to make the decision." You weren't always happy with it, but you had the feeling that he did the best he could, given the circumstances. So, you'd come away from it not altogether unhappy with what happened.

GRAYSON: Well, basically, you're saying he was a good manager.

NIER: He was a very good manager and he didn't do it by being cozy with you.

GRAYSON: How long did this project go on?

NIER: Well, it started in about 1970 and of course, the landings took place in 1976. Then, there was some work for another year or two. Of course, some things continued, because those orbiters kept going for a couple of years, and people were collecting data. And the pictures on the surface kept being taken for a number of years. So, there was activity. But, I dropped out of it in the end of 1976.

GRAYSON: I guess there were a series of publications.

NIER: Yes. They summarized the work. There was very little overlap between the members of the team. [Michael B.] Mike McElroy, who was the theorist, sort of worked with everybody, and he and I published on the mass spectrometer results. He and I also had a publication summarizing the work on the neutral mass spectrometer.³⁷ It appeared in the *Journal of Geophysical Research* in 1977. That's in the list of publications that I gave to you. The outstanding thing that came out of this, was first of all, we got the profiles of the various atmospheric constituents. One of the very exciting parts of it was the Russians had had a landing on Mars a few years before. And as their vehicle entered Mars, they had housekeeping data coming back. The intention was that the real data was to be sent by radio telemetry later on. But something went wrong, and no data ever came back, so they based whatever results they had on housekeeping data. They had a spectrometer on a parachute, as this thing went down, so they were apparently doing something at the time. The housekeeping data monitored, among other things, the ion current of the little ion pump, which was used for evacuating the instrument. It showed an abnormally high current. And this, they attributed—erroneously, it turned out—to high argon in the atmosphere. Because sputter-ion pumps don't like noble gases and read high. They knew enough about the pressure in the atmosphere and so on, that it couldn't be that, so they assumed that there was a lot of argon in the atmosphere. The value they deduced was 25 percent or 30 percent argon in the atmosphere.

Our instrument was considered a minor instrument, compared to the biology instrument, the organic analyses and so on. So, we were never high on the totem pole among the things of importance on Viking, but on the morning of 20 July 1976, all the eyes were on us, because we would determine how much argon was in the atmosphere. Within a few seconds, when we saw our first prints coming off, it showed that it was only, a percent-and-a-half or something like that. And the reason there was so much excitement was, first of all, it was of theoretical importance, but from a more practical standpoint, it turns out that the little ion pump on the GC-MS instrument would not have worked properly had there been as much as the Russians had

³⁷ A.O. Nier and M.B. McElroy, "Composition and structure of Mars' Upper Atmosphere: Results from the Neutral Mass Spectrometers on Viking 1 and 2," *Journal of Geophysical Research* 82 (1977): 4341-9.

surmised. They might not have gotten their organic analyses had there been that much argon, because the pump might have given out, and then it would have never worked; and that was one of the important instruments on board.

So, for a few minutes, on the morning of 20 July all eyes were on us. I even have a picture. It turns out that Klaus Biemann's son, who was a high school kid then, was a very good photographer. And he went around taking candid shots. He probably took thousands of pictures of everybody on the project during that time. He had free run of the place, and this little kid would run around and get under tables, and snap pictures of people. [laughter] So, he got a picture of several of us from the teams, poring over these first spectra that came through. He later published a book, at no cost. Well, we paid for it, but he made nothing on it. He did it as a high school project, if you can imagine a kid in high school, a whole picture-book of candid pictures of Viking. It's one of the wonderful things that happens every once in a while. I didn't even know the picture existed until a long-time afterwards.

GRAYSON: [laughter] I suppose then, all the upper earth atmospheric studies that you'd done with these types of instruments had really gotten you to a competent stage for the Viking project. You had a wealth of experience.

NIER: Yes, well, you see, we'd also gotten other experience along the way. I was going to say, we had about a dozen rocket flights before that, and then we were on what was called the "Atmospheric Explorer-C, -D, and -E" satellites, which were earth satellites. One was a polar orbit, one was equatorial, and one was something in between. These had what was called the open-source mass spectrometers. So, we had gas going directly into our instrument, as contrasted with another instrument, which was provided by the Goddard people, which had pin-hole leaks. We had things in equilibrium. And that's why we were able to tell the difference between atomic and molecular oxygen. The atomic comes in and combines to make molecular, and so, you have a mix-up between the two. But we could, with our retarding potential schemes, could tell the difference. That was the first use that was made of the retarding potential technique. Although, it had been suggested by a guy by the name of [C. Russell] Russ Philbrick at Air Force-Cambridge. He never succeeded, unfortunately, but he'd really thought of the idea, and tried to use it before that unsuccessfully. But this Atmospheric Explorer work was going on at the same time in the early 1970s, when we were developing and working on the Viking project. So, these things went side-by-side, and of course, each benefited by the other.

GRAYSON: Sure. A good case of serendipity.

NIER: That's right.

GRAYSON: What about the biology experiment that Biemann was on? Did they talk to you about their instrument at any time?

NIER: Well, you could find out all kinds of things here. [laughter] It turned out I was approached in 1961 or 1962 by people at Jet Propulsion Laboratory. By that time, Harold Washburn had left Consolidated. There'd been some re-organization of the place, and he was no longer there. He got a job with Jet Propulsion Laboratory in mass spectrometry-related things. There was a young fellow by the name of Charles Giffen, who worked with him and the two of them visited me. I knew Harold from way back. We were friends from when they first built the Consolidated instruments, and you saw the picture of him and me with the Consolidated-Nier instrument in the late 1940s. They approached me about building a miniature instrument to go to Mars. And they gave us a contract to build a small instrument.

So, we came up with this thing, which I showed you, which fit in an attaché case and, had the combination ion-pump and main magnet for the sector. And, that was it. The mission that it was supposed to be on never came off. It was supposed to be 1965 or thereabouts. They'd never gotten it ready by that time, anyhow, because it takes so long on these things. But, anyhow, that's the goal they were working for. Well, time went by and when I got on the Viking project in about 1970, I discovered that there was going to be a gas chromatograph-mass spectrometer combination on it. Chuck Giffen, who I referred to earlier was working on this. I discovered that they had adopted the Nier-Johnson geometry for their instrument for the GC-MS. The thing that I had provided for him was a plain sector 90 degree instrument. But while they didn't adopt that, they did include the combination ion-pump magnet and main sector magnet. I hadn't heard anything about this. I'd been out of touch for years. Anyhow, that was the way that they were going to go, because with the double-focusing instrument, they could get pretty good resolution up to mass 200 with an instrument of modest size. So, they were working along that way.

Well, anyhow, when I got on the Viking project and learned about everything that was happening, I became acquainted with Klaus Biemann. I found out that they were having some problems at the Jet Propulsion Laboratory, on getting the GC-MS finally into shape, to turn over to somebody to manufacture. So, I was asked by [James S.] Jim Martin [Jr.], and [Gerald] Jerry Soffen, who was the Project Scientist for the Viking project to join Biemann's team. Soffen had been a Jet Propulsion man before that, but was transferred to NASA for the Viking Project. They asked me to help on the spectrometer development. So, I was on two teams. I was head of my own team, and was also on Biemann's team, and we were trying to work on the Atmospheric Explorer satellites at the same time. And I was also teaching classes, by the way. I was no longer chairman of the department, however, which was a disadvantage; because I no longer was free to come and go like I could before. I had a boss. Anyhow, I helped along as best I could, and in the one winter—and I've forgotten which winter it was 1973-1974 or 1974-1975—I commuted to Jet Propulsion Laboratory from Minneapolis thirteen times in four months. [laughter] I missed Christmas week, I think. But otherwise, I went back and forth every week. Flew down on Monday afternoon, came back either Tuesday or Wednesday night, taught classes, most of the time. Luckily, I had a wonderful substitute to teach my classes, because I

was teaching a class that met every day of the week. I worked with the people, with Klaus, and with the various technical people, Chuck Giffen and the other people there at Jet Propulsion Laboratory. I had a pretty good time at it, because they are all wonderful people, so I had a lot of fun in working with these people, and it certainly paid off in the end. I worked also with the Perkin-Elmer people. They built the spectrometer in whatever that town is, outside of Los Angeles [California], to the south.

GRAYSON: I'm not sure.

NIER: Yes, I also spent time over at Beckman Instruments which built the gas chromatograph, and also with Litton. Wait a minute...was it Litton that had the prime contract for this instrument? I think they did. They were on the other side of Los Angeles. It's a real problem, travelling to these three places, about as far apart as you could get. You couldn't hit all three in a day. But it was worthwhile. Klaus Biemann and I became very close friends.

GRAYSON: Was there any, discomfort initially, with the manager of the project suggesting that someone like you should be a part of both.

NIER: Not on Biemann's part. He was delighted...Klaus was delighted, and, he never claimed to know much about instruments. He just knew how to use them. He was certainly very, very good at interpreting the data; how you ought to run the chromatographic columns, and so on, to get the results that you wanted. So, it was really a very good team. We had people who were experts, and they had people on the team who knew about gas chromatographs, and an English guy, who was very, very good, and others too that I don't remember. It was a very happy experience. John [F.] Oro who was down at Houston [Texas] was on that team.

GRAYSON: Yes, yes.

NIER: He and I became great friends too. So, the whole thing was a very happy experience.

GRAYSON: So, after that experiment did you fly anything to Venus?

NIER: [laughter] Well, in the meantime, I had another post-doc, Konrad Mauersberger, who came from Bonn [Germany]. I'd met him a few years before, as he came through here once. That group used to fly rockets up at Churchill [Canada], up at Hudson Bay, and he came through on one of those trips. I met him here, and I met him in Bonn also, and discovered that he was finishing up there, and was looking for a job. So, I got him to come here as a post-doc,

and he started working on various spectrometers here. But he had some very bad luck. For instance, he had developed a beautiful miniature instrument, which was going to fly on a pair of satellites called the Dual Air Density Satellites. It was a project out of Langley [Virginia], where you were going to have the spectrometers on two satellites, one at high altitude, and the other low. And, he did all the beautiful work on these miniature instruments. Everything was fine, except that something went wrong in the launch, and the rocket was lost, including the instruments, and that was the end of that project. So, that's what he spent several years on. But he picked up other things, and then joined our staff. He's now a full professor in the department, doing very well, and he's become an expert on the stratosphere. He's flown miniature instruments on balloons, and he developed this beam-entry system with liquid helium pumps. He has done some very nice measurements of ozone, including the isotopic composition of ozone which is very interesting in itself, water vapor, and CO₂ in the atmosphere.

But, that's a long, big introduction in answer to your question. He had come from Bonn [Germany], and Ulf von Zahn, who was his mentor there, was interested in getting into this sort of thing. He was kind of an entrepreneur in getting into things of this kind. And he wanted to be in on the Pioneer Venus program. He persuaded us that we should provide our design of the Atmospheric Explorer instrument for that. So, we were co-investigators with him. We provided the details of the instrument, they built it in Germany, and we commuted back and forth during this whole time. In fact, we took one of those instruments which I showed you down there, that were used for the Atmospheric Explorer work, changed the English dimensions to metric dimensions, and made some improvements here and there, and that's the thing that went to Venus, to measure the upper atmosphere. It was essentially identical to our Atmospheric Explorer instruments, larger than what we had on Mars.

GRAYSON: So, you weren't really directly involved in the Venus experiments?

NIER: Not directly.

Mauersberger was more directly in it than anybody.

[END OF AUDIO, FILE 4.7]

GRAYSON: We have just wrapped up the exploration of interplanetary atmospheres. Before we leave that, I understand that Project Galileo is about to take off.

NIER: They're waiting for the launch.

GRAYSON: Do you have any hardware on that flight

NIER: No, none at all. There is something I forgot, however, that goes back into the 1950s when we were doing all of these other things we talked about. One of my very dear friends in Geology, [Samuel S.] Sam Goldich, G-O-L-D-I-C-H, was a superb geochemist. One of the early people who believed in doing quantitative geochemistry. He was a little older than I. He felt that the geology department should get seriously into the geochronology business. He was so good and so persuasive, he convinced me that we should cooperate on something. I couldn't turn him down. We built an argon instrument for them so they could get into the isotope geology business. That would have been the 1950s or so. John Hoffman was a graduate student at the time and he helped on that. We helped them get started on the work of doing argon analyses. Sam was very good at doing the difficult things with potassium and so on.

GRAYSON: So, the work that we have discussed so far brings us close to the current period, does it not?

NIER: Well, we've covered a fair amount of that. In the 1970s we worked with the Atmospheric Explorer, the Viking project to Mars, and the Pioneer Venus program. These were all things that took place in the 1970s really. We had a modest program supported by NASA for just instrument development in general for these kinds of studies. I had an ongoing thing which, unfortunately, NASA phased out at a later time. Nevertheless, it was very helpful during the time that it went on. So that's what we were doing during that period. We had a lot of publications from the atmospheric explorer satellites; often in collaboration with other people, because the program was supposed to be an interdisciplinary venture, and there was supposed to be a lot of 'togetherness' in it. And there was. People shared data. We had a direct line from Goddard where the data came in. We could get hardcopies right here of things that were happening a few minutes after they came into Goddard from the satellite. There was a lot of good stuff coming in. And I had various students and post-docs working on that at the time. I didn't have too much to do directly with that work.

GRAYSON: It seems like a number of the most recent papers are back on the cosmological side.

NIER: Yes. Well, then what happened was, with the Challenger accident [Space Shuttle Challenger disaster on 28 January 1986], and even before that, there was less money available for the kind of research that we had been doing. And certainly after the Challenger accident, this was definitely the case. So, I had to find other things to do if I was to keep on.

Well, what is interesting? I looked at the helium problem in the extra-terrestrial material. Little had been done. We had a grant from the National Science Foundation for the

geology instrument which I showed you. We saw that with that relatively small instrument, we could get quite high resolution. If we used it statically, since it had a small volume, we could get good sensitivity. The sensitivity of the instrument was such that without doing 'heroic' things, you could get one count per second for about 10^6 helium atoms with an ion counting multiplier. That isn't very many atoms.

GRAYSON: That's a damn small amount.

NIER: There's 10^{19} in a standard cubic centimeter. This would be only 10^6 so the pressure is pretty low.

GRAYSON: You're talking about 10^{-12} or better atmospheres?

NIER: Correct.

GRAYSON: And you were able to get these results without heroic efforts?

NIER: Yes! Anyhow, what has happened in the meantime, there were all these wonderful instruments that you could buy. So, a lot of people who were doing this sort of geophysical/geochemical work, which we were able to do before because nobody else had instruments; now, they could buy instruments to do their thing. And they knew a lot more about the chemistry, so to try and compete with these people who had automated, computerized machines was going to be kind of difficult. Especially since we had very poor facilities. I had very little money for this sort of thing. So, what could I do that was profitable and I wouldn't be running head on into people who had hundred times the funding that I had. I thought well, I could pick some problems that were too difficult to do with standard instruments. I had the good fortune of attending a conference in the early 1980s that Bob Pepin had something to with. It was held up in a resort in northern Minnesota out of season in a place way far away from anything else. So, all you could do during the three days there was to stay there and eat your meals there and attend the meetings. There weren't any distractions. I listened to what these people were talking about and listened to my good friend George [W.] Reed who was a geochemist at the Argonne Lab and found out what he was doing. He was looking at anomalous mercury which he thought he had discovered in some meteorites. I thought that might be a good thing to tackle with a mass spectrometer.

Then, the other thing I learned about was the cosmic dust. We'd gotten acquainted with Don Brownlee from the University of Washington. He'd been invited here as a colloquium speaker to talk about these particles. I thought that it would be kind of interesting to look at the helium, since this is of fundamental importance. I thought I could tackle this kind of problem

which was a little too difficult for the standard instruments. And besides, I was retired and I didn't have to prove something tomorrow morning! So I could tackle these impossible problems. It didn't make that much difference.

GRAYSON: When did you retire officially?

NIER: 1980—nine years ago. I looked into this and it was at the same time that we got the grant from the NSF [National Science Foundation] for the work for the geology people to study rare gases. It turned out they didn't continue that. So, I had a little money left over from our upper-atmosphere studies, so I built the instrument that I showed you for our own use, figuring the geology people would take the other one away, and I wouldn't have anything to use. Then we got going on the mercury. I wanted so desperately to prove that George Reed was right on the anomalous things that he had observed because I liked him and everybody was saying that you can't do it by neutron activation as he had done. They said the only way you could do it was by nuclear counting, and that you couldn't do it by mass spectrometry. I was hoping that I could prove that it could be done, but unfortunately, we never were able to verify what he got. There didn't seem to be anything wrong with the work at Argonne, but the fact of the matter is that we were not able to verify it. It may have been a problem with the extraction. In fact I still have some other samples that we haven't investigated. That's about the time I had my heart attack. That knocked me out for a while and I couldn't work actively. In the meantime, we got going on the cosmic dust particles from the stratosphere. I had gotten acquainted with Don Brownlee more and found that he had deep sea samples which were not nearly as valuable, and also believed to be of extraterrestrial origin. Just in the last year, we looked at the first of the stratospheric particles, and just before last Christmas, late November or early December we looked at eight individual stratospheric particles.

GRAYSON: When you look at an individual particle from the stratosphere...is this cosmic dust?

NIER: Well, the ones we looked at were definitely cosmic dust. The people at Houston sort this dust to find the cosmic ones. I don't know how they do it. The particles that are truly cosmic are few and far between. They collect all of these particles on fly paper. This is just a paddle covered with silicone oil which the pilot of a U-2 plane puts out when they get to high altitude.

GRAYSON: This is strictly a scientific use of the U-2 plane.

NIER: Yes, NASA has one.

GRAYSON: Is this the one at NASA Ames?

NIER: Yes.

GRAYSON: I remember seeing one when I was visiting there once. They just take this thing up as high as they can get it and sample the dust up there.

NIER: They have a paddle that comes out of the wing. They expose that for many hours, as much as they can, because there aren't many particles.

GRAYSON: So, this collects the stratospheric dust?

NIER: Yes. If the pilot remembers, he pulls the paddle back in again before he comes down! [laughter] It doesn't always happen, you understand! There have been a few cases where they forgot. You get quite a bit of dust then! The paddle goes to the people at Houston and they sort the stuff out. They know from the structure under microscopes, and they've learned what is debris from all the solid fuel rockets and what is cosmic dust. The atmosphere is just full of that junk from the rockets. They pick the cosmic dust out and mount it and catalogue it.

GRAYSON: So, they have all these individual catalogued dust particles. Then you stick them in a mass spectrometer?

NIER: We request them from NASA and they have a committee passing on requests and the last time we asked for eight of these cosmic dust particles. I have another eight which I haven't looked at yet.

GRAYSON: Your specific interest is in the ratio of?

NIER: The helium-3 to helium-4 ratio.

GRAYSON: The ratio represents spallation?

NIER: Well, no, you hope it isn't. That buggers the thing. You'd like to look at something primordial. So, it may be primordial but it may have superimposed on it stuff from the solar wind. There may also be spallation. You try to untangle all of these processes to get at the primordial ratio of the heliums in the cosmic dust.

GRAYSON: If it is primordial, this gives you information that is useful for?

NIER: Presumably, it tells you what the solar system was like in the early days.

GRAYSON: And then that gives you other insights.

NIER: Yes, that's the idea.

GRAYSON: The early days being shortly after?

NIER: Well, five billion years ago roughly. The big bang took place ten or twenty billion years ago. There is a lot of interest in this. And it isn't clear where these particles come from, but they are extra-terrestrial. That is definite. Some of them may be meteorite debris and some of them may be from asteroids. Some may be from comets because when a comet comes in, there is a lot of dust comes out. That's the kind of thing that people are trying to do. Of course there are other efforts going on. People at Washington University have looked at the neon and argon ratios. And then there have been other studies with electrons and X-rays and what-not and mineralogical studies as well. There is a lot of work going on on these individual particles.

GRAYSON: Did you ever do anything with the other classes of meteorites; the stony irons or the carbonaceous chondrites?

NIER: Personally, I never did. No.

GRAYSON: I thought I saw a paper on your publication list that had something to do with the Pueblito de Allende, [a carbonaceous chondrite].

NIER: Oh, wait a minute. That was looking at mercury in Allende. That was when we were trying to verify the results of George Reed.

GRAYSON: Okay. The only reason I asked about that is that Clarence [J.] Wolf and Ram [L.] Levy and I had done some work on analyzing the Allende on the organic side. Carbonaceous chondrites have quite a bit of organic material in them. And there is this problem of trying to find out where it is coming from. It is an interesting problem, and I think it is still a problem where there is a lot of controversy. I guess we've come down to the end of the publication list. Although a couple more have been added to the list you gave me in preparation for this interview.

NIER: Well, there is the review article and abstracts on recent things. I've given a paper every year at the Lunar-Planetary Science Conference, and there was one this spring, given on the eight stratospheric particles. That's the most recent.

GRAYSON: What are you finding in the stratospheric work that is going on now?

NIER: Well, lots of helium was found; but, it isn't quite clear how much of it is primordial. A lot of people think that it may be from the solar wind. You see, this stuff is bombarded all the time by the solar wind. The helium doesn't have the right isotopic composition, but on the other hand, if you're willing to allow for diffusion effects, you can get selective enrichment. You can make up all kinds of scenarios to explain it, and that's sort of the way people approach the subject these days. We've looked at helium and neon. We find a very peculiar ratio of helium to neon. The primordial stuff, or what the solar system seems to be made of, has a ratio of helium-4 to neon-20, on the order of eight hundred or so. That seems to be accepted. And for some of those deep sea samples, we found numbers like four hundred, which comes pretty close. So, it looks like there may be some relationship. But, in these eight particles that we looked at, the ones where we got enough data, the number is more like 20 or thereabouts, which is quite different. Well, you can explain this by saying that you lost a lot of helium by diffusion, but there is already so much more helium per gram in this stuff than in anything that had ever been seen...that you'd have to have many times more. It is a little hard to swallow. So, I don't know. I think we just don't know.

GRAYSON: So, they are rich in helium, on an absolute basis.

NIER: Yes, yes. Very much so. The amount is approaching a standard cubic centimeter a gram. Of course, these particles only weigh a nanogram.

GRAYSON: Of course.

NIER: But it's about that kind of number. It's even more. It's as high or higher than soil samples that were on the surface of the moon, which had really been bombarded by the solar wind. It's right up there; which is really exciting. So, when you're talking about diffusion of gas out, you have to do a little swallowing while you say it. It's a mystery, and what we're doing now, the very thing that's going on at this very minute, as you saw there, is to look at the argon also. We've looked at the helium-to-neon ratio, and now we want to look at the two in ratio to argon, because if there are diffusion effects, it ought to affect those ratios, of helium-to-neon and argon. You have another parameter in there. We can't do the isotopes of argon, unfortunately, because the Ar-40 is so dominating. But nobody else can do it either, so you look at the Ar-40 and you relate that to the Ne-20 That would be another step forward, and that's what we want to do next.

GRAYSON: Okay, so, from a scientific viewpoint, I guess we've gone through a reasonable amount of details on the various things that you've been involved in over the last fifty-nine years?

NIER: Well, not quite...fifty-seven.

GRAYSON: Well, I remember...when you came down a year and a half ago, to give a talk to our group [The Midwest Mass Spectrometry Discussion Group] it was fifty-seven years of mass spectrometry, as I recall...

NIER: Well, I didn't include myself necessarily, you see.

GRAYSON: Oh. [laughter]

NIER: I used Bleakney as a starting point, which is about 1930. No, I've been at it now, actively, since 1934.

GRAYSON: Okay.

NIER: I suspect I've worked longer and more continuously in mass spectrometry than anybody ever has.

GRAYSON: I think that's probably right.

NIER: I don't know who has, worked consistently. You see, there were so few people in those days, and most of them aren't living anymore. [laughter] I have an edge on the people who were. But I think that nobody else has worked continuously in this way. I think this is probably true.

GRAYSON: Well, what I'd like to do at this time, is just touch base on some other issues, particularly with regard to the American Society for Mass Spectrometry. They are underwriting this activity; so I suppose that it would be desirable to get some of your feelings, or attitudes or comments about interaction with that group. Can you recall the first ASTM meeting that you went to and tell us a little bit about that.

NIER: I really don't remember, exactly when. It was certainly in the early 1950s. And, I think—as we discussed before—my first meeting with mass spectroscopists other than the people that I knew from other connections was the meeting at the Bureau of Standards, 1951, which was sponsored by the Bureau of Standards. I remember the title of that was “Mass Spectrometry in Physics Research.” So, it was really restricted, more to the things that we were working on, things related directly to physics.

GRAYSON: And that's the meeting with the picture of the attendees.

NIER: That's the picture that you have which would certainly have included most of the outstanding people in the world, who were using it in physics research. So, I guess I was at a meeting in the early 1950s, there was a meeting in St. Louis years ago that I remember. And there was a meeting in Pasadena in the 1960s, late 1950s?

GRAYSON: I don't know. My experience starts out in the early 1960s, and Pasadena sounds like it might have been earlier.

NIER: Well, it probably was. You see, Harold Washburn was still holding forth, he was still a big-shot at Consolidated in those days.

GRAYSON: My impression was that the very beginning of these meetings was the CEC user meetings.

NIER: Yes. See, I would have had nothing to do with that at all. I do remember a meeting I went to. I had my first 35-mm camera which was bought in 1954, and I had the camera with

me, so it was after 1954, but it couldn't have been very much after. And then, I also remember, there was a meeting in San Francisco in those early days, because I remember visiting John Reynolds at Berkeley, and he had a number of us at his home. There is a picture of Mark Ingrham, and me, and a few others, in front of his house. So, that would have been some time in the 1950s. I guess I can find out when, if I really tried hard enough.

GRAYSON: Well, no...I was just curious, because I know that your career is a very prestigious one, and I can see the volumes of ASMS lined up there on the wall. But you're not a real regular attendee?

NIER: No. I suppose I've been to four or five ASMS meetings in the years. And I think, as I told you, one of the problems was, I was always teaching classes, which met everyday of the week, and these meetings always lasted a week. And this was fine for you government, or industry employees, but it was not very good for people teaching classes. [laughter] So, it was one of the drawbacks. And, I think, as I explained, also, one of the problems was that the meetings gradually drifted away from general aspects of mass spectrometry. There was heavy emphasis on the application, to what you might call organic chemistry problems, which are very important—I don't want to belittle that at all—but, my point was it was not exactly along the line of things that things I was interested in. You see, my interests had been more in nuclear physics and geophysical and geochemical problems.

GRAYSON: But, you did attend some of the meetings.

NIER: Yes, definitely. I attended one in Minneapolis here, a few years ago, but beyond that, I know I was at one in St. Louis, and I'm sure I was at one in Pasadena, that I know for sure, and probably one in San Francisco; and in Houston a few years back.

GRAYSON: So, what scientific conferences did you find most useful in your work?

NIER: Well, I used to go to the American Physical Society meetings regularly, but then my work drifted away from that. So, I regularly went to the American Geophysical Union meetings, for years. And, in recent years, I've become a member of the Meteoritical Society. I was never a member of that in the early days; then I found out they had wonderful meetings every other year in Europe, so the first one I went to was in Mainz a few years ago, and I had to become a member. The registration fee was ten dollars less if you were a member, so I had to scurry around to find somebody who would vouch for my character, [laughter] so I could become a member, and then I joined in time for that meeting. That was my first of the Meteoritical Society, although I had been interested in meteorites, as you know for many years. As a matter of fact, I'm chairman of the advisory committee to the Nininger meteorite collection at Arizona

State [University]. He had this wonderful collection part of which went to Arizona State. NSF gave the money for it so they said there had to be an advisory committee of outsiders to oversee this. So, I was on the original committee which was set up, and I think I'm the only survivor. Yes, Carlton Moore, who was the director of the project there, I guess he's an ex officio member there. So I'm an outside member. And there's been a turnover, other people...I'm still a member of that committee. So, I've been involved in that for many years.

GRAYSON: How many years, roughly, would that span?

NIER: Boy, uh, I suppose 1960 or thereabouts. It goes back...

GRAYSON: Okay, so, twenty-some-odd years.

NIER: ...at least twenty years.

GRAYSON: Which brings up the point. Obviously, you've had the opportunity to serve on various grant review boards, committees, and the like...

NIER: Well, I used to be on some of the Office of Naval Research Committees, and, on an NSF committee, the Physical Science Committee. I was on some of those. I've been on several National Research Council committees. One, looking into the fellowship program, and so on, overseeing everything. And I was chairman of that committee for a couple years. These things turn over, so...I've not been very active lately. Some people are in all kinds of things like that. I never have been. I was just on an occasional thing. They probably found out that I wasn't a good committee member.

GRAYSON: In what sense?

NIER: Well, I wasn't an eager beaver that would step forward, and try to take charge of everything.

GRAYSON: I see. What about journal/editorial activities, and that type of thing?

NIER: Very little. I've refereed an occasional paper, but I've never done anything on editorial boards.

GRAYSON: And professional society offices?

NIER: Never. Oh, I was president of one of the sections of the American Geophysical Union. I've forgotten the name of it now, it was several things I didn't know much about, but it, sort of, fit in with the isotope geology business, so I was a president of that for a couple of years. I guess they call it "president".

GRAYSON: Now, is there any time, during this whole career, that you, kind of, felt like giving up on mass spectrometry and moving into any other area?

NIER: Not really. I deplore the idea that people spend their whole life continuing work on their theses. I seem to have done just that. But there have been interesting applications along the way. There always seemed to be so much to do in other applications, so I just stayed with mass spectrometry. I didn't see any good reason to change, because there seemed to be so many interesting things to do here.

GRAYSON: And you just...kept doing them. [laughter]

NIER: That's right.

GRAYSON: I noticed that you published a number of articles in *Scientific American*, and in encyclopedia references to mass spectrometry.³⁸ How did that come about?

NIER: Well, I was asked to do them. The editor called me. I have one on my desk right now, that I'm over-due on. I'm supposed to write an update.

GRAYSON: How does your name get involved in these things. Do you have any feeling for that?

NIER: I don't know. They find out, somehow, that you're supposed to know something, and so, they contact you.

³⁸ Alfred O. Nier, "Mass Spectroscopy," *Collier's Encyclopedia*, vol. 13 (New York: Collier's, 1950): 251.

GRAYSON: Writing an article like that probably requires an entirely different style than a scientific journal.

NIER: Yes. Yes.

GRAYSON: Is that a difficult thing for you to do?

NIER: Yes.

GRAYSON: [laughter] Are they pretty helpful, the journal editors, do you think?

NIER: Yes, the people who run these things are very good about it, but the research you have to do to write these things usually involves matters that you don't necessarily know much about in detail yourself. So you try to prepare to cover various topics to be thorough, and I find it pretty hard.

GRAYSON: And the writing style also has to be different.

NIER: Yes.

GRAYSON: In terms of the depth, you can assume very little on the part of the reader.

NIER: That's right. And you have to write it in a way that's comprehensible to people, and they're not necessarily experts, and so, it's tough.

GRAYSON: But, you have one of those to work on as well.

NIER: Yes, I have one right now on my desk.

GRAYSON: [laughter] Which you could be working on, if we weren't here.

NIER: [laughter] If I wasn't doing this.

GRAYSON: I had gone through the publications list and marked one or two odds and ends of papers that I thought I'd like to ask you about. There was one entitled ["The Practicality of the Impractical."]³⁹

[END OF AUDIO, FILE 4.8]

GRAYSON: What prompted this?

NIER: Well, I was asked to give a talk at the Minnesota Academy of Science, which is an interdisciplinary group involving high-school and college science teachers. I was giving a talk on how important basic science is, in the practical world. I can speak from first-hand account, and that's what I was talking about—our work in mass spectrometry, which has turned out fairly important in some of these practical applications. I told about the mass business, how you could measure molecular composition from the precise masses, and some other examples.

GRAYSON: Okay, the title drew me in a little bit.

NIER: Yes, well, other people have said similar things. I'm sure that title's been used by other people.

GRAYSON: We had mentioned briefly the articles that you had published with Thorness on some interesting details. But before we get into that, here is one on simplified emission regulation.⁴⁰ That really is your fundamental ion-source design, right?

NIER: What number was this?

GRAYSON: Number fifty-six.

NIER: No, I think this may be something else.

³⁹ Alfred O.C. Nier, "The Practicality of the Impractical," *Minnesota Academy of Sciences* 32 (1964): 12-16.

⁴⁰ E.B. Winn and Alfred O. Nier, "A Simplified Emission Regulator for Mass-Spectrometer Ion Sources," *Review of Scientific Instruments* 20 (1949): 773.

GRAYSON: Mass spectrometer ion sources...1949.

NIER: No, I see...fifty-six.. Winn was a graduate student, worked with us, and this design; instead of regulating the current, we added a space-charge grid in there, sort of like a vacuum-tube.

GRAYSON: Okay.

NIER: This was, a simplified thing, I don't think anybody's ever used it. It wouldn't have as big a range to compensate over. But it seemed like an interesting idea. As far as I know, nobody else ever picked it up, and we never followed up on it. Probably should have.

GRAYSON: Well, the whole idea of feedback control of the filament-trap current; you developed that, didn't you?

NIER: Well...I don't know. You see, as I say, that whole business was based on the publication of an ion-gauge controller, which this guy [Louis N.] Ridenour did. Wasn't he later president of the University of Pennsylvania or something? He was an outstanding physicist at Princeton, at the time, about when I was a graduate student, or shortly after. I know I met him, about that time. And he published something about how you could control ionization gauges. You see, people in those days used to have to fiddle holding the emission constant, and gee, this is so sensitive to pressure.

GRAYSON: Yes, sure.

NIER: So you had to fiddle with this all the time, to monkey with it so you could have the right constant emission. Ridenour published something on how you could do this with a feedback circuit. And then, we picked this up for emission regulators for spectrometers, in about 1941, or thereabouts.

GRAYSON: Okay. So, it didn't really represent a primary contribution, it was mostly somebody else's work.

NIER: Correct, somebody else's work that we picked up.

GRAYSON: “An Automatic Chart-Plotter for Lecture Room Demonstrations.”⁴¹ That’s a curious number. Tell us a little bit about that.

NIER: Yes. [laughter] Well, I became interested at one time, in the whole question of demonstrations, for lectures, in large class rooms. And this was before the days of closed-circuit TV and VCRs. I’ve forgotten...when was the date on there. Was it in the 1950s?

GRAYSON: 1951

NIER: Oh, 1951 even. See, that’s a long time ago. It occurred to me that you could have, simply built, an oversized chart recorder; similar to the commercial ones that printed on little pieces of paper. Why not do this three feet high? And so, we built this thing, with a servo mechanism between our lab and the lecture room. I remember giving a colloquium talk in our big lecture room, and we used a piece of wide paper. I think we used some of brown wrapping paper, probably, three feet wide and four or five feet long. I could trace out a whole mass spectrum on that, and the whole audience could see it at the same time. It was kind of fun. And then we could do experiments. I had an assistant in the lab, who would do things, who would change the sample, or would hunt for a leak in the vacuum system, tune it to some constituent, or so on. So, we did these experiments that would be child’s play with closed-circuit television. But at that time it was novel. As far as I know, this instrument was another one of those things that nobody else ever did. [laughter] A one-of-a-kind thing.

GRAYSON: Well, as I say, there are some interesting titles here.

NIER: Yes.

GRAYSON: There has to be, with the number of them. Let’s see, the next one that caught my eye was 111. “Device for Remote Opening of a Vacuum System.”⁴²

NIER: Well, that was where you could cut off an aluminum cap. Thorness was the genius on this. We worked together on it where you had twelve little cutter-wheels in a little raceway. It looked like a ball-bearing race, except these were little wheels. Copied really, from what you had on a tubing cutter or pipe cutter. As it turned, they bit in to the wall—and we had to have a

⁴¹ R.B. Thorness and Alfred O. Nier, “An Automatic Chart-Plotter for Lecture Room Demonstrations,” *American Journal of Physics* 19 (1951): 416.

⁴² R.B. Thorness and Alfred O. Nier, “Device for Remote Opening of a Vacuum System,” *Review of Scientific Instruments* 33 (1962): 1005.

thin wall. You had to actuated it with these little gunpowder squibs which looked like little Sylphon bellows, except they were filled with gunpowder, and you exploded them with an electrical current. We were the first ones to do that trick. Other people did similar things. You see, you wanted to have the mass spectrometer evacuated, until it got up into the upper atmosphere, because it would never pump down fast enough to use since the rocket flight is all over with in a few minutes. So, you'd keep it evacuated. Other people had ceramic cups covering the source, and then you hit them with a hammer to bust it. They were amazingly successful, and apparently there weren't too many disasters. They took strobe pictures at the time of the Atmospheric Explorer project of this process. And people who had other instruments on the Atmospheric Explorer had these cups on there. It looked like a snowstorm when these things busted, because you had not only the big pieces, but there was a lot of ceramic dust that went all over, and mass spectrometers don't like insulating dust in the sources. So, these things of ours worked very well; they cut off cleanly. The squibs were mounted right on the cup, and this was spring loaded to fly away. So, you lost the squibs, which might have gun powder leaking, although they were sealed squibs.

GRAYSON: Yes.

NIER: You lost all of that, and it was clean. The only thing that was left was the half of a cup with a sharp edge on it.

GRAYSON: Yes. I noticed Thorness appears to be a regular author on some of these.

NIER: Several, yes.

GRAYSON: Now, this was the fellow who worked with...

NIER: He was the head of the shop. I took him to New York with me to head my shop at Kellex during the war. After the war, when we came back from the Manhattan Project, our machine shop was greatly expanded. Whereas he had been the sole man before, and these two other guys working independently, it was all unified under him, and it gradually grew to be about twenty or twenty-five people. So, at our peak we had something like twenty-five people working there, and he became head of it. The experience that he got in New York, hiring people in New York City to work as machinists in the shop, when all the good manpower had been already drained.

We didn't even get going until late 1943, and he got to learn how to handle people. He was a marvelous manager! Had no formal training for it, but he sure was a fast learner. So, he built a shop here under very adverse conditions, because our salary scale was not commensurate

with what industry was paying. The industries, like Honeywell, General Mills, Mining, and so on, could employ people with unlimited overtime, for instance, at premium pay, which we weren't allowed to do. So he had to convince people it was a good idea to come here, to work for lower salaries without these fringe benefits. We managed to get the work-week increased to 45 hours to sweeten the thing. We did get some compromises from our personnel people. We were caught in between on this thing, because our salary scale was out of line. But, he built a very good shop, under very adverse conditions. Wonderful guy.

GRAYSON: And then, there's also, the tube bending with ice-water.

NIER: Yes, and he was the one who devised the idea to use ice water for bending, so you didn't get kinks in the tube. Then you flatten it by filling it with ordinary water, plug it, the pressure goes up when you squeeze it to flatten it but it doesn't collapse in the middle to give you this figure-8 cross section You get nice flat tubes, as you saw earlier.

GRAYSON: So, when you flatten the tube, you just fill it with water?

NIER: Fill it with water, and squeeze it with the hydraulic press. You'd let out a little bit of water so that it didn't burst. [laughter] But he became an artist at. Again, this is the kind of thing that just takes a lot of art and experience. But he was very, very good. He learned what rate to press it. Of course, you have pressure gauges on it. And he learned at what rate you bleed out water to reduce the pressure so you don't get out of line. He became very skillful in making these things.

GRAYSON: So, you'd bend it first, and flatten it second.

NIER: Yes. Other people had bent things before. That was no innovation, they'd been doing that for maybe hundreds of years. But, they filled the tube with something that was nasty that you wouldn't want in a mass spectrometer vacuum. He filled it with just plain water...distilled water.

GRAYSON: It's the old problem with the vacuum requirement. What about number one hundred and twenty-two: "Mass Spectroscopy—An Old Field in a New World."⁴³ Now, what was this "Old Field" business? [laughter]

⁴³ Alfred O.C. Nier, "Mass Spectroscopy—An Old Field in a New World," *American Scientist* 54 (1966): 359-384. Reprinted in *The Physics Teacher* 5 (1967): 5-13.

NIER: Well, I was a Sigma Xi lecturer. I had to give this speech, go around to various smaller institutions to talk about something. I had to think up an interesting title, and so I talked about some of the modern developments of mass spectroscopy as of that time.

GRAYSON: I guess that concludes the discussion of the papers I wanted to know more about. Wait a minute, there's one more. I guess there are a number of biographical memoirs that you did, with the National Academy of Sciences.

NIER: Yes.

GRAYSON: And those would be, like I see the one on Tate. Well, obviously, I can understand the connection there. There was another one, published somewhat earlier than that.

NIER: That was the earlier one, the one on John Williams. You see, Tate had been my advisor. There's an interesting story about the Tate memoir. When a National Academy member dies, they try to get other members to write the memoirs. It was 1950 when he died. I had just been elected a member. I was considered very young. People thought I was too young to write on somebody so prestigious, and so, they asked non-members sometimes. Professor Buchta, who was head of the department, and had known Tate for many years, was asked to write it. But the guy was so busy, he promised everything to everybody, but he couldn't possibly do everything he wanted to do for everybody. And so, it just slid by, and nothing happened, and he died. He retired and died in the meantime. Now, Merrell Tuve, who was the home secretary for the National Academy, T-U-V-E, a former Minnesota guy, was very interested in promoting Minnesota. He was director of the Department of Terrestrial Magnetism of the Carnegie Foundation, and did a marvelous job of things there. And Van Vleck, the J. H. Van Vleck, who'd been at Minnesota, the Nobel Prize winner [Physics, 1977] for his work on magnetic phenomena at Harvard, found out that a memoir had never been written on Tate. This was several decades later, and so, they approached me. Van Vleck said he would help on it, if I would carry the ball. Well, it turned out he did a lot of the writing, probably more than I did, because he had one of those photographic memories. He remembered many things from Minnesota days, because he knew Tate so well, professionally. They were contemporaries over the years, and so we wrote this together. But it was very difficult to do, decades later. Because many of the people one should have contacted were no longer around, so, it wasn't as good of a job as it could have been. But, we did the best we could, under the circumstances. It didn't do justice to Tate, who was a truly great man.

GRAYSON: You've been the recipient of a number of honors, over the years. Rather than go through all of them, I'd just like for you to pick out the couple that you think were the most important to you, and the ones that you value. I don't want you to rate them or anything, but

obviously, I do, I guess. [laughter] Which do you think were the most interesting, and well-received ones that you really valued?

NIER: I don't know. These are all different. Obviously, being elected to the National Academy of Sciences was important. And since then, I was elected to the Philosophical Society. That sort of followed...and then the American Academy of Arts and Sciences. Those are sort of the big three. Well, most of my recognition has been in the geological area. The Geochemical Society gave me this Goldschmidt Award a few years ago, which was very nice. Before that, I got the Arthur L. Day Medal of the American Geological Society, which was truly a solid-gold medal. I repeated Archimedes' experiment on the medal, one time [laughter], immersing it in water, weighing it, and indeed it is essentially pure gold.

GRAYSON: The right density. [laughter]

NIER: The right density, anyway. That's right. [laughter] That was very nice. I was honored by the American Chemical Society with its Field-Franklin [Frank H. Field and Joe L. Franklin Award for Outstanding Achievement in Mass Spectrometry] award a few years ago [in 1985]. Well, all of these things have been nice in different ways. In different areas. The Day Medal was for somebody who applied other sciences to geological problems, so that was really for the geochronology work. The Goldschmidt Medal, that was a geochemistry sort of thing, so it was important in some geochemical problems. The medal from the American Chemical Society, the Field-Franklin award, was for applications to chemical problems in general. So, that was sort of nice, but they're all different. So, I don't know, you can't rate these things.

GRAYSON: Yes. [Frank H.] Field just retired in March.

NIER: Quite recently. I knew both him and [Joe L.] Franklin.

GRAYSON: You mentioned the Philosophical Society. Do you have any interest in philosophy as an area?

NIER: No, not particularly. The Society was started by Benjamin Franklin in Philadelphia, and it was about the development of useful knowledge. I think there's some other catchword that goes with it. So, it's devoted to all kinds of interesting things. The meetings are kind of fun. We're going to one next week. I haven't been for years.

GRAYSON: Oh, really?

NIER: They meet in Philadelphia for two and a half days, and people speak on various topics, in the sense that people used to years ago. These are experts, who talk to general crowds of people.

GRAYSON: Kind of like *Scientific American*?

NIER: Yes, that kind of thing, and again, they don't talk down to people, but they talk to them. It's a lot of fun to attend. It's a very relaxed meeting. They put you up in a hotel free of charge, during the time that you're there, and you get your meals as part of it. They have some endowment. You have to pay your own way to get there, but while you're there, you're the guest of the society, which is, kind of, nice.

GRAYSON: Yes.

NIER: It's kind of fun. As I say, it's a very homey sort of a thing, and I'm sorry I haven't gone more often. Very interdisciplinary. There are social scientists, humanities people, natural scientists, all kinds of people.

GRAYSON: I was curious if, during the work that you performed over the years, if there was any point when you knew, or recognized yourself, how important some of these results were going to be. Did you ever feel like you were on the verge of doing something that was truly earthshaking while you were doing it?

NIER: Well, I think that the closest that I came to that was the variations in common lead, which is probably the most unfortunate thing, when you stop and look back at it. Doing that opened up a whole new area, and I was aware of it at the time, but unfortunately, I didn't exploit it. That was just too bad. I was too busy. If I had followed up on that I'd have been generations ahead of other people, because nobody else had instruments as good as I did for many years. I could have done much more, and could have come up with the age of the solar system.

GRAYSON: Which would have been really fundamental.

NIER: I certainly could have come up with the fact that the age was more than two billion years. But I didn't quite do enough, and I didn't know enough about geology, and was kind of advised by some people that I shouldn't stick my neck out on things that I didn't know much

about. I should have done more on that, but the war intervened. That was the thing...you see, the very time I would have done this, I got so involved and interested in the development for the Manhattan Project. And of course, the isolation of U-235 was certainly a very significant thing. And I think the development of these various instruments for the Manhattan Project...I've recognized them as important. Other people may not have. I was filling a void that needed to be filled.

GRAYSON: Yes.

NIER: I think I did a pretty good job on developments for the Manhattan Project. The leak detector certainly was a beautiful innovation. And the fact that we could monitor the entire plant, at one time, this was sort of an interesting thing. Nobody's ever done anything like it since. [laughter] That's forty-five years ago...so, it was an interesting accomplishment. I got a lot of satisfaction out of it, yes.

GRAYSON: Well, I think, as we had mentioned yesterday, probably the gaseous diffusion process...that plant would not have gotten to the stage that it was in as short of a period of time, had it not been for your work.

NIER: Well, probably not, it would have gotten there eventually. None of these things are that way...where it's go or no go. But certainly, I suppose a significant thing was the fact that we were able to analyze the samples from Columbia University made by Dunning, Booth, von Grosse, and colleagues. They were able to make small separations; they changed the isotope abundance by about a half a percent. And then, we could prove that it was unequivocally so. Which showed that, with a few stages, they could enrich the uranium that way. Certainly, that was a selling point that gave people confidence, that you could extrapolate this a million times, or so, and go ahead. Incidentally, the main plant, in Oak Ridge, got ahead of the pilot plant, that was supposed to be built in Jersey City [New Jersey], because they moved ahead so well on the entire program. The pilot plant ended up being used for studying welding techniques, etc. The main plant got ahead of the pilot plant in the development that went on, and that was because of so many things went well. You see, they could bypass things that they planned to do in the pilot plant, which they found weren't necessary. So, that was certainly a significant thing that happened. I think, the precision mass work we did after the war, which made possible identifying molecules from their exact molecular weight, would certainly have to rate as fairly important; because that opened up whole new areas or research.

GRAYSON: But, did you understand the implications of that, at the time?

NIER: Yes, yes. I should have said more about it.

GRAYSON: Yes, right...quite obviously.

NIER: I said that you could identify molecules from their precise weight. After all, J. J. Thomson, in a sense, did it, but not for the precise weights. You see, the thing is, we had the first mass spectrometer that could do this. And you could put things in at will. Well, it didn't take long for the Metropolitan-Vickers people, and John Beynon, to catch on to the significance of that. They saw right away, and people interested in this sort of molecular structure business, saw this, it was just something waiting to be done. And they went ahead, on it, full-speed ahead.

GRAYSON: I can tell you an interesting anecdote about that particular thing, because I just finished looking at a transcript of a tape that Beynon had made with Graham Cooks. Beynon went to Metropolitan-Vickers, or Met-Vickers, or however you refer to it, and asked them if they could provide a machine with certain specifications. So, they quoted him some resolution figures, and essentially said to him, "The machine will work with this good of a resolution until you put your damn organic crap in it." [laughter] Metropolitan-Vickers was saying, "We can build something that will give you this resolution, but when you stick your damn sample in it, it probably won't work." So, the point he made was that they were really, kind of, reluctant to sign on the dotted line to performance figures because...

NIER: Yeah, yeah, I understand.

GRAYSON: ...from the physical viewpoint, and the work that you'd done, and what they'd seen, and so on, it was obvious that you could get the resolution, but, they were really afraid that it wouldn't be usable for organic samples.

NIER: I don't think they had the experience we had in dealing with organic compounds. Because, we'd been using them in the mass work, and I don't think I would have given him that answer; because I think we knew enough about these instruments to know what you could put in. There's a limit to what you can put in the instruments, obviously, but as long as you hold the pressure down, you can do quite a bit. We'd been putting all kinds of crazy things in for comparison masses, when we were doing metals; so this was nothing new to us. I think they were being more cautious because they didn't have the experience.

GRAYSON: Well, they obviously didn't.

NIER: They were being cautious, because they had to sign on the dotted line guaranteeing performance.

GRAYSON: Right.

NIER: And, money was involved. As soon as money is involved, it's a different ballgame. [laughter]

GRAYSON: [laughter] Right. But, it's an interesting idea that they knew that you could do it, so all they had to do was repeat the work. Let's see, I had a number of other issues I wanted to touch on, before we wrap things up here. Obviously, I don't suppose you have any interests that go beyond research work. Well, yes, you do, you go camping and boating and skiing. Tell us a little bit about some of the other activities that you do when you're not in here, doing research.

NIER: Well, not too much. I have a summer cabin up in Northern Minnesota and we spend some time there. I suppose I get in the usual long weekends. I'm sometimes there for a week at a time, but I never spend much time there. I do that and go to various meetings and take trips. I was remarried in 1969, and my wife had been a secretary here in the department. In fact, she was my secretary for a number of years. She had many friends, and I have many friends, largely younger people who'd been students in my classes, who were guys who were a little older than normal graduate students after the war. So, a lot of these people, some of whom were my students, got together from time to time. My colleague, John Williams, who had been in charge of the van der Graaf generator at Los Alamos work during the war also had students. We had many, between the two of us, plus Charlie [Charles] Critchfield's students. He was a nuclear theorist in the department. We have many former graduate students who are now retiring or retired from Los Alamos, who went there after the war. [laughter]

GRAYSON: I see.

NIER: So, we have dozens and dozens of friends. Both my wife and I like to do outdoor things, and there were those wonderful people there, who were such adventurers, who like to think of going to the bottom of the Grand Canyon. We're going to the North Rim of the Grand Canyon this coming fall, and my wife will let me climb part way down. Not all the way, because she's worried about something happening to me because of my heart. But we will go down part way, and some of the people will go all the way. We've been there before. I wasn't allowed to this last time, but one time we went all of the way to the bottom of the South Rim. We've been to Glacier [National] Park, climbing around there and various places. We do these different things. We have one or two expeditions a year. We were in Hawaii with the gang recently. The rest of the time, I seem to be so busy, there isn't much time to do anything else.

[laughter] I've been three times on trips that were a better part of a week since January, and will be leaving for a combination of vacation and meetings at the National Academy and the American Philosophical Society at the end of April. And there'll be something else in May; we'll open up our cabin. And then we're going to Europe. The Meteoritical Society meets around the first of August, so we'll stretch that into three weeks while we're over there...I have lots of friends in different places.

GRAYSON: I'm getting envious.

NIER: [laughter] Well, I made wonderful connections in Europe, as I say, through Mattauch, who I met for the first time, face-to-face, in 1951 at that meeting that you have a picture of. He visited here right after that, so I met him and Hintenberger, Ogata from Japan, Richard Herzog, *the Herzog*...he's still living. He's not well, his health is not well. He lives down in Florida somewhere. And [Wolfgang] Paul, the man who invented the quadrupole. He was at that meeting too. He's on the picture as well. You meet all of these people, and of course, having met them once, when you go over, you visit them again. So, I have all of these connections in different places, so we can just jump from place to place.

GRAYSON: [laughter]

NIER: Claude Allègre in Paris, and the people in Bonn, Van Zahn and Paul, the people in Bern, the people in Zurich, the people in Vienna, and especially the people in Mainz. I'm even a member of the Max Planck Gesellschaft.

GRAYSON: Oh.

NIER: I am a *Auswaertige Wissenschaftliche Mitglied*.

GRAYSON: [laughter]

NIER: You can figure that one out... [laughter]

GRAYSON: It sounds like an outside...

NIER: Foreign scientific member of the society. I also have friends in England, and various places, through the three-year meetings there.

GRAYSON: The Triennial Conference?

NIER: Yes. And there are the meteorite connections. So, I know a lot of people. It's not as many as some people do, but I know enough that I get around.

GRAYSON: Yes. [laughter]

NIER: [beeper beeping] Sorry.

GRAYSON: No problem. This is one of these real big, difficult to handle questions, but I'm sure you've probably reflected on something like this at some time. There's been a tremendous number of technological advances in our society, in our world; many of which you've lived through and seen, and implemented. In a very broad, overall sense, which of these would you consider to be among the most important? What would you rate as the top two or three things that have happened since you started into science?

NIER: Well, I suppose the invention of the transistor. You would have to put that at the top, because this has influenced everything that's happened since. I mean, the whole computer technology, communications, it has influenced so much.

GRAYSON: It would definitely have to be among the top things in your opinion?

NIER: You would have to put that...well, if not the top, certainly one of the most significant things. And, as I think I told you, I knew the three people who were involved.

GRAYSON: Oh, really?

NIER: John Bardeen and I were the two people who came here to fill openings on the faculty in 1938.

[END OF AUDIO, FILE 4.9]

NIER: We started here together in 1938. He was a theorist, and I was an experimental person. We had been good friends then and still are very good friends. He left to work for the [U.S.] Navy during the war and I worked on the Manhattan Project. As I've told people; he's the guy who won parts of two Nobel prizes so I should be considered the failure of the two appointments at the University in 1938. [laughter]

GRAYSON: And you were mentioning that you knew the others who worked on the transistor.

NIER: Well, the experimental man on the project was Walter Brattain, who was a graduate student at Minnesota in the 1920s. I only met him after I finished up. Met him for the first time in 1936, when I was visiting Bleakney, when I went east to work with General Electric company. I took a trip down to Princeton one weekend and met John Hipple, who was student of Bleakney's. Then we drove up to New York, and I headed home to Schenectady, and we stopped and visited Walter Brattain. He was the experimental man, on the transistor, the third one was [William] Shockley, Bill Shockley.

GRAYSON: Yes.

NIER: I'd met him independently of the others. I didn't know him like Brattain and Bardeen. They had the Minnesota connection, Bardeen on the faculty and Brattain, as a student here. So, I knew all three of these guys. And they were good. [laughter]

GRAYSON: From what I've read, when they actually made the first transistor and demonstrated it, it was just not considered to be anything. It was like Tate said to you... [laughter]

NIER: Yes,

GRAYSON: I think it took a while for people to really appreciate the implications of it.

NIER: Well, I think this is probably true, except that it's not surprising. Bardeen's understanding of solid-state physics was really profound. You know, this guy was really different.

GRAYSON: So, even when you knew him here he exhibited unusual ability?

NIER: This guy was really an order of magnitude better than the rest of us. He had insight into physical phenomena. You know, he and I wrote a paper together on the thermal diffusion column.⁴⁴ Look at the publication list for about 1941. He became interested while I was separating isotopes, and there'd been some papers by people, explaining the performance of these columns. He condensed the thoughts in these papers, in which people went on, and on, talking about all kinds of things...he condensed all of this to about a page. So, he, sort of, reviewed their stuff, and came down with everything on one sheet of paper, which condensed everything they had done, in a much simpler, easier to understand form in a very short time.

GRAYSON: Here it is, it was number twenty-four.

NIER: When was it published?

GRAYSON: Publication number twenty-four, 1941.

NIER: That was on describing the performance of the column. He and I published this together.

GRAYSON: That's, kind of, an interesting connection.

NIER: Yes.

GRAYSON: I went through this publication list, and noticed a lot of the co-authors, but I never connected this Bardeen with *the* Bardeen.

NIER: That is *the* Bardeen. But, his understanding of things was just so different. He really had what it took. So, it's not surprising that later on, his interpretation of low-temperature phenomena near absolute zero was so good.

⁴⁴ Alfred O. Nier and John Bardeen, "The Production of Concentrated Carbon-13 by Thermal Diffusion," *Journal of Chemical Physics* 9 (1941): 690.

GRAYSON: So, is there anything else you'd like to rank up there with the transistor? Obviously, a lot of things have derived from it, as you mentioned, computers and communications and so on.

NIER: Well...it's hard to say. That certainly was the thing that changed so many things in the world. That has to go down in history as a major development. If you have to name a half a dozen things through all of history, I think you have to include it, because this revolutionized so many things.

GRAYSON: Yes.

NIER: And, it still is. You're talking about the word processor you're going to do this on. This lead [to] the whole development of microcircuits, chips, and all kinds of things. One of the things that has taken place, which is certainly not of the magnitude of the transistor, is the application of instrumentation to scientific problems of all kinds. This really came out of World War II. The appreciation of the need for good instruments; and, if you have them, you can do all kinds of things.

In my own experience, there were only a handful of people, who could operate, build mass spectrometers, and use them, before the War. As I say, we were the only people in the world who could measure uranium in 1941. And, we certainly had the best instrument for measuring things like carbon, in 1941. We were ahead of people, and it wasn't that there weren't problems that people would have liked to have done with carbon, it's just that they couldn't build instruments and do their thing at the same time. So, people like myself could build instruments that were superb, but we didn't have the necessary grasp of what you could do with them. This, of course, was the problem I had with the geology application. I could have found out about the age of the solar system, if I had known more of the geology, and understood the lead business better. I simply didn't know enough geology to appreciate that I had something very important.

If you could make these tools available to other people, who didn't necessarily have to know how a mass spectrometer worked, but could use it, they could apply it to problems in their fields. I think that this is one of the very important developments of our time. This came about as a consequence of World War II, where, suddenly, after the war, money became available, especially through the Office of Naval Research. The Navy had really top people who appreciated how important science was to the welfare of the country. So money became available for people to buy instruments, and then, since money was available, companies could build instruments. There was some incentive for people to do this. I think that the revolution in instrumentation of all kinds came out of that period, and mass spectrometry is just one example. You can now get an instrument to do any darn thing you want it to do. You've got the money, and somebody will build it for you. Well, the idea that you would spend money to do this before, nobody ever thought of, you see.

GRAYSON: Yes.

NIER: Or, if they thought of it, they never got anywhere. So, I think that that is a very important development of our time; which has certainly changed the world.

GRAYSON: You're saying that our government did something in a technological arena that was ripe for change.

NIER: Well, certainly, what they did, in the Office of Naval Research, in those early days, was to support basic research of all kinds. That was certainly very good. And, of course, the National Science Foundation was patterned on the Office of Naval Research. The difficulty with the concept, however, is the idea that you can do all sorts of things. In the military it means there is no end to money. They have an infinite reservoir of money. So, they can think of all kinds of things they would like to do, where there used to be some limit on what these people could do. There's no limit now. Therefore you have all of these sophisticated weapons systems, and then you have to have counter-weapon systems, and so on. The thing has just gone wild in the military area. There used to be some budget restraints on things, that limited how far you went. Now, that's all opened up, because there's been this appreciation that you could make ever-more sophisticated devices, and so on. So, this is a terrible negative side to the whole picture. The same thing that brought you all these wonderful positive things, has brought with it all these terrible, negative things. But I think an interesting thing was brought up at the meeting of the American Chemical Society at the symposium, on the history of fifty years of nuclear fission by Richard Rhodes, who wrote the wonderful book on the making of the atomic bomb. He pointed out that you can say all you want to about the bomb business, but the fact of the matter remains, the likelihood of a major war is less today than it's ever been. Because nobody dares risk it. You may not like this stand-off, because of the terror part, but the fact remains, that war has gotten too dangerous. So, you can't have a world war. And this may be one of the very positive things that comes out of this whole awful business. I don't know...but it certainly is an interesting aspect of it.

GRAYSON: I think it's fairly obvious, too, that if the United States had not decided to vigorously pursue the development of atomic weapons, that would have been perhaps a worse outcome.

NIER: I think we'd be worse off.

GRAYSON: Basically, it was something that was going to happen. People may argue the various pros and cons, but it was something that was going to happen.

NIER: Well, once the means had been shown that you could do it then there's no end to where these things can go. This is the awful part about it. In a little different context, I remember interviewing Harold Urey one time. This was before the landing, on the moon. He was always interested in the moon and he was here to give some lectures on the moon. It was one of his pet subjects. He had criticized some of the geologists and the things geologists were saying because they didn't know any physical chemistry, and they didn't know that some of the things they were saying were thermodynamically impossible. He reformed a lot of thinking in the geological sciences as a result of his input. But he made an interesting remark at the interview. This may have been before the Apollo program was underway, I don't know, but there were people were arguing pro and con: should you send people to the moon or not? And his answer was that the question was not relevant. He said, "The means are at hand for doing this, there are people who want to do it. It will be done." I thought that this was really, a very interesting comment, wasn't it? It's on a tape here, I think, somewhere. It was on a television program, or maybe it was a radio program, where I interviewed him for our local station at the University. And I thought that that was interesting, and certainly, he was not in favor of spending money on the hoopla of sending men places, as contrasted with spending it on science.

GRAYSON: Sure.

NIER: He said he was not in favor of that, but he was very realistic about it. He accepted the reality.

GRAYSON: He wasn't going to get in a stew about the fact that we could spend the money in other more profitable ways.

NIER: That's right. He said the means are at hand, and eventually, it would be. And I think this is true about many things that we've seen in history.

GRAYSON: When I was looking over your publication list, I took the liberty of arranging it, in a rather different order. I want to conclude by talking a little bit about it in the order that I have here.

NIER: [laughter]

GRAYSON: One thing you can see, in this graphic arrangement, is that there are a number of periods when there are peaks and valleys in output. I suppose that's natural in anyone's activities. The first valley in your publications, was around 1942, 1943, 1944, 1945. I suppose that's related to the fact that everything you did was classified at the time.

NIER: That's right. And there was no science being done. It was all of this application business. I dropped out of sight in the 1940s.

GRAYSON: You dropped out of sight in the 1940s. That was essentially the kind of commitment that you were making to the whole business of the war effort.

NIER: Yes, yes, yes.

GRAYSON: Okay, it cooled down again around 1954. There's always a few publications in there, but only a couple.

NIER: Yes.

GRAYSON: Between 1952 and 1956. Is there any reason for the lower output?

NIER: It may be statistical. [laughter]

GRAYSON: Just a statistical fluctuation.

NIER: Well, the mass business was beginning to cool off. And then, I got interested in the meteorite business. That took a little time to get up to speed so that's probably not surprising.

GRAYSON: Okay, well, it goes back up to a more respectable rate after that. [laughter]

NIER: [laughter]

GRAYSON: You maintained a high level of performance for a number of years, and probably around the mid 1960s, it slows down a bit. In that period, most of the work was in the area of

upper atmospheric studies. There were a couple of papers a year coming out on a fairly regular basis.

NIER: Yes.

GRAYSON: And then, there is a peak here, towards 1976 or 1977, which represents work in the?

NIER: Well, it was the Atmospheric Explorer business. There were all kinds of joint projects which went on with other people.

GRAYSON: Okay.

NIER: So, a lot of those publications are phonies in a sense. My name may be on them, but that's when you have all of these big group efforts.

GRAYSON: I see you didn't make as many contributions to the work as in other efforts.

NIER: True. That explains the anomaly.

GRAYSON: So, as we get to the present it's slowly tapering off, but I see where you still have a couple of papers coming out of here.

NIER: Yes.

GRAYSON: Now it seems like the more recent ones are back into the cosmic dust.

NIER: Well, we're getting back into things again. And we have some things waiting to get published. I have a paper pending...I have to rewrite it.⁴⁵ It is on our deep Pacific work. It came back for revision. They said they accepted it, provided we rewrote it. And, I'm in the middle of that now.

⁴⁵ A.O. Nier, D.J. Schlutter, and D.E. Brownlee, "Helium and Neon Isotopes in Deep Pacific Ocean Sediments," *Geochimica et Cosmochimica Acta* 54 (1990): 173-182.

GRAYSON: Oh. You still get those kind of notices, too, huh? [laughter]

NIER: Yes, oh, yes, yes, very much so.

GRAYSON: [laughter] That's very encouraging to the rest of us.

NIER: Encouraging, yes. So, that's being rewritten. We've got results on the variations of helium isotope ratios in the natural metals, for instance. I've reported this in abstracts for meetings. That's in the list of things. It had been shown in Russia before that there were some anomalously high helium 3-4 [ratios] in some materials. We did a more systematic study on many natural metals and found some very interesting variations. There may be a thousand times as much helium-3 compared to 4, in something like native copper, which you can find in the Upper Peninsula in Michigan. We've done samples like that. It's terribly sporadic, and you always worry that you made some mistake on the sample, but I think it's very real. It may be tied to this latest flurry, about the fusion business. [The controversy about 'cold fusion' had just gotten underway.]

GRAYSON: Oh.

NIER: There is a possibility that you have in nature, cases where you might have had something like this [cold fusion scandal] going on very locally, and this would make helium-3.

GRAYSON: This is the most recent thing with cold fusion?

NIER: From Utah.

GRAYSON: Oh, really?

NIER: Yes.

GRAYSON: Now, that would be interesting.

NIER: Yes.

GRAYSON: These anomalous high concentrations could be like remnants of this cold fusion phenomenon?

NIER: Yes. That's correct. I don't know. But that's one of those things. But our measurements are very real. We found, for instance, in processed metals, say a piece of iron, the chances are the helium-3/helium-4 ratio is not atmospheric. There's always helium in everything. It doesn't matter. I'm talking about small amounts, you understand.

GRAYSON: Sure.

NIER: Anything you pick up outgasses helium. But, you'd think it would be atmospheric. The explanation people have given is "Well, that's because there's so much tritium being generated, that decays to helium-3, and what do you expect?" But, there's far too much for that, and what's more, it occurs in metals that are pre-war.

GRAYSON: Yes.

NIER: We have samples. In fact, I just got some on my desk this last week—of pre-war metals. We got them out of the shop, so they hadn't been bugged by tritium. There's so many sources of tritium. It is used a lot for diffusion studies in metals, and so any metal that may have been reprocessed could have this in. The tritium decays to helium-3 and stays. So, you see, you expect this. So, we're looking at old samples from pre-war time, which we have in our shop. I even filed some iron off of a hinge of a house of mine, which goes back before the war. [laughter] It may be that the explanation is that you had a heavy rain after the bomb tests, but you can't explain so much of this. And the striking case is one of a mineral, a native metal called Josephinite. It's an iron-nickel alloy. And you find isotopic ratios in there that are a hundred times atmospheric.

It was observed by the people in Bern, but apparently, their experiment was wrong, and their vacuum system was contaminated with helium-3 they'd used for tracer studies. It's sort of understood that they bugged it. We looked at the same material, and by coincidence, we get the same high values of helium-3/helium-4. But there's no explanation for it. And the frustrating thing about it is, you file a little bit off of one corner, and you get something...you file it off of another corner, and you get something else. So, you say, "Gee, I really must have contaminated this one," when you didn't contaminate that one. Then, about the time you do it, you're very careful and you file somewhere else, and you find a high value again. So, they're very local effects. The ones at Brigham Young University, Provo [Utah] who have observed cold fusion. They don't make claims of getting energy out of it. Like the people over at Salt

Lake City [Utah]. But they do point out that there could be some things in nature going on related to this, and that you may have cases where there's strains in the material, so you squeeze the deuterium there even tighter together. In which case, the transitional probability of the muon going in and causing these disintegrations is even greater. So, they speculated on that in an article that's in press right now in *Nature*, or it has been submitted. I don't know, it may be related to that.

GRAYSON: That would be interesting.

NIER: That's the latest thing. But we should publish on this stuff, on our metals anyhow. I gave an abstract at a meeting, to talk about that, at the Meteoritical Society last summer in Fayetteville, Arkansas, of all places.

GRAYSON: [laughter]

NIER: They meet all over the world. They met at Fayetteville, Arkansas.

GRAYSON: That's an interesting place to do it.

NIER: Yes...well, it happened to be a very good department down there, doing meteoritic studies. That's how we happened to meet there. I talked about our metal studies and found a lot of people interested in it.

GRAYSON: Now, I recall that when we were planning this interview, you had a couple of proposals that you were working on. Can you just tell us a little about those?

NIER: Well, very quickly...renewal of our work on extraterrestrial material. That was due. That goes to NASA at Houston, which is the home of that kind of activity. And we've had support from them for four or five years. Hopefully, they'll continue to support us. That had to be 1 April. Then, the other one was the continuation of our work in instrument development, that capillary work that I showed you. It looked like an interesting way to extend the use of the instrument. Then, we'd like to do some other developments in spectrometers in general. This was terrible to try to get those both out at the same time. This was just dreadful!

GRAYSON: So, the point of all of this is that you have an active research program going on even as we speak.

NIER: Yes.

GRAYSON: And that this whole “retirement” business is mostly hokum I think.

NIER: Well, I don’t know. As I say, my wife thinks the only thing I’ve retired from is my salary.

GRAYSON: [laughter]

NIER: And she is correct on that. Fortunately, we are able to live on my annuities and investments. So, money is not a great problem. But there isn’t enough time to do everything.

GRAYSON: I like that, “retired from my salary.” I’d like to wrap up with one question here that I’ve thought about a little bit. What I’ve seen, in looking over your record of publications, and trying to get prepared for this interview, is a man who has the attention span of a three-year-old. [laughter]

NIER: [laughter]

GRAYSON: I’ve arranged your work in a table with different columns for different areas of pursuit with the years as rows. So, basically, in the instrumentation column, there’s kind of a constant output. But, the rest of the work, seems to slide diagonally from the upper left-hand corner with one set of interests in the beginning, to the lower right-hand corner with present interest. Now I’ve divided the interests into ten or so columns over your career. The thought that occurs to me, that new people coming into a field, like nuclear atomic physics, will see your name as a reference to some of the early work in isotopes. Likewise, people new to geological studies will see your name referenced in some of the early works there. People coming into following biological processes and metabolism by tracer elements, and so on, will see your name at the beginning of this field of study. It’s the same way with every one of these fields, the cosmological studies, and so on. For instance, the Nier-Johnson geometry is a name that is well-known in organic applications of mass spectrometry. Once again, your name and work is there in the beginning of it. What I’m saying is, different people in different areas of study see you as different persons. Some would think you might be an organic chemist, others a physicist, others a geologist.

NIER: Who do you see Al Nier as being?

GRAYSON: Besides being a three-year-old kid, who keeps running around from one thing to the other. That's kind of a complicated [question].

NIER: Somebody who's fighting to hang on. [laughter] The world is changing so fast, to keep up with it is real difficult. You know, I'm not as smart as most of the people, so I have to work harder. This is one of the problems I have. Well, I try to find areas where there's interesting things to do. And, without trying to appear too noble on this business, I sort of enjoy helping people in other areas. So, the biology was certainly off the beaten path. But, some of the most important things, were done that way. This early work done with the people at Ames...Harlan Wood, I see in [publication] number twenty-five, for example, where we provided them with the heavy carbon and did the analysis.⁴⁶ Later on Wood built his own mass spectrometer, built his own thermal diffusion columns, did this beautiful biochemical work, all at the same time, there at Ames, before he left. He was on the faculty here for a while before he went to [Case] Western Reserve [University]. He's still there, he retired. He became head of...I guess it was the biochemistry department, there.

But, that [paper] mentioned important things. This is where I had played a part in it. A very minor part: we provided the material, we did the analyses, and made it possible. But that is certainly among the more important work that was done in biochemistry in recent years. And we played a little part in it. So, I had some fun and helped one of our people in the botany department at the same time. There were some publications, but nothing spectacular like this came out of it. And, I helped one of the people in botany, Alan Brown, who's been gone many years from here, but he started some oxygen work...what did he do? I've forgotten what it was. Paul Boyer, who was a very good biochemist, used to be on the St. Paul campus. Did he overlap you, [Tom Krick] by chance?

KRICK: No, he didn't overlap me, but I've heard of his work.

NIER: Well, I helped him get going on the oxygen stuff. And a lot of this came about through the campus club, where we had lunch yesterday. You see I knew these people independently, socially...we belonged to the same dancing club, and things like that. It was a faculty dancing club. So, I knew him from other connections. So, it was kind of funny. You'd see these problems these people would have, and I'd make these crazy, wild promises, which I had no business making

⁴⁶ H.G. Wood, C.H. Werkman, Allan Hemingway, and Alfred O. Nier, "Heavy Carbon as a Tracer in Heterotropic Carbon Dioxide Assimilation," *Journal of Biological Chemistry* 139 (1941): 365.

GRAYSON: “Gee, we could help you on that. That’s easy to do!” Well, it usually was more difficult than you thought. But, somehow or other, I got involved in these things with other people.

[recording paused]

GRAYSON: At lunch, we were going to talk about the A. O. C. Nier business. Do you want to go on about that? I’ve got my machine going here on a condenser microphone.

NIER: Well, my full name is Alfred...is it running?

GRAYSON: Yes.

NIER: Alfred Otto Carl Nier. That’s the way I was baptized. My father wanted to have my first name be Hans. It was going to be Hans Otto Carl Nier. My mother said, “No, we are in America now, and that’s just too German.” So, that part was changed to Alfred. There were two uncles. The Otto was an uncle on my mother’s side of the family...he was actually her sister’s husband. And, Carl was a half-brother of my father, who’d spent most of his life in St. Louis. He lived in St. Louis. Uncle Carl. So, I was named for these two. When I grew up...I didn’t have to use my signature very much as a kid. But I dropped the second initial, because that made me different from other kids. And I didn’t want to be different, so I just had the one initial. And that was how I started publishing...my first publications, which you saw on the list there, just with the ‘O.’ So, you start this kind of thing, and you want to continue, otherwise the tabulation of your work gets confused. And this went on until the war, when there was all of this security business. So, in about 1942, I got a call from Washington. What they had discovered was...I apparently had these two different names, or were these two different people? So, I got a telegram which said, in effect, “What the hell is your name, anyway?”

GRAYSON: [laughter]

NIER: So, at that point, I gave them the full name. But having started and published a fair number of things before then with just the ‘O’, I thought I should continue. So, all my publications are with ‘O’ except one, where I was a joint author; it slipped through with a “C” in it. So you’ll discover, in that whole list, there’s only one publication with A. O. C. Nier. It will be an interesting exercise to see if you can find that one. Be that as it may, I use just one initial, just for consistency. But practically, everything I sign now, except occasionally when it’s something like this, where you worry about the confusion, I use both initials. That’s the answer.

[END OF AUDIO, FILE 4.10]

[END OF INTERVIEW]

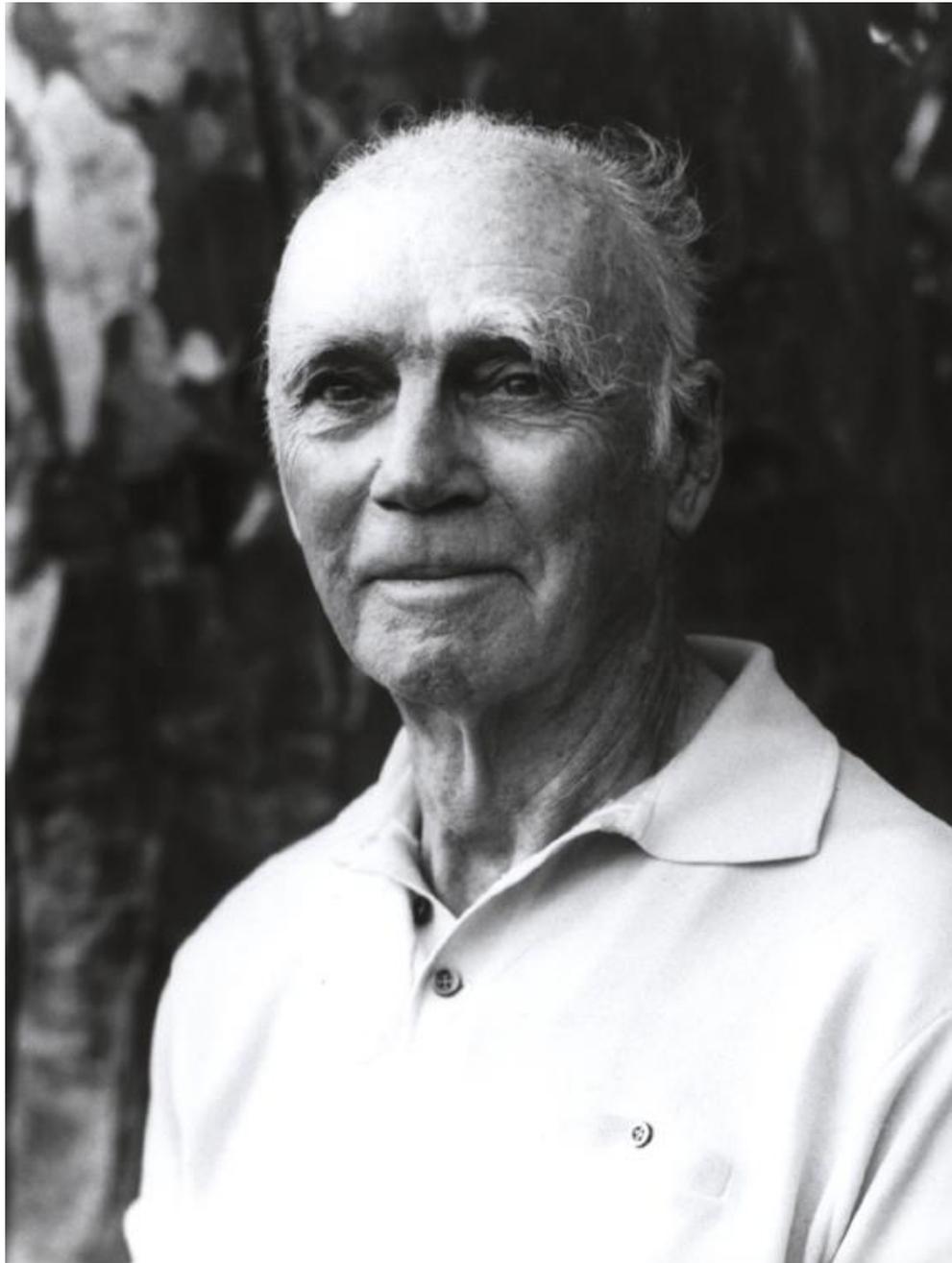


Figure 1. Walter Bleakney, inventor of cross beam mass spectrometer in 1929. Photo taken about 1983, when Bleakney was 82.

Mass Spectrograph in Rem 68

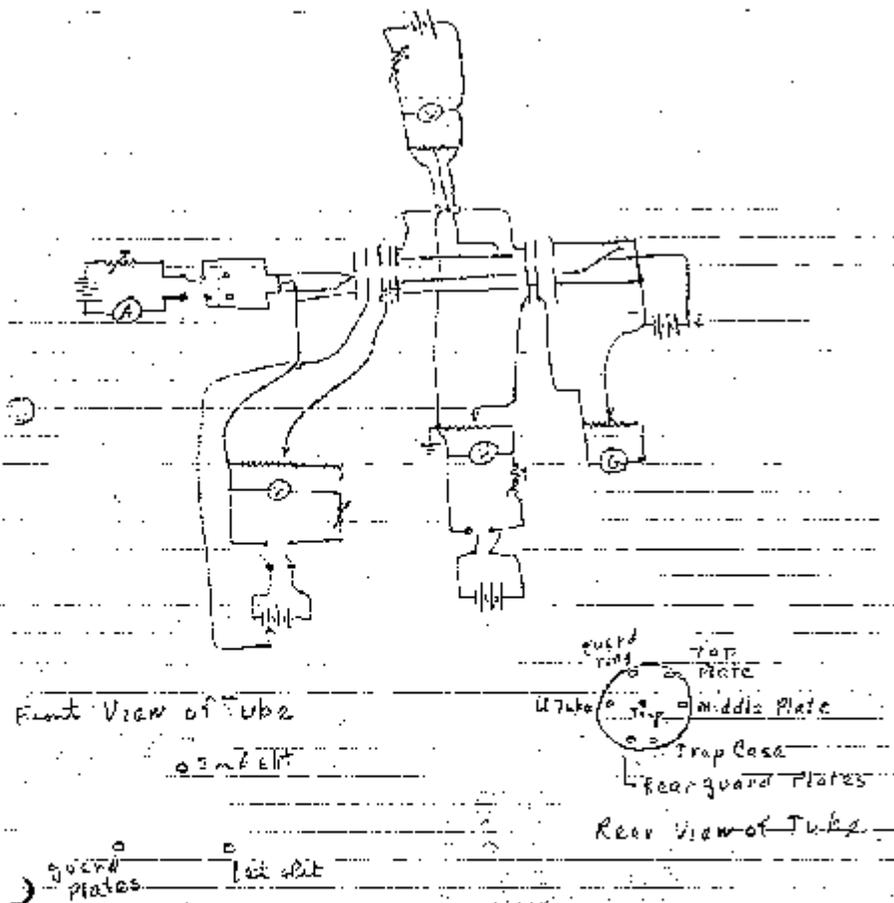


figure 2. Electrical schematic for 180° solenoid mass spectrometer used for isotope abundance measurements by A. G. Nier, 1934-36. See Ref. 4.

4/21/35		Easter Sunday		Hall report data of Friday	
	W	W	W	W	W
	12.28	9.0	18		23.9
	25.32	4.0			
	32	42.5	17.7	24.2	12
	22.05	62.8		43.1	
	1	35.7	17.5	18.3	10
	15	57		39.3	
	2	83		62.3	
	→ 25	86.6		68.9	
	3	76.9		59.2	
	35	59		41.3	
	4	53.5		25.8	
	45	78		62.3	
	5	47	17.4	29.3	8
	55	86.7	1	68.5	
	6	40.2	17.3	42.9	7
	6	76		52.7	
	67	77.3		60	
	75	57.6		40.3	
	8	39.5		22.2	
	85	29.2		12	
	9	42.8	17.3	25.5	8
	95	38.7		21.7	
	33	42.2		24.9	
	85	51.8		34.5	
	1	56.4		39.1	
	15	53.1		35.2	
	2	42.9		25.6	
	25	32.2		15.9	
	3	28		10.7	
	35	60.3	17.4	42.9	10
	4	56.5		39.1	
	45	59		41.6	
	5	68.1		50.7	
	→ 55	73.7		56.3	
	6	71		53.6	
	65	66.5		49.1	

Figure 3. Original data of C₆H₆ spectrum, 1935.

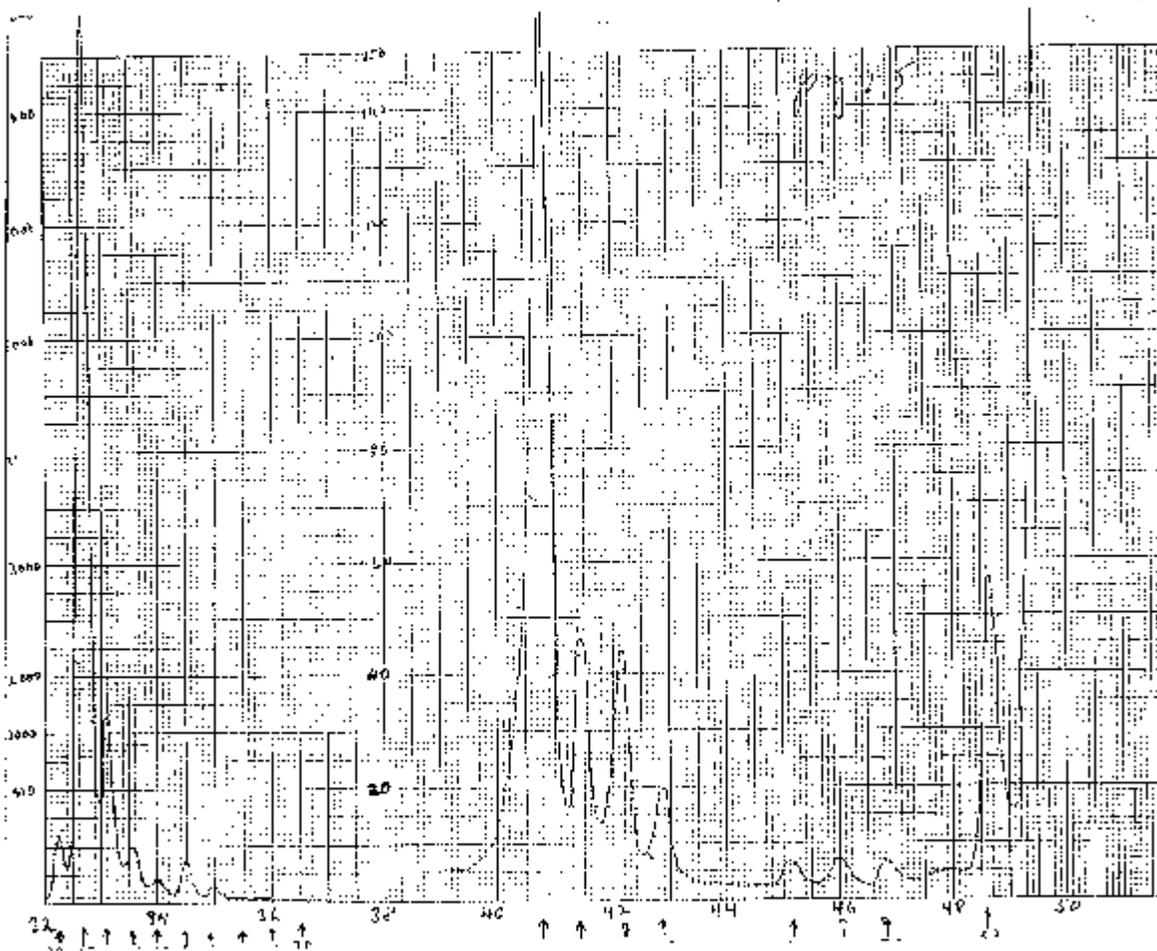


Figure 4. First plot of C_6H_6 spectrum, showing 6, 5, and 4 carbon groups.

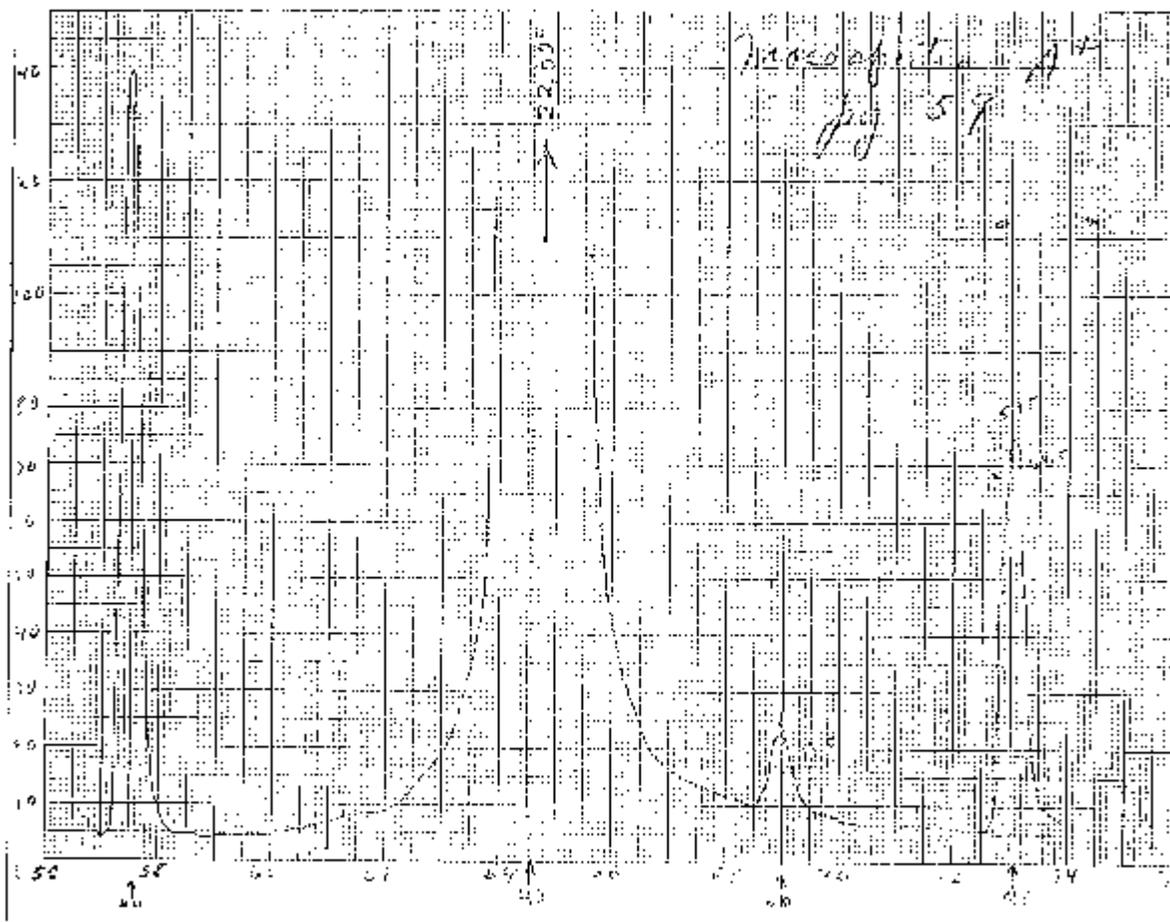


figure 5. First mass spectrum showing A^{38} (early 1955).
See Ref. 4.

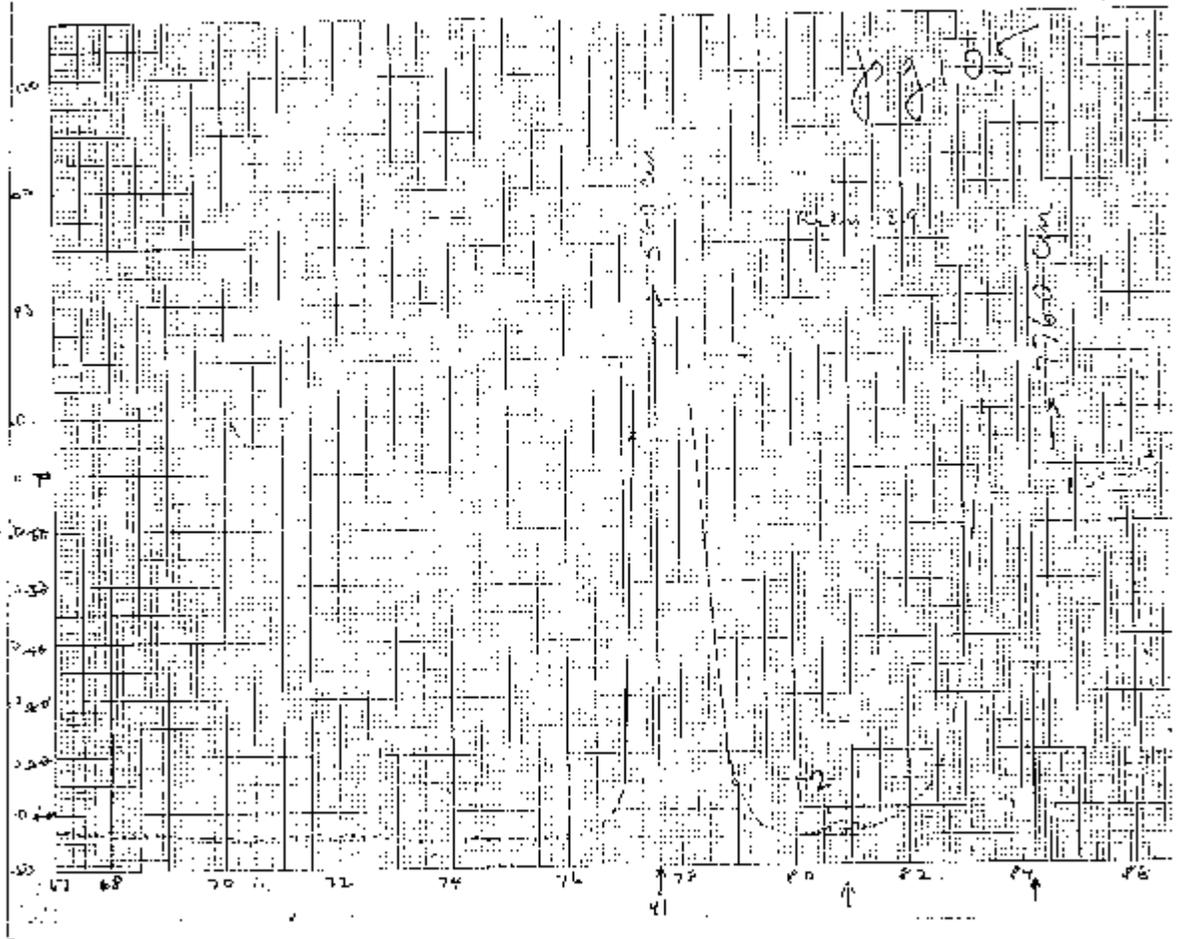


figure 6. Potassium spectrum showing K^{40} (early 1935).
See Ref. 2.

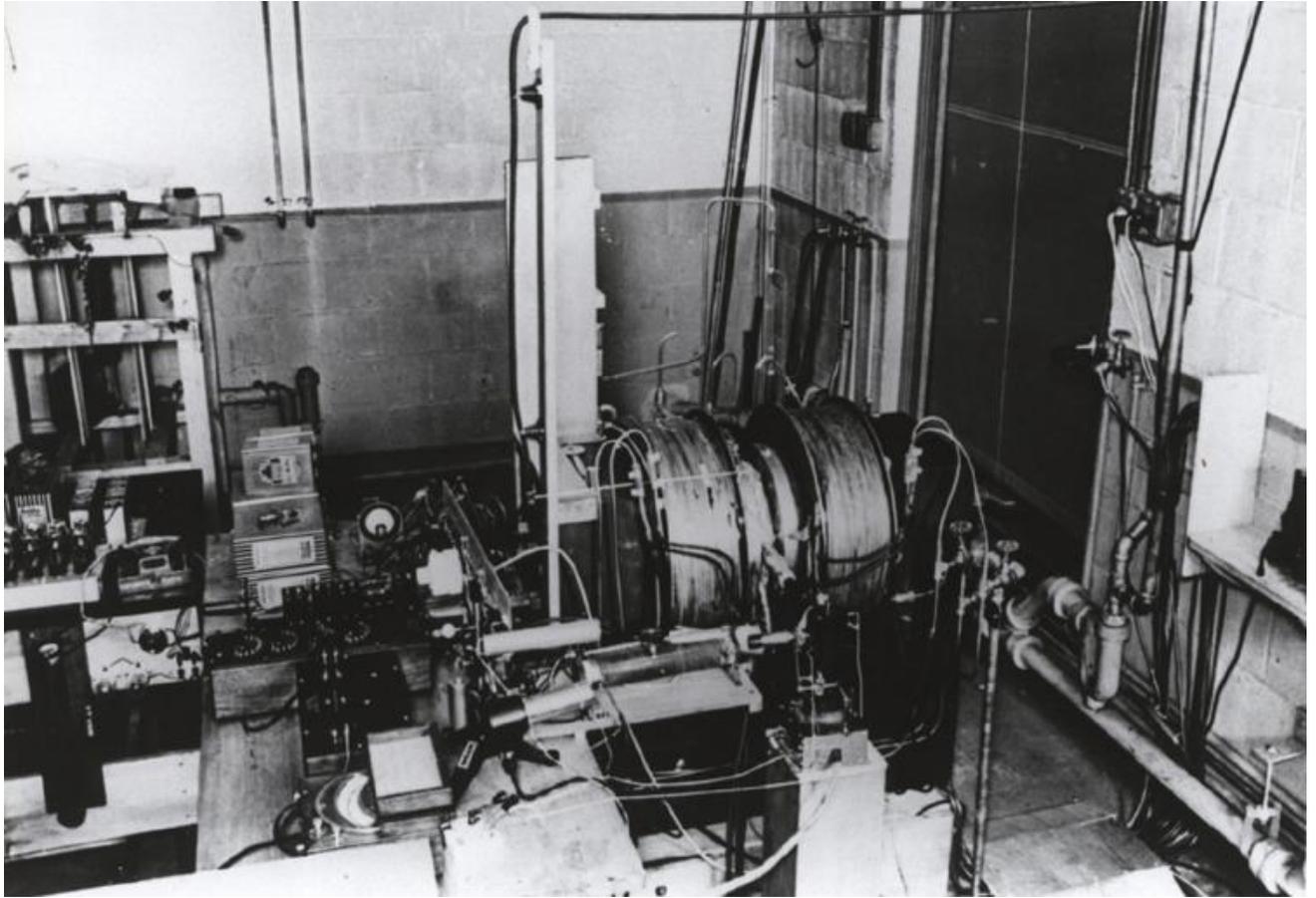


Figure 7. Photo of Harvard 180° mass spectrometer used for isotope analyses of U, Pb, Hg, Xe, Kr, etc.

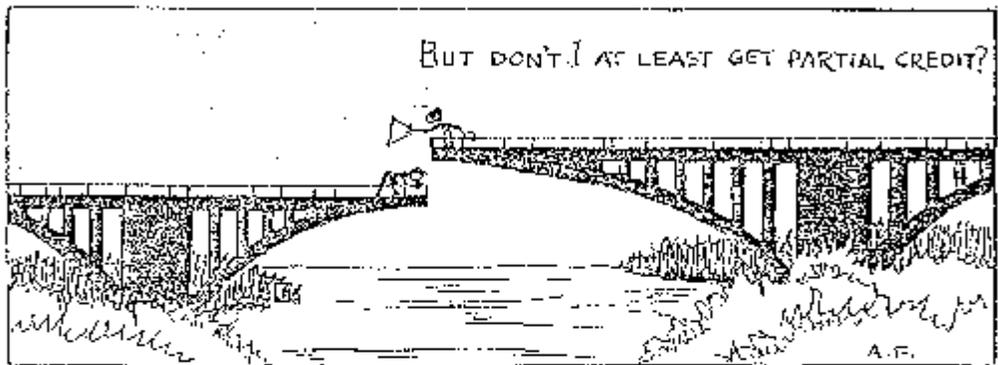


figure 3. Cartoon about 'partial credit.'

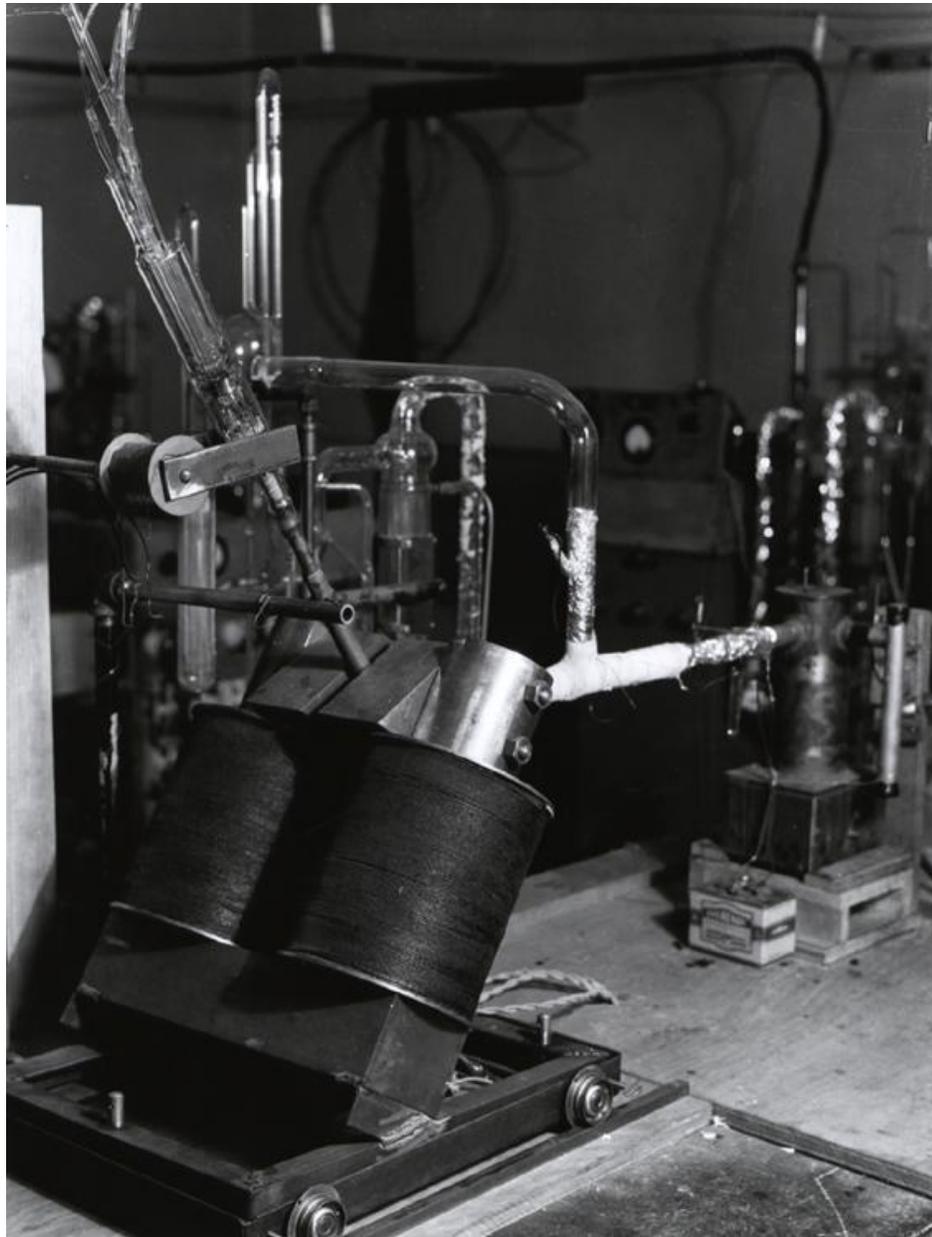


Figure 9. Photo of original 60° magnetic sector mass spectrometer.

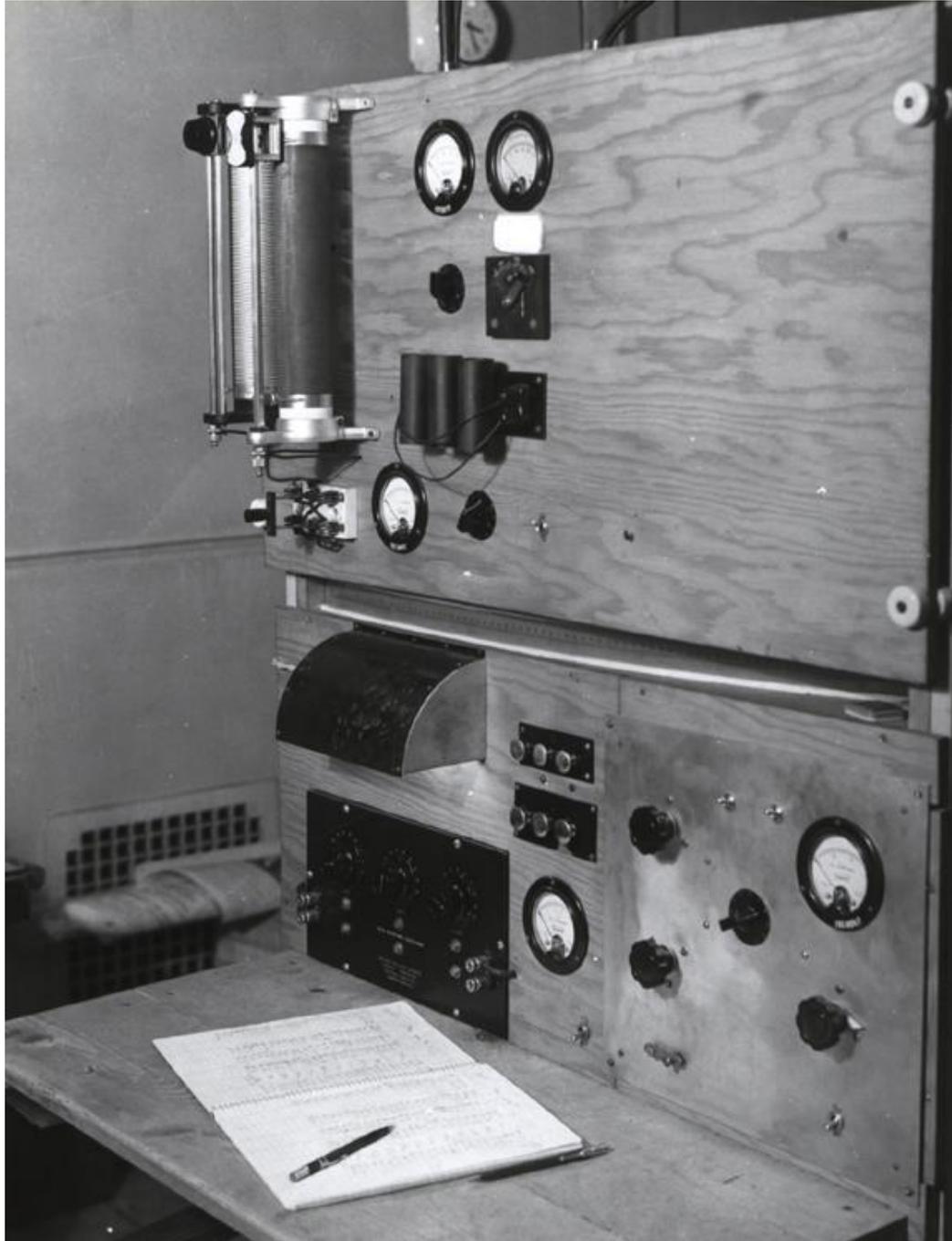


Figure 10. Control panel for original 60° mass spectrometer..



Figure 11. Ion source for early 60° mass spectrometer (circa 1941).



Figure 12. Nier at control panel of Minnesota 180° and 60° mass spectrometers, 1938-1941.



Figure 13. One of original seven uranium analysis mass spectrometers constructed at University of Minnesota, 1942-1943.

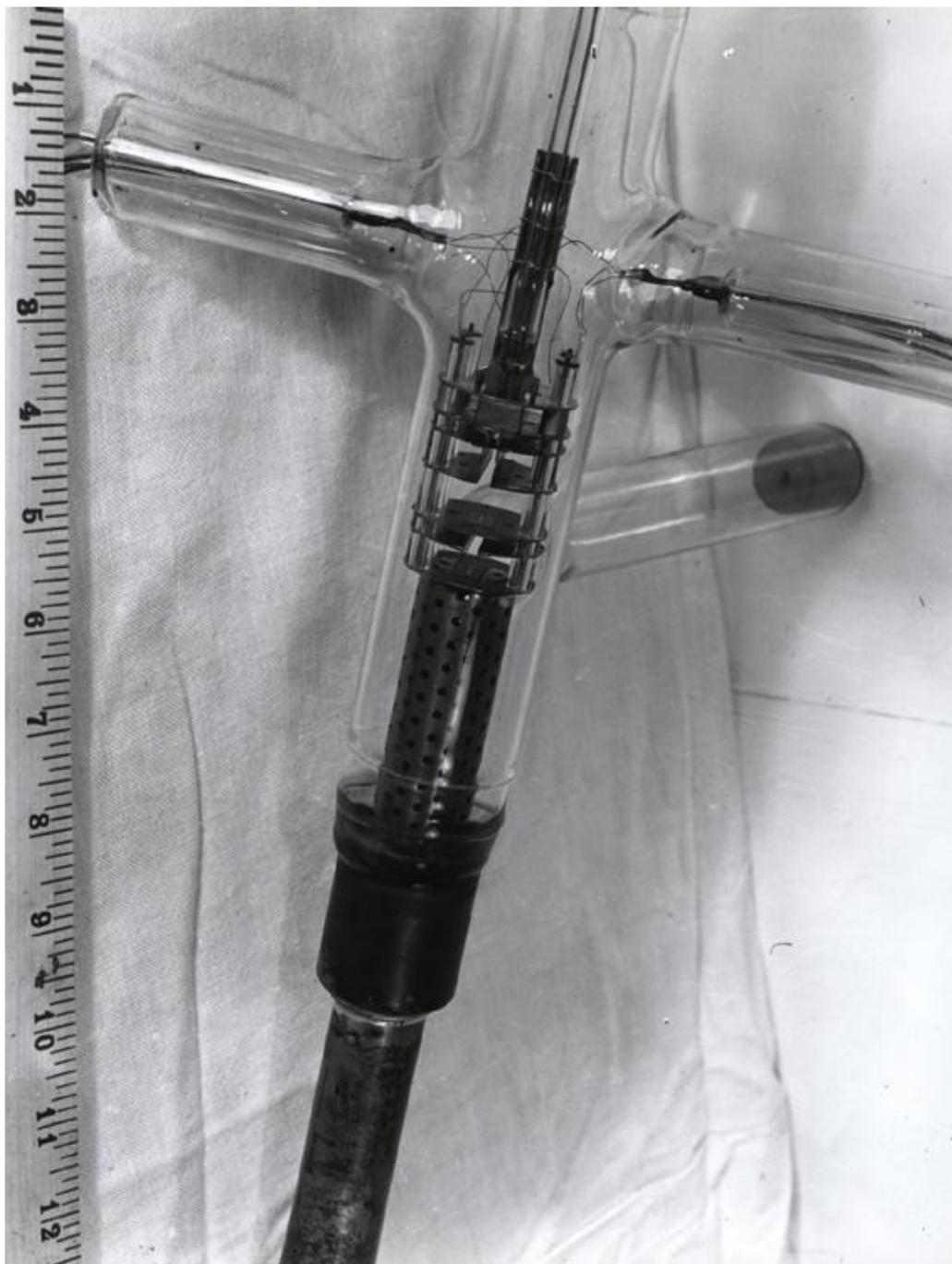


Figure 14. Source of uranium isotope mass spectrometer tube. See Figure 13.



Figure 15. Spare hydrogen isotope mass spectrometer tube with internal permanent magnets.
Constructed 1942-1943.



Figure 16. Another view of Figure 15.

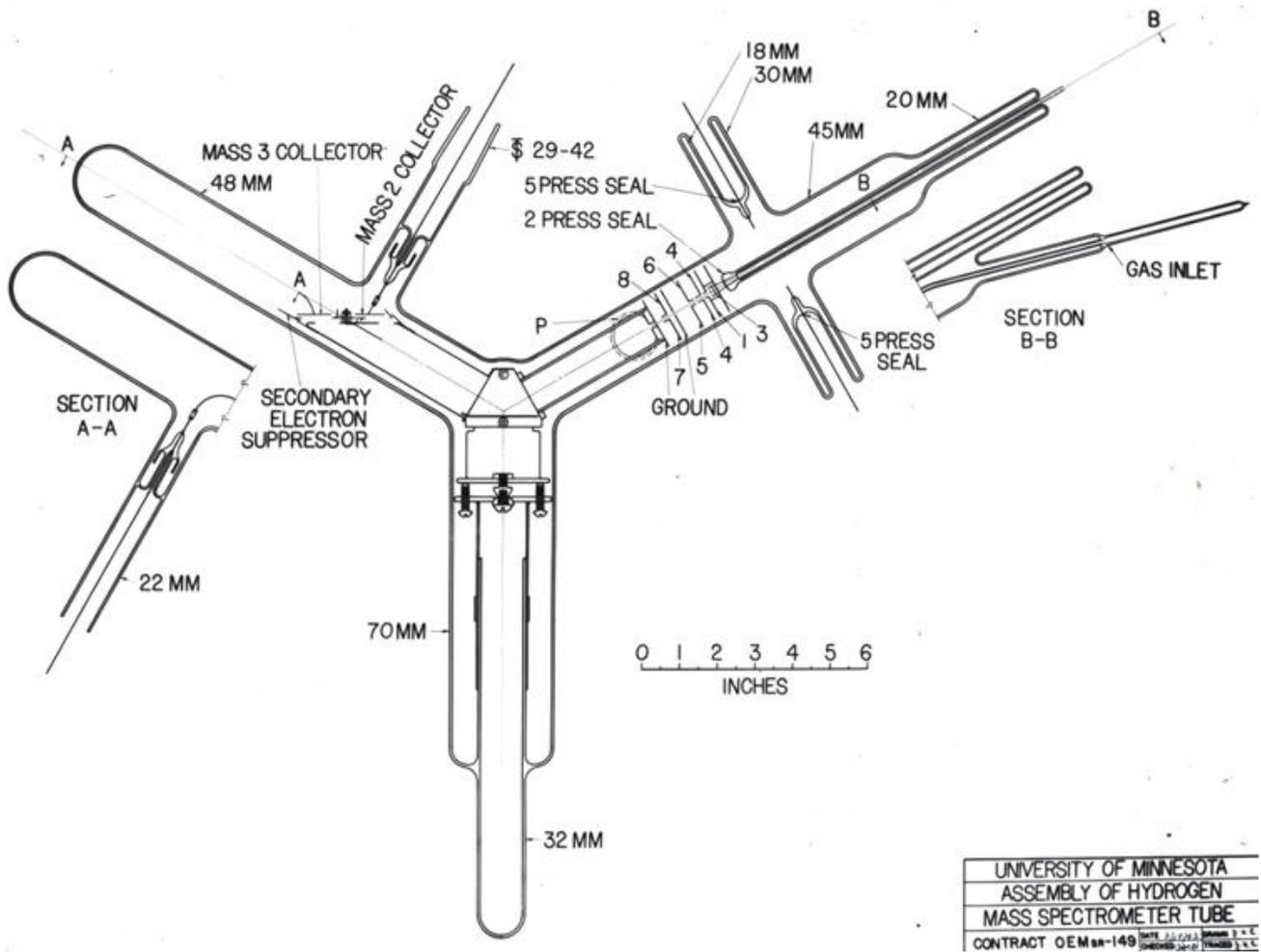


Figure 17. Schematic of dual-collector hydrogen mass spectrometer tube.

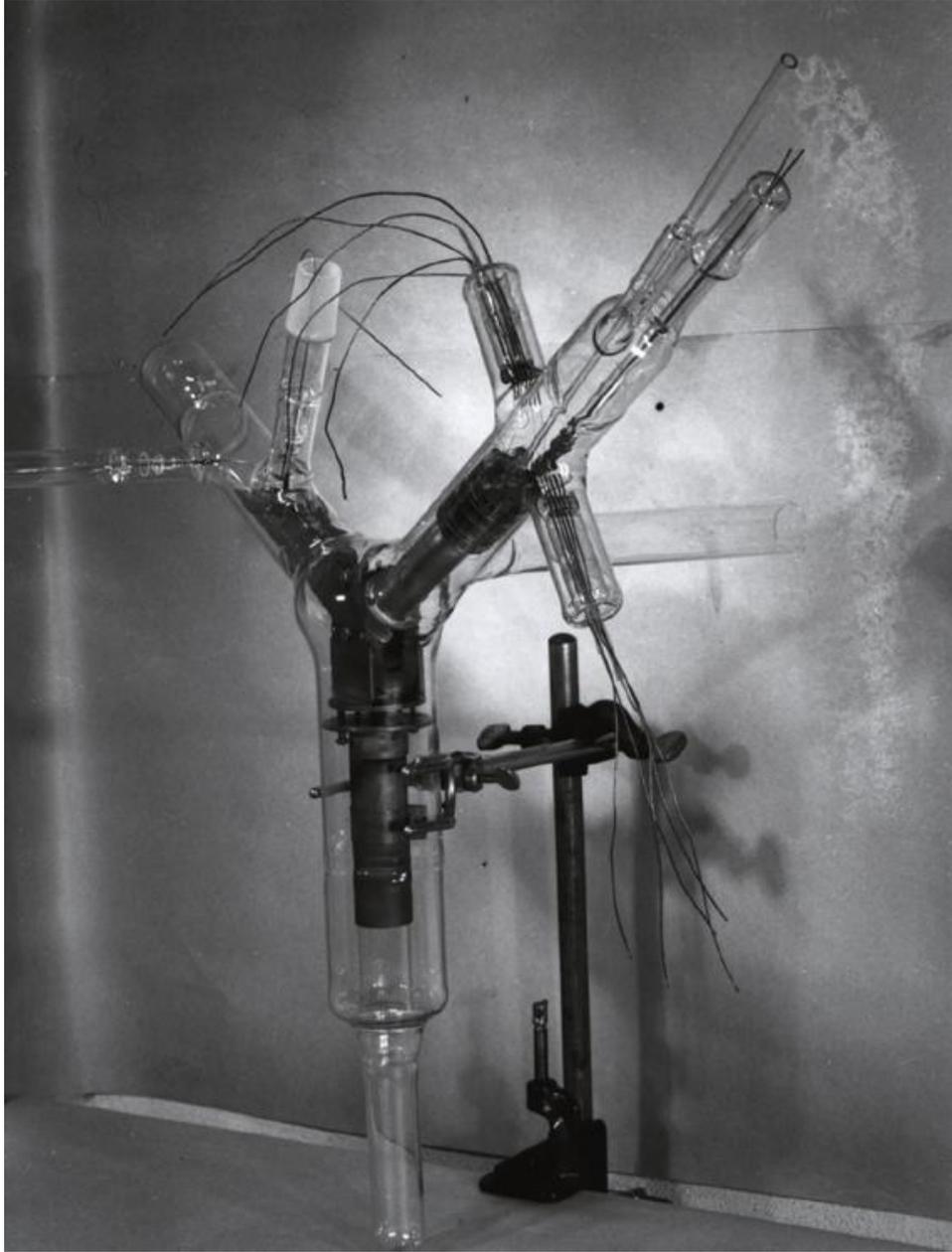


Figure 18. One of original hydrogen analysis mass spectrometer tubes (circa 1942).

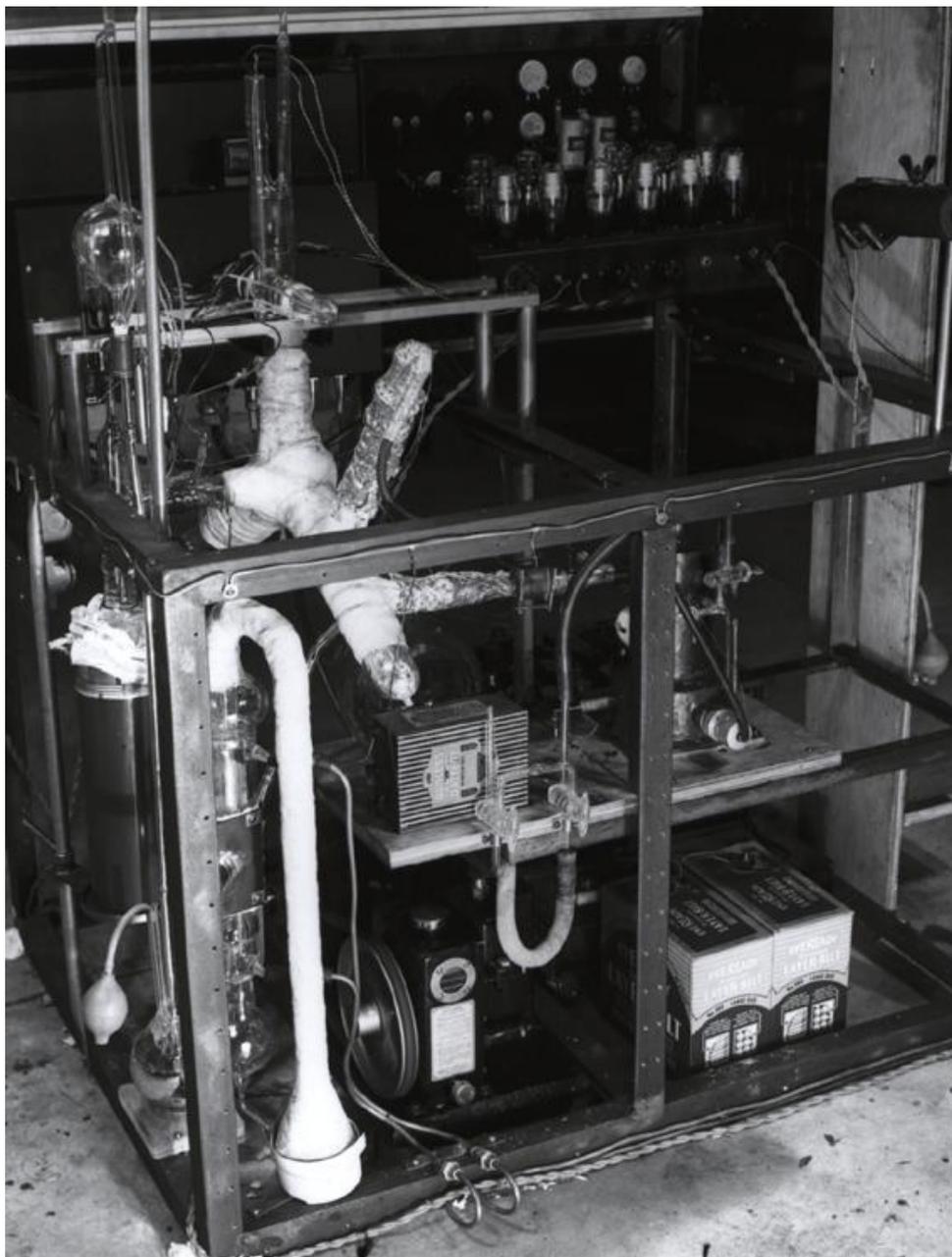


Figure 19. Original hydrogen mass spectrometer.

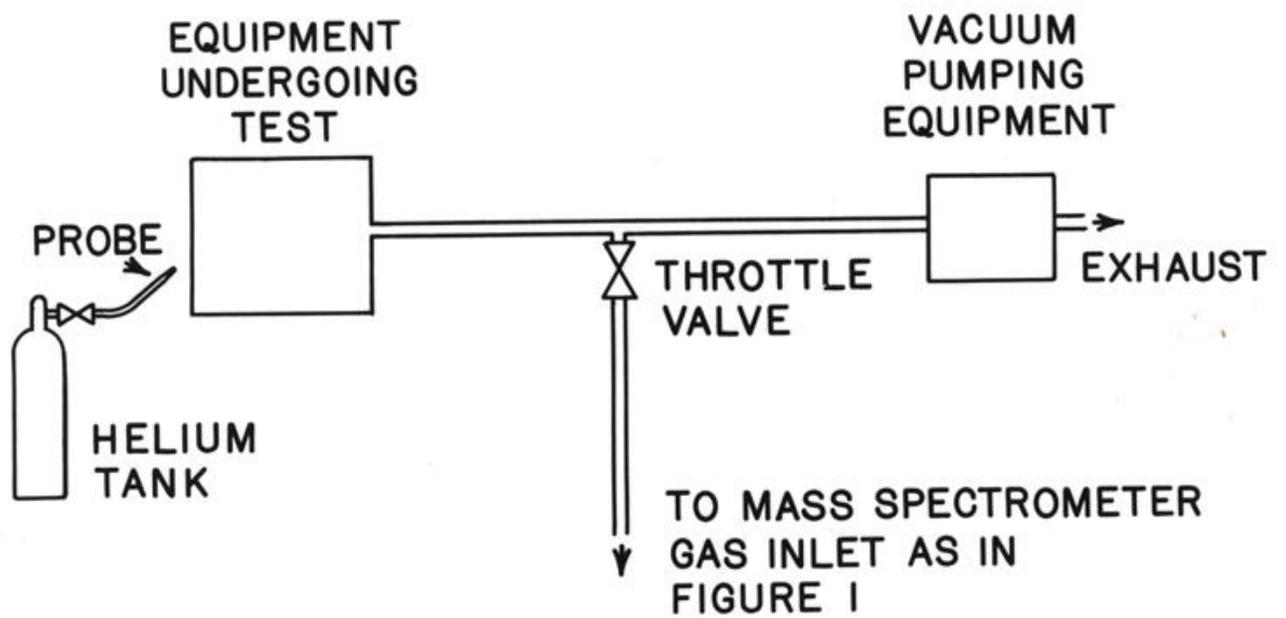


Figure 20. Schematic showing helium leak detector.

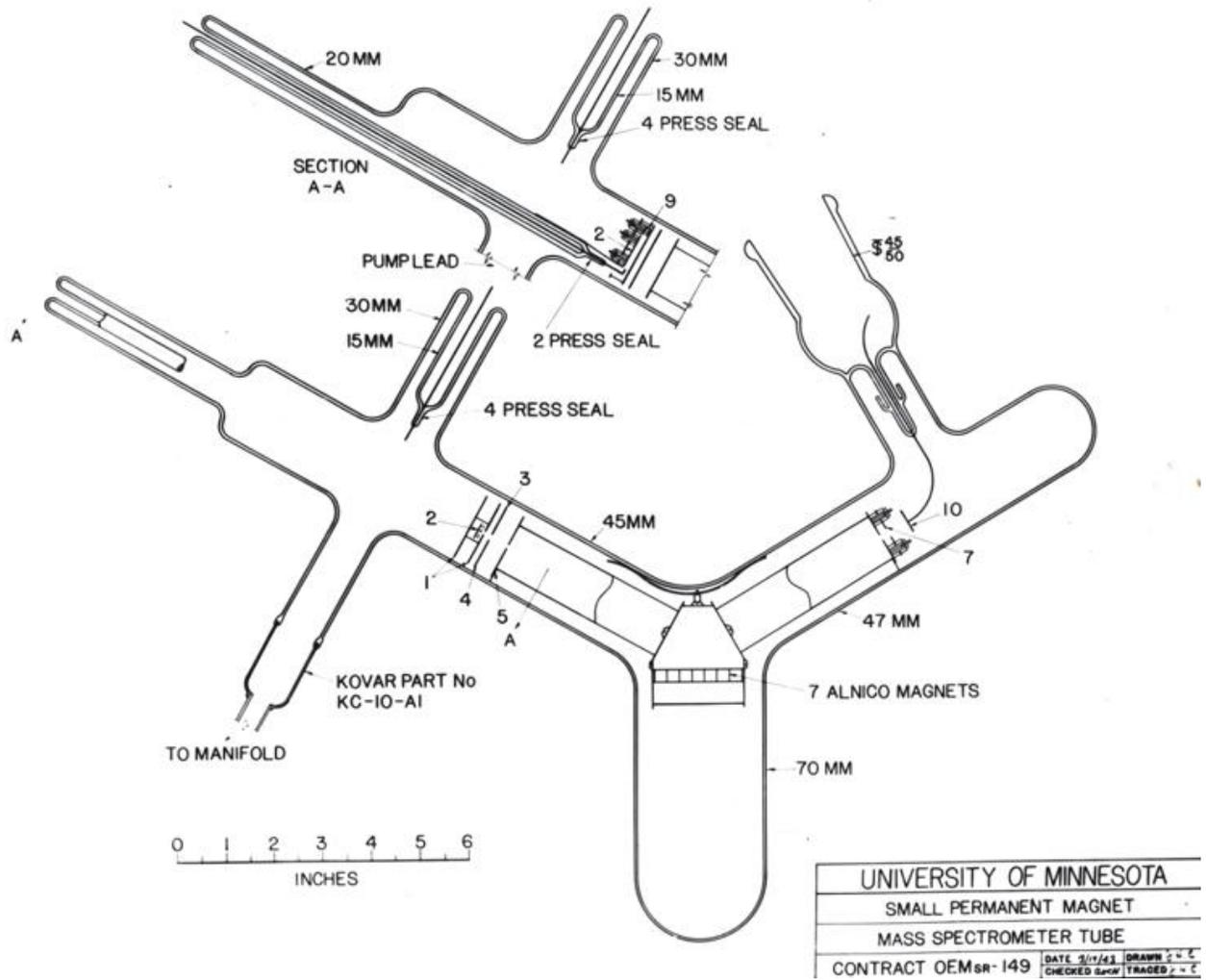


Figure 21. Schematic drawing of prototype helium leak detector mass spectrometer tube.

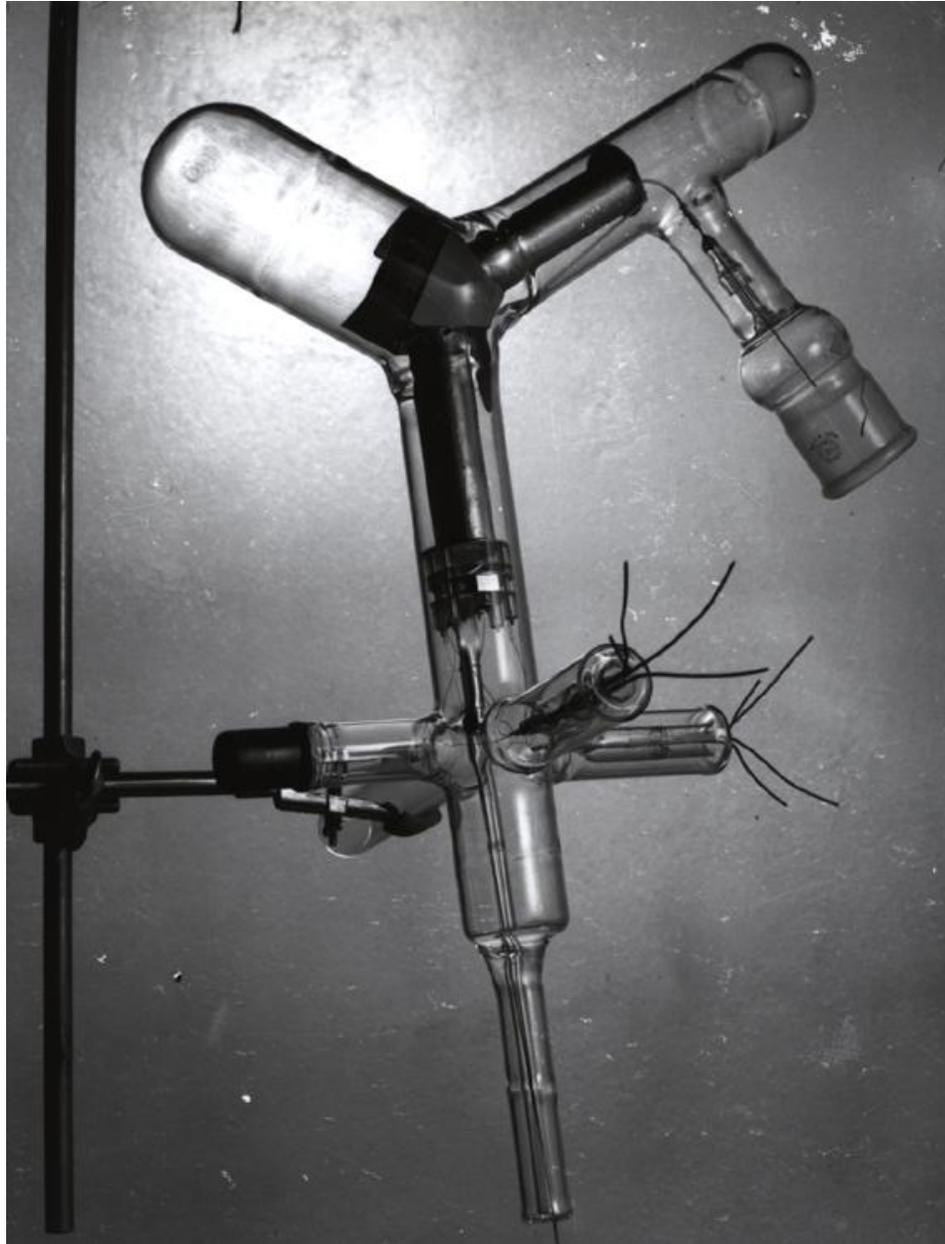


Figure 22. Original helium leak detector mass spectrometer tube.

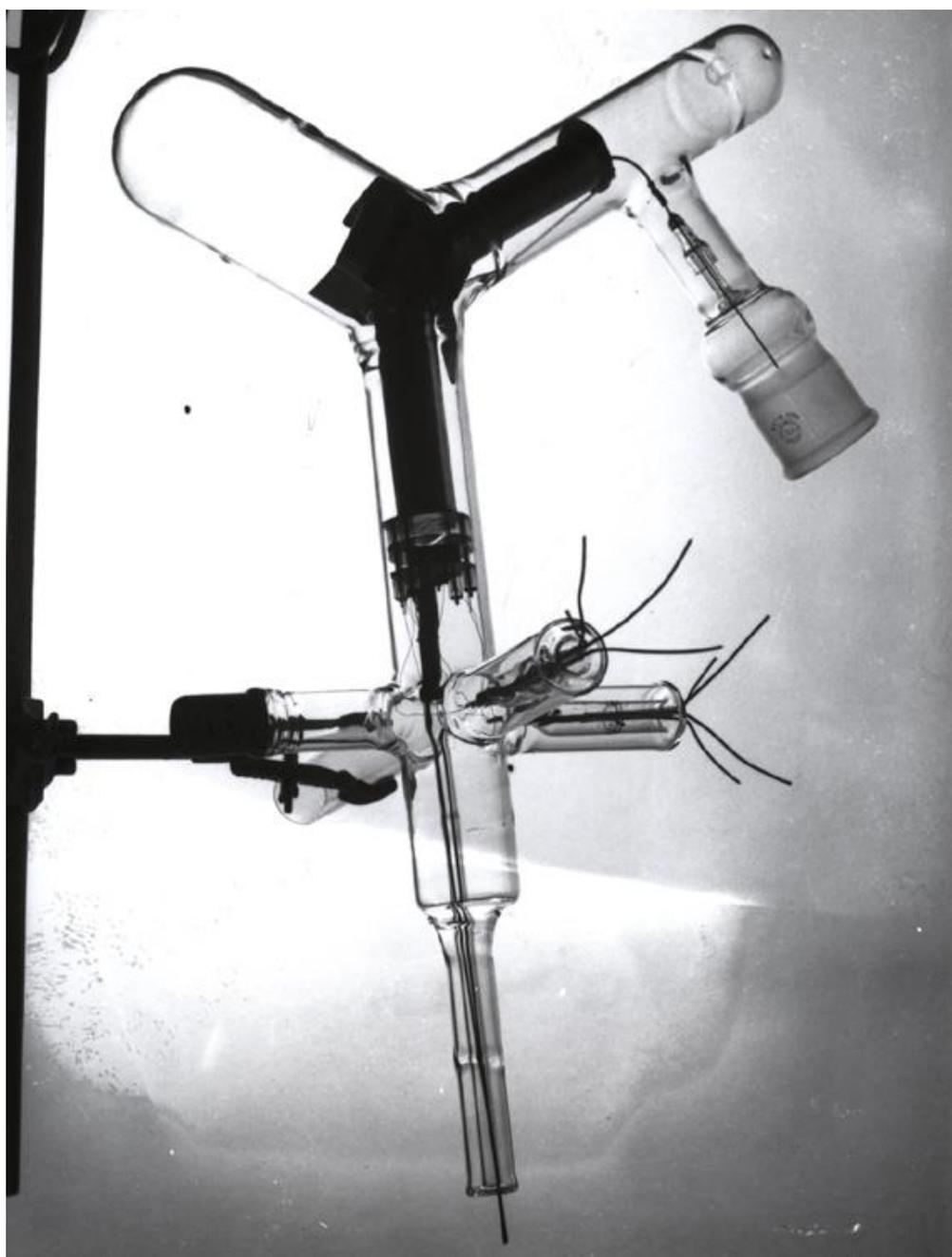


Figure 23. Another view of Figure 22.

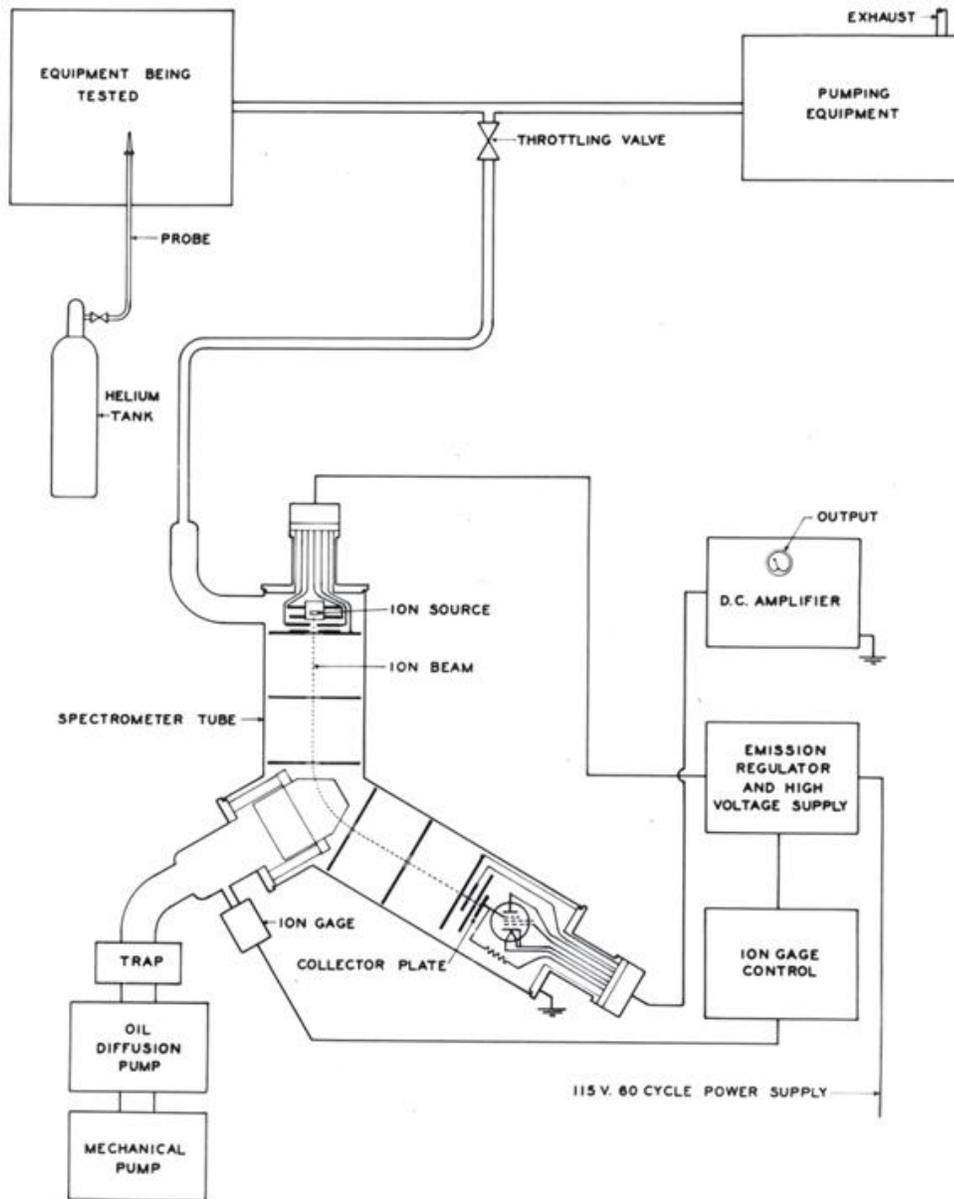


Figure 24. Helium leak detector schematic.

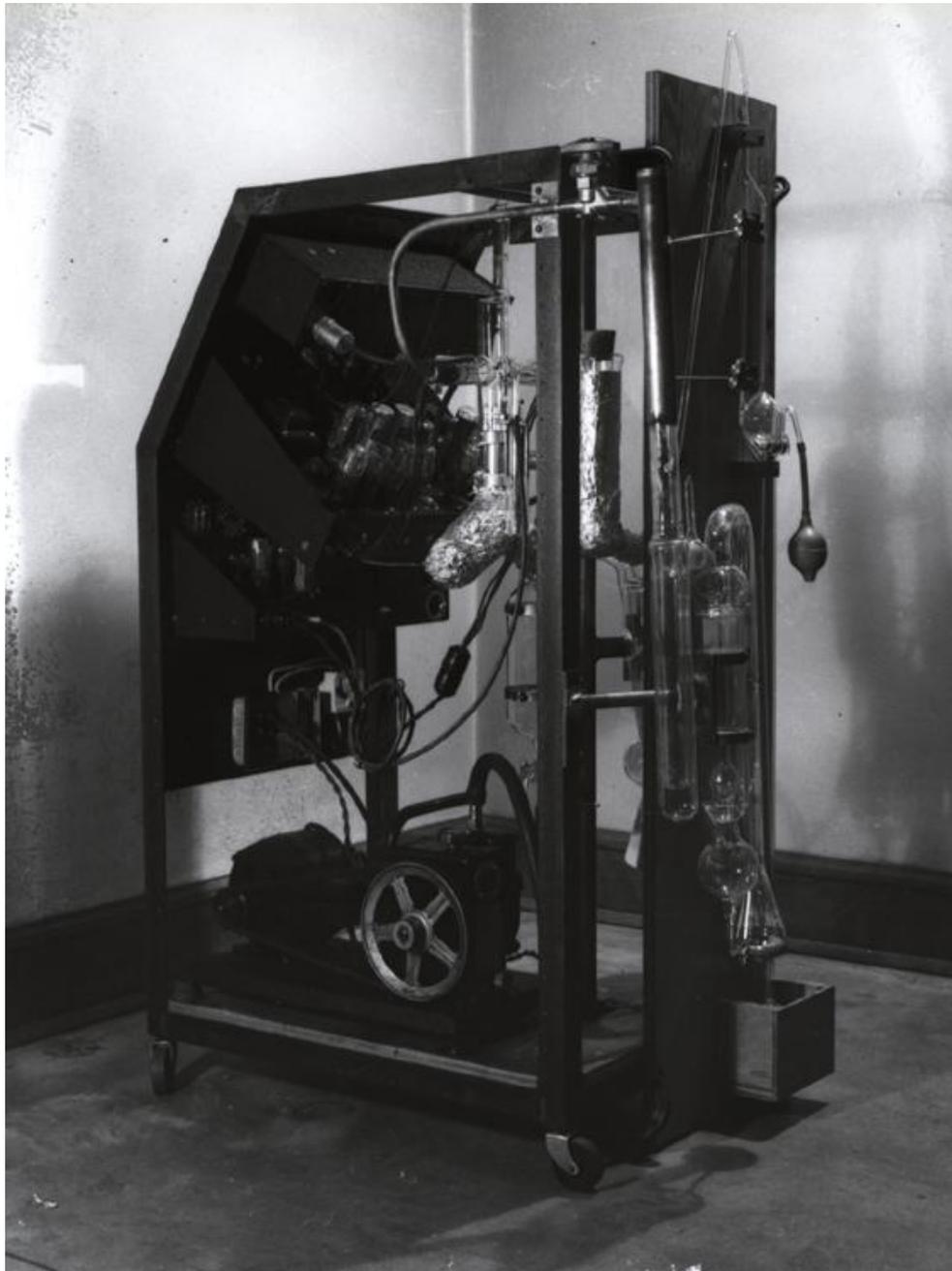


Figure 25. Original portable leak detector.

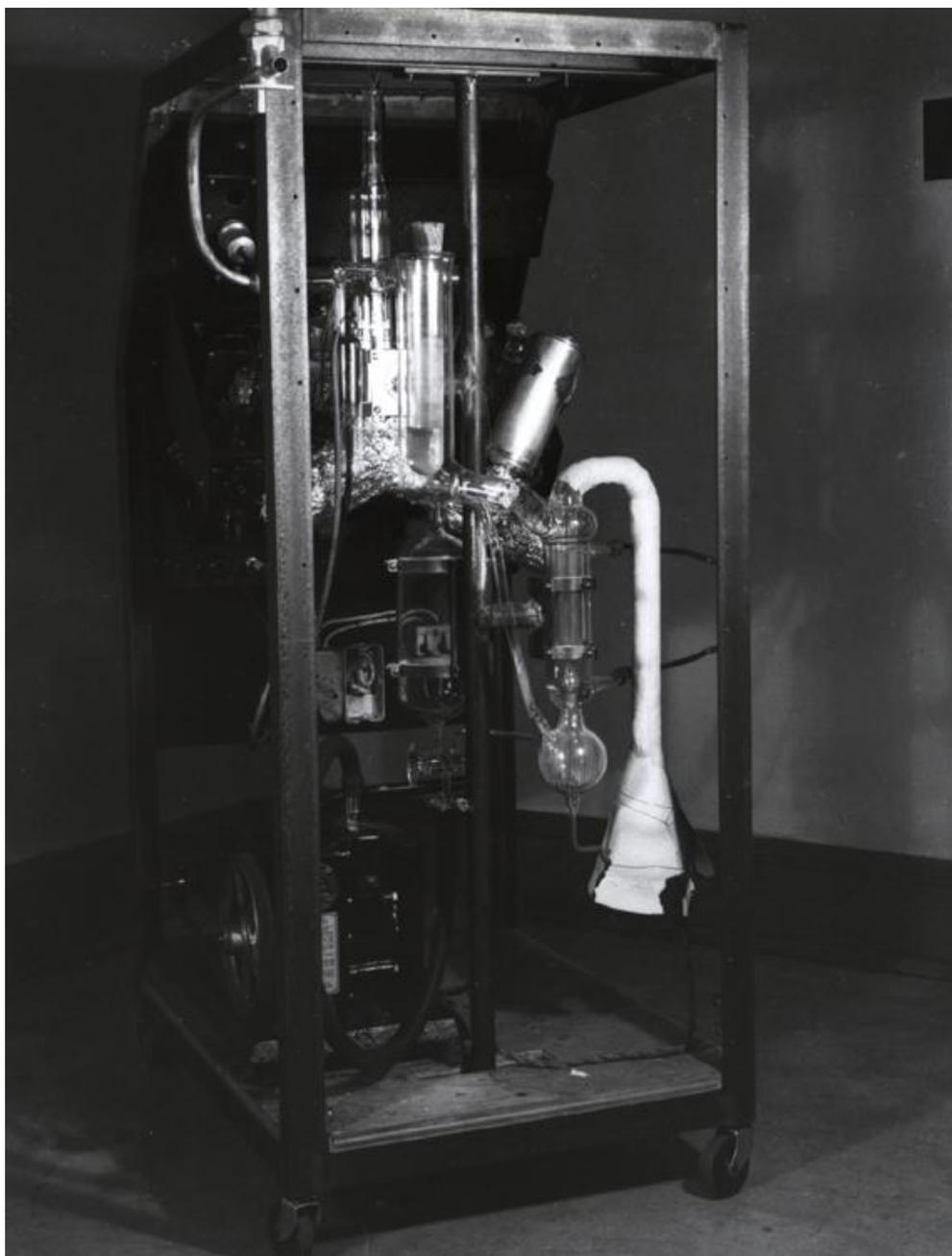


Figure 26. Another view of Figure 25.

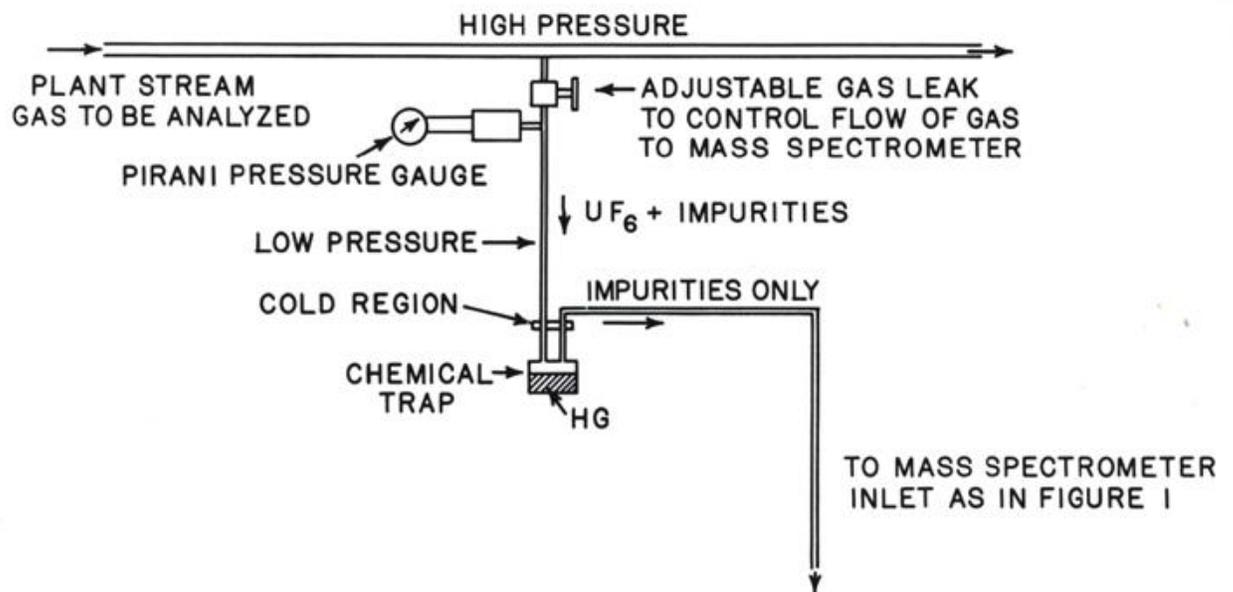


Figure 27. Process stream sampling of uranium diffusion plant.

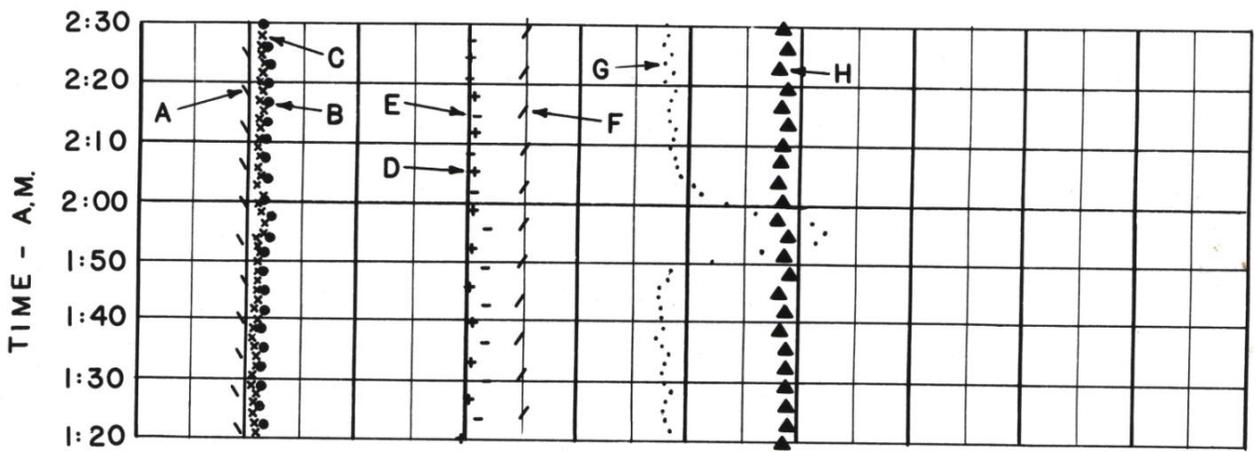


Figure 28. Strip chart record from Line Recorder.

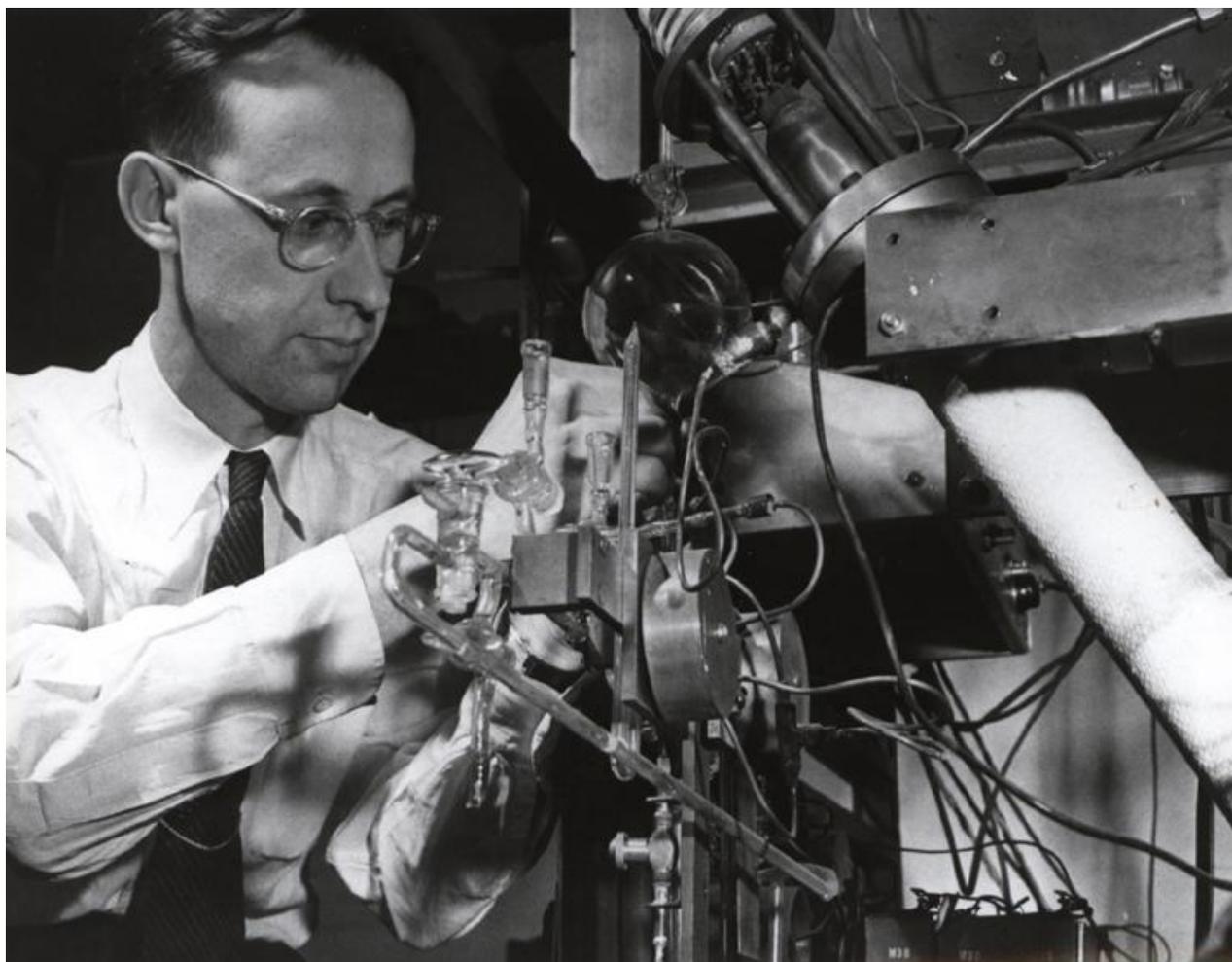


Figure 29. L. T. Aldrich as graduate student at the University of Minnesota.

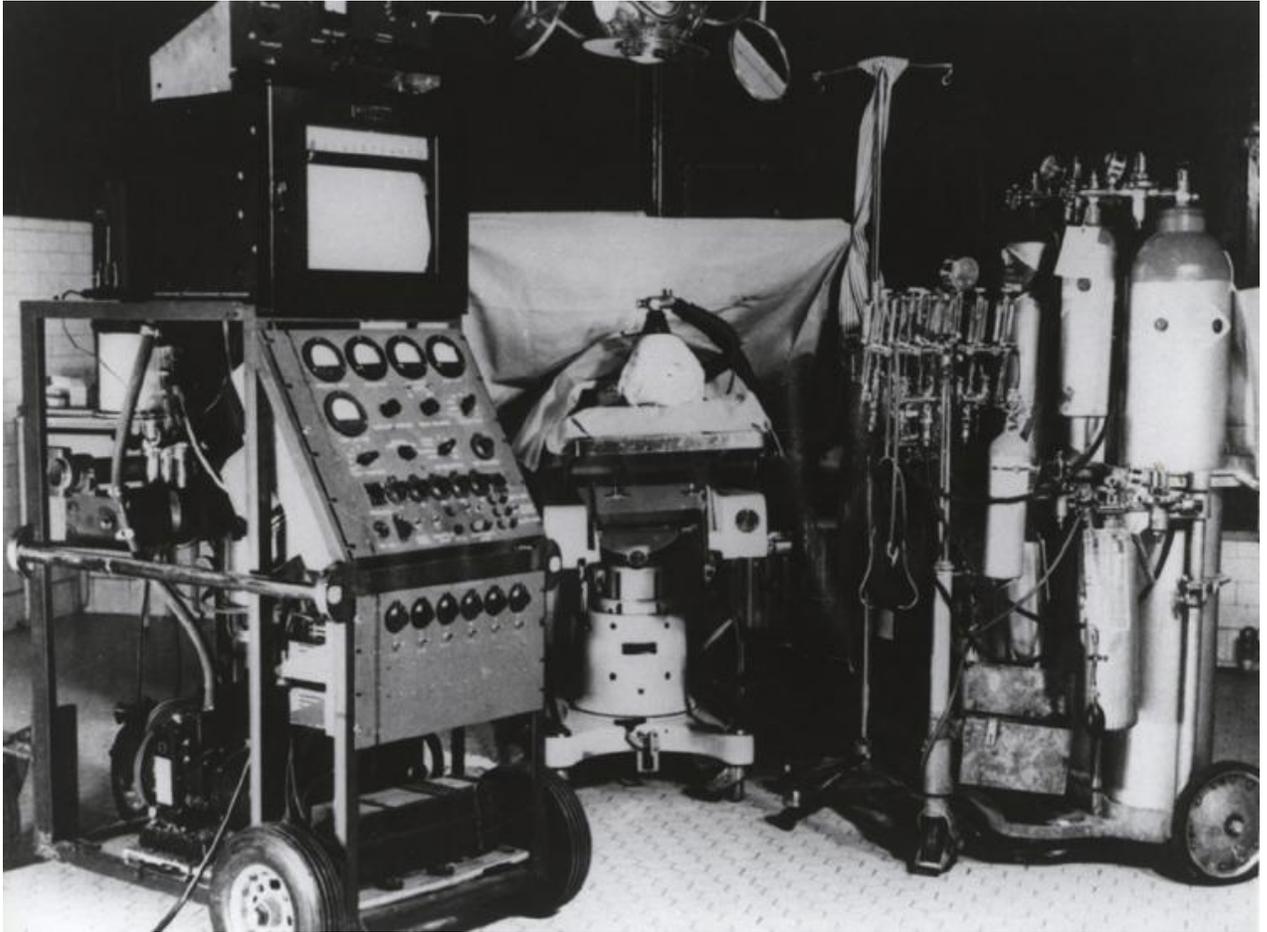


Figure 30. Portable mass spectrometer for use in hospital operating room to monitor respiration.



Figure 31. 180° mass spectrometer tube.



Figure 32. Group picture taken at 1951 Mass Spectrometry Conference at National Bureau of Standards. Circular 522, U.S. Government Printing Office, 1953.

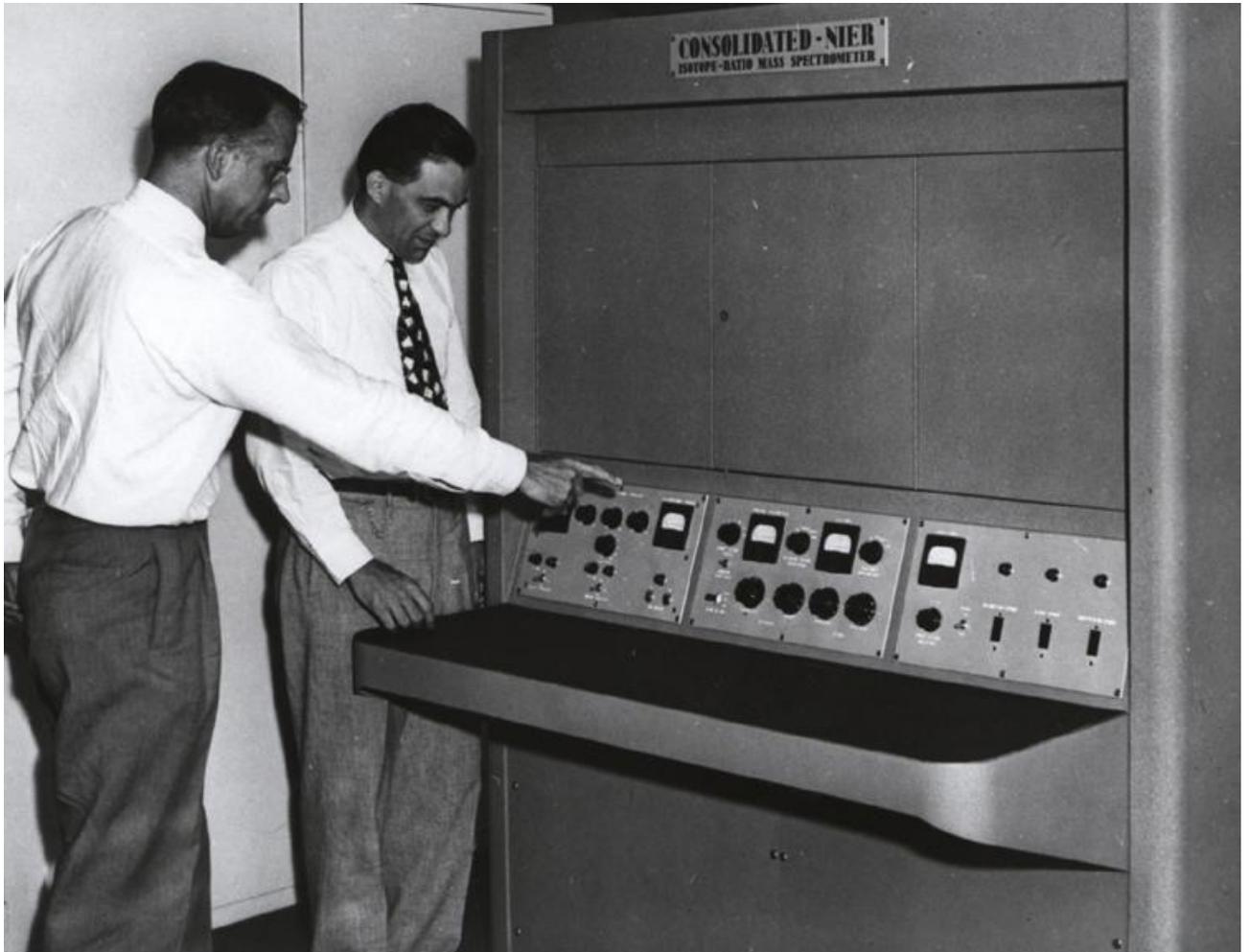


Figure 33. Nier and Harold Washburn, VP Consolidated Electroynamics, examining control panel of early Consolidated-Nier mass spectrometer.

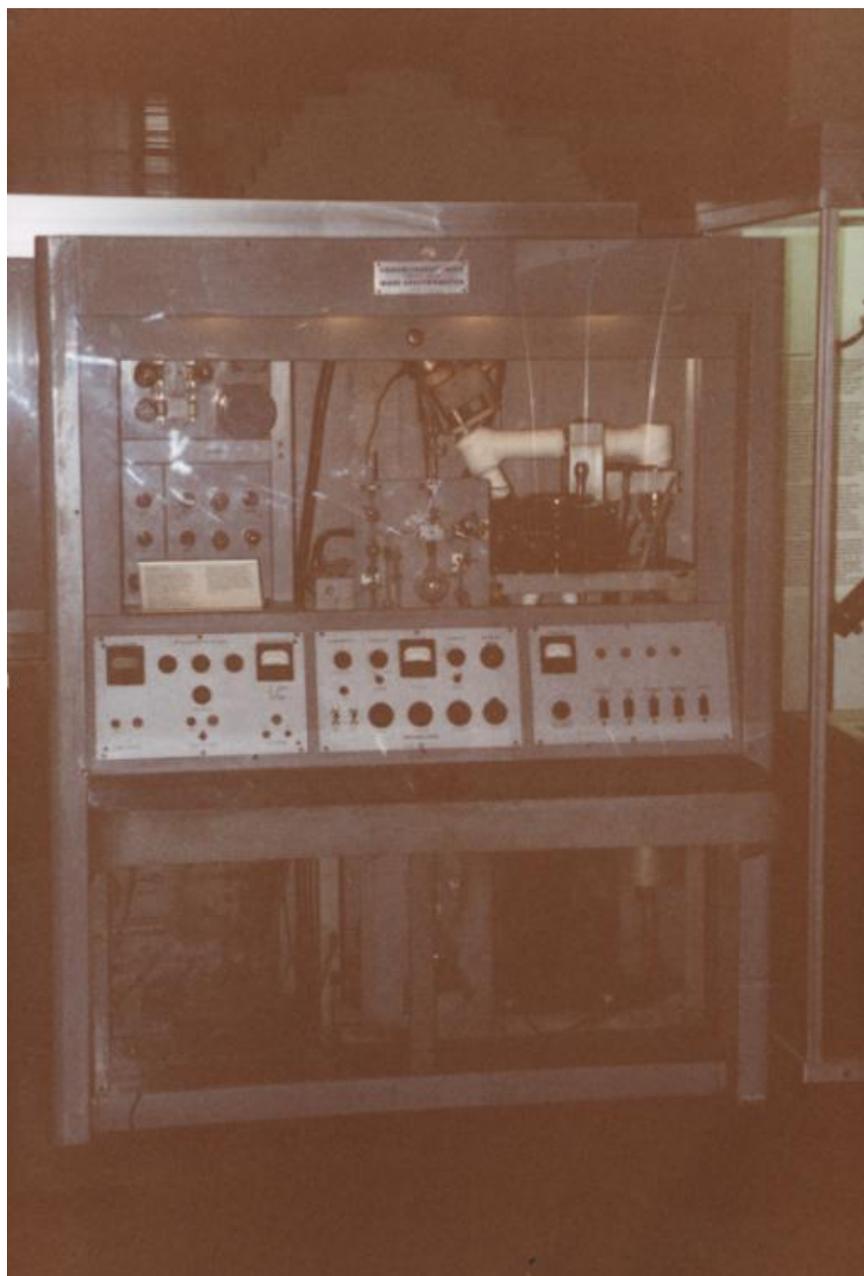


Figure 34. Consolidated-Nier mass spectrometer at British Museum in South Kensington, U.K.

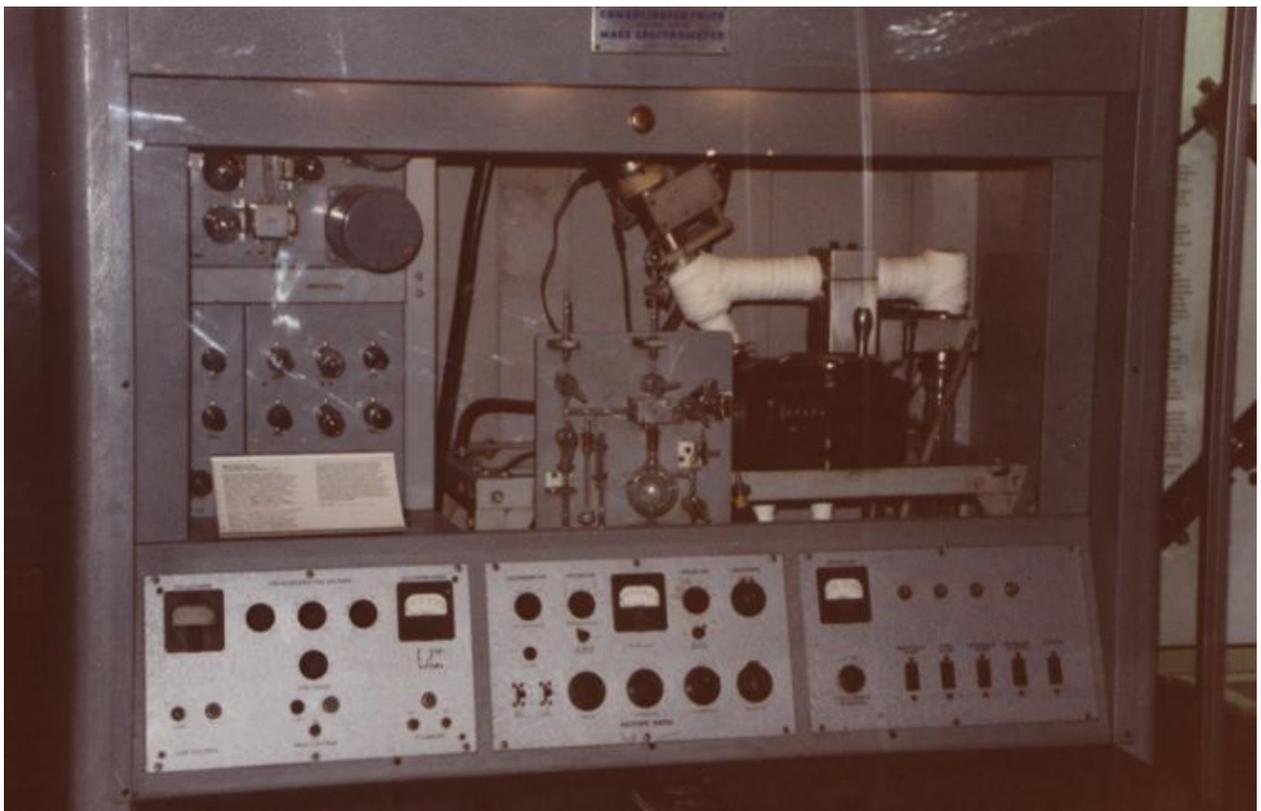


Figure 35. Another view of instrument in British Museum.

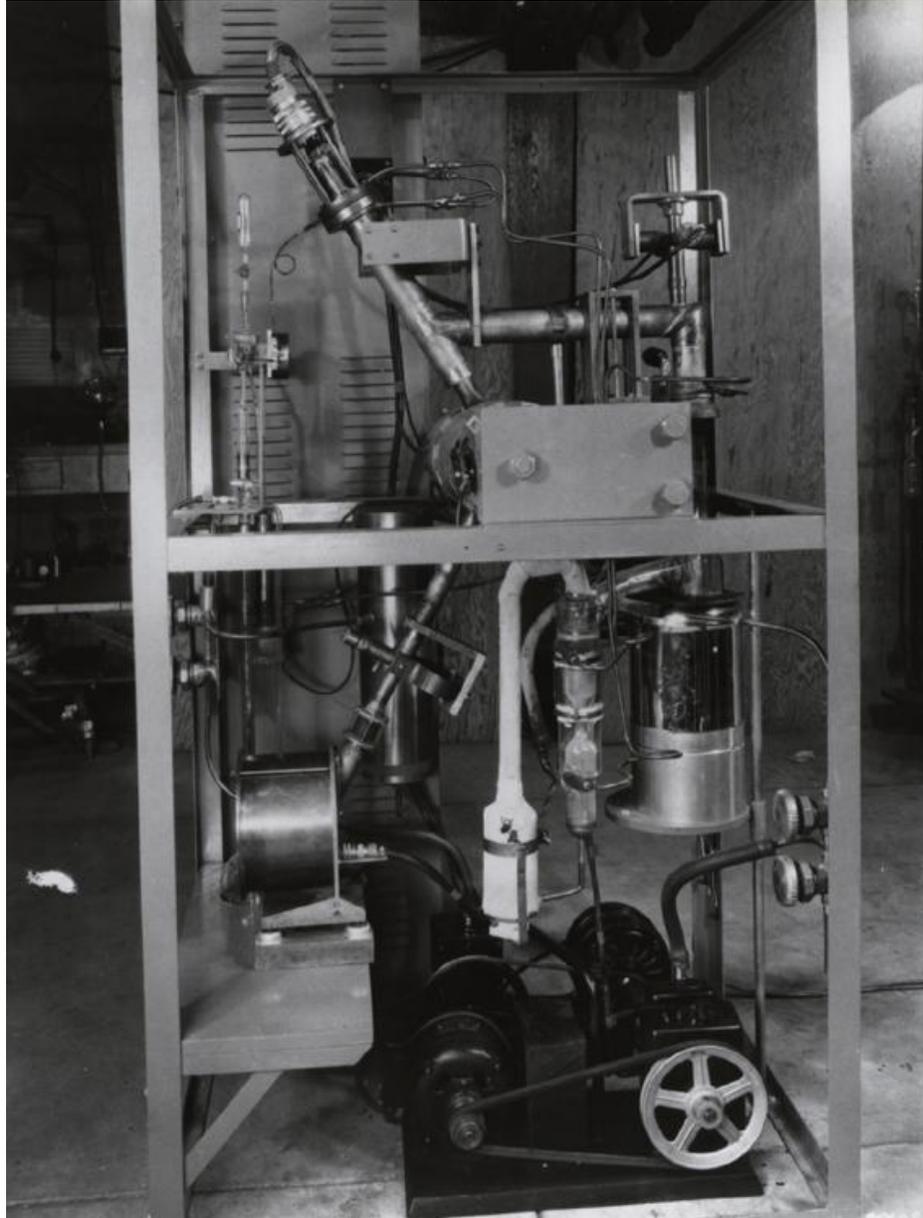


Figure 36. Prototype of Consolidated-Nier mass spectrometer.



Figure 37. A. O. Nier and two individuals from Consolidated Electrodynamics examining 60° mass spectrometer, which was the prototype for the Consolidated-Nier commercial instrument.

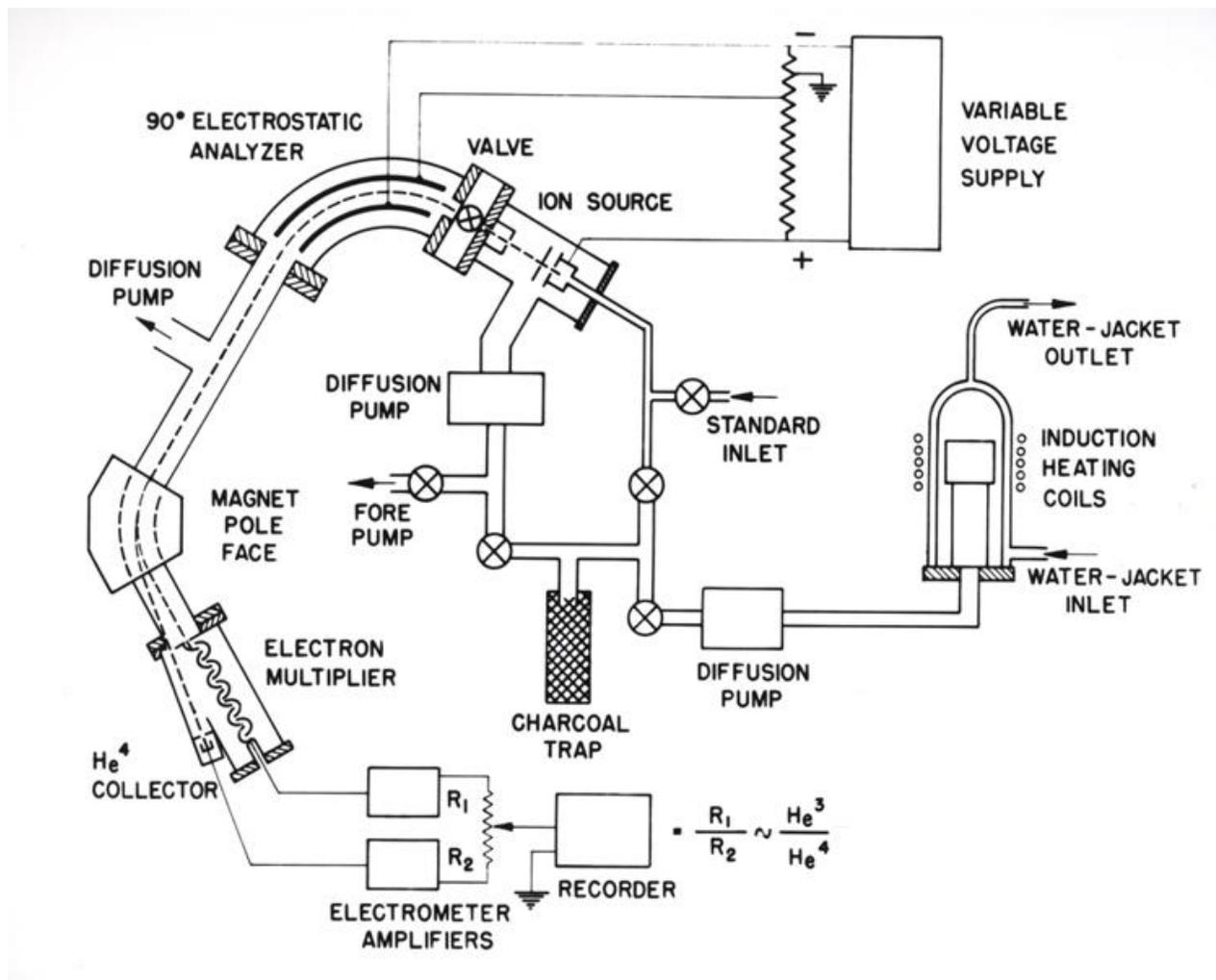


Figure 38. Schematic of a double focusing mass spectrometer system for determining He-3/He-4 abundance ratios.



Figure 39. One of several early 60° mass spectrometer tubes constructed in 1941-1942 period.



Figure 40. Another view of Figure 39.



Figure 41. 180° mass spectrometer constructed at Harvard, 1937-1938.



Figure 42. Another view of Figure 41.



Figure 43. Another view of Figure 41.

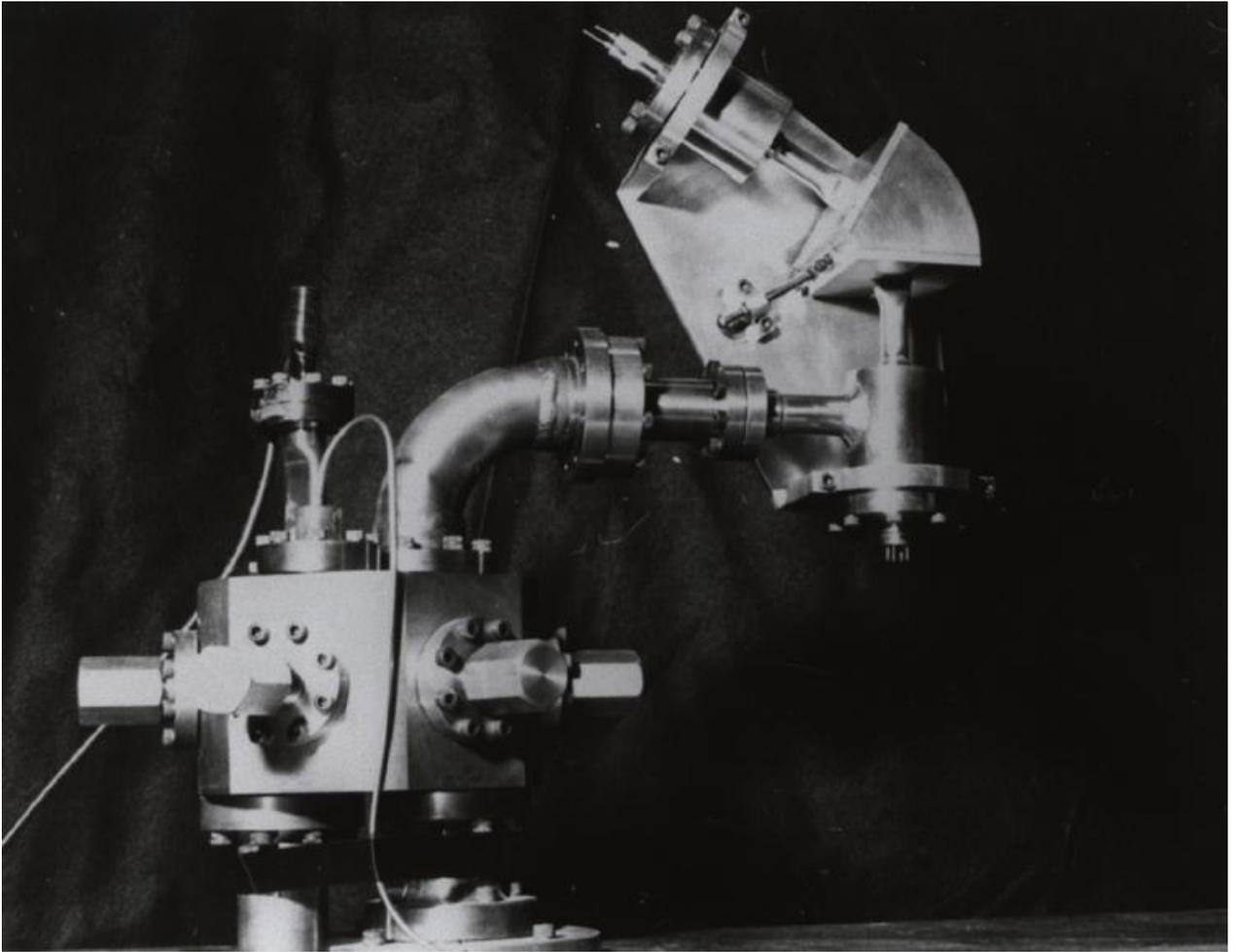


Figure 44. Miniature 60° magnetic mass spectrometer with permanent magnet. (laboratory prototype)



Figure 45. Collector end of early 60° tube. Note Kovar seal joint where glass housing over collector plates joins copper flight tube.



Figure 46. Another view of Figure 45.

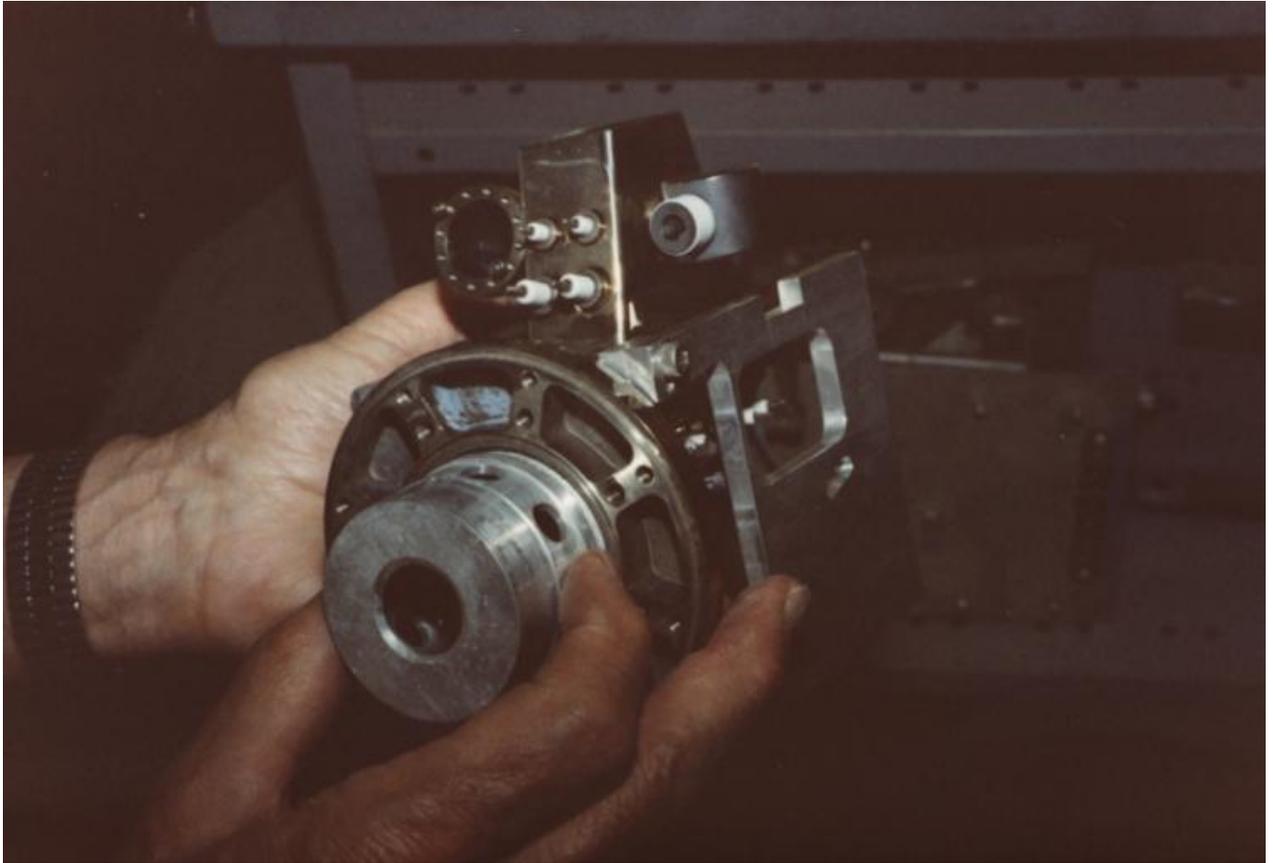


Figure 47. 1" magnetic radius Mattauch-Herzog mass spectrometer flown in rocket to upper atmosphere (circa 1980).



Figure 48. Prototype Mattauch-Herzog laboratory mass spectrometer.



Figure 49. Another view of Figure 48.

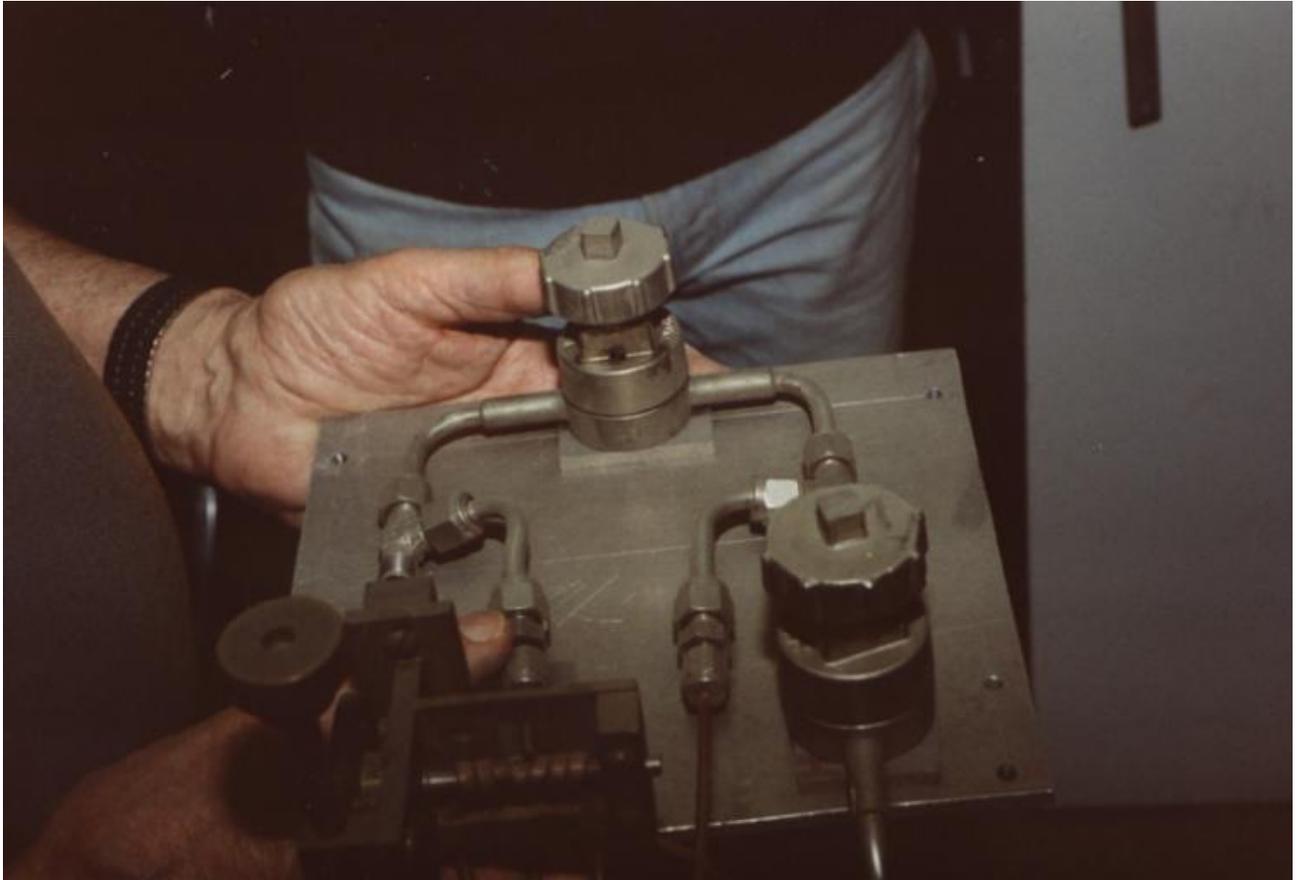


Figure 50. Manifold employing Hoke valves developed under auspices of Manhattan Project for use with UF_6 and other corrosive gases. Also shown is a spiral adjustable leak of type used for admitting UF_6 to isotope mass spectrometers during World War II.

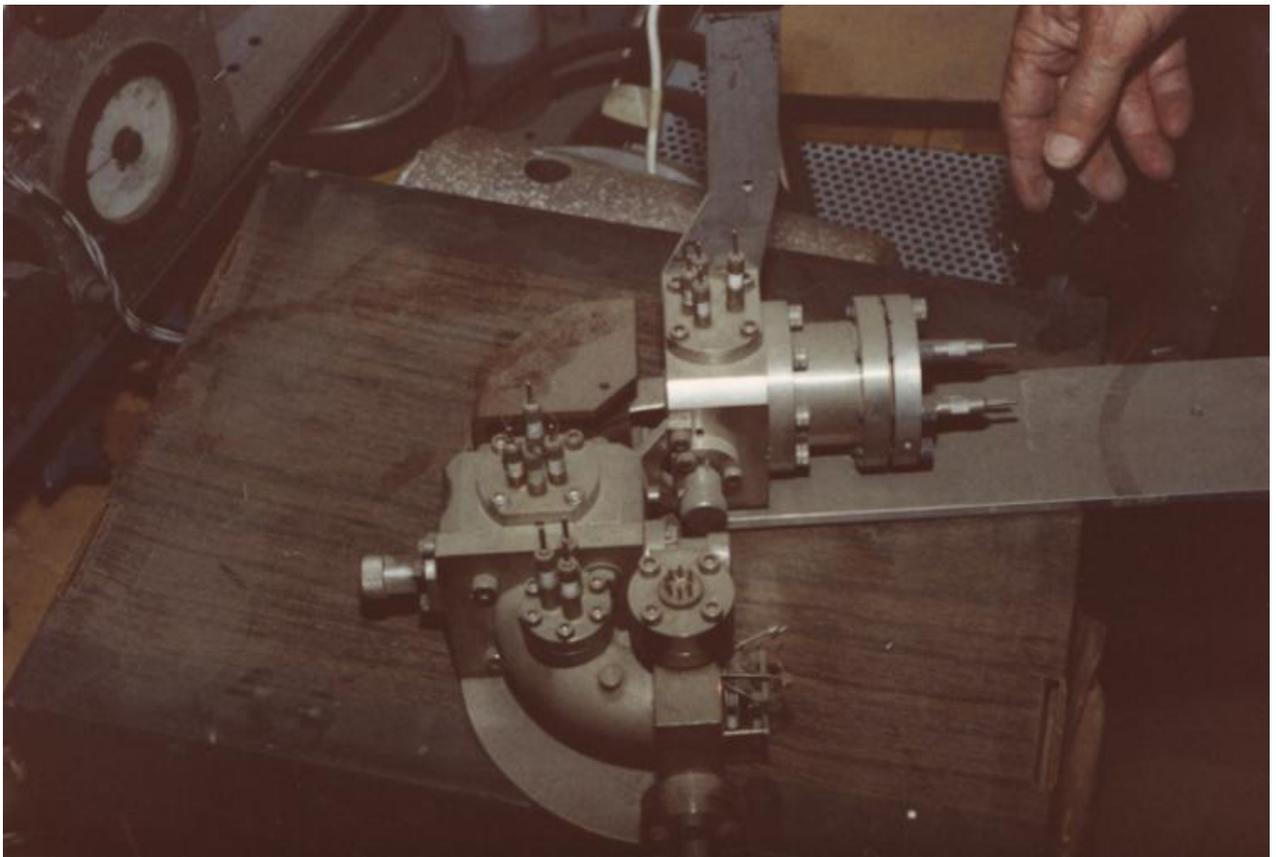


Figure 51. Prototype 90/90 double-focusing mass spectrometer employing Nier-Johnson geometry.

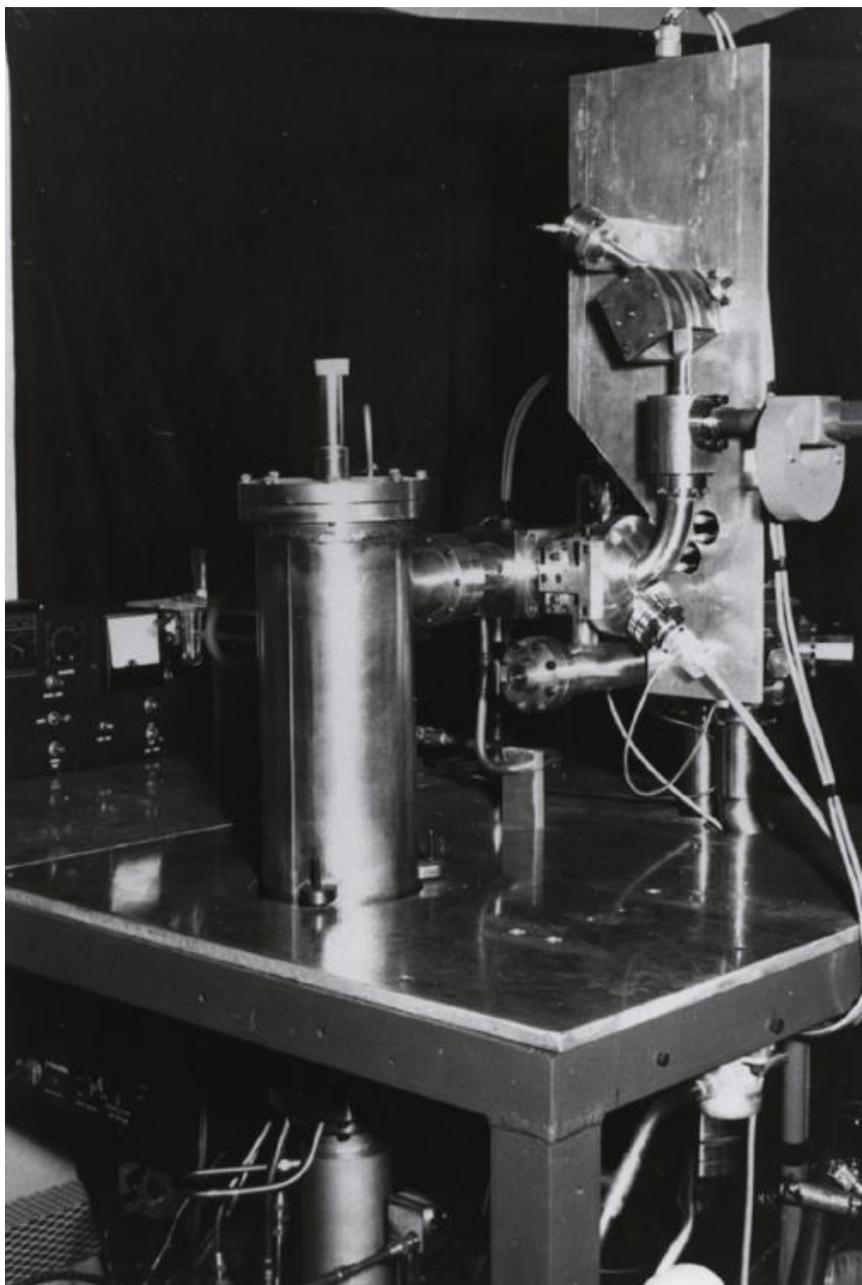


Figure 52. Prototype of miniature double-focusing mass spectrometer with Nier-Johnson geometry and permanent magnet.

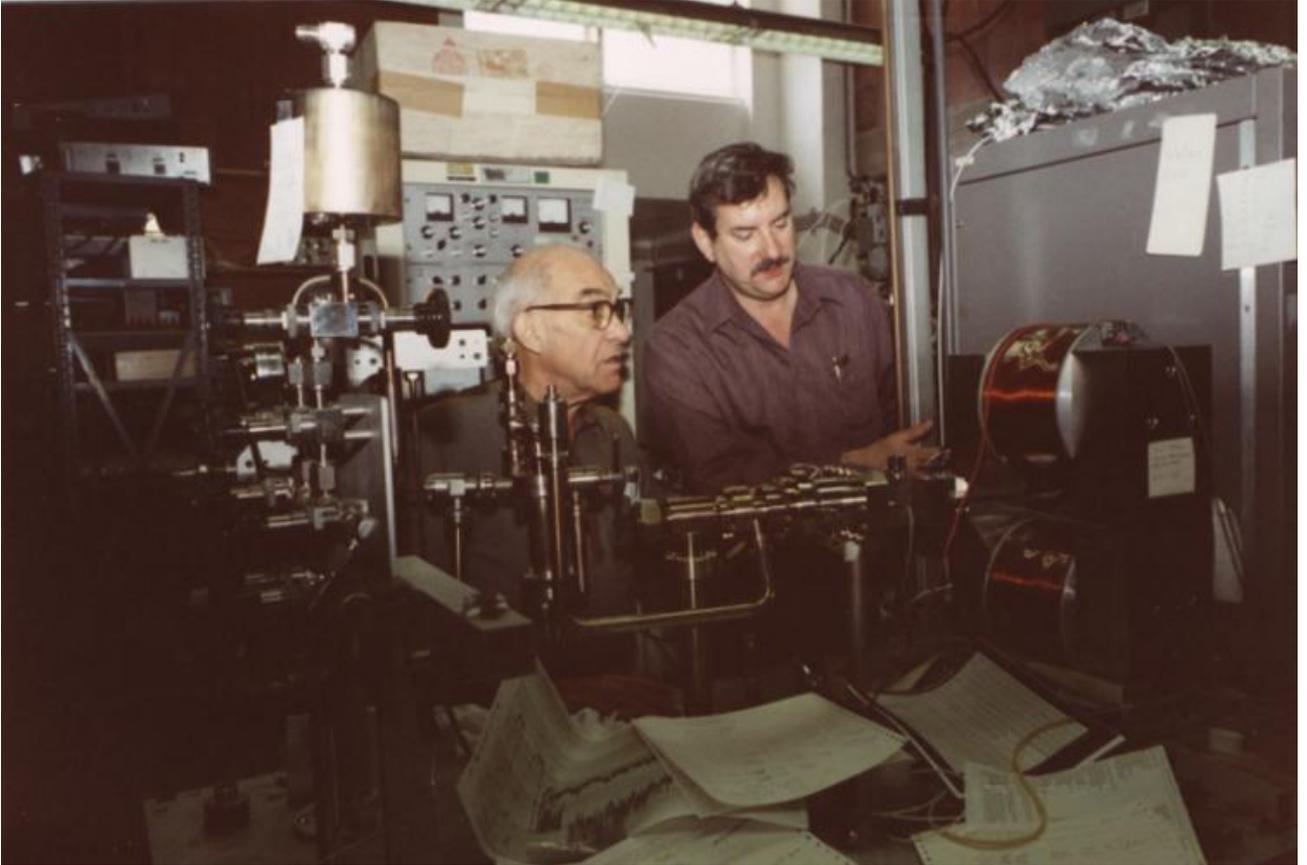


Figure 53. Noble gas analysis instrument.



Figure 54. Another view of Figure 53.

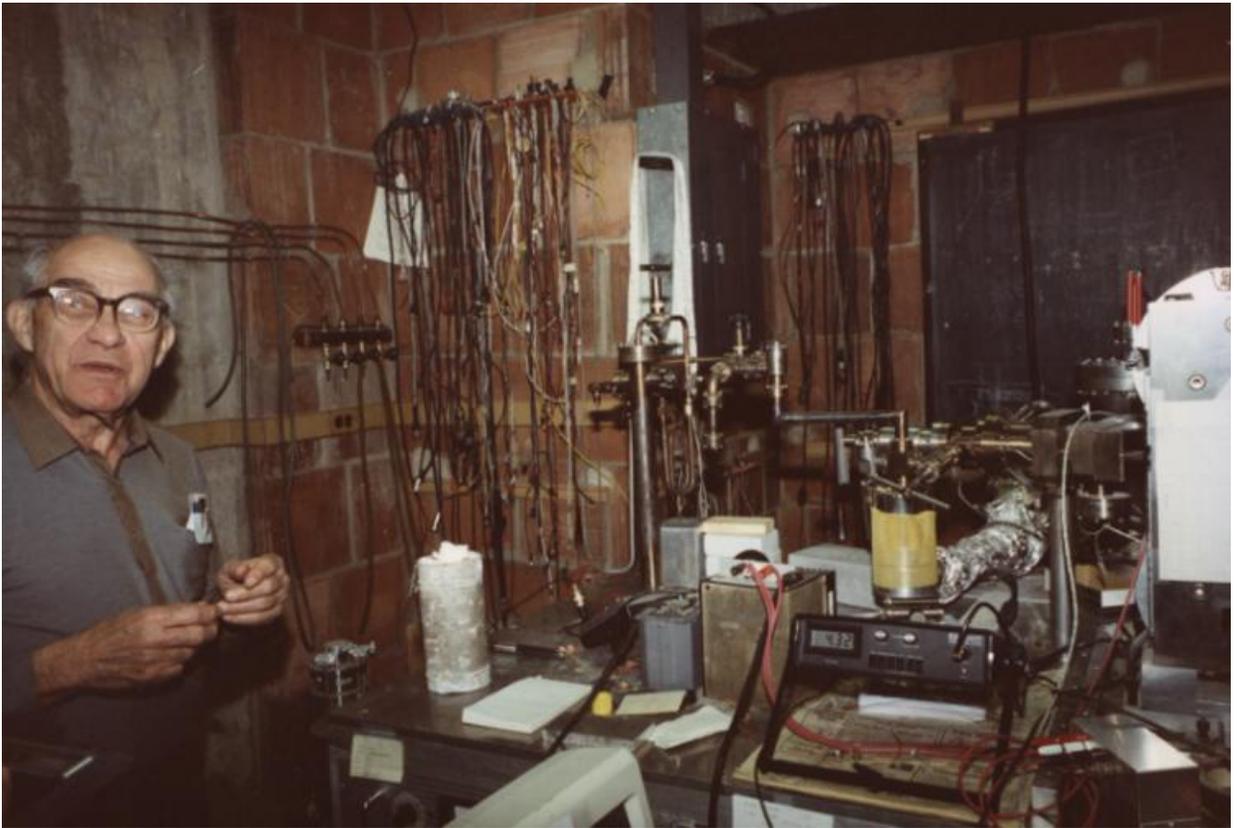


Figure 55. Another view of Figure 53.



Figure 56. Another view of Figure 53.

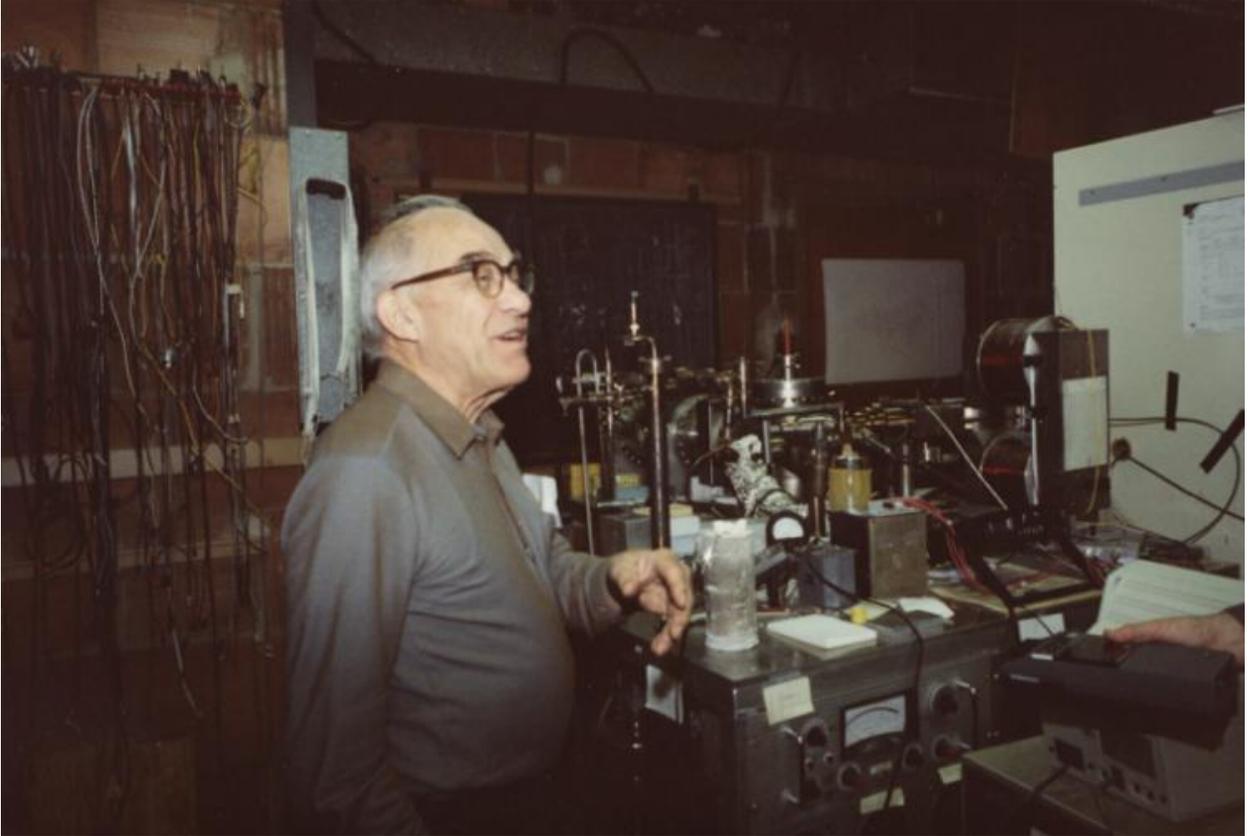


Figure 57. Another view of Figure 53.



Figure 58. High speed molecular beam facility.

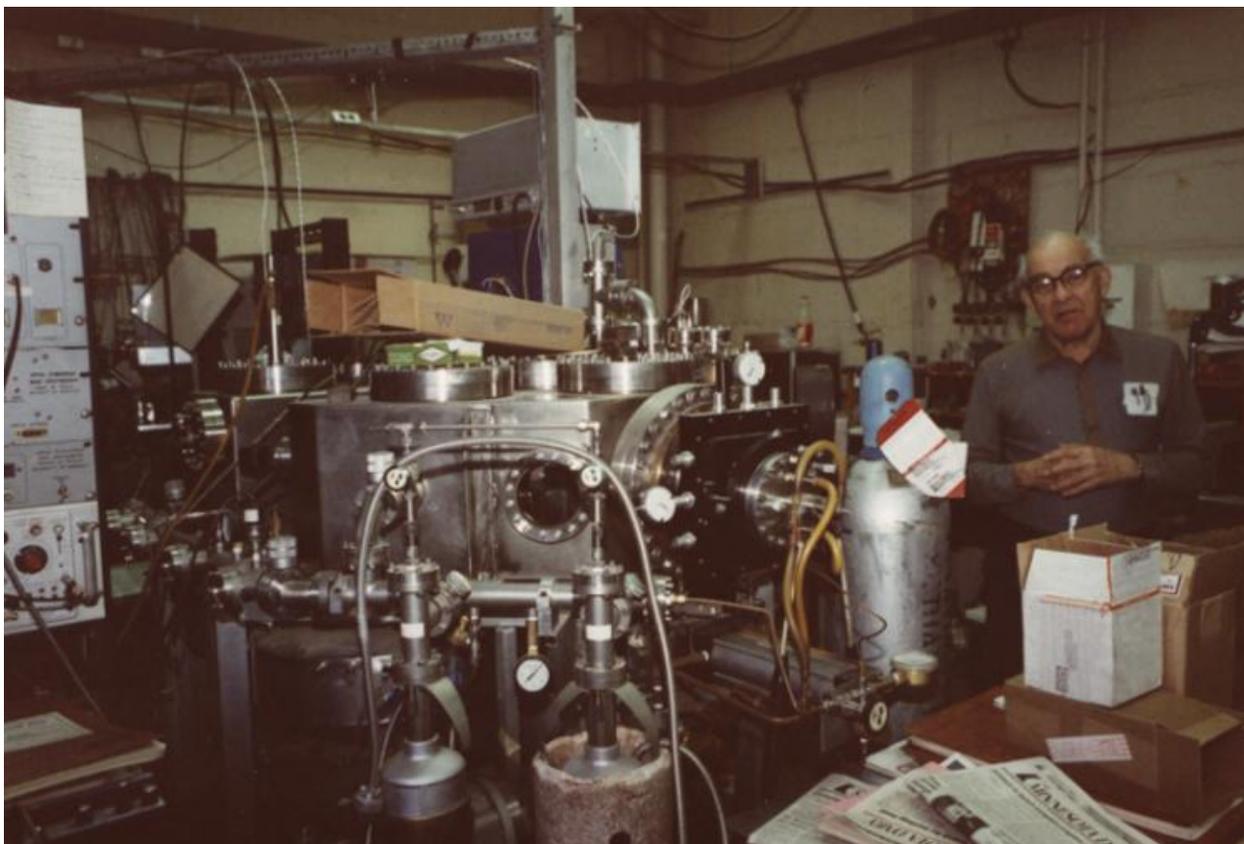


Figure 59. High speed molecular beam facility.

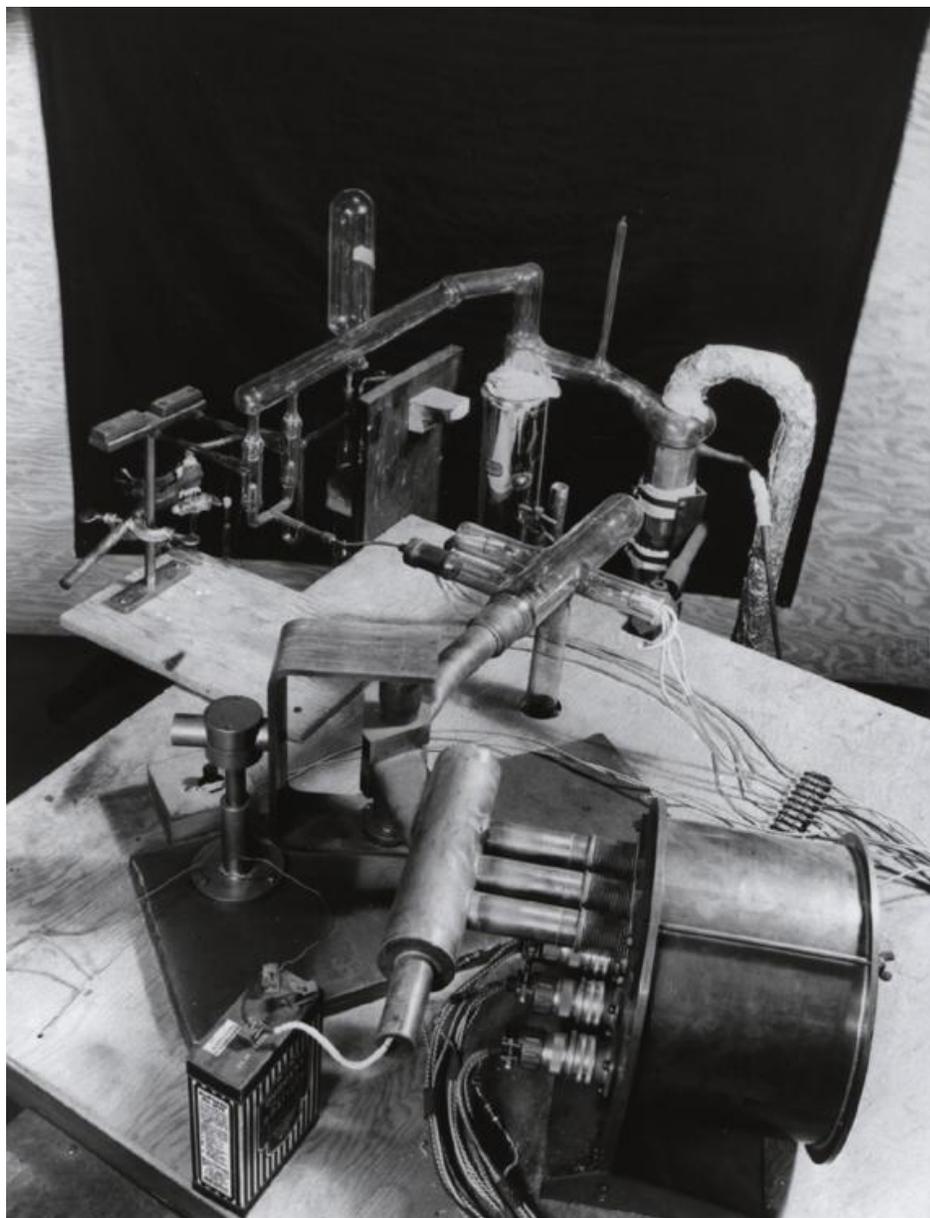


Figure 60. Three collector mass spectrometers for hydrogen isotope analyses. Post war metal model.

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