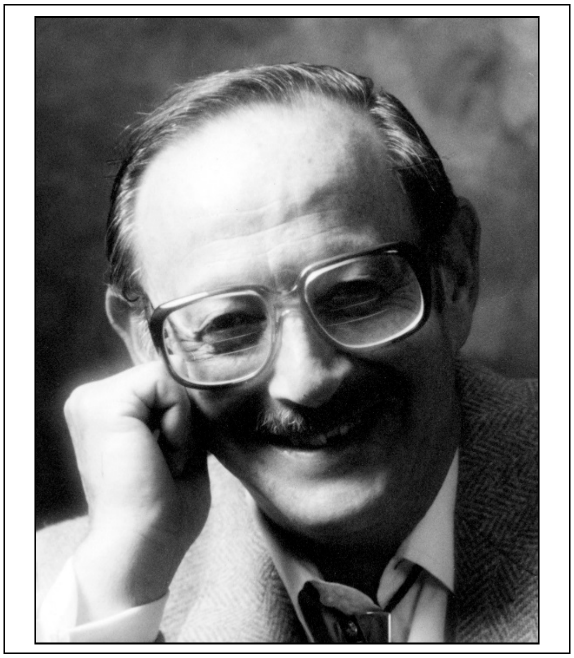

Gerald Wasserburg, ca 1980s
Photographer, Florence Helmberger

GERALD J. WASSERBURG
(1927 – 2016)

INTERVIEWED BY
DAVID A. VALONE

April 25, May 3, 10, and 17, 1995

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Geology, physics, geophysics.

Abstract
An interview in four sessions, in April and May 1995, with Gerald J. Wasserburg, John D. MacArthur Professor of Geology and Geophysics, emeritus, in the Division of Geological and Planetary Sciences. After a stint in the U. S. Army, Dr. Wasserburg matriculated at Rutgers University, then the University of Chicago (BS 1951); graduate school at Chicago (MS 1952; PhD 1954). He joined the Caltech faculty as assistant professor of geology in 1955, becoming full professor in 1963 and MacArthur Professor in 1982.

In this wide-ranging interview, he discusses growing up in New Jersey during the Depression, his early interest in crystals, his army service in WW II. At war's end, he studied geology at Rutgers under the GI Bill. Prompted by Henri Bader, he transferred to the University of Chicago in 1948, where he also took courses in physics. He recalls the intellectual excitement there; comments on geochemistry and geophysics at Chicago and Caltech in early 1950s; work of Harold Urey,

Harrison Brown, Clair Patterson, Samuel Epstein; his own work on natural gases and dating meteorites. Recalls blowing up his laboratory at Institute for Nuclear Studies. PhD work with Urey and Mark Inghram.

Settling in as assistant professor at Caltech; difficulties building equipment. Conflicts with Patterson, Leon Silver, Charles McKinney. Continuing work on decay constants of natural gases and dating of meteorites; building of mass spectrometer Lunatic I. Recalls courses he taught; comments on geosciences curriculum at Caltech. Comments on Caltech colleagues Barclay Kamb and Robert Sharp and difficulties with Silver over areas of study. Recollection of film project with Richard Feynman titled *About Time*. Concluding remarks on lack of "intellectual saints" in geology as opposed to other physical sciences.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

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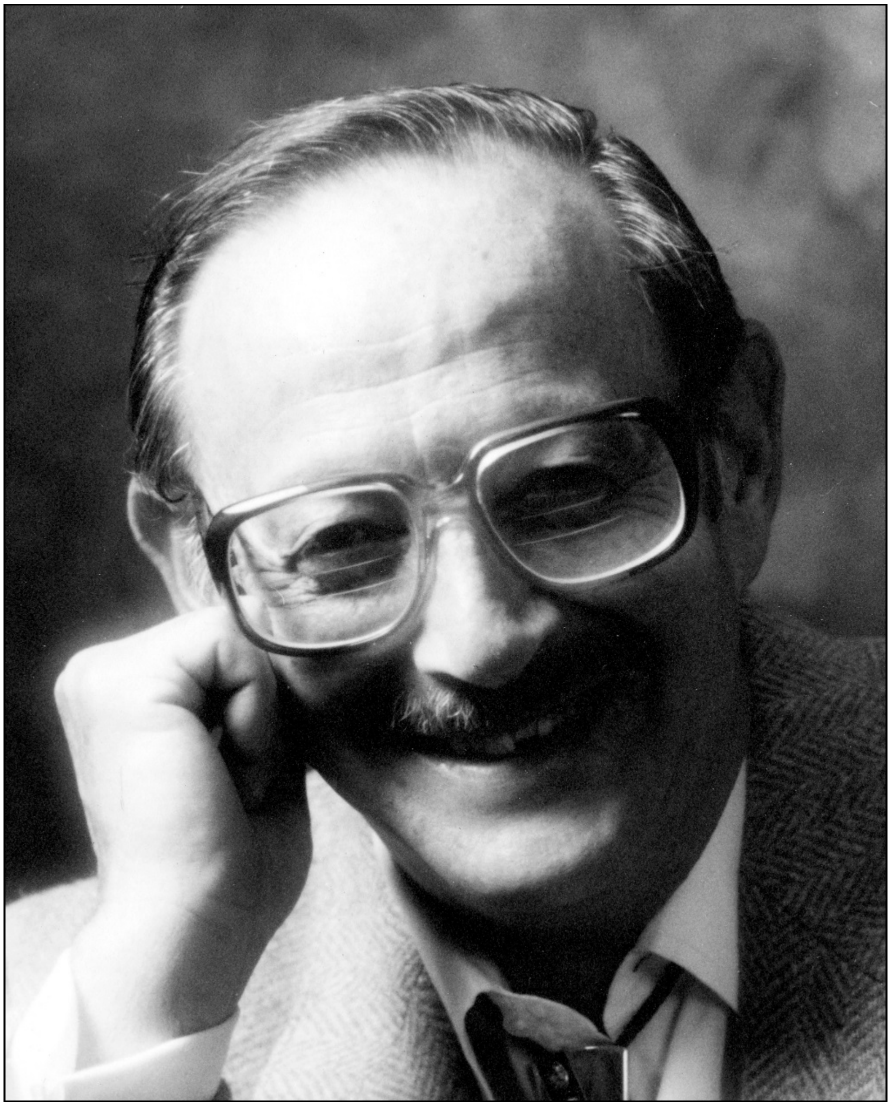
ORAL HISTORY PROJECT

INTERVIEW WITH GERALD J. WASSERBURG

BY DAVID A. VALONE

PASADENA, CALIFORNIA



Gerald J. Wasserburg, ca 1980s
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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Gerald J. Wasserburg **by David A. Valone**
Pasadena, California

Session 1	**April 25, 1995**
Session 2	**May 3, 1995**
Session 3	**May 10, 1995**
Session 4	**May 17, 1995**

Begin Tape 1, Side 1

VALONE: To start out, I'll have you tell me a little bit about your early life: your family, your background in New Jersey, and some of your experiences as a child.

WASSERBURG: I was born in New Brunswick, New Jersey, on March 25, 1927, in Middlesex County Hospital. My mother [Sarah Levine Wasserburg] had spent most of her life in New Brunswick. She was born in New York. Her parents were Morris and Minnie Levine. They emigrated around 1878 or 1880 to the United States from the Russian-Polish border—I think from near someplace called Bialystok, but I don't know any details. My father, Charles Wasserburg, was an orphan. He was born in New York City also. I remember meeting his stepmother only once, in an old people's home when I was a little kid. I remember grim circumstances, and that's all I remember. My father's father emigrated from Rumania. The only relatives I ever knew were from Vienna—it was a Jewish family. My [paternal] grandfather's trunks were stored in the basement, and my grandmother always used to make preserves there—pickles, herring, onions—and used to make wine, too. I used to play down there as a kid. I remember we used to unpack the trunk; my father's father was an optician of some sort, because there were lenses in the trunk—but nobody knew anything else about him.

VALONE: Do you know how your family came to be in New Jersey? Since your parents' parents were all from New York.

WASSERBURG: Well, my father was raised by his sister, Rose, and that was it. She was married without any children. And my mother's father—whom I referred to as Grandfather, because he was the only grandfather I knew—was an extremely erudite and competent guy. He left New York at the turn of the century, moved to New Brunswick, and went into the house-painting business. They moved first to Perth Amboy and then to New Brunswick, and they were there a long, long time; my mother went to high school there.

Then he got into the construction business and the housing business, as well as the painting business, and he was quite well off. That is, they had, I remember, a fancy apartment, right on the corner where I went to high school, on Livingston Avenue. He built some speculation houses—three of them on May Street, way up at the outskirts of town. But when the Depression hit, everything was lost. And one of the houses was supposed to be for my mother and father, one for my grandfather and his wife, and one for my Aunt Bessie, who had long since gone off to live in Shamokin, Pennsylvania.

So most of the family connections were in New Brunswick, which is where my father, my grandfather and my grandmother, and my mother are buried—in Jersey red clay. My mother grew up there, and as a young woman she had been rather well-to-do, and then the Depression hit. That was really a mess and caused a lot of strain in the family, as you can well imagine.

My mother used to have high expectations for herself, but the role of women was very different from what it is now. In fact, my sister uncovered this incredible letter from New Brunswick High School. This was a long time ago—before the First World War. The letter says, "Dear Mr. Levine, Your daughter Sarah is an extremely competent and capable person, and a brilliant student. And we would like to recommend that she be allowed to continue and graduate high school. And we would hope that considering her exceptional abilities, you would see your way clear in some possible way to allow her to finish high school." The answer was no. So I guess she went then to some secretarial school and helped out in the paint store with my aunt. Which was the role of women then, you understand. It was a very sobering letter; we found it just last year.

VALONE: But you were always impressed with your mother's ability, you said.

WASSERBURG: Well, I used to fight with her all the time. But this was a window back into what the circumstances were like in society. And it was sobering to read, "Please allow your child to graduate high school." That was a long time ago in our country—another world.

Her sister, Bessie, moved to Shamokin and married. There were a whole bunch of brothers in that family. I don't remember the sequence relative to World War I, but [her husband] died and then one of his brothers—following, I guess to some extent, a Jewish tradition—married her. That was Uncle Morris, who was very dear to me. Bessie was kind of the queen of the family. Because here was this woman who was a widow, a young person living in a small coalmining town in Pennsylvania in the early 1900s, and she had to earn a living. So she set up a dress shop and traveled to New York City to buy stuff. She was a woman of the world, and that was always something of a conflict between her and my mother. Aunt Bessie was quite a cookie—she was something else!

I'll just tell one story about her. She was a wonderful cook. Really ran a household, with three children—Albert, Bernie, and Edith, who were all older than I was. When she was sick and dying and had to go into surgery, I called her up—I lived on the West Coast then. And I said, "Aunt Bessie, this is Gerry. How are you feeling?" And she said, "Well, Gerry, I've got to go under surgery on Monday." I said, "Well, I hope everything is all right." And she said, "Well, I spoke with the surgeon, and the surgeon told me, 'Bessie, you have no reason to worry. Everything will be all right.' And I said to him, 'Doctor, I am not that sanguine about this matter. After all, it's my ass and not yours.'" [Laughter] She was quite a character. And perfectly lucid at all times. And incisive. And tougher than nails.

So the family was centered in New Brunswick and Shamokin. Holidays—Fourth of July, Thanksgiving, Passover—went back and forth between the two families. I remember walking up and down the hills in Shamokin and watching the miners come home at night all black.

Anyway, those were the closest members of the family. Then there were a bunch of derivative relatives, most of whom I get confused and don't remember. I couldn't tell who was really a relative or some kind of higher-order cousin or whatever it was. Of Bessie's three children, the oldest one, Albert, who died about six or seven years ago, was always an interesting problem. He was a brilliant guy, and he ended up quite well off. He did all sorts of interesting, wild things. During the Second World War, he was in army intelligence breaking Japanese codes. He was apparently very good at it, and he never talked about it. You would not know a thing about that; it was all sealed, boxed up. But that's what he did during the war. The other guys in the family—my uncles, Morris and the other guys—had all been in the army during the First World War. One of them was gassed. My father didn't serve in the First World War, because he was an orphan. My uncles were all members of the American Legion, so we used to read *The Legionnaire.*

In New Brunswick we were kind of on the outskirts of town. And to some extent, I think my mother always felt some sense of sadness and bitterness because of having been very well off and then not, with just enough to survive on.

VALONE: What were the strongest influences on you when you were a kid? It sounds like your grandfather was one.

WASSERBURG: Well, he was. And he was insistent that I learn Hebrew. Sent a tutor to the house. He died when I was twelve or something like that. And then more money was lost, and my grandmother had to move in with us.

VALONE: How did all of these events that were going on around you—the Depression, and problems with your family's business affairs—how did those influence your life?

WASSERBURG: Well, there was always a concern about having nothing. My father, whom I've not said much about, worked as an insurance agent. He was a bit of a high flier when he was younger. There are pictures of him in his sports cars—fancy-looking things. He was quite a dandy. He was quiet and mostly stayed away, and my mother was kind of the boss of everything and very demanding, including of my father, who mostly just hid. He was a very kind guy. My mother, I think, got pretty hard to deal with and demanding.

I had one sister, Libby, who was very close to me. She was younger. We played together, and we got along very well except for the normal sister-brother conflict. We were very affectionate.

Oh, I should say that my mother had always wanted to be an opera singer. She wanted to sing in church choirs, and my grandfather forbade that. So we had this old windup Victrola she used to play, with a wonderful collection of Caruso records. She'd go around the house singing all the time. She had a beautiful voice. We had a piano downstairs, and she was always playing the piano, and we'd sing songs. She loved to entertain and play bridge and cards with various friends and stuff like that. We were never poor, but we were strictly lower-middle class, with pretensions of being upper-middle class where there was no income. But my family wasn't particularly worse off than anybody else's. I remember people coming to the door, asking for food, and people were selling things in the streets. We were about sixty miles from New York City—which nowadays you'd think is close, but back then you'd go in only once every several months. And my mother could regale us with tales of her youth and what she used to do when she was in the big city.

The general situation during the Depression was tough, and it stayed tough for a long time. I can remember getting a job for twenty-five cents an hour. When I was older, I remember my father, sitting in front of the house in, I think it was, a Model A, telling me that I should begin to contribute to the family. I got madder than hell; I never forgot that. I'm not very proud of myself, but it's a true story.

As kids, we were well treated, but there were lots of things we couldn't do. Nobody could buy you a bicycle or something like that, and I wore hand-me-down clothes. I can still remember getting clothes from my cousins in Shamokin even if they didn't fit. I remember being resentful of that.

As far as influences in science, it was really not a house with any reading. Particularly as time moved on and my grandfather got sick and died. I used to go over to his house and sit down at this wicker table and play chess with him. He was very, very smart, very knowledgeable. He was kind of the shining light of the family.

One person I can single out as a positive influence was a college professor who lived about a mile down the street, by the name of Alfred C. Hawkins. He was a mineralogist. And for some reason, which I don't know, he would take the neighborhood kids in—there were something like a half dozen of us, ten years old, eleven years old—and teach us about crystals and minerals. So we'd go down to his house, and he'd sit there and have a cup of tea or something, and he'd show us crystals and explain stuff. And then he'd take us out on little field trips. He also had a beautiful daughter, not much older than I was. I figure I owe him a great debt. He was eventually fired as professor of mineralogy at Rutgers. I never knew what the details were.

The times then were very bad. The political situation in the country then is what people now tend not to remember. It was very, very dangerous. There was grave unrest because of the Depression, which had everybody going crazy, because the government never knew that it should do anything about it. It didn't have any mechanism to do anything about it. And there were a lot of unemployed people who were very unhappy. Things were coming apart at the seams.

Concurrent with that was the rise of the Nazi Party in Germany, and a very, very strong growth of outfits like the German-American Bund which were quite strong locally. They made life, I would say, very bad. And that was coupled with an anti-interventionism regarding the situation in Europe. There was a centrist attitude here, an isolationist attitude—that we should separate ourselves from that. Coupled with this very strong growth of Nazism in Europe, which then sparked and stimulated that priest, Father Coughlin, who had a powerful effect on the nation—which a lot of people don't really remember. I used to listen to this, you know, and you'd hear, "All Jews are bad people." And when you're a kid and you listen to this, you wonder, What did I do? Or what the heck is this about? So that time was not just difficult, it was exceedingly difficult.

VALONE: So all those external things that were going on, you felt impacted strongly on your own—?

WASSERBURG: Oh, very much so. I rarely talk about it, but it got very, very difficult.

There was a fear of Bolshevism and Communism. And if you joined a union, you were a Communist. Well, I was always very anti-union. In fact, the Shamokin branch of the family were all Republicans—and are to this day. But there was this notion that the Bolsheviks were going to take over the country: "The Nazis were right—the Jews are evil." The country was bankrupt; nobody had jobs. Toward the middle to late thirties, the local hooligans would parade up and down the street on which you lived, yelling, "Kill the Jews!" Which you tend not to forget. And death threats would appear in the mailbox. So it was not exactly the most pleasant of circumstances. It was difficult.

I was a very good student, and I skipped a lot of grades. And then I just bottomed out. I really resented things, and I think the state of the world turned me into a hooligan. But there was one key person, Miss Keim. If I could ever find her grave, I would put a magnificent bouquet on it every year. I was a troubled young man; and she started me doing biological experiments and chemistry experiments in the lab, and she went over my notebooks and talked to me. She was, I think, the first person who strongly and positively nurtured any scientific abilities and interests I had—this was before Hawkins.

VALONE: About how old were you then?

WASSERBURG: Junior high school. I had been a fine student in grammar school, but by the time I got to junior high school I was terrible. She was an enormous positive influence, a great person. She was a science teacher, and she would work with you, and set you up, and let you borrow a microscope and do some experiments, talk over your notes. I set up a little microscope in the laboratory of the basement of the house, and I would get an argon bulb and find fluorescent minerals. So I became the local mineralogy whiz—a kid who could show off and give talks at school. And that was very, very positive.

In high school, I was a terrible student—poorly disciplined, wild, unruly. And I began to hang out with some people who weren't so good—and some who were very good. There were a large number of Italian families around there, and they were very friendly. We'd play together and shoot baskets and play Ping-Pong. And they were very kind to us. They used to take me down to the beach when I was a kid.

There were strong ethnic and anti-ethnic feelings at that time; there were the Wops and the Jews and the niggers, and so on. So we would have a funny thing going in this little group: We'd call each other "Hey, Wop!" "Hey, Jew!" and stuff like that. But it was all among the best of friends. It tended to defuse some of the venom, which was very difficult to live with—that is, you were basically made to feel you were an inferior person, or something like that. And these kids were from wealthy families, so it was kind of a pleasant and fun thing.

In high school, I flunked plane geometry three times. I just wouldn't do anything. I became a hellion, which was, I think, a response to the societal pressures—I don't know the answer to that.

VALONE: You lost interest in school?

WASSERBURG: In everything. I was nothing but a fuck-up. Occasionally I could do things. I remember in biology class I did a magnificent dissection. American schools then were really a disaster. Kids were wild. Teachers did not get the respect they should.

Oh, and then I was a Boy Scout, and that was great! I became a Life Scout, and that was a big thing. I didn't become an Eagle Scout, because I couldn't swim far enough. We used to meet at the First Methodist Church, and it was a great experience. We'd go camping. I began getting merit badges. I could just do that like crazy; that was just fun. And my family was quite happy with that.

When I got interested in mineralogy, I remember that my father and my mother would take me to various places in New Jersey to look at mineral deposits. They'd sit in the car and have a picnic, and I'd go running all over. They were proud of what I did, but they didn't know anything about it.

And my parents took me to the 1939 World's Fair. You have to think a little bit about what the world was like: That's when we were just beginning, in the most modest way, to see some small light in the economy—and here was this world of the future. There was an exhibit from Brazil, with all these wonderful crystals. I was so impressed; it was just absolute fairyland! So I wrote somebody in Brazil, and somehow some woman whom I've never met, and to whom I am incredibly indebted, got together boxes of wonderful crystals and mailed them to me, and we started a correspondence. Well, of course, that really hooked me. That was just the best thing.

Then there was a guy in the state of Washington with whom I set up a correspondence. He would send me thunder eggs and rock samples. Here's this man I've never met who found that there was this kid who was interested in rocks and minerals, and we began to send things back and forth, and that was just heaven. I'm sure somewhere in a drawer someplace I still have those specimens.

These were positive influences in what otherwise was not a very positive situation at all. That sort of thing was worth more to me than you can ever imagine, in giving me a positive goal and a sense of beauty. With all the badness I saw around me—which was quite a bit—the crystals provided a sense of beauty and order. I could fall in love with them and see something beautiful in them. And I learned that if I could find something that I could love and caress and study, that was a form of enormous satisfaction—and I think pretty much a guiding principle.

There was some general notion that I should go to college, but the question was, How could anybody get to college with a record like mine? My record was terrible. And who could pay for it?

Then the war broke out. And the corrective action that the Roosevelt regime took to bring the society back to some functioning gave the country a sense of direction. And some of this other business, which was so divisive, began to at least alleviate itself at some level.

When the war first started, there was something called the Ground Observer Corps, or something like that—in some indirect way associated with civil defense. That was a big thing, and I became an expert at identifying aircraft. I would go out all hours of the day and night, go stand in the field and look for aircraft. I'm not sure it had any effect at all in terms of the defense of the nation, but that was the kind of positive action the citizenry took. That was a major formative thing in my early high school years, in these otherwise rather disruptive circumstances. I became an expert on recognizing aircraft; and I imagined that sooner or later I would go into the air force. And I studied and read stuff. You felt you were a party to what was happening.

Then there was a family by the name of MacNamara. He was a warrant officer in the army, and they were regular army types. Mrs. MacNamara took me under her wing and actually convinced me to become a regular army type. She was an incredibly intelligent person, and that's another grave I would like to visit. A great woman—she took me in and began to tutor me and pay attention to me, outside of the conflict in the home. She gave me a sense of direction and character.

VALONE: Would this have been when you were sixteen or so?

WASSERBURG: Fifteen, sixteen. At any rate, to put things back in sequence, I finally decided I was going to join the army. Well, I had a problem. One of my friends was a very bad person: William Erroll Banks—"Jughead" Banks. I think basically he was evil. His family took care of the property around some of the estates in town—gardeners and general handymen. So Jughead Banks and I were going to join the army. I was under age. What I had to do was get my birth certificate fixed up so I could join. We increased my age by about a year and a half or something like that. We got a candle and burnt where the date was, so it was slightly brownish. And then we penned in on top of it whatever date was going to allow me to get into the army. Which I then did. Jughead joined the navy.

When the family found out I'd joined up, they had this family meeting about what to do.

There was Aunt Bessie, my father and mother, and Uncle Morris. "Well, either dear little Gerry will end up in the reformatory, or he'll be in the army. What choice do we have?" So they didn't protest this move.

I enlisted when I was sixteen and a half, and I wasn't taken until I was seventeen March 1944]. Of course I thought I wanted to be a buzz boy and a fighter pilot, but of course it never happened.

Begin Tape 1, Side 2

WASSERBURG: So I joined the army—they finally took me in. My serial number was 12206488. I went to Fort Dix, in New Jersey. They put me in the Signal Corps. It was terrible! And I was hardly prepared for it. I was a little runt. I was just seventeen, not particularly tough, and I got pretty sick; I got folliculitis, and my body was all boils. It was not nice for a long time. I remember them putting me in the wards with some of the guys who'd come back from the South Pacific with jungle rot; I was not in much better shape.

Finally, they sent us off. I thought we were going to Asia to fight the Japs. Instead, I went to Europe and landed on Normandy in, I think, September of '44. We were assigned to housecleaning around the Normandy beaches and maintaining Signal Corps posts there. And when the shit hit the fan, when the Bulge started, I volunteered to go into the infantry. They just assign you at random, of course. But I became buddies with this guy named Keith, whose brother had lost his legs—or at least one—as a bombardier in a plane that got shot down over Germany. And Keith was going to go to the 2nd Division. So I hopped off and joined him in the 2nd Division.

VALONE: Was that where you had been assigned?

WASSERBURG: No, it was completely different. So I stayed with that outfit and crossed the Rhine and into Czechoslovakia—came back in reasonable shape. Keith got shot in Leipzig, but he made it out all right.

I remember that when we were in Czechoslovakia I decided I'd had enough of this crap, so I went to the captain—his name was Callahan—and I said, "I enlisted under age." He said, "How old are you now?" I said, "Eighteen." He said, "Fine. You're in! Stay." So I was a combat veteran; I've seen some shooting.

VALONE: The military toughened you up and gave you some sense of discipline?

WASSERBURG: Some, yes. We went up the Elbe; then they decided we weren't going to take Berlin, so they sent us off to Czechoslovakia, where we met the Russians. Then part of the outfit went farther and almost got up to Prague, but the Russians said that was too far, so they came back. Then we were going to be sent to assault the Japanese. So they quickly got us out, put some holding troops on the line there with the Russians, and shipped us back to camps in Texas, with a short leave at home.

And then the bombs went off, Hiroshima and Nagasaki, and that was the end of that. So then I went to Camp Swift and then finally got released, after getting into a lot of trouble and getting sent to the guardhouse for insubordination, refusal to obey orders, assaulting a superior officer, and some minor things like that, most of which were unjustified, in the sense that you have to consider the circumstances. You're living in a violent place. Like you're sleeping and the sergeant comes in dead drunk from town, sits on top of you, and begins to beat the shit out of you. And you fight back, but he's the sergeant. Enough said about that. But somehow I managed to get out. In fact, there was a West Pointer, whose name I can't remember right now, who was the battalion commander. He got me out of the stockade, where I was sentenced to six months and two-thirds, which means six months in the stockade and two-thirds of your pay taken.

Then I began to read about nuclear weapons and stuff, which I didn't understand anything about at all, because I knew nothing. I couldn't even clear fractions then.

I finally got out of the army and had to decide what I was going to do. I had a piece of paper that said I had had considerable experience in outdoor living and would be very good in forestry and road construction. But I decided—in spite of the advice from the MacNamaras to become a soldier—that I'd had enough of that, so I went back to high school. By then I was quite serious, and I graduated. Since I was in New Brunswick and lived with the family—I was still wild and kind of tough—I applied to Rutgers.

VALONE: You must have made a conscious decision at that point to focus yourself.

WASSERBURG: [Laughter] I'm just telling you what happened—I don't know. Well, I clearly felt that I should graduate from high school. I was quite skilled with words and competent in class, but I couldn't do any math. So my father took me—not far from where he's buried—to a guy who was [mathematician Charles Proteus] Steinmetz's lab assistant. He was a tutor, and I would go down to his house and this guy would teach me how to clear fractions, do proportions, do simple algebra. That was something! I liked that. Again, it was a little bit like the crystals, in that it was a world I could control and manipulate and understand.

I went to night school for veterans; this was at Rutgers. It was just becoming a state university. It had been a private school—an old Ivy League school founded before the American Revolution. So I went to night school there for a while, and I just did great. I'd sit up late at night; I'd go to the library, sitting day and night on the library floor, studying stuff, reading stuff. Just going like crazy. History. Anything. I was going to be a geologist.

So then I applied to go in the daytime. They gave me tests, but I could not pass the tests. They gave me quadratic equations to solve, and I didn't know how to do anything. So after a while, the psychologist said, "Well, young man, we can't admit you, because it says you want to be a scientist and you have no qualifications to be a scientist. Your skill with words is fine. Maybe if you want to do history or some other thing like that, but you can't do science." And I said, "Fuck you! I want to be a scientist." They said, "You can't. We won't authorize you. You have no skills." I went outside—and I was a semi-hardened character by then—sat under this tree, and I cried like a baby. Then I walked back in and said, "I'm going to be a scientist and you're going to let me in. And I'm a veteran, and I'm here on the GI Bill. And by God I'm a combat veteran. And I don't give a—"

So they let me into Rutgers in the daytime. As my mother used to say, "You went to school at night, and they found you could read in the daytime too, so they let you in."

I hated chemistry, because there was this great big lab, and I couldn't pass that. I did do very well in math. There was a guy, Earl M. P. Lovejoy, in my class. He had been a naval officer, and we became good friends. And I remember that there was a professor of mineralogy named [Albert S.] Wilkerson, a real shit, who would give us these problems. I became obsessed with analytic geometry and all this kind of stuff, and I could do all these really nice things—at least I enjoyed them, and I was quite competent at them. But there was this thing, *sin* X, and things like the index for a fraction; I didn't know what it was—not a clue. So old Earl would take me aside. We'd go drinking beers, sit down someplace, and he would teach me trigonometry, which I did not know. So you learned not what the normal classes taught but rather what you were interested in; we would interact, and then I would go get a book and learn it.

I guess somewhere along that line, I began to bum around in the West, in Colorado.

Oh, there's somebody from New Brunswick I didn't mention—Jonathan Orr, on Livingston Avenue, a mile or so from my house. This was a physician's family, the Orr family—well-educated and intelligent people. Their son Jonathan and I more or less grew up together. Well, Jonathan is now a physician and lives in Texas and is a big shot in medicine, in DNA and RNA and all this stuff. So it was a funny mixture of people: The number of accomplished people produced in that small town was not trivial. In fact, there's a friend of mine from La Jolla—Jim [James R.] Arnold [H. C. Urey Professor of Chemistry, UCSD]. My mother played at his aunt's wedding. He's a member of the National Academy. I never knew him as a kid, but there was something that went on there, within certain levels of the society.

Anyway, at Rutgers I did very well in geology. I cheated on a geology test once—I remember that—and I was caught. And the professor called me in and said, "You had some notes in your book, which you must have looked at, didn't you, young man?" And I said, "Yes, sir." He said, "I did not see that. Never do that again, please." That was the end. I never forgot the incident. I tell that story because that was a real, clear thing I did that was wrong. But the reprimand was precisely what I needed.

VALONE: Did he know you were interested in going into geology?

WASSERBURG: Oh, yes, I think so. He was a very good guy. I mean, I screwed up. "Don't do that again—do you get the message?" And that was it. It shows some character in the person who was guiding me, which improved *my* character a great deal. He was a foursquare guy, with good sense, a good person.

As time went on, he and I would go drinking. He asked me, "How are you going to get a job?" I said, "Well, I'm going to be a geologist. I'll work for an oil company." He said, "Oil companies don't hire Jews, Wasserburg. Let me explain that to you." So here I was, back full circle to where I was before.

Nonetheless, I was pretty stubborn. Then Henri Bader joined the department. He was Swiss. He gave a wonderful speech about me when I got a medal from the Geological Society [Arthur L. Day Medal, 1970]. He had me working in a laboratory doing sediment analyses and looking at rocks and stuff like that—sending me out in the wintertime to knock down an icicle. I'd bring it in. He'd say, "Saw it up." And I'd saw it up and then we'd study it, and you could see the inclusions inside the ice crystal. And they were fantastic. And he'd tell me to develop a theory about this. And of course, that was just great!

Then I said, "I'd like to learn more about crystals." He said, "Well, you'd have to learn group theory and stuff like that, and crystal structures." I said, "All right, fine. Will you teach a course?" He said, "All right," and he started a lecture course. I got a great grade in it—A+ or something like that. And he kind of adopted me. I try to visit with him and his wife to this day, as much as I can. He's now quite old and feeble. I visited him last December, and they're in bad shape, mostly just due to age. No children, and they both basically adopted me as their child in an intellectual sense.

After a while, Bader said, "Wasserburg, I think you should go to a better school. If you're going to do anything in geology today, you have to know physics, mathematics, and chemistry. So what you want to do is go to a school where you can learn that." Now, this is around the end of 1947. I said, "All right, I'll try that." And he said, "I think I'll go to a better place, too."

So I applied to Princeton, where I was turned down. And I cried like a baby, because that's where Einstein was—although he wasn't at the university at all and I was too stupid to know that. A French friend once said to me, "Einstein is your hero, and also your motivation in some way." And that was true, for a variety of reasons—and also goes back to political issues: Here was one person who could be held up—who is not a moneylender or a crook but is an intellectual of some legitimate substance.

I was admitted to the University of Chicago. Thank God I didn't go to Princeton! It was in 1948 that they let me in there. And they had this wonderful program—run by the chancellor, Robert Maynard Hutchins. You could place out of any course you want, and I began to take those exams. So I didn't have to take any biology, because I could do it. Or music, or art, because I'd read all this garbage—history and so on. I could take what I wanted: physics, math, and chemistry. But I was registered in geology. Those were the days of the high standards at

Chicago. I was not burdened with taking a large number of courses in order to get a degree.

I was still very troubled—and I suppose I still am very troubled, always very threatened, for a variety of reasons. The war was not that far past.

So I took physics courses. Those were the days when William Houlder Zachariasen—that's Fred [Fredrik] Zachariasen's father—was head of the physics department. [Enrico] Fermi, [Edward] Teller. It was a pretty formidable place. And I got an A in physics. So people began to pay some attention to me, because it was unusual for somebody from a culturally deprived background like geology to get an A in physics. Fred Zachariasen [Caltech professor of theoretical physics] was in the same class. I knew a lot about crystals, so I could teach them about optics, and we would do stuff back and forth.

Arrangements were made that I could get my degree—there was no degree in geophysics then—if I passed the physics graduate exam. Then I would not have to take stratigraphy and paleontology. The University of Chicago was a place that was just so intellectually alive—you cannot imagine. All the kids there were interested in doing science, or whatever the heck it was, at the highest possible performance levels. [Harold C.] Urey was running a series of seminars in geology about the formation of the solar system, with Harrison Brown. Kalervo Rankama was there. All these people were fantastic. So that was the place to go swimming, and I was very, very fortunate. Everybody had to be smart and know things, and that's where I learned everything.

VALONE: Was there not a sharp division between graduate students and undergraduates then? It seems that it's that way now at Chicago.

WASSERBURG: I'm not sure I follow. I was a screwball. I was a war combat veteran, and these frigging punk kids in class, who were sixteen or seventeen years old, were actually a hundred times smarter than I was. We were all mixed up together. It was terrible! Because I had to take freshman physics. I had never had a physics course in my life. I had to take beginning math. All I'd had was an elementary calculus course. So suddenly I was in class with these kids who were much younger than I was, and they were really smart, and I was not so smart. But I was absolved from taking all those courses in the humanities, because I was now well enough read so that I could pass all the exams they gave on that. That removed me from total immersion into the undergraduate program, except as it was in the sciences.

VALONE: But in the sciences, you were really starting from scratch.

WASSERBURG: Oh, yes! This was the first-year physics course. And the first-year math course, with a bunch of kids who were whizzes. And two years earlier, I hadn't been able to solve a quadratic equation. So it was very difficult, and I worked like a madman. I just had to learn and had to know. And that's what I did. But I was with the graduate students in geology. So I went back and forth.

VALONE: Who were some of the people you worked with when you were at Chicago? Did you say that Henri Bader came with you to Chicago?

WASSERBURG: No, Henri Bader quit Rutgers and did something else. And then he was one of the founders of the Snow, Ice, and Permafrost Research Establishment for the Corps of Engineers, because he was a real expert on ice. In 1950, I was his field assistant in Alaska.

VALONE: So you maintained your association with him even though he was elsewhere?

WASSERBURG: Yes, I would go and see him. He was my intellectual father.

Chicago was a place for all sorts of excitement. When you're running up and down the halls of the physics department, there were still guards guarding the plutonium, wherever it was in there. Everything was secret. All the people from the Met Lab [University of Chicago Metallurgical Laboratory] days were there. Cyril Stanley Smith was there—an extraordinary scholar. He translated [Biringuccio's] *De la Pirotechnia.* And it was a place of extraordinary scholars, where as a young person you were in awe, in the presence of the finest intellects of the world. And you learned. And all your friends were obsessed with that. The students were fantastic. If you want to pick famous people, a very small group of people in that period of time in science, it's a hell of a list. Chicago was full of absolutely brilliant people—innovative, doing things. Cesare Emiliani, famous for his work on using oxygen isotopes in the deep sea relating to the sea level and climate change, was there—also a classmate. It was a hell of a good group of guys—not all easy to get along with, including myself. But a wonderful intellectual environment. The professors were brilliant; the students were smart and working like hell. The veterans really were great. I got my PhD when I was twenty-seven years old [1954]. You could do just fantastic things.

I had no money. Even with the GI Bill. I remember the chairman of the geology department saw that I had holes in my shoes. "Young man, where are you going with that?" I said, "They're my old army boots, of course." He said, "Here, buy yourself some shoes."

VALONE: But in terms of actual steady financial support, you didn't have any, other than the GI?

WASSERBURG: I had the GI Bill. And then after a while, it turns out that I would have to get supplemental. I remember going to the dean and asking permission to work for Harold Urey as a lab technician to make money. And he said, "Why, you already have the best. You're the [Rollin D.] Salisbury [Memorial Fund] Fellow." And I said, "But that pays $300 a year." [Laughter] Or month, whatever it was. There's no possible way you could pay tuition or eat or anything like that. So I got put on as a lab technician, because I had to make some money on top of the GI Bill in order to survive. I became Urey's lab tech and ran the instruments. This was in the basement of Kent.

I remember a story. Toshiko Mayeda was another lab tech. She's a very dear friend, and she is still there at Chicago. Born in Japan—a wonderful girl! I told her I was going to get married. I was sitting there, running the instrument. Urey came in, and she said, "Professor Urey, Gerry Wasserburg, the student who runs the mass spectrometers for us, he's going to go off and get married. So he's going to take a week off. Is that all right?" Urey said, "Yes. He's getting married, is he? I suppose that's a good thing," and then stormed out of the place, because I was supposed to work.

But Chicago was full of extremely smart people, highly motivated, new things happening. All of the things that happened in nuclear physics and chemistry were open to new applications; suddenly you could see all sorts of possibilities. It was all growth and new ideas. You had somebody like Urey, who was talking about crazy stuff that nobody else had talked about. People were talking about cosmic problems related to geology and Earth sciences. And I should say that when I was still at Rutgers I'd begun to be turned on to this. I saw a notice that some guy named George Gamow was going to give a talk at Princeton about the formation of the elements. So I got a group of people together and we drove over to Princeton to attend this lecture, in which George Gamow talked about the Big Bang and nucleosynthesis. Gamow was this big Russian, red-haired—had a voice that was squeaky. A great, great scientist. And this big guy would stand up there with this squeaky voice and make some crazy announcement about the beryllium barriers, or something he was going to do. And [speaking in an accent with a high pitch] in the back of the room stands Eugene Wigner and says, "I listened very carefully to this"—accents that were just strange to me—"And I do not think you have taken the entropy into proper consideration." Then he sits down. What the hell was all this about? I didn't know, but it was fantastic—best show on Earth! George—whom I got to meet later and know a bit—was talking about making the elements. So there was, in 1948, full recognition of that problem, which then grew into the paper by Alpher, Bethe, and Gamow [R. A. Alpher, H. Bethe, G. Gamow, "The origin of chemical elements," *Phys. Rev.* **73**, 803-4 (1948)]. Alpher, [Robert] Herman, and all those great things they did, which preceded what happened here with Burbidge, Burbidge, Fowler, and Hoyle—which *led* to that. Everybody was trying to make the elements then, in 1948—all the people who had been doing nuclear physics. They understood that issue, and they also knew relativity. So by that time, in '48, I was intellectually aware enough to read announcements of another school and see something that was interesting. I didn't have a clue what was going on in Chicago then—in fact, nothing had really started there—but there were people doing some fantastic things, figuring out how the elements were made.

GERALD J. WASSERBURG

SESSION 2

May 3, 1995

Begin Tape 2, Side 1

VALONE: Last time, we left off with your arrival at Chicago.

WASSERBURG: Yes. I want to go back to something else. I didn't want to suggest that I and my family suffered in some extraordinary way during the Depression. The situation was difficult then—the economics of the family, the stress on the family. And the state of the nation, of course; it was very seriously threatened then. All those things together made life rather difficult, if not impossible. But not that much harder [for us] than for a lot of other people.

With regard to the war, I didn't say too much about it. I have to give a talk to high school students on May 8th about what it was like, and I've been thinking about that. I just want to say that I was in the 1st Squad, 1st Platoon, Charlie Company, 1st Battalion, 23rd Infantry, 2nd Division as a rifleman, 745 spec number.

Photo taken in Leipzig

I had a long enough tour of combat duty so that I learned a little bit about what you might call real life, or the facts of life. And short enough so that I didn't get blown apart. I don't know if there's much more to say about it, except that I was very proud of being there and surviving as a combat infantryman. I called up a friend of mine who became a sergeant major at the end of the war, who was severely wounded in the left shoulder, right above the heart—to talk to him about what I might say to the high school kids. But mostly what you talked about were funny things, which really didn't have to do with the really bad things, so that's kind of all that we talked about. And it was not—there were no real evocations in that discussion, in which I was looking for help from this old friend of mine from fifty-one years ago, about what things were like. Because mostly you just talked around what happened, until it was over.

Then there was another comment you made which I really wanted to jump on you about, in which you referred to the great days of the University of Chicago and said it was from ’50 to ’52. Now, I don’t know where you got that from, but that’s terrible. And I remind you that I graduated in ’54, and [UCSD chemist] Stanley Miller graduated in ’54. And some other people graduated around then. It went far beyond ’52. If one thinks that the great days of geochemistry terminated at the University of Chicago when Harrison Brown left [1951], that may be some bullshit that some people here like to talk about, but it’s absolutely, patently, not true. Nothing was happening here. Everything was still happening at Chicago and persisted far beyond that. What happened at Caltech was a transplant of what existed in Chicago and continued to exist there substantially after Harrison Brown’s departure. So there should not be any confusion about that in the record. After all, Stanley Miller was famous for the origin-of-life experiment he did with Harold Urey [1953]. He and I were classmates there. And Harmon Craig was there then; he stayed at Chicago until ’55. So there were some extraordinary individuals and contributions made then. And the developments at Caltech—which were exceedingly important, and of which I’m very proud—should not be thought of as a complete transplant, leaving a zero residue; that was hardly the case at all. In fact, the experiments for which Clair Patterson [Caltech professor of geochemistry, emeritus] became famous were run at the Argonne National Laboratory on a machine built by [University of Chicago physicist] Mark Inghram, on which I also worked. Patterson had to fly back to Chicago to make the measurements, because there was nothing to measure at Caltech. And there was at that time—and has persisted—a very strong resentment at the idea that it was really a Caltech experiment. It was an experiment that was initiated in Chicago, of which further steps were taken in Pasadena, and then it was brought back to Chicago and the Argonne National Lab to do the actual work. It would be quite improper to describe it in any other way. I can remember Mark Inghram being very unhappy at having it described as a Caltech experiment, when Inghram was a co-author on the paper [“Concentration of uranium and lead and the isotopic composition of lead in meteoritic material,” C. Patterson, H. Brown, G. Tilton, and M. Inghram, *Phys. Rev.* **92,** 1234-35 (1953)] and the experiment was done in his laboratory—an experiment that was a carry-on of what had been done before. So I hope that the record doesn’t get confused by some made-up Caltech version of what the facts are.

VALONE: Actually, I wanted to talk a bit more about the origins of the geochemistry movement that developed at Chicago and was, to a certain degree, transplanted to Caltech. Do you have a sense of the intellectual origins of this movement in geochemistry?

WASSERBURG: Oh, beyond a shadow of a doubt. I mean, it's perfectly clear. We had all these absolutely brilliant people.

When I got to Chicago, Harold Urey and Kalervo Rankama were there, who had been working on a great compilation of geochemistry in more or less the classical Schmidtian sense. [German mineralogist and petrologist] V. M. Goldschmidt was still alive then; I guess he was still in England—he didn't have many more years to live [d. 1947 in Oslo—ed.]. The lectures were given by people like Rankama. There were a whole bunch of Finns there, who were involved in analytical chemistry regarding geochemical things—like [H. B.] Wiik and Oiva Joensuu—a whole bunch of Finns from the school of Helsinki, who had followed the classical Scandinavian geochemistry that Goldschmidt founded and had access to things he did. A guy named Gunnar Kullerud, who took his PhD there also in experimental petrology on sulfides, he was a member of the Norwegian underground, a first-class guy. Hans Ramberg was there from Norway, and his wife; both of them were professional geologists. He had a broad interest in thermodynamics and geologic systems—someone with whom I always fought but owe a great debt to, intellectually and morally and personally. Urey and [Harrison] Brown decided to teach a course on the origin of the solar system together. They came over to geology and began to give these lectures on some interesting and exciting stuff. No one knew anything about it. But they began to formulate ideas, each of them in their own way. Harrison was the more intriguing lecturer; Urey was always more profound—or seemed to be. And then many other people were invited to talk from all sorts of different fields—in solid state physics, on rather advanced techniques at the time.

What one was looking at was people like Harold Urey, a Nobel Prize winner, who had played a major role in the development of nuclear weapons and nuclear energy during the war. Harrison Brown was always imaginative—and kind of a promoter, and he stayed that way most of his life, although he was a man of great intellect. But Urey was always really a scientist and Harrison was a wonderful game player. They gave these lectures, and of course you're a student sitting there before these great men, particularly Harold, and you think, Well, this can't be all bad.

In chemistry, we had Bill [Willard F.] Libby, who was then just starting to work on the carbon-14 business. So there was this kind of interplay. You had people like Mark Inghram, who was just about to become professor of physics, who was a mass spectroscopist. There was dissatisfaction with working on nuclear weapons and nuclear energy, and a desire, particularly on the part of Harold Urey, to do something different. Urey would get these wonderful ideas; he was basically a chemical physicist—a very great scientist and intellect. And most of these people wanted to do something other than just nuclear weapons, nuclear reactors, and stuff like that. There was this enormity of alternatives in the natural universe which were connected quite closely to nuclear physics.

In addition, that permeated the whole faculty. So it was recognized that [geochemistry] was an interesting thing to do. And there were problems like double beta decay, which one could not measure directly but could measure in geological samples. All this tended to bring cosmic problems together with nuclear physics problems and geological materials. And people who were no longer interested in the weapons stuff wanted to do something new and different; they were interested in applying their new knowledge and technologies to problems on the universe as a whole. It should be remembered that in 1948 the theoretical physics community—people like George Gamow and his students at George Washington University—were trying to formulate a theory of the origin of the elements. And it was that effort which originally stimulated me in this area—a talk Gamow gave at Princeton about the origin of the elements. I think I mentioned that in the last session. It was called nucleogenesis then, not nucleosynthesis.

There was the problem of how this was done in the Big Bang. And Gamow and Herman and Alpher wrote a series of extremely important papers that were tied to all these problems. It should also be recognized that there was, at that time, a conflict between the ages of terrestrial rocks—no age of the Earth was in fact well known—and the Hubble constant: the Hubble constant was less than some of the ages of the rocks. So this, then, was a source of regular discourse. I remember that when I was working on a timescale, and Patterson was working on a timescale, people would come in and give seminars, and the major issue was what the Hubble constant was, compared with the ages we were getting for rocks, because there was a manifest discrepancy. And I remember somebody finally coming in and shoving it in the right direction, so that you could see there was a chance that somebody had something right, particularly the cosmologists.

So there was a ferment there that was strongly interactive, and if one could tread both sides of the street, one had a great deal of excitement. A diverse group of people was interested in all of these things. There was no real geochemistry; in fact, my master's degree [1952] was a degree in geology. I couldn't get a degree in geophysics, because there was no geophysics program. It was just an arrangement between existing, essentially academic organizations and empires which allowed some students to walk across the street and work with somebody else. And within the geology department itself there was a great deal of ferment, because people like Kalervo Rankama wanted to look at broad-scale geochemistry, using standard spectroscopy techniques to look at abundance of elements. We used to get copies of V. M. Goldschmidt's notes from his book, *Geochemistry*, which he was writing pretty much near the end of his life, when he was dying. We had these mimeographed sheets—we called them "purple poop sheets"—and we would run them off, so that we would have copies of various chapters of the book as they were being written, which came, I'm sure, through Rankama or somebody like that. And then there were people like Hans Ramberg, who kept pressing for diffusive transport as a major means of moving elements and geological systems, which I always thought was nonsense. But that forced me to worry about diffusion all the time, and thermodynamics. So then I began to try to get strong and understand these things.

Some people were brought in. One person who was brought in was Fritz Laves, who was in Germany through the war. He and his wife and family were very fine people. And that was interesting. It was kind of hard for me, because this was, you know, not that many years after the war, and that guy had been on the other side. But these were really good people; in fact, they had suffered substantially themselves. He was a solid-state physicist and an X-ray crystallographer. And an awful lot of the native talent within the department were intellectual nerds and resented a whole bunch of new things, so that when some of us would come in and do vector calculus and stuff like that, for reciprocal lattices and structural determinations, we found ourselves with a professor who was incompetent to handle it. The only guy who could handle it was Fritz Laves, whose assistant I was for some time.

In any event, there was that ferment bringing in new people—not the people you would normally bring into a regular geology department. So there was sort of a revolution, or turmoil. There was this interaction with the people in physics and in chemistry, who had broader interests—somebody like Bill Libby, who was starting to find natural radioactivities like, for example, carbon-14, and use this as a dating tool.

So all these things were going on concurrently. And Urey got this wacko idea—which I think he got in a discussion on one of his great trips—of using thermodynamics and isotopic shifts to calibrate temperatures. And he then set up the laboratory and he brought in people like Sam Epstein [Caltech William E. Leonhard Professor of Geology, emeritus] and worked together with Heinz Lowenstam [Caltech professor of paleoecology, emeritus; d. 1993], who was a paleontologist, to collect samples of fossils and things like that, where they could try and measure this. Some of it was half-baked, in the sense that the technologies were not fully developed for that. So then suddenly one had to use the technologies, find out how [Minnesota physicist Alfred O. C.] Nier and Inghram and [Minnesota physicist Edward P.] Ney had made new mass spectrometers to permit better measurements, and build these spectrometers in a funny environment—which even then was difficult, because there never was any money in the Earth sciences. Chemistry and physics thought this was some wacko business. I remember Urey explaining to me that when he got a grant for something like $50,000, there was enormous resentment that money like this should go to supply a laboratory, because that amount would keep an army of geologists in the field, running around, making geological maps and hammering on rocks and making thin sections. And that problem has persisted to this day within the profession.

But the thing that was most important about Harold was his Nobel Prize [in chemistry, 1934], which gave him a pulpit, so he could speak with a great amplifier. And when he did something, people—while they might look askance at his actions—would have to pay attention to him.

At the time, nobody knew anything about the rest of the solar system, although astronomy then was not completely galactic in nature. G. P. Kuiper, who was an ardent colleague and an ardent opponent of Urey's, and who founded the institute in Arizona [the Lunar and Planetary Laboratory, University of Arizona], had a deep interest in both astronomy and astrophysics and in things regarding the Earth and the planets. So that was an extremely fertile place. [Subrahmanyan] Chandrasekhar used to come down at least every couple weeks and would come and ask me what I was doing and talk to me about the results; mostly, he was worried about the Hubble constant. So, when you're a student, that's a pretty high-class bunch of people, and that's whom you were with all the time. There was a desire among the people in the geology department to keep people in the geology department doing things more related to geology, and that conflict was there then as it is now. But you could walk over and do something across the street.

So the milieu was an exciting one; people were trying new things, had new technologies that had never been applied before, which were developed during the war and could be applied to natural systems. After all, you didn't have big enough accelerators; they were just starting to build the synchrotron when I was a graduate student. Marcel Schein ran a group doing cosmic rays, and there were millions of beautiful young ladies who were looking at emulsions that had been exposed to cosmic rays, trying to find funny particles.

VALONE: Why was it women who were looking at the emulsions?

WASSERBURG: Because that's what you hired then. It was low-cost, talented labor. There were very few guys who did it. It was very hard for women to be involved in things. But these were jobs, and technical jobs.

But the point of my story was the fact that a large number of the observations that were made, even in physics, involved natural systems. So the whole approach to problems was, you might say, closer to old-fashioned physics than new-fashioned physics—although there are a whole bunch of new-fashioned ways to explore cosmic phenomena, where you cannot get energies high enough—where you'd need detectors the size of a small town or something like that.

So I was half in physics, and I took all my courses in physics—or a large number of them. The bridging was healthy and easy, particularly if you could go walk on the other side of the street. And I was fortunate enough to get myself trained so I could do that, following H. Bader's advice.

VALONE: Do you think that particular institutional structure was unique to Chicago at that time—that kind of communication among and between departments?

WASSERBURG: To answer that in a meaningful fashion isn't easy; those same structures could have or could not have existed elsewhere. The important thing—forgetting the structures—was that you could do that. Now, let me just tell you a story. I did a master's degree in geology on diffusion of oxygen in silicates. My girlfriend—who's now my wife [Naomi Wasserburg]—was trying to type my thesis; she hated me for the equations. Those were the typewriter days, and the manuscript had to be handed in perfectly. It was terrible; I don't think either of us will ever forget that—but there was no money around, really, for anybody's help. In any case, one person who was very kind to me then was Sam Epstein, who helped me learn some chemistry and glass-blowing, and thermochemical calculations. He and I were once very close. This is not the case at all now and hasn't been for a decade or two. But he was very, very kind to me then. He was a postdoctoral fellow with Urey, and he used to live behind Urey's house.

Then I stopped doing that, after I got my master's degree. I went shopping around—what to do? I went to talk to Harold Urey. Everybody in geology was happy that I was going to do this solid-state diffusion experimental theoretical work. I'd done some X-ray crystallography. I almost worked for Willy Zachariasen, Fred's father. Fred used to pass me notes in class—questions on whether you could determine the oxygen atoms in some crystal structure. The father had done the structure of plutonium and stuff like that. And I almost did that, except I had some trouble doing crystal structures. I once tried to do a crystal structure and the goddamn thing was hydrating while it was in the X-ray machine, so every time I took another picture the lattice dimensions kept changing, and they were kind of expanding outward. I decided that was enough of that. I thought that was terrible. That was when I used to do the calculations for Laves' structural work, which involved calculating Fourier transforms. At that time, there were no computers; there were half-baked desk calculators. Calculating a Fourier transform, with all the phase angle shifts and the sines and cosines, you'd have to do all this arithmetic. Put in the numbers, multiply them out, add them up—look up the sine and then the cosines and multiply them together; many, many significant figures, and then sum them over 10,000 terms. I was always worried that Fritz would publish something I calculated, because there's not a chance I did it correctly. [Laughter] Anyway, I found that that was not much fun. I learned a hell of a lot about crystal structure and studied the [W. L.] Bragg stuff. And I'm sure I still have the original Bragg books, underlined in red, trying to learn the basic physics that went with scattering theory—which I did not know.

So I decided finally I would go find out what Urey was doing in the lab. There was my friend Sol Silverman, who was a chemist; he had also been in the infantry during the war, several years older than I. A big guy and a great reader; I owe a lot of my intellectual cultivation to his prompting me in all sorts of ways like that. He was very good in thermodynamics, and he was working for Urey—did some of the original fundamental work on oxygen silicates, which was technically very difficult and was emulated here some decades later. Went off into the oil industry.

So I talked to these guys over there. Sam Epstein was there as a postdoc. And [J. M.] McCrea was there; he was, I guess, a student who was apparently brilliant. [Charles R.] McKinney was there as an electronics engineer.

I don't know if I mentioned this last time, but there was within the whole physics community and chemistry community a general interest in these broader cosmic issues. I don't have the times exactly right, but Murph [Marvin L.] Goldberger [Caltech president 1978-1987] was at Chicago then; he was an associate professor of physics. Somewhere along the line some guy named Murray Gell-Mann came there, too [1952].

I took the first course that Murph Goldberger taught in quantum mechanics, which was a complete disaster. I still have the notes. I promised I would deliver them to him someday—make him sit on them or eat them or something like that. It was a wonderful course; I learned an enormous amount. But it was a young man's first course with a theoretician, trying to teach you all the wisdom he imagines he knows, plus showing how smart he is and showing off. A friend of mine was Bob Ginsburg, who's Murph's brother-in-law. So we always had a relationship through there. In fact, when Naomi and I got married, Mildred Goldberger [Murph's wife] gave us a beautiful set of Hiroshige prints, originals, which they had gotten from some estate sale. She insisted upon giving them to us for a wedding present. We still have them hanging in the house; they're absolutely exquisite.

I took mechanics from Dick [Richard L.] Garwin, who's younger than I am. Fermi said he was a genius, and I guess he is. Statistical mechanics from Maria Goeppert Mayer. And then a course from Teller; I can't remember which one. Solid state physics from Fermi. Quantum theory of fields from [Gregor] Wentzel. And magneto-hydrodynamics from Chandrasekhar. So I was reasonably well vested in that part of the business. And a substantial amount of geology.

Everybody was kind of talking to each other. And right after the war, the Institute for Nuclear Studies, the Institute for Radiobiology [& Biophysics], and the Institute for the Study of Metals had been founded. And you would see these people all the time. These were the great gods—and you're some peasant student. And of course there was all the political turmoil. Finally the Oppenheimer business broke and all that. Everybody was at it all the time—very, very knowledgeable about social issues. The *Bulletin of the Atomic Scientists* was being founded there. In the intellectual milieu I grew up with, either you drowned or you had to swim very hard, because action was all over the place, in every area. There was a sense of issues that transcended you as an individual, which were really societal: What's going to happen with nuclear weapons? What will happen if the Russians do this? Did we do right at Hiroshima? The [Julius and Ethel] Rosenberg case came up. We had the McCarthy era, in which the threats to our society were far more severe than people today seem to understand. I worry about my sons' failing to see that there were good periods and bad periods in society, and that the bad periods were really quite bad. The McCarthy era was a terrible battle, until finally Eisenhower fought back. In general, there was a rise in anti-intellectualism, in espionage. We were naturally the good guys, with all the weapons, and we should always have had them—which was, of course, absolutely hallucinatory. There's a song I used to sing to my kids: "Atomic power, atomic power, made by the mighty hand of God"—it was a song sung in Texas and other parts of the South, but of course we used to switch the words around: "Made by the mighty hand of Enrico Fermi." But the notion that this society should always be wealthy, growing, benevolent, and incredibly powerful, and that all these powers were our god-given right, and anybody else that had it had stolen it from us. And then, what were you going to do with this terrible capability? So when I look at the young people of today— There was not as much naïveté then. There was hope, but no naïveté.

VALONE: Do you think that people are just more detached from those issues now? Do they feel that they can't have an influence?

WASSERBURG: I don't know. Beats me. Then, people felt they had something to say about it and were concerned in various different ways.

The institutional structures that were built up at Chicago then were the Institute for Radiobiology. Then there was the Institute for Nuclear Studies, and then there was the Institute for Metals—built over the dead bodies of the departmental structure in the place—which trespassed on the authority of the existing departmental structure and helped encourage some of this ferment, because that took things out of the departmental setup. It weakened the departments in certain ways.

So, of my two dissertation sponsors, Harold Urey and Mark Inghram, Mark was in physics and Harold was in chemistry. Mark, who later became chairman of the physics department, would never become a member of the Institute for Nuclear Studies. He was running part of Argonne National Laboratory—that was a separate matter. But on the campus the institute superseded the powers, because suddenly you got new buildings, new equipment, new capabilities, new shops, and so on. So when you ask, "What were the circumstances that prevailed then?" There was a breaking of the egg by all this new structure, which transcended the existing structures and contributed to the intellectual ferment.

Begin Tape 2, Side 2

VALONE: Well, I think that covers some of the most important issues, especially the fact that these institutes were founded against the will of some of the scientific leaders. So clearly there was a higher level of administrative decision operating here.

WASSERBURG: Yes, and it opened up what the departments could or could not be—and also threatened them. Now the seminars became important. There was a Thursday afternoon seminar in the Institute for Nuclear Studies which everybody from every institute came to. Fermi, [Leo] Szilard, Teller, everybody. Herb [Herbert L.] Anderson, Bill Libby, Harold Urey, Subrahmanyan Chandrasekhar—the whole place was full of those guys, and they were playing hardball. And that's a great way to grow up. We'd sit in the back of the room, praying to God that nobody would ever call on us to say anything. But you got to hear the best stuff in town, in all areas. I watched some wonderful shows—resentment from some of the gods and demigods.

But in any case, I finally decided I would think about doing something with Harold Urey. Urey's secretary's name was Lucille McCormick; she was a great woman. There's an expression in German: "an entry-room dragon." She was a classy lassy. She became jointly the administrative secretary for both Harold Urey and Bill Libby.

So I said to Urey, "Well, I've been thinking, sir, I might be interested in doing a dissertation with you. Do you have any suggestions?"

And he said, "Well, I think you should measure argon-40 produced from potassium-40. I think that's an important problem in meteorites."

I said, "Why?"

He said, "Well, some guy named [E. K.] Gerling had some funny results, and I don't think they're right. And somebody's got to do it right, so why don't you do it?"

And I said, "Well, what do you get out of it?"

And he said, "Well, it's about timescales. I want to know how old these meteorites are." That was the end of the discussion. Which shows that when somebody points you in the right direction, it's half the battle. And when I decided to do that, I then went back to the geology department and told them I had decided to work with Harold Urey. That was just not very popular, because I was a prize student, and I was going to go over and do this crap. This wasn't real rocks. But I could do it.

VALONE: They didn't stop you.

WASSERBURG: They didn't stop me. "We're losing him" was the view. But I told you this story to show you that there was a mechanism for doing this—I *could* do it.

Through all this, I never was any good in chemistry. All my courses had been in physics and geology, and the only attempts to take chemistry courses turned out badly; I think I failed one, and I got a "D" in thermodynamics. When Caltech considered hiring me, [Charles] Hewitt Dix—he was a great man, one of the professors here—he saw that I had gotten a "D" in thermodynamics, and he said, "I can't stand to have anybody this ignorant here." But I had also gotten an "A" or a "B" in statistical mechanics, so he said that would kind of compensate for it. That's the way it was, and I finally learned thermodynamics, and I've taught it for many years.

But you could do all these funny things and get out of some of the regular geology stuff. Since I'd had field experience with Henri Bader, I didn't have to go to Baraboo and map the quartzite and do that stuff. I had done fieldwork on ice floes and glaciers in 1949 and 1950; it was the first time anybody had done that, and I was Bader's assistant.

So the environment at Chicago was extremely stimulating. There were these seminars, and the students would all go out together with Clyde Hutchison, who was a theoretical and experimental chemist—did very important and sophisticated work. And we were all mucking about; everybody went to seminars and other things, and I always felt that it was compulsory that

I learn the physics and chemistry that my classmates and colleagues were doing. And so I did. I learned enough of it so that I became reasonably competent. Tony [Anthony L.] Turkevich was down the hall; that name doesn't mean anything to you. A truly great man—he's been retired for quite a few years now. He was one of the people who Fermi thought was really smart. A great scientist, and he can do anything. As sharp today as he always was. He has kind of a raw, bullfrog voice. His father was the metropolitan of the Russian Orthodox Church of the United States, or something like that. And Tony was inside all the war work at the Met Lab and was a close confidante of Fermi. And technically and intellectually he was a person of the highest possible level you can imagine. Ed [Edward D.] Goldberg, who was a student of Harrison Brown's and one of the founders of a lot of environmental chemistry, was also there. George Tilton, who was later a professor of geochemistry at Santa Barbara, was also there. There's a list of people like you can't believe! If you were to take them out of the field, there would be no field today. And I don't say that because I'm friends with some of them; some of them I'm *not* friends with. But there's no doubt that somehow the place was lively. It attracted bright people, and the people, besides being bright, were dedicated and working hard, just did great things, and they all fed off one another.

And there was this continuous exposure, at least for those people who straddled the [interdisciplinary] gap. The people who stayed within their disciplines still did pretty good things, but if you could straddle the gap you could do fantastic things. My classmates who could not solve the simplest differential equation or could not pass a basic physics course except by cheating—I just couldn't understand that! There were all these things going on, and we wanted to do them—some of us wanted to do them.

I was very good friends with Harmon Craig during that time. Then some crazy bastard came over—a guy named Hans Suess. He was brought over by Harrison Brown and Harold Urey [1950]. Now, Hans Suess is a very peculiar person, and I have given several speeches in his honor. Hans's grandfather was Eduard Suess, of Vienna, who wrote *Das Antlitz der Erde—The Face of the Earth*—which is the classic book on geology. Hans is in some fractional part Jewish. Somehow, he and his wife, who was an ardent Nazi, survived the war. He was involved in the German heavy-water project, and I know stories about his visiting V. M. Goldschmidt when the Nazis took Norway, and they're not very good stories. He was always kind of a funny guy. Always a complainer, but absolutely brilliant.

Anyway, he had written some papers on short-lived nuclei, in Germany during the war and right after it—and Harrison, I guess, had seen them, and so Hans got brought over. He had this big fuzzy head of hair; he was a tall man, slovenly—looked like the crazy guy with the time machine in the movie *Back to the Future*. And this was shortly after the war. When [Carl Friedrich] von Weizsäcker came over and talked about the status of the early solar nebula and the collapse to make the sun and the planets, Willy Zachariasen would not talk to him. This was something like five years after the war. Zach was a Norwegian, and had been very much in support of the Norwegian Underground.

In any case, no one knew what to make of Hans Suess; he was an interesting and strange guy. I got to know him pretty well, and he affected some things I did. Both he and Harrison Brown had suggested short-lived nuclei as a tracer of what went on in nuclear processes. Urey was of course quite on top of that also. I met Hans Suess in the hall one day, and he mumbled to me something about xenon—he always mumbled. I was starting this thesis work on argon, and I had figured if I was going to do argon, I ought to do helium, neon, argon, krypton, and xenon. I had no modesty at all; I was going to do the whole geochemistry of all the rare gases, since basically almost nothing was known. We met in front of the elevator, and Hans said, "Yaah, man, you should—xenon-129, *das gut!*" I said, "Why?" He just stepped into the elevator, and that was the end of the goddamn discussion. But it was very important.

He and Urey began to work on the nuclear systematics regarding the abundances of the elements, which originally Goldschmidt had done. Goldschmidt had done the definitive work, which was quoted by all the astronomers, like Bengt Strömgren—comparing cosmic abundances with solar-system abundances, separating the volatiles from the involatiles. And it was Goldschmidt who used the silicon normalization business—showed the relationship to the sun and gave the basic stuff. And all this talk about the new things that were done—they were very important. But the basic work was done—and I get angry when they don't cite that work, because there are important things that were done in the Suess-Urey compilation, but the zero order of things were really done a decade before. And Urey didn't like this, but that was true.

Anyway, so Suess was also doing carbon-14 counting. Bill Libby had gone from carbon-14 to doing tritium and strontium-90 from bomb fallout, and everybody was trying to copy this, of course, and everybody was stealing from people at Chicago, because that's where all these things were happening. People were worried about weapons fallout—this was when atmospheric testing was going on. And Libby was monitoring atmospheric. And I got Suess's lab in Kent. Suess switched from carbon-14 to something else, and then I got to inherit his lab. So one night I come in. And I'm blowing a glass and I made a mistake, and I overpressurized something and it blew a stopcock and punched a small hole in the wall. No tragedy, except that I had to work several hours to fix the equipment. Then I went away on a field trip—or I went on my honeymoon; I forget which it was. So I came back—it was after I was married, so maybe this was after my honeymoon—I came back to the lab, and I took out my key and I opened the door, and I said, "I'm in the wrong room!" It was midnight or something. I thought I was losing my mind; none of my equipment was there. It was all gone. And I had spent months building this stupid stuff. So I looked at the room number—it was the right number, and I could see the hole in the wall. I think this was a Saturday night. Sunday was hell. So Monday I walk in, absolutely distraught, and I appeared in Lucille's office and said, "I'd like to see Professor Urey." I was quite upset. So she got me an appointment and I went in to see him. I said, "Professor Urey, I don't know what's happening. I went to my lab in Kent, and all my equipment is gone."

He said, "Well, how could that be?"

I said, "It's not there."

"Oh!" he said. "I thought you'd left! So I gave it to someone else, and they ripped it all out." [Laughter] Well, the desire to commit murder was kind of high.

So then they had to give me a new lab, which was in the basement. And I have to say now: A month ago, I got an invitation to give a speech for the 50th anniversary of the Chicago institutes. This is a funny thing—to be called back to talk there, to memorialize the anniversary of that old place. I was quite honored, and I'm glad to go. Goldberger's going to speak, I'm going to speak.

So I had this place in the basement there, and of course Urey was about as practical as nothing; all he'd say was, "Go ahead, go do it, that's wonderful, do the experiment." So I had to get authorization for stockrooms and stuff like that. There was nothing in this place—just floor, ceiling, walls. I had to plumb it, put in electricity, put up the racks. I suffered, but I learned an incredible amount and built everything from scratch.

VALONE: That leads me to the third area I wanted you to talk about, which you have touched on—which were the technological developments that were going on, particularly new instrumentation, which obviously you were building. Was there a good amount of support for that sort of technological innovation generally, or only within the subgroup you were working in?

WASSERBURG: Well, it was really true in places like the institute. It was true in the physics department, and parts of chemistry. In the geology department, there was just an X-ray machine; the new techniques, the new technologies, were going on elsewhere. They were certainly not in geology. You had a microscope, and you had the vertebrate paleontology stuff that was going on. The biggest changes there were X-ray machines and the intermediate-pressure stuff, which is still going on.

So it wasn't in the geology department, it was in the other places. People were building a cyclotron—a humongous enterprise. You had to build a building to put it in. Urey got some money to build some mass spectrometers. We had to modify them. Epstein was working on the chemistry; McKinney was doing some of the engineering; there were electronics engineers there. There were shops—glass-blowing shops, machine shops; you could design stuff. Getting materials sometimes was very difficult. To this day, in the basement of Arms [Charles Arms Laboratory of the Geological Sciences, at Caltech], I have nickel pipe—quarter-inch wall nickel pipe—which in my student days I had for making a vessel for decomposition of silicates, which I still haven't thrown out, because it was so hard to get. All that nickel was going to Oak Ridge [National Laboratory] for uranium hexafluoride separation systems, and you couldn't get nickel anyplace in the world. And to get nickel casing—heavy-wall nickel casing—was just impossible. So I still have it; it's kind of stupid, because I could just pick up the phone now and order it, but at that time getting some of that stuff was really hard, even with the war being over.

But you had a shop, and a storeroom you could go to to get valves, plumbing, O-rings, things like that. And that didn't exist in the geology department at all. We had nuclear-resonance experiments going on. People were designing and building equipment. The instruments that Urey and his associates refined were originally designed by Mark Inghram and Al Nier and Ed Ney at Minnesota. I wish I could remember other names now. R. J. Hayden, D. C. Hess, Charlie [Charles Martin] Stevens. Most of these people worked at Argonne National Laboratory, which housed CP-2—Chicago Pile Two. A lot of the nuclear technology associated with any research enterprise after the original CP-1 was built out at Argonne. Plus successor ones. Inghram had a joint position at Argonne and was responsible for the mass spectrometry group, which is related to nuclear monitoring, nuclear processes, and nuclear weapons. And in fact most of my doctoral thesis, a large part of it, was done at Argonne, because that's where the instruments were. A whole bunch of instruments were built then, in response to atomic energy needs and weapons-related needs. That's where a lot of the big instrumentation was. And very substantial staffs.

Inghram would build instruments on campus also. There were some major things done by Mark in instrumentation—incredible! Nier and he were two of the principal innovators in the whole field. But there was this multiplexing of engineering and science talent, plus the expectation that, well, if you needed anything you could design it and build it. So when I took my PhD degree [1954], it was a joint one with Harold Urey and Mark Inghram. I had a hard time. I had two sponsors whom I had to satisfy. One was an experimentalist, the other was predominantly a theoretician. And I was between a rock and a hard place, but that's how I learned.

But the level of technology I was involved with was not high-level technology at that time; it was a substantial level of technology, clearly related to war technology.

VALONE: [Inaudible question about degree to which funding of nuclear research depended on practical applications.]

WASSERBURG: I could comment on this. The original big grant that started here when Harrison [Brown] came here [in 1951] was an AEC [Atomic Energy Commission] grant of $100,000 dollars a year, which is about $1.5 million in current dollars. That was given to Harrison, and he was in charge of the empire; and the people he brought were Heinz Lowenstam, who came as a professor, Sam Epstein came as a postdoc, Clair Patterson came as a postdoc, and then McKinney came as an engineer. So it was a one-man empire in that sense. And the AEC [Atomic Energy Commission] wanted to know about uranium reserves and a whole bunch of stuff, and they needed an academic basis for research, and clearly they understood that then; and now you have to worry about what's understood right now—which, let me tell you, is very serious for the country. There was actually substantial feedback. I can tell you that a couple of people worked in my laboratory, and the results of technologies that were developed here at Caltech were re-injected into the weapons business and changed how almost everything was done. So they have more than gotten their money's worth.

[At Chicago], the experiments that I and my colleagues conceived involved working on extremely small amounts of material—for instance, a silicate inclusion from an iron meteorite is about a millimeter on a side. That's all you've got. The question is, How do you do that? So we were forced, by the choice of experiment, to handle smaller and smaller amounts of material without screwing it up. The weapons people and the reactor people, on the other hand, had incredible amounts of garbage. When they were measuring plutonium, they had plenty of plutonium, so they just raised background higher and higher and contaminated everything. And they did this by counting systems—counting the decays rather than the atoms. And they had small cities underground doing the counting, which got filthier and filthier as they were doing larger and larger samples—while we were doing smaller and smaller. The lethality level—the dosage level—they were having to face was ludicrous. One of the people who worked with me, Alex Gancarz, went down to the weapons laboratory in Los Alamos and set them up. There was this big battle. Now there are just empty buildings for the old stuff, without any risk to your health. But there are still places doing it the other way. You know, you can use a millionth or a billionth of what they're using and still get all the information out, but they're stuck in this old technology, which I thought we had tried to address earlier.

So I think the expectation that something would come back to the system from its investment has been manifest in the work of Ed Goldberg, of [Yale geochemist] Karl Turekian, who's here now [as a Sherman Fairchild Scholar], of myself, and many other people. We have trained a large number of people who are key technical players or managers in waste-disposal problems, in monitoring things. There are unfortunately not enough of our progeny to do what's necessary, and the support for this is always in jeopardy. Some of my students are in waste control for DOE [Department of Energy]. They're not very effective, because the bureaucracy and the legislation is a disaster. But they're knowledgeable people—people who took their PhDs with me.

Let me tell you a story. The detection of neutrons then led to people asking, "Can you see neutrons coming off the moon from cosmic ray [bombardment]?" This is the neutron albedo, or neutron glow, of a big target in space. [Richard E.] Lingenfelter—he used to be at UCLA, brilliant guy—did many of these calculations. When there was a chance of going to the moon, I designed with my colleagues an experiment to detect how long rocks had been sitting on the surface exposed to cosmic rays, by integrating the number of neutrons they saw. This then developed into a whole new type of enterprise. But the point of the story is that the person who did his dissertation ["Neutron stratigraphy in the lunar regolith," Caltech, 1974] on that was G. Price Russ III, who's now employed by the Department of Energy doing monitoring and control of [radioactive] waste, and it also was a microtechnology that has this application. The gamble that the universities could provide something for the system has been more than adequately demonstrated. And the reasons are funny. I mean, I couldn't give less of a damn about the applications. I was concerned about, Well, you've got a rock sitting on the surface of the moon or buried a meter below: How long has it been sitting there? Oh, yeah, we got neutrons, we can do that!

GERALD J. WASSERBURG

SESSION 3

May 10, 1995

Begin Tape 3, Side 1

WASSERBURG: I want to say something about the question, or issue, you raised last time about why things seemed to go the way they did at Chicago, and the institutional relationships. It's my opinion that the reason things happened were the circumstances of new technology, people looking for new fields, and the creation of new institutions which of themselves may or may not have been a good thing. But that broke the eggshell, and it may have been the basis for the scrambling. And resources were then available in new areas, and they were multidisciplinary in a complicated kind of a way.

VALONE: I think when we left off last time, we were talking about some of the more general issues of what was going on at Chicago during the period you were there. I want to start today to talk more about your own personal experiences in graduate school, and some of your relationships with the individuals at Chicago at that time. I think you also mentioned at one point that you wanted to say something about when you were in Colorado.

WASSERBURG: Before I went to Chicago, I got a part-time job. I decided I was not going to become a paleontologist—which was an infinitely smart move—but go into hard-rock geology. So I got a job as a mucker in the mines of Leadville, Colorado, working in the Resurrection Mine. It was one of the experiences I will not regale you with. But I had some experience working a muck stick and drawing and blasting and working in the mines down there. I learned a lot about life, again, and something about mining.

VALONE: You said you had decided wisely not to go into paleontology. What steered you away from paleontology?

WASSERBURG: Once I started getting involved with crystal structure and the chemistry and physics of things, that seemed infinitely more interesting than learning the stratigraphic column and all the stuff that went with it. And also, since I tend to forget everything all the time, I would never have survived that much memorizing.

VALONE: So it was also just a move toward the physical science side.

WASSERBURG: Yes. And also the beauty of crystallography and of basic physical and chemical laws that had very strong consequences. From limited observations and relationships, certain general rules emerged which could be tested, and that was what I found interesting.

VALONE: Let's talk a little bit about your graduate work. As I recall, you said that when you were looking for a topic for your dissertation, Urey suggested that you work on the potassium-argon relationship. And initially that didn't seem to have struck you as being a good problem, or an interesting problem—or were you somewhat concerned about that problem?

WASSERBURG: Well, it was just like, "Well, why don't you do this?" And then the relationship was never clear, nor was it in fact clear to Harold. He just thought these were funny numbers that might be interesting and should be checked on, and that was the end of the matter. Any architectural sense—of whether it would have any relationship to anything else—was of absolutely no interest to him at all. It just looked like an interesting direction to go in, and there was nothing more than that. It was not a discussion in which you were shown a panoptic view of all the diverse implications and consequences. This was a good training vehicle, in a new area where people knew virtually nothing, and somebody would have to do something. And presumably you would find something interesting with relationships there.

In fact, sometimes when I talk to students, they want some guarantee that [a dissertation topic] will bring them into a land of wisdom and greatness, or something like that. I have to convince them that there's a point where you can have all these wonderful visions but you have to stick to the straight and narrow and then branch out from that to find the connections that will make things real. And that these connections always exist, if the problem is innovative and good. But the guarantee that this is going to solve the puzzle of the universe is not usually a good way to solve a problem. And people frequently want that: "Well, how do I know this is going to be a good problem?" Well, you have to have a nose for the scent of a problem, whether or not you know for sure that it's going to be a winner. In my case, Harold just wanted to go in that direction—that is, people didn't know about it and I should do it. So I said, "How do you do it?" [Laughter] And he said, "Well, go talk to Mark Inghram."

So I went to Mark Inghram, professor of physics, and he said, "Yes, well, we've got mass spectrometers and we do this and that." And yes, he would be co-sponsor of my dissertation.

People in the geology department were disappointed that I was forsaking the promised land of silicates and real geology and leaving the building to go over to this strange world. A friend of mine, Julian Goldsmith, was disappointed that I was leaving the church to go over to work with that crazy chemist. But I did.

It was a funny mix of all these people across different departments. We had a study group, which involved Joe Bill McClure, who was my roommate and is still a very good friend. Bill [William P.] Dumke, who went off to IBM and did some wonderfully clever things. A guy named Werner Riesenfeld. We would get together and give lectures to each other on some theoretical thing. I did group theory; and [Fred] Zachariasen did something else. This was outside the classroom, so you kind of enhanced your knowledge in a much deeper way than you would just by doing the homework problems. And we did homework problems like you couldn't believe it. And everything I do today I got from the University of Chicago—all the intellectual muscle-building.

One of the professors was Val [Valentine L.] Telegdi [Caltech faculty associate in physics], who had just come over. Nina Byers, who's emeritus professor of physics at UCLA—one of the rare women physicists. One of the guys behind me by a couple of years was George Backus, a very famous theoretician, who took his PhD in theoretical physics. He had proclaimed a vow of abstinence when he was studying for the exams, and he placed the highest in the class. And his wife was in some kind of erotic frenzy, because he would not go to bed with her for six months or something like that while he was preparing for the exam. He came out ahead of everybody, and Fermi said, "Yes, he's very good." I remember the party when we finally passed the exams—wild, by our standards then, not by anything current.

Other people in the class: There was Harmon Craig, who was a year ahead of me. Sol Silverman, who was a year ahead of me, was also a combat veteran. Paul Edwin Potter. Fred [Frederick J.] Kueller and his wife. J Harlen Bretz was a professor, and a mean son of a bitch, but a very great scientist, there's no doubt about it. And a young woman by the name of Jean Simmons. It was very hard for women in those days, because we didn't really think they should be scientists, I guess. That's just the way it was. Some became very successful, like Nina Byers.

VALONE: It sounds as though there were actually more women at Chicago than at a lot of places at that time.

WASSERBURG: Yes. They have a great tradition of doing that. Chicago admitted blacks in the 19th century. I once got an alumni medal or award from them [1978 Professional Achievement Award, University of Chicago Alumni Association], and the person who [word unclear] was a black guy who had worked his way through school while being a porter in a Pullman car. It was an enormously emotional experience for me to hear this guy, who had come from absolutely nothing and had educated himself. And some women were also. So the university had a history of that, I think.

So I had to build a new lab down in the basement of the Institute for Nuclear Studies, after Urey gave my lab away. It was very difficult for me. First, I did not have the experimental experience, and secondly, I was just left all alone to do whatever I had to do in order to put things together. And that, of course, was also the big win, because with all that blood, sweat, and tears, which I found very difficult and painful, I learned one hell of a lot.

I blew the lab up once. Sam [Samuel K.] Allison was then director of the institute. I had gone back at night to rebuild the glass line. I had to make some glass with metal seals, and I had to clean out all the mercury from the pumps. I cleaned that with nitric acid and then tried to dry them more rapidly with acetone. And it evaporated and apparently some acetyl nitrates ended up inside the thing. And I sat down in front of the thing with my safety glasses on and goggles. And I put this tungsten lead through with a sleeve on it, which I fixed up. And I hit the joint with the torch, and the whole room went up. It blew me over, destroyed the lab. When I came to, I was on the floor. I was bleeding so bad I couldn't see anything—just covered with blood. Fortunately, the torch didn't burn me up. The place looked like somebody had riddled it with a machine gun or had set off a couple of hand grenades. I realized I was not in the best of shape, so I started to get up to find out if I could see. I had wounds on my face and on my head; blood was just gushing from scalp wounds. It was nothing serious, but I couldn't tell whether I was blind or not. I started to go down the hall—obviously I could see, because I found my way around—came up in the main entrance of the institute. It looked like somebody had opened up a can of red paint. Coming down the hall was Murray Gell-Mann, whose greatest personal risk at this time was probably falling on a pencil or something like that. He looked at me somewhat aghast, and I said, "Murray, I think I'm hurt." And he said, "Can I help you?" I said, "Yes, please take me to the hospital." He said, "I don't know where it is." So naturally he decided to go to the phone book to look it up. I said, "I'll tell you." His car was in front of the door, and I apologized for getting blood on the car. He took me to the hospital, and they took care of me. So I got home late, with my head in a turban. After they sewed me up, I was not too bad. I was very lucky.

The next morning, when the janitor came in, there was this pool of blood going out to the street. And the room was completely blown apart. Blood all over the place. The pumps going *blup*, *blup*, *blup*. And nobody knew what the hell had happened. So they called Sam Allison. And the question was, Where was the body? Well, the body was back in bed, not in that bad a shape. He used to tease me about that. And some years later, working in a lab on the third floor of the institute building—I was doing potassium analyses for my dissertation and doing some preparation, since I didn't have the chem lab. And somebody had a great big vat of carbon disulfide in there, which is flammable. I went down the hall. And all of a sudden it went up in smoke—and that makes smoke like you can't even believe. I went in to put the fire out. And you couldn't stay in that room for hardly any time at all—you couldn't breathe. I remember trying to shut the thing off and there was absolutely black smoke. I ran down and called the fire engine. And I was out in front of the building, waiting for the fire engine to come. Well, Sam Allison was in the director's office looking out. The fire engines came down the street, and he said to me later, "I heard those fire engines and I saw you, Wasserburg, and I thought, Oh my God, he's done it again! Thank heaven you were waving both hands so I knew you were all right." Of course, it wasn't my fire; it was somebody else's. But it was kind of a joke. Allison was a very fine gentleman, and a hell of a fine physicist.

So I got started on doing this stuff. I had no information to go on. I was worried about the decay constants of potassium-40, which were not known. I was worried about where were the rare gases in nature. And the only real serious work that had been done was by some guy named [John William] Strutt, Lord Rayleigh, at the turn of the century. So I read all of Strutt's papers to try to find out about this: Where was the neon and how did you do this? And then I tried to build this great big apparatus to measure all these things, which was just absurd. I built it, but trying to do all these things at once was just a monumental thing for one person.

And Urey was only interested in meteorites. He would not see a rock at all. I worked between Mark and him, and neither he nor Mark, basically, came into the laboratory. I cannot remember Mark coming into my lab at the university; I *can* remember him being at the lab, of course, at Argonne National Laboratory. In fact he would always be there, any hour of the day or night, on weekends. My wife says my working habits, unfortunately, remind her of M. G. Inghram III. And anytime I came in, I could talk to him, and he would grill me in great detail, as if we were doing the experiment together—as to why it was a good experiment. I learned an incredible amount. And that's what *I* do now. If a student comes in, I'll go into the lab, mostly to find out what the logic of the experiment is—does this test this, is this covered, is that covered, did you think of that? When it came to building something, Mark would say, "Well, there's a wiring diagram in the drawer; you can take that, or go get a book." Since I'd never done anything like that in my life, it was absolutely awesome and very painful. But I learned an incredible amount. He was then developing the high-temperature mass spectrometer with Bill Chupka. And most of the multitude of things Mark invented he has not been remembered enough for—for being the great experimental physicist that he was.

Robert Gomer's work on field emission was with Mark. The whole business of photo-ionization processes with mass spectrometry was all with Mark. John Reynolds was also a student of Mark's. John's now retired as chairman of the physics department at Berkeley. George Wetherill was a student of Inghram's. George Tilton was a student of Harrison Brown's. Patterson was there then. So there was quite a cluster of people.

Now, what I wanted to do was to try to understand the origin of natural gases, about which I knew bloody little, nor did anybody else know anything about it. And I was probably going in too many directions at once, which was OK. The final focus was to try and determine the decay constants of potassium-40 in natural systems and apply that to dating meteorites. But in doing that, I felt it was imperative to try to date some rocks on Earth, where you could get samples. And in some ways, Urey was very much a hands-off sponsor. That is, he would talk to you some, but he mostly wanted the results. In fact, one of the differences between Mark Inghram and Urey was that Mark talked about experiments all the time and Urey talked about the interpretation of data and broader issues. And there was that kind of balance, back and forth.

I can tell a funny story about Urey. I was trying to clean up the background of these lines, working at very high levels of vacuum at the time. This was a great big glass apparatus, glass all over the place, which had to be baked out with mercury pumps transferring gas, and charcoal traps—mostly mercury traps. And I was trying to clean these up. Sam Epstein was a substantial help to me then, because some of that stuff he had done at Chalk River, or wherever Sam was during the war in fission-product cleanup—because xenon and krypton are also fission products.

I used to strip to the waist there and use a great big smoky torch, blowing smoky flames out, so you didn't collapse the glass—and so, over this great big acreage of glass, smoking it out so that all the adsorbed gases would disappear off. Finally, I ended up with some contamination due to some tracers present, after months and months of work. This was after rebuilding from the explosion. So I went up to Urey's office and I said, "Professor Urey, I would appreciate it if you would come down to the lab with me; I want to talk to you about some of the experiments." On the way to lunch, he stopped by the lab. He was wearing a coat, because it was wintertime. He saw all this glassware. He said, "The glassware looks dirty to me." I said, "Yes, sir." He said, "Well, why is it dirty?" I said, "Well, I've been taking a smoky torch and heating it up to drive off the adsorbed gases to get rid of contamination. You know, one percent of the atmosphere is argon, so you don't get much out of a sample."

"Oh," he said. "OK. Well, it looks too dirty for me. I'll tell you what, young man. You get a toothbrush and a bucket of soap, and you clean that up, and you call me back when it's clean." That reminded me of when I was in the army and some officer assigned me to clean out the latrines with a toothbrush. But I did this, and I told him, "Yes, sir, it's clean." He came down again and said, "Now, that looks much better, young man. Now, what do you want to talk to me about?" I said, "I'd like your permission to break the line and tear it down and build a smaller one that's more compact." He said, "That's fine," and walked out of the room. That was the end of the discussion. [Laughter]

In the beginning, all these things were still classified. We're talking about the early 1950s. There was plutonium in safes and all this kind of stuff. And you couldn't go into certain rooms. I went in to see Mark Inghram, and I said, "I've got to build this thing to clean up these gases." He said, "Well, I guess people know how to do that." I said, "Is there some paper, or some design?" He said, "You've not gotten your secret clearance yet, so I cannot show you the papers. Why don't you talk to me about it, and I'll try and guide you." So he's sitting there with the paper, looking at it. And then he would talk to me about it, so that I could begin—but I couldn't see the paper because it was a secret. It was stupid! In fact, the guy who worked on it was Sam Epstein, which I didn't know at all. So I had to learn how to wrap this and that, and put platinum wires and ribbon around quartz tubes and put them in a vacuum, and heat up calcium—stuff like that. All this was kind of standard stuff; you can go out and buy it commercially now. But I learned a lot then.

VALONE: Did you eventually have to get a secret clearance?

WASSERBURG: Oh, yes, I got secret clearance. When I used to work at Argonne, on the weekends, I can remember that when you went to the stockroom you had to have a guard with you with a tommy-gun. A lot of weapons testing was going on. So you'd sit outside until they let you in. So I got to know the ropes at Argonne. And I got to work on the electron-impact and thermal-ionization machines at Argonne, since there were none on campus for this sort of work. The thermal-ionization machine where I did some of the work was the same one that Patterson used when he came back from Caltech and made the measurements that were then published with Inghram.

But back to Urey. I decided that the only way to find out what this method meant was to apply it to terrestrial samples which you could date by an independent method, and when I would go out in the field he would get absolutely livid. And I'd come back with rocks and stuff like that. My wife remembers a trip, because there were ticks. It was in northern Michigan. There was a rock which was M 1A standard, which for a while became the argon standard and the potassium standard. And then I went all through New England with people there, following up on what Al Nier had done for uranium-lead and thorium-lead ages when he was at Harvard. And then I got samples and tried to compare the argon-40 and potassium contents with the uranium-lead ages to try to establish the decay constants, because they were not that well known. It was Von Weizsäcker who had guessed, from nuclear systematics, that potassium-40 was radioactive, because there was a lot of argon-40 in the atmosphere.

This was kind of a competition between Nier and Mark Inghram. Nier measured potassium-40 and had done some early work with Tom [L. Thomas] Aldrich, who assisted him in Minnesota. So I tried to calibrate the techniques with uranium-thorium-lead ages. Urey really wasn't interested. The only thing Urey was interested in was the meteorites; he wanted to know what the age of the meteorites was.

This was all concurrent with the lead work that Patterson did. Some of Patterson's results—the critical results—were made public at the Williams Bay [Wisconsin] meeting the year before our stuff was done [C. Patterson, "The isotopic composition of meteoritic, basaltic, and oceanic leads, and the age of the Earth," Conference on Nuclear Processes in Geologic Settings: Williams Bay, Wisconsin, September 21–23, 1953, pp. 36-40]. But it was all in the same framework.

So Urey was not happy with my doing that; he felt it was a waste of time. But I insisted on doing it, and I did, and it was a good piece of work.

During that time, there's a whole bunch of stirrings going on. There's [F. A.] Paneth's work on helium and meteorites. You have to understand, people were getting ages like ten billion years for iron meteorites. And, of course, people had failed to pay attention to nuclear reactions from galactic cosmic rays. And there was a famous paper by [Carl August] Bauer ["Production of Helium in Meteorites by Cosmic Radiation," *Phys. Rev.* **72**, 354 (1947)], who pointed out the helium production from cosmic rays. And then there was the question of what was the uranium content of iron meteorites—which is essentially zero. There were all sorts of numbers in the literature which were orders of magnitude off. So everything was kind of crazy. There was a whole lot of frenzied activity and cross-relationships. What was the uranium in stony meteorites? What was it in iron meteorites? Most of the data were on iron meteorites, and all the uranium-thorium data were completely wrong in the literature. And [H.] Hamaguchi, [G. W.] Reed, and [A.] Turkevich, all their permutations; they were trying to resolve and clarify this great cosmic mystery of what the heck was going on with uranium and thorium and helium in the meteorites. And of course, they found that all the other data were wrong.

Begin Tape 3, Side 2

[Beginning of tape is garbled. It picks up with a story about Wasserburg telling Urey that he was having trouble getting lab supplies.—DV]

WASSERBURG: He said, "What do you mean, young man?" I said, "Well, sir, I go down to the stockroom and I ask for things. And unless they tell me exactly what I want, I have to stand and wait forever, and then if they don't fit, it's too bad—I can't go in back to fit things together or find out what they have."

He said, "That's terrible. I'll see to that." So he goes storming down to the stockroom. And here's Harold Urey, Nobel Prize winner, a great man. And he comes back about twenty minutes later with tons of all sorts of junk. He says, "Young man, I don't know what your trouble was, but they were extremely nice to me and gave me everything I wanted." [Laughter]

I tell that story to my students so if they have similar problems they'll tell me, because I will not tolerate that kind of garbage.

The Ureys used to invite us to Thanksgiving dinner on a regular basis. The Epsteins would be invited—they were living out behind the Urey house. And all the students and the people in the laboratory would have Thanksgiving dinner at the Ureys. But trying to talk to Urey was kind of difficult. If you told him something he wanted to hear about, that was fine. But if you tried to talk to him about something he wasn't interested in, the eyes would glaze over and he would go to sleep while he was looking at you. I remember sitting there—he was sitting on the couch—and I said, "Professor Urey, I've been worrying about this Poynting-Robertson Effect," which is the relativistic effect that has dust grains falling into the sun and stuff like that: radiation. And I said, "I'm worried about this calculation. There's a factor of two there. Now what about this thing?" And suddenly I realized, after a while, that he wasn't there anymore. He was glazed over and he was gone. When I stopped, he woke up, and then we switched over to something he wanted to talk about.

There was a terrible fight at that time between Urey and G. P. Kuiper. And after a while, though they had once been very friendly, they were extremely antagonistic. A funny thing, because both were great scientists and both made major contributions in their own way.

Urey's obsession with the solar system as a whole, and the chemistry of the solar system, was of course a key focus, which I think has governed my whole life since then. And Mark Inghram's obsession with meticulous and well-constructed equipment and experiments was also a major influence.

Harmon Craig was Harold's bright-eyed boy. Harmon was a personable guy and very excitable. He and I shared an office. His leg used to shake. He'd sit there across from me, and that leg would be going; I can't even do it that fast. He'd drive me right up the wall. But we were for many years very close. Harmon was getting data from some clever experiments on carbon. A large amount of it was based on equipment somebody else built, which he utilized in extremely clever ways and very valuable ways, and with thorough physical insight. But there was great resentment on Sam Epstein's part, because Sam was designing and building the equipment and Harmon was getting the data. And Harmon would then talk to Harold, because the data was the only damn thing Harold wanted to talk about. Sam felt he was just being used to build the stuff, and he developed an ulcer. Of course, they both did some extremely important papers. Very often, though, Harmon Craig did things that were refinements of what he had learned from somebody else—which were better formulated but not fundamentally different. And that was the cause of some grief. Of course, Sam is such an obscurantist that it's hard to tell when he's said anything important. So it was kind of difficult, and I understand that. Harmon would write very clearly and incisively. The question is, where the creation is—I don't know. But there were strong feelings, which lasted through many years, from Chicago to Caltech.

Harmon was the favorite of Harold Urey's, and nobody else basically existed. Urey would forget your name; he never forgot Harmon's name. Or he'd switch names, but he would never do that with Harmon's. It was kind of uncomfortable, like you aren't really there. I remember when Urey once introduced Sam by the wrong name after working with him for several years; we used to talk about that a lot. And I have many thoughts about that, which I will not here express.

VALONE: I'd like to talk a little bit more about your own vision of what your work was about, and your own choices of work. As you said, one of the things you did that Urey wasn't very happy about was going out, finding actual geological samples, and using those as part of the way you were getting your data. Why do you feel that that was important? Did you just need another way to get at—?

WASSERBURG: Well, the basic thing is, you didn't know the decay constants. The method had not been tested. Here was some stuff that was supposed to represent ancient solar-system material, but there were all sorts of crazy numbers. Not a single reliable measurement had ever been made of any [meteorite] age at all, by any method that was self-consistent in any way. It seemed to me that if we were going to date an object, there was terrestrial material you could date and interrelate and establish: Well, if I can do it here and get a certain consistency, then I have some basis for taking the other data and bringing them together. Otherwise, you just had a table of random numbers. There wasn't any solar system timescale or any cosmic timescale. The Hubble constant was not known at all then—it's not well known now—and we had very few data points to determine. The most reliable data we had were Al Nier's papers from the forties, and that was it. Geologic time was the issue. There was a National Research Council publication published by some guy, [John Putnam] Marble, which just glued together a bunch of papers on various things. There was nothing! There were common leads, and there were some uranium-lead ages on minerals, where you measured the total amount of uranium, the total amount of thorium, and the total amount of each of the individual lead isotopes. Those were the only data that fit anything. No other methods existed. The question was to try to bring something together, and it seemed to me compulsory to do that. So I read about Nier's work. I felt that the only rational way to proceed was to get one chronometer that people knew how to measure, with half-lives that were reasonably well determined, and to compare that with the other thing, which (a) you didn't know how to measure yet and (b) didn't know the half-life of. So that was the first thing to do. And trying to do that with a meteorite was absurd, since nobody had ever dated a meteorite at all. So you had to do this—and with the geological and mineralogical background I had, it was straightforward to try to do this. Then I went out to Strickland Quarry [near Portland CT] and all the old places Nier had been given samples from, and collected all these things and brought them together and began to measure and compare them—which Harold did not like at all, because I was supposed to be doing the meteorites.

Then of course there were a whole bunch of technical problems, one of which was how do you measure potassium? At the high levels it was easy, but I had to do everything myself. And after a while I began to get a little bit of help from guys at Argonne National Lab to do the standard bulk chemical analyses. It was a pretty stressful time. I had to drive fifty, sixty miles in an old beat-up car, or bum a ride with someone—and sometimes I would stay there overnight—every time we did an experiment. So it was not a luxurious circumstance. I was doing this with Dick Hayden, who would quit at five o'clock. And the old machines were not easy to work with. Hayden was a very smart guy and a hell of a good experimentalist, but politically not my type.

At any event, that's what I felt should be done. And I established data of extraordinarily high quality, with absolute calibrations for the numbers of atoms. So there was no hanky-panky about it; the results were very, very good.

I thought the most intelligent thing to do was order the best, clearest crystals, which most likely had suffered the least diffusion, because the size of the crystals was large, and I settled on potassium feldspar. So I compared potassium feldspar and uranium-thorium-lead ages, and established what the half-life was. Some years later, George Wetherill found that I was completely wrong. It turned out that the diffusion of the feldspars was regular diffusion, and it had lost almost 30 percent of its argon. And mica, with the very fine-layered cleavage, was quite retentive. But I had made that choice objectively. I did the experiment based on it.

I wrote some papers on diffusion models—this was in '53—and described the method and published. Then I was invited to participate in the great Williams Bay conference [Conference on Nuclear Processes in Geologic Settings, September 21-23, 1953]. As a student my job was to be the recorder, to record all the sessions on a wire tape, which was a complete mess. Because when they got fouled up, let me tell you, they really got fouled up. They would get twisted and then all hell would break loose.

There were a whole bunch of greats there. That's when [Clair] Patterson came in and presented his first rough calculation of the age of the Earth. That was a great conference! I remember Chandrasekhar's wife walking through the center of one of those buildings there in a sari [which was] flowing after her. And Bengt Strömgren, the astronomer, and his family. I became friends with him later. I remember being brought up to look at the moon through a telescope—brilliantly bright, almost painfully bright—which then, of course, became an obsession both of Harold Urey's and later on of mine. The Williams Bay conference was a mixture of some of the people who had done the old things and a bunch of people doing the new things, which was predominantly the Chicago group. I think Nier was there from Minnesota. And other than that, it was the new world, except for a few random people. Some of the new people were full of wonderful ideas, like looking at neutron capture in elements like gadolinium, because it has a high neutron cross section. I forget all the names. Anyway, the conference was full of ideas—if you've got neutrons, what can you do with them in the natural sciences? And stuff like that. And there was a pointing out then of the problem of alpha-N reactions from uranium-thorium [word unclear] oxygen silicon as recognized by many people in nuclear physics—by Turkevich and in George Wetherill's dissertation, which he was partly presenting then. And Harry [Henry George] Thode [from Canada], with whom Sam Epstein had worked and studied during the war years. Thode had got Sam his first job here. So all the problems of fission, neutron interactions, alpha interactions, nature—all that was kind of mixed together in one big bag. And that's what we did for a living; we still do it for a living. And it's what the field is.

VALONE: Did you have your own tentative results at that point?

WASSERBURG: No, no. I had just some beginning results—I think that came six months later, or eight months later. I had some data, but not enough to make any pronouncement.

VALONE: But you could begin to see at that point how your data were going to fit into the larger picture?

WASSERBURG: There wasn't the larger picture then. It was just lots of firecrackers going off. [Maria Goeppert] Mayer and [J. H. D.] Jensen were there. They had the apartment upstairs. I used to play Ping-Pong with them. There was the famous Mayer-Jensen coupling, for which they later got the Nobel Prize in physics [1963]. But that's another story. There were lots of dramas and interesting things going on. I don't think there was any coherence to it—it's just that people were doing nuclear things. And Tom Aldrich was there—beginning to explore things, trying to measure the decay of rubidium. Nobody really knew a hell of a lot, but there was a great big broad field to play in, and there was just lots of elbow room. Nobody had the technical development; the resources given to this field, you understand, were minuscule. This was not like the guys who were building the cyclotron or the betatron or something like that. These were people who were working as offshoots of these major efforts, taking small fragments and doing things which were— Truman Kohman was there, who has always had broad cosmic things in mind but who was a nuclear chemist. So it was a great meeting, very important—historically important.

Somewhere, associated with that meeting or before, I met a guy named Henry Faul, who edited the book where my first article appeared summarizing the work that Hayden and I had done. I think that was '53—the book appeared in '54. It was translated all over the world. It was called *Nuclear Geology* and had a funny mix of people in it, but there was some good stuff in there.

Now, there was a fundamental difference between the people who came from the nuclear chemical side and the people who came from the geological side. The nuclear chemical side basically counted so many nuclei from the parent and so many nuclei from the daughter and asked, “How do you actually measure them? And how do you interpret them?” The people from the geological side just wanted answers. And there is a culture in geology, which lasted for many, many years and persists to some extent to today—in which chemists were hired by the geologists to gather data. And some magnificent contributions were made by some: Washington’s collection of analyses of igneous rocks and so on, a monumental piece of work that many people had to gather to get this incredible database. There were other such efforts, of course, gathering data in this broader sense. But that meant you ended up with—in the geological departments—people who did the analytical chemistry for the geologists. And the technologies they used were governed by what they could produce to keep their bosses happy, not necessarily having anything to do with the problems. This can be seen in the work of the great [English geologist] Arthur Holmes, with whom I had some correspondence as a student about some problems with ages.

Now, Arthur Holmes really couldn’t calculate anything. He was a brilliant guy, and his book had a lot of influence on me—although he also offended me in his book. You might read the first chapter of *The Age of the Earth*, by Arthur Holmes. [In the 1937 edition,] he made a great point of saying that the Hebrews had contributed nothing of scientific substance to geology. [This sentence does not appear in the original (1913) version of *The Age of the Earth*. The Caltech Geology Library has the 1913 edition and a revised and expanded 3rd edition published in 1937, which does contain the offensive sentence and was itself a revise of a 1927 version. This sentence may also have appeared in the 1927 2nd edition, but the library does not have it, so there was no immediate way to tell—ed.] This was part of that crazy eugenics business—a very peculiar thing. But the book was enormous. He posed a whole bunch of problems in there which were quite interesting. Of course, the real hero is [John] Joly, who in 1910 understood almost everything, and most of Holmes’s book was just work on what Joly had said. He was an Irishman. A great scientist, brilliant man. He foresaw most things a half century or more before.

Arthur Holmes understood only that [in assigning ages] you measured the uranium and the lead—the total uranium and the total lead. The fact that uranium had two isotopes and lead had four—that he wanted like a hole in the head, because you couldn’t do that. You measured the bulk uranium and the bulk thorium and the bulk lead. He had a big fight with Al Nier about the constancy of the atomic weight of lead. And Al Nier showed beyond a shadow of a doubt that lead did not have a constant atomic weight—it was changing all the time. And the reasons were perfectly straightforward nuclear chemistry and just elementary. But there was opposition to this, which I finally understood later, when I got the [Arthur] Holmes Medal [1987, European Union of Geosciences] and thought about having to give a speech. I didn't want to talk about this business too much. If you had to have a mass spectrometer to say what these data meant in terms of age interpretation, Holmes was out of business. And he never really understood. In my correspondence with him, I got some crazy nonsense back. He didn't understand which equations were being solved, in some kind of way. And I didn't understand, except I thought he was a feebleminded old man. But he wasn't feebleminded; he was a brilliant scientist. But this was a big fix.

The lead-alpha method—which is the same method Holmes had relied on—was used by the United States Geological Survey and many places through the end of the 1960s; you had a system where you put in and you counted the alpha particles. So you had the uranium total and the thorium total, and you measured the total lead, and you didn't need a mass spectrometer. And then you supposedly had the age. You had horseshit! You had something that was confusing. Maybe half a century ago or more, that method was legitimate, but it persisted, because of this institutional thing of, Do we need this modern fancy-type machine to do this in our church? And that attitude persisted until the great revolution that started in about 1954.

VALONE: But you said that the USGS kept up that old method through the sixties.

WASSERBURG: Yes, because of this inherited attitude. That's my interpretation, and I don't think it's wrong. Because I remember the discussions on it: Should the geology department have a mass spectrometer?

This is the difference between the basic sciences and the applied sciences. And I straddled them. There's a balance that has to be struck between the development of new techniques and the demands for gathering the data necessary to steer the ship. Each has its own intellectual discipline and function. And the difference persists until today. We had a faculty fight about that a couple days ago, in which somebody was recommended for an appointment, and the issue was, Well, what this guy wants to do is buy a million-dollar machine, which is an item of commerce. The answer is, "Well, yes, but it will produce lots of data." The statement by a couple of us was, "But this is not an experimentalist. This is somebody who's buying a machine to milk the cow." "Yes, but it will give us the data we need." That may be true, but you have to see that there's a difference.

GERALD J. WASSERBURG

SESSION 4

May 17, 1995

Begin Tape 4, Side 1

VALONE: To start, I'd like you to retell the story about Urey and the first set of results you were getting [i.e., the garble on the previous tape.]

WASSERBURG: Yes. The story was, I was trying to measure potassium in meteorites in a reliable way and an exact way. All the data in the literature had been wrong, as it turned out, because the levels were low and the techniques used were just butcher-shop techniques—not commensurate with what they were at that time. So by studying and asking a lot of questions of people, I came up with a procedure for using isotope solution with very clean chemicals, a large number of which I made myself in the lab. I set up a procedure to measure potassium.

Urey was always results-driven and very interested in the results and what their implications were. He had hired a guy named George Edwards to be in charge of his lab. Edwards was an interesting guy. I learned a great deal of philosophy from George. He was a Scotsman. The first thing he did when he came into the lab was to empty all the drawers. I think he came in on weekends and did that, and he put the things back into other drawers. So he was the only person who knew where things were in the lab. That gave him some measure of control over everybody, which obviously was the purpose of the exercise.

Unbeknownst to me, Urey had set him to determine the sodium and potassium content of meteorites for cosmic abundances. I found this unfortunate and not a good thing. That is, if you have a student working on a problem, or a postdoc, and you happen to want the data a little bit faster, and you give the problem to somebody else, you set up an internal conflict, which is really not my idea of a good way to run a lab. You can have conflicts and competitions, but not to the point where people are at a great disadvantage.

I went in to Urey when I got my first results, and I said, "Here's what I measured for potassium in. . . ," whatever the name of the meteorite was. He said, "Well, George Edwards and I have just written a paper about this. And this is in disagreement with what we got." I said, "Well, that's what I got. I did it three times, and I think it's right." He said, "How did you do it?" And I explained it to him. The next thing I knew, the paper got rewritten with minor changes, recalibrating stuff all to my numbers. It then appeared, and I got a minor citation—which I thought was really quite inappropriate [G. Edwards & H. C. Urey, "Determination of alkali metals in meteorites by a distillation process," *Geochim. Cosmochim. Acta* **7**, 154-168 (1955); G. Edwards, "Sodium and potassium in meteorites," *Geochim. Cosmochim. Acta* **8**, 284-294 (1955)]. I was very bitter about that for many, many years. And I hope that I don't trespass like that in my career. I think it's important sometimes to get people in competition, but not to leave people out in that kind of way. And particularly when the basic data was somebody else's.

VALONE: But in general your relationship with Urey was quite good?

WASSERBURG: It was very difficult. He and I were very good friends, but he was always very distant. I had enormous affection and respect for him—there's no question about it. He would drive me crazy with this not paying attention and wanting the data fast, but he was a very good dissertation sponsor and a great man. I learned an enormous amount from him. And I was a friend of the family—a friend of his and their children.

I'll tell a story about Harold, which is important to tell. When I was writing some of my papers, I was working predominantly with Dick Hayden at Argonne National Lab, under Mark's guidance and Harold's guidance. When the first papers were written, Urey would not be a coauthor on them. He said, "Well, you did all the work and you wrote the paper, so that's all." I thought that was a very good and generous thing; I give that an A-plus rating. And they were very important papers. One of them was the first argon ages of meteorites with the decay constant, which I determined—which later turned out, as Wetherill showed, to be wrong. But it was the right numbers, and was concurrent within the short time after the time that Clair Patterson had made his lead determinations. So things looked pretty good then. I remember I got fan letters from India and stuff like that, because things appeared in newspapers all over the world. And that number, and Pat's [Clair Patterson's] numbers, were what really pushed things on the Hubble constant side—4.5 billion years or so, bit long for the Hubble constant.

And then the other part of my dissertation was looking for xenon-129, which Hayden and I published, which is a very famous paper ["Time Interval between Nucleogenesis and the Formation of Meteorites," *Nature* **176**:4472; 130-131 (1955)] —a one-pager or two-pager that appeared in *Nature* and was cited here by Willy [William A.] Fowler when he started to work on nucleosynthesis—or nucleogenesis, as we called it in those days. It was for many years the only connection, or bounds, on the limits between nucleosynthesis and the formation of the solar system. So I've been in that business for a long time.

Did I tell the story about Urey and the slide rule? Well, I think it was after I got my degree. Lucille McCormick said, "Professor Urey wants to see you." And Harold was sitting at the desk calculating something with one of these old slide rules which were around—it must have been about 50, 60 centimeters long; it was a big thing. We didn't have any calculating machines then, really. And he was using a great big magnifying glass to look at the crosshairs, and he was writing something down. I was standing there. He looked up at me, surprised to see me standing there, I think. He said, "Wasserburg, what do you want?"

I said, "Yes, sir. You wanted to see me."

"Oh, yes." He put down the slide rule, he looked at me, and he said, "Wasserburg, don't call me 'Sir.'"

I said, "Why, Professor Urey?"

He said, "Don't call me 'Professor.' Call me—" and then there was silence. He couldn't think of what it was I should call him. Surely I couldn't call him Harold. Finally he just picked up the slide rule and went back to work, while I was still standing there. [Laughter] It was years later before I could call him Harold. He was a great man, and a man of great substance. He was very much involved in the Rosenberg case then, and The *Bulletin of the Atomic Scientists*, and telling everybody what was going to happen if somebody put nuclear weapons on a boat and shipped them to the United States to blow up New York City, or something like that.

I've got to tell this story. One day, he called me into the office and said, "I'm sorry, I can't speak to you very long now, young man. I've got an important engagement." I said, "Well, I won't take but a minute of your time." He said, "I'm very troubled." I said, "What is it, sir?" He said, "Well, some people want to know what I think about the possibility of somebody trying to smuggle in an atomic bomb—say, on a Polish freighter, into New York Harbor. I don't know anything about that." So I said, "Well, sir, then maybe you should not talk to them." He said, "Oh, I couldn't do that." [Laughter] Which is the price of fame.

His wife, Frieda, was a great woman. They used to host us many times. It was an example to my wife and me as to how we should treat students and postdocs—welcome them in the home for dinner and parties, and things like that. So that was a very important relationship. But it was distant, because I don't think he was very fond of me for a long time. And I can't say that I was very fond of him. After all, he was one of my two thesis sponsors, and that doesn't go along with fondness.

I remember that Harmon Craig called me up the night Urey died [January 5, 1981], and I spoke at his memorial service. I'd been visiting him quite regularly. My wife and I would go down and visit with him all the time, talk to Frieda and talk to him. And when I spoke at the service, I showed pictures of his book *The Planets* [1952, Yale University Press], including the original cover, which read "Harold C. Urey, Nobel Laureate, *The Planets*." And I pointed out that one of the great impacts he had—other than being a wonderful innovator and full of ideas and incredibly brilliant and imaginative—was that the planets had not been a concern of astronomy in the decades before his book came out. The Earth was just an observation base from which to look at the universe, or the galaxy. And it was that combination of his intellect with his position—the fact that he had won the Nobel Prize and was a public figure, recognized for various kinds of public service, scientific and otherwise—that made him a major player. So his recognition of the field helped more than any other single thing to bring it about.

VALONE: And that field is planetary science?

WASSERBURG: Yes, in the broadest sense. And cosmochemistry, in the broad sense.

VALONE: Last time, at the end, you suggested that we start off this time by having you tell me a little bit about your thirteen-page dissertation ["The age of meteorites; (argon-40–potassium-40)–dating," PhD dissertation, the University of Chicago (1954)].

WASSERBURG: Well, all I had to do was put together some reprints of my papers. And I guess I had three or four papers. I think the total length was about thirteen pages, or something like that. And I stapled them together. Now, you were not allowed to have coauthors on any papers which were your dissertation. So I had to arrange to get special reprints made—since I had coauthors—in which I was the only person. And I've always felt guilty about that, because on some of the papers I never had, for some years after, any copies left of the ones with all the authors on them.

In all modesty, it was a very famous piece of work. It basically established the aspects of modern dating, including diffusion theory and dating of igneous rocks and meteorites and sediments and cosmic ray effects, and cosmo-chronologic signatures. In fact, I gave an abstract at the AGU [American Geophysical Union] meeting in 1954, which included the dating of sedimentary rocks from orthogenetic minerals and the timescale between nucleogenesis and the formation of the solar system. So it was a pretty broad arena, and I had lots of elbow room. So they hired me here at Caltech in 1955.

VALONE: I'd like to ask you about the attitude at Chicago, after Harrison Brown left to come to Caltech [1951].

WASSERBURG: Well, Harrison was always a promoter—a brilliant man, and always a promoter. When he came to Caltech, he was basically looked upon as a transplant from the Chicago side. And so was Patterson's work, which was justly famous and which was using the capabilities and analytical tools developed at Chicago, while there was nothing at Caltech. So at Chicago there was resentment to some extent, and Harrison was looked upon more as a promoter and a user of people than anything else. And he and Urey, who used to be very close, certainly were more or less estranged. When I came to Caltech, when I was finally offered a job here, my main concern was that I would not fall into Harrison's domain—that I would not become his property.

I should say something about finding a job then. In the first place, there weren't any jobs. Second place, what in God's name was anybody going to do with somebody who's some kind of half-baked physicist, half-baked geologist? There was hardly any place for such people in any geology departments. So I have to tell two stories: One is, I was considered for a position first at Minnesota, where Al Nier was one of the great masters of mass spectrometry; Mark Inghram had obviously been talking. Al invited me to Minnesota; he was head of the physics department there. They thought this was pretty good stuff, and maybe they should try to bring me into the fray there. So I went out there and I stayed at Al Nier's house. And I gave a seminar—I'm sure it was a lousy seminar. There was this great big old-fashioned amphitheatre, and here was this kid sitting there. And I measured this, and I did that, and this is the K constant, and this means this, and so forth. I was not a pro; I was just a kid. I'd made these wonderful measurements, and they were certainly significant, but I was obsessed with the details and doing things correctly, and stuff like that.

This was a joint seminar of the physics and geology departments. The approach Nier had taken was that physics would give me a joint appointment if geology would give me a joint appointment, and physics would then give me a laboratory and full support; it was an incredibly generous offer. And Minnesota was one of the two centers of mass spectrometry in the United States. Anyway, there were a whole bunch of guys at this seminar. So there were questions: "Did you do such-and-such? What about this branching ratio? How do you know that the gamma lines or the beta counting measurements are right?" So then somebody stood up in the back and said, "Wasserburg, I wonder if you could tell me what the purpose is of measuring absolute geologic time?" I thought, Well, this poor ignorant bastard has got to be a physicist. So I explained, "Well, as you know, there's this time sequence—like A is before B is before C is before D—which is typical stratigraphic time. And this does not permit rates or actual timescales. And if you have absolute time, then you can know rates and kinetics and processes and things like that. And for most of the geological record, there is no ability to get even the relative times, not to mention the absolute times. So these are very important. It's sort of like the difference between kinetics and thermodynamics. Thermodynamics will say, 'This is more stable than that,' but the rates at which these things—" And I began to talk about basic physics. Well, the guy who asked the question was the head of the geology department, and I didn't get the job. In later years, Nier and I used to joke about this all the time; he always felt that it was the worst mistake Minnesota ever made.

But this was what the field was like then. That is, there was no place to go to, because no geologist could imagine having somebody doing experimental physics in the building.

Then I got a job offer from Penn State. O. Frank Tuttle was my host. He was a very famous experimental petrologist, and I had done some experimental petrology, so he offered me a job. I was supposed to teach introductory classes, and then I could try and set up a laboratory doing something relating to physics.

Meantime, out here they must have had some meetings or something like that.

Oh, I did get a job offer from Chevron Oil Field Research Company. There were people there who knew me very well—Sol Silverman, who worked there and had been a year or two ahead of me at Chicago. And then the head of the company was always snooping around Chicago—N. Allen Riley, who was a very good guy and was always looking for real talent and innovation. So they offered me a job, and they feasted my wife and me. They offered me some stupendous salary—$8,000 a year or something like that. Since my salary then was $3,000 a year, it was a lot of money.

And then people here—mostly, I guess, through Sam Epstein—heard about me. I was not an unknown person, having developed a whole bunch of new techniques in a short period of time. Heads of oil companies would come to Chicago to visit me and see what was going on, and big shots in the field. There were all sorts of things happening in the oil and gas business related to using modern techniques in sedimentation and stratigraphy.

So I came out here and I gave a speech at the GSA [Geological Society of America] meeting in Los Angeles. I remember that, because there was an unpleasant incident involving a student from Berkeley who was working with John Reynolds. I gave a paper on the dating of sediments at the meeting, and I was sitting at a table having lunch with my wife, and zillions of people wanted to talk to me, because it was all exciting stuff. And this other young person came and sat down on the floor next to me, and began asking me how I did all this stuff, and I explained it in some detail. He then proceeded to go home and try—and to some extent succeeded—in beating me into print, which I have never, ever forgiven him for.

Then I visited Caltech and gave a few seminars. I talked about the oldest rocks on the Earth, which I had also determined at that time. Because I was full of ideas—and I had this wonderful new shovel. I had had a correspondence with some guy in South Africa who looked at the sediments and stratigraphy there and found the oldest conglomerate he had ever found. I said, "Would you get me a cobble from the conglomerate; I'll find out the age of the rock," which had to be older than the boulder field it was in. It was 3.6 billion years old, which stood as the oldest rock on Earth for many, many years.

So I was just having a ball. I was just like a kid in a candy store. I could recognize problems, and I could do them. I gave a series of talks out here. I remember being drilled by some guys. One of the guys in the audience was a student by the name of Lee [Leon T.] Silver [Keck Foundation Professor for Resource Geology, Caltech].

Then I went back to Chicago. And sometime afterward, Bob Sharp [Robert P. Sharp, chairman of Caltech's Division of Geological Sciences, 1952-68] came through, on a cold winter night, and asked me to meet him at the airport. And we just went walking around the airport. And he said, "How would you like to come to California?" It was cold and windier than hell in Chicago, with ice on the streets, and this didn't sound too bad. So they hired me, and I came out here.

When I came out here, the question was, Where was I going to be? I got $5,000 a year as my salary. And we didn't have any money. My wife stuck with me through all this nonsense. So I tried to get settled in here. I was in North Mudd [Seeley G. Mudd Building of Geophysics and Planetary Science], next to William Otto, who was curating fossil vertebrates and dinosaurs—mostly carnivores—for the museums and stuff like that. I guess behind my office was Bob Sharp's office, and then all the people doing paleontology up there. Sam Epstein had just been made an associate professor. Patterson had brought his postdoctoral fellows. Lowenstam was already full professor at Chicago and was brought here as a professor.

I had no lab. I was used to being around paleontologists, but it was a funny place. And the question was, What was I going to teach and do? I remember being grilled by Lee Silver. I was sitting on a bench in one of the labs. He wanted to make sure I really knew enough mineralogy and petrology so I could teach a course. I managed without any effort to convince him that I knew goddamn well a hell of a lot about the business. So he backed off a little bit. He and I were therefore antagonists for forty years. There was a guy named [James A.] Noble, who was a professor of economic geology, who did not talk to me, to my knowledge, the whole time that he was a member of this faculty. Hugh Taylor [Sharp Professor of Geology] was always a great admirer of his, because he did, quote, "real geology" or "real economic geology," or something like that.

So what I did then was to try and finish up some of the papers I had done in Chicago, and particularly stuff on the potassium-argon dating of sediments—with competition from this character out at Berkeley. I wrote a major paper on that, which is really the rest of my thesis, which was published in '55. ["Ar^{40}-K^{40} dating." *Geochim. Cosmochim. Acta* **7**, 51-60.] In fact, Patterson's paper on the age of the Earth in which he incorporated the argon data I got with Dick Hayden was published about then ["Age of meteorites and the earth," *Geochim. Cosmochim. Acta* **10**, 230-237 (1956)]. Patterson had never written anything much, except a paper on the isotopic composition of lead in stone meteorites [*Geochim. Cosmochim. Acta* **7**:3, 151-153 (1955)], a paper that he and I had a hell of a fight about. I don't know if I told you about that.

He had the data. And I said, "Well, you have to calculate this up from that." He said, "No." I said, "The number is going to sit by itself, and I will not do anything with it. That doesn't make any goddamn sense." And he said, "I won't do that." And that's how the paper was published. It was only later that he actually did the calculation. It was really funny. He wanted the number to be pure by itself.

In any event, I had problems as to what to teach. So Sharp took me aside, and I was sent out to Tick Canyon, to see whether I could teach part of the field geology series. So it was decided that I was to teach mineralogy, which I did for many years. And then I tried to teach a new and innovative course concerned with modern developments in spectroscopy, solid state physics, phase transitions, stuff like that—rather than the standard stuff. It was around that time, I think, that I was interrogated by my fellow junior professor, Lee Silver.

VALONE: Did you have a sense that the geology division was moving in a new direction?

WASSERBURG: Oh, yes, that was clear. When I was hired, the people who were hired the same year were Clarence Allen [professor of geology and geophysics, emeritus], Lee Silver, and Frank Press [director, Seismological Laboratory, 1957-1965]. Then there was Murray Gell-Mann [R. A. Millikan Professor of Theoretical Physics, emeritus]. So out of that group, Caltech did not do badly.

I'll try and give you some framework for where I sat. I sat up on the third floor of North Mudd, surrounded by the paleontologists. Where my labs would be was not clear. The basement was the property of Harrison Brown. He ran this great big empire, and most of the people who were there were just postdocs and staff. He had an electronics engineer and mechanics and everything else. The rest of the division did not have, in simple English, a fucking thing. It was a vacuum. There was no capability for modern science except for one place, and that was the Seismological Laboratory, which was off campus and was inherited from the Carnegie Institution of Washington. It had its own engineering capabilities, machine shops, and electronic shops. And the geology department, except for what Harrison had in the basement, was basically isolated and separate from any modern science. It was not a place with infrastructure—and in fact it still doesn't have it.

Now, you have to look at the list of faculty there, one of whom just recently died—Al [Albert E. J.] Engel, who was responsible for the revolution. And Bob Sharp was a key leader in the revolution. It was Bob who really decided it was time to do something different. And with the support of Linus Pauling [then chairman of the Division of Chemistry and Chemical

Engineering] and Lee DuBridge [then president of Caltech], he brought Harrison Brown here and set this new direction, which had started at Chicago and was obviously something that was really a comer. So they transplanted it, and they did a very successful job. Caltech was the center of the world for many years in that field, and Chicago was in the junior position—which has never been accepted very well by Chicago and has left very strong feelings.

So I had two problems: What was I going to do? I didn't have anything; I had to get money. I think I had $15,000 or something like that. And I had no access to anything. And the obvious thing for me to do was rare gases, since I had started out doing that. So I was dicking around, writing up what was left over from my dissertation for another paper. I also had done some statistical mechanics and thermodynamics.

I have to tell this story, because it's an important one. While I was waiting to do something, in 1957 I published a paper. It had to do with the role of water and silicate melts ["The effects of H_2O in silicate systems." *J. Geol.* **65,** 15-23]. And to this day—forgiving me the factor-of-two mistake I made, which I caught, and nobody else caught, thirty years later—that really was the basic description of the problem. In fact, in its own right it was a very, very distinguished piece of work. It didn't go by the normal cookbook stuff that had been done, although I used a lot of some of the classic stuff that some very fine scientists had used, and it explained one heck of a lot: Why water affects the melting point in silicate systems; how many atoms there are in a [words unclear]; the entropic contributions; and how much water was as H_2O molecules and as OH groups in the silicate melt.

The significance of this work can be seen in the following way: While it was rejected, it's now, I think, recognized as true, with various refinements, some of which were established by people elsewhere. And then later it was confirmed by [Caltech professor of geology Edward] Stolper that you could see the H_2O and the OH lines and see the difference. And, in fact, I had a theory, published in 1957, for which some other people assumed a great deal of credit, where one can even look at the theoretical numbers, which I published much later, showing that there was a very good approximation to all the observed data. Then, that was applied to the kinetics of water transport silicate melts in a series of papers with a young man by the name of [Youxue] Zhang, who worked with Stolper and me, based on the theoretical treatment, which I'd given as the general diffusion theory published much more recently—because I didn't do any more of that. Zhang won the F. W. Clarke Award, the leading prize in geochemistry for young men, two years ago, and he was kind enough to write me a thank-you note. So that 1957 paper was the cornerstone paper and was really a fundamental paper, really opened up things. It sat for a long time, because people were unable to understand it, but it was clearly the right thing. I wrote it in ’55, ’56, when I came here, because I didn’t have a lab.

So I had to find a lab and set things up and get some money. My wife and I first lived in an apartment way up on North Lake Avenue. And then, I guess, we finally bought a house after a year or so. Her folks gave us $5,000 to help make the down payment. It was an expensive house; it cost $17,500.

The one thing people miss in a lot of this is the major revolution that happened at American universities. Before that, most of the people at universities were really pretty well-to-do people—children of people who had money, or something like that. But I was a product of the GI Bill, and I had what I made. And it wasn’t expected that wives would work. So now you had a generation of scholars and intellects who basically earned a living and were not necessarily from landed gentry or wealthy families. There’s some substantial difference, and kids don’t seem to recognize that.

So there was the problem: Where was I going to work? Well, there was Patterson down there, mucking about. And he and I were good friends. Sam [Epstein] was doing his own thing. They were just getting the lab going in some fashion. And there was Chuck McKinney, an electronics engineer who used to work on the betatron in Chicago. And of all those people, the only person with any technical competence in instruments was Chuck McKinney. Clair Patterson couldn’t build a box. He did elaborate and complicated chemistry, and was incredibly good at it, but he hated hardware. In fact, to this day he gives speeches against engineers—he always hated them, said they were anti-science. And there was this machine, which was a copy of Mark Inghram’s machine, made by Chuck McKinney, and not well made. There were copies of machines from Chicago that had been made for Sam, and that’s about all there was. There was a wonderful gentleman by the name of Curtis Bauman, who was a machinist who worked in CES [Central Engineering Services]. And there was Victor Nenow, who’s a genius, who’s still here, who’s an electronics engineer—but he’s not really an engineer, he’s just a genius, and much loved by my sons, my wife, and by me, and respected enormously.

I wanted to build a mass spectrometer. And the question was: Where would I build it, and where would I get the resources? That was very difficult.

So I got some money from the institute, and I got some support. But the only way I could do anything was to go through Chuck McKinney. I could not get two flanges cut and bolted together. I could not deal with the machinists or the electronics branch, except for this one person, who was essentially the czar of the whole electronics-mechanical capability of all of Harrison Brown's empire—which is about the size of my empire right now. If you just calculate the dollar equivalent, it was a big show. And Harrison was never in the lab; he was giving speeches and shining mirrors. A very brilliant guy, no question about it—worried about world peace, which was probably more important than what I did, but that was the game he was playing.

Then I wanted to get access to the thermal-ionization instrument, which I'd also used in Chicago. I could only come in at odd hours, and that wasn't my turf—I was in the gas business. So I heard about the work that [Masatake] Honda had done at La Jolla with Jim Arnold, looking for cosmic ray interactions with iron meteorites. And I said, "Well, I can do the potassium-40 on that." So I made arrangements to write a note with him, trying to discover K^{40} produced by cosmic rays. And I finally got on the goddamn spectrometer there and made the measurements so I could get some action and some play and do some science that was interesting. I had very little access to the machine; I was given time only late at night. I was not part of the empire—and with damn little support. Finally, my nose was quite out of joint. I don't remember the details anymore—I can't remember, which is probably a good thing. I went moaning to Bob Sharp and said, "This is intolerable."

Begin Tape 4, Side2

WASSERBURG: They decided I was going to be too disruptive downstairs in the center of "Brownian Motion," as it was called. So they decided to get rid of me and take me out of the basement. I was being too disruptive, because I wanted space, I wanted access to equipment, I wanted to build something, and I wanted to go to work. And I undoubtedly had several other job offers.

Finally, they moved me over to Arms. There were rooms with nothing in them, and benches, and old zinc-plate-covered chem labs. I was supposed to get that up and going, which I did. I tried to build a mass spectrometer, but I could get nothing out of the shops. Nothing! McKinney would not authorize anything in the shops. I remember walking into CES and saying, "I'd like to get these flanges machined." The answer basically was, "Who in God's name are you?" They were building stuff for the synchrotron; they were building stuff for the telescope. They were building big, fancy stuff. And here's this little nothing who wants something. I was used to working at the Institute for Nuclear Studies and at the Argonne National Laboratory. Bill Libby had said, "You know, I'm going to set you up in a big government laboratory, so you can really carry this stuff forward." And that was a big advantage for me. I could make designs, talk to a machinist, get something built, and go do science. And now here I was at a place where there was nothing! And I would plead for help from the Seismo Lab, whose director [1946-1957] was a very fine gentleman, Beno Gutenberg—a great man. I got along very well with him. I got along well with Frank [Press]. But that was really their turf and it was only by special dispensation. There was nothing available, not even a real shop facility that you could use. And I had to cut steel, make an ultrahigh vacuum system, and design stuff.

The explosion came—and it's a little bit out of time sequence—when McKinney went off on a vacation. And I sat up and I drew the working drawings for frames, mounts, and part of the spectrometer and walked them over to the shop, and said, "Build them." And they did. And he went nonlinear.

VALONE: When McKinney got back from vacation?

WASSERBURG: Yes. I mean, who was I to go around him and do this? I got ulcers during this period of time. It was very difficult. In the first place, the situation was that all the other people were scared witless that McKinney would walk out the door, because they couldn't build a goddamn thing without McKinney. Well, I could build any frigging thing I wanted without McKinney. I just wanted to go to work! And that was really the source of great stress between Lee Silver and me, because around that time Silver was publishing nothing; and McKinney was a senior research fellow, or something like that, who had published nothing, had done nothing except build some machines which were copies of those Mark Inghram built in Chicago. And they weren't very good copies. They were not as good as the machines I had worked on that Inghram, Hayden, and Hess had built a long time before. And these two were basically in charge. And I, as a young person who came in, was not given any open playing field at all. On the other hand, there was some recognition that I had substantial promise and somehow they were going to find some way to allow me to go ahead. But it was a very difficult time.

Silver was not publishing any papers. And we're all up on the tenure-track business. And he and McKinney began to feed on each other. They began to huddle together—because I was supposedly trying to take over. Well, all I wanted to do was go to work. I wanted access to Curt Bauman and Vic Nenow and I wanted to go to work. And if I couldn't go to work, I wanted to go someplace else. So it was very, very difficult.

Then, when I built the Lunatic I [a mass spectrometer for making high-precision measurements of lunar samples obtained by the Apollo missions—ed.], Sharp finally had to intervene, and he said, "Well, yes, I guess you can go to the shop." But it was with incredible pain—it was not, "Go, baby, go."

Wasserburg with Lunatic I, the first fully digital mass spectrometer with computer-controlled magnetic field scanning and rapid switching. Donated to the National Museum of American History in 2009. (Photo taken in Wasserburg's lab at Caltech in 1978.)

There were a bunch of forces at work, dominated by the fact that nobody else wanted to build anything. Sam [Epstein] could build vacuum systems, that was clear, but nobody else could actually build real steel hardware, and didn't want to. They just wanted the numbers. And I was disrupting the system, because their favorite builder, however good he was or was not, was going to be upset, and therefore they would lose this wonderful guy who's building them this junk. So they were trying to keep McKinney happy and keep Wasserburg at Caltech.

VALONE: Was this over the first three or four years you were here?

WASSERBURG: At least five to seven years. It lasted a long time. McKinney finally had to leave [1961], because he never did anything, and he wasn't very good at what he did. He just did what the other people didn't want to do and did it better than they did. It happens all over. Here was a guy on the support staff who just had authority far beyond what he ought to have.

VALONE: It sounds as though you were, more or less, lucky that you had a large backlog of stuff you'd done at Chicago that you could continue to work on while you were trying to work out all these practical problems.

WASSERBURG: Oh, yes. I developed ulcers. I remember sitting at the mass spectrometer late at night with incredible pain in my back and I didn't know what it was. It was very stressful. And while there was support, there was not support to really go ahead.

I want to back up and tell a story. Remember, the history was that there was a whole bunch of geochemistry done in geology, which I think I talked about before, where a chemist would be hired as a bench chemist to provide the master thinker with the data. And that was his only function, almost exclusively. Except for what was done at the Geophysical Laboratory of the Carnegie Institution of Washington. You had none of this here, except in geophysics, which was the Seismo Lab, which was a separate setup, following the Carnegie structure. The rest was a bunch of people who were geologists—and I am also a geologist— who were locked into what they did. When you asked for equipment, somebody said, "Well, that kind of money would support me in the field for twenty years." And that attitude has persisted to this day. The ability to design and construct equipment was very, very limited, and it was very, very difficult.

As far as my teaching went, I taught mineralogy. And the question was, what to do that was new and different. Clearly the students were not trained in any basic physics or mathematics. So when I first came, I taught a course called Geo Math, in order to tutor the students to make sure they understood basic calculus, basic matrix mechanics, strain theory, Eigenvalues, series—all the stuff you would think anybody had in a sensible education. What I was used to, they did not have—how to solve simple differential equations, and so on. And that's what I taught for several years, and then I finally stopped teaching it. It was a tough course, but it was a good course. It should have been published as a book—but I don't like books. And many people who were my TAs were people who are now professors of geophysics all over the place. None of the geologists could be TAs in the course, because they couldn't do it, but the guys in geophysics could.

When I first came here, I went to Bob [Robert F.] Bacher, who was then chairman of the Physics, Mathematics, and Astronomy Division, and I asked for a joint appointment in physics. He said, "We don't do that here." I said, "Why? A good deal of my training is in physics. I want to be associated with people doing physics." And he said, "Well, you can do what you want, but—." Bob and I ended up very, very good friends for many years. But that was a wall that was carefully built and not to be crossed. I wanted to bridge the fields again, but here it was rather difficult.

In the Geological Sciences Division, geophysics was seismology, and then there was geology. Geochemistry never had a curriculum—as it does not today. It's still basically treated as the handmaiden of geology. The members of the geology faculty had more to say about what constituted geochemistry than anybody else.

That system has basically stayed invariant. That is, to this day, when I teach a course—if anybody deigns to come—most of the students from geology don't take it, because it's too hard and they bloody well are going to have to solve these problems. And the techniques are no different than those I lectured on in the course called Geo Math forty years ago. People in planetary sciences will take it and do fine. That's what they have for breakfast. So this schism that I describe in 1955 is the same schism that exists today. The fact that you have to teach a course in basic and elementary mathematics and physics and problem-solving ability to graduate students at the California Institute of Technology in geological sciences—and it still has to be done—shows you what the balance is.

Then I came up with the idea that we ought to change the curriculum in general and come up with something different, in instruction in petrology. So I designed a three-tiered course. It was called Geology 211a, b, and c. The first quarter was igneous petrology, the second was metamorphic, and the third was structural. Each quarter was taught by a pair of professors—one of the older guys, who did the classical stuff, and one of the new people, who would try and bring in the new stuff. Dick [Richard H.] Jahns [professor of geology, 1949-1960] and I taught igneous petrology. I did thermodynamics. We went into the field with classes. I went to work in Wyoming, looking at deformation stuff. Wrecked a field vehicle, which got to be known as the Wasserburg Memorial Carryall, because the top had dropped in six inches as I rolled it. Worked all the labs of the classes, did the regular petrology and petrography, and also did all the theory. The problems then were as good as they are today. There was a classic problem I gave, called the Adiabatic Elevator: What happens if you take a parcel of matter deep in the Earth, raise it up through the adiabatic elevator of the Earth's mantle, and what are the consequences for phase changes, melting, and so forth, heat flow on the surface of the Earth? All that stuff was done in the course: diffusion theory, phase diagrams from a first-order thermodynamic point of view, and the comparison with the observed phase diagrams and theory. It was an excellent course—the prototype for all the courses that have happened in the world since then. Though I never wrote a book on it. Now there are lots of books like that. I'm not sorry that I did not write books; I wanted to do science.

But those were the three courses. Sam Epstein and Al Engel taught the metamorphic petrology course. And the third quarter was taught by Barclay Kamb [Rawn Professor of Geology and Geophysics] and Clarence Allen. So there was a real binding between the old guys and the new guys—you had the new guys out in the field looking for rocks with the old guys, plus the students. That was a major revolution in how things were taught—an example of what geology might be in a broader, more modern sense.

One of the first students in the class was H. [Hugh] P. Taylor—a professor here—and he got an A+. He could see mistakes I made in the exam problems I gave. He was a brilliant student, and he has taught that course since, so he obviously learned it well.

I served on Barclay Kamb's doctorate exam [1956]. And his thesis sponsor was Linus [Pauling]. Barclay was brilliant. On the exam, Linus was impossible. If I asked Barclay some difficult question, and Barclay couldn't answer it, Linus interrupted and told me the goddamn answer. I finally turned to him and I said, "Linus, this is not your examination; this is the candidate's exam."

The story about Barclay was that the best thing in the world for him would be to get out of here and go someplace else, away from Linus, away from everybody else. So when he was offered a job at Chicago, people here began to pull their hair out, including Bob Sharp, who lost his marbles. I said, "Let him go. Three years later, you can hire him back. But just because he's the smartest guy you ever saw in your life, he's got to go someplace and grow up." But he was conned into staying here, which I think has done him a perpetual injury, in terms of his own scientific creativity and maturity. He took all of his degrees at Caltech; his thesis sponsor was at Caltech; he's married to Linus Pauling's daughter. And he was never out in the real world. If he'd gone away as a young person, I think he would have flourished, because he's absolutely brilliant. His brilliance is not at issue at all.

VALONE: You slipped in that you thought Bob Sharp had started to lose his marbles.

WASSERBURG: Well, you see, Bob would have pets and favorites. I should say instead that he showed very poor judgment in that sort of thing. For instance, he liked dear old Lee [Silver], and stuff like that. Sharp is a wonderfully manipulative and imaginative guy, and he's done a lot of great things for this institute. But in that particular case, he was afraid of losing his favorite child. And he's persisted in that until this day. That is, when he gets up and makes a speech, he has to mention some of these people—who in fact have not truly dignified themselves by any superb acts of creativity or anything—because they were his favorite children. But I don't think that's very smart. And in fact, some of these people, if allowed to go away, might have come back a hell of a lot better for having matured.

The remarkable thing about Bob is what he accomplished with his own background. He's a very classical person who understood none of the theory whatsoever—who went ahead and said, "This is imaginative, we should do it." And then backed it and somehow got it to go, and found some pretty talented people. But as far as his own background was concerned, none of this was his religion, you understand.

And Bob had great battles; he had to fight with a lot of the old people who were here. I should go through the story about how we got rid of our paleontology collection. There was blood on the floor one meter deep.

So I got sent upstairs. I finished the machines and built the [name unclear] upstairs, and got things going, and started to do a series of studies on rare gases in nature. And there were a whole bunch of papers.

One of my first students was Bob [Robert Eugene] Zartman, who wrote a paper with me. John Reynolds let me use his spectrometer at Berkeley while mine was still under construction, and he was a joint author on the paper—a classic paper on the origin of helium wells and natural gases, which, as far as I know, nobody's ever done any better today ["Helium, argon, and carbon in some natural gases" *J. Geophys. Res.*, **66**:1, 277-306 (1961)]. And then we studied what might be pristine gases coming outside the Earth. And that led to a later paper, which was an outgrowth of the work of Bob Zartman's, of a key sample: the paper by [W. A.] Butler, [P. M.] Jeffery, Reynolds, and Wasserburg on the discovery of xenon-129 excesses in the Earth ["Isotopic variations in terrestrial xenon," *J. Geophys. Res.* **68**:10, 3282-3291 (1963)]. And that was based on the original observations of no atmospheric contamination. This gas with Bob Zartman, had done, and then opened up the discovery of short-lived nuclides in the Earth unambiguously, which was not a bad piece of work—for which, of course, the guys at Berkeley should get substantial credit for finally doing the analyses.

So then I did fission products, xenon, and a whole bunch of stuff. And it was very hard to find students, because gases are not rocks, and people want to study rocks. And they did not want the discipline of ultrahigh vacuum work. So I never had many students, but the ones I've had are— The list would more than speak for itself.

So I pursued that work. Plus the dating work, developing the dating techniques. I should tell a story about George Wetherill.

Wetherill went to DTM [Department of Terrestrial Magnetism, Carnegie Institution of Washington] after he left Chicago, and he and [George] Tilton were there. Tilton was working on zircon ages, uranium-thorium-lead ages. Wetherill was doing some rare-gas work, and the original serious effort to do rubidium-strontium dating was done with Tom Aldrich at DTM. And they tried to get DTM to hire me, but DTM figured it had enough people from Chicago. And they were kind enough to show me how they were doing rubidium-strontium dating, because the first serious effort really made was by [Eugene M.] Shoemaker at Chicago, who was a postdoc with Urey.

So I went to DTM and I learned how they fixed their ion sources. And I came back to try to do that here at Caltech. And they were very kind and very helpful, and I never forgot that— And Tom Aldrich particularly.

When I started to do the rubidium-strontium work here, Lee Silver and I were carpooling together. We were driving up Hill or Allen one day and he said, "Jerry, I understand you're thinking about doing rubidium-strontium." I said, "Yes." He said, "You have the gases; I have the solids."

I didn't know what the fuck this was about. I have lived with that for forty years now at this place, and I have hated that. I have had two division chairmen come in and tell me that. And I will not tell you what obscenities occur to me now, and at the time they walked in, after the shock passed. He was going to own this and he was going to own that, as if you could own the periodic table. I didn't even understand what the subject of the discussion was. I mean, I do what I think is interesting. And I don't steal something from somebody else. You can't steal an element—it's part of nature. I said, "What do you mean?"

"Well, I can [words unclear] the machines down here."

So I said, "To hell with that, Bud! I'll do what I want to do and what I think is interesting. I don't think you own the elements; I don't think I own the elements. I'm doing it, and I'm going to do it."

Life was not very easy then. I couldn't even imagine some of these attitudes. I can't imagine, in a physics department, you and I together driving in a car, and you telling me you own pions, or something like that. I heard very commonly in this division, "That's my element," or "That's my isotope." I've had Bob Sharp come in and tell me I should not do lead because lead belonged to Clair Patterson. I've had Gene Shoemaker [geology division chairman 1969-1972] come in and tell me I couldn't do lead and uranium because that belonged to Lee Silver. I do not understand. I have never heard such crap in my life. I have made most of the major contributions in several of these fields. I don't understand what these people are talking about. This is what I call "the outcrop and the quadrangle"—"I am mapping this quadrangle, and you are mapping that quadrangle." I didn't have any quadrangle! I was just trying to do science. I cannot imagine somebody telling me I should not do an element because it's going to injure somebody else. So I've got very bad feelings about all that kind of stuff—deeply bitter and resentful—and I'm very happy that I managed to do it successfully, because I had fun when I did science.

I remember Lee Silver meeting George Wetherill once in La Plata County, Colorado. And he told Wetherill that he [Wetherill] didn't belong there, because that was "Silver County" and he was Lee Silver. And I remember Lee Silver trying to chase me off outcrops in the valleys out in the Mojave, when Marvin Lanphere was my student. That was his area, and Bob Sharp backed him on this. So there was this persistent turf business, which has stayed in the field for a long time, and it was very strong here and did not go away rapidly. And I'm not sure it's gone away now.

Then I started to do rubidium-strontium work and the argon work in the field. And all these were fights with Lee Silver—because he owned Texas, he owned New Mexico, he owned Arizona. I didn't understand any of this, I really didn't. I'd just get in the pickup truck and go someplace with a student or a postdoc and start to collect some rocks and find out what was in the literature, and measure this, and do the separations. That was all his. So that conflict has gone on now for thirty-eight years, and I suppose one of us will take it to the grave with him. It's not healthy and it's bad news. And for a guy that smart and that unproductive, I don't get it. And he was, in quotes, a "real geologist"— as opposed to a geochemist who just incidentally happened to be an expert in geology.

So then I got the rubidium-strontium set up and did the calibrations. Wetherill was very helpful in that. I started to do that work, and then started to do lower-level work and improve the techniques a great deal and try to apply and compare the methods. I had the rare-gas lab going; I was looking at natural gases.

There's a basic problem: People want to date rocks, but nobody wanted to study processes; there are very few students who are going to do that. So Lanphere felt that part of his dissertation, half of it, had to be mapping a quadrangle, because otherwise he would not be a real geologist. And then the other half would be ["II. Geochronologic studies in the Death Valley–Mojave region, California" (1962)].

I made a movie at that time, with Dick [Richard P.] Feynman, called *About Time*; it was part of the Bell Labs Science Series, or something like that. It was a hell of a lot of fun. Dick was a riot. I was living up in Altadena in some old house, and a chauffeured limousine would pick me up and the family would stand in the door to see me, the old man, get in the car to be driven to Hollywood. Used to sit in Jack Warner's office and argue. It was really interesting—met the stars and saw all the fakery and wonderful stuff.

VALONE: How did you get involved in that project?

WASSERBURG: I don't know. They wanted to make a movie about time, for public television. Dick was going to talk about relativistic time, and I seemed to know a good deal about geological time. So I talked about the age of the solar system and rocks.

They reconstructed my office on the set, which was incredible. Some guys visited me at

Caltech. Lanphere, my student, was sitting in front of the mass spectrometer; I was in back of it, trying to wire something in the back of the chassis. So they came in and turned to Marvin and said, "Professor Wasserburg?" and Marvin said, "No." And I said, "Who wants me?" To them I was just some guy with a screwdriver. They said they wanted to do something with me, and some guy came in and began making sketches of my office and my lab. I said, "Don't you want to take pictures?" So I went down to Warner Brothers, and they had built a replica. It was a wonderful thing to see. I walked in, and I had a chart of nuclides hanging in my office, and they had a chart of nuclides. The whole thing: books, pencils, rocks. Very clever people; they were extremely perceptive. I had a lot of fun with that.

I do want to say that one difference between geology as a science and physics or chemistry or mathematics is that physics or chemistry or mathematics imagines itself to have saints—special people who are pretty high up on a pedestal and have provided closer and closer visions of whatever the truth might be. I don't think that's true in geology. I think that geological figures—historical figures—are just used as examples. I think that there are no really great people in the minds of the profession, in that almost everybody is more or less equal, except for minor advantages. The general notion is, "Well, I could have done that, too." The sense of priorities is not the same. In some fields, if you make a discovery, it's recognized as *a* discovery. And that then is some kind of advantage, recognized for a good or bad reason. I don't think that's the case in geology; we're all, more or less, the same. And I think that's the governing thing: There are no intellectual saints in the profession, and that's not true in most of the other sciences.