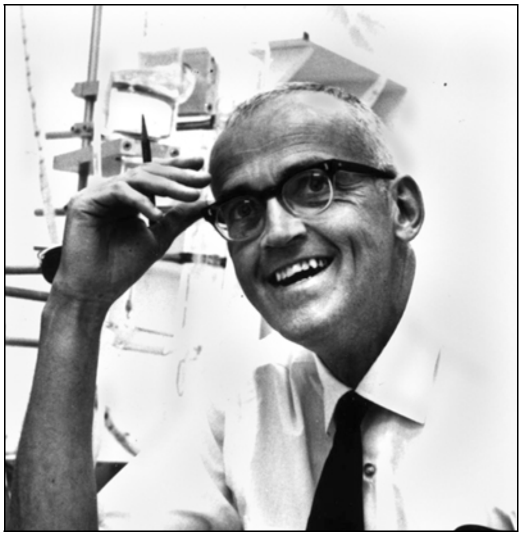

Clair C. Patterson
(1922-1995)

INTERVIEWED BY
SHIRLEY K. COHEN

March 5, 6 and 9, 1995

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CALIFORNIA INSTITUTE OF TECHNOLOGY
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Subject area
Geology, geochemistry

Abstract
In this interview in March 1995, nine months before his death, Clair C. (Pat) Patterson, professor of geochemistry, emeritus, talks about his early interest in physical chemistry; his education at Grinnell College, in Iowa; his stint on the Manhattan Project at Oak Ridge; and his subsequent graduate work at the University of Chicago with Harrison Brown, where he measured the isotopic composition and concentration of minute quantities of lead with a mass spectrometer. He received his PhD at Chicago in 1951. After a year there as a postdoc, he came to Caltech with Brown, who established a geochemistry program in the Division of Geology. By 1953, having measured the isotopic composition of primordial lead in iron meteorites, Patterson was able to determine the age of the earth at 4.5 billion years. He then turned to a study of the natural levels of terrestrial lead and discovered that in the modern industrial environment, lead concentrations had greatly increased, from such sources as leaded gasoline and the solder used in food cans—with a corresponding increase in lead levels in human beings. He discusses his investigation of lead levels in seawater, oceanic sediments, and polar ice cores and his calculation of the rise in environmental lead levels beginning with the mining of lead in Greek and Roman times. At the end of the interview, he discusses his current interest in the evolution of different

neuronal networks for two kinds of thinking, utilitarian and nonutilitarian—and his belief that this is illustrated by similarities in utilitarian thinking in the Old and New Worlds, while their cultural (nonutilitarian) development was dissimilar.

Administrative information

Access

The interview is unrestricted.

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Clair Patterson in the laboratory, circa 1952, the year he came to Caltech. His breakthrough article on the age of the earth—4.5 billion years—was published shortly thereafter (C. Patterson, “The Isotopic Composition of Meteoric, Basaltic and Oceanic Leads, and the Age of the Earth,” *Report by the Subcommittee on Nuclear Processes in Geological Settings*, National Academy of Sciences, 1953).

California Institute of Technology

Oral History Project

Interview with Clair C. Patterson

by Shirley K. Cohen
Pasadena, California

Caltech Archives, 1997

CLAIR C. PATTERSON

ORAL HISTORY PROJECT

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Clair C. Patterson by Shirley K. Cohen
Pasadena, California

Session 1	March 5, 1995
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Session 3	March 9, 1995

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Cohen: I'd like to start this interview with you telling us just a little bit about your background—where you were born, a bit about your parents, a bit about your growing up.

Patterson: I was born in a small town in the middle of Iowa that was located in the midst of farmland—rolling prairie-type farmland—in central Iowa. There was a small school. Boyhood in this little town was sort of centered at that school. And all the students knew each other for twelve years. It was sort of a tribal interaction.

Cohen: So you were in one school the whole time.

Patterson: One school the whole time. People didn't move in and out. The school had 100 students—the sum total for all the grades. [Laughter] So there was a close personal interaction throughout that time.

Now, as I grew up, we spent a lot of time learning things about the world that most youngsters in cities don't learn these days.

Cohen: Were you all from farm families?

Patterson: The farms were around us, and some of the students were from farms. We took time to participate occasionally in farm life. We saw crops being planted; we knew how they were being planted, and we saw how they were harvested. We knew about farm animals: We saw farm animals procreate; we saw them being butchered; we saw them being fed. We saw cows being milked. So we were aware of the farming activities.

Furthermore, we were in an area of a river, woodlands. And on weekends some of us—two or three of the boys—would go to the river bottom and stay overnight over the weekend. Our mothers would give us sandwiches or something. It was about four miles away. We'd learn how to swim and fish. We boys were by ourselves.

Cohen: How old were you then?

Patterson: We were between eight and twelve years old.

Cohen: So you were expected to have good sense at this early age?

Patterson: Yes. We took care of ourselves. We stayed there. We built fires and cooked the fish that we caught. There were no adults with us at all. Doing that, we learned about plants; we learned about animals. Also, we learned how to hunt. Our parents gave us shotguns—a little tiny thing called a .410. It's a small shotgun that wouldn't carry very far and wouldn't hurt very much. So we hunted rabbits and squirrels; we learned how to use weapons and shoot animals.

I learned that at the river bottom—this was called the Skunk River, by the way [laughter]—there were what we called the Indian mounds. These were old burial grounds. And I collected arrowheads and various things from those mounds. Also, there were animals that would be dead or killed. I would recover the bones and take them back to my home, and then I'd reassemble the bones so I could see which type of animal that was.

Cohen: Is there a reason why you would have had this scientific feeling about this? What sort of thing did your father do? Or your mother?

Patterson: My father was a rural mail carrier. Both my parents were college-educated people. My mother was a member of the school board, and she saw to it, along with the superintendent, that I got educated. First she got me a chemistry set. I remember when I was a little, tiny kid, she told me, "Well, Clair, when you were very small, you asked me, 'Why is a drop of water round?'" [Laughter]

Cohen: So it was already there.

Patterson: [Laughter] So we started on that stuff. And then when I got to seventh or eighth grade, they started getting chemicals for me. And we had a basement, where I built a bench and some shelves. There was a sink down there. I built myself a little home laboratory. Of course, the school didn't have this sort of stuff.

Then, when I was in ninth grade, I had an uncle who gave me his chemistry laboratory workbook from the chemistry course he'd taken in college. So from then on, I taught myself chemistry in my basement.

Cohen: Did you share any of this with these friends of yours?

Patterson: Well, they weren't interested, really. The only way we shared it would be when I'd come back to school and the teacher would say atrocious things that were totally wrong. [Laughter] And then I would get up and give a little explanation of how it really was. But my colleagues didn't care. You see, we were like cousins. Each person had certain characteristics, and we accommodated ourselves within our tribe to those various characteristics. Mine was that I would get up and explain how things really work. That was my job. They didn't resent this; it was all part of the whole deal. [Laughter] The science teacher would say something about electricity being a fluid, and I had to explain to them about electrons. You see, I learned the periodic table; I learned qualitative inorganic analysis and all this sort of stuff. I taught it to myself. And the school procured the chemicals that I could use for all those purposes.

Cohen: So you were really encouraged—at home, and by the fact that your friends listened to you.

Patterson: That's right. I would say that the major thing in this whole process was there was no retribution for being outspoken or a dissident—if there was quality in what you were doing. I mean, there had to be a demonstrated reason behind what you did that showed there was some worth to it. You couldn't just be quarrelsome or negative. So my parents always allowed me to go off in any wild direction I wanted to go, provided it had a sound basis—if it could be demonstrated to be a worthy thing. It didn't have to be acceptable, because it would be different. I was always different from most youth. But that's crucial.

Cohen: It sounds like you had wonderful parents.

Patterson: Well, I would say the situation was such that they could not have done it that way in a city. It was the social context also. So that kind of social context, I believe, is crucial for establishing at an early age the awareness that creativity is not to be trampled just because it's divergent from ordinary views. And you can do that in this small type of environment and population. That really is very, very crucial.

Cohen: Did you have any brothers or sisters?

Patterson: Oh, yes. I had a brother and a sister. Our brother was a champion athlete.

Cohen: He wasn’t interested in a chemistry set.

Patterson: No way! [Laughter] He was a champion basketball star. And my sister was in all sorts of girl stuff.

Cohen: So there was never any question about your going on to college when you finished.

Patterson: Oh, no. That was accepted. It was taken for granted. Among my schoolmates, not many of them went to college. But going to college wasn’t considered a weird thing.

Cohen: Do you remember what year you graduated from high school?

Patterson: In ’39—I was born in ’22. I was only sixteen; my birthday occurred after I graduated.

I went to Grinnell College, a very small, excellent college, also in Iowa. There the faculty treated us just like they were parents. There was a close interaction between the faculty and the students.

Cohen: How big a school was it?

Patterson: It was 800 students, all four years.

Cohen: How far was this from home?

Patterson: I would hitchhike home to do my laundry. [Laughter] It would take a half a day or day to hitchhike. I can’t remember; it was 150 miles or something like that. And I worked.

Cohen: Grinnell was a private school?

Patterson: Yes.

Cohen: A secular school?

Patterson: It was originally a Congregational school, I think, but it wasn’t a religious school. But we did have chapel. And religion was to be considered—it wasn’t to be forced on you. You sort of absorbed the idea that religion was part of your whole social structure and life, although it wasn’t necessary for you if you didn’t want to.

Now, my religious background was that my family belonged to what was called the Unitarian Universalist Church. It’s sort of a liberal-type Christian church. My grandfather founded the church in Mitchellville that we went to. On Sunday I’d get up real early and go on cold winter mornings to build a fire to warm up the church. [Laughter] My mother was a big wheel in that church. And the minister used to tell me a lot of things—philosophy and all that sort of stuff.

Anyway, when we got to Grinnell, the way that I was treated was, again, I could be a renegade. Not a communist type—it had to have some substance. For example, in chemistry, I loved the fact that you could go in the laboratory and work your heart out. You could do anything you wanted to. You could play all sorts of games.

Cohen: Was that your major subject?

Patterson: Yes. I spent a lot of time in chemistry. And then quite a bit of time in physics.

Cohen: Can you remember a professor that inspired you?

Patterson: Yes. The professor of chemistry, as a matter of fact, gave me a job cleaning out his church. Now, he was very conservative; he was a Christian Scientist. Can you imagine a professor of chemistry being a Christian Scientist? He was a very wonderful person.

Anyway, I had never drunk beer before; I had never smoked before. So when I got to college, I came across beer and I got drunk. I was only a freshman, only seventeen. Well, I drank too much beer, and I got inebriated one Saturday when I was supposed to be cleaning up the church, and I preached a sermon. And the professor found out about this—a drunken sermon! So then I wasn't able to work in his church; I had to shift jobs. [Laughter]

Cohen: But he was still good to you in the chemistry lab.

Patterson: Oh, he and I got along very well. In my third year, I blew up one end of the organic lab doing some experiments. You see, I would go beyond what I was supposed to be doing. And I was doing something with some diazo compounds. They were very fragile and unstable things. And I wanted to do some molecular chemistry and that sort of junk. Well, it blew up, and he and I had to clean up that mess. [Laughter] But he endured that. Oh, I loved physical chemistry, because I could do all sorts of things.

My wife, Laurie, and I met each other and became bonded in college.

Cohen: Did she start the same time as you did?

Patterson: Oh yes.

Cohen: And I know she was a chemistry student.

Patterson: Yes. She came from a little town on the west side of Des Moines; I was from a little town on the east side. Her mother's family had come from Mitchellville—my town—and then they had moved away when she got married. And her mother and my mother grew up together as little girls. But Laurie and I never knew each other then; we only met at Grinnell.

I helped Laurie. She would drop things in the laboratory and fumble around.

Cohen: Maybe she did it on purpose, so you'd help her.

Patterson: She resents very much for me to say this, but it nevertheless was true: that I was very good in the laboratory, but she got A's and I got the next grade down. We had honors, and superior, and basic, or some stupid thing like that—we didn't have A, B, C. Grinnell was a very outstanding-type school; they didn't do that sort of stuff. But she got honors, and I only got superiors, because I was the renegade, again. I wouldn't do the homework. I always wanted to do what I thought was the right thing to do, and I wasn't going to do what the rules said. So she got better grades than I did. But we got along very well in science. She took chemistry and physics together with me.

Cohen: We are getting toward the war years now, aren't we?

Patterson: Yes, this is right during the war years. At the end of our four years at Grinnell, the United States had gotten into World War II. I applied to graduate school to get a master's degree at the University of Iowa in physical chemistry. This was 1943 and '44. We got married when I was in graduate school.

I got a master's degree in nine months. And the war was getting pretty heavy. So I said, OK, I'm going to join the army. I guess that was when we invaded France, or something. I got very worried about this stuff and thought, "I've got to do my part." And Laurie said, OK, she would join the marines.

Then there was a chemistry professor at the University of Iowa who said, "Patterson, you've got to go to the University of Chicago and work on the atomic bomb."

Cohen: But he wouldn't have known about the atomic bomb then?

Patterson: Yes, because that's where he went.

Cohen: I thought it was such a big secret.

Patterson: Oh, it was only a secret to chimpanzees who didn't know what they were doing. He was going to the University of Chicago to work on the atomic bomb, and he wanted to take me along. I went back to my draft board and said, "I want to go in this army and get killed." And they said, "You've got to go. You can either go on the atomic bomb as a civilian or you can go on the atomic bomb in the army. If we draft you, you're going down there." So it was more or less that I didn't have too much say about it. So Laurie reneged on her enlistment in the marines. [Laughter]

And then we both went to the University of Chicago to work on the atomic bomb. We got married just before we left the University of Iowa.[1]

Cohen: Was Laurie getting a master's degree also?

Patterson: No. She was working to pay our expenses, I think.

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[1] Laurie Patterson writes: Pat and I left Iowa City to work on the Manhattan Project in 1944, soon after we were married and at the instigation of Dr. [George] Glockler, Pat's professor at the University of Iowa. In Chicago we lived in an apartment hotel across the street from the Museum of Science and Industry. We were unhappy in the city, doing work we thought would "let the genie out of the bottle" much too soon. In the late summer of 1944 we returned to Iowa for a weekend for Pat to enlist in the army. He had applied once before, during our senior year of college, but was rejected because of near-sightedness. Now however, the physical requirements had been lowered and he felt he would be accepted. I enlisted in the Waves. Three days later the draft board reported they could not draft Pat because of his high security rating and he must return to the University of Chicago. Fortunately I had not yet turned in all of the required papers and was not formally a Wave.

When we returned to Chicago we were asked to meet with the colonel in charge of Manhattan Project personnel at Fifth Army Headquarters. Pat felt he was the only young male on the streets of Chicago and was a "draft dodger." The colonel suggested that he send us to Oak Ridge, where there were many young people. He was remarkable for his empathy for two youngsters such as ourselves. So we went to Oak Ridge for the duration of the war, where we worked at the Tennessee Eastman electromagnetic separation plant.

Oh, she had a lot of jobs. She was working in a Davenport arsenal. She was working in a paper institute. And then finally she ended up working at the University of Iowa hospital laboratories just when we got married. Then we both went to the University of Chicago together and worked on the bomb in the lab —the Metallurgical [Laboratory, it was] called.

Cohen: Whose lab was this? Who was in charge there?

Patterson: The people? I'll talk about that later. Mark Fred was my boss there, and I don't know who Laurie's boss was.[2] You see, I had become a spectroscopist. I had done molecular research in molecular spectra back at the University of Iowa for my nine-months, whiz-bang master's degree. And while I was there I got into atomic spectra a little bit. So now at the University of Chicago I was doing atomic emissions spectroscopy. They were analyzing the various products of the uranium when it disintegrated.

After a bit, Laurie and I said, "Oh, we've got to go down to Oak Ridge, because this is where they're making the atomic fuel." So we went down to Oak Ridge, and that's where we spent another year and a half or two years, working at the uranium-235 electromagnetic separation plant.

At Oak Ridge I got into mass spectrometers. You see, the isotope of uranium that they wanted was uranium-235, which is what you made the nuclear bomb out of. But 99.9 percent of the original uranium was uranium-238, and you couldn't make a bomb out of that. But the little tiny bit that was U^{235} had a different mass, and you could separate them using a mass spectrometer.

Now a mass spectrometer is where you take the uranium, stick it in there and ionize it so it's got a charge; each little atom of uranium has a charge. You accelerate that sample through an electric field and get it moving. And then you put it through a

[2] Professor Burton from Notre Dame, according to L. Patterson.

magnetic field, and the magnetic field will bend the lighter isotope more than it will bend the heavier one. So it separates the two isotopes. They had little collection boxes where they collected them. So you could take a bunch of this stuff and put it in, and then when you got it out, you had the enriched 235 over in one box.

Cohen: It sounds simple, but I'm sure it's not as easy as all that.

Patterson: Oh, it was hideous! These mass spectrometers were about ten feet wide and twenty feet high and deep, with big magnets outside between them. They had those magnets arranged all around, like a football track. It was about the size of a track around a football field, too—a quarter of a mile. It was hideous, it was awful. It was like a mass spectrometer factory. The magnets were wound, by the way, with silver taken from the U.S. silver deposit, because silver transmitted the electric power easier and better than the copper. That all went back to the Treasury after the war.

Anyway, Laurie and I worked in separate laboratories, first analyzing the chemical purity of the U^{235} that was coming out. Then later I switched over to these little mass spectrometers that were used to determine the isotopic composition of the product. You see, they had to reprocess the uranium. They would collect and take out the U^{235}, and then they'd put it back and process it again. They'd do that two or three times until they got 99 percent U^{235} from a sample that was only a half a percent to begin with.

Cohen: Where was all that uranium coming from?

Patterson: From big uranium mines in Colorado. They would ship the uranium to Oak Ridge, and they would transfer it there to a chemical form that they could put into the mass spectrometer.

Cohen: Now, you were a civilian all this time.

Patterson: Yes, Laurie and I were civilians.

Cohen: Were you provided with housing?

Patterson: Yes. We had little houses. Oak Ridge was made up of these little military-operation-type houses. We had a little dog, and we went back and forth on a bus to work every day. The house was buried in the mountains. And then on weekends, when we had time off, we'd walk up into the mountains behind, and we would see the people who had been living there for 200 years. I mean, you'd call them hillbillies. But, you know, their annual income—before the Tennessee Valley Authority and the Oak Ridge Atomic Energy Commission business—was 200 or 300 dollars per year. There would be a little wagon, or a truck wagon, that would go through the bottom of each valley, winding its way through on a little road. The houses were up on the sides. And the women would come down and buy things each week from this wagon. So they never got out of there very much. I never looked into the local economy—what they grew or how they lived. But we went up and down two or three valleys. These people didn't like these city slickers coming through there, but we did it anyway.

Cohen: Is this when you met Harrison Brown?

Patterson: No. He was there, but I didn't meet him until after the war. He had done his original diffusion work back at Columbia University, and there was a diffusion plant at Oak Ridge also. There was electromagnetic separation and diffusion separation. [Harold] Urey had done some of the theoretical work that was related to that mass diffusion stuff. That's where he got his ideas about isotopic fractionation being a function of temperatures. This was the insight that enabled him to develop the concept that led to what we call paleotemperatures, the measurement of temperatures 200 million years ago. This is Sam

Epstein’s stuff.

Cohen: Was Harold Urey at Oak Ridge?

Patterson: No. He was the guy who did the theoretical work at Columbia that the Oak Ridge diffusion plant was based on.

These guys during the war developed these concepts, you see. And they kept them on the shelf. They knew that they were working as engineers on a hideous weapon of warfare. They were the same type as my mentors at the University of Iowa—like the guy who told me, “Patterson, we are saving democracy for the world against fascism.” These professor-mentors, who were no longer at the university but working on the bomb project, they told young people like me that this was the thing to do. This hideous crime that we were committing was a necessary thing.

Cohen: But did you think this way then?

Patterson: No. It was afterwards.

Cohen: So let’s talk about how you thought about it then. Then, you just did your work. You felt that this war had to be won, right?

Patterson: Yes, I guess so. The essential thing was that during that time, I learned a lot of new ideas and concepts and patterns of thinking. So when the war ended, I said, “I want to go back to the university. I love the University of Chicago; I’m going to go there and I’m going to get my PhD in science and study some of this important stuff.”

So Urey, and [Willard] Libby, Brown, and all these guys, a whole lot of them, flocked back to the University of Chicago. And all these ideas that had been cooking around in their minds during the war then came to fruition as goals.

Cohen: Are we talking about basic scientific ideas?

Patterson: Yes. This had nothing to do with making the atomic bomb. These were scientific concepts that dealt with atomic physics and chemistry.

So when I went to the University of Chicago, Harrison Brown found out about me, and he said, “Hey, Pat, look, you’re familiar with mass spectrometers. Now, here’s this other youngster, George Tilton. What we’re going to do is learn how to measure the geologic ages of a common mineral that’s about the size of a head of a pin and it has uranium in it but no lead. It’s called zircon.” You know the jewelry zircon? Well, there are little tiny zircon crystals that occur as a minute trace constituent of common ordinary igneous rocks. When those rocks crystalize and form from magma, a whole lot of different crystals form in there; among those are tiny bits and pieces of zircon. And they have uranium but no lead. And as they sit there, the uranium decays to lead, and you can do uranium-lead age measurements. However, the amounts of uranium in there are about only a few parts per million, and that’s decayed to just even smaller parts with a little bit of lead.

So what Brown wanted Tilton and me to do was to develop mass spectrometric techniques to measure amounts of uranium and study the isotopic compositions of amounts of lead that are 1,000 times smaller than anything that anyone has ever looked at before.

Cohen: Let me back up a little bit. You entered the University of Chicago as a graduate student to get a PhD. You did not enter at Harrison Brown’s invitation. It was after you were there that he found you.

Patterson: Yes, after I was there and started taking courses. I’d only been there for a few months when he took on Tilton and me. He was looking for students who had backgrounds and knowledge in mass spectrometers. George hadn’t, but George had worked with uranium a little bit before, so this is how George got into it.

So that's how I first met Harrison and got into this business. The students at Chicago had to take a lot of courses to give them the background in whatever topic they were going to major in. And I was in the Chemistry Department.

Cohen: It sounds like it was a very exciting place in the early fifties.

Patterson: The last half of the forties. I worked for about five years on my PhD, and one year as a postdoc, and then another year as a research assistant here before I actually—well, I'd measured the age of the earth. But before that, George Tilton and I had determined how to measure the ages of these zircons. And that blew the whole thing apart.

You see, three different methods had been developed for measuring ages: the uranium-lead age, the potassium-argon age, and the strontium-rubidium age. They didn't even know what the half-life of rubidium was when we started this stuff. I didn't work on the latter two; I only worked on the uranium-lead. I was the lead man and Tilton was the uranium man. Tilton only had to measure concentrations. I had to measure isotopic compositions. And that is different. And Harrison Brown says, "Well, Pat, here's the deal. Once you do that, then here's what you do." Brown had worked out this concept that the lead in iron meteorites was the kind of lead that was in the solar system when it was first formed, and that it was preserved in iron meteorites without change from the uranium decay, because there is no uranium in iron meteorites. Now, this is crucial, because when other parts of the solar disk of the planets were forming—for example, Earth—they took in both lead and uranium. Therefore the lead in the earth today is a mixture of two things: the primordial lead that was in there at the beginning and the lead that has been created by uranium decay since the earth was formed.

Now, there are two isotopes of uranium that decayed and there's also thorium, so you have three different isotopes of lead. So the whole thing gets mixed up. You've got all these separate age equations for the different isotopes of uranium and different isotopes of lead that were formed. And it was not known what the isotopic composition of lead was, in proportion to these different isotopes in the earth when the earth was first formed.

Now, there's a bunch of equations that these atomic physicists—Al [Alfred] Nier, for example—calculated. It's so marvelous how they worked all this stuff out. And if we only knew what the isotopic composition of primordial lead was in the earth at the time it was formed, we could take that number and stick it into this marvelous equation we had. And you could turn the crank and, *blip*, out would come the age of the earth.

So Brown said, "Pat, after you figure how to do the isotopic composition of these zircons, you will then know how to get the lead—you will have it all set up. You just go in and get an iron meteorite—I'll get it for you. We'll get the lead out of the iron meteorite. You measure its isotopic composition and you stick it into the equation. And you'll be famous, because you will have measured the age of the earth."

Cohen: And what did you say?

Patterson: I said, "Good, I will do that." Do you know, he said, "It will be duck soup, Patterson."

Cohen: Did he ever work in the lab with you?

Patterson: No. He came in one time. He was trying to show us something. It [an ether-extraction flask—L. Patterson] blew up in his face. He had to stick his head under the water faucet. [Laughter] He was better out of the lab.

Anyway, let me tell you. In working with George Tilton, we finally got this method worked out. What we had done was, Brown had gotten us a granite rock that was formed at the same time as some uranium ores were formed, so the age of this uranium ore was the same as the age of this rock. And they had used old classical methods to determine the age of this uranium ore. We only had six or seven geologic ages—that's all we had; there were no more—and they were from ores, you see. This is where you had gram amounts of these ores, and milligrams of lead to put in a mass spectrometer to determine its isotopic composition. I had to use micrograms—1,000 times smaller—for the age determination.

Well, we knew the age of this rock, and old George would determine the amount of uranium in these zircons, and I would be working on the lead. When I'd come out with something, we'd take George's uranium and my lead, and knowing the age, we'd compute how much lead should be there and what its isotopic composition should be. "Not right, Patterson!" Our experimental results didn't fit the calculations.

Now, I tracked back and I found out there was lead coming from here, there was lead coming from there; there was lead in everything that I was using that came from industry. It was contamination of every conceivable source that people had never thought about before.

Cohen: When did you realize that? Did this just come to you suddenly?

Patterson: Oh, of course not. Say the age result you get is wrong. Why? You go back and you track it down. And you say, "Well, it must be that." Then I would go and have to analyze that thing I thought was it. Well, that was harder to do than analyzing the stupid zircon. You had to set up a whole mess of simultaneous equations with two unknowns, and then do that, and then finally get an estimate of what it was. So it took me years and years to work out where the lead was coming from and how to get the lead out of these things.

Cohen: When did it occur to you that this lead was coming from all over?

Patterson: I found that the numbers were wrong when I was analyzing the zircons. There was lead there that didn’t belong there—more than there was supposed to be. Where did it come from? Well, it might have come from this, it might have come from B, it might have come from C. So we’d look at lead in C; we’d look at lead in A. Well, how do you analyze lead in A? It turns out you’re contaminating lead in A when you’re measuring lead in A. So therefore you have to set up these simultaneous equations in order to gradually get into all of that.

Cohen: Did people believe you when you first said this?

Patterson: No, of course not. Anyway, so I discovered it after years and years. It was working with those zircons that enabled me to become aware of this enormous contamination problem. Brown said he thought it was merely a matter of reducing the sample size, of technology, of finding the laboratory techniques. I reduced the amount, but that wasn’t the problem. I could reduce it by a factor of 1,000; that only took me a year or so. You just play games, you know, like an engineer. That wasn’t the problem at all! When you reduced the amount, you ended up with contamination. You couldn’t take a little speck of something when you had tons of lead in your laboratory from all these different sources.

So I gradually learned how to do that. That’s why I became aware of the contamination problem, because I kept getting the wrong answer for lead in these zircons. We knew what the amount of lead should be, because we knew the age of the rock from which it came, and because of George Tilton’s measurement of the amount of little tiny bits of uranium in there. We could calculate how much lead there should be and what its isotopic composition should be, and it kept coming [out] the wrong number. So I had to figure out why—to go to all these sources for different possibilities. So therefore I found out all about this contamination. And in the process of finding this out, I learned how to analyze very low concentrations of lead in everything.

Cohen: Did you already set up a clean lab there?

Patterson: No! I was forced to make the clean lab as a consequence of these discoveries. Why would I have a clean lab? No one else had a clean lab. It didn't exist. It came about as a consequence of these discoveries. I discovered that your hair— You know Pigpen, in Charlie Brown's comic [strip], where stuff is coming out all over the place? That's what people look like with respect to lead. Everyone. The lead from your hair, when you walk into a superclean laboratory like mine, will contaminate the whole damn laboratory. Just from your hair. [Laughter] And lead's coming from your clothing and everything else. So I learned the beginning of how to analyze lead at very low concentrations in common ordinary things that people had never thought about.

Now, there were tens of thousands of published numbers of lead concentrations in these common ordinary things. They were wrong! They were high, but they weren't nearly high enough.

CLAIR C. PATTERSON
Session 2
March 6, 1995

Begin **Tape 1, Side 2**

Cohen: I'd like to talk a little bit more about your interaction with Harrison Brown over the years that you were his student.

Patterson: Well, I got my PhD [1951] at the University of Chicago after five or six years of work there. George Tilton, his other student, and I had worked out and developed and published a paper on how to determine the ages of little tiny zircon crystals in the rocks. This was very important, because this is one of the three major methods that was subsequently used to delineate the geological history of the earth. It was a very, very fruitful type of pioneering work. But I hadn't yet gotten to the measurement of the age of the earth and studying the lead in iron meteorites, which Harrison had told me was duck soup five years earlier.

Well, at that time—and I was just getting my PhD—I said, "Well, Harrison, I really would like to continue this work and measure the age of the earth and get the lead out of the meteorite. But I need to work as a postdoc here at Chicago to do that." And he said, "OK, Pat, go ahead. You can work in the new labs they're building over there, at the Institute for Nuclear Studies." So I wrote a little proposal to the U.S. Atomic Energy Commission. They had a postdoc program. They had financed George Tilton's and my work—my work on lead, George's work on uranium—for the five previous years. They would give the money to Harrison, and Harrison paid his predoctoral students. And since they had financed us for work that led up to this, I wrote a new proposal saying, "Here, I'm going to do this." They turned it down. They said they weren't interested in measuring the age of the earth.

I cried on Harrison's shoulder. He said, "Pat, that's all right. I'll rewrite your proposal in my name." And you know, he's very good at explaining things to people in a nonscientific way—in an engineering aspect way that says of what use it is, you see. So he rewrote the darn thing. *Boom!* I was awarded a fellowship, a postdoc, and that's the money I used to begin. I did about half the work for one year in getting lead out of meteorites to do this. But I still hadn't done it. And then Harrison got offered a big job over here at Caltech in the geology department. And he brought me along with him.

Cohen: And you just said yes?

Patterson: Yes, of course. Because I wanted to continue this work. So after he came, he got more money out of the Atomic Energy Commission to build a mass spectrometer here, to build me a laboratory to work in, among other things. And in that laboratory I isolated iron-meteorite lead. But I didn't have a mass spectrometer built yet, so I flew back to the University of Chicago. There was another professor there—Mark Inghram, in the Physics Department—and he and Harrison had worked together. It was his mass spectrometer that I had used with George Tilton to do all this age work. Mark had built a brand-new type—a new version—that I could use to measure this stuff. It was wonderful. And that was the data that I used to publish this paper delineating the measurement of the age of the earth. This was in 1953. This was the first measurement of the age of the earth that was published.

Now listen, Shirley, it was not understood by the geological scientific community at all—how this was done, or anything about its meaning or significance. Furthermore, only a few nuclear geochemists understood what was being done there. And they were all busily working in these two other methods—potassium-argon dating and rubidium-strontium dating. None of them knew how to isolate lead without getting it dirty. However, the age work that George Tilton and I had done for our PhDs—that had excited some people. That got them to also start working on isolating lead from these little things. And they made improvements over Patterson's mass spectrometry method that enabled us to reduce the level of lead contamination by another factor of 100. [Laughter] But they still couldn't control the contamination well enough.

At Caltech I immediately used their new technique in my new laboratory to push down the contamination control and the amounts we were working with. And that was how I was able to do this lead measurement in the iron meteorite successfully. But it took a long time before that tiny handful of people could catch up and repeat my measurements. It took them years and years and years before they could do this. And it was a dozen years, literally, before this number got in the geology textbooks.

Before then, the age of the earth was very vague. It was some billions of years. Back before World War II, it was what was known as sort of a mystic number. Then after about twelve years, the correct number began to appear in the geology textbooks, but they never said how it was determined. They said, well, it was due to uranium-lead geochronological measurements. But what was said was incorrect, of course. It wasn't until maybe ten or fifteen years later that a few of my colleagues were able to really do this correctly. You must recognize that this number that I had measured related to the time of the coalescing of this planet out of the solar disk. You see, there was the formation of all the other planets going on, too—big ones, little ones. Even the asteroids, which were swirling around. This was the time of the segregation from the solar mass into this separate little body, Earth, swirling around the solar mass.

Now, that is a finite period of time. I mean you have—who knows? Do you have a billion years for this to take place, which is a substantial fraction of the time that's passed since the earth was formed, or was it just a very short time? Well, they were working on that, and I didn't give two hoots for that. My attitude was, "I don't want to work on that stuff anymore. What I want to work on is about the evolution of the earth—what happened to the earth itself during the time it was coalescing.

That’s what I want to work on.”

Now, the reason why I wanted to do that—or that I could do that—was because we could use lead isotopes as measures. I know this doesn’t have any meaning to you. But we could do it because the isotopic composition of the lead was changing. It was dynamic, because uranium was decaying all the time and there were three radioactive progenitors of three different isotopes in this lead that were being added all the time the earth was there. The earth was dynamic. These parts were moving around, all over the place, and you were separating uranium and thorium from each other and from lead, due to their different chemical properties in these different components that were moving around. Some had sulfur, some had oxygen, some had silicon. And these different components would grab on to different chemical strengths of the lead, uranium, and thorium and segregate them in different parts, so that the proportions of lead and uranium and thorium would change for millions and hundreds of millions of years at different areas. And the lead within would have a different isotopic composition. And you could track this. You could follow it.

So, today, by looking at rocks—here you have a rock this old, there a rock that old—you could look at the lead in there and you could begin to put together a picture of how they had been related in past times, their chemical relationship. And then you could get other people’s work to help you interpret what that chemistry meant in terms of position in the earth. And then we could get times.

So that’s what I was interested in. That’s what I started out doing. I said, “To hell with this damn stuff about cosmology of the sun to the planets.” There were other people who were very interested in that. So they worked their hearts out to prove I was wrong.

Cohen: They didn’t like your number?

Patterson: No, no. They wanted to make discoveries. And in order to make discoveries, you can’t just prove somebody’s right. That’s no discovery. You want to prove that somebody’s wrong: “Here’s the right way. This is the new way.” You see?

So I had some of the best, most able critics in the world trying to destroy my number.

Cohen: Where were these people working?

Patterson: In various universities in this country and in Europe. These were nuclear physicists and nuclear chemists, trying very hard.

Cohen: But meanwhile, your work was supported all these years.

Patterson: No, it wasn’t supported. They were trying to prove it was wrong.

Cohen: I know. I meant you were getting grants to do this.

Patterson: No, Harrison was getting them. Let’s get back to Harrison. In all this time, I was trying to shift back to using lead isotopes to do this. Now, in order to do that, believe it or not, Harrison got money from the Atomic Energy Commission to do this kind of work at Caltech. He was telling them fibs, actually. He was talking about, oh, how my work was related to uranium, of course. He went through all these calculations, and he told the Atomic Energy Commission how there was enough uranium in ordinary igneous rock that if you ground that rock up and then leached it with hydrochloric acid you would get enough uranium to use in an atomic generator that would be equivalent in energy to 10,000 tons of coal. It would pay for the energy not only of grinding up the rock, which required energy, but you would have left over huge amounts of extra energy. In other words, 10,000 tons of coal would equal the amount of energy of the uranium in one ton of granite.

Cohen: And they bought that?

Patterson: They bought that! And it was that kind of sales pitch he used. [Laughter] Now listen, you know what I would say. I would say, “Well, I want to know how this chunk of North America evolved and then got thrown around and came over here, and how this other chunk came up later. And we want to know when this chunk came up and when that chunk came up, and how they were related to each other. What was their ancestry?” And the Atomic Energy Commission would say to me, “To hell with you, Patterson! We don’t care about that stuff at all.” But that’s the way I would write my proposals. And I never got funded. But Harrison would get them funded for me.

Cohen: But he wasn’t here all that many years, was he?

Patterson: Yes, he was. He came here as a young man from the University of Chicago, well recognized and famous. He got into the National Academy of Sciences very early, when he was young. And he was very, very capable. How many people could have solved the problems I did? Nobody would have the stupid perseverance, you see, to pursue it.

Cohen: But he saw that.

Patterson: Maybe, I don’t know. But he talked me into it. And he could talk other people into doing a whole lot of other things.

Cohen: Who else did he have working with you here?

Patterson: Professor [Leon T.] Silver, who’s a professor now, was a graduate student when Brown came here. Brown came here in ’53, and Sam Epstein came at the same time. Sam was a protégé of Harold Urey, and Silver was a protégé of some very good geologists in the [U.S. Geological] Survey. They had given him an enormous, very very solid, good foundation in geology. And Epstein had this enormous good foundation in nuclear physics and geochemistry of paleotemperatures, and all that stuff.

Cohen: You were a dream team.

Patterson: Yes. Professor Silver taught me a lot of geology. But I built my own laboratory. And I started out working in this geological stuff, and Silver was helping me and working with me and guiding me. Epstein started on paleotemperatures. We had Professor [Robert] Sharp here, who was in ice and snow. And that got Sam into studying recorded temperatures of polar ices and that sort of stuff. He pioneered that stuff.

Harrison tended to get into the social aspects of this stuff. Maybe he felt a little guilty over participating in the atomic bomb project—which was an evil sort of a thing—and he was trying to make restitution and all that stuff. So he really spent a lot of his time doing a whole lot of things—part of it as foreign secretary for the National Academy of Sciences. He did a lot of international operations that dealt with trying to correct the ills of society through scientific books and things.

Cohen: He was very interested in population, too.

Patterson: Yes. Well, in resources more than in population. How do you get equitable distribution of resources? He worked hard on that.

Cohen: So when did he actually move out of the geology building into Baxter? Because as I understand, he had an office in Baxter.

Patterson: That’s right. He shifted about two-thirds of the way through his time here. He finally moved over there and was professor of humanities, doing something. I’ve forgotten his title over there. Then there was an East-West Center that was formed in Hawaii, and he moved there. He was able to work over a larger scope in a social way. He was very forceful, very interesting. I stopped interacting with him when he moved over to Baxter and I had to start getting my own money—which I failed at, of course. But Brown protected me.

There was an important phase here in Brown's helping me get money. He shifted from the Atomic Energy Commission input—this is a lot of money that these guys gave out over the years. Now it doesn't sound like much today, because there's a factor of 20 difference in the cost of the dollar, but if you multiply what we were getting times 20, it's in the millions that we were getting, over and over.

About four or five years along, Brown had a new idea. I was studying sediments. In order to figure out what was happening in the past, I would have to get oceanic sediments. You see, the rocks would erode, they'd have lead in them, and then they would form sediments. But you would know; you could measure the age of the sediments. And they're out in the oceans. I wanted to sample all the continents of different times, and the oceans were a mixing reservoir. And we would look at this mixture in the sediments as a function of time.

So I was studying that stuff. And Harrison said—he really was a brilliant guy politically—"Oh, heck, the oil companies should be interested in this." "Why?" "Well, because if Patterson is looking at these sediments, the isotopic composition of the lead is a tracer that helps identify the stage, or the age, to characterize the time or the type of sediment that you have." So he convinced the oil companies that they should finance my research because it would assist them in identifying oil deposits. You know, when you drill a core, you're looking at bands in a rock. And if you measure the lead isotopes in there, it can give you more information than you had before. It could help characterize the type of sediment, so it could help you locate and identify oil deposits and reservoirs here and there. So they started. It was a national consortium of oil companies that had this big research fund where they doled it out to help them do this stuff. Harrison got money from them every year, huge amounts, to fund the operation of my laboratory, which had nothing whatsoever to do with oil in any way, shape, or form.

Cohen: That’s called basic research.

Patterson: Yes, but you see, he could do that stuff! To me it was just a falsehood. If I’d written it, I would have said, “Scientifically, this is what . . .” And nobody would care two hoots about it. Therefore, he helped me. He got the money for me, until a very bad thing happened.

Cohen: Were the grants in his name?

Patterson: It was sort of dual—to Patterson and Brown, something like that. No, he got them to give it to me. I was the recipient. Look, he had so much power and prestige, he didn’t give a damn about this sort of stuff. He wasn’t interested in that anymore. All he wanted to do was to have an operation going. He was getting money for others of his people for other reasons from different agencies.

Cohen: So he wanted to have all these scientific operations going on, even if he wasn’t . . . .

Patterson: He was real good for Caltech at drawing the money in. Even when he was over in the humanities, he was getting money for various things.

Cohen: So who, besides you, was he getting grants for?

Patterson: The guys in the geology department. I don’t know. I didn’t pay too much attention to certain stuff. I was buried. I’m a recluse.

And then a very bad thing happened. We were studying the sediments, and we found from measuring the lead in these sediments how much lead had been passing through the oceans and depositing in these sediments. Now look, there's two kinds of lead: There's a soluble lead that's in the water—it's sort of a water lead—and then there's lead in particles. These particles are what the sediments are made out of. However, a small fraction of sediments are made out of residues of organisms that are living in the water and fall down. Like zooplankton poopies, and they fall down through this. And this is four miles of water!

Cohen: And you've got all these things coming down.

Patterson: Yes. it's full of various things. And they all had lead in there. Well, when the zooplankton residues got into the sediments down there, there'd be a chemical reconstitution going on, doing what we call formation of autogenic minerals. It would be a rearrangement of that stuff over a few thousand years, and it would rearrange itself into lattices of minerals. You could get the lead of those minerals by just taking a piece of sediment and treating it very gently with a little diluted acid, but you wouldn't get the lead in the clay particles that had migrated out from the rivers and then fallen down and formed the bulk of the sediments. You wouldn't touch that lead. You'd only get this little tiny amount of lead that had been in the zooplankton, because that soluble lead collects on the outside of their little bodies. Those guys had gathered up this soluble lead, and they knew the greater formation of these sediments, because old Ed [Edward D.] Goldberg down at Scripps in La Jolla had come up with a beautiful concept, and he'd worked out a dating technique. It was a Scripps operation, but it was a big, gigantic one. I don't know whether this was paid for by the government or by the oil companies, or by both. It was some big drilling project called the Moho,and it was something that had never been tried before—Walter Munk and those guys. They had a ship, sitting out there, and they'd lower drills down through four miles of water, and then they would bring these cores back up. The cores were down at Scripps, and we got segments of that stuff. They came from all over the Pacific. There was a pattern, scattered throughout the basin of the Pacific. So we could see what was coming from China, what was coming from North America, what was coming from South America, at different times. Different stuff came at different times, depending on the climate.

Now listen, this isn't over thousands of years or even hundreds of thousands of years. I mean, we're talking about millions of years. So we were looking at this stuff.

In addition to dating this stuff, old Goldberg and a guy by the name of Gustaf Arrhenius—who's the grandson of the famous Swedish chemist [Svante] Arrhenius—had worked out this autogenic mineral business. Now, they didn't study lead, but they knew that this mineral was reconstituted zooplankton poop, you see. And they had identified this mineral, which nobody had done before. I knew that when I leached it with acid, I'd been taking their mineral and getting the lead out of that mineral, which came from the soluble lead.

When we measured that, using the ages of the sediments, we had a knowledge, a measurement, of the rate of the past flow of lead through the oceans all over—and this was millions of years ago. So we knew the quantity, the rate per square centimeter of sediment surface, the bottom of the ocean, grams per square centimeter per year. We knew how much was flowing through there.

Then I got some data from the rivers. Now these were idiots who were measuring lead in river waters, who didn't know anything about how to measure lead.

Cohen: Who were these people?

Patterson: Oh, various institutions, measuring lead in river waters. Patterson was the only guy who knew. You saw this picture here? Here they were. They were measuring lead in river waters here, and they didn't know what they were doing and they're wrong. OK? And I knew that! Because I had previously worked out how to do the measurements for meteorites.

So I took their data for river water and I multiplied by all the rivers of water how much water there is in the oceans each year. And I came out with a number for lead that was 100 times greater than the amount that we had measured that was flowing through the oceans in the past.

I thought, Something is wrong here. Are these guys wrong? Or is there really that much lead coming into the oceans today?

So at that time we were working out methods for taking what we call a profile. A ship would sit there and it would lower something and grab some water, hoist it up, and then it would lower it deeper and hoist it up. We'd collect water up and down for vertical miles, and we measured it.

Cohen: Did you have somebody on the ship doing this? You weren't on the boat yourself?

Patterson: Yes, I was! And I got sicker than a dog!

Cohen: So you did go out on the boat to get the water.

Patterson: You bet I did! I didn't know what the hell I was doing. I hated it! I got seasick. Old Hans Suess, he would go on a ship down there, and he'd walk across the bow and vomit over the other side, because he'd get sick just walking across. Well, I was about the same way, but I wouldn't vomit until I spent about a day on it.

We lowered this stuff down and hauled it back up. And we got profiles. Now, a profile. . . . Here's the top of the ocean, here's the sediments. You look at the concentration; how does the concentration change with depth? We found a huge increase in the upper portions of the oceans, which decreased to lower concentrations with depth. Now, why is that? Why should the lead be so high?

Now, the waters don't mix that rapidly. And the waters up here are much younger than the waters down there. It takes a long, long time for them to mix. So I made some calculations.

What about the lead in gasoline? If you took all of the ocean—we only had a profile for just part of the Pacific, and actually part of the Atlantic, later—but if you took those profiles and you extrapolated from that over all the world's oceans, the amount of lead equaled what was being produced from gasoline. It could easily be accounted for by the amount of lead that was put into gasoline and burned and put in the atmosphere. We had more tons put in the atmosphere from lead gasoline than we could see in the upper part of the world's oceans right there.

And that's what caused the problem. The oil companies were financing my work. We're in serious trouble.

Cohen: Even Harrison Brown would have trouble with that one.

Patterson: Oh, he did! And that's when he disassociated himself from me. He stopped getting money from the oil companies, and I had to start getting it myself. I wrote a big paper, and I said, "This lead is coming from leaded gasoline." *Wham!* They stopped my research. They not only stopped funding me, they tried to get the Atomic Energy Commission to stop giving me anything—they were still giving me some money. They went around and tried to block all my funding. But I'm so stupid that I didn't know. I couldn't do anything about it. Harrison could have, but he was out of it then.

I needed money, a lot of money, because since I got this idea about lead coming from gasoline, I wanted to look at the record. Where do you see that record? You see it in the snow that never melts in the polar regions. It comes out of the air, which has lead in it. Lead is in the snowflakes. It goes down, and you have a layer there. Next year you have another one.

Cohen: Did you already have these cores?

Patterson: No. No way whatsoever. Look, some of my colleagues, like Sam [Epstein], never stepped out of the laboratory. They wouldn't know what it would be like to go on a boat and collect this water. What I did was, there were some new types of government projects being sponsored by the National Science Foundation. The International Geophysical Year was one. Harrison Brown had started a lot of these. And then there was also something about oceanographic things. And our government started pouring money into that. This was in the sixties.

What I did then was, instead of Harrison financing me, I would go to other universities and work, and there would be the Harrison equivalent of professors there, who could talk. They had silver tongues and golden tongues. And we would put in cooperative proposals. I would be the super scientist who would get the data for them, and they would submit the proposal.

Cohen: Where did you go?

Patterson: I was here! In cooperation. These other guys would write the proposal, and our names would be on the joint proposal. And part of the money would come to me, part of the money would come to them. My scientific work would support the continuation of that work. So they would keep getting money. And they would be paid for this.

Cohen: Who were some of these people that you worked with?

Patterson: Oh, let's not worry about that. They were in various universities around here.

What happened was, I started working up north, at the North Pole. I flew up there. I had some Japanese colleagues come here. And this money paid for their salaries while they were working here as visiting postdoctoral fellows and that sort of stuff. And it paid for my laboratory. It was an enormous expense.

Look, we had to collect the concentrations of lead in the snow up north, and it was 1,000 times lower than the concentrations of lead were in the "pure" laboratory water in most laboratories. I had to measure concentrations that they couldn’t measure. In other words, these scientists were using the purest water in the laboratories as a baseline, and they couldn’t go below that level. That level was 1,000 times above the levels I had to measure in the water in the snow up north. I had to see variations of that with time and the techniques for doing that at these levels were not at all developed. At that time, I required a block of snow about two feet in three dimensions—a two-foot cube of snow. And then we had to dig shafts down—200 or 300 meters deep—to go back in time, to go back to, say 1700 A.D., or something, to get these blocks of ice over a period of time to see what was happening. And I had to have these huge gigantic plastic containers to hold this ice, to melt it, so that I could bring back all this water from these various depths. And then we analyzed the water here. But the volumes of water were gigantic! I mean, we had these ten-gallon containers with water that started as two-by-two-by-two-foot blocks of melted snow. But those came from even larger blocks, where we sawed it out of the walls of this tunnel while we were wearing acid-clean plastic gloves and suits and using clean saws. We’d shave off all the layers and then get them in there. And then we had to haul them up out of the tunnel, all encased in these special big containers. And then put them in the hut that was up there and warm them up until they melted. And then drain them off into these jugs.

Now, all of that equipment was cleaned back down here, in vats of acid. It had to go through three stages of cleaning, and then it had to be all sealed up in plastic and flown up in these gigantic cargo planes. [Laughter]

Cohen: How many people did you have working on this operation?

Patterson: I had kids—summer students—working with me. Up north it was Caltech summer students, and down south it was New Zealand summer students. But the military paid for the transport. It was the army engineering up north, and it was the Seabees or navy down south.

Cohen: Now did you arrange all of this yourself?

Patterson: I arranged it, but the payment of this stuff—you know I can't really remember how this was financed. God must have arranged for me to get this money in some way or other, because I certainly didn't have the ability to convince people to do this.

Anyway, we got it. The money was there, and there was a lot. So we collected the snow up there, and we brought it back here and we analyzed it and found huge concentrations of lead increasing over the last centuries, since the 1700s until now—about a 200- or 300-fold increase in the concentrations of lead. And these concentrations were so infinitesimally small compared to what other people were used to measuring that no one else could verify this. It was impossible. It was beyond their ability by factors of thousands, or tens of thousands. So no one could verify what we did. So it was sort of sitting there for a while.

Cohen: But you believed it?

Patterson: Yes, but I certainly lost all my funding from the oil companies. And at the same time, I had proposed a concept that I call biopurification, which has to do with what the natural level of lead should be in people. And it worked like this: You start out by looking at the calcium in our bodies and asking, "Where does that come from?" You track it back—you look at the food that we eat, you look at the organisms that made that food, and you keep going down the food chain until you come to plants. Then you go down to the earth, to the ground, the soil. And you go from there to the rocks that the soil came from. You follow that whole pathway of calcium—from rocks to soil to plants to herbivores, to us. In going that way, it so happens that there are calciumlike trace metals like barium, but it's very small abundances—a tiny, tiny infinitesimal trace compared to calcium. Barium is like calcium, but it's chemically different. For one thing, it’s a very massive atom. It has different chemical properties than calcium—not grossly different, just enough different so that barium is poisonous as hell. When we evolved through all these millions of years of evolution, nature devised an exclusion mechanism for handling this. Each of the organisms in this food chain has to have calcium, but nature evolved a process for the exclusion of barium when they take calcium. There’s a positive transport for calcium; in other words, there are certain kinds of proteins that grab ahold of the calcium and pull it. Well, they don’t do that efficiently for barium. Barium has different chemical properties, and these proteins evolved so that they don’t work for barium. So you have 100 units of calcium here and you’ve got one unit of barium here on the outside of the membrane in your gut, for example. How much goes into the systemic blood that goes to your liver? Ninety units of calcium get transported, but only about five percent of that one unit of barium. So there’s an enormous reduction of the barium-to-calcium ratio—barium over calcium; instead of 1 to 100 in the gut, it’s now reduced to .05 parts to 100 parts in the portal blood that goes to the liver. You get a tremendous reduction.

That’s why I call it biopurification. You multiply that reduction over these three or four stages, going from rock to us. And I was able to make this calculation because we had data from the atomic bomb testing project, where they were looking at radioactive barium and radioactive strontium. They’d do a test, and there would be fallout. And they’d look at the ratio of these things—radioactivity now—to the calcium in the hay that the cows ate. And then they would look at the cows’ milk. The barium level went way down, because of this very factor that I’m talking about. There’s a pass through the mucosal membrane of the gut of a cow, and this radioactive strontium and barium would be left behind; it would go through this exclusion mechanism. So I had an idea about what those factors were.

Now, it turns out that lead—and I didn’t know anything about this; I had to dig it out. And I went to the Harvard Medical

Library; I was a visiting scholar at MIT. I was invited to give lectures on the age of the earth stuff. They invited me to give all these lectures about lead and the age of the earth to the students. Our family moved there; we were there for a year. And you know what, instead of worrying about these lectures on this lead isotopic stuff, this was when I developed this concept of biopurification, and I wasn't supposed to do that. I was supposed to be working on these lectures on the evolution of lead in the earth.

Cohen: So you didn't do it?

Patterson: Yes, I did it, but not very well, and they hated me. One time they invited me over to Harvard to lecture on this stuff. And I said, "We know what the level of primordial lead in the solar system is. Look, it's OK. The most recent—" [tape ends]

Begin **Tape 2, Side 1**

Cohen: So, you were making a bad name for yourself at MIT and Harvard. About what year was this?

Patterson: You know, I can't remember. The early sixties. Anyway, I said, "The heck with this. You don't have to worry about the age of the earth—primordial lead. It's OK; my number is OK. You don't have to worry about it. Because the new data from the solar spectrum show that lead dominates; the solar occurrence of lead is 1,000 times the solar occurrence of uranium. So there ain't enough uranium in there to alter . . . . I couldn't have made a serious mistake, so it's OK! I mean, the earth is as old as I said." Then I said, "Look here's what's really important—now look at this." And I gave this new data about biopurification. [Laughter] I said, "Here is the lead-to-calcium ratio in rock. And here it is in our food. And I measured some of it. And here is the ratio in us." Now, it so happens that lead is in our bones. Most of the lead in our bodies, it goes with calcium.

Cohen: The lead comes in with calcium?

Patterson: Of course it comes in with calcium. And it’s distributed in the body much like calcium—not on a molecular basis; there are different proteins, but over all, in a general, morphological distribution, it’s in our bones, OK. Ninety-nine percent is in our bones.

So therefore I knew what the lead/calcium ratio is in average people today. But they made mistakes. They couldn’t even measure lead properly in bones, but I used their data anyway. So I had the ratio going from rocks to food to people. And do you know, the ratio of lead to calcium in people was about the same as that in rocks?

Now, I compared that with barium. I said, “Now look, this has got to be wrong. Here is the barium-to-calcium ratio in rocks.” I got this from the atomic bomb stuff. They measured barium in our food, and they measured barium in our bodies. They had to measure, because they were measuring radioactive barium. So I had this data from my old atomic-bomb evil people. [Laughter]

And you know what? The barium-to-calcium ratio in rocks was way up here. It was actually 100 times greater—it dropped in our food, and it dropped in us, by a factor of 100. And I said, “Look, lead and barium is wrong. The barium ratio shows that lead should be 100 times less than it actually is in us today. We are being poisoned by lead. And guess where it is coming from? Look at the ocean. You see this curve with all this lead up here? That’s coming from tetraethyl lead. Why do you think it took me all these years to measure meteorite lead properly in the laboratory? We are as contaminated as the laboratory.”

They thought that was a pile of crap! They said—no, they didn’t say, they thought and said later—“Patterson, would you please start worrying about science instead of this health crap.

What a waste! Here you are, you measured the age of the earth, and you're worrying about tetraethyl lead. And this stupid stuff about lead in bones."

But I was right. The barium ratio went down a factor of 100. And you know, when we finally actually measured it—it took about twenty-five years to do this accurately—it's a factor of 1,000. You see, I predicted it was a factor of 100. I was off, the wrong way. [Laughter]

Cohen: So they didn't invite you back.

Patterson: Oh, they liked me, it was all right. They were even thinking about putting me on the faculty, I think. But I came back here and I told Bob Sharp [then chairman of the Division of Geology], "I ain't leaving here. My laboratories are here and I want to stay here, and the heck with it." He raised my salary, because he was afraid I was going to leave. That's when I started going to the poles to get ice and started going to volcanoes and measuring lead coming out of volcanoes, and developing complicated devices for getting seawater. Look, the ship is covered with lead, and it's oozing lead all over the place as it moves through the water, so there's a local contamination problem. How do you measure seawater from a ship in the middle of the ocean, if the ship is spewing lead all over the place? We had to devise these very special devices.

Cohen: What was your motivation at this point? Were you thinking in an environmental sense?

Patterson: No, I was not! Science, science, science! I wanted to know, What is this natural level of lead? I didn't care two hoots about verifying what the contamination was. I was forced to measure the contamination in order to arrive at what was the natural level.

Cohen: So you were not being driven by environmental issues whatsoever?

Patterson: I was not. But there were friends and colleagues who were environmentalists, and they used my work. My work was used to get the lead out of gasoline. As a matter of fact, I wrote a paper on this biopurification concept [in which] I said, “We have 100 times more lead than we should have.” And that’s when I really got shot down by the oil companies. But when other people around learned about this, they seized upon that, and that was used by them. “Well, here is scientific evidence suggesting. . . .” You see, they wanted to get lead out of gasoline. So it was instrumental; this was the impetus that began providing the scientific foundation to get lead out of gasoline. Because before, all they had for evidence was people who were being poisoned in the factories. The government was taking elaborate precautions, which they forced industry to follow when they started doing this in the thirties. How to protect people making this lead tetraethyl. Do you realize that one drop on your skin of pure lead tetraethyl will kill you? One drop! It takes about two or three weeks, and you die with clinically similar symptoms to rabies hitting the central nervous system. It passes the membrane that gets into the brain, and it poisons the brain. And it takes about two or three weeks, and you’re dead. One drop. [Laughter] And you know, people wash their hands in this stuff [gasoline]. And do you know why nothing happens? Because it’s more soluble in the oil in the gasoline than it is in the lipids of your skin. And so you only die slowly from lead poisoning.

So the government was protecting these workers. And that took care of it. It was only in the manufacturing process that you worried about the toxicity of lead tetraethyl. Once it’s out, it’s in gasoline and it’s oily stuff. Did you know that when you ship lead tetraethyl from these factories, it’s handled just like it’s a poison-gas weapon? Were you aware of that? The railroad tank cars, they’re all sealed and protected until they get to the refineries where the lead tetraethyl’s mixed with the hydrocarbons. This is super poison gas. It’s handled very, very carefully. We don’t do this anymore, but we did, and this was the way it was done. Now we put it in ships and then ship it south to the Southern Hemisphere. [Laughter]

We also went to the tops of mountains to measure lead levels. Harrison was out of it then. He was no longer in this.

Cohen: So who here at Caltech did you work with?

Patterson: I brought in postdocs from various countries and universities. They swarmed in here. This was the mecca, where people came in and out.

Cohen: Who became your protector? Bob Sharp?

Patterson: As a matter of fact, he tried to protect me from the oil companies. Yes, he did. He did his best. I went to the National Science Foundation, and he may have helped there. They knew I’d measured the age of the earth, and since they were more scientifically oriented, they could understand this stuff. So I got some money from the NSF for quite a while. And then I shifted to NIH [National Institutes of Health] and HEW [Health, Education, and Welfare] and that sort of stuff. And then this International Geophysical Year. But I had colleagues who were not working with lead but who were working with other things that were related to lead—other elements or other aspects. And we put in proposals together to get support for collection procedures, the costs of fieldwork, and then I could add my laboratory and my salary and my visiting colleagues.

Cohen: So it was a lot of cooperation with a lot of people.

Patterson: Yes. And then they could use my findings as a glowing example of what was being done by these cooperative research projects. Because other people couldn’t measure these numbers; it was extremely difficult. And they had to come to my laboratory from all over the world to find out how to do that.

Then, after they had learned these things, they could go back to their laboratories and then gradually start to do it for themselves, you see. But it took years for this flow, back and forth, for these kinds of things to develop. That’s how I got funded; it was by this cooperation. Of course, I’ve been turned down throughout the years. If I wrote a proposal with science—down, no way, out. It had to be the way these people could talk. It had to sound reasonable, even for the National Science Foundation. Actually, the NSF gave me some research money just for pure science. I struggled along with them for—

Cohen: Well, but I think your work was already quite recognized.

Patterson: No, no. Well, I don’t know. As far as measuring the lead in snow, I think it was 1980 before people—twenty years later—that people finally began to. . . . With my work, it always seems to have taken two decades. By that time, I was teaching people in the laboratory—people from England, from France, from Australia, and from Japan—and they began to be able to acquire abilities to do this. The Russians, never.

I went to Russia one time. Under the Communist regime it was impossible to do good science in Russian laboratories. I went to visit some of the people over there. They respected me, and we liked each other, but they couldn’t leave their laboratories to come work in mine, and they couldn’t do the work themselves. It was pretty bad. But except for that, with the French, British, Japanese, and Australians, it was easy.

Cohen: So you continued on with this work?

Patterson: Yes, this includes land areas, too. Not just oceans, volcanoes, and the poles, but also the land areas—plants and animals and high mountains. And finally we got a picture. Now, this is before the cadavers and the ancient Indian bones. We got a picture that confirmed and clearly showed that Earth’s entire biosphere was heavily contaminated with industrial lead, emitted into the atmosphere from smelters and from automobile exhaust. And that urban areas were further polluted by other sources of lead being moved around—solder in cans and this sort of stuff.

Well, that was clearly established. At the same time, I had been investigating the history of the production of lead. Now Shirley, these historians, they're not scientists. There are historians of science, but they don't do any science. But I did work with some marvelous British archeologists who had marvelous ideas. There were one or two Americans, also, who could take archeological data and understand the scientific implications of this stuff in terms of not just climate but the factors that were effective in establishing new developments in cultures. Some of those guys are very good.

I would parasite on some of their attitudes and concepts to develop an understanding of the development of metallurgical techniques with respect to lead and how lead was involved in that. I went back 9,000 years when all this metallurgy began, and finally lead came into the picture two-thirds of the way along. And then I showed how lead was related to the development of coinage in metal. And then business. I made quantitative calculations, based upon data that were available from historical records, of productions of lead. These dated back to about 1910 and 1920s and later. They would go into ancient Greek mining areas, and they would rework the stuff. Because there was silver in the lead stuff, and they wanted the lead also. So we had quantitative data for how many tons of these waste heaps they were using.

Cohen: Let me backtrack a bit. When you started to do this historical work, were you still doing other work?

Patterson: Oh, yes, it was simultaneous.

Cohen: So how much of this was sort of your hobby on the side?

Patterson: It wasn't a hobby; that was science, buddy. I was buried passionately in it. It took part of my time. I worked out ways to estimate from ancient data how much lead had been mined by the Greeks and by the Romans.

Cohen: Where did you get this data?

Patterson: In this development of the age-of-the-earth business, about twenty years later, one of the scientists—one of my colleagues—came along and said, "Patterson, you didn't measure the age of the earth. We did!" You see, they were working on this all this time, to prove that I was wrong, and they couldn't. But they said, Well, it was their measurements that established that number, not me. And he said, "God gave you that number." And I said, "Claude, God didn't give me that number. I'm not a religious person. It's highly improbable that there would be a miraculous thing that I would discover this number. It's impossible." I said, "Claude, here's what you do. You guys get together, write a proposal to fund me so I can live on an estate in southern Italy. And then you give me questions to ask; I will ask God. God will give me the answer, and I'll tell you guys. But while I'm living on this estate." [Laughter]

Cohen: Who was this that you were telling this to?

Patterson: Claude Allègre; he's a French guy. He was disgruntled because somehow God had given me that number. [Laughter]

Anyway, this miraculous number that I got—you asked me how I got these numbers. It was hideous! Back and forth, and up and down. Look, there were [no records] directly. There were none saying, "Well, the Romans produced so much lead." I had to figure it out indirectly, about five different ways, things put together in a logical sequence until you arrive at production. One of those, for example, was, How long did a silver coin last once it was minted in Roman times? How would you go about figuring that out? Well, I had to start with silver coins. What is the life of silver coins in the United States today? We have some data. You may not know this, but the U.S. government had required certain national banks to take the coins in the bank—so many thousands of coins—and count them and look at the dates on the coins. They had to do this for a whole year at various places. And from that data—knowing the quantity that had been minted in a given year—they had these banks all around the country measure the abundance. From the frequency distribution of the coins, you could do a mathematical equation and find out how long they lasted. So I knew what the half-life of coins was in the United States today. So then I could work back, and I got some data from England, and I got some data from Canada. Look, you had to make corrections. What would the corrections be for robbery and pillage and loss on boats that sank, that sort of stuff. I went through all of this. I came out with a half-life of silver in the Roman era of thirty-five years. A half-life means, if you start out with 100 silver coins, how many years before half of it's gone? Thirty-five years.

So in about eighty years, virtually all of the silver stock upon which the power of the Roman Empire depended was gone—and these historians, they don't give two hoots about this. I wrote some papers about this stuff, and the only people who cared about it were *Time* magazine and a few other people. Historians didn't give two hoots for the fact that it was coinage that was a crucial factor in allowing business to operate, which in turn divided the power responsibility of the nobles into separate entrepreneurial segments and broke the political power structure that existed in the Sumerian and Babylonian and Egyptian times before. The Greek Empire invented coinage around 700 B.C., and *bam!* You had business, and why? Because the nobles each became business entrepreneurs and they had to have the power and they had to divide up into equal representation of their various powers within that social power structure. And no more did you have a powerful emperor. That was gone; it disappeared. But in this new environment, art, philosophy, mathematics, these things were allowed and nurtured into separate social institutions. You could actually have a philosopher who is nothing but a goddamn philosopher. You couldn't do that before in this pyramidal structure.

And this was all because of coins. And where did the coins come from? Lead.

Cohen: So the silver was just a little bit.

Patterson: It was just a little tiny bit [in comparison to] the lead. So we're looking at one of the world's greatest evils being the mother of science. [Laughter] And do you think these historians would pay the slightest bit of attention to this? Nobody gives two hoots about it.

So I calculated what was the rate of production. I went back in time to estimate for various factors, and this is one example—the half-life of coins. So I got this curve for the production of lead, and I published this twenty years ago—around the 1970s. Well, some of my colleagues that I'd been teaching how to analyze lead in polar snows, they got together. I dug the shafts back to around the Industrial Revolution. I wasn't able to go back to 2,000 years ago, the time of the Romans and Greeks. So I couldn't measure what the lead concentrations were there.

And then, the ice cores that I did in 1980, they were too old. This was way, way down below 2,000 years ago.

So this French guy, he and my Australian colleague and a few other guys got together and said they'd use these new techniques, and they measured what the concentration of lead was during Roman and Greek times, through cores, using the techniques that we had developed in my laboratory here. Guess what their curve was as a function of time? It fell right on top of my lead production curve. I had published this curve. And then they called me up and said, "Pat, we've got your curve. It's the same damn curve."

How would you have felt? Do you think I was proud? No! You know what I said? "That proves for 2,000 years we have been unable to understand the evil that we are doing to ourselves and the biosphere." Because you see, this lead was coming out of the

Greek and Roman smelters into the atmosphere, going around the Earth, part of it working its way up, and was incorporated in snow that fell at the North Pole. And you know what? Two thousand years ago, they knew about lead poisoning. They could associate the ill poisoning effects with lead, but do you know who was affected by it? The slaves working in the mines and smelters. And who gave two hoots for those slaves? They were war prisoners and they were criminals. And they lived about five years. I made calculations about that: How many died in the Roman mines. There were millions over a period of 200 years.

So my response to this was. . . . Do you think it was one of feeling proud? No way! I knew the damn figures were right in the first place, but OK, this is proof. And you see, this is the other thing. When they look at what Patterson has found out, and they say, “Oh, this is true because of this work here that we’ve got,” they’ve eliminated a huge contribution to lead in people and children from lead in soldered cans. I was right there when they started, and you know, the people who manufactured welded cans wanted to make me an executive in their business. They were so grateful that the government shifted over to welded cans. It’s initially a more expensive can. And getting the lead out of gas, the same thing.

That is not a victory! We haven’t accomplished anything. It’s way back, when the Romans were mining the stuff, they shifted it to the slaves. We haven’t learned a thing about this. We haven’t learned why we think and do these evil things. And that’s what lies behind the story, and that’s why I’m in human consciousness today. Because I looked at this picture, and I asked, “Why did we do that? What were the factors that caused this?”

Cohen: Let’s get on with our chronology here. You continued this work through the 1970s?

Patterson: Yes, the calculation of production and all that stuff. We haven’t gotten to the cadavers and the Indian bones.

CLAIR PATTERSON
Session 3
March 9, 1995

Begin **Tape 3, Side 1**

Patterson: I'd like to talk a little bit about the last fifteen years or so of my work, which has focused more on what is the lead content of people. You see, before that, we were looking about the environment—the oceans, the atmosphere, plants, and animals. So I began to focus more on what about the lead in people themselves.

The main problem there is, you have to know whether or not the lead we have in our bodies today is what we call "natural." By natural, we mean that which was in human beings 20,000 years ago, when there was no lead technology of any kind. In order to try to get that measurement, make that comparison, one thing that people have considered doing, or tried to do, is to look at ancient buried bones. But what most of these people didn't understand when they did this is that from the standpoint of thermodynamics—the chemical stability of various compounds—there's lead in soil moisture that comes in contact with these bones. The lead in the bones is contained in what we call calcium phosphate crystals. The bones would absorb lead from the moisture simply because the calcium phosphate is not as stable chemically as lead phosphate. So if you bring lead in solution from the soil moisture into contact with the calcium phosphates in the bone, the calcium will be displaced by the lead, and you've got lead phosphate.

So over thousands of years of burial, you end up with more lead, tons of lead, in the bones which wasn't there to begin with—which masks the biological lead that used to be there. And this is what we discovered. There's hundreds of times more lead.

So how do you deal with it? What we did first was look, and it so happens that there are apatite crystals of different sizes in the body. Some apatite crystals have a very small surface-to-mass ratio, and those are the ones that are in the enamel of your teeth. And then there are other crystals that have a very large surface-to-mass ratio, and those are in your ribs. Then we have some that are intermediate that are in your long bones—the femur, for example. So what we did was to take people living today, whose bodies were in medical repositories.

Cohen: Now you're saying "we." Whom are you speaking of?

Patterson: Always it's I and my colleagues.

Cohen: Were these geologist types? Or medical types? Or anthropology types?

Patterson: No, they were chemists working with me from other universities, and here as research technicians. These are academicians.

So we went to the medical repositories and we got bodies, and we took out their teeth, we took out segments from their arm balls and segments from their ribs, men and women. We had a dozen or so of these people. We brought this material back to the laboratory; we analyzed the lead in these things. And this established the biological differences and biological levels of lead.

Then we went and got ancient Indian bones that were thousands of years old, from two different sites in the southwestern United States, where I knew from archeological and anthropological information that there had been no metals or smelting or making of glazes for ceramics. And we analyzed the same things—the enamels from the teeth, enamels from the long bone, enamels from the ribs. And what we got was a relationship of different amounts of lead being added to the bones and the buried bones, depending on their size. In the people of today, they all had about the same concentration of lead in them. But do you know where that cluster of lead was? It was 1,000 times higher than the relationship to the lead in these Indian bones. Of course, it rose in the Indian bones to come within maybe a factor of 10 of what this cluster was. But it never got that high in those bones, and it varied by a factor of 100 within the different types of bones themselves.

We went through a lot of analytical figuring out. We looked at barium also—both barium and lead in these bones. And looking at the barium-to-lead ratio, the lead-to-calcium ratio, we were able to work out mathematically a whole lot of stuff. That way, we could then understand what made up the tiny residue of biological lead, which happened to be in the enamel of the teeth of these ancient people—what was the natural concentration of lead in their bodies. Because we showed from the cadavers that it would be the same today. And that’s where we got this 1,000-fold difference.

Now, I had predicted, decades before, [that] there was a 100-fold difference. Well, it turned out it was a 1,000-fold difference. This is for the average person living in the United States today—now of course there’s a variation there. And that 1,000-fold difference is only a factor of 4 smaller than the concentrations we know that you have in your body when you are in the hospital with some acute form of lead poisoning of one kind or another. You go from 1,000 to 4,000, you go to the hospital. Well, what about from 1 to 1,000? Shouldn’t there be something wrong with you there?

I myself didn’t ask at that time when we made these discoveries. Many of my colleagues immediately jumped on this—not the people working with me but scientists who were concerned about the environment and about people being hurt by all this. They wanted to use this information—and they did—to reduce the effects of lead on people and the environment today. This is important in getting lead out of gasoline. It was crucial in getting lead out of food-can solder and getting lead out of glazes. Actually, now they’re getting lead out of paint.

But I myself asked, “What is the meaning of this? How did we think? What led us to poison the earth’s biosphere with lead?" Then I, therefore, shifted to trying to figure out how we thought.

Well, this is [when] I evolved this new concept of human consciousness in terms of pathways—neuronal circuitries—that are used within the brain to think in two major different modes. And what I've come up with is a utilitarian type of abstract rationalization. Now, this is not low-down level, first-order type thinking. This is, "Oh, I have a problem. Now, how shall I solve this problem?" You're confronted with a problem, so you think about different types of solutions. And there's a logic involved in this type of thinking, so it's abstract rationalization thinking, in response to environmental challenges of various kind.

The new concept is: One response is a utilitarian type of thinking, and the other is a nonutilitarian type of thinking, in the sense that one type of thinking is where you're involved in a conflict with the environment. You're trying to solve and deal with conflicts in the environment presented to you by the environment—social, physical, all kinds of things like that.

The other type of thinking is: You're not in conflict, you're trying to understand. The individual brain sees something, or becomes aware of something, and it asks, Why? Not "How can I solve this challenge?" But "Why is that? Why is a drop of water spherical?"

Cohen: You're talking philosophical?

Patterson: No, it's nonutilitarian. Yes, of course, it could be philosophical; this is one subunit of that type of thinking. Artistic thinking, that is also it. It is the type of thinking that is involved in true science, the type of thinking that's involved in the formulation of religious myths. In other words, religion, art, science, philosophy, history—all these are nonutilitarian types of thinking—provided you're not an economic historian, you see. Provided you're not involved in medical research to discover a cure for cancer. That is utilitarian thinking. That is not what I call true scientific thinking, which is just for understanding for its own end.

I got into this when I first measured the age of the earth. No one cared about it. Even today, people don't care how old the earth is. In fact, less today than forty years ago, when I measured it.

Now, why did I come up with this? Well, first of all, I got into this because I had worked out some of the basic knowledge concerning lead production over the past 10,000 years, the metallurgy that led to the production of lead. I've taken other people's information and put it together into a story related to lead. I actually worked out the production of Roman lead. Extremely complicated and very sophisticated, and I worked all that out.

But this also got me to understand the relationships between social interactions and—in this particular case—the metallurgical developments and metallurgical technologies. By golly! For example, historians haven't paid the slightest bit of attention to the fact that the need for getting silver out of lead is when lead production really began—when they wanted the silver to make coins. The Greeks discovered coinage. As I told you before, this is crucial. This is a fundamental aspect of factors related to the development of the thinking. I discovered in all this work that there's a crucial difference in the archeological evidence for the New World and the Old World and corresponding stages of development of cultures. In the New World, in the music, there are no chords. In the Old World, there are. But the stages in metallurgical development are identical. They are identical, over 5,000 years in two different places. There are three major stages and substages. These are not single people doing this; it's not even one generation. This is over 5,000 years in both places, doing the same stupid things! But not for music, not for cosmogonies, not for language. Therefore, it's obvious that although the brain thinking often follows different patterns within these two cultures, when they think utilitarian, they think the same. And if they are the same, then the networks that are used are tight, frozen—I call them hardwired. But the networks used for nonutilitarian thinking, since in the development of the brain—the HSS [*Homo sapiens sapiens*] brain—through hominid evolution over the past five million years, the utilitarian type of abstract rationalization is crucial to this evolution. But not the nonutilitarian thinking, the meaning of something. Why is a drop of water round? That wasn't helpful in order for the tribe to endure. But it was helpful in the following way: There was a cohesiveness, a binding, that was related to that type of thinking, which was expressed in terms, say, of myths, of cosmogonies. In other words, religion. It sort of bound the tribe. And those tribes that were held together more strongly by that type of thinking were more successful in surviving than the ones that were more erratic and independent, individualistic. Therefore, there was an evolutionary factor in the development of this ability, the potential to think in this nonutilitarian way. But it wasn't so hardwired. And that's why we see a difference in the Oriental background and the Caucasian background. There has been a genetic drift between the two populations.

Cohen: Where is the stage where you present these ideas in your field?

Patterson: Just in recent years, I published a paper presenting this idea. And when I was developing this concept, I talked with Roger Sperry about this. And he said, "Pat, go to it! Write your paper. This sounds pretty good." You see, the idea is that different neuronal networks are used for these two different things, because if you had the same neuronal networks, then not only should the technology be the same but the music and iconography should also be the same. And they're not. And with this demonstrated contrast, that means that we had to have a genetic influence. That means it had to influence—what? And I've said it influenced the neuronal networks. We have regional differences in the brain. And that's when Sperry and I got together, because he was the split-brain man. I didn't get the paper published until after he passed away [April 17, 1994]. But now I have some brain people interested in that idea. And they said, "Pat, you have to modify this." So I'm working on this, and trying to redevelop this. I'm working on this now, but in the following way: This is a fundamental foundation to do more. Namely, to try to elaborate to the very primary beginnings for youngsters starting out in different fields—in history, in philosophy, in religion, as well as in just science itself—to try and introduce scientific types of approaches to understanding these topics. The words that I'm using are not understood by present historians, by present theologians. They don't know what I'm talking about, because there was a lack of communication. So I'm going to try and write and communicate with young people, whose minds haven't been frozen yet.

Cohen: Where are you going to find these young people?

Patterson: I'm just going to try and write it. I don't think I'll live long enough, even to get it finished. I'll try. You see, they have to look at what are the causes and the factors. You have to go back and look at it scientifically—the historical development of our thinking. We have to go back 9,000 years. We have to contrast the two main different cultures that we see in the New World and the Old World. And then we have to take segments. It's easy to take metallurgy—it's a material thing. You see, science today has always been focused on the material world. So the academicians in other fields—the humanities—say, "Well, science, that's not the real thing. We want to go with human consciousness. It has the spirit." And you see all these things about, "Well, it's the spirit that counts. It's not this reductionist logic by the scientists." But it is the stupid reductionist logic by the scientists, which is true, that has arrived at the point where we can look at ourselves in a way that these other guys who are truly stupid can't do. Because they do not have a logic matrix, a matrix of logic that depends upon its development in the way that science does. The logic matrix of science is 2,500 years old. There isn't any logic matrix of art, or history, or anything that is that old. There's just chunks and pieces of it floating around, unrelated to each other. I want to introduce a coherence. I want to take those things and have some young people take those things and bring them back and start tacking them onto the logic matrix of science and branch out from there.

Cohen: So this is how you envision your work now?

Patterson: Yes. And through writing, and perhaps through some lectures, and so forth, I might get some of this started.

Cohen: So you'll be here part of the time, and up in your place in The Sea Ranch [near Santa Rosa] part of the time, doing this?

Patterson: Yes, both places.

Cohen: So you're no longer in the laboratory. You're now working in the world of ideas, using your scientific background? Is that correct?

Patterson: Yes.

Cohen: Let's discuss some of the honors that you've received and which have meant something to you.

Patterson: Well, half a century ago, when I came here, I had a reverent regard for the Nobel Prize. See that picture of Urey there? See the other guy up there? He's a very famous physicist, cosmologist. Anyway, these guys, Nobel laureates, I knew they were good scientists and I respected them. And therefore I respected the Nobel Prize, because of that. But this award and honors business, I'm just not. . . . Well, OK. In the basic operations, it is a manifestation of something worthy in science—namely, a bonding. It's an activity that tends to bind together. I say, it is an activity. Only part of this activity has that property of welding and bringing together and strengthening and leading the scientific community to go on, giving it vigor and power to proceed. It is the work by the colleagues to get those prizes awarded that's really important. They have to go around and they've got to argue and fight and quarrel and try to convince other people that what one of their members has done is worthy. So it's sort of a manifest trying to say, "Look, what we're doing is great, and here's a person who's doing what we're doing and therefore should be recognized." So it's a welding type of operation.

On this Tyler Award, for example, I made a slide of all the people who had worked together on this stuff before the award was made.

Cohen: This is an environmental award, isn't it?

Patterson: Yes, an environmental award. You know, the name Patterson is lost in the cluster of people involved in that thing. It was a community effort of people working together, believing in each other, and developing each other's ideas, and putting things together. It isn't a single person.

Cohen: No, but it doesn't take away from the pride of having gotten the award.

Patterson: I don't have any pride, I'm sorry to say. I have zero pride in any award. All I feel is obligation, obligation, and obligation. I'm sorry, but that's my personality. I feel obligated and obliged. When I was down in the Antarctic, I was kind of crusty and did things, but now my work has been recognized per se in the Antarctic. They named a mountain peak after me. They've named an asteroid after me.

Cohen: Instead of the word "pride," can I use the word “pleasure,” gratification?

Patterson: No, not gratification. No, it is an awareness of the worthiness of a communal spirit of science. It’s not a personal thing at all.

Cohen: But it’s still the person that gets the award.

Patterson: That doesn’t make it personal. It’s simply a manifestation, as far as I’m concerned. Now, I have colleagues who are intensely personal about this stuff. They are gratified. But I am not. And that’s the way it is. Period.

Cohen: Can Laurie be proud of you?

Patterson: Oh, yes. But it’s very difficult for her to get along. I certainly wouldn’t marry myself if I were her. Bless her heart. [Laughter]

Cohen: You were elected to the National Academy of Sciences some years ago [1987]. Did you have any feeling about that?

Patterson: Yes. Instantly I focused my analytical propensities on the function and operation and nature and quality of the Academy. And the Academy didn’t rank very high. The defects outweighed the positive aspects by many orders of magnitude. [Laughter] Some really eminent scientists have resigned from the Academy. But I didn’t want to resign, because I think there are a lot of good people in the Academy who are trying to do some worthy things. And my mentor, Harrison Brown, he worked hard trying to do what he did. He accomplished a lot of good things. I think the Academy needs gigantic reshaping. [Laughter] But how much time do I have? I’m trying to save the world. If I could help save the world by remodeling the National Academy of Sciences, I would do that. But I don’t know which is more important to do first. [Laughter]

The Academy needs reshaping badly. You see, I participated in Academy operations, the National Research Council, which has functions that involve solving problems—social problems the government needs to have solved, that sort of stuff. I don't know whether you've seen some of the reports they've issued. Some of them are totally . . . . Oh! Have you seen the last article put out? The second revision by the National Academy, on what is a scientist? I'll tell you it is totally, completely, absolutely wrong. The people who wrote that are not scientists. They do not know what science is. That's one of the things wrong with the Academy. They go around and pick out these—they're not members of the Academy who do this stuff.

Cohen: So you're a member of the Academy, which is the highest honor a scientist can have in this country.

Patterson: There's an obligation there, though. And so you look and you find out what's wrong with these things, and you find out they're terribly wrong. It's coupled with an obligation.

I'm the only guy to have turned down a professorship at Caltech.

Cohen: Now why?

Patterson: You can't tell from my answers to all the other stuff? I think the goal of tenure of young scientists is wrong for scientists. It's totally improper to seek tenure as a goal. What you're seeking is the exhilaration, emotion, of discovery, of an understanding—that's what you're seeking. You are not seeking tenure! All right? So therefore, like a fool that I am, I said, "No, I will not. This is wrong." Well, that made everyone angry. But that's the way I am.

Now, at the end of my career here. . . As a faculty member they took care of me all along, and then finally, at the end, they said, "Well, Patterson, would you please sign off as a professor, and then things will be all right." So I said, "All right, I'll sign off as a professor."

What I said is that they should grant limited tenure—just a decade or so—to young people, so that they're free to make stupid errors and make silly asses out of themselves and not be fired for ten to fifteen years. Then, at the end of that period, they would come back to peer review. There would be no people tenured for the rest of their lives, only subject to peer review to see how you're doing. Look, after tenure, most of these guys just lay back.

Cohen: But you can't just throw them out in the cold.

Patterson: Yes, you can. The peer review would make them shape up. But the whole thing is too bureaucratic. And I'm wrong; it won't work. So I said, "To hell with it!" I'm not that kind of person; I can't handle this type of stuff. Here I am, I'm going to save the world, but I don't know how to do it. So how the hell can I do it?

My relationship with my colleagues here is one of. . . . I tend to be. . . . I'm a very withdrawn person. You saw my office here—double wall, double door, double windows, no sound whatsoever. I look out over the mountains. But this solitude needs to have a foundation of interpersonal relationships with your colleagues. It has to be there, otherwise you can't go on. I remember Sam Epstein, who's been my colleague for the last forty years. Ten or fifteen years ago, he got sick on something. I went roaring into his office. I said, "Sam, you cannot be ill with this sort of stuff. You are obliged to remain healthy and stay alive while I'm alive. Now, I don't give a damn if you die after I do. But before then, I've got to have you around. Don't get sick!" I yelled at him like that.

There is this need, you see. They have to be there. But I am a solitary person.

Cohen: You must have some good feeling for this institute for allowing you to live this way.

Patterson: Oh, yes. The institute has a whole lot of things wrong with it; everything has something wrong with it. But because this institute existed, I existed. Had this institute not existed, I would not have existed, and that’s a fact.

Cohen: You don’t think there would have been anywhere else where you could have lived this life?

Patterson: No. I would have been a molecular biologist at the University of Ohio, fighting, unhappy, quarreling, and not being able to accomplish anything. Caltech provided this environment inadvertently; it didn’t do it intentionally. It was just there. It just happened to be coincidental, and quite advantageous to the whole thing. It was a magnificent opportunity.

Cohen: Probably a symbiosis, one gave to the other.

Patterson: Oh, of course. Well, everything is opportunistic and environmentally determined. Look, I’m stupid, all right? I’m not some brilliant person. I’m a little child. You know the emperor’s new clothes? I can see the naked emperor, just because I’m a little child-minded person. I’m not smart. I mean, good scientists are like that. They have the minds of children, to see through all this façade of all this other stuff that they know is stupid nonsense. They just don’t see it the way other people see it. So, I’m not smart.

It’s only circumstantial. That’s why I don’t feel any honor; I don’t recognize honors. Because it’s not there; I’m not qualified to be honored. It’s accidental.

Cohen: And you’re just the vehicle for these things to have happened; and the name Patterson is on them. Is that what you’re saying?

Patterson: No, the individual is crucial. You see, everyone of us is different. We have these crucially important, significant, totally unique contributions that each individual can make, and environmental circumstances determine whether or not that particular individual can make that contribution. And in my case, it was true. That doesn't mean other people don't have a similar potential. There's a variety of different kinds of contributions that people can make.

Cohen: Not having been a professor all these years, you've probably not been allowed to teach.

Patterson: Yes, I have. Not being a professor doesn't mean you don't teach. I didn't give a damn for the title. I functioned as a professor.

Cohen: Did you enjoy teaching?

Patterson: Of course! I did a lot of teaching. But I am not a good teacher in the following sense: I don't pontificate. I interact with students in a class. Most of my colleagues are like me in that we're not very good . . . . OK, I'm probably mediocre as far as being a classroom teacher, but there are some who are exceptional as classroom teachers. Now, the real teaching is to bestow or endow the fire and the passion in your students to go on. To create new scientists. Now, some of my colleagues have had a marvelous power to do this. I regard myself as a failure in this regard. But I did better working with visiting young professors from other foreign and domestic universities, who already had the fire. I considered my students as colleagues. We worked together, shoulder to shoulder. And when you do that, you sort of take away the opportunity for individual awareness of the glory of discovery, of emotion. I shouldn't have shared it with them; I should have allowed them to experience it individually. And then that fire would be better, more effective. So the people I worked with best were young people from other institutions that learned from me. They already had the fire, but then it became inflamed. They knew which direction to go, and then they took off like rockets. But that wasn't me. They were already set to go; all I had to do was light the fuse.

These college students and younger kids—there's where the real teaching is. My wife, Laurie, was that kind of a teacher. She tended to know how to do that. And you know, we never understood each other. Each one of us would talk about our stuff. I would go visit her classes. But she was structured and shaped the way that her students—

Cohen: But she worked with younger students.

Patterson: But that's crucial. That's where you begin, to work with these young brains. That's a crucial time. That's when I started, in this little old town, when I built my own chemistry laboratory. And that's the kind of teacher Laurie was—none of the rote business, sit there behind a desk and get up and write on a chalkboard, this sort of nonsense. So, I was not that great kind of teacher. But in firing already young scientists-to-be, I did pretty well that way.

Cohen: Would you have anything different if you could do it again?

Patterson: Well, there's a Nobel laureate poet, [Albert] Camus. He wrote an essay called "The Myth of Sisyphus." It dealt with suicide—about being alive or dead. I'm a manic depressive, of course, because I'm very, very depressed overall, all the time. But being alive physically is only a manifestation of being alive emotionally for 100,000 years. You see, we don't die really. [Laughter] Physically we do, perhaps. But we're part of a whole. We're a unit; we're *Homo sapiens sapiens*. We are brain containers. This thing generates thinking and emotions. Here's the important thing, up here. Now, right now, the way things are right now, the emotions within an individual brain can't be communicated. You don’t know the emotion of the artist who composed that music. We do not know.