An Oral History of

JAMES ARNOLD, ERNEST ANDERSON and R. ERVIN TAYLOR

On JUNE 3, 1996

ARNOLD: Oral History tape devoted to radio carbon dating at the University of Chicago. It's June 3, 1996. And this tape or series of tapes is the first of a series which I hope to create and leave with our friends at the Special Collections at the UCSD [University of California, San Diego] Library on various aspects of major scientific things that happened that I have been around for. What we're going to do today, then, is to talk about the experience of two very young apprentices with Willard Libby and the late 1940s. And I thought we'd start by Ernie Anderson and then myself describing some chronology of our connection with the radio carbon project. And then, we'd proceed to questions from Dr. Taylor. Oh. Dr. Erv [R. Ervin] Taylor, UC Riverside, author of the book on the history of radio carbon is with us. Dr. Anderson and I were co-authors with Dr. Libby and young apprentices with him in the creation of radio-carbon dating. Ernie, why don't you begin then with how you became connected with Libby and the project?

ANDERSON: Well, I first came to the University of Chicago in the late winter of 1942 to work on the metallurgical project which later became the Manhattan district. I was employed then as a graduate assistant who was assigned problems in analyzing various special materials. As a graduate student, then I was able to start taking some courses. This was interrupted two years later in '44 when I was transferred to the laboratory of Los Alamos to continue making like an analytical chemist. When the war was over, I returned as quickly as possible to Chicago. I got back there, I suppose, probably February of '46. I resumed my graduate student status taking the required courses and I applied for a graduate assistantship to help pay for things. Libby chose me as his assistant and my first assignment was simply to build up the basic laboratory.

ARNOLD: Ernie, when did you begin with Libby?

ANDERSON: Began— Unfortunately, my memory is quite hazy about this. I believed it happened quite soon after I returned and after I got into the swing of things. Whether it was still in the spring of '46 or whether it was later—I don't know. Libby's laboratory notebooks say that period would probably establish the date at which we really began doing something as opposed to the scuff work of building a back line and collecting shielding and making some counters.

After the lab was running and my guess would be that this was perhaps the summer of '46 subject to verification in the notebooks. Libby said that he had some samples of methane coming from the Houdry Process. This would be paired samples of biological and enter methane and they had been isotopically enriched by [Aristid von] Grosse and his crew. We expected them to find radio carbon in them. So, for some period of time, and again the time scale escapes me, I measured these samples of methane and the gas sample powder and sure enough, just as Bill had predicted, we found radio carbon at approximately the right level. The measurement was fairly easy because of the isotopic enrichment and the amount there was up by factors of several hundred. The next step was to eliminate the need for the isotopic enrichment which was very slow, expensive—required lots of material. And Libby suggested that I should take this on and try and adapt his screen-wall counter to accomplish this task. I'm not quite sure when in the scale of things this became my thesis assignment. The discovery of radio carbon was definitely done while I was simply an assistant. My name is on the discovery paper, but I was really just doing technician work at that time. When I passed around for a thesis subject having gotten to the point of qualifying for thesis research, Bill suggested that maybe I'd like to continue and measure the contemporary essay. In other words, how much radio carbon is there present in a new sample? Does it vary with geography or latitude or anything of that sort? And by that time, we had the screen-wall counter adapted so that we could measure the contemporary essay without too horrendous an operation. At that point, I asked him, well, what about the work on the discovery. Will that be of any use? And his only reply was "Well, it won't hurt." But in point of fact, my thesis was entirely on improving the apparatus and on measuring the C-14 activity in contemporary samples. In the course of which the apparatus seemed to be stable enough and sensitive enough that it might be applied to old samples and the possibility of archaeological dating seemed reasonable. At that point, Jim joined the team and he became responsible for the archaeological side of it. I was responsible for winding up the contemporary essay and helping to keep the apparatus running. Which Jim says was very difficult, in which I seem to remember as a breeze. But that's a matter of opinion. In the spring of 1949, I had completed my thesis and being very discouraged with the city of Chicago and the housing available and the number of personal problems, I immediately ran back to Shangri-La in Los Alamos and stayed there for the next thirty-five years. Okay. That's mine.

ARNOLD: Okay. Let me move in with a similar history. I got my degrees, including my graduate degree, at Princeton, connected with the Manhattan project—the part of it, which Harold Urey ran the separation of uranium-235. Looking around for what to do at the end of the

war, one my teachers, John Turkevich, without asking me, recommending me to his brother Tony Turkevich who was a new faculty member at the University of Chicago as a potential postdoctoral fellow at the university. And I dually was awarded this fellowship and turned up in Chicago early '46. There was surrounded by infinite amount of high-class intellectual talent, Noble Laureates, and future Noble Laureates in droves—and one of the people there was of course Bill Libby. Being a chemist, I wanted to learn about radioactivity and I was going around talking with the five or six nuclear or nuclear-related chemists there. I'll jump over some other things and describe my first interview with Bill Libby when I got to him on the top floor of Jones in his office at that time. The first thing that he did was suggest to me— He wanted me to come work with him. He suggested to me the topic of determining whether there was a stable tritium isomer along with the known radioactive tritium isomer that had been discovered by Alvarez a few years earlier. I rejected this problem. I thought there wasn't and I didn't see why should go to a lot of trouble to find out something where I thought I knew the answer. He then pulled another folder—I don't remember that one. But he tried to sell me that project and couldn't. And then, I'd never forget this. He pulled out the drawer in which the folders were and to my eye there was something like fifty of them. Each of them with a few pages. And I thought to myself, I'm not going to get out of here. At any rate, the third folder he pulled out revolved carbon-14 in a different way. Martin Kamen, a dear old friend of mine now, had with Samuel Ruben discovered the isotope carbon-14 just before the war—'37 or so. And he made a small amount enough to verify that it would indeed be a useful tracer isotope. What Libby had done near the end of the war was to use his influence to put a slug of beryllium nitride into the Hanford plutonium reactors. And he headed back. And it contained by calculation a millicurie, which is a large useful amount of carbon-14 activity. In my task, I wanted to learn about Geiger counters and everything. I'd never seen one despite being on the Manhattan project. My task was to make that chemical separation and in return, Bill undertook to teach me how to make Geiger counters and use them and got me into all the technology as it then was. This took about two months. He was marvelously patient with me. And in the end, we had barium carbonate containing this radioactive tracer. And I moved on to work with first, Edward Teller and that was six weeks and then with Nathan Chugerman [?] who taught me a great deal. And that was my year at Chicago. I then went off to Harvard for year. I had another postdoctoral fellowship there. And near the end of it, the events which Ernie has described, the progress that had been made and the funding which the Viking fund as it was then called—later, the Wintergrand Foundation [?] had provided to him. He called me up just when I was starting to look for a job and offered me a position on this project. Now, stepping back a little bit, I was intensely interested from day

one. My father was a very serious amateur archaeologist. He was the American secretary of the British-Egypt Exploration Society, for example. Which means a money raising position. And I had grown up with Egyptian archaeology. When I told my father about this—Christmas time 1946—he got very excited. And when I got back to Chicago, there was a box from the Metropolitan Museum in New York, from the curator of Egyptian archaeology, Ambrose Lansing. With ten samples in it. We were all very much amused later particularly by the dry dregs of Egyptian beer which was one of the ten samples. But I was very much chagrined from receiving this box from Mr. Lansing because as far as I knew in 1946, this was a conversation piece. This was a fun thing to think about. I hadn't really been aware of how serious Bill Libby was when I showed him the package in January '46 just before leaving for Harvard and apologized to him for overselling the project first to my father and then through him to Lansing. Bill's reply—often he didn't use many words— I don't remember his using any. He simply took the box from me—

TAYLOR: Excuse me but you mean '47?

ARNOLD: I mean January '47. Thank you very much. After Christmas. And he took the box, put it on the shelf above his desk. And I said to myself, "My God, the man is serious." And went off to Harvard and didn't think much more about it until I got the phone call from him late in that year inviting me to come back. I do not remember hesitating at all. I simply joined the project. One little amusing footnote is that Lansing had said to me and to Bill that these were unknown samples. He wouldn't tell us the dates because then— He said answers could come to him and he could check. And having grown up with Egyptian archaeology, they were not unknown to me. So, we treated them in that way and they were the known samples. Now, all the things that Ernie described had gone on in my absence. The discovery, the building up on the equipment, and the coincidence as well as the use of this screen-walled counter with solid carbon for the measurements. So, when I got back, it was possible—and I underline the word possible, not easy—to make measurements of contemporary carbon without enrichment. However, all the same, one of my first tests from Bill on my return was to supervise the construction of an isotope separation column and a thermal diffusion column. I knew nothing about thermal diffusion columns at that time, but there was a French engineer, Denis Tanguy [?] from the Houdry Process Corp. And he came around and we—he designed the column and I checked his calculations and then set up the construction of it at the famous University of Chicago shops, which was my introduction what a magnificent machine shop can do. So that was built. When I asked Bill Libby why he wanted to, his remark was that he still was not sure that this was going to be precise enough to do good dating and, in any case, it would extend the range of dating

availability by twenty thousand years or whatever. It was the enrichment level. That turned out to be a blind alley. We calibrated it. It worked. When last heard from it, it was still in the stairwell at Kent, but was never used. That, I think, is enough on the chronology. Erv, we probably should cut you in for what you want to talk about.

TAYLOR: One of my interests here is to ask the questions of the people who were there when this technique was in its developmental phase. For those of us who came later, we're awestruck by the impact it had. Trying to untangle how it originally got started. Let me ask you a question. At the time— Neither of you at the time that you began the project, how much did Bill Libby explain what he was up to in terms of creating a dating method? How much was he explicit to you about what he was up to?

ANDERSON: Well, to me— This is Ernie. To me, I don't recall that he said anything about that. I think we were always talking about the contemporary essay proving the existence and that sort of thing. Somewhere along the line, I must have known about the, namely, detailed measurements of the contemporary essay. As I recall, was presented to me as not only verifying the calculations, the predictions, but also providing a base line. But I really can't say when I became aware of this.

ARNOLD: My story is quite different. You see, whenever it was that we first talked, I had the advantage of this family background in archaeology. And I have always thought that one of the reasons that—I hope it wasn't the only reason—Bill Libby called me up and invited me back was that I had that background and I understood how rare it was in archaeology broadly speaking that secure chronologies existed. Egypt was one of those places which is why it was a good place to start. But whole broad areas of archaeology were dependent on very indirect reasoning of one kind or another for dates. And as I found out later, archaeologists had sometimes had disputes ranging into decade over decades as to what was the correct date for this particular horizon. So, I certainly felt that the story was queer and that was of course then the part I was invited to participate in so naturally he shared that. I became— In fact, when I arrived, he turned over to me, for better or worse, the relations with the archaeologists. At first, with certain individuals. I'm thinking of John Wilson at the Oriental Institute from which we got ill-fated sample or two. And then later, the committee that was set up— One of the smartest things that Bill Libby ever did was assemble the committee headed by Frederick Johnson. And I was the interface with them, and later with the individual archaeologists who sensed that.

TAYLOR: But Christmas of '46 appears to have been a crucial date. And you heard of—

ARNOLD: As far as I remember, that's when I heard of it. I don't know how I missed the June paper which made some very serious hints in the physical review on that subject. But as far as my memory extends, it was before Christmas but not a lot before Christmas.

ANDERSON: Well, as I recall, Bill later said that he didn't want to talk about this because of such an absurd idea would be ridiculed. I'm sure he was really protecting his priority.

TAYLOR: This is a question I wanted to ask. It's come down in tradition that one of the reasons that he didn't talk about it early on was that— Well, let me ask a question rather than offering it. Why did he not talk about it earlier? He later said that he had come up with the idea when he read a paper in 1939—corps paper about the presence of C-14 [carbon-14]. So, if he had the idea in '39 and you didn't hear about it until Christmas of '46, what was going on in the interim?

ARNOLD: Well, the Manhattan project was going on in the interim. Let's remember that Bill Libby was an assistant professor in 1949. In fact, one of the most interesting transitions was his transition from a very brilliant and promising youngster at [University of California] Berkeley among other brilliant and promising young faculty members there. Going off and spending some— Harold Urey told me that Bill Libby and Tony Turkevich were the first two people he hired for the Manhattan project. For his part of it. So that must have been '41 or thereabouts. So, he spent the whole war there. And then, of course, he was in very close contact. He became a senior there and made major contributions. In particular, the patent on the diffusion barrier and the name of those two people. Proud of that. So, he arrived in early '46 having had no real opportunity to do anything about it at all. And of course, with neither equipment nor money, an empty room for a lab and all of that sort of thing. So, there is a lot still to do. And that's where the time—

ANDERSON: Well, your beryllium nitride is an important thing at this point, isn't it? In other words, the half-life of C-14 was quite up in the air

ARNOLD: It was between a thousand and a hundred twenty-five years in the tables. So, when I first arrived.

ANDERSON: So, it really had to be tied.

ARNOLD: You'll recall that Libby mounted a campaign with Antonia Engelkeimer as the corresponding person to Ernie and myself to make the measurement of the half-life. That was going on in parallel while these other things were happening. And that was the first really good— I think there was already a measurement somewhere around five thousand years, but it was very crucial.

TAYLOR: There was a footnote in Libby's book that someone—and I'm never quite sure whether with him or somebody else at one of those—at one of the Manhattan project centers had measured the half-life and it was wildly off. I mean, the value in the footnote—and I looked it up—was something under twenty-five thousand with error of ten or fifteen or twenty percent.

ARNOLD: I believe that.

ANDERSON: Yes

ARNOLD: His was certainly was the precision measurement and it held up very well. Tony Engelkeimer but as she was in the same position I think that Ernie and I were in. The apprentices.

TAYLOR: Let me ask question. Getting a little ahead of the chronology, but picking up on this point of the half-life. As you know, the original was on the order of fifty-seven hundred, which in retrospect was closer to the number that was subsequently used for the calculations in fifty-five—little over fifty-five hundred. Do you happen to know why it was chosen to substitute—? Do you remember the dialogue going on at the time when they decided to use the so-called Libby half-life rather than the original experimental thing?

ARNOLD: Well, there were two separate determinations. There were two separate papers. And— What is it? Fifty-five eighty-nine or whatever the number was. Was also a Libby number, I believe.

TAYLOR: But in the book— In the text of the book, he lists a series of calculations. This is where I saw the first reference in the footnote to the attempt to determine what it was during the war. And the he says that he— In the book and in one of these early papers, he had also said that he had averaged the most likely values and come up with the fifty-five sixty-eight values rather than the original fifty-seven hundred number. Do you recall any dialogue or discussion?

ARNOLD: I think there was some discussion but I think my fallible memory— I seem to have a little more confidence in mine than Ernie professes for his but we should all remember that it's almost fifty years. Is that it put heavy weight on the second published half-life of Engelkeimer and Libby. Incidentally, we should remark in connection with these papers that Libby used every possible means not to be the first author on any of these papers, although he was the senior author in every possible respect. Ernie and I schemed several times and I remember the one that was caught by him in page proof and corrected it. So, these were always other people who got the credit. But at any rate, that's my memory and I'm afraid that's about as far as it goes. I don't know whether anyone else's determination was ever in that or not

ANDERSON: I thought that— Did Campbell have a number that was very close? But averaging available data, of course, is the usual way of compromising when there are discrepancies.

TAYLOR: As you know, as discussed when they turned out, if he had kept the original number it would have been closer to what was later looked on by man in all this

ANDERSON: Sure. But this of course was not known. You had these reputable measurements. They had error bars associated with them and so the objective thing is average with perhaps proper waiting, the available determinations and hope that you're getting closer to the truth. As you've point out, you may not be. But you don't know which of the disputed values is the correct one at the time.

TAYLOR: Another question that came up and I've never heard any discussion of it other than some of the communication I had with you, Ernie, by phone— And it's not an important part for the scientific, but it's perhaps from the historical point of view. Is it— You have suggested at one point that Libby could have coined the term radio carbon himself.

ANDERSON: I believe he did.

ARNOLD: I don't know anything to the contrary. You see, there were other—

ANDERSON: There were precedents of them.

ARNOLD: Yeah. There were precedents. And also, of course, there were other radioactive isotopes of carbon. Carbon-11 was the one that the Martin Kamen and Ruben used twenty-minute half-life and that's what drove them to look for another one because for studying photosynthesis that was pretty embarrassingly short. But the— I think that, you know, many

times Carbon-14 is the label as such still. But radiocarbon dating certainly— That pair of words, that phrase is Libby's. I can't imagine there's a question about that.

ANDERSON: The use of radio as a prefix dates way back to the studies of the uranium series where you would have several isotopes which would appear at different points in decay scheme radio.

ARNOLD: Radio-thorium—

ANDERSON: Radio-thorium, radio-itinium— So that, the nature of the terminology was classing, but the particular application of the department, I believe was—

TAYLOR: In looking up some of those papers I noticed in some cases they were hyphenated—more often hyphenated than not. The unhyphenated radiocarbon— This is— Only a historian would be interested in this.

ANDERSON: Let's say that punctuation and spelling is a very fluid area.

ARNOLD: It's not supposed to be, but it is. I would like to pick up one thing that you were in and I wasn't and ask about it. You've mentioned earlier the use of screen-walled counter which of course Libby had invented before the war and used for measurements of long life, natural radioactivities, [*inaudible*] and so on. The other major change—innovation was the use of any coincidence which of course Libby did not invent. That was a technique in use of cosmic ray physics already before the war. But it was a novel idea to use it for counting radioactive isotopes. And I wonder essentially did both these things—did the first counting set up that was used to endeavored a count of radiocarbon—did those things come along at the same time or were they in some sense sequential?

ANDERSON: I'm sorry. I don't remember. I would hope perhaps if one had the Chicago notebooks which perhaps are at UCLA by now, one might be able to determine when we did what. It is certainly true and ironic in a sense that one of the first things we looked at was gas counting of carbon dioxide. Well, carbon dioxide, of course, is the worst possible founding gas of electron affinity and everything. Perhaps it was my incompetence, but we abandoned it rather quickly and Bill insisted that the solid sample counter was the way to go. And we did comparative calculations which I later published in the Danish Academy of comparing and detailed the gas counter and the solid counter. And from the viewpoint of a choice at that time, it looked like a toss-up. The screen-wall counter was competitive with the gas sample counter and

it had what we regarded as the tremendous advantage in view of the state-of-the-art Geiger-Muller counters. Tremendous advantage that you would measure sample and background alternately with the identical counting gas.

ARNOLD: I totally agree with that and I think that if you've gone the gas counter route, which ultimately took over the world, I think it would have been much more difficult at the early stage. To get a reproducible filling was certainly something that didn't come along till later. And particularly for CO_2 [carbon dioxide].

TAYLOR: Well, let me make sure I understand. This is the first time I've heard this. You originally did attempt to use a CO_2 counter.

ANDERSON: We took a look at it. Yes. As I said, my first job for Bill was to set up his counting lab which was to get it equipped so we can investigate all kinds of counters. And so, you're going to count carbon— Well, alright, can you count it as a gas? And it was not a deep-seated research objective. We didn't have any particular confidence in it, but we had to take a look at it.

TAYLOR: Do you recall what the major problem you immediately encountered with the CO_2 counter?

ANDERSON: Yeah. The darn thing wouldn't work. Yeah. Carbon dioxide has a strong electron affinity.

TAYLOR: Subsequently, the problem was traced to the amount of impurities in the gas.

ANDERSON: Well, I thought it as carbon dioxide itself that—

ARNOLD: Well, it has to be very, very pure.

TAYLOR: Very pure. Because that's where I kind of viewed it as. But the issue was— It turned to be as Jim knows— The issue turned out to be the tremendous purity of the gas you had achieve.

ARNOLD: As far as I remember, it was Gordon Ferguson who had [Athol] Rafter in New Zealand. First succeeded that. Other people used— Hans Suess used acetylene—

TAYLOR: But you used methane— In other words, you looked at the CO_2, you looked at methane— Methane worked?

ANDERSON: Well, methane was a matter of choice on the basis of the thermal diffusion plan. And in that connection, not only was methane a good gas for thermal diffusion but rather, uniquely and fortunately the city of Baltimore had a sewage disposal plan in which they produced methane by fermentation. Biological methane. Live methane. So, with methane, one immediately had easily available both petro-methane gas and biological methane matter.

TAYLOR: I'd like to also ask the question that— Also this is another question. You mentioned that you thought as when the— You were using a screen-walled counter that was originally— It worked well for you, is that right? I get an entirely different view from reading the literature and listening to other people who tried to make it work. Ferguson, for example, and other people—

ANDERSON: Jim was concerned with something that would work reliably and give him answers. I was playing with an instrument that— Gee, this thing's a lot of fun, you know. It's like your computer. There are things that fouls up on you. Well, alright, we'll learn, we'll work around them. So, I think our attitudes were quite different. In other words, here's something that some time doesn't work at all and, to me, hey, it'll work half the time. Well, to Jim it only works half the time. I think perhaps that was part of the problem. Coincidentally, Jim asked—or somebody asked something about at a coincidence counting—where it came along. I discovered only much later from when I was at Copenhagen building the radiocarbon dating lab there that we had been anticipated by at least a decade in the use of anti-coincidence shielding to reduce the background of a counter being used to measure radioactivity. I forget the name of the Dane who did it. It's published in the Danish Royal Academy. And sure enough, he had the idea. He did it. Didn't work very well for technical reasons. But if you want to delve deeply into the obscure literature and find out who first used anti-coincidence counting in this context, it was done by a Dane.

ARNOLD: It's reasonable to presume that Libby was not aware of this.

ANDERSON: I'm sure he was not. Nobody was.

ARNOLD: But coming to this issue of reliability, there were a number of stages in this process. The first encounter I had with this screen-walled counter was one of Bill Libby's pre-war counters, I believe it was. glass envelope. The center wire was by a very elaborate process that Ernie is much better with hands than I. Unhooked from the spring. There was a spring unhooked. And then you took off the end and so forth and so on. And as far as I was concerned

this was a horror from the beginning. But then the next stage was a counter mounted from one end. A metal counter mounted from one end. The final design that was used to complete Ernie's thesis and to publish—do the sample on the first date list was due to Bob Schuch [Robert L. Schuch] whose name needs to be mentioned here somewhere. Our technician on the project. Someone that Ernie had known earlier in life and persuaded to join us. He was just very good, practical technician and engineer. And these were— First, they were still waxed together with this wonderful awful de-condense ski wax that Bill loved. Then later, O-rings were used which Bob persuaded Bill. Bill was very hard to persuade to improve any piece of apparatus that worked however marginally.

ANDERSON: If it works, though—don't fix it.

ARNOLD: Yeah. They're famous— The notorious example that I first encountered, there had been the circuitry for handling the pulses and making the—driving the mechanical recorder which cyclotron specialties recorder which moved a relay every time there was a count. And Ernie had lashed together a breadboard with, I remember, some brass sheet and some screws— Just literally on a board.

ANDERSON: And a tube hanging upside down.

ARNOLD: Tubes hanging at loose ends— Just to see if it worked. And that was in use for years because Ernie and I between us could never persuade Bill that we ought to be packaged nicely and so on. It was sitting on top of the shield. And I think it was only after it had fell off the shield at one point and smashed that he was with great reluctance agreed to turn it over to the electronic shop and let them— You know, give them a nice proper box.

ANDERSON: Jim wasn't the only one who had trouble with the screen-walled counter. Some other of Bill's students who had to use it in the course of their investigations would curse it roundly and would be unable to put it together—

TAYLOR: Now, your criteria of what had worked if it worked fifty percent of the time it worked.

ANDERSON: I thought that was doing pretty good, yeah.

TAYLOR: One of the things that— The question of what type of counter to use was that in the literature later it suggested the reason that he chose to use screen-wall counter was the fact that it gave Geiger pulses as opposed to gas counters.

ANDERSON: That was certainly part of it, yes. Yes. Amplifiers were a problem in those days. Especially for people who didn't really understand the electronics. People like me—I could throw something together, it would work but it was not sophisticated as Jim pointed out.

TAYLOR: What was the nature of the power supplies at that time for the high voltage for the experiment?

ANDERSON: They were simply— what do they call it? Pie section rectifiers. That was the reason—

ARNOLD: Was it circuit— I dimly remember some phrase—Schmitz circuit [?]?

ANDERSON: No. That's the trigger. The high voltage supply—that's the reason that tube was hanging upside down. One of the problems was the high voltage supply would get in the ways. And one of the sources of noise was that those high voltages leakage across the socket that you plug the tube into. So, I said, "Well, we'll leave out the socket." I'll solder wires to the pins on the rectifier tube and hang it upside down in the air and it eliminate that source of noise.

TAYLOR: Did you ever consider to use batteries for the potential across the counter at that time?

ANDERSON: Did we use batteries for the drive-in, drive-out ones?

ARNOLD: Maybe so. Years later, I used them for—

TAYLOR: Gordon Ferguson— That's what the New Zealand used to solve the problem of noisy power supplies was to use batteries.

ANDERSON: No. As far as I know—

ARNOLD: I don't think that batteries were available necessarily at that time.

ANDERSON: After all, you're talking about leading a thousand volts or so. We operated that?

ARNOLD: Something like that.

ANDERSON: And then, kilovolts started to come by.

ARNOLD: Then later on, there were these nice three hundred volts called three hundred-volt batteries. And since you weren't drawing any current it's because we used them in La Jolla for the level counters later.

ANDERSON: A battery, especially the kind that were available then, with low internal impedance was quite dangerous. You could draw a slug of current out of it and if you stack up a thousand volts that's capable of putting out twelve amps, well you've got a real lethal machine.

ARNOLD: Whittle-makers, I think they call them

TAYLOR: But since you wouldn't be drawing any current for the counter potential—

ANDERSON: You didn't need it, but on the other hand, low current batteries were not readily available.

TAYLOR: What was the original background rate for the first time you put the counter together? Do you recall?

ANDERSON: Oh, I think when you just stuck an end to a shield, it was around a hundred fifty counts a minute. And then, by the time we finally got the adequate coincidence working property, it was below ten.

ARNOLD: My memory— When I arrived in Chicago to find this technology and being in memory for what it's worth says that in February '48 when I got back, the background inside a lead shield and with the anti-coincident circuit hanging in the way that Ernie has described was about forty counts a minute. And the signal of contemporary carbon was about four. And as Ernie says, over a period of years, you left in spring '49, I stayed with the project through '52 basically the first eight list [?]. What I remember finally was a background of four, which was what you just said, and a count rate of six because we got rid of some of the other things. So, I don't remember that happening in any single big jump. I remember it as a ball nibbling away, nibbling away. Radioactive getting the cleaner shield

ANDERSON: Then iron liner.

ARNOLD: Iron liner, electronic improvements, better materials for the counters, et cetera.

ANDERSON: Slow and tedious.

ARNOLD: Yup.

[END OF PART ONE, BEGIN PART TWO]

ARNOLD: Maybe change the subject a little bit and talk about the lab— How the lab worked— How we worked with each other during— So the study— The main part of the project. I mean when things were going along, when you and I were still here. You were still here— together. And the three of us were the team with Bob Schuch as the technician and general fixer of problems. Ernie and I had a great time together once we got acquainted. We decorated the two-seventeen Jones— The lab where the work was done in a couple of ways. One was to put up art of various kinds. We had many discussions about painting and this and that. And another was to put up three by five cards with little slogans on them in which the most everyone will recognize is the Mark Twain in quotation. "There are three kinds of lies: lies, damn lies, and statistics." And then there was the one which ended up in your thesis. And I don't know remember, Ernie, whether that was actually posted on one of these cards, but it was a quote from a non-existent classical Latin writer named Cebious Lecater [?] who was not invented by us but by an author named James Branch Cabell who wrote a book called Jurgen.

ANDERSON: Cabell. Rhymes with babble.

ARNOLD: Cabble. Thank you. I always forget that. And his hero was always getting out of— or trying to get out of difficult spots by making up sayings and attributing them to this fellow. And if I quote you correctly, Ernie, you will let me go by and otherwise— The subject is moral judgment. "Moral judgment does not alter the effect of a measurement. It merely makes it possible to obtain that result."

ANDERSON: "Does not alter the result of a measurement."

ARNOLD: "Merely makes it possible to obtain that result." The point about this little homily is that when counters are working say fifty percent of the time, you have to choose which data to use and which data to discard. And at first, all this improved as we went along. But at first, this was a quite subjective judgment. You would listen. You would sit in the room. It was great strain on one's emotional stability. You would sit in the room hoping to get some data and you would

be hearing a count. De, de. You know, we're talking something in the ballpark for a count of second or less. Count of minute or less. Sorry. And all of a sudden— Brrrrrrr! And then you would say some four-lettered words to yourself and you would go over and try to fix the problem. Or you would look at a result, maybe you would go out of the room. And you'd come back in and suddenly there were ten counts there which didn't seem right. And so, just as in this notebook account of the first ancient measurement which we're going to talk about some time, there are— The counts were ten-minute counts. You would record them at the end of ten minutes. And if it was lousy, you would cross it out. Or as in this particular thing, we got to the point where we didn't even bother to write them down if something had been a-burst. Now this is all quite appropriate in some ways, but in other ways, it was rather hairy. And the way to attack it was not to try to make more sophisticated ways of judging which was good data and which not. Although we did have one important one which was a strip-chart recorder that recorded every ten counts or something like that so we could run through the knife. Because until we did that, we didn't run through the knife. But nonetheless, what was involved was the transition from a situation where this kind of judgment is always called for to a situation where you had a working instrument which broke down now and then, but which basically was working. And so, that transition was important. Well, anyway, getting back to the slogans— There were others. I think there were three or four, but I don't remember.

ANDERSON: Remember the Keats? Apostrophe to his background?

ARNOLD: Yes. Go ahead.

ANDERSON: "Bright star, would I were steadfast as thou art?"

ARNOLD: Yes. That was very nice.

ANDERSON: Well, the sit-watching thing is certainly very important. One of our frustrations was that Bill would often come in when we had first started to count a sample. And he would sit there and he would watch the thing for oh-maybe five minutes. The statistical precision was negligible at that point. He would look at me and say yeah. He's says, "That's right." [*inaudible]* The minute he would leave, the counter would begin to drift. The rate would begin to go down or the rate would begin to go up. And there we were. He would come back. "Well, how did it come out?" Generally, after an extra week of work fighting things and tuning things, it would come back to the level which he saw. But he could never quite understand. He saw it was correct. What did we do? Why did it not stay there?

TAYLOR: Let me make sure. At this point, your anti-coincidence count—the net count was about a count a minute?

ARNOLD: Well, a little less. As I said, it started out— It was forty counts— I didn't say it right. Yeah. It was a few counts a minute, actually. It was—

ANDERSON: Tens of counts a minute. Every ten seconds or five seconds.

ARNOLD: It was tens of counts a minute at the start and it got down to be four counts a minute. So somewhere in the middle there at around ten counts a minute is probably my picture of it

TAYLOR: Okay. Your four counts a minute was the final.

ARNOLD: The best.

TAYLOR: That was your background. And the contemporary was how much above—?

ARNOLD: Comemory was six—was net six at the end. And that meant that toward the end, you could listen to the recorder while you were reading or something and you could tell right away whether this was a live sample or a dead sample. At the beginning, you didn't have a clue. You had to sit down, as we did at this notebook entry, and accumulate a lot of data, a lot of records, add them all up. No computers in those days. March out the calculator and do these things before you even knew whether you were in the ballpark or not.

ANDERSON: Now one of our best handles to try and eliminate this subjective judgment was a matter of Poisson statistics. Statistics of random event tells you if you observe sixteen events, the standard deviation is the square root of sixteen or four. Alright. So, in the notebook Jim was writing the countdown every ten minutes or so. We later got a gadget called a traffic counter. A sort of thing you put beside the road with a rubber tube and a car runs over it and it records the time in which it happened. We later hooked one of these things up to get a continuous record. And now you see, you could say, well alright. If during this period of time, twenty-five counts are coming in, the standard deviation darn well better be something around five. And if the standard deviation was two or one, you'd say, "Uh-oh. We're counting a steady pulse of something or rather." If the standard deviation was twenty for a cluster of these things— uh-uh. It isn’t working. It's picking up random counts. And so, the internal consistency check, the statistical distribution of repeated counts was the strongest tool at least in the first approximation to tell

you whether or not the thing was running or not. And also, you could look at a long record and say—well, gee. The statistics are fine up to this point and now they go to pieces. Alright. This represents the point at which something went wrong. Presumably the previously data can now be lumped as a unit and the statistical precision will be much higher and we can compare that with other large groups that were acquired later

ARNOLD: Now, that's one way that both of us, I'm sure, got Poisson statistics absolutely embedded in our brains.

ANDERSON: Absolutely.

ARNOLD: I tried to teach it today to students who find it very mysterious. And I'm very impatient with them because it's like, you know, I learned how to drive a car a long time ago, what do you mean you can't drive a car?

ANDERSON: But the derivation is quite straight forward

ARNOLD: Oh, it is. But nonetheless, it is not intuitive to people who have not encountered this.

TAYLOR: At low numbers, it doesn't always come out right until you get it a certain number of counts to play with, right?

ANDERSON: Yeah. The standard deviation has its own standard deviation. Well, the fundamental statistics is binomial and the first approximation upward leads you to Poisson, and then the third approximation is as various things get large gets you to the Gaussian, or normal, distributions. Poisson is good enough for this sort of things we were dealing with, although binomial would have been more precise

ARNOLD: Well, in fact, one of the problems with this cyclotron recorder where you heard a click every time there was count was that random events are not against something intuitive and which is much more likely that you get two counts closer together than you think it is. So, when you got dot-dot, you generally sort of clutched. But one learned to wait for a burst if there was going to be a burst

ARNOLD: The highest probability for a radioactive disintegration is immediately following the west one. That probability may be extremely low, but it's higher than in any time further on.

TAYLOR: When you expressed the error in terms of calculating error to cite—when you calculate dates, you decided on one sigma.

ARNOLD: Yup.

TAYLOR: Was there any thought give to whether you should cite one sigma or two sigmas?

ANDERSON: I think one sigma was just the simplest, the least buttering of the data. One sigma follows directly from Poisson's statistics. Now, if you want to worry about probabilities ninety-nine percent— Sure, use a higher sigma. But one sigma is the simplest.

TAYLOR: There was a later story that one of the reasons that one sigma was chosen, whoever did it, was to do two sigmas would give you such a large error that you'd have a hard PR problem with the people you were showing the dates to. Is that apocryphal story?

ARNOLD: I think it's apocryphal. I don't think Bill, who certainly made all the decisions, was thinking that far ahead. I'm reminded as you speak of a topic I was going to introduce some time which is the reception of the dates, data that started coming up by the archaeologists at that time. And we had considerable difficulty introducing people who had never used statistics before or mathematics particularly to what those errors meant. And in the first time I went— The first public presentation on a scientific meeting of radiocarbon dating was done by me at the Society of American Archaeologists. It was a meeting with which there was the scented samples of known age. And then, that was followed later on by a meeting that I attended some years later of the same organization. By this time, with some other people with carbon-dating labs and talking about it. In the first group, the general tendency was to say alright, the date is say, forty-six hundred years plus or minus two hundred years. Therefore, the probability of its being between forty-four and forty-eight is one, and that probability of its being outside that limit is zero. We had great troubles. I ended up writing in fact— There was an issue of a journal that Fred Johnson - I don't remember which journal it was. But—

ANDERSON: American Antiquity.

ARNOLD: American Antiquity. Right. He invited me and I did put in a little essay how the data would be interpreted. And it took a little while.

TAYLOR: I recall reading that. It was very helpful when I was grad student.

ARNOLD: Good, good. Thank you. Anyway, the point was that as far as its PR perception is concerned, we were talking a foreign language when we began by quoting errors at all. And I think it made very little difference in the end because, of course, it didn't take long for the more intelligent people to pick this up and use it correctly. And the less intelligent came along a little slower, but they did. They were critiqued by their peers. So within, say, five years or so, this was no longer an issue. Well, coming back to this business of how things ran and what it was like. One other aspect of our work in 217 Jones was that laboratory space was very much at a premium. And so, the people working in that lab were not just Ernie Anderson and me however many hours we put into the day, and Bill Libby popping in and out looking over your shoulder just when you didn't want him to. There were also a fair number of other people using the same system. Hilde Levi, the Danish nuclear chemist that Ernie and I met at that time. Delightful person. She was working in there. Nathan Chugerman [?] used it for some equipment. I encountered at one time nine people who had keys to or recognized access to that room. It was a devil of a nuisance in one respect because it wasn't a big room. It was four hundred square feet or something like that. On the other hand, it had its positive features. You could go out of the lab and somebody else would do something for you. Hey, close that switch, will you at such an hour? And got some very, very interesting conversations out of some of these people as well.

ANDERSON: I remember poor Stan Aldridge [?] trying to count osmium tetroxide as a counting gas. Remember that?

ARNOLD: Now that you've mentioned that.

ANDERSON: Stan was looking for the osmium-rhenium beta decay and osmium tetroxide was the gassiest material he put in the compound.

ARNOLD: The worst event. Maybe HF [hydrofluoride] would be worse. But it was awful. We had all sorts of people coming and going. And then we had summer visitors. One person's name whose should be mentioned is Robert Merrill who was archaeology graduate student who hung around.

ANDERSON: No relation to the opera singer.

ARNOLD: No. But at any rate, Libby was impressed enough with him to hire him for the summer and he was very useful. He both languages and especially he worked on sample

processing. And I remember he was trying to figure out how to extract the straw from sun-dried bricks.

TAYLOR: Was he?

ARNOLD: So, he ended up on the faculty at Dartmouth.

ANDERSON: He was a social anthropologist as opposed to an archaeologist

ARNOLD: Right. Well, at any rate, but he was one. Bob Adams who went on to fame and fortune later and became the director of the Smithsonian was a student of Robert Redfield's. No actually, a student of Bob Braidwood's.

TAYLOR: Merrill actually wrote the first article in American Antiquity that as I recall actually talks about radiocarbon dating while he was still a— [*inaudible*]

ARNOLD: So, we had all these people coming and going and of course, we were interacting. We were in this wonderful institute where all these brilliant scientists and graduate students like Gannigan Lee [?] and Murph Goldwyn [?]—people who went on to Nobel Prizes and whatever. So, we had a lot of stimulation from that source. And then there was the Oriental Institute and others. I guess I should put on the tape the incident with the second sample that we measured, which I think I've advertised before. The first sample, which was taken out of the box from Lansing, was a chunk of wood from the step pyramid at Saqqara in Egypt, which was the first multi-story building that's been preserved. Very remarkable structure. We measured this sample first in the funny, strange notebook that I recorded it here, which I've just shown you. And got a good age. Got an age that was within the era. And then, we wanted— Before we announced to the world that things were coming along, Bill was thinking of progress reports and more funding and eventually— Before the committee existed, Bill sent me over to Oriental Institute to see Professor John Wilson who was then the head of that institute and get a sample which would be half way in between. Which would be about 0 BC or AD. And that was obviously the Ptolemaic to me and to Wilson. The Ptolemaic period in Egypt from which there's just lots of stuff. So, I went over there and Wilson undertook to provide us with a sample and he did. And by now we are in late ‘48. Things are moving. And the samples measurement was getting more reliable all the time. And we measured this sample three times. Full pressed counts. And three times I was getting the same result as Ernie was getting as he measured his contemporary essay. And the lump in the stomach got bigger and bigger. And I was of course keeping Bill in

daily touch with this, and finally he said to me, "Go over and talk to him." So, I took my lab notebook over and I talked to Dr. Wilson. This is just a scene I could see to this day. I told him these results. He was sitting at his desk and he looked up at me bright eyed and said, "Well, you must be right." I had this overwhelming urge to punch this old man right in the face.

TAYLOR: How many months of work did you ever do?

ARNOLD: It was probably more than a month. The way I always remembered it, it ruined my Christmas. The second one—you're not feeling very secure because all of this— There could be many— Nature is always smarter than you are. There could be many reasons why it wouldn't have worked. Bill was very well aware of that and we were. So, the story was that it was a piece of money case bought from a reputable dealer in Cairo.

ANDERSON: Swore by the board, veered of the profit. That it is from of tomb of [*inaudible*] himself.

TAYLOR: It's an oxymoron to talk about a reputable dealer in Cairo.

ARNOLD: Of course, it is. And when that phrase came around, we all realized that Ernie had just embellished it a little, but why not. But at any rate, Bill is of course— I was talking to myself, appealing to the heavens. Bill was quieter. He just called Wilson up on the phone and asked him what the most valuable sample in the collection was. And the answer was there was a throne chair from Akhetaten from the Tell el-Amarna, from the 18th dynasty figure. Famous Akhetaten. And there was this perfectly preserved throne chair in his glass case. And then Bill said, "I'm sending Jim over this afternoon for a leg of that chair." And I think it was about that time that John Wilson truly realized what he had done. What we did in reply— The committee came into being— I don't know whether it was a direct result of that but it meant probably it was already on the way. At any rate, committee immediately existed thereafter and Don [Donald] Collier who was at the field museum noted that there was a funerary boat from the 12th dynasty from Sesostris in a huge case in the field museum. And we got a deck board from that boat. Nobody thinks things like that. So, that was our second sample and it was very reassuring. And after that—do you remember?—they provided us with the other four or five samples—with that other three or four samples that went into the first figure that was reproduced many times. And the decaying curve—we didn't even plot it on log paper. It was curved. And everything, you know—sweetness and light, it all worked. In fact, one of the things to say there was that it worked better because the errors were still quite large. The errors were—by the time even of the

publication of that paper, the best counts were two hundred years or something of that sort. Now we know now that the deviations from the curve were larger than that back in Zosser's [?] time. But for most of the rest, it was okay. So, we didn't see the errors. And I thought later that if the thing had started out being accurate to fifty years—precise to fifty years, there would have been a lot of skepticism as we discovered the wiggles. But the thing was well embedded by the time the precision had reached the point where we could see the wiggles.

ANDERSON: Yeah. Once the basis is established, then you can use Hermes quotation. If you make a measurement and get you what you expect, you've made a measurement. If you get what you don't expect, you've made a discovery. So, deviations became a discovery rather than-

ARNOLD: Yes. That was Han Suess'—well, Devry's first and then Hans Suess.

TAYLOR: Did you or anyone reflect on the fact that what would have happened if the John Wilson sample was the first sample rather than the second sample?

ARNOLD: Oh. I don't think we would have been discouraged by one. No, I think Bill— I'll speak for myself. But you know, these things were talked about among us daily. I mean, this was very, very serious business. And I certainly thought one reason for jumping at the chance was that I thought that the odds were very good. And he clearly did, too. One thing to say here is that Bill had had a reputation before the war—well, "Wild Bill." This was one of his nicknames at Berkeley. And because there was a certain slap-dash quality to his science. And he would think of something and play with it and get some sort of supporting result and write it up. He never treated carbon dating that way. Carbon— He was acutely conscious that this was the most important thing he had ever done. And something that if it worked would make him famous. Everything— What really struck me working with him because I could still see the other side. That part of his personality in speculating about other things was very present. But when it came to what had to be done to make radio carbon convincing, he was as conservative as anybody you would find. But that had its other side and it's stimulated by your question. He wouldn't have given up with one shot. He would have wanted to know what's wrong, why, let's keep going, make some more measurements, and track this thing down.

ANDERSON: Well, I think what you've just described is one of his strongest points. Get in there quick and dirty, survey the landscape, get some general ideas— If it's holding, probe deeper and deeper. But as you say, be sure it's right at the end. He probably learned a little bit from his

erroneous early discoveries at Berkeley. But as you say, he did appreciate the importance of this and everything had to be nailed down tight.

TAYLOR: Later on, when he wrote some ruminations on the history of radiocarbon dating he mentioned a couple of things. I wonder what did it check— I wanted to ask you about it if I could. In '80, he wrote an article about the history of how this thing developed. And he suggested at that point, while he was developing it that he kept it a secret for a long time. And he apparently only told Harold Urey what the goal of his research was. Now, this was a statement he made in 1980 in retrospect to what had happened that you were personally involved with. What would you comment upon that conflicting statement?

ARNOLD: Well, my guess from the fact that I knew about it, and not unless memory has played me totally false, from the paper in Princeton Review. It suggests that his memory was at fault. He loved— The Libbys, the Ureys, and the Mayers, and the Westheimers [?]—four couples— were very close friends and they partied a lot. And I was in a great situation because I was young and unattached and I got in— I didn't have to give parties. All I had to do was go to parties. And there was a great deal of drinking at those parties. And Bill had an odd habit of suggesting themes for discussion at parties. There was one called the fossil model-T Ford that you probably encountered at some time. The notion was there's really nothing new in the world if we were able to dig up the record. Ten million years ago, there was another intelligent race and civilization and all that sort of thing. And they invented the Model-T Ford and so on. This was not—

TAYLOR: Was he even in part at all serious, or just purely—?

ARNOLD: He— In between. And I—for what it's worth—was telling this story certainly very soon after the event. That's how I heard about it. And it was a— So he was not keeping it a deep dark secret is what I'm saying. But he was certainly within—certainly did not hear about it. He was not writing papers, calling up the press. I mean, he was not rambling about it very freely. He did, however, later start talking to some of the people. I remember particularly Robert Redfield at Chicago. He was a famous social anthropologist who was intensely interested and so much so that he kind of tried to climb on board and join the project

TAYLOR: A social anthropologist?

ARNOLD: Yes.

TAYLOR: Oh. That's interesting. Do you know the motivation or the context?

ARNOLD: Yeah. The motivation was it was very exciting.

TAYLOR: Totally out of his area of competence.

ARNOLD: Well, I understand that. But at any rate, perhaps I didn't really intend to bring that up, but since I have— Just say that our interpretation— I remember being upset enough after one lunch with Redfield and Libby and listening to Redfield essentially assume that what was correct. That Libby needed some expertise on that side. And just to invite himself on board and I made Bill a quite angry speech. You could speak quite freely to Bill. Let's say he gave it back to you hard, but you could speak to him. And I told him I had come out here to work with him and I had every intention of giving him the very best I knew how, but that working for this gentleman was not part of my contract that I had entered back under. And he basically said, "Oh, forget it. It's just talk." But at any rate, what I'm saying is he had told some such people—now we're talking when the project had actually— when the work was under way. Certainly, it must have been hard to keep it quiet when everyday there—this project was—

TAYLOR: Ernie, did you ever sense that he was keeping this a secret? Is this a surprise to you to hear this? About the secret aspect of it?

ANDERSON: I don't recall ever being impressed one way or the other that there was a secret, but it wasn't a secret. My reaction when I began to hear later that there was a secret and it was just that well, two things. He's both protecting his reputation as he claimed and he's not casting any clues to potential competitors and things.

TAYLOR: In reading some of Libby's retrospectives, I got the distinct impression that he'd sometimes embellish details to your effect. Is this correct?

ARNOLD: Well, certainly embellished details. Listen, you'll hear some of that today. I think it's almost impossible to avoid. But yes, I think he— Well, it was in his biggest scientific triumph in his long life—not long enough, but at any rate, his life—

TAYLOR: And he recognized that throughout his career?

ARNOLD: He recognized that right from day one. So naturally he thought about a lot of— He romanticized about it a little bit. He earned the right. Certainly, at the time when I was on the

project, when it was proceeding, I certainly was under no strict juris of secrecy. Obviously, we were not going to make any claims or say before we had the data in hand. But one thing— If following Ernie's interpretation which I tend to agree with— Let me share another old Libby aphorism with you. When he was— Especially when he was seeing me off on my own, which I started leaving him in Chicago, he would always tell everybody what you're doing was the aphorism. And the justification was this. Said first of all, anybody with any sense is going to realize if you say you're doing something that he started behind. And if he has any respect for you, he's going to leave you in peace. Secondly, if he thinks he can beat you and he can, then that's your fault, not his. So, he might as well. And thirdly—and this was the point—you can learn from people. And this is— more or less. I will admit that there had been times when I haven't followed that policy, especially with particular individuals who didn't seem reliable. But at any rate, he was—he did in general follow this policy. And by the time I had joined the project, he had such a lead that— I kind of imagined a scientist who is smart enough and quick-moving enough to have gotten ahead of him.

TAYLOR: Is it Bob Grossa [?] that he made the remark what can he do?

ARNOLD: That was Redfield.

TAYLOR: That was Redfield—what can he do?

ARNOLD: Yeah. Bob Grossa [?] was another person that tended to move in. I wonder whether we should break. I'm beginning to feel it. Is that okay? Let's do it.

[END OF PART TWO, BEGIN PART THREE

ARNOLD: —is the advisory committee and its role which I think was major. And just saying how that worked. And then we can go on—

TAYLOR: Okay. Why don't you start there? And then I'll pick mine up.

ARNOLD: Okay. One of the things that I think ought to be recorded in a history like this is the importance of Fred Johnson and his committee in making carbon-14 as productive and as widely accepted as it was early on. What they undertook to do was, in the main, two things. One was to screen samples. First of all, the known samples so that we would have no repeats of the sort of thing that had happened before. And they procured those samples in the main after the

incidence we described. Secondly then, the unknown samples. The decision was that we would in the first group of unknowns choose certain problem areas where carbon-14 might be expected to be particularly useful. And they did so. And they recruited specific archaeologists to submit samples in these different areas. I suppose ten or so archaeologists were involved in that first choice and undertook to explain to these people what was needed. All of that was of major importance because it meant if we had done this on our own, my knowledge of a particular special aspect of radiocarbon dating, one which needed radiocarbon dating relatively little just because so much was known—would not have extended to the breadth that was required to do that job. And if we had floundered around picking people that we thought were interesting, our judgment might very well have been doubtful or have been seen to be doubtful. Fred, in particular, and the others knew where the bodies were buried and they acted accordingly. The results were very good. The other thing was— And I think here particularly of Don Collier [?] at the museum because he was right there in Chicago. Well, I would call the daily query, situations that came up involving the treatment of samples, involving— perhaps this charcoal is contaminated by roots. Getting back to the archaeologists and digging it— All of that was a constant source of assistance. Flint [?] was rather different because the two creeks horizon which was the first glacial horizon that we dated was almost the only purely geological thing that appeared in our first date list. But he picked that out and he was certainly a major source of our confidence that things were really working that way. I remember—you may, too, Ernie—the two of us having lunch with him one time. Richard Foster Flint [?] was about the tallest man I have ever worked with scientifically. He was six feet nine. Something of that sort. Seeing this fellow look down on you from his— way up there was an experience. But he was a very, very good field geologist. Very good at making clear those things.

TAYLOR: When you gave him the dates, what was his reaction? Do you recall?

ARNOLD: Yes, yes. We gave him the dates. His reaction was, "Oh!" I think he was thoughtful. He certainly did not reject it outright. We know now the calibration curve that the date is somewhat older than that. But the twenty-five thousand years sort of conventional date that existed at that time— I think I'm not— Let's say with the usual reservations about my memory, my impression is that he had already had some feelings that that was too long.

TAYLOR: Because that was one of the major impacts. In the early radiocarbon dates, as you well know, was a significant reduction in the terminal Pleistocene.

ARNOLD: That's right.

TAYLOR: Just that one two creeks date. I can't think of any single day early on that made such a significant impact in ————— [*inaudible*] studies.

ARNOLD: I think that's right. And we felt that way too and then later when Hans moved in and really flushed that out with a whole series of measurements with other samples from other parts of the glacial record I'm sure that helped to convince skeptics because everything was very consistent.

TAYLOR: Except ————— [*inaudible*]. Almost ————— [*inaudible*] of course totally rejected them.

ARNOLD: I have Ernst Antibbs [?] on this list. Just looking at his name—let me comment on that. I met him in summer '49. I decided I needed a vacation. We had just been at it steadily. And decided that an archaeological dig would be a charming thing to do. And consulted probably Don Collier [?] and he introduced me to Paul Martin [?] and the permanent dig which the ————— [*inaudible*] museum had in western New Mexico. And ancient American Indian sites.

TAYLOR: Which site was he digging that year?

ARNOLD: It's just a tiny little village just near—half way down New Mexico near the Arizona border. About an altitude of 6,000 feet. Maybe before we're finished I will recall back the name of it. But they had a permanent up there. I had never been west of the Mississippi. It was absolutely wonderful scenery. I think we may be sitting here because I fell in love with the West at that particular moment. At any rate, I went there, and there was Ernst Antibbs [?]. And he was very kind to me. He undertook to give me some elementary lessons in I-science [?]. And he did his very best with me and it was very good. I just enjoyed the whole experience and we got to be good friends. And then we got into a situation where the dates in the southwest which fit his estimates really quite well were embraced. But when we started getting dates elsewhere, especially in the eastern US and in Europe, he developed a theory which he carried to his grave that the decay rate of carbon-14 depended on the degree of moisture in the sample. And we— Somewhere, I don't know—in your files in the archives here, there may be some exchanges of letters with him. I get these long hand-written letters. They were very friendly. I mean, it was not denouncing me or us, but he kept believing— And he made reservations, "I not a physicist..."

But he kept coming back to this point and I kept giving him the best explanations I could why that just couldn't be true. And that's where it was left. Yes. But he was the cream of the eyeball geologist when it came to getting a good estimated date. Bill Libbey always had the habit— Every time we got a sample, Bill insisted that the submitter of the sample or someone that they trusted gave us a date that they believed. Bill's idea was, you know, sort the sheep from the goats—find out who's good. And if you had asked Bill at any time, he would have told you that Antibbs was the head of the class.

TAYLOR: That's a new piece of historical data. I wasn't aware of that.

ARNOLD: And other— And he had—this was no surprise—the archaeologists of that area because he had been their standard resource for write-along. And in fact, of course, he would also disagree— Let's say he estimated eighteen thousand years and we got fifteen thousand years. We thought that was pretty good.

TAYLOR: Was he counting vars [?]?

ARNOLD: No, no. These were not vars [?]. And it was basically stratigraphy and relation to climate—the climate that was present at that time. He never— Well, well, no I don't want to make that comment.

ANDERSON: My understanding is it had to do with the extrapolated from the vars [?] sequence in Scandinavia across the Atlantic to the Great Lakes and then tried to do it West. And in retrospect all the books say he made some serious correlation problems.

ARNOLD: Yeah. That's right. The odd thing is he came out fine in the southwest. It was the steps along the way—the var dates in Sweden were already way off.

TAYLOR: He was at De Geirs? [?]

ARNOLD: Yeah. He was a student at De Geirs [?]. Or De Geirs [?]—however it's pronounced. And the same way in New York state. The errors resulting out from bad calibration because he was often in Sweden. De Geir was off. And so Antibbs was off in New York state or wherever he was doing these things. But he was right when he got to the Arian areas.

ANDERSON: He was a very good geomythologist. Incredibly good geomythologist.

ARNOLD: I can remember his despair with me. He'd take me to something and say see here. I was looking at tan soil everywhere. And he was saying what we saw over there— I was saying if you say so. But I liked him very much. And yes, this was a problem. There were other people who challenged dates and some of them were right. I think perhaps my favorite all time archaeologist was Bob Braidwood [?]. There was never one whose company I enjoyed more. And when we told him that Jarmon [?] was seventy-seven, seventy-eight hundred years old, he said no, it's older than that. He was very nice about it. He's a friendly man, but he was right, okay? I mean, he was seeing the difference between— I mean, the calibration curve didn't yet exist. And when the calibration curve came into being, he was vindicated. By the way, I've been exchanging correspondence with him over the last couple of years. He's still around. So, it was— That was one example where one of the people involved. Of course, he was interested in pre-history, so that was one of his overall concentration. So, he was one person who challenged us correctly.

ANDERSON: Was it Chicago— Or was it Lamont who ran Sweden wood samples where you had wood totally preserved and then degraded—physical degradation of the wood? And it was Antibbs who submitted the samples, trying hope against hope that there was a significant C-14 difference in the wood as a function of its degradation?

ARNOLD: We never did that.

ANDERSON: That must have been Lamont.

ARNOLD: That must have been Lamont, yeah.

ANDERSON: So, I didn't hear the— I didn't know about the moisture part.

ARNOLD: Yeah. That was his theme in his correspondence with me and I think in a paper or two. He wrote some papers on this subject and I would sigh each time. People are hesitating. Let me get back a little bit. This is not a complete of subject. To the Society of American Archaeologist and the reception. I commented earlier about the first reaction to the dates. The second time I went was about four years later. Larry Culp [?] was there. He was speaking of Lamont [?]. Beth Ralph [?]. I don't remember— I think Crane [?] was the other person. We had a panel. And what struck me most there was the change in the tone of the conferences as a whole. Not particularly—obviously carbon-14 had made some progress. There were— It certainly must have helped the archaeologists to gain some conviction when we were all pretty

much checking each other and verifying in independent laboratories. But I was so much struck by the decay of the type of paper in which I would describe as the house of cards. Some is particularly— I remember with hope will ————— [*inaudible*] and relatively recent things like that where— By the way, some of our results were misinterpreted by us and friends because the errors were too big to really do a precise job. But the big change was the absence of the schools of thought about chronology. Because people began to realize there were going to be answers if there weren't answers already. And so, there's no point in, so to say, spending thirty years defending a point of view when a few dates could shoot you down. That really was a very warm feeling. It's a feeling you've done something, you know?

TAYLOR: Let me pick up on a question that I'd like if possible to comment on. This is when Libbey later had reminiscence about the early history of radiocarbon dating in a Nobel lecture. He made a comment. Let me just— I have the quote here and I wanted you to comment on it if you could for the purposes of the record. Is that when he in retrospect went back and made these comments—see if you can provide some commentary on it. He says, "The research and the development of the dating technique consist of two stages: the historical and the pre-historic epics. The first shot Dr. Arnold and I had was when our advisors informed us that history extended back to only five thousand years. We had thought initially that we would be able to get samples all along the curve back to thirty thousand, put the points in, and then our work would be finished..." Can you provide some commentary on where you thought that sat in his retrospective on how things happened? Because clearly you knew—

ARNOLD: Yeah. I mean, again, the peculiarities of my upbringing, I was perfectly familiar with this before I ever met Bill Libbey. The oldest dates I guess are Sumerian rather than Egyptian, but it's a very close match. And that was all— It was part of my instruction as a youth. So that statement was not correct where I'm concerned. Not because of any special insight on my part, but because of the accent on my background. I think he— There is an element there of color. He had a kind of way of taking pride in his working-class origins or whatever—however you want to describe it. And he certainly tended to dress things up sometimes. I think it was not as much a surprise. After all, the first sample we had come from Ambrose Lansing [?] and already had that background. So, it can hardly have been a novelty to him entirely.

TAYLOR: Another question that came up— In going through the early date lists, I found the only unpublished Chicago date. And wondered if you remember any context around it. This is when you ran a charcoal from what was labeled as a Mousterian [?] level from a French

archaeological site and got about nine hundred years for the date, which is clearly anomalous. In the original galleys for I think the second date list, it was taken out and there was a comment that said, "We don't want to make the archaeologists unhappy." Does the context of— ? That's the only unpublished Chicago date. Do you have any sense what that was all about?

ARNOLD: Well, you reminded me. I had forgotten the incident. I had forgotten the date. But now that you've reminded me I do indeed remember such an event. And remember, that's the second date list. Some curious thing how Libbey's style changed a little bit after I left the project. The biggest change was not this. The biggest change was something I had said to him again out of my family background which is going in doing the project— The agreement was no religious samples while I'm associated with the project. And the point of that was not being against making studies of religion. The point was that I had been brought up with the notion that if you tell people that their cherished religious beliefs are wrong, they get very angry. And so, you didn't want to do that in a stage when you're trying to establish the method. Bill accepted that and acted on it, although the first thing he did after I left the project was to date the Dead Sea Scrolls.

TAYLOR: That's another question I had.

ARNOLD: And that was dated on cloth wrappings that were found around it. That was just fine. I mean that was— I certainly had no qualms about that since it seemed to be totally clear that they were real. I think he would have enjoyed the Shroud of Turin episode that came later very much, but he didn't get to see that. As far as this sample is concerned, I indeed with your reminder remember that there was such a sample and that we concluded, obviously, that we didn't know why it was some misinterpretation—some misidentification— Whatever it was, it was not what it purported to be.

ANDERSON: Like Ptolemy.

ARNOLD: Yeah, yeah. Like Ptolemy. Except in case, I don't think there's any question of its being fake. It was some mistake. We had earlier ones which we avoided. Despite our instructions, I remember two or three samples that came impregnated with ———— [*inaudible*] paraffin and we never measured them. Now this one, what the source of the problem was so far as now obviously limited. I remember ———— [*inaudible*] says it was not all clear. And I would judge that Bill felt there was a personal situation where some, perhaps elder statesman or grand old man might be subject to ridicule or something else of that sort. In

general, we—it was understood. I'm sure that when the committee solicited samples, they made it very clear to the submitters that those dates are going to be published. And that worked effectively as quality control as well as their choice of materials because it must have made vivid to them as it became vivid to John Wilson after the event I described. That particular one—except that I told the story later—was also not published as a matter of fact until I decided enough already and told the story.

TAYLOR: You mentioned the Dead Sea Scrolls. That was later added as an update to the so-called curve of nodes. But as you know, the date for the Dead Sea Scrolls was a subject of great deal of debate at the time and it certainly was not a known age sample. Now was that— That's probably after you left the project.

ARNOLD: Oh, absolutely. The measurement was made after I left the project. So why that was done was not— Although I think probably the controversy was over by that time. Or at least over so far as big jump. Big percent.

TAYLOR: Well, they had— The date showed it wasn't fake. Okay. There's still debates about—in the literature. People are specialists in that area about the exact age range.

ARNOLD: So, a hundred years or something like that.

TAYLOR: No. Like three or four hundred.

ARNOLD: Ah. As much as that.

TAYLOR: Yeah. Five, six, seven.

ARNOLD: I say I wasn't aware of that.

TAYLOR: Back to the period where you were in the lab—both of you— I had a conversation with Fred Johnson before he died at length about his remembrance about the early years from his perspective in archaeology. And he mentioned something to me that I have been trying to get—see if I could find the documentary support for a long time. I wonder if you could shed any light on that. And that is, he had heard early on that several very prominent chemists verbalize or the oral tradition was that radiocarbon dating would never work. And Fred said later people—later after the Nobel prize, particularly, they took— He has stories about them burning their own files if they had correspondence to that effect. Is this a new?

ARNOLD: Absolutely unknown to me. There were people— There was one incident which caused certain confusion that I was partly responsible for. Before I left Harvard on my way to join Libbey, I was still under the impression that we're going to have to enrich samples in order to date them. And I had a talk with a Harvard archaeologist whose won't come to me. At any rate, on the subject of sample sizes and things of this kind— And since the— If we had had to— The sample size for counting had been what it was and then we had to multiply it by the factor of how much material. So, you're going from, say, ten grams of carbon which you want a multiple sample, so that's twenty. And then that's carbon. And when it's wood or something of that sort, that's sixty or seventy. And that's when it's very new, but when it gets older, you know— We started talking about hundreds of grams essentially, of sample. And his reaction was to kind of throw up his hands and say, oh, these samples size gets that large. And if you can— Of course, I also had mentioned the laboriousness of doing the separation. Bottle-neck would have been the counting, it would have been the separation because these thermal diffusion separations took weeks—two weeks—anyway. Maybe you could cut it short, but— So he started spreading this story among his friends. He had every right to. I'm not criticizing him for that. And the notion spread among this circle that while, you know, this was on paper, this was a great method of practice or whatever.

TAYLOR: Because of sample size.

ARNOLD: Yeah. Because of sample size. Now as far as that other chemist— Interesting that maybe Fred was dressing it up a little bit. I don't think of him so much inclined that way. But there may have been skeptics. I mean, I think it's like some other things that I can think of where it would be very easy to think of ways— We ourselves were conscious of ways that it may not— We're essentially exchanged. Very difficult in the absence of data to know whether exchange reactions go on or not. Suppose some of the organic components of typical biological samples we worked with exchanged would-be carbon dioxide in the atmosphere. Organics in the soil and incorporate material. Have a history. We didn't know that. And of course, that was treated with degree so that nowadays people separate cellulose and they go through many such careful purifications. Especially you talked at lunch about a sixty-thousand-year-old sample. You didn't just take it right out of the bag, did you?

TAYLOR: Weeks and weeks of extraction.

ARNOLD: I believe it. And now— So the point is that there were perfectly legitimate for asking critical questions. And in fact, some of those questions were valid so far as some samples were concerned—as far as precision was concerned. And I don't see why anybody who— I'm sure I myself have looked at something, some great ideas that some other people had said it would never work. In this work, I didn't go out and commit ————— [*inaudible*] I congratulated them.

ANDERSON: There was an aphorism once that if an expert tells you something will go, it will go. If an expert tells you something won't, just ignore him.

ARNOLD: Yeah. So, I think at least nothing like that—unlike Ernie—nothing like that ever came to my notice. Bill— There were people you know that did not like Bill Libbey. I mean, Bill—it must be said—was a person who made both friends and enemies easily. I was extremely fond of him, although not at all times of day. He had a habit— This may be a good place to refer to his habits with students and young people like myself. That if you work for him and thought you were good, he worked you over very hard. I remember— I'm sure Ernie you shared some of these experiences. He would give you some tough challenge and then we would go ahead and do it. We'd show him the results and he would say something like, "Obviously" or "Why didn't you get it sooner?" and you would get mad. You realize that you had deserved to be complimented for this. Now, of course, on the outside, he was boasting about you to everybody in sight. And his people could do no wrong. I have seen letters of recommendations from him to my eminent colleague here that probably the truth was the reverse also. I mean, we were just the greatest young scientists that had ever been seen. And that was his face to the outside world. But he kept raising the bar. I mean, if you did something, the idea was he could do more. And I'll tell you, I came out of three years from that absolutely re-made. I was simply a different person than I went in. I had— My skills were better; my confidence was better in every possible way. I was just way, way more ready to become a successful academic scientist than I had been before that. But it was strange later on. I remember one particular student who was just a bright as any of his other students, which means very bright indeed. But lazy. No ambition. Just easy going, good at sports—which is something Libbey liked a lot. So, he tolerated that for a while. And I noticed and all the other students noticed the likes of Sherry Rowland [?] and you know, Wolfgang— absolute top-caliber people. But this fellow was always treated very pleasantly. And I asked him once. Why are you so nice to this guy and you're so tough on people I named me? He said, what's the use? He didn't see any point in exercising his attack mode—Vince Lombardi [?] mode on people that he didn't think he could do anything with. And

that was the game he was playing with us. He was going to make us the best scientists we could be. And by God, I must say, I don't know how I could be any better than he made me. I wanted to embellish on this a little bit because of the interesting contrast between two very close friends with Bill Libbey and Harold Uri who were working both in the same sort of field. Uri was doing the temperature scale of carbon-13, carbon-12, oxygen-18, oxygen-16. And both of them were in fields they had never been in before. And both of them turning out some very remarkable students. And Uri never did this football coach thing at all. Uri was absolutely straight-arrow. If it was exciting science the student was turning out, he got excited about it. It was not exciting science, he didn't work over the student. He just didn't pay any attention. And the students turned out just as well. Either way. So, I don't know whether Libbey's technique was in some way ideal. It didn't always work, even with very good people. I could cite people that he crushed. Just couldn't take it. You know, they just lost confidence. Well, I guess I'm no good. One in particular. And yet, my enthusiasm, my little speech a few minutes about his effect on me is absolutely true.

TAYLOR: Do you think it was conscious thing with him?

ARNOLD: Yeah. Oh sure.

TAYLOR: No question about it?

ARNOLD: I don't doubt it for a minute that he was simply— I mentioned much earlier the first stage of this process is starting on my ————— [*inaudible*]. I mean, that was different. That was questioning somebody that you never saw before it his life with the goal with making him discontented with limited objectives essentially. And that was stage one with me as well. But the stage two, where you're making a person feel that he's got to try harder—that I think was absolutely routine but conscious. Yes. I'd like to take a bit. Let me go back a bit because I'm looking at my notes here and seeing something that we having treated yet. And it's a question for Ernie again. Chemistry. In order to convert the samples to carbon, having made the decision, the way we did it was to burn the samples, convert them to carbon dioxide, and then react the carbon dioxide with metallic magnesium and reduce the carbon dioxide to carbon— Carbon and MgO, leach out the MgO. That was developed by the time I arrived. I didn't— Except for little polishings which we did with everything. How did that come about? Was that a trial-and-error process or did it work the first time?

ANDERSON: Oh. Basically, it worked the first time we tried it. The problem was you had to be careful about how rapidly you let the CO_2 in, otherwise you'd burn up the apparatus. I don't know whose idea it was, except it's pretty obvious that if you've got carbon dioxide, you've got carbon, okay— But there was the matter of diffusion block in the system. But other than that— Oh, but you see, I had this memory different than yours. I remember everything was rosy. With this magnesium reduction— was just if he does the counters were and I regarded as the normal state of hostile nature.

ARNOLD: Well—

TAYLOR: You're one of the few. Everybody I talked to has interviewed—that had to do with solid carbon counting would count as horrors

ANDERSON: Yeah. Well, it was a horror. You can't imagine a worse thing to keep free from contamination than activated charcoal which is what this stuff was. Prodigious surface area. One of the things that saved us, I think, was just that we got this going before there was any atmospheric—

ARNOLD: I was about to say that.

ANDERSON: In the absence of that, if you stay away from a lab that had been used for radium, it was straightforward but it required considerable manual dexterity because you had to take this powdery fluffy gunk and put enough ————— [*inaudible*] to hold it together and then you had to carefully paint it on the inside away a long cylinder. I guess we later got the idea of painting it on the foil and then curling the foil. It was not easy to do. And of course, if any of the carbon came loose during the counting process, you're done.

TAYLOR: How often did that occur early on?

ANDERSON: I would say less than fifty percent of the time.

ARNOLD: I don't remember it was big problem. It certainly was never in the later—

TAYLOR: I know Gordon said when they tried it early on, it was mess.

ANDERSON: There were things falling off? One of the things, of course, was how you shook the sample cylinder. If you were a little bit incautious and slid the cylinder down too abruptly like— a bang would come off. Maybe they didn't use enough agar.

ARNOLD: Well, they had the disadvantage of not picking it up from us. I mean they were doing everything on their own. I instructed several people in that technique and I don't remember anybody coming back to me with horror stories. But again, of course once the gas had been developed, I understand the preference very well. It was quite hearty and of course after three years of doing it, I got very good at it. I mean, like anything else you do every day. But these questions of how much agar and just the way—just the risk motion—

TAYLOR: Why did you choose agar, by the way?

ARNOLD: Well, we didn't use very much, but that was a—

ANDERSON: A natural glue.

ARNOLD: A natural glue that would be easy to get off again in case you wanted to do it. That seemed like at least— That was probably before my time too, but it— I had no qualm with it. It wasn't like the alcohol. The amount of agar was very small.

TAYLOR: Did you have any combustion when you developed a method of reduction? What yields were you getting early on?

ANDERSON: I must confess, we never bothered to measure them.

TAYLOR: In retrospect, tell me what was the approximation was like.

ARNOLD: Well, we did see thirteen— By the way, we didn't even do that in the early days. But since Uri had the machine right there, it was very, very easy and he was cooperative. He's got these little gas tubes we would fill. You could usually tell by appearance quite well when you're at complete combustion. But certainly, any anomaly in the C-13, C-12 ratio would be an alarm bell.

TAYLOR: But you never published any of the stable isotope value. Any of the earlier literature. Was there a reason for that or was it—?

ARNOLD: I think at first it was a lack of interest. And you know, the errors were large enough that I don't think that there was any reason to particularly— The only one that I remember being an absolute terror was one of Ernie's samples, which was the seal blubber.

ANDERSON: Oh, that! [laughter]

ARNOLD: So, you know, the whole Antarctic continent was uncovered and it was the goodness of looking everywhere. And Ernie was directed— I don't know who turned up the fact that there was some seal blubber in a freeze locker—

ANDERSON: Bill had a contact with somebody on the expedition. As a matter of fact, maybe there was not enough time for that. It was my impression that it was—if not collective, especially for him, was selected for him as a pre-existent sample.

ARNOLD: Well, any rate, I remember complaints from the organic chemists. Because of course, about the smell. We were doing this, you understand, not under the hood because there was no reason to do it in the hood. Normally, it was a closed-system and the combustion was reasonably complete and there was nothing— You know, what was there to worry about if there were a rather few oxides of nitrogen or so? But it revealed it was not enough to be bothered with.

ANDERSON: Not only that. This stuff was stored in a commercial beaker used for depository. And the proprietor of the depository began fussing at us. You couldn't have that junk in his deep freezes any more.

ARNOLD: Because it was like a skunk magnified. And that was a kind of disaster.

ANDERSON: Well, the ironic part of that was that the seal seemed to be a weighted sample the far southern hemisphere, but it turned out that they ate fish, but they just migrated down from the equator. And so, the whole thing—

ARNOLD: Well, in addition to that you have the whole ocean, land approximation. Which— Well, there's a story. I think I want to throw in a few stories here. This is the first shell sample. It's in the first wait list. And Chesapeake bay oysters. And this came about in the following way— There were celebrations periodically in the Libbey group. Sometimes, the more typical celebration was that some good event had happened. We'd passed some test or other. And then Bill would bring in a bottle of very good liquor of some type. Like twelve-year old scotch or something of this sort. And the tiny beakers—25cc beakers—would be filled up with it just to really make a break. And there were many— I could tell a couple of more of these events. But one particular— There was a restaurant called Morton's. A sea food restaurant not far from campus. And one day, Bill invited us all to lunch. I can't remember the occasion, but I remember it was one of these celebrations. And we were sitting around and probably had a beer or two.

Nobody was feeling terribly distressed. And the question came up about extending the method to shells. And so, on the spot, we had been sampling oysters and the question was— So somebody called Mr. Morton [?] over to the table and asked him where the oysters came from. Because of course, we needed the proponents and so forth and so on. He said they come from Chesapeake Bay. And the question was, how do you do? And the answer was Morton's only serves the best oysters. The best oysters come from Chesapeake Bay. And we were so entertained by this syllogism that we collected the shells on the spot and we did the measurement. And again, the error was pretty large, even though the result was low, these were contemporary shells. They were— it was below Ernie's general curve. But not— Maybe it was one and a half sigma or something. It wasn't impossibly below. And it was only later that it was realized that not only is that correction quite substantial, but it's also not extremely constant as you go from one part of the world to another or one situation to another. So that had to wait for other developments. While I'm on the subject of celebrations, there's one other—

[END OF PART THREE, BEGIN PART FOUR]

ARNOLD: —numbers. And we correctly concluded— So I think there was more than just this particular off-hand—

TAYLOR: You never published this, did you?

ARNOLD: No. I think we believe— We've gotten reservations. But I believe our ground rule was that if we went in, knowing or believing strongly that the result would not come out right, we were not obliged to published when our expectations were met. And we charged that off to method-development. We didn't feel that there was anything— Especially because the archaeologists or whoever it was that furnished this to us was no party to our decision to run it despite the difficulties. Well, I wanted to go a little bit with another one of these celebrations that I remember very well. At one point early on, Bill went to New York to give a talk at a scientific meeting. It must have been very close to the time that I talked to American archaeologists. And the New York Times had a reporter there. And the next day, there was a story in the New York Times which headlines read, "Scientist stumbles on new dating vector." And this was more ability— you know. I don't think even— I think many— I myself or any scientist would have done what he did. To start by saying, well you know, I'm not an archaeologist. I'm not an expert in this field. Usually apology. Most scientific talks begin with something self-deprecating. And that was

his thing. And he spoke of stumbling on the idea or something of this sort, which might indeed have been true in some very general sense. And we were very annoyed—not equal amused and annoyed because it had been three years of hard slugging at this point before this stage had been reached. And so, Bob Shook [?]—like I had mentioned before—had an idea. He had some friend who was a sign painter. So, the friend made out of metallic paints a bronze plaque on a sheet of paper which was just to paint it on. Which read something like this: "On this spot, W.F. Libbey, 40—" which was always the way that things go. He was forty years old. That's the stumbled ————— [*inaudible*] for three years. And ————— [*inaudible*] on the carbon-14 dating method. That was partly a little big at Bill for letting this happened, but partly also just to, you know—kind of good fun. He took it with very good spirit. We found a window shade to cover it with. And we had him cut the cords so that it flew out, then we had a little bit of a party. It was a warm family, you know. We've been talking about this Vince Lombardi [?] business, but he had a way of conveying his pleasure in what was going on. And I think we never doubted his support of his. And these were simply some of the illustrations of it.

TAYLOR: What was— During the years you were there—either of you—what was the sample that you remembered the most about? Maybe it was— You've already indicated. Maybe the two Greek sample. But does any particular sample come in mind.

ARNOLD: No. If I would say if you asked me that, my reply would be, and another, the Crater Lake and the Mount ————— [*inaudible*] sample. The sandals. Rope sandals. What the story there is that when the Mount Mazama blew up and created Crater Lake [?], the cause—I saw myself later— All of eastern Oregon pretty much was covered with a thick layer of volcanic ash. And one place that was covered was a cave whose mouth was sealed and the archaeologist opening it up found something like seventy pairs of rope sandals. And the guess date was already well-known because Mount Mazama [?] had been dated at various places. Seven or eight thousand years which was the fact. And we saw these things. They were beautifully made. Some of them showed evidence of wear. I remember one with a thong on it, to hang on a peg. How we said, "Ah, impossible!" These things are too beautifully made to go back that far. And so, it was a real shock—and pleasant shock—to find out that no, they were exactly that old. I would pick that one as the most exciting surprise, and pleasant surprise, of the ones I remember. Certainly, there were quite a few that were very interesting. But—

TAYLOR: What was yours, Ernie?

ANDERSON: The one I remember best? Oh, it was the first— When we measured the methane—the Baltimore methane—and discovered the contemporary stuff.

TAYLOR: How about of an archaeological sample? Are you— You were already gone by the time they started to work on the samples?

ANDERSON: Well, one I remember perhaps because I was given a sample of it was the Peruvian mummy cloth. Which was frightfully problem—

ARNOLD: Yes, that's right. I remember someone remarking to me— It was Don Collier [?], who was specializing in that field. That's better cloth than the people down there wear today. It's nasca and I have some of it upstairs in my cabinet. That's the other trophy I have besides a piece of the ————— [*inaudible*]. I have to say, though, coming back to Ernie's comment which I can well imagine—my own is basically similar. It's this thing. It's the first measurement of the Zosser [?] sample. And the realization— I've told this story many times. I'd tell it again. The realization— I was— Bill did not press me as far as I know to calculate the result before I felt that—before we both felt that was enough statistics to accumulate on a Saturday in June which is I hope it will turn out that the first calculations start on Saturdays because that's how I remember it. I sat down right after lunch and did that calculation. And I saw just basically that it was half-way in-between. And I was just still floating on air. I went out of there and walked around the streets in a kind of happy daze. And the other part of the story is when I— Of course, Bill Libbey was out. I was off somewhere, so I couldn't tell him. But when I found myself coming into the house and I was living in an apartment on top of the Libbey house then, I was thinking about backgrounds and contamination and electronic difficulties and I said to myself, I got— You know, I just had this wonderful news an hour, an hour and a half ago, whatever it was, and now I'm already back in trouble again. But I— You know, it's fifty years later dandier and I still feel it.

TAYLOR: I cherish the comment you made I think before on— You were the— For two days—

ARNOLD: No. Two hours.

TAYLOR: Two hours. You were the only person in the world that knew that radiocarbon dating worked.

ARNOLD: Yes. That's correct. And it may have been— But it's certainly not as long as two days. I think that two hours is a better guess. Bill did appear and I showed him the results and we were quietly happy together.

ANDERSON: Going back a bit, when we were talking about the decorations and things we put on the calendar, did we cover the cyanide?

ARNOLD: No. We didn't cover the cyanide. Okay. In this period when Ernie had searched that everything was working so far—

ANDERSON: The only person I've ever heard say that.

ARNOLD: Despite that perfectionist as we were, we were a little bit discontented. And of course, it came and went. You know, whenever you have something that's operating half the time, it doesn't operate twelve hours a day. It comes and goes. Just when you think it's getting ahead, it'd fall back again. And I wrote a letter to my father saying something, kidding a little bit, about the curse of Tutankhamun. And I had heard these stories from him forever about this purported curse which was a journalistic invention after the discovery of the marvelous riches of the Tutankhamun tomb. And some time a few years later, one of the key people died suddenly. And people started saying, oh— There is no question that all of the pharaohs' tombs had inscriptions on them consigning to whatever hell their religion had. Anybody who broke in and violated this sacred place. But it was indeed a question to whether this curse work. And my father talked about one archaeologist who had died at ninety-five and all of these sorts of thing. So, it was a bunch of nonsense as far as he was concerned. But I wrote him a letter to this vain and asked him and I hope he understood my tongue in my cheek that he could provide me with some religious— He was a very outspoken atheist. If he could provide me with some religious token which might be helpful. So, he sent me a little coin-like object which was the seeing-eye of ————— [*inaudible*]. The big, long— My gestures wouldn't go onto the tape. Long eye with— And these apparently had to be purchased somehow by the worshippers who wanted to get into the certain parts of the temple. And my father spoke of bushel baskets full of them which were collected at certain archaeological sites and that's why it'd be easy for him to pick up on. And Bill and Ernie and I discussed this when we got the letter back and with the emblem [?] in it and the issue was— Do we put it inside watching the equipment or outside watching the experimenters? And we made the correct decision. We put it outside. And things improved

immediately, by the way. And some of the photographs you can see this little thing taped to the shield looking at us.

ANDERSON: That was one of our better acts of ———————— [*inaudible*]

ARNOLD: Yes. Well, we had— In any project of that sort, there are always little things that are sort of fun stories afterwards. The range of materials, of course, was very wide. I'm thinking of Junius Bird [?]. I don't know whether his samples had arrived before you left. But the— Junius Bird [?] was a— I guess the American Museum of Natural History in New York. And he was interested in pre-history, early man. And he sent us—the technical word is coprolites. Sloth dung from some of the caves. And he sent these things along with a couple of limericks as to the reasons for sending these particular samples. I wish I could recall them verbatim. They're quite amusing. All I remember is some of these coprolites rare— Well, something with care toward dating past eras for Bird. And I can't even remember the second line. But at any rate, things like that also kept us entertained. One other example. I had a very dear friend and teacher. Henry Eyring was a famous theoretical chemist at Princeton and a devout Mormon. He entered the bishop of the Mormon church at Salt Lake. And he was one of the most unusually deeply religious people I know because he was—had a good sense of humor about it. I liked to kid about religion. He didn't have anybody around him who was a Mormon, so he was not dealing with the faithful. And so, we got used to him banter. And a few— In the carbon-14 era, this was while I was a graduate student out of the carbon-14 era and I ran into Eyring at a meeting and he poised the thong question at me. He said there are people in the church—I didn't have to ask which church—who are perfectly willing to make the earth four and half billion years old and all this stuff, but they hold to the Garden of Eden and man being created 4000 BC. And do you know—do you have any clear evidence for the existence of human beings before 4000 BC. And it happened that I have the perfect answer for that. Because we had gotten from another archaeologist as part of the program I'm talking about, some painted arrows from the cave—Lovelock [?] Cave in Utah. Which was not anywhere. It was as I put it, God's country. Zion, right? And they were beautifully carved and they were painted with primary colors. Some natural vegetable pigments. And as it happened, I had logged them in and I had done everything myself up to the calculation of the result and sending it to the archaeologists. And the date was something like 7000 BC. It was nine thousand years old. So, the answer was questionably yes. And I remember the conversation because of what followed. I was thinking of some kidding remark to make to him and what I thought of was, "But of course, God can do

anything. So perhaps He created the arrows in the cave, you know, at such and such a time." And point of the story is his reply. He said, "Oh yes. Of course. But let me put it this way. If God cheats, I won't play." I thought that was just a wonderful remark. I thought that was just a truly memorable remark.

ANDERSON: Well, that sort of echoes Einstein. "Subtle is the Lord, but He's not malicious." Or alternatively, He doesn't roll the dice.

ARNOLD: Yes, yes, yes. But I think Eyring's remark can be interpreted at many levels and I enjoy all the levels. I have one more topic to break in while I seem to be talking. It's important to remember for the record that Bill—as soon as we had reached the stage of a date list, as soon as we had reached the stage where the method was in our eyes well established—began to cast around for people who might be interested in setting up laboratories. It was not a passive thing with him. He made the remark on several occasions that he did not want to be pope for a whole generation of assistant professors of archaeology whose tenure depended on how the dates came out. And so— And of course, he didn't have to wait long for customers. And the last thing— Oh, I stuck my head in from time to time. I was right there. On occasion, elsewhere. But the last formal job I did for him before moving on to my own room, my own office, my own building, my own laboratory was to set up a course. Shall we say, one-on-one education of series of people who established carbon-14 labs. Larry Culp [?]. We mentioned Lamont Dardy [?]. Beth Ralph, University of Pennsylvania. One other I'm blocking out at the moment. And Hans Sous [?]. Now, Hans was at Chicago in that period and had made the decision which in retrospect is really pretty amazing to basically devote most of the rest of his career to doing this. Hans was a highly original scientist and so he obviously had to be deeply impressed. His approach to my instruction was very, very different from the others. The others were taking notes all the way. I had them finally— Like driving, you take the test. Each of them prepared a sample and executed all this under my watchful eye. And you know, by that time, it was pretty cut and dry. No great difficulty about doing it. Hans came around and listened to me for a while. Never took any notes and after a little while he just stopped coming. And I found out later why he had ideas of his own. He didn't mess around with black carbon at all. He just went straight to acetylene and that was how he did it. First Yale, which didn't come very far. Then to Washington and then here. And it was very important to Bill. I remember his great pleasure, his delight when Hans sent him a letter with some of the early glacial dates. And he could see that this was good work and he felt that a very important milestone had been passed.

TAYLOR: You mentioned some of your early dates. Later on, or I don't know at what point— Let me ask the question. At what point did Bill get interested in the early teachings of the New World? Because when I knew him— He was the person who initiated the work at Tully Springs [?] which was the first large-scaled Paleo-Indian excavation in the North Americas. That was really his pushing at UCLA. Do you— What I ask you about the most interesting sample, one of the ones that of course stand out to archaeologists is the first date on ————— [*inaudible*] which was wrong, not because it was a wrong date. It was because the archaeologists hadn't been able to identify a secondary deposit. Do you have any sense of—?

ARNOLD: Oh. He was very much interested, so was I— I must say. But he was—yes. I think his interest dated back to his getting acquainted with the range of archaeology and problems. I think he picked that out quite early because the dates—the guess dates were all arranged. There was no— There should have been something that would have worked.

TAYLOR: You didn't— Did you work on the—? See, he dated— Chicago dated a Tully Springs date.

ARNOLD: No. I don't know about Tully Springs but I— But the original fulsome [?] sample—

TAYLOR: You worked on.

ARNOLD: I think so. That was on the first date list, wasn't it?

TAYLOR: Yeah, yeah. That's when it got four thousand.

ARNOLD: Yeah. The answer there— I think the best way to say what I did and what I didn't do is— That was some small exceptions one way or the other. But basically, my— The first date list and my presence on the project were pretty contiguous. That is, after that I moved on and that was pretty much the break point. Again, not carbon fast, but as a rough rule, if it's on the first date list, I did it. I may remark in that connection that although I no longer remember the details, I became convinced about ten years afterwards that one or two of the samples whose dates seemed wrong were sample that had been misidentified by me. That is, I was the curator. I took all these things in and the samples would go into polyethylene bags with the polyethylene labels sealed inside the bag with a number written on it. And the numbers went up into the four hundreds I think by the time I left. And I was not experienced at that. And you know, confession is good for the soul. I'd like to believe that I never made any mistakes, but I think I made a

couple. And I could no longer tell you which ones they were or what my circumstantial evidence was. But I think there were a couple. So that's another source of problems. I couldn't dare to say whether the fulsome [?] sample was one. Probably not.

TAYLOR: Well, that turned out to be alright because there was a clear explanation. Nobody questioned that the dating was reasonable because it's a secondary channel. And when Cook [?] went back to look at the citing, he had missed it the first time around in the thirties when he collected the original samples. But when you look at the side profile again, he recorded it. It clearly came out of the secondary channel. Had nothing to do with fulsome [?].

ARNOLD: There were a few other cases where people went back and found something wrong. And I think that's not—that's probably power for the course. Nothing—

TAYLOR: Well, there is several, though. I think I probably at one time— It's been another ten or fifteen years, but seven or eight of the old Chicago samples had been re-dated. And I think with only one exception was there any, in terms, just analysis. Only one I knew about gave a very different age.

ARNOLD: You're talking about the same samples?

TAYLOR: Same samples

ARNOLD: So, there would be no question of—

TAYLOR: It's amazing how consistent some of the early dates are in retrospect.

ARNOLD: I think they're basically good. I certainly don't know anything except— It's a question of this question of purification and extraneous carbon. It might have turned out that was the worse problem that we ————— [*inaudible*]. It wasn't terribly bad. There were, undoubtedly, cases— Well, there were cases— Again, we were already alert for that. And I remember some samples coming in that we rejected out of hand. You could see the roots just like the paraffin ones I spoke of. In fact, one conclusion we came to fairly early on was that it's extremely advisable to have fresh collections rather than get back to museum archives because you don't always have the records. Somebody may have done something that the present curators had no idea of. And if it didn't jump out at you, you could mess up. Well, we're running down. Are there any—?

ANDERSON: I'm happy to listen.

TAYLOR: Well, I was hoping you'd have a special sample, but it turns out to be your first methane.

ARNOLD: Well, isn't that special enough?

ANDERSON: I remember the very first bang. Besides, Jim was the one who did the dating. If I had a second memory, I suppose it would be the CO_4. I don't remember things.

TAYLOR: Well, our equivalent of that was when we burned coprolites at UCLA. Ah, the whole floor stunk for about three days.

ARNOLD: Well, I must say, you know, there are places in this record of the first Zosser [?] sample where I think the laboratory notes are in your handwriting. I could pick them out maybe off-line with this discussion. But, you know, you and I worked in together. I'm sure that I took some readings for you and you took some readings for me. That could hardly be— Maybe we don't want to keep the tape for it. Let me see if I can—
[Tape stops, then resumes]

ARNOLD: The cyclotron incident.

ANDERSON: Cyclotron or betatron?

ARNOLD: No, it was cyclotron.

ANDERSON: It was still a cyclotron.

ARNOLD: There was a big cyclotron. Alright. And additional remark, an incident we did not describe. Libbey was frequently though about and received suggestions to reduce the background of the counter by going underground. And we did two tests. The one that I'm talking about here, the ————— [*inaudible*] studies was building a big sector cyclotron at one end of the building. And there was something— At one moment, there was something like twenty feet of steel above your head if you were in a little room underneath this cyclotron. The whole building was under construction, so it was opened. Ernie and I— The contractor provided these long ladders so that we could get down there.

ANDERSON: It's about a two-story climb down there.

ARNOLD: Yes. And we got them. And probably a crane to let the shielding down and so on. And we set up down there to see how much difference this would make. Chicago was not a good place to get into a deep mine. But this was a real opportunity with the heavy shielding. Well, that was all very well and good. I remember our delight that Bill Libbey never came down there, so that was a way that we could—

TAYLOR: So, you took your only existing system down?

ARNOLD: No. We had a spare, I think. I think there were two, by that time. Not the— You know, maybe it was the only existing shield.

ANDERSON: It was the only existing one.

TAYLOR: So, you disassembled the shield.

ANDERSON: This must have been back when we still had just a lead-brick shield.

ARNOLD: Well, I think there were iron plates.

ANDERSON: You're right. Yeah. I remember those.

ARNOLD: Okay. This was alright. And the meson [?] count, the count that was eliminated by the ACs [?] went down very markedly. I don't remember numbers. But we saw that we were doing something. However, different colors began to arise. There were kids, of course, in the area. And little kids seeing all this activity came around and came into the building to watch us. And then people started shying pebbles down at— Because the room— Here is the— Can't do this on the tape, but picture the cyclotron as a rectangle. There's a big yoke. These whole pieces in the middle. And then the service room below it extended out and there two little slots that you could clamber in and out of and got materials in and out of. And they were throwing these pebbles, but for an hour or so, they were not getting anywhere. Then they found the range. I remember Ernie and I getting up and yelling at them and of course, they scattered to the four winds. But either then or some soon thereafter date, we came back one day when obviously they had been down there and they pushed the shield over. And the shield collapsed. And our counters and anti-coincidence counters were smashed. And that was a set-back. Since I'm telling the story, the other was going into the steam tunnels. And somewhere, again, in the

wilds of the places that we can't find, there are pictures of Ernie and me in our undershirts dripping sweat measuring the— We were only down about ten or twelve feet below the— We moved the man-hole cover with the ————— [*inaudible*] with the appropriate Chicago authorities. And brought our equipment down there. That was a real eye-shocker. We got out of there as soon as we could.

ANDERSON: There was an interesting personal footnote on this cyclotron-shield business. My third child was being born. And of course, well, that's my wife's job. I've got counting to do. And there were some difficulties in the delivery and the doctor asked her where's your husband? Can we get a hold of him? And she says, "Oh he's in a six-foot cement pit under the cyclotron." And the doctor says, "She's delirious."

ARNOLD: Picture you tangling with some of the Capone gang or something of the sort.

ANDERSON: But anyhow, the minimum background we got didn't change. It was just that the shield embedded the removal of some of the same thing, but the anti-coincidence was saved.

TAYLOR: So, it wasn't reduced. Most of the background come from proton—?

ARNOLD: ————— [*inaudible*]. But what it was, I wasn't even sure of the result you just state. I'm sure you're right. And they had been local gamma flocks or something else of the sort that would be indifferent. At any rate, these were things that took us out of the norm and result— We had various other disasters. Of course, you can't go through any long period in the lab without encounter problems. And this was particularly true later when again, beyond Ernie's time by quite a bit and almost beyond mine, when Libbey for reasons I have never understood hired a couple of technicians who were real clowns. And all sorts of odd events took place as a result of their amateurism, including— The worst case was— This was now when we were in the new building. Bill decided he wanted the portable shield. And they built a shield which they put on a table specially constructed and unsoundly designed by one of them. And I heard one day down the hall a most terrible crash and went running. And there was this fellow standing, shaking on the safe side of the collapse. He never knew how he got there. It was collapsing right on him. He got out. He never could remember how. But it wasn't properly cross-braced and just hold on.

TAYLOR: Portable shield for what purpose?

ARNOLD: Oh, I don't know. I suppose he had the idea of going into the field with the apparatus.

TAYLOR: Well, they finally did that. When I was a grad student, we built a portable gas counting system, put it on a truck, took it out to an archaeological site at Malaga Cove of all places and ran one day. And that was the only time. So, he was interested in doing that way back then.

ARNOLD: Yeah. That's my best remembrance. Doubtful about why this was done.

ANDERSON: Well, the very first shield that we used when we were still measuring Baltimore methane was a portable shield. It was a huge massive cart with wheels on it and it contained just a tight-fitting all-lead shield. We had to lift huge brick off the top of it to get to counter, but of course, with the gas sample counter we didn't have to get into it very often. But I'm trying to recall what his reasoning about that was. I think he figured well, I might want to use it in one of my other labs or somebody might want to borrow the shield and so, make it portable, wheel it down the hall.

ARNOLD: Yeah, yeah, it may have been something like that. Bill was, of course, a creative person.

ANDERSON: Great imagination.

ARNOLD: Yeah. One other— The tape's on now. I might mention that Bill's lab in the new building reminds of a week which was memorable because in that week I was—gave a guided tour at the laboratory to two prime ministers. Éamon de Valera and Palme was the—Olof Palme was the prime minister of Sweden. And it turned there must have been some kind of UN conference in Chicago. And it turned out that both of them had a strong interest in seeing Libbey's laboratory. And Libbey was out of town, so I did the honors. De Valera was a particularly fascinating character.

ANDERSON: He was quite an intellectual.

ARNOLD: Oh yes. And very sharp. So that was another rather striking memory. Anything else, folks?

ANDERSON: I could describe our troubles with the screen-walled. I don't whether I interjected with this before or not, but you certainly weren't the only one who had trouble with it. A lot of it is other graduate students who had to use it—tried to make one or fiddle with it. And in terms of constructing it, they were complete failures. I always subscribe my success to the fact that when I was a kid I used to build models of sailing ships, so I was used to straying fine wires and moving ————— [*inaudible*] across the shrouds, and that sort of thing.

TAYLOR: You built all of the detectors that Libbey used?

ANDERSON: Not all of them. Well, we bought some of them eventually.

ARNOLD: But Bob Shook [?] certainly must have some of the construction.

ANDERSON: And then Bob joined us, yeah. But the anti-coincidence counters— At first, I made them, and then Bill decided he could buy them from radiation counter labs. And so, I happened to be working for radiation counter labs so I made them.

TAYLOR: Let's see, radiation counting labs was a woman—

ARNOLD: No. A man named Ernie Wakefield [?].

ANDERSON: Yeah. It was a private— Yeah. Ernie Wakefield. Yeah— Or you're thinking of—

ARNOLD: Oh, yes. She was—

ANDERSON: She made the boron tri————— [*inaudible*]

ARNOLD: She made a lot of the counters.

ANDERSON: Oh, yes! Nancy Wood [?].

ARNOLD: Nancy Wood. That's right. That's right.

ANDERSON: She made those lovely split mike thin window counters.

ARNOLD: She made all sorts of things. I think at some time or rather we had Nancy Wood counters in the lab as well for anti-coincidence counters. But later and certainly in the later designs, of course, we carried various things further but we started using gas-float proportional counters and then it became much easier because they didn't have to be very gas-tied.

TAYLOR: Oh, so you did use float counters for your anti-coincidence.

ARNOLD: That's correct. Here at La Jolla, that was the—

ANDERSON: For anti-coincidence?

TAYLOR: Oh, at Chicago?

ARNOLD: No. Not at Chicago. I don't remember it at Chicago. I can't recall for the anti-coincidence. But I didn't do very much of that at Chicago. But at Princeton and later at La Jolla.

TAYLOR: Well, that's what I use.

ANDERSON: I'd like to think we're using a float counter for the sample for the screen-wall counter, but why for the anti-coincidence? Geiger tubes are so reliable and more faithful.

ARNOLD: Well, the solar ————— [*inaudible*] these days are proportional counters.

ANDERSON: But they had to have gas flowing through the darn things.

ARNOLD: So?

ANDERSON: Well, it's that extra complication. You've got this big gas cylinder and a bubbler—

ARNOLD: Yeah, yeah. But a gas cylinder lasts six months or something like that. No. They're very, very troublesome.

ANDERSON: It was the Englishman who said that's not the way you make toast.

ARNOLD: Alright. If you'd like. There are a lot of things we did very differently after we left Bill. Although, of course, we held everything to his original impotence, but we did try to improve the way things went. Well, folks, maybe we should really declare it. I think we've—

[END OF INTERVIEW]