Charles A. Barnes, 1993

CHARLES A. BARNES
(b. 1921)

INTERVIEWED BY
HEIDI ASPATURIAN

June 13 and 26, 1989

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Pasadena, California

Subject area
Physics

Abstract
An interview in two sessions in June 1989 with Charles Andrew Barnes, professor of physics (now emeritus) in the Division of Physics, Mathematics and Astronomy.

Dr. Barnes discusses the March announcement of Drs. Stanley Pons and Martin Fleischmann of having produced “cold” nuclear fusion in a tabletop experiment at the University of Utah. Recalls his reaction and that of his Caltech colleagues; the paucity of information; coverage by the *L.A. Times*. Details his collaboration with Nathan Lewis, T. R. Wang, Stephen Kellogg, and Steven Koonin in vain attempts to replicate Pons–Fleischmann experiment. Growing skepticism in the scientific community; Steven Jones’s paper in *Nature* reporting neutron flux; claims of cold fusion by Texas A&M and Georgia Tech; Caltech’s Kellogg Radiation Laboratory colloquium in Beckman Auditorium.

http://resolver.caltech.edu/CaltechOH:OH_Barnes_C_coldfusion

Discusses efforts by Pons, Fleischmann, and University of Utah officials to get money from Congress to establish cold fusion institute. Recalls May American Physical Society meeting in Baltimore and presentation of Caltech data; Koonin's accusation of Pons and Fleischmann's "incompetence and perhaps delusion;" Caltech president Thomas Everhart's embargo of the word "fraud."

Pons and Fleischmann's censoring of DOE's visiting committee; committee's June visit to Caltech. Possible motives of Pons, Fleischmann, and the University of Utah. Pons and Fleischmann's paper in *J. Electroanal. Chem.,* involvement of Cheves Walling and claim of production of helium-4; work of Harwell lab, Italians at Frascati, and group at Los Alamos. He concludes by noting that Kellogg lab continues to pursue aspects of the phenomenon while doubting it will prove a useful power source. Notes difficulties with a peer reviewer of Lewis et al.'s *Nature* paper on Caltech findings.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

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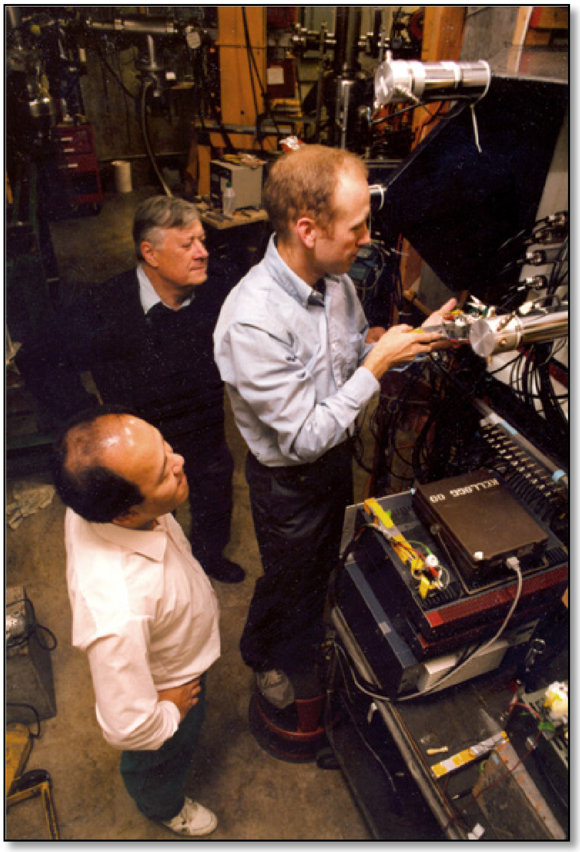

Charles Barnes and research fellow T. R. Wang look on as research fellow Stephen Kellogg inserts a fusion cell array into the neutron detector, in their attempt to replicate the Pons-Fleischmann cold fusion experiment. Caltech Archives

Chemist Nathan Lewis, theoretical physicist Steven Koonin, and Charles Barnes, collaborated on measuring neutrons and gamma rays. Caltech Archives

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ORAL HISTORY PROJECT

INTERVIEW WITH CHARLES ANDREW BARNES

BY HEIDI ASPATURIAN

PASADENA, CALIFORNIA

TABLE OF CONTENTS

INTERVIEW WITH CHARLES ANDREW BARNES

Session 1

1-31

Notified of Pons–Fleischmann University of Utah announcement of cold fusion, morning of March 23rd, 1989, by R. Seki. Frustrating attempts to get more information. Collaboration with N. Lewis and electrochemists in Noyes. Barnes's skepticism coupled with hope. Flawed P-F paper in *J. Electroanal. Chem.* Involvement of S. Koonin, T. R. Wang, S. Kellogg. Superiority of Kellogg laboratory's instruments. Failure to replicate neutron results of P-F experiment. Reporting by *Los Angeles Times*. Looking for gamma rays; failed replication attempts. May 8th Electrochemical Society meeting, Los Angeles. Claims of cold fusion by Texas A&M and Georgia Tech. Caltech scientists' decision not to release any results before submission of their paper.

19-31

Paper in *Nature* by S. E. Jones re neutron flux. Kellogg colloquium in Beckman; reporting by L. Dye of *L.A. Times*. W. Fowler's suggestion of fraud. Pons, Fleischmann, and University of Utah's attempts to get money from Congress to establish cold fusion institute. American Physical Society meeting Baltimore, May 1-2; presentation of Caltech data; Koonin's accusation of Pons and Fleischmann's "incompetence and perhaps delusion." Caltech president Everhart's embargo of the word "fraud." Barnes's talk on cold fusion work on Alumni Day; others' objections to Koonin's characterization. Pons and Fleischmann's censoring of DOE's visiting committee.

Session 2

32-46

June visit to Caltech by DOE committee re Caltech results reported at May 22-25 meeting in Santa Fe. DOE visits elsewhere. Consensus of scientific community on Pons–Fleischmann. Barnes's speculation re motives of P. and F. Role of S. E. Jones and University of Utah officials. P. and F.'s publication in *J. Electroanal. Chem.* pre-empts their communication to *Nature.* Involvement of C. Walling and claim regarding production of helium-4. Reasons for their blunders. Possible "element of wishful thinking." Work of Harwell lab, of Italians at Frascati, and group at Los Alamos. Scant information about original P-F experiment. Mixed effect on attitudes toward the scientific community. Kellogg continues to pursue aspects of the phenomenon; unlikelihood that it will provide a useful power source. Difficulties with a peer reviewer of N. Lewis *et al.*'s *Nature* paper.

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with Charles Andrews Barnes **by Heidi Aspaturian**

Pasadena, California

Session 1 **June 13, 1989**

Session 2 **June 26, 1989**

Begin Tape 1, Side 1

ASPATURIAN: This is an interview on cold fusion with Dr. Charles Barnes, as an appendix to his oral history. I'd like you to cast your mind back to March 23rd, 1989, and tell me how you first heard the news about [Stanley] Pons and [Martin] Fleischmann.

BARNES: Well, the first information that I got about this claim of cold fusion was from a Japanese research fellow. He's actually a professor at Cal State Northridge: Dr. [Ryoichi] Seki, who does most of his research work here at [W. K.] Kellogg [Radiation Laboratory] and is here several times a week. Seki had picked up a message from some Japanese news service about a press release that had been released a day earlier in England by Mr. Fleischmann. Most people don't know that Fleischmann had apparently given this information to the *Financial Times* of London a day earlier, because of an upcoming bank holiday. So I said, "Gee, that's very interesting. What do they say?" And he told me what he could remember of this report. I guess it wasn't very long after that that I got a call from Bob Finn [Caltech Office of Media Relations]. I think that was the history; you know, it was three months ago now. And maybe it was also Seki who gave me a copy of this very brief *Financial Times* article. It said that at noon—our time, I think—on the 23rd, there would be this press conference at the University of Utah at which all would be revealed. But we'd started working on this, or thinking about it already, at this point.

ASPATURIAN: So this was a day before the actual press conference?

BARNES: No, it was the morning before it. The release must have been out the day before, because it had been in the *Financial Times* of London several hours earlier, way early in our morning. You see, they're nine hours earlier than we are here. So if this thing had come out in the *Financial Times* there, at say, seven or eight o'clock in the morning, that would have been eleven o'clock the night before for us.

So I heard it the morning before the press conference on March 23rd. The big deal was to try to get as much information from this press conference as we could. In fact, we struggled all the rest of that day trying to get a copy of that press conference transcript. In fact, I don't think that Bob Finn did get a copy of it that day.

We were getting a lot of information, because it was all over the news, and we were just picking up everything we could from the news and busily talking here in Kellogg about what we could do about it. My personal feeling was that since our business is really doing nuclear fusion—that's what nuclear astrophysics is about, not of these particular reactions mostly, but other reactions, to be sure—the ideas they were talking about were pretty much our bread-and-butter ideas. That, plus the fact that we had in house a very good neutron detector built by Professor [of physics, Ralph] Kavanagh and one of our research fellows, Stephen Kellogg, which was many orders of magnitude more sensitive than the neutron detector that had been used in Utah, convinced me that we were certainly going to be in a position to do a much better measurement job. The other thing was that the Utah people claimed to have detected the neutrons by seeing the gamma rays that were produced when the neutrons were thermalized and captured in hydrogen, in the water bath of the calorimeter. We also, I realized, had much better gamma-ray detectors than what they apparently had been using. But there still wasn't very much information.

In Kellogg we were just putting together whatever we could lay our hands on and wondering what to do about it. We realized toward the end of the day that if we were going to do anything about it, we were going to either have to learn how to do electrochemistry—which is a subject that none of us really knew a lot about—or we were going to have to find somebody who could and would be willing to supply this information. In fact, I was busily turning over in my mind phone calls to [professors of chemistry] Harry Gray or Fred Anson or somebody like this to find out whom we should talk to over in the chemistry division. And sometime either late in the day or the next morning, I can't remember which, we found out that the people over in chemistry in Noyes [Laboratory of Chemical Physics] were wondering about the opposite side of the question. They certainly understood the electrochemistry, and they knew what was being claimed there, but they were busily trying to figure out whom they could get to who knew something about detecting neutrons, which was apparently something that they hadn't been involved in.

ASPATURIAN: For the purposes of this interview, can you recapitulate very briefly what it was that Pons and Fleischmann claimed to have done?

BARNES: They claimed to have seen a large amount of excess heat—that is, heat released far in excess of the amount of power that was put in to run the electrolysis cell. So that was the principal claim, and a lot of different numbers were being bandied about. But they were talking about watts of output, which is very easy to calculate if you assume that the reaction is one of the standard fusion reactions that occur when you bombard one deuteron with another deuteron and make them fuse. The energy-release numbers are well known, and if you say, OK, you're going to produce one watt of excess power, or five watts, or whatever, it's easy to calculate how many fusions per second must be occurring. So something like 10^{12} fusions per second would be required if the reaction was the one that produces D + D [deuteron plus deuteron] goes to a neutron plus helium-3. They were claiming evidence for a very much smaller number of neutrons. In fact, something like 40,000 per second, which is a factor roughly of 10^9 smaller than would correspond to the heat.

ASPATURIAN: Do you recall what your first reactions were when you heard about this, in terms of how believable it was, how plausible any of it sounded?

BARNES: Well, I have to admit that I was a little bit skeptical. On the other hand, you know, we tend to be open-minded about things. I suppose my feeling was colored a little by the hope that maybe it really would work. I realized that many of the claims that were being made for it were a bit too rosy-tinted, but even leaving that aside, I recognized—as most other people did—that if this effect really was occurring, then it was possible that there was a very large and rather inexpensive source of energy being made available, and that in principle it certainly would not produce gases that are responsible for the greenhouse effect. It wouldn't produce gases that destroy the ozone in the far upper atmosphere. And it would probably produce fewer radioactive products and fewer neutrons than nuclear fission does, although claims that it wouldn't produce any of this were not correct.

But anyway, Pons and Fleischmann's principal claim was that they were seeing large amounts of excess energy. They were seeing some neutrons, and they did realize that the number they were seeing was orders of magnitude fewer than would be consistent with the amount of heat they were claiming to measure. They said, or proposed, that some entirely new kind of fusion was occurring. That was even more exciting to us, because of course it would be interesting to know what other kind of fusion could be occurring. As I said before, we were—well, *I* was—skeptical. We had worked on these fusion reactions for a long time, and in particular we had, just a couple of years ago, measured a weak fusion reaction that occurs when you bombard D with D—namely, the reaction that goes directly to helium-4 with the production of the 23.5 MeV gamma ray. We had measured this at low energies, and it turned out to be scientifically very interesting to us. But one thing we could see was that it was occurring at about one ten-millionth of the rate of two other well-known reactions that are the predominant way that fusion occurs. So I realized right away that perhaps there might be some strange, fluky way that this particular branch, which goes to a gamma ray plus helium-4, would suddenly, at very, very low energies, be much more intense than the branches that produce neutrons or protons. I was just genuinely excited about this, as well as having some kind of feeling that maybe something could come of it.

ASPATURIAN: How did you and your colleagues feel about the fact that Pons and Fleischmann had published this information via press conference and not through the usual scientific channels?

BARNES: Well, I certainly felt very frustrated by it—mainly because we couldn't get any information. [Laughter] That's a practical consideration—or a pragmatic one, if you want. In a sense, that was sort of at the front of my mind, because we were already, on March 23rd, seriously trying to discuss what we might try to do about this, and the natural thing that you would like to do is to duplicate the experiment and see if that would let you duplicate the results. "Replicate" is the word people more correctly use. But we were faced with the trouble that even the next day the pictures of the apparatus that were in the newspapers were not very clear, although we could get some idea from these pictures of what the apparatus looked like. Putting that together with what our electrochemist colleagues knew about the electrochemistry, we could form a pretty good picture of what they were actually doing, if indeed the picture they showed was of the apparatus that was actually used. Even to this day, nobody knows whether that's so or not.

ASPATURIAN: Did you or any of your colleagues here in Kellogg make any effort this first day to contact Pons or Fleischmann or the University of Utah to get details directly from them?

BARNES: I didn't try to contact Pons or Fleischmann directly, but either the next day or perhaps the day after that—certainly very early on—Nate [Nathan S.] Lewis [associate professor of chemistry] tried to contact them. And that was proper, because as an electrochemist, Nate knew both Pons and Fleischmann professionally and had talked to them at meetings. They knew who he was. That gave us the best chance of getting some information. Now, I don't really remember exactly which day this was, although it was pretty early. It was actually very hard to get in touch with them, because they were simply— You couldn't get through on the telephone.

ASPATURIAN: And how did your collaboration with the Lewis group start?

BARNES: Well, we were kicking around these ideas, and I guess we had decided we were going to go ahead and try to replicate this experiment. Nate Lewis had been calling around the campus and had called somebody in Kellogg, and so we were in touch with him immediately. It turned out that what they could bring to the experiment and what we could was completely complementary. So we were teamed up, and actually they were making [electrolytic] cells on March 24th, and I guess I'd have to check back to see whether we got a cell on the evening of the 24th or on the morning of the 25th. [It was in the afternoon on March 24th .—ed.]

ASPATURIAN: They were making them in Noyes and bringing them over to Kellogg?

BARNES: Right, right. We still—none of us—knew very much about exactly what we should be doing. [Laughter]

ASPATURIAN: It does sound like that.

BARNES: As you can tell from Doug Smith's article ["Quest for Fusion," *Engineering & Science*, Summer 1989], which details a lot about making these first cells, pieces of information kind of leaked to us through the grapevine for many days after this, and each time that we'd get some new information, it would mean that we'd change our design and try something different. So we kept running through many, many variants of how to prepare the palladium and the current levels and the electrolytes in the D_2O to make the liquid conducting. But because we had this really good neutron detector in Kellogg, we decided that one of the earliest things we would try to do was to see if we could replicate the results claimed on the neutrons. So, it settled down to the extent—well, it was not exactly settled down; it was really turbulent and furious. But it was clear that the division of effort would be that Nate Lewis's group would make these electrolysis cells and we supplied some of the materials for them. They were trying to get materials, too. There was a definite shortage of materials everywhere to try to replicate the Utah experiments. Palladium became almost unattainable from the usual sources for a while, and we had some old palladium.

ASPATURIAN: Here at Kellogg?

BARNES: Yes, and I had some gold foils and pieces of gold that I'd had for other purposes, and I contributed these as anodes to some of the cells. And our half of the experiment was watching the neutrons, and we had a lot of fun doing that. The neutron detector has a certain background level, which had been pretty well perfected—that is, measured and stabilized to a near certainty—before this cold fusion uproar started. It would register maybe 80 counts of background an hour. Now, this background, even to this day, isn't totally understood. Some of it comes from neutrons produced in the room by natural radioactivity—alpha activity in the walls. Some of it comes from cosmic rays that we haven't vetoed. One of the things that makes our background as low as it is is that most neutrons produced by cosmic rays are produced by muons. And so we veto any muons that go through the detector. But we didn't have the whole of the counter covered, just the upper hemisphere. There's a possibility that a few muons could leak through that active shielding. And, of course, the muons could stop outside the cube somewhere—outside the neutron detector—and make neutrons. And even though we have a lot of neutron shielding, again, some of these neutrons could get through the shielding and be detected inside the detector. We don't have any doubt that within these various possibilities there's a perfectly readily understandable explanation of the background counts. In fact, they behave very much like classic cosmic-ray measurements. They do wander up and down a little bit with time, which is exactly what's been known for five decades with cosmic rays. So we had to intersperse runs with the cells in the neutron detector with backgrounds, because of course we must make sure at all times that we know what the background is and what the foreground is.

Now, I don't remember exactly which day it was, but within a few days we had decided that we would have a go at looking for gamma rays. What we did first was set up a germanium detector, which has the property that it gives very precise energy resolution, although it isn't the highest-efficiency detector available. For gamma rays lower than perhaps 5 or 6 MeV, the marvelous resolution of the germanium detector makes it really superior. We set that up and starting looking for gamma rays, actually of any energy but principally focused on the low-energy range, and then set up again a couple of days later—a 3-inch-diameter-by-3-inch-long sodium iodide scintillation counter, which is a better detector for looking at the higher-energy gamma rays. The efficiency of the germanium detector gets quite small for high energies. At some point, we did get a copy of the Pons–Fleischmann paper. It was a copy that had been Xeroxed—not Xeroxed, but faxed, I think—at least several times.

ASPATURIAN: Was this an alleged preprint of their article?

BARNES: It was a preprint, yes. At that time, we imagined it to be the preprint of the article that they were supposedly submitting to *Nature*. It turned out later that wasn't the case. It was not the *Nature* preprint.

ASPATURIAN: What was it?

BARNES: What Pons and Fleischmann had done is that they had apparently submitted two articles, one to *Nature* and one to the *Journal of Electroanalytical Chemistry*.[1] The second one was the one that we had gotten as a fax copy, or via the grapevine—what its path was coming to

[1] "Electrochemically induced nuclear fusion of deuterium," *J. Electroanal. Chem*. 261: 301-8 (1989).

us, I don't know. It had been faxed many times and it was running all over the country at this point, from one fax machine to another. It was, actually, distinctly hard to read by this time. [Laughter]

ASPATURIAN: I can imagine, yes.

BARNES: Really. But we managed to put it together all right. So I had this article, and I was sort of struggling with it. And I could see that what they had done is show a figure of what was supposed to be the pulsite spectrum for the detection of a 2.2 MeV gamma ray coming from the capture of thermalized neutrons in hydrogen. The paper had mystified me a couple of times, or actually several times. It showed evidence of having been written in a tremendous hurry, and there were clearly mistakes in it. For example, in an equation they gave a value of 2.5 MeV for this gamma ray. But in the figure, it was labeled 2.2 MeV, which is about right [for this reaction]. Now, the thing that bothered me in looking at this—since I didn't have much to do except wait for these counts to accumulate—was that I could see that the width of the peak was too narrow. I measured it with a ruler, and even with that crude technique I could see that the peak was too narrow. It was too narrow to be due to neutrons being captured in hydrogen, which is what they claimed it was. And in fact, it was easy to see that it was something on the order of half or less of the width that it would have to be. It wasn't a small error. It was really very strange.

At this point, I was pretty disgusted. I said, "Well, we can't use that for anything, because it's just not possible. It's just incorrect." Stephen Kellogg actually took the 3-inch-by-3-inch scintillator that we had and put a neutron source in a bucket of water so the neutrons would be thermalized and captured in the water, just as was being claimed by these people, and he immediately verified that the width of this peak in the Pons–Fleischmann paper was hopelessly too small. He also showed something else that, I guess, in a way had bothered me but that I hadn't really focused on when I was looking at the figure. And that was that just below the full-energy peak that they claimed they were showing, the count rate goes up again in a very characteristic way into what's called the Compton edge. It's an unavoidable consequence of a basic process of detecting gamma rays in a scintillation counter that you get this secondary rise in the spectrum below the full-energy peak. And once we saw this, we could see that that was also missing from the Pons–Fleischmann results, and that simply just confirmed that this was a kind of rubbish peak. Well, it turned out that some people at MIT who were a little cleverer than we were had been watching, I guess it was, the *MacNeil/Lehrer NewsHour*, and they'd seen, either in an interview there or on footage of the actual press conference, that one of the members of the group had held up a graph of the complete spectrum, not just the peak that was shown in the paper we had. These clever fellows got in touch with the [PBS affiliate] TV station in Salt Lake City and got a videotape of the program, and from the videotape they were able to take stills of the complete pulsite spectrum. What they found was not only—as we had seen—that the peak was too narrow and didn't have a Compton edge but also that it wasn't even a 2.2 MeV peak. It was a 2.5 MeV, in agreement with this erroneous thing in the paper.

ASPATURIAN: In the text, but not in the figure.

BARNES: And so these people, I think, deserve great credit for their ingenuity in managing to get a copy of the original, complete spectrum in spite of the fact that Pons and Fleischmann were apparently doing everything possible not to give out any information. I never thought of the TV, but they did.

ASPATURIAN: You found this out via the phone from people at MIT?

BARNES: Yes, I guess they phoned Steve [Steven E.] Koonin [professor of theoretical physics, on leave at the Institute for Theoretical Physics, UC Santa Barbara] first. We were in touch with Koonin, frequently, between here and Santa Barbara, mainly by computer.

ASPATURIAN: Was there a lot of back and forth between you and colleagues at MIT, Los Alamos, Stanford, and other places during this period?

BARNES: There wasn't a whole lot of back and forth on this subject. There was a lot of back and forth between us and Steve Koonin. And Koonin was actually spending, I guess, a good deal more time intercommunicating with other people and passing on to us any information that looked useful. But most people were in precisely the same position that we were—they just were struggling to try and get some information, and there wasn't any forthcoming.

ASPATURIAN: How did your colleagues and you feel about the fact that you were essentially dropping your own research to go ahead and pursue this piece of unusually released scientific information?

BARNES: Well, I can only speak for myself, of course.

ASPATURIAN: OK. I thought you might have discussed it.

BARNES: Well, we had lots of discussion on this subject. [Laughter] The small group here at Kellogg and Nate Lewis's group, which is much larger, over in Noyes. Certainly, I reflect our opinion that *if* this effect was real, it was clearly the most important thing for anybody to be working on. And if it was false, it was very important to show that immediately, before people started doing really foolish things and making foolish plans for the future and so on. So, no, I didn't myself have any real doubts [about the importance of trying to replicate these experiments]. Certainly, the two people who were most actively engaged on it here in Kellogg were our two postdoctoral fellows, T. R. [Tung Rung] Wang and Stephen Kellogg; and until he went to Princeton, a graduate student named Bruce Vogelaar also participated. They were fantastically good at getting this whole program launched from our side of it, which was the side of the neutron and gamma-ray detection work.

ASPATURIAN: When did the first neutron-detection experiments start here?

BARNES: Well, I believe that we were making neutron measurements already on March 24th.

ASPATURIAN: Really? Was this before or after you and Nate Lewis's group had gotten together?

BARNES: No, no. We certainly made no neutron measurements before we got the first electrolysis cells from them, OK? That had been the agreement that we'd come to completely spontaneously, and it didn't require any argument or discussion, because they themselves were already making electrolysis cells. They understood the electrochemistry, and we had the wherewithal to do the neutron and gamma-ray work, so it was a natural division of labor.

Immediately, we started measuring backgrounds with the neutron detector, and as soon as we got the first cell from them we put it straight into the neutron detector.

ASPATURIAN: Was there a big crowd of people down there when the first measurements were taken? Were there any expectations of any sort whatsoever?

BARNES: Well, you know, everybody was talking about this business by this time. I guess about half of Nate's group, at least, had come over with the first cell, and—

ASPATURIAN: How many people was that, roughly?

BARNES: Oh, the total number of people who were down there in the lab when we tried this must have been twenty or more. It was just a huge crowd. Because they collected people as they walked along, and people from Kellogg also were all excited. They knew what was going on. Everybody was buzzing about it. When these guys appeared, of course, they all just migrated down to the lab. There was great excitement, and I don't know what we really expected. I suppose that I thought that when we put this thing in the cell, we'd immediately see a big increase in the neutron counts from above background. To get a good look at the numbers, we would have to go back into the other room where the computer was, where all the other data were displayed. But we had an indicator right there on the neutron detector, because there was a scaler that we could look at, and we could see on it that nothing was happening. And my first feeling was of tremendous disappointment. I suppose that I really thought that this must be right—that there must be neutrons. Remember, this is before we'd found out, figured out, this terrible blunder about the neutron detection at Utah. So we really believed it—you know, you supposedly are making thousands of neutrons per second, and we had a detector that was 100,000 times as efficient as the neutron detector at the University of Utah, so it should just have gone crazy. You could just tell *instantly* that nothing had happened to the count rate on the scaler. And I was just really disappointed. I knew everybody else would be, too, but, you know, there was just a kind of silence. Just stunned silence from everybody. Nothing happened. And then we went into the other room and looked at the computer output, and, of course, we got the same messages. Nothing happened. I guess I'd mentioned to—I think—Doug Smith that after I realized that there weren't any neutrons, or at least no large number of neutrons, I guess I was kind of secretly relieved. Because I suddenly realized that we had all of these people standing around this thing, and if it suddenly was producing neutrons at a level—not like the level that had been reported at Utah, but at the level that should have been there for the amount of heat they were claiming, then everybody would have received a really serious neutron dose. And I realized that, in a way, we'd been a little bit careless in not preparing for the eventuality that there would be a big blast of neutrons. But I also realized that the reason that we hadn't done anything about it was because Pons and Fleischmann were still alive to give this press conference and that therefore whatever the neutron flux was, it would have to be at some modest level. It wouldn't be too dangerous. I'm sure that if that information hadn't been in our hands, we would simply have tried to keep people away from the cell.

Of course, putting the cell in the neutron detector doesn't suddenly turn the neutrons on. That just detects them. I mean, people had been carrying this cell all over Noyes, OK, with the current running in it and everything. So, in a way, at least, there was a minor consolation, perhaps: that nobody was going to get irradiated as a result of this. That didn't make up for my really immense disappointment that there wasn't an instant response.

ASPATURIAN: You've mentioned a couple of times that your neutron detector was 100,000 times more sensitive than the one at Utah and also that the gamma-ray detector was much more sensitive. How did you know what the sensitivity of the Utah instruments was?

BARNES: Well, you're asking me to really stretch my memory on this. It must be from information that was given at the press conferences.

ASPATURIAN: I see.

BARNES: Because that's the only information we had. I mean, they obviously didn't talk about the— Some of this, of course, may be that I'm filling in details from later information that became available when I got their preprint. I knew that we had a superior neutron detector. But it's a little hard for me to be sure at what point I recognized that we had one that was 100,000 times their sensitivity. I don't know just when that came; it may have been several days later. But there was enough information in their press release. There was something about how both their neutron detector and a gamma-ray detector had been borrowed from the university's radiation-safety office, which is not the way you get state-of-the-art instruments. And that information *was* available in the press conference. So I knew that we had vastly better equipment than they had. I knew that we had a state-of-the-art neutron detector. But this 100,000-times-more-efficient figure that I put on it could have been from information that came later.

ASPATURIAN: I see. I was curious, because it seemed like such a precise figure. During the first week and a half of this, the media was all over the story. The *Los Angeles Times* interviewed you, I believe. How do you feel they handled it? Do you think they were responsible? That they did a decent job?

BARNES: Well, to say that the *L.A. Times* interviewed me was really not quite a clear statement of exactly what the situation was. What happened was that—and maybe you better not publish this—Bob Finn asked me if I would talk to the reporter from the *L.A. Times*, Mr. [Lee] Dye, and I realized, as soon as I talked to Mr. Dye, that he really didn't know very much about his subject. I felt that I could do some good by trying to explain to him my understanding of what had been said and what was happening and my understanding of how the reaction should go and how it might go if it had gone some different way. I had several conversations with Mr. Dye that were the basis of articles he wrote, in which my name—by agreement—wasn't mentioned at all. I was just trying to help him. But, you know, he kept calling me every couple of days. At some point, I guess I agreed to try to answer some more questions and said he could use my name. And I was, you know, trying to make sure that he understood these things as correctly as possible. I think, for the most part, the articles were about as good as one could expect from somebody who really didn't know the subject very well and was sort of working out the answers to questions he was asking me. You see, it was really not even a real interview. But he would ask these specific questions, and I tried to explain in my answers. He obviously learned something, because then he'd ask another question, but what I was trying to do was to give him an education in nuclear physics in a very short time. I tried, and I guess he actually quoted me in a couple of articles. To be sure, there were some things that got into the articles that were actually erroneous. They weren't very serious, but, for example, he called helium-4 a rare form of helium, and everybody knows, of course, that helium-4 is, by a very large factor, the abundant form of helium.

One of these articles I got somewhat piqued at because of the headline that had been put on it, which I realize now was probably not Mr. Dye's fault at all, since I understand that reporters have very little control over what headline writers put over their articles.

ASPATURIAN: I believe that's true.

BARNES: And other people have assured me that it's true, but I was somewhat annoyed because this headline said something to the effect of, "Cold fusion violates laws of physics." [Lee Dye, "FUSION CLAIMS ARE AT ODDS WITH BASIC LAWS OF PHYSICS," *Los Angeles Times*, April 12, 1989] Well, I would never have used the words "laws of physics," because, you know, physics is not a collection of laws. And I wouldn't have said that it violated it. In fact, I was quoted in the article as saying something like if the reports, as they were emerging, were correct, we were all going to learn some new physics from them because they weren't consistent with what we already knew about the process.

ASPATURIAN: Which is not the same thing as saying they violate the laws of physics.

BARNES: It's not the same thing at all. But the worst thing was that he had this headline, and the first words in the text, immediately under it, were "Professor Charles Barnes, professor of physics at Caltech...," and so on.[2] And so for those people who never read anything but the headlines or the first lines of an article, I was tied, unfortunately, to this headline. I got a lot of bad mail about that.

ASPATURIAN: When you say "bad mail," do you mean from your colleagues in the field?

BARNES: No, no. Most of my colleagues in the field understood that this is not language I would have used. No. Throughout this whole period of three months, I received a large amount of mail from random individuals around the country. Most of it is perfectly harmless and most

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[2] The article begins: "Charles A. Barnes is a highly respected physicist at Caltech, and like others in his field, he believes that he understands the basic laws of physics that govern such fundamental reactions as the melding of two atoms into one through nuclear fusion."

of it is from very well-meaning but rather untutored people. But occasionally some of the letters were a bit strong, I thought, and I didn't like the language.

ASPATURIAN: Was this the first time that you had experienced a lot of media coverage of something you were working on?

BARNES: Well, it certainly was the biggest media coverage of something I'd been working on. It wasn't at all the first. It was certainly the widest coverage. These articles that were in the *L.A. Times* of course went all over the country. People were calling me from Seattle and from Idaho and gosh knows where, having read these articles and wanting to know more about what was in them. So there was a naturally very wide interest everywhere in the country. I guess, in retrospect, I shouldn't have taken too much exception to this, because it really wasn't Lee Dye's fault, I'm sure. On the other hand, I was also extraordinarily busy at this point. We were getting busier by the hour, it seemed, and we were running more and more cells, and we had set up another gamma-ray detector that would do an even better job of registering 23.5 MeV gamma rays.

We were looking for gamma rays, particularly for low-energy gamma rays, to see if, just on general principle, any of them were emitted. And at some point, because of the calculations Koonin and [Michael] Nauenberg [professor of physics, UC Santa Cruz] were running at Santa Barbara, it became clear that, by rights—by which I mean, in the simplest way of understanding fusion—the fusion of a proton plus a deuteron going to a gamma ray plus helium-3—should occur at low energies with a much bigger yield than D + D fusion. So in fact we actually ran some cells with both hydrogen and deuterium in them just to see that we could look for gamma rays. We realized that there was always a possibility of some hydrogen contamination of our cells anyway, and so we felt it was essential to look for these gamma rays. But, in particular, we were more and more concerned about the reported neutron flux. I guess we still hadn't really completely rejected that report at this point, but we understood that there was clearly a discrepancy between the claim of the Utah people that they were seeing some 40,000 neutrons a second and the amount of heat that they were getting, and that if somehow or other the reaction was producing helium-4 plus a 23.5 MeV gamma ray, then we should be looking for this particular gamma ray. As I say, the first attempt to do this was with a 3-inch-diameter-by-3-

inch-long scintillator, which certainly would detect them, but we also realized we could do a better—a much more sensitive—search for those gamma rays with a bigger scintillator. And so we switched to a much bigger scintillation counter. For the remainder of the time—up to the present, in fact—we've been running all our samples simultaneously with the germanium counter to look at the low-energy gamma rays and with the big scintillator to look for high-energy gamma rays.

ASPATURIAN: Do you remember roughly when you brought in the larger scintillator? Would this have been a week and a half in, or two weeks?

BARNES: It could have been. We were awfully busy trying to keep up with things. It could have been delayed for a week and a half.

ASPATURIAN: Do you recall it dawning on you during this period, or becoming more likely to you and the people you were working with, that perhaps this was all scientific error? Not on your part, I mean, but on the part of the University of Utah?

BARNES: Yes. Well, it certainly—I would say it went a different way than we had hoped. Our conviction that this whole thing was possibly a giant error rather grew. I don't think it occurred to us so suddenly. But you know, we were convinced something was not right as soon as we found this terrible blunder that had been made by the University of Utah people, where they had measured this erroneous peak that had nothing to do with detecting neutrons. Even to this day, nobody knows for sure what that peak was caused by.

ASPATURIAN: Pons and Fleischmann still aren't talking about it.

BARNES: That's right. The truth is, jumping ahead a little bit, that by the time of the Electrochemical Society meeting [May 8, 1989] in Los Angeles, the first thing that was done by Fleischmann and Pons was to completely withdraw their claims of having seen neutrons; and incidentally, they also completely withdrew their claim of having seen helium-4, which was made in a separate press release a few days after the original one. But anyway, that was consistent with the fact that we weren't seeing any neutrons. But it seemed to us that it was going to be essential to do our experiments with high sensitivity, because as long as there was a chance that you could really have cold fusion, this would be a tremendous scientific discovery, at the very least, and might still have been something important. The real conviction that Pons and Fleischmann were totally wrong grew as our electrochemical colleagues prepared sample after sample, electrolysis cell after electrolysis cell, using completely different techniques of pre-melting the rods and various other things, everything we could think of. In fact, everything we picked up from the grapevine, newspapers, or BITNET, or anywhere, immediately caused us to try a different kind of cell. We kept trying more and more cells. I can remember sitting down with Nate Lewis's group. In fact, all of us went over to Noyes early on, just a few days—well, probably a week and a half—into the business and had a kind of thorough discussion of the electrochemistry of these electrolysis cells. By this time, Nate's group had run enough of these cells, and they were still getting no heat. And, you know, it just gradually became clearer as we tried more and more of these samples that the chances we were going to see any excess heat was going down.

ASPATURIAN: And at the same time, your group was not—

BARNES: And we were coming up with zeroes on the neutrons and zeroes on the gamma rays on all of the cells that we were running.

ASPATURIAN: What were the reactions here when the word came out—I think it was about April 9th or 10th—that Texas A&M had detected heat and that Georgia Tech had found neutrons?

BARNES: Well, we were certainly pretty excited about it. The two reports are in a sense coupled by the fact that they are both claiming cold fusion, but the same thing is true of those two reports as was true in the initial Pons–Fleischmann thing: There are so many orders of magnitude—perhaps 10^9 orders of magnitude—between the heat output that Pons and Fleischmann were claiming and the neutron flux that they were claiming, albeit erroneously. The factor between the heat claimed at Texas A&M and the neutron flux claimed by Georgia Tech was even larger than that. They were detecting a much smaller flux of neutrons than had originally been claimed. And so, in a way, those two things are decoupled, and by the time that we got this far, we were convinced that there wasn't going to be any significant number of neutrons, but we thought there might be a few.

ASPATURIAN: You mean by the time these two universities—?

BARNES: By the time these two people reported. Now, it turned out that we were able to immediately get in touch with people in Professor [Charles] Martin's lab at Texas A&M. Because one of the research fellows here had actually worked in that lab and knew people there well.

ASPATURIAN: Here in Kellogg or in Noyes?

BARNES: In Noyes. One of the electrochemists had worked in Martin's lab. So there was a certain amount of conversation back and forth, and we asked them if they had tried the experiment with H_2O. They said that they were doing it. We asked them if they'd tried looking for neutrons, and they said they were doing that, too. Well, I don't think they *had* been doing that. [Laughter] I think that was when they started doing it.

ASPATURIAN: Had you tried it with H_2O here?

BARNES: Yes, we'd been working with samples with H_2O and D_2O and mixtures of them. Anyhow, it was just a couple of days after the Georgia Tech report that Georgia Tech withdrew these results. Now we felt a lot better about that, but we had been sure they had been wrong, because the neutron flux they were claiming—albeit not very big—was many factors of 10 bigger than what we were seeing, and we simply felt quite sure they had to be wrong.

ASPATURIAN: I believe it was about this time that the report came out from the materials science guy at Stanford and also from MIT—the fellow who tried to patent this. Hagelstein? [Peter Hagelstein, a member of the electrical engineering faculty at MIT, developed a model purporting to explain cold fusion, and in April 1989, per the *New York Times*, MIT applied for patents based on his work.—ed.]

BARNES: Yes, well, we were certainly concerned about those reports, of course, because we had to include the possibility in our thinking that we were simply not doing the right things and that maybe this was still a real effect somehow. As I said, I felt personally very confident that the Georgia Tech results were wrong, because it was just so big a clash with ours. It also might have been possible that they had stumbled on some trick that we hadn't. The one thing I worried about with the Georgia Tech neutron results was that I didn't know that much about their neutron detector, but we got a fax of their press release the next day, and right away I just knew we had an infinitely better neutron detector than they had, and then I was not so worried about it.

ASPATURIAN: One thing that interests me is that while Pons and Fleischmann conveyed everything to the media, you and your colleagues essentially kept your doubts and your reservations and so forth to yourselves throughout this entire period. What was the reason for that? You did not report anything to the press.

BARNES: Well, we certainly— There were, in my opinion, mixed reasons. It wasn't all because we were nice fellows, OK? But you know, that was an element of that. And also we ourselves were suffering from the fact that no information had been released by Pons and Fleischmann, and we'd had to struggle to try to find out what they might have done, using every kind of subterfuge possible, every kind of rumor that we could pick up. We decided that we didn't approve of this method of releasing scientific information, and that if it was at all possible, we wouldn't release any information until we'd submitted a paper. Somewhere in this period, we got [Steven] Jones's preprint from Brigham Young University.[3] That was actually the first preprint we got from anybody.

ASPATURIAN: This was before you got the Pons and Fleischmann—

BARNES: Well before we got the Pons and Fleischmann preprint, yes. The Jones one came through the system pretty fast. Don't ask me how. I'm sure that Steve Jones didn't intend to send these out to everybody in the country, because he was under the impression that he had an

[3] S. E. Jones, *et al*., "Observation of cold nuclear fusion in condensed matter," *Nature*, 338: 737-40 (1989).

agreement with the people at the University of Utah that they would simultaneously submit papers to *Nature*. It came as a complete shock to him when they issued this press release first, and he decided that he wasn't going to do that. He wasn't going to have a press release. However, copies of his preprint did leak, and we got one after just a very few days and several days before we got a copy of the Pons and Fleischmann paper.

ASPATURIAN: When you say "leak," he was not responsible for its being premature? They just got out, in other words.

BARNES: I don't know. He may have distributed a limited number of them. You see, we were actually in a pretty good position to talk to Steven Jones, because he'd been to this lab [Kellogg] the previous year to give a seminar on muon-catalyzed fusion. So people here knew him and knew his work. As it happens, I was in Germany at the time he gave his talk here, but on the strength of the fact that he'd spoken here, he's always been very friendly to me, and I've had no difficulty talking to him. I certainly had difficulty getting in touch with him the first two or three days, but that was a simple matter of everybody in the country trying to call him. He simply couldn't take everybody's phone calls. So it was, in fact, several weeks before I had a chance to talk to him. But we did have his preprint, and we had admired his decision not to have a press release and go public in the same way [as Pons and Fleischmann]. The other thing, honestly, that might really have been a consideration, with myself and maybe with some of the others, was, we're thinking there's still a possibility that there is still some trick, and that we find this trick—maybe it's in the method of preparing the palladium, or maybe there's some ingredient that has to go into the electrolyte that we don't about yet, that we might figure out if we keep working.

There was *always* a possibility in our minds that this effect could still be real, in which case we would have looked pretty stupid if we'd gone public with lots of press releases and so on, just at the time when suddenly it was being discovered how to do this. But, you know, that was a less altruistic kind of motivation. We didn't have any conflict between these two motivations, and so we could certainly hold our heads high and say that we weren't going to give out information until we'd submitted a proper paper, but we also had this other additional inducement. As a matter of fact, it wasn't just anything to do with this particular episode. I mean, it's a general principle of mine that when I'm trying to answer a question in an experiment, I want to be sure of my answer before I give it. At this point, though, while we were becoming reasonably sure that this thing was some sort of blunder, we weren't, of course, absolutely sure.

ASPATURIAN: I notice that Lee Dye managed to somehow cover the Kellogg colloquium that was held before 800 people in Beckman [Auditorium, April 21st]. Do you have any idea how he got hold of that information?

BARNES: Yes, yes. I have no idea at all how he managed to know about this meeting and also how he happened to get in there, because I know that Bob Finn and Hall Daily [director of Caltech Media Relations] were watching the doors as people came into the hall and were looking especially for members of the press. They didn't see him. But it is evident that Lee Dye was there. Now, I don't suppose that he would necessarily have had a connection with this reporter from KFWB? You know, there was a KFWB reporter living on the campus at this point.

ASPATURIAN: Oh, that's right. Jack Popejoy.

BARNES: He was supposed to be living primitively, as one would have to live primitively if there'd been a major earthquake or something.

ASPATURIAN: That's right. He was operating out of a tent.

BARNES: That's right! And so he'd chosen a less-than-primitive place to set up a tent—on the Caltech campus somewhere. [Laughter] And because of the general word-of-mouth business about the Kellogg seminar, he could hardly have missed hearing about it, and I suppose it's possible that if he was a friend of Lee Dye's, he might have contacted him. But it's more than likely that somebody else on the campus is a friend of Lee Dye's and simply called him.

ASPATURIAN: Was it disconcerting to find information talked about in a supposedly closed session put on the front or the third page of the *L.A. Times* a day later?[4]

BARNES: It was a little disconcerting.

ASPATURIAN: You did not realize it was going to appear?

BARNES: No, we certainly didn't. In fact, we had purposely embargoed this meeting. In fact, Lee Dye in his article said that the detailed results were embargoed until a paper was submitted. Of course the most important result was that we hadn't seen anything in the heat, neutrons, or gamma rays. And that *was* in his article. [Laughter]

ASPATURIAN: I was amused by the fact that he said Nate Lewis sounded sheepish. I thought he sounded anything but sheepish.

BARNES: I don't think Nate was the least bit sheepish. I think Nate was a little bit incensed about being called sheepish. People were kidding Nate, and they'd say, "Bah!" [Laughter] So anyway, I don't know how Lee Dye even got let into that, but it started with the people in Kellogg saying we'd like to have just a Kellogg seminar on the subject, for the people here in this building. Well, it seemed like a good idea. When it started out, it was going to be Nate and me, and we'd just divide it. In the course of conversations with Steve Koonin, it became clear that he wanted to be part of it, too, so it became a three-way circus.

ASPATURIAN: Had he come down from Santa Barbara by this time?

BARNES: No, he was still at Santa Barbara. Of course, he came down here—made a special trip down.

ASPATURIAN: I see. So he made a special trip for this.

[4] "Scientists Grow Frustrated Over Fusion Claim," *Los Angeles Times*, April 23, 1989.

BARNES: He had his family still up there, I guess. He used to come visit here periodically. So anyway, I guess what happened was that early in the morning on the day of the seminar, I realized that there was going to be a seating problem, so I fixed it up so we could have 151 Sloan [Laboratory of Mathematics and Physics], which will seat eighty people, instead of the twenty that we can put in the Kellogg Library. [Laughter] I guess a little later somebody said, "Look. Eighty isn't going to do it. You'd better get 201 East Bridge [Laboratory of Physics]," so I reserved that. While I was at it, I picked up the phone and called the Office of Public Events, particularly [OPE manager] Jerry Willis, with whom I'd worked when I was chairman of the [Institute] Programs Committee, so I knew him quite well. I said, "Jerry, we may be in a bind. I don't know whether we can fit our audience into Bridge or whether we're going to have to have Beckman." He had something scheduled for the evening, so that was a little bit of a problem for him, but he said, "What we'll do is, we'll hold it—we won't make a commitment until we hear from you, but we would have enough time to clean the place up after the Kellogg seminar." I think they had promised the auditorium to some nonprofit organization. Anyway, I guess at about two o'clock somebody said, "Well, who's going to make the decision as to where the meeting is going to be?" I said, "Well, I don't know. Are you?" He said, "No, you are." So I said, "OK. It's my judgment that there are going to be more people than can get into Bridge," which will hold about, I guess, 225 or 250. I'd been in this position before, having had to move seminars from Bridge to Beckman quite often.

I'd tried, incidentally, in the meantime to get Ramo [Auditorium], which fortunately was busy, because it wouldn't have worked. Well, as it turned out, I said, "Look. This is just too clumsy, having to move people from Bridge over there at the last minute. Let's just make the assumption that there are going to be more than we can put in Bridge, and we'll go to Beckman." I called Jerry and said, "We'll be there at four o'clock," and so that gave him time to get ready. People sort of went around the campus changing the signs once again. [Laughter] We had posted signs for the room changes on the doors of the rooms, so that if anybody had come to the Kellogg Library, they would immediately get directed to 151 Sloan, and from there, there was a sign directing them to Bridge, and from there right away there was a sign saying Beckman. As it turned out, a sufficient crowd collected that I think hardly anybody went to Bridge at all. I just saw this mob of people moving across campus. [Laughter]

ASPATURIAN: Streaming toward Beckman. Were you surprised at the size of the turnout?

BARNES: I was tremendously surprised.

ASPATURIAN: Were you? Why?

BARNES: Well, because I didn't think that we'd let anybody know, you see. I figured that it was not known. I had no idea that the news had spread like that.

ASPATURIAN: But you realized it had spread enough to make a move to Beckman advisable.

BARNES: I don't know. I guess people had been calling me. Enough people called saying, "Are you really going to have this in Bridge?" that I guess I began to realize that this is getting out of hand, it isn't going to work.

ASPATURIAN: I was one of those people. I called Liz Woods [staffer in Kellogg], and I had that question.

BARNES: Well, she was calling me, too, every few minutes. Meanwhile I was busily trying to prepare my talk, because I didn't have any idea what I was going to say at that point. Since it was only a third of a seminar, it was easy.

ASPATURIAN: Was your collaboration with Nate Lewis and his group the first time you had done a major interdisciplinary project—outside physics, I mean?

BARNES: Well, it's certainly the first time I'd ever been involved in an interdisciplinary thing on this scale, yes. One thing I should put on the record is that from my point of view, this was a very, very successful collaboration. There was so much enthusiasm on everybody's part—their people and our people both—that it was really an extremely happy collaboration. People were willing to work most of the night as well as all the days. People worked very hard, and there was mutual respect. I mean, they were doing things that we didn't know how to do very well, and we were doing things that they didn't know how to do. It wouldn't be complete without acknowledging this tremendous advantage of the fact that we were able to bring these complementary techniques together on a problem.

ASPATURIAN: About the time of the Kellogg colloquium in Beckman, I got together with your colleague Willy [William A.] Fowler [Institute Professor of Physics, emeritus] on an entirely different matter. And he delivered himself of the opinion that he felt this was quite possibly a deliberate fraud. Was he alone in this view?

BARNES: He thought there was a deliberate fraud?

ASPATURIAN: Yes.

BARNES: I don't know what people thought about it. I didn't think it was a deliberate fraud, and I still don't. I think that Pons and Fleischmann blundered somehow. I think that they're seeing some kind of chemical heat from bad measurements. But one thing I'm certain of is that they're not seeing nuclear fusion. I don't really understand why they were getting the results that they did. The one thing that did create bad feeling among many people, including me, was that Pons and Fleischmann, along with various officials from the University of Utah, were not only lobbying for funds from the Utah legislature—they also went to a committee of Congress [the House Committee on Science, Space, and Technology—ed.] asking for a large amount of money to establish a big institute for studying cold fusion, and made the most of the case that they had, and completely ignored any contrary evidence. In fact, one of the officials of the Utah party made this famous statement: that we couldn't even afford to spend another month waiting to see if these results are verified, because if we did it would be just like television and VCRs—the Japanese would beat us to it, which would be worse. The congressional committee was very sensitive to that. Then finally somebody asked the Utah delegation how much money they were talking about. The president of the university, Mr. Chase Peterson, said, "Well, $25 million would be a good start toward it."

ASPATURIAN: A good start?

BARNES: I think that that was a little bit fraudulent, OK? I think that any truly honest person would have said, "Look, there is now contradictory evidence coming in from various places. We think our results are correct, but we understand that this is something that has to get settled. I expect that within a few weeks we'll be able to get a better picture of how this looks, and then, if it looks promising, that will be a time for making a major national effort to do this." I don't blame that on Pons and Fleischmann.

ASPATURIAN: Throughout your six-week or eight-week effort, did you personally ever speak to either Pons or Fleischmann?

BARNES: No. I could have talked to them at the electrochemical conference, but I didn't actually see any real point in it, especially as they'd started this session on cold fusion by withdrawing the results on the neutrons, just completely abandoning those results. So that left our principal activity completely disconnected from what they were claiming. I was also, of course, thoroughly familiar with what Nate and his colleagues were getting on the calorimetry, and we had discussed this many, many times, had gone over it backwards and forwards. I was convinced that Nate and his group were correct in the way they were doing it. It was my feeling that the biggest part of our results was actually the calorimetry, because that directly addressed Pons and Fleischmann, not Jones and the other people who were reporting small neutron fluxes. I think it was perfectly correct that Caltech contacts with Pons and Fleischmann, to the extent that they existed, were by Nate and not by me.

ASPATURIAN: I think that about ten days or two weeks before the electrochemical meeting, the APS [American Physical Society] meeting was held in Baltimore. Is that roughly a correct chronology?

BARNES: I think it's even closer than that. My recollection was that it was barely more than a week earlier. It was a very short time. [The APS meeting was held May 1-2, 1989.—ed.]

ASPATURIAN: And I believe Nate Lewis made a substantive statement for Caltech at what was essentially a physicists' symposium. How were arrangements made that he should be there? Was Caltech unique in having an electrochemist comment on this at the APS meeting?

BARNES: I don't know. There actually were two sessions. There were a huge number of papers—it started out to be one special session and eventually ran for two nights in Baltimore, at which there were forty papers presented.

ASPATURIAN: On the cold fusion issue alone?

BARNES: On cold fusion, right. Pons and Fleischmann, as you know, were not there.

ASPATURIAN: Yes, I do know that. But I believe Dr. Jones was?

BARNES: Jones was already at the meeting, because he had submitted an abstract to talk about his cold fusion results. But when they decided to organize a special session—which they could hardly have avoided doing, considering the enormous interest around the country—the people who were in charge of organizing the special session certainly invited Jones to speak. They also invited Pons and Fleischmann to speak. And a number of other people. Then they also accepted, in addition to the invited papers, contributed papers. That is what filled out the two nights' worth. When Pons and Fleischmann declined to speak, that made it possible to put the Caltech work on as an invited paper.

ASPATURIAN: I see.

BARNES: I think it is true, and I think it's not immodest to say, that we had by far the most complete set of data on all aspects of the problem of any place in the country, including national labs at that time. It was an internal judgment here that Nate would give the paper and not me, because the most dramatic part of the Pons–Fleischmann claims dealt with the electrochemistry. Just on the basis of numbers, there must have seventeen or eighteen people in Nate's group that were part of the effort, and there were only four of us.

ASPATURIAN: By that time, did you have your data written up in preliminary form for distribution?

BARNES: Yes. Oh, we wrote it up. Nate presented our data. He was very careful to say that this was "the work of physicist Charles Barnes and myself and our colleagues." You can see that in all the press releases and things. So, our results were largely complete at that time, and in fact we had started writing a paper, and as soon as we got back we beat on this paper for several days and sent it off.

ASPATURIAN: It was also at this meeting that Steve Koonin made his famous statement about the "incompetence and perhaps delusion" of Pons and Fleischmann. Doug's [Smith] article said that he had discussed that pretty thoroughly with a number of people here. Were you in on those consultations?

[*Four lines permanently redacted*.]

BARNES: I'm going to be a little bit careful, even so, in saying that I think it was true that Steve had planned to use some words that were stronger than the words he used.

[*Six lines permanently redacted*.]

ASPATURIAN: I see.

BARNES: I personally said, "I think you shouldn't do that." And I guess a number of other people felt the same way about it. Nate and I had discussed how we were going to handle it. We said that we don't have to, sort of, you know, use any tough language, because the whole thing will die on its own just from the scientific results eventually if it's wrong, and we don't have to get involved in that. In fact, somebody or other— I don't know how it happened—maybe even Koonin himself might have asked for guidance or something, or it might have come through Nate, or it might have come through somebody else—but anyway there was a chain of people that included the chairman of [the Division of] Chemistry and Chemical Engineering, then the provost, and finally the president. The president [Thomas E. Everhart] absolutely said, "Under no circumstance must the word 'fraud' be used in any communication that has Caltech's name connected with it whatsoever." That message certainly got to Steve one way or another.

ASPATURIAN: It sounds like it did.

BARNES: And he didn't use those words. But I'd been talking to him, because we'd been talking nearly every day, and he says, "You know, we really must, because of the large public attention to this, strive for maximum effect on this thing." And I said, "I don't really agree with you. I think the effect of our paper will be devastating, even if we don't use any judgmental language whatsoever." That was the only communication I had with him on the subject. I did not know what he was going to say. In fact, I was privately nonplussed the next day, when I saw in the paper that he'd said that Pons and Fleischmann were suffering from incompetence and delusions. A lot of people took great exception to that.

ASPATURIAN: Really! I did not know that.

BARNES: Oh, yes. This came up when I gave a short talk on the cold fusion work during lunch hour on Alumni [Seminar] Day to the alumni over in Beckman Auditorium.

ASPATURIAN: I heard that. I thought it was excellent.

BARNES: Oh, it was terribly disorganized, because the introducer sort of used up about eight minutes of my valuable time, which was only, sort of, twenty-five minutes total. I guess it was interesting enough, but I didn't feel very happy about it, because I had to abridge it so much and it seemed to me kind of disorganized. But in any case, you might not have noticed, but a lot of people came up afterwards to ask questions, and I knew we had to get this crowd outside, because, of course, Si [Simon] Ramo [Caltech trustee and the day's General Session speaker] was going to talk pretty soon. I went out to them, first to the lobby of Beckman, then outdoors, and, you know, there were still thirty or forty people talking to me an hour later when Si Ramo's talk was over. A couple of them were quite forceful about it. They said, "Don't you think that Caltech was unwise to use such strong language?" I said, "Well, first place, I didn't use such language. It's just not my style. You mustn't think that Caltech used any language whatsoever in this regard. This is an individual matter." I said, "I presume you're referring to Dr. Koonin's remarks." They said, "Yes." I said, "Well, he felt very strongly about it. He felt that he had seen our results, and he'd talked to a lot of other people who had been doing experiments. He'd done some theoretical consideration of his own. On the basis of that, he considered that these people had made some mistakes in their experiment, and that they were certainly so persuaded that this had to be fusion that they weren't thinking straight." And I said, "Those are the words that I would have used. A direct translation of those words that takes less time to say is just that they were 'incompetent and deluded.' That's not my style, to use that language, because I personally felt it wasn't necessary under the circumstances." Then when the DOE [Department of Energy] committee decided to go to the University of Utah, or at least to send a subset of the committee, Pons and Fleischmann refused to talk to them unless three members of the visiting committee as constituted were removed.

ASPATURIAN: I did not know that. Who were the three members they wanted removed?

BARNES: Koonin, [Richard] Garwin, and [Mark] Wrighton.

ASPATURIAN: Garwin is with IBM?

BARNES: IBM.

ASPATURIAN: And Wrighton is with?

BARNES: I've forgotten where Wrighton is. Maybe MIT, but I'm not sure. [In 1989, Wrighton was the head of the Chemistry Department at MIT.—ed.]

ASPATURIAN: And they were removed from the committee?

BARNES: Yes.

ASPATURIAN: Can that be done?

BARNES: Not removed from the committee. They were removed from the subcommittee that went to Utah. There are twenty people on this committee, and only six, or seven at the most, went to Utah.

ASPATURIAN: I have a few more things I'd like to ask you, but we've run out of tape.

CHARLES ANDREW BARNES

SESSION 2

June 26, 1989

Begin Tape 2, Side 1

ASPATURIAN: So, the DOE [Department of Energy] commission was here on Tuesday [June 20, 1989]?

BARNES: Yes.

ASPATURIAN: What did they have to say? Anything of interest?

BARNES: Well, they had to ask a lot of questions about our work. I think they had certainly done their homework ahead of time, and they knew what we had reported at the Santa Fe meeting [May 22-25], which was run by Los Alamos [National Laboratory]. So in many ways they questioned us in some detail about the results we'd presented at Santa Fe, with special attention to the calorimetry, actually, because at the moment they're certainly most strongly focused on the question of whether there are large levels of energy output, which is what was claimed in the Pons–Fleischmann results. Different groups from this twenty-man committee had visited Pons and Fleischmann. The day before they came to visit us, I think, virtually the same group had been to Texas A&M and had talked to essentially three different, slightly connected groups there. After they talked to us, I think they were, if I understood correctly, going to Stanford the next day—

ASPATURIAN: To see Robert Huggins? [Huggins, a professor at Stanford, also attempted to replicate the reported Pons–Fleischmann results.—ed.]

BARNES: To see Huggins and his collaborators. And I think after that, they were going to bring people to Washington to talk to them—for example, delegations from Los Alamos and [Lawrence] Livermore [National Laboratory] and various other places. But we were one of very few groups where they made a visit to the actual site. I suppose in some way I should be flattered, but perhaps not too much, because it was apparently very easy for them to fit in Pasadena on the way from one place to another, and they certainly, of course, wanted to go originally to Utah, to Salt Lake City. There were some problems about that, as I mentioned earlier. What happened was that of the six or seven people they proposed to send there, three of them [Garvin, Koonin, and Wrighton] were rejected immediately by Pons. He refused to talk to them until those three were replaced by others.

ASPATURIAN: So the group was reconstituted for that particular site visit?

BARNES: Yes.

ASPATURIAN: What would you say at this point in time is the general scientific consensus regarding the validity of the Pons–Fleischmann experiments?

BARNES: Well, to the extent that there is a consensus, which is always difficult to be sure of, I would say that perhaps 90 percent of people think that there isn't anything in this Pons–Fleischmann-type experiment at all.

ASPATURIAN: And your view?

BARNES: I would say probably that's also true in my own view; but not because it's a consensus. I have to go with the results of our own experiments. We did really try a great variety of different preparations of the palladium, and we tried a certain number of different electrolytes. We certainly looked very hard with pretty high sensitivity for neutrons and gamma rays, and we didn't see any. We have to make our judgment on the basis of our own measurements.

ASPATURIAN: In your opinion, how do you think Pons and Fleischmann got into this—well, announcing through the press and making the kind of unprecedented visit to Congress to ask for money and so forth? This is very unusual. How do you think this happened?

BARNES: I don't know all of the facts, of course. I suppose that eventually they will come out, one way and another. I think I know most of the story. I think that Pons and Fleischmann had been pursuing this idea, you know, at their own pace, for a long time—they said five years.

They had gradually become convinced that they were seeing excess heat produced in this kind of experiment, but I think they were truly not very sure of it. I think that perhaps what happened was that when they applied for some funding to continue their work, the proposal was sent up to [Steven] Jones as a referee. I guess Jones recognized that there was a certain possibility of conflict of interest, because it appears that he had switched over to trying to use electrolysis as a way to get very large amounts of deuterium into materials. After many years of study of muon-catalyzed fusion, he was still trying, somehow or other, to produce fusion without muons, although it's not exactly clear what his chain of thought was. As with Pons and Fleischmann, he may have thought that somehow or other a large effective mass for electrons, as sometimes occurs in solid-state physics, would help in catalyzing the fusion. Most of us, in fact, *don't* subscribe to that theory, because the concept of effective mass of an electron in a solid is a kind of macroscopic concept. It has to do with the response of many, many lattice sites to the motion of the electron, and this can be described as a large effective mass. But if one is going to catalyze fusion of two deuterons, then the object with the large effective mass has to really be very close in terms of atomic dimensions, a small fraction of atomic dimensions from the nuclei, to do an effective job of catalysis—and most of us think this effective-mass concept doesn't mean anything at such small distances.

However, whatever the reason, Jones had switched over to electrolysis and recognized right away what Pons and Fleischmann were doing. I believe he got in touch with the funding agency and told them there was a problem—a potential problem, anyway—between his acting as a referee on this and the fact that he was also working along similar lines. Now, it's more than likely that the agency would have replied, "OK, we've noted that you might have a potential conflict of interest, but we'd like you to go ahead and referee it anyway." They very often do that, in fact, but they certainly appreciate and expect people to reveal any conflict of interest if there is a chance that there might be.

So again this is somewhat hypothetical on my part, but I imagine that that's what happened and that Jones actually got in touch with Pons and Fleischmann at this time, since he realized they were working along similar lines. Whether they had known this beforehand or not, I don't know. I have no way of knowing. I just have to assume that that was the first occasion on which they got together somehow, and both knew that the other was working along parallel lines. But then what happened, I think—one can conjecture a little more firmly, maybe—is that they started to discuss the question of publishing results, and that Pons and Fleischmann are said to have told Jones that they did not want to publish for another eighteen months, that they were really not ready to publish. That Jones had said, "I'm sorry. I'm in a spot where I have to go ahead, because I sent an abstract in to give a paper at the Baltimore meeting of the American Physical Society, and I have to honor that commitment. There's an abstract that's going to appear in the published list of abstracts for this, so I have to present this." I suppose, at this point, that Pons and Fleischmann got pretty alarmed, because what they perceived was that somehow Jones might get the credit for something they thought was their discovery. I can only suppose that they started talking to Mr. [James J.] Brophy, whose official position was something like vice president for research at the University of Utah.

ASPATURIAN: I think that's correct.

BARNES: And he presumably in turn got in touch with Mr. Peterson, the president of the university. It was a kind of universal feeling that this was a University of Utah discovery; that they had been working on it much longer than anybody else. They were seriously concerned about the question of priority and quite probably influenced a certain amount by what they perceived as the potential importance, from an applied point of view, of this work. I don't personally have any doubt that they encouraged Pons and Fleischmann to go ahead and publish in order to preserve their priority in this business, to establish it by publication. I think, as the statement is made by Jones, that Pons and Fleischmann on the one hand, and Jones on the other, agreed to submit simultaneous letters to *Nature*, on I guess March 24th , in which they would both state what they'd been doing, and these would act as a kind of claim stake—for a while, at any rate. In fact, Jones proceeded to write such a letter, and I guess he submitted it at the agreed time, as far as I know. There was apparently also a letter written by Pons and Fleischmann, and again, I have to assume it was sent about the same time.

ASPATURIAN: They had an agreement to submit jointly to *Nature*?

BARNES: Yes, as far as I know, they probably both submitted as they had planned to *Nature,* and I have no reason to suspect that it wasn't about March 24th or shortly afterwards, as they'd originally planned. What Jones didn't know, and apparently nobody but Pons and Fleischmann knew, was that Pons and Fleischmann had also submitted an article to the *Journal of Electroanalytical Chemistry*. They viewed this as a much fuller publication. Maybe they thought that, with the referee and so on, it would take quite a while for this to be published and in the meantime their letter in *Nature* would come out and eventually this one in the *JEC* would come out. And then, again, on top of this, there was certainly a strong urge toward patenting. I don't know what the dates were, but I suppose that in both universities there were probably attempts to file patents at this point. But on top of all of this, and probably at the urging of Mr. Brophy and Mr. Peterson, or of people working with them, it was decided to have a press conference on March 23rd.

ASPATURIAN: The papers in *Nature* were due to come out when?

BARNES: Well, there was no particular due date on them. They would have to be refereed, and then they would come out whenever they got around to it.

ASPATURIAN: I see. So that was the famous paper that was then sent back from *Nature*.

BARNES: Yes. What happened was, that aside from the press conference and all the things that followed from that, the paper they had submitted to the *Journal of Electroanalytical Chemistry* appeared in record time, without refereeing or anything. The editors of the journal said this had not been refereed and that they were publishing it without refereeing because of its interest.

ASPATURIAN: When was it published?

BARNES: I'd have to look that up, Heidi. I don't know the date exactly. The journal carries it; we can easily track it down. It was certainly after we had received a faxed copy of the article, but it was still pretty early in the game. I'm sure it was less than a month after it was submitted that it was out.[5]

ASPATURIAN: Which is pretty fast.

BARNES: Oh, it's very fast. It could only be done without refereeing it. Anyway, when this article appeared in the *Journal of Electroanalytical Chemistry,* the Pons–Fleischmann paper and the Jones paper submitted to *Nature* were still going through the referee process. Of course the *Nature* referees obviously had quite different comments on the two cases, because the two letters were different. In the case of the Jones paper, they did certainly require and ask for some small modifications or perhaps augmentation of some points, which Jones proceeded to make. And his paper duly came out in about the usual length of time. What happened in the Pons–Fleischmann case was that the referees were still working on it, and apparently had some quite serious reservations, when out came this other article in the *Journal of Electroanalytical Chemistry*, which, though in my opinion hastily written and really very short on details, nevertheless had more information in it than was in the letter they'd submitted to *Nature.* And so naturally the editors of *Nature* said, "Look, in the first place, our referees want extensive revisions of this—they want more documentation, more information—and second, and in particular, we're not going to publish something that is completely outclassed by another publication of your own which has more information out." Pons and Fleischmann simply replied at that point that they were too busy to do the revisions because they were trying to get more results, and they simply withdrew their *Nature* article. In a sense, from the original point of view of staking their claim, that would already have been accomplished by the *Journal of Electroanalytical Chemistry* article.

ASPATURIAN: Given the fact that both of them, especially Fleischmann, had good reputations in their field and they had spent five years, as they said, working on this, how could they have come to make such fundamental mistakes in some of their basic science?

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[5] The Pons–Fleischmann paper, "Electrochemically induced nuclear fusion of deuterium," was accepted by the *Journal of Electroanalytical Chemistry* on March 22, 1989, and published April 10.

BARNES: Well, if they did, indeed, make mistakes—

ASPATURIAN: Yes. I'm operating on that assumption—

BARNES: Assuming—for the purposes of the question, anyway—that this is a blunder, it's not too difficult, because if you think of the things that they claimed in their first publication and then shortly afterwards with Professor [Cheves] Walling, the two original things claimed in that article were first, the large amounts of heat, which they claimed represented large excesses compared to the input energy, and second, that they'd seen neutrons. Then, in the article with Walling was the claim that they'd seen helium-4. Because they recognized early on—at any rate, already in the first version of their paper that I saw—that there was a large discrepancy between the neutron flux they were claiming and the amount of heat they were claiming. And in fact, this amounted to a factor of 10^9, a factor of about a billion discrepancy. And so they immediately said, "Well, it must be that this is some new kind of nuclear fusion process. The nuclear fusion is not proceeding by the reactions that most nuclear physicists would expect to dominate." So that's why they were primed for the next thing, which was the claim of having seen a large amount of helium-4. Now, the helium-4 claim was not only their blunder but also a blunder by Professor Walling. In the first place they had too much helium-4 and didn't realize that. They hadn't calculated— It turns out they had, according to us, 30 to 300 times as much helium as they should have, if it had come from the fusion producing all of the heat they were claiming. So *this* time they had really overproduced helium by a large factor, which is not possible. The other thing was that if they'd produced helium-4 in their palladium rods, the helium-4 should have stayed in the rods. It shouldn't have come out, and what they were claiming was to have seen it in the effluent gases mixed in with the deuterium and oxygen, flowing out of their cell. So that claim failed very quickly. The neutron thing failed because I think Pons and Fleischmann, and presumably also the student who was working with them, Mr. [Marvin] Hawkins, were not familiar with the measurement of neutrons or other kinds of radiation for that matter. They had borrowed some instruments from the [University of Utah] radiation-safety office which they didn't really know how to use. So they made a terrible blunder there, because they didn't know how to use them properly, and they produced a result that was simply, on the face of the published information, easily discernible as being wrong. We saw it was wrong here. I just threw up my hands when I saw it. I said, "This has to be wrong. This is absolutely impossible."

Some people at MIT did an even better job on it than we did. They managed to get a copy of the whole pulsite spectrum, as we talked about before, by calling the TV station in Salt Lake City where they'd seen somebody holding up the whole spectrum. They found something that we hadn't seen—namely, that the peak not only had the wrong shape and the wrong width but it was even at the wrong energy by a large amount. So that died, and in fact, those two things—the neutrons and the helium-4—were essentially withdrawn by Pons and Fleischmann at the time of the electrochemical conference in Los Angeles. Now, in both those cases, I would have to say—well, especially in the neutron case—I think that they blundered there because they didn't know what they were doing. And it was their choice, evidently, to proceed this way rather than to go and find some people in the Physics Department or somewhere else who knew how to measure neutrons and who could have done this correctly. The heat is a much more subtle problem, and that was, of course, the most dramatic claim they made.

ASPATURIAN: Also, as electrochemists, presumably they knew more about the calorimetry—

BARNES: Yes.

ASPATURIAN: —than they would have about the neutron detection.

BARNES: Indeed, that was their field, electrochemistry. At one point, I know that Nate Lewis asked them some questions about the electrochemistry and the difficulties, and instead of getting some help on the subject, all he got was a statement back from Stan Pons that we should all remember that spin was discovered by enthalpy measurements and one shouldn't look down one's nose at enthalpy measurements.

ASPATURIAN: What kind of measurements?

BARNES: Enthalpy, which is heat measurements, essentially.

ASPATURIAN: It doesn't sound like a very useful response.

BARNES: Well, it wasn't a very useful response. [Laughter] He clearly wasn't going to tell us in any detail what he was doing, but he was simply saying, "Well, you better keep at it and try it again, because you must not be doing it right, and you mustn't try to run down the heat measurements, because they're important." But what we have learned ourselves is that there are very many ways that you *can* fool yourself with the heat measurements. In the way it was done here by Nate Lewis and his colleagues, they were able to distinguish between the short-term and long-term temperature changes on the one hand and actual changes in energy on the other. What we observed was that there were many glitches in the temperature. There were lots of ways that this could change slowly and even suddenly. People who were simply in a position only to measure temperature could be badly fooled in trying to calculate the energy. Nate and his colleagues found many other things that were potential errors. I'd be glad to go into them if you wanted, but they would probably better come from Nate directly.

ASPATURIAN: Yes, when he gets along to the point where he does his own oral history.

BARNES: Sure. But it's tricky, OK? So Mr. Pons's statement notwithstanding, electrochemical measurements of this sort—enthalpy measurements—are not trivial, and there are many ways that you can be wrong about it, and we would have to assume, on the basis of our measurements, that they made mistakes in this, even though this was their field.

ASPATURIAN: Should good scientists presumably—I mean, they were considered reasonably sound scientists; I think in the case of Fleischmann, unusually so—have made mistakes like this? Do you think there was an element of wishful thinking involved or some other factors?

BARNES: Well, I think there was certainly an element of wishful thinking. They would have to have been rather men of iron not to have become enthusiastic about this, because they were convinced that they had something here. Maybe they were being a little bit influenced by their own enthusiasm about it. On the other hand, as I have been informed, either correctly or incorrectly, they were really *not* ready to publish at this point, and it's possible that they might eventually, in a longer time, have found out that there was something wrong with these measurements. It's possible still, of course, that they're right and that there is some trick that nobody else knows about except these people. Now, when I say "these people," I really have to say "Pons," because of the following: It is now a fact that after a little over three months of work on this project, a large group of people at the British atomic energy lab [Atomic Energy Research Establishment] at Harwell, England, who have been working directly with Fleischmann—who had been a long-term consultant of theirs anyway—reported after one month that they hadn't seen anything, and now, after three months, they've quit. They've said that there's nothing there.

ASPATURIAN: Are you still following up with experiments here, in an effort to determine if perhaps there is some validity to this?

BARNES: Well, we're certainly still doing experiments. I guess we're doing experiments that are more aimed at trying to see if there is some validity to these low levels of neutrons that were claimed originally by Jones and his collaborators. If you had fusions going on at the rate that was claimed by Jones, and if these fusion reactions proceeded according to the relative intensities—to the relative yields that we imagined they should—there wouldn't really be any point in trying to do heat measurements at all, because you wouldn't see any of them. It would be far below the level of detectability. We're talking about a heat level that is not down by a billion but by 10^{13}—a factor of 10 million times a million from what was claimed by Pons.

There's also another group of actors in this whole scenario at the moment, and that is a set of experiments that I might refer to as dry experiments, as distinct from those done with electrolytic cells. And these were first done by a group of Italians at Frascati, mainly people from the University of Rome. They actually found that by taking titanium and enclosing it in a pressure cylinder and then putting high-pressure deuterium gas in with it and then cooling this down to liquid-nitrogen temperature and letting it warm up, they saw some evidence for neutron emission. Again, people were tentatively very skeptical of this. It was not a terribly advanced detector that they were using. Already, people using a similar detector at Georgia Tech and other places had got spurious results, due to temperature effects and so on. There was a lot of skepticism about it. On the other hand, some of the people in these groups were known to be thoroughly competent people. Then there was a confirmation of a slight variant of it at Genoa. I've heard nothing more from the Genoa people, so maybe they finally withdrew. I have no idea. In the meantime, a collaboration has been set up between Jones and two or three independent groups at Los Alamos and, in particular, one of these collaborations has seen—or claims to have seen—bursts of neutrons, both in this kind of dry experiment—

ASPATURIAN: Similar to that done in Italy?

BARNES: Similar to the Italian thing, yes. They also claim to be seeing bursts of neutrons in a few cases from electrolytic-cell experiments of the Jones type. Though I don't actually think that this is cold fusion, it's still a very interesting result. We're trying to see if we can replicate it. Up to this point, we haven't seen any bursts of neutrons, nor have we seen any continuous low-level neutron emission either, with rather strict upper limits. But, you know, we can do a little bit better yet, and we will continue to try to do this.

ASPATURIAN: You had mentioned in the last interview that you were in contact with Jones off and on. Was he able to give you any information about how Pons and Fleischmann's experimental apparatus worked or what he thought was involved that you were not able to get elsewhere?

BARNES: In the first place, I've been in contact with Jones personally only two or three times, but we have had information indirectly from Jones through mutual friends and so on. So we have had quite a bit of information from Jones, and he has been very free about giving information. We haven't any problems getting information from him, and so far we haven't really encountered any problems from Los Alamos, either. But in neither case have we obtained any information from them about the Pons and Fleischmann work. In fact, I'm not sure that these people know any more about what Pons and Fleischmann have done than anybody else does. I mean, I have no evidence that they know in detail. To be sure, Jones must have found out something about it, if the scenario that I outlined before is correct—that is, if he was actually a reviewer on a proposal of Pons and Fleischmann. On the other hand, if the proposal was as scanty in experimental details as their publication, he wouldn't have learned very much about it, because none of us learned much from their publication, or from the innumerable communications they made to the press in the earliest stages. There was a minimal transfer of information.

ASPATURIAN: Do you have any thoughts on how this has affected scientific research or the attitudes of the scientific community as a whole? Has it had any impact?

BARNES: Oh, I think it's had a variety of effects, depending on individual people. There are some people who are enormously angered—and that is not too strong a word—by the procedure of Pons and Fleischmann and the way they handled this whole thing from the beginning, and in their continuing failure to divulge any information about what they were doing. Other people were also frustrated, perhaps, but decided it didn't matter anyway, because they were going to set up and do their own experiments, which is basically where we were. It didn't matter whether Pons and Fleischmann had it first or not. I mean, they had certainly, by their publication, established a priority for discovery, assuming that the effect was real. We couldn't see any reason why they couldn't then go ahead and divulge results, because the published paper would have protected their patent rights, at least in the United States and in most Western countries—probably not all of them but in most of them. So none of us could really see why they were not divulging information, and in fact people began to wonder and worry whether the thing was non-reproducible and whether they [Pons and Fleischmann] were having difficulty reproducing the results themselves. That still seems to be a real possibility.

Many scientists were worried about the possible bad reaction against scientists on the part of the lay public, because the lay public, first of all, had built up this tremendous expectation that all the world's energy problems were going to be solved by this inexpensive and very clean form of energy. Of course, it was oversold as being very clean. It would have been perhaps cleaner than fission energy, but it would have some of those same problems. It also might even turn out to have been oversold on the grounds of expense, because at this point, even if Pons and Fleischmann were completely correct, we know nothing about how this could be scaled up to a large enough production process. What we do know is something about really fundamental physics, called thermodynamics, and we know that if you're going to have an efficient transfer of energy from heat to some other form of energy, such as electricity or mechanical energy, it's a great advantage to have the heat developed at a very high temperature. Right away, most of us supposed that if this effect were indeed real, it probably wouldn't work at high temperatures anyway, because the high temperatures would drive the deuterium out of the palladium. It was probably also oversold to the public on the fact that it would be cheap. So anyway, we thought there might be a backlash there, and some scientists still think there will be.

ASPATURIAN: In terms of funding cuts?

BARNES: Well, it could eventually show up in innumerable ways, one of them perhaps in funding cuts, although, you know, most of us weren't really very concerned about that. But we're much more concerned about the continuing recruitment of people into science in the decades ahead. We *are* going to have a greater and greater problem in this country, as we have fewer and fewer people willing to make the effort to qualify themselves for a general increase in the technology of our culture and civilization, and especially people willing to pursue science and engineering to achieve this. There is no question that it takes more self-discipline and is probably harder than many other things. We felt that this might work badly and that we'd see even more of a decline in the number of people electing to do these things. Now, I myself don't think so. I think that, for the most part, laypeople understand that the scientists were trying their best to make this work, and if it doesn't work, well, it was bad luck. I think that an enormous number of people were suddenly acquainted with the possibility of fusion, and my experience with young people who are interested in science is that the fact that it didn't work out is not going to discourage them. I can't tell you how many letters I've had, how many essays I've read in the applications of students who want to come here, that say something like, "I know that Einstein was unsuccessful in unifying all the forces of nature. What I would like to do most in my life is to do this, to succeed in it." OK? You see, these young people, these bright people, ambitious people, aren't really turned off by a failure. But they can be excited by the possibility of something they'd never thought of. So I'm optimistic anyway that this won't work to the disadvantage of science and engineering but will actually help out.

ASPATURIAN: So, finally, as we close, where does the whole cold fusion thing stand now?

BARNES: It stands, at the moment, a little bit unsettled, because we haven't heard from Pons. In fact, we haven't even heard any comment recently from Fleischmann, although he had earlier said that it might happen that we will end up with egg on our faces, and that's still a real possibility. But until Pons actually says, "I'm afraid it isn't really working, and there must be some error in it," there's always going to be a lingering doubt that maybe other people haven't figured out how to do it, or that there is something that is a little hard to do, that conditions for it aren't understood, and that maybe it doesn't happen very often for Pons, either. That's always a worry. On the other hand, I think most physicists and most chemists would really, at this point, believe that it was an exciting, exhilarating period but that it was never a real effect; and that it just took a while to realize that nature isn't so easily fooled, that nature has made it difficult to produce fusion, and that you can't fool nature like that.

ASPATURIAN: Anything else?

BARNES: Well, I think it's still interesting for us to pursue this additional low-intensity phenomenon, to see if it exists. As I said earlier, I don't think that it's cold fusion. I consider it's more likely to be a hot fusion—that is to say that under conditions of high deuterium content it may be possible to produce surface cracks in palladium or in titanium, and that sometimes these cracks may be generated with a potential difference across them, which is high enough to accelerate deuterons against deuterons. So that we might be seeing the ordinary fusion process—D + D goes to a neutron plus helium-4—that we can study in the lab. So by making all these cracks in these materials, we may have produced a whole bunch of miniature accelerators sourcing hot fusion. Now, you might say, Well, what good is it? Is it going to be a power source? At this point, I don't think anybody visualizes any way that this could ever be a useful power source. But it might, for example, turn out to be an interesting technique for investigating strength of materials or structure of materials or metallurgy, as another method of finding out under what conditions these things crack and what you can do to enhance or prevent cracking. If it turns out that this is really what's happening, we're certainly going to have a shot at trying to duplicate it. In fact, we have some really bizarre ways of trying to duplicate it that I'm not going to talk about. But we'll probably not pursue it long-term.

ASPATURIAN: You said you think the effect might be one of hot fusion rather than cold if the process you envision is taking place. Do you have a paper being planned to elaborate on that at all?

BARNES: We haven't prepared a paper on this, because, in fact, this time we really haven't seen any neutrons in either the electrolytic or the so-called dry-type experiments. However, we have some tricks that seem really worthwhile to try, and we're going to try them. You can be sure that if we see something, we will publish it, and promptly.

We're having—I wouldn't say difficulties, with our other paper. Our full paper on our work on palladium was submitted to *Nature* on May 22nd, I guess. We just heard a few days ago from the editor that they'd sent it to two different referees and one of them was, I think, a little bit tardy. But when the editor queried him, he said, "I think the paper's fine. It should be published *exactly* as it is. No changes." However, the other referee had a long list of pages of objections, comments, and changes. We have tried to meet these comments where we felt that they actually were justified and would improve the paper. On the other hand, this referee is clearly one of the very small handful of people who still believe that what he refers to as the "Pons–Fleischmann effect" is real. And he was trying his best to defend this effect, and of the things that were most clear-cut in our paper, he was saying, "Well, that's conventional now. It's now been said at meetings," and so on. Of course, the longer he can hold up publication of our paper, the more of this information is going to be said at meetings.

At the same time, we have decided that some of his comments are just wrong, and we have resubmitted the paper with some small revisions, and with statements regarding some of the things this referee has said, indicating that we are convinced that the referee is wrong and we would prefer *not* to make these changes. We're waiting now to see what the editor is going to do about the paper. It's gone back. But in the meantime, the paper has now been in the hands of *Nature* for well over a month, and with every week that goes by there's more information coming out, and so our paper is gradually becoming less and less topical. On the other hand, in the way science should be done and usually is done, a paper that is based on sound work—and it clearly will carry the original submission date—will and should be published. The only real question is whether the editors will now accept it and publish it.[6]

[6] The paper, "Searches for low-temperature nuclear fusion of deuterium in palladium," by N. S. Lewis *et al.*, appeared in *Nature,* vol. 340, 17 August 1989, pp. 525-30—ed.