

Oral History Transcript — Dr. Michael Goldhaber

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Interview with Dr. Michael Goldhaber
By Patrick Catt
In Berkeley, CA
July 20, 1995

Transcript

Catt:

It is July 20, 1995, we're in Berkeley, California, and I would like to start off by asking if it is okay if we tape record?

Goldhaber:

Sure.

Catt:

Okay. The second thing is any quotations that I intend to use, at least when it comes time for the defense...well, in the writing of my dissertation, I will clear them with you to check for inaccuracies...

Goldhaber:

And I assume before any publications as well?

Catt:

Yes. And also if you'd like full transcripts then I will send those to you at your convenience, of course. Now then, the first question deals with your biographical information and with your immediate family's involvement in political activism. You can go back as far as you want.

Goldhaber:

Well, basically I think the answer is no. My parents were refugees from the Nazis and basically came of age when the Nazis were coming of power, they were very scared off for political action and positions. So the only thing at all like that that was my mother had some position in the Jewish Council which was established by the authorities in Munich. It was how they would talk to the Jewish community and she was the youth representative and told me that she was very disappointed that no more resistance to the Nazis was propagated by the other members of the Council, and that she tried to do that. That was about it really.

Catt:

Do you know approximately when your parents came to the United States?

Goldhaber:

Yes, my father came in 1938 and my mother in 1939, after they were married. However I should say that one of my father's best friends was Leo Szilard who, as you probably know, had a lot of involvement before coming to the U.S., and he certainly influenced me to a certain extent.

Catt:

In what ways?

Goldhaber:

Well, I was very impressed with his taking an interest in political things. For some reason that greatly impressed me. He wrote a novel, *The Voice of the Dolphins*, about how politics was done and how scientists could be involved in changing the world. I found that very interesting. And I was very interested in his anti-bomb position and also, I think, in other friends of my parents, especially while they were in Urbana; Eugene Rabinowitch, who was the founder of the *Bulletin of the Atomic Scientists*. So I knew about his work too and I knew about all the anti-bomb work, and although my parents had very limited involvement in that...I mean they subscribed to the *Bulletin*, but that was about it. I was quite impressed with that. And I remember my father was a member of the National Academy of Sciences when I was about 15, and every member got sent a copy of Linus Pauling's book *No More War* and I read that with great interest, and that attitude I found very exciting. So for some reason those things affected me greatly and I found myself pulled in that direction very early.

Catt:

Your father or your mother, did they ever ally themselves with one of either the Democratic or the Republican parties, or one outside of then- "mainstream American politics?" In other words, were they ever a member of any socialist or communist organization?

Goldhaber:

No, my father's position has always been that he wouldn't register for the primaries because he didn't want to enlist any party affiliation. [Comment deleted] But on the record they were very non-specific about their relations. They never supported any radical or left-wing, or right-wing position. I remember, however, at the time of the Rosenberg Trials, my grandfather told me that they were being punished because they were Jews. But my grandparents also were not socialists or communists or anything of a sort. And they had sort of vaguely liberal positions to the extent they articulated them.

Catt:

Now you mentioned that you were in Champaign, Illinois. Where did your parents first settle when they came to the US?

Goldhaber:

They settled in Urbana. I lived there until I was about 8. And then they moved to Brookhaven on Long Island. And we... it was an interesting thing that the Atomic Energy Commission ran Brookhaven Laboratory and somehow made the decision that they would not build residences there. And so the idea was that the people, the scientists at the laboratory, were to mingle with the rural community. So we found ourselves, and all the others, just had to find housing in the various towns some distance away, so we settled in the town of Bayport. My parents still live there as a matter of fact. It was a small, basically white, homogeneous, mostly Republican community. Even in my earliest days there, I remember I was always interested in politics. When I was about 6, well, actually even younger than that, I was about 4, Harry Truman announced "Meatless Tuesdays." Yes, this was in 1946. I always remember this and it galled me because it seemed reasonable to me that he'd tell people not to eat meat one day a week, but why did it have to be Tuesday? And so I became a Republican on the basis of that very libertarian position, I'd say.

Catt:

I didn't know that.

Goldhaber:

It's probably one of the facts of history that's mostly forgotten. So I stayed a Republican in my own mind, [Comment deleted]. When I was fourteen [14], I was suddenly inspired by a speech by Adlai Stevenson and so I switched over and became a Democrat, debated Democratic positions and so on and so forth. That was my early politics pretty much.

Catt:

How about your interest in science?

Goldhaber:

Well, since both my parents were physicists, it just came with the territory, so to speak, and my uncle and aunt were also physicists a little later. They came to this country later and they were quite a bit younger than my parents. My brother, who is 2 years older than I, majored in physics at Harvard where I went later. I too, after thinking of being rebellious as far as going into biology maybe, ended up settling on physics, in part because it's just what everybody did in my family.

Catt:

And, of course, this was around the time Szilard said, “well, physicists can solve the problems of biology.”

Goldhaber:

Right. Yes. I didn't like the idea of blood and I didn't want to deal with killing animals so that was one of the reasons I turned away from biology at an early age. I actually did some experiments with bats when I was a freshman at Harvard. I had an early interest in animal behavior but I gave that up, however.

Catt:

How did you react to the McCarthyism of the 1950s?

Goldhaber:

Well, that was something again my parents were very much against, and I became against it. I remember once we went to Washington so my parents could attend the convention of the American Physical Society and we were sitting at a restaurant and Joe McCarthy was sitting at a nearby table with Jacob Javits, who at that time was a congressman known to my father, and I remembered glaring at McCarthy. I really had a distaste for him that I didn't want to hide. And later on, Javits came over to the table and acted like he didn't want to make too much of the fact that he was sitting at the table with McCarthy because he guessed we might be constituents and clearly were unhappy. So definitely a negative reaction to that whole episode.

Catt:

So, going up to at least through graduate school, as you're getting more and more into science, were you still able to keep up with the political issues that were going on?

Goldhaber:

Oh, I was very interested in what was going on in the world and never really lost sight of that. And when I was in graduate school of course the Vietnam War would begin, and when I started graduate school in 1963, Kennedy was assassinated and so forth. In addition to that, I pretty much already had chosen my dissertation adviser who was Sidney Drell, and Drell became one of Kennedy's and then Johnson's science advisers. So I felt that direct connection with what was going on in Washington. And another thing that may be of interest is that a number of other physics graduate students and I held a little weekly lunch which we invited faculty members to come and talk and to talk not about science but about other aspects of their lives. I was at Stanford and one of the people who came was Leonard Schiff who at that time was...

Catt:

That's Schiff?

Goldhaber:

Yes, well known for his book on quantum mechanics. But at that time he was chief science advisor for the Air Force. He also had a large Air Force contract, sponsoring primarily what was then called the Institute of Theoretical Physics, or for Theoretical Physics, and we were all somewhat skeptical of him. By this time a lot of the graduate students had a kind of antiwar or at least an anti-military position.

Catt:

This is in 1963?

Goldhaber:

Well, you're right, this is in 1965. But we asked Schiff why did the Air Force support science? And his answer always struck me as incredibly revealing, why in fact does it support theoretical physics. And now that I think about it, I should say that as far as my choice of field, particle theory, I was very aware about the bomb and I really wanted to look for a field that I felt would have no applications at the time to be completely pure. This was a decision, incidentally, I made while an undergraduate.

Catt:

This, your decision to go into "pure" research, it comes a little bit later, all of this?

Goldhaber:

Yes. In 1966. Well, I'll tell you more about that. Actually it comes earlier, in 1962. I was very interested in the purity of science, and I was very concerned with why is the Air Force supporting this if it has no applications? Schiff came up with a whole list of reasons that I believe are true for why the Air Force did it. Namely that they felt that they needed a lot of minds so they could have someone on tap when they needed them. They needed a lot of trained people. Also they felt a lot of people would be trained in theoretical physics and get good training, but then not be able to get jobs. They might be willing to take jobs working for the Air Force, whereas if they had looked for people who had the direct training, they might have gotten less smart people or less motivated people. And basically also that somehow the pure science would back up other science that would be valuable to them. Schiff had a very organized mind and I think that his answers were based on reflection and a clear understanding of what was going on. And I found it utterly remarkable and incredibly disillusioning.

Catt:

With what Sidney Drell was doing, did you ever question where the funds were coming from to do your research in graduate school? Or was it just something that wasn't discussed?

Goldhaber:

I did, definitely, but let me talk about this and then I'll talk about that okay? Drell worked at the Stanford Linear Accelerator Center (SLAC), which was 100 percent funded—as was Brookhaven National Laboratory—by the Atomic Energy Commission. Now, I always have had bad asthma and when I was just out of Harvard, my parents really pushed me very hard to spend the summer at Los Alamos where we had gone several times supposedly partly because of my asthma.

Catt:

Sounds just like Oppenheimer.

Goldhaber:

Yes. Actually, I didn't know Oppenheimer had had health problems, until recently, and that was why he went to New Mexico. So I very reluctantly agreed to do this but because I was very

involved in sort of anti-bomb protests at Harvard or at least on the periphery of them to have a real distaste for bomb building. And I went to Los Alamos very suspiciously and then had concern, for example, that I would have to get a security clearance. I was also concerned to make sure I never learned any secrets. My primary concern was that then I would constantly have to censor myself, and I didn't want that inhibition. I tried very hard not to, but people did thrust things in my face that were stamped "secret" and so forth. And the person I was working with in Los Alamos was Stanislaw Ulam. He considered himself, certainly, the co-inventor of the hydrogen bomb. I had known him for a long time, and I really liked him. But at the same time I was very disturbed about the bomb work.

He himself did not continue to work on the bomb but he was kept there on the basis that one day he might do something that would be of interest. He had complete freedom to do what he wanted. I remember at that time the neutron bomb was being discussed. And he looked upon it with disdain as a method of "killing the enemy and stealing their watches," as he put it. He just was totally uninterested in working on it. But one day he insisted...I told him I didn't want to know any secrets, so he insisted on getting his...he actually had to get his secretary to open his safe because he could never remember the combination, and he pulled out the paper that he "co-authored" with Teller on how to make a hydrogen bomb and he thrust it in my face. But all this made me very sad, I mean about the connection. I was very worried about the use of the bomb and the implications of the bomb. And I was there for two summers and this paper comes out of my impressions of what went on there very much based on that time, on what I saw.

Catt:

And for the record, this paper is entitled "Seduction at Los Alamos?"

Goldhaber:

Yes. I was in Los Alamos during the summers of 1963 and 1964. I would guess I wrote the "Seduction" paper in 1969. I still completely think this is true and that it also went along with what Schiff said because I knew quite a number of fellow graduate students of mine who ended up at places like Los Alamos and originally were doing theoretical, or if you will, "pure" physics, but then they were told at a certain point you can stay only if you start working part-time on weapons work. So it was a whole system of seduction. It was very well developed by that time.

Catt:

So you would agree that with this relationship, between the government in part being a large supporter of pure scientific research, scientists were almost working against their will, because in fact, that was the system of support and that it was almost a subterfuge?

Goldhaber:

Right, that's right. I felt it was almost a subterfuge and that people were unwilling to look at that. While I was at Stanford certainly, Sid Drell would fly once a month to Washington to be on the advisory committee panel and he was—it turned out later he never really told me this himself but I found out afterwards—that he was head of the panel of the advisory committee on the use of technology in Vietnam. And he was very interested in so-called electronic warfare at one time, which I found morally very objectionable at the time certainly, and I found this whole involvement very pernicious and very self-serving. So I was increasing aware that the

government was paying for this, and that this was a problem. Well I may come back to that, there are some points I'm forgetting right now. But the whole war increasingly bothered me as far as science went, and I think at that time it was also, to some extent, casting doubts on why I wanted to be a physicist at all. And partly it was connected with the war and in the sense that you couldn't avoid complicity in doing that. So one reason I went to Rockefeller University afterwards was that at this time I had what seems to me now rather a naive idea...

Catt:

In what year did you go there?

Goldhaber:

That was 1968. I was starting my postdoc there. Right, so one reason I went to Rockefeller University was that this was a place that had gotten its money from the Rockefellers in 1902, or something like that, and that it was now basically removed from the world and so forth and it didn't take government funds. After I had been there for about 4 months, I was involved in some anti-war protests I helped establish, my boss, Abraham Price, told me that I was being paid by the Air Force which nobody had even bothered to mention to me. So I was pretty shocked by this. I was very upset. And when I...well, let me say some other things...a whole series of demonstrations and protests and so forth were going on all during this time, I would come to San Francisco and march against the war and so on and so forth. And obviously physicists were doing a bunch of different things against the war. I knew Charlie [Schwartz] at that time.

Catt:

How did you meet him? Do you recall when you first met him?

Goldhaber:

Well, I think I met him through my brother [Fred], but possibly in some other way, but my brother had been a postdoc here in Berkeley from 1964 to 1967, and Charlie was friendly to him. And through that I met him and...and I don't remember exactly what it was, but I remember Charlie asking me to pass around some petitions among physicists at SLAC. I think that it might actually have concerned the issue of whether... No, that doesn't seem right. I was thinking it might have had to do with the question of whether to hold the meeting of the [American] Physical Society in Chicago. I think that came later. I think it had to do with...

Catt:

Sentiments on the war, or should the APS oppose the war?

Goldhaber:

The war, yes. Right. Should the APS oppose the war and what Charlie said about that. Yes, it was in early 1968. I don't know if you have the copy of the thing I wrote about scientists as war criminals but that came a little later too. But I did pass some material around. I also did my own independent thing while I was trying to finish my dissertation. Robert Kennedy was shot and then Martin Luther King, Jr. was shot, and I remember being very upset because I thought that SLAC should close down and have a memorial for Martin Luther King, and instead, T. D. Lee insisted on giving a seminar, it was scheduled for that date I guess, and he more or less said that science should stay out of politics and... Oh, that was the issue. The question that was

debated was should scientists be “on tap or on top?” And of course Szilard had the “on top” position and I completely agreed with that. That in effect scientists couldn’t trust others to make the decisions for them because they wouldn’t really understand things necessarily. And the idea that scientists should be essentially servants to the power elite, which seemed to be the position of Drell and people like that, I found very unacceptable. And so I remember that the day was really charged. In addition I should say that although I might have been one of the few who became really active, virtually all the young theoretical physicists who I knew well, or at SLAC at least, were strongly against the war and opposed with various degree of ridicule or hostility to what Drell was doing.

Catt:

Speaking of “young Stanford physicists,” there’s two names I have here...

Goldhaber:

Leo Stodolsky would be one. [Comment added on proofed transcription: “I am not quite sure why I gave his name. I don’t recall his being particularly vociferous in anti-war positions, etc., nor his being at Stanford. He did feel misused by Drell who, he thought, had plagiarized his physics ideas.”) I think most of the other people’s opposition was more informal. They just had an opposition to it. I remember one person who was pretty strongly against it when I think about physics around that time. He actually had been in the Army at one time himself, his name was Albert Finn. And we wrote, or co-wrote, a paper together. If you ever find him, I would be very interested. I lost track of Al Finn in about 1970. So I was by no means the only person at that time and at this location who was concerned. For example, many of the graduate students at Berkeley were involved in the various sit-ins that occurred. And there was, before I left Stanford... oh, let me say this in terms of what I particularly did, perhaps aside from a little bit of politicking in high school, the most serious thing I did politically next came at Stanford when I felt that the response of most students to the assassination of Martin Luther King was insufficient and it was in the community known as East Palo Alto it was unincorporated and was mostly black and it was very poor, and they didn’t even have the money to incorporate. Meanwhile, Stanford was spending money on building four-lane highways and basketball gyms and fraternities and all kinds of things that seemed highly questionable to me as necessities. And they even had regulated federal housing money to do all this.

So I organized something called the League for Stanford Sacrifice. Basically I came up with the position, which I wrote, that said Stanford should take some of its vast endowments and give it to East Palo Alto. And basically my idea was that whites should work against racism in the white community much more than the other idea which is we’re all going to teach black people, tutor black people, and that would be a good thing and that’s what we should do, which felt to me like missionary work that I didn’t like the idea of, and basically very patronizing too. So I got a few people to do that and ended up sending letters to all of the faculty members and got very, very few responses. Basically the whole thing died very quickly, but that was sort of my major introduction into politics that have nothing to do with science.

Catt:

How was this effort received by the staff or faculty at SLAC?

Goldhaber:

Well I don't remember to what extent they knew of it. If they did know of it they never said very much that I can recall. They did know by this point that I was fairly radical and they didn't really pay that much attention to it as far as I can recall.

Catt:

Okay. Did you vote or participate in the 1964 and 1968 elections?

Goldhaber:

Yes, actually in 1964 and 1966, I did precinct work for Johnson against Goldwater. After that, I was very convinced that Johnson was the right person. And at that time, I recall, my adviser and I were in accord. I had, and I proudly wore my Scientists and Engineers for Johnson and Humphrey button. And I had had this old...I mean my roommate in college's father had been a good friend of Hubert Humphrey, so I had this old sort of respect for Humphrey but I very much admired him at the time. So compared with some other people I certainly wasn't terribly radical. In the 1968 election, I joined the Peace and Freedom Party, but I also remember voting for Eugene McCarthy, so I'm not exactly sure what happened at the time there. By the time of the general election I was in New York, and I actually can't remember how I voted or whether I held my nose at that point and voted for Humphrey or whether I voted for Benjamin Spock or something like that. I'm not really sure.

Catt:

But definitely not Nixon?

Goldhaber:

Definitely not Nixon. No. And so about that time I moved to New York and my interest in politics had grown. Shortly thereafter, I came back here [Berkeley] for a visit and that was the time that Charlie got me interested in signing on the SESPA declaration, which he had pretty much written, although I think I came up with the name. It wasn't a very good name I must say. And I revised what Charlie had written. We, and to some extent my understanding has been that Marc Ross and, to a lesser extent, Marty Perl were kind of passive co-authors of this thing, but we certainly did actively promulgate it. And so I became something bizarre like national secretary, or something or other, I can't remember. I think the first meeting might have been in New York actually; the [American] Physical Society was meeting there in early 1969. And so I founded a SESPA chapter at Rockefeller University which was the most frustrating thing that I've ever done because every week new people would come to the meetings and I felt it should be run very democratically, but the new people always had different ideas as to what we should do. And, therefore, my democratic principles were entirely in conflict with anything ever getting done, I eventually concluded. It was really quite a ridiculous exercise in some respects but we learned something. Meanwhile, because I was becoming much more political, I got involved in politics at Rockefeller University. I found the most radical people there, who soon disabused me that this was some place remote from the world, at least as they saw it. They were, some of them at least, fairly rigorous Marxists and even members of the Communist Party.

Catt:

Did they have the loyalty oath in place at Rockefeller at this time?

Goldhaber:

At Rockefeller University?

Catt:

Yes.

Goldhaber:

I don't recall anything remotely resembling a loyalty oath, no. But the thing was, as became apparent a few weeks after I arrived, was that the Rockefeller family sort of ran the place and certainly the administration. And a lot of people there had done a lot of studies and had shown the connections between different Rockefeller holdings, and those I think that they controlled indirectly, and showed there were an interlocking corporate directorships that encompassed the directorships of the Rockefeller and so forth. And Frederick Seitz, whom I've actually known since I was a child, turned out to have a lot of Rockefeller connections and other right-wing connections. He became president of the university at that time. And students also had just worked-on the basic class dynamics of the place, had dug up things like there were 3 different kinds of toilet paper used depending on the status of the people who would be using the toilet. It was quite absurd, I mean, the level that it went too. Some of this struck me as more amusing than anything else and I didn't get as involved in the class issue as in other things but because, at that place, postdocs were considered to be faculty. So I found myself in an interesting position. I ran for various offices. I became a member of a brand new faculty executive committee that was attempting to take power away from the President, for there had never been any faculty senate or anything like that there.

Catt:

Was your main responsibility at this time research or teaching? Did you have to teach?

Goldhaber:

No, there was no teaching. I mean there were only a few graduate students, really, and aside from an occasional friendly comment there wasn't much to teach or anyone to teach. The place only had graduate students, was run on a...the graduate students actually were also considered almost the equivalent of faculty, they were definitely a part of the elite of the institution. They were all given full scholarships and so forth. It was a very odd place in certain respects. It was, in some ways, an ideal ivory tower but then it had these odd connections; they mostly had to do with the investments and how they were handled and things of that sort. But what eventually became of interest to me was that David Rockefeller, who was Chairman of the Board, was also one of the most influential people in the country, and was backing the war. And I actually was very instrumental in getting the faculty to strike during the Cambodia invasion in 1970 when there was going to be a board meeting.

The students actually didn't go on strike but the faculty did. I secured that, my one great political triumph. As far as SESPAs work, of course, a lot of that took place at various [American] Physical Society meetings, or in support of protest at AAAS meetings, as far as I was concerned. And I recall various visits to Washington, for instance, to oppose the anti-...I mean at the time, I would say the annual Washington meeting of the [American] Physical Society to oppose ABMs, for example. I did a lot of political work in connection with that. In my second year, in attempting to run what had sort of become New York SESPAs, or SSPAs as it

was then called, I ran into Rod Wallace who had come—he had been one of Charlie’s [Schwartz] students—and he had come to Columbia. He and I didn’t exactly share our views on how the world worked, but it was close enough so that we could work together. Basically I ceded to him the running of the organization and I became more of an idea person. And the idea I came up with was that we should find somewhere we could picket on a weekly basis. And the place we found was called the Riverside Research Institute just outside Columbia University. It was previously known as the Columbia Electronics Lab. And we enlisted the support of Seymour Melman, I don’t know if that’s a name you’ve heard of previously. He’s actually an industrial engineer.

At the time, he was a professor of engineering at Columbia. He had been a long-time leftist who’s now probably 80 or so. [Comment added on proofed transcription: “He is the author of many books on how militarism is, in his opinion, economically distorting, e.g., “Pentagon Capitalism.” While I oppose militarism as fully, I disagree with his analysis.] But he still writes pieces about the need for disarmament, conversion and things like that. I would write little pamphlets or leaflets to hand out to the people who worked at the Riverside Research Institute. I would dictate them over the telephone to him, and his secretary would type them up, and when they were ready, we would go there. Joe Shapiro was also involved in this work. So, we would hand the stuff out on, as I remember, often freezing cold mornings, and listen to all these electronic engineers who were going into this place. We didn’t originally know what they were doing. But they somehow revealed to us the secret that they were working on, what eventually became Star Wars basically. They were working on laser anti-missile devices. So we tried to get them to think of their consciences, and through Melman we even tried to get them into other jobs; jobs not involved with this type of research. In the end, I don’t think anyone took the bait, but at least it gave us a chance to surface.

Catt:

And roughly what period was this?

Goldhaber:

It was from the fall of 1969 to mid 1970. Of course there were a lot of other anti-war things going on at that time that I was involved in. And by the end of my time at Rockefeller, I spent full-time essentially doing political stuff. By that time Rockefeller had a sort of strike going on, I wrote the strike newsletter there, that came out three times a week, and I just got very involved in politics as full-time as I could at this level.

Catt:

You’ve anticipated my next question. How about your science at this time?

Goldhaber:

My science was sort of dissipating pretty much at this time. I think I had begun to, perhaps even earlier, to realize maybe I didn’t really want to do science my whole life. But I was still sort of doing it. I tried actually to stay at Rockefeller to do something much more connected with my politics. Maybe I could...I forget exactly what it was, but it had to do with my ideas about how technology, on it’s good side, was permitting great automation that would prevent people to live in a essentially a world where there was no real need for money as I saw it. And, you know, a world of abundance that people can share things and I was interested in figuring out how to make that a reality. I was very unlearned and naive about what other people did. I

wrote a position paper to that effect. My actual...the rest of my science, I did some. I didn't do much, but it seemed less and less interesting, and I just sort of began to feel also maybe connected to that, maybe not, but understanding quarks and then getting almost beyond quarks to some basic, fundamental unified field theory or something like that, was going down to something so simple it seemed to have no interest whatsoever. It just had these few fundamental principles and nothing else. And thing was, it occurred to me, that I had seen the mind of God and God is an idiot. It made me feel very discouraged about proceeding in physics and I realized I was much more, interested in the social world and its incredible complexities. So I more and more moved in that direction.

Catt:

Did you ever have an impulse that maybe you could do, like Szilard had done, take the expertise or the methodology of being a physicist, and use it in biology, or maybe even in psychology, or in sociology, or in some other academic field?

Goldhaber:

Well, I mean I don't know whether it went quite that way.

Catt:

Did you ever entertain thoughts of doing that, let's put it that way?

Goldhaber:

I have only one mind. I mean I did have some interest in things biological, and at one time I put some energy into working on the question of what causes cells to differentiate. But it was a theoretical thing that I was doing. Yes, I think indirectly I would say yes and I still do today... and that's all I do practically. But in terms that vary almost from day to day, in the sense of how much I feel I want to be involved politically in any way...I guess apart from my disillusionment with science. Well this sort of takes the history forward but I don't know how long you want to go on. (tape recorder turned off) I'm now talking about when I was at the University of Arizona, from the period of 1970 to 1973, and even though I'd already had ambitions to leave physics, I didn't look very hard for any other kind of position. I ended up being offered an assistant professorship there and that seemed like pretty nice thing to do since there weren't very many available and it was a full-time position in my field. So I grabbed it, and moved to another dry climate, to Tucson. It was obviously a strange place to try to be political although Tucson is relatively the more liberal part of Arizona.

Catt:

Goldwater was from Arizona, correct?

Goldhaber:

Goldwater was from Arizona, that's right. And most of the state is Republican but there was a long tradition of Democrats representing Tucson, not necessarily. Well, for instance, I think Morris Udall was the congressman from Tucson then and was so for a very long time. I had become a member of the Council of the Federation of American Scientists, and I went to those meetings which my former adviser Sid Drell, who was another Council member, which was rather strange because at this time I felt he was a sort of war criminal himself. That's the trouble with that. Anyway, I was involved in the chapter of the Federation, although I don't

think I ever actually joined it, I was just on the Council of it. There were quite a variety of different political things which did happen in that strange climate, but most relevant, perhaps, was when I organized the boycott of Los Alamos, or I attempted to organize the boycott of Los Alamos, which meant that people should not attend talks by speakers from Los Alamos. And my feeling was that this is the one way in which scientists can influence the desirability of being in that place because it had a lot of money and had a nice climate, etc. And that was probably when I originally wrote the piece about seduction of scientists. I wasn't really terribly happy to be doing this, but I felt I sort of had no choice.

I began to be thinking of myself as kind of, I don't know, an outsider, by standing outside the various physics colloquium and holding up big signs and writing and handing-out leaflets that I put into all the faculty's and graduate students' boxes the night before any speaker from Los Alamos or Livermore was there. And I remember also at one time, I was on a Ph.D, I guess it would be a qualifying orals, for somebody in electrical engineering, and we asked him what his project was about and he said very proudly it has to do with the hydrogen bomb. And he didn't say too much about it; it was classified obviously. But I simply asked him, I couldn't resist asking him, had he thought about the moral implications of it? And the other people on the committee were aghast that I had done this. So I really had the strong sense that it was this incredible fight against a scientific establishment that didn't want any moral questions or moral issues to be raised in any form. And that at the same time it was probably willing to take money, no matter how tainted it was, from the government or from wherever. So I found it increasingly difficult from that point of view to do science or to feel good about what I was doing. I also had another qualm, which came more directly from the kind of politics I'd been exposed to at Rockefeller, which was essentially I could justify my work on the grounds that it had no applications.

Catt:

And what was the nature of your work?

Goldhaber:

I was working on gluons, or trying to work on gluons. I didn't really have anyone else to work with there, but I was trying to write some equations talking about gluon binding. I don't think I was terribly successful. But anyway, the main thing was I was just also becoming very discouraged about why I should be given this privileged position of essentially being allowed to play games, or that I was doing something that I could justify to myself as long as it had no applications. Meanwhile, I became more aware of the injustice in society and of poverty. For instance, at that time I had a nice rented house and I had a gardener, and he was an Indian [Native American], and one time he didn't come and it turned out it was because his daughter was pregnant and he had to drive her 60 miles on dirt roads to the Indian hospital on the reservation because that was the only way, the only place where she could get care. And it just struck me, here this guy works very, very hard and he has this very hard life, and I am supposed to be playing doing what interests me and I'm doing it under the assumption that it will never have any implications. And I found that, understandably, very difficult. I also did not get tenure at the University of Arizona partly for my political involvement and probably also because of my lack of publication at the time. So I moved to Berkeley. By that time I had gotten interested in a number of things but particularly psychotherapies and things like that. Also while I was in Tucson, I helped found something called the Free University of Tucson which supposedly would allow wide-ranging thoughts on all sorts of subjects, a typical "free university" of the

time.

Catt:

Was that something analogous to the New University Conference [NUC] project that was going on in Chicago and elsewhere?

Goldhaber:

No, I don't think so. It was more like, I don't really know what that was like, but actually while I had been at Stanford there was something called the "Mid-Peninsula Free University" that was going on in the '60s. And basically it was a combination of radical, far out and new wave sort of thought. I mean all kinds of subjects might be allowed there. I remember there was one course given by someone named Larry Tessler who is, now at least as far as I know, still a top official at Apple, and it was on people heaps. And it was about what it felt like for people to sort of fall into a heap together. It was sort of a very wild, free-wheeling period and the Free University of Tucson would have courses on sexuality and courses on...and I can't remember too many of the others, but things that wouldn't be taught normally in a university, and I had some ambitions in the direction of starting a great sort of academy that would take this idea and just extend it in really extremely wide-ranging courses that were really meaningful intellectually and so forth. As it was, the offerings were very piddly, intellectually. When I came back to this area I got involved in something called the...I was looking for something having to do with psychotherapy, and I started being involved in the training program in the Berkeley Radical Psychiatry Organization, I guess it was called, which existed in the... started I guess in the late '60s, it continued in one form or another through about the mid-1970s. And it had a lot of Maoist elements and it was constantly reducing psychology. I mean I liked it because it was...I had actually been interested in possibly taking courses at Esalen if that were possible. And I looked at the Esalen brochure and they mentioned their various...

Catt:

And just for clarification, Esalen is...

Goldhaber:

Esalen is...the Esalen Institute is located in Big Sur, and it's still a place that does unusual forms of psychotherapy and other forms of therapy in various kinds. They were a very "hippie" kind of organization, I thought. But then I received their brochure and among the places they listed, that they had consulted with, were the U.S. Army and Lockheed. And so I felt less good about them. I wanted something really pure so I got involved with this Berkeley Radical Psychiatry Organization, I believe it was called. Anyway, that it certainly wasn't consulting with the Army at least as far as I knew. The problem was they were very concerned with various Maoist notions like demystification. For instance, they took what was an already simple theory, a very highly simple theory in psychology, and created a new one that's very much an offshoot of Freud, created by somebody named Eric Burne. You know Freud had the id, the ego, and the super ego, and Burne's had the child, the parent, and the adult or something like that, and they (i. e., BRPO) added a different character—the pig as they put it—that represented internalized oppression that was the oppression that came from certain social ideologies and standards and so forth.

And it began to reduce all of psychology to the pig, through internalized oppression. So even

for me it seemed too simple. And also what I became aware of was they, the BRPO, had a principle that meant that everybody was equal in this organization. And I discovered that meant that everyone was unequal, and that hiding under this equality was tremendous inequality of power that was never articulated because it couldn't be articulated because supposedly everyone was always equal. Actually it was quite evil and when Jonestown happened, it reminded me of Jonestown, and it reminded other people of it too. There was really a certain horrible sickness that developed in the organization that spewed this very subtle kind of doublespeak. And so I really was significantly disillusioned by that sort of experience. Meanwhile, I got involved in a different organization called the New American Movement which I don't know whether you've ever heard of it, it was never very big, it eventually emerged into what's it called now the NSA, I think it's called...no...or is it called the DSA...yes, the DSA, excuse me, the Democratic Socialists of America, which is combined with an organization—Democratic Socialist Organizing Committee, or DSOC—started by Michael Harrington. I don't know if you've ever heard of him but anyway, it's the official representative in the U.S. of the Socialist International. And NAW was a merger of former communists, former anti-communists, a whole bunch of different people, who had very democratic principles, and was against racism, sexism, homophobia, for the environment, somewhat green in its outlook, and basically committed most of its time to working within the Democratic Party in effect. And we ran a school here in Oakland, right near the Berkeley border, called the East Bay Socialist School. I got very involved in that. I held a lot of classes there, took a lot of classes and so forth.

I got very interested in Marxism and I began to write a book on science and capitalism. I was still somewhat involved with the science-politics issue but less so. I began to move away from that...I mean we had some meetings with Charlie [Schwartz] and so forth. I somewhat got involved in questions of Jewishness and Holocaust events, and things like that. But I continued to teach a course on science and capitalism and I worked on a book on that, although I got more interested in more general social theory. Eventually I got interested in microprocessors and what they were about, and out of that I wrote some articles...well an article in Socialist Review on microprocessors and the social origins of them and what they could mean and so on. And then I moved to Washington, D.C., to the Institute for Policy Studies...well I moved to Washington, D.C. and then I figured out where could I be and it seemed like I could either get a job working for a contractor for the CIA, which didn't interest me in the least, or I could try to find my own money to work at the Institute for Policy Studies. So I chose that course even though it was very hard to find money, I wasn't very good at doing that. And I did work there...

Catt:

Roughly when was this?

Goldhaber:

This was 1980 that we're talking about. I was in Washington from 1980 to 1987. I was at the Institute from 1981 to 1986. And I did a lot of work connected with technology policies. I organized a conference on Technology for Meaningful Work that was sponsored by the Rodale Organization in Pennsylvania.

Catt:

That's Rodale?

Goldhaber:

That's right, yes, in Emmaus, Pennsylvania. And the conference was there in Pennsylvania and there were a wide range of people from leftist to people who worked at the World Bank, and it was kind of an interesting conference but it didn't really lead to any very dramatical decisions, I must say, although I still am very interested in the whole area of technology and work. I then wrote a book called Reinventing Technology, and that was about the last thing I did at IPS. I then started working on...and it's still my current project, which is trying to understand the economy of post-industrial society. And I'm just completing, or revising a draft I should say, of a book called The Attention Economy, and it's not clear at this point who is going to publish it. I thought it was going to be Doubleday but we're not sure now.

Catt:

Okay. In Arizona did you teach any courses on the social responsibility of scientists, or values and science, or science and society?

Goldhaber:

They're not formal courses, so I didn't teach them. I certainly...I mean I was teaching things like statistical mechanics and particle theory and general relativity. I certainly held forth with any student who was interested but there was either no room in the curriculum or no interest on the part of the faculty for my teaching that. There was a general resentment on the part of most faculty members about values-related things, although I had a few supporters who were interested in that. I don't recall it ever coming up that I teach such a course, or whether I never thought of it or they never thought of it. But I never taught anything exactly like that there.

Catt:

Now, as to why you left science, did you have the sense that your expectations—of what you thought science was going to be— weren't being met? In other words, had you subscribed to the view that science is basically a good thing, that it is neutral and objective, and that it represented the highest forms of thought, and then discovered in actuality that it wasn't?

Goldhaber:

Well, I certainly by this time did not subscribe to the view of neutrality, and I certainly still don't, although my current views are very complicated and I couldn't begin to condense them for you at this point. So that was certainly one thing. I didn't feel that science was neutral and I didn't feel it was used in neutral ways. In my book, Reinventing Technology, I do discuss this to a brief extent in some of the opening chapters. At some point I plan to write a longer book on this. But at that time I would say my views were much less developed in reality and...however it's difficult for me to tell entirely what all to separate the sources of my disillusionment which came from the unusual way I had gotten into science in the first place. Namely, that it was just what my parents did. For instance, I remember when I was at Harvard, I was feeling very homesick but I knew every single senior faculty member in the Harvard Physics Department, and they had all been to our house, so it felt like this was home. So I think it was partly strong family loyalties and natures of that sort that got me interested in science. I think at the same time out of that came a certain questioning of whether I would be good enough to live up to the standards of my family. So that was coupled with, I think, the fact that I was younger than the other three people who had founded SESPA, and I didn't have tenure or anything like that, and I just couldn't focus on science. I found politics more interesting to a large extent. And I would

say that I had a very hard time sort of finding a way to balance all my different kinds of interests and talents, to focus on them all at any one particular time and on one particular scientific project. So it was a combination of the moral factors, the real sense that science was stealing from people, in effect, that science was helping lead to war, and that good science was really impossible. There was no “pure” kind of science; science was not neutral. And no one was doing the kind of science that would appeal to me anyway, even if I could even find them—which I couldn’t—at the time, and I was aware that I couldn’t. So it was the personal concerns and the social and philosophical concerns I would say that led me away.

Catt:

When you entered graduate school, do you intend on pursuing a career in academic science?

Goldhaber:

Oh yes. I never thought about it. I mean it was just something I took for granted completely. Of course, I thought I would be like my father and I would sit in my office and think. He was an experimenter but he still primarily sat in his office and thought and talked with them people and so forth, and would come home and sat in the study and thought. And so I would do the same thing. My mother was a more active experimental scientist in the laboratory but somehow she wasn’t my model. I think also there was a disillusionment in a different sense in that they lived in, a certain way, a kind of golden age of science and it was possible to get a lifetime job, good jobs with few requirements, for instance they very rarely had to apply for grants or anything of the sort, and they could do what they wanted. And so I always had the idea that I would be able to do this, and I think, other realities started to shake this... for instance, when you had to apply for grants in which you would have to describe what you would be doing in advance, and that you would have to teach a lot. And so in some ways, I had had this very infantile concept that it would just be this wonderful thing. And I had a whole different set of conflicting objections to what was going on.

I found out later, really when I was quite grown up, that my parents had tried as hard as they could to work on the atomic bomb even though they didn’t have clearance and they had contributed a lot to the bomb, which I also felt abhorrence about. In fact, I found that out while I was at Arizona. It also contributed to my unease. Somebody had told me that he had heard a rumor that my father had tried to make the bomb all by himself which he couldn’t possibly have done. But he had tried from well before the Manhattan Project got off the ground from knowing Szilard who also was the progenitor of the bomb, I know that, I know this part of it is true, that he actually taught Szilard nuclear physics and then he tried to do everything he could do to figure out what kind of research would be required to make the bomb work, so that he could do it, and he and my mother did that research. And it then was classified and they couldn’t look at it.

Catt:

I’m struck by your almost paradigmatic categorization of the generational difference that seems to have polarized, on one hand, physicists and on the other, scientists in general. I mean, it seems that your parents and other physicists and the rest of scientists were poles apart, especially in the 1950s and early ‘60s when, as the historian of science Paul Forman calls it, “physics was fun.”

Goldhaber:

Yes.

Catt:

You didn't have to think about all these issues as you were growing up, I mean, such as the ones you faced in the late '60s. Then all of a sudden something happens and now society starts to look upon scientists differently, not so much as heroes but as villains, perhaps. So your generation of scientists, if you will, had to face this unlike your parents. Did it somehow effect your relationship with your father, or both your parents, for that matter? Did he, or did they want you to stay in science, even when you explained to them why you were getting out, or even while you were thinking of leaving it?

Goldhaber:

Oh yes. I mean he, both of my parents were very, very upset that I left science and that I got involved in the politics of science also. And they gave me lots of reasons not to do it, especially my father. And at that time, he was still director of Brookhaven Laboratory so, I guess there's another side, I mean although he wasn't political he did have strong opinions about certain things, and that certainly influenced me. And so it was a mixed thing. I looked up to him very much. For instance, in 1961 he became director of the lab and one of the first things he did was to terminate all classified research there, not because he opposed war work per se or at least he never expressed that as his reason, but because he hated secrecy. And he didn't want there to be guards at the gate and things like that. And I really looked up to him for that. Also during the Vietnam War, when Charlie [Schwartz] was fighting with the leadership of the Lawrence [Berkeley] Lab for permission to speak, my father always protected the right to speak, despite a great deal of hostility against that, especially on the part of the non-scientific staff of BNL. I respected him very much for that.

But at the same time, he found it very, very hard for anybody in his family to do anything overtly political. That really was...again I have to think about how much of this I want to be published, I mean if you publish this as a book...I assume he's not going to read your dissertation somewhere but... Anyway, as to your general question of the disillusionment of society with science, there's another side of that too that I would call really iffy and that was... that when I would say to people...when people would say to me, you know, I met for instance in Tucson, "What do you do?" and I would say "I'm a theoretical physicist," their reaction wouldn't be, "Oh, physicists do bad things." That was extremely rare. In all my life, I've gotten that very rarely, although I have. But much more common was sort of reaction by rejection which seemed to be "Oh, you're so smart, you must be so smart to understand that stuff...I can't possibly understand it." And then leaving to talk to someone else, so you felt you were a freak, you felt you were kind of in isolation, you know, on your own. I mean if I had lived in a place where there were lots and lots of academics, I would have had a different sense. But what I sensed was that in fact physics was very, very isolated; theoretical physics was extremely isolating. I'm not so sure I'm that much less isolated in my life now, but I somehow thought I would be if I did other things.

Catt:

Okay. Pushing on to other social movements from the period, how sympathetic were you to the New Left and the student movements, by which I infer SDS [Students for a Democratic

Society]?

Goldhaber:

I was never involved with SDS partly because I didn't really know about it until it got really wild and flamboyant in the late '60s. I am somewhat friendly with a lot of the early founders of SDS now as a matter of fact. But I had good feelings about early SDS, not good feelings about it when it got violent or quasi-violent. I guess that's about it.

Catt:

Okay. What about the equal rights, or the women's movement?

Goldhaber:

I was very, very supportive of that partly in addition to everything else because I was always aware of what it was like for my mother to be a scientist.

Catt:

One of the platforms of SESPA was for more representation within science for women and other minorities. With this in mind, I want to ask a two-part question. First, did you have discussions outside of physics with other disciplines? And second, did you actively try to recruit minorities, especially women, into SESPA?

Goldhaber:

Did I try to recruit women and minorities into science? That's sort of the easier one. I mean certainly, anytime I taught or anything like that I always tried very hard to do that. In terms of encouraging people who might have abilities or who felt that maybe they shouldn't go into the field, or had questions about it, I really tried to encourage them. As far as recruiting people into SESPA, I think that at that time we were trying to recruit as many people as we possibly could. I don't think that we singled out women. And I must say I had the feeling that we were not doing them a favor by getting them involved in this tangential activity, necessarily. There were very, very few minorities...well there were Asians, I did encourage Asians when I moved to Berkeley and when I was teaching there in the summer. I tried to recruit some Asians Americans, mostly students, not into science, they were already pretty interested in that, but into political outlets. I found that very difficult. There weren't very many others, and I myself always had this idea that you don't try to force yourself on people. I never was fond of that type of recruiting. I mean you send out leaflets, you do things like that. You don't try to do missionary work. Somehow I think I've always been at odds with that idea. I guess partly was based on the fact that I always learned that Jews don't proselytize and you don't try to pull any strings. But certainly you could try to make people welcome if they came. And I very much did try to make any women that would go to meetings welcome. And there were a number them, I believe, in Berkeley. When I was at Rockefeller University there were plenty of biology students for instance that were...you know maybe they weren't specifically interested in SESPA or Science for the People, but they were certainly interested and being radicals, in one way or another, a lot of them had been long before I met them, and I certainly was interested in encouraging them with that and in encouraging them to be scientists. Most of them really were very tough people and they had to be... to be where they were. I would also say the same with the case of my mother. I feel, perhaps, when I was a teacher at the University of Arizona I helped to encourage a couple of people who otherwise would have been discouraged.

Catt:

Did you know Deborah Solomon, who was a biology graduate student at Columbia in 1969?

Goldhaber:

No, I don't think so.

Catt:

There's a reference to her, as belonging to New York SESPA.

Goldhaber:

Yes, I might have known her but I don't...it doesn't really ring a bell.

Catt:

Okay. How about the Black Power movement? Or the civil rights movement in general?

Goldhaber:

The civil rights movement, I was always very strongly in favor of it, although I never went south. But I did do things as I told you about in East Palo Alto. And I certainly, at one time, was very admiring of even the Black Panthers, although I cooled on them eventually. I've always been a pacifist pretty much. The idea of carrying guns has not held a long term appeal for me. When I was in Washington, for instance, I got to know James Forman who was one of the founders of SNCC [Student Nonviolent Coordinating Committee], and I really felt very privileged to be able to know him. I found that he had a very low opinion of himself, and I really tried to do what I could to you know raise his opinion of himself, because he had done some really historic things. So I wouldn't say that I've ever been incredibly good at that sort of thing, but I have tried to do it in certain ways and certainly in principle I was always willing to fight racism in America. I think scientists as a whole probably are not that good at this kind of thing. I remember though when I was at SLAC, once [Richard P.] Feynman came to SLAC, his only reason for coming was to take part in a program of teaching black high school students who were there for the summer. And so I think that was another way, it was part of the marketing scheme for Stanford. I guess I've done very little in the way of specific teaching about alternative views of society, but I feel people who can do it are great.

Catt:

How about your involvement in the "March 4" movement in 1969?

Goldhaber:

Yes. Well that was a big thing as far as SESPA was concerned, and I was one of the leaders of organizing activities in New York. I can remember we had a set of conferences at Rockefeller and we invited a lot of outside people. We had a march also. I can't remember where it went exactly, but I believe it went by the Riverside Research Institute, Columbia University, and I think downtown Manhattan. Anyway, I remember it did take place. And that was one that was organized with somebody who was at that time the head of something called the American Orthopsychiatric Association. And I remember working with him and it was the people in Boston pretty much on this. But I must admit I'm drawing kind of blank as to what we actually did.

Catt:

Were you ever arrested, either in connection with this or any of your other “radical” activities?

Goldhaber:

I’ve always had a kind of phobia about being arrested that perhaps relates to the Nazis. And so I was careful never to be... Except once when, this was in 1985 incidentally, I was in Washington at the Institute for Policy Studies, everyone at the Institute was going off to be arrested in front of the South African Embassy where we had understood in advance that the police would release us pretty much after a couple of hours. And we wouldn’t be charged and stuff like that. I was still very nervous about it I must say. I got into the police van, I was afraid they were going to gas us, not with tear gas but poison gas. And so I have been very paranoid about that. But in fact we had to sing to get arrested, something that I’m not very capable of doing. It’s illegal to sing within 500 feet of an embassy, by the way.

Catt:

I didn’t know that. What were your feelings towards the US space program? Politically, it was one of the cornerstones of Johnson’s administration, to continue to fulfill Kennedy’s legacy, but after the escalation in Vietnam and all of the other things that were going on then, people began to ask if we should really be doing this, right?

Goldhaber:

Well, I remember when the first rocket reached the Moon. I was on a train going into Paris, and I was happy that I was nowhere where I could see it or hear it. And I had very skeptical feelings about it. I think I also had a certain amount of awe about the whole thing as most other people did, and about certain other aspects of the space program. You must remember, I had come of age in the Sputnik era and I had, certainly to some extent, benefited from that as other people did in science. And I think I regarded it, however, as a kind of pointless exercise in a certain way. I mean yes, getting people on the Moon was very adventurous and so forth, but it didn’t lead to anything. And I’ve never liked the idea of colonizing outer space. I once wrote something about the main use of the space shuttles, probably for entertainment, and, you know, people could go up into the space, they would be good for things like sex, sports and gambling, and that’s probably the main values it would have...and all in a gravity-free atmosphere. I’ve always been very, very skeptical of the arguments about manned spaceflight. But again at some points, I’ve had continuing concerns about how the U.S. decides its budget. The space budget has always been much less than the military budget and I did feel in general it was a better way to waste money than building weapons.

Catt:

Go on.

Goldhaber:

I lost my thought.

Catt:

That’s okay, we’ll move on. Did you have much interaction when you were in New York with the Boston group or with other SESPA chapters, or even with other radical organizations? I know that Stony Brook had a chapter of SESPA, and that they too had to face the issue of

classified research on campus...

Goldhaber:

I mean I had a fair amount of interaction with Boston in particular. Certainly in the first year or so that was quite constant.

Catt:

With whom?

Goldhaber:

Well again I can't quite remember his name...oh, I think the principle person I talked with was Joel Feigenbaum, yes. He was one of the student organizers at MIT, you know, for the March 4 movement. I'm not sure what happened to him.

Catt:

He wasn't on the list?

Goldhaber:

No he wasn't. And Ira Rubenzahl wasn't on it either. I wrote to Ira Rubenzahl, who was the other person I had a lot of contact with in Boston.

Catt:

Right. They were more identified with the Union of Concerned Scientists, organizing the student segment of that called SACC [Science Action Coordinating Committee].

Goldhaber:

Right, yes. Well, I mean originally they were sort of the same. I did have some involvement with the Union of Concerned Scientists. I had a lot of involvement eventually with the FAS, the Federation of American Scientists, and certainly went to their meetings and so forth. In addition to that certainly other organizations that...well, with all kinds of different left-wing groups that met in New York at the time. Which is really quite varied from...I don't quite remember their names right now, but there was a sort of Puerto Rican equivalent of the Black Panthers and I remember I addressed their meetings. I went around and gave talks about antiballistic missiles all over the place, to numerous groups in New York, but mostly I was preaching to the vaguely converted, at least, and to old leftists and New Leftists and church groups. So in general, yes, I was very involved in a lot of what was going on.

Catt:

Did you take part, did you actually march during the 1969 APS meeting when there was the march on the White House against the ABM?

Goldhaber:

Yes. In fact I was one of the leaders of that and I went in and talked with Lee DuBridge afterwards, along with four other people. Mike Casper was one of them... The march in Washington from the Sheraton Hotel, where the physicists were located, to the White House, was one of the things I led and helped organize and where I felt we accomplished something

because the leaders of the march were invited to go see Lee DuBridge. I happened to know Lee DuBridge, who was Nixon's science adviser then, and who was the former president of Caltech; I knew him through my father. And I remember being very vociferous. I mean, the other people who sat at the meeting basically were very shy and didn't want to say much, and I felt this was our opportunity and we had to say something. So I was very vociferous and I said that for thus and thus reason, I can't remember them all exactly right now, the ABM is really going to help promote an arms race and will promote attempts to gather first-strike capabilities, and we really have to avoid that at any length, and that it's an extremely dangerous thing and we shouldn't do it.

By this time, that was a position which was actually quite well established within the physics community. But still it was a radical position or so it seemed. I mean, for instance, I think Sid Drell basically agreed with that position at the time. And I think we had some effect, in that, in fact, the administration did change its tune somewhat on the ABM, and we did end up working towards an ABM treaty. And we felt there was a real change. So that was one of the places that I felt we had some effect on the actual powers-that-be by coming out and speaking very boldly. I felt very proud of my accomplishments, of our accomplishments in doing that whole thing. And, of course, about 100 people had marched. There was a great feeling of elan in planning the march and carrying it out and, I believe, that was the first time physicists as such had ever marched anywhere.

Catt:

Quite historic.

Goldhaber:

Yes.

Catt:

How active were you in the APS at this time, in the context of organizing which eventually became the Forum on Physics and Society?

Goldhaber:

Well, I mean, by the time the Forum came along, I was certainly very active within the APS. That was in 1972, I think. Now I was involved in the early starting of the Forum, and I was also involved in things like the opposition to meeting in Chicago, which was in 1970. I remember I was the one radical spokesperson who went to count the ballots or who watched the ballots being counted when we voted whether or not to meet in Chicago. So I did have a fair amount of involvement with both the inner and outer workings of the APS. And I remember several occasions when I was talking with the guy whose name I can't quite remember at the moment, who was the permanent secretary of the Physical Society and...

Catt:

Bill Havens?

Goldhaber:

Bill Havens, exactly, yes. And I had a number of interactions with him.

Catt:

How would you characterize him, or these interactions?

Goldhaber:

He was always the opposition, I would say, basically very leery of anybody radical, but also very concerned with just a few big things in the APS. He was a sort of dull and gray flannel-like in a certain way, always maintaining control of himself. So they were not very friendly meetings, but they did take place. He was and still is at Columbia. The other people in the APS, like the man who was the treasurer of the APS, Karl K. Darrow, and who was the other person who counted the ballots with me, I felt much more connection with him as a human being than Havens or the other APS officers. I had actually known him previously. So many of these people, like Ed Purcell and Luis Alvarez, unlike others who were young physicists coming up, I had known a long time. So I had a different take on them somewhat. I found myself in some various close, strange, situations. For instance, once we protested at another Washington APS meeting, I think it was in 1972, and we protested the man who had become Nixon's science adviser, the one who followed DuBridge, Ed David [Edward E. David], and David was going to address the APS banquet. Well, we all felt he shouldn't, mainly because it conflicted with what we in SESPA were trying to do in physics. I mean David was very establishment and all that. So we in SESPA wanted to have a boycott of the banquet where this was to be held, a very major thing for me even more because, in fact, my father was getting a prize from the Physical Society at that very same meeting. Well, my father worked out some compromise whereby people would be able to speak up, to some extent, against what David had to say. And so while I was active in physics I had a lot of involvement with the Physical Society. At this particular Washington meeting, I should say, I also gave my talk on scientists as war criminals, and it was delivered to a packed hall, I think maybe 200 people were in attendance, and it was one of the 10-minute papers that anyone could give. It was an exceptionally high turnout for any set of 10-minute papers, and I felt quite good about doing this, radical though it was. The FAS was also a certain focus of my activities, but that's a separate question.

Catt:

What about the so-called Schwartz Amendment, if you recall introduced by Charlie in 1968, in which he wanted the APS to decide, or to vote on, whether it should discuss political and social issues involved with physics?

Goldhaber:

Yes. I was definitely supportive of that, and working for that.

Catt:

I recall there was some controversy over the counting of the ballots and the fact that the ballots were sent out at certain dates which would not allow for them to be returned in time. So the Amendment was sort of defeated by nefarious means.

Goldhaber:

Well, I wasn't involved in that controversy. That's when I was involved in getting signatures at SLAC. I didn't yet have my Ph.D. so I didn't get quite that involved in the Society at that time. But certainly I was very supportive of that.

Catt:

Did you feel that, perhaps, the APS was dragging its heels in dealing with the issues, of not wanting to open up and have these discussions on social and political issues?

Goldhaber:

Oh yes. Well, I definitely felt that at the time. I felt that the APS was certainly dragging its heels and that it was obvious why. I mean, people didn't really want to rock the boat, didn't want to talk about these things. And I think in a lot of ways, I would still suspect it would probably drag its heels on things of that sort.

Catt:

During this period, did you feel that the time was ripe, that there were a number of scientists, at least enough of a power base of scientists or of physicists, with which if you could organize around an issue, say the anti-war issue or the social responsibility issue, that you as scientists could...

Goldhaber:

Make a difference? Well, at first I certainly thought that. I had the naïve view that all you had to do was to tell people about what was going on in science and they would open their eyes. But at the same time, I guess, I was also aware of the varying connections scientists had with the military and so on. I thought that a lot of people who weren't involved in military work at least would take my side. For instance, I was hopeful when I was trying to get people to boycott Los Alamos and Livermore and what actually happened there was sort of instructive. I remember people would say they were very sympathetic, they couldn't do it or they understood my feelings but they were sorry they were going to go in, and then a certain number of people who showed up laughed in my face or acted like I was on pot or something of that sort. But I certainly felt that there was some hope for rational discourse and, at the same time, I also realized there was this tremendous shying away from politics and people would hide behind the excuse that they didn't understand these things, they didn't understand people, that they didn't know about these matters. And I guess I would say in retrospect that some ways I didn't know about people or society or politics or economics or very much else either then. And I just assumed that I did. But I also think that they were wrong to kind of feel like they could get out of it.

Catt:

Did you have the sense that you were doing something quite radical within science at the time? Do you recall if your colleagues were giving you the appellation of "radical" at this time?

Goldhaber:

Well, I would say yes and no. I mean, in some ways I felt quite radical, in other ways I felt I was simply doing what I had to do. And therefore it wasn't radical at all. That I couldn't not do it. I would be looked upon as a coward if I didn't. And that once I had stated my position I had a duty to carry it out in various ways, that was one thing about it. And so I did feel that people told me I shouldn't do it, and I think they viewed it more as a peculiarity in a lot of ways rather than I was some radical on the whole. I definitely felt, though, that scientists had a whole range of political feelings and opinions and that this was one of them that was perfectly honorable and was the right position. And so it didn't seem all that radical since it was the right position.

Catt:

Could you, perhaps, articulate what that “right” position was?

Goldhaber:

Well, I mean, people such as myself have certainly evolved since that time, but I would say the position was that scientists had to be aware of and take responsibility for the uses of science and technology, and that they had to look at the indirect functions of their research, and that they couldn't just do their science and act as if what happened in the lab was disconnected from the world. And that, in fact, they had a great deal of responsibility, an enormous power in the world because science is... later on I coined the phrase “science is an unbounded action.” By that I mean, for example, once you figure out how to make the atomic bomb, let's say, that secret is then available forever. It's the opposite of a secret, really. It can never be not known again however you say it. So when you're doing something in the laboratory that you intend to publish at least, you're taking an action that will have an effect for all time. Therefore you have to have a lot of thought about whether the effect of this action will be. As I discuss in my book, for instance, you can't say that you're fighting this particular dictator by building this weapon because this weapon will then be put in the hands of every future dictator of the same sort. And so you as a scientist have not less responsibility than the average person who, let's say, goes off to war and fires shots or somebody who shoots somebody in the street, but more responsibility.

Catt:

So did you see that somehow science, or scientific knowledge at least, was...I do not want to say being misappropriated, but when you say that there is no end to the scientist's responsibility for the knowledge he or she produces or publishes, especially as a theorist...well, how far does that responsibility go? Is there some absolute, or is it even feasible to draw a line to say, to a certain extent, that since the knowledge will be diffused and disseminated to so many people, and that the old adage, “Well, if I don't do the research, someone else will,” in other words, that the individual scientist can't be held accountable for the knowledge that they produce?

Goldhaber:

No, I never agreed with the statement that if I don't do it someone else will because I felt that a lot of things that scientists did were very hard to do and not that many people could probably do them, at least in a given time and place. And that science was also much more socially specific that it admitted. That is, that it had a context in which it was done and you wouldn't do the same science in another social context. And so it was simply untrue to believe that all of these inventions and discoveries in science were simply mimicking what was out there in the world anyway. It was actually created in something that was doing the very specific and sharply-defined act for which it was designed, and as such, it then was possible for inventors or discoverers to have a moral responsibility assigned to them. And then the question became what was the limit of this responsibility? Well I felt it was when you can foresee the consequences and you have to look, you had to think about that, really. And it would depend on the context to some extent as to what kind of consequences you can foresee, but you had to look at the negative ones as well as the positive ones. I think my attitude towards science has, like many other scientists, always been somewhat conflicted in that, on the one hand, I'm very curious about things, and on the other hand, I have this reservation about knowing the unknown.

But I would say well you might be very curious about what would happen if you cut off somebody's head or something like that, but you don't go ahead and do it. And so your curiosity, interesting as it is, has to be bounded by your moral sense. And I was very influenced, for instance, by the example of—or the bad example of I should say—the Nazi doctors doing their experiments and I held that all of science was warped more than it's willing to admit. And I think that was my point about scientists as workers that, for instance, in building and designing a bomb or anything, doing work that was directly related to a bomb, that you knew was directly related to it, it was as I put, the moral equivalent of leaving a machine gun in the Street where anybody could just pick it up and shoot whoever they wanted to. And that you weren't actually shooting the people but you were almost guaranteeing that somebody would be shot. And so the question of whether science is good, basically good, I would say it's much more complicated than that. I think I would certainly say that right now, that certainly you can say some of it basically good. I can't say it's simply basically bad either. Because certainly rational discourse seems to have value to me. But you have a very complex moral equation that I don't think you have the right to ignore and that you have to take very seriously. And I think the great failing about science, although it's hard to see exactly how it is structured otherwise, is to be very, very insistent on not looking at that issue. I don't know whether Charlie [Schwartz] mentioned this example to you, but for instance, he once tried to require that all the students in his class...

Catt:

Take the pledge?

Goldhaber:

Right, to take the pledge or at least show that they had thought about issues. Oh, I loved the pledge. I administered the pledge to a lot of people. I was very concerned about it. I took it extremely seriously when I signed it. And I found it shocking that while there's the Hippocratic Oath for doctors that's taken for granted completely, and is part of the graduation ceremony at medical schools, the scientists would allow nothing of the sort. That even broaching the subject was somehow a violation of academic freedom. It was, after all, a very powerful statement. You know, at the time, I talked with my uncle [Gerson Goldhaber], who was on the faculty at Berkeley in the Physics Department, about this pledge, and of course he took the view that we can't do that, it's a violation of academic freedom. And he otherwise was a fairly liberal person. But he took this position and it was more or less a gut reaction. And I think while I understand that people are afraid of a kind of theocracy taking over, I don't think that justifies the attitude that anything should go. The difficulty is, of course, people take science because they want jobs and obviously the jobs they are likely to get will require them to be immoral. But I think that actually intensifies the need. If you are going to try to do science—which is probably one of the main reasons I'm not doing it—that just intensifies the need to raise these moral issues and to insist that they be taken up and considered somehow. At the same time I'm very agnostic about how to do it best because I don't believe that if you say force people to take an ethics course it means anything. I mean, people will get an "A" on questions and then go ahead and be as unethical as they please. But you know, some way of really raising the issue I believe refreshing people's minds about it, seems to me something that I would advocate if I knew how to do it.

Catt:

So maybe a moral component, a moral ethos of science is what was lacking, and that was the goal of the radical scientists? Perhaps it was not an attainable goal, but one in which you saw

yourself, during this period, as trying to raise the consciousness in the scientific community to at least think about the issues?

Goldhaber:

Oh absolutely. I did and I feel that...

Catt:

And do you think you were successful?

Goldhaber:

Much less successful than I would like to be. But in some ways I feel that the anti-war movement as a whole, and even the movement against weapons, was perhaps more successful than they realized. And it was a success that had to deal with very slight changes in peoples' behavior and interests that pulled them slowly away from their willingness to do war work or weapons research. And I feel this in general, that the New Left always felt it had failed the American people. For instance, when the Right more or less came to power in this country, the New Leftists felt they had completely failed and had been defeated. And certainly they had hoped for, I don't know, democratic socialism, but we didn't get it, at least ostensibly, although I think we came closer than perhaps we realized. But I don't think it was a failure in terms of the way that certain issues get addressed and even though it's much vilified now by the Right, I still think that it's enormously changed how people in this country view themselves. I would say as corollary scientists do view themselves differently. What I personally contributed to that...I feel it probably had some effect but I really don't know, and it certainly is a difficult thing to work at it and see that the main effect is probably that people vote against you for tenure and are probably a little bit more opposed to you than they would otherwise be, and that you find there's no job that you feel you can accept whereas other people are going ahead and having good careers. And sometimes it feels very painful to see, to think that, "Boy, it sure is a lot easier if you don't have to protest." But I don't know that I would make the trade.

Catt:

Did you ever sense that there was a movement, a radical science movement that was taking place? That there was an identifiable segment of scientists who were reacting to the state of American science at the time?

Goldhaber:

No. Well, I think that's unquestionably so. I think the only question would be what were its boundaries and how they could probably define it exactly. I'm very leery of the term "movement." I mean I've thought a lot about these issues because...

Catt:

How about we just define what is a "radical" scientist, and the meaning of it?

Goldhaber:

Well again, radicals, let's say radical scientists, are scientists who find themselves at odds with essentially the centrist position among science to beforehand, because there were always some radical scientists. But namely those who feel a whole set of possible values. One is certainly the whole moral issue connected with weapons and war, and the Vietnam War in particular, than

concerned with all the others, like feminism. For example scientists complicity supporting interests, like the notion of male superiority or white superiority in science and so on and so forth. Or the way genetics tests are done. I would say that, certainly, awareness of and, to some extent, involvement with that and taking it seriously certainly grew, as far as I know, in the period I'm talking about. And this was a new generation of scientists who were not, let's say, part of the general 1930s "lab rats" or the scientists' movement of the 1940s. They had a wider sense, I think, of skepticism about the value of science itself and about how to do it than the earlier generations did. For instance, leftists tended to be very pro-science, off hand. Some people, for example, who were very new age in touch with things, astrology, witchcraft...I have very little sympathy for that. I mean I do think...my sympathy is complicated. I do feel that one has to have a higher degree of respect for other ways of knowing things that pre-scientific or non-scientific societies have had. I also think that one cannot simply accept it as equal. And I find myself very torn and I haven't fully defined that. But I do think I have an affinity for that certain openness in this period, and that I think a lot of young scientists still would be very hesitant to work on weapons or something like that. And I feel a lot of them are very moral and ethical and have a lot of concern about the type of research they will or will not do, and that indirectly, perhaps, I have influenced them. It's hard to tell, but why not?

Catt:

Did you have much correspondence with international movements, I mean radical scientists or radical science organizations abroad, during this period?

Goldhaber:

Much would an exaggeration. But certainly some. I mean, when I think about it, it was very haphazard and informal at that time. People I knew who came from other countries were here for a while or people who went to other countries and so forth would have something in common with them. But I guess another group that was formed later was the Computer Professionals for Social Responsibility. I've had a lot of contacts with them. I knew people in Norway, for example, were very involved with them. And people in Italy. Certainly some others went to China and talked with Chinese and I think some of my articles have been translated in China. There was a big center in Mexico. And there was some people I got to know from that. So, indirectly I knew lots of contacts. Maurice Bazin, for example, has been very involved in science in places like Chile, Brazil, and Portugal. And he right now, for instance, has a tremendous network of contacts and basically he's much more interested in the issue of valuing alternative approaches. For example, in showing how people who weave baskets at Mozambique are using interesting forms of mathematics that are different from western forms. And how to teach science in the Third World and so forth. And he has huge international radical contacts, much more so than I, I must say.

It certainly does exist at one point or another. I guess I'm somebody who's come to be skeptical of formal organizations a lot. I've seen a lot of the meetings we had in SESPA were a waste of time and where I felt, at least when I was in New York, at least we basically went around in circles and so forth. And also the organization was too amorphous. It's impossible that everybody agreed to some common set of goals or something like that. It became nothing but the magazine in the end, and then sort of disappeared. So I think a lot of the activities went on more informally and the connections we built still exist, and I feel like other people are perhaps much more involved in let's say the day-to-day use of the internet or something like

that. I know there's an internet network of people concerned with lots of environmental issues and things like that. There is certainly a huge network of people who are out there who perhaps never were scientists officially or they're not physicists let's say who are a full-time professionals dealing with the issue of weapons internationally or in lots of ways. I definitely feel that there is a movement internationally that's incredibly vast and complex and I know only a little about it and I've talked to people from India...when I was at IPS, I talked to people from, I don't know, 25 or 30 other countries and in one way or another, I found that there certainly existed, in places like India, a radical critique that was, in certain ways, a very different form. And the U.S. was by no means the center of it, I don't think.

Catt:

Do you feel that SESPA or the radical science movement seemed to dissipate around 1974, which may or may not correspond with the U.S. finally getting out of Vietnam? I mean, it seems like around 1974 there's a lot of fractionation that goes on in the New Left. You see people in science starting to gravitate or become polarized around issues like race and gender. Do you think the same thing happened with radical scientists?

Goldhaber:

No. I think some of them moved away from SESPA so I guess there was some dissipation.

Catt:

When did you leave the organization?

Goldhaber:

When I formally left I'm not quite sure. But when I stopped being or stopped identifying myself as a scientist, I think the center of my political activities spread out and I think that was one thing that happened with quite a number of people, Al Weinrub for example. I mean if you weren't being a scientist it didn't seem like the right focus of your activity. A lot of people became more political than SESPA would allow. Others...I do think that it lost a hold in science. I mean if you looked at what it was doing in the mid-1970s or certainly by the mid-1980s or something like that, very, very few people involved with it were formalized Ph.D scientists, especially in the hard sciences, and I think the war or the movement or something in general like the New Left, faded and divided and people went in all different ways. Some marched back to the establishment, some further away to the extreme left. So yes, it certainly dissipated, and I don't exactly know how to think about that except it was somewhat disappointing in the long term. I don't know what our exact goals were, certainly to get hundreds of thousands of scientists to sign the pledge, and try to transform science and build paradise in America. That hasn't happened yet but it's not different from any other movement of the period we're talking about, so I don't feel like that's a special sign of failure.

Catt:

Do you think the image of the scientist in American culture lost or gained something because of the radical scientists' outspokenness, or because of their overt entry into the political arena?

Goldhaber:

It's interesting because I would never have guessed 20 years ago that scientists would almost have no image nowadays. But it sort of feels to me as if they don't. That for the most part

people have no image of science any more. People have no sense of their...maybe that's because science has gotten too big and it's divided up into specialties or something like that. But, you know, the concept of the mad scientist or the idea of the guy in the white coat or anything like that's a thing of the past. There's nothing that I know of that has come along to replace them as a generic science personifier. So I think the average person has no image of science, really, as such or even thinks in those terms exactly. Whether we had anything to do with that I don't know. I mean I just don't have a clear sense that we did effected the image of science much at that level.

Catt:

I'm interested in the question, "Is there a scientific method?" As you go through the training to become a professional scientist, it seems to me one must learn to question authority. I mean, if you want to add to the corpus of knowledge and to do that, you have to challenge the received view. I think most would agree with that, and that it almost seems that if scientists were trained to think this way, that this impulse to not go along with a status quo would allow more of the scientists out there to want to gain the autonomy that scientists have always dreamed of, one of those notions of just give us the money and leave us alone and we'll go at it. And I'm curious as to what restrains or constraints were put upon scientists. I mean, you always get the sense when talking with radical scientists like yourself that everyone felt that there was going to be this great awakening, this great rush that people were going to start talking about the issues, people were marching and organizing, then yet something happens and people start dropping out or returning back to the labs. Is it the scientific community itself that puts pressures on the individual to stay in line?

Goldhaber:

Well, it seems like you're asking a couple of different questions and I'm not quite sure I understand their connection exactly. Well let me return for a moment to the question of the scientific method. I don't believe there is a scientific method, and I think it's kind of an oxymoron to say that it, that science trains people to question authority because scientists get trained with being much more accepting of authority but then they don't in a certain way or the good ones don't completely. But if there's always a tension there it seems to me between the old and the new and that tension gets played out sometimes in the very smallest scales and then sometimes in a much larger scale. But it's always it seems to me in the life of every individual scientist a kind of strain or stress that they feel when they do inevitably come to question authority or they don't, or do they succumb to authority in just doing very routine sorts of things? How that relates...I mean obviously it does relate somewhat to...also I would say that different scientists, to a different extent, do science in very different ways, have different personalities and different philosophies that actually get manifested to very often both in their politics or in their general approach to the world and to their particular sciences. And you can see links in the way they do things, theoretical physics for example, and what they're politics are in the world, and so I would maintain that a very pragmatic approach in doing theory, for example, often goes along with a very kind of pragmatic political approach. So there's a whole level of complexity there.

As to the question of why in a sense what made the movement dissipate so far as it did dissipate within science, I think you can look at it in a number of ways. One is obviously that a movement will if it... if it does succeed in energizing people it will lead to an explosion in which

some of them get completely out of the spirit and they're no longer part of the central core of whatever it was they were doing before. That in a sense is what happened with me and with many others. Others, you know, never really reached that level of excitement to look at it in those terms and fell back into the core position and found that the people who were pulling them on and nagging them on and so on were not around any more and so they left. And to some extent of course that was forced upon people by the contingencies of having to get a job, working in a job they didn't like but needed, or something like that. A number of other people figured out ways of keeping up both personalities but somewhat separate from one another and continued to do science in a fairly standard way while put their political energy into something else. And a small minority of people, I think perhaps, were able to do both simultaneously. Richard Lewontin was obviously older and able to do that, and so was...what's his name, he isn't on your list...I don't know how much he was involved in the science movement but... oh, you know, the paleontologist at Harvard?

Catt:

Stephen Jay Gould?

Goldhaber:

Stephen Jay Gould, exactly. You know, he's an example of someone who at least until fairly recently was always interested in the political side of science, and someone like Steven Rose in Europe too, and he managed to combine it with the way he did science. I knew many others, however, who could maintain their political opinions but just no longer did anything about them or would say they maintained them but it wasn't so evident in practice. I think it obviously had to do with their jobs and security, and also it's a personality issue. If you're sufficiently crazy like me in certain ways to just say well I'm not going to make any compromises, you know, you don't. And if I have to choose between doing science and feeling moral about it, I won't do it. It had to do with the age you were at the time, how dedicated you were to it, how much you felt that you needed to be doing this and this is... in any political movement, I mean that's why people aren't still marching in the streets over Bosnia, let's say. And I think there was something else too, which obviously was the age, and then there was the intellectual exhaustion which led partly into a division of the movement into a hundred different directions.

I mean those people who went off and started doing the rioting and obstructionist "body work" or something, I didn't consider them any part of the movement and I didn't have a great deal of respect for what they did. Intellectually I might have liked those people. And then there are those who, to some extent like me, felt that this question was so interesting that they... that I made it my life's work, although you may not be able to recognize the question in what I'm actually doing now. I feel there's a linear connection in how I started being interested in how science was related to society, such as capitalism. And so I got interested in the particular example of the microprocessor and I wrote my book on technology, somewhat as a side issue, but then I returned to the question of what is the connection between science and society, I mean, how do new technologies arise and what will modern society use them for and how that can lead to a new kind of economy, and trying to relate all those questions like that.

So to me there's an incredible continuity in everything I have done and thought about it. And I finally figured out how to work in my intellectual and moral concerns in a way that is

sometimes, at least, satisfying to me. And in the course of that, I left the movement much, much earlier roughly 15 years ago. I very explicitly realized I didn't feel going around and organizing people was something I wanted to do any more. I didn't feel that marching was something I wanted to do any more. I guess I have organized a few things like getting 2,000 people out singing on a New Year's Eve to protest the invasion of Iraq in 1991. But basically that was just lobbying other people and my basic sense is what I am best at is trying to develop ideas and hopefully putting them out in the world and see what happens to them. If nothing happens, I did my best anyway.

Catt:

The role of the scientist, when he or she is speaking from a position of expertise, do you think they have a special role in society, I mean in the politics associated with American democracy?

Goldhaber:

Yes, although it's certainly complicated. I definitely think scientists should not be silent, should not accept the "on tap but not on top" model. They should not necessarily assume that they have the right to tell everyone else what to do. For instance, when Newt Gingrich says, "Well, we'll listen to the scientists about what kind of research to fund or about how much AIDS research to fund," in some ways that's better than the Jessie Helms approach where we won't fund any at all. But it doesn't seem to me quite adequate because you can to a certain extent determine the outcomes by what you choose to support. And so it's an oversimplified view to say that scientists know what is good and what is right. On the other hand, I think scientists do have things that they have to say to people about all kinds of issues. For example, the spread of AIDS and that of other viruses, and they need to raise their voices about things that concern them and to figure out ways of doing that which somehow are involved in the democratic process. Imagine something like what if scientists were to discover a large meteor that may have one chance in 100 million or something, of hitting the earth this year, and it will take an enormous amount of money, say a trillion dollars, to develop the necessary means to destroy it. Therefore do you invest any money right now, and if not, when do you do so? And I think that's one that needs to be raised.

People haven't found ways of raising those kinds of issues or to take a more moderate one, the greenhouse effect let's say. In general, I think that science has a lot to say about a lot of issues and it's not the kind of science that implies there's one truth and we know it. And the demystification of science would be very helpful in that process. For example, the issue of race. I mean scientists can certainly contribute the idea that there is no genetic marker for race. It's clear and distinct and there are no racially distinct subgroups. On the other hand, I don't think you could leave the whole issue to scientists either. I think it's part of a whole discussion. Now I do feel that, to some extent, the fruit of science is, in some ways, the level of rational discourse within society and as a whole, at least superficially, is quite a bit lower than it was. And I wonder whether rationality will be the hallmark of the new era. So I have a series of doubts about the role of science as such. I think, if you look to the fairly near future, you see the possibility that science, in effect, is a historically bounded way of developing knowledge, and that it existed in the era of the mass production of goods. And as you move away from the mass production of goods, I'm not at all convinced that you can speak about the same kind of enterprise. So I'm questioning what kind of discourses will take place.

Catt:

So, do you see science or even scientific knowledge as being socially constructed?

Goldhaber:

I certainly see scientific knowledge as socially constructed. Whether I exactly see reality as socially constructed, I would say so in a large measure, but not completely. If you look at the works of the French sociologist of science, and I'm thinking of...oh, what's his name? Oh, he wrote the book *Laboratory Life*...

Catt:

Bruno Latour.

Goldhaber:

Bruno Latour, yes, thank you. I think you'll see that he's on to something, to a fairly substantial level, and that what you can read into what he writes is then, in fact, the commodities as social constructionism. It's very essential in kind of the verification of scientific truths. If we don't have light bulbs and running water and all kinds of things like that around, or homogenized milk and so forth, it becomes harder and harder to verify that whatever truth of science is really true. Well, I'm getting into my whole theory which is complicated. But let me just say that it's quite obvious that to the extent that commodities are certainly socially constructed and how we view them as what they become certainly depends upon our whole mental outlook. Then in equally strong ways, scientific results and discoveries are socially constructed, and the nature of the reality we develop and understand, at least, is based upon the particular way we have of constructing our whole approach to the world.

Catt:

And since science is socially constructed, therein lies power. I mean since it's a process of negotiation, obviously not everyone, I mean you would like to think of science as being on equal playing fields but...

Goldhaber:

Yes. But clearly it isn't and you can't. I don't believe you can imagine a completely neutral playing field in the world. And that you also can't...I mean as I see it to the extent that science will keep going and so forth, science remains definitely bounded by other human interests. To put it in a different sort of way, I think scientific theories, following Imre Lakatos if you've ever looked at his work, successful scientific theories are those that really promote the most results the fastest, he more or less implied, and that those are the theories that continue to hold up. But what "the most results" means, really has to be examined more closely. And what it really means is "the most interesting results." [i.e., the greatest number of] And the question is who defines interesting? And that, then, is where, as I see it, inevitably social conditions and cultural factors leads us towards constructionism. And there is no science that can escape from that fact. I mean, we know it's seemingly more interesting to talk about the origins of the universe than the origins of this table or something like that, though it's not abundantly clear why that should be so.

Catt:

That's true. Oh...we're just about to the end. The question how does radicalism and your

involvement in that change the way you do science is, I hazard to guess, very substantially.

Goldhaber:

Very substantially, yes.

Catt:

What do you think was the big impulse for the emergence of radical thinking, if we want to call it radical, in the late '60s? Specifically, do you think there was one issue or event that stimulated scientists into action?

Goldhaber:

Well, I mean I think the real impulse was not an issue so much as a sense of powerfulness and of having done all the easy things already, and that therefore we were a society that is clearly on the edge of something new. We had fulfilled the basic hopes of long life, good health, everyone having a house and a car and chicken in every pot and so on and so forth. And there was a huge, burgeoning new generation, all that entered into it. And then partly as a result of that sense of omnipotence, came the war and the civil rights movement and the space movement and the opposition to the war, they all came out of a sense that now we could do it all and we could have it all, and then the question would be what was the "all" that we can have. And that obviously was a very multiple impetus. It was partly youth and idealism that we were allowed to have because we lived without want, we lived without a sense of real threat, of fear from external forces other than us, and we as scientists I think felt especially privileged and powerful because we had our minds on the secret, as it were. We knew things that other people didn't know. And we thought that we were very in touch with how the world could work in some way.

Catt:

Did you feel cheated, or slighted by American society because your generation of scientists did not have the same opportunities as did your parents and mentors in science? I mean, have you ever wondered why there was such a backlash against traditional American culture and why you have the rise of the New Left during this time? Could it be the fact that in American culture it's always for the next generation that you want the world to be a better place and that things will be better for your children. And you've mentioned your parents and how they lived in the golden age of science when they didn't have to worry about writing grant proposals, the money was coming, and they could work on whatever interested them...well perhaps not that. But then all of a sudden in the 1960s, science entered a time of limitations where money for science dried up at the same time society was making more and more demands of its scientists. Do you think there is some connection between the two, perhaps in relation to the radical activism in science?

Goldhaber:

It would be an ironic thing but you have to remember the one reason that came about was because science grew so much, and so many people went into it because it was felt that now would be the golden age of science. I mean, we looked upon the 1950s as a preview of what science was going to be: big. And so it was partly that science had grown so big that it was no longer a tiny blip in the national economy. And it actually showed up on budgets and, of course, the war forced budget cuts. I think that Johnson was wrong; you couldn't have guns and butter

as he put it at the time. And science equates to butter to some extent, it was a little bit both perhaps, but since you had to make choices it turned out suddenly that the scientific elite was not quite as powerful as they thought and, as a result, could not fund everything. So that created part of the pressure. You know, I think during the '60s we all had this idea that the science budget will just keep expanding forever and that there are no limits to it. And it turned out the constituency for science was much smaller. Whether we affected that constituency ourselves, I don't know. I suppose you could make some argument that at some levels we turned off certain congressmen who didn't want to fund science because we were being so radical and uncooperative.

Catt:

Like Mike Mansfield?

Goldhaber:

Mike Mansfield, I always thought, was in our camp. I mean I liked the Mansfield Amendment. Charlie Schwartz certainly took it very seriously. But obviously it's true that in retrospect you could say that a lot of the science budget, as far as most of their country is concerned, was a stealth budget that people didn't know about, they just took it as something that went along with their general support of weapons and so on. And the idea of funding science for its own sake was never really raised and you could argue that we raised the wider issue of why should we have science, what is science for. That was the question of why we should have a science budget that was so large. And I think that, in point of fact, the scientific establishment, that attempted to come up with the rationale for promoting science, always had a very shaky set of arguments and an almost implicit set of arguments I felt they were totally willing to sell science for any and all reasons that they felt would appeal to the public. For example, science is good for weapons. And I do think that was one of the issues that we raised, that science had in fact deliberately sold itself to the devil because that was the easiest and simplest thing to do. And then since we raised the issue why should you have science, what is science good for, although I don't think we firmly answered it, we did throw it into question. So it's perhaps no coincidence that at that very time these questions came up within the larger society. That's partly why it came up for us. But they're intertwining and complex...

Catt:

Right. What about the issue of a Department of Science as something that we're seeing today which was first raised...well it was first proposed in 1863...but 1970, Congressman Emilio Daddario had a set of hearings to study whether the U.S. should have a federal Department of Science to get away from this pluralistic notion of funding for science, and that the federal government should have a body of scientists that will decide okay we're scientists, we know what are the most fruitful areas of research, just let us take control of how we should fund scientists and have physicists and biologists and psychologists and what nots on it, and let's let scientists decide what's best for science. How did you feel about that?

Goldhaber:

Well, I think that the difficulty that you would come up with in such an approach is basically the difficulty that, let's say organizations like the NSF still have now, which is how do you decide the overall level of the budget to devote to science? And clearly that's a complex political equation. And I mean I myself in my book *Reinventing Technology* at least propose a completely different method of developing support for science on the basis of the questions

that were important, on the basis of the ultimate social value of science in society, and it would start a very different notions about a whole series of different agencies and so forth that would be within different government departments and would have different responsibilities and out of that would come to some sense of what science should be done. So I don't think most scientists have ever felt very strongly one way or the other about a Department of Science, but I think most have also not felt that it would change things. Because for one thing, the National Science Foundation did operate like a Department of Science to a large extent, although it didn't elude their having to solve fiscal budgetary problems.

Catt:

And it doesn't have a Cabinet appointee either?

Goldhaber:

Yes. But I don't know whether, I mean I don't think most scientists would have a strong feeling one way or the other about what that would be to have a Cabinet appointee. If you appointed some person like the Secretary of Labor, well who listens to him anyway?

Catt:

This is true. What was your most vehement confrontation during this time, from 1968 to 1974? Does one stand out above the rest?

Goldhaber:

Well, it depends on how you define a vehement confrontation. One that I was most happy about was when I was at Rockefeller University and because of my work protesting against the Cambodia invasion, which generally had indirect connection with science really, we ended up in David Rockefeller's boardroom at the Chase Manhattan Bank with all his flunkies sitting around, and I more or less threatened that there would be war crimes trials eventually and that if people didn't take a more responsible attitude on the war in Cambodia that they could be tried. And that got a lot of very red faces around the room, but people were very polite about it, as was I really. I suppose I had this idea that nobody can tell what would happen in 15 years from now and so you had better shape up. And then, in fact, two or three days later Rockefeller himself actually came out against war. I always felt maybe I did that, maybe I scared him into doing something about it. But that was one time, I don't know whether that was vehement in the sense that you mean, like an intense confrontation. I also remember one confrontation with Bogdan Moglich, who's a high-energy physicist, that was part of fighting over Ed David and whether he should be allowed to speak at the Washington APS meeting. And Moglich was all in favor of David speaking. Anyway, he basically attacked me and said "you're doing this because you have psychological problems," and stuff like that. I remember being taken aback by that kind of ad hominem remark. And then there were my confrontations at the University of Arizona over Livermore and Los Alamos. I mean there were people who had a huge Department of Defense grant or wanted to have one, and they regarded me as some kind of geek from outer space for speaking out against the DOD funding all this physics research. But I never had a vehement confrontation, in the sense of a violent confrontation, no.

Catt:

Do you know if you were investigated by the FBI?

Goldhaber:

I used to hear occasional clicks on my telephone, but no, I never actually have checked into that. I never had any reason to suspect that I was. Well, there was some big reason...I remember when I was at the University of Arizona, I had some people who I knew from New York stay at my house. And it turned out that they had some gunpowder and I made them leave because they had this gunpowder and they said they used it to load ammunition. Well, I didn't like that. Then they were arrested for possession of pipe bombs a few days later. It turned out that the federal prosecutor was the wife of a physics professor who was my senior, and so I was sure that their telephone was being bugged, but I called them up and I told them that she wasn't very smart, and that they didn't have to worry. It is interesting that they eventually refused to plea bargain, insisted on going to trial, and she dropped the charges very angrily because she couldn't find a witness in the end. And she then resigned not too long thereafter. But I had no idea whether she heard whatever I said to them and she never gave it away, but that was the only time. I never felt I was doing anything really all that interesting although I used to sort of pretend that it was. I would answer the telephone, "Hello buggers" and things like that. But you know it was a game and I didn't take it all that seriously.

Catt:

Did you ever travel to Ann Arbor or Madison or Chicago or wherever, to deliver a talk or give a speech for SESPA?

Goldhaber:

Oh, I gave talks in Salt Lake City, in New York, in Boston, in Washington, of course, and once at the University of Maryland. I remember being very excited about Stony Brook also, when, in 1972, I was giving talks right after the Cambodia invasion occurred. There was a lot of student unrest then, and I was going from to campus to campus, on the East Coast mostly, I just felt like the revolution was in the air. And I remember it was really exciting to see people respond they way did. I mean, all one had to do was to denounce the invasion, the war, or the government, and you got people up out of their seats. I believe, now that I think about it, I believe my talk was part of a SESPA-sponsored symposium, or something like that, on the use of science in the war. I must say it was one of the few times I felt like I really connected with nonscientists about what we in SESPA were trying to do.

Catt:

What effect do you think all the bombings had on campuses in 1970 and 1971, especially the effect on student unrest?

Goldhaber:

Well, there was one at Rockefeller University and I remember that had a very chilling effect. I mean everybody got really quite frightened, both pro and con the war.

Catt:

And when was that?

Goldhaber:

It was 1970, in the spring of 1970. It must have been May. It came after the campus had been quite mobilized over the issue of Cambodia. And it just frightened everyone and got things

quieted down and made everyone much more wary of both sides. That was my only firsthand experience of a bombing. And there was one at Stanford, but that was after I left. I think that one was in 1970 too.

Catt:

Yes. And there were bombings, also in 1970, at Harvard and Madison.

Goldhaber:

Yes, Madison. You know, I don't really recall what the effects of the Madison bombing was. I think the most intense bombing, in terms of its effects on me, was the Christmas bombing of Hanoi and Haiphong by the Nixon Administration early on in the Cambodia invasion. I mean, here we're suppose to be getting out of the war, and then Nixon does this. It really provided the spark for violence, I feel, because the bombing was so appalling, so frightening.

Catt:

Frightening in what way?

Goldhaber:

Well, I think we, I mean the anti-war movement, just felt that we really had lost at that point. We just felt like there's this out-of-control power and we've already reached the limits of our protest. After all, Nixon had just recently won the election. We felt like we couldn't do much of anything and it felt like it was different from anything Johnson had ever done. It just came out in an extremely ugly and surprising way. I mean, we thought we had made some progress towards peace. And I think at that moment, I feel that was really the turning point in the dissipation of the movement. We just, at that point, felt very weak. It did not feel like the war was winding down. It felt like the war was going to heat up but then the methods used to get peace, I mean supposedly this was done in the name of getting peace, but it was so contrary to what we wanted. This is slightly off the subject, but let me say another thing that I found very, very worrisome and disillusioning was what happened in Cambodia in terms of the Khmer Rouge. I had already had a dislike of Noam Chomsky because I felt he was quite willing to keep on having his own government contract, Defense Department contracts, helping him supposedly with his linguistic studies. I felt tremendous disillusionment with anyone who would support the administration. I mean he... for giving objective and statistical arguments about why there's only one million and not three million Cambodians who were killed. And this from one of the leading anti-war activists, at least among intellectuals. And so I had had very idealistic ideas and notions about stopping the war, then suddenly all of this happens. It was very difficult to deal with.

Catt:

Were you ever approached to join the Institute for Defense Analyses, or it's Jason Group?

Goldhaber:

No, because I think my politics, I mean I don't know if this was the only reason I wasn't approached, but I think my politics were too clear before that. I made it very clear to Sidney Drell who would have been the person who would have invited me. I was utterly opposed to Jason and what it stood for. He used to go off to Jason in the summers, and I thought it was a terrible place for a scientist.

Catt:

Did you ever refuse to take a contract or to do scientific research for the government over religious or moral grounds?

Goldhaber:

I never actually refused it. Well, only once did I refuse a contract which was actually very clearly offered to me, but I certainly refused to go after a huge number of them. The one that I did refuse came at a time when I was really very much out of money.

Catt:

When was this?

Goldhaber:

I had moved to Washington; this was in 1981. And that was one was connected with the NSA, and, of course, there was no way I could do that.

Catt:

Did you know Curtis Powell? He was one of the New York City Panther 21, he was a biochemist, and was a friend of SESPA-members Jon Beckwith and Herb Fox and...

Goldhaber:

I don't think I actually knew him. I had heard about it though.

Catt:

Did you ever attend any of the week-long summer retreats organized by Boston and New York SESPA? I believe they were held in Voluntown, Connecticut, and were called "Insaners?"

Goldhaber:

I never went.

Catt:

Was there ever an attempt to have a national meeting of SESPA?

Goldhaber:

Yes. We had several national meetings as I recall. I never went to any, well, except the ones that took place at the annual AAAS or APS meetings. The annual AAAS meetings, especially, we considered our "national" meeting because it would be the only time each year when members of all the various SESPA chapters would gather in one place. And since a lot of the new members of SESPA were not physicists, I mean especially in the early 1970s, it really didn't make any sense to have a national meeting at the APS. As you probably know, the AAAS was designed to accommodate political discussion among scientists, or so we believed, and so it became the natural site for our national meetings. I believe there was a real national meeting of SESPA later on the 1970s, but I was already out of the organization by then.

Catt:

What about your involvement in those?

Goldhaber:

Well, I was quite involved in those. I remember once I went to Washington [Comment added on proofed transcription: "This was at an APS meeting."] and I happened to get by chance the only room they had available which was a huge suite which they gave me for the price of a single room. So we had essentially all night meetings continually in my room, 26 or 30 people strategizing, planning and doing whatever else to organize our activities. That was a very energizing experience, I must say, being involved in those. It was very, very exhausting, however, and one got very little sleep.

Catt:

I take it you were a witness to some of the SESPAs tactics of confrontation where someone would be talking and people in the audience would stand up and heckle or try to shout the speaker down, or...well, in one case it got to the point where...and I believe this was in Madison...where the wife of this speaker was so upset at the SESPAs protesters, who were there heckling her husband, that she took out a knitting needle and stabbed one of them?

Goldhaber:

Actually I myself always seemed to take a very...I mean even though I felt a great deal of anger and so forth, I think I took a basically polite view of things. I'm sure there was a certain amount of booing and heckling, but I was never actually present at a major confrontation. I'm totally repulsed by the idea of forcing somebody to leave the stage or something like that. I mean certainly there were people who had a lot of prevail, as I say in the case of Ed David, but there was certainly a lot of opposition to him. On the other hand, a deal was made for him and it was peacefully addressed. I don't think...for instance, I wasn't on any large campus very much at the time except the University of Arizona where the radicals were such a minority we would have been the ones heckled mostly. Rockefeller University was just too small for that sort of thing basically. I mean we did heckle in a very polite way, but it was sort of understood. We were going to be polite about this, and when we went on strike we didn't really shout people down. And at the APS I know we never shouted anybody down. But I know things like that happened. They were very much on the periphery, however. I mean it certainly happened while I was at Stanford let's say, Hubert Humphrey was confronted and so forth. But I as a rule never went to such confrontational meetings.

Catt:

Do you remember the last SESPAs meeting you attended?

Goldhaber:

Well, it was after I was in Berkeley, post 1973, and it was run by Charlie. We would have very small meetings as a rule. In fact... or while I was trying to do art we would meet in my store front but there would only about 4 or 5 people who would show up, which I felt was the ideal size for a meeting. This was in 1974 as I recall.

Catt:

You said you knew Joe Shapiro?

Goldhaber:

Yes. Well, he was one of the people who got involved in Science for the People in New York,

and we met while starting the action against the Riverside Research Institute near Columbia. We were quite friendly. And he is a great deal older than me, and whenever I returned to New York I often visited him in his apartment. He was in a certain sense more radical I think than I politically. I would say he has kept doing things politically he feels are important.

Catt:

How about Maurice Bazin?

Goldhaber:

Maurice Bazin, yes. We met at the APS meeting in Washington. And we became good friends and we're very good friends right now. Somehow he went off all over the place and I went all over the place but we kept meeting up somewhere. I remember once he came and stayed a few nights at my store front in Berkeley and then he showed up in Washington with a new wife. Then he came here with another new wife. And now he has one in Brazil. He leads a complicated life. But he also shifted, as I said, he was older than I, I guess he's 61 or so. And he's one of the older people who most significantly also shifted. And as I said he went to Chile, to Brazil. He is certainly more to the left than I, for example. He's a strong supporter of Castro, I would say. I mean, I certainly don't approve of the embargo against Cuba, but I am very disillusioned by Castro.

Catt:

How about Pierre Noyes and his lawsuit, which took the stance of the 1946 Nuremberg War Crimes Tribunal?

Goldhaber:

Well, you know, that was very interesting. While I was a graduate student I didn't really have much sense of him most of the time nor he of me, I guess. And then later we became fairly friendly. When I first got involved in Science for the People he was fairly active in it. And I also had a hard time because I felt like he was doing science that had gone off the deep end and... well, this is fairly confidential and I don't know if I want you tell him that. He tried to explain it to me but I really didn't understand what he was doing, and I had a lot of doubts about it. I didn't really follow the details of the lawsuit. I knew about it, certainly. It was a very interesting strategy, I must say. Along this same line, I remember being involved with Charlie [Schwartz] and his opposition to...I guess it had only vaguely to do with science... it was opposing Nelson Rockefeller's nomination for Vice President. This was in 1974, Gerald Ford had just become President, and he wanted Nelson Rockefeller for his Vice President. Well, I had all this information on the Rockefellers' because I had been at "Rock Tech," as we called it, and...

Catt:

I'm sorry, what did you call it?

Goldhaber:

"Rock Tech." or "The Rock." But anyway, we had a lot of doubts about Rockefeller. Well, you remember what I said previously about him and all of his connections to corporations. I mean I guess it did have something to do with Rockefeller's position on nuclear weapons and his association with Kissinger, for instance, but this was only very tangentially related to science.

Well, I must say before I ever got involved in SESPA, I was interested in one particular lawsuit that never got very far. In 1968, I had the idea of suing the government to declare the draft unconstitutional. I remember I went and talked to some lawyers at the National Lawyers Guild.

Catt:

Was this in Stanford?

Goldhaber:

It was San Francisco. And they assured me that it couldn't be done and that there was nothing to be gained in pursuing such a suit. But that was the only one I really got very involved in. I was very interested, however, in the issue of war crimes in Vietnam. And I passed around copies of Telford Taylor's book and remember convincing Jeremy Stone, he was good friend's with Henry Kissinger, that...

Catt:

Jeremy Stone?

Goldhaber:

He was the head of the FAS and a son of I. F. Stone, in case you know who that is. He doesn't like to mention it but...

Catt:

For the record, I. F. Stone is...?

Goldhaber:

I. F. Stone was a radical who for a long time ran a weekly, which I used to subscribe to in the '60s, and it was pretty radical; it was called I. F. Stone's Weekly. It basically took apart what the government said on the important issues of the day and offered a more radical interpretation of those same issues. I think he had been fired from the large newspaper where he had worked for years, and he just put out this weekly independently for many years. He was a much revered figure on the Left for a generation or more. Sadly, he died a few years ago. Anyway, Jeremy Stone was a mathematician who became the head of the FAS and was much more establishment than his father, or he tried to be. But nonetheless, I remember one day he was speaking at a debate at the APS, and it related to Vietnam, and I...

Catt:

Do you know when this was?

Goldhaber:

This was in 1972. And I remember I raised my hand and said, "Henry Kissinger is a war criminal." And Jeremy [Stone] said, "That's impossible, I just had lunch with him." And on the basis of this strange conversation I handed him Telford Taylor's book on the Nuremberg war crime trials and Vietnam. Telford Taylor, you know, had been one of the judges or the prosecutor at Nuremberg. Anyway, Jeremy turned around on that subject and admitted that there were war crimes in Vietnam and that Kissinger was involved. And, of course, Jeremy didn't want that concept spread too far, but it was a big concern of mine at the time.

Catt:

I know at Berkeley they had staged a few mock “war crimes” trials. Charlie was very involved in those. The next-to-last thing I’d like to ask you is, who was your closest confidant during this period?

Goldhaber:

Well, I suppose it would change from time to time, but at certain points it was certainly Charlie [Schwartz] and other times it might have been Joe Shapiro, and certainly when I was in New York it was Rod Wallace.

Catt:

Rod Wallace?

Goldhaber:

He’s one of the people on your list. He was a student of Charlie’s who went to be a graduate student at Columbia. He took over the sort of day-to-day organization of New York SESP. He was always a very pragmatic person. He never believed, you know, that Vietnam was anything more than a state. I mean, he saw it that way. He always said that Vietnam was just endemic of the corruption, of the decay in our political system. But in other ways he was very militant. I don’t know what became of him. I’m trying to think if there’s anyone else who really had an influence on me.

Catt:

Bill Davidon?

Goldhaber:

No, not really. I know him, and I know about him. But I think Charlie for the longest time...I would say he had the most influence on my thinking. I lost touch a little bit over the past few years with Charlie, and I guess it gets harder and harder to penetrate his shell concerning his ideas and so forth. Well, in some ways Maurice Bazin. I mean I’m not quite sure what his exact involvement with SESP was, but certainly right now I would say we’ve become very close.

Catt:

Okay. Is there anything on this topic that I’m looking at which we haven’t covered that you’d like to bring up at this point?

Goldhaber:

Well, I think it’s a very interesting topic. I feel as though it’s such a large part of my life, and I assume it probably is of other people too, that it’s going to be very hard to grab hold of. I certainly wish you a lot of luck on that. I think you’ve raised some very interesting questions and I’m glad I had a chance to try to answer them because it has stimulated my thinking. I do want to leave you with the feeling, that I’m sure other people have said too and I’ve already certainly intimated at, that the one reason we kept going, kept doing what we did, was that there was just a great deal of excitement and pleasure in doing it. And it just developed like...it did feel like science, in the sense that it felt like discovery. It felt like we were going into a new era, into a new area, which was really unknown to all of us in terms of what the interaction

between science and society is, and what there is to think about. And so in some ways it had part of the excitement of science itself. The excitement of being engaged in discovery, coming to the sense of those ideas. And then, of course, obviously there was the tremendous appeal of being the radical, of “épater les bourgeois” or something of that sort. We were rocking the boat and so there was a lot of pleasure in it. And it certainly wasn’t just about our morals, but it also had the other side, of feeling the tremendous drudgery involved in radical activism, which I guess is also not so alien to science. You know, getting up at dawn and standing there handing out petitions is sort of...well, let me put it this way, the drudgery of political work is somewhat like science but it also is part of the “traditional standing in front of the factory gates” that seemed to connect us to an old, hard, and yet very glamorous past.