Oral History Transcript — Dr. Dan Bolef

Interview with Dr. Dan Bolef
By Patrick Catt
At Irwin, Pennsylvania, Sewickley Township
February 13, 1997

Transcript

Session I | Session II

Catt:
This is the continued interview with Professor Dan Bolef. It is February 13, 1997. Your involvement with SESPA and Science for the People, when did you first...I don't want to say join SESPA, but started taking the newsletter or the journal or at least becoming aware there was a group out there of scientists.

Bolef:
When did I first what?

Catt:
When did you? In 1969, February, if we could start off with that, were you at the New York City annual meeting of the APS, where SS scientist of Social and Political action was established.

Bolef:
No, and one reason for that is that I'm a solid state physicists so I usually went to the March meetings. Sometimes I went to the annual meetings in Washington they are usually held in Washington, as you know. But I wasn't at the one in New York. But I became acquainted with SESMAN very quickly and subscribed and had their back issues of their magazine, and I use it in courses and talks that I gave. And one of my students, I'm sorry I forgot his name, ended up being one of his editors of Science for the People. I found them helpful, but perhaps not quite on the same wavelength as I was on. I agreed with much of their politics except for the evaluation of China and the book Science Walks on Two Legs, which you are very familiar with. I haven't come across a comment of mine. Do you mind if I read a few sentences? I'm commenting on Chapter 7 of Science Walks on Two Legs. This was in a study group I think. "We are not well prepared for this chapter." Why after the almost unqualified picture of a rosy fairytale of happy collective life, no classes (that means social classes) and everyone almost striking for the good of all (underline), all the people with the correct line always propounded by the revolutionary committee in charge (which is what the first six chapters give you) should there be mental break downs in China (because Chapter 7 deals with mental health). I go on from there. This points out my long-standing feeling that the group that they are responsible for SESPA was gullible. Extraordinarily gullible in many respects. For example, they accepted now and the great revolution as being the God send, what they were fighting for. It was bull, pardon the language. And we now know much of what appears in Science Walks on Two Legs was just complete fabrication and cover up of the real situation there. So I find a little bit of this in the organization and the magazine. Their political views with respect to Vietnam I agree with one hundred percent. But there really weren't as critical as science as a discipline, especially on basic research as I and perhaps several others like we were. And so I didn't usually go to science with people in magazines or profound understanding of where we scientists stood with respect to our contributions to the world.

Catt:
Was there anyone within the organization, obviously, that [???] [???], Charlie Schwarz for example, that you had correspondence with? Or you felt was on the same wave length as you?

Bolef:
Charlie Schwarz I felt was the most effective and outstanding [???] scientists of that era. I wrote to him occasionally, especially for material that he had and the copies of articles he had written. There was almost nothing that he did or wrote that I didn't feel was a profound contribution to the radical scientists that I supported. And therefore that contradicts what I was saying about science of the people, and I don't even remember how close he was to SESPA. Was he the guru?

Catt:
Well, he was one of the four that established the organization.

Bolef:

Okay, that surprises me a little, because I found many of the articles rather ethically and morally appropriate, but not nearly as critical of scientists as I was at the time. For example, when I sent in my article, which you have, they accepted it then refused to publish it. The refusal was childish — the reasons for their refusal. It was childish from this point of view. They wrote back say they were not publishing it because it was a product of one person, one professor, and was not a group effort and it did not reflect the views of a community of people who had brought it out. They were one hundred percent wrong.

Catt:
A one collective of one in this case.

Bolef:
No, no, I was the collective. The Committee Support Resistance was a much more community-oriented group, probably the SESPA was. Very rarely did we do anything unless we consulted as a group. And that article actually reflects it. So I just I used the magazines [inaudible] that will both say that was the thesis of my thinking, I would say that Andre Dorse(?) was closer to being that person than SESPA.

Catt:
Did you ever attend any of those Science for the People, SESPA meetings?

Bolef:
No.

Catt:
I was given the name Stuart Lederman. Does that ring a bell?

Bolef:
Yeah. Lederman.

Catt:
He was listed as the first contact of the magazine.

Bolef:
Pat, I was a strange activist who stayed at home. For example, The Community Support Resistance received support from Nome and from Resist, and we interacted with them —we corresponded with them, we wrote them. And we attended there yearly meetings when they sort of went over things that they wanted to have a report of what we were doing. I never went to one of those meetings, never went to Cambridge. Steve Graham went. He went about four or five time to represent us. I liked that. He was younger than I, he wanted to go. I didn't do that, I stayed at home and did work. And in fact we had one of our finest compliments from Charles because he asked their advice on what activities were best to resist the war in Vietnam, and they had read our report that I had written to them and interacted with him on several circumstances. He said, "Steve, we can't tell you. We've read your report. You tell us because you know much more than we do, and it sounded better." I always looked at that as a compliment. He was talking about the draft that he gave the Council. This shows how close we kept in touch with the efforts of the groups out there. They actually read what we sent in.

[inaudible passage C garbled] and most consistent in his understanding of the political military social situation from the late '60s and '70s, and there is nothing anybody can say to me about Noam Chomsky that would affect my regard for him. There are dozens of documents to prove that Dan Bolef gladly asked and received money from DOD for research. It's true that at some point, mainly 1970, I publicly refuted it and never again received, it except for funding from the DOD. At the same time in many writings and talks I gave after 1970, I was likely to point out that my receiving money from the National Science Foundation, which I am still doing, was not so much different than receiving money from DOD. If you, if a historian of science takes the trouble someday to write a book on what were the applications over the long run of research supported by DOD versus research supported by the National Science Foundation with respect to military applications, I bet you might be surprised, maybe not you but others would be surprised at the fact there was very little difference. Because one doesn't know as you well know what the result of applications of basic research would be. So this distinction is almost trivial in my mind excepting government, money from the government that spends a large part of its budget on military weapons of mass destruction. It's pretty much the same no matter what agency you accept it from. With respect to Noam I can believe that there are hundreds of people out there including some radical scientists who don't know the facts, in fact I don't, of this accepting money from DOD. Because as I said in the very first sentence, anybody could say Bolef was a fraud. He accepted money from the DOD. I'd like to hear Noam's side of it.

Catt:

Which that was one question I was going to follow up with, we can address it now. There is the article around the March 4, 1969 where you say you support MIT and its desire to get rid of DOD, military research on campus, the Lincoln instrument lab is what they are after. And in this article which was in the St. Louis Post says that, "Oh incidentally, Professor Bolef, who has Air Force money starting in 1965, running five years…" In some ways it was very accusative, it was very pejorative article. It was pointing the finger saying well, here someone saying that DOD money shouldn't be on campus at the same time he is taking DOD money on campus. This was making you out to be a hypocrite. And in March and June you had written that you had taken that in '65, by that time it was 1969 and you said you were not going to take any more. Obviously you couldn't stop the grant and the way you just portrayed your sentiments about NSF and Air Force. I mean it's still l the federal government and it's hard to differentiate between the two, but in 1969 people didn't really make that. I mean they did separate it's okay to take money from the NSF, but boy, you don't want to take that DOD money.

Bolef:

Well I consider that the somewhat of a scam. It took me a long time to decide that. And these are complicated [?] human beings have that for anybody to accuse me of being inconsistent and no longer ruffled my feelings. Because I was learning, and still learning, we scientists are so programmed to believe that science is good for human kind, that the results of our research in the long run can only help people now in the world and can only result in good and a new learning in itself is good, new knowledge in itself is good. And it took me a long, long time to realize that was B.S. That depending on the cultural or political situation in which we find ourselves, scientists are much more likely in the twentieth century to be for killing then for living. And so it took me a life time to learn that. And I don't apologize for having gone through the stages of kindergarten on up and my understanding of the implications of science. And I would never if I had known today but

Catt:
Do you think perhaps — I'm trying to and your sentiments on the Science Walks on Two Legs, that this was one of the reasons why scientists were really looking at third world countries in the emergence of science education? I know Gar goes to Cuba and comes back, on your Vitae you have reference Gar Allen who was with the Venceremos Brigade along with Marty Lebowitz.

Bolef:
That's right. Oh I liked that, Vince really.

Catt:
Vince, I'm sorry. That this you see people Art Osgood and Ethan Signer and other radical scientist go to third world countries, North Vietnam, China, Cuba and coming back saying here's a place where science is not treated differently outside the political order that it is here. It's not separate, it's integrated, and therefore it is science for the people (if I many use the slogan for a second) that Scientists are in tune with what is going on in society, they know that what they are doing inside the lab is going to have application, it is going to have benefits, and possibly it is going to have ramifications that it can be negative on some people, yet they are aware of this and they are trying to interact. Whether it is the United States, it's being funded by the military for the most part. Let's just deal with physics for example which is the big recipient of the largest and most physicists could care less what was going with the fruits and labor. So it was a way that they educated themselves, and in doing so, perhaps they were giving up and say on American science and okay, maybe the system is too entrenched. Or am I wrong?

Bolef:
I like, I like your approach. For example, you jumped way ahead off from back to what you were saying, you jumped way ahead. The physicist I admire most, the physicist alive today that I admire most is Max Born I admire completely. But the physicists I admire most is a woman named Danvada Shiva. She is the most stunning scientist in her understanding of the uses of science, applications of science, and the meaning of science to third world people. I think she was actually from India. But I don't think that there is anyone who has reached her understanding of the nature and uses of science today. She has written a number of books and many articles. I can appreciate the people who wrote Science Walks On Two Legs. I can appreciate those who thought that the Soviet Union might be the great beautiful society of the future. They were wrong, and I'm sure many of them became disillusioned. Only the stubborn ones would not admit that now we're installing [?] them as mass murders. And it's a shame. Because there weren't many people, and many scientist I'm sure, who thought that they could build a society and science refused to help the ordinary person and that destroyed people. And I'm sure that the writers of Science Walks On Two Legs meant some of those beautiful idealistic people that had been unfortunately in the long run used by the aggressive, paranoid, power hungry, greedy men like Mao (Tse-tung), demented men like ??? and others back then. Cuba may be the exception. You can't point at Cuba as having committed genocide under the group of people, certainly not their own people. You know point the fingers and say a role ??? ??? ???. But I went through many enthusiasms, but certainly didn't in 1976 this is written, by 1976 I did not think that China was an example of science of the people.

Catt:

Did you ever travel to the third world country like Cuba, go on one of the brigades down there?

Bolef:
No.

Catt:
If we broaden that to say, I hate to stereotype governments, but socialist/communist countries.

Bolef:
Oh, yes,

Catt:
I know you went to the Soviet Union.

Bolef:
I went to third world countries, yes.

Catt:
During this period.

Bolef:
People who did work similar to mine, I traveled to many except for a year's sabbatical in Europe during 1971, my travels were mainly associated with my work. Conferences and so on. The group that did work related into my area, which is a small area in the south of ???, were in Kazan in the Soviet Union in Leningrad, in Paris, in Geneva. Those were the main groups. Berlin. And I visited them. I spent time with those groups, but especially with the Soviet Union. I must have gone there three or four times. I spent quite a bit of time there, and for some reason, I walked without hindrance anywhere I wanted to go in Kazan, ??? on and camp once and said what are you doing here? And they kindly escorted me off, but they were very nice about it. I guess I'd go through a gate without knowing I was going through the gate. It was clear to me that the Soviet Union was a poor third world country, basically. And that far from being a threat to us, being a head of us with the missile gap and the bomb gap and the space gap and the APM gap and all the other gaps we invented, the poor Russians were impoverishing themselves by their government concentrated huge amounts of money on these physicists and nuclear arms effort and the nuclear power effort. It was a horror story to see. A little bit of that got across in my era, but more of it in reading the right book, the right articles and books. But I couldn't believe the poverty in the Soviet Union. And the suffering of the ordinary person.

Catt:
And also a scientist at this time, there is movement with the APS a lot of Russian physicists wanted to —

Bolef:
But they were privileged, they had boshes [?], country homes and so on that others didn't have. They took me to them. I became friends with a number of them. But just to give you an example in 1960 I stayed at the Rossiya Hotel.

Catt:
Can you spell that please?

Bolef:
R-O-S-S-I-Y-A Hotel in Corners by the fence. Beautiful. It is still there. And Russians weren't allowed in the hotel unless they worked there and then they had special badges. And I dressed very informally and walked the streets of Moscow with some of my friends. And one day I had come back and the guards, the doorman at the door, one of them started to stop me, what was this shabbily dressed person going into the Rossiya for. And the other guard took him by the shoulder and stopped him and went like this, and he told the other person to look at my shoe. He looked at my shoes and passed me through. I had western shoes. At that time, Russian men had just cardboard shoes, that's all they had, the women and children came first. If I hadn't had decent shoes I wouldn't have gotten in there. Russian men showed extreme poverty. ??? bureau. So they could tell just by looking at my shoes I was okay, that they could pass me through.

Catt:
Shoe gaps. To go back to activism, scientific societies with the American physical science, in '69 you have the ABM. That really becomes an issue for radical scientist, how much were you involve with that obviously the background in studying nuclear weapons.

Bolef:
Pat, I'm gone through my papers since you've come aboard and found literally hundreds of talks to classes that I did, and a number of them were that weren't ABM. Usually most of them after the period we are discussing, after '73. I spent a lot of time on ABM. For example, I worked very closely with Nuclear Freeze as one of the main speakers in St. Louis, but only in St. Louis. To answer your questions, I did not engage in any national group with respect to ABM, but I was very active in St. Louis and surrounding areas giving talks in St. Louis and Carolina, places in Illinois, in St. Louis and Missouri.

Catt:
Did you attend the spring '69 meeting in D.C. where Brian Schwartz had orchestrated a session on the technical aspects of the ABM. Obviously that was what he had in mind where they had George Raftin (?), Hans Bethe, and a few others pros and cons of the system have a debate and then turn in over to the audience. And afterwards SESPA had an informal ballet: do you agree with this, should we get rid of this. And then of course there is a whole ??? in favor of no, let's abandon this. Which causes some problems, because Nixon is the President, and one of the first things he does is oh, we are going to have this system.

Bolef:
I followed that closely and I read about it, and I talked to people who had been there but did not go myself, but well, of course that's my position. And I most admired Berthe. I think [inaudible], but I never read a paper of his that I agreed with.

Catt:
Fair enough.

Bolef:
I remember being at a FAS executive meeting in which we were discussing topics to address, and I mentioned I thought we should that would be worthwhile assessing the role of basic physics research in contributing to the war effort. And you can't believe how quickly I was jumped on and flattened and completely told off by several people that were there, leaders were Rathjens, I guess it was Herb York in '52 who essentially that was a stupid topic to bring up, and basic research was the best of all worlds, and nothing in basic research contributed to the military anyway. I was told off quickly.

Catt:
So I take it you never went to any of the Pugwash conferences.

Bolef:
I have read literature on it. No, I was a home boy.

Catt:
So what about, you said you went to solid state meetings. In '69 in the spring, the meeting was in Dallas.

Bolef:
I didn't go.

Catt:
You didn't go, okay. I won't ask you about that then. What about the forum. I think we talked about this, what, here's an entity trying to get a foothold within the establishment within physicists trying to say we should talk about social responsibility, we should talk about these issues, and at every turn the officers were saying no, as physicists we shouldn't be doing this, our job should be doing research, teaching and doing research, that's it that's all we do, we shouldn't be talking about social and political issues. And here is this group by Brian Schwartz, Marty Perl, and a few others saying no we should. How did you react, did you follow this?

Bolef:
I wasn't part of it. I supported it in any way I could. I encouraged people to support the idea and to become members after they came to a [inaudible].

Catt:
I won't say a division, but —

Bolef:
We can choose [???]. I've always belong to it, I still do. I think it was a marvelous thing to do. It certainly forced people to, scientists to think about the implications of what they are doing. It helped [???] were accepted, especially [???] the [inaudible passage]. No, from Arkansas, from Arkansas. Yes, I had to give a talk to the University of Arkansas, and gave a physics talk, first of all, on my research on that, and [???] invited me to be the [inaudible] physics students [???]. [Inaudible passage]. I respected [???]. I repeat that the [???] university [???] that I was never a radical scientist.

Catt:
At least on the national scene.

Bolef:
I was never a radical scientist. I was a scientist who was a radical, and I worked with community groups and all these talks and activities and worked with Gis. I got arrested for picketing in the Air Force [???] Center in St. Louis. I did those things, but I didn't get any national attention.

Catt:
I was going to ask you about the IEEE. You remember that? Again in the national meetings, groups like Committee for Social Responsibility in Engineering (CSRE) was organized the IEEE meetings and saying okay, let's talk about the issue. You see the same thing in the American Chemical Society and in the American Mathematical Society. All of these high societies to the time wrestling with the issue of social responsibility, and should we have institutional space where our scientists where our members can go and discuss these…an open forum.

Bolef:
I was all for that strongly, and I spoke for that, it's just that I wasn't a member, I was completely inconsistent, totally inconsistent on that. Because the IEEE held a national meeting in St. Louis in the early '70s, they asked me to — the division of Ultrasonics, Sonics and Ultrasonics, and asked me to organize the meeting in St. Louis at the Chase-Park Plaza Hotel. Who was the person in charge, I can't remember, the organizer or director…I was the chair person. And spent a lot of time on that and I did a good job, and none of it was radical. It was all done as part of the organization to making a good meeting for people in my field to attend. I don't pretend that I was consistent of my radical approaches to the science. I was chairman of the national league.

Catt:
I'd like to probe why.

Bolef:
I enjoyed my research. I got along well with the other people in the field. They respected me. You should read the book I showed on my seventh anniversary I received that [???] on my former students, disagreed with my radical point of view but we respected me as a researcher. And it's like living two lives. You give part of yourself to one life and part of yourself to another. That's what I was doing. It was only after I retired that I gave up the one completely, the physics I gave it up completely.

Catt:
What about this organization, Science for Viet Nam (SfVN). I have a couple of letters here. Well here's a letter from Naomi Culver and David Culver, I don't know if you know David Culver, who is an ecologist at the University of Chicago and at Northwestern, and his wife, saying that thank you for your inquiries, but we don't have anything to do with physical sciences right now and if we find out that we do write to you and then one a little bit later. This is both 1972, you had sent some books to them, what did you think, how did you — I'm just curious of your experiences with this organization —

Bolef:
They are a grassroots organization. They were related to communities rather than just to academics, and went into the community to educate people. I was a great admirer of Science for Viet North American. There were some of the students who worked with me who spent a great deal of time in Science for Viet Nam, mainly women. [Inaudible], and I helped them as much as I could. I think [???] is involved with them too. So even though it wasn't my main effort, I certainly supported them.

Catt:
And then did you ever go to meetings?

Bolef:
No, I had literature, but I didn't go to meetings. I think I did go to a couple of activists meetings, just to show you I took it out of St. Louis. I went to the great — the teach-institute. I guess Madison takes credit for starting the teach-ins, but my own impression was that Barry Commoner was the main force behind the teach-ins, and he was course in St. Louis. So I spent a lot of time at teach-ins. And we had crowds of people attend our teach-ins at Washington University. I was always one of the organizers and one of the speakers of these teach-ins. And when Barry went to Madison for a national meeting of the teach-in leaders, I went there with him. So the three or four of us came to St. Louis. Actually we were fairly active group and we helped set up national coordinating organization at Madison.

Catt:
Do you know when this was?

Bolef:
I would say 1972, '71 or '72. It was when we were bashing the Bundy brothers and the others set of brothers, who were they? Political scientists. Because they were the ones who were fronting the Kennedy administration. When did Kennedy get elected?

Catt:
Kennedy, '60.

Bolef:
When was he assassinated?

Catt:
'63.

Bolef:
Okay, this was in the sixties. Not the seventies, but the sixties. Definitely the sixties. We were taking on the Bundy [???]. But they were the ones who tried to justify our aggression in Vietnam to the people in the United States. Tried to expose them.

Catt:
Okay. I'm going to stop this just for a second. [inaudible]

Bolef:
In Holmes Lounge there was a mass of students, three or four hundred students, both undergraduate and graduate, come together to discuss how they could move the university to get rid of the ROTC. They would be very democratic discussion led by this tiny little graduate student named Terry Koche (?). And everybody would be free to speak up and give their opinion. But then they would always appoint a group of five or ten of the students to summarize, sit down and write up a proposal for what the informal orientation of anti-ROTC should do. Always at that meeting there was a very popular handsome young political scientist instructor named Michael Ledeen, L-E-D-E-E-N, who was very popular by the students and always would make suggestions for what they should do and how they should act to get rid of the ROTC. And this went on for months. And in later years, we were to learn from Michael Ledeen that he was an informer who told the administration everything that happened there. That's modest compared to his later career. He went on to become a functionary in the state department, I think. I'm not sure exactly what position he had. And he's written books and they are books written about him. And there have been near times front page articles about Michael Ledeen. Michael Ledeen was specialized in Italian political science and he was proficient in Italian. It turned out in the later years and in the late '70 or '80s, he was notorious, as politely you would call him a negotiator; in the New York Times article he was called an agent, an informer for the U.S. government, and responsible for much of our response to activities in [???] in Italy. So if you look him up, you'll find he became a rather prominent figure. This was the young man who came to those meetings.

Catt:
He had another name, Rosenthal?

Bolef:
Theodor Rosebury. One of your questions that I didn't answer properly earlier question you asked me was there any case of persecution, Theodor without an e at the end, Rosebury, R-O-S-E-B-U-R-Y. He is deceased. He was a biologist that somehow ended up in the dental school, teaching in the Washington University Dental School, which no longer exists, and a well-known professor. He spent twenty years or so doing biological warfare research for the U.S. Army. At the famous laboratory in Maryland.

Catt:
Fort Detrick.

Bolef:
He was one of the leading researchers at Fort Detrick. And came away completely disillusioned and spoke out and wrote a great deal about and against biological warfare. Never, however, reveal any of his secret material. And he was very popular with students and with faculty like myself. Because we learned a great deal from Theodor Rosebury. The wife of Rosebury is still alive and a very beautiful person. But the administration of course was embarrassed by him in some respect because of his— well, they were embarrassed by anybody who interfered with fondling of money from Washington D.C. and places that were associated with it to the faculty members of Washington University. Because grants are important, as you know. And Rosebury talks and articles actually ended up writing some very popular books, not specifically about biological warfare but biology in general after he left Washington University. And one day they

had a chance to get him, because the local right wing newspaper, which no longer exists and I forgot what it was called, in St. Louis had it in the headlines on the front page of the paper, how come —- because… We had met a friend at the St. Louis airport, and the reported from this particular newspaper knew who that friend was. That seemed to have been a little bit of a set up. The personal friend of Theodor Rosebury was an official of the US Communist Party. And Theodor Rosebury was hung on the front pages of this newspaper. That [???] the University administration at Washington University quietly and rather gently circulated the notion that perhaps Rosebury should be retired from his professorship at the University. And they thought this up at the Senate Council. I was still on the Senate Council, so I protested in another meeting with other faculty members for railroading another faculty member [inaudible] newspaper. And so we drew up petitions and made it public that Rosebury should be supported and not retired prematurely. The pressures on Rosebury were so great that he decided it wasn't worth the trouble. He was pushing 65 or so. And so he left. Of course, he received a fairly decent pension. But that was one way of getting rid of an embarrassing scientist. The effort like that was urged on them with respect to me, but it was never tried.

Catt:

Barry Commoner, just your impressions of him. In some cases he starts in the '60s and early '70s to take on these roles, going from being at the university level of having his name associated with [inaudible] to all of a sudden let's distance ourselves from him. And of course you mentioned about when the biology department wants to come together, zoologists and botanists, and of course he was a botanist, there were some problems there.

Bolef:

[Inaudible]. One of the best public figures in the United States. And one of the most congenial, gregarious, and friendly scientists that I've ever known. Egotistical, sure of himself. Smart. Always wanted to be in charge. So I went along with Barry almost 100% [???] because he was dividing [inaudible]. I contributed what I could. I contributed quite a bit to the magazine and gave talks to the various projects that we had. But Barry was just a [???] leader. One you could respect. He never, ever got involved deeply with [inaudible]. That's understandable. He had done so much with respect to the environment [inaudible sentences]. Talk about a person who was not only courageous but scheming. He knew how to organize groups of people to get things done. [Inaudible sentences] It was orchestrated from the medical school, because they detested Barry Commoner because of his approach to science was quite different from theirs, his environmental approach. [Inaudible sentence] There was actually a hearing on Barry Commoner. It came to the point where they were coaxed to get rid of him. At this public hearing he gave a presentation presenting his point of view, and he had organized other faculty members who were [???] members in order to [???] to support him. You know, they just didn't have the authority, their efforts to have the University to evict him from the University for being a loud mouth and a trouble maker. [Inaudible sentence] a group of us got together [inaudible]. [Garbled and indistinguishable portion of the tape]

Catt:

…and that who you are or where you are coming from given the context of the time when you go into do research that there is not impingement upon the results. What we take to be as a fact, we have to agree is consensual, we have to negotiate, you have to go to your peers. But somehow you can clear all the personalities and the locations and the variables out and say we agree that this is a phenomenon of nature you can be produced anywhere anytime. This is a

fact.

Bolef:

That's a really embarrassing question. Difficult question, because I visualize myself in midst of all this morass of political and confrontational activities talking with my graduate students and doing experiments myself. All of my graduate students did 9/10 of the work. And there is no question in my mind, absolutely no question that one can pretend that that's true, that you can isolate yourself from all the other emphasis that oh, yes, pretend that we were always successful in that pretense when we are in the laboratory or when the theoretician gets into a calculatory interest with his or her calculations, and we certainly can pretend we succeed in that. Easy to succeed in that. Whether it's just a pretense or whether it actually occurs, that isolation of your basic scientific work from the world around you, that's a very difficult question. Believe it or not, I'm going to say something that just contradicts so much of what I really think. It does succeed for many of us. You find yourself caught up by the intellectual challenge and the hands-on experimental challenge of finding out or resolving a question or proving a theory. And you believe why you are doing it that it is unconnected to all that stuff out there. Yes, you really do, and I did. Not for all of the time, but for a fraction of time that I was immersed in it. It is an intellectual event which enables you to think that it is not related to the real world. I have to say that I am confused by it, because I was able to do it and yet, if I step back and step back into my real world, I can say it wasn't isolated, but affected by all this other social economic and military stuff out there. Yet, I fell into it. I'm sorry I can't give you a clear answer.

Catt:

Some argue that science is in itself a political activity, who gets to play the game. How do you get into become a scientist is involved in okay, well you can go to universities, you have to get a Ph.D., let's say. Meanwhile some politics, but once you get out, how you become an academic scientist, or industrial scientist from what do you do to get funding, to do your research involves politics again, who becomes on your team. All that is fine and dandy, it's just part and parcel of what it is to be a scientist in today or in American society in American democracy. But when you get to the issue is the fruits of science, what do scientists do, they produce new knowledge. That's what they are after. Now how that knowledge if it is knowledge for application or for its own sake, is beside the point the fact that that knowledge in itself is a political activity, dealing again with negotiation. You come out with a paper of an experimental find, it's got to go through a peer review, once it gets published people are going to respond to it saying oh, I see Bolef has come up with this experiment, it's got this, I totally disagree, and this is why. And through a series of negotiations over a course of years, centuries, whatever, we finally come to a point of closure, that okay, we take it for a fact. But some people would say two hundred years from now or in physics, if you publish a paper today, five years later, people are going to come back and say well that's not a fact that was obviously a dead end. We're on the right track now. It's still going on so you can't say there are such things as facts from science. Because science, it if it is a corpus of knowledge they use knowledge not facts, it's always changing in that.

Bolef:

Oh, I like that. I like that way of saying, and you have said it very well. The only disagreement I have with it that summary is the subjective time restricted impression, conviction, okay, of

people who are actually doing the work at the time they are doing it. That this is purely an intellectual, self-motivated search for something — knowledge, understanding. I marvel that some dominates some carries you away, but you lose all feeling for or consideration of these other elements that you have described, these political social elements. I have to know what your politics are and it doesn't mean that your results as you point out the results can't be corrected later on, and that the whole manner in which your work is viewed will not change later on. But the fact that you are committed to a scientific approach and analytical search for truth — it is clear in my mind that many people, I was one of them, are so caught up in that very process they carry that you can convince yourself that there is such a thing as pure search for truth. And that you are doing it. I convinced myself at the time. In retrospect I feel that I wasn't being manipulated, I wasn't being told to go into a certain direction. I actually was doing it of my own volition. You or somebody else will have to tell me that I'm not.

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