

An Interview with
GRANGER MORGAN

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Conducted by Andrew Goldstein

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Abstract

Morgan, a program director in the Computer Impacts on Society Program at the National Science Foundation (NSF), reviews his education and early work with computers in an educational setting before describing his work at NSF. He discusses his interactions with Peter Lykos and Fred Weingarten and the difficulty of obtaining meaningful proposals in the Computer Impacts on Society Program.

GRANGER MORGAN INTERVIEW

DATE: 27 November 1990

INTERVIEWER: Andrew Goldstein

LOCATION: Pittsburgh, PA

GOLDSTEIN: Can I ask you to briefly describe your education and background and how you became involved with the National Science Foundation.

MORGAN: I did an undergraduate degree in physics at Harvard. I grew up in New England and I went through high school believing that the only interesting problems in the world were in the physical sciences. I got off to Harvard and discovered that I couldn't defend anything I knew on the social side, and rapidly discovered that there were all sorts of interesting things -problems in the social sciences. Harvard, as you know, doesn't have minors, but I did the equivalent of a minor in modern history, and at the end of graduation had some ambiguous feelings about exactly what I wanted to be doing with my life, but sort of by inertia got carried on into graduate school and the physical sciences. I had also taken a moderate number of engineering science courses also at Harvard.

I went on to Cornell to do originally a Ph.D. in astronomy and space science there, got started and concluded maybe I didn't want to get myself locked into a traditional research career. I had gotten, while at Harvard, very interested in problems of development and particular development issues in Latin America. I spent the summer of my junior year working in the Jicamarca Radar Observatory outside of Lima, Peru and so concluded maybe I had better try the other side of the tracks for a bit, and so arranged to do an experimental masters prior to changing fields. I did the first electron density profile measurements of the ionosphere using the Arecibo 1000 ft. radio telescope, and then left and went to UC Berkeley as a graduate student in modern Latin American history. I got the best grades in my life, and for a variety of reasons decided that it had been a mistake, that I really liked the sciences more than I had been willing to admit. As a Latin American area specialist I would be sort of tied to that area, but that my interests were, in fact, rather broader and they principally focused on the interface between science, technology and society. In those days there weren't doctoral programs like the one here in Engineering and Public Policy, I knew I had to have a Ph.D. in something. I looked around and figured I could get a Ph.D. in applied physics faster than I could get one in anything

else. My thesis advisor, Henry Booker, from Cornell had just left to go to UC San Diego to start a new department. I picked up and went there as one of the first two doctoral students knowing going in that I would have to set all the social interests aside and simply do experimental science long enough to do the Ph.D. but without the intention really of ever pursuing a career in experimental science. So I did. I did an experimental degree on a radio star scintillation problem.

As I was finishing up my degree, my wife had been going through parallel sorts of transitions. She did an undergraduate degree in math and computer science, switched to social anthropology. She was at that point a graduate student in social anthropology. This was the late 1960s. We were very concerned about issues of poverty, integration, social equality, and my wife and I had both sort of independently reached the conclusion that one of the biggest impediments to upward social mobility was cultural rather than simple technical knowledge, that to get a job as a receptionist requires not just that you're bright but that you be comfortable in the business world and your personal relations, and understand the majority culture comfortably. Those are hard skills to develop. So in looking around for high status jobs which involved a need for intelligence but perhaps not lots of cultural prerequisites, we concluded that computer programming and a number of related jobs held some of those potentials.

So, just sort of on a lark the summer of my final year as a doctoral student, I tried an experiment. There was a group of neighborhood youth corp kids on the campus. The neighborhood youth corp was I think in the Department of Labor - I'm not sure. It was a federal program to put inner city kids to work in the summer time. So there was a bunch of these kids on the campus. And I arranged to get them off for a few hours with pay to come and take an introductory course in programming, the basic concepts of computer organization and computer operation. I was an experimentalist. I had a dedicated computer there in the lab that was mine. In those days that was reasonably unusual. I had written a bunch of machine language, fast Fourier transform, and other signal processing programs for it, built a real time interface to it and so on. So I had a nice little system, and we used that -it supported FORTRAN - as I ran this course. Rather to my amazement and everybody else's it was a big success. The kids just sort of lit up and got very excited about it. My thesis advisor, Ken Bowles, who was in radio physics, had sort of gotten into computers through signal processing and was in the process of shifting into computer science as his new field. He

was running the computer center at UC San Diego at that time. And he wanted to have the center sort of focused outward a little more involving the community, and so encouraged me to pursue this interest a bit. So I went to work. I realize this is a much longer-winded answer than you wanted, but you're getting it. I went to work, trying to figure out how might I actually fund and pay for a program to do a pre-vocational motivational program for high school age kids, and actual vocational job training for young adults in the computer area. Over the course of some months I managed to talk Safeway out of a 40 foot trailer truck and Montgomery Wards out of a lot of things like paneling. We outfitted this as a mobile classroom. Xerox Data Systems, then was called SDS, gave us a bunch of nice logic circuitry, which we used to develop a bunch of logic labs. So we outfitted this thing as a mobile classroom. We developed a bilingual Spanish, English curriculum. We ran courses both pre-vocational, motivational and also full vocational courses all around the San Diego County, principally in the black ghettos and Hispanic barrios, for a year and a half, two years. And it was a sort of a hand to mouth existence. We got a bit of in-kind support from the university. I managed to talk to what was then Gulf General Atomics into hiring an ex-Marine, a black programmer, to function as our principal instructor. We got a number of undergraduate black and Chicano students in computer science as instructors, and managed ultimately to get both some Foundation and some state education funds to run the whole thing.

So, here I was, a couple years out from a Ph.D., with a Ph.D. in applied physics running a program in vocational education, and intent on a career on problems in the general area of technology and public policy. I decided maybe it was about time to get serious about my life and figure out how I could get there from here. When you're in a university, of course, teaching is one vehicle to sort of get launched on in many directions, so I arranged to offer an undergraduate course on a set of problems in technology and public policy, including a bunch of energy-related stuff, stuff on the air traffic control system, and a number of other things involving social impacts of computers. It was very successful. The department was quite pleased with it. It was the depth of the Reagan governorship in the state of California. Mr. Reagan was squeezing the University of California very hard. There were no new positions to be had in the system. So I had been given a visiting faculty appointment, and things went well and I was renewed and was given another visiting faculty appointment. The department made it clear they would like to figure out some way that I could stay on, but it was also clear that as long as Mr. Reagan was governor the University of California

would not grow. I finally decided, "Gee, I'm not going anywhere at all." So I decided it was time to look around and find something else. And about that time word came back to me through a faculty member at UC San Diego who I guess had had some contact with NSF and who had learned that there was an opening in what was then the Division of Math and Computer Science for somebody in the area of Computers and Society. In fact, Peter Lykos had been recruited to go put together some effort. John Pasta recruited him. I don't think John really had a clear sense of what the program was going to do, but the Peter somehow then managed to get authorization for a second person to come in under the Intergovernmental Personnel Act (IGPA) -- I think he actually created a bit of a flap at the time, because he insisted on advertising in *Science* or some general circulation magazine. I think at that point NSF wasn't into advertising for its positions; it does now. Anyway, I learned about it basically through that ad, which I think had been spotted by somebody else in the faculty. There was a round of interactions and they had developed an announcement for these positions. And you would have to check personnel records, or perhaps Fred can tell you, but then Fred Weingarten came in, I would guess on the order of six to nine months after I got there. I can actually remember that Peter was a little annoyed that Fred was sort of foisted on to him. But in fact, Fred made very substantial contributions once he got there so Peter didn't stay annoyed. So that's how I got there. Where do you want to go next?

GOLDSTEIN: So you were one of the original officers at the inception of the program?

MORGAN: Yes, I mean there was some bits and pieces of activity already that Peter had gotten going when I got there, and I really don't recall very much about what was already up and running. Peter had a good sort of general sense of what ought to be happening but relied heavily on Fred and me to flesh it out and develop some specific programmatic ideas. And I was digging around -- you may already have gotten this. This is a paper that mainly Fred and I wrote that was presented at ACM 1973 which describes the program. I came in initially as an assistant program director and then somewhere partway through the two years got promoted to program director. Peter left -- his term was up -- sometime during the first year, I think. But I could be wrong; don't rely on me for any of the times. I would guess that probably about that time I became a program director and Fred had come in as program director, so we each had a program. Together they constituted the Computer Impact on Society program. I ran Computer Impacts on

the Individual (At NSF he was "Fred" to everyone. Over at OTA, where he's been until very recently, he was "Rick" to everyone, so you've probably heard both names.) Fred ran the Computer Impact on Organizations program. From my point of view it was a very positive couple of years. First of all, a large amount of time in the early days was spent on sort of trying to figure out what we ought to be doing with program design. A series of workshops got funded I think through the Institute For the Future. We funded a number of other workshops as well. So the first year or so was heavily spent on trying to figure out what to do in program development and going out and sort of telling folks in the field about what we were up to - trying to get good ideas and get proposals. We had rather serious problems getting these proposals. I don't know how familiar you are with the general literature of Computer Impacts on Society, but it was then and it remains today a literature that's sort of 90% bullshit and 10% substance. It's got a real problem. So we as programs had a lot of difficulty finding good, substantive research. And it's my impression that that situation continues. After I had been gone for some years the programs basically dried up and Fred went to doing other things at NSF before he moved over to OTA. They might have dried up in any event, but certainly the fact that it was very difficult to find really first-rate research was a major contributor. You can state all sorts of interesting, neat problems in this field. However, the number, or the fraction which you can actually state in researchable terms and structure as something that you can actually do as a piece of research is much smaller. And so we had a fair amount of difficulty, despite the fact that we spent a lot of time out sort of actively soliciting, beating the bushes, trying to persuade and cajole, and get people to develop ideas in that sense. And we did develop a number of very nice programs. We supported a couple of nice projects in the early stages of computer-based conferencing. You can find [information] in the files. I did enough processing of proposals of jackets to get a very thorough sense of how the Foundation works. And in terms of my subsequent professional career that was very useful. But not so much what I was swamped by the administrative responsibility, but by the time I left I had sort of settled into a regular program officer's mode. But in the first year - year and a half - I was very much more in the intellectual, creative mode. In addition, it was a neat time to be at NSF, because this was the early days of the energy crisis. This course that I taught at UC San Diego had had a substantial element of energy-related things. And while I was at NSF I was able to find enough time to produce my IEEE press book in the energy area. And the Foundation had a bunch of committees underway to try to figure out how the Foundation as a whole ought to be responding to energy-related matters. So I was one of the handful of people in the Foundation who'd actually done quite a bit of

homework on energy-related matters before they got launched on this. And so I ended up serving either officially or unofficially on a number of these things. And that was very educational and productive from my point of view. I also, because I had these broader science, technology, society kinds of interests, was... I mean, once the people in NSF RANN - RANN was Resources Applied to the Nation's Needs, which was a program that was sort of probably two-thirds of the way through its life cycle at that point. The guys in RANN learned about my interests once I had been there a bit. So I got used fairly heavily there as well on the occasional site visit and on... various review capacities and other things. Both these energy activities and the NSF and the RANN activities were only sort of tangentially tied to computers. But the result was that I got, over the course of this couple of years, to, in a number of ways, sort of develop and expand my broad science and technology public policy knowledge.

Since we're still sort of in the personal history mode, let me get out of NSF into this place (Carnegie-Mellon), just for your own interest. By then, Mr. Reagan was no longer governor and there were positions that needed to be filled at the University at California. So I looked at two jobs. I looked at two jobs, incidentally, at the time of going to NSF. I had two offers. In addition to the NSF offer I had an offer from Charles Zraket at the MITRE Corporation to go there as a regular staff member and work on energy systems problems. And I had a hell of a time deciding which was the right way to go. This was a very tough decision - practically almost flipped a coin. When I was ready to leave NSF I was on a two-year intergovernmental personnel act position but John Pasta had made it clear to me that if I wanted to continue on as a program director that he would figure out some way that I could do that. I had sort of decided that I didn't really want to process paperwork much more, and that the job was going to entail more and more sort of normal program director's jobs, and that I was well up on the learning curve and it was time to go on and do something else. I basically wanted to go to an academic research position that would let me try to continue to do research in technology and public policy issues. As a result of all my energy-related stuff, I had begun to know Phil Palmedo and others at Brookhaven National Labs, and they were working hard on trying to persuade me that I ought to come to Brookhaven to join in their new group in energy policy research. But I really wanted to go to a university, and by then the University of California had managed to develop a new position. And in addition, this place (Carnegie Mellon) had an opening for a joint appointment between Electrical Engineering and Engineering and Public Policy. I looked at both of them, and it was clear that, professionally, Carnegie-Mellon was far superior to UC San Diego, but

we had loved San Diego, and had gotten to know it in ways we never will get to know other cities. Both our kids were born there, and so, I, in particular - probably more than my wife - wanted to go back there. I'll spare you the details since they're irrelevant to your interests, but basically, things broke down. UCSD wanted me if I was willing to do 80% engineering and science and 20% policy. I wanted the reverse ratio, so I said no. I had declined Carnegie Mellon and gone off to Brookhaven on a visiting appointment, but the word got back to Carnegie Mellon that I was on the market and they should try again. So I came back and have been here ever since. Excuse that meandering diversion.

GOLDSTEIN: You mentioned some of the early work on one of the energy projects. Were you recruited by NSF because of your work in energy?

MORGAN: I don't think that's what made me desirable to NSF. Once I got to NSF and knew quite a lot about energy there were suddenly a lot of opportunities for me, but I am sure that that had nothing to do with Peter's interest in recruiting me to the job. It was that I was somebody who had fairly substantial basic computer science skills. I had taught an introductory couple of courses in computer science at UC San Diego. I had built real-time interfaces. So I had some fairly substantial applied skills. And I had strong public policy interests and some accomplishments in the sense that I had run a course and I had published a paper of the public policy variety. And so Peter was looking for somebody who was computer literate and who had policy skills and interests. And it was that that got me there. The other things that I talked about were things that sort of happened by happy accident once I was at NSF and these various opportunities presented themselves.

GOLDSTEIN: Was there anything in your particular background kind of skills and knowledge base directed the efforts of the CIS program? How were those objectives met?

MORGAN: The three of us, and particularly Fred and I did an awful lot of brainstorming together - wrote a lot of sort of neat pieces and decisions, orchestrated these various workshops. As I say, I think probably Peter already had the Institute for Future Planning starting by the time I got there. And so, you know, it was a wonderful education for me

- I mean, we were running all these workshops, which hauled in good people on various topics, which we had selected because we thought they fit the kinds of things we wanted to be doing, we would haul in a bunch of really neat people for a couple of days to sort of thrash through this and try to make sense out of it. And our job was to sit there and sort of digest it all and say, "Now, what out of all of this should we distill into trying to shape our program?" And so the program ideas basically emerged from interactions among the three of us, and from the ideas which spun out of these workshops, which we would then pick up and distill and go off and do more homework. So Fred and I spent quite a lot of time certainly more than the average program director today gets to spend and probably more than is normal under any circumstances, trying to basically structure and invent programs. And a lot of time was spent writing and criticizing each other's drafts or various things. I haven't got any of this paper anymore and I don't know how much of it would still reside in files at NSF. We have checked, but, as I say a year or two ago, probably actually even longer than that we threw out a couple of cubic yards of stuff. I have got a couple of files here that I have pulled out, but they don't mean much.

GOLDSTEIN: Can you recall any of the workshops or the participants who've been influential?

MORGAN: Well, I mean the series that Roy Amara ran was sort of the key set. And what you needed to do probably is go... We're talking about something that happened almost 20 years ago, and so you need to go and pull those files. You'll find, I suspect, fairly full descriptions, and I am sure that we wrote various summaries think pieces that derive from them. But I'm not going to be able to produce much in the way of blow-by-blows.

GOLDSTEIN: This stage that you described to me earlier as intellectually stimulating, the impression you gave me at the earlier time was a period where you had substantial intellectual interplay?

MORGAN: By the time I had left we were spending most of our time in running the day-to-day operations of our programs. Less time out beating the bushes. A fair number of people with interesting proposals were coming in across the transom. We ran, incidentally, with probably greater program director discretion than is often the case now at NSF. At least, many programs at NSF now whose panel reviews and don't rely as much probably on the

independent professional judgement of program directors as I think the division of math and computer science, or at least the Office of Computer Activities did in those days. So that even once we got into the sort of turning the crank as a normal program mode there were interesting professional judgements required. But yes, the first year, year and a half, while we were sort of fitting the pieces together and figuring out what we were going to do, was certainly the most exciting, as were these various non-Office of Computing Activity functions that I found myself drawn into elsewhere across the profession. Well, I mean, the two that I described - the stuff that NSF RANN and the energy-related stuff.

GOLDSTEIN: Were you satisfied that this sort of activity had stimulated good proposals?

MORGAN: No, never really very satisfied. I guess I would say, really, in retrospect, a handful of good proposals. But, and as you'll see when you look in there and if you will look through the jackets a lot of the early stuff is sort of workshops and idea-generated. But it was very hard to stimulate proposals that from the point of view of the physical sciences or good social science work that involved good experimental design.

GOLDSTEIN: Research design?

MORGAN: Yes, lots of sort of shoveling words - gobbygook types. It's probably not a big surprise, given that that's the way a very large portion of work in this field is and always has been. But it was frustrating.

GOLDSTEIN: What sort of researchers did you get when you beat the bushes? Who did you regard as the most apprehensive? Did you work with a computer scientist, social scientist?

MORGAN: Well, it was an interesting mixture. I mean, the job brought me in contact with a remarkably wide variety of people. Yes, there were some folks in computer science who had developed interests in the social impact side. For example, we did a number of things in the security privacy area, and some of those people were mainline computer scientists who had gotten interested in some of those other policy dimensions of these issues. Anita Jones at

Carnegie Mellon, Lans Hoffman at UC Berkeley, Willis Ware at the Rand Corporation. But then there were also those who were basically social scientists of one sort or another, had developed computer-related interests. We funded several survey research kinds of things, to try to get some sense of public perceptions: computers, information technology. So it was a fairly wide set. The security privacy stuff was clearly some of the highest quality work, because it was closest to being like real computer science, and so it was easier to propose reasonable things. That work was fine. The computer-based conferencing stuff was fairly high quality. There were some man-machine interface things that weren't too bad. But generally it was very hard to get proposals that had good treatment of the social science part of a lot of these issues. And I mean, if you look at the field today, I mean, look at the social history of the telephone. There must be several tens of shelf feet of writing on the subject, and there probably aren't more than four to six inches of shelf space of reasonable stuff that's written on them. Well, the volumes are larger, but the ratio is about the same with the social impact of computers.

GOLDSTEIN: It's interesting with the man-machine interface that this was the period where computer graphics were beginning to explode.

MORGAN: Well, there was a bunch of activity around the office, and I can remember looking at early Sutherland proposals. I guess he was at Utah in those days. But I can't tell you whether it was our program that was funding that stuff or if it was another program. I got used fairly regularly by several other [programs]. It was a small office. We all talked to each other. If anything looked sort of like it might have interest to our program I would tend to know about it. I don't think our program was supporting Doug Engelbart at Stanford, but certainly that's another set of activities in man-machine interaction that were going on but not that it was NSF money going there. As I say, we are talking about details from a long time back.

GOLDSTEIN: Well, then, just the general environment of the NSF office. You say there was communication between different programs?

MORGAN: Yes, it was a peculiar office in the sense that unlike any office that I have been in before and unlike the

offices that I have been in since. Most of the offices I have been in I have had control, so I could shape them. This was a group that didn't socialize. With the exception of Fred Weingarten and maybe once or twice Peter Lykos, virtually no interaction outside the office. Now, I suppose that's the norm across most of America, but it had never been the norm in any place I had been before, and I remember being rather surprised by it when I got there, and it has certainly not been the norm any place I have been since. Nevertheless, within the office I remember it as friendly - certainly no vicious office politics or anything like that. Pasta was accessible. I can recall a number of occasions after hours when he'd drop by or I'd bump into him. I remember, for example, one extended conversation with him about the limits of predictability that was sort of a hallway conversation I guess held roughly in my office door. I had a fair amount of interaction with Arthur Melmed, who was running a program on educational technology - the so-called Ticket system; a cable television-based educational delivery system that the MITRE Corporation had put together, and the University of Illinois' PLATO system. And Melmed--I don't know if you have talked to Arthur yet or if you will talk to him but Arthur had basically told him. (I presume that these records are mildly privileged.)

GOLDSTEIN: They're certainly not being distributed.

MORGAN: Well, Arthur told me that basically he wanted to fund the University of Illinois PLATO system in a major way and couldn't do it if it was the only thing to support it, and so basically drummed up the Ticket program at MITRE in order to have a second thing to support so there would be a couple of programs. Fred and he and I got along well and interacted quite a lot. I have had a fair number of interactions with Arthur over the years since then. Other interactions around the office were more limited because I didn't have that much to do with sort of a standard computer science program. I knew the three or four of the program officers reasonably well, and there was communication when proposals came in.

GOLDSTEIN: Cooperative communication?

MORGAN: Yes, I remember it as a friendly, positive sort of involvement. Another aspect of the Foundation, though, that I remember being very sort of dumbfounded by, the director, who at that point was Guy Stever, who incidentally

had been president here (at Carnegie Mellon) and had left this place. Stever was not real popular here when he left. He'd been running at a loss--in deficit, and the place was sort of in hard times. And when I got here I got suddenly a very different impression of Guy Stever than I had had when I was at NSF. But Stever was director, and he would run annual program reviews in which he would have each program develop fancy color viewgraph presentations in the briefing room, with rear projection on frosted glass screens and so on. The whole program staff would spend several days doing dress rehearsals of these things with assistant directors. And finally, Stever would come in and this thing would be run and there would be a few perfunctory questions. And then he would move on to something else, and I could remember thinking to myself, "Good lord, this isn't that big of an organization. If he really wants to know what's going on around here why doesn't he just sort of wander into people's offices occasionally and ask them, "What's going on?" Because I had the decided impression that if his principle vehicle for learning about the organization were these reviews plus up-the channel communications through the ranks that he wasn't going to have the foggiest notion of what was actually happening down in the trenches. Now, I don't have specific recollections at this stage, but I do recall thinking at the time that the director would have been better off if he had spent some more time out in the organization talking to people. In my own case, my dad had known Stever when he was a young man--I guess they carpooled together or something. But I never had a personal word with him. I had a number of nice associated interactions with senior people outside of the division, in some cases outside of the directorate that were not personal but sort of personal/professional in the sense that they didn't involve my actual work. They were sort of broader science policy questions. In this context I had a particularly enjoyable and productive set of interactions with Wayne Gruner.

TAPE 1/SIDE 2

MORGAN: This [looking at list in paper] was what I was actually supporting at that point, which would have been in the early stages, so you will see when you look through some examples. So there's a lot of workshops and things like that here, and in the later stages I think there was less of that. And then we go through and we sort of lay out an argument for the kinds of things we would like to be supporting programs. Now, we were writing this sort of stuff, and as I say, Fred and I basically wrote this. Peter was on the verge of leaving. We were putting out this sort of stuff

because we were basically saying, "Here's a bunch of neat problems and what we see as the issues in the world." We were hoping that people would hear these papers or read them and go away and get excited and put proposals together. And of course many people did, but what came in was not what I had hoped.

GOLDSTEIN: In terms of the results of the programs sponsored, can you point to any policy you may have advocated or anything?

MORGAN: I think we had some significant impact in the privacy area. For example the work done by Rein Turn and Willis Ware at Rand. We had some impacts in computer science. And we probably had impacts on getting several careers going. Rob Kling, for example, who is now on the faculty at UC Irvine has continued to work actively in this area. I suspect we supported some of his work. I was still basically a physical scientist, rapidly doing my homework on the social science side. So I can remember, for example, some head of corporate planning for some major bank, who was a participant in one of the Amara work shops, talking at great length about decision analysis, and the decision analytic formulation of problems. I'd never heard of this. So there was a lot of personal learning on things like that. In the sense that rotators and IGPA people are a two-way street - that is, in addition to their providing benefits to the Foundation, to have people know a lot about their fields come in, the possibility that at least under some circumstances there's a lot to be learned at NSF as a rotator. This is something which has always been the argument, the sort of party line at NSF. I am not sure that it's an argument that's advanced with as much vigor today as it once was. It probably actually could stand a bit of reinforcing. I have the impression that NSF has become a considerably more bureaucratic organization than when I was there. When I was there we had fairly serious staff problems. If anything, the problems have gotten worse.

GOLDSTEIN: What sort of staff problems and how have they gotten worse?

MORGAN: Working with GS-3, 4, or 5 secretaries can be a remarkable experience. They are often just not very good. Things weren't great when I left there, but I have the impression that at least in some of the parts of NSF I have dealt with lately this problem may have now gotten worse, particularly compounded with a sort of a more bureaucratic

approach to life generally. It was a bureaucracy when I was there but I don't think it was quite as bad as it is now. I recall on-going problems with staff quality.

GOLDSTEIN: Did this environment contribute to your decision to leave?

MORGAN: No, my decision to leave was much more fundamental. I wanted to get on with my university research, and I figured that I could afford to invest a couple of years. At that point I was in the mode of saying, "Gee, life goes on a long time. I will just do a couple more years." And that's true. Now I am at a point where I say "Gee, life sort of careens by at a pretty horrendous rate!"

GOLDSTEIN: I understand that you say it's difficult to find good policy-oriented research projects?

MORGAN: Well, there are a couple of reasons for that. First of all, it's a hard area - it's hard and it's messy and it's very hard to control stuff. If you state problems narrowly enough that they're researchable they're not very interesting problems. But the second reason is that with the exception of a handful of investigators, most of the social scientists and many of the computer scientists working in these areas were second-rate. And the best people were off working on problems in the core of their disciplines. Many of the people who were dabbling with these interesting problems at the interface were those who weren't good enough or were not really making major contributions in the mainstream. Now, there's nothing inherent about that. This department (Engineering and Public Policy) that I run is a department where we have a lot of first-rate people doing stuff at these interfaces, but even today it's extraordinarily hard to find and recruit those people. There weren't very many people of that sort. In those days, of course, there had not been on-going sources of support for people wanting to do research on these kinds of problems. There wasn't a large pool of good investigators out there.

GOLDSTEIN: Are there any that stand out? That you actively solicited?

MORGAN: Yes, we actively solicited quite a lot. I suspect actually that you'll do best if you look at the program

jackets I will have trouble producing specific stories. Fred and Peter and I were out on the road visiting people, talking with investigators, talking with people who would come into the office and sort of cajoling, encouraging and suggesting. It was not a passive, sit around and wait for the proposals to roll in. We were very much in the business of trying to stimulate good proposals.

GOLDSTEIN: How about the policy-oriented work that you did support? Was there any uniform political orientation of researchers? Or maybe among you as program reviewers, did you review as a group of a like mind ?

MORGAN: You're asking was there an ideological cast to the whole business?

Not a lot. Some of the investigators clearly were... particularly some of the investigators in the computer security and privacy area were clearly strongly concerned about civil libertarian kinds of issues. Fred Weingarten has had a very strong interest in civil liberty, privacy related questions, and when he got over to OTA worked hard on that, almost to the exclusion of a lot of other interesting policy problems in the information and computer technology area. I think there are lots of other interesting policy questions these days in telecommunications policy that have nothing to do with civil liberty associations.

I don't recall any sort of strong ideological perspective among most of the investigators. I guess the number were perhaps a bit on the more liberal side. But that's probably because those were the kind who got drawn out the sciences. Certainly as program officers we worked rather hard to avoid political or ideological orientation on the job. I mean, I can recall that being the source of Peter Lykos' frustration when Fred was brought in. I don't know the circumstances, but I think there was some sort of political intervention to help get Fred in, although he'd worked previously with Pasta. So Peter was a little annoyed because he thought that Fred was sort of pushed on him as a political consideration. I don't know how Fred got the job but once Fred was there he was a very sharp guy and didn't bring a strong political orientation.

GOLDSTEIN: Do you have anything else you would like to add?

MORGAN: I think you have extracted from me rather more than I thought had actually remembered about those years. No, I mean the other things that I could talk about would have more to do with broader issues at NSF. For example, Wayne Gruner was a physicist who was developing a congressional testimony for the Foundation, who subsequently went off to Japan and was the NSF Japan officer for a while, who is now retired, I am sure. I can recall that he and I had a number of very interesting discussions about sort of the delicate balance between packaging stuff for the Congress versus developing a research agenda which makes sense in terms of professional taste and professional criteria within the terms of the disciplines. It's a delicate balance where you really wanted to principally let the sciences themselves drive what it was you were setting as your research priorities. And you got, in a democracy, all sorts of pressures on Congress. The need to be able to sort of justify and package some of this stuff in a way that makes it essentially sound like it will have social pay-outs. I was heavily involved, both while I was there and for a while after I left in a series of grants that looked at research on environmental and ecological systems. I don't think there are other things I can talk about that would be terribly useful to your specific issues.

GOLDSTEIN: Thank you very much.

END OF INTERVIEW