

An Interview with  
ETTORE INFANTE  
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Conducted by William Aspray  
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Abstract

Infante describes his work at the National Science Foundation (NSF) related to mathematics and computer science. He discusses the separation of mathematics and computer science within NSF; various NSF computer science programs, predominantly the Coordinated Research Program and CSNet; the work of Kent Curtis; and NSF's interaction with other funding agencies, especially the Defense Advanced Research Projects Agency.

ETTORE INFANTE INTERVIEW

DATE: 14 November 1990

INTERVIEWER: William Aspray

LOCATION: Minneapolis, MN

ASPRAY: Let me begin by asking you if you would very briefly summarize your background, education, and career before you went to the Foundation.

INFANTE: I was an undergraduate student at the University of Texas. As an undergraduate, I studied both mathematics and engineering, obtaining two degrees. As a graduate student, in effect I studied what you could call applied mathematics. After I got my Ph.D. I stayed at Texas for two years. But then in 1964 I went to Brown in the division of applied mathematics where in effect I stayed through the ranks. I stayed there officially until 1984. The division of applied mathematics at Brown during the time I was there, except for the last few years, had computer science as part of its activities. I forget exactly but I think that computer science became an independent department at Brown in 1977... 1976, something like that. And it was only a couple of years later, maybe three years later, that I went the first time to the Foundation. I was in the National Science Foundation twice.

ASPRAY: I didn't know about that. I only knew about your latter term.

INFANTE: The first time I went to the National Science Foundation was 1979 or 1980 and I went to be the program director of the program in applied mathematics, which is part of the division of mathematical and computer sciences.

ASPRAY: What attracted you to the Foundation?

INFANTE: What attracted me to the Foundation? First of all, I had always been supported by the Foundation - the previous 10, 12, 14, 15 years at that point - probably at that point, yes, 16. Let me tell you a little bit of a story. At Brown I was part of a group which was called Lefschetz Center for Dynamical Systems. It was a group that had been founded by Solomon Lefschetz originally at RIAS, or the Research Institute for Advanced Studies in Baltimore, which was part of the Martin Company. And then at a certain point, frankly, the senior people associated with RIAS

felt extremely uncomfortable with Martin Marietta and Martin Marietta with them, and so they all left at once - something like a group of 20 people. And all of them went to Brown to the Division of Applied Mathematics except for one person. That person was Rudy Kalman who instead of going to Brown chose to go to Stanford.

This group, which was the group that I was supposed to join at RIAS, well, they called me over the summer saying, "Don't come to RIAS; meet us in Providence," which I had to look up on the map, needless to say." This group formed within the division of Applied Mathematics a fairly large group. I joined this group as, I don't know, I would call it a post doctoral. My title at the time was assistant professor (research). But then I became a permanent member of the faculty. Some time around 1973 or 1974 the director of the Center for Dynamical Systems, Joe La Salle, started getting sick. Maybe it was a little bit earlier than that.

Let's see, the center at the time had a very, very large externally funded budget - maybe in the neighborhood of a million dollars, which in those days was a lot of money - from all sorts of agencies: the National Science Foundation, the Army, the Navy, the Air Force, NASA. Joe started getting sick; first he had emphysema; then he had a series of disturbances; then he had a terrible bad back. The long and the short of it is that he didn't pay attention to the Center and especially to its financial resources. And that happened at a time also when those were the bad times in Washington; budgets were being cut right and left. Anyhow, one day the members of the Center woke up to the fact that there was quite a debt in all of the accounts, that the things were a little bit in shambles. I suppose, because I was the youngest full professor at the time, I was fingered and told, "You do it and straighten it out." And so at that particular point I became the associate director but also acting director, because Joe was always sick; he was never there. So, in effect, for a couple years I spent my time forging his signature.

ASPRAY: I see. [laugh]

INFANTE: Because frankly, Joe, I thought, had a great deal of the moving spirit of the organization. And I felt that simply what I was going to do was I was going to do my best. Well, it turned out it took me a couple of years to sort of straighten the mess out; it took quite a bit of work, but in two or three years I was done.

It turns out that sometime in 1976, 1977 a program director, whose name right now I cannot remember, in Applied Mathematics, who had been recruited from Los Alamos had made a real mess of the program in Applied Mathematics ending the year in something like half a million to a million dollars in debt out of a total of four million. The Applied Mathematics community was pretty upset. And the community, especially SIAM, I would say, played not insignificant role in this. And frankly, Peter Lax, I think, played a pretty strong role in this. They told a colleague of mine, Jim Greenburg, to leave Buffalo to go down. He did his very best and he pretty well solved the problems. But at the end of the year, as was typical in those circumstances, he really wanted to go back to his university. And so I was sort of recruited into going in to finishing up cleaning the mess. And I suppose that the reason I was recruited was because I had developed a reputation for having cleaned up the mess at Brown. I went down and I finished cleaning up the mess, and things went rather well. So at the end of the year I went back to Brown. I think it must have been in March, April, maybe even as late as May, everybody at the Foundation knew that I was going to go back to Brown. There was no doubt about it that although I managed to straighten out the Applied Mathematics program, which is one of probably 15 programs in the Division of Mathematical and Computer Science, the division as a whole was not held in high regard within the National Science Foundation. There was no doubt about the fact that John Pasta was dying of cancer. It was a Joe La Salle situation a little bit all over again.

One day Bill Klemperer, who was the assistant director of mathematical and physical sciences, a chemist from Harvard, barged into my office and he said, "Jim, I want you to stay here and I want you to become the deputy director to John Pasta?" "What for?" And it came out that he really felt that John needed some help. And he said to me at the time that he didn't have much faith in the people who were in number two command, either in the Computer Science section or the Applied Mathematics section, and the fact that he had liked what he had seen that I had done straightening out the Applied Math. I explored the possibility. It became quite clear that John Pasta just didn't want me. And John was to die a year later. Frankly, I wasn't very eager to stay. I wanted to go back to Brown to pick up what I had left. And frankly, there's no doubt about it that John Pasta was regarding me, had I accepted that position, in a very different way from the way that Joe did. Joe was really eager for my help. John really felt that, I don't know, that maybe I was there holding a candle at his funeral when he wasn't quite dead. I went back to Brown

and picked up what I had been doing. John Pasta died that year. It was something like June, if not July. But again, Klemperer called me up and said that... (Peter Lax was on the Science Board at the time) he said that he and Peter Lax had been discussing it very hard and they felt that they really wanted me to come to come back. And Peter twisted my arm a little bit. And so I said, "Fine, I'll come and do it for a couple years." It's a very strange story - especially if you continue the story. You see the mistakes that you make; you become an administrator; you straighten up the mess and then you're given another mess to straighten out. And you straighten that one. And then you wind by having Ken Keller calling you up, "I have a mess in the Institute of Technology," and then creating your own mess in the end.

ASPRAY: [laugh] So what did you find when you arrived the second time?

INFANTE: It was a rather complex situation, but I would say there are two sides of the house. One was mathematics and the other computer science.

ASPRAY: And really, for these purposes I am only interested in computer science.

INFANTE: Okay. Let's say the situation in the division was divided. Mathematics had its own particular problems. I think mathematics had really followed a set of policies over the previous 10, 12, 14 years and frankly, did not have the respect of the rest of the scientific community and the rest of the people at NSF. For example, they were not funding any graduate students. They had a very large number of grants - very, very small, tiny grants.

Computer science had not done this at all. On the side of computer science there is no doubt about it that computer science was suffering, as computer science was doing at all the universities, of being the youngest kid on the block, not having the scientific tradition and the elder statesmen that are so important in an organization like NSF. Lastly, that many of the people associated with the program in computer science were not highly regarded within the scientific, bureaucratic structure of NSF. It was almost identical to the situation that you would see at that particular time at the universities. I mean, the people who were computer scientists in senior positions were not computer

scientists. For example, I thought that the section head, Kent Curtis, of computer science was a very devoted individual, but he didn't have a Ph.D. He had never held an academic appointment. Although very astute in many ways, he hadn't developed some of the skills of some of the people in the other divisions, most notably physics and chemistry, and, for that matter, material science. Those people had a very large and powerful community sitting out there backing them up, which computer science didn't have. And that he had not developed the skills that I think people develop in certain academic circumstances.

Also, those were the go-go days of computer science, right? Boy, was it hard to get the people on the go-go to come and work in the National Science Foundation. So many of the staff members inside of the National Science Foundation, although there were some very good ones and very thoughtful ones, there were some that just weren't very good. As a matter of fact, one of the bad things that happened again was that there had been a major networking program started by the Foundation that indeed was what became CSNet and its evolution since. And I guess that Bill Klemperer (I wasn't at the Foundation at that particular time) had become sufficiently uncomfortable with it that he had taken a person from chemistry and he had forced him into computer science to manage the project. Again, it was a lack of confidence. I think in many cases this lack of confidence frankly was not justified, but nevertheless it was a fact.

The other problem that there was is that whereas the National Science Foundation always somehow - whether it be in chemistry, even in physics, I would say, and certainly, above all, in mathematics and in astronomy - really knew itself as being the primary force in the scientific establishment. In computer science there was no doubt about it they were playing second if not third fiddle to DARPA. So, anyhow, here was a division - mathematics and computer science - in which there was an enormous amount of intellectual respect for the component part of mathematics, and absolutely no respect on how they were conducting their business. The other component, which didn't have the traditions and reputations and scholarly history behind it, and again, it was felt that they were marching in the shadow of another agency. You put those two divisions together - that division was not competing well at all. And let me tell you, those chemists and those physicists were unbelievably aggressive. And they used everything available to them. I think it is rather interesting to see. You see, as long as John Pasta was feeling well and was doing well... John Pasta -

people had an enormous respect for him. But the minute that John started getting... And you see, John must have been sick for at least four years and really sick the last three... last two, at least. But very sick. I believe he died in 1979, I think. No, 1980. And during the period in which John was sick, it seems to me that again what was a less than felicitous situation became even a slightly worse one. Am I answering your questions?

ASPRAY: Yes, you are answering the questions very well. Did it make any sense to have mathematics and computing together in one division? Was there any interplay, interchange between them?

INFANTE: It was not an unreasonable arrangement. And remember, even today, statistics is with mathematics, right? Yes, there was some interplay.

ASPRAY: But you're telling me that they weren't particularly building on that relation in any major way.

INFANTE: No. No, indeed that was missing. And you know, one of the reasons I stayed a third year, which was not at all my plan, is because at that particular point, given the way the situation was evolving, I had recommended the two divisions be split. And they were split during the last year I was there.

ASPRAY: That was the thrust of this part of the discussion. I wanted to get at why you thought that was appropriate. Who else was part of this discussion and what the different views were on the split and the eventual formation of SIIS?

INFANTE: Let's see. There was an evolution. I think it must have been during the first year I was division director, Langenberg was the deputy director. At that particular point Bill Klemperer had gone back to Harvard, and it must have been the second year. One of the things that I tried to do very hard during the time I was at the National Science Foundation was to really get this division with these two wings to really try to climb up out of the holes in which they were, which to my mind was not justified. I did this by taking two opportunities that I saw coming and really working very hard on it. On the mathematical sciences side of the house was the David Report. There was an

interest in the part of some of the people in the National Academy of Sciences. There were some individuals working at it, and so I really threw an enormous amount of energy on that side. Indeed, I provided all the data. On the computer science side of the house I saw a great opportunity coming down the pike. The product was not on the computer science side of the house, but it was on the computational science side of the house. By putting together a number of activities it resulted in a Lax report. And I wrote most of the Lax report together with Don Austin. The Lax report was written on my desk as Peter Lax was sleeping. [laughter] Are you familiar with Peter?

ASPRAY: I have met him.

INFANTE: Well, you know, Peter is one of those people that has a slight touch of narcolepsy, right? And he's very funny, because you go to a lecture with Peter Lax and pretty soon he's asleep, and then at then at the end of the lecture he'll open his eyes again and he will ask the most intelligent question that is asked by the whole bunch, right? [laughter] I don't know how he does that.

But anyhow, I tried to sort of bring the thing up. My sense was that the sociology of the two disciplines had become sufficiently divergent that I felt that, although perhaps my intellectual viewpoint might not have been... You know, from my intellectual viewpoint you never wanted to split things, especially because once you decide to split things, where is the boundary, right, of the split? And no boundary ever makes sense. On the other hand, I thought the fact that the National Science Foundation is not an intellectual organization; it is a funding organization, right? And I felt that the time had come. And so, what I did is I proposed some sort of a splitting. The thing got a little bit mucked up in the process, because there were some people - in particular, Kent Curtis - that really wanted a directorate of computational sciences. You know, the Foundation has directorates, and there are the directorates and then the divisions underneath. And there was a reason for that. And Kent also had his own very good reasons. Again, you know, fundamentally, computer science... for that matter, also mathematics... It's very hard to live with a physicist and a chemist, right? The bench experimental scientist... they don't have the same outlook either of the mathematicians, whom I believe have more of an empathy with theoretical physicists, but not at all with experimental groups. Similarly, in the case of computer science it was a pretty strong feeling on the part of some people, especially Kent

Curtis, right, that they didn't want to be in something called MPS (Mathematical and Physical Sciences). That indeed, their ties were going equally strongly to other components. For example, there was a computer engineering program at that particular time, which was in the directorate of engineering. And lastly, there was another not unimportant player of the National Science Foundation at that time. He had a budget of about eight or nine million dollars a year at that time. Let's see, what was the name of that division? Howard Resnicoff was the division director.

ASPRAY: The one in information?

INFANTE: Right. Information Science and Technology, IST. This one was in a third directorate, right, which was at that particular time STIA. So here you had three different directorates, each of them with divisions. In the case of STIA and in the case of mathematical and physical sciences... As a matter of fact when we think about it the STIA program had the division - the division of information science and technology. MPS didn't have a division, right, in computer science; and the case of engineering there wasn't a division - it was just a program as part of the division of electrical and computer engineering. There was something wrong in a system that cut across the whole thing, right? In fact, I think what Kent Curtis wanted was to have what has happened now, or what happened three or four years ago when as a result of the big scientific computing endeavor sort of the different pieces were pulled out within the Foundation and a new directorate was set up. I think the desire to do that was perhaps postponed by a period of three or four months; what happened at the time was the splitting into divisions and then it took another two or three years to pull the components together. And in effect what it took was the creation of what I would like to think I had a great deal to do with, which was first the Office of Advanced Scientific Computing, which then wound up from an office becoming the umbrella that formed a new directorate.

ASPRAY: What can you tell me positive about computer science during your time there in terms of which programs did you think were working, working well?

INFANTE: I think all of the programs were working. Remember, the sort of things I have told you, in which I perhaps have emphasized the negative, were matters of perception a great deal. The perception of computer science inside of

NSF was not in my opinion any significantly different than it was the perception of the new departments of computer science within universities, right? It was suffering by youth. It was suffering by the fact that many of the senior people that then called themselves computer scientists were refugees either from physics or from a certain aspect of mathematics. But for example, I kept on looking at grants, and they all came to my desk, both in mathematics and in computer science. I thought that there was an enormous amount of excellent work being done in all of the components. The one that, frankly, I felt more capable of... if not passing a judgement, which I certainly wasn't capable of doing. I mean, I do not think of myself as a referee for a proposal in computer science], but neither would I use myself as a referee in topology. My taste told me that a great deal of the work that was being done, for example, in what was called the Theoretical Computer Science Program was very, very interesting and very, very good work. I was there at a time of the birth of the Coordinated Experimental Research Program. Kent Curtis deserves the credit for that. It was something he had been pushing for a period of time. Let's see. I am not even sure that I am telling the truth. Maybe it was born the year before I got there. You see, I was there one year, then I went back to Brown, then I came back as division director.

ASPRAY: I think it was either 1980 or 1981 that it was started.

INFANTE: Right. And you see, I was gone 1980-81. I was there at the Foundation as the division director 1981-82, 1982-83, 1983-84. I think it must have been somehow in the 1980s the decision was made, but to tell you the truth I spent so much time with the damned thing I don't know if - let's see, the first four Coordinated Experimental Research awards were made to Berkeley, to Purdue, to Washington...

ASPRAY: To Wisconsin, maybe.

INFANTE: ... and Wisconsin. And I am pretty sure that those were made, were announced and put out the door three months before I got there as division director. At the same time a similar initiative was started on the other side of the house, which were the institutes at Berkeley and here at Minnesota. I had hit the ground and I had been there for only two or three months when what looked like a budgetary crisis hit. And that's when those programs, which

were big initiatives, were very much in danger. And Kent Curtis was the one that really deserves the credit. I had to work for a couple of years - two or three years - to try to get that particular thing going. But a little bit, you see, he was continuing to live in the shadow of DARPA. Because DARPA had set up, what? MIT...

ASPRAY: Stanford, Carnegie Mellon.

INFANTE: Stanford and Carnegie Mellon. And indeed, I don't know how well-known a fact it is that the Berkeley proposal that resulted in that coordinated experimental research award really was almost a joint DARPA/NSF endeavor. In essence, Kent Curtis was trying to get NSF to play bigger boy in this area, and he was using some of the same means. But perhaps the other model was also the materials research laboratories - trying to get an aggregation and trying to get a mean of funding, to bring the level of on the one hand socialization, on the other hand visibility for the computer research community.

ASPRAY: I had heard the CER program characterized as one in which they were trying to bring another set, starting with four or five schools, but then maybe going out to as many as 30 up to the level of the big three or four that had been DARPA-supported schools. Is that the way that it was seen around there?

INFANTE: I would put it in a slightly different way. You know, back in 1979-1980 the computer research world was really divided into two sets - the DARPA schools, and what was then described as the outer darkness.

ASPRAY: [laugh]

INFANTE: The general idea was to try to get 15, 20, 25 schools. I don't think, at least in my analysis, it was ever the case that it would be possible for the National Science Foundation, with these many universities, to have each of them at the level of the three and four DARPA schools, but anyhow at least to get those particular universities, and a good number of them - 10, 15, 20 - so that they would be sufficiently attractive so as to retain faculty. You know, at that particular time an individual was getting his Ph.D. at Carnegie Mellon or at MIT or at Stanford. Either those

people would trade among themselves or those people had a very, very strong feeling that, no, they didn't want to go to any other place; they'd rather go to industry. And one of the reasons was that those departments out there did not have the kind of infrastructure at all, especially at the level of equipment. And indeed, you see the Coordinating Experimental Research Program put all of its emphasis in equipment, under equipment infrastructure. The purpose of the Coordinated Experimental Research Program was to get a good-sized set of universities, between 15 and 25, that would develop the instrumentation, computational instrumentation infrastructure, so as to be able to retain faculty and frankly increase the number of Ph.D.s that was desperately needed at that particular time.

ASPRAY: Right. I see a few departments that I think of as successes from CER - Wisconsin, Massachusetts...

INFANTE: Cornell.

ASPRAY: Cornell? Overall, do you think this was a successful program? Has been a successful program? After all, it's still going on.

INFANTE: Oh, yes. You know, in this area of instrumentation computer science had to make up what I would consider 20 years of lag behind where the comparable departments of chemistry and physics would have been. I mean, the National Science Foundation would be funding chemistry and physics. Well, since the founding of the National Science Foundation was instrumentation, with post-doctorate, with graduate student, if you were to go to one of the places where there had been a success - let's pick Wisconsin... If you were to have gone to Wisconsin in 1978 you would have found that there was a chemistry department - not badly instrumented, pretty good - a large collection of graduate students and post-doctorates. If you were to go to the computer science department, on the other hand, you would find a few rather large grants in the hands of people like Larry Landweber. I mean, in his particular case, at that particular time, a very significant activity in the area of networking, but on the other hand, frankly, no infrastructure. I mean, the infrastructure had been built up in chemistry courtesy of the National Science Foundation from the 1950s and even before that as part of the university. That period, it seems to me, that from 1950 to 1975 - in the case of computer science; something completely missed, partly because there were no departments of

computer science, partly because the investment that the National Science Foundation had made in computation had gone in university computer centers. And fundamentally the computer centers were interested in servicing the entire university, not in computer research. So the purpose of the Coordinated Experimental Research Program was to try to really put an emphasis on the computer research side of the house.

You know, during the late 1960s, early 1970s, the National Science Foundation, again that division under Pasta, had made very significant investments in providing computational resources to the universities. But I don't think it had done very much for computer science, per se. And perhaps the reason it hadn't done much for computer science itself is because computer science didn't exist. And so the purpose of the Coordinated Experimental Research Program was really in a certain sense to play this catch-up, because at that time DARPA had really played a significant role, an enormous role at MIT, Carnegie Mellon, and Stanford, and then later a little bit at Berkeley.

ASPRAY: Could you talk a bit more about networking at the Foundation? Support for networking? It's a topic that I see over a many-year period, and there's low level interest, but I never see programs getting going. I can never figure out what position the Foundation had compared to all the support that DARPA had put in in an earlier period.

INFANTE: I must confess, I really started reflecting on this matter in 1980, 1981 - it was pretty late. On the other hand, let me remind you that CSNet came into being in 1981. In fact, when I got to the Foundation I thought I knew quite a bit and I knew quite few of the members of the community in the mathematical and the more theoretical aspects of computer science. Frankly, I had never put my hands on an IMP before I showed up there. The impression that I was left with when I got to the Foundation is that the people in computer science had wanted, for a long period of time, to try to set up some sort of networking. But on the other hand they were really playing second and third fiddle to DARPA - namely, that DARPA had a good system going at that particular time. Let's see, in 1981 you could certainly use DARPA between certain sites and very easily at 10 kilobits and in many cases at 56 kilobits. There were very few sites at the universities - about five, six, seven, eight of them - and NSF had wanted to do this. Before CSNet, you know, the Telenet system came into being and that was being used.

My sense again that that was an area where the failing of computer science inside of the National Science Foundation... I remember participating in arguments with assistant directors who were totally unsympathetic on the expenditure of money so that people could write notes to each other by computer. Again, in his usual fashion, Kent Curtis really kept at it. He kept at it; he kept at it - I think he got a great deal of help and aid from DARPA, especially from Kahn, because Kahn was really the animating body in this business until finally, successfully, he decided to go out with this CSNet program. But notice again, in the case of the CSNet program, that in itself was a rather rocky endeavor. It was a rocky endeavor because why? It was a rocky endeavor because on the one hand it was managed by a group of three senior individuals, of which Larry Landweber was one. Secondly, it was in effect at certain technological levels managed by BBN and SRI. And at a certain point, before I showed up as division director, Bill Klemperer got so uncomfortable with the whole business that he took a person from the division of chemistry, pulled him out, shoved him, gave him a presidential appointment in order to pull this stunt, put him in the computer science action, and gave him the charge of managing this endeavor.

ASPRAY: This was Kern?

INFANTE: Bill Kern, right, who is now Dean of Sciences at Ohio State. Indeed, as division director, one of my tasks very often was to try to smooth the feathers between Bill Kern and the other people in the computer science division and in particular very much Kent Curtis. Because Kent Curtis, and I think rightly so, felt unappreciated and felt he did not have the confidence of the higher-ups within the National Science Foundation. Let me be blunt, the natural successor of Bill Pasta on that job would have been Kent Curtis. And the reason why I was sort of dragooned down there was because they didn't want Kent Curtis. You know, facts are facts: the world doesn't run necessarily along the lines of facts but along the lines of perception.

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INFANTE: Kent was always very thoughtful, very methodical, very careful about how he did things. It's quite something to compare Kent Curtis with Kahn. Kahn could sell anybody the Brooklyn Bridge. But yet, Ken really

knew his business, right? But I think if Kahn had not known his business and he would have sat across a political appointee or somebody and tried to sell him the Brooklyn Bridge he would have succeeded. Kent Curtis just didn't have that in him. He had lots of trouble selling some things, though very, very good. CSNet, CER. Did you ever meet Kent Curtis?

ASPRAY: No, I didn't.

INFANTE: He was a person that I think up to a certain point who could be described as a person who was a little bit shy, and he seemed undetermined. He was not an imposing personality. He was badly hurt by my appointment; he obviously wanted the job. He probably deserved it. One of the reasons for the problem in that particular context, you see, is that mathematicians and many more academics on the theoretical side of the house - theoretical computer scientists - they just couldn't stand the idea. That was one of the problems of having this rather broad division. And indeed, if you think about it a little bit, it looked like the Institute of Technology, right? It would be very, very difficult to have a dean who's strictly an engineer or a dean who is strictly a scientist. You have to find just the right people who can have their foot in both sides. So you see, it's not per chance that they tried very hard to find somebody who was either a theoretical computer scientist or an applied mathematician, right?

ASPRAY: Sure. It makes imminently good sense.

INFANTE: Well, up to a point.

ASPRAY: One could have vision from whatever background...

INFANTE: Right. But the jobs at the National Science Foundation, in my opinion, strongly depend on the faith and support on the part of the scientific community. And in many cases, somebody who might be very, very good at doing a sort of particular task, just because he doesn't have just the right attributes or has been in the right circuits... You know, no matter how broad a scientist you are, right? I mean, the minute you are in a division that has 20

programs that range from networking to topology, and then you have statistics. What you really know something about that you could be used as a referee is one [program] - it's five percent. But people put a great deal of faith and I guess what they judge as the taste, hopefully the catholic taste of somebody that is given responsibility for the whole spectrum. And I guess the judgement was, especially on the part of the mathematical community... You see, I thought the mathematical community always felt very comfortable with John Pasta. So did the computer science community. And so did the upper-ups in the Foundation, because they knew he was a powerful individual. It's too bad that he got sick when he did. You know, he died of cancer. A terrible mess. I mean, they cut him up. Let's see, by the time he had died he had had over 20 operations. You know, it was one piece one week, one month and a half later another piece.

ASPRAY: Why is it that you left the Foundation?

INFANTE: I never intended to stay. I regarded my service to the Foundation in the same way that... I regarded, as an academic, my service to the Foundation the same way as a committed civilian would regard the service in the military. It was something that has to be done. I mean, that's an organization that had supported me; they had provided me resources for 20 years. I think everybody in the scientific community, especially if they have some talent that can be used, really owe it to an organization, be it the National Research Council, be it the National Science Foundation. And we do. I mean, we all referee proposals. I have there one proposal to referee which is this big. Have you ever heard of anybody asking the National Science Foundation for - "Gee, it's going to take me ten hours; send me a consulting fee."? No, we do it frequently. And I think it's the same regard.

No, I never intended to stay. Again, because they wanted to appoint me quick and fast I had an unusual appointment at NSF. I had a presidential appointment, which is most unusual for a division director. At a certain point I remember the successor of Bill Klemperer asked me. He said, "Jim, do you want to have it converted to a regular appointment?" I said, "Absolutely not." I am too much of an academic. As a matter of fact, I used to be a professor. [laughter] Here I am saying this thing to you and you are talking to somebody who happens to be a money counting dean. [laughter] But see the bad things NSF service can do to you. [laughter] You really get

screwed.

ASPRAY: I want to close up soon because I know you have many other things to do. Are there any particular other topics you would like to talk about in regard to computing in this period that you were there?

INFANTE: I would say that there were two really major thrusts. One was the CER and the other was one on networking. The other one just starting to come up in the area of computing was the end result of the supercomputing endeavors.

ASPRAY: Yes, that had started at that time?

INFANTE: Oh, yes, because, you see, I put the Peter Lax report - I think it must have been 1983 is the date of the Lax report...

ASPRAY: I think that's right.

INFANTE: And I pushed that one very hard, and I thought I played a rather significant role in that, especially because, frankly, I am the one who convinced the office of the president. And I am the one that, during that particular time, convinced the then-director at NSF that this was something that I thought the Foundation should run with. It's funny that I felt this endeavor was a little bit damaged, but this is my own view. Subsequent to the Lax report things were going very well, and I thought definitely there would be an initiative. There were three things that happened that I wasn't very pleased with. The first thing that happened is that I really expected in this area a level of collaboration between the National Science Foundation and the Department of Energy, which did not take place.

ASPRAY: And why not?

INFANTE: Once the Lax report raised the level of understanding and visibility of this activity to a very high level, I

really wanted this activity to march forward, and I wanted the computer science and the mathematics community to really play a big role in this activity. The men of chemistry and physics woke up that something was happening. Together with Ken Wilson they decided they wanted a cookie. Subsequent to the Lax report there was another report written, which in my opinion was not very good. And indeed, in my opinion it was a little counterproductive. Something that was called the Barton report.

ASPRAY: I haven't seen that.

INFANTE: It was written in something like six or eight months, but it was something that said, "Gee, given that the Lax report set down these things that needed to be done, this was the action agenda and now we are going to do it. Barton was the division director of physics, who at the time, because the assistant director of MPS had become the director of the Foundation, had become the acting assistant director of MPS. And therefore, he was overall the manager, and needless to say he took this in his own hands, which, however, he took in his own hands, in my opinion, more with the interest of the physicists, okay?"

ASPRAY: [laugh] Yes.

INFANTE: He got Kent Curtis to collaborate with him. And it seems to me it was a result of that report that, frankly... You know, one of the bad things in Washington is competition between agencies, right? And, frankly, it was quite clear that NSF wanted to hold its own. There was no doubt about it that the DOE and the bomb makers just didn't want to have anything to do with those funny academics, right? The hell if they were going to let them inside the fence, right, either electronically, much less in person. There were those two elements.

And then the last element was is that in the Barton report enormous, fantastic predictions were made, which I thought were double of what they should be. And, indeed, it has turned out to be that way, exactly. I mean, they had proposed at that particular time a hundred million dollar a year program. I kept on saying, "Hey, folks, we're likely to start at ten million," and indeed we did. "And I think very rapidly it's going to 20. By the time it gets to 40 to 50,

that's it, folks." I never felt, you know, that you could have in a proven, advanced scientific computing more than something in the level of a five to seven percent of the total budget of the Foundation - this is exactly what it is today.

It seemed to me that that particular report, in a certain sense, on the part of the elite that says, "Gee, we don't want it. We bomb makers want to do our own thing. Those people have this big program. Let them do it on their own." And the enormity of the program delayed it within the Foundation. And the aspect of the particular delay, which I think to my mind was the worst mistake that the Foundation made in this area. If you expect to have a hundred million dollars and the technology is really marching fast, networking doesn't take the high level of priority that it should take, right? Say, "Gee, let's wait a little bit more, because by then, instead of being at 56 kilobits we will be going at T-1, then at T-10." And especially, "Gee, we're really going to have lots of money then if we can do it right," right? And indeed, my greatest criticism, I think, of the way that we did things at the Foundation in this area is not to put more emphasis on networking in comparison to computational engines. And lastly, is that I think that in some of the policies that were followed by National Science Foundation, I think that we're a little too generous on the computational engine side and not enough on the research. And indeed, my impression today is that there is a hell of lot of computational cycles available out there, and networking still isn't what it should be.

ASPRAY: Well, thank you.

END OF INTERVIEW