Abstract

The main topic is institutions in computing. Traub begins by discussing why computer science has developed as a discipline at some institutions but not others. Institutions that are highlighted include Stanford, Berkeley, University of Pennsylvania, MIT, and Carnegie-Mellon. Traub discusses his experiences as chairman of the computer science departments at Carnegie-Mellon and later Columbia. Other topics include: industrial and government funding of computer science departments (in particular the role of the Advanced Research Projects Agency of the Defense Department); the relationships between academic centers, such as M.I.T., Stanford, Columbia, and Carnegie-Mellon; and the importance of educational institutions to regional centers of industrial computing. At the end of the interview Traub returns to a topic of his earlier interviews, his experiences at Bell and Watson Laboratories.
ASPRAY: This is a session on the 29th of March, 1985, with Joseph Traub in his office at Columbia University. It is the third in a series of interviews, and the main topic in this session is institutions in computing.

Why don't we start by talking about why certain universities - Pennsylvania, Harvard, Columbia, which had strong early activities in computing - didn't develop into first-rate research and teaching centers in computer science?

TRAUB: It seems to me that in each of the institutions, that have become the major centers one can usually identify one or two people who were responsible. I'm speaking of people who really cared about building institutionally.

There was a program at Carnegie-Mellon in 1962; there was a precursor to the Stanford department also in the early 1960s. Purdue may have already had something like a department, though I'm not sure it was called a department, by 1962. Certainly there were departments at Stanford and Carnegie in 1965. The beginning of academic computer science is in the mid-1960s or perhaps early 1960s in the United States, to the point where now practically every major university in the country has a computer science department, or, in a couple of special cases, an electrical engineering and computer science department.

Because that's all happened during the time I've been active, I can see who was responsible. For example, why should Stanford have been a leader? I think it was due to one man, George Forsythe. He had the vision that this was going to be a discipline, and the energy, and obviously support from some key people. I believe the Babbage archives actually have material on George Forsythe. George was a man of great character and moral force who made that happen. At Carnegie it was Perlis, Newell, and Simon initially. When Perlis left, I went there at a crucial point that I will come back to, which could have been a turning point for the department. But first, I'll identify the people. Perlis, Newell, Simon, and myself in terms of the builders. With MIT, I simply don't know. You could say it was a
great science and engineering school and therefore it was inevitable, but it didn't happen early at Cal Tech, for example. It's just now happening at Cal Tech.

At Berkeley, there were two departments: and two departments are perhaps worse than none, because there was a bitter battle between Abe Taub, who headed a department (he's a mathematician who'd been recruited from Illinois to head the computer center and to build a computer science department in the School of Letters and Sciences), and Lotfi Zadeh who was the head of the electrical engineering and computer science department. Or perhaps, it was called Electrical Engineering, but had people working in computer science. In the late 1960s the country watched while this bitter battle was waged at Berkeley between the two departments. Engineering wanted it more than Letters and Science, and Engineering won. There's an Electrical Engineering and Computer Science Department of great distinction, with a Computer Science Division, so it's got to be Zadeh, who gets much of the credit. He was the winner, certainly, in that battle of getting Berkeley to form one department, and that's now a department of great distinction.

At Penn, I don't know the history of why they were not successful, despite their enormous head start with the Moore School. It's only recently that Penn really has made a push under Joshi, who is the chairman of computer science. But for a long time there was only limited activity.

At Columbia, where there was the IBM Watson Laboratory early on, which in fact was one of the few places in the United States where one could do research in computing in the 1950s, there has been a long history of not being able to get going in computer science. There were various efforts made. Mathematics tried at one point: mathematics said, "We can do it." There was an Electrical Engineering and Computer Science Department. There was a group within Mathematical Statistics. I'll talk more about the Columbia history later, but I'd like to say now that it simply couldn't get going. What finally happened, and again I'll talk about this in some detail later, was that there was a Dean, an extraordinary man named Peter Likins, now President of Lehigh who said, "This is the most important thing that I can do as Dean." There was Associate Provost Norman Mintz, now the executive vice-president, who very much supported this, and Peter recruited me. That was five and a half years ago, and now Columbia has one of the
There are two possibilities in answer to your question: there was no one willing to devote a fair amount of his life to building the institution, or there was such inertia in the institution that one couldn't do it. It could have been either, or some combination. Certainly at places that were successful it seems to have been one person, often with very little institutional support. For example, the amount that Carnegie-Mellon University put into the department annually in 1971 was two hundred thousand dollars. And at Stanford, my recollection from when I visited there in 1966 is that the total budget from the university was on the order of a hundred fifty thousand. There was very little support. It was the key people - one, two, or three people - who built the great departments.

ASPRAY: When you started, you spoke, just about people, and then you broadened it to talk about institutional support at least in the sense of the willingness of a few administrators to allow somebody with some ambition or initiative to come in and do things. What about support from the computer industry or from the government, in terms of financial resources?

TRAUB: When one looks at the best departments today and at their history, it turns out that the agency most responsible for major chunks of funding was the Advance Research Projects Agency of the Department of Defense. That is not to say that the National Science Foundation hasn't helped: it has heavily supported basic research in computer science. In fact, fairly recently, over the last five years perhaps, it has started a program called the Coordinated Experimental Research in Computer Science, giving major funds for equipment. Also more recently, IBM in particular has been extremely generous, for instance, to Columbia.

Let me start with ARPA, because I think they're crucial. The Information Processing Technology Office of ARPA was led by a group of visionary directors - Licklider, Bob Taylor, and Bob Kahn - who felt this was enormously important for the country and identified the few places in which to put major resources. For example, at Carnegie-Mellon, to return to the budget in 1971, when I showed up, the budget was two million dollars for the department. Two hundred thousand from the university, 1.8 million from DARPA. "There had been major funding at
Stanford, for instance the AI lab, and major funding at MIT. In fact, the Big Three - Carnegie, MIT, Stanford are usually called that in computer science, though I don't know who coined the phrase; I don't know whether I did or somebody else, but it's simply part of the lingo and it has been for a while - all had major and long-term infusions of DARPA money. By long-term I mean for the last twenty years. At Columbia, we are now one of the top five in terms of DARPA funding. Berkeley, by the way, is the fourth. But Berkeley has gotten mammoth funding, and that has been simultaneous with their move into the top group. I know how crucial DARPA funding was to Columbia. Again, when I tell you the Columbia story, I will in fact come back to that.

Now, let me mention IBM because they have been so important somewhat more recently. For instance, at Columbia - which is what I know best - when we first started this department, even before I came here, in the spring of the previous year Peter Likins and I went up to IBM to talk to Ralph Gomory the Vice President of Research, and to talk to the Director of University Relations. Likins indicated that I might be coming to Columbia, and asked if IBM would be prepared to help. It turned out that, two years before, Columbia had approached IBM. IBM had laughed and said, "Get your act together first." And then Pete now had a real live candidate, and he went up and said, "If Traub comes here will IBM help?" I wrote a proposal in November of 1979, my first full year. In December, we had a gift of $600,000 to attract faculty and students. And I can tell you something that hasn't been announced yet, which I've just found out, that early next week the President of Columbia will be receiving the initial check of four million dollar gift from IBM for the support of this department. IBM has been enormously generous. The reason I'm focusing on that is that many other segments of the industry have been willing to give machines. IBM has also been willing to give financial support.

ASPRAY: I've got several questions regarding your statements. One has to do with ARPA's philosophy. Was there an underriding philosophy of excellence, a desire to support a small number of strong centers?

TRAUB: Yes, absolutely. It's absolutely an elite. There's no peer review; so what you need are people with good taste and judgement in the office. It's very different from NIH, NSF, and so on; no peer review; mammoth support for a few places; and really basic research. For instance, artificial intelligence. All the basic artificial intelligence work in
the United States in the universities was supported by ARPA, as was that in places like BBN and SRI. The philosophy was of major support one, very elitist, for relatively few institutions.

ASPRAY: The other thing I've wondered about often is the relationship between academic centers of excellence and major regions of computer industry. For example, Route 128 and MIT, and Stanford and Silicon Valley. Carnegie-Mellon doesn't seem to fit that very well.

TRAUB: Oh, you're just a little bit behind the times.

ASPRAY: Maybe I'm wrong.

TRAUB: You're just a little bit behind. Let's start with Stanford and MIT, and then take Carnegie. There is no question about it: the high technology belts are directly associated with the great universities, and in fact in the case of Stanford one can make a direct trace from Frederick Terman to some of his students, such as Hewlett and Packard whom he urged to go and do their thing, and to Fairchild Semiconductor and the spinning from that. But it was directly from Frederick Terman at Stanford that the vision came which eventually led to Silicon Valley. And Route 128 is certainly due to MIT. There's also the Research Triangle area, and more recently, although it's too early to see what will happen, is MCC's move to Austin to be near the University of Texas.

Carnegie-Mellon is very interesting. There is Pittsburgh, the absolute epitome of the Industrial Revolution, blue-collar workers, and so on. In the early 1970s, Simon, Newell, and I were talking about "The greening of the University through computer science," and then eventually changing Pittsburgh and western Pennsylvania. So we built a great computer science department. We then strengthened nearby departments, such as electrical engineering, mathematics, psychology. When Dick Cyert became president, he had the vision that he would use computing to change the university. Today I think in some ways Pittsburgh is the most interesting example. I became aware of Carnegie in 1971. At the time, it was a respectable but not great science and engineering school. Carnegie-Mellon, Lehigh, RPI, maybe Case-Western, were all of a class. It had just been fused from the Carnegie
Institute of Technology and the Mellon Institute of Research, which was industrial research - a marriage, by the way, forced on the two institutions. That is, it was forced on the Mellon Institute by the fact that apparently the Mellons were no longer prepared to fund the Mellon Institute.

So there was, as I since understand it, since this happened before I got there, a forced marriage, with not entirely happy partners. As a matter of fact, there were some severe political problems in some areas, such as chemistry, where there were departments in both institutes. I remember how it upset me when I would tell people in the early to mid-1970s that I was from Carnegie-Mellon, they had never heard of it. I remember actually placing phone calls and the operator asking me where I was from. When I said, "Carnegie-Mellon," the operator would say, "I've never heard of that before." That really upset me.

That was the picture, in the early to mid-1970s of Carnegie-Mellon. It was a respectable engineering school, nowhere in the class of Cal Tech and MIT as an engineering school, nowhere in the ball park of the Ivy League, Stanford, Berkeley, and so on as a great university. In ten years, Carnegie has boosted itself to the point where when Mitterand visits the United States, when he goes to the high technology areas, he goes to Pittsburgh as well as Silicon Valley. He gave a medal to Ray Reddy, who started the Robotics Institute. Business Week, the New York Times, all the media, when they talk high technology, there's always Carnegie-Mellon. And by the way, Pittsburgh is simply being transformed, and is viewed as a high technology area with many companies being spun off. Pittsburgh is now always listed as a high technology area, and in some ways the most dramatic because it started from the lowest base. What's happened in a mere twelve years is the transformation of a university, and the transformation of a city and a part of the country. I think the belief in Pittsburgh now is that the future is high technology.

ASPRAY: But it is true, for the case of Carnegie-Mellon, that it grew strong much more quickly than an industry built around it, whereas these seem to be happening coeally in the case of MIT and Stanford?

TRAUB: Forty years ago Stanford was referred as "the farm". It was viewed as a school for rich kids and wasn't taken very seriously. It was only in the post-war period that Stanford became a great university. Silicon Valley came
later. At Carnegie it all happened in the last 10 or 12 years; we're watching it happen.

ASPRAY: But, for example, in the Boston area, as MIT developed, there were a number of closely related things going on - many spinning out of the war, actually, through the Rad. Lab and such. But there were people whom they could call on; there were companies they could call on for local support as the program at MIT was starting to get built up. They could place many of their graduates in the immediate area. It doesn't seem to me that Carnegie-Mellon could that easily draw on the Pittsburgh community as it was growing in the 1960s the way that either Stanford or MIT could.

TRAUB: There was a five million dollar gift from Richard King Mellon that gave a chair to Herb Simon and did some other good things for the university, though I don't think it was crucial to the future of the Computer Science Department. What I used to say in the early 1970s is that what's extraordinary about Carnegie is we don't deal with any company within three hundred miles of us. That is, our closest contacts were with DEC up in Massachusetts, with Xerox in Palo Alto, and later on, by the late 1970s, with IBM. That was to change, of course, with Westinghouse being involved with the Robotics Institute in 1980.

I used to say to people at Stanford, "Look, you guys have it so easy. You've got great quality of life, you've got a great university, and you've got all that industry. We don't have any of the three." Then later I really realized, in fact, that that is one of the things that gave Carnegie it's absolutely unique flavor in the 1970s: we all put everything into the department, because there was no place else to put your energy. There was no place to consult, there was no industry, there wasn't that much to do in the city, so we put it all into the department. In a sense, it's one of the reasons Carnegie was so good and so cohesive, whereas at a place like Stanford there were so many distractions, both in terms of scenery and other companies. So, you're absolutely right. Carnegie was not fostered by local industry, but it created high tech in Pittsburgh.

ASPRAY: Why don't we turn from the general to the more specific examples? Why don't you tell me the story of the building up of Carnegie-Mellon's computer science program?
TRAUB: In the academic year 1970-1971, I was on leave from Bell Labs at the University of Washington. In fact, I had a tenured professorship there, and although I was still on leave from Bell Labs I could have stayed; but I felt somewhat isolated in Washington. I liked Seattle very much in terms of people, but I felt somewhat isolated scientifically. The nearest major city and great university were eight hundred miles to the south. I talked to Alan Perlis in January or February of 1971 and he mentioned they were looking for a head. An executive head rather than a chairman, is what it's called at Carnegie. We made arrangements that I would visit there on the way back from the Spring Joint Computer Conference in Atlantic City. I visited Carnegie in February for a day, met the faculty, and was so taken with the faculty and the students that I said I'll stay an extra day. I remember having lunch with a number of the faculty and the administrators and being asked questions such as, "Should there be an undergraduate program?" and I'd say, "Well, that's a difficult issue. I should hear the pros and the cons." It was a very comfortable visit and Perlis said, "I will be in touch with you on March 1." On March 1st I was offered the headship to Carnegie.

Now, one thing that's interesting about that, is that not only did I have no real administrative experience - I had headed a small research group at Bell Labs, but I certainly had no academic administrative experience - but I'd never been a part of any department. I may have told you this story earlier: that I went to an undergraduate college, so I'd never been involved with a real research department. I'd gone to City College, which has only undergraduates. I'd been under a committee on applied math at Columbia, not a department, and I had then gone to Bell Labs. Even at the University of Washington we were a Committee reporting to two different schools. When I became head at Carnegie, not only did I have no experience, I'd never even been in a department; and I think in some ways that was good. It really left me looking for mechanisms with no pre-set ideas.

Even in the spring of 1971, before I came to Carnegie, Allen Newell and I would have extended conversations late in the evening. Well, it was the evening for me, it was probably eleven p.m. or midnight for Allen. We'd talk an hour at a time, and I grew to greatly admire Allen. There are several people that have just been enormously influential on me, and one of them was Allen. When I was to go to Carnegie, we would still continue that: in the evening we would often have these long discussions. I would run down an agenda of questions and we would talk for an hour. I so
enjoyed those conversations, I would sometimes actually close my eyes and let Allen's voice wash over me. Just a couple of weeks ago I was on a conference call with a search committee looking for a new director. Allen's a member of that search committee, and it reminded me of those days. That was the beginning of my discussing things with Newell. I was already drawn into making decisions, even though Perlis was still the head.

TAPE 1/SIDE 2

TRAUB: Alan Perlis was the founding chairman at Carnegie. He had been offered the Higgins Chair of Computer Science at Yale. He'd had some physical problems, and he said, "That's enough of running this department." He had been a towering intellectual figure. In fact, Alan Newell later was to tell that they were really worried how the department would stay a great department after Perlis left; he had so dominated the department intellectually. Perlis was a man who rarely wrote anything. His influence is entirely - and it's very unusual for a scientist in the twentieth century - the laying on of hands directly. The man has probably written just a couple of papers. It is all oral, yet his influence has been enormous, first at Carnegie and later on at Yale. Alan Perlis is really one of the great men of computer science, and though I generally disagree with most of the things he says, I always just love to listen to him. The man is extraordinary. Beautiful twinkling blue eyes, and intelligent; an extraordinary man. And he had given a certain flavor to Carnegie.

ASPRAY: Can you describe the nature of the program that he'd built up? What were its strengths and weaknesses?

TRAUB: It was very strong in experimental computer science. I'll go over a little later who was actually at Carnegie in the summer of 1971. It was not so successful in theory. In fact, the theoreticians had left; they had been rather unhappy. What had happened at Carnegie is that Perlis - who's really extraordinary, a genius - had apparently lost interest several years before, and people had started drifting away. It was more than drifting; it was almost a hemorrhage. In a short amount of time Carnegie lost Albert Meyer and Mike Fisher to MIT, Bob Floyd to Stanford, Tim Standish to Harvard, and Perlis himself went to Yale. A number of other key people had left. In fact, within a
period of three years, half the faculty had left Carnegie. When I showed up there in July of 1971, there was a serious possibility that the department would go subcritical - that if one or two more people left, the department would collapse. It was that bad. In fact, something Allen Newell told me years later, when the department was a great success, is that the faculty had decided that nobody would leave the first year. They would give me one year to see if I could change things, and then they were free to leave. But they had made an agreement, a pact among themselves, not to leave the first year.

Perlis and I overlapped by about a week, and then Perlis left. He briefed me on the faculty, on some of the students, on the administration. That turned out to be enormously valuable. Now, the people who were there were small in number but extraordinary. There was Allen Newell and Herb Simon. Herb and Allen had made an arrangement that Herb would focus on psychology and that Allen would focus on computer science. Herb is an extraordinary man, a polymath, one of the great social scientists of the age. He has made contributions to about four fields, each of which claims Herb Simon as its own. I had more contact with Newell than with Simon because of their arrangement.

In addition to Newell and Simon, Bill Wulf was a young assistant professor. He was to go on to become one of the great men of software engineering and multiprocessor architecture and now is the president of Tartan Labs. Raj Reddy was a young faculty member. There was Nico Habermann, who later succeeded me as chairman of the Computer Science Department. And there was another extraordinary figure, Gordon Bell, who is considered the father of the minicomputer. There were only about seven or eight of us. It was certainly an extraordinary group, but a very small one; my greatest concern was that I was entrusted with this - of course, Carnegie had quite a reputation - and if I didn’t succeed, this department was going to go down the tubes.

There were a number of priorities, and the greatest was to build up the faculty. That first year we interviewed thirty people. In one week, we had interviewed five people, and we hired four of them. They included Dan Sieworiek, who's now a full professor at Carnegie-Mellon, and Sam Fuller, who left Carnegie-Mellon and is now Vice President of Research for DEC. Those were among the catches of that very first year.
The second problem was to diversify funding, because we had ninety percent of the funding from ARPA, $1.8 million, and $200,000 from the university. I also decided I wanted to redo the Ph.D. program.

ASPRAY: Before you go on to that, tell me something about the diversification of funding.

TRAUB: For instance, within a couple of years we succeeded in getting a million dollars a year from the National Science Foundation. I wanted to get the percentage of ARPA money down, but ARPA put in so much money that it was hard. Even some years later, ARPA was still supplying something like sixty to sixty-five percent of the budget, but then it was ninety percent and more. In fact, when ARPA went through some hard times, or at least when it wasn't putting its money into the academic sector in the mid-1970s, we suffered a cutback from $1.8 million to $1.5 million, with a threat of going down to $1.2 million. It was a time of concern. But we got money from NSF and from ONR. We got some industrial funding.

Then I managed to convince the university to increase our budget. That wasn't easy. I dealt with the Dean of Science who reported to a Provost, who reported to the President. We were in the Science College rather than the Engineering College. The reason was that Alan Perlis had been chairman of the Math Department, and wanted Computer Science in the same college as Mathematics. I would negotiate with the Dean of Science for positions, and there would be a bitter battle to get a single new position. It looks, in retrospect, as if it were inevitable that Carnegie would become a great department, a huge department, an impregnable department. It didn't look that way in the early 1970s. We were very small, and I wanted to build. I managed to fill the open positions rapidly. But it wasn't the case that the administration supported us. People have actually said to me, "Well, it was easy at Carnegie, since you had the support of the administration." That was not true in those days. For each position there was a major battle, and for each raise there was a major battle. One year, the raises were so tiny, around three percent, and I said, "The most important thing is to keep the current faculty happy; they're great." I took a position, and didn't fill it so that I could use that money to give people the proper raises. It was very hard going those first three or four years, and it wasn't the case that we were being showered with money.
ASPRAY: I can understand the University's position on having to guarantee tenured faculty slots. If ARPA money were to have folded at some point, they would have been stuck with a large faculty budget.

TRAUB: That's true, although these were simply assistant professor positions. We had a philosophy, which in fact I have at Columbia also, of almost always hiring people with fresh Ph.D.'s. That is, there were so few top senior people in computer science, and it was so hard to move them to Pittsburgh - at least in those days, and again, the department was very different in the early 1970s from the way it is now. Now, in fact, the department has made a couple of senior appointments, but we never brought in a senior person during the time I was at Carnegie. We've always grown stars. With just two exceptions we've done the same with Columbia. It's so risky to bring someone in, unless the person is absolutely a superstar. I'm afraid to bring in tenured people, and I tend to be very conservative. So I was just asking for assistant professor positions, but Carnegie-Mellon did not yet realize that its future was in Computer Science. It just wasn't clear in those days. It's clear now, but it was not clear then.

In fact, later on, when Dean Bothnerby was replaced, Schatz was the acting dean. We used to have budget meetings over lunch and I'd say, "I wish we wouldn't have these budget meetings over lunch because I can feel it in my stomach every time we have these discussions. Carnegie-Mellon was no different than any other place in that it was hard to try to increase our budget at a time when the university wasn't increasing its total budget.

ASPRAY: Can you tell me about the Ph.D. program?

TRAUB: Several years earlier, there had been a revision of the program. Apparently the programs had been rather strict, and the students had revolted back in the late 1960s. Now they didn't have to take required courses for the Ph.D. I felt the program needed to be further improved. We created a series of mechanisms that are still used at Carnegie and now, at Columbia, and some of which are used at Stanford. One is the Black Friday Review where every student is evaluated by the whole faculty. There was the marriage committee, where the first year students indicated what areas they wanted to work in, the faculty indicated in what areas they wanted help from the students, and a student-faculty committee married them. I was shocked when I got there at the quality of some of the students. I think there had been some carelessness the previous couple of years. Students were admitted to the program who
shouldn't have been there. One of the first things I did was to terminate something like twenty percent of the students. They just didn't belong in an elite department. Something that was true of Carnegie when I got there, and continued to be true was what we used to refer to as the family, because there were enormously close-knit relations between the faculty and the students. Something else rather unique about Carnegie was that, though it caused stresses and strains at times, we worked with very large projects, which is one of the things Carnegie's famous for. For instance we built C.MMP, a multiprocessor machine, and an operating system for it called Hydra. Those were huge projects that weren't being done at other Universities. That's one of the reasons for Carnegie's success.

ASPRAY: Was there a particular curricular slant, philosophically, that you tried introducing into the program that wasn't there before? For example, did theoretical questions become a major component that every student, or many of them, had to take as part of their course of studies?

TRAUB: Curriculum was not something we focused on. The feeling was that all the students that got into that program knew how to get A's in courses. They'd done it in high school and college. We told them, "We know you can get A's. This is now a new game. You have entered an apprenticeship in research. We want to teach you how to research, and you will learn to do research by working with a master." So courses were deemphasized. You did not have to take any courses. What you had to do was pass a set of qualifiers, comprehensive exams that checked that you had breadth in computer science. And you had to write a great thesis. Curriculum was not our strong point. I remember Dick Karp, who is a very dear friend, a very distinguished computer scientist from Berkeley, visiting us at Carnegie back in the 1970s and expressing some concern about the fact that our curriculum was so weak. Typically, the incoming Ph.D. students might take courses the first year, but after that it was all seminars and there were no courses in greater depth. Yet I noticed that later one of the reasons Berkeley got so strong was that they were hiring the products of the Carnegie program, who were enormously desirable. Our focus was not on curriculum, but really on research at the Ph.D. level.

The major research areas at Carnegie were artificial intelligence, with Newell and Simon Reddy as the leaders, and architecture and software. We built a multiprocessor machine which, though it may not have been the first
multiprocessor machine, was certainly the first multiprocessor machine on which papers were written and so contributed to scientific progress. It was a machine called C.MMP, which was sixteen processors connected to sixteen memories. This was work done primarily by Bill Wulf and Gordon Bell. Later there was CM’, which was a cluster machine. That is, you had very high bandwidth within the cluster, and very low bandwidth between the clusters. Architecture was very strong. We also had great distinction in the area of programming systems, with people like Bill Wulf and Nico Habermann. In the theoretical area, the focus was on computational complexity. But Carnegie’s great strength and great contribution was experimental computer science. I intentionally integrated the areas: in fact, that tends to be my philosophy anyway, not to separate out theory, but to integrate it with experimental computer science. Those were the three primary areas, AI, architecture, and software systems. In each of those three areas we were certainly as good as anybody in the world.

You mentioned the Princeton experience and your studies of the mathematics department at Princeton in the 1930s, and the fact that the person who was the organizing institution-builder was not necessarily the best mathematician there. I’ll discuss that in terms of my own experience. I played very different roles at Carnegie and Columbia. At Carnegie there were some towering people: Herbert Simon, a Nobel laureate, a polymath and an extraordinary individual; Allen Newell; Raj Reddy - the list goes on - Gordon Bell; Bill Wulf. My feeling was that my job was to build a great department, although I always also did my own research. I should add parenthetically that the one reason I admire Newell and Simon so much is that they are just as great close up as they are from a distance. Sometimes people are great from a distance, but when you see them close up they often have feet of clay. What’s so remarkable about Newell and Simon is that they’re even more impressive if you get the chance you see them up close. They’re great scientists, but they’re also great for the institution; they are extraordinary men.

My role at Columbia has been rather different. Not only did I come here to build the department at a University that was described to me by my peers as a desert in terms of computer science, but I’ve also been one of the couple of major scientists and intellectual forces the department. I play both roles which has been rather hard. That is, Columbia has been harder than Carnegie, because at the beginning I had to do essentially everything: represent the department at the university, create a sense of self for the department, guide the department, and conduct my own
research. At Carnegie I was absolutely satisfied to play primarily an institutional role. I so admired people such as
Newell, Simon, Reddy, Bell, and Wulf that I was happy to have them do the science. I was active in research but I did
not view my own work as central to the department. Here it's been rather different.

Just to sum up Carnegie, there really was an enormous sense of a unique departmental culture, and there was even a
certain style in which we spoke, certain mannerisms. One, which I picked up from Allen Newell: "Are we there?"
meaning "Are we finished?" "The rational person principle," meaning that this is what any rational person would
agree to; that was often used. We had an enormous sense of loyalty to the department. One really has the sense,
looking back on it now, and we had it even then, of an absolutely golden time. It was the Golden Age.

Today, Carnegie is huge. We used to say in the mid-1970s, "What if we doubled?" Allen Newell was always a great
visionary, so he would say, "Why don't we suddenly double the faculty?" I should say that we grew pretty rapidly.
When I got there, there were seven or eight, in addition to a couple of people whom we let go fairly shortly. That was
Carnegie in 1971. When I left in 1979, there were fifty teaching and research faculty. Essentially, I tripled the
department in every dimension. Then there was to be another enormous burst of growth, of at least another doubling
in the next few years. Carnegie today is always described as enormous. There are faculty who hardly see each other,
since it is so big. There are, of course, advantages. A world expert on almost any question is down the hall. But
Carnegie in the 1970s was really a Golden Age. There were almost no meetings. Once a semester we would have a
Black Friday meeting when we'd evaluate the students. Typically there might be one other meeting of the faculty the
entire semester to deal with some particular problem; for instance, if ARPA cuts our budget by one third, what's our
contingency plan? Maybe once a semester or once a year I would have a long-term planning meeting of the senior
faculty in my home over drinks and cold cuts. It would be an open-ended meeting, perhaps six hours of long-term
planning. With the way I ran the department, people knew that if they were concerned about an issue I would
consult with them. People have often have mentioned that to me, that they always knew they'd be consulted, and
they trusted me.

In fact, if there's one reason for my success at Carnegie and Columbia, it's that the faculty trusts me. That goes a
long way. It means that I can go to the administration and speak for the department, that the department will support what I say and that I really care about the department. That trust means that you don't have to have a lot of committees hammering out things. It means you don't need a lot of meetings. Now, we do meet regularly at Columbia and we're creating the culture.

One of the remarkable things about Carnegie was that it was a rather complex, powerful department whose faculty rarely met as a whole. Yet we all were extraordinarily close. Electronic mail made a difference, of course, but mainly it was trust. I would convene a group of three or four, and we'd make a decision. There was a management committee of the main entrepreneurs on the faculty, and we would meet on a more regular basis. Why did I leave paradise? My mother recently said that my father once described a book to her, in which the author said memories are the only paradise from which we can never be pushed out. Memories are the only Eden from which we can never be evicted. I'm trying to translate it from the German. Why did I leave Carnegie? I was very happy. It was an extraordinary place. I sometimes said I had the best job in computer science. We really worked like dogs. We really worked hard; but there was never any mickey mouse, never any tension in the department. Terrific staff, terrific people, and no tension; and fighting within is so wearying. We knew we were good. The department trusted me as head, so I had a great deal of autonomy, which I didn't abuse. The senior faculty - Allen Newell, Herb Simon, Raj Reddy, Bill Wulf, and Gordon Bell - were very supportive of a strong head. They liked that; they liked somebody who could run the department, so there wasn't the distrust that there often is in academia. Also, it was a permanent position. That is, you served as head as long as the faculty were happy with you.

Why did I leave paradise? The primary reason is that my wife Pamela was really unhappy in Pittsburgh. The original arrangement was that we would stay there at least five years. As five years stretched to six, and seven, Pamela became increasingly unhappy. I had my department, but she really did not like the city; she's a woman who'd grown up on the West Coast, in the Bay Area, and was far away from her family. She was not very happy with her job at the University of Pittsburgh. She essentially gave me an ultimatum: she said she was leaving, and would I care to join her.
I looked at places in either California or New York. New York was purely serendipitous, because just at that point, Peter Likins and I started to talk about Columbia. I gave Pamela her choice, because I had also been talking to USC about building a department there, and there was some other place I was talking to. We'd spent seven years at Carnegie, and then in the eighth year, 1978-79, we were at Berkeley. Her father was dying of leukemia, and she wanted to be out there. I took leave and I said to Pamela, "I'll go where you want to go. We spent these years where I wanted to be." She picked New York. That's one reason why we came to Columbia.

There was also a pull in that there was an extraordinary man here, whom I'll be talking about, and that's Peter Likins, who's really the man who got me to Columbia. Let me go on now with Columbia, but I should also return later to both Stanford and Berkeley because I saw them at crucial points in their development. Our courtship began when Peter Likins came out to Berkeley, and made arrangements to see me during that sabbatical to get my advice. As he said later, also in the hopes that it would lead to more. I gave him my picture of academic computer science, the Ivy League, and so on. We had a very pleasant couple of hours together. We had lunch together at place called the Three C's in Berkeley, a coffeehouse. We hit it off so well, I rode with him on the bus to the airport so we could keep talking. Then he left something on the bus, and I mailed it to him. I remember visiting Columbia in December, just before Christmas in 1978. I knew that Jeff Ulman, who was at Princeton and is now at Stanford, was one other candidate for the chairmanship, and I don't know who else was being seriously considered. On the first working day of that new year, January 2, 1979, Peter Likins called me. This was only about ten days after the visit. He said they had selected me, and asked if I would come to Columbia. There were several months of discussions: I prepared some lists of what I wanted for the department, what I felt I had to have, and Peter said "yes" to everything. It was already the beginning of that relation, which in some ways was very much like those long talks with Allen Newell before I went to Carnegie.

TAPE 2/SIDE 1

TRAUB: I brought Pamela here the last week of March, 1979. I remember that week we spent here very well. We saw *Sweeney Todd* on Sunday afternoon before the Greek Independence Day parade. We met with the president of
Columbia, Bill McGill, who as I recall had a private meeting with Pamela, which was most unusual. We met the Provost, now President Michael Sovern, and Norman Mintz, Executive Vice President, and I met some of the key other people. It was a wonderful week. At the end of the week, Pamela gave me permission to consummate the relationship. So we decided on Columbia.

I'm so much involved in the institutions that I go to. The reason I think I've spent so much time discussing what happened as I was transferring to Carnegie, and then to Columbia, is that those institutional relationships are very important to me. I want some assurance that I will be happy at the institution I'm going to, because I've put so much into the institution.

The summer of 1979 we drove back from Berkeley to New York, stopping off at Pittsburgh on the way. My secretary, Sharon Burkes, who had already gone to Alabama with her husband, offered to come back to Pittsburgh now, in the middle of the summer, for a week to help me pack. It's that kind of loyalty that I've been lucky enough to have from my staff. I could never have asked Sharon to come back; she had already left. She said, "I want to help you pack." So she came back for a week. We came to Columbia in August of 1979. I have to paint the picture for you: I hate very hot weather, hot humid weather. It was ninety degrees, and my boxes arrived from Pittsburgh; I had seventy or eighty boxes of materials even after Sharon and I had thrown out a lot. I was given a tiny office without windows by the Mathematics Department where I could store this stuff. I was in shorts and a T-shirt, by myself, directing the movers where to put things and checking it off. I was given an office on the thirteenth floor of the Mudd building, a little office, where I could be temporarily.

There I was at Columbia. The reason I mentioned this, is that I had come from Carnegie-Mellon, which had its extraordinary support system, a huge administrative staff, a huge technical staff, fifty teaching and research faculty, eighty superb Ph.D. students, one of the several leading universities in the world in terms of the recognition of computer science and the computing infrastructure. I came to Columbia, where there was no computing infrastructure. There was a computer center that in those days had perhaps a couple of IBM mainframes, and I think one DEC 20. The whole engineering school had one 11/45 computer. Carnegie's Computer Science Department had
fifty computers. It was a real change. What was enormously helpful was the fact that Peter Likins supported me. I had a vision of what I wanted to do at Columbia, but it was very hard at the beginning. I hired the first secretary, Norrie Brassfield. She sat in an office across the hall, a small office without a window, which she shared with a graduate student. I had to do everything by myself; there was nothing here. Not only was there no infrastructure at the university in terms of the computing, but of course there was nothing in the department. I would battle with the personnel office. I would write my ads with a certain flavor: I didn't say I wanted a good typist, but said I wanted someone smart or motivated to work in the exciting area of computer science. Those weren't the words, but that was the flavor; I was looking for smart, motivated people, and the personnel office wanted me to say how many words of typing I wanted. I would battle with the personnel office. They'd change my ads, and I'd make them run the originals. I would ask for stationery from the publications department, and they made mistakes. I spent hours with the publications department trying to get stationery and so on. That's the way it is when you start.

What had been the earlier history of computer science at Columbia? IBM's Watson Lab had been there but it had been closed down in the late 1960s, early 1970s. Maybe it was inevitable once IBM decided to open Yorktown Heights, but I think in those days nobody at Columbia cared. And there was a CS effort, as part of the Electrical Engineering and Computer Science department, and another effort within Mathematical Statistics. The two departments would fight with each other over people, and nobody could do any effective recruiting. There was a national survey done about a year or two before I got there by Conway, at Cornell, who did a study of seventy departments in Canada and the United States. Out of those seventy departments, Columbia's program - there was no department - was ranked sixtieth out of seventy on a par with Simon Fraser and the University of Calgary. That was how computer science at Columbia was viewed.

There was some rather bad blood between the two efforts, Electrical Engineering and Computer Science, and Mathematical Statistics. I inherited the faculty from those two departments and formed a new department. I must say here, looking ahead, that the split turned out to be very successful. Since that split, computer science has prospered, and electrical engineering has boomed. Next Wednesday, there will be an announcement that the Center for Telecommunications Research of Electrical Engineering has won a national competition for a center of excellence from
the National Science Foundation. Columbia and MIT are each getting twenty million dollars. They are the two
biggest winners. That department has simply taken off, so in fact, it turned out to be best for everybody.

There were three senior faculty here, Steve Unger and Ted Bashkow from Electrical Engineering, and Jonathan Gross
who came from Mathematical Statistics, and about eight assistant professors. My sense was that not one of the
junior faculty belonged in the department. I am very unhappy unless I am in one of the great places. I mean places
like Bell Labs, Carnegie, Berkeley, and Stanford. I love to have excellence around me and get really unhappy if I don't
have that. I wanted to build a great department. I felt that's what I fought for. The fact that I had a chair wasn't a
victory; that was just $t=0$, where you start from, and I wasn't about to sit back on my chair. The job was to build a
great department, and unbeknownst to Columbia, to see to what extent computing could change one of the great
research universities. My sense was that the Ivy League had enormous excellence, but that they really didn't know
what to do with computer science. It's changed very much in the last five or six years; the Ivy League has really
taken off. It's really being viewed as a major discipline. I always had the picture that this was as important a
discipline as physics or mathematics, but that certainly wasn't the vision of Columbia. My task was to convince one
of the great arts and sciences universities in the United States that computer science was - I sometimes refer to it as a
liberal art - really central. Furthermore, to help change the electronic infrastructure of the university was also on my
agenda, although that's not what I was hired for. In some ways, that's more interesting and harder than building a
great department.

ASPRAY: So, with the possible exception of Peter Likins, no one anticipated that you'd really build up a real center
of excellence. Is that right? Or did they have that in mind?

TRAUB: I think that was the hope, and Norman Mintz was certainly very helpful. The College of Arts and Sciences
contributed five positions, because they gave up computer science when it was put administratively in the
Engineering School. They must have had some hope and some sense of new development. You don't hire the
chairman of perhaps the best department in the world without the hope it's going to be a repeat. I can also tell you
that Peter Likins said to me back in my first year and I once reminded him that he had said this, "Now Joe, you're
getting thirteen positions." Don't come back for more." Well, today we have something like twenty-four positions, a
new building - we need more space - we're now talking about a new building (not just for us, but for Electrical
Engineering and for research). I didn't know how big Columbia would be five and a half years later; we succeeded
even beyond my expectations.

I wasn't afraid; I was sure I could do it, although obviously things had to break right, and it was not easy. But
coming from Carnegie, I had enormous confidence that I knew the best, that I had a taste for the best, and that I could
build a great department. Again, of course, some things had to break right.

ASPRAY: So what were the steps you took?

TRAUB: Here I was, with this one little office. There was faculty scattered around some other offices, but in terms of
headquarters, one office. It was a rather dark office up on the thirteenth floor of the Mudd building. There was no
structure, no staff, no machines. The first thing I did was that I told Jonathan Gross, who's now my Associate
Chairman, "We've never been able to recruit faculty to Columbia. We have to get rid of all our junior faculty."

He said, "You can't do that." We've got two thousand students to teach this year, and next year 2,600. How can you
get rid of all the faculty with so many students? You can't recruit."

I said, "We're going to recruit, and we're going to get the best. The only way to get the best is not to keep the people
who are here now." They were good people. They went to some very fine institutions. I was going to compete with
the top institutions in the United States for absolutely the best fresh Ph.D.'s for our faculty - that was the only way to
build the department.

Within two years, every single untenured faculty member was gone, eight of them. I just brought in a whole new
faculty. Essentially new faculty. There were only three people, who were tenured, who were left from the initial
faculty. There were no Ph.D. students, so we formulated an elite Ph.D. program, separate from the Master's, that
today has sixty students. Today, we have a faculty of twenty-four. We get on the order of six to seven million dollars a year in outside support. There's the money from ARPA. We've been designated the New York State Center for Computers and Information Systems. A huge gift from IBM is about to be given to us. There is a major equipment gift from Bell Labs, gifts from Hewlett-Packard, etc. In my view, after the top three, there is a group of universities that includes perhaps ten universities: Berkeley, Cornell, Yale, UCLA, Illinois, schools like that; that's not a complete list. We're in that group today. We know who we're competing with for faculty positions, and for Ph.D.'s. That's how you measure how good you are. We lose more than we win to the Big Three, and we beat almost everybody else.

In that first year, we held colloquia for potential faculty members. The previous year, apparently, they would have two or three people in the audience, Jonathan told me. We managed to get ten or twelve people in the audience that first year. We hired some wonderful faculty who were to be crucial to the building of the department, because people are everything. We had to get the faculty, that was crucial. You also had to have administrative staff, technical staff, and students; but the faculty is what makes the department. Sal Stolfo, who has just spun off a company that's based on his work called the Fifth Generation Computer, apparently agreed to come here one hour after I said I was coming. That is, Jonathan Gross called him and said Traub is coming, so Stolfo said okay. The next year we recruited David Shaw, Yechiam Yemini and Michael Lebowitz. John Kender came the following year. Then by the fall of the second year I was able to go down to DARPA and make a presentation with my young faculty. They said it was the best presentation they've ever heard from anybody they were not supporting. Bob Kahn said, "We're going to support Columbia." We became one of the five university computer science departments with the largest amount of DARPA support: Stanford, MIT, Carnegie, Berkeley, and Columbia.

Another job was to convince Columbia of the importance of computer science. I spoke to the President, the Provost, the Dean. I was invited to talk to the Board of Trustees. I tried to convince them that this is the discipline that will, as far ahead as I can see, make the difference. It will simply revolutionize science, the humanities, the arts. It is no coincidence that a university like Stanford has come up in the national standings. Now Carnegie, which was a fairly ordinary university in the early 1970s, almost a technical institute, is today always on the list of major universities. I
believe computer science, both institutionally and intellectually made a profound difference. For instance, it's not just a matter of being able to solve certain problems posed in physics, chemistry, and biology and so on. The modelling in those fields has actually been done using computer models, computer paradigms. Some of the top physicists in the world are now using computers. Nobel laureates, like Kenneth Arrow in economics, are using computer models in their thinking. That all had to be made convincing to a university where computer science was almost unknown, and rather looked down on. One of the things of which I was absolute determined was that we were going to be one of the elite Columbia departments.

Peter Likins was just crucial. I've talked about how important the meetings of Allen Newell were to me. I used to say, "The best thing about being head of the department at Carnegie-Mellon is that I have unlimited access to Allen Newell." When I would feel like chatting with Allen Newell, I could spend an hour with him. That's wonderful. It was the same with Peter Likins. The high point of my week was when I'd have a whole agenda prepared - I always do, whenever I meet the Dean, I don't want to waste his time, I have a list of things I want to discuss - and I'd walk into Peter's office and we'd go bang, bang, bang, bang, down a list of ten or fifteen items, and it would be settled. It was such a pleasure to be with the man. He's such an elegant speaker, a man I just admire profoundly. I just feel enormous affection for Peter.

At the end of my first year, Columbia selected a new president. Bill McGill stepped down, and Michael Sovern, the Provost, became the President. He designated Peter Likins as one of the three provosts at the university. We had an acting Dean, Ralph Schwarz, and for two years, while Ralph was acting head, Peter was the Provost. With Peter as Provost and me as the head of the department, I had to be very careful that Peter didn't feel that I was somehow abusing my relation with him. Yet he was wonderfully supportive. He was tremendous as Provost, getting us the money for the building and so on. I'll come back to the building, because it's very important to the department. Peter and I, in perhaps November of my third year at Columbia, went out to Exxon to make friends for the university. We got there early and Peter said to me, "Well, we're here early. I was going to tell you later in the day, but I'll tell you now. I've not yet announced this to anyone at Columbia, but I have decided to accept the Presidency of Lehigh."

Then we had the meeting. We drove back in separate cars, and I was crying the whole way back to Columbia.
That Christmas was as bad a time as I can remember. Three things happened, three losses. I'd lost Peter, who had been enormously important to me. It had been important to have the support of someone like that first as Dean and then as Provost, especially since one feels so vulnerable at the beginning when one starts with a new department.

You are so dependent on one or two key people. I'd lost Peter, and I had tried to recruit my former student, who know is an extremely distinguished professor, H. T. Kung from Carnegie. I offered him a chair that we had available here. He looks even better today than he did when I tried to recruit him. He's the father of systolic arrays, a man with an enormous reputation in theory and in hardware systems. I went away skiing at Christmas. Before Christmas I thought I had him, but I was nervous about it. I went skiing, and what was weighing on my mind were three things: I was depressed at Peter's leaving, and I just had a sense it wasn't going to work with Kung even though he told me he was coming. It had been an incredible coup. I had announced to the faculty just before I went skiing, "We've got Kung." I'd done all the negotiations in secret, and I announced he was coming. The third thing was that martial law was declared in Warsaw. My closest coworker, Henryk Wozniakowski, who's now sitting in the office directly opposite was there at the University of Warsaw. I was out of touch with him and desperately afraid for his life. I was sure he'd been interned and that he was in a concentration camp. He'd been very active, pro-Solidarity. It was the most depressing time. Pamela was not with me; she was in California with her parents. I was so depressed, and thinking of it afterwards, it was that triple loss of Kung, of Peter Likins, and of Henryk Wozniakowski. Kung is still at Carnegie, I did lose him; Peter has gone to Lehigh. I have had very strong support from the Dean of the Engineering School, Bob Gross, and the Provost of the university, Bob Goldberger, but none of that did I know at Christmas of that third year. Wozniakowski, in fact, is now here.

To sum it up, it was much, much harder than Carnegie. At Carnegie, I had a sense when I went there that a national treasure was being placed in my hands; I was the guardian and I had to keep it. Carnegie had lost half its faculty, and was terribly vulnerable in those days, and it was hard to build. But there was enormous wisdom in the department and enormous political clout. We were certainly the most powerful department at Carnegie. Then I came to Columbia, where there was nothing, and everything had to be established: the culture of the department, the attractiveness to Ph.D. students and faculty, getting the funding, and convincing the university that this was
enormously important. It's a competition. Only a couple of departments can get major resources. Almost no university these days has the resources to investment in more than a couple of departments. To convince this university that that should be computer science was pretty hard.

I'd like to turn to the building. I was determined that there was going to be a beautiful environment. Carnegie had something called Science Hall, which is a rather ugly building from the outside, almost brutalistic, but a very comfortable place to be in. I was determined to build a good building; I felt the building was very important, and the Mudd building which houses the Engineering School leaves something to be desired. I managed to convince the university to let me pick the architect. Jim Polschek, the Dean of Architecture, gave me a list of architects that he recommended, and I looked at their work. I didn't interview them or look at slides or anything - apparently that's the way competitions are done. I just wanted to walk into their buildings and see how I felt. And I picked the firm of Mitchell, Giurgula. Mitchell, Giurgula had done the Biology building, Fairchild.

Shortly after I picked them, and we'd started developing the plans for this building, the tiles started falling off Fairchild. The university said you, "We may have a lawsuit, so you cannot continue to deal with Mitchell Giurgula."

After nine months of working with them, and developing the program, I was told I had to look for a new architect. So I started a new search. This is the way you spend your time when you're building something new. You do everything: you're doing your research; you're making sure some of your colleagues can stay in the United States, so you're battling with the Immigration Office and the Labor Department; you are recruiting students and faculty; you are building your staff because that's crucial and gives you leverage; you're picking architects. You do it all.

I did another search and I picked Kliment and Halsband. I picked them because I really liked how warm their style was - I walked into some office space they had renovated downtown. Secondly, I picked them because they were very small. If I picked Pei or Johnson, I'd be dealing with the second or third level people. I picked somebody whose work I really liked - it just made me feel good - whose clients obviously respected them, and whose success at Columbia would make an enormous impact on their professional careers. And that's just what was to happen. They built this gorgeous building for us. The building has won numerous prizes. It was written up by Paul Goldberger, the
architectural critic in the *New York Times* recently. It's been written up in many architectural magazines, it's won prizes, it has been described in *New York* magazine as one of the several most beautiful academic buildings in the country, and the most beautiful building built at Columbia since McKim, Mead and White. It's just a wonderful place.

I didn't want the architect to make a statement, but I wanted to make the statement. The statement was, "Computer science and technology is a human undertaking." Science and technology are not cold and sterile, but are very human. The building has to be human and comfortable. The building's enormously important to me. It's been very important to the department in terms of its identity, and for our recruiting. In every way, people just love being here.

A little over a year ago, we had a convocation for the building. Ten friends that I respect gave talks at a two-day celebration of this building. Columbia gave an honorary degree to Herb Simon. It was a splendid occasion.

ASPRAY: Now, shall we move to Berkeley and Stanford?

TAPE 2/SIDE 2

TRAUB: I spent a very significant semester at Stanford. I showed up there in January, and I was there until the end of July 1966. At that point, Stanford was already very well known, even though the department had only been formed in 1965. When I got out there, what was particularly striking was that the place was held together with chewing gum and string. The Computer Science Department was in a low-rise, two-story wooden structure called Polya Hall. George Forsythe, for whom I had enormous respect, was the founding chairman. He died of cancer in the early 1970s, when he was still quite a young man. What's remarkable to think is that when I went out to Stanford, George, who seemed so senior to me and for whom I had so much respect, was considerably younger than I am today. Yet he seemed like such a senior man, and of course he was. That was twenty years ago. He had extensive files on research ideas, had a stream of outstanding students, was founding chairman. Nonetheless, he was a
somewhat remote man. He was not a man into whose office you'd drop and chit-chat. But he was very much respected and admired by his colleagues. He was one of the men who started academic computer science. George is remembered in all sorts of ways with various prizes, and competition, and so forth. The other people who were there were John McCarthy, whom Forsythe had gotten fairly recently from MIT, Ed Feigenbaum, whom he had brought down from Berkeley, and Gene Golub, who had been hired as a fresh Ph.D. student. Golub was on sabbatical that year. I think those were the key people. And there was Klaus Wirth, the originator of PASCAL and Euclid.

Probably one of George Forsythe's major mistakes was that Klaus Wirth did not get tenure at Stanford. The reason is that he had had four students working on his projects, a compiler project. When Forsythe heard that not one of the four students would get a Ph.D. thesis out of this work, that they'd simply been working on the project, he was upset enough to indicate to Wirth that he wouldn't be keeping him. Wirth has turned out to be one of the most influential people in the world in the design of programming languages.

There was a budget of a hundred to a hundred fifty thousand dollars. Yet Stanford was already a great mecca. What's remarkable is how strong it looked from the outside and how fragile it was. That was to be my feeling when I got to Carnegie in 1971, how fragile it was. You lost one or two key people, and the department was subcritical. You were that thin. This was, by the way, before Don Knuth and other luminaries went there. Of the people who are now at Stanford as major figures, only Golub and McCarthy were then on the faculty.

Finally, I'll talk about Berkeley. I've already indicated the beginning of the Berkeley story, which is that they had a mammoth battle between two departments, and that was a disaster. All the energy was going into fighting each other. That was finally resolved with the Engineering School getting an EECS department, with a CS division. I decided early on that I was not going to spend my summers at Carnegie; that I had to get away if I was going to keep being active in science. I spent my first summer, the summer of 1971, at Carnegie; I felt I had to. The summer of 1972 I spent in Boulder at the National Center for Atmospheric Research. The summer of 1973 I spent in Don Knuth's office at Stanford. The most interesting anecdote of that, perhaps, is that I was looking at Knuth's files one day, because I'm an admirer of his, and it turned out that the first paper he ever published, and he had a reprint of it, was in
Mad Comics. It was something he had done as an undergraduate at Case Western. In fact, I took a copy, left him a note, and put it in my Knuth file. It was when he was off in Norway, and one of the things he did there was write "Surreal Mathematics," the one piece of fiction he's done.

Then Dick Karp invited me to Berkeley. Pamela and I liked Berkeley so much intellectually, and as a place to be, that we went back, for a while, every Christmas and summer. Now I go in the summers and, in fact, we bought a place out there about 1975 or 1976. I have a very strong relation with their department. When I am involved with a place I always try to help institutionally.

I should say, by the way, before I drop the subject of Stanford, that I've had a long relation with Stanford. For instance, from 1972 to 1975, I was on their Presidential Advisory Committee on Computing and chaired the Committee my last year.

At Berkeley I tried be helpful. I would always meet with the Chairman of the Electrical Engineering and Computer Science. There was a sequence of outstanding chairman: Tom Everhart, who's now the Chancellor at the University of Illinois; Dave Sakrison, who died, unfortunately (we had become quite good friends); George Turin, who's now Dean of Engineering at UCLA; and the current chairman Don Peterson. I always tried to give them some discreet advice on what would be good for computer science, what would be good for Berkeley. I tried to do the same for the people who were the heads of the Computer Science Division, and I told them that Berkeley was outstanding in theoretical computer science. They had Dick Karp and Michael Blum, who are simply superb. But, they essentially had nothing in experimental computer science. I thought it was very important for the future. If they wanted to play in the big leagues and play with the big three, they had to do experimental computer science and, in fact, they had to have large projects. There were things that they simply couldn't do: one or two people could not design and build a machine, or work with operating systems, software, and so on. They needed larger projects. I kept hammering away at that. I don't know to what extent I can take credit for that, or whether it was inevitable, but they hired a number of very good people from Carnegie-Mellon, they brought in experimental computer scientists, they started getting ARPA funding. Today Berkeley is one of the top ARPA contractors and is a major force in experimental computer
science. I was carrying the Carnegie-Mellon message that computer science is not just a theoretical subject, that you must do experimental computer science also, and that that generally requires major funding, and large projects. That certainly happened to Berkeley, I don't know to what extent I can take credit or whether it was inevitable.

I showed up in Bell Labs in September of 1959. I had an enormous longing to do significant research. There must have been something about the labs that gave me that. That is, I realized as I was doing my Ph.D., that I had the skill for doing research, which I didn't know I had. I really liked it, and seemed to have a gift for it. I had this enormous urge to do something significant. The sense one had at Bell Labs in those days, say from 1959 through the mid-1960s, is that it was one of the couple of places in the world for doing research in computer science. It had an enormous sense of excellence. IBM's Yorktown Research Center was set up later.

One really had the sense that we were certainly one of the two best non-academic centers in the world. There was Bell Labs and, later, IBM research. Remember, that there was almost nothing going on at the universities. There was no department at Stanford, or Carnegie, or any place. So one had the sense that it was as good a place as any in the world for doing research in computing. In those days, there was no computer science department or computer science colloquium. There was simply a group within the mathematics department. The head of the mathematics department when I first got there was named Prim, who was to be succeeded shortly afterwards by H.O. Pollock. Within the department there were some groups. One of the groups was doing some work on computing. There were people in it like Vyssotsky, now an Executive Director at Bell Labs, and M.D. McIlroy in that group.

When I got there, we had an IBM 704, then a 709, then a 7094. What one had of course were IBM machines, because all of the other places had IBM machines, and that was the way to be in touch with them. Then, in the early 1960s, we got a GE machine and got involved in the Multics Project. I was a very junior person, and essentially devoted my energies to doing research. Even in those days I did try to do some institutional things. For instance, I began a computer science colloquium. I was at Murray Hill, and there were other Bell Laboratories locations at Holmdel and Whippany, and it seemed very foolish to me that people would drive from Whippany or Homedell, which is an hour's trip, in order to hear an hour's colloquium at Murray Hill. So I set up a very primitive communication facility where we
would have talks. I would invite people from around the country that I admired to give colloquia at Murray Hill. Then we would beam those, voice only, one-way only, to Homedell, Murray Hill, and Whippany. I was simply ahead of my time. I couldn't understand why the world's leading communication company didn't seem to know what to do in that situation. But when I tried to deal with New Jersey Bell, I was treated as shabbily as any other customer - probably shabbier than most, because at Bell Labs we're the shoemaker's children. So I set up this very primitive arrangement. I had the right idea, which was of course that one didn't want people moving, but that one wanted to beam the colloquia. It really gave me an opportunity to meet interesting people. I would simply pick somebody, and they probably figured they were being invited by Bell Labs because some muckamuck at Bell Labs decided that they should be invited. Well, it was just me, a beginning member of the technical staff, who decided they had done interesting work and invited them in for the colloquium.

Towards the mid-1960s, Bell Labs decided to set up a computer science department. Initially, I stayed in mathematics research; I did not immediately move over to computer science research, mainly for some personal reasons. Then I did move to computer science research. The Executive Director was E.E. David, Jr. (who later became President Nixon's Science Advisor, and is now the President of Exxon Research). Sam Morgan was the first director of computer science. He was essentially an applied mathematician who had never done any research in computer science. I moved into the Computer Science Center and headed a small research group there. In 1966, I went for a sabbatical out at Stanford. I think it was inevitable by then that I would go to a university; it was just a question of when I would go and where. It was finally to be in 1970 that I took a sabbatical at the University of Washington. In 1971 I was to become head at Carnegie.

I'll tell you one amusing anecdote about my leaving Bell Labs. I went to see Ed David to tell him that I'd decided to go out to the University of Washington in the spring of 1970. And Ed David said to me, "Now, that's a very serious move. Are you really sure that leaving Bell Labs is what you want to do?" I said, "Yes, I've really thought about it, and I've decided." A couple of weeks later, he left for Washington to become the President's Science Advisor.

END OF INTERVIEW