

An Interview with

EDWARD FEIGENBAUM

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Conducted by Pamela McCorduck

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Edward Feigenbaum Interview

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Abstract

Feigenbaum discusses the formation and growth of the Stanford University Computer Science Department and its acquisition of facilities. He recalls how IBM and Control Data Corporation replaced Burroughs as the university's computer vendor because of the need for large-scale computing. He explains his effort as head of the Computation Center to centralize all university computing activities, and the failure of that effort in the 1970s with the introduction of minicomputers on campus. Feigenbaum also details the department's financing, including government support (ARPA, National Science Foundation, Office of Naval Research), equipment donations from industry (IBM especially), and faculty salaries. Feigenbaum credits George Forsythe for the department's initial success in key areas such as numerical analysis, systems, and artificial intelligence, and hiring talented faculty such as John McCarthy.

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EDWARD FEIGENBAUM INTERVIEW

DATE: 12 June 1979

INTERVIEWER: Pamela McCorduck

LOCATION: Stanford University (Stanford, CA)

McCORDUCK: This is a conversation with Edward Feigenbaum on the 12th of June, 1979 at Stanford University. Ed, let's go back to when you were first approached by Stanford University to join the computer Science Department. Do you remember?

FEIGENBAUM: Yes, there was nothing special about that particular event. I was an Assistant Professor at Berkeley. I was shopping around, although I wasn't terribly unhappy at Berkeley. The closest place to shop was the Peninsula; I knew John McCarthy from the work in AI and it was essentially a non-event being recruited to Stanford. It was mostly McCarthy's doing with Forsythe's encouragement and help.

McCORDUCK: You knew then that there would be a department or in fact did it exist already?

FEIGENBAUM: No, it didn't exist. If I remember correctly, I'm not positive of the dates, but I think I arrived here on the official day that the department started, January 1st, 1965. It was the spring of 1964 that I was looking and I came here the first day of '65.

McCORDUCK: So, it was all very clear by the spring of '64 that the department was going to be put together.

FEIGENBAUM: Yes, it was clear. I just don't remember much about that time.

McCORDUCK: Now, as I recall, one of the first tasks when you got here was to chair a blue-ribbon committee on the computer facility for Stanford--what its future might be.

FEIGENBAUM: The IBM 360 was announced in the period of, I guess it was post '63, maybe '64. Forsythe had had numerous discussions with people at IBM about moving into the next generation of IBM equipment. Stanford had an IBM 7090 which was the workhorse of its computing at that time and it was very happy with it and it was also very happy with a variety of arrangements that IBM had at Stanford at the time, including collaboration with the Scientific Center. There was even a room assigned in Polya Hall for a collaborator from the IBM Scientific Center to be resident there so...

McCORDUCK: Was it Gene Amdahl then?

FEIGENBAUM: No, not that I can recall. It was not Amdahl, it was somebody whose name I can't remember. A person who was in the numerical analysis area and worked with Forsythe. So, it became time to think about an upgrade of equipment and that led to the appointment of a President's Committee on Computing which I chaired. The reason I chaired this was that very soon after I got here, I discussed with George Forsythe the possibility that he would step down from one of his two roles -- Computer Science Department and Computation center Director, and give the latter one to me; I was interested in that job, and he had expressed some concern about doing both of those positions in a time when the Computer Science Department had to grow. So, it was kind of a try-out period: if I could run this committee well, and everyone was happy with the way it was going, then maybe they would offer me that job, which they did later on in the summer or early fall, something like that. The committee met with representatives from a lot of different computer companies. In the end was disappointed with presentations it heard from Burroughs. Burroughs equipment was in here early; there was a small Burroughs machine followed by the B-5000. The B-5000 was a much beloved machine by the people around here because of the way it handled Algol and the fact that its operating system was written in a version of Algol. The Burroughs people did a fairly poor job of selling. The IBM people and the CDC

people both offered Stanford alternatives that seemed to be pointing in the direction of big computation which is what we saw happening at Stanford, so we considered those in the end most seriously.

John McCarthy was pushing for a Digital Equipment alternative in the form of a PDP-6. We didn't lean in that direction; the AI Laboratory itself moved in that direction which eventually led to the PDP-10 and multiple PDP-10s around the campus. We went off in a much more standard direction -- IBM direction again with serious disappointments along the way.

The deliberations of the President's Committee, as I remember it were non-acrimonious, but they did show up a tension between the computer science interests and what I would call the more mundane interests of the engineering school and the other big users. You might call them the big Fortran number crunching interests. but, we assured them that the computation Center would run as a no-loss cost center that would be highly responsive to the needs of the big users with big budgets who buy computing and that the fact that a computer science professor was going to run the thing was not going to change that and, in fact, that's the way it turned out.

There was another tension which got decided one way with a certain amount of stress and eventually ended up -- the actual situation in the '70s was that it ended up in the other direction. That was the tension between the large central computer and the mini-computers located in departments. the mini-computer revolution was essentially inevitable. It was going to happen -- it was going to affect Stanford. We chose not to let it happen in 1965; we opted for a strong centralized computation center idea because of the presumed economies of scale of the large central machine, in hardware, in our ability to attract gifts and discounts, and do on. Therefore, during my period as Computation Center Director, I essentially was engaged in a continuous battle against the mini-computers, and eventually lost for all the right reasons. I lost, but it was struggle, and mini-computers dominate the campus now. There are a very small number of large computers and well over a hundred mini-computers of the PDP-11 variety.

There was also a big decision made at that time to bring all of the -- not all of the, but to bring a substantial number of the large computer centers under one direction; to try to realize the economies of scale in software and programmer talents, systems software. We therefore pulled together the Campus Computing Facility which acquired a so-called time-sharing system, a model 360/67 which was a big disappointment, a very large number crunching machine at SLAC, 360/91, and an experimental time-sharing 360 Model 50 in the Medical School. The latter was prompted by Josh Lederberg's motivation to get the Medical School into the computing business, the NIH resource sponsorship. The idea was to make the system very friendly to users and therefore we wanted to establish some nice, comfortable, convenient time-sharing system. We did it on the simplest model we could think of, which was a large memory model -- a mass storage with a mass main memory was available on the 360/50, called bulk core at the time and we invested heavily in bulk core on the 360/50 and built a time-sharing system called ACME. That was done by Gio Wiederhold and a crew that he brought in. I had known Gio from Berkeley and hired him down to Stanford to run the Medical School computing facility.

The SLAC Computing Facility was run by another Comp Center Associate Director, Chuck Dickens, who is not SCIP Director at Stanford. He was Bill Miller's right-hand man in building up the SLAC Computer Group. The Campus Facility was run by Rod Fredrickson, another person I knew from Berkeley, from the Survey Research Center at Berkeley. He coined IBM and was a local sales representative during the acquisition process for these machines and subsequently I hired him away from IBM to actually be a facility director here and he ran the 360/67 shop.

In retrospect, I would say that seeing how things actually turned out, I probably made a serious blunder for Stanford in not going the route McCarthy suggested in that although we did satisfy the very large IBM need for computation and we did integrate the facilities, the long delay in IBM's time-sharing software which has essentially persisted into the present time; that is, there is essentially no good IBM time-sharing system, meant that the majority of Stanford's users were deprived of that excellent development well into the '70s, until the time that McCarthy was able to convince the University to fund the LOTS computer facility. And even so, very few of our users on campus except the students who

use LOTS, really get the kind of excellent time-sharing service that the computer scientists via the AI Lab and Sumex have had for years. In retrospect, maybe that was the time to have moved toward time-sharing although it was hard to see at the time and knowing the IBM alternative was a sensible and conservative route and everyone wanted IBM cycles, IBM was certainly the provider of the most generally available and most widely available software. We used to think of it as their being an immense sea of software in which we could swim, in which we wouldn't have to invest heavily. By we, I mean not only the whole campus, but SLAC and the Medical School as well. But I feel sad that the excellent time-shared operating systems that came along on the PDP-10 and the software that got developed for it were not available at Stanford for quite a long time. We weren't the only ones who made this blunder: Carnegie-Mellon went to a 360/67, Michigan did the same. A number of key computer science places at the time went in that direction.

I'm not at all sorry that we didn't go in the CDC direction. I don't see anything particularly interesting or excellent happening along that line.

I don't know what else I want to say about the Computation Center during those years. It was an exciting time but one full of a great many tensions as IBM struggled to get its act together, its software act together. After the introduction on the 360s, there were long delays of all kinds, particularly software delays. The time-shared operating system for the 67, TSS 360 was just a bust. I take some pride in the fact that we predicted the bust on the basis of a Ph.D

thesis that one of my graduate students did, a Business School graduate student called Norm Nielsen. I set Norm to producing a simulation model in very great detail of how that TSS would operate and Norm's thesis runs, his simulation predictions done in the summer before the big bust was announced, showed that it would perform very poorly and Norm's performance predictions were borne out. Norm knew from his simulation precisely why this system would perform poorly, made numerous trips to Mohansic, New York, where that operating system was being developed, was unable to convince the IBM people doing it to what was going to go wrong with it. They just didn't believe the simulation model and somewhat later, months later, they announced that the system would be long delayed, was not performing properly, and because we had warning about this from our own simulation model, we were all geared up to

move quickly to the standard IBM operating system, OS/360 and to paste on some quasi time-shared adjuncts to that system. One was a text-editing system adjunct called Wilbur which is now quite well known; it proliferated all around the country. the other was a mediocre time-shared adjunct for running programs called Orville, and that never really flew at Stanford; it was too inefficient.

I think a couple of personalities stand out in that whole situation. One was Rod Fredrickson himself who is an amazing systems person and manager. Rod has had a varied career since then; he just changed jobs and I don't know where. He's in Washington somewhere and I don't know where. He went to RAND from Stanford and ran the RAND computation center for some time until becoming a Vice-President of Mathematics in Princeton. But Rod was a very, very strong personality. Very difficult to work with, very easy for a superior to control, very difficult for a subordinate to live with, but was so phenomenal in his understanding of the IBM 360 and the OS/360 that he just had enormous respect from his staff and therefore although they were motivated daily to quit because of how heavy a hand he used in managing them, they wouldn't because of the tremendous respect that they had for him. so as a consequence, the whole staff stuck together through the late '60s when Rod was running it. Rod was just absolutely an amazing systems person. I've never met anyone like him ever.

The other very strong personality that showed up at the time was an IBM salesman named Jack Merkin who made it his job to understand us, understand our operating environment, our fiscal environment, our academic environment so well that he just was considered one of us, just a spectacular performance by an IBM salesman. He also understood the system very well. So from our point of view he was a total no-bullshit guy, just absolutely perfect IBM link and subsequently rose rapidly in the IBM organization until he was the Assistant to the President of IBM, so Jack's office was in Armonk. Jack was slated, I think, for big managerial stuff at IBM but quit the headquarters to become a Vice-President of a software firm in San Francisco and the tragedy is Jack was killed last Christmas. He drowned at Acapulco in a rip tide. So he's gone. But he was just spectacular.

McCORDER: Maybe we should talk a little bit about the fiscal environment. when you look in the archives, George Forsythe's papers, particularly notes to himself, are full of complaints about how essentially niggardly Stanford was treating him about funds -- continually feeling under-nourished, under-funded and he was constantly fighting for money. Now, I'm curious to know how you managed to convince Stanford that it should go to the next generation of machines and then how you proposed to fund that next generation.

FEIGENBAUM: I can't answer that question in detail because it has been so long and I don't have the numbers in front of me. It is true that Stanford did not invest a lot of money in the growth of computing at that time. Stanford was investing a lot of love and giving people openings and opportunities to make it on their own if they could, but was investing very few dollars. It seems to be a Stanford philosophy not to invest dollars, to invest a lot of love. Currently, for example, the sponsored research of the Computer Science Department exceeds its University budget by a factor of 10. The University essentially is putting in very little money to the most prestigious department in the country. It is providing the lubrication and the opportunity to go out and grow with other money, particularly the government money. that's essentially what Stanford did for Forsythe at the time. It opened the door to growth, but it didn't provide much money for growth. Well, how did the Computation Center grow? I don't know the details of how the original Burroughs machine was acquired. I do know that the IBM -- well, maybe I can reconstruct a little bit about the Burroughs B-5000 anyway.

IBM offered the 7090 as it was doing at that time at a very large discount; that was a 60% discount, if I remember what was going on at the time. Stanford took advantage of that. It also received a large gift from IBM to motivate the other 40% of the cost of the machine and to motivate the software and staff and all that. If I remember correctly, that was a million dollar gift in some variety to Stanford and I believe the local mythology has it that Forsythe took the million dollars and bought the B-5000 and that was the way we got both machines in here; the machine that was necessary for the Fortran number crunching and keeping up with the rest of the world and the machine that Forsythe really wanted, which was the B-5000.

Now Stanford has a very large engineering school. It has large and well funded science departments so it was possible to project into the future both when Forsythe was doing it and when I was doing it, a growth of sponsored research revenue that would be able to support the machine. We set up the Computation Center; I can't remember whether it was in Forsythe's era, I believe it was, or it might have been in my era, but we set it up as a separate cost center, operating as essentially a non-profit business within Stanford University. We then had to do a business analysis of what the expected revenues would be. We had to establish a depreciation schedule for the machine, judge what the rates would be for the machine, what the staffing would be, what the overall budgets would be. In that process I found the Stanford financial people extremely helpful. I can think of two critical people -- one was the Dean of Research, Hugh Hefner, and the other was the Controller, Ken Creighton, who were just extremely helpful in planning every detail of the budgeting and management of that cost center over time. And in the end it looked like, with a certain Stanford subsidy, and I'll explain what that was used for in a minute, and with a sponsored research revenue growth projected from the 7090 and B-5000 era plus a well-discounted machine, (I just don't remember what the discount was, but I believe in the 360 era) plus some kind of IBM support and again I don't remember the details of it but a thing that started out as IBM buying back time from Stanford, a so-called IBM buyback, for the Scientific Center in Palo Alto, ended up by being essentially a gift of that time to the Stanford students in one form or another. The details escape me. The details for the historical record are all present in depositions that Bill Miller and I and others gave at the IBM-CDC suit, or maybe it was the IBM-Telex suit. But on record there are depositions that describe that whole story where I researched the whole thing in some detail. that is a matter of public record, I would guess in that case. Although, that's a very good thing for the Babbage Institute to look into -- as to whether the out-of-court settlement required that all the documents go back and be shredded. I'm not sure about that and if so, then an enormous amount of history has been lost. If IBM and CDC agreed to essentially can those documents forever, then some of the best history of this whole period has been lost. I don't know about that; that will have to be looked into. But those were a matter of public record in the Court of New York at some point.

So, we worked out a financial plan. The reason I postponed telling you about the Stanford subsidy was because it's worth getting into a small piece of the history of the costing of computer centers at that time. I think one of my major contributions as computer center director in that period as turning around the entire federal government on an issue which I regarded as absolutely stupid economic policy on the part of the federal government. It was causing enormous economic dislocations in the University. We spent immense amounts of time turning that around and eventually succeeded in turning it around for Stanford. It was almost a Pyrrhic victory as far as other computer centers were concerned because very few other computer centers picked us up on it.

Let me describe this briefly because I think it is important for the record. Under the Circular A21 of the Bureau of the Budget which is what the Office of Management and Budget was called at the time, BOB Circular A21 required that university facilities like wind tunnels or computer centers, large university facilities, be charged to the government in a scheme that was called average cost pricing which meant that after the fact, the hours of utilization of the device would be divided into the cost of running that device for the year (for the period, generally a year), that would give a dollar per hour figure and that was the figure that in retrospect projects should have been charged. It was average cost pricing and it leads to anomalous situations like the fact that Washington National Airport is very heavily used and therefore its landing fee is very low, thereby encouraging more flights to land there whereas at the time Dulles Airport was very lightly used and therefore its landing fees were very steep, thereby discouraging airlines from using Dulles Airport. That's the anomaly. In the computer situation at Stanford what it means is that in the early period of the acquisition of the machine, when few people know how to use it, the rates are incredibly high because of low utilization and therefore people are discouraged from using it. In the latter years of the machine if they've managed to get over the hump, and utilization is high, as the machine approaches saturation and one reaches out for the next machine, the prices are incredibly low, thereby contributing to super-saturation. An extremely stupid policy. It's a well-known stupid policy. It's in the early chapters of every economic textbook that average cost pricing introduces anomalies.

In addition, the government was insisting that the University close its books on the operation once every twelve months. Why twelve months? Well, that's a nice period. It's convenient and auditors think in terms of twelve months and you have to understand that the policies were in control of the auditors, not in control of the government economists. The economists know how stupid this operation is but they're not in charge. The accountants are in charge; the so-called bean-counters are in charge. Well, what we needed was a situation in which we set prices. The prices were those prices which would encourage the proper level of demand; the proper level of utilization of the various services. Even though we might lose money in the early years of the system, we ought to be able to make money in the latter years of the system and break even over N years rather than 1 year, and smooth the load over a long period of time. In addition, I wanted a flexible pricing policy which is what it came to be called, that would allow me to adjust the prices as demand fluctuated so just to clear the market for computer services at any given moment. Taken to the extreme, what this means is that it is essentially a commodities market for computer time and as demand dropped at certain hours of the day, the price would fall, trying to stimulate demand at that time. And as the demand rose, the prices would rise accordingly. I never achieved that, even though it is possible for a computer operating system to do market clearing calculations, I was not able to achieve it because I was never able to convince Rod Fredrickson to program the system to accommodate that level of detail. He felt the complexity, the economic complexities that I would burden the user with was intolerable and he would just never do it, so it doesn't matter if you're boss or not -- if Fredrickson wasn't going to do it, he wasn't going to do it, he wasn't going to do it. But he and I reached an accommodation on matters in which we, with the government's permission, we introduced flexible pricing that would allow different prices for express runs and we taxed large computer runs in the middle of the day very steeply so as to discourage large runs (large being after 5 minutes the price would go up substantially, after 10 minutes the price would double). We fed that additional revenue back to reduce prices for night-time runs. We had all kinds of categories of service that had different prices, but those varied basically by the shift or by the quality of service; it didn't vary moment by moment as I had originally intended.

With the help of Ken Creighton and his organization of the University controllers, plus enormous boosts by some federal economists in the Defense Department, starting with a friend of mine, Herschel Kantor, who was working for Alain Enthoven in the Office of the Secretary of Defense for Systems Analysis. Remember that this was the time of Robert McNamara's "whiz kids," the economists and operations researchers brought in under the Systems Analysis rubric. Alain Enthoven was in charge of those economists and through the help of Herschel Kantor and Arnold Moore. Herschel was an old friend of mine from Carnegie Tech days, Arnold Moore was a colleague of mine at the Assistant Professor level at Berkeley in the Business School. Pat Parker was another one; Pat was also Assistant Professor colleague of mine. Those people wrote a paper on flexible pricing and the anomalies of average cost pricing in computer centers at universities which appeared in the Journal of Business at the University of Chicago. That armada of intellectual clout and political clout working through Alain Enthoven who worked through Robert McNamara who was capable of understanding the economic issues, got the issues firmly in the focus of the attention of the Bureau of the Budget which got them under prodding from Ken Creighton's association, to make a concession to a thing called the Stanford Pilot Agreement.

In the Stanford Pilot Agreement we essentially won everything we had set out to win. It was supposed to be a pilot agreement for a test to see if other universities should go in this direction. Other computer centers knew about this and should have jumped on the bandwagon. They would have been granted pilot agreements also. Essentially nobody did. Essentially everyone was interested in sheltering themselves under the cost-clearing umbrella of knowing that federal grants and contracts would somehow clear the cost of the center every twelve months and they would never have any hassle. They wouldn't have to worry about an N year period; they'd only have to worry about a one year period. I think that's the reason they didn't jump on the bandwagon, but I'm not sure.

In this agreement Stanford had to agree and was quite right, that all user whether federally sponsored or not, would be charged the same rate. No matter what cost schedule we established, within that cost schedule federal projects, Stanford student work, Stanford internal work would all be charged the same rate which was only fair. That meant that

whatever subsidy Stanford was providing for this facility was used to establish a giant bank account for Stanford's own use -- non-federally funded use of the computer. Thus, we established a system in which this bank account, which at one point was running three-quarters of a million dollars a year, would get divided up into sub-bank accounts which were given to the deans. the deans gave sub-sub bank accounts to the department chairmen who passed money out to the faculty who passed money out to the faculty who passed money out to the students for running jobs in courses. Eventually it would end up that something like \$25 per student per course of this bank account. Of course, the bank account was "funny money." It was not real money, it was subsidy money given as a lump by the University at the beginning of the year which was used to charge non-government use.

That system worked well right up until the time when essentially the initial period of computer use was over. Stanford then saw that money once again as real money. Once a new plan was established, that money would show up as new Stanford commitments in student computing and at that point they decided to invest a portion of that money in the LOTS computer facility. So that's where that money in now -- it is supporting the LOTS computer facility and it is supporting it at the level of a few hundred thousand dollars a year. I believe it has never risen back to the level of three-quarters of a million dollars; it's a significantly lower level, I think, but I'm not sure about that because I'm not into that right now.

That's all I want to say about the computer center.

McCORDUCK: Let's get back to the Department. It had a very mathematical flavor when you joined. In fact, nearly everybody ahead of you was a mathematician. You and Miller were two of the first people who had decidedly non-mathematical backgrounds. Was this a deliberate policy on the part of Forsythe?

FEIGENBAUM: Bill Miller was a Physicist who headed the Math and Computer Science Division at Argonne so Bill was much more mathematically inclined than I was. That was just to correct that statement. I think that Bill was half a

mathematician. I believe it was a deliberate policy of George's to build up the artificial intelligence area. Whether that meant that he wanted to build it up with non-mathematicians I don't know. We never discussed that. Certainly his first outreach into the artificial intelligence area was John McCarthy. He plucked John McCarthy out of the world and decided that was the person he was going to put his bets on initially. I think it was my Carnegie Tech reputation and partly my reputation from *Computers and Thought* that focussed John's attention on me. I'm not exactly sure why George allowed that to be pursued other than maybe John's interest in it. I just don't know.

George was a very, very broad person. I think that's one of the George Forsythe miracles was his ability to see across all of computer science and to spot those things which were going to be hot which perhaps were not hot at the time. Artificial intelligence was certainly a very fledgling thing at the time that George recruited McCarthy which was, I think, 1963. It was hard to see where artificial intelligence was going to go. There were very few people in the field and yet George somehow felt that was important and began to build in that area.

I think one other thing about George was his perception that computer science was an inter-disciplinary science. He himself had been involved in meteorology calculations, and many other inter-disciplinary pursuits at UCLA. I think he was therefore inclined to view computer science as a bridge discipline that would bridge between cleverness at using this marvelous new tool and the needs of other disciplines such as engineering, physics, perhaps the behavioral sciences. I think that was part of George's vision. Probably no accident that Miller was a physicist was brought here as a physicist for example.

I made a few notes about Forsythe; maybe I'll just mention a few other things about George while we're on the subject. I think the first and perhaps most critical, most important for the life of the department was the tone that Forsythe set in the department. George was one of the most beloved of all people that you will ever meet. He was a person that everyone had immense respect for, regard for, genuine emotional attachment to. Even though he wasn't a very emotional guy, he was just such a wonderful person; a person dedicated to other people and their growth and their lives

and he was very hard working, very attentive to the needs of everyone. He had such excellent taste in everything he did that people were just very much dedicated to George. He set a tone in the department which I can best call the no-hassle tone. Everything just worked so smoothly under George that everyone got the impression in the early days that whatever it was you wanted to get achieved around here, you could get achieved without fighting for. All you had to do was to talk about it; George gave you as much scope as you wanted. If there were any nascent hassles, George took care of them right away. And the department grew from the outset as being a zero-hassle place. No one ever got the idea that the Computer Science Department was somehow a political animal, that one had to fight for this or that -- just do your own thing and share it where possible. And that's the tone to the present day; it's something that other people cannot understand -- they somehow believe we're lying when we tell them it's that way and yet it's an absolutely zero-hassle place. It's not that the faculty don't care, that they're very busy doing their science, they're perfectly willing to let the department chairman worry about things provided he does it in a way that every once in a while they'll check and if it's the way they think it ought to be and it's done gently, then the department chairman has the scope that he needs to do things. That all started with being able to trust Forsythe, then being able to trust Floyd, and so I find being chairman around here a very simple job.

Forsythe also established in line with this no-hassle policy, the idea that really ultimately can be traced to Fred Terman, which is called "every tub on its own bottom." Meaning that every faculty member somehow gets himself established with a funding source, with professional research people, with students if he or she chooses to do that, and then every faculty member is a duke of his own kingdom. And that the Computer Science Department as a whole basically is a collection of these research dukedoms hung together by a common thread of academic concerns in teaching and supervising graduate students and doing the normal academic things. But that basically we were built with the flavor of a research institute, that did graduate student teaching. The department almost has a European flavor to it in the sense in which it is dedicated to computer science research, with graduate student training viewed as research apprentice training in the various projects.

In line with that, it was the intent and the policy of forsythe that we should grow very rapidly on what's called "soft" money so that although George constantly complained about the University not attending to the financial needs of the Department, it didn't stop him at all. What he did was to parlay that small amount of money as far as he could by simply putting us on half-Stanford salaries and letting us go out and raise the other half salaries through the various funding agencies, ARPA, NSF, ONR, all the various funding agencies. We were in fact leveraged 50-50 until just a few years ago when the University decided that that was risky and put us on a 75-25 base. But still, a significant percentage of the faculty members are still leveraged 50-50 on "soft" money. That allowed us to add faculty members at a time when growth was necessary in the Department. We could have taken the other path; we could have said "all the faculty members will have their salaries guaranteed 100%." That's called being on gold and we chose not to be on gold. We chose to do the risky thing, play the odds. We've never lost; we've always been swimming in computer science research money and we currently are also. We never lost by that strategy, in fact we grew rapidly, so that at one point for example, we had something like 18 bodies on the faculty with less than 10 full-time equivalents. We also leveraged ourselves by joint appointments with other departments, particularly Electrical Engineering in the systems area. We set up a joint laboratory with them, Computer Systems Laboratory, inter-departmental laboratory. That was in the mid to late '60s. I don't remember the exact time but it was a joint arrangement done by George Forsythe and John Linville, then Head of Electrical Engineering, that would allow E.E. entre to the computer area in an inter-departmental way that would involve us. Ed McCluskey was hired as the person to build that inter-disciplinary laboratory.

McCORDUCK: (Question incomprehensible)

FEIGENBAUM: Well, it did, but everyone perceived, quite correctly, that the core of computer science research was in systems research: its architecture, its operating systems, its languages, and therefore we had to build in that area. So, the point I am wanting make is that Stanford grew on "soft" money and it was a conscious decision to do that.

I also want to mention that although Forsythe gets most of the credit for this and in fact was the implementor of all of these policies, the intent to make Stanford great in computing came from a joint decision of Fred Terman's and Al Bowker, and it was that early decision which led to hiring Forsythe and the scope that they gave Forsythe to maneuver for that was really the most critical early decision.

I think also very important at that time, the mid-60s, was the build-up of support by ARPA for information processing research. That had begun in approximately 1963 when Licklider went to ARPA to form two branches of ARPA, the Information Processing Techniques Office and the Behavioral Sciences Research Office. Licklider was a very perspicacious and intelligent investor in his choice of who to place his bets on. He had placed some of his bets on a team consisting of Harry Huskey, Dave Evans, and myself at Berkeley, and on John McCarthy here at Stanford and on Al Newell and Herb Simon at Carnegie-Mellon, on the Project MAC and MIT AI Lab teams at MIT. So, in the early days of our department, we had good solid multi-year funding from ARPA to do quite avant garde things -- to do time-sharing research and do artificial intelligence research. When I moved to Stanford, I essentially became part of the ARPA Project at Stanford. That was really the seed of what is now a very large research enterprise. The sum total of the research that McCarthy and I have together in our projects is about \$2 million worth of research per annum. But ARPA's support was critical at that time.

No one should ever underestimate the importance of ARPA in the life of computer science in the period 1963-1970. Their funding essentially made the discipline what it is today and people always tend to forget that. There was a sequence of people like Licklider, like Ivan Sutherland, like Bob Taylor, like Larry Roberts, who were just spectacular innovators in terms of their perception of what this field needed and their savvy in getting the money to make that happen. And I also think in the life of Stanford, IBM was very important. I think immense amounts of support from IBM in computer hardware and research gift support in the '60s was really important for us in getting this show off the ground. And I think that's where social policies just clobbered correct scientific policy. I think the anti-trust actions against IBM which dried all of that support was just terrible.

TAPE 1/SIDE 2

FEIGENBAUM: I was going to tell you about what I saw as some of the critical decisions in the Forsythe era that the department had that shaped the way it appears to the outside world today.

One of those was a decision that was made at a faculty meeting in Forsythe's office (we were that small that we would meet in his office): Not to try to cover computer science in a broad spectrum way, not to try to find faculty members to cover every niche of computer science that we saw at that time; instead to concentrate our resources and provide a critical mass in a few significant and key areas that we were interested in. One, obviously was numerical analysis, one obviously was artificial intelligence. One of them we discussed before was systems, and there were a few more that I'll mention in a minute. For example, we never had anyone in the area of information retrieval, or we didn't have anyone even in the area of data bases until fairly recently when Gio Wiederhold joined the faculty. So that was a critical decision. We wanted to build what Terman used to call "spikes of excellence, peaks of excellence," rather than to try to spread ourselves out across the whole spectrum.

The second critical decision that I give Forsythe immense credit for is the hiring of John McCarthy. John McCarthy was a young guy, brilliant, ripe for the plucking, didn't feel he was being treated quite right at MIT. Forsythe recognized the potential of McCarthy even though McCarthy was way outside of Forsythe's own research area, and that was an enormous help in getting the department going. John was a person of great vision and brilliant insight in computer science.

Then I think the next big decision was a combination of Forsythe's and McCarthy's, and that was the decision to pursue Bill Miller at Argonne as a way of building a strong link between us and the emerging linear accelerator center. They were going to have big needs for computers. Forsythe saw the opportunity of getting a shared appointment between

SLAC and the Computer Science Department that would benefit both, and somebody located Miller, I'm not sure if it was McCarthy or Forsythe, but McCarthy went out to Argonne to talk to Miller and recruited Miller. Miller had told me that McCarthy's visit was crucial in own decision to come. That was a key decision.

The next key decision was a pair of decisions. It was a decision to pursue both Don Knuth and Bob Floyd simultaneously. There were some rumbling in the attempt to get Bob Floyd from Carnegie Mellon. Floyd suggested that perhaps the best way to make him happy at Stanford was to recruit his friend Don Knuth from Cal Tech. We had heard stirrings about a brilliant young guy down at Cal Tech who was writing this book on the art of programming. Notice I said "the" book, not the "second" book. And we did initiate the pursuit of those two and that was really a formative decision. We got those two both at the same time when we were a young faculty and growing. They had both been very strong intellects that have molded this department, particularly Knuth.

Those were really critical decisions of Forsythe's years in office. We did have one set of difficulties that are worth recording because those circumstances may never occur again and so it is worth saying that it occurred once. The circumstance of trying to maintain an academic routine that required an assistant professor to serve two three-year terms, essentially six years, for the University to get a look at a person before committing to a lifetime tenure appointment versus the explosion and the demand in the field which was pulling at our assistant professors at the end of a three-year term, giving them the idea that a tenure appointment could be achieved at the end of three years in academia rather than six. We lost at that time Klaus Wirth, Raj Reddy and Joyce Friedman, three quite spectacular scientists in the field because they were being offered or thought they could get tenure level jobs without having to put in another term as an assistant professor. It was and is relatively unthinkable for Stanford to do that and those people then just left. Even though Forsythe spent a good deal of time trying to make them understand that each of them had a very good chance to get tenure here if they would just wait, calm down, put in a little more time. So that also meant the turnover that resulted therefrom led to an impression out in the world that Stanford didn't give anyone tenure when in

fact that wasn't the case. The case was they were pushing us too fast for tenure. There have been quite a lot of tenure appointments recently in the department; in fact, the department is almost fully tenured.

Well, that's the end of my list. Do you want to ask me some more questions?

McCORDUCK: There must have been a certain point where it was clear to you that the Department was no longer good, it was great. Do you remember what that point was?

FEIGENBAUM: No, I honestly don't. I have no recollection of a feeling that at any particular time; it's just been a slow growth of that. I just don't.

McCORDUCK: When we were talking earlier about the kinds of things Stanford was doing, particularly getting into the second generation of computing, there were very few other universities that were doing that. How did it feel to look around the landscape and see that you were out on this technological limb?

FEIGENBAUM: I don't remember that bothering me at all because I didn't feel that we were out on a technological limb. McCarthy had been running a time-sharing project here. We understood completely what was inside of CTSS which was the big time-sharing system at MIT. So feeling that we had a total grasp of the technical situation vis-a-vis time sharing, I didn't feel that I was out on a limb. We were just coming out of the days when you literally could know everything there was to know about computing and the few of us who were around here basically felt that we did. I, myself, hadn't specialized nearly to the degree that I have now because there was much less to know. So, I felt I could understand any piece of anything that was around. Then we weren't out on a limb because people that we knew and respected were moving in the same direction that we were, particularly MIT, Carnegie-Mellon, and the University of Michigan were all doing about the same thing at the time we were doing it, moving in the 360 direction. so, no

impression of being out on a limb at all. Probably I felt we were out on a financial limb but we had a plan so in the end it worked out.

McCORDUCK: It's a great curiosity to me why places that should have been better since they were there first aren't better, in fact some places don't even have any academic computer science. I'm thinking about some place like Harvard which was in on the ground with a Mark 1, things like that, or the University of Pennsylvania. Have you any idea as to why those places withered and died when parvenus came along like Stanford and took off?

FEIGENBAUM: I wish I had an answer when I don't and it's one of the things that puzzles me most. I puzzled about it when I wrote the preface to the last annual report here and I said that excellence was a very fragile thing and that the mountain was very steep and when you lose your grasp, you fall fast and it's hard to climb. Worse than that, if we only understood what made it, we'd know how to move up that mountain but since we're not understanding the process, it's even more precarious.

I got asked the same question by the dean who took Harvey Brooks' place at Harvard. He came here and said, "Let's be frank. I know that we don't have much of a computer science department. You do. How did you build it? What makes excellence?" I had nothing to tell him really. I've raised this question with my colleagues trying to get an answer. I don't know what it is. In the end, I think it has something to do with the quality of the individuals, that's why I regard the decision to promote a person to tenure as the single biggest decision this department ever makes because the individuals are so critical to making the department go. It's very hard to pin down what that's all about. The individuals have to have very high standards for themselves and Don Knuth is an example of one of those. They also have to draw on an academic tradition of the highest excellence and that's where I regard my own link to Herb Simon as being so immensely valuable. It's impossible to lead an academic life without being inspired by what Simon was like at Carnegie Tech and I'm sure that the others around here have had similar experiences that have moved them to some sense of taste and judgment that leads to high levels of excellence.

I'm trying to think of some other factors. I regard the Forsythe legacy of a happy, quiet department where people have a very high regard for each other and each other's work and people hassle each other almost not at all as having been extremely important to the growth of the department, namely toward good science -- maybe not in the same scientific direction. I think of that as being an immense Forsythe legacy and if we ever lose it I think we're in trouble because if people get unhappy, if we got into fights of one kind or another or Don Knuth decided to leave or John decided to leave or something like that, that would be devastating. I think even losing one or two would be hard to recover from. Each one forms a little piece of a structure that is kind of self-supporting. I think that's important. It's said that the rich get richer and it's certainly true at Stanford that Stanford has a view of excellence that says, "The best place to put your very limited resources is to place those limited resources in the hands of winners." So, if you're a winner, you become a bigger winner. If you're a loser, you become a bigger loser. As soon as it was clear that we were becoming a winner, then Stanford has been very supportive. Forsythe wouldn't write those letters any more. We almost never get turned down on any kind of reasonable appointment we want to make, getting a full professorship for Jeff Ulman was no job at all. I have this appearance of being this great chairman who works miracles, but I'm not. It's just easy. That's because the rich get richer. So partly it's Stanford's pursuit -- once you have the ball rolling and are very good or perceived as very good, you can get better fast.

Then the next factor, I think very important factor, was the fortuitous availability ARPA funds in the mid-60s to McCarthy and myself. That seems to have had a major effect on who is excellent in computer science. Although there are places that had ARPA money at the time that have not matured; either they've dropped their ARPA support or they never made much out of it. I don't know exactly why. The ARPA support was very, very important. It helped to get the momentum going on students and research. That's why I'm pushing strongly for a similar NSF program, a Centers of Excellence program for the early '80s that will do the same for some other institutions.

I also have a very elitist view of the great man theory of history. I don't believe that the large masses of people contribute very much, especially in science. Science is pushed along by the giants. And we happen to have lucked out with an environmental building superstar in George Forsythe and he somehow had the savvy, the knack, the intuition, the good taste, the judgment, whatever it is, he was one of those superstars. Had we tried to build as, say Harvard did, with Tom Cheatham, we might be in Harvard's condition. Maybe it was Cheatham, not Harvard. Carnegie Tech, alias Carnegie-Mellon, in the middle of Pittsburgh, a no place town -- there's nothing there, but there is Newell and Simon, two giants and great environment builders, and Alan Perlis of course who is no longer there. He was another great environment builder. It's hard to know.

There definitely is this "good get better, better get best" phenomenon. Unfortunately for those departments who made tenure decisions they can't take this advice, but if you have a mediocre computer science department, tear it down and start again. If you have tenured faculty, that's hard to do, and they all do.

One of the greatest puzzles of all is the University of Michigan. They were early and good and now they're in the I don't know where, second rank, third rank, I can't even remember where they ended up in the survey of computer science departments. I don't know what happened to them. It was just a question of, I suppose, bad taste in choice of problems, betting on areas that just didn't work out, like John Holland's brand of automata theory. It's nowhere. Maybe it's good mathematics, maybe it's personally interesting, but it wasn't on the main line to anywhere, whereas most of what the MIT people touched, and what we touched, and the Carnegie people touched were on the main line to somewhere. Good choice of problems.

McCORDUCK: Not always apparent at the time. In another context you told me that you took a lot of flak from people who didn't understand what Dendral was all about and didn't understand why you spent so much time playing around with it.

FEIGENBAUM: Yes, that's right, and yet in spite of the fact that that work was off the main line at the time, it was strongly supported by ARPA. Somebody at ARPA had the right vision or maybe I had the right persuasiveness to

convert them to that vision. Maybe I got that persuasiveness from Newell and Simon. You can't tell where the roots of all that are.

END OF INTERVIEW