An Interview with

LOUIS FEIN

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Conducted by Pamela McCorduck

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Abstract

Fein discusses his involvement in establishing computer science as an academic discipline. In 1955 he joined Stanford Research Institute (SRI) as a computer consultant and was asked by Frederick Terman and Albert Bowker to design a computation curriculum. He describes the difficulty in establishing computer science's autonomy from engineering programs. Fein also describes his contacts with the University of California - Berkeley, the University of North Carolina, Purdue, and other institutions. He recalls his presentation on computer science departments at the l962 Munich meeting of the International Federation for Information Processing (IFIP), and how his plans were accepted at many academic institutions throughout the U.S. and Europe. Fein concludes with his views on the future of computer science, which entail a name change to "synnoetics" and a corresponding conceptual redirection to the interaction among intelligent beings, including humans and computers.

LOUIS FEIN INTERVIEW

DATE: 9 May 1979 INTERVIEWER: Pamela McCorduck

LOCATION: Palo Alto, CA

McCORDUCK: This is a conversation with Dr. Louis Fein on May 9, 1979 in Palo Alto. Our purpose here is to collect historical, anecdotal information about the formation of academic computer science in the United States.

The first time I came across the mention of your name as I was going through the archives of George Forsythe's papers in the Stanford archives, was in a letter that you wrote to Frederick Terman and in this letter you said that "it has been almost thirty months since we last discussed the formation of a graduate school of computer science at Stanford as recommended in my report commissioned by Dr. A. H. Bowker." Could you tell me how you got involved and how Bowker happened to commis sion you to do this, and what some of the recommendations were?

FEIN: In 1954 and 1955, I was in southern California installing and operating at the Naval Air Missile Test Center at Point Magu the RADAC, the Raytheon digital automatic computer which I had built as chief engineer at Raytheon Manufacturing Company between 1948 and 1951. I had, after leaving Raytheon, formed the Computer Control Company which was an organization made up of people from Raytheon that worked for me. On October 30, 1952 I formed the Three C's, Computer Control Company, because after the acceptance tests were passed for the RADAC, the RADAC was then whipped from Waltham, Massachusetts to Point Magu, that's right outside of Oxnard. The Navy department asked me if I would go out to Point Magu together with some engineers and install and operate the computer center there. The RADAC was built and actually was intended for the data reduction at Point Magu. They insisted however that I must go personally. I had some engineers working at that time in Belmont who later moved to Framingham. So I and some engineers came west to install and operate the RADAC, and it was in fact the first computing center on the west coast. I came out in early 1954, as a matter of fact January 1954. In August of 1955, we had gotten the computer going and the people in the east had developed some modules, computer modules, which we called T packs, which were, as far as I know, also the first components, single components out of which you could

build computers; it was sort of a tinker toy notion, and we built the very first I believe - modules. There were only two kinds, one a power supply module, and the other a logic module which could also be used as a memory -- a one bit circulating memory at one megacycle rate. So we had an eastern operation going that were building these T packs, trying to get people to buy them as individual components as well as trying to get people to order computers built out of these. And in the west we were doing the RADAC, operating the computer center, with the software and the hardware maintenance and the whole bit. In August 1955, for a variety of reasons, mostly administrative, I left Three C's and decided that I would write, consult, and teach perhaps. As a matter of fact that was August 10, 1955. Immediately after leaving Three C's. by August 11, I had, as I recall, seven or eight contacts already made, and by August 18, I was consulting for five or six computer outfits. Mostly, of course, in hardware design, because the term software wasn't as yet coined. I think Herb Grosch coined it 4-6 years later. One of the first assignments I'd gotten was with SRI, the Stanford Research Institute then, who had a contract with the Bank of America to build what later became ERMA. So I was consultant to SRI, who was the consultant for Bank of America, building a prototype of ERMA and they are right here in Menlo Park. I had also gotten an assignment from Sylvania Electronic Defense Laboratory to work on some computer hardware, and I also worked for Electro-Data, RCA, but I rather liked it up here and so we moved up to Palo Alto in January 1956. I was almost walking distance to SRI and to Sylvania and when I had to go down to Pasadena for Electro-Data or to Camden for RCA or to New York for IBM, I just went, but Palo Alto became my home base and I've lived here since. I've been a free lance independent computer consultant since August 10, 1955, which I believe also might be the longest in the world. Well anyway, I was consulting for SRI and for Sylvania even while I was living in Oxnard. I had not yet moved up here between August 1955 and January of 1956. One of my friends introduced me to Al Bowker who is now the Chancellor at Berkeley, but then was the Provost at Stanford. I may not be remembering accurately; he was either Provost or Graduate Dean, and perhaps later became Provost. Al was interested in the proposals that IBM was making people of the following type: "We will give you a 650 for free if you will give a course in scientific computing and one in business computing." And Al also had heard a little about this computing business and I suggested to him that I thought that computing fits in a university. I didn't quite know how. But it was clear to me and apparently to him that computers would develop in such a way that most of the disciplines in the university would have need for them at least as auxiliary devices. The question of whether or not computer science, not yet the computer itself, was a discipline worthy of study by the

university was not yet settled but in my enthusiasm I thought it might be and so Al Bowker commissioned me to make a study on what might be called "The Role of the University in Computers and Data Processing." I spent a year studying the matter and I studied it in the following way: there were some few universities already who had a 650. For example, at Wayne University they had a machine that they had built themselves; the digital differential analyzer of Bush was over at Wayne, (incidentally that is where I first met Joe (Weizenbaum) who I later hired to work with me in California). This was Wayne University in Detroit, Michigan; it was then Wayne rather than Wayne State. I believe the guy's name there was Arvin Jacobson, an avid promoter of computers. Anyway,a at that time also I went to every computer conference held and I talked at bars and computer conferences with the likes of Al Perlis, John Carr, John McCarthy, who was then at Dartmouth, -- what, at Pittsburgh? -- Herb Simon and tens of others (there weren't that many) about the notion of the place of computers on the one hand as a science in itself, like geology and economics, and mathematics as a discipline worthy of pursuit in a university, and on the other hand as another tool, a super-calculator, if you please. That is one way in which I studied the matter. The other way in which I studied the matter was this: since my job was to write a curriculum, design a curriculum, and I had never done anything like this, (I knew about computers to the extent that anyone knew about computers in those days, but I didn't know anything about curriculum design) and it is my style to learn about the principle of things before I apply these principles to what I am doing. So I spent a considerable amount of time trying to define for myself what is a curriculum? What the hell is one of these things? And I asked myself (since I lived here I looked in the Stanford catalog and there were descriptions of curricula in education, English, business and mathematics, geology, economics, etc.,) and I wondered why is it that economics and geology is in a university and why is it that plumbing isn't? What makes a discipline worthy of pursuit in a university and thereafter how do you define a curriculum for that discipline? That was an elementary set of questions. Well I went to the great experts at the School of Education at Stanford University and I asked, what is a curriculum? How do you distinguish one curriculum from another? What are its characteristics? And I could get no answer, because they didn't know. I went to people, I remember specifically in geology, in chemistry and a couple of other disciplines and I said to them, "Why is it that this subject is studied in a university, what makes a study worthy of pursuit in a university?", and they would all, to paraphrase every one of their answers: What the hell do you mean? I got my degree in a subject, and I applied for a job at Stanford in Geology, and I got a job in Geology, so why the hell are you asking me why geology belongs in a university? What kind of stupid

question is that? So it became clear to me that I had myself to define a curriculum. I had to define those characteristics, and identify those characteristics which, how shall I say, describe a subject, so that if it has these characteristics, it is worthy of pursuit in a university. Obviously it's like many other things, it just grew like Topsy, and political considerations probably more than anything else were responsible for getting this rather - than that adopted in this public educational institution, which as most public institutions are - they are really public employment/ they are employment institutions. You get things done and accepted for other reasons than exclusively academic merit. Because I had to dig into the characteristics and identify those characteristics which made a discipline worthy of pursuit by a university, this as usual motivated a lot of other thoughts about computers and about other subjects and I came to this analytic type of observation. I took the subjects that were studied at the universities and put each one into one of two classes. One I called disciplines and the other I then called supra-disciplines. I was very unhappy with the notion of interdisciplinary activity. I am still unhappy with it, and I identified - I distinguished disciplines from what I call supra-disciplines and of course both distinguished from what you might call interdisciplines. I was a little unhappy about putting the term interdisciplines in the same category as discipline and supra-disciplines and I will tell you the reasons in just a moment - well, I will tell it you now. People who then and now talk about interdisciplinary activities are really speaking about projects which require, in order to carry them out, specialists in various disciplines. And so if someone for example in our day, in the computer business, when we were building the computers we would hire engineers and mathematicians; sometimes for mercury delay line memories, chemists. So it was legitimate to call - the building of computers and interdisciplinary activity because you hired people who were specialists in particular disciplines to combine their resources to build a thing about which you could not say - this is a product of the chemical discipline, or the mathematical discipline, etc. Although when you consider the individual disciplines themselves, then let me get to the distinction between discipline and supra-discipline -- I noticed that mathematics had the following characteristics: that people studied mathematics as an end in itself. They would develop theorems in mathematics, and they still do. There was another use of mathematics, another way people studied mathematics and applied it. I also noticed that in almost every other discipline like biology, statistics, economics, geology and even political science, that they all use mathematics and engineering as a tool. They weren't concerned with developing new prime number theorems although in many of these instances in order to solve a practical problem in chemistry or engineering, to identify a problem which later a

mathematician took on, you developed a theorem which you could then apply. Here with mathematics was different than biology in the sense that mathematics could be used and was used as a tool for the use in biological sciences and you could study mathematics by itself and not worry about its application. As a matter of fact, even in those days you could take courses in engineering, mathematics for engineers, differential equations for engineers, mathematics for chemistry and so on. So I called mathematics then, and I still do, a supra-discipline, because it is applied to many different problems, areas, and disciplines and it is a study in itself. A discipline was one which was largely studied on its own and you didn't have a large overlap with other disciplines. You didn't have, at least in those days, biology for chemists, or geologists, or geology for musicians. And then it occurred to me that computer science would develop into a supra-discipline and it was doubly reinforcing for - me when I was asking the question, what makes a subject worthy of pursuit in a university, that there were some disciplines which were surely worthy of pursuit, but clearly all supra-disciplines should, even just by naming them, immediately fall into the category of being worthy pursuits by a university. This was my line of thinking.

McCORDUCK: In hindsight I can see that this is very clear and very appropriate. What I want to know is how did you see it then?

FEIN: It was very clear to me then, even with the small amount of information we had about computing. Even then, we did a little bit of numerical analysis and solved differential equations and wrote the difference equations, and it was clear that differential equations were applicable to many, many fields, even then. So even from the point of view of mathematics alone, computer science (I didn't name it computer science until I was looking for the name in the middle of 1956). If one says that mathematics is a supra-discipline and applied to everything else, if one then says, "Yes, but if you are going to solve these differential equations on computers you have got to first transform them to difference equations and then you have a computer solution, not as distinguished from a mathematical solution, but different from a mathematical solution so that even then I could see the applicability of computer solutions to problems useful in biology and wherever you might use differential equations models. Obviously there are other models (mathematical) other than differential equations and it was clear that if somehow you could take these models and transform them to a numerical analysis, so all you had to do was add, subtract, multiply or divide, which is what

any of these dumb boxes could do, then it seemed obvious. I think if it weren't obvious to most people what also wasn't obvious to them was that mathematics was a supra-discipline in the sense I have just mentioned. Because, once having accepted that, then the next step was easy. Of course obviously, computers were obviously the things that people studied in themselves - they were continually making new designs - it was a subject like mathematics that you developed on its own, the hell with application. And programming was just getting rolling and Grace Hopper by that time had already started, I think, some automatic programming. We had some assembly language then but I don't -- anyway automatic programming was in the air and I was acutely aware of it. I would go home at night sometimes to see if - damn it, when I first started to program we all had to write 1011011 - write in literal binary codes - and finally we got an assembly language, so instead of writing 1011, which was the code for add in the RADAC, I could write ADD, and the translator in there would change it to 101101, and it became clear that "Wouldn't it be nice to write integrate" - I didn't know how to do this but it was clear that this was a very involved and complicated thing, at least at that time and here is a subject which is worthy of study by itself. A lot of people were obviously going to go into it and do it. If I could go home and try to write what was later called "compiler" obviously people are trying to do this and so here was something people were going to study on its own and it was clearly already applicable. I mean I was applying it at Point Magu to a whole variety of problems, engineering problems and so on, and it was just a small step to see that it was going to be applied to a whole variety of subjects that, certainly where mathematical models were useful, and where in turn those mathematical models could be transformed.

McCORDUCK: In other words your total immersion, let's say, in the computer culture was what was opening your eyes to all the possibilities of computing - that people who could not see these possibilities were people outside computing who said that they did not understand what was going on. Is that what you're saying?

FEIN: I didn't have any contact with people outside; the people outside weren't the resisters, it was the people inside who were the resisters. George Forsythe and Herriot and Ed Feigenbaum and, these people were the resisters.

McCORDUCK: Why do you think that was?

FEIN: Oh, well I will relate some anecdotes... I didn't bring them the report that I brought to Stanford.

McCORDUCK: Let me just get the chronology of this straight. Apparently you were to give this report, even though Al Bowker asked for the report, you were to in fact submit it to Terman, is that correct? Because it seems to me that the letter that I just quoted to you...

FEIN: That is correct.

McCORDUCK: So Terman must have been Provost by then.

FEIN: Terman was Provost when I first came, and Al was dean of the Graduate School.

McCORDUCK: So in the course of officialdom, even though Bowker had commis sioned this report from you and had sent you off on this year-long study, the report in fact went to Terman.

FEIN: Actually Terman had to okay my doing this report and I don't recall when I wrote this letter that you say, this thirty month later letter. Al may have gone by then.

McCORDUCK: It was the fifth of December, 1960.

FEIN: Oh, so Al was still there - I don't recall why I wrote it to Terman, Al may have been away...did I carbon copy it to Al?

McCORDUCK: Yes, in fact it was Al who sent his copy to George... anyway, go on about this report of yours.

FEIN: Okay, so having gone through these kinds of thoughts, where I had to define a discipline, a supra-discipline; what makes a discipline worthy of study; why I think computer science, for which I needed a name - as a matter of

fact I remember talking it over with Al and I don't recall what names I started with - anyway, I ended up with computer science - and in October 1957, I delivered a report to Al, and I will give you or Paul a copy, I can think of some places where I might be able to locate at least one copy for you, unless you need it immediately...

McCORDUCK: No...it would be helpful for me to have it before I talked to Bowker, which will be two weeks hence.

FEIN: The title of that report was "The study and evaluation of the status of university programs in the United States in computers, and data processing related fields including recommendation to Stanford University on its potential role in these field." Now to get the status of university programs, you know I talked to Al Perlis, John Carr and all these other people all over the country. I met them either on one of my consulting trips or at a computer conference. I was doing a lot of both at that time so without expense to Stanford, because I was these anyway, I was able to gather all this information - without travel expenses. Well, this report came out and as I recall, one of the anecdotes, I gave a copy to West Churchman who apparently was very interested in it - West is a philosopher and all of that, and he thought it was a great thing. He organized a small seminary to be held at Berkeley (and I don't recall exactly when that was and I regret so many times not having kept a date book on these things) and as I recall around the table, Harry Huskey, my good Derek Lehmer, Julie (Julian) Feldman, pretty sure Ed Feigenbaum...at least those and I may think of some others before we finish the interview. West introduced me, saying I had a notion that universities should be involved in computers and that there should be a separate computer science department. And I made my pitch. And Harry Huskey said he saw no need whatever for having a separate department. He was doing computing in engineering.

McCORDUCK: I must say this is very funny in light of subsequent history.

FEIN: Yes, and that is where it belongs. You know, saying essentially I'm crazy...I mean what's the point in doing that. And Derek Lehmer - he didn't see any need for a department. He was, even at that time, trying to use the computer that Morton built (Morton was the computer man at Berkeley in those days and they were building a homemade computer). I think Lehmer was trying to use it for prime number calculations. And Derek Lehmer patted

me on the head and...crazy idea...as a matter of fact, one of my sons later went to Berkeley, well two of them went to Berkeley, one of them, Danny who was with Mario Savio during those days and once he ran into Derek Lehmer and he said that Professor Lehmer said, "Oh, your father is Lou Fein, that crazy guy that wants...I thought he was joking, but...(laughter).

And Julie Feldman also said there was just no need...it's just crazy. And Ed Feigenbaum also. Herriot, you know Herriot, I used to talk locally in Palo Alto, a lot at Stanford on this idea, and he was -- hostile was an understatement -- to the idea. I mean he would get up and shout, "What you want it pie in the sky and you can't have pie in the sky!" And that was the reception I got. George Forsythe... I met him at a Los Angeles Conference before I came to Stanford -- and he asked me about Stanford because he was considering either Berkeley or Stanford and I told him I thought Stanford was the place to go. He was interested in numerical analysis, and he had also worked on SWAC over there, and I told him I was working for Stanford and trying to persuade them about a computer science department. a I was already in the middle of the study and I had the outline of what I was going to recommend and he finally came to Stanford. After he came to Stanford his position was - well equivocal; he could go for it or not go for it. There was a very, very strong opposition in the mathematics department and George was in the mathematics department and Dave Gilbarg who was the chairman of the department, who was a very close friend of mine, because his boy and my boy were on the same little league team, and you know we used to go to the movie together with the families. But Dave thought computing was like plumbing. We don't have plumbers studying within the university and what does computing have to do with intellect? I am exaggerating of course. He was murder on it and he may still be to this day. And George wasn't sure he wanted a separate department either. Herriot though the pie in the sky, crazy. So the opposition came from insiders not outsiders, who didn't understand that mathematics is a supra-discipline. It...I had a talk with Terman, because after I presented this report, I talked a lot with Al (Bowker) and then Terman. Al had started the statistics department at Stanford and statistics had the same kind of history of resistance by academics and mathematics departments for introducing
a pedestrian study like statistics, which is like plumbing, into the university. The way in which apparently Al got it going was to get a contract from ONS for one person, get a little project going and then hustle another contract from maybe the Air Force and get something going. And after 8 or 9 or 10 years he had gotten something. And I couldn't

see with the vision I had, doing comp uter science by getting a little dinky computer from IBM, a 650 and rustling up two courses over night which is exactly what Al and Jerry Lieberman did. One night in two hours or three hours, they wrote a curriculum, a syllabus for business applications, scientific applications, gave it to IBM the next morning, and got a 650. And of course a lot of people were doing that and I wrote about that in later papers...I will try to get you a packet before the week is out. I thought that (incidentally, I will have to tell you what goes in the report - maybe I better tell you that now). In the report was first the philosophical support statement and arguments for why computer science is a supra-discipline as I called it, why it needs a department of its own and to get more theorems so to speak, make better computers, better software and so on, and why it was applicable to all these other departments.

So I envisioned a set of courses in a department itself, actually in a graduate school to start off with, because we really didn't know much about computers for undergraduate study, so we had to start out with a graduate program, I thought. And I laid out a set of courses, not only a set of courses for the school itself, but for other departments, computers for - medicine, computer for so--, and a research program, and some dollars required and where it might come from, and I wanted to start it with some seed money that wasn't merely a computer center as many people were doing. And in those days I could hardly get people to distinguish a computer center which was a job shop from an academic department because they didn't understand it.

McCORDUCK: You really see that laid out in Forsythe's papers - the tearing of hair over what the relationships between the department and the center should be -- I mean it is just a non-issue nowadays but then it seemed to be very difficult.

FEIN: And this report lays it out, and in my other papers which I suppose you should see because George took a lot of his stuff from these papers. I used to talk to George a lot about this. Anyway, the relationship between the computer center and the computer department was identified at least. In a later paper, called "The Computer Related Science of Synoetics in the University in the Year 1975." Did you ever see that one? Well, that lays out the whole bit. It's a scenario where I pretend...this was written in June of 1961...actually it was written in 1960, but I pretend a president of the university is delivering a talk in 1975. Looking back at the history of computing in his university and

how first they built a computer center and people thought, "Gee, this is our computer department," and the internal fighting and bickering - "Is this plumbing or is this not" - all of which came after my thinking in 1956 and then by 1961 I had gotten a pretty good handle on the whole. In other words, the history of computing in the sense I am now describing it to you for universities, I think is very well summarized in that paper. Anyway, so the question was with Terman, but Al first and then I later went to Terman, was "How do we get this off the ground?" "This is a great vision you have, Lou," and Al...there was a difference in philosophy between Al and me. I laid out a structure, an organization and I thought that even if we didn't have a bunch of computer geniuses around in which you can set up a school...you have a bunch of slots and then you get the best person you can to fill the slot...and Al didn't believe this; he said "It doesn't make any difference what the structure is, because what we need is to get a von Neumann out here and then things will go well." The great man theory.

TAPE 1/SIDE 2

It's clear that with that view he didn't think I was that great man and he didn't want in principle to set up an organization and then fill it with people who were available, great or not. Meanwhile, Burroughs, I believe, was dangling some equipment in front of his eyes for free because all of these manufacturers are very interested in getting universities to pick up their equipment for sales promotional purposes and Al, I think, got a very good deal from Burroughs. Burroughs gave him a machine and he, I think, started what was essentially a computer center and it isn't clear to me when he decided that George should be the one to do that and that maybe alter, I don't know exactly what Al was thinking because he stopped talking to me about it - that's why I had to write the letter about it's 30 months since...I guess Al appointed George to start with the computer center and maybe to give some courses in numerical analysis.a Exactly how it developed, later on, I was just not aware. Another kind of anecdote is a perception I have which may have been a misperception, but I recall that after a few years you might know that Harry Huskey became head of the computer science department later on at Santa Cruz, and Feigenbaum someplace else, and Feldman at Irvine and I felt - and George here - and I felt that at computer conferences I felt that at computer conferences I'd see some of these people down the hall and when they would see me they would sort of avoid me. I felt that - maybe it was a misperception but that is how I felt about their treatment.

McCORDUCK: Do you know why?

FEIN: I understand very well why. Also Terman, you know Al's notion was the great man theory, and Terman was telling me, "Well, Lou," he says, "We can't just set you up like you want, but why don't you do like what Al did with statistics: get yourself a little contract from ONR and hire yourself one person and so on and after a while do like he did." And I couldn't see that at all, and with hindsight I am still right.

McCORDUCK: Now I inferred from the text of the letter you wrote that Terman at some point must have responded to you and said, "Look, dozens of faculty members come to me with hot propositions. Yours is no hotter than anybody else's. It's interesting but it's not revolutionary so please leave me alone already." And you were arguing in your letter that it wasn't just another hot proposition but that it was in fact revolutionary which he didn't seem to see or at least denied.

FEIN: I don't recall, I don't recall, but what I do recall is that he was such a friendly warm man, Terman, and he was really trying to be helpful. What I really recall was, "Do like what Al did with statistics, get a little contract here and a little contract there..." I don't recall that he didn't think it was a hot idea, because after all he had to be there to help commission the report in the first place.

McCORDUCK: I just took notes from that letter and essentially what he had said and what you were responding to was, "It's just another hot project," and you said, "No, it's the introduction of a new discipline."

FEIN: Well, okay, I guess it happened like that. I don't recall when I was talking to Terman about it, whether Terman was talking to me as provost or as an electrical engineer; he was a very prolific writer in electrical engineering.

McCORDUCK: Now, in as far as this resistance was concerned - what do you think accounted for it?

FEIN: Well, the usual political explanation - Lou Fein brings in an idea, "An outsider is bringing in an idea and it is clear that he is bringing it in because he wants to be the chairman of the department and I prefer that I be the chairman of the department and so I will resist it." The resistance to innovation anyway, and secondly the threat.

McCORDUCK: Do you think people thought of this as a threat? In what way?

FEIN: Yes, it's threatening in the sense that they themselves typically weren't in the field and monies that might normally come to them might be diverted to this new department. I had this experience at Purdue. I was invited to Purdue by Tom Jones who is now a Vice President of MIT. He got hold of one of my papers and he thought it was the greatest thing since the wheel. He invited me to Purdue where he was Chairman of the Department. And his people almost murdered me - they didn't want anything to do with this. Oh, after I left, two years later, they had one...And that's my explanation. I think anyone who brings in a new idea anywhere that requires money from a common budget poses a financial threat and obviously a professional one.

McCORDUCK: I think that was one issue that I had more or less forgotten, and that is the enormous amount of money that computer science needed to get under way. When people were talking about computers in those days they weren't just talking about a multimillion dollar machine, they were talking about 1960 multimillion dollar machines which was a tremendous amount of money - it's small shakes now but in those days it just seemed phenomenal and I had forgotten how much scrambling around there always was to try to buy a machine or get one's hands on a machine in those early days. I think you have touched on, quite rightly, the fight for scarce resources.

FEIN: Of course immediately after my report came out in 1957, we were looking for a million dollar machine - the 650s were sufficient because we didn't know any better. The RADAC and UNIVAC, they were three million dollar machines - we weren't going to play with that. Well, I continued even though I was rebuffed at Stanford and almost every other place. Well, I wasn't rebuffed in the sense that people rejected the idea. Terman would have been happy to have me get an ONR contract and do it that way.

McCORDUCK: When you say get an ONR contract, did he mean to take you on as a faculty member?

FEIN: Yes, that's right. And even Tom Jones of Purdue would have been strong enough to get me on as a faculty member to give one course and kind of try it out, instead of get in there and design a whole system and then perhaps give one course which was part of the whole bit. They wouldn't do that. When I say rebuffed I mean one of two things: rebuffed in some cases where they won't consider the idea of computing being a discipline and worthy of pursuit at this university.

McCORDUCK: Well, let me ask how many universities did you approach with this idea, do you remember specifically?

FEIN: Well, I remember Purdue; John Carr at the University of North Carolina, who pushed the idea but I don't remember that he - what he said about being in support of it or not; West Churchman seminar at Berkeley; Stanford; I talked to John McCarthy who was at Dartmouth but that was just incidentally, I didn't go to Dartmouth directly and say, "Would you do that?" And I talked to a large number of people in that mode without going to the President and saying, "How about this?" Actually, in a sense I pushed the idea in papers I wrote later that were really motivated by the work I did for Stanford in this consulting report, and I gave these papers at the ACM conference. For example, here is one in September 1959, "The Role of the University in Computers Data Processing and Related Fields." That was September 1959. And then in 1961, this "Computer Related Science of Synoetics at the University in 1975" which got all sorts of responses. As a matter of fact, I can't prove this, but from all the responses I had gotten from people many computer science departments both in the U.S. and in Europe have a good deal of their genesis in the materials coming out of these papers, and they told me that.

McCORDUCK: It is certainly the case that it was not long after that that such departments started being formed. I have a little question to ask you which is somewhat along the same track and is a great puzzle to me. Why did those places that were really first with the most as far as computing is concerned, - we are talking now about the Moore School at the University of Pennsylvania, Harvard with its Mark I, now you tell me Wayne State which floors me, -

places like that which were really in on the ground floor never took off as essentially computer science departments. Did you approach any of these people and if so can, anyway can you account...

FEIN: Yes, I remember going to Oettinger at Harvard and he told me I was crazy to add to your...I think that the reason that these people, while they were at a university, it's true, thought of computers as a - I wouldn't say a toy - but an exotic calculator, that had hardly anything to do with science except as a - just as a Marchant hand calculator that had nothing to do with astronomy although astronomers used it. They were people interested in puzzles, the mathematicians among them, and they just didn't see it as a part of the academic structure of a university - just part of the apparatus, and an apparatus is important but it's not academics. You talk to people like Howard Aiken and Jay Forrester at MIT and Eckert and Mauchly was a little mo re of a scholar-scientist than the rest of them. But they were making a thing - happened to be at a university - they would make it anywhere. You have to also recall that most of these people were supported by the Department of Defense, during the war - it was a war effort, and that was where computers came from, the analogue computers as well as the digital computers, and you are dealing with war and what the hell does that have to do with the academy? The academy might be being used in the war effort but you don't think of the things you did typically as making war materials. You say, hey this is part of the academy and science. People didn't. They way in which you got money for this was to promise the moon, make a proposal, it's a way later on to support graduate students and all of that - it was an employment enterprise, not a research enterprise - that was just incidental, as is the case with so many things. And I think that might have been an important reason for people not seeing what was originally financed by and motivated by the war department. It should occur to people that, hey, this is part of the role of the university in its academic enterprises, as distinguished from its war making enterprises.

McCORDUCK: Do you think people might have perceived this grand plan of yours as kind of plan to aggrandize Louis Fein?

FEIN: I have no idea. The reason I think not is this: consider this anecdote about Berkeley and West Churchman. I didn't come to them and say, "Hire Lou Fein as Head of your new Computer Science Department," I didn't say that to

any of these people. I said, "Here's a plan, you guys do it, and adjust it to the peculiar needs of your own university, your own finances or whatever. The only place where I recall I wanted to do it was first at Stanford, and when they turned me down, then I went to Purdue, and actually at Purdue it was their invitation. But most other places I was saying, "Here, here is a blueprint. I am presenting it to you, you do it." As you can see I can't recall whether I actually made any applications to very many universities so that I should do it. So it's possible that the people at Stanford or Purdue, which are the two places where I recall making direct applications so to speak or at least that was part of the conversation. One of the reasons for their turning me down is that they weren't interested in their perception of my self-aggrandizing ambitions, but it surely can't be true about other places. Incidentally, I almost made a proposal in England and I will get you some material on that. And I got some pretty good reactions in England and not such a good reaction in Sweden. I had written a proposal for a European computer science department. I went to see Axel Linigren who was a Swedish billionaire who was a philanthropist. He had supported some computer companies in this country. I went to see him in Sweden about starting in one of the Swedish universities computer science departments. This was in 1962 as I recall. I was the chairman of the session at IFIP in 1962 in Munich on starting university computer science departments. I think at that time I went over to see Axel.

McCORDUCK: Do you remember who else was on that panel?

FEIN: Well, I organized it and chaired it. I have some reprints for you and I'll get them. The U.S.A., I don't remember his name was from Penn--and there was a guy from Germany on it and I think a guy from England I think, somebody from Amsterdam - so with respect to countries that I deliberately introduced the notion, tried to get them to pick it up, in Sweden in England and here. And while they didn't pick it up immediately, it was clear to me from the letter I had gotten and people I had spoken to since have told me. First they interested their administration in the whole notion by putting one of my papers under their noses and things got rolling that way. People might have used my papers for *their* self aggrandizement, but not the other way around.

McCORDUCK: This leads me to another question of a sort of personal nature, and that is that by the time you run into the fifth or sixth person saying, "I think you are crazy, Fein, go peddle your papers someplace else," why didn't

you just pack it in and say, "Okay, I can't convince these people and its just not worth the effort?"

FEIN: It's a style of mine, a lifestyle. This is a repetitive experience of mine. There are other issues about which this happens and I ultimately pack it in. Artificial intelligence, my history on that is almost identical. While in artificial intelligence I wasn't the builder, I was a critic, mostly because I was a builder in a certain, in a sense that the computer consulting and writing and teaching I was doing obviously - it's a matter of labeling as to whether you can call what I did computing or artificial intelligence and that's the basis on which I could make may critiques. Obviously, at least the technical parts. But I was very active in artificial intelligence mostly as a consultant with SRI for many years and I wrote, like I wrote in the Artificial Intelligentsia which was almost the last paper I wrote in the field. I kept at it until I finally quit. Why didn't I quit earlier? Again, it's lifestyle. I knew I was right and it's hard for me to quit when I know I am right. It's a reinforcement for me. It's an impetus to go on, when I know I am right when the world thinks I am wrong.

McCORDUCK: Well by the mid sixties, you had a handsome and full vindication in the sense that the computer science departments were starting up all over the country. Even now, twenty years later, they are finally reaching the Ivy Leagues. Columbia is about to start a computer science department. Yale has had one for quite awhile. Harvard is scrambling hard to get a computer science department going.a It isn't given to many people who are visionaries to see their visions come to pass but in this case you have at least seen it come to pass. How did you feel when you saw these things happen as you had predicted and in almost the way that you predicted?

FEIN: Well, it was a great satisfaction and reinforcement to know that I was right. It's a matter of regret that Lou Fein's ideas were taken but now Lou Fein. That's a summary of how I feel about it. Again I must say that this is a repeat of other experiences in my life. I am engaged now in a project that I don't think we need to put on the tape, but maybe sometime we can talk about it.

McCORDUCK: It's funny because I go down this list here of people you approached and it is just amazing. Well U.C. Berkeley - that was catastrophic and continued to be catastrophic until pretty recently, and some people would

say it's even catastrophic right now. Those people simply could not get their act together.

FEIN: And that is clearly internal politics. I used to laugh when some of them would ask friends of mine about Lou Fein. "Suppose we could get him to come here." They would never ask me directly, because...that place I guess is the prime example of what it means - of the expected reaction one might get to bringing in one, a new idea which might be threatening to them but more importantly, even when it's there, the internal war that goes on that makes it impossible for the idea to germinate and get going. It would have been too bad if it was a place like Berkeley where computer sciences might have first gotten started, would have maybe deteriorated and then people would say, "you see, we tried it and it didn't go" and the reason it didn't go is not because of the merit of the idea but because there is in a sense corrupt disciples, or the disciples who corrupted the idea. I don't recall who I talked to at MIT but they really turned down the idea of a computer science department - to this day they don't like its name. I'm trying to recall where else I might have gone.

McCORDUCK: Well, UCLA was also probably a possibility, with SWAC.

FEIN: A man named Leondes, is he still over there? There is one other place where I didn't want to go, that felt me out, that was USC. They wanted me to come and as I recall the reason I wouldn't go was that they wouldn't accept the distinction between a computer center and a computer science department. I had the impression that what they wanted me to do was to run the computer center. So it isn't that I solicited them, they asked me. There was an intermediary whose name I don't recall, but an early pioneer in the field who started at Los Alamos in nuclear work - oh, Frankel. He used to work with von Neumann, I don't remember his first name. He used to consult up here with General Electric when they had the computer department in Sunnyvale. Joe used to work here too, Joe Weizenbaum. Well, where shall we go in this conversation from here, there are other papers later that had new names, you know I am a name coiner. So shall I just...

McCORDUCK: Yes, why don't we just go over those? My main job is to capture the very early period and I think we have done that nicely, and it's raised all kinds of issues that I can bring up with people in the future and put them on

the spot and find out why they were resistant.

FEIN: They might not remember that they were resistant.

McCORDUCK: Yes. Anyway, let's do the overview and cover some of the things you have thought about.

FEIN: Well with respect to the basic notion of computers, as, what shall we call them, as mental aids to human beings, there is a notion in biology which I found very useful in applying to the relationship between man, people, and computers, well really between computers and computers between people and people. The notion of symbiosis in the plant and animal kingdom and the insect kingdom as well. There are a whole variety of symbiotic relationships. You can have two dissimilar organisms living together in a mutually advantageous way so that together they are in a much better way physically than they would be separately. This is called commensalism in biology. I think mense means table, I may be wrong. There is another symbiotic relationship which we all know about called parasitism where one organism is really leeching on the other. One does the work and the other is a parasite. There is a relationship called helatism where one is a master and one is a slave. And the word symbiosis, which was coined in about 1850, comes from the Greek root - the two pieces of the sandwich is sym like symphony which means together and sis is the normal ending for a condition where two dissimilar organisms live with each other so that together they are better off. The quality of life, so to speak, is better together than separately...at least for one of them, depending on whether we have commensalism or parasitism. Well about 1959, we were starting to use terms such as man-machine relations and people were talking about the interaction - interactive systems between people and machines, and I thought, well, we need a word to describe the fact that when people and computers work together, a person programs a computer and the computer then computes the program and does a job which normally used to be done by people alone. Now here we have a situation wherein two dissimilar "organisms" like computers and people combine not their physical resources like in symbiosis but their mental resources, so that together they are better off. The quality if you please and the quantity of the intellectual output is better than each could do separately. That is what we mean when we have man-machine relations and interactive computing, so I thought, well, what should that name be? I thought for a year on that, until I realized the parallel with symbiosis and as soon as I recognized the

parallel with symbiosis, and as soon as I recognized the parallel with symbiosis, you know, the formation of the word was obvious. The two pieces of bread in the sandwich should be sym or syn...together -- the Greek root is syn, synchronous, synonym, but it is sym, symphony because of euphenic reasons, the m sound; so I needed syn or sym depending on what I am going to have as the meat in the sandwich, sis at the end; and now I need the root, the meat in the sandwich, and I know I just need to look for the Greek work meaning mental and that is nous. Noe is - I don't know Greek but I checked it out with a Greek scholar. So synoesis is the parallel of symbiosis. So I coined the term synoesis, parallel to symbiosis. I then coined the term synoetic system, parallel to symbiotic system, and I then decided that what we are really dealing with, typically nowadays, is not computer science - we do that as a branch of a much broader science which deals not only with individual entities having mental capabilities like a person or a computer but also deals with synoetic relationships between them. And so in my 1961 paper, that was the fantasy paper about what was going to happen in 1975, I have the president of that university telling about in the last few years, we have changed the name of computer science department to a synoetic department because this is what is going on, and it *is* what is going on. I coined the term synoetics then in 1960, first published in 1961, and since then in my consulting business I call myself Synoetic Systems, so sometimes when Lou Fein is hired as the consultant to... when I send in my bill and my tax bill to IRS, it's Synoetic Systems. I give some people a start but it's been 18 years now and one day that term is going to be picked up. You may have seen in the last several years that UNIVAC has used synergic systems, meaning working together, and they haven't used synoetic because it is mine but is clear that what people are building now, what IBM and the rest are building now are synoetic systems, they are not computer systems. As a matter of fact you can have an analog computer-digital computer synoetic system (two dissimilar organisms that combine so that together the output is more potent than either could do separately). You can have people - people synoetics and we do; we have synoetic systems of people - people since the very beginning. The teacher-student relationship is a synoetic system and one day that will pick up and I've tried very hard to get people to pick it up but...well, I also, in still another paper, coined another term which is like synoetics. If you ask the question, "What should you call the science of intelligence, you have been concerned with artificial intelligence - if there is no such a thing as artificial intelligence than obviously there must be in distinction to natural intelligence. And so the question is, or the question for me, is what name should we give to the science of intelligence, natural or artificial? And I have such name, and wrote a paper about it a very long time ago. It should be something "ology"

right? And the question is what do you put in the front? What is the prefix of "ology"? Well, you look at the Greek like I did for synoetics and it is nous and it's noo or noology which is the science of intelligence, artificial or natural.

McCORDUCK: I haven't seen the word but I have seen the notion of some phrase that accommodates both natural and artificial intelligence and in fact that is very much a buzzword right now. The phrase that's being used is Cognitive Sciences. The idea is precisely what you are talking about. It's an idea whose time has come 15 years later!

FEIN: Well I did that noology paper like this. I split the world differently than we have in the past. (Science has split the world.) In the past science has split the world into the biological sciences and the physical sciences. Those are the two big categories. And we take a subject for study, a thing and we say it is biological or physical and then we have more sub categories, obviously. What I did was to take the world and divide the world into biological and physical sciences and these are attributes of living things and these are attributes of non-living things (indicating to Interviewer). So living and non-living. That's how we study the world. That's how we do things in school, in industry, everywhere. And we build up a vocabulary that reflects this basic division. I decided to take the world, divide it this way: intelligence and non-intelligence. In other words, if you take intelligence - something that is intelligent could be living or not living and this is called artificial intelligence.

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So if you split the world into intelligence and non-intelligent things or processes as distinguished from living and non-living. So this is intelligence and none-living and this is intelligence and living so we used to call this natural intelligence. And this is non-intelligent and living so these would be viruses and bacteria, right? And these would be resisters and tables, okay? If you split the world this way, then you are now dealing with the noological sciences, and these are the biological sciences and just as these are the non-biological or non-living (we happen to call them physical and I think it is a very bad term - bad in the sense that this should be the negation of this, and by their names it certainly is not the case) so since I'm not stuck with the names I call these the analogical sciences. And I identified what is parallel to physiology in noological terms and then had a whole genus-species with Latin names.

But the point is that when you make the parallel which I always like to do because that is my mode of thinking (you can tell from some of the things I've done). It's an accident in a sense that at one time we both split the world into these categories; suppose we split it into these kind? How would the names of sciences come out, and the names of things? I think it is an important way to look at things. But people weren't listening so I finally quit.

McCORDUCK: Well, you have covered the waterfront, haven't you?

FEIN: Well, it's a lot of avocations, and I don't have the last eight years of stuff where I've dealt in an entirely different field.

[Both now look through other papers.]

McCORDUCK: It looks like by 1968 you have a talk here called "The Computer Field, What Was Promised, What We Have, What We Need." You've become a gadfly.

FEIN: I'm very sensitive to the term gadfly because it's applied to me again in various other situations. A gadfly is someone who is just hopping all over the place and not making very serious pokes at things, just superficial commentaries. That's what I think of as a gadfly.

McCORDUCK: No, my view of gadfly is those people who keep you honest.

FEIN: Yeah, yeah, okay, that's fair. I like that.

McCORDUCK: I think every one of us needs someone who comes around and asks all the hard questions and that's my view of what a gadfly does and you get angry at them because they ask hard questions which you don't know how to deal with and which you wish they hadn't brought up, but its still very useful to have in one's life or in society or whatever.

FEIN: The life of a gadfly is tough. You have to have independent resources. And that includes financial resources but it means psychological resources, too.

McCORDUCK: That would be harder to provide than the financial.

FEIN: [After much discussion about peanut butter] Gosh, doesn't history want this?

McCORDUCK: As long as I have you captured here I am going to ask you one last question and that is: what kinds of things do you see happening with computer science? You have been right on the money so far - are you willing to make predictions for the future now?

FEIN: As I did earlier, I always need to distinguish between computer science as a study in itself which means an improvement there, what with the equivalent of getting new theorems, would be to get new structures, new computer structures, new programming structures, and new ways to apply hardware structures and programming structures to other disciplines. That is the applications: the computers for engineering or biology as distinguished from computer period. I think the - let me make one other characteristic which I call synoetics as distinguished from computers as a single entity, not in combination with other intelligent entities. I think the science of - I need to go even further now - I haven't thought about this real well - noology, the science of intelligence has a subpart, synoetics, which in turn has a subpart, single computers of single people. I think noology won't be so names, will be developed as a science and we will learn more about what characterizes intelligence--and I must say I took a shot at doing that in my noology paper. Again I use a parallel. When you ask some biologists what characterizes a living thing as opposed to a non-living things, at least they used to say that a living thing has two characteristics: metabolic and reproductive. And a non-living thing may be reproductive but not metabolic or the other way around, but they had trouble with viruses, as I understand it (I am just not a biologist) and anyway they tried characterizing and I suppose they are still trying to characterize the living as distinguished from the non-living thing. And by that parallel I suppose we will never get really to a sharp distinction between intelligence and non-intelligence, but I have taken a shot at finding

two characteristics again of an intelligent thing and I have said it's a parallel to reproductive and metabolic. I've said that it is adaptive and cognitive. I don't know what people would pursue that line by taking on themselves trying to answer the question what characterizes an intelligent from a non-intelligent thing. They won't come up with what I came up with necessarily or whatever. But I think that has to happen. If you asked for a forecast then the distinguishing between intelligent and non-intelligent things in the world and identifying what characterizes each. It isn't clear to me that people are even trying to ask the question let alone be known.

McCORDUCK: No, we move along on the assumption that we know what intelligence is until we are asked to define it, we get all flustered and say, "Well you know what intelligence is, anybody does."

FEIN: Just ask the person who financed my contract, I assumed everybody knew! So that noology in the sense that I described it, I think needs to be pursued. I don't know how to forecast that it will be, I can only say it needs to be, and if I had the money I would fund the study for making such a distinction. With respect to synoetics, combinations of people and people, and people and machines, and machines and machines to do intellectual tasks better - that's clearly going on and will continue to go on, not under that name but that's not much of a forecast to make; you can see that happening every day. With respect to computers themselves, well, I don't know what to say. Bubble memories are here, chips are smaller, multi-megabyte memories are going on chips, no longer electronic, now bubble. I don't see many new organizations on the chips, if you can get them all onto bubbles, who cares about organization anymore - you can do everything so cheaply there is no need for refining anything - everything is brute force. I don't know how to...

McCORDUCK: That wasn't a very fair thing to do, you weren't expecting to be asked that. Well, let me bring this to a close. I wound up taking up more of your time than I should have. This has been a conversation with Dr. Louis Fein on May 9, 1979 in Palo Alto for the Charles Babbage Institute.

INTERVIEWER: We just have a little addendum here regarding the name computer science. Do you remember when you first thought of using that?

FEIN: Yes, I first thought of using computer science in 1956, when I was working on the study for Stanford University.

McCORDUCK: Okay. And to you knowledge that was the first use of that phrase?

FEIN: To my knowledge that was the first use of that phrase. It wasn't published in a report until 1957 and so far as I know even that was the first publication of the phrase computer science but it occurred to me obviously in writing the report because I deliberately looked for it. It wasn't as if it just occurred to me out of the blue. It was a conscious search for an appropriate term for the name of the department, in this case a school dealing with computers and data processing related fields.

McCORDUCK: This concludes the addendum.

END OF INTERVIEW