Computer Oral History Collection, 1969-1973, 1977

Interviewee: John Todd
Interviewer: Richard R. Mertz
Date: February 24, 1971
Repository: Archives Center, National Museum of American History

MERTZ:

This is an interview with Professor Todd at the California Institute of Technology, conducted on the twenty-fourth of February 1971 at his office in the mathematics department by Richard Mertz. Professor Todd, would you describe your early background and training and what led you into mathematics?

TODD:

I think my first interest in modern science, and particularly engineering--when I came to the secondary school in Ireland, in a Methodist college, instead of taking the normal course of Latin, French and Greek, I decided to take only French and engineering. And until I went to university, my interests were largely in engineering. But, I found that I needed to know a little more mathematics to do engineering properly and I transferred to mathematics, and perhaps have been still learning mathematics until this day.

MERTZ:

Were there, in your family--any engineers, or scientists?

TODD:

My parents were elementary school teachers, not specialized in mathematics.

MERTZ:

I see. When you felt the need to acquire a greater degree of training, involvement with mathematics, was this largely in the field of analysis, in applied mathematics? Differential equations?

TODD:

No, the courses which, the program which I took in Belfast was in mathematics and mathematical physics. When I went after that as a graduate student to Cambridge, this was Downes College in Cambridge, I specialized in analysis with Professor Littlewood. And, for several years, my interests were in rather abstract analysis. And it was only during the war that I was brought back to numerical--or rather, not brought

back--introduced to numerical work.

MERTZ:

Did this--you were at Cambridge in the thirties?

TODD:

I was at Cambridge in '3l to '33 and then I was, for a short time, at Belfast, and then I was at the University of London when the war broke out.

MERTZ:

I see; and you were on the staff, the faculty—

TODD:

Of Kings College in London.

MERTZ:

And my first assignment during the war was to the Mine Design Department at the time when acoustic mines were being dropped.

MERTZ:

This was in the Admiralty?

TODD:

In the Admiralty, yes, in Portsmouth, in fact. And I realized that my training in analysis would be more useful in better handling numerical problems than in actual mathematical physical problems as such. And during my first two years with the Admiralty in Portsmouth, I developed ideas along these lines. The Nautical Almanac Office, which was part of the Admiralty, was the biggest center for computation in England, and it was natural that one should try to make use of that on computational problems. And I was, after some negotiation, able to get myself transferred to the Headquarters in London and to set up an Admiralty Computing Service to handle, to centralize the problems of computation arising in various Admiralty departments. And we recruited staff and detached them to the Nautical Almanac Office, of which Mr. Sadler was Superintendent, and they got training in computing. The equipment then available was hand machines, Brunsvigas, some electrical machines and the National accounting machine.

MERTZ:

This was around l939 and '40?

TODD:

This was about '42. It took time for the scientists to get into the war, in England, too. I have got actually a report on this here. If you like, I can probably get it. It's just across the corridor here.

[Recorder off]

A description of this computing service is given in an article in Mathematical Tables and Other Aids to Computation, Volume 2, in l947.

MERTZ:

The article is entitled, I believe, "The Admiralty Computing Service," Number l9, July l947. Well, then it was at this juncture, when the Admiralty Computing Service had been established and was functioning that you first met John Von Neumann?

TODD:

Yes, that's correct. Von Neumann came to--he was studying underwater ballistics, I think, of some kind, and explosives. And he came, and my wife had known him earlier, and he came to inquire about what was going on in this area in the Admiralty and elsewhere, and we were in contact then for some time. In this connection, the episode which was described in my memorandum occurred.

MERTZ:

You mentioned that your wife had known him earlier. Was this when he--prior to his coming to the United States?

TODD:

Yeah, I think this was in Goettingen or Vienna or some Congress. When you speak to her, you can find out more details about this. She has got a remarkably good memory for these things.

MERTZ:

Yes, and, if I recall correctly from your description of the events, he later wrote a letter which described, set forth in some detail, his--the role which his visit to the Admiralty Computing Service and your group played in his own work.

TODD:

Yes, yes this is correct.

MERTZ:

Well, you continued on with the Admiralty Computing Service?

TODD:

Yes, after, after, at the end of the war, I was undecided whether to continue--whether to return to my pre-war activities or to continue with my new interests. And the decision was in favor of my new interests, partly because of an invitation to visit the Bureau of Standards, which occurred in l947. If I go back a little bit: whenever--the experience that we had in the Admiralty Computing Service led us to recommend the formation in England of a National Mathematical Laboratory, and such an organization was set up as part, as a division of the National Physical Laboratory. And similar thoughts were going through people in America and Dr. Curtiss, who was then head of this group at the Bureau of Standards, who knew about my activities, invited me to come to take part this and, in particular, the original invitation was to take part in the activity at Los Angeles.

MERTZ:

This was the newly created Institute for Numerical—

TODD:

The Institute for Numerical Analysis. When we arrived in l947, this only existed on paper, and so our first three months were spent in Washington. And then we made contact again with Von Neumann, who had started his Princeton project, and he invited us to spend some time at Princeton, and we spent, I think, the first three months of l948 in Princeton. And only got to California in April or May of '48.

MERTZ:

I might go back just a little bit. In the waning years of World War II and the immediate post-war period in England, with the Admiralty Computing Service and, I gather the establishment of this new facility, national facility, along with that, were there parallel efforts in increasing the mechanical capabilities of computing devices?

TODD:

Well, I have forgotten the exact dates of the Princeton--I'm sorry, the Cambridge activity. Professor Wilkes, in Cambridge, visited the Moore School, also Professor Hartree had been there, and was interested in the ballistics for a long time, as one of the associate fellows of the differential analyzer in England. And about that time, there certainly was activity in building the EDSAC. In fact, the Cambridge EDSAC was one of the very first machines that were going; whether it was going before SEAC or not, I just don't remember.

MERTZ:

It actually was in operation in, for those days, surprisingly short time.

TODD:

Yes. Now, you see, at the same time, one of the central figures in this whole area is Turing. Now Turing was a very abstract logician. During the war, he was presumably engaged in cryptographic activities in England and had presumably some concern with equipment there. And he was brought, at some stage, to the National Physical Laboratory to design a machine for this new National Mathematical Laboratory, and this was called the ACE--the Automatic Computing Engine. And we, at that time, or toward the end of the war, Mrs. Todd was working at the National Physical Laboratory in a different group, in the flutter group under Dr. Frazer. And we had contacts with Turing over these, during these times.

MERTZ:

Do you happen to recall who today would perhaps be one of the more, or most, conversant of the individuals who are still involved who would know of the activities of Turing and his group at that time?

TODD:

Huskey, Professor Huskey at Santa Cruz spent a year, I think, at NPL, and he would certainly know of the engineering activities. There are still some of the early people of this group. I mean Goodwin, I think, who was part of our Admiralty Computing Service at one time, went to this National Physical Laboratory, set up after our project was disbanded at the end of the war. He would certainly know the history of this.

MERTZ:

And he is presently—

TODD:

He is presently the Director of this mathematical division, I think, Applied Mathematics Division, of the National Physical Laboratory. The first Director was Womersley, who died, and he was succeeded by Goodwin and presumably... . He has much of the records; oh, and the other person who was there from the beginning was Wilkinson, who was more interested in the use of the machine, the numerical methods, rather than the actual design. But, there is quite a lot of information available.

MERTZ:

I see. Do you recall any individuals during this period, aside from the ACE group, who were active and who are still around and available, in addition to, well, Wilkes and Wilkinson.

TODD:

Yes.

MERTZ:

For example, did some others go from the Admiralty Computing Service Group out to other—

TODD:

Most went to the National Physical group. Some of the names, I think, are mentioned in my laboratory... [sotto voce:] I'll get it. Fox, who is Professor at Oxford, Olbern [?] is Professor at Maryland now--he was in this group.

MERTZ:

Oh, he was?

TODD:

Yes, he would be someone you could easily get hold of. And, of course, Mr. Sadler, who was the Superintendent of the Nautical Almanac Office. Those documents [?] would know a great deal about this, too.

MERTZ:

When the EDSAC was functioning, do you know who were some of the principal figures, from a user point of view?

TODD:

I suppose that J. C. P. Miller, who was one of the old-fashioned hand computers, who moved to Cambridge, to the Cambridge University Mathematical Laboratory, and has been there, I think, since the time of EDSAC, and has done a great deal of computing, he would know quite a lot about this.

MERTZ:

And he is still--in Cambridge?

TODD:

He is still there. Yes. He has visited this country several times, but not recently. [Pause]

MERTZ:

Do you happen to recall at that time--the period l946 and '47, just shortly before you came to Washington--in the United States, this was a rather crucial period in terms of the kinds of financial support. Decisions were being made as to whether or not support was going to be given to the development of various machines after ENIAC, and was there--could one make any kind of parallel in England for that period? As to the extent, or whether or not there was a debate in terms of--to go ahead and allocate substantial funds?

TODD:

I don't think there was much. I think the EDSAC was built very cheaply, you see, and that must have been the only machine, I mean apart from the ACE, in the National Physical Laboratory. There are some troubles about the development of the ACE, I mean, there are no doubts about that. Turing was a Prima Dona, he was a wonderful mathematician, and, I think, he also knew more about the tubes than the electronic engineers there, and this caused some friction; and so, I think, it was arranged that he'd be let go, and he went to Manchester and then set up a group there. And this was where Williams was, and Kilburn, I think, and so forth. And so that was the next center of installment.

MERTZ:

The catalyst, more or less, for this was Turing?

TODD:

Was Turing. Yes--and Williams. And I'm not quite sure exactly where Williams came into this. But, it seems to me that the groups were then at Cambridge, the NPL, and this Manchester University. Of course, the Manchester University had the history of the differential analyzer. Hartree had been there. They had

MERTZ:

a tradition of computing machines.

TODD:

of computing machines, but not--what this was. So we visited Turing in Manchester.

MERTZ:

Do you happen to know whether there was much contact between, communication between, Von Neumann and Turing?

TODD:

I think there must have been, because I think Turing was in Princeton at--and I know that they had talked over at least this--there is this famous paper of von Neumann and Goldstine on matrix theory, on the inversion of matrices, and there was a parallel paper by Turing which was published about the same time, and certainly, I think, that there was contact at that time. Of course, as you know, at Princeton there was quite a big lot of foreign visitors, Stifel, who set up the Swiss activity and Fruger and Aas [?], who were in Sweden, and these were the ones who were...

MERTZ:

At Princeton. Well, to say nothing also of the number in the United States who later moved on,

TODD:

Yes, yes, sure.

MERTZ:

at the University of Illinois,

TODD:

Yes, yes.

MERTZ:

and JOHNNIAC, and others. Were they there in '48?

TODD:

I was there in '48, yes.

MERTZ:

I see. One thing I'm having a little difficulty in identifying, although Professor Kaysen's office at the Institute is helping to locate it, that is a visitor roster, lists of people who spent time at the Institute during the period. This is something we haven't yet been able to [do]. It is a very useful thing to be able to say, well, we know that so-and-so was there for x months or as a—

TODD:

Wasn't there a history published, of the Institute for Advanced Study? I know I got a copy of this, quite a big book. I have it at home, I don't have it here; but this probably does give an indication of the formal visitors who were there, who had, so to speak, legal, temporary appointments.

MERTZ:

Yes, I believe Professor Felix Gilbert was active in producing part of that history that you're referring to.

TODD:

Is that so?

MERTZ:

As a historical seminar of the Institute or something. But there is a listing of the formal--?

TODD:

I would think so. I mean, this came a long time ago, I can't remember. But it might be worth looking at.

MERTZ:

Well, there was, then it would be fair to say that there was not a superabundance of funds, perhaps either in England or the United States, in '46 or 7,for funding any expensive machine projects?

TODD:

No. This only came quite recently, quite--in the last few years--quite a lot of development. You probably have seen this ONR report on computer activities in England. Have you seen this one?

MERTZ:

Is it recent?

TODD:

Yes. This was a five part—

MERTZ:

Ah yes: [Reading:] The British Computer Scene, Part I. The Government Impact on the Computer Scene. ONR Technical Report ONRL 2167, dated the 20th of April 1967, from the branch office in London of the Office of Naval Research. The authors Cowie--C-O-W-I-E--Beaman, and Macon.

TODD:

I think this is six parts. This is six parts, I think.

MERTZ:

I see.

TODD:

Yes, it was six parts. So this will certainly give the recent history.

MERTZ:

well, then, when you appeared on the scene in Washington, the Institute for Numerical Analysis was an idea, not a reality.

TODD:

This is, this is true.

MERTZ:

Did you meet Sam, Sam Alexander?

TODD:

Oh yes, and Lubkin, all this, yes. All these things, of course, you probably have all the reports of the Applied Mathematics Lab at the Bureau.

MERTZ:

We do not have them all, but they have them at the Bureau.

TODD:

I mean, this, if necessary, I have a full set here,

MERTZ:

I see.

TODD:

which gives the full staff. Which is really very interesting, because this Bureau of Standards group played an enormous part in training people all over the world in this area. I mean, at that time, we were coding for imaginary machines, various problems, and trying to design the most efficient set-ups. Our only equipment, I think, then were the IBM Card Programmed Calculators, I think.

MERTZ:

The CPC.

TODD:

The CPC. These were about the most modern things we had. After this year in Washington, in Washington first and then in Los Angeles, I went back for a year to England, and then came back to Washington in 1949; and I then was the Chief of the Computation Laboratory.

MERTZ:

And when you returned to England, did you return to the—

TODD:

To Kings College in London. I was on leave.

MERTZ:

You hadn't then completely decided whether you were going to

TODD:

No, no. No.

MERTZ:

abandon your earlier—

TODD:

This is true. So, 1949 was a very bad time in the English universities, and when I got a reasonable offer from the Bureau of Standards I, I was as startled by it as by coming here.

MERTZ:

Was it economically a very difficult time in the English universities?

TODD:

Yes, the salaries were not good, there were very few positions available, those of us who had been in the war found that, had difficulties, found that changes had been made while we were away--I refer to the curriculum and things of this kind, which needed to be changed, and it was very hard to organize these changes. We did try at that time to set up a central computer, or central applied mathematics institute at the University of London, but financing activities in the University of London goes in five year blocks, and we were not voted in at that time, and so it looked as though it would take five years before something would start, and so it did. And so we decided to come to Washington. [Telephone rings. Recorder off] Well, as I say the financial situation for staffing a university was pretty poor at that time; it improved considerably in the next couple of years, and since then there've been a mass of new universities built, so the situation is quite different now. But then, this was 1949, I think, we came back, and during this time, the SEAC was being built, and I think the first time I used an actual computer was on, I think, Good Friday in 1950. And I know the problem which I did on this, and this was to generate, factorize, the Fibonacci numbers.

MERTZ:

Was that the first problem on the machine? JTY:I think Dr. Franz Alt--you've spoken to him, haven't you?

MERTZ:

Yes.

TODD:

Well, he may have run a factorization program before I did. 'm not quite sure. But these were the first two problems which were run, I think, on the machine.

MERTZ:

That's roughly about--is about the time the machine was actually built.

TODD:

This was when it was built, when it started.

MERTZ:

And on the staff--Well, I take it that prior to this, there had been some theoretical coding problems?

TODD:

Oh yes, yes. During, as I said, my first stay of 1947 we had done quite a lot of these, but the first actual real-life use of the machine was then.

MERTZ:

Was there any call made upon the users to assist in developing any kinds of test which was used on the machine, or were these largely done by the engineers themselves?

TODD:

My impression is that the testing was done by the engineers, on the whole. All our programs at that time, that we wrote, incorporated many checks. We were very, very distrustful of the machines. We checked almost everything, and we knew about the unreliability of the machines, and so we arranged to have dumps made of every five or ten minutes, so that we would not lose work, and there is a very curious indication of the unreliability of the machines implicit in some results which I published in my book on numerical analysis in connection with Monte Carlo. The Monte Carlo problems are a very good test-- well, are very easy to code, and amusing, and we did a lot of problems of that kind initially, and we made several experiments. And the number of walks, random walks which were taken, were different in these experiments; a tidy mathematician would have the same number, but the reason for this, these odd numbers occurring, was that this was when the machine broke down. And, the next--I mean, I think at this time, the computing lab, the laboratory at the Bureau of Standards, was the first general purpose computing center equipped with automatic computer.

MERTZ:

Was there any problem posed by demand for machine time to use by various users? Which--I would assume that, in your position, you would have to make some decision on allocations?

TODD:

There were great troubles about this kind [of thing]. This machine, the SEAC, was built by the Air Force, for--under the sponsorship of George Dantzig--for linear programming problems. And he was very insistent, rightly, that it be used for Air Force problems. In particular, there were certain problems about the deployment of forces during the Korean War. Some of us were more interested in exploiting the machine in general contexts, so we had some discussions about that. Of course, the engineers wanted to develop the machine, too, and so there was discussion there, too.

MERTZ:

They wanted, what they might call, fiddle time.

TODD:

They wanted fiddle time. And there was a perpetual argument about the delivery of the machine. The transfer--we wanted the machine transferred from the engineering building to the mathematics building. We thought this would keep the engineers away, a little bit. And I remember perpetual conferences with Dr. Condon concerning this transfer. Decisions were made and reversed several times each day. I think the next critical phase was the time of the hydrogen bomb. And more computing time was needed there. And I visited Los Alamos, discussed this with Dr. Teller and the other people there.

MERTZ:

Did you talk to Nicholas Metropolis?

TODD:

Yes, I suppose so, yes, yes. He was at Princeton, of course, too. And--I have forgotten exactly who were concerned at the time. But it was then decided that the hydrogen bomb effort was to have priority, and they refused to allow us to contemplate moving the machine, because it might get damaged in movement, but I was able to persuade the Atomic Energy Commission to set up a new building, next to the machine, for the mathematicians. And this was done, it was possible then to build a building costing less than a hundred thousand dollars without too much red tape. And this building was built, and we moved, the mathematicians moved, next door to the machine.

MERTZ:

It's a piece of the prophet going to the mountain, when the mountain won't come to the prophet.

TODD:

Yeah. And then, somewhat later, the machine was moved back into the mathematics building. It was several years later; I've forgotten the exact dates.

MERTZ:

Roughly how large was your staff?

TODD:

The staff was about a hundred, up to one hundred.

MERTZ:

This includes your programmers?

TODD:

Everybody. The secretaries, also. The listing is in these Bureau of Standards documents. Then we had a crisis, of course; several crises. One, which led to the disbanding of the Institute for Numerical Analysis, at Los Angeles: there was some political trouble about us, the computing center, and unfair competition. We were competing with private industry on subsidized machines. And this led to the transfer of the whole operation at UCLA to the University by the Bureau of Standards. This must have been in 1953 or so.

MERTZ:

Was this at all contemporary with the other, the thing involving the Director of the Bureau of Standards,

TODD:

Astin?

MERTZ:

Astin.

TODD:

I think this was a separate activity. But then, there were other troubles at the Bureau of Standards, in connection with the ADX2, and also, in general, when the Eisenhower administration came to power, we had to submit a twenty-five percent cut in my staff and this was a very unpleasant time. Fortunately, we were able to get most of our people resituated and we were also able to keep the better ones. This is a natural phenomenon. So—

MERTZ:

So then, was it primarily a budget cut in connection with the idea of unfair competition that led to the demise of the Institute for Numerical Analysis?

TODD:

Yes.

MERTZ:

In '53?

TODD:

About '53, I think, this was.

MERTZ:

I see. Did that have a serious impact so far as the West Coast, the stimulus which that organization provided to the West Coast for computer—

TODD:

Well, this had a serious effect, I think, on numerical analysis the world over. Because this was a great center, you know, and the listing of staff: Everybody had spent some time with us. And it has gone downhill since then. As of now it hardly exists as a—

MERTZ:

Did you have much contact with the Institute for Numerical Analysis when you were

TODD:

In Washington?

MERTZ:

in Washington.

TODD:

Yes. We spent several months each year out in Los Angeles.

MERTZ:

I see.

TODD:

This was, oh

MERTZ:

That was a very

TODD:

convenient,

MERTZ:

convenient arrangement.

TODD:

Yes. And then, when the Institute for Numerical Analysis was formally disbanded at UCLA, we reinstituted it in Washington. It was still a formal section of the Applied Mathematics Division. And, at that time, I--having served five years or so as head of the computing center with all its problems--decided to change to the more academic atmosphere of the Institute for Numerical Analysis and then Dr. Abramowitz, I think, took over as chief of the Computation Laboratory.

MERTZ:

Was Cannon there at that time?

TODD:

Cannon had been there since the beginning.

MERTZ:

Since the beginning.

TODD:

He was at first in charge of the Machine Development. There were four sections within the Applied Mathematics Division: Institute for Numerical Analysis, the Computation Laboratory, Statistical Engineering, and Machine Development. And originally, the Machine Development was concerned with making specifications for machines for the Bureau of the Census.

MERTZ:

The UNIVAC I.

TODD:

The UNIVAC. And, naturally, some friction arose between this and the engineering division. And, I think, at some time then, Machine Development was out, unrealistic name, I think it became, Mathematical and Physical--Mathematics and Physics

Section--and Cannon remained in charge of this, and then he was finally appointed the Head of the--the Chief of the Division, when Dr. Curtiss met.

MERTZ:

During the period from the inception of the Institute for Numerical Analysis in Los Angeles--which was about 1948?

TODD:

1947.

MERTZ:

47, yeah--to 1953, who were the leading figures? Many people came

TODD:

Yes.

MERTZ:

and passed through these, before you were probably—

TODD:

Well, I think, for instance, Hartree was--well, I think of the Director as included--Lehmer, Rosser, Fritz John from New York University, then--I think those are the--again this can be checked on the documents, but these were the Directors. I mean, Stifel was there, Ostrowsky was there, many distinguished American visitors.

MERTZ:

This perhaps accounted, the last two you've just recited, might have accounted for the fact that a number of distinguished mathematicians did come to stay and spend time there.

TODD:

Yes, yes. This was a first-class mathematical research institute. And Dr. Curtiss, who was the Chief of the Division, really supported this group very well. Saw that they had the best, the best--in library, he built up a really wonderful library there, must be one of the best in the world in this area. Part of this building of the library was due to Professor Forsythe, at Stanford, I believe it was he who became very interested in this ..., and this library now is a sort of departmental library at UCLA, but unfortunately they have not kept up the numerical computer side. In 1953, this was really a first-rate, specialized

library. It still has the aura, and the material up to then.

MERTZ:

Well, then, it tended to decline locally, once sponsorship was more or less withdrawn from the Bureau of Standards. What--I take it, it couldn't really be replicated in Washington, the way it had been during the period from '47 to '53.

TODD:

No, not on such a big scale. But we had very good people there, Professor Philip Davis, who is now at Brown, was there, Morris Newman is still there. Again, we have quite distinguished visitors, Wieland, Ostrowsky, again, we can discuss this with Mrs. Todd sometime, who has an encyclopedic memory of these things.

MERTZ:

I see. And this transition took place about '53 without--there was not quite the library facility, I gather,

TODD:

No, No.

MERTZ:

that had been built up.

TODD:

I'm not sure exactly when, whether it was '53 or '54, but about this time, but until 1957, when I left there, we had, not as big a group here, but really, I think, quite an unusually strong group there.

MERTZ:

Was there any diminution of support for the activity over the years, as time went on?

TODD:

No, I don't think so. I mean it was always hard to get money, but we had, I think, adequate--we had to worry about getting enough money from the Office of Naval Research for the actual research activity. During that time--yes, I might mention that--oh, about 1955 computers were becoming more universal in the universities and so on, and there was no one to run them properly. And so I had a project to train university staff in numerical analysis and, generally, computer operations. And I got a considerable grant

from the National Science Foundation to organize such a training program. Bakely [??] said, "this will never--you come to us for six months and you can go from an assistant professor of mathematics to a professor of computer science," or something of this kind. And we got about fifteen people to come for six months; and we gave them training, lectures in the morning and working on the computer in the afternoon, and the results of some of the lecture material was published in this Survey of Numerical Analysis which I edited later. This program--let's see the actual date [leafing through a file]. Yes, this actual grant was made in 1957.

MERTZ:

I see, aha.

TODD:

From 1955 we had thought about this and planned it, and finally we got the money in 1957.

MERTZ:

From the National Science Foundation.

TODD:

From the National Science Foundation. And that was about my last activity at the Bureau of Standards.

MERTZ:

Was this, more or less, a one-time occurrence, or were they—

TODD:

No, I was just going to say that. I left in the summer of 1957, and this course was repeated by Professor Davis, who took my position as head, chief of the Numerical Analysis section, a couple of years later. Effectively the same program. Now, I should say that we were very happy at the Bureau of Standards there, but we were--are--disturbed by the fact that the Bureau was going to move out to Gaithersburg. And this was our main reason for moving. We were always, both of us, were always interested in teaching and although we did a little of that at American University it was not genuine, full-time teaching.

MERTZ:

full-time, academic climate.

TODD:

But one of the reasons why we could keep good staff in the government employment in Washington was that, being where we were on Connecticut Avenue, any of our friends who came to meetings in Washington, and had an hour to spare, could come and visit us. And we kept in contact with academic mathematics, we were able to recruit, and so forth, and now the thought of moving twenty-five miles out made this sort of thing completely impossible. And this was our main reason for leaving.

MERTZ:

Once the Institute for Numerical Analysis had ceased to exist at the campus of UCLA, did you still maintain fairly close contacts out at the West Coast, or did this tend to reduce itself once the Institute was reconstituted in Washington?

TODD:

I have really forgotten. I imagine we did keep, we certainly did keep in contact with the people at UCLA. If I may go back a little bit, just--one of the ways that we got this Institute for Numerical Analysis on the map was, I think, in 1951, there was a jubilee or centenary, a jubilee, I suppose, of the Bureau of Standards. And various symposia were--special grants were, appropriations, were made for various symposia and one was to our mathematics group, and one of the first or, probably, the first symposium on matrix calculations was held in Los Angeles in 1951. And Mrs. Todd really organized this, and since then there have been symposia on this in many countries, many places, in Gatlinburg, for instance, many years, so this was the way we got international figures to come to this. So I should say, I think we still kept, maintained our contact. But they were not so close.

MERTZ:

As they had been when you were at—

TODD:

As they had been when we--yes.

MERTZ:

I take it that you found this area a congenial one to come to, and you came up to the West Coast from the Bureau of Standards, didn't you?

TODD:

Well, Cal Tech is a wonderful place to be.

MERTZ:

Now was this at a time when there was increasing interest [in the] development of pedagogical—

TODD:

Yes, this is true. Up to then, here, although some graduate student, I think, Mr. Prankalter [?], built machines, an LGP 30 I think it was, here as his thesis or something, the numerical analysis was non-existent--was pre-historic. And my, shall we say, assignment was to modernize this in this period, and this is what I have been concerned with since then. The machine which we had here originally was a Burroughs 220, I think, it was--a Burroughs 201 maybe, and then we moved up the scale. And probably will now move down on the scale because of financial troubles.

MERTZ:

What is the equipment ?

TODD:

We now have a 360/75 and a PDP 30--[no], PDP 10-- which services our consoles, so we use the PDP 10 for elementary classes and the 75 for our later

MERTZ:

serious research.

TODD:

research and classes.

MERTZ:

Was it, then, more or less incumbent upon you to establish, organize a curriculum for instruction, not only in numerical methods, but as applied to computers?

TODD:

Well, yes. I think this is what I was saying. The machine was there, and, I mean, I can--One of the first things that I did here was: There was a rather traditional course given in numerical methods, which was purely theoretical, which had no practical work; so my first activity was to make sure that our numerical analysis courses had two hours of lab periods per week, and this has continued.

MERTZ:

Do you happen to recall off-hand, and this is not something that precisely is important, but your impression--from what departmental areas of Cal Tech did you get the greatest student interest?

TODD:

At some stage the biggest interest was from the geophysicists. They--Professor Press, who is now at MIT was then head of the seismology lab and insisted that all his students should have two full years of courses in numerical analysis and he had a very good group of students, I remember, in a class of thirteen we had eleven geophysicists. And these people did very well; they are all now full professors here.

MERTZ:

I see.

TODD:

But, otherwise [pause] the audience is mixed, is mixed. Few mathematicians.

MERTZ:

Would you say that the engineering schools tend to be a fairly constant source of student population for you?

TODD:

Yes, yes, oh yes. The physicists sometimes think they are too clever, and they don't need to learn numerical methods, they just pick this up out of their heads. The engineers are not so, are not so

MERTZ:

More modest.

TODD:

snobbish, and will come.

MERTZ:

Have you noticed, one observation that has been made repeatedly to me by mathematicians, particularly applied mathematicians, in academic and research settings, that there is an increasing sense of a cleavage between mathematics departments and the field of applied mathematics broadly conceived, which was less so, at least in their

opinion, generalizing a bit, in the days of someone like Von Neumann rather than it is today?

TODD:

Yes, this, I think, is true. We have here an unfortunate situation. We have a Department of Mathematics and a Department of Applied--a group on Applied Mathematics, which is in a different building, and an Information Science group, which neither of the mathematicians have anything to do with. This is all very, very unfortunate. I think one of the troubles is that it is difficult to find somebody who is an acceptable head of a group in mathematical sciences. Von Neumann would be, but—

MERTZ:

Von Neumanns don't grow on trees [chuckle].

TODD:

Don't grow on trees. And so it is very difficult to find somebody who is as equally acceptable.

MERTZ:

It has been said, and perhaps you might wish to comment on this, that Von Neumann did play a role in the academic world, at least in the United States, in making computational problems in applied mathematics a bit more respectable among many mathematicians who could not really--it is very difficult to condescend to a man with the stature of von Neumann, and he commanded the respect and his interests therefore could not be viewed, perhaps, with the kind of disdain that some purists, at least in mathematics—

TODD:

Well, this is true not only for von Neumann here, but it's also true for Turing in England.

MERTZ:

I was going to ask you.

TODD:

Because he was a first-class logician, and he was making the subject respectable.

MERTZ:

Did he, had he lived longer, do you think that his impact would have persisted?

TODD:

The latest, I mean, Turing's latest work was on morphogenesis. He had moved away from mathematics into biology. I think this would have been a great thing for biology and also for mathematics. I remember talking to him about these things and finding it very difficult to follow, because these were--[it was] quite unfamiliar territory to me, and I don't think, I don't think his latest work was ever published.

MERTZ:

It is interesting that this kind of shift one also can observe among others of his era who were interested in computing machines. George Stibitz, I mentioned, went to the medical school at Dartmouth, and some of the very last things that Von Neumann did also—

TODD:

Well, Von Neumann's interest, you see, in automata and self-producing mechanisms these were lectures given here, originally, you see. And, I suppose, I would have thought that if Turing or Von Neumann had lived, I mean, there would be more rapprochement, like between the biologists and the mathematicians, ... it takes people like them to interpret the problems.

MERTZ:

If you could, in just a, perhaps, a somewhat summary fashion, describe what, in your view, the near or long-range future holds for the field of applied mathematics as an independent field within mathematics and computers in the academic world, which is the one that you have been the most intimately associated with.

TODD:

Well, my feeling, I think, on the whole is that it is not right to separate pure and applied mathematics. That there is mathematics and some part is pure, some is applied. And my own, personal feeling is that computer science is applied mathematics; and that this whole group should be all together. My present, personal feelings about the machines are that I get disturbed by the fact that we change machines every two years, and I would like to be given a machine now and guaranteed to have it, the same machine, for ten years, because, ok, we can improve machines, but we have not by any means exploited the ones which we have. So what I'm talking about, I think, is standardization of, let's say, of machines. I haven't time to keep up to date with every change of the machines here. I have rap--yes, I enjoy machines. I use them. At home we have two hand machines, Brunsvigas, which we use; I would use the consoles here, but with these batch processing machines, the 75, for which I have to have so many control cards which change every week, I think this is just keeping the many mathematicians away from the machine.

MERTZ:

So we are creating a barrier between the user and the machine rather than—

TODD:

Yes.

MERTZ:

This is an observation that Richard Poirier [?] made when I was down there.

TODD:

So I think that what we want in universities are middle class machines which you keep for quite a long time, and if we have--and we will--we do have problems which are beyond their scope, then we can make pilot studies on the small, our local machines, and then, having specified exactly what we want, then we can get someone to do the programming for a big machine, or if it's really necessary to learn it ourselves, but I have found this troubling, this change; rather distressing. I also like this question of interactive machines. I'd like to see the progress of calculation be altered, during--according to output. This is very difficult to do with batch processing machines; it's possible if one thinks in advance of all the possible outcomes of the problem, but this is not economical. So I feel I want a middle class machine rather than a

MERTZ:

Large, expensive one.

TODD:

than a larger one.

MERTZ:

Do you think that perhaps the current economies which are being imposed in general, might produce that effect

TODD:

Yes.

MERTZ:

inadvertently?

TODD:

Inadvertently, I think, yea. I am just writing various memoranda on this. I mean, what we're saying is that there should be central regional machines or national machines, but for the general university, the ordinary university, middle class machines, several middle class machines. I fully agree that you get out of the big machines when you are doing a massive calculation. But we are not always doing a massive calculation. And little, pilot experiments. I think the other thing, too, that is troublesome in this area, is the lack of standardization of programs. And the quality of the programs. Just now I have my students doing some problems about the error function, and they calculated values of the error function, using the program which came with the machine, and also with the most up-to-date program which I know; and the answer is that for forty calculations of the error function, the best program took zero milliseconds, and the program in our system took ten milliseconds, so that there is a factor presumably of twenty or more going wrong.

MERTZ:

Between the program on the machine and—

TODD:

between the program in the machine and the best possible one that is in the published literature. And this is true not only here, but it's everywhere.

MERTZ:

Do you foresee any change in the somewhat segregated views that the purists and applied mathematicians within mathematics departments—

TODD:

I think this one proves, you see, because the pure mathematicians who get assigned with machines are going to go out ahead of the other ones, they can make experiments quickly, and experiments are a part of mathematics, to make conjectures and so on, and it's much faster to do these on a machine, and you can throw away wrong ones, in thought, and I think that when the ordinary, I should say, the conservative mathematicians see that their more adventurous colleagues who use the machine are beating them, they will join in, too.

MERTZ:

There is a certain seductive quality in terms of using the machine itself and seeing what can be done when the potentiality is a reality.

TODD:

Yes.

MERTZ:

But it's an optimistic view of the future.

TODD:

Oh?

MERTZ:

It's an optimistic view of the future. Well, thank you very much, Professor Todd.

[End of Interview]