Computer Oral History Collection, 1969-1973, 1977

Interviewee: John V. Atanasoff (1903-1995)
Interviewer: Uta C. Merzbach
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BOTH: [Brief exchange on starting tape recorder].

ATANASOFF:

Well, you see, I might just say a few more words about Wisconsin.

MERZBACH:

Yes.

ATANASOFF:

You see, in 19-- in the fall of 1929, why, Dr. J.H. Van Vleck took a year's leave of absence and went to Germany, I believe, I'm not quite sure, went to Europe for associations and study. And they had to have a good man in residence to cover the work that he had been doing. And they selected -- oh bless me, I see his picture perfectly well -- a well known theoretical physicist from Zurich.

MERZBACH:

Wentzel?

ATANASOFF:

What?

MERZBACH:

Wentzel?

ATANASOFF:

Wentzel. That's right, Wentzel. How did you know? Do you just know everything?

MERZBACH:

Well, no, just from your –

ATANASOFF:

Had I previously mentioned it? Yes; you remembered it and I didn't. All right, Wentzel came and – he'd recently been married at that time and it was kind of a honeymoon for him. And he commenced lecturing that next year in theoretical physics, advanced theoretical physics, which in those years meant, principally, quantum mechanics. And his English wasn't very good, so he said to me, "Atanasoff, you sit near the front, and then every time I don't know a word in English, why, I'll say it in German and you try to translate it for me." Well, my German was not too good, but my big word German was good, from a pretty extensive use of German even in those years, and I did this for him. And that way I was continuously supplying words for a lecture for Wentzel. Well, Wentzel was a very able man, and I was doing this thesis, which had been suggested by Van Vleck, on the polarizability of helium. I was having a hard time and, to tell you the truth, I found Wentzel rather more helpful than Van Vleck during that period, and the thesis came along alright. And I got it finished by the end of the summer session in 1930, and obtained a Ph.D. degree at that time. Now, I don't know, it was sometime during the middle of the summer of 1930, the exact date I don't know. It was one of the summer commencements. And I remember when I received this degree, I was worried about how the Graduate Dean would say my name, and I said, "I'm afraid he will stutter" and Dr. Warren Weaver said, "whatever he does, he won't stutter."

BOTH:

[Laugh].

ATANASOFF:

And this is an aside, a slight character sketch of this man who was Dean of the Graduate School at Wisconsin, in those years, his name escapes me, too, now. I've mentioned it before you since -- Slichter.

MERZBACH:

Oh.

ATANASOFF:

The elder Slichter, not the young man, but the elder Slichter, and he was still Dean. And I had some association with him during my stay at Wisconsin. He was formerly head of the mathematics department, you know. And of course, J. H. Van Vleck's father had formerly been head of the Mathematics Department, do you know?

MERZBACH:

Yes.

ATANASOFF:

And, you know, the Van Vlecks were wealthy people, in contrast to the typical university professors. This story should be recounted for posterity. You know, I knew the elder Van Vleck, too, but he'd retired by the time I got there, I'd seen him on numerous occasions and at meetings. The elder Van Vleck says to J. H.: "John, what do you want for Christmas?" And he says, "I want a railroad timetable. I don't know how old he was, I think, seven or eight at the time; have you heard this story?

MERZBACH:

No, no.

ATANASOFF:

He says, "Well, yes, of course, we can get you a railroad timetable if that's what you really want, but," he said, "we can just go down and get one." He said, "You can't either because I want the big thick timetable that tells the time of all the trains in the United States." Well, his father owned railroad stock and so, pulling the strings, he managed to obtain a railroad timetable for John Haswell -- I believe that's his name, J.H. And so the boy sat right down and memorized the timetable, completely. And I last saw him ten or fifteen years ago, you know he was Head of the physics department at Harvard, and then later he's retired now. And the last time I saw him he still had in mind the timetable of every train at every station in the United States. He -- literally, and absolutely, I tested him in the most minute detail. I remember last time I visited him in Boston I was at his house and his wife was sick or something, and he wanted to do something nice for me, so he brought out a box of chocolate. Well, I don't know, we were just sitting there talking and talking, and I ate a piece or two of chocolate and was looking at the box and it said "Dexter Island." "I've been to Dexter Island." [Laugh]. And he said, "where is Dexter Island?" And I said, "it's 23 miles west of Des Moines." He said, "no, it's 21." This is the way he -- [laughing] I remember once I was planning a trip for somebody and so he'd always accept such a challenge and in those days we mostly went by train, you know. And he would make out timetables for anybody at the drop of a hat without any reference to any books and give them a complete schedule. And he kept right up to date. And he was always writing long letters to the railroad companies telling them they had their trains disorganized; sometimes they would take his advice.

MERZBACH:

Aha, aha.

ATANASOFF:

Well, he was an interesting, a very intense man, who I'm sure deserved his post at Harvard. He -- the story is told about him going to the theatre and taking time off during the intermission to work out a physics theorem; he was very intense. And then I have to continue a moment by telling you that during -- the University of Wisconsin was visited by P.A.M. Dirac. And here is Van Vleck. He only had one graduate student and that was me, nobody else would -- they didn't like him, he wasn't liked as a professor at the University of Wisconsin; I don't know whether he was liked at Harvard or not. And he didn't have many people to let his hair down to, and I noticed that he was inclined to let his hair down a little bit with me. And he said, "Atanasoff," he said, "I want to admit to you that that man Dirac gives me an absolute inferiority complex." He said, "I can't think of anything that I know that he doesn't know better." And, you know P.A.M. -- did you ever see P.A.M. Dirac?

MERZBACH:

No, I've never seen him.

ATANASOFF:

You know who he is, by reputation?

MERZBACH:

Oh, yes.

ATANASOFF:

Well, P.A.M. Dirac was so dispossessing because he was a clod of an English country boy. I mean his manners were nice, but they were clumsy and his personal traits were those not of a skilled and educated person, and yet he was rapidly rising head and shoulders above most of the physicists in the world in those days; rapidly rising. And, he walked around the campus with his hands clasped behind his back, do you remember Europeans doing this?

MERZBACH:

Umhum.

ATANASOFF:

Walking with their hands clasped behind their back? It's a typical European trait, that even in those days had entirely disappeared, if it had ever existed, in the United States. Well, I got the degree at the end of the summer -- let's get back on the subject a little bit better -- and Iowa State University had offered to take me back or something. And so they gave me an assistant ... professorship in the fall. And I returned to Iowa State

University as, at first as Assistant Professor in Mathematics; and it was the -- you see, it was the, you know, more technical school of the Iowa institutions; Iowa State University, then called College. And I was determined to get ahead and it wasn't perfectly clear to me exactly how to do it. I worried more than I should have about this, I guess, because it was pretty quickly clear that I could compete technically very well with the staff that then existed at Iowa State University and this is an immodest statement but a factual one. I remember during the last three or four months, occasionally something like this is said to me, and I have heard from two or three members of the staff who were there at the day that I returned, one of these men said, "I'll always remember that you could learn anything without half trying." And, of course, one of these men in the last three months has told me this. It isn't true, it isn't at all true. It's just that I labor so intensely in the vineyard, but by myself. And I remember that. I wonder if other people--I don't really have any very easy way of telling how much others work for what they get--but I know that I work almost continuously and I had formed that habit in those years. Last week Alice said, "you seem to be a self-starter," because I just keep without necessity now, I just keep right on plugging. Well, what happened then was that I knew that I had to give some graduate work; and, pretty quick, there came a need for a course in dynamics and I had had -- well, and I'd wanted to give a course in dynamics because I wanted to learn the subject of dynamics new and fresh and better. And I taught dynamics, essentially covering the material in Whittaker, a large fraction of material in Whittaker I used to cover. It being the principal source, although I remember that I, I remember that some things were better put by Sommerfeld than by Whittaker. Do you know, I have had a little association, in those years, I had a little association with Sommerfeld. He was a kind of a friend of the Head, the man that was then Head of the Mathematics Department at Iowa State University--E.R. Smith, and they had gone to school together in Germany.

MERZBACH:

Oh.

ATANASOFF:

In -- Munich, I believe. They had been in school together many years before in Munich, and -- I believe. No, Goettingen. I'm sorry. Correct. It was Goettingen that they were together. And so when he was in this country he would come along and visit with him and I saw him and then later I saw him quite frequently at -- do you have any knowledge of those post graduate lectures that were always given on theoretical physics, and maybe yet, at the University of Michigan?

MERZBACH:

I've heard of -- do you mean in the early thirties?

ATANASOFF:

In when?

MERZBACH:

In the early thirties?

ATANASOFF:

No.

MERZBACH:

No.

ATANASOFF:

It was the late thirties.

MERZBACH:

Yes.

ATANASOFF:

It was in the late thirties that they commenced. Not in the early thirties, but they were going by '35, they were going by '35, and I don't know whether they were going in '34 for sure or not. And Uhlenbeck was there, and was a permanent professor at the University, and Laporte was there at that time, and I can name two or three other important, good men, and one particularly good man in the theory of molecular spectra was there, and then they got visiting professors in from Germany. And we had, and I would go and spend the summers there, and this gave me a chance to, you know, being somewhat isolated in the Midwest, gave me a chance to bring as much contact with the physicists to the world as I could get. At that time during those years I met such people as Pauli, and, Bohr; Schroedinger was there, and, my goodness, most everybody else that was important in theoretical physics was there in those years. And we heard one good lecture after another, and it was very, very. And then, while I was there I had some association with the mathematics department also. And I am trying to tell you of one man who was a Russian from Odessa who became a member of the staff at the University of Michigan and who was there at the last I knew and his name was –

MERZBACH:

Karpinski?

ATANASOFF:

What?

MERZBACH:

Karpinski?

ATANASOFF:

No. No. Rainich.

MERZBACH:

Rainich. How do you spell it?

ATANASOFF:

Well, you see, you're going to have trouble with this, because -- R-A-I-N-I-C-H, I guess, but I don't know whether that's right or not, whether I'm spelling it right or not. You see this is an abbreviation of a Russian name which is much more complicated.

MERZBACH:

What did he work in?

ATANASOFF:

He worked on everything. And for many years, he was one of the most prominent members of the staff of the mathematics department at Michigan. And I haven't seen him in ten or fifteen years, but he was there ten or fifteen years ago. And I had some very pleasant and excellent associations with him. And he was somewhat, he left Russia on account of being a Jew; they'd let him get out. And he was a professor at the University of Odessa, I know. Isn't Odessa in the Crimea? Yes, I guess so. Well, I commenced teaching this course in mechanics and then I -- the mathematics department consisted largely of older members and the physics department was - you know, mathematics had become somewhat static in a good many working institutions at that time, and I knew that my chance of advancement would be enhanced if I had more association with physics, and finally I brought this about, and became, first, Assistant Professor of Mathematics and Physics. Then, perhaps a year later, I don't know when, I took an office in the Physics Building, since they had more room than the people in mathematics did, and I could have a private office there; and that number was number 52 Physics. And I would suspect that I took that office, perhaps in 1933. And I then commenced to -- during these years I had, you do remember that I was an engineering graduate, I was graduated in electrical engineering. So I felt as if electronics would be a pretty important tool, and this is kind of important for the story that we're trying to tell, and so I really had given no attention to

electronics until the early thirties. And then those years between '30 and '33, I studied electronics assiduously, beginning with a book which is a predecessor of one I have right here at hand, which has an interesting history. Well, I have -- I always buy 'em because of those days -- well, here, this one. There was a kind of a ham radio outfit in the United States

MERZBACH:

Oh, yeah.

ATANASOFF:

and they had a handbook, and so I commenced reading the handbook, and then I commenced reading books on vacuum tube theory by Van der Bijl and others, and in the course of two or three years I sufficiently perfected myself so that I commenced to direct graduate work, graduate experimental work in the field of electronics.

MERZBACH:

Now you had been doing -- you had played around with radio and—

ATANASOFF:

Not before '30. Hardly any contact with electronics until 1930.

MERZBACH:

Aha.

ATANASOFF:

Hardly any. I didn't even own a radio until 1930. And so, in those years, I built two or three radios, and I studied -- well, you don't want to see that, but you just -- I just --it was in those days, it was the simplest fairly comprehensive thing that was written in electronics. And I remember I began by reading this and afterwards when I had graduate students who needed to grasp electronics, and they very seldom came to you with any preparation in electronics, you'd have to train them in electronics yourself and I would always have my students read this first, a few chapters of this, first. And then we would go on from there. By 1935 or '36 or something like that, I have a Robert Willson's thesis, which is an electronic, which is a thesis on -- well, we're applying it to the dynamics of quartz, but the mechanisms used in analyzing the dynamics of quartz were electronic.

MERZBACH:

Well, I'm still confused about something as far as your work with radio -- and maybe I

got it mixed up; but I thought that somewhere in the early twenties you had done some fooling around with receiver sets or something like that?

ATANASOFF:

No, it was in the early thirties.

MERZBACH:

It was in the early thirties.

ATANASOFF:

Yes. Oh yes, it is true that in 1923, I did -- but this was while I was in college, before I graduated from college,

MERZBACH:

Yes.

ATANASOFF:

but that's all. And I mean to say this is so miniscule by comparison. Well, my goodness, maybe, suppose, I spent three days at that time, that's the sum total of it.

MERZBACH:

Yeah.

ATANASOFF:

Not three intense days then either. So it's a very trivial contact really, until the early 1930s. I mention it, you know they keep prying at you, you know you have to be careful or your vision will get distorted, but they kept at "what's your earliest contact with radios?" and I remembered this and recounted it. Now having done this, you see, it mustn't be magnified, because this was, we didn't learn electronics, I don't think there was a -- there was not a single discussion in class in electrical engineering in those days of electronics. And I had a little -- yes, it's true -- well, that isn't quite right. I'm trying to be exact. It's true that I also read electronics in those days before I left college. But then really no serious work done in electronics until 1930. Suppose we say: Before 1925 I had studied three or four books in electronics and had worked, which amounts to quite a good deal, doesn't it, really, if you study three or four books fairly carefully, and I'd worked on one radio receiver, for a short space of time, just two or three days, this instance that I'm thinking about. But, really, my electronic work, in any serious way, didn't commence until after I had a Ph.D. in theoretical physics. Now -- and I got very intense and did it on

my own without any contact with the University. I didn't have any, oh I don't know, if anybody was teaching a course, but it didn't make any difference, because they wouldn't have been very good. So I gave myself a course in electronics. And then I went on and, we'll say about 1935 or '36, I'll see if Robert Willson's in here; I don't know. We'll find the date of Robert Willson's thesis, and that will give us the date. By that time I'd commenced to offer to direct graduate work in physics, and, in spite of the fact that I was a theoretician, they came to just about let me do anything I wanted to do at this time, and I went ahead and did it. Now here it says that in September 1935 I was made Associate Professor of Mathematics and Physics.

MERZBACH:

That's '35 already.

ATANASOFF:

1935.

MERZBACH:

Apparently you started your graduate teaching about three years before that, right?

ATANASOFF:

'32 yes. '32; '31 or '32 and I don't know. Yes, it says September '32 and that may be about right. I probably thought that over pretty carefully. I'd been interested, before I went to Wisconsin, I'd been interested in IBM punched card equipment. And there was A. E. Brandt, who was a statistician, and he is a friendly man. And he's still a friendly man. I ought to see him again before he dies. And he and I would talk about IBM tabulators and he was in statistics, and Statistics had the tabulator. And the tabulator was there because of the pressures put by Henry Wallace, do you know who Henry Wallace is?

MERZBACH:

Yes, yes. We spoke of that briefly last time.

ATANASOFF:

Yes. And I guess that Iowa State rather earlier than most institutions, I'm not quite sure, probably had IBM equipment. I think they had it there, they had it there before '25, before '25, and I remembered that. But, they wouldn't show us, IBM, they had disciplined their repairmen and their repairmen were generally agreeable, but they wouldn't discuss the internal technical aspects of the equipment very much; either because they didn't know it or because they were told not to discuss it. So I was determined to do something with IBM equipment and, it says here in '34 and'35 that Brandt and I worked on the use of

IBM punch card equipment to analyze complex spectra. I have a -- we have a publication on this. What I did was to guess how I would -- I had to get inside the computing machine and I wasn't allowed to change circuits, you see. So I had to guess how I would have built it if I had built a computing machine, built an IBM tabulator. I guessed how I would have built it and talked to the machine in that language, in order to fool the machine into doing the process which I wanted done. And I just poured that data in and it came out just as I wanted it, and it all succeeded and that's the way I did that job.

MERZBACH:

Now how did you happen to pick the particular part, I mean, was the problem first in the use of the machine or was it a combination of the two?

ATANASOFF:

Oh well, you understand, I was a theoretical physicist and this problem of analyzing complex spectra was very much before people at that time. So the problem was there, and the pressures of the problem were there. And I, of course, had the background in the machine and the familiarity with it and the two came together at that time.

MERZBACH:

And where did Brandt come in?

ATANASOFF:

Well, he went along for the ride, -- this, I don't want this published. Now he did a lot of things, don't misunderstand me. He did public relations for the firm, and he fended off the authorities who might have objected to an IBM tabulator being used for something which it was quite not what it was planned to be used, and he made all these relations and I have no objection to his doing it. But he didn't understand it. [Chuckle]. He wouldn't have known how to remake the machine and how to make it do it. But he understood it after I told him about it. And I know when I needed parts, why, he'd go around and scrounge them for me, and I had no easy sources of parts.

MERZBACH:

Well now, when you say parts, this implies that you did .. do something to the machine.

ATANASOFF:

Build something, you had to build some special adjunct equipment to go with it.

MERZBACH:

Yeah, now, how did that go over with IBM?

ATANASOFF:

Well, it wouldn't have gone over at all well, but Brandt managed to maintain the relationships -- I don't think any of those people ever knew it. We knew they wouldn't like it. And they would state that Iowa State College was violating their contract with them.

MERZBACH:

Oh exactly, yea.

ATANASOFF:

I'm sure they did.

MERZBACH:

Those were still -- well, let's see now, that was -- yes, the machine was sold, but that would have been the --

ATANASOFF:

What's that?

MERZBACH:

That would have been the maintenance contract, right?

ATANASOFF:

Yea, well, you understand they -- no, it was leased.

MERZBACH:

It was leased.

ATANASOFF:

It was a leased machine, you see, and, oh goodness, by this time the Federal courts hadn't come down on IBM. And IBM was an autocrat, and, you know, they wouldn't sell a machine to anybody. The Federal Government forced them to offer their machines for sale. And they only sold them to the Russian government. And did I tell you the story about the Russian government?

MERZBACH:

No.

ATANASOFF:

Well, it happened that when Hollerith was in its infancy, when the Hollerith Machine Company was in its infancy, they took it around to the Department of the Census of the United States, and the Department of Census said we have no need of that and they were out of funds. But the Imperial Russian government of the Czar did use it in their own census, and so they always sold their machines to the Russian government and to no one else.

MERZBACH:

That must have been about the first two decades, right?

ATANASOFF:

Yes, yes. It's an interesting sidelight, you see, on this. There is this element of the history, I can't remember the exact details, but I'm sure that element is correct, I've heard it from a number of places. Well now, why, you know IBM don't care what you do with their machines. You can hook it up with anybody's else's machines at all, but things were entirely different in those days.

MERZBACH:

That's right. Well, those parts that you had -- I mean, precisely, what did you add to the machine?

ATANASOFF:

Well, you know, electrical parts, relays and contact points and the like, which were not easy articles of commerce in those days as they later become. And I can give you, I guess my publication shows it, but I can give you almost exact minute details of this machine from my own memory. Nobody's shown that much interest in it. But –

MERZBACH:

I mean, just briefly, what was it that required you to modify the machine?

ATANASOFF:

You see, what actually happened, the machine received data -- [(digression concerning

tape recorder)] – What actually happened was: the frequencies from various lines of the spectrum were punched on cards, and the machine compared these cards in twos, and it took the difference in these frequencies in order to derive, by way of the Ritz combination principle -- if my memory suffices, and I'm pretty sure it does – the basic level from which these lines were derived. The analysis -- the machine -- this was normally done by hand, numerically, in those days, but by using this machine, much more data could be processed. And it was the problem of getting the machine to compare these cards in pairs to take the differences, to punch these differences on cards, and then to verify the existence of these levels by the getting coincidences in these differences in various combinations. That was –

MERZBACH:

The source of the problem.

ATANASOFF:

It was, you might say, it was a problem in this. It really is very elementary. You know, don't you, that an atomic spectra is derived from levels, that we have basic levels, atomic levels in there. That when a transition occurs between level A and B that we take the differences in energy levels of these two and then by the Rydberg constant -- at any rate, you know, the differences in energies is admitted in a frequency [nu], such that h[nu] equals the differences in the levels and, by using, and from this can directly be derived the Ritz combination principle that I spoke of a moment ago; namely, that if you take the differences in the frequencies of spectral lines it permits you to uncover the levels from which they originated. And the machine would do this process. We analyzed one or two spectra with the machine at that time. I myself was not active in the analysis of spectra, so that the machine didn't get used extensively. And I'm sure that such machine methods have been used since by others in studying spectra.

MERZBACH:

Well, did that particular experience tell you anything as far as the application of punched card machines is concerned?

ATANASOFF:

Oh, you know, you don't know where any idea that you may have comes from. It's pretty clear that ideas have to have soil to originate from and only a rich experience can give vent to ideas. And also you can add the theorem that there is nothing new under the sun, which is a direct consequence of this fact. And, well, of course, it serves, it's one of the items that serves to keep my interest concentrated on the whole subject of the computing field. I think it's interesting from that point of view.

END OF SIDE

ATANASOFF:

I had been interested in computing machines for a long time. But about this time, let's say about the year 1935, I commenced to focus my attention on the differences between analog and digital machines. Now I was interested in methods of solving practical, applied problems. One of the problems, of course, that is practical and applied and is useful even today is solution of Laplace equations. And I was, sometime during these years, I came up with the idea of an analog solution of Laplace's equation. And this was worked on by Mr. Lynn Hannum and later on by Glen Murphy and I. And we have a publication by Glen Murphy and myself on this subject, which pretty much outlines what was done in those days, and this was a direct application of an analog method to the solution of equations.

Now, also about this time I commenced to have a series of graduate students in mathematics who were working on the practical solution of problems. And details of this work can be obtained by locating the thesis of such people as Mr. Cook, Mr. Gross, Mr. Thorne, and perhaps one or two more people whose names escape me. Oh, Mr. Charles Wells, I do want to add to this list. Mr. Charles Wells was studying the possibility of getting a reasonable mathematical approximation to the ground state of the lithium atom. A lithium atom, you see, has three external electrons and the actual delineation of the wave functions is a disagreeable, hard subject. This commenced to bring back to us in full force the problem.

MERZBACH:

When was this?

ATANASOFF: I'm going to have to do reference in order to get these dates. You see, I suspect that Charles Wells was the earlier one of these. I don't know why I don't have these theses right in front of me, I might be able to pick 'em out here, but I don't believe they're in there. I don't know why, but I just don't think I have it here.

MERZBACH:

Well, we can supply that.

ATANASOFF: You can, but it's just messy as far as I'm concerned. ... I wish I had -- what do we have on Charles Wells' thesis? [To Mrs. A.:] Can you find me a reprint of the one on Charles Wells? That would be useful. Now Mr. Cook, I remember, was working on approximate methods for solving partial differential equations. Mr. Thorne applied such methods to a plate, and Mr. Gross went into more detail on the theory of methods of solving systems of equations. I ought to have this whole matter ordered, and I'm a little confused as to what the order was in those days. But these men commenced to focus my attention on the problems of solving partial differential equations. Now since these partial

differential equations were, in the main, linear, they were, of course, linear operational equations, and they were part of a sequence which we recognize as being -- part of a system which we recognize as being much larger than just a partial differential equation. We knew that in the same, in linear operational equations, there're also such things as integral equations. And we knew that they embraced the whole field of linear operations in the sense of Banach, and we knew that when any attempt was made to solve such systems, they gave vent to systems of linear equations and, as the problems became more complicated, these linear systems became larger and larger. They could be larger because we wanted more independent variables, or they could be larger because we wanted a better approximation; in either case the linear systems grew. And so we commenced to focus our attentions on practical methods of solving linear systems of algebraic equations. Now what these students did, perhaps culminating in the work of Gross, was to develop what we regard as the most general approaches to the methods -- and I believe they still remain this -- the most general approaches to the –

ALICE ATANASOFF:

Do you need another one?

ATANASOFF:

No, this is all right. Here's -- this probably was done in '35, it was accepted for publication in '37, so that would be -- oh yes, well -- oh, the thesis was submitted in '35, it says that the thesis was submitted in '35.

MERZBACH:

This was probably the earliest one, right? [?] in 1939 and, I guess, the other one probably—

ATANASOFF:

Yes, that's reasonable, I think, yes. Now, from the mathematical point of view, I think it's important to examine the work which we did in those days. We developed the idea of biorthogonal expansions. The idea of biorthonormality was not original with us. But the exact functionAL system, functionAL system, A-L, not functional but functional, at any rate in French you'd spell it E-L-L-E, and in German you'd say Funktionenfunktion, and this approach to the derivation of biorthogonality and its specification, I believe, it was original with my students. A number of ideas eventuated from this, one of which was a relationship between the expansion of Taylor and the expansion in terms of orthogonal systems of expansion. The expansion of Taylor became another illustration of biorthonormality. Do you recognize this, is this generally known today?

MERZBACH:

No, no.

ATANASOFF:

I don't believe it's generally known even today, it's a strange thing. And, you know, I got sidetracked into other things and we didn't go too far with it, but it's contained in these theses. I have in my desk a paper which I might have copied for you, I don't know, maybe you'd like to read it, why don't I have it copied for you, which I gave to a meeting of the American Mathematical Society in Columbia, Missouri, in '35, '36; and it may be in here. ... You know, I've got to try to perfect this, and there are things, important events which are not in here. I can get the dates when I was down there easily enough, now where is this, where is this article? Alice can find it for me. I found it in my original file when I was looking for the files on this computing machine and pulled it.

Alice, do you remember that article? I've got an article here somewhere on the relationship between Fourier series and orthonormality--where would that paper be? I have it here near me in my desk and I don't know how to lay my hands on it.

MERZBACH:

Anyway, you gave it at the Columbia meeting?

ATANASOFF:

At a Columbia meeting of the American Mathematical Society.

MERZBACH:

I can get that date.

ATANASOFF:

Well, you can, but it wasn't published, so many papers weren't in those days. And I don't know the date. I remember distinctly, I was well pleased with this paper and Professor Chittenden, Professor of Mathematics at the University of Iowa, who was certainly the best mathematician in Iowa at that time, told me it was the best paper given at the conference. I felt very highly honored because of this remark of Dr. Chittenden. Now let's see, I'll find this for you, and I'll give you a copy of that paper. I have it the paper. Now this paper showed that--it worked out details of this relationship, so that one came to a uniform attitude towards the whole theory of approximation in terms of series, a subject that I told you I had begun a long time before. And it also demonstrated that, as a matter of fact, not only were Taylor expansions a special case of expansion in biorthonormal functions, but also, all biorthonormal developments were a special case of the Taylor theory. And the title of the paper was, "A Generalized" -- now it comes to me -- and the title of the paper was "A Generalized Taylor Expansion." And there was a simple

generalization of the Taylor concept which permitted all orthonormal developments to be brought into the Taylor form. And not only could they be brought into the Taylor form, but the error term in the expansion also took on one of the conventional forms given for a Taylor series. You were always searching for the approaches to the problem of how you could estimate the possible error of an expansion. Now you may not know, but the estimating the error of an expansion which is orthonormal is not so easy, but estimating the error in expansion of a Taylor series had certain stabilized and well-known forms. And, one of these forms, and I remember the reference there, the reference was Advanced Calculus, by E. B. Wilson. Is there such a book?

MERZBACH:

Yes.

ATANASOFF:

This particular form that was used is contained in E. B. Wilson. I remember that. Funny thing, some things come to you that are useless and not so direct. But I can show you that in a moment. I think I have E. B. Wilson here, and I can probably pick that out.

MERZBACH:

Mhm.

ATANASOFF:

I've owned that book since that day. But it's in here somewhere. We could find it, but I don't think it's worthwhile; I think I've said enough. I don't want to say too much, but I don't want to say too little. Because I think that what actually happened to me was quite largely controlled by these mathematical meanderings of my mind with the relationship, and I gradually saw that, and I have no reason to doubt it in the slightest sense, I gradually saw that this numerical approach to the problems of solving partial differential equations, and, in general, in more general approximating the solutions of linear operational problems, and, in a larger sense, all operational problems, that this numerical approach represented the only way out for mankind. I just make that general utterance, and then we can go on. Now as a result of this, my students were coming up with these large linear systems. And I commenced to beat myself around the head and tried to find ways to get at the solution. Since I had done this work with Hannum, you remember, on analog methods, my first inclination was to attempt analog methods for this.

MERZBACH:

When did you start doing the work that eventually resulted in the—

ATANASOFF:

I'm sure I was going on it by '34.

MERZBACH:

Aha.

ATANASOFF:

And by the time I got into this work with -- by Wells. Now, Wells is a member of the staff of the University, of Michigan State at Lansing and he has been there for many years and is a senior, maybe retired, or nearly retired by this time. Bringing Wells along, it was a complicated job for both of us. I note, read here that something about our efforts, and this stuff all looks so familiar.

MERZBACH:

During this phase, while you were considering analog development, did you pay any attention to the machines that were being built for solving systems of equations, such as the Wilbur Machine, for example? The mechanical—

ATANASOFF:

Wilbur?

MERZBACH:

Yes, W-I-L-B-U-R.

ATANASOFF:

I don't seem to remember the Wilbur machine but, of course, I was well acquainted with the Vannevar Bush effort, the differential analyzer.

MERZBACH:

Did you consider building some form of the differential analyzer at any time?

ATANASOFF:

Well, you see, the differential analyzers never had much sufficient power to operate in the field of partial differential equations. And almost all my interest was in power. And that just simply meant that the limitation of the Bush machine was very severe.

MERZBACH:

Were you at all aware of the efforts of Hartree, you know, to apply them to partial differential equations?

ATANASOFF:

Yes, yes, this I knew about.

MERZBACH:

Ah.

ATANASOFF:

Hartree. Yes, now you know, I was reading papers by Hartree all the time. And he and I were more or less, in a way, working in the same field, you see. And yes, I knew of Hartree's efforts. And now I can't remember the details of his efforts at the moment, so I don't know just exactly how to relate it to my machines -- to my thoughts and my mental gyrations, shall we say that first. Now, I didn't get very far with analog methods, I saw that -- you know, do you know what the principal of floating decimal is?

MERZBACH:

Umhuh.

ATANASOFF:

Well, you see the floating decimal always almost get you into trouble. You just don't ever have enough accuracy if you're attempting to work in analog forms. You'll be trying to solve a triangle and the triangle will not be a strong triangle. And you have to have a surplus of accuracy in order to work these things out and know where you stand, you have to have a surplus of energies. Let's stop a minute. I need a break.

[Recorder off]

ATANASOFF:

[Digression on loose part of tape recorder].

During these years I was interested in the theory of approximation, because I felt as if a properly handled theory of approximation would permit us to commence to approach the solutions. I might make this general remark, you see, it is just fortuitous if the differential equation has a simple answer, because if one understands the field of all differential operators, it transcends to a high degree the fields of all elementary functions which people customarily use to express the answers to differential equations. So the answer to

differential equations must be much more complicated than those simple forms which we use. And the only methods of representing functions which are of sufficient strength to permit you to handle this much enlarged problem, is, of course, these infinite processes, such as expansions. So expansions, or put otherwise, the theory of approximation, is the only possible approach to the solution of differential equations. Now, yes, and I think that probably, I will take pains to copy and send you the paper which I gave at the Mathematical Society in Columbia, Missouri on a subject which is closely related to the work of my students and the title of which is, "A Generalized Taylor Expansion." This is a generalization of the methods of Taylor, which I have never seen anywhere else. But this generalization has such breadth of scope that it comprehends wide possibilities in the expansion of functions. As a matter of fact, formally at least, it comprehends all orthonormal expansion of functions, which, of course, is a great scope. Perhaps we'll quit here. I wanted to get that much in.

[Recorder off]

ATANASOFF:

Do you want to remind me where we were?

MERZBACH:

Yes, we had talked about your early explorations of computing machinery. You had mentioned your analog phase –

ATANASOFF:

Analog approach to the—

MERZBACH:

Analog approach. We had, before that, talked about the punched card project, the problem on atomic spectra. And I think we were ready to -- oh, I had one other question that I wanted to ask in relation to analog devices. You know, that machine by Mallock, did you ever look at that at all or know anything about it?

ATANASOFF:

Where was it, I don't know?

MERZBACH:

That also was up in Massachusetts; MIT.

ATANASOFF: Mallard?

MERZBACH:

Mallock, M-a-l-l-o-c-k.

ATANASOFF:

I can't even remember, apparently. I thought, I don't want to pass by the differential analyzer too quickly. As a matter of fact, I studied it pretty carefully and saw what its possibilities were. I, almost at once came to realize that the problems that I'd experienced with limitations and accuracy -- When I say, "I'd experienced," this is mostly a mental experience, not an actual, physical experience with an actual machine, but I realized that the limitations were very close. And then my study of the Bush differential analyzer showed me that the limits of accuracy were very great there, were very sharp there. And this is -- now: The other thing that I mentioned, which I want to emphasize again, was that, while I believe that I have seen some effort to use the Bush differential analyzer in a solution of a partial differential equations, it is very hard to see just how very much progress could be made in this direction. I followed those attempts,

MERZBACH:

Yes.

ATANASOFF:

but they didn't look very likely to succeed to me. I also want to say, in passing, that, at a later time, I conceived of a possibility of making the Bush Differential Analyzer a digital device. And I have some records in my papers of my having such thoughts, and I remember them well. This was an effort to overcome one of the shortcomings of the Bush Differential Analyzer. We would have, instead of mechanizing the Bush differential analyzer with such devices as ball and disc, or wheel and disc integrators, we would be mechanizing it with digital computers, which would have computed with arbitrary accuracy, any of these matters that you wish. And one would attempt to put together a machine that -- with interconnections which were similar in general structure to the -- ah, well, I'm inclined to call it the meccano toy -- do you know what a meccano toy is?

MERZBACH:

Yes.

ATANASOFF:

A meccano toy computing machine which represented the Bush differential analyzer.

MERZBACH:

Right.

ATANASOFF:

In other words, if you wanted to change structure radically, you had to change the machine. But you would do this, but the parts would be composed not of analog elements, but of digital elements throughout. There is some possibility of progress in this direction. This would have satisfied the accuracy criticism of the Bush differential analyzer, but it would not have satisfied the spread into many dimensions which, of course, I was to find the inspiration for progress in this direction in the work of Banach, and Frechet and Hilbert.

MERZBACH:

Roughly at what period did you contemplate this notion of a digital differential analyzer?

ATANASOFF:

This was later. It was in the period -- it was about 1939 to '41, in that interval. I don't think that I ever put too much time -- well, I know I didn't put very much time on it. I don't think that I ever attached too much importance to it. But Mauchly was interested in building a differential analyzer. And, of course, in the conversation with him, this is one of the things that eventuated from that.

MERZBACH:

Well, maybe we should go back first now and see how –

ATANASOFF:

Now, you know, that I had explored the possibility -- It seemed to me that a man could commence, in view of the theoretical approaches, which I will not describe here, but which are covered pretty well, let's say, in Gross's theses, two theses by George Gross, one a master's and one a doctor's, and which represent Gross's and my thoughts on these subjects. And also the paper of which I'm going to give you a copy of, called "Generalized Taylor Expansion," illustrating the kind of thinking that we were doing. It seemed to me that if we could just have a machine that would solve, that would furnish solutions of linear algebraic equations of arbitrarily high order, large systems of equations, why, that we could make real progress. And this whole thing became an obsession with me at this time. Now this was occurring in the year 1936 and '37.

MERZBACH:

Let me interrupt just one more time.

ATANASOFF:

Surely.

MERZBACH:

Somewhere in that period -- I think I saw a reference in your deposition that you had also contemplated the possibility of linking, you know, regular calculating machines together? Could you—

ATANASOFF:

All right. Yes, well, this immediately follows this.

MERZBACH:

Aha.

ATANASOFF:

Now, there were several mental attempts along this line. One of them was the possibility of just taking a whole lot of computers of the character of, let's say, Monroe tabulators, and hooking them together – I'm sorry, Monroe computing machines -- and hooking them together, so that they're all driven from a common shaft, they're all shifted by a common mechanism. Now it's easy enough to see that if you do this, that you will have a system which will enable you to solve linear systems of algebraic equations. Because you could put the coefficients of one equation on the dials of the machine and put the coefficients of another equation and the constant term on the keyboard of the various machines--one machine for each coefficient. And if you wanted to solve 30 by 30 equations, you would use 31 computing machines, in order to put the constant term on one. Then, you can certainly, every time you operate the machine no matter where you have shifted to, why, you get a new equation which corresponds -- which passes through the point of intersection in space, in hyperspace, of the two original equations, and you can so manipulate the combinations that the coefficient of one is lost. This is a method of solving linear systems which is as old as the hills and which was known to -- I don't know who first invented it, but it was known for hundreds of years, at any rate.

Now, in the first place, there was no large supply of thirty computing machines. In the second place, it occurred to me that even the computing machines that we had were rather limited -- that were available in those days -- were rather limited in the number of places that they had available. And I knew that I would be in continual trouble, because the coefficients would be running off one or the other of these machines as I did this job, running back and forth all over this long line of computing machines and watching them with my eye. But even more important was the problem of getting numbers into and out

of these machines, data in and data out was absolutely -- would be entirely manual, and would be very difficult. Such a system would have been a good beginning on the solution. It would have done much better than anything we had at the time.

MERZBACH:

Were you aware of anybody else trying to work with a group of machines like that?

ATANASOFF:

No, I have no such knowledge. Now, there was another attempt, which is in my records, which you know about. And that was the attempt to use IBM tabulators for the solutions of systems. Well, I have some notes on this subject and you and I could try to work out these notes and see what they mean. But I do remember very clearly. Of course, this was following the precept of my operations with A. E. Brandt on analyzing complex spectra, was again the same process of sneaking it on a computing machine and plugging something in it and making the machine do something it didn't want to do. But, be that as it may, I didn't go very far because of the very limited capacity, we'd have only 80 significant figures, total for all fields--base 10 digits--which should be 240 bits, wouldn't it, roughly. And this is just impossible to encompass, just didn't give you sufficient power to encompass any problem that interested me much. And I saw that even if I succeeded completely, this was the largest IBM tabulator made, one that we didn't have at hand, we only had a 40-place tabulator at hand at Ames at the time. And even the largest one, which -- there's only 80 significant figures on the card, and that's all you can have on the tabulator, you see, so you were just stuck from the beginning in capacity. Data in and data out looked better, because there were ways of punching data in and putting it in from cards, and so that was more favorable. Now those were two of my early attempts at digital solution of linear algebraic systems. Now I should remind you that I also had that analog solution that I flirted with for some time. And this was based upon the so-called setup boards which were used by electrical engineers -- you've heard me say this before, haven't you? -- for solving systems of transmission lines which could be turned to this purpose. But that had its obvious limitations of accuracy.

MERZBACH:

Did you do some -- did you fool around with these boards at all?

ATANASOFF:

Not at all, no, no, ratiocination and armchair experimentation is all I used; or all I wanted, may I add further! Because it is the cheapest and fastest form of experimentation. It's cheap for obvious reasons, but it also is all-encompassing, and you don't have to wait while some piece is manufactured in a machine shop.

MERZBACH:

Mhm. It's true.

ATANASOFF:

And there are lots of things where you can't decide it by armchair experimentation, but if you can it's overwhelmingly the experimental method of excellence. Is that down pat? I still feel the same way. Eh, now -- [chuckle] I guess I should tell you that this became sort of an obsession with me. You'd know competition is hard in a university, and the life of a university pres -- professor is not easy. And the question of progress in research, I had by this time, by the year of 1935, had almost entirely left the undergraudate teaching, so I was given no credit for undergraudate teaching. I was teaching, perhaps, three graduate courses, three graduate courses, say, perhaps 6 to 8 hours a week. And then I had graduate students who were writing theses. And then, well, I obviously was going to get stars in my crown by the amount of research I turned out, and there were sharp men around me who were critical about such things, and they didn't give me trouble, but I felt that I was concerned about this. This, also my own desires, all mixed into this potpourri of things which drove me, and I was driven extremely hard. Perhaps if you would just look over--I think it's pretty well written. At this point, you can look over my testimony in regard to where at night, in the fall of 1937 or the spring of 1938, when I got the concept -- I ought to preface that a little: I knew that I had to have new computing elements, and I commenced working diligently on this and the base two idea occurred to me and it was all in my head. And by the use of base two numbers in place of base ten numbers in computing machines and the advantages of it and how much easier it was to build instead of having discs which had 10 states, I would have a little mechanical element which had two states. And I also, of course, thought about the possibility of this being a magnetic element. And other aspects of this are recorded in my writings and I don't need to give the details here. But these things were going through my head. And then came this night ... when I returned to the college after an early dinner, and it just seemed the world was all upon me. And it ended up with my driving 189 miles and setting down in a honky-tonk over in Illinois. And then as I left that honky-tonk some hours later, I realized how progress could be made and I came back and started in serious earnest to try to move in this direction. Now is this sufficiently detailed?

MERZBACH:

Yes, because we –

ATANASOFF:

We have all the other material.

MERZBACH:

We have a good account of that.

ATANASOFF:

Yes, we have -- you have quite a lot of -- I might also add for completeness here that, of course, I was aware of the great advantages of doing this, of pursuing this subject in terms of electronics. And it became, long before I went to the honky-tonk, I understood that if I were to build electronic digital machines, that electronic machines would be so fast that they wouldn't have to be at all complicated in order to encompass a great deal of computation. Now I think that –

MERZBACH:

Eh, before we get into the, now, the actual development of your machine, in that same year, in '37, you made some approaches to IBM; could you expand on those a little?

ATANASOFF:

Yes, you see all the time I was interested in computing machines, and I wasn't sure that I wanted to remain -- oh, you know, everything passes through my head all the time, and when you get into looking at a man's innermost efforts, it makes him look -- well, I was really pretty happy, it's not that so much. I just was never happy. If you understand the difference: I was pretty happy, but I was never happy. I always had ants in my pants, as it were. And so I considered -- I realized that this was a great field and an important one -- that maybe I should switch to this. And I made some approaches to IBM and also to Remington Rand, which is a predecessor of Sperry Rand, you know. And because they were the two companies building tabulators and the tabulator market -- well it had some elements that I wanted and I didn't want -- you have the records on this and the records speak for themselves quite clearly. And, I know, I finally drove some executive -- and I can't find this letter -- and I know I drove him to the point where he told me that IBM would never use an electronic computing machine. I had such a letter,

MERZBACH:

Aha?

ATANASOFF:

and I'm afraid I took it out to show somebody, and it didn't get back in my files, and I don't know where it is.

MERZBACH:

Now also there's something that puzzles me. That same year you did a publication with someone on measuring viscosity of eggs with a torsion pendulum. Now is there any relationship [laughing] between that and anything we've been talking about?

ATANASOFF:

No. [Chuckling].

MERZBACH:

I didn't think so.

ATANASOFF:

You know these, again, these were great years of my life, weren't they? I was just stirring up everything in all directions.

MERZBACH:

How did you get involved in that particular project?

ATANASOFF:

You understand I've always had catholic tastes

MERZBACH:

Yes.

ATANASOFF:

in science. [laughing]. Let's see. [Still laughing]. Well, this is something. Well, let's see. It turned out that they had a rather aggressive young man who was head of the poultry husbandry department in Ames, by the name of Wilkey, H. L. Wilkey, Henry Wilkey was interested in eggs and making all kind of measurements on them. And he said, "we have a feeling that the viscosity of eggs is important, but," he said," we can't do enough tests because we have to destroy the egg in order to test it." And so then I told him how to do it and so we published that paper; that's what happened there. And, of course, that took me several weeks of work. And what was the date on that?

MERZBACH:

Well, it was published in '37.

ATANASOFF:

OK, so you see it came right in the middle of all this other effort.

MERZBACH:

Right. Well, that's why I—

ATANASOFF:

Right in the middle, just like everything else, everything's happening or nothing. And did you know that somebody has been interested in that paper here lately?

MERZBACH:

Really?

ATANASOFF:

Yes. I don't know who, I'll have to stop and remember, but --.

MERZBACH:

Now also at this time, and I guess this would still be before your trip to the honky-tonk, were you thinking about patentable notions on computing machinery? I'm merely led by this reference here.

ATANASOFF:

Yes. You know, you see, during the summer of 1937 I was working on these base two numbers and the abaci involving base two numbers, namely the counters involving base two numbers. And I also had the general idea that electronic circuits could suffice to do calculations if properly manipulated, although nobody in the world at that moment knew how. And I realized that patent -- that I ought to consider it. And you know, you have, the first place you're looking for an outlet of ideas. Now, ordinarily, in university research you don't have any trouble -- you just publish it and that's the end of it. But it was clear to me that this wasn't the .. way to handle this. And so I wanted to disclose these ideas to these computing machine companies in the hope of getting a return, but I didn't want to do it with complete disregard for patent values.

MERZBACH:

That did lead you to communicate with some patent attorneys

ATANASOFF:

How's that?

MERZBACH:

I say, that then led you to communicate with some patent attorneys about these questions in 1937, right?

ATANASOFF:

That's right, yes it did.

MERZBACH:

Ok.

ATANASOFF:

Now here we have the George Gross, ... these things were drawn from George Gross's letter. I asked George Gross to try to remember, in order to complete my own records, I asked him to try to remember what he could remember of this, and he didn't put it together, and I believe that these items were specifically drawn by Charles Call, George Gross's correspondence. Charles Call did a lot of this, this is a compositive of the work of two people, J. V. Atanasoff and Charles Call.

MERZBACH:

Yes, I realize, this is the reference to your discussions with students in 1938 about base two and complements

ATANASOFF:

But you see the timing

MERZBACH:

and so on. Hm?

ATANASOFF:

You see the timing is a little bit different than I remember it. This was done in 1939, I'm still monkeying with analog devices.

MERZBACH:

That's the Laplaciometer.

ATANASOFF:

Yes, the Laplaciometer. That's an analog device.

MERZBACH:

Mhm.

ATANASOFF:

You find these things a little bit fuzzy, my memory is a little bit fuzzy about the relative.. But, you see, after visiting the honky-tonk, then -- either later 1937 or early '38 -- then I had a period of development. And all this time I was developing my ideas and trying to get 'em in shape so that I could do something with them. And then it turned out that it wasn't until March 24, 1939 that I actually went to the Research Foundation, the Research Council, for money in order to move ahead, because I had to have help. I just knew that I couldn't make any effective progress just by myself at that stage. And you didn't dare go there, this man Lindstrom, and Lindstrom was, I believe, chairman. Lindstom and Buchanan were the two men there, they were the two men; and they were very sharp men, both of them. And you just simply wanted to have a good, clear-cut case and be able to know what you wanted to do before you fooled around with them. So we have a year or so where things were in a formative stage and where I'm working on it and trying to plan how the machine would go together before I actually went to Lindstrom in regard to this matter.

MERZBACH:

Now you had been working on it actively then for over a year, right?

ATANASOFF:

Before what?

MERZBACH:

Before you went to the Research Foundation.

ATANASOFF:

Oh yes, more than a year; two years, at least. At least, two years.

MERZBACH:

Now, when you say working actively, this means -- when did you start construction of this prototype that was finished in the year '39?

ATANASOFF:

That was started in the fall of '39, wasn't it?

MERZBACH:

Well, I thought that I saw somewhere, I recall, that it was completed by the end of '39; no?

ATANASOFF:

Yes, it was, but it wasn't started until the fall.

MERZBACH:

I see; I see.

ATANASOFF:

But, you see, all this work that I'd been doing meanwhile has paid off. Because I was very sure of exactly what I wanted to do at that stage. We started it in September and completed it by the end of the year.

MERZBACH:

So, in other words, up to 1939 you had been setting—

ATANASOFF:

Thinking.

MERZBACH:

thinking and setting things on paper. Not on, on the golf course.

ATANASOFF:

Right; and by myself. And September of '39 I had Clifford Berry, and by Christmastime the computing elements were working. Now, that's the timetable. And you see, really, that --more than two years I'd been working on it more like three years, before I actually had Clifford Berry there. Oh well, in a larger sense even longer than that. But quite exactly on this computing concept, for at least two and a half years before Clifford Berry entered the picture; but it paid off well, because we almost knew exactly how we were going at it, and the thing, we were able to put it together and make it work by the end of the year. I mean, and actually compute. It actually did computation by the end of the year. And Sam Legvold will swear to that. He remembers it. He remembers seeing it. And

that's one other reason that you should talk to Sam Legvold; he'll give you a little feeling for the, oh, you know, in little ways, I'm not attempting to be inaccurate in the slightest, and I don't have any high fallutin' ideas about human effort, but Sam Legvold will give you a little feeling for the times. He's a very mundane, down to earth sort of guy and you'll like Sammy and you'll put in a nice period with him.

MERZBACH:

Good. Well. ... Let's cut it off.

ATANASOFF:

All right.

END OF TAPE