# Computer Oral History Collection, 1969-1973, 1977

**Interviewee:** John V. Atanasoff (1903-1995)
**Interviewer:** Henry Tropp
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TROPP:

This is a continuation of my discussion with Dr. J. V. Atanasoff. The date today is May 11, l972, and, as is usual in our meetings in Dr. Atanasoff's home, we're sitting in his bedroom study.

During our discussion a week ago, Monday, on May lst, we were talking about the paper on "Generalized Taylor Expansions" that you gave at the mathematics meeting in Columbia, Missouri: we had also talked about the prototype which was completed, roughly, in Christmas of l939, and the work that you and Clifford Berry had done from about mid-September to the turn of the year, at the beginning of l940. And you mentioned that there were some other things that you wanted to discuss concerning the paper on generalized Taylor expansions, the paper dealing with the operators on a set of functions.

ATANASOFF:

Well, I had--I was dumbfounded to find how simple an explanation the whole affair was, this long after the fact, and how readily you grasped the central ideas of this concept. What I had done back in l939--I was impressed with the fact that the remainder formula for Taylor's expansions were substantially simpler than those for other types of expansions. Not that such remainder theorems did not exist, but I was struck with the simplicity of the formulas for a remainder in the Taylor expansion. I commenced to try to investigate what the inner essence of the Taylor expansion was that permitted this simplicity of remainder formulas. I found out that I could describe the essential features of the Taylor expansion in quite a different way than Taylor had used, in terms of operators, an operator "D," an Operator "I", and a functional "F", and a function "phi" in the Taylor expansions

TROPP:

Now what—

ATANASOFF:

the function "phi" was one. The functional was the evaluation of a function at a fixed point a. In Taylor's expansion the Operator "D" was the derivate, and the Operator "I" was the integral from a to x--the inverse integral from--no, the inverse differentiation--the integral from a to x, or whatever you're talking about that was being operated on. Well, I derived some relationships involving dI, D and F, and these--and then I said, "we will define a generalized Taylor expansion as an expansion which satisfies these same algebraic relations." And we thus came to a set of--to a set of expansions which were much more general than Taylor's expansions. These algebraic relationships between "D", "I", "F" and "phi" were sufficient to permit the derivation of the simple remainder. It was quite general. I, later, proved that, in quite a general sense, all expansions are generalized Taylor expansions by proving how to derive "D", "I", "F", and "phi" for any expansion, whatever. So that the generalized Taylor expansions came--bound, came to describe almost all expansions. I may--I gave this paper at Columbia, Missouri--what was the date?

TROPP:

Uh, December--early December l939.

ATANASOFF:

In December of l939, I remember I went down at Columbia, Missouri and stayed overnight with my friend Robert Vaile, who is still a friend of mine, living in California now.

TROPP:

That's V-A-I-L-E?

ATANASOFF:

Robert Vaile, V-A-I-L-E, yes. And I remember one more instance, and Bob has always been strong with liquid refreshments, and that evening he plied me so vigorously with liquid refreshments

TROPP:

Haha.

ATANASOFF:

that I could scarcely stagger out to go over and give the paper. I managed, somehow, to get through the paper, and was well pleased with the comments which followed.

TROPP:

One of my questions relating to that paper that I neglected to raise when we were discussing it last--a week ago, was: When you look at these differential and integral operators, which you call capital "D", and capital "I," as you wrote them near the end of the paper in matrix form with elements along the diagonal or just above the diagonal, could you conceive at that point in time of being able to find this remainder term, using a mechanical or an electronic device to perform these operations for you?

ATANASOFF:

Well, I had—

TROPP:

Since you were just dealing with zeros and ones—

ATANASOFF:

You're really drilling—

TROPP:

and the delta function—

ATANASOFF:

You're really dril--you're really dealing with a dream, you see. I didn't go ahead and formulate practical methods for this approach. I had in mind that it would be possible, just a far-reaching dream about the possibility. I--in discussing a remainder--a remainder formula with Professor Sendov [?] up from the University of Sofia, I suggested this approach, and he said he would--that it was new to him and he would have a student work on it. I don't know whether this has come about or not, I haven't heard from him on this subject.

TROPP:

I haven't had a chance to check the literature in, you know, a similar vein as I told you, I need to find people who work with functional operators to see roughly what the state of the art was at that time, and in terms of series expansion approximations, which is an area of serious investigation now, and has been for quite some time--where it stands at this point.

ATANASOFF:

I might just add that this paper is a little bit out of line with my computing machine work. I mention it here because it showed the kind of thinking we were doing. Mr. George Gross was attempting to derive the most general linear methods for reducing a differential equation an operator in infinite algebra to assist him with linear equations, so that it could be approached for--as an approximation method, and in this formulation of operational equations into algebraic equations, of course, if--the concept of the functional is foremost and rather epitomizes the whole art. These methods include those in the historic ones of Rayleigh and Ritz. And the--also the one that goes under the name of Trefftz, T-R-E-F-F-T-Z, or something like that, in which—

TROPP:

It's not one that I'm familiar with.

ATANASOFF:

You don't know that one. This is the one in which the--we have solutions of the differential equation, but we approximate on the boundary. Instead of having solutions which satisfy the boundary conditions but approximating in the differential equations, these are both obvious elements of our generalized algebra of reductions from linear operational equations to linear algebraic equations.

TROPP:

I guess I know of the method, but just the name was not familiar to me.

ATANASOFF:

Of course, there's some--some limitations have to be applied, because, of course, the infinities involved in the linear operational equations of very high order, while those involved in linear algebraic equations must have something to do with being countable—

TROPP:

All right, then you're worried about the poles which you run into as you approximate on the boundary?

ATANASOFF:

Oh yes. You've got singularity problems all over the map, and many of these problems--and they exist to this very day, and as far as I know they're not completely encompassed and never probably will be.

TROPP:

Let me—

ATANASOFF:

And then all of these--and all of these methods, and I'd restricted myself to linear problems, I was well aware of the existence of non-linear problems right aside of this, but we really didn't, [we did] almost no work at all with non-linear problems.

TROPP:

Let me back off to the prototype with an item here that I noticed. We looked at a picture today of the logic element, and I noticed in looking at the chronology that you talked about working up the details of a five dual triode, and a three pentode prototype, and I wondered if you would like to explain this in some detail, as to how this operated, and how you used it in the prototype, and what the principle of it was.

ATANASOFF:

Now this--I'd like to go back just a little bit to a trip to the bar in Illinois, and even before that. You remember that I mentioned that, of course, the nearest thing that we had to an electronic computer was the scale-of-two counter, which would add one successively as long as you wanted it to, and it was perfectly efficient as far as this operation was concerned, and most people thought that this would be the logical approach to electronic--uh, this isn't quite right. I, myself, thought that this would be a logical approach to the electronics of a computing machine. I started playing around with the scale-of-two counters, and in my hands they didn't work very well. I didn't work on it very long, and I--it was one of the things that my failure in connection with scale-of-two counters, not particularly failure, but my failure to visualize how the scale-of-two counter could be used in a more general computing machine was part of the burden which I carried to the honky-tonk in Illinois. And what happened that night was that I'd by-passed the scale-of-two counter--counting circuits as an approach to the system. I had a--I'd devised a black box which didn't work like a scale of-two counter. It worked differently than that. The black box had, at each instance, the components which entered in from the quantities being added and subtracted, or whatever, and it furnished--it furnished at each digital position, it took cognizance of the two numbers being added or subtracted, and of any carry into that column, and it issued two output commands; one to be the sum or difference, as the case may be in that column of the scale-of-two, and the other one to be the carry into the next column.

Now, notice here that this represents a departure, a sharp departure from previous--from my previous thinking, and it is one of the great reasons why I consider this trip to the bar to be the instant of my best--my best advance in the comprehension of the computing machine. That and also, at this moment came to me the concept, during this trip came to me the concept of regenerative memory and the concept of sequential operations in numbers, so that the high speed of the electronic digital systems could be properly used to--in computing without unduly complicating the circuits.

TROPP:

Well, let's take a look at this single unit--these five dual triode tubes, and the unit itself as it operated. Can you explain, verbally--I know that this would be much easier to do with diagrams and, you know, drawing circuit diagrams and sketches; but can you, verbally, describe this particular unit?

ATANASOFF:

May I just say--may I begin by just saying, again, remember what I conceived of at the honky-tonk was a black box. I didn't know what the inner guts were going to be, but it had three inputs; one from the upper number, one from the lower number, and one from the carry-over into that place. And it had two outputs; one the answer in that column, and the other one for the carry-over into the next place which had to be retained in the memory until we were operating in the next place and put in--back into the black box itself...

TROPP:

These were the reason, then, for having five dual triode tubes, or—

ATANASOFF:

Well, now when you commence--I just started with this definition, and remember that somewhat over a year elapsed before we actively got to work on this computing machine. And during that time one of the things I did was to work--was to just, by methods of main strength and awkwardness I, knowing the operation of--being fairly skilled, I would say, in the electronics of the day, I devised ways to use vacuum tubes to make them furnish the inner workings of the black box, which is described as above. Now, it's hard to give details of this in any--much of any more detail in a useful way without the use of diagrams, and it's very, very hard to completely prove the logic--I mean to say, at first glance, unless you used some specialized techniques which are well known today, and which everybody does, and without thinking. But I started this in that day without knowledge of Boolean algebras. I knew Boolean algebra, and if I worked on the logic of how these systems would work by main strength and awkwardness, as I called it, I realized that there is an algebraic concept involved here. And I have in my files one piece of paper reminding me of my thoughts about this, but I never went on and identified this algebra as a Boolean algebra, and that was done much later by someone else, although I had vague talking acquaintance with the work of Boole at that time.

TROPP:

You're saying that essentially what you were doing was a logical circuit design

ATANASOFF:

Right

TROPP:

in terms of what we now call Boolean algebra, algebraic approach to—

ATANASOFF:

Yes, only I was doing it by just main strength and awkwardness.

TROPP:

Right.

ATANASOFF:

And if you attempt to go through, step by step, through these steps, you can spend a good deal of time, and I don't believe that any verbalization that I could give that process, here, would aid this discourse particularly, but my notes contain plenty of detail.

TROPP:

On this--I was going to say the document referred to in this paper is labeled Q-2, would that contain a detailed description of this?

ATANASOFF:

I--it would contain--I imagine it would contain a circuit diagram.

TROPP:

Well, I guess it's an addition unit is what you'd call it.

ATANASOFF:

Yes.

TROPP:

ASM are the initials you referred to here; what is ..

ATANASOFF:

ASM means Add-Subtract unit--Add-Subtract Mechanism.

TROPP:

Mechanism, umm hmm.

ATANASOFF:

Add-Subtract Mechanism was the name we gave to the logic element at that time. We did not call it a logic element.

TROPP:

This may be an irrelevant question, but on November 24th in '39 and again in December of '39 you indicate here that a--I would guess Professor--F. W. Bubb of Washington University in St. Louis has inquired about a Laplaciometer. In what context was his inquiry? Or, if it has no relevance for our discussion, then we can just skip it.

ATANASOFF:

Well, you remember that I have always had an interest in analog computers as well as in digital computers, and the Laplaciometer was a machine for solving Laplace's equations in two dimensions. And Mr. Bubb, I'd either discussed--I'd either given a paper on it at some mathematics meeting, or I'd discussed it with Mr. Bubb, and Mr. Bubb was interested in it for some of his own use. I don't know what more came of it.

TROPP:

Well then, back to the prototype, we had mentioned in our last discussion that the single element prototype was essentially operative before Christmas of l939. And what kinds of tests were you running on it that you would describe, that fit the description of operative?

ATANASOFF:

Fit the description of what?

TROPP:

Of being operative.

ATANASOFF:

Why, all this machine did was just add and subtract. And numbers had to be inserted in a very clumsy way and then, as addition or subtraction took place in a single cycle—

TROPP:

On .. one of these logic circuits as we would call them now.

ATANASOFF:

On the ... a logic circuit was involved, and, of course, the regenerative memory was involved, and the memory, the memories which were being used were essentially condensers,

TROPP:

Mhm.

ATANASOFF:

and the addition or subtraction took place--it took you longer to do it on this machine, because of the slowness with which entry was had, the input-output apparatus had not been worked on or devised at this stage—

TROPP:

Right. How were you clumsily putting numbers into this prototype?

ATANASOFF:

Well, one way was to just take a charged wire and touch the condensers, and then start the machine up. The condensers would last long enough to get the numbers in the machine by that simple procedure. Afterwards, Clifford Berry devised a method of putting the numbers successively on the different, on the different condensers while the machine was running. And I remember Clifford Berry's work, but to tell you the truth I don't remember exactly how it worked. Then we had to read--take the--to read the numbers which existed in the memory. We had to use a cathode ray oscilloscope, and—

TROPP:

Mhm, you mentioned that—

ATANASOFF:

and that way we pulled a number out and we could verify that the addition and subtraction were correct, and it took almost no time at all up until the bugs were worked out well enough so the machine was adding and subtracting correctly.

TROPP:

I'm going to push your memory probably fairly hard with my next query. The prototype you indicate as being successfully operated before Christmas of l939. But it's also clear that prior to Christmas of l939, perhaps by only a few weeks, you and Clifford Berry, together, had formulated your overall concept, in a sense the overall architecture, of

ATANASOFF:

[Cough]

TROPP:

what we can call relatives of the prototype of your big machine.

ATANASOFF:

That's true. As a matter of fact, I don't know when the first idea of the big machine came forth. You must remember that I was ever driving towards the solution of systems of equations, and this was the reason that I was building computing elements. I wasn't building them for general purpose computers at that moment. I was building them for the specific purpose of getting an equation-solver, so the equation solving part of it was part of this thing from the very beginning, and well before Christmas time, l939. I had commenced to trying to formulate in these terms--in the proper terms, the schemes which I'd previously envisaged, which are herein recorded for methods of solving systems of equations. Really, this was no invention of mine, it was just merely a mechanization of known algebraic methods for solving systems of equations in which elimination would occur. And I was going to build a machine that would contain the coefficients of two equations, and would eliminate between them and result in an equation of one less independent variable.

TROPP:

Now, it's clear that this prototype is not such a machine.

ATANASOFF:

The prototype was just pure computing.

TROPP:

Right, just merely a machine to add one digit or, or a set of numbers.

ATANASOFF:

Right.

TROPP:

But, it's also clear that you and Clifford Berry had, in a sense, thought out what the machine was going to be like while you were still testing the prototype, before it was totally operative.

ATANASOFF:

Well, we had. And we had advanced quite a way. We worked very hard on the logic circuits and trying to protect them so that they would be ready. And this process went on before the--December of l939, and immediately thereafter. And the--I believe that the prototype used the eight tube, five dual crowns and three pentodes--was that the correct enumeration?—

TROPP:

Mhm.

ATANASOFF:

type of logic circuit, but we knew that we needed to give this thing another reading and streamline the logic circuits. And we knew we needed a physical frame in which to put the parts, and we knew we had to have a lot more apparatus and so the problem was to collect money and the facilities for doing the job. And almost at once in the new year we--I'd made a guess as to how large the system would be, and I might have to back up, and I had a frame constructed--a steel frame constructed.

TROPP:

Which is the one I saw a picture of?

ATANASOFF:

That's right. It's contained--do we have a date on that picture? We have a picture of this frame with some of the units mounted in it, dated April 22nd, l940 and—

TROPP:

I notice, the shaft is there—

ATANASOFF:

And so the--yes.

TROPP:

And one of the, it looks like—

ATANASOFF:

Logic elements.

TROPP:

one of the logic elements in the back, and you've got your ..

ATANASOFF:

Driving mechanism.

TROPP:

driving mechanism off on one side.

ATANASOFF:

Now, this, by April—

TROPP:

In any case, the frame was built earlier.

ATANASOFF:

Sure. So the frame was built very early in the year, indeed.

TROPP:

Do you remember how long that shaft was?

ATANASOFF:

Oh, it was—

TROPP:

That would give us some idea as to the size—

ATANASOFF:

the length of the machine. I think the length of the machine was about--it was about 5 1/2 feet.

TROPP:

Um hmm. So that's about a five foot shaft, then.

ATANASOFF:

Yes. And you can tell from this date that this work couldn't have advanced so rapidly if we had not had a lot done before December of l939.

TROPP:

Now, just turn the page of the album, and the very next picture I came to was the memory drum, and a date on it of July 7, l940, which indicates its conception was also much earlier, because there it is completed.

ATANASOFF:

Yes, and as a matter of fact, you see, each of these things required a lot of labor, and most of this labor was new, there was no precedent for it. I know that when I commenced trying to fit the condensers into a compact drum with the components which were available in those days, I soon found out that to employ the standard paper condensers we would have a drum that was ungainly in size, and so I commenced trying to find ways to reduce the--reduce the size, and ended up by buying from the Johnny Fast [?] Company of Chicago, a manufacturer of paper condensers, the inner workings of the paper condensers. The other condenser manufacturer refused to sell me the inner workings. I have turn-down letters from a number of--I believe from Aerovox and Ova-lear [or Lehrer][?]. They said you--if you start fooling around with the inside of condensers it wouldn't work. However, Johnny Fast said, "you'll have no trouble, just do as we said," then they sold--they were stored with dryer--components were shipped to me stored with dryer, and they were inserted and potted in place. And so the drum was one component which we made up, but the inner workings were manufactured by Johnny Fast who were paper condensers.

TROPP:

Johnny Fast in, in what?

ATANASOFF:

And they were in Chicago, I believe.

TROPP:

Oh, in Chicago. Now, can we talk a little bit about the concept of the memory drum?

ATANASOFF:

Yes.

TROPP:

I think from a conceptual point of view what led you, and Clifford Berry, jointly, to this idea or to its ultimate design and function within the machine.

ATANASOFF:

Do you realize that as far back as the--as far back as the trip to Illinois, and even before, I commenced to realize that for my purposes a condenser memory would be highly advantageous, because a condenser memory would--could be charged from the plate of a vacuum tube and would, itself, serve to actuate the grid of a vacuum tube, so no amplification would be necessary. In other words, it worked at a level suitable for direct input and outputs from electronic circuits. And the only concern that I had about the condenser type memory was that it would--that the condensers would discharge. So, along the way, Clifford Berry and I were making tests of condensers, and we got time constants of the paper condensers of the day from one to several minutes. That is, it took one several minutes for the voltage on the condenser to drop to 1 over e or half or anything you please of the original value, if you adjust the order of magnitude statement, of course.

TROPP:

Now, in terms of the overall design of the machine, though, we now see these condensers in a drum, on a shaft, and, in terms, you know, of the timing, the rotation of the shaft, I guess, it's that concept that I'm trying to get you—

ATANASOFF:

Well, you understand that this concept was also contained in the prototype. That the prototype and the big machine were identical in this concept,

TROPP:

Mhm.

ATANASOFF:

but the composition of the condensers into a drum, in place of using them on a disc, as the prototype did. The prototype used standard, manufactured unit paper condensers, from the trade of the day. And these were mounted on a disc. Now, of course, this was part of the concept of this machine that the--that the clock mechanism of the Atanasoff-Berry Computer, or the prototype either, as far as that goes, was mechanical--the clock was a mechanical clock. But the computing frequencies were, of course, all handled by electronic means.

TROPP:

And I guess, in a sense, they were what we'd call asynchronious in that each unit had to wait for the other.

ATANASOFF:

They were synchronous, yes.

TROPP:

They were synchronous?

ATANASOFF:

Yes, they were synchronous. They were synchronous. And now, the composition of the condensers in the drum in terms of the technology of the day, caused all manner of difficulty. We--you must remember that this was so long ago that even the characteristics of the plastic for drums was in doubt. And somebody convinced me, I believe it was the Bakelite--some engineer of the Bakelite Company--convinced me that I should use cast Bakelite for the drum. And I have in my files purchase orders for cast Bakelite drums. I got two of them before I gave them up and went back to laminated--triple laminated drum.

TROPP:

You mean they were going to cast them to size for you?

ATANASOFF:

Yes, they did. They--on order, they cast drums; but this was, in other words, a solid plastic drum. But the plastics of the day just wouldn't stand the operations. Say, by the time you drove holes through them for the contact points, why, they would fail. And we--I believe that I had two orders of two each, and after those failed I returned to the paper laminated type of material. There's a question in my mind as to whether the paper laminate would be sufficiently well protected from the moisture so that the time capsule [?] of the condensers would be satisfactory. But I have no memory of any difficulty along this line.

TROPP:

I noticed in the chronology that the chassis was finally constructed by April of l940. The other question that seems to come up from these notes has to do with the tubes. Now, in the prototype you had these dual triode tubes, and I noticed in January 4, l940 that you had written to Sylvania regarding tubes; are you looking for a special kind of tube, or a tube with different characteristics than the one that you had in the prototype?

ATANASOFF:

Well, I was writing to the--I was writing the tube companies in connection with a large--in an attempt to get a large supply of tubes at a low price. And we were buying so many tubes, you see, in the logic circuits alone, we had 210 tubes, but there were, I guess, a hundred--roughly a hundred tubes in other places in the circuit; so that the total tube complement of the entire machine was perhaps, approximately, 300. I can probably make a better count by taking pains. And it turns out then, in parts of the circuit thyratrons were used, so we had thirty or more thyratrons in the circuit.

Now, the problem of an actual design of the logic circuit--we had to make some estimates of the time during which we could charge the condensers. And I had to know something about the impedance of the outputs--the output of the device in order to get the charging within the time limit. And my first calculations showed I needed a little more power, a little more impedance, and at that stage we were using pentodes but a new calculation--but pretty soon I found out that I can reduce the size of the condensers perhaps tenfold, and when I did this then they--then it was perfectly simple to charge the condensers from the output of the--of a standard triode.

TROPP:

Mhm. Then you were also faced with the problem of recharging them in case they lost the charge?

ATANASOFF:

Right.

TROPP:

During...

ATANASOFF:

We were charging them as we made the calculations or recharging them. Both these operations required a certain amount of power. And we got our design worked out so that we could just use more or less. Now we commenced using a tube called [6F8G], and then I commenced summing up my filament current, and there existed a dual triode called 6C8G at that time. And I commenced investigating, and all this was being done during the early months of l940, or maybe even before l940, maybe in December of l939. And I--Clifford Berry and I argued back and forth, and this 6F8G roughly had half the plate impedance of the 6C8G, as I remember it now. And the question is could I get by with the 6C8G, because it only used half the filament current. The filament current looks like an enormous amount of stuff for 2l0, for 300 tubes, roughly speaking. And we did make the change to the 6C8G, and the logic circuits for the big machine--what we called the big machine, which we also call the Atanasoff-Berry Computer for the equation solving prototype, we used 6C8G, seven of them in each logic element, and there were thirty logic elements active in the machine at any one time, that's requiring a total of 210 tubes. And there were, as I say, other tubes in the system so that the total tube complement was approximately 300.

Now, the 6C8G used three tenths of an ampere, if my memory serves me right, of filament current at six volts. And so three tenths times 300 would make ninety amperes at six volts requiring a filament transformer of 600 watts, approximately. A sizeable transformer--no such transformers were available and we ended up, why, just constructing one. The filament voltage at the time was six and three tenths volts.

TROPP:

I was--I think it's important to jump far ahead in time for just a moment, and then we'll come back, and that is the machine, although conceived in early l940--in fact actually conceived in l939—

ATANASOFF:

No, l937.

TROPP:

Well, l937, but by l939, about the time of the prototype—

ATANASOFF:

We had a working prototype.

TROPP:

You had a working prototype, and you have a pretty specific, overall logical design of the big machine.

ATANASOFF:

Right.

TROPP:

Now the big machine, as conceived, was completed at what point?

ATANASOFF:

Well, as a matter of fact, the big machine in some senses was completed by--during l94l, by the end of l94l. In other senses the big machine was never completed.

TROPP:

Right. I understand that.

ATANASOFF:

Now, there are other parts of it, and we'll discuss

TROPP:

Right.

ATANASOFF:

the parts that were completed—

TROPP:

I just wanted to wrap up one thing, in the sense of--you have a realization of the big machine, approximately eighteen months later

ATANASOFF:

Yes.

TROPP:

from the l939 overall conceptual date. Were you ever able, on this level of the machine, to solve systems of equations simultaneously?

ATANASOFF:

Oh, well, no, a system was never solved, but the machine was able to store and eliminate a variable, the controls--sufficient controls were present to enable it to do that, and that was done either in late--in l94l or early '42, I forget which. And the--probably, early in l942, two equations were stored in the two drums, and I don't believe that they have--what, thirty equations and thirty unknowns; they were somewhat smaller than that. I think they were ten, roughly ten unknowns that were put in. And these coefficients were put in that machine, taken off IBM cards and stored in the machine, and all this worked fine. And now, you see, one part that was still in the experimental stage was the base two input and output device. And this part of the apparatus worked spasmodically, and in the appropriate place we will discuss that in the future. But, you see, the base ten card reader, which, of course, used an IBM tabulator, Hollerith-type card with eighty places, it worked fine and we were able to punch cards and put equations in the machine perfectly all right, the real problem was to, was not in the elimination, the apparatus for eliminating the unknowns was under, was well in hand, but the problem was to get the numbers out of the machine, and that meant a tedious verification of the coefficients of the equations, which was obtained by eliminating an unknown between two equations, and that was verified.

TROPP:

So the new equation which would have to go back in—

ATANASOFF:

Right. But now the--as I say in the future we will discuss at an appropriate place, the base two card punch and the base two card reader, and the problems which we had with that. You realize in this original work of ours on the computing machine everything had to be built in the house. There wasn't--we couldn't use anything of anybody else's. We couldn't even use a single piece of apparatus from the IBM tabulator. Our card reader had to be designed anew because the IBM card readers would not fill our needs. We had to read the card in a special way in order to get the--in order to hold the coefficients on the card. And—

TROPP:

You also, I gather, needed a larger card with more spaces on it.

ATANASOFF:

No. We used standard IBM cards for putting the numbers in, but we had to use a number of cards in order to get all the coefficients on them.

TROPP:

I see.

ATANASOFF:

Now at that stage we had the cards, and we had punches, you see. I didn't want to have to build a punch, too. I had--at one time I was tempted to build a brand new card, but I'd have to build a brand new punch. Now, even building a card punch was an enormously hard job. And for an experiment of our facilities and so I decided to use standard IBM cards and standard IBM punch. Now there we come to something that was actually used as it was. I'd forgotten that. We actually used, for punching the IBM cards we actually used a standard IBM punch. But the IBM reader had to be constructed. Of course, IBM card reader had the base ten numbers on it. And as the numbers were passed into the machine they were automatically converted to the base two numbers by using the mechanized conversion table. And the machine did this automatically. So, if you started out with the base ten number, you could easily--you could easily put them in the card reader, convert them to base two numbers, and store them in the drums, and they would stay there forever, I guess.

One other item that I ought to record, perhaps, is the following: In reading the IBM cards, I used a standard IBM metal brush. And I still have in my possession some of these metal brushes which were obtained on a sub rosa basis from International Business Machines. [Laugh].

TROPP:

I know--oh, here they are--February of l940 you state you obtained brushes for the big machine.

ATANASOFF:

Yes.

TROPP:

And that was strictly sub rosa you had to—

ATANASOFF:

Yes, I obtained them as a gift from the mechanics, and, I don't know--maybe we finally got a shipment from IBM headquarters. But I didn't get much assistance from IBM on the whole matter.

TROPP:

Another point that might not be relevant unless you discussed some of these things--I notice in l940, both in the spring, you mention a couple of visits with Warren Weaver while he was in the hospital. Now were you and he talking about mechanical and electronic computation at any point during these visits?

ATANASOFF:

Yes. You see Warren Weaver, Thornton Fry, and Sam Caldwell were associated with a--with Vannevar Bush in what is called OSRD, early in the war.

TROPP:

Right.

ATANASOFF:

Now, I don't know exactly what the date was that they became associated, but I believe that it was after my visit to Warren Weaver at the hospital.

TROPP:

That didn't occur until l94l.

ATANASOFF:

I believe that's right. Now, I believe that I called Weaver's office while I was in New York and they told me that he was in the hospital, and I went around to the hospital to visit Warren Weaver, and I had told him about my interest in computing machines, and he was trying to help me out--help me to obtain funds for computing machines. He was then director of the natural sciences of the Rockefeller Foundation.

TROPP:

Right. Of course, they were involved primarily then in differential analyzers and building larger differential analyzers to solve larger differential equations and to do ballistic tables.

ATANASOFF:

Well, they weren't really involved with that at that stage. You know—

TROPP:

The Rockefeller Differential Analyzer, the one that's so famous, has already been built.

ATANASOFF:

It had?

TROPP:

Right. I could be wrong, but I think by '36 or '37,

ATANASOFF:

Oh. I see.

TROPP:

possibly not till '39.

ATANASOFF:

Well, Warren Weaver really wasn't, wasn't concerned with that.

TROPP:

Sam Caldwell.

ATANASOFF:

Oh, yes. Now when you come to Sam Caldwell, that's different. Sam Caldwell was, was--we're getting our dates mixed up, and it comes because we're not sticking to a strict timetable, here. But we've jumped ahead.

TROPP:

Right.

ATANASOFF:

Warren Weaver's associated with Thornton Fry and Sam Caldwell, and later, in l94l, they all came out and just didn't sell the machine. But we'd better stick to '40 for the moment.

TROPP:

Right.

ATANASOFF:

Now, I think--Warren Weaver was in the hospital and he had a kidney infection, and he told me that they had used a new drug, and, I believe, it must have, at that stage, been a sulfa drug, and that, since he was such an important man in the Rockefeller Foundation he was able to get a dose of this sulfa drug, and it cured him, and he felt as if he wouldn't have survived, if he hadn't had that drug. And I believe he's told me about that. And then, I believe, that, maybe on this date, that I visited Warren Weaver, the OSRD was just being conceived. It may well be that Weaver first told me about it laying in a hospital bed at the time I visited him in--when was that ? In 19—

TROPP:

The spring of 1940

ATANASOFF:

The spring of 1940.

TROPP:

and then you give a second visit, which has a date attached to it, and I--this is late April 1940.

ATANASOFF:

Yes. I suspect that Warren Weaver had already had--that it was already in people's mind to formulate the OSRD and commence war-connected research, you see. The United States, the army of the United States, was considered to be very backward. And there were memories of World War I days where--when we entered the war and our technology was so backwards we had to depend overwhelmingly on the European sources for such things as airplanes and—

TROPP:

Right. Of course, at this point in time we are already involved in lend-lease, so even though we're not formally at war we are committed

ATANASOFF:

Heading in that direction.

TROPP:

and people like the President and Bush are moving in a direction that indicates some future involvement on a direct basis. There's another notation here that I think is worth exploring, in about the same time period. There's a notation here to the effect that you tell IBM and Remington Rand that your computer is faster and cheaper than their equipment. Now, in one sense I can't think of anything that either of those two companies have which is analogous.

ATANASOFF:

Oh no. I was just merely stating that my machine will do such jobs--will do such jobs as were being done with their standard tabulating equipment.

TROPP:

I see. So you were thinking of—

ATANASOFF:

I was comparing it.

TROPP:

Right.

ATANASOFF:

They had, as a matter of fact, both companies expressed almost no interest at all. They--I have a letter from IBM stating that--- by somebody, and I believe it was Mr. Clement Erhardt [?], who was the Director of Market Research at IBM--I think it was signed by him, to the best of my memory, I don't have the letter at hand--in which he stated that to the best of his judgment the International Business Machine Company would never build an electronic digital computer, or never have any association with any such device. Not a very carefully thought out document.

TROPP:

Are they interested in the conceptual aspects from the standpoint of their business machines?

ATANASOFF:

Their interest was very slight. There's a whole lot of correspondence between them and--between us about this, and you have copies of this correspondence in your files. And I think that it's fairly easy to judge from this correspondence that their interest was not great or there would have been some action taken; I even considered leaving the university and going to work for one of them, but I wasn't given any particular inducement.

TROPP:

The other notation that I note here is relevant to Raytheon where you have the date April 23rd in Washington, D.C. where you're discussing the Atanasoff-Berry Computer with

Dr. C. G. Smith of that company.

ATANASOFF:

That's right. And then a Vice President in the company took an interest in it; his name escapes me, I presume I can find it in the files. I think I have a letter from him. And he went all out to provide me with anything I needed for--that they could conveniently supply. And I know he sent me several cases of 6C8Gs, which considerably advanced my fiscal position.

TROPP:

Well, I am gathering from this chronology that you spent a period of the spring of 1940 on the East Coast. These visits to Warren Weaver, the discussions with IBM, Raytheon, and so on, and I guess the one thing that I would like to probe in more detail is your visit to Howard Aiken at Harvard, which is listed here as April 26, 1940. And at that point in time, of course, Howard Aiken is beginning to construct what is now known as the Mark I.

ATANASOFF:

Right. Now, the visit that Howard Aiken--that I remember--I wonder if there's some mistake. I remember only one visit to Howard Aiken. At that time the Mark I was in operation.

TROPP:

That would have been 19—

ATANASOFF:

A couple of years later, I think.

TROPP:

Well, it would have been [the] 1942,'44 period, closer to '44.

ATANASOFF:

Yes. Yes, I remember that visit, but I don't remember a visit at--we'll have to look up—

TROPP:

Well, I think the item I referred to is just the bibliography on computers

ATANASOFF:

Oh yes.

TROPP:

you received from him, but I'm more concerned with any casual conversations you and Howard Aiken, had because conceptually both of you began in approximately the same year. In 1937 Howard Aiken has a document which outlines, philosophically, and in very specific form, precisely what he thinks high speed computation ought to be able to do and how it can be—

ATANASOFF:

I commence to remember, now. I think the principal discussion at that--between Howard Aiken and myself was relative to the relative advantages of vacuum tubes and relays at that early visit. I'm commencing to remember something. I remember now, since you've reminded me, that my notes show that I received a bibliography from him at that time. I remember that he did give me such a bibliography, and you have a copy of it in your files, and I can find that. It's—

TROPP:

Yes, it's indicated as Document U-47E.

ATANASOFF:

Yes—

TROPP:

And I'm sure I can

ATANASOFF:

We'll have to get it.

TROPP:

files. I guess I'm more interested in the topic of your discussion and, for example,

ATANASOFF:

I rem--as--as—

TROPP:

did he talk about Babbage, the Babbage concept of sequential calculation, the necessity of the machine to do iterative processes and the means by which one could actually realize this kind of computation?

ATANASOFF:

You realize—

TROPP:

I say, I'm pushing your memory, I realize, to the brink.

ATANASOFF:

Yes. I think Babbage was probably mentioned between us. I remember the principal discussion was about the relative advantage of the relays and-- And I didn't get through my head the logic--the logical system which was being used in his machine. I didn't get it through my head at that time, and I still don't know what it was, in detail.

TROPP:

I'll have to send you a copy of that 1937 paper, because the realization and the theory--the concepts are really very much the same.

ATANASOFF:

As mine?

TROPP:

No, no. As what ended up.

ATANASOFF:

Oh, ...what ended up. I see.

TROPP:

It's a very easy machine to understand, conceptually, once you see the philosophy of Howard Aiken relative to computational needs and the ways in which this is going to be done.

ATANASOFF:

Yes. He did build a relay machine, did he not?

TROPP:

Yes.

ATANASOFF:

Now he--I'm wondering—

TROPP:

I mean the Mark I is an electromechanical machine, in the sense it's got a lot of gears, just like the Babbage machine, and the, you know, the ten gears where each of the--twelve gears, I think, actually.

ATANASOFF:

Yes.

TROPP:

So it's mechanical. And you have this large shaft, and you have all these gears,

ATANASOFF:

Yes.

TROPP:

so that you're putting the numbers in the base ten, and then you have the relays

ATANASOFF:

Yes.

TROPP:

in connection with this. There were no tubes in this machine.

ATANASOFF:

Was the

TROPP:

Now, the Mark II—

ATANASOFF:

the memory--his machine has a memory, and is the memory of his machine rotating discs?

TROPP:

Rotating gears.

ATANASOFF:

Is that the kind of memory that he had in--that's what it was.

TROPP:

Mhm.

ATANASOFF:

So in--so his was closer to Babbage's original concept.

TROPP:

Well, in a sense, it's called a Babbage machine

ATANASOFF:

I see. It—

TROPP:

because it's an electromechanical realization of the Babbage concept.

ATANASOFF:

Right. Thank you very much and I--

TROPP:

It also looks very much like a--somebody has said that it's sort of like a power plant in its overall design. The shaft is sixty feet long, which gives you so much computational ability; if you want twice as much, then you increase the length of the shaft.

ATANASOFF:

Right. Now, I may have known these details at one time, but I had--I couldn't have reproduced them, and thank you very much for the explanation. I remember that we were discussing, and he felt that the vacuum tubes would prove to be unreliable in computing machines.

Now, I have to admit that, you know, I thought so--I was fearful that this might happen to be true, myself.

TROPP:

I was going to ask you about that in terms of, you know, of tests you were running on tubes, and the tubes that you were using. There's no indication that you were worrying about checking methods. Obviously, you--if these tubes burned out you were concerned with just replacing them.

ATANASOFF:

Well, the truth was, the truth was that we had a test set, and every logic element was put in the test set and tests were run on it. And the logic-- and the systems adjusted so that the primers could vary in either direction, any primer in the machine could vary in either direction. And for a considerable amount of change in the primer the machine would continue to operate. And I, you know, I told you that I was very fearful that the vacuum tubes wouldn't be reliable, myself. Finally I came to the logical concept that any computing machine of the digital type, or a fortiori of the analog type, would encounter difficulties with components. Now, that's true of ordinary computing machines with gears, such as Aiken's machine as you just described it to me. Now, if you take any primer, such as the distance between the shafts--if the distance between the shafts has a certain range of magnitudes, why, the machine will work, and if it's a digital machine it'll give absolutely correct results. Well, the range of values for that primer, if you exceed this range, the machine will go far astray, and substantially stop working all together. It's that way with vacuum tubes.

But we had carefully worked--during this period which we are attempting to recount now, this is one of the many things that was being done. And we were very busy people from, let's say, September l, l939 until the middle of 19-- clear through l940. And we were working in the design of the logic elements so that they would have sufficient range of parameters to permit them to work in a practical way. And, as a matter of fact, I don't even remember a single logic element ever failing us after it had been tested. Our design was so conservative; we were so fearful of difficulty along this line that we decided on such conservative basis-- I think I told you that Clifford Berry designed another vacuum tube, another Add-Subtract Mechanism which used only five tubes instead of seven, five envelopes of dual triodes instead of seven envelopes of dual triodes. And--however, the design of the seven tube elements proved to be more conservative because the primers could vary more and still have the thing operate successfully, so we stuck by that in the construction of the--of the machine for solving systems of equations.

TROPP:

Can I back off to one mathematical question before we close?

ATANASOFF:

Sure.

TROPP:

Since you were concerned with solving systems of equations, and since you're concerned with approximating methods, in terms of the overall design of the machine, how many digits did you conceive of that you were going to have to put in in terms of the coefficients of these equations in order to get the approximation level--one that would be acceptable to your needs?

ATANASOFF:

This whole matter was considered carefully by us, and I don't know, now, whether we came up with the right answer or not. I suspect, we didn't. But the way we--the answer we came up with was as follows: That we felt as if, in terms of the needs of the time, that we would construct this machine with sixteen base ten digits which corresponds roughly to fifty base two digits. And so each--a ring of condensers on the drums contained fifty condensers.

TROPP:

I see. So in a sense you had a sixteen

ATANASOFF:

Place.

TROPP:

place input.

ATANASOFF:

You have a word--you might say word.

TROPP:

Word, right. Sixteen bit with a--

ATANASOFF:

Fifty bit word.

TROPP:

Fifty bit word, rather. And—

ATANASOFF:

This isn't too far from present practice, either.

TROPP:

Given that, what kind of place accuracy were you going to expect, say, out of a system of twenty equations and twenty unknowns, or thirty equations and thirty unknowns? Does that give you something on the order of magnitude of seven or eight places?

ATANASOFF:

Yes, that's exactly right. That's exactly right. Yes. Yes, we expected if we got--if we got something--if we got five plus places, we would--but we expected certain-- Well, I'll use an analogy, certain triangles would be weak and certain would be strong; or certain systems of equations would be weak and certain ones would be strong. And we knew we'd run into some that would give us a poor answer.

TROPP:

At this point in time you really weren't worried about the numerical analysis, abstract—

ATANASOFF:

Well, we thought of this about weak and strong systems and solutions. At that time we thought about it. So we just had to guess and make some headway. You know you can spend your life on logic,

TROPP:

Oh yes.

ATANASOFF:

and you ought to do as much logic as you can, you ought to do as much work as you can, and pushing ahead with the work is very important, too.

TROPP:

Now in a progress report in July of '40--and I think this is the last question I'll ask you this afternoon--you indicate that you're making good progress, and the chronology pretty well documents that; and--but your statement that this computer will solve the hardest problem in one to three hours, I guess, is one that you can either comment on now or think about and we could talk about—

ATANASOFF:

Oh, I think I'll never comment on that,

TROPP:

[Laugh].

ATANASOFF:

Today I would never use any such words, of course. But you know we—

TROPP:

No, I'm speaking of, in terms of July of l940, though.

ATANASOFF:

Yeah. It looked as if--it looked as if it would make real progress, and, of course, it would. If you had, if you had the power to solve twenty-nine equations and twenty-nine unknowns in a few hours—

TROPP:

All right, this was something that was mechanically either impossible or exceedingly difficult.

ATANASOFF:

Oh, it was; why, you couldn't afford to pay a computer to do it. It was almost an impossibility for the day, and so it looked as if we were making very substantial progress, and it's in such terms that you could understand this remark; but—

TROPP:

Then you were seeing this solution, then, of, we'll say, thirty equations, just for a number, to finally reducing it to a single equation which could be solved being somewhere on the order of magnitude of one to three hours?

ATANASOFF:

Yes. That's right. And,

TROPP:

And then—

ATANASOFF:

and the machine would also do the resubstitution of the one variable to get the others, and so forth, and the whole thing would be broken down; and we even were contemplating methods of inverting the matrices and such things as that with this system, linear system. Obviously it will go into such--go into such—

TROPP:

I realize that you would never make that kind of a statement, today, but had the machine ever been totally completed, would the things that you envisioned in the early forties--how would that guess stand up?

ATANASOFF:

Oh, about right. It's about right. Of course, it had been calculated, gone through, it's about right. We timed out the various operations involved, and, you remember, that with what had to be done manually was to--you know it would punch the reduced equations on base two cards, but the base two cards had to be handled manually, so you had to keep track of them. You'd have, in fact, first you would have base two cards with twenty-nine equations and twenty-nine unknowns and the constant terms, and you'd have base two cards with one less, and one less, and one less, so you'd have a pile of base two cards when you got through. Well, you'd have, you'd have--what is it? It's--you'd have 225 base two cards before you got through with that system of equations, so you'd have to handle 225 cards, but the machine handled them pretty rapidly.

TROPP:

My other question is probably pretty far-flung in connection with this. Suppose the tube had failed sometime during the operation, thinking now in terms of the overall logic of the machine, by the time you replaced that tube, discovered that it failed, replaced it, would you have to start the problem all over again?

ATANASOFF:

Yes, you would. Yes, you certainly would.

TROPP:

That was how I saw the machine,

ATANASOFF:

Yes, that's exactly right.

TROPP:

that you'd...stop it and then continue from where—

ATANASOFF:

Oh no. Oh, we could, we could—

TROPP:

Well, if you caught it at the—

ATANASOFF:

we might use the previous calculations as a check, and we might locate the time when the tube failed, but I've had some sort of a notion that the whole design was pretty conservative. We just almost--I have to depend upon Clifford Berry for this, but I remember what Clifford Berry was talking about, and we never talked about such things as tube failures--however, these tubes had been aged, and on wax [?], and came to us, and occasionally--I remember when he tested these units he'd just plug in tubes, you know, to test 'em, and maybe make an adjustment or two. Everything would be--and in the process of testing 'em he'd perhaps reject a tube or two. But that was--very few of 'em were ever rejected. I remember that even when he was assembling the big banks of condensers, they were all tested ahead of time and the problem was, suppose one fails after he got it in there, what in the world would we do? So we put an extra two or three rings of condensers on there, so that we could switch from the ring that had a bad condenser.

TROPP:

Use it as a back-up...

ATANASOFF:

But then later we found that we could actually go into the inside of this drum and fish out our condenser and put another one in its place. And that was done on one or two occasions.

TROPP:

You know, I keep saying this is my last question, and I won't ask anymore last questions, I'll just quit for the day and thank you very, very much. We've had a really very exciting discussion earlier today, that I'm sorry I didn't record. But we'll get to those topics later, I'm sure, in our discussions.

ATANASOFF:

Now, we might just record here what those topics are. Why don't you just name them, so we'll have a record of them.

TROPP:

Well, we've been talking about your Bulgarian experience, and the Academy of Sciences, and your visit to Bulgaria, and your visit to your family's homestead in Bulgaria, the relationships you've had with various individuals in your journeys in the academic world, contacts with people like Professor Jago [?] from Hungary, and stories relating to Von Neumann and Norbert Wiener, and others of similar stature; and I hope that, as I say, at some future time we'll pick up some of these anecdotes in terms of your stories with them. Particularly I want to get the Norbert Wiener visit to the Chinese restaurant [chuckling], and his ability in Chinese, but I'll—

ATANASOFF:

And I want to get down here some of my reminiscences about Professor R. A. Buchanan, who really plays a role in this involved picture of my computing machine, and even as late as a couple of years ago gave a deposition on this--on his memories of this computing machine.

TROPP:

He is still located in Ames, Iowa.

ATANASOFF:

He's in a very advanced stage; he's still there, and he gave a deposition, the deposition was read into the trial in Minneapolis last summer.

TROPP:

Hm. Well, thank you very much, sir.

[END OF TAPE].