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Interviewee: John V. Atanasoff (1903-1995) and Alice Atanasoff
Interviewer: Bonnie Kaplan
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It's July 17, 1972, and this is a discussion involving Miss Bonnie Kaplan, Dr. J. V. Atanasoff, and his wife, Alice, [held in the West Conference Room of the National Museum of History and Technology, Smithsonian Institution].

KAPLAN:

I was interested in finding out more from you what the specifics were of Clifford Berry's contributions to your machine. What exactly did he do?

ATANASOFF:

Clifford Berry was a young man when he came to me. He was very young. He had been a bright student. Without knowing, I would guess that he was no more than twenty and he seemed younger. He was a boy that had studied electrical engineering and he was a very bright student, considered one of the brightest students in the Department of Electrical Engineering. He was referred to me by Dr. Harold Anderson of the Electrical Engineering Department of Iowa State College at that time. A conversation or two between us convinced me that he was the man I wanted, and I had Harold Anderson's recommendation as well. Those two determinations proved to be absolutely correct. Clifford Berry was so bright that I knew within a week that he would make major contributions.

Now it happened that the shape of the machine had been pretty largely formulated by the time that Clifford Berry came to me, but there were many, many details which were very important to the over-all success of the machine that were contributed by Clifford Berry. This was so evident that I actually drew a patent contract between Clifford Berry and I, so that we would hold all of our patent ideas on this subject together and that we would divide the proceeds, if any came about, which historically have not happened. I mention this here just to demonstrate my high regard for Clifford Berry regarding his abilities.

I had developed a circuit for the black box, the so-called logic circuit for the machine, before Clifford Berry came into my service, but he took that and immediately commenced to put it into practical electronic form and he also invented a new circuit or two. I believe the circuit that I had invented employed eight envelopes. I believe five of them were dual triodes and three were pentodes, although the Latin enumeration is done purely from memory and may not be exactly.

KAPLAN:

When did the seven-tube circuit come in?

ATANASOFF:

However, later we came to a seven-tube circuit. This changed from a circuit involving pentodes to one involving just dual triodes. You see, we were able to do with seven because we--the triode circuits were contained two in an envelope, so you see two times seven is fourteen and two times five is ten plus three is thirteen, and that's the correspondence that existed between those two circuits. That was worked out, that change from the circuit which contained five dual triodes and three pentodes to a circuit which contained seven dual triodes, was done with Clifford and I, but Clifford also invented a circuit which contained only, I believe five envelopes, ten dual triodes--I mean ten triodes--five dual triodes, ten triodes--I mean ten triodes – five dual triodes, ten triodes. You know, a dual triode means they put two vacuum tubes in one envelope, so sometimes we speak in terms of one cover or one tube. Sometimes we speak in terms of tubes or envelopes and sometimes we speak in terms of the elemental groups which are contained therein.

KAPLAN:

Your original idea was to use an eight-tube circuit?

ATANASOFF:

Well, remember this is in no sense an original idea. This was one realization of my concept of the black box.

KAPLAN:

Did you work out that --

ATANASOFF:

I had it worked out, yes.

KAPLAN:

Then was it your work that made the seven-tube circuit effective, or Clifford Berry?

ATANASOFF:

It was Clifford Berry and I together that got the seven. I believe that was mostly my

work, but then he developed a newer circuit which contained five dual triodes. We did not use his circuit because it was slightly more critical in operation, namely we had to have conditions just a little more exact in order to have it work, and with seven elements we had more latitude in the conditions under which the system would work. Now I could go into the theory of this if you wanted me to, but I'm trying to stick to Clifford Berry's contribution.

Clifford Berry--words of this kind in no wise give you the idea of Clifford Berry's contributions. He was a fellow worker. He and I labored together in doing this job. He was a great emotional advantage to me, to have Clifford Berry there. He was a highly organized man and when we got ready to construct something, why there'd be lists made and everything would be there and Clifford Berry would go to work on it and it would get done. When Clifford Berry said, "This circuit is working," the circuit was working. I didn't have to go back and examine it to make sure it was working. Clifford Berry took complete charge of the constructional features of the machine.

Now that doesn't mean that at times there would be difficulties and Clifford Berry and I wouldn't labor together, but, of course, I had other duties and Clifford Berry's primary duty was to associate with the machine so that he was right there with the machine at all times. It reached a place where I wouldn't go in and turn on the machine and demonstrate it to people. I always depended on Clifford Berry to demonstrate to people, because if Clifford Berry did it things worked better than they did when I demonstrated myself.

Clifford Berry was thus a great emotional support to me, a great constructural support to me, but that by no means limits his services. He had imagination; he was capable of new conceptions. Many new conceptions had to be had. Nothing was routine at this stage. Everything had to be done anew, so you can see what we were up against.

KAPLAN:

What were some of those new conceptions?

ATANASOFF:

I'm having trouble with remembering specific one. I probably could if I would just stick to this subject.

ALICE ATANASOFF:

Should you start with the fact that you built an iron frame and then decided what would go into it later? [laugh]

ATANASOFF:

That doesn't answer her question. That's not specific.

KAPLAN:

How much, for instance, did he just carry out your original plans, or did he really modify some of the ideas that you had?

ATANASOFF:

My orders were given only in the most general terms, and they could only be given in the most general terms. I had a full schedule of other things to do and a great many of the electronic circuits owe their ultimate detail to Clifford Berry's efforts. This was in a day when everybody didn't know how to devise electronic circuits and Clifford Berry didn't know too well to begin with, but he was rapidly reaching a place where he could do it. Clifford Berry was getting an education at this time, but it was proceeding very rapidly. He was maturing and I consider him a man of extraordinary ability.

KAPLAN:

Did he do most of the electronic design work?

ATANASOFF:

Assembly?

KAPLAN:

Not the assembly, the planning out of how the circuits should be.

ATANASOFF:

That was divided between us, but when it came to assembly it was almost entirely his work. You see, first it's a general concept of how a thing works. I perhaps gave my full share of that in every case, even though I, of course I furnished these conceptual details for the beginning of the machine, but later on I also added concepts. But then the electronic details had to be worked out. I would say in this part of the machine that we learned strongly to Clifford's efforts. I did some, but Clifford did more. Then when it came to construction, why Clifford did most all of it. He had other men working with him that did some too, in the actual construction.

KAPLAN:

Was he more capable in working out the circuits than you, or was it just because of the lack of time and you left it to him?

ATANASOFF:

No, you understand that I had several years on him and these years tell in such matters. No, it wasn't that I depended on Clifford for those details, but in the end because of time limitations I did depend on Clifford for those details and he did them very, very well indeed. This doesn't help perfectly, I understand, but--

KAPLAN:

And you can't remember anything specific, other than the card punch?

ATANASOFF:

You understand, the card punch was my original conception, but then it came to the details of working it out, Clifford worked these out as a thesis project and got a Master's degree in physics as a result of working out these details. You remember also that he worked out the five tube, five envelop—five dual triode and ten triode circuit, which was a new conception. Today it wouldn't be called a new conception, but you remember in those days a logic circuit had never been heard of before, so here he was working out a brand new logical circuit which had come right out of the blue. You have to remember that these years were different from the present ones. Today a student in Clifford Berry's position would work out circuits by himself because the methods have all been developed and they're taught as the lore of logic circuits; AND, OR, NOR circuits and all this class of thing. It's all very well known today, but here we were hammering out the original, an original of these, in those days.

KAPLAN:

You had mentioned before your mystical feelings towards tube workings. Did Mr. Berry share the same doubts that you had?

ATANASOFF:

Repeat the question, please.

KAPLAN:

You had talked about how you were not convinced until actually the machine worked, that you could really do things with vacuum tubes. Did he feel the same way?

ATANASOFF:

I think so, I think so. I think each one of us--we're very critical people, both of us, and Clifford Berry had my suspicions that something would go wrong with it. Finally we commenced to get our logical feet on the ground. [laugh] Then we developed a theory of how digital computing machines worked. We started with a mechanical computing

machine and we realized that a mechanical computing machine is controlled by many parameters, parameters of internal dimensions or fixed quantities which exist inside the machine such as the distance between two shafts. Now if anything happens to these parameters, due to the wearing of the machine or some deterioration or whatever, and some of these internal parameters change, well then the machine will continue to compute and compute exactly until the parameters have changed too much and they would suddenly cease to compute or compute very erratically or compute not at all.

When I realized that this was true of a mechanical machine then I knew, on thinking the thing over, that exactly the same thing was true of an electronic machine. And that has been amply proven in the years which have passed. All electronic computing machines will work throughout a range of parameters. The range of parameters may be small or large. You prefer that the range of parameters through which the machine will work will be large; therefore it will be less critical in its operation. But you remember when computing machines, when the ENIAC was first constructed it was said to work eight minutes a day or some such length of time, and the range of parameters through which it would work was not large enough or the parameters were not sufficiently controlled.

Then may I return for just a moment since I'm on the subject, to Clifford Berry's five-tube computing element. The trouble with that was that you had to control the parameters more closely in order to make it work, than you did with the seven-tube -- seven-envelope, fourteen dual triode. I'm comparing the 5-envelope, ten-triode circuit and the 7-tube or envelope, fourteen-triode circuit. The second one did not require such close control of the parameters and hence was to be preferred. But we also had voltage regulation to hold voltages constant inside of our circuit, but remember we were not working with transistors; we were working with vacuum tubes and, I believe, in some ways the vacuum tubes are preferable to transistors, but most ways the opposite is true.

KAPLAN:

Was it this variation in parameters also that made you choose the 7-tube circuit over the 8-tube circuit?

ATANASOFF:

No, that is a long story. That is a long story. The variations between the 7-tube circuit and 8-tube circuit came about in the following way. The pentodes were put in there because they had lower output impedance, and we had to charge the memory in a limited space of time and I thought at first I needed the higher power in order to get the energy into the output circuit. I hadn't computed it exactly, but I made a computation and it seemed to me that the lower impedance of the pentode was advantageous in getting the power into the memory, which was a system of condensers.

KAPLAN:

The pentodes were used in the 8-tube circuit?

ATANASOFF:

The 8-tube circuit had 3 pentodes in it; I believe it was 3. Then we changed to 7 tubes called 6F8G that had low enough impedance in the plate to charge the condensers rapidly enough to keep up with the speeds at which we desired to operate. Then later the 6F8G's, 7 of them in each computing module, each add-subtract mechanism, each logic circuit, complete adder and subtracted. Each of these tubes used six tenths of an ampere filament current, and the filament current added up to a tremendous total. We wondered if there existed a dual triode which was called the 6C8G (A?), which used half as much filament current. We commenced to experiment with that and in the end we found out that even this last tube would charge the condensers fast enough, providing we made the condenser small enough. At first we thought we would have to make the condensers larger because this caused part of the confusion, you see. We thought we would have to make the condensers larger in order to make them hold their charge for a sufficient length of time, but when we commenced doing quality control on the condensers which we could purchase, why we realized that the condensers which we were getting had a slow enough rate of discharge so we could use smaller condensers and still have them remain charged long enough.

So in the end we came up with smaller condensers and a 7 dual triode logic circuit in which each tube only used three tenths of an ampere filament current and that was the way the large machine was constructed. I believe that the prototype, on the other hand, which was mentioned in history, which was constructed and operated in the fall of 1939, contained the 8-tube circuit which consisted of 5 dual triodes and 3 pentodes.

KAPLAN:

Had you worked on the 7-tube circuit before the building of the prototype? And then decided to put it in the main machine, or did that come after the prototype?

ATANASOFF:

Well, I expect you'll find that while the prototype was being perfected, that the circuits were also being studied; and the 7-tube circuit was actually in existence before the prototype was working.

KAPLAN:

Now, let me see if I have this straight. You changed to the 7-tube circuit because you didn't need the power output as great as you got from the 8-tube circuit?

ATANASOFF:

Yes.

KAPLAN:

And the input was less for the 7-tube circuit?

ATANASOFF:

No, the inputs were substantially the same, but remember they're two kinds of inputs--single input and the power input. The power input in the 7-tube circuit was less but the single input was the same.

KAPLAN:

So even though you ended up having an extra tube because you had fourteen there --

ATANASOFF:

We finally used that extra triode. It's used in a very peculiar way in a circuit and a way that I don't perfectly understand at the present moment.

KAPLAN:

O.K. Getting off the subject of Berry, you mentioned somewhere that during the time you were working on the prototype, you had very few people to talk to at Iowa State about it. And you were asked whether or not A. E. Brant was there and whether you were able to get any sort of psychological or interest support from him at all. You commented that Brant was not there, but that even if he had been there--he wasn't there at the time, but even if he had been there, that he wouldn't have understood what you were doing.

ATANASOFF:

That's true, but he would have given me a lot of moral support, on the contrary. You realize the difference? Some people give you moral and intellectual support; some people give you intellectual support and no moral support; some people give you moral support and no intellectual support. Brant gave me a large amount of moral support. Alice and I saw him down in Florida four months ago and he's still alive. He's ten years older than I am and he's still alive and he' still giving me moral support.

KAPLAN:

Was he at that time--were you corresponding with him?

ATANASOFF:

No, we were out of contact, but he would have been good to have had around. He'd have come over and patted the machine and smiled and acted--he would have understood the general principles, but he wouldn't have understood the technical details. The work he and I did, he didn't understand the technical details, but he furnished the moral support for the work.

KAPLAN:

This is in the complex spectra work?

ATANASOFF:

Yes.

KAPLAN:

He didn't understand any of them?

ATANASOFF:

No, he didn't understand them.

KAPLAN:

What part did he play in this complex spectra analysis?

ATANASOFF:

He furnished the moral support for the project. It perplexes you. He didn't know what a complex spectrum was. He didn't know how it was generated. He worked with IBM tabulating machines all the time. He didn't understand any of the inner workings. I didn't understand any of the inner workings, so I just made a guess how I would have built it if I had built such a machine, and so I built an additional machine to work with it which would work with it if it were made the way I would have built it, if I had built it. Nobody would give me a circuit diagram of an IBM tabulator. I just guessed at them and it turned out that my guess was correct and the ensemble worked perfectly. This may seem to you like a strange method of operating, but that's the way it went. This invention of the type of circuit Brant was incapable of doing and I had to do that invention of the circuit.

KAPLAN:

What beside just smiling at you and patting you on the back did he do to help with the complex spectra?

ATANASOFF:

Well, for instance, if I needed any parts he'd go around and steal them for me. [laugh] IBM was incapable of withstanding his assault. If they had anything stored anywhere and we wanted it, why it would soon appear in my hands. This was very useful and important to me and everybody that works with A. E. Brant--A. Brant right now is down at the University of Florida and he at the age of 86--no, 88--there he is at the University of Florida occupying an office and whenever anybody wants any help they come to him and he smiles at them and makes a few suggestions of a general type and gives them encouragement and everybody goes away and everybody's happy. This is a kind of positive euphoria which Mr. Brant has always radiated in large quantities. You know what euphoria is?

KAPLAN:

Yes.

ATANASOFF:

So you understand.

KAPLAN:

Getting back to the question of support, did you really seriously feel the lack of having people to talk to or people to encourage you?

ATANASOFF:

Yes, yes.

KAPLAN:

Did that affect your work at all?

ATANASOFF:

Yes, yes.

KAPLAN:

How?

ATANASOFF:

Oh, you know, indeed. You see, you can't get up with enough zip. When you have to furnish all the zip yourself then you have--for instance, you've heard that I'm working on

an alphabet now. I'm doing this all by myself and my zip is low and pretty quick I'm going to get some associations with some people and then I'll get more zip and zing, and I'll draw from these other people. This is a strange effect of people working together. They should work mostly by themselves but they should have access to each other to --

KAPLAN:

But you went ahead with your machine anyway? You went ahead with your machine anyway.

ATANASOFF:

Yes, I did. That's just because I'm a tough individual.

KAPLAN:

But was this lack of interest on the part of other people at all important in your dropping the machine eventually or never returning to it?

ATANASOFF:

No, not really. I don't blame anybody else for that. The exigencies of the war were very heavy at this time. The United States was going into, well we weren't sure that the United States wasn't going into a state of collapse. The emotional status at the time of Pearl Harbor, you know, was extreme in the United States and the pressures were very great. Yes, Pearl Harbor occurred during the history of this machine, you know, and Pearl Harbor was on all of our minds. We all thought we should do everything we could to advance the national defense.

KAPLAN:

While you were working with the Navy didn't the computer projects come out? Didn't the Armed Forces push at all for some sort of a computer mechanism?

ATANASOFF:

Well, let's see. During the war, as far as the Navy went, nothing of this happened. That was not true of the Army. The Army had a tremendous amount of calculation for ballistic tables, and they got interested in a computing machine and hired Mauchly and Eckert to work on computing machines. After the war was over the Navy then got very interested in computing machines and for a time I was in charge of research on a computing machine for the Navy. This was after World War II.

KAPLAN:

Do you know a more specific date?

[Recorder off and back on]

ATANASOFF:

Late in 1945 the Bureau of Ordnance of the Navy initiated a computer project at White Oak, at the Naval Ordnance Laboratory where I was located, and I was put in charge of that project. This project continued. It was supported by the Naval Ordnance Laboratory on a very weak basis. We had plenty of money, but no help. We were doing the best we could to get help. During this period I secured the services of Dr. A. E. Brant to furnish me moral support for the project, [laugh] and we continued not to have personnel. The personnel were extremely short at that time, and the Naval Ordnance Laboratory did not consider it sufficiently important to put enough personnel. I myself had complete charge of a division at the Naval Ordnance Laboratory, the Division of Acoustics, and I had all these duties as well as duties in connection with the computing machine. These were added duties.

KAPLAN:

What did you do with the computing machine project?

ATANASOFF:

We commenced to lay the foundation for a new computing machine.

KAPLAN:

Was that at all similar to your previous machine?

ATANASOFF:

It had dissimilar elements and it had similar elements. I can put it that way.

KAPLAN:

Could you elaborate on it a little more? What did you do? What ideas did you have?

ATANASOFF:

One of the new ideas that was being examined, we knew that the condenser memory would no longer do and were looking for new kinds of memories and we did considerable—

KAPLAN:

What bothered you about the condenser memory? What made you change your mind?

ATANASOFF:

It required a switching circuit as its input, which in the case of my original machine was done mechanically. I wanted to have no more mechanical parts in the computing machine, so we were passing into a phase when we were using an electronic switching--a cathode ray tube as a memory. And there were many concepts, at that time we were devising concepts as to how a cathode ray tube would be used in a memory.

KAPLAN:

Who else comprises the "we"? Who else makes up the "we"--you and-?

ATANASOFF:

Well, there's a man by the name of Calvin Morse; a man by the name of David Beecher; a man by the name of Bob Elbern; and a man by the name of David Barbrough; and of course Ernest Coltrud. There were other people on the project. Ellington wasn't there. He'd left the laboratory by this time. In the end the whole form of the computing projects in the United States took a different form and the program at the Naval Ordnance Laboratory, in spite of my best efforts, had not grown very much in personnel and the Navy decided to drop the project.

KAPLAN:

How interested were you at that time in getting a new machine built? Was it one of your primary concerns; did you really want another computer?

ATANASOFF:

Well, the Navy wanted another computer.

KAPLAN:

What about you? Were you still interested in computing problems?

ATANASOFF:

Oh, yes, I was still interested in computing projects, of course. I wouldn't have been doing it; I would have told them, no I wouldn't do it, if I hadn't been interested. I wouldn't do it otherwise, but there were sharp delimitations on to how I could--by this time computing machine business was becoming very large business.

KAPLAN:

This was by '46?

ATANASOFF:

Yes, and '47. The thing stopped in late '46, the project was dropped. It just about went through the calendar year '46; that's about how long the project was there. It was a short-term project and by this time everybody realized that you had to have a large crowd and you had to do very specific research and you had to develop all the components of a computing machine in a very thorough-going way. Although I had more men on this machine than I ever had on my old machine, nevertheless, it was to be a much larger machine with a much higher performance and we had to have many more men in order to carry out the project.

KAPLAN:

Is there any reason why you didn't get involved in the other computer projects that were going on?

ATANASOFF:

Well, you understand they weren't at the Naval Ordnance Laboratory. I was employed at the Naval Ordnance Laboratory and I had a complete division there in my charge at the time.

KAPLAN:

So you couldn't contribute anything to--?

ATANASOFF:

I couldn't quit the project without I resigned. I could have easily resigned if I had wanted to and gone off and associated myself with another computing project, but I didn't do that, rightly or wrongly or however.

KAPLAN:

In the subsequent years have you gone back to the computer idea? Have you really felt the need for working out more --

ATANASOFF:

I have the interest but I haven't been physically able to keep up with it.

KAPLAN:

Well, what about,say, in '48 or sometime right after the war, and projects were over? Why didn't you go back to computers?

ATANASOFF:

Because I was doing other things.

KAPLAN:

So it was not an overriding interest at that time then?

ATANASOFF:

It was an interest and a great interest to me. The rest of the story is rather peculiar. I did not at that time, in 1946, realize the great importance of the work which I had done in the over-all computing machines. I didn't realize that, as a matter of fact, the concepts which I had were the best that there were in the computing art. I didn't realize I was the best man--strictly speaking, I perhaps had the best grasp of the elements of computing in 1946, as good as anyone. Historically speaking, the ideas which I conceived were more advantageous than those of others, but I did not myself realize that that was true. In retrospect I realize it now.

KAPLAN:

Which ideas are those that you think were superior?

ATANASOFF:

Let's just name one. The logic circuit had been derived directly and solely from me and I didn't realize that the logic circuit had not been devised by other people.

KAPLAN:

Didn't some other people make logic circuits eventually?

ATANASOFF:

Yes, they did.

KAPLAN:

They had no connection with you whatever?

ATANASOFF:

Oh, yes, they did. You know about the litigation and Mauchly and Eckert. Mauchly and Eckert commenced to use logic circuits and they were derived directly from mine, according to some people's concept, but at least influenced by mine historically.

KAPLAN:

Do you think you could trace that influence at all?

ATANASOFF:

Yes, it is traced in the litigation.

ATANASOFF:

I don't want to. I asked you to go to the litigation and see the charges which have been made there. It's futile for me to do it when other people have done it so much better, you see, Bonnie. If I do it it appears to be self-serving and unduly egotistical.

KAPLAN:

I think in this project it doesn't matter. I'd like for you--what you know, what you say, and what your views are.

ATANASOFF:

I'll tell you what you should do. You should read the evidence given in litigation by myself and by Mauchly and by other people who have given litigation there and then make your own judgments as to how this goes. I think that's the right way. I'll make a statement that this is true--that I did the original logic circuit. I've gone that far and that in itself looks egotistical and self-serving, but I believe it's true and I believe that it will be established that that was true.

KAPLAN:

Did you tell Mauchly what you had done with the logic circuit?

ATANASOFF:

Mauchly came and visited me and understood my machine.

KAPLAN:

That was in 194l?

ATANASOFF:

He arrived on the l7th of June, 194l.

KAPLAN:

And did you explain to him the workings of your circuit?

ATANASOFF:

Yes.

KAPLAN:

So he knew when he left there exactly how it worked?

ATANASOFF:

Yes. The evidence is in the record.

KAPLAN:

And what about Eckert? Did you have direct contact with him?

ATANASOFF:

No, I had no direct connection with him with any information.

ALICE ATANASOFF:

And at NOL you met Eckert.

ATANASOFF:

I met Eckert once at NOL later, much later.

KAPLAN:

I don't know what NOL is.

ATANASOFF:

Naval Ordnance Laboratory.

KAPLAN:

Was that after Eckert and Mauchly had done their work?

ATANASOFF:

After they had done some of it, but not all of it. This was while they were in the process.

ATANASOFF:

Did you have any discussions with Eckert then about computers?

ATANASOFF:

Yes, but it was specifically about what's called a quartz memory.

KAPLAN:

Using quartz crystals?

ATANASOFF:

Quartz crystal memory.

KAPLAN:

Was that ever implemented at all later? Did they ever make a machine or any ideas using the quartz crystal?

ATANASOFF:

I believe that the EDSAC used the quartz memory.

KAPLAN:

Was that your idea also?

ATANASOFF:

No.

KAPLAN:

Did Eckert come to you and --

ATANASOFF:

He came to me because I was an expert on quartz in this case.

KAPLAN:

But he had already conceived the notion of using a quartz memory to record information from you?

ATANASOFF:

I think so. That's not due to me.

KAPLAN:

What other ideas, would you look back on and in retrospect say were the ideas you had at the time that were used by other people?

ATANASOFF:

Well, you know I used scale of 2 and, of course, there were other people with a scale of 2 concept, but I had the scale of 2 concept in computer machines and I had the regenerative memory and I had sequential calculation. I certainly assembled the first electronic digital computing elements which were ever devised.

KAPLAN:

Did these notions, like your scale of 2 and the binary operations and sequential arithmetic--how many other people knew about these?

ATANASOFF:

I think you're the historian; you should know.

KAPLAN:

From your contacts, did you tell many people about them?

ATANASOFF:

As a matter of fact, at that time I only told Mauchly and some people in high prestigious positions in the scientific world. Now who they told, of course, I do not know and so--I was attempting to keep it relatively quiet, Bonnie, but I wasn't entirely succeeding, of course. You never can in matters of this kind.

KAPLAN:

Did you want to keep it quiet because of the patenting?

ATANASOFF:

Yes, I did. I planned to keep it quiet because of the patent situation.

KAPLAN:

What was the patent situation? What happened?

ATANASOFF:

What happened was that Iowa State College ruled that they would take responsibility for the patents.

KAPLAN:

When was this?

ATANASOFF:

Oh, I suppose it was 1941 or maybe 1942, or maybe 1940, in that period. We can find the historical documents –

KAPLAN:

Was it after you had your prototype working and you were working on the big machine at the time?

ATANASOFF:

I was working on the big machine and they would get the patents and there was a patent contract drawn between Iowa State College and me involving these terms. That patent contract is a matter of record and it's in your files too. Now I left it to Iowa State College, but I participated in finding a counsel who was supposed to be a man of sufficient skill to draw a patent on such a complicated machine as this. This was very difficult because very few people would even think about attempting--very few patent attorneys would even think about attempting to draw a patent on such a subject.

Then the patent didn't get drawn, and didn't get drawn, and didn't get drawn for various reasons during the war and afterwards. I was writing letters continually about why this and why that and supplying information to a patent attorney and I was a busy man besides. There is not much question as the thing stands today that I did supply him with

plenty enough information to get a patent, but that he did not do it and the exact cause why he did not do it is open to question. I have attempted to draw from everybody involved an exact resolution as to why the patent was not obtained, but it was not obtained.

KAPLAN:

So none of these ideas were ever patented?

ATANASOFF:

That's true.

KAPLAN:

That's too bad.

ATANASOFF:

Oh, I don't know that it is; I don't know that it is too much. A patent is just another approach to life and this has worked itself out.

KAPLAN:

Did you want any publicity at all for your ideas, or any sort of recognition or credit?

ATANASOFF:

Sure I wanted recognition, sure. I would have had a kind if recognition if I could have gotten a complete set of patents. If I had gotten a complete set of patents at that time I would have had recognition but not much money.

KAPLAN:

But you would have had the recognition?

ATANASOFF:

I would have had recognition, but of course the facts are that I'll have the recognition anyway by the time you and Henry get through with the project. Recognition's going to be cleared up.

KAPLAN:

I'm curious as to why in much of your other work you never published articles, or you

had ideas and didn't really pursue them and subsequently found out that someone else had made something big out of them. Were you just not at all ever interested in publicizing your ideas, or did you --

ATANASOFF:

You mean--when this current litigation started--you see, the current litigation started because of the Mauchly and Eckert patent, and these patents were held in suspense in the Office and the last one issued in ’63, I believe--the ENIAC patent issued in ’63. So it gave them a new life and then they had basic control of the computing industry for l7 years. That would be from '63 to '80, wouldn't it? They commenced to get ready to sue the other companies and to force all the other companies to pay the royalties. The other companies attempted to defend themselves and the defenses which they could contrive were many, of course, but their principal one was that as a matter of fact Mauchly and Eckert had invented these things but that they had sworn a false oath, that as a matter of fact that they were derived from Atanasoff. These companies and, of course, attorneys like Charles Paul (Call?) and his associates, Henry Holiday out there have all been engaged in this exact pursuit.

KAPLAN:

Right. Now I'm talking about other things. For instance, you mentioned that your Master's thesis had a new approach in it and that you used a method that was very, very little used at the time but eventually became a wide-spread method due to your work on your Master's thesis, but that you never published your Master's thesis.

ATANASOFF:

No, that's not true about the Master's thesis.

KAPLAN:

Did you publish it?

ATANASOFF:

No, I didn't publish it, but it wasn't worth publishing.

KAPLAN:

The impression I had gotten, for instance, was that some of the methods which you had used eventually became very important.

ATANASOFF:

No, you must be very careful to quote me when you quote those things.

KAPLAN:

Not that they became important because of you but just --

ATANASOFF:

I have some little hint in my mind as to why you're taking the current trend, and I believe there's some justice in it. I'm not complaining about the trend but let's make sure that you go over in detail each of these things in which you assumed that something like this happened and then I'll try to tell you the facts and refer to each one separately, because of facts. Now, my Master's thesis was a good enough Master's thesis, but it wasn't published and I would be even more embarrassed if it were published.

KAPLAN:

No, I'm just curious because it seems that several –

ATANASOFF:

May I tell you this? When the litigation finally started, why the attorneys went to the Patent Office and got a list of all my patents. I think the number was 33 patents which I have which are issued in my name, so I'm not entirely devoid of patents. You just don't know anything about them. [laugh]

KAPLAN:

No, it just seemed that there have been any number of times when you would come up with some sort of an idea, and somehow it had gotten lost.

ATANASOFF:

You must realize--let's talk about this. Bonnie you raising the question with me. There's an element of truth in what you say and there may be more of an element of truth in regard to J. V. Atanasoff than there may be in regard to most men. Let's go over it and you'll find that what is happening is something which is--which may be brand new to you but which is experienced all throughout science and throughout technology. I have some very strange instances of my knowledge, grasp of knowledge, in which I failed to mature. I've talked to many other men, many brilliant men in this world, and most of those brilliant men when their soul is bared and they get right down to tell the truth, they tell similar instances too. We've all done it, Bonnie. The facts are that it makes me squirm in my seat if I think of it.

Let me tell you about one, and then let me excuse myself as well as I can for that and

then you'll sort of catch a semblance of the spirit of the whole thing.

Now, it turned out that one day I was in an office in Azusa, California, and man walked in and commenced to tell us about something. Now the word for this thing is in connection with this new light-emitting thing called the laser. The man hadn't gotten but a sentence or two out when I said, "Let me complete the lecture." I got up and told them what a good invention it is and exactly how it worked. I had never heard anything about it before in my life! I invented it on the spur of the moment. Ever since I've been kicking myself because I didn't invent the laser! Well said, well done. But now think how many other people didn't invent the laser. This included two men whose names I will give you now. This included Albert Einstein and P. A. M. Dirac, two of the greatest physicists that ever lived, but they didn't invent the laser either and they originated the concepts on which I based my concept of the laser. So don't be too hard on me. Life is like this, Bonnie.

KAPLAN:

No, I'm not trying to be hard on you.

ATANASOFF:

I'm putting this in the record on purpose.

KAPLAN:

I'm just trying to understand why when you would come up with an idea that you fully worked out and... never follow through.

ATANASOFF:

That's all right, Bonnie, perfectly all right to do that. Time is limited, you know. You've got many things on your mind. You're scrambling hard and you've got many things on your mind. I'm not sure that I did as well as I should. Don't misunderstand me about that.

KAPLAN:

I'm not trying to condemn you either.

ATANASOFF:

I'm not saying you are, but I'm saying I'm not sure I did as well as I should, but a man does work under very complex situations, you know.

But since I've known you, I've invented this explanation as to why I didn't invent the laser. [laugh] This explanation as to why I didn't invent the laser is a very good one.

Now I can give you another one which is equally forceful and which without any question I had the elements at my fingertips. You know when I was a very small boy they used to use as detectors of radio signals, they used to use crystals, and they used to use in particular galena crystals. Then people were fooling with galena crystals and they showed that galena crystals would oscillate under very special conditions, they would oscillate by themselves. This meant they were generating radio frequency. Now here was the very kernel and heart of the transistor, right before my very eyes and I knew all about it and I wondered about it and spent hours on it. If I had pursued this I could have invented the transistor and it would have been invented several decades before transistors were invented. But I didn't do it. You can put that in the record too, Bonnie.

KAPLAN:

O.K. You mentioned somewhere that you went to Wisconsin for your Ph.D. Iowa State had never given a Ph.D. in mathematics at about the time when you decided to go to Wisconsin for your Ph.D.

ATANASOFF:

Let's see. Wait, just a moment --

KAPLAN:

The date I have on that is 1926. By then Iowa State had still never given –

ATANASOFF:

I went to get a Ph.D. in 1928, and at this time Iowa State had never given a Ph.D. in mathematics or in physics.

KAPLAN:

I was curious as to what was the status of mathematics and physics at Iowa State then.

ATANASOFF:

All of the better institutions were advancing and giving higher and higher courses, but some of them had given the Ph.D. and some had not. Of course I went to Wisconsin which was one of the more highly developed at that time, one of the more highly developed schools in the Midwest. They had given many Ph.D.'s, although I was looking over the list and I can show it to you, Bonnie, if you wish. During the last month I was looking over the list of all the Ph.D.'s from the University of Wisconsin, and my Ph.D. was obtained in 1930 in theoretical physics from the University of Wisconsin. There were not too many Ph.D.'s which were given before my time, and there are many have

been given since.

KAPLAN:

Was that common throughout the country or was it just --

ATANASOFF:

Oh, that was the state of affairs. Harvard had given more, of course, but still even Harvard hadn't given Ph.D.'s, not many--I imagine probably ten times as many or maybe fifty times as many PhD's have been given since that date as before that date.

KAPLAN:

It seems the only significant work that was going on at Iowa State in the late twenties, then, was the statistical research?

ATANASOFF:

No.

KAPLAN:

Was there anything happening then in math or in physics or electronics or engineering there?

ATANASOFF:

Well, I must be very, very cautious what I say now. Of course there were things going on there, embryonic ideas that were being developed. There were some laborers in the vineyard of life there at that time. They weren't very productive. Perfectly true, it's hard to be productive. Now I might say that while many problems--while they were applying statistics to many problems in agricultural at Iowa State College, because there was much demand for such solutions, the methods which were being employed were not being originated by the members of the staff at Iowa State College. These people were merely aping the methods used by others in the main, and so you can't call them original even in statistics, too original. I don't mean to say, I don't want to decrease the art of the research which was being done at Iowa State College in those days. I believe that statistics at that time was in no better shape in a basic way than the other parts of mathematics or physics or engineering. It was just merely that there was an overwhelming sum of money available to move ahead and do the routine and grind out answers say, questions in regard to agriculture and the inexact sciences--the biological sciences.

KAPLAN:

What was the general intellectual feeling at Iowa State? Were you pushed to try to do something different or new or important, or was it just that you were there and you were supposed to teach and that was good enough, or what?

ATANASOFF:

I don't know. I always felt pushed. I got a great inspiration from people I was associated with at Iowa State College, in spite of my story about lack of interest in computing machines. I couldn't interest anybody. The man across the hall--I went across to see him and he said that computing machines would never be good for anything because they would never drive a streetcar. Now those were his exact words and he doesn't like to hear me describe those words. I tried them on him in the last year. He doesn't like to have me repeat these words at him … he doesn't like to have me repeat these words to him, but as a matter of fact he said those exact words and as a matter of fact, A) it wasn't very important and B) it turns out that computers are used for driving streetcars today. He was wrong both ways. [laugh] But it is true that I am sure that if I had had a body of other interested men who were active in these fields it would have been easier for me to work. I don't mean to say that my colleagues didn't give me much support in other ways. In many other ways I got much support from my colleagues and always have.

KAPLAN:

What was your reaction later in the forties or fifties when other computing machinery came out and other ideas in computing became evident and computers started being used and appreciated and realized?

ATANASOFF:

Well, I knew I had done this original work in computing machines. I told you that during none of this time did I come to realize, as I do today, how fundamental and important my original work at Iowa State College was.

KAPLAN:

When did you first start realizing how important that was?

ATANASOFF:

After the current litigation.

ALICE ATANASOFF:

Within the last three years, four years.

ATANASOFF:

Within the last four years. Then I did something which I hadn't done before. I read the ENIAC patent (the ENIAC patent hadn't issued until '63) -- I read the ENIAC patent and I realized that Mauchly had taken ideas which I had devised and used them in the construction of the ENIAC patents. Now, I don't know whether he realized where he derived those ideas but he used them, whether he derived them from me or not. One could argue that he had those ideas in his mind, having visited me, and then went ahead and used them and never even realized the connection, because the pattern of his machine is somewhat different than the pattern of my machine. However, there are a few instances in which it is perfectly clear that he must have had the memory and must have known that he was infringing upon my original concept. I could name a few of these instances.

KAPLAN:

Would you?

ATANASOFF:

I don't think I will at this time, no. I think I will reserve that for some future--this is a matter which is still under litigation and the litigation will have a much more substantial voice in making statements of this kind than any voice which I can raise.

KAPLAN:

At the time when Mauchly's machine came out and Aiken's machine came out and IBM got into the field, and Sperry Rand -- did you have any feelings about the matter? Did you look at these machines and just say, “wow, it's great they're doing them,” or did you look at them and say, “this is what I could have done.”

ATANASOFF:

As a matter of fact, I looked at the ENIAC and said, I'm not interested in having any connection with it.

KAPLAN:

Why?

ATANASOFF:

Because it wasn't a very effective machine, and I didn't like its end results and I didn't like the insufficiency and I didn't like this and that about it. I felt the same about the MARK I and the relay machine. These machines would not have served the ends for which I was reaching.

KAPLAN:

Did you feel any motivation at all to push your own work?

ATANASOFF:

Yes, I had motivation but not much in the way of opportunity then. If--if I had been told at the Naval Ordnance Laboratory to stop what you're doing, devote your full time and attention to computer machines, and if I had been supplied with the staff necessary to have done this, I can assure you, Bonnie, that history would have been written with different words.

KAPLAN:

How were your relations with the faculty –

ATANASOFF:

I might just add one more word to that and that is this. My superior at the Naval Ordnance Laboratory at that moment was Dr. Lynn Rumbough. In the end Dr. Lynn Rumbough made a complete statement about my efforts in connection with computing machines and he admitted clearly to me that my accomplishments had been entirely satisfactory, considering the resources which the Naval Ordnance Laboratory gave me.

ALICE ATANASOFF:

You once said you were very much involved with –

ATANASOFF:

Here's another thing--while I was working on computing machines my own Acoustics Division was carrying principal responsibility for instrumentation of this project called Crossroads--do you know what Crossroads was? Crossroads was the first atomic explosion after the end of World War II, at Bikini Atoll. Newspapers during the last week have been carrying the pictures of some of the ships which were sunk. They put some cameras down in the bottom of the bay at Bikini and they have photographed the battleships which were sunk there at that time with this explosion, and this just illustrates what my position was. In other words, during the year that I was working on computer machines I had to first plan the operations that are in here. Let's see. In March or April I had to start instrumenting for Crossroads. On June 30th the operations began and July 25th was the last explosion. I had to plan instrumentation, see that it was carried out, and go to Crossroads and spend 8 weeks at Crossroads myself during that period.

KAPLAN:

Today when they run atomic explosion experiments, don't they use computers for timing?

ATANASOFF:

They didn't in that one. There wasn't a computer in existence during Crossroads.

KAPLAN:

Did anyone foresee then that you would have a need for some sort of --

ATANASOFF:

Oh, yes, we all had visions.

KAPLAN:

Was there any work being done at that?

ATANASOFF:

Yes. Well, yes, we won't say anything about that, Alice. Alice has just reminded me that in the year after the computing machines were closed--in the summer of 1947 that I did another instrumentation job in which I went to Europe and spent 4 or 5 months working on instrumentation for an explosion at Helgoland, off the Bay of (Munde?) in North Germany, a very large explosion. I did all of the instrumentation for that and received an award from the Geophysical Union for that job and a citation from the Admiral.

KAPLAN:

Was this instrumentation electronic?

ATANASOFF:

Yes.

KAPLAN:

Did it use any logical circuitry at all?

ATANASOFF:

It didn't use any computers. They used logical systems, but no computers.

KAPLAN:

Did you design any of these local circuits?

ATANASOFF:

I designed all—I and my staff designed all the instrumentation. The reason I'm telling you this is to show you I had another full-time job besides this one. This was very much of a tacked-on affair. I had 5 or 6 men, perhaps, working on the computer machine, 5 or 6 men was nothing in this age of computing machines.

KAPLAN:

Your computer interests it seems have always played second fiddle, that there was always something else.

ATANASOFF:

Have been forced to under these circumstances, yes. Oh, this is perhaps enough of this... Now you want to try to go on to another question. What was it?

KAPLAN:

I wanted to ask you how your relations were with the faculty and students and people at Iowa State, when you were there in the thirties.

ATANASOFF:

What kind of relations did I have with them? Very good relations.

KAPLAN:

Good relations. You had many graduate students working for you.

ATANASOFF:

Yes, I had more graduate students than anybody else. I wouldn't be surprised if I had the largest group of graduate students at Iowa State College. That probably wasn't so; probably over in Biology they had larger groups, but in Physical Sciences I had much the largest. Everybody attempted to associate himself with me. So I would have, you see, here we were just beginning the Ph.D. work during these years at Iowa State College. I'd gone off and got a Ph.D. and then I was back and then they commenced to give Ph.D.'s. I didn't give the first Ph.D. in physics, but I gave--I don't know whether I gave the second, but the third or fourth issued under my--and after that I issued more Ph.D.'s than anyone else and perhaps more than all the rest of the members of the physics faculty put together. Between the years 1935 and 1942 I issued eight Ph.D.'s. Now, that may not seem like too

many, but it's a very large number as things were going at Iowa State College at this time and I issued 20, under my major professorship issued 20 Master's degrees and 8 Ph.D.'s during this period when I was working on a computing machine too.

KAPLAN:

Was this popularity in part because of the nature of the projects in which you were involved?

ATANASOFF:

Because--well, I have to resort to self-adulation again, I have to resort to self-adulation. Why was I popular? First because I was the toughest man there, and made people study the hardest and people had the feeling that if they took a degree under me the degree would mean something. Second, because I was imaginative and devised new projects of various kinds which people could work on.

KAPLAN:

Most of the projects that I've heard about are the ones that dealt with numerical techniques and …

ATANASOFF:

You see, that was the field that I more or less selected. Of course you haven't heard about--I had 3 or 4 Ph.D.'s in quartz and you know those Ph.D.'s dealt with instrumentation and anisotropy, vibrations of anisotropic solids, and the like.

KAPLAN:

You also had experiments in egg viscosity that you were doing.

ATANASOFF:

Egg viscosity.

KAPLAN:

What other kinds of projects were you directing?

ATANASOFF:

Well I worked on philosophical projects in methods of approximation in the solutions for linear operational equations.

KAPLAN:

Why do you call that philosophical rather than mathematical?

ATANASOFF:

It was mathematical, I'll say mathematical. We were trying to change the philosophy of approach to these mathematical problems.

KAPLAN:

You were trying to generalize work … in that the different approaches ... (NOT CLEAR)

ATANASOFF:

Do you know what … is?

KAPLAN:

I don't know exactly what you did.

ATANASOFF:

We were trying to generalize … and we were trying to broaden them.

KAPLAN:

You would consider that a new change in approach?

ATANASOFF:

Yes, I think it was. I think that would have received a great deal more attention if I had had time to advertise it and explore it.

KAPLAN:

You mean attention from the rest of the profession?

ATANASOFF:

Yes, attention from the rest of the profession, I do mean that, if I had had the time to explore it, to go around and give papers. But when you've got yourself spread so thin, you see, you're getting out ideas and ideas and you're pushing ideas and pushing them so very, very hard, but at the same time you're pushing other things at the same time, why you've kind of got your hands full.

KAPLAN:

Have you done subsequent work in--I guess in numerical analysis? I guess today that would be called numerical analysis.

ATANASOFF:

Yes, it would.

KAPLAN:

Have you been doing —

ATANASOFF:

No, I haven't. Since I left and went to the Naval Ordnance Laboratory in the first place I had a long period--let me tell you, Bonnie, about my life since then, you see, from there. See I got to the Naval Ordnance Laboratory and then all my publications became classified. That doesn't mean I couldn't have written them.

KAPLAN:

Even the things you had done before you went there?

ATANASOFF:

No. All the things I was doing then. Things that were current then all became classified, things then. Then I had a company, and the output of a company is not papers but money.

KAPLAN:

Why did you go into business?

ATANASOFF:

Why did I go into business? Well, let's see why I went in business. Well, Bonnie, I'll explain it to you a little bit. In the first place I had this supernumerary position in the Federal Government and I had received honors from the Federal Government. I might tell you in connection, you _____, I've done a number of very striking things. Did you ever read about pressure mines?

KAPLAN:

The work you were doing with the mines in the Navy labs?

ATANASOFF:

Did you hear about the mines that were being shipped to off Japan? If you had watched the literature very closely about the mines in Japan, you would have noticed that one of them was a pressure mine.

KAPLAN:

Are you talking about the literature today?

ATANASOFF:

I'm sorry, the mines that were sown off Hanoi. One of those was a pressure mine, and I was the only man that ever invented a sweep for a pressure mine.

KAPLAN:

And you invented this during the work with the Navy during the war?

ATANASOFF:

Yes. I haven't squired enough but I've squirmed around a bit in the world. You see, I invented this pressure mine and the Navy was so pleased with it that they gave me the highest recognition.

KAPLAN:

And probably classified it also?

ATANASOFF:

I'm not sure it isn't even classified today--I'm not even sure about that, but it was classified for many years. And this invention--it was not entirely successful, but nobody else had the remotest idea how to go at it and I did the best job that was done at the time. The Navy was so pleased that they gave me their highest award, which was a Distinguished Civilian Service. You see, it's still the highest award in the Navy, I think. I got that for this work.

KAPLAN:

I think it's the highest that a civilian can get anyway.

ATANASOFF:

Yes, it's the highest civilian award of the Navy or the Armed Service, I think. The results are, Bonnie, that then I had this period when I was in charge of the Navy's Fuze Program, and then there was reorganization at the Naval Ordnance Laboratory and I saw that the time had come when I had to change my pattern of life if I was going to change it. So I decided to incorporate and form a company of my own and go into business. You realize that if you're a college professor your output is publications, but if you're employed in the Navy your output is ideas and honors. It's very important to get honors if you're in the Navy. If you go into business then your output is what?

KAPLAN:

Well, you want to earn money but you usually do that through ideas.

ATANASOFF:

Money, money. You want to turn the ideas into money. So I got into this and I didn't do phenomenally well, but I made myself independent during this period. I didn't do phenomenally well; many people have done better.

KAPLAN:

Was your motivation financial?

ATANASOFF:

Money, money, sure it was.

ALICE ATANASOFF:

Also you were committed to all your friends who had come with you and you were responsible for protecting their money as well as your own.

ATANASOFF:

Yes, you see, they all came in. I invested two-thirds of the money out of my own pocket. They had quit their jobs. You know, there is a strange mixture of people in the world and some people have a great affinity for me and some people have a great antipathy for me. There were enough of them that had affinity for me so my friends flocked in and put in a third of the capital. Then I had to make the whole thing work and I had to convert myself not only from a scientist to a salesman. So I had to get right out and sell million dollar projects in order to make any money.

KAPLAN:

What kind of projects were these?

ATANASOFF:

Research and development projects for the Armed Services to begin with. And then later I was converting the company. Then I had gotten the company going pretty well and sold it out for a fairly large sum of money to Aerojet General of California.

KAPLAN:

You made a company so that you would sell ideas essentially to the Armed Forces, so that you were doing what you did before only now you were doing it privately.

ATANASOFF:

Yes,. I just worked night and day and then--everybody was happy. Everybody that put money into my business was happy. There was no question of any unhappiness on that score. So I succeeded in that, and consequently it made me so I could eat now. I don't have to worry about the future; I can eat and sleep. I didn't do much better than--oh, you know, a doctor if he works hard all of his life, he can make a million dollars and I did more or less like a doctor.

ALICE ATANASOFF:

You didn't quite make it.

KAPLAN:

I don't think a doctor could quite make a million dollars either.

ATANASOFF:

I didn't have a lifetime at this; I just had 4 or 5 years to do this. So that made me relatively independent.

KAPLAN:

I don't know if this is a fair question to ask, but were you a very good salesman?

ATANASOFF:

Some people say I'm very good. I haven't been universally good, but some people said I was the best salesman (in sight?). Sometimes I was, too.

KAPLAN:

I don't know if you're going to want to continue over to the other side of the tape. That's up to you, but if not then I have one or two really short questions and we can finish this little bit here.

ATANASOFF:

Oh, there's lots more tape here.

KAPLAN:

Is there? It looked like just a little. But in the transcripts you were talking about, for your counter and memory elements, that you had considered using magnetic elements and many, many times the court recorder wrote down "thoroughmagnetic".

ATANASOFF:

It means Ferromagnetics.

KAPLAN:

That's what I was wondering, if you really meant ferro, f-e-r-r-o-.

ATANASOFF:

Yes, I've written in--I don't know why your copy doesn't have it. I can furnish you with a list of corrections which I've furnished to them.

KAPLAN:

I don't know if we have them, but if we don't –

ATANASOFF:

If you don't have them just let me know. I have an official list which is filed with the court, correcting all those things.

ALICE ATANASOFF:

By page number and line number.

KAPLAN:

That would be good to have.

ATANASOFF:

Yes, but there are many other corrections in that testimony.

KAPLAN:

That was something that puzzled me because I'd never heard of thoroughmagnetic.

Another very short one is, in some of the early things I read from your taped transcript there's a name that appears that's spelled as G-r-o-s-c-h and he's the person at Iowa State. –

ATANASOFF:

George Grosch--you spell it wrong. G-r-o-double s.

KAPLAN:

The transcript people spelled it wrong. I just wanted to check if he was really the same person as George Gross.

ATANASOFF:

Yes, George Gross, yes.

KAPLAN:

I want to look and see if I have anything else that I really wanted to get to today.

I'm really interested in science fiction and maybe this question will be somewhat colored by it. I know that Gernsbach's early magazine, before he started publishing a science fiction magazine, he occasionally would go off into speculation and eventually just put them all in a science fiction magazine. When you were reading Gernsbach did these speculations impress you at all?

ATANASOFF:

Well, you know that's very strange, that I never had anything to do with his science fiction. I never came in contact with it. I came in contact with An Electrical Experiment, I believe was the name of the first one that I came in contact with, and I learned some elementary electronic theory from his publications. Gernsbach really is a man who deserves a great deal of credit for beginning this publication of popularized science in the United States. He is historically a very great figure. You know his family are still publishing one publication and it's called to this very day--I got a long distance call from a man and he told me he was from the Gernsbach Publishing Company and he wanted me to subscribe to—

ALICE ATANASOFF:

Was this electronic World?

ATANASOFF:

No, not Electronicss World, that's not the name. I don't know – the name of it escapes me. I take it right now. He called me long distance so I commenced talking with him and I told him I had been in contact with the Gernsbach outfit for many, many years, erratically perhaps but nevertheless covering a great many years. He told me, this salesman had known Mr. Gernsbach himself, Hugo Gernsbach, and that his son is now President of the company and they still publish in this field with just one publication and I believe, I imagine they're near bankruptcy. I don't know; I imagine that they're slowly going to get phased out. The world changes and men come and men go, you know.

KAPLAN:

At least he's getting –

ATANASOFF:

But he asked me to come by in New York and visit this publication. He said the son would like to live over old times with me and maybe I'll do it some time when I'm up there.

KAPLAN:

At least Gernsbach is getting some sort of recognition now in that the science fiction libraries give Hugo awards every year.

ATANASOFF:

Yes, he was the first writer of science fiction, but you know I never read science fiction. You know I never have and I've tried but you know directly I come up to an unrealistic part of it and then I'll cast it all aside. I just can't stand diverting from realism. There's always a diversion, deviation from realism and I can't stand it. So I've never been even entertained by science fiction. I realize that science fiction has often been pretty much like reality.

KAPLAN:

... About Clifford Berry again. What was his motivation in getting involved with you and then continuing to work with you. Was it at first strictly just to have a job while he was going to school?

ATANASOFF:

Well, you know, he and I had common interests and we had a fine time.

KAPLAN:

Before he started working with you –

ATANASOFF:

He had to do something. He was a graduate. Jobs weren't particularly plentiful, but here was a job, it didn't pay much of anything, but it was a job at which he could get a Master's degree, in which he could advance himself in the art, and without doubt he did. Besides that, he was interested.

KAPLAN:

But he didn't know anything about it before?

ATANASOFF:

About computing machines?

KAPLAN:

About computers

ATANASOFF:

Oh, no. You see it was electronic, and he was interested in electronics.

KAPLAN:

Is that mainly why--he continued to work on it then mainly because he was interested once he got started?

ATANASOFF:

Interest is more important than money in getting people to work on things. Much more important than money. You've got to sell interest or else you're not going to make progress.

KAPLAN:

I'm curious again about the emotion with the tubes and whether or not they'd really work. Were your doubts only because you were unsure about the variation in the parameters, or was there some sort of feeling about tubes and electricity and electronics?

ATANASOFF:

You know, all of what you say is contained in--well, when I finally got a full formulation and grasp of the primary concept, then everything else, all the other doubts that I had were epitomized in these words, in these words in regard to various parameters--all the mysticism and esoteric eventualities which might have prevented vacuum tubes were all exactly described by the variation of parameters. I knew all I had to do was to control the parameters and then they would work.

KAPLAN:

Was that the origin of your mysticism in the first place?

ATANASOFF:

Oh, mysticism is a characterization of an unknown quantity, isn't it? When things are

unknown they're mystical and when they are known they are no longer mystical.

KAPLAN:

Are you saying that the notion of using tubes was so new that you couldn't quite –

ATANASOFF:

Sure, you couldn't understand it. I didn't have any formulation of how it would go. You know I didn't to begin with. You have to start.

KAPLAN:

Oh, but that tubes were possible to use in electric circuits and they could do all sorts of miraculous things in radio.

ATANASOFF:

You're speaking from--even you yourself have concepts in connection--you're not an expert in vacuum tubes but you already have certain attitudes towards vacuum tubes which would have appeared very strange and unknown at that time.

KAPLAN:

I'm trying to determine what was the attitude toward vacuum tubes, say, in 1935 or sometime around when you were thinking about them. Were they still considered marvelous, mystical devices?

ATANASOFF:

They were. Yes. I think so. I think, of course, we had books on vacuum tubes and I could see exactly how the parameters of vacuum tube varied, but you see I was starting in a new application of vacuum tubes and how this was going to work out in practice – and I had to get a -- A man proceeds by making some mental images of things and the strongest mental image which gave me strength and courage and certainty was this one about the parameters which I described to you. When you formulate a theory and the parts fit, why then you can extrapolate from this theory and then you are able to predict that in the future vacuum tubes will work in this given way. That's what a theory is about and if you don't formulate a theory you can't.

ALICE ATANASOFF:

Is it true that electronic was not used until later; that it was electrically?

ATANASOFF:

We had radios then.

KAPLAN:

What were you feelings towards radio?

ATANASOFF:

I understood exactly how they worked.

KAPLAN:

Were there any kinds of mystical questions in your mind about radios?

ATANASOFF:

You're pursuing the mystical. I'm pointing out to you that the mystical is the state of a man's mind when he's working on something and doesn't exactly understand it. You know, take Jehovah--Jehovah smiting the rock. Now do you have a mystical feeling about Jehovah smiting the rock?

KAPLAN:

Yes.

ATANASOFF:

That's because you don't understand it, isn't it? You don't understand the mechanism by which Jehovah smited the rock and the rock issued forth the water. You don't understand that, do you?

KAPLAN:

You presumably knew how vacuum tubes worked and why they worked and how you could --

ATANASOFF:

Yes, but I didn't know how they worked in computing machines and I didn't know whether they would work in computing machines, therefore I said I had a mystical feeling about that.

KAPLAN:

Was that not knowing how they would work only because you didn't know the variations in the parameters?

ATANASOFF:

That was because I hadn't even formulated the parameters theory. But after I formulated the parameters theory and I knew the dynamics of the vacuum tubes, then I studied a book by Vandervijl -- v-i-j-l --which was one of the early books on vacuum tube theory. I think I'd studied that at that time and I read the Radio Amateurs Handbook at that time and so I understood pretty well how vacuum tubes worked.

KAPLAN:

Were the main applications for vacuum tubes at that time in radio?

ATANASOFF:

Sure.

KAPLAN:

Were they being used for other things too?

ATANASOFF:

Yes, for amplifiers, for amplifiers for telephony and for radio, and that's about all.

KAPLAN:

Were they used for switching circuits for the telephone?

ATANASOFF:

No, no. You know, a switching circuit is another name for logic circuits and I invented the logic circuit, and I did it at this time. I won't say there were no applications in which vacuum tubes switched before that time. Of course that isn't true, but speaking in the large why I was just getting ready at this time or had just finished inventing logic circuits.

KAPLAN:

Did you know of the other switching applications that were being done?

ATANASOFF:

I can't remember any emphasis on any of them, but then the fact that you could do some

switching with them was pretty obvious.

KAPLAN:

Assuming you already knew something about them.

ATANASOFF:

[laugh] I wonder what Henry Troop is going to think when he reads this. Are you going to make him listen to this tape?

KAPLAN:

I'm sure he'll want to.

ATANASOFF:

[LAUGHTER] Have you come to a stopping place directly?

KAPLAN:

How about right now? Would that be all right?

ATANASOFF:

This is good; we'll go on from here. And you come up and visit me, huh?

KAPLAN:

Oh, I'd love to, and I really do want to know how the machine works.

[End of Interview]