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

This is a continuation of discussions with Dr. J. V. Atanasoff. The date: Today is the 24th of April l972.

ATANASOFF:

This is J. V. Atanasoff on the -- on April 24, l972 at the Smithsonian Institution continuing my interview on my personal history, and on the work which I have done on electronic digital computers in the last work, in the last dictation, I had described in some detail the work which I was doing at Iowa State College, now University, and the efforts that I was making to enlarge my computing effort. I got myself in rather a cold sock. It seemed that every thing I wanted to do depended upon more powerful computing means, and, specifically, these more powerful computing means related to the solution of large systems of linear, algebraic equations. The reason these equations were linear is because they were derived from linear operational equations -- linear differential and linear integral equations, and other types of linear operational equations. The method of deriving these equations from the algebraic equations to infinite algebraic system of equations from the infinite algebra which corresponded to the operational equations had been a study of some of my staff and of myself. Mr. Gross and I had worked extensively in this field and we had many methods, of not only the original Rayleigh-Ritz, but many other methods of deriving algebraic systems from—

TROPP:

It sounds as though we were talking about this last time. Essentially you were talking about a system of equations where you had ten number of unknowns and equations, and then in some cases it was infinite. In terms of computational needs you were talking about the solution of a sub-finite system of this with, in most cases, the same number of equations as unknowns.

ATANASOFF:

That's right.

TROPP:

We repeated that—

ATANASOFF:

You see, in some cases, of course, these differential equations, operational equations -- or operational equations do have exact solutions, but in most cases no exact solution is known in terms of any known functions. You must realize that the number of known functions -- number of functions which are known to man are very limited and must ever remain so, and that by this time we already saw that this was the situation and that it would not be possible to proceed by developing new functions to solve new differential equations and then use new infinite processes for deriving these functions, but that we would have to go directly from past -- directly from the differential equation to the depiction of the solution, which is a function of unknown character. These unknown functions and their characteristics, of course, have been the subject of much study, both before the years in which we were working, and since. And their approximation, of course, always requires an infinite process. The infinite process which we were prepared to use is the following, in reality, it represented the solution of a certain finite number of linear algebraic equations which is a sub which is a portion, a subset, of the infinite algebra which is depicted accurately by the differential equation.

Then we solved this system, then we solved the larger one and if the two solutions were approximately the same, and if they passed other tests, they were considered a satisfactory solution. We had many ways of knowing whether they were satisfactory, but nevertheless this process of whether they were satisfactory or not represented our greatest bar, our greatest obstruction, our greatest lack of rigor in the whole method. The proper handling of this situation required that we develop equations which represented the margins of error of these solutions obtained in this infinite process -- the process of successive approximation in terms of larger and larger systems of linear algebraic equations. And these limits of error, who were not known well in that day, and I believe that there are more marked deficiencies in this area remaining to this very day.

There are several different attitudes which could be taken towards this problem. One of them is that in view of the enormously increased computing capacity of modern machines, that the exact rate of convergence does not matter, but the very simplest considerations show the error in such assumptions. Now problems are easy to devise that will confound the computing capacity of the largest machines ever invented.

What we're doing here is working on an arc. Too much emphasis -- except in a routine way -- too much emphasis cannot be laid on the skills that the man who plans the operations of the machine, and programs these operations into actual machine operations, possess and there's no escape from this. These facts were known to me from the earliest day and I see no escape from them today. We may add to these words a statement that, of course, not only the greatest mathematical skill, the skill is needed for these operations, but also the greatest of physical realizations of the facts of the case, so that they are not incorrectly depicted as the mathematics is programmed.

Up to this stage you can see that I have been working on systems of equations -- systems of algebraic equations -- from the computing point of view. We introduced our problems to these systems, and the systems were the problem. We have stated previously the enormous -- the rapid way that the work of solving systems increases as the number of equations goes up. And in those days all systems of such equations were practically solved by methods of elimination and substitution, which are commonly known, and which were generally executed by such computing machines as [a] Monroe or a Comptometer, or something of this kind.

Now, this pressure on me in regard to such systems of equations was enormous. And I had made several abortive attempts to get at these solutions. One of them was recorded as an effort to make an IBM tabulator solve systems of equations. Another was recorded in terms of coupled Monroes, coupled in a special way. In a way, these efforts were a total loss, because a clear examination of the results showed that neither of these machines possessed the computing capacity necessary for an effective resolution of practical problems.

TROPP:

That was primarily the -- the reason that these didn't work, I gather, was primarily because of the tab equipment or the serial hookup of mechanical equipment's inability to handle a system large enough to make the process worthwhile, you would just have to have redesigned the tab equipment, or had to have had so many Monroes that it was prohibitive.

ATANASOFF:

Yes, I think that's right. You see, as a matter—

TROPP:

Theoretically it was possible, but practically it was beyond the realm of capa-- of capability.

ATANASOFF:

You see, as a matter of fact, neighbor of these systems was actually, physically constructed. They were arm chair experiments in which I just visualized the various methods, making drawings, perhaps. I have in my files some drawings relative to this period, and, however, the real problem was in the total computing capacity.

Now, these efforts were not a total loss, because they did serve to develop the logic and the rationale by which the machine would work, which would solve such equations if it had sufficient computing capacity, and this matter never changed. The last machines on

which I worked employed methods for the solution of the systems of linear algebraic equations which were quite similar, only with small modifications, to the ones which would arrive during this period when these abortive efforts to secure these operations in terms of Monroe's and IBM Tabulators.

TROPP:

If you can, although we can check the files, I've got the folder of some of your sketches and notes, but do you remember, roughly, the approximate date when you were beginning to think about using conventional equipment and trying to adapt it to your needs?

ATANASOFF:

I could give a date better if I did the research through my files, however, it was, roughly speaking, in the years l935 and '36. At this point -- I have been accused of building a single-purpose machine. At this point, really, I turned away from this single purpose, that is, the solution of equations. The format, or the program for solving systems of equations, was then known to me. It was a problem of computation, and computational means, and getting cheaper computational means that was before me.

TROPP:

By cheaper you mean cheaper in terms of man-effort and man-time?

ATANASOFF:

Yes. Cheaper in terms of -- cheaper in terms of dollars and cents per computing ability, because the – you could imagine compounding even these crude machines of the day into large enough structures to solve these systems that I wished to solve, but it was astron – it was commencing to being entirely beyond the means of any institution with which I had any associations, to pass into such applications.

The problem was how much it cost you to do a multiplication, or a division; how much it cost you to construct the machine, the capital costs and the operating speed. All these were factors in the problem that I was solving. So, you might say that at this stage I turned aside from the special purpose machine, and entered directly into computational means, and basic devices for computing. It's true I always held in the back of my mind their eventual application to the approximate solution of partial differential equations, and hence to the solution of linear -- of limited linear systems, but I knew that the crux of the whole situation was in the computing element itself, and I commenced to examine all computing machines that had been constructed, the structure of them, and to commence to devise new computing structures. And—

TROPP:

Let me back up a minute, here.

ATANASOFF:

Go ahead.

TROPP:

When you say you investigated all computing machines that had been constructed, were you -- are you talking primarily about the ones that you saw in the statistical laboratories or the offices of Iowa State College, or were you also reading the literature in terms of the historical development of mechanical calculating equipment—

ATANASOFF:

I was also reading literature, if it was available to me. You know, I didn't have the resources of the Library of Congress at hand, but I had a number of pieces of written work. I had all the standard encyclopedias which had covered the subject in a rather systematic way, and I had some other books which covered some of the elements in a simple way. We were -- realized that I could not go into every detail of every machine. Some of them, from their avowed purpose, were inconsequential for my purposes, but I was looking at the guts of these machines. The primitive computational elements, and, of course, among other machines I examined the abacus, perhaps being the first such systematic machine. And my study of the abacus left its imprint on the early writings on the subject, because I used the word "abacus" to describe the word that other people -- the describe the element that other people would ordinarily call counters. I was -- my manuscript says abaci instead of counters. I thought that abaci was better, and I still believe it is, although it never caught on and I guess I was the only writer on the subject that ever used it. Even my assistant, Clifford Berry, did not like the word. I liked abaci better, because I realized that counters would not be -- would not be rotating discs, which the counters up to that day had, and I wanted to choose a neutral word, which didn't carry this connotation of rotation. Rotational counters looked and proved to be reasonable for the base ten number system, but almost at once I was examining -- I had commenced to examine the use of other number systems for computing means. And this examination of other number computing machines took me some months.

TROPP:

The question that automatically comes to mind is: Why bother with other number bases? We've grown up with the base ten; it seems to work for everything we need. How did you come to even think of other number bases?

ATANASOFF:

Well, I will attempt to give you as clear a picture of this as I can. It turned out that I was

interested in numbers and number systems. and systems of measure, like bushels, and pecks, and quarts, and pints, and pounds, and tons, and things of that kind at a very early age. I realized that -- that these systems of measures carried something like another number system into the picture. I commenced to realize this on my own. And then I discovered a book in my mother's library, a book that I now believe to be Robinson's *Arithmetic*, which covers in its most advanced stages other number systems. Now –

TROPP:

Like the binary, and quinary, and duodecimal, or—

ATANASOFF:

Other number systems. And I believe not binary. I believe it did not cover the binary. But it didn't matter, because once you gave me the idea I could extend it fast enough, even in those days. And this book -- you know how school was taught in the early days in America? A book on arithmetic would not be sharply cut off to one year, but would tend to represent within its covers a great range according to the interests and demands of the times. It would tend to cover a great range of interests beyond those which were necessary for a year's instruction. Well, Robinson's *Arithmetic*, if this is a correct name – and this is a name that my mother claims is correct for that book. My mother still lives at this instance, and within the last year has told me it was Robinson's *Arithmetic*.

Robinson's *Arithmetic* showed you how to develop number systems to other than base ten, and, in particular, it espoused the use of base twelve number system. And I remember that it added -- had to add two digits, eleven and twelve, and it used an "e" for the eleven and a "t" for the twelve. And -- oh no, a "t" for the ten and "e" for eleven. And the twelve, of course, passed into the one zero area. And then I had one other little hint in regard to the matter; at this stage I had studied some advanced mathematics and in a book on the functions of a real variable by Hobson certain theoretical proofs were given in which he employed the base two number system. And, of course, in terms of what I'd already studied in Robinson's, the base two number system immediately became part and parcel of my bag of tricks.

TROPP:

You're saying, essentially, that Hobson -- and I'm, although I know Hobson's work, I can't remember this was using a logical calculus of an "either-or," "and," "or" kind of basis, where essentially you had two--

ATANASOFF:

Yes. It turned out that you could have given the proof in any number system, but the proof was much easier to give. I forget the proof, I forget what it proved. I have not bothered to look it up, but I believe it is in Hobson. It was either in Hobson's book on the

theory of real variables, or else it was in some reading which is associated with the course that I happened upon this use of the base two number system, and this was the -- and these two items furnished the basis for my general knowledge of number systems. So that I was equipped to think clearly and easily in other number systems than ten. At that instant I did not know which way to go.

At this stage I did not know which way to turn, because I had no knowledge that anyone else had ever thought of developing computing machines in other bases than ten. My first eff -- my first thoughts were to increase the base. There are various reasons for this. Increasing the base would decrease the number of significant figures which were necessary for a number of a given size, And, too, there was an irrelevant fact, a rather irrelevant fact, that it is perfectly easy for a man to memorize multiplication tables much higher than by ten, and as far as mental arithmetic goes, this would speed a man up. It's clear that if we had a sixteen by sixteen -- a base sixteen or a base twenty-five, or a base twenty, or something of that kind, a man could easily memorize the multiplication tables and the addition tables, and the work would progress much more rapidly if mental calculation were the problem.

These false senses did not last long because it soon became apparent to me that if I wished to simplify computing machines and duplicate them simply I needed not a machine that would memorize the multiplication table for twenty-five times twenty-three, or some such number, but a machine in which the multiplication table was a mere nothing, a mere element -- a mere bit of logic of the simplest kind. And this shoved me rapidly downward from the base ten to lower bases. I knew that I was getting into trouble by this change, as I moved away from ten. I knew there would be great resistance to the change from base ten. I knew that changing from base ten would probably be harder than changing from the English to the Metric System, or from the Julian calendar to a good modern calendar of design to simplify the life of man. I worried about these -- about this fact for many weeks.

I finally rationalized the matter in the following way: that there might well be two number systems in use in the world, the base two number system and the base ten number system. A translation from one to the other would not be too difficult and special machines could be perfected for this purpose, but that the base two number systems would be used for studying basic scientific questions where reference -- where continual reference to the base ten number system would not be necessary, while the ten -- while we would have other computing machines which worked on the base ten number systems, which would be used for such subjects as bookkeeping and banking.

This gave me the impetus to examine more critically exactly what lower base should be used, and I attempted to work out some mathematical treatments of how -- of the speed with which computing machines would work at various bases. I remember one system of analysis that I used concerned the speed of multiplication. And when I had solved this problem for the base which furnished the highest speed of multiplication, I came out with the base "e". Well, the base "e" didn't look like a very good base for a number system,

and I had the choice between two and three. And examination showed that two and three had equal speeds, and, hence, I chose two, because it represented the simplest mechanical structure. And I had thus reached a momentous decision that from this point on my computation would be in terms of the base two.

TROPP:

It's interesting that you came to the decision to use the base two, or in this case the base three, either one, on the basis of speed rather than the basis of technology, which is a different kind of decision than the one which later resulted in the use of the base two in high speed computational equipment, because it was the on-off states of elements like relays or tubes that were the determining factor of the base two. And it's interesting that you did a speed analysis for the determinations long before any kind of mechanical or hard-ware is coming into the picture.

ATANASOFF:

Well, this is not exactly right, because already were being formulated in my mind the various kinds of, what I called "abaci" which would be used for the various bases. And the abaci were the counters, and the counters were the "on-off" devices, as you call them, in the base two, but they're "on," "medium," and "high" in the base three. And I had at hand abaci for either. It is -- it was partly the abaci that led to the conclusion that I should use base two because an abacus for base two was simpler in structure than the abacus for base one, and when I've said this I have said the same thing that you said.

Even after I decided that the base two was probably the logical system to be used for theoretical or for scientific purposes, I continued to carry and study the possibilities of base three and even base ten. And, in terms of base ten, I attempted to resolve the base ten into a base five and a base two in multiple to simplify, so the logic -- so I could use the logic of the base two system in constructing a system of the base ten. I, ever a fearful man, I was fearful that I would make a mistake at this juncture. The pressures for base two were high, but I felt the pressures of society for other bases, and it was literally months before I shook off this concern and turned resolutely towards the base two.

TROPP:

Uh, in your conversations with colleagues at Iowa State, were you finding other people who were interested in the same problems, who had thought about it, that you could talk to?

ATANASOFF:

It is a very strange situation that at Iowa State College as I worked on this computing machine I had no one to talk to except my own graduate students. I have -- I attempted to have many conversations with other members of the faculty. Dr. Allen, who was a good

theoretical man in mathematics, was never interested in mechanisms, and he always had some suspicion that mechanisms were not quite pure. And I remember another member of the Physics Department with whom I had a conversation, and he says, "Atanasoff, computing machines are no real good, they will never drive a streetcar."

TROPP:

Don't turn that off yet, I want to ask you about your friend A. E. Brandt; did you have conversations with him about it?

ATANASOFF:

By this time A. E. Brandt had left Iowa State University. You know A. E. Brandt, what he would have done? He would have encouraged me and understood nothing. He would certainly encourage me and he would have understood nothing. These -- this realm of thought was rapidly rising above the level to which Mr. Brandt had attained -- attained. Mr. Brandt had a peculiar bent. He understood the elements of statistics, and he interpreted those for common, everyday people who did not. He was always friendly to them. He always put the interpretation in a friendly way, and to this day -- this very day, he is down at the University of Florida doing exactly that, for exactly those kind of people and he is really rather successful in this field. Now you remember that he and I worked together on this -- on a couple of projects previously recorded here. Now his role was to go around and cajole the IBM representatives and salesmen so that they wouldn't absolutely block my progress in the construction of the machine.

I in no wise wish to do Mr. -- Dr. A. E. Brandt an injustice. When you were working on a project with him you, perhaps, got more help and joy out of his presence than those of most men. And it's a very strange -- it's a very strange state of affairs. He was always willing, he was always helpful, he would supply things which at times seemed beyond his normal understanding, and I have never heard of anyone that ever worked with Dr. A. E. Brandt who found it other than a joy. Gradually, my mind was firming in the direction of he base two, however, because I saw that in the solution of differential equations the base ten number system didn't enter very extensively. There would only be one or two numbers at the end, or at most a few which had to be translated from the base two to the base ten system, but enormous amounts of calculation that had to be made in the intermediate steps. And so a machine which could rapidly and cheaply make calculations in the base two would be of enormous benefits in these applications, and, as I could easily see, in many others. These were -- I should go back again to the abaci. The abaci for the subject were all around me. Do you know the little thing -- the little device that clicks when you press on a little spring? I call them clickers. They're used sometimes for notifying -- for a lecturer to use to notify the man who is operating the slide machine when to change the slides. They're used by children, and there also is a kind of clicker which is made, which
when warmed in the hands can be laid on the table and when it has changed its temperature a certain amount it will click and jump high into the air. And these are

devices with two states. All you need is two states for my abaci, and a clicker had two states. I had other clickers in mind; some of them mechanical: a little toggle joint that could swivel from position A to position B, the positions between A and B being eliminated by a spin mechanism, so that there wouldn't be any half-way measure about it. It's either going to be "A" or "B".

I was thinking about magnetic elements. Little pieces of magnetic material which when polarized in one direction would represent the number "1" -- one of the states; and when polarized in the opposite direction would represent the number "O" -- the other state. We have passed, now, to using the designation zero and one for the two states of the abaci, and we will continue this -- this language throughout the discussion. This magnetic abacus -- we would now call it a magnetic memory -- has since those days achieved a high level of success in practical computing machines. It did not -- I did not choose this for the construction of my first machine, because the mechanisms which permitted the connection of this state into the vacuum tube circuits with which I had resolved to accompany the device were not as simple as one other which came to mind.

In the last paragraph I have mentioned vacuum tubes. Vacuum tubes in this connection should not be given such a casual introduction. I should say more about why my mind was turned to vacuum tubes for computing machines. I realized I could build base two machines without the use of vacuum tubes. I, however, was faced with the fact that I – that in order to get cheap computation I would not only have to obtain mechanisms that were cheap in capital cost, but extremely rapid in operation, because the cost per computation depends upon the speed of operation. And I was, at that stage, very familiar with vacuum tube techniques, and it commenced to play a larger and larger role in my thinking. This—

TROPP:

Had the idea of relays ... come to your attention as—

ATANASOFF:

Yes, I even explored at some length relays with two states, on and off. And these came to my attention, but I realized that if I could get the right kind of an abacus and the right – and couple that with the right vacuum tube circuits that speeds which could be achieved were far beyond those which were available with relays. I also felt that they were probably more reliable, because relays historically have contact troubles. It is true that today the Bell Telephone System has reduced the relay to a rather reliable device, but in those days telephone exchanges and the like did not have such a good reputation.

The argument between tubes and relays is worth one or two more remarks. I felt that a relay had -- could suddenly fail due to a bit of dust resting on one of its contacts. I knew that tubes did not have a good life, at that stage, in those years, although the life was -- the mean life of most tubes was, perhaps, two years of continuous service. I realized that

tubes ... I realized that tubes would continually change their characteristics -- would slowly change their characteristics as they lost emission, or as other factors slowly changed -- made changes in the primaries which controlled the characteristics of the tubes. Nevertheless, the radios which were in common use in these years were very reliable devices. They were -- they had been in existence for ten years, and they had been perfected, and when they failed it was usually due to some element extraneous of the vacuum tube. I realized that I was greatly worried by the fact that the vacuum tube characteristics would slowly drift, and I wondered how this was related to the structure of a mechanical computing machine. My mind revolved around such considerations.

Finally, I came to the following specie of argument: A computing machine proper, as I Called a digital computing machine in those days, is as follows: If you have a computing machine made of gears and you slowly let some parameter change such as a bearing \ wear, that the machine would continue to compute accurately until too much wear had taken place and then it would suddenly cease to operate or operate at all well. It would fail in a catastrophe, as it were. This is different from the kind of failure that occurs in analog machines, where as soon as any change in characteristics of the elements of a mechanism take place, the accuracy of the computing machine commences to fail, and will fail gradually with variations in the parameters which describe the tolerance of the elements of the total mechanism.

I thus came to the conclusion that vacuum tubes in a computing machine would work within a certain range and variety of, let's say, the filament or heater emission. As long as the emission was within these limits, it would work and then there would be a sudden and catastrophic failure of the device. And I was making calculations to see if a machine built with a number of vacuum tubes could be practical. You know, besides this there was a mystical element in the whole matter, which I well remember. The mystical element went like this: Is it possible that you can take a thing which has always been built with gears and metal and make it work with a vacuum tube? A vacuum tube, that is nothing but an envelope filled up with streams of electrons? And this mystical element possessed me, and possessed me to a considerable degree. I have always felt that I was free of such mystical elements. And I believe to this very day that I'm freer than the average man is of such affects, but this mystical element possessed me and possessed me strongly. It possessed me until the very day I saw my first electronic digital computing machine working. Part of my resolve to employ the base two number system and my explorations in connections with the various kinds of abaci or on-and-off elements which would represent the two states, zero and one, of this number system, I was a long ways from a computing machine. I realized that these elements would have to interact with many other elements in order to do computations, and I was simultaneously exploring these possibilities. I have stated that my inclination towards the use of vacuum tubes, and electronics, for devising these other elements -- I was doing this in spite of my mystical fear that vacuum tubes would never -- would never prove to be a satisfactory element for computing machines, because I felt that if this were not true, that if one could not do it with vacuum tubes, man was against an impasse beyond which he could not prevail and that a dead end would occur in man's efforts at more and more complex computation.

There are various elements or structures which came to mind, and one of these elements was naturally a scale-of-two counter which may be thought of as a very rudimentary computing machine. The scale-of-two counter was in common use in those days, and the application that I remember was in counting cosmic rays, or the impulses due to the particulate portions of cosmic rays. These scale-of-two counters were made so they could count at rates up to hundreds of thousands of times per second, and I believe that a million times per second represented more or less the upper limit that was visualized for these devices at that time. That is my memory of the state of the art. Of course, I could see how such devices as this could be used in connection with a computing machine.

TROPP:

I'm not familiar with that -- I'm not at all familiar with this kind of counting device and I wonder if you could take a second to describe it?

ATANASOFF:

Well, it involved the formation of what we call a flip-flop. A flip-flop is usually involved in that day a flip-flop involved, and still usually does, two vacuum tubes, or, today, two transistors, connected together so that each feeds the other. Now, the device had two states and, hence, fell naturally into the hierarchy that we were envisioning. Either the first vacuum tube was conducting and the second vacuum tube was cut off, or the reverse is true. This device was commonly called a flip-flop, in analogy with a pancake which could have side A up or side B up.

TROPP:

I guess I'm—

ATANASOFF:

Then, in order to count at higher rates, we put a series of these -- we put a number of these in series so that the -- that when one flopped it would cause the other to change to either the flip or the flop position the next one. And so on and so on down the chain, so that at each stage the number of impulses is divided by two, and by recording the state of each of these devices, the exact number of flips or impulses which had entered the input of the device could be ascertained.

During these days we were building scale-of-two counters. I remember that we got them to work, but not very well, and not very reliably. I'm sure that in the hands of other observers they were much more reliable. I am not at all sure but what this lack of reliability of the scale-of-two counters in my laboratory diverted me and diverted our work into another channel of approach to the whole system of computation, and perhaps had even a lasting effect upon the history of computing machines.

It will be understood that during this entire period I was not working full time on computing machines; it could only occupy my spare hours. I had what was called a full schedule of work, comprising, perhaps, six to ten actual classroom work hours in graduate courses, not in undergraduate courses. Fifteen hours was more or less standard load for a man in undergraduate hours, where the courses were repetitious. But where continual study and reading and reference was necessary it was believed that six hours was really enough for a post-graduate instructor to teach. Six good classroom contacts. In addition to this, here was a member of various university committees, who had various purposes, which can be imagined around a university, and in addition I had these students who wanted to do theses, and for some reason, which I cannot fully understand, the students were flocking to me in increasing numbers, and so in the years between 1933 and 1942 I did twenty Master Degrees and, I believe, seven Ph.D. theses, and this was a very large load for any instructor anywhere.

So things were not moving too fast. Although this was a prime—a prime interest of mine, and I kept trying to get back to it and get as much work done on it as possible. But we have come to the year 1936 or 1937, and I'm working on such things as scale-of-two counters, and flip-flops, and off-and-ons and zeros-and-ones, and devices which would portray the two states of the scale-of-two number system, and the possible interactions between these devices and some other mechanisms that I had gradually formulated in my mind that if I can get fast devices which I called—which I called abaci—scale-of-two abaci of high rapidity, and then could use vacuum tubes to represent the other elements of the device I didn't know how in the world the whole thing would go together. I'd reached this state. And my graduate students were continually getting themselves hung up because of computational needs, and I was getting myself in an awful state, because of these demands and the fact that the graduate students could not make real progress because of this lack of computing facilities and the fact that they had to spend a considerable part—I had to allow them to spend a considerable part of their graduate career in operating a computing machine in order to get a job finished so that we could give them a degree.