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Interviewee: Arthur H. Dickinson
Interviewer: Henry S. Tropp
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TROPP:

This is a discussion with Mr. A. H. Dickinson and the date is March the eighth and we're in Portland, Oregon. Why don't we start, Mr. Dickinson, then with your own career and how you got into the inventing atmosphere at IBM.

DICKINSON:

Well, after I graduated from Union College in Schenectady I took graduate work at Massachusetts Institute of Technology and following that was on the staff of MIT for about two years.

TROPP:

Can we date those periods? For example, when where you at Union College? What was your degree in?

DICKINSON:

I was at Union College between 1924 and 1928. My degree is B.S. and B.E., and I was at MIT commencing in 1928, and the graduate degree was given in 1930 and that was a Master's degree in Fuel and Gas Engineering. I thought at the time I would be going into the public utility field, but that had collapsed due to the Depression, so I stayed on at MIT. The work at MIT was on an outside project for the Brockton Gas Light Company in Brockton, Massachusetts and it took about two years. It became obvious that at the end of that period my services would no longer be required, so I started looking for other work. One of the persons I contacted was James W. Bryce in IBM whom I had known since 1920, when he came to Binghamton, New York, for a short period of time and lived near our home and kept his car in our garage; and between the period of 1920 and 1932 when I was looking for work we had kept in touch with one another. I visited him occasionally and we exchanged Christmas cards. Mr. Bryce wrote back and said that he would be interested in having me come to New York and talking to me. I did go to New York and he asked me to come with IBM.

TROPP:

What was his position right here with them at that time?

DICKINSON:

Well, I think you would say he was chief scientific advisor to Mr. Thomas J. Watson, Sr.

TROPP:

Can I back up with just one question?

DICKINSON:

Sure.

TROPP:

That period at MIT is an interesting one because Vannevar Bush was very active at this point. People like Harold Hazen were there. Did you have any contact with that group?

DICKINSON:

Well, Vannevar Bush accepted me at MIT when I went there for an interview and I had no more contact with him or Hazen.

TROPP:

Did you have any contact with any of the other people in the Electrical Engineering Department?

DICKINSON:

Not really. So I had some vacation time coming from MIT, one month's accumulated vacation and that December of 1931 I spent on my own time in Endicott, New York, meeting different people whom Mr. Bryce sent me to and learning the product line of IBM, particularly the record controlled machines. The 601 multiplier had just been introduced into the line and fortunately I spent a great deal of time with that machine because it exemplified many machine principles, specifically concurrent read-in, sequential read-out, left-and right-hand components of partial products type of multiplying, and many switching circuits which were quite fundamental not only to that machine but to some of the other machines in the business.

So having spent that month I went to New York and became assistant to Mr. Bryce and started engineering some of his ideas and seeing to it that they were engineered in a way in which patent applications could be filed. This put me in contact with Mr. Harry Goss of the firm of Goss and Jamison, who made the patent drawings, so I learned a great deal about making patent drawings; and very little of Mr. Bryce's patent applications were handled by the IBM Patent Department, but rather by the law firm--patent law firm--of Cooper, Kerr, and Dunham, and specifically Mr. Felix Thomas.

The training that I got with Mr. Thomas was very beneficial because he... in preparing the applications it was necessary to spell out to him on a step by step basis the manner in which the various machines worked and so it was quite an experience in being able to... or getting to be able to spell out machine operations in a clear manner and on the basis that there was not too much confusion, and this went on for quite a number of years and... so... one of the things that occurred, I think sometime in possibly late '33 or early '34, Mr. Bryce asked me to accompany a salesman and go up to Columbia University and to the Pupin laboratory, specifically where I met Dr. Wallace J. Eckert and... Eckert and... Eckert then had a standard group of machines and was, as he explained to me, was performing calculations on the moon--that is, determining perturbations, as he called it, called them--of the moon. But he felt that this is... Dr. Eckert felt that if he could get some improvements in ours machines that he could better do his work.

At about this point the salesman bowed out because we very rapidly got into the mathematics of perturbations of the moon, which Dr. Eckert painstakingly explained to me very clearly and from his clear explanation it was possible to specify exactly what he wanted in our machines and... I'm not sure the department that was contacted in Endicott but the information with regard to the machine changes was conveyed to that department and in due course Dr. Eckert received the improved machines.

TROPP:

Now this is the equipment that I think we now have on exhibit at the Smithsonian.

DICKINSON:

It could well be. There was no so-called relay box at that time. The machines were not interconnected. The cards required in the process were carried from machine to machine. This is at first. Some time after that Dr. Eckert made known that he felt that the machines could be interconnected so that the information could be transferred more readily between the machines or the machines could be inter-controlled more readily and this led to the construction of his so-called relay box. I had nothing to do with this at all. I just knew of it. I saw it being installed by William Fitzpatrick, who was then head of Customer Engineering in New York and at that point my contacts with Eckert diminished until a later time in the business and... but at least that was the first time that I realized that our machines could be used for effecting mathematical computations.

TROPP:

I'm very interested in Wallace Eckert and I guess maybe we'll wait until we get to that later period, but any information, insight, knowledge you have of him would be most helpful to other work that I'm doing.

DICKINSON:

Well, Dr. Eckert in my book was not only brilliant but he was real human being. It was a pleasure always to be with him. We could and joke and yet get serious about things that he felt should be done and this lasted throughout the years that... of course he died just a couple of years ago. But he was a real person, a lot of fun, and had an extremely clear way of expressing what he was doing and what he wanted. So...

TROPP:

That was your first introduction to the fact that the IBM machines could be used in a much more versatile manner?

DICKINSON:

Very definitely, very definitely.

TROPP:

Specifically in the case of scientific calculation?

DICKINSON:

Yes, but of course there didn't seem to be much demand at that time for the machines to do scientific computing. That... that came a good many years later. If there was scientific computing done it was probably done in rather orthodox accounting steps, but... So I, from then on was very busy with Mr. Bryce in working out things with him. One of my first... I'd like to go back just a little bit. One of my first inventions was a change in the keypunch for Dr. Eckert, which made it possible to convert a card which had a true amount to a negative amount and indicate that in the card.

TROPP:

Do you remember when you were able to do that?

DICKINSON:

Well, I'd have to review the patent and I have not done that.

TROPP:

I see.

DICKINSON:

I mean, to really get into any of the details of my patents I would have to put in a great deal more time than I've been able to up to this point.

TROPP:

Well, yeah, but if you can remember what the particular patent is I can get a copy of the patent and do that myself; but we can get to that later, I can make a list of some of those patents.

DICKINSON:

OK, I can give you that. Well... I have broken up the work which I did in field of innovation into four rather arbitrary periods because of the kinds of things that were done during those periods.

TROPP:

I think that's very good way to start...

DICKINSON:

Now this is not referring to any patents in particular. It is possible to refer to particular patents but I would first just like to outline...

TROPP:

Yes, I think we can do the patents separately without the tape.

DICKINSON:

All right.

TROPP:

In terms of specific patents I think I can make a list of what the patents are.

DICKINSON:

Well, without going into the details and the differences between one machine and another, let's take the period from 1932 to 1935 when a great deal of my effort was devoted to engineering of Mr. Bryce's ideas. One of the things that he was interested in was improving the things that the 601 multiplier could do. Consequently a large number of different types of machines were, applying multiplying but not applying plus adding or subtracting, were devised by him. I guess I was joint inventor on some of them but a large number of patent applications... different machines were designed and patent applications applied for during that period.

TROPP:

Well, then your prime concern then was for improving arithmetic operations on a variety of machines, not just the 601?

DICKINSON:

No, I'm sticking pretty close to the 601 multiplier.

TROPP:

The 601.

DICKINSON:

That's right. Now, Mr. Bryce felt that the record card was not the only medium that data could be recorded in and consequently devised a rather complete line of machines controlled by the continuous medium, film. There were machines to record on film; it means to handle film; it means to effectively sort film and to reproduce film.

Mr. Bryce realized that there was one more form of mathematical computation which IBM handled in a rather cumbersome manner--that is, by reciprocals on the multiplier. I'm referring specifically to that type of computation which is dividing--and he realized that multiplying perhaps occurred half as often as adding and subtracting and that dividing occurred maybe half as much as multiplying. Nevertheless he felt that we should give considerable thought to different methods of dividing. Consequently a rather large number of different types of machines--different types of dividing machines were thought out and patent applications were filed on them. Mr. Bryce felt that a separate dividing machine might not find support in the line of IBM's machines--that is, machines sold or rented to customers and he indicated that considerable thought should be given to seeing whether or not multiplying and dividing could be accomplished in the same machine and after some study it was found that common mechanisms could be employed to effect multiplying and then by changing the plug board these mechanisms could be rewired with respect to each other and dividing computations could be employed or realized in a common machine.

One of the schemes that came out of this work on combined multiplying-dividing machines was that called the all nines multiple-multiplicand or the all nine multiple divisor, and it was the most sophisticated method of multiplying and dividing that we had; and eventually it was incorporated in the Harvard machine as the multiplying-dividing method...

TROPP:

Is this the method that's kind of called the end-around because of the fact that you just change one of the digits until you finally change the one at the far left? Is that... some other...?

DICKINSON:

No, no, I think that must be come other... Well, let me give you an example of how it multiplied. Having read the two factors into the machine, the multiplier amount and the multiplicand amount, before any computation started all nine multiples of the multiplicand from one through nine were very quickly formed. Having formed such multiples then the machine was placed under control of the multiplier amount and two columns of the multiplier amount were handled in one machine cycle and one complete multiplication and product would go into one accumulator and the second multiplicand would go into another accumulator. Then this occurred until the complete multiplication by the multiplier amount had been completed. So, for example, if the multiplier amount comprised of eight denominational orders and there was a significant digit in each multiplier order, in four machine cycles the multiplication would be completed, because it worked on two columns of the multiplier at one time in one machine cycle.

Now, in dividing the on-line multiples of the divisor were quickly formed after the amounts were read into the machine. Then the on-line multiples of the divisor were first compared with the left-hand region of the dividend amount and the highest going multiple was then selected on the comparison cycle and on the following cycle subtracted from the dividend. Then... it was then followed another comparison cycle but with the remaining left-hand portion of the dividend and the highest going multiple of the divisor selected would then be subtracted. So if... so in two machine cycles a complete dividing operation was completed, in contrast to the standard--pretty much standard over-and-over addition, or over-and-over subtraction method of dividing.

Well that pretty much covers except for some miscellaneous machines or schemes that occurred during this period and I'm not sure that we want to get into those.

TROPP:

No. One of the things I would like to ask about at this point is about Mr. Bryce. I wonder if you would care to digress a little bit and talk about him, since you worked directly under him and he was involved, as the head of this section, in so many of the developments.

DICKINSON:

Well, Mr. Bryce formed the IBM Patent Department in 1926 and he was not head of the Patent Department but he resided in the Patent Department as sort of a consultant to the Patent Department and that is where we were physically located when I went into the business. So we were in close association of a daily basis with IBM patent attorneys. Now Mr. Bryce himself was very much of a gentleman. In my book he was an engineer's engineer. He was very kindly, very thoughtful. He was very shy except with those of us who were associated with him. I was the first to be associated with him on a long time basis. He had previously had other people working with him but they didn't seem to stay very long.

Mr. Bryce had really established the atmosphere. It was certainly an unhurried one. There were no meetings; there were no reports; there were no deadlines. It was just do the best you can as rapidly as you can and if you get into a bind on something, if you want to talk it over come in and I'll talk to you about it. Sometimes he would tell me to go sleep on it which meant, just forget it for a while and get off on something else, and that proved to be very helpful because it gave you a little chance to think about how you might get yourself out of the bind.

TROPP:

How large was the department when you first joined it, in terms of the engineers who were working on developmental ideas?

DICKINSON:

Well, we were the only ones...

TROPP:

Just the two of you?

DICKINSON:

Just the two of us. Now that is not to say that the other engineers in the business weren't also having ideas of their own, but we were the only ones at that time that really were dealing with... doing applied science, if you want to call it that, or applied engineering. We weren't out to enlarge that world's knowledge by doing fundamental work, for example, in physics, but we were interested in applying new instrumentalities to the business or doing things in different ways or doing things that the company might want to use in the future, but which we were not sure about at that time. But at least we were trying to lay a foundation in the form of patents and patent applications so that the company could move into areas or different areas than it was then in, or go into the same areas in different ways in the future. So of course it was fascinating to see these ideas coming Mr. Bryce. He had a very clear way of explaining what he wanted. He had a very clear way of preparing a fundamental drawing and many times he would think of more than one way of doing the same thing. He would ask me to work up both and he would then... sometimes he would have maybe three ways to do the same thing and I'd work up all three ways and they would be discussed with him and he might select one of them or he might combine them or he might just use two of them. In other words, he encouraged people--and that not only was myself but others who joined us later--to really think of not just one way of doing something but as many ways as we could of accomplishing something and...

Now, Mr. Bryce was very encouraging to me and in an atmosphere of that kind it was possible to have ideas and a lot of them at first were old and therefore not patentable, but he kept encouraging and said, "Well, sometime you'll make it," and, well, that's how it came about that it was just being in that atmosphere of finding encouragement. And of course as time went along one could perceive areas which needed attention that weren't being done and one could start to investigate and see what would come out. So... and he did that not only with me but with all of the other people who came with us at a somewhat later time. I mean, he was encouraging to everybody and... An interesting thing was that he very seldom unless he had an idea or wanted to discuss something with you privately would he call you into his office. He would come around and sit down and in other words, every day he was in he'd plow the field. He'd come around and see us and kid with us and he was interested in progress if there was any progress to report, but he never demanded to know how thing were going. He could sense that and again he would tell the others if they were having problem, and they confessed to him that they were having problems, he'd say, "Well, go sleep on it," and so...

TROPP:

Very low pressure atmosphere?

DICKINSON:

Extremely low. I like to tell this, that he sometimes went to Europe in the summertime and on the day before he would sail his going out of the office to go home was no different from any other day. There were never any instructions left as to what was to be done or not to be done. I guess he had the confidence in us that we would just go right along as if he were there, which we did.

Well, Mr. Bryce was inherently a very shy man. I've seen him walk into a crowded room and just sort of go to a far corner and stand with his hands folded in front of him or behind him. He was not too well known by particularly the salesman in the business. Of course he was known by the other engineers in the business and he was always very glad when, if myself or some of the others were invited to these meetings, if we would sort of go and talk to him, he'd sort of warm up and... but he was quite uncomfortable with large groups of people. He was not a member of any of the engineering societies and... but he certainly was a great thinker. He certainly was interested in all sorts of things out in the world and took great delight in talking about some of his reactions to things that were happening. [LAUGHTER]

TROPP:

I gather that he was very close to Mr. Watson, Sr. Is that...

DICKINSON:

Yes, Mr. Bryce was very close to Mr. Watson, Sr. Mr. Watson, Sr. delegated many different responsibilities outside of developing...

Mr. Watson, Sr. knew that Mr. Bryce would have ideas but he would assign Mr. Bryce, for example, to investigate special situations and to advise Mr. Watson as to whether they should be undertaken or not undertaken and quite specifically when IBM was approached by Howard Aiken, it was Mr. Bryce who interviewed Aiken--or rather Mr. Watson, Sr. as I understand it turned Aiken over to Mr. Bryce and it was Mr. Bryce who listened to what Aiken proposed and... so... and the recommendation was positive to Mr. Watson and that's why the company undertook to develop the Harvard machine, also known as the Mark I.

Well... yes, Mr. Bryce was very close to Mr. Watson, Sr. and I think their relationship was very strong because Mr. Bryce told me one time, he said, "If Mr. Watson, Sr. tells me to go do something, I go do it whether I believe what he wants should be done." But he said, "If Mr. Watson asks me for an opinion on something, I give it to him straight," and he said, "I think Mr. Watson, Sr. appreciates that."

TROPP:

I think that's a very important point.

DICKINSON:

In other words, Mr. Bryce never would second guess anybody. If his opinion was asked on something he'd come straight out, not in an unpleasant or forceful manner but in a way in which you could accept his opinion if it were negative as far as you were concerned, because what he was really interested in was what is best for IBM. Now, one of the things that Mr. Bryce instilled in us was, in connection with ideas... was, he prepared always to answer the question, "If this idea is successful what will it do for IBM?" and so we... [LAUGHTER] we many times had to justify our ideas and we sure had to be ready to answer that question if he put it to us, because... Sometimes it was not necessary for him to ask us that question because I think what we were trying to do was rather obvious that if it were successful it would help IBM out.

So... well, I could go on and on about Bryce but...

TROPP:

We may come back to him after we've...

DICKINSON:

The second period, which again is rather arbitrary, extends from 1936 to 1940 and during this period there was not only continuing work on dividing machines but also multiplying and dividing machines. The next different field of Mr. Bryce's interest during this period of time was using a magnetic medium for controlling IBM's accounting machines. He had a number of cards prepared by our card paper manufacturer, Hollingsworth and Whitney at that time, filled with magnetic material. That is, the magnetic material was interspersed in a continuous manner with the paper fibers of the cards and we not only found out that discrete spots could be placed on cards in the manner of punched holes, but also that they could be handled and processed by machines. Now we did extremely little experimental work but enough was done to enable Mr. Bryce to develop a rather complete line of machines to handle magnetic cards.

TROPP:

Both to put information on the cards and to read it off?

DICKINSON:

Yes, that's right--to apply information to the cards and to read it off and to process the data.

TROPP:

There's an interesting link that may exist that you may want to comment on. This is near the period when Ben Wood, of course, was working with testing machines, machines to read test data and to score them that we're now so familiar with, but this was during that period of development. Was there a tie-in with Ben Wood's work in some of this?

DICKINSON:

No, no. No, there was no tie-in with Ray Johnson who...

TROPP:

He's the one who actually developed...

DICKINSON:

Who actually developed Ben Wood's idea of using... but I don't know whether Ben Wood had the idea of using...

TROPP:

That was Ray Johnson's idea.

DICKINSON:

I think it was Ray Johnson's idea.

TROPP:

It was Ray Johnson who brought it to Ben Wood who then made sure that Mr. Watson knew of it.

DICKINSON:

That's right, that's correct.

TROPP:

It was Ben Wood who was interested in the need for being able to handle test results.

DICKINSON:

That is correct.

TROPP:

But as to the actual method, this is Mr. Johnson.

DICKINSON:

There are two other things that are worthy of noting that Mr. Bryce was interested in during this period of time and that was a neon printer, neon tube printer. In other words, a revolving drum would in one revolution and in all the columns of the drum would have all of the alphabetic and numeric characters; and inside of the drum, on a common axis, was a line of neon tubes, one for each column--only one tube for each column. And the drum would be continuously rotating and say if an "A" was called for to be printed in column one, then the circuitry of the machine would flash the neon tube when the "A" was opposite the printing line; and of course the recording medium was a film; and some work was done in that area.

Now another thing which interested Mr. Bryce was the possibility of displaying characters in true number from on the face of a cathode ray tube and consequently a few devices using the cathode ray tube to display either a solitary character per tube or a plurality of characters per tube was worked out for him and the way this was done was that the... if the beam of the tube,, or if the grid of the tube was unbiased and the beam showed on the face of the tube, what one would see was what we called King David's seal, which was an outline of every possible character. In other words, if you concentrated on it you could see an "A" in the king David's seal and you could see all the other characters all the way down to "Z", and then you could see the numerals, but you had to concentrate. But here was this seal-appearing display on the cathode ray tube.

Now, if for example it was desired to print a "1", then by circuitry in the device the beam would only impinge on the screen during the time that the "1" was being generated by the pattern--that is, by the deflection plates. On the other hand, if an "A" was desired then the circuitry would be altered and the beam would be allowed to impinge on the screen and an "A" would be visibly displayed (displaced?). But only... in other words, the machine was controlled so that selective portions of King David's seal were selected as it was being generated by the deflection plates and during that period of time the grid would be unbiased to display.

TROPP:

What was the motivation for the cathode ray tube display as well as the neon tube printer? What were some of the pressures that led to thinking of those...?

DICKINSON:

Well, the neon tube printer inherently was an extremely high-speed device in comparison to our ordinary electron-mechanical printers. In other words, it was a very fast printing device. Now the idea behind the cathode ray tube, for example, was that it could be used as a signaling system very rapid signaling system, say in an office or a building or a hospital or anything... for example, a call system was required--I mean, a very rapid inter-service display of information.

TROPP:

I see... It's interesting because these are the first items that you mention in terms of your exposure to what we now call the branch of electronics.

DICKINSON:

That's right, that's very true, and subsequently improvements were made on the cathode ray tube as a display device. I'll eventually get to a point where it was used and by that I mean we were able to introduce more electronics into the cathode ray tube selecting circuits and consequently simplify them.

TROPP:

More on this second era and even going back to the earlier period that you discussed, I'm curious as to some of your contacts outside of IBM. I think of MIT or Bell Laboratories or some of the research going on other places and some of the needs that people like Bryce or Mr. Watson were seeing as to the future.

DICKINSON:

Well, in the sense that you asked the question, our group did not make contacts with say MIT or Harvard or any of the labs like the Bell Labs and son. We... the needs of the business were sometimes... well, a great many of the needs of the business or what we thought were the needs of the business were originated with ourselves. But I would also like to point out that we came in contact with salesman more in those days, because for example the Hundred Per Cent Convention, which was the yearly sales convention, would be held in New York and we would be invited to attend and... you just came in contact with salesman, you introduced yourselves or they introduced you or they knew about us and during the course of conversation it would come out that, gee, if I had a machine like this I could do so much for my customers. So the principal source of ideas in those days was inter-company, I mean...largely. It... we just didn't contact laboratories in the sense that you asked that question.

TROPP:

But I gather you were knowledgeable about what was going on in various fields in terms of...

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DICKINSON:

..... I say look at them--we would read about them and wonder whether or not they would be useful in our business, and I will come to that--one specific occurrence of that--which proved to be very beneficial at a later point. Now one thing in working with film that Mr. Bryce did not do was to provide for converting from punch cards to film--we would call that reproducing--that is, reproducing information in cards on film frame, one film per card and... or going from the developed film back to punching on the cards and... I devised a number of machines for doing that and then I will explain how that proved to be useful in the future.

TROPP:

The film development in this early period is interesting because later on when the high-speed computational devices were at the forefront in the latter part of the forties and early fifties film, of course, photoelectric cells and film were one of the methods that people followed, in terms of getting information in and out of these machines.

DICKINSON:

Very true, very true. Well another interest that I had was representing information in record cards in a different manner than punched holes and very briefly, I would use a different frequency--a wave form of different frequency to distinguish one denominational column over another. Now that could be done photographically. It could be done... well it was done chiefly photographically where a different wave form would be photographed on a card which bore a photographic emulsion and a number of different machines were developed for handling that type of information.

Another thing that interested me during this period was sending information by radio from an originating point to a distant point and three different types of transmission of record data were worked out and patent applications were filed.

TROPP:

That's interesting, using... Maybe you ought to elaborate on that for a little bit. I'm having... I'm just not familiar with it.

DICKINSON:

Well, the thinking on that was that a particular location might not want to have a whole line of IBM machines, but a central location might have a complete line of the machines for processing data; and the idea was to send the information from a distant point to the central point by radio. Now one of the schemes which were developed--I think of it as a symphony orchestra. I mean, when a great many instruments in a symphony orchestra are playing you have all kinds of frequencies. If you wish you can concentrate on the oboe or you can concentrate on the clarinet. Well, one method was to assign a different frequency to every column, not in the record card, but... The card would still have punched holes, but for example, if a card bore information at nine in all of the column, then as the card was analyzed all the frequencies would be transmitted at the nine differential time, so you had a complete orchestra and at the distant point there would be fillers, one for each column, so that a particular frequency would only go and actuate the counter mechanism in the particular column.

TROPP:

I see.

DICKINSON:

In other words, just by way of example, if you had a thousand cycles, a tone of a thousand cycles in the lowest column, then the filler at the distant point would be responsive to a thousand cycles, but... Now, then, the separation would be greater but just by way of example, if the filler in the highest or eightieth column--if the frequency was ten thousand cycles, then the filler at the receiving end would be responsive to ten thousand cycles, and that's how at the receiving end the different columns would be distinguished between the thousand cycle, because between the thousand cycle and the ten thousand cycle there would be an ascending scale of frequencies.

TROPP:

I see.

DICKINSON:

That's the symphony orchestra arrangement.

TROPP:

Right, right. Starting with the string bass, running up to the priccolo. [LAUGHTER]

DICKINSON:

That's right. Well, I think one thing that is of interest during this second period is the work which was done in the field of applying electron tubes to the business. Some time in the first period, probably the latter part of the first period, Mr. Bryce walked into my office one morning and said that we ought to be thinking about applying vacuum tubes, capacitances, and magnetic to the business. Well, that was a real challenge and I chose to apply the vacuum tube to devices which would not only generate timed impulses but would be responsive selectively to the timed impulses to perform at first the algebraic computation of adding and subtracting.

In the early part of 1936 it was possible to talk to Mr. Bryce about a machine, or an array of tubes which would permit adding operations of multi-denominational numbers in the ten notations to be accomplished.

TROPP:

So that you were thinking of a bank of ten tubes to represent each positional place?

DICKINSON:

Well, it was more involved than that. It was a rather involved arrangement of basically ten tubes per column plus a cathode ray switching tube for each denominational order plus phase producing circuitry. In fact, Mr. Bryce's conclusion after I explained this to him was that it was not only too involved but took too many tubes. So again there was a challenge to come up with something that was considerably simpler and considerably less involved. The solution to the problem did not come too readily during this period until 1938 and the fundamental thinking was to employ a condenser, one... that is, one for each...or ten for each... no, one for each column, denominational column.

Now, the fundamental thinking was to put one unit of charge on the condenser if the digit was one; to put two units of charge on the condenser if the number was two; and so on up to nine units of charge if the number was nine. But it was realized that a charged condenser would only hold its charge for a limited period of time.

TROPP:

And so it had to be regenerated continually.

DICKINSON:

That is correct and that is where the... the way the problem was solved. The problem was solved by using basically a saw tooth generator for each column in connection with a condenser and the saw tooth generator would build up the charge on the condenser and then of course the thyratron tube would break down and discharge the condenser and start all over again.

Now the next step was to see how this could be used to differentiate the numbers from zero to nine and it occurred that this could be done on a cyclical basis if the saw tooth generator could be moved from one phase--that is, the zero phase to the five phase--in its operation--in the cycle when the number five was added to zero; and the next step was to put on paper all of the things that would be called for when any number was added to another number to place the oscillator... or to see whether the oscillator would wind up in a phase which was representative of the digit previously in the denominational order as a result of adding another digit or the same digit to it.

TROPP:

Did you also have the problem of carry, so if your five was the first digit and the next digit that you were going to add in the same column was a seven, then you want to make sure that not only would there be a two in that column but you had to make sure that there would be some kind of additional charge in the next column?

DICKINSON:

Yes. Well, let me say that by drawing on a paper all the wave shapes of a saw tooth generator used in this manner, it showed that adding could be accomplished. Now let me explain how that was affected. Let's say that again the original digit was zero. The oscillator would be in the zero phase with respect to one complete cycle of the oscillator. Now if the number to be added were five, then commencing at the five time in the cycle if the oscillator frequency was doubled at the end of the cycle, that the oscillator would move out of the zero phase into the five phase. So in effect the fly back of the oscillator would be moved over, as you viewed it in the cathode ray tube, say from the left and to the center point and at that time it would resume its original basic frequency.

TROPP:

Which meant that it would keep regenerating the five (phase?).

DICKINSON:

Five, and you would see the return stroke in the cathode ray tube in the center of the screen.

TROPP:

Then what you were doing was shifting it out of phase to the proper frequency and then keeping it in that new phase continuously.

DICKINSON:

Yes, but in making those patterns on paper I discovered that only one basic change in frequency had to occur and that was to simply double it for a period of time proportional to the digit which was being entered.

TROPP:

Oh, I see, so then you did the doubling and all you added was a time shift then, a timing mechanism that would give you the proper ratio to get each of the ten digits that you wanted.

DICKINSON:

That's right. You can... let me give you a little different example. Supposing that you had an index wheel with nine... ten digits around its periphery, visible to you through a slit, and that wheel was continually rotating at a fixed frequency and if the amount was zero, then the gate opened in the machine--all you would see would be a zero and then it would close. Now if you had the mechanical means and you wanted to enter a five, if you made that wheel rotate twice as fast for five increments of time, then when the slit opened you would no longer see the zero, but the five would appear, so there is a mechanical analogy here.

OK. You spoke about the carry. Here again this showed up in the pattern in a much unexpected manner. Let's take for example--you gave the example--supposing there was a five standing in the counter and you added seven to it. Well, at the seven time you start doubling the frequency... or, sorry, you'd start doubling it... yes, at the seven time you'd start doubling the frequency and you would continue that to the end of the cycle for seven increments of that cycle and in that time there would not be one fly back but two fly backs.

TROPP:

Right, that's right, yes.

DICKINSON:

So...

TROPP:

That would give you the...

DICKINSON:

That would give you the indication that a carry was required. So the circuitry had to be developed, as far as the carry was concerned, to recognize... to ignore one fly back and to not ignore or to be responsive to two fly backs. Well then in a following cycle in which no adding was done...

TROPP:

Mm-hm. So it was like a hesitation as you moved from position to position.

DICKINSON:

Right. In the following cycle then a "1" would be signaled from the carry mechanism, say of the units order to the input to the tens order and during that second portion, or during that portion where no adding was done except to take care of the carries, then the "1" would be added to the next higher order.

TROPP:

Mm-hm. It was just an instruction then, saying whatever information you have in terms of the number of rotations, change it by adding one more to it.

DICKINSON:

That's right. Now the other portion of the carry that you had to take in account was that if the tens order was receiving a carry from the units order and the tens order stood at nine, you not only had to add a "1" in the tens order, but you had to add a "1" in the hundreds order. In other words, you had to provide in your carry circuitry the provision for recognizing when an order stood at nine and received a carry and pass it on--and pass it on just as far as it had to go.

TROPP:

So then everything worked sequentially?

DICKINSON:

Well actually it was a concurrent carry, because the circuitry was devised so that the nines would be recognized all at once. So wherever a "1" had to go in they all went in at the same time. It was a concurrent carry.

TROPP:

Right.

DICKINSON:

Now that's a little hard...

TROPP:

It's hard to visualize at this point in time, going back to think of these thyratron tubes that could do things so rapidly.

DICKINSON:

Well, no, the circuitry--and it would take some time to point it out to you, to get my mind back on it--but it would recognize the nines instantaneously, because the nines were there and the circuitry was set up...

TROPP:

Because that particular special situation was the one that could be the most... would cause the most difficulty, so you were in a sense setting up a special method for the circuitry to recognize the occurrence of nines.

DICKINSON:

Yes, it was in the circuitry. So that's where we took off from.

TROPP:

This was in 1938?

DICKINSON:

Yes. Of course, that's when I started to confirm all of this by actually building a crude breadboard mode. Now, at center point it was realized that thyratrons operated at maximum upper frequency of I think it was--that is, regular thyratrons--would operate at a regular... would repeat maybe up to 30,000 cycles per second. Now I may be off by 10,000 cycles or so but anyhow there was an upper limit by which you could use a saw tooth generator or an oscillator, a maximum speed with which you could get this thing to respond to input pulses. So this of course led my thinking to how to apply hard tubes, because we knew what hard tubes would go very much faster.

TROPP:

At this point in time was the company at all concerned or interested in doing research on vacuum tubes itself or..

DICKINSON:

I'll come to that later.

TROPP:

OK.

DICKINSON:

I'll come that that later. The answer at this point is yes, very definitely. Now, on one of his visits to New York Howard Aiken--during this period of time--Howard Aiken came to see Mr. Bryce and Mr. Bryce brought him in to show him what I was doing in applying vacuum tubes. Whether Aiken perceived this or whether I mentioned it to him I do not recall, but in any event I explained to him that I wanted to complete what I was doing with thyratrons but try to start applying a hard tube to electronic accumulators--that is, registers for receiving multi-denominational amounts in the tens notation on an algebraic basis--that is, adding and subtracting. Aiken came right back and said, "I think I've got a circuit for you. I think there's a paper that you would be very much interested in by Stevenson and Getting."

TROPP:

So what was that last name?

DICKINSON:

Getting.

TROPP:

G-e-t-t-i-n-g?

DICKINSON:

Yes, and in a few days Donald gave me a copy from Aiken of the Stevenson-Getting circuit which appeared in the Journal of Scientific Instruments and it was a flip-flop.

TROPP:

[LAUGHTER] I'm surprised! It's easy looking back to...

DICKINSON:

Using our tubes. Well, that immediately opened the box and as soon as I could I constructed a Stevenson-Getting circuit and got it to scale--that is, flip back and forth. That was the first step to get in elements and tubes and so on that would function in the manner that Stevenson and Getting taught. And sure enough it did.

Well, the next step was to see whether or not there was a way in which a second flip-flop could be hooked up to the first flip-flop to step... if the first flip-flop was on, to step to the second flip-flop and put it on; and after it was on, to put the first one off. And by Jove, I got it! So the next question was, can I hook up to them? Sure enough it came about so... I had to begin with first order, which was responsive to impulses, which would step along and when it got to the end come around and start all over again. And of course when it stepped from the end to the beginning that was indication that it had gone from nine to zero, so other flip-flops or a flip-flop plus amplifier tubes were employed to respond to the stepping from nine to zero and to again in a second portion, in which adding was not done, to put in the carry in the next higher order. And of course again the fundamental principle of carry had to be realized that, if that next order stood at nine, it had to go to the next higher order and as far as it had to go.

Well, of course I got this thing to go at a considerably higher speed than I was ever able to get the thyratron circuit going. As a matter of fact, I really didn't put much effort in trying to speed up the thyratron circuit because of realization of the upper limit at which you could repetitively fire the thyratron tube, and we certainly didn't want to think about special thyratrons which we knew would go higher.

TROPP:

Right, and as I say, this was a parallel to what, when you decided this upper limit, the group at National Cash was developing special thyratrons which would go higher in order to overcome that deficiency that you mentioned.

DICKINSON:

So... well, after that came a whole series of electronic devices responsive to impulses to add numbers or to subtract numbers.

TROPP:

Was there any thought at this point to doing your arithmetic in other than a decimal mode?

DICKINSON:

No. Let me tell you that the possible application of the binary to our type of work--it didn't make any difference how the binary was handled--whether it was electromechanically or vacuum tubes--Mr. Bryce had the feeling that the amount of equipment required to convert from decimal to binary, and the amount of equipment required to convert from binary to decimal in the output would outweigh the amount of equipment required to simply stay in the tens notation.

TROPP:

I think at that point you would have to say he was absolutely right. He was right for a long, long time, really. It's only recently that you say it doesn't make any difference in terms of amount of equipment involved.

DICKINSON:

So it wasn't... well, you can see that in effect our thinking was conditioned to stick in a tens notation and get devices going in the tens notation that were at considerably higher speed than we were capable of doing on an electro-mechanical basis.

TROPP:

I think in terms of that feeling and that decision...

[End of Tape 1, Side 2]

[Start Tape 2, Side 1]

DICKINSON:

There's one other thing that I think should be mentioned and right at the moment I do not recall, although if necessary I can get the information: Either during the first period or the second period, which is about coming to a close we, investigated the use of relay matrices for performing arithmetical computations.

TROPP:

By a relay matrix, then, you're talking about a square or rectangular array of...

DICKINSON:

That is correct, of a group of relays and essentially the relays would shift circuits... the circuits in the matrix in accordance with what was to be added. In other words, if you added a one to zero, then the lines would be shifted over non-increment effectively, so instead of the zero relaying in the result group being energized, the amount would shift to the... or the one relay in the result group would be energized and... This happened to be done incidentally on a combinational basis where the number of... the shifts would be one, two, and I don't recall whether the next shift would be three or six--and six, or four and eight, but you could have--if the digit to be entered was a nine then you'd have the one shifting relays picked up or circuits made and you'd have the eight shifting relays picked up, so that effectively if you added nine to zero your ninth relay... or maybe the one and eight relay would be picked up in the result column. I'd have to check on that.

TROPP:

Well now, this looks like an addition table as you would write it on the board for a youngster, except that you have relays and shifting instead of...

DICKINSON:

That is correct.

TROPP:

the intersection of columns and rows.

DICKINSON:

Now, that type of adding and subtracting was intermediate, as far as speed is concerned, between or electron mechanical methods of adding--that is, in the accounting machines and the electronic methods of adding; and at that time it was quite natural to think of something like relays for adding because we didn't know whether we would be using vacuum tubes in the business for adding and subtracting or other mathematical computations and so the fundamental work which was done either during that first period or second period, I'll show, proved to be useful as time went on.

TROPP:

In this same area was there... well, not was there... I gather that there was a good deal of research on different types of relays going on within the company, or am I wrong?

DICKINSON:

Well, the IBM relay was a pretty standardized item for years and years and years. It might vary as to the number of contacts or whether it was a make relay or a break relay or a transfer relay, but Clare Lake at Endicott was the only one that I know of that was interested in relays and he was interested in getting a smaller relay then the standard IBM relay and he came up with the wire contact relay. I'm pretty certain that this was prior to 1936; and also he came up with a much smaller version of the electron mechanical counter position--that is, he came up with a pluggable kind of thing for each denominational order, which was... we used to call it the watch case counter. Now, they both were successful and I have the notion that if those two devices had not been available, that the Harvard machine might no ever have been undertaken.

TROPP:

I think you're probably right because again we're talking about philosophy of people and I think at that point in time it's fairly safe to characterize Professor Aiken's philosophy as wanting to build with what was available.

DICKINSON:

But of course that's what we did...

TROPP:

And had the devices not been available to accomplish the purpose, the machine might not have been even feasible.

DICKINSON:

Well, the mere physical bulk of the machine had it had to use the old standard IBM relay and the old standard IBM electromechanical counter--I have no idea what the size of the Harvard machine might have been.

TROPP:

The counters I guess are about two inches square on that machine.

DICKINSON:

Yeah. So there again, and I want to point out that the way Lake was able to do this, he had assignments--you see, all the other inventors during this period of time had assignments from Mr. Watson to build certain kinds of machines. So Mr. Lake had his assignment for certain machines but he was able to develop the watch case counter plate and the wire contact relay because Mr. Watson, Sr. would allow each of the inventors a certain sum of money to spend on anything that they wanted to and that's how, I am very certain, these two things came about, and they came about you see with no notion that they'd ever be used in a machine like the Harvard machine.

TROPP:

Right, well, but again trying in with my statement about Howard Aiken, once he had conceived of the notion of this sequential calculator, his next job was to find out what was available that would make it a realizable result; and I think it was through Ted Brown that he learned that IBM did have the on-the-shelf technology to realize this conception and so there you begin to see the mesh of, here's something I went to build and here's the technology it will require and we in a sense are not really interested in developing a special technology for it. Now who is it that has the technology available?

DICKINSON:

That's right.

TROPP:

And then you begin to see the mesh of these two groups in their intellectual capacities.

DICKINSON:

Well, the next period that is arbitrarily chosen is that between 1941 and 1948.

TROPP:

Before we go into that next period, the second period that you just talked about is the one in which the contacts were occurring between IBM, Harvard, and Howard Aiken. You mentioned the personal contact that you had. This proposal came to Mr. Bryce's attention, if my date is correct, sometime in November of '37. Then Howard Aiken was invited up to a meeting to discuss his proposal.

DICKINSON:

Well, I can look that up because...

TROPP:

I have the correspondence on that so I guess approximately the latter half of 1937 and I just wondered if you care to talk about that development in terms of the knowledge that you had and your relationship with Mr. Bryce, Mr. Hamilton, Mr. Lake, Mr. Durfee--people who were working on that machine.

DICKINSON:

Well, we were so busy in our own group that I really had practically no contact with Aiken or Lake or Durfee or Hamilton during that period.

TROPP:

You don't remember casual conversations on some of the problems that were going on and some of the...

DICKINSON:

I didn't... I knew of none of those. The only thing that I can tell you during that period of time that just comes to mind--they called me up to Endicott and took me into the shop room where they had the jack shaft, as they called it, of the Harvard machine the basic timing shaft--and... This is getting a little ahead of the story but they had started to work on what I'm going to talk about a little bit later, which is the Aberdeen relay calculators and in those Aberdeen machines we used relay A. Mr. Lake and Durfee agreed to build the Aberdeen machines and they called me up and said, "Come on in and look. We can add on relays." [LAUGHTER] And sure enough, they had this hooked up to the long jack shaft...

TROPP:

It was about sixty feet, wasn't it?

DICKINSON:

... and over here a bank of relays which maybe was sixteen inches wide and sixteen inches deep and they said, "Look, we're adding."

No, I have no knowledge of the problems that arose in connection with the Harvard machines.

TROPP:

Well of course the period that... you know, that second time period is the conceptual period. This is the period before even attempts to build the machine occurred, which was, you know, when decisions were being made to go ahead and... than how to do it.

DICKINSON:

Well, I got into that--didn't get into that because you see, the war period came along and Mr. Watson Sr.'s instructions were that the engineers should devote themselves to the war effort, so that sort of changed the character of our patent development group in New York because Mr. Bryce said, "Get yourselves some war jobs and get going," in effect, although it was possible to continue with electronic circuitry and accumulators... And I'd like to sum up what happened as far as I was concerned. In addition to having basically ten flip-flops per denominational order, it was possible to simplify a denominational order by going into the quinary system--that is, the system of fives.

One other thing that occurred was a matter of serendipity. The tubes which were being used in the counter circuits--hard tubes--had grid caps and quite by chance each of the flip-flops had a neon light to show whether the flop-flop was on or off, so I could check the accuracy of adding and subtracting. One day I touched a grid cap, which I religiously kept away from because... just because I didn't want to get a shock... or just keep my hands away from the wiring, because if any mistake was going to be made I want it to occur as a result of a missed operation of a flip-flop rather than a provoked sort of thing. But anyhow my touching the grid cap did provoke--I don't remember which--the flip-flop went from either on to off, or off to on, and I thought to myself, "What happened here? Ah: capacitance." I discharged or the tube discharged to me. And I thought: "Now I wonder if I took out the pintles which connected between one flip-flop, both for putting the following flip-flop on or the preceding one off--I wonder if I could substitute condensers?"

Well, I did and immediately the number of tubes required in any denominational order halved, because I could do this flipping and flopping and stepping along through capacitances rather than actuating pintles.

TROPP:

[LAUGHTER] That's marvelous. That got you into this quinary system. Serendipity is right.

DICKINSON:

Well, quite a few... quite a bit... quite a large number of thins electronic--either circuitry or registers or accumulators if you want to call them that--were constructed using different methods.

TROPP:

By the time the war broke out, where would you say you were in terms of using electronic methods or other methods like the relays towards facilitating computation? Is it possible to summarize your status when the war broke out?

DICKINSON:

We were ready... you see, when we got the assignment of the Aberdeen machines we had to make a decision as to whether we would try to go... use vacuum tubes or use relays, which were faster than the old electron mechanical methods, by multiplying. Well, we just hadn't had enough experience. We just weren't ready.

TROPP:

I realize that. I think it's clear in that whole period nobody was really to make that jump in terms of experience. But how did you feel about the potential at that point in time. Suppose, say, the war had been delayed about a year or so. Were you pushing along well now so that you might have been ready if you hadn't been diverted into the pressure of war? I guess what I'm really looking for, and this is an unfair question, and that's a kind of state of the art as you saw it when the war came along with its new pressures, to divert you.

DICKINSON:

Well there again we knew the relays were reliable and we had a method of adding and subtracting and in connection with the Aberdeen machine I worked up a method of multiplying with relays. There was something that you could go in on the shelf and pick off and wire up and get results, whereas a lot more experimental work would have to have been done using vacuum tubes.

TROPP:

OK, I just wanted...

DICKINSON:

Right at that time I hazard a guess that just the mere numbers of tubes that would be required and the space and the power requirements just might have killed it... killed things... could well have killed the use...

TROPP:

Of course the heat dissipation problem was going to be another one. I just wondered about the early machine.

DICKINSON:

That comes along a little bit later.

TROPP:

A really major problem. OK, I'm sorry...

DICKINSON:

No, that's perfectly all right.

TROPP:

I'll let you go on to the end of that third period now, where I interrupted a number of times.

DICKINSON:

Well, again I'm not quite sure who directed an emissary from Cal Tech to Mr. Bryce, but I got called into the act and got the assignment. Again Mr. Bryce approved of the project of building highly accurate analog digital devices for--measuring devices--that is, in effect wind scales which would respond to the various factors which were measured on a... by the plane being flown in a wind tunnel.

TROPP:

What we call reduction of data from...

DICKINSON:

That is correct. Now...

TROPP:

Do you remember who the emissary from Cal Tech was?

DICKINSON:

Yes, Peter Serrell and Ernie Sechler perhaps. I don't know whether there was just Pete and Ernie or Ernie, but I think it was either one of those guys.

TROPP:

How do you spell their last two names?

DICKINSON:

Serrell is S-E-double R-E-double L and Sechler is S-E-C-H-L-E-R. He's head of the wind tunnel out there--or head of the Department of Aeronautic at Cal Tech now. I don't where Pete Serrell is.

TROPP:

I can check with...

DICKINSON:

Now this was an extremely interesting project from various points of view. In the first place it involved a system, and once the data was punched in the punch card--it was also simultaneously printed on a printer--but the punch card would then be taken to a group of our standard machines to reduce the data to something that was meaningful to the aeronautical engineers. Now at that time the company had what called systems service women and the Los Angeles office assigned us a systems service woman to work out the steps by which these computations were performed and she did a marvelous job.

TROPP:

Do you remember who that was?

DICKINSON:

Yes, Katherine Kalin. She lives in Darien, Connecticut.

TROPP:

How do you spell her last name?

DICKINSON:

K-a-l-i-n. But at our end we were responsible for the devices which would respond to what were termed wind gauges and they were just like a scale. They would show on a dial the quantity which was being measured and its amount.

TROPP:

It might be the flow of air over a surface, pressure...

DICKINSON:

Well, it would be something like drag and yaw and...

TROPP:

Lift.

DICKINSON:

Lift, and different elements that are required to be known about a plane. A (selson?) measured the angle of attack and turn. Now I think there were at least seven wing gauges and two selsons. The interesting thing was that in back of these wing gauges there was a rod which actuated the pointer, which moved 1.157 inches from zero to full scale.

TROPP:

To the maximum?

DICKINSON:

That is correct; and one of the basic requirements was to measure that with an accuracy of plus or minus one part in 10,000.

TROPP:

This was beyond the accuracy of most analog devices.

DICKINSON:

Oh, far beyond, far beyond. Well, to make a long story short, we used a very finely micrometer screw which Cal Tech made for us and I'm speaking now of just one wing gauge because this was (true for all of them?) and this wing... this screw bore a very light magnesium rod itself and the screw was gear attached to a counter having an electrical read-out and essentially a first hunting system was employed where the counter and the screw would be turned in a positive direction until the magnesium rod hit a contact on the scale arm and at that instant the amount standing in the counter would be transferred to relays; and this forced hunting occurred at two a second, up and down two a second. We had means of doing it at four per second if they wanted it.

Now the required accuracy was achieved so that the mechanisms were--except for the screws--were constructed by an outside firm in New York, to our specifications. Incidentally, in a lot of these things that I'm talking about, I just don't want to give the impression that I was the sole inventor.

TROPP:

Oh, I realize that.

DICKINSON:

There were a lot of people involved and if it weren't for them I don't think some of these jobs would ever have been done.

TROPP:

That's an interesting project because, when you look at the immediate postwar period, that same problem was still around but the environment had changed. We moved from the slower aircraft to the higher speed aircraft with the advent of the jet engine and so this just compounded the problem of getting this data and reducing it to a useable from.

DICKINSON:

That's correct. Well, to make a long story short, this equipment was in that tunnel for fifteen years. Now I want to come to a couple of interesting things. The tunnel was expensive to operate and we were told very bluntly that they weren't going to run the windmill ? (wind tunnel) for us to test any equipment that might be faulty. Now, this was a new philosophy as far as IBM was concerned, because IBM had the policy of running its machines and the service men would try to determine what was at fault with the standard accounting machines, which meant down time for the customer, but here was a situation where the down time was limited to five minutes.

Now how did we handle that? A thing like the micrometer screw, a thing like the electromechanical counter, all of the wiring and the relays were sectionalized...

TROPP:

What we now call modules.

DICKINSON:

Well... and they were constructed in a way in which they could be gotten out of the system very rapidly. Now that meant that we had to furnish them with spares, so we did. Now, what did the poor service men do? Well, the way we solved that problem was to construct a simulator in which the conditions comparable to conditions in the wind tunnel could be simulated and the apparatus checked and corrected.

TROPP:

So that he could service the equipment...

DICKINSON:

Independently...

TROPP:

Independently of the wind tunnel. You pulled the module, he put it on the simulating equipment, and he could check it and repair what was wrong, and then it would become a replacement.

DICKINSON:

That's right...

TROPP:

For the next time that section went down.

DICKINSON:

That's right.

TROPP:

Fascinating.

DICKINSON:

So I think you see introduced here are two kinds of thinking which were quite new, not necessarily entirely new, but in a complete system...

TROPP:

Well, they're so much a part of our current environment that we tend to forget that they weren't always around.

DICKINSON:

And we never had any complaints and so far as I am aware, we went out there and installed it and we brought back from... this was not only installed at the Cal Tech wind tunnel but it was installed at Curtiss-Wright in Buffalo, and I think at Consulted... Consulted...

TROPP:

Consolidated Voltee, yeah.

DICKINSON:

...in L. A., and at a later time improved equipment was installed at the University of Maryland, but in the meantime the main customer engineer who came out to be taught what the equipment was came back to New York and assumed responsibility for a group which was ______________ working in this particular area. So...

TROPP:

Did you get involved at all in the problems related to reducing the data that came from the wind tunnel to useable form?

DICKINSON:

Well, OK, I knew that they wanted, because it was in the specification, and I developed a very cumbersome set of steps for reducing the data on our standard machines and Katherine Kalin saw that and threw up her hands in horror and... oh, she did a marvelous job. She just really saw what was the heart of this thing and got it down on a much simpler basis, so that the data could be reduced in a much faster time.

TROPP:

Using what was then standard equipment?

DICKINSON:

Standard equipment, that's right. Now the only nonstandard equipment was the printer in the wind tunnel itself, because it didn't have to add or subtract. It just had to print so... but the rest of the equipment--the reproducer or the punch in the tunnel or the other equipment in the computing room was IBM standard equipment.

TROPP:

It's interesting and I guess to someone who's interested in analog computers it would be fascinating to see how you increased the accuracy of these devices so that you could get one part in 10,000... because you were really increasing it by a power of ten to ten squared essentially.

DICKINSON:

The reason is that Cal Tech made these beautifully accurate screws, micrometers. They had the means of doing it and that was the sole reason. If they had been inaccurate we couldn't have met the specification or... well, I say unless we could have found somebody that could make accurate screws as were required.

TROPP:

Yeah, or you would have had to live with data that was accurate at best to one part in 10,000.

DICKINSON:

Well, that's about what the standard transducer was in those days. Well, Mr. Watson of course was very interested in this project. I might say I'm not sure how he treated Consolidated Voltee and I'm not quite sure how he treated Curtiss-Wright, but he gave the equipment to Cal Tech.

TROPP:

That's interesting because, you know, we talked earlier at breakfast about his relationship with educational institutions. It seems to have been unique, his whole attitude towards education.

DICKINSON:

So that about... he didn't come out to the dedication. He sent Charlie Kirk, who's no longer living, out so we played a minor part in the dedication. Then he sent Ned Douglas up to Curtiss-Wright when it was dedicated, so... both institutions were very fair about their publicity as far as IBM was concerned.

TROPP:

What approximately was the date of the dedication at Cal Tech?

DICKINSON:

We started installing in 1944 and completed in the early part of 1945.

TROPP:

So the machine was operating then until about 1960 or somewhere...

DICKINSON:

Fifteen years, fifteen years. OK. Well, the next war job that we got into was a request from the Aberdeen Proving Ground that we furnish them with calculators to compute ballistic tables faster than they could turn them out on Bush's differential analyzers. So I think McPherson and I went down and found out what their problem was and what data they had as input and how they wanted it manipulated. Then another time Lake and I went down because Lake had either taken an or given the assignment of constructing the machines and he--Lake--thought that two 601's could do the job by hooking them some way together, which was never quite clear to me.

Well, the 601 as a multiplier was not a fast machine. I mean it certainly did what it was supposed to do but it certainly didn't turn out cards at l00 a minute and so knowing of what we had done in the field of relay adding I proposed the use of relay adding and subtracting on a kind of a reproducer kind of machine, where you had at least two card feeds and at least a single punch and Lake bought it and... So we took off from there and I had the job of figuring out how to multiply. Well, the machines were built and two of them were sent down there because James Cunningham insisted on paralleling operations, so that if you got into errors--well he didn't, you know, want to rely on a single machine which might go haywire. He wanted to be able to have two machines and be able to detect which machine wasn't operating correctly. I don't know how he did that but--of course you could compare results but it was the correct result I assume he would know by feel or something like that.

Well, anyhow, that took care of that job. The next thing that we got into was an inquiry from the Radiation Lab at MIT for what became called the data repeater. Now essentially what this machine did--it was key operated and an operator looking at a radar screen would have knowledge of which was a friendly plane and which was an enemy plane. He would set the altitudes in keyboards and cathode ray tube indicators would not only indicate the difference but which plane had the higher altitude or had the advantage. Here again the computing was done by relays, but the display was with cathode ray tubes, which we had for display purpose considerably modified--I mean simplified.

TROPP:

Just to help me in the future looking, could you give me some of the names of the people, both at Aberdeen and at the Radiation Laboratory, that you had contact with during these particular projects?

DICKINSON:

No. Just let me make a note. The machine was taken to the Radiation Lab in Cambridge and was demonstrated but it was not accepted and why it wasn't accepted I don't think any of us know. My understanding was that we were in competition with some sort of device being developed by GE and that they won out. Now what it was I don't know because you know the Radiation Lab at that time sort of was off limits.

TROPP:

Right--lots of different things going on.

DICKINSON:

They practically--I'm just being symbolic--they practically put blinders on us as we walked through the lab so we wouldn't even get the slightest notion of what was going on. [LAUGHTER]

TROPP:

That was fairly typical though of the high security jobs that were going on during the war.1

DICKINSON:

Well, OK. Now sometime during this period--I'm not sure that the war ended but it may well have been, Mr. Watson called a group of us together in his office and said that he wanted an electronic--a wholly electronic machine to perform computations and he gave the assignment to me; and three of us worked together on that--Byron Phelps and Carl Burkforce. Now I'd like to mention some of Brukforce's work prior to this time.

In between what we were doing on war jobs, because there were periods when others were doing things for us and we had some time, one of the assignments of Burkforce was to take whatever we knew about in the way of electronic circuits for adding and subtracting and moving them out of the 10,000 cycle response that is, an impulse repetitive rate of 10,000 cycles--up to as high as he could go and he didn't experiment with every circuit because it was obvious that, from just the experience that you gained over a period of time, there wasn't any point in trying to make it go fast. But there were other circuits which were inherently simple and which would manifest the digits zero through nine, which by virtue of their simplicity and the number of elements and the number of tubes had the possibility of going much faster.

Now, he was successful in devising two fundamental types of counter circuits which could be combined in denominational orders, which would go between 75 and 100,000 cycles per second, but he even went further. He devised a trigger circuit which responded to thirteen million impulses per second. When he tied that to a second flip-flop its frequency of response dropped to 7,500--7.5 million cycles per second.

TROPP:

Just almost in half.

DICKINSON:

In half, and when we hooked it up in one of the arrays which would be responsive to the digits zero through nine and manifest them, the frequency of response dropped to about 3.6 megacycles. But it showed then that we were in the ball park as far as realizing what we should from our electronic circuits.

Now to come back to the association of Phelps and Burkforce and myself in getting together a model which eventually resulted in the 603 electronic multiplier. Now the model had limited capacity but it did add, subtract, multiply, and divide and it did it at the rate of passing the cards through the machine at 100 a minute; and the basic frequency of the electronic circuits--they had to respond to pulses at least of 50,000 cycles per second, which was lower than the upper limit which was done as a factor of safety. We didn't push it to the upper limit where there might be failures.

This machine was announced at the Business Show in New York in 1946 and when it went into production its capacity was enlarged for multiplying and dividing was taken out, but that machine went on the market in 1946. There again in that machine we, to use your expression, the various fundamental elements in the circuits were contrived as modules, so that the service man based on his experience could very quickly locate which module was acting up and could replace it with spares.

TROPP:

Did you also have simulation systems so that he could...

DICKINSON:

No, we really didn't need it because all he needed was an oscilloscope and a pulse generator. Well, I mean we didn't have a test unit because you see these were all pulse type responses circuits, so all he really had to do was to go through and see whether pulses were getting through.

TROPP:

Find out where--that either a circuit or something was loose or...

DICKINSON:

That's right. Now there's one interesting aside that I'd like to point out. After the model was built I got a gal to come and test it and from the accounting department at world headquarters I got just--I don't know--10,000 cards randomly punched as to two things--multiplier and multiplication, or the dividend and the divisor; and it was her job to feed these cards through the machine, and we had a method of detecting errors. Now the interesting thing is that she couldn't--she couldn't correct the error and go on--she was stopped until Burkforce or Phelps got to the machine and found out what the error was. Now the reason that was the best thing we ever did was we got the machine tested out in jig time.

TROPP:

Well, you said the 603 when it was first built did not perform division; did you do division by multiplication or reciprocals?

DICKINSON:

Well, I suppose they did. This is the machine that went out into the... the machines that went into the field. The model machine would...

TROPP:

Yeah, I know you said the model machine would but the commercial machine could not perform division.

DICKINSON:

Not at that time. When we got in with the 604 then that machine would divide.

TROPP:

What was the reason for taking division out? Did it simplify it significantly, or was the demand that much less?

DICKINSON:

No, it was simply a matter of capacity. I think we were limited to six or eight denominational orders as far as the multiplier and multiplicand were concerned and... maybe we had a limited capacity result counter too. Maybe we only kept part of the result, but this was a matter of capacity and not changing the machine design or the electronic cabinet, because in order to increase the capacity and include multiplying--or dividing as well as multiplying--the cabinet--there had to be a complete redesign job and the company didn't want to stop for that.

TROPP:

I see. They wanted to get that machine out.

DICKINSON:

They wanted to get that machine out, but they didn't feel that the capacity as far as dividing was concerned was sufficiently large to warrant, because you see you could--whatever the capacity was there could only be eight columns in the dividend and a possibly eight in the divisor. Well now, the divisor might not go at all, but if the divisor were only one digit it had the possibility of going seven or eight times in getting your result. That's the kind of thing that you run into in applying division to a machine.

TROPP:

That's been a hang-up from the very, very beginning--shortage.

DICKINSON:

Well, we know that... I mean... Well, I suppose... Back earlier you asked me whether or not we did anything in the way of developing vacuum tubes and I think I said, "Yes," at the time, because it was during this period of time, specifically in 1946, that Mr. Watson asked me to establish a vacuum tube laboratory, because we were using ordinary tubes and they had their useful life and it was plain to people to see these high-speed machines sort of conking out now and then because that's what an array of tubes would do. So he said, now I want you to--my notes say five and some other notes say two--but in any event he wanted me to get more than one vacuum tube expert into the business. Somehow or other I found out that there were only twenty-five vacuum tube experts in this whole country.

Well, to make a long story short, after interviewing a couple of vacuum tube people the third one, Barid Corsund, who is now with Hughes Aircraft, was prevailed to come into the business from--I don't know whether it was Sylvania or Hytron, but he manifestly had a great deal of experience and he was intrigued by the problems that we were encountering, particularly the life of tubes. So eventually he was established at 310 Fifth Avenue, New York; given a sum of money to go out and purchase equipment and hire people. He traveled around to various locations where tubes were being used and found out from the engineers what kinds of tubes they thought they would like and would be useful to them in the business. In addition I had my own notions, which I explained to Corsund and of course one of the things was a long life, getting something that had a longer life than the ordinary vacuum tube. Also I wanted to get more elements in a common envelop, like getting a double pintle or a quadruple triode to simplify the number of things--the physical space required for housing tubes.

Well, he got the equipment together, he hired fine people, and he started getting results and he did develop a double pintle; he did develop a quadruple triode; and he did develop a longer-lived tube.

TROPP:

By longer life, approximately how many hours are talking about? Ten thousand or less?

DICKINSON:

Well, I would say longer life than ten thousand hours because well... yes, I would say longer life than ten thousand hours. Now this was sort of concomitant with the operation of the SSEC. I mean there's an overlap here with the next period, because the SSEC went into operation in 1948 and then we immediately began to experience sadly the burning out of vacuum tubes and fortunately some time after the machine had gone into operation Corsund came up with these longer-life tubes and it proved to be an excellent place to test these tubes because he was able to construct a fair quantity of them.

TROPP:

So he did set up a manufacturing operation?

DICKINSON:

Oh yes, he set up a manufacturing operation at 310 Fifth Avenue. Now this was not a manufacturing operation in the sense that RCA might have a manufacturing... this was not a quantity matter, it was a quality matter, so he had to have and he put very close controls on the way things were done and the materials that were used and so on, which didn't occur in the ordinary tube company.

TROPP:

One of the problems of course was keeping any dust particles out of the envelop.

DICKINSON:

So we, or he or the SSEC people agreed to put some of his tubes in, in some critical area--I don't remember which--and by Jove, they certainly proved themselves as lasting longer. Now another thing that Corsund did was to advocate preventative maintenance. Now as far as tubes were concerned he found out or he knew that certain things that could happen to tubes would happen a certain length of time ahead of the end of their useful life and the preventative maintenance that he advocated was to remove all the tubes in a given section, be it an adding section or a storage section, and to replace them with new tubes all at once, and that proved to be a helpful thing too as far as... if you did it soon enough ahead of one undiscovered, you had to put in... you had to periodically test to see... to get these telltale signs that something was going to fail and instead of replacing that one particular tube you replaced the whole bunch.

TROPP:

That's interesting because roughly at the same time at MIT they were building the Whirlwind computer. I could be wrong but I think one of the things they did there was to run the tubes for a set number of hours and those that were still running after that period of time were then put in the machine, because the data that they had led them to the conclusion that the longer a tube was able to run the longer its life expectancy, so what they were interested in was finding the tubes whose life expectancy was essentially unlimited.

DICKINSON:

Well, the work which Corsund was doing was so important that he moved out of New York and took his equipment and as many people as were willing to go, to Poughkeepsie, and there the scope of his operations enlarged and he made more tubes and proved that he could make more tubes, or make tubes which would last longer and to the specifications of our engineers.

TROPP:

At that time was Mr. Watson thinking of manufacturing tubes commercially, because I gather that this move--the new setup had more characteristics of an assembly line than he'd had in New York, at 310.

DICKINSON:

Well he got more equipment, a larger machine, and generally made possible our going into tube production because we couldn't prevail upon the tube companies to supply us with tubes that we wanted. This was because of various factors occurring at the time. The field of electronics was becoming wide open. Television sets were coming in and the use of tubes was increasing. They wanted to produce as many of that kind of tube--that is, the standard tube--as much as they could and they didn't want to fool around with experimental work. They were more concerned with improving their processes and cheapening their processes rather than doing experimental work on other tubes. Now we did get tubes made on the outside but they never were really satisfactory for various reasons. Either they would change the elements that were used in the tubes or they wouldn't conform to our specifications. This was true after Corsund went to Poughkeepsie, so in some of our machines he furnished all the tubes and...

TROPP:

That's new; I haven't heard that before.

DICKINSON:

So I told Mr. Watson that the laboratory had been established and he never came down to see it. He might have seen it when he went to Poughkeepsie but I don't know that, but when I told him that it had been established and what we were doing he was satisfied, so there is one more thing that should be brought up in this period and I think that...(end of tape)

TROPP:

This is a continuation of my discussion with Mr. Dickinson, still the same day and it's later in the afternoon; and I'll let Mr. Dickinson continue from there with the last--or an additional item in this third time period.

DICKINSON:

Well as a matter of fact there are two items in the third time period. One is perhaps you might inquire or wanted to inquire about whether I had any part in the SSEC.

TROPP:

Yes.

DICKINSON:

Well, there again Mr. Watson called a group of us together and told us what he expected in the way of a computer to out-compute anything that was in existence at that time and he wanted us to come up with some sort of a specification to meet those requirements.

TROPP:

Can you give any remembrance as to what his motivation was for wanting such a machine at that period of time?

DICKINSON:

Well, knowing Mr. Watson I would say that he wanted the best of everything.

TROPP:

If he had one it just had to be better than anyone else's?

DICKINSON:

That's correct; and perhaps somebody had talked to him about the shortcomings of other computers or he wanted to have something that was much better than the Harvard machine, or whatever Aiken was doing with his Mark II and Mark IV machines. I really don't know the motivation except that he wanted it. Now those of us who were present spent several days together trying to come up with a specification as to what the... what eventually became the SSEC was to... how it was to perform and how fast it was to perform, what its capacity was, and we even got a little bit into the details of how data was to be stored or manipulated and what sort of devices might be--have to be engineered in order to realize the results.

It was thought that I should be in charge of the electronics of it, but after thinking it over it would mean that I would be going to Endicott a great deal of the time and having to spend considerable time Endicott. I didn't feel that I should get involved any more than I had been in just trying to assist in getting together the specification as to what the machine should do. So I sort of backed out and that gave me a change to continue the patent development work. So that's my only relationship with the SSEC.

TROPP:

In that period of early discussions do you remember Wallace Eckert's role and involvement in...

DICKINSON:

Oh, he was very much involved in that and he had very much in mind what the machine was to do and how fast it was to run and he really was the moderator as far as suggestions were concerned. I mean he either indicated that he could accept it or didn't or he had ideas as to what should be accomplished. Of course we knew it was to be electronic and...

TROPP:

Who took over your role of the electronic supervision?

DICKINSON:

I'm not sure that I recall. It could have been one or two people at Endicott.

TROPP:

If you remember their names perhaps I can remember also. I think I know but I was just reaching for the name just as you are.

DICKINSON:

Could you refresh my recollection?

TROPP:

I'm trying to--I'm going through exactly the same thing.

DICKINSON:

Oh, oh, I see. Of course somebody had to and... I don't even know that my notes would indicate just who...

TROPP:

I'm trying to remember too because I talked to some of the people who were at the dedication and I'm sure the proper names came up at that point, of the people involved in the design of the SSEC.

DICKINSON:

I'm sure that they were given credit for their role.

TROPP:

Well, we can go on then unless there are other things that you want to say about the SSEC.

DICKINSON:

Well, I haven't really anything more. I think I did speak about the fact that sort of obliquely we tried to help out in the SSEC by getting Corsund to direct his attention to getting tubes that had a longer life and aside from encouraging that...

TROPP:

Of course one of the problems in this time period connected closely with tube life was the reliability and the up time, if you will, the operating time of the early electronic machines, so that was an important aspect.

DICKINSON:

I think the person that could tell you about the operation of the SSEC would be William Reed. He's now retired but he's a brother-in-law--or rather a cousin-in-law of Mr. Watson, Jr.

TROPP:

Uh-huh. Where's he living?

DICKINSON:

Well, he was living in Greenwich, Connecticut, on Round Hill Road.

TROPP:

He shouldn't be hard to find then through the IBM organization.

DICKINSON:

I would think that they'd probably very definitely know where he is, but I'm sure that he could tell you much more about the kinds of problems handled by the SSEC and some of its... its operating characteristics, how it behaved because he... well, he was more or less the official guide to visitors on the SSEC.

TROPP:

What was the other item that you...

DICKINSON:

The other item is that of the turning of your attention to--of our department to the application of transistors to the business. The transistor was announced, of course, by a letter in the Physical Review, as I recall it, of June 1948 and from what was said--the letter was rather short--but from what was said it seemed as if the transistor should very definitely be investigated and looked into because it had the possibility of infinite life, power requirement only in the work circuits, and a small physical size, all of which were beginning to become problems in the application of the vacuum tube to IBM. So in December of 1948 our group went all out in investigating the transistor.

TROPP:

In addition to that one short article which is now famous, what were some of your early information sources on the transistor at that point in time?

DICKINSON:

Well, not very much. There was a subsequent article, much longer, which came out also in the Physical Review, which described the transistor in more detail and we could gather some more information but we were faced with quite a different situation with regard to the transistor--different from that of the vacuum tube. Vacuum tubes of all kinds were available and we didn't have to develop vacuum tubes except to improve them as far as IBM was concerned, but we could go out in the open market and buy vacuum tubes which we could make go in our circuits for computers. But we had to educate ourselves in making transistors and getting transistors that would last longer than the first time you put them in circuit and there were all sorts of problems in connection with getting characteristics that were desirable--characteristics that we would expect from the transistor. Transistors that we made in the early stages differed one from another. This was quite baffling.

TROPP:

They were also very expensive in that early period.

DICKINSON:

Well, we made our own because there were none available on the market at that time. As soon as we could, we put in an order for some Western Electric transistors. They were a long, long time coming and we just couldn't wait.

TROPP:

Did you have any contact with people like Bardeen or Shockman?

DICKINSON:

None.

TROPP:

You worked primarily from the little bit of written information you had?

DICKINSON:

Yes, and as we gained experience day by day by day; and we gradually were able to get transistors which lasted longer when we started testing them and putting them into circuits and trying to get their characteristics.

TROPP:

Who were some of the people in your group at that time?

DICKINSON:

Well, Carl Burkforce was the chief experimentalist and Bob Paulsen (who now lives in Poughkeepsie and is still with IBM) made the transistors for us. Eventually we got a physicist by the name of Jeffrey Knight, Jr., and he tried to guide us in some of the investigations that he thought we might make or interpret some of the data that we were taking as we tested transistors. Bob Roth in our group made a transistor tester in which we could vary the spacing between the whiskers; we could put in whiskers of different types; we could vary the pressure that the whiskers were applied to the crystal. It was a very fine, very accurate machine in which we took reams and reams of data.

TROPP:

There's an interesting thing that I'd like to have you expound on for a minute and that is the shift from vacuum tube technology to transistor technology is two different technological worlds, which meant that you had to get into a whole new field from what you had been working in for the past fifteen or twenty years. I guess I'm curious about how you made that shift.

DICKINSON:

Well, it wasn't a hard shift to make because there was just an intuitive feeling that if we were successful with the transistor that we would have a useful device at IBM.

TROPP:

Well, I guess I was thinking more of the, you know, the different physical theory related between the two worlds.

DICKINSON:

Well, now, I understand the import of your question. The physics of the vacuum tube we left to Corsund and didn't think of much about ourselves. In other words, we were not concerned with what made a vacuum tube tick. Because of the limited personnel that we had in the group we couldn't get into the physics of semiconductors very far, nor did we attempt to. In other words, we didn't set up an establishment where we could inject impurities or make different types of crystals and examine the relative merits of germanium versus silicon or any other semiconductor. We just directed our attention to getting not only the transistor itself but eventually getting transistors which were reproducible and applying them to our type of circuit. In other words, we were trying to get circuits which would flip-flop or scale or amplify; and we tried to get circuits which we could interconnect and make devices which would add multi-denominational numbers. That was our objective and as I say we just didn't... we read everything that we could and tried to understand everything that we could get our hands on relating to the transistor, because we did not at that time get a transistor which corresponded in its characteristics with a Western Electric Type A. In other words, we got current amplification over an extremely limited range--that is, an alpha greater than one. But we were able to produce transistors in which we would get voltage amplification.

Now it took us quite a while to get to the point where transistors could be made that one transistor could be replaced in a circuit for another transistor and make sure that the circuit would operate like you could with a vacuum tube.

TROPP:

You mean uniform characteristics in every transistor you produced?

DICKINSON:

We got very non-uniform characteristics and we... it took us quite a while to get to the point where we could make a transistor relatively uniform transistors. Now what did we do? We realized that we did not get a transistor with alpha greater than one but realization was had that if you combined the transistor with a vacuum tube, that you could realize scale circuits and flip-flops, and that you could connect them in tandem and get arithmetical results.

So the vacuum tube, for reasons which we didn't explore, had a tendency to take transistors--to accept transistors of diverse characteristics and sort of bring the characteristics into a set of common limits.

TROPP:

In a sense stabilized them within the range that you were interested in.

DICKINSON:

It stabilized--that's a very good word--is stabilized the transistor and the vacuum tube, I would think now from hindsight, had the faculty of recognizing the differences in characteristics between our various transistors and, as you say, stabilizing them so that we got circuits that used a vacuum tube. Now where we could use transistors as amplifiers--voltage amplifiers well then we didn't need a vacuum tube because the transistor was simply to amplify and if it didn't have the same characteristics why you could adjust the corrective resistance and get the output that you wanted.

Now somehow or other we had the feeling that a lot of our difficulties in securing transistors of uniform characteristics was the matter of burning of the whiskers to the surface of the crystal and we developed a--I don't know whether it was Burkforce or Paulsen--developed that we called a burnt in circuit and it comprised essentially a circuit in which increments of current could be added--increased--to the electrodes and we could see via an oscilloscope circuit which Burkforce developed for displaying characteristics--that we could develop these characteristics in a burnt in process, so that if they were narrow to begin with we could make them grow in extent, so that we got a range of voltage amplification that we wanted.

Well, and we also--now the burn in was done principally on the collector and we would usually just stabilize the emitter by giving it a single shot of relatively high current and that might change the collector characteristics somewhat but never to a harmful extent. Now this burn in not only gave us transistors with relatively uniform characteristics where we could take a transistor and insert it in another circuit for the transistor that was in that circuit and get the circuit to operate, and that's what we were looking for and so that is about what we did. Now eventually we got the transistors from Western Electric, the Type A transistors and there again (Lon Wood?) learned what he wanted to do with transistors having an alpha greater than one and he tried to make bi-stable circuits just using the base resistance and without a tube. Well he got very poor results. I mean he could not... he ran into a great deal of instability, not only in the transistor itself--a single transistor, but trying to substitute one transistor for another, one Type A transistor for another.

So there again we turned to the vacuum tube and he developed some circuits using the Type A transistor which employed a vacuum tube in conjunction with it and as a result the circuits stabilized and he was able to substitute one Type A transistor for another Type A; and there were a lot of unanswered questions as far as our work was concerned but we just couldn't tackle all the problems.

TROPP:

Were you the only group at that time at IBM working with transistors?

DICKINSON:

That's right. We turned all our work and instruments, that is up to that point, over to the

Poughkeepsie Laboratory and they took off from there. Now, there the program enlarged into understanding the physics of the transistor and developing transistors for IBM. Now we continued in our own way of going further by developing pulsing circuits, bi-stable circuits, and circuits which were peculiar to IBM's machines--that is, specifically circuits for adding, subtracting, multiplying, and dividing.

TROPP:

When did you first begin to see the circuitry involving transistors, possibly in conjunction with vacuum tubes and possibly not, begin to show up in the conventional IBM equipment?

DICKINSON:

Well, while we continued to use transistors in connection with vacuum tubes, that was not in Poughkeepsie's thinking. Poughkeepsie was thinking about all-transistor devices, which was rightly so. I mean...

TROPP:

... eventually they were proved right. I was just wondering in this intermediate period before transistors became useable, you had gone to any machine which had any combination of those.

DICKINSON:

Tubes and transistors? No, we did not. We built circuits which, as I say, would add and subtract, multiply and divide and that's about as far as...

TROPP:

I see, but they were never then reproduced in a production model?

DICKINSON:

No, no. The first machine that I can think of today that could be characterized as a transistor machine is the 607 and that never went into production.

TROPP:

Could you describe the 607 to me then, since I don't know it.

DICKINSON:

I can't because I don't know it myself. I mean it was a... I think it was simply a single model machine. I may be wrong but I couldn't... I would say that it was simply... I would guess that it was simply a transistorized version of the 604.

TROPP:

Sounds reasonable.

DICKINSON:

So, as I say, right up to the end and when we were doing this work, why we were fussing with transistors and tubes. Well, I think that just about winds up phase three.

TROPP:

OK. Are you ready to begin phase four or do you want to take a short rest?

DICKINSON:

I'll take a short rest.

[BREAK]

I've got to return before I go any further to two other interesting aspects of phase three. The first is that we did make a contribution to the ENIAC in that the Moore School of Engineering people and... I'm not sure that either Mauchly or Eckert--I guess they were there for the first meeting and they requested that IBM furnish them with the card input and card output equipment--that is, the card reader and the card punch for the ENIAC. Well, I was made responsible for seeing that that was done and I assigned it to one of our associates, John Wheeler; and I went to Philadelphia several times, not many times, to learn from Eckert and Mauchly or both of them, or Brainerd, just what they wanted and they specified what they wanted and... Wheeler, once we got the specifications, Wheeler had absolute charge to seeing that those machines were provided. If he was running into problems I sort of would step in and try to get him ironed out, but for the most part he was very able to handle it and we eventually supplied them with the card reader and the card punch.

TROPP:

What was your reaction to ENIAC?

DICKINSON:

I never saw ENIAC. I for a long time had no knowledge of what they were doing.

TROPP:

Of course that was also a classified project.

DICKINSON:

Yes it was and that was made very clear to us the first time we went there, almost to the point that we weren't to inquire and they weren't going to tell us and I think at some point there's an indication that Wheeler learned... maybe saw the machine, but I never did because I was involved in other matters and I had perfect confidence in Wheeler, as I did with all our associates, that once they got a responsibility they knew they had to... and if they got into a bind they could call on me to help them out.

TROPP:

Did you see ENIAC after it was announced...

DICKINSON:

I never saw it, never saw it.

TROPP:

If you're in Washington I'll show it to you.

DICKINSON:

I think I've seen a portion of it at the Smithsonian, but I never saw it when it was in Philadelphia or at Aberdeen. That's right.

Now another portion of phase three that must be mentioned, that the Naval Communications Annex came to us and wanted to--IBM to supply them with a machine which would transfer information from card on to film.

TROPP:

You had mentioned this earlier and said you would get it.

DICKINSON:

That's right, and R. I. Roth, who was in our group, was given the task of engineering and developing that machine, which he did and which was eventually delivered to the Naval Communications Annex.

TROPP:

Did you ever get involved any more in this whole problem of handling information on film, either input or output information reading, or putting information on film and taking it off later on?

DICKINSON:

Only the last thing that I was at the least involved in was seeing that the Naval Communications Annex got the machine they wanted.

TROPP:

I see.

DICKINSON:

And... well... in the sense that you've asked the question, I never was...

TROPP:

Because in this--near the end of this period that you're talking about and into the next phase--time period--this is one of the areas that's investigated for the input of data in computers and I wondered if you had any role or any involvement in that.

DICKINSON:

No I did not. Now I'll come to it in phase four where I did employ film for a specific purpose. Well, we continued right along employing the transistor in our types of circuits. We continued along building electronic circuits and devices and I will not be any more specific than that. In other words, I'm just trying to indicate that our interests in those areas continued if not to a lessening degree because or interests in other areas. Now one of the things that interested me was the fact that the wind tunnel devices, analog-to-digital devices, were relatively slow, albeit they were highly accurate. So I directed my attention to a lot of different types to analog-to-digital devices employing principally vacuum tubes, transistors where they could be employed, but primarily the devices were devoted to reading either linear motion or rotary motion and certainly we realized--or got devices which were very much faster than the wind tunnel equipment because they were electronic.

We did have some problem getting the accuracy that we had in the wind tunnel equipment, one part in 10,000, but we were able to achieve one analog-to-digital device which had an accuracy of plus or minus one part in 2,500 and ... But my feeling was that it made just as much sense for IBM to read instruments directly and translate the information into digits and punch up cards, or record it however it wanted to, as it did to punch out the cards manually. In fact where instruments were involved it seemed to me that it was foolish to translate or have a person go around and read the instruments and put the values down and then hand them over to a punching room to punch up cards. Why not do it directly, because in my experience over the years thousands and thousands of instruments were read and it seemed to me that in many, many instances the data had to be reduced or processed to get some meaningful results. That's why I devoted my attention to that area and consequently a lot of different devices.

TROPP:

This period you're talking about is after 1948?

DICKINSON:

Yes, this is commencing in 1949, phase four. One of the devices which used analog-to-digital equipment was two in tandem, where in effect it was an integrator, an electronic integrator where the output of one became the input of the next one and I had the feeling that a lot of computations could be made by a fast-acting integrator, particularly to determine boundary conditions or limits; and once those limits had been determined--perhaps roughly--then the discrete data could then be employed in detail by a digital computer to really compute the fine points that would be more evident from working with digital computers. But I thought of digital ________. an integrator using analog-to-digital devices electronically would be an in-between machine.

Well, another thing I interested myself in was code conversion, going from say binary code to decimal code or decimal to binary or coded decimal to binary--and all sorts of combinations of going from one type of code to another using vacuum tubes and transistors where it was possible. Nothing ever came of it but at least the company could use it if it chose to, because I thought that the time was perhaps right (ripe?) where we should go from one code to another very rapidly and that it might be useful in computers.

TROPP:

Just turned out you're right.

DICKINSON:

From time to time I used our machines for various purposes and it seemed to me that there was an awful lot of manual manipulation involved--that is, operators running machines and so on. For instance, every time you made a pass through the sorter the operator had to collect the cards and put them back in the hopper. So one of the phases of my thinking was, what can I do to get the operator less involved--in other words, making our standard line of machines more automatic from certain respects and ... so one of my interests was the automatic plugging up of the plug board from cards for, say, an accounting machine, so that by putting in the proper instructions in the cards the various circuits in the plug board would be automatically connected up to give the desired results when it was placed in the accounting machine, because the plug boards were getting enormous. They were forests of wires and it took a long time to plug up a plug board. I thought, well if that could be done automatically why that would save that operation.

Another interest was automatic sorters, where the cards would start through and they'd be sorted and then they would be collected and fed through the other way and then they would go back and forth, and back and forth, as long as it was necessary to sort; and I worked up a couple of those.

TROPP:

Were there any results from either of these last two...

DICKINSON:

No, no, because you see the times were changing. We were getting away from the old line of machines and working ourselves into the computers from the very simple computers which would do anything that the standard equipment would do, up to the very sophisticated computers. I was always interested in increasing the amount of information that could go on any one record medium. In other words, in a standard IBM card a role is assigned to each of the digits plus the coding at the top for alphabetic information; and so I worked on various schemes for in effect being able to get all the digits in the space equivalent to one punch in a row. In other words, I would increase the capacity of the card for handling information several folds and one of the ways of doing it was to employ what I termed a rate manifestation of the digit. In other words, if you had a zero you would have no slope, but if you had a one you would have a very modest amount of slope. If you had a nine you would have a very steep slope. Do you follow me?

TROPP:

Mm-hm.

DICKINSON:

So that at the time the voltage being read by...

TROPP:

Is that that thing you did at zero?

DICKINSON:

The amount of light being sensed by a Photostat and producing a voltage proportional to that amount of light--that is, an increasing voltage proportional to the real change of the slope. When it reached a predetermined level it would be at a differential time along--of the plateau, so that a nine would be represented ahead of seven and so on down to the one.

TROPP:

So you were really thinking of a derivative with respect to time?

DICKINSON:

That is correct and there I recorded the... one of the ways of recording as I remember it was to have a cathode ray tube going back and forth very rapidly and putting a voltage on the vertical deflection plates proportional to the digit that was to be recorded would in effect expose the film to a wedge after the film was... and then it would appear as a wedge after the film was developed.

TROPP:

Mm-hm. Then you could read it back in much the same manner...

DICKINSON:

That's right, that's right, and that was an interesting project. The getting of circuits which were linear and which would distinctly come up to the plateau at the proper time representative of the digit--that was quite a thing to develop. It's very simple but yet it took quite a bit of attention to get it developed.

TROPP:

Only simple after it's done.

DICKINSON:

Yes, that's right. Well, throughout all these four phases there were a lot of miscellaneous things developed which I have not mentioned.

TROPP:

I realize that. I think you mentioned well over a hundred personal patents, as well as more than that number of joint patents--close to 300 combined.

DICKINSON:

Well, they were... no, the 135 of mine might be sole or joint. I was not involved in any of the other 176 patents. They were Bryce's and those of our associates.

TROPP:

I see, I misunderstood then.

DICKINSON:

No, they would be joint or sole, but not with me.

TROPP:

But you did have 125 patents?

DICKINSON:

135. Yeah, so...

TROPP:

Let's talk a little about... OK.

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Interviewee: Arthur H. Dickinson
Interviewer: Henry S. Tropp
Date: March 9, 1973

TROPP:

This is a continuation of my discussion with Mr. Dickinson. The date today is the ninth of March, 1973. We were going to talk about some of the developments that you were involved in, some of your basic patents that ended up in various IBM machines.

DICKINSON:

All right. One of the patents that I got was realized in a punch which was made especially for Dr. Eckert at the Watson... or not the Watson lab at that time but the Astronomical Computing Bureau. It was a one-only sort of thing and, as mentioned yesterday, it took an amount that was positive and converted it to its complementary--or complement amount.

TROPP:

So he could deal with both positive and negative...

DICKINSON:

Positive and negative numbers. Now one machine that I didn't mention which was either developed or... developed either in phase one or phase two, about which we spoke yesterday, was called the factorial multiplier. It was sort of a special machine which was built by Mason Cunningham in Endicott, and he and I are joint inventors, and it... as I say, it was sort of a special purpose model to prove certain principles and it is covered by at least one patent and the machine never went into production, but the model machine that was constructed was sent to the Watson Lab where it proved to be quite useful.

TROPP:

Right. The need for factorials in mathematics is fairly common.

DICKINSON:

That's right. A number of the patents have claims reading on the Aberdeen relay calculators and there is one patent directed specifically to that machine.

TROPP:

Would you want to give me the patent number that's directed specifically to that machine?

DICKINSON:

I'll have to look it up. I don't... I just have the patents here which have claims reading on the machine but I don't... I'll have to look up the specific patent that reads on the Aberdeen... or that was based on the Aberdeen machine. The wind tunnel equipment is covered by two or three patents and a number of our patents have claims that read on the Harvard machine.

TROPP:

I've seen the basic patent on that machine.

DICKINSON:

Yes, well that was in the name of Lake and Durfee and Aiken and Hamilton and Aiken, but prior work which resulted in patents had claims in them that read on certain features of the Harvard machine.

TROPP:

I think I have a document that covers that. It's sort of an IBM written history of the Harvard machine in which it indicates the components and the technology that went into it and where the basic patents were. I'm not sure that that goes much beyond... it may not go beyond the newer patents relevant to that machine but I'm assuming that the others had already been covered.

DICKINSON:

Sure. Also a number of our patents have claims which read on the SSEC and the same was true of the CPC. Now I think that pretty much covers patents which resulted from work not specifically directed to many of the machines which I have just mentioned. Now others of our patents read on machines which were... which had claims reading on machines which were manufactured and sent out and rented by IBM and they're of course, as they were called at that time, accounting machines or calculators or multipliers; and those machines include the 601, the 602, the 602A, and the 603, the 604, the 605, and the 607. Two or three of our patents also have claims covering features of the 701 and 702, also the 940 and the 620.

TROPP:

Nothing on the 704 or 705, which were later machines than the majority of the group that you mentioned?

DICKINSON:

It's different for me to answer that question because for a period of time it was my responsibility to collect information with regard to any of IBM's patents which had claims reading on particular machines. In other words it was a centralized file of how our patents--all our patents were being used and that phase of my responsibility was terminated. The responsibility for keeping track of IBM's patents having claims reading on its won machines was transferred to the individual patent locations throughout the country, which were growing in number. Now, there are other phases of patent usage which I think sometimes are overlooked and the information regarding such usage is almost impossible to obtain and I'm referring specifically to our patents which may have been cited against IBM's own patent applications.

Now we know that that is true, that it happened, but the extent to which it happened I have very little information.

TROPP:

It would require somebody to go through all of the patents.

DICKINSON:

That's correct. I've gone through a few of the patents and found out that patents issuing to our group were used against IBM's patent applications. Now this may seem to sound like a negative aspect of the work which we did, but actually I feel that the fact that some of our patent applications were somewhat anticipated, was a good thing for the business. If machines were constructed according of those patents in which our patents were cited, we didn't have to go out and secure licenses or pay royalties.

Now another phase of the patents issued to the patent development group is the extent to which they were annoying to competitors of IBM and there again the information is awfully hard to get. It's obtainable but it's a monumental job to get... or just scan all of our competitors' patents and see to what extent either our patents or any of IBM's patents proved to be annoying.