RESEARCH DIVISION
Technical History Project
October 29, 1980

Memo to: The Saphire File

Subject: Summary of Interview TC-1 with W.J. Eckert 93 pp.
7/11 and 7/20, 1967

Eckert became interested in serious computing when he started work in astronomy on graduating from college in 1925. Every astronomer at that time was expected to acquire competence in computing. Principal tools were the logarithm, the desk calculator, and Crowley's Table of all the products of 3-digit numbers. There was also Peter's Table of 2-digit by 4-digit numbers, with which, using double precision, you could get a 4 by 4 product.

He came to Columbia in 1926, after a year at Amherst, where he received the master's degree in astronomy. At Columbia, he was a teaching assistant who also looked after the new observatory that had been built on top of the Pupin Laboratory.

He became an instructor the second year at Columbia, while also taking courses, but within 5 years he had a Ph.D. from Yale (1931). Probably came across Comrie's work at Yale.

Soon after starting to study astronomy, Eckert specialized in the (high precision) astronomy of position. His doctor's thesis was in celestial mechanics, under Prof. E.W. Brown at Yale. While at Columbia, Eckert saw IBM machines in the laboratory of Ben Woods, an educator interested in scoring and "discussing" (sic) new type tests. He had persuaded IBM to install computing machines to help him in the "discussion" of these tests. Eckert could see that these machines would be of great help in astronomical calculations.

The equipment included a tabulator, a sorter and a reproducer, and a special tabulator called the Difference Tabulator which had been built specially for him. It had ten 10-digit counters and it was possible "to transfer by addition or subtraction from one counter group to another," so it could do first and second differences of various orders without reading in a new card every time - and at 150 cards per minute, 80 digits per card - tremendous adding power.

People had devised ingenious methods for its use. One was called the Mendenhall-Warren-Haller method of computing intercorrelations - now called progressive digiting - "a good way of getting sums of products and mass production."

He observed that Babbage spoke of a Difference Engine and an Analytical Engine. The former was capable only of linear operation - good for systematic tablemaking, as was Ben Wood's big tabulator. But Eckert was interested in general scientific work, and needed all of the operations including the use of tabular functions. He needed an Analytical Engine.

(He hadn't heard of the Analytical Engine at that time.)

Eckert understood that IBM had just marketed a multiplying punch, and in about that time they came out with tabulators capable of subtraction. The Difference Tabulator "was also not a credit balance machine.... If you wanted subtraction you normally used two counter groups" and somehow subtracted one from the other, "so, by the time we had the direct subtraction accounting machine and the summary punch and the multiplying punch... it was only a matter of getting the proper engineering to make these functions more general and hence suitable for computational work."

Eckert was able to get the proper equipment from IBM, with Ben Wood's assistance, because Mr. Watson, Sr. was interested in novel uses of the machines as long as they were socially useful - even though not profitable. Eckert didn't have to worry about the economics, and he had the machines by 1933. They included "a multiplying punch with special sign control and other equipment in it, and a subtraction tabulator which was somewhat special in that it had four 10-digit counter groups instead of five 8." Eckert also got hold of the pluggable relay box from the big machine (the Difference Tabulator) which by then was unused by Ben Wood. (Agrees with Saphire's comment that it was the so-called Columbia machine of 1931, which was out of use by 1933.) It had been accompanied by a big box of relays in series with the plugboard terminals. Eckert made this box part of his control system.

(Digression on Ben Wood's activities. He had Warren working on an automatic test score (scorer?) but it was never successful. Rey Johnson developed one independently and brought it to IBM and Wood, who later used many machines based on it.)

More on the equipment Eckert got from IBM: Scientific computation required a multiplier capable of producing negative results from operands of different sign. "In business at that time, a negative number meant that you had to call the Vice President," "Somebody had overdrawn his account or done something horrible." One of the IBM engineers rewired the multiplier to produce the correct sign, depending on the X punch on the card.

One very effective thing Eckert did was use the (new) reproducer to reproduce selectively (based on their X punches) table values from table cards previously merged with the detail cards. Computation was arranged so that these tables (which might, for example, be difference tables) were created for the occasion, interspersed with cards containing the argument, as indicated above, and thus avoid "computing all of the values that required this value function."

Eckert had gotten the equipment easily, using as contacts Ben Wood and many IBMers - Halsey Dickinson, Steve Dunwell, John McPherson, sales people, and Customer Engineers. Always had a warm spot in his heart for the CE, who could tell him things the salesman didn't know, including what changes were easily possible, and even getting permission to make them "when nobody was looking." It was a gradual building up of the power of the machines and what he could do with them. Everybody knew Mr. Watson was interested, and Eckert thinks that made things easier. There wasn't an itemized account for rentals.

Those machines were used for astronomical computations until 1940 when he left for the Naval Observatory, and were still running then. (Comment from later page 29: Nobody - observatory or other - had anything like this setup. As the war required computation for many purposes, large amounts of punched card equipment were set up by people who (by implication) had visited Eckert's setup: Los Alamos, Aberdeen, Dahlgren, and Naval Observatory. "Really, a very large part of the computations that affected the war were made on IBM equipment." "The ENIAC, for instance, was developed during the war but it didn't do any war work. The IBM machines were going constantly.") "The whole top of Pupin was full of machines during the war."

(Pages 18-21: Fascinating discussion of how the equipment was used, in the 1930's, to solve equations of planetary motion. Can't do justice in a summary; recommended reading.)

As for "program control": There were two kinds of computations - batch processing and the kind requiring an elaborate program. Where you could use batch, with the enhancement due to the selective table cards described earlier, this was the most effective use of punched card machines, because you didn't tie up the whole system to use one machine. All of the interconnections among the machines were switchable and pluggable, so that the "computer" configurations could be converted to this efficient "batch" configuration in ten minutes.

Numerical integration required the system setup, in which a mixture of mechanical and punched-card programming was used. The sequence of use of the various machines was planned to minimize the number of complete pases; in the numerical integration, there were, for example, about three card handling operations in two minutes. General approach was to use the mechanical programmer for the broad outline of the problem, and punching on the cards for filling in the details. Numerical integration is cyclical. After, say, one hundred arithmetic operations, you replace the data you started with by the results you have just got and do it again.

For this they had the "control box" which had a number of disks in which they filed nicks to tell the different machines what to do on each of about 20 steps (CJB Note: This equipment appears to be pictured on pages 114 and 115 of Computer Perspectives, the Charles Eames book). You didn't use all of them unless you needed them, and the rest of the control was by cards. "X" punches in the various columns, for example, could change the number of differences you were using.

This assembly of machines left much to be desired, but it was a compromise between what was available and what would be most effective. They felt they should start by using reasonably simple equipment and not spend a lot of time and money on development; and they got a lot of experience. They wrote their program so that, for example, when an error occurred, they could pick up the cards from a few steps earlier and do that portion over. The different methods were very sensitive to errors, so they were easily caught and corrected as just indicated. "It has been amusing to see people struggling with this same problem with the more elaborate computers of recent years."

(In answer to the question of whether Eckert saw this operation, at the time, as pre-modern computing, modern computing, or what?) Well, it was revolutionary in that it didn't involve desk calculators or hard-copying of the numbers. It made it possible to do, say, a hundred times as much work as you could do by hand. Which is more interesting, the first step of 100 times or the next - resulting in a total of 10,000 times? Making and using tables is still done, but the economic balance has shifted in favor of computing each case as you go along. (CJB Note: Further discussion again emphasized the Eckert philosophy that computing was his objective, and his predisposition to use existing machines as ingeniously as possible, with as little modification as possible, to accomplish it. He has always worried - perhaps excessively, he acknowledges - about getting the most out of his equipment. Clearly, the development of computers per se was not the uppermost goal. We must look elsewhere for those with that obsession.)

Eckert had been a Professor of Astronomy at Columbia when he joined the Naval Observatory as Director of the Nautical Almanac (a prestigious job - civil service - Eckert was invited after a national competition.) When he arrived, N. O. had only desk calculators - not a single machine that would print figures automatically. He was able to set up a sizable computing center using pretty much the same kind of IBM equipment he'd had at Columbia. By then, however, the alphabetic accounting machine was available. Not that Eckert wanted the alphabetic feature, but the machine was designed for greater flexibility. "We could stick in wheels that counted by 6's instead of by 10's" and, by placing these properly among the decade counters, they could degree-second indications. They did not build a sequence calculator, since they were doing only batch processing. They spent more time at the making of tables, and automatic printing.

His first task as director of the almanac was to design an air almanac for the rapidly expanding air force. By speaking to IBM, he got equipment in an astonishingly short time, and by January 1941 had the first issue approved by the Armed Forces and into the training camps. The air almanac tells the navigator quickly, for any time of night or day, when the sun, moon, and several other brighter celestial objects at the time are. For the first time, no compromise was necessary because of difficulty of making tables - and the navigator got exactly what he wanted.

At about the same time Eckert was duplicating the batch aspect of the Columbia operation at Naval Observatory, Aberdeen, Dahlgren and Los Alamos were doing the same - ballistics and planetary motion requiring comparable computations.

The almanacs had to be set in type, and the printing establishment introduced errors. Eckert was pleased with the method devised to check their work. They had a keypunch operator (they were easily available) copy the printer's proofs, and then compared these cards by means of a reproducers, with those from which they had tabulated the output that was sent to the printer. Eckert's cards, of course, were very skillfully checked by procedures referred to earlier. Even after they had devised methods of creating reproduceable copy for the printing firm, they used this method, just to detect illegibly printed characters.

The method of preparing plates by photography from the tabulator output was used for about a year or two. It didn't produce really satisfactory results, because the tabulator line has character spacing that is too wide in proportion to its height. So Eckert went to IBM, and Ron Dodge at Poughkeepsie developed a table-printing, card-controlled typewriter, which produced a fine table. (A second one like it had been delivered after twenty years, and was in use at the time of Eckert's interview.)

Mr. Watson invited Eckert to join IBM, in 1944, and Eckert did so, apparently in March, 1945, about five years after coming to the Naval Observatory. His assignment was apparently to assist the scientific users of computing equipment, help IBM to understand their requirements, and hire new people as required; "it was more or less of a blank check." After about three months at 590, he set up some scientific computing equipment in Pupin laboratory by invitation, and for 6 months or so they were helping people from (e.g.) Los Alamos, and John von Neumann was in contact with them. ENIAC was being built and "it was in the air." The Harvard machine came in there, so there wasn't any question of building special scientific calculators, but what to build and where to get the equipment.

Then at the beginning of 1946, they got a little building on 116th Street, and that was the beginning of the Watson Laboratory. They assembled equipment as fast as they could. They had engineers assemble experimental models; they had relay machines by Pete Luhn and others. Early, they got Halsey Dickinson's electronic multiplying machine. "Of course von Neumann was also aware of the Harvard machine and the IBM relay calculators at Dahlgren and the ENIAC, and we all knew that sooner or later we would have electronic calculators that would be generally useful." They acquired Thomas, Seeber, Grosch and others. Seeber didn't come directly from the Harvard Comp. Lab, but had been there.

The arrangement with Columbia was exceedingly informal, and it was understood that either party could terminate it. IBM, bought the building (a run-down fraternity house) and gave it to Columbia; and Columbia allowed IBM to occupy it. IBM spent a lot of money making it suitable for a lab, but it worked out quite well for many years. When "this building" (evidently the later Watson Lab building on 115th St.) was acquired, the same arrangement was made, but there was a letter stating the agreement, it being a much more expensive undertaking.

Eckert was made a Professor of Celestial Mechanics, Thomas a Professor of Theoretical Physics, and the tradition was maintained - people carrying Columbia appointments but at no salary beyond their Watson Lab salaries - until eventually 7 or 8 of the people had academic appointments.

The Watson Lab had two interests. First, computing and applied math and its relation to the computer industry. Second, what should machines be like to be most useful. He had the applied mathematicians - Thomas, Seeber et al. For the second problem he went to MIT, where the wartime Radiation Lab was closing down, and hired Havens, Walker and Lentz.

Scientists were invited to come and use the equipment - which always included experimental machines - and they came from all over the world. There were a number of engineering models of relay computers, including two that Luhn had built. One was a fast arithmetic unit that they attached to an accounting machine and made a sequence calculator that was very much like the later CPC. The arithmetic was a little slower, but the programming was more flexible.

Eckert and his staff had discussions with all the engineers in the company, including Hamilton, Lake, Beattie, Page and Luhn. Hamilton had built the Harvard machine, "which used wheels." Now the question was what could be done with electronics. Palmer and Dickinson were there, and had built electronic counters and electronic multiplying units. There was an intensive, useful interchange between the computing scientists and the engineers. In batch processing the flow is simple, but with the automatic computer you want numbers to flow in all directions. It was decided that there was to be an SSEC and that Hamilton was to build it and Seeber would work very directly with him on it. (Thomas, Seeber and Eckert, actually, all kept close to it.

When development got underway, Seeber went to live in Endicott where Hamilton had his lab. Hamilton had had no experience with electronics, but he got some electronics people to work with him, and finally the design was solidified. It was a compromise based on how many elements of each kind an engineer was willing to guarantee would work.

(CJB Note: Pages 25-31 contain a rather detailed discussion of design trade-offs, and of the final design, of the SSEC. Probably can't be condensed, and should be read.)

At the dedication, the maching was able to computer each position of the moon in a few minutes, where formerly a man could do two positions per day and barely keep ahead of the moon. Problems in all areas were put on the machine - optics, atomic energy, hydrodynamics, astronomy, etc. - and it was decided not to charge for machine time because the agency with the most money would tie it up.

"This machine was really a full-blown stored program calculator and I don't know how this came up but there never seemed to be much of a discussion about it between Seeber and myself. Now the Harvard machine had instructions on a paper tape and when you once had the instructions, that was that. You just played them off. The astronomy computer which preceded it allowed a certain amount of flexibility... you could change the program cards a bit.... But we felt that this was not adequate, that you really had to be able to make choices as you went along and that the instructions should be arithmetic so that you could... use the calculator itself to operate on the instructions.... There was no difference between instructions and data as far as the storeroom was concerned."

Seeber, in Endicott with Hamilton, was making decisions on detailed choice of instructions - e.g., do you branch on plus or on minus or on both? Later Ken Clark went up and his job was to program the dedication problem under Seeber's direction. Becky Jones, one of the Watson Lab staff, worked with Eckert on outlining the program, so all Ken Clark had to do was covert it to instructions.

(Discussion of machine failures - frequency and how they were handled.

Technology had been pushed about as far as one could.)

A group of programmers was set up - about a dozen people - that included Rex Seeber, Clark, John Backus and Don Quarrels. Programmers were drawn from disciplines - not necessarily mathematics - that were quantitative and rigorous: math, engineering, physics. This group later became Applied Science. FORTRAN came out of it.

Comparing the SSEC with other machines of the day: "ENIAC was not a general purpose computer." The thing it did well was the ballistics problem - faster than the shell would fly - very dramatic. The Bell Relay Calculators would be more like the SSEC, but the SSEC was the first general-purpose, stored-program electronic computer - electronic in the sense that the operations that had to be fast were all done electronically.

There had been no influence from outside, on the SSEC, as far as Eckert was concerned. He was "not one to run around. If I had a problem I worked on it." Seeber had worked on the ASCC at Harvard, but the programming on it was not very powerful. Hamilton, of course, got invaluable experience from its design and construction. Between Seeber, and Hamilton with the mechanical people and the "electronics boys," "it was a powerhouse of design and construction." Hamilton had had reservations about so much electronics in the machine, but Palmer and Dickinson had made electronic arithmetic units.

Eckert said he knew nothing of the "interchange" between Watson, Senior, and Aiken around the time the SSEC was unveiled.

Havens, Walter and Lentz, from MIT's Radiation Lab, had worked on microsecond circuitry, were convinced nothing in the future would be slower than that, and had nothing to do with the SSEC with its radio-tube circuits and modest pulse rates. They went in their own separate ways - Havens toward the big calculator and Lentz toward the miniature, including the personal automatic computer he designed and finally marketed.

The Navy asked whether Eckert's lab would build a calculator for them, and Havens' ideas for a large, fast computer seemed to meet the need. Eckert and Thomas helped in defining what the machine would do, Jeenel was responsible for the programming (had had experience on the SSEC), and von Neumann was involved as a consultant for the Navy.

NORC had very few types of pluggable units - essentially only one circuit type for the whole multiplication function. (Perhaps this refers to the microsecond delay circuit used throughout the machine.) It used Williams tube memory, but the Navy eventually replaced that with core. Eckert thought (1967) the machine was still in use. It had been running at Dahlgren, beside a Stretch.

Eckert was not much involved with such considerations as UNIVAC, or other competition. "With the SSEC we had... the stored program calculator.... NORC was a sort of a culmination of the big fast electronic calculator," and while it was being built the Watson lab was shifting its emphasis to physics, largely solid state. Is policy was to stay small, so areas of investigation that became key to the business usually grew elsewhere. Art Anderson worked with Walker on Spin Echoes, then went to San Jose. John Lentz joined the cryogenic project at Yorktown and then returned to a different project.

(Winds up with repetition of the philosophy of Watson Lab, from the management of which Eckert (1967) had been withdrawing for three years. Watson Lab fulfilled its role of initiating scientific work in the company, and counts among its distinguished alumni Havens, Anderson, Tucker, Holtzberg, Reisman, Rosenheim, Walker (and others such as Garwin and Lentz discussed earlier.)

C. J. Bashe

/ccc

WATSON LABORATORY
NEW YORK CITY

November 30, 1967.

Memorandum to: D. E. Udell

Reurned herewith is copy of the portion of the interview with Dr. Eckert in the IBM Oral History Project which you are proposing to submit to DATAMATION magazine. With copy of this memorandum, I am returning the complete draft to Mr. Saphire. In both cases I have attempted to clean up obvious things but the general sense of the interview reads very well. If I can be of further help, kindly let me know.

H. K. Clark

Encl.
cc- Mr. Lawrence Saphire
(with enclosures)

SMITHSONIAN INSTITUTION
THE NATIONAL MUSEUM OF HISTORY AND TECHNOLOGY
WASHINGTON, D.C. 20560

January 10, 1972

Mr. I. J. Seligsohn
Manager, Museum and Exhibition
Department
IBM Corporation
Armonk, New York 10504

Dear Mr. Seligsohn:

As I indicated in our telephone conversation last Wednesday, there are a number of requests I would like to make related to your historical holdings.

The Project does not have an oral history interview with Wallace Eckert. An interview with Mr. Eckert had to be cancelled last spring due to his ill health, and he died before another was scheduled. I wondered if it would be possible to give us a copy of your tapes (TC-1 in the IBM Oral History File) and the appropriate transcript. Should you decide to give us this material, there are two possible ways in which it can be handled:

(1) If you desire that the material not be made public, we can, at your request keep it in our locked file. Under the condition it would be available only to specific requests that you have agreed to.

(2) It can be handled as we would one of our own tapes. That is, I would edit the transcript, and submit it for appropriate clearance to you and/or Mrs. Eckert. A copy of the SI Release form is attached.

A second request emerges from page 29 of Mrs. Brennan's "The IBM Watson Laboratory at Columbia University: A History". Do you have a copy of Prof. von Neumann's address at the dedication of NORC? If you do, can we obtain a copy for our files?

Again I would like to thank you for all of your kind assistance.

Sincerely,

Hank

Henry S. Tropp
Principal Investigator
Computer History Project

SMITHSONIAN INSTITUTION
THE NATIONAL MUSEUM OF HISTORY AND TECHNOLOGY
WASHINGTON, D.C. 20560

RELEASE

NAME ______________________________

Date of Interview: ____________________

I, the undersigned, understand that the record of this interview becomes a part of the National Collections in the National Museum of History and Technology. I also agree that the interview will be used according to my wishes indicated below:

I. _____ I place no restriction on use of this interview.

II. _____ I wish to limit uses of this interview.

A. Except for passages subject to the restrictions noted below, the content of this interview may be made available to interested groups or individuals as follows: (initialed items indicate my wishes).

_____1. A copy of the transcript may be read in the Smithsonian Institution.

_____2. The tape of the interview may be heard at the Smithsonian Institution.

_____3. A copy of the transcript may be obtained by an interested party subject to standard procedures of the National Museum of History and Technology.

_____4. A copy of the tape may be obtained by an interested party subject to standard procedures of the National Museum of History and Technology.

_____5. Other (specified on the attached page).

B.

______ 1. Passages marked by me in red in the attached transcript are to be temporarily deleted from copies of the transcript and/or tape made available under the above provisions until ________. (Where more than one date applies, the passages have been marked accordingly.)

______ 2. Passages I have deleted in the attached transcript are to be permanently deleted from all copies of the transcript and erased from the taped copies of the interview and no record will be retained.

Date ______________________ ______________________ (Signature)

Mr. K. M. Seggerman, Jr.: 1/24/72

For your review, the Eckert transcript requested by the Smithsonian Institution. Do you see any problem?

J. Barton

John Barton
Ext. 6433

[illegible]

IBM CONFIDENTIAL

Not sent

January 31, 1972

Memorandum to: Mr. D. L. Polenz

Ken Seggerman has reviewed the Wallace Eckert transcript for Litigation Analysis and feels that it may be given to the Smithsonian, but with several provisos:

1. We should take up the Smithsonian on its suggestion that the transcript be kept in a locked file and made available only after we have approved specific requests.

2. The Smithsonian should not circulate word of the transcript's availability, but should merely respond to requests.

3. Introductory identification of the project and tape numbers should be deleted.

You indicated you wanted to look into the Yale situation before we proceed further.

J. Barton

John Barton

JB:scj

cc: Mr. E. Nanas

IBM CONFIDENTIAL

February 14, 1972

Memorandum to: Mr. A. P. Smith

Subject: Wallace Eckert Oral History

Attached is the letter from the Smithsonian Institution asking for the IBM oral history interview with Wallace Eckert.

We feel it would be appropriate to send the transcript. However, we would like to take up the Smithsonian on its suggestion that the transcript be kept in a locked file and made available only after the Eckert family and IBM have approved specific requests.

If you and Mrs. Eckert feel this would be appropriate, we can arrange for the retyping here.

John Barton
Corporate Information
Ext. 6433

JUB:dac

November 16, 1972

Mr. Henry S. Tropp
Smithsonian Institution
The National Museum of History and Technology
Washington, D. C. 20560

Dear Henry:

Attached is the record transcript we discussed. As we agreed, we would like this returned when you have finished your research. We assume this will be within the next several weeks.

Please let me know if I can be of further help.

Sincerely,

John Barton

JB:gb
Enclosure

May 21, 1973

Mrs. Wallace Eckert
160 West End Avenue
New York, New York 10023

Dear Mrs. Eckert:

Enclosed are the three copies of Dr. Eckert's oral history transcript that you requested. As we discussed on the telephone, we would appreciate your keeping the history in the family for the time being.

If we can be of further asistance, please let me know.

Sincerely yours,

John Barton
Corporate Communications

JB:eb
Enclosures

444-7011
2613

Eckert

~~Fellow~~

- ~~Seymour Koenig~~ Research
- ~~Dick Garwin~~

Arnie Smith - Eckert son-in-law
X2790
1133

Seggerman:

1. Not make it public. Smithsonian Keep in locked file. Should not advertise that or broadcast. they have it

This is interview To 1 in the program.

#1

This is Interview TC-1 in the IBM Oral History Project on Computer Technology. The location is the Watson Laboratory in New York City. The date July 11, 1967, interviewer Lawrence Saphire of IBM and to be interviewed, Dr. Wallace J. Eckert.

S. Dr. Eckert, can you give me your present position or your past position?

E. My past position was Director of the Watson Laboratory. I became interested in serious computation when I started work in astronomy on graduation from college in 1925. Astronomy has always been an area where computing was important and it was assumed that every astronomer at that time would acquire competence in this area. The tools of the trade consisted of a number of things and one used these various aids according to the problem on which he was engaged at the moment. The logarithm, of course, was the principal stock in trade and everyone in astronomy was skilled in the use of logarithms. An interesting sidelight on this was even fifteen years later when I went to the Naval Observatory, the people from the University of California were always doing well on the Civil Service examinations because they had been taught the use of logarithms and were more efficient in their use than many of their competitors in other universities.

Other things we had were Crowley's Table which gave all of the products of three figure numbers and these were very effective in certain types of computations. There was another table called Peters' Table that gave two-digit numbers... products of two-digit numbers by four-digit numbers. The idea of this was that by using double precision with this table, you could get a four by four product. There was, of course, the desk calculator and this was quite effective. The one I had on coming to Columbia was a Monroe, which did ten by ten muliplication and division. Figures were all written by hand. There were no automatic devices for reading and writing of figures. There had been a few uses of the machines -- the ones which I call multi-register adding machines -- printing adding machines which one did use for different things. But essentially the desk calculator, the logarithm, and Crowley's Table constituted the stock in trade of the astronomer for his computational work.

S. When did you come to Columbia?

E. In the fall of '26.

S. Where were you before that, Dr. Eckert?

E. Well, after graduation from college I spent the summer at the Yerkes Observatory, which is part of the University of Chicago, and then I spent the academic year at Amherst getting a master's degree in astronomy.

S. So actually you started your professional career at Columbia?

E. Yes, I came to Columbia as a teaching assistant with the duties of looking after the observatory. They had just built a new observatory on the top of the Dupene Laboratory and part of my job was to see that that was put in shape for the use of the students and for others.

S. Was it at this time that you came into contact with Com~~ery's~~ri's work?

E. Well, my memory is a little hazy but the next few years I was teaching at Columbia. I was made an instructor the second year I was here and between teaching and taking courses at Columbia and trying to find a research problem, it took me five years to get a Ph.D. at Yale. This was '31. Somewhere along the line I came across Com~~ery's~~ri's work, probably at Yale.

S. Let me just ask you a question about astronomy, even though we are going to talk a good deal about computing, automatic computing and computers. Did you have a special field that you worked in in astronomy as you got your Ph.D.?

E. Yes. Soon after I started studying astronomy, I saw that I was going to work toward more exact parts of astronomy rather than the astrophysics, which was newer but not usually done with the precision of the older astronomy. The astronomy of position, of course, requires high precision in its computation compared to most astro-physical problems and it was obvious that I was going to specialize somewhere in this area. At the time I came here, Columbia had no graduate work or research in astronomy and so I maintained contact with the Yale people from the outset and took courses here that would lead toward a degree at Yale, but the guidance came from Yale.

S. Well, at what point along the line, Dr. Eckert, did your thoughts start turning to the automatic computations of astronomical data?

E. I think as soon as I saw the IBM machines in Ben Woods' laboratory. Dr. Wood was an educator interested in scoring and discussing tests of the so-called new type. At that time the true/false and multiple choice tests were rapdily...their use was rapidly being expanded and Ben Wood was one of the leaders in this and he had persuaded IBM to install computing machines to help him in the discussion of these tests. And naturally, when one saw the power of these machines on that kind of work, it was evident that they would be of great help in astronomical calculations.

S. Was that in 1926 when you first came or after that?

E. No, it was some time later. I can't remember the year. He had two young men working for him. One's name was Warren and the other was

Mendenhall. Mendenhall was later drowned in a boat accident, and I spent a good bit of time with Mendenhall. We were casual friends. I don't recall the date when Mendenhall died. Then I was busy with my research for a while and later took up the question of the automatic computer more systematically.

S. What kind of research were you doing at that time?

E. I did my thesis in celestial mechanics with Professor Brown at Yale.

S. I told you, didn't I, that he was my professor at one time also.

E. E. W. Brown?

S. Brower.

E. No, this was E. W. Brown. That was a long time ago. Brower came to Yale while I was a graduate student.

S. I see.

E. With my research work, though, it was not done with the automatic computer. That was done with a Monroe.

S. Well, when did you....you said that you got interested in automatic computation when you saw Wood's machine. Could you tell me a little bit about what struck you about them and how you started thinking in terms of applying them to astronomy?

E. Wood had quite a bit of equipment. He had an ordinary tabulator or accounting machine and a sorter and reproducer or gang punch, as they called it in those days. He had both a reproducer and a gang punch. And he had this special accounting machine or tabulator called the Difference Tabulator, which was built specially for him. It had ten digit counters, ten of them, and it was possible with this machine to transfer by addition or subtraction from one counter group to another. This made it possible to do first and second differences of various orders and summations without reading in a new card every time. This thing would read cards at standard card speed which, compared to sight reading, was very fast -- one hundred fifty cards a minute with eighty digits and adding them in various registers meant that it had tremendous adding power. And several of the people had devised ingenious methods for using this machine. One of the methods was called the Mendenhall-Warren-Haller method of computing inter-correlations. I think some people at Iowa were thinking along the same lines at about the same time. I'm not one hundred per cent sure of priorities but this method, which is now called Progressive Digiting, was a very good way of getting sums of products and mass production.

S. Had you ever come across the Schutz machine which was after ~~Babidge~~'s Difference Engine? I believe that it was in Buffalo that it was in use.
Babbage's

E. I think it was in Albany. No, I hadn't any experience with those machines. The Dudley Observatory at Albany, where the Boss' catalog originated, had one of these machines at one time. I never met the older Boss. His son was there when I was a graduate student. I had not visited the observatory at that time.

S. In retrospect, do you think knowing about a machine, such as the Schutz, would have helped you or was it just that these machines were unknown, more or less, so that people never went to investigate them and their possibilities?

E. Well, Babbage spoke of a Difference Engine and of an Analytical Engine. The Difference Engine is capable only of linear operation. In other words, it can take differences and make summations. This type of device is very effective in systematic tablemaking. But my work wasn't systematic tablemaking. I was interested in general scientific calculations where you must have all of the operations, including the use of tabular functions, and so forth. So that you really need an analytical engine. The big tabulator of Ben Wood, while it was used with ingenuity to form sums of small products, it didn't form the individual products and it was not capable of....it was not an analytical engine. It was a Difference Engine -- a very fine Difference Engine, but still limited to linear operations.

S. But I take it you were looking around, at least in your mind's eye, for an analytical engine.

E. Well, as soon as I saw Ben Wood's machine. I hadn't heard of the analytical engine at that time. It seemed that in the IBM machines there was the capability of performing all of the operations necessary for professional computing. By that time I spoke to some of the people, and I understood that IBM had just put on the market what they called a multiplying punch, which was a machine that would read numbers from cards, form their products and punch the answers on the cards. The Difference Tabulator was also not a credit balance machine. You could only make it in numbers. They came out as complements, generally. At that time, if you wanted subtraction you normally used two counter groups. If it was a positive number, you added it in one register and if it was a negative number, you added it in the other. Then somehow or other, you subtracted one from the other. An algebraic sign control was not on the standard machine. During the time that I was worrying about these problems, they came out also with the direct subtraction tabulators or accounting machines. So, by the time we had the direct subtraction accounting machine and the summary punch and the multiplying punch, it was obvious that it was only a matter of getting the proper engineering to make these functions more general and hence suitable for computational work.

S. Did you pursue this possibility with IBM people?

E. Yes, I learned a little more about what was available and what would have to be done to make a computing installation that would be generally useful and I was able to -- I don't remember the details, but at least the proper equipment arrived in the Dupene Laboratory and we were able to make it operate as an astronomical computation.

S. What year did the equipment arrive?

E. 1933, as I remember, was the time when we had most everything we needed.

S. How did you go about getting this equipment? Did you have to convince people around Columbia to bring it in?

E. Well, Ben Wood knew Mr. Watson, and Mr. Watson was interested in novel uses of his equipment as long as it was socially useful. I think he would not have been interested in novelty, in the sense of novelty's sake. But he was interested in education and science and when he heard that these machines could be useful in this area, even though not profitable, he was willing to sponsor this experiment. So the machines arrived, and I didn't have to worry much about the economics of the situation.

S. I see. Did you....

E. But we did get a multiplying punch with special sign control and other equipment in it, and a subtraction tabulator which was somewhat special in that it had four 10-digit counter groups instead of five 8. At that time, we needed more than 8-digit arithmetic. Ten was really necessary. I also was able to get hold of some of the special equipment. By that time, Ben Wood's big machine had been disbanded and I was able to get the pluggable relay box from it, which I incorporated in the control system for the new computer.

S. That was the so-called Columbia machine of 1931. That went out of use by 1933.

E. Yes.

S. You used its parts.

E. Well, the Ben Wood Difference Tabulator they called it. It was accompanied by a big box of relays and these relays were connected with a plugboard on the top of this box so that one could run plug wires from the Difference Tabulator plugboard into this box and out the other side of the box and back into the tabulator. This meant that they had much more flexibility in that machine than was built into the machine itself. This came with the machine. It was designed that way. That made it very convenient for me, because I could then take this box and plug it into my new machine and use that as part of the control system. It was a big switching box really.

S. And, at that point, the Wood machine was no longer in operation for him?

E. That's right.

S. Just to digress a second. What was the reason for his abandoning that machine?

E. I don't know. It may have been....well for one thing, Mendenhall had died and Warren had left, and the experimental part of Wood's activity had subsided somewhat. Also the machine was old. Like all special machines, it had problems that made bugs that one doesn't usually find in a commercial machine, because in using the commercial machine, they improve them as time goes on. But a special machine is built and if there are any weaknesses in it, they get worse usually instead of better and the ordinary serviceman doesn't know how to cope with them. One of the problems, at that time, was that some of the resistors would go out. So in the course of the computation you would have to take the machine apart and hunt for the bad resistor and put in a new one.

S. Some of the later computers were a little like that too, weren't they?

E. Yes.

S. Just to finish up on this Columbia machine, was it doing computations that would make it analogous to a computer, or how would you size that machine up in the two years it was in operation?

E. Well, in the first place, it was a great Difference Engine. It would read cards and do differencing and summation operations and printing in mass production and this was the first machine that did that...that had that capability. The forming of sums of products was really a trick and that, of course, was very efficient in forming inter-correlations between variables by a combination of sorting the cards and adding the various combinations, you can get sums of products and this method was a very profitable one. As a matter of fact, I used some of these techniques later in the installation of Wood's for individual products, simply because it was more economical in certain problems where you needed sums of products to do it that way. At that time the first multiplier....well multiplication was quite expensive because it took a number of cycles and the time was long. So if one could avoid multiplication, he saved money on his calculations.

S. What project was Wood pursuing? I know of him mostly being interested in test scoring and educational type things.

E. These were his principal things. He was interested in giving tests to large numbers of people, scoring them and discussing the results; by inter-correlation of one sort or another, trying to evaluate them and keep the records and all the various statistical aspects of a big testing program of that kind. For instance, he tested all the schools in Pennsylvania at one time, so that he had a great deal of this work and the discussion of the results, and the grading of the tests, were important to him and he had different approaches to this. There was one kind of test where he would have an operator go down the test, and according to the score, would press one of three or four buttons on an accounting machine and the machine would enter this result in various places, with various waits, according to the type of test it was; and that machine ran hours, and hours, and hours. He had Warren working on an automatic test score and it never was successful. But Ray Johnson, who is now with IBM, developed a test scoring machine independently and brought it to IBM and Dr. Wood, and this became the basis of many test scoring machines that Wood later used.

S. In what way did he use the Columbia machine that he got in '31 that you took parts from in '33 for test scoring, or did he use it for another purpose?

E. No. This was used for the things I've mentioned. The test scoring machine was a small accounting machine with special features on it designed especially for scoring tests. That ran for years.

S. Let's go back to the IBM equipment that was brought in to help you with your work. You had mentioned additions that were made to the machine modifications. Did you ask for those modifications, Dr. Eckert?

E. Yes.

S. And they were put in?

E. Yes. For instance, they were all toward generality. At that time the accounting machines were designed for business and always they were specialized machines. There was a sorter. There was an adder. There was a multiplier. And the idea would be to take a stack of cards and work out a simple program on the machine and then run thousands of thousands of cases on this simple program and either print the result or punch it on a card or both. In science, you need greater flexibility. In business at that time, a negative number meant that you had to call the Vice President. It meant that somebody had overdrawn his account or had done something horrible. Whereas in science, positive and negative numbers were equally respectable and when you multiplied a positive by a negative, you wanted to get a negative, etc. So that complete sign control was one of the minor things. There were other common operations such as interpolating in a table. When you look up functions and their differences and then you want to interpolate, you have a very common operation which is the form of A plus or minus B times C. Well this was not one of the standard features on the multiplier. I think it was quite a nuisance, but one of the engineers in IBM did rewire the machine so it would do this as an automatic thing, depending upon the X punch on the card. One of the very effective things that is needed in scientific computation is the use of tables. Nowadays most of our machines, fast electronic machines, evaluate the function from some kind of a routine. But computation was quite expensive in those days in a machine, so that the use of a table was very economical and the punched card....tables on punched cards played a very important role for a number of years between say 1933 and very recently. The ability to consult a mathematical table in machine readable form was one of the things that we accomplished and it was very effective. About this time the reproducer came out. We called it the high speed reproducer. This would read cards and transfer information from one card to another and it had the ability to do this selectively, according to whether or not the X punch, as we called it was on the card. So we would take our table cards and put X punches on them to identify them and then we would merge with the table card the detail cards which contained the arguments for which we wanted values of the function. We would merge these, put them in the reproducer and at 100 cards a minute we would get off the values of the function in the first, second or third, or as many differences as we wanted. This made a very economical way of getting functions or computations of a special kind. As a matter of fact, we would in the interest of economy, arrange our computations so that we would make tables for the occasion and then use them rather than computing all of the values that required this value function.

S. Let me just again go back a minute to a non-technical aspect. In getting your equipment from IBM, did you deal directly with T. J. Watson or did you deal with the people who were around Columbia -- any IBM salesmen? What was the process by which you personally got involved with IBM?

E. I don't remember the process. But in the course of the years, I had contacts with many people including Halsey Dickenson, Steve Dunwell, John McPherson, various sales people, customer engineers. I always had a warm spot in my heart for the Customer Engineer, because I could get information from him about the machine which the salesman didn't know, and I could get ideas of the things that were easily possible and get permission for them to make a change or sometimes even make it when nobody was looking and it didn't hurt anything. But it was a gradual building up of the power of the machines and the things we could do with them.

S. Well, back in '33 when you first got these machines and you were making the approaches, whom did you contact to do this, or did you do it through Dr. Wood? In short, how did the machines actually get here? Who made the decision to bring them in and with whom personally did you have contact back in '33?

E. Mostly through Dr. Wood but then through the salesmen and through the customer engineers, etc. But I think it was just since everybody knew that Mr. Watson was interested in the project so I think that things were a little easier than they would have been for an ordinary customer.

S. In short, the request went through channels and back came after a while your machines.

E. I doubt if the request was even in writing. I don't know. I think we did have a contract on the multiplying machine but we certainly didn't have an itemized account for the rentals on the machine.

S. Well let's basically get back to the use that you were telling me about a few minutes ago. How long did you use these machines, this particular first group?

E. Those machines were in use until they were really worn out during the war. We did astronomical computations on them until 1940 when I went to the Naval Observatory and the lab was still running at that time. Then as war work was taken in, the machines were run on a long shift basis and by the end of the war the equipment ..... and in the meantime of course they got in much more equipment. The whole top of Dupene was full of machines during the war.

S. Well in the '30's, what kind of results were you getting? Were you pleased with them? What did you see you needed? Can we talk a little bit about what the computational situation was and what it was developing toward, if anything, at that time.

E. I think the thing that interested me most personally was the ability to solve the differential equations of planetary motion in an automatic manner. This was numerical integration, with two or three coordinates, 10 digit precision, an arbitrary number of differences. We used the difference method because this gave us a check and we could use an arbitrary number of differences which you could shift very easily during the integration, so that when the planet was near the sun you had to take short intervals or else take higher differences. We always used the same ten day interval but we would take more differences at that time. When the planet was far from the sun we used fewer differences because the computations went faster. This was a very satisfactory method. It took two minutes to do a step for say 9th differences and 10 digit accuracy and perturbations by the major planets. This was checked -- automatically checked -- so that if there was a mistake you could see it on the record and back up a little. We had a very effective method for backing up (telephone rang...few words apparently lost here). motion by numerical integration. This method was very effective.

The operator had to take a few cards in his hands at one or two places there in the two minute cycle and pass them from one machine to the other but otherwise the thing was automatic. The record appeared and you could see that it went along. In the case of an error, which was comparatively rare, it would show up and you could go back and pick up the cards, three or four steps back and then go on from there. This system was used a great deal by graduate students and by myself and an assistant. There were a number of very precise orbits that came out of that that were used for many years. So aside from the planetary motion, there were two large data reduction projects. One of them was for Yale and the other one was for Columbia. The Yale Observatory at that time was engaged in making a star catalog by means of astronomical photographs. They would collect these large photographic plates, measure them, and record the results and then there was an elaborate amount of computation that had to be done -- the allowance for recession, mutation, aboration, refraction, etc. The trigonometrical transformations with about seven figure accuracy and finally the square solutions, etc. and we made a lot of analyses and calculations and that took quite a bit of machine time. In this there was a great deal of use of special tables which we constructed for the purpose for which we used the machines both to make the tables and to use them on the problems. Another...

S. This was the data reduction work?

E. Yes. Very elaborate data reduction, you might call it. The other data reduction was Professor Schultz's Columbia program in photometry. This resembled the Yale program in that it used photographic plates and measures were made and results reduced and put into catalogs. In this one, the computation wasn't as heavy as for the Yale program. On the other hand, it was rather more automatic because the program was at Columbia, and we had the equipment soon after the program was started and this made it possible to go further in and do earlier data acquisition. In this program we had a list of stars and their positions in the catalog and we used this. These flight ascensions and declinations were punched on cards. Machines were used to compute the coordinates on the plate. They were run through an interpreter and the cards were then taken to the photometer and the observer would pick up the card, look at it, read the setting, set the instrument, put the card in a punch, read the instrument and then punch the results on the card as she went along. And it was found that it was more accurate for the girl to punch than to write the number. She made fewer mistakes and the punched cards then were available for the data reduction operation and for preparing the results for the printer. So this was probably the earliest case of a punched card automatic data acquisition and reduction system.

S. Was this the same machine that I had once seen here that John Lentz had had a hand in develop ng?

E. No. This was in the early 30's, starting in 1933, and the whole program from then on was mechanized in this fashion. The John Lentz thing started after the Watson Laboratory, which was ten years later.

S. What about the program to make the machine work?

E. There was another big area and that was stellar statistics. We had large amounts of astronomical data on punched cards which were available for statistical discussion, etc. In fact one of these catalogs, the Boss catalog which we put on cards in the early '30's, is still in use. They have a copy of it in many places in the world.

S. In running these machines Dr. Eckert, how did you arrange the mechanical program, you know, the regular machine program? Did you hand feed the cards, the ones you wanted, or to what extent was it automatic, a stored program.

E. There were two kinds of computations. One was batch processing and the other was one where you had to use an elaborate program. And of course with the... where batch processing could be done... it was more efficient than where it couldn't be done because when you are using the machine as a program calculator, you have to have the whole machine standing, and while one unit is operating, the other might not be. So your overall efficiency is not great. On the other hand, when you are dealing with batch processing as we were in much of the data reduction, it was possible to get very effective results and results which could not be obtained with a purely sequence calculator. The use of the "table look-up", for instance, and many of these progressive digiting, for instance... these might sometimes give you an increase in efficiency of five or ten, even. One has to remember at that time that the expensive thing was multiplication. The cheap thing was sorting and adding. On the adding machine you could read cards at 150 a minute and add as many as 40 digits from a card which is an awful lot of arithmetic. On the other hand, the multiplier took perhaps a dozen cycles ...card read cycles to do one multiplication of 8 x 8. So the difference in efficiency is very important.

S. When you are talking about expense, you are talking about expense of time because you weren't really paying for these machines. Is that right?

E. Not paying for them but still one has pride. He doesn't want to.... this was the only installation of its kind in the world and we felt that somebody, Mr. Watson and his company, had put a good deal of money into it and that it was our responsibility to use it effectively. So I've never enjoyed using machines in a sloppy way. Even now, when I have an assistant, I usually tell the assistant the price of the machine per hour and say that if one is spending more on machine rental than on the person who is programming it, that one should really keep these things in mind because a little bit of programming can waste a lot of money. So one is not happy if he is just wasting valuable resources, regardless of who is paying for it. So that the batch processing was done that way, and in getting this equipment together and using it as a computer, all of the connections between the machines were pluggable and had switches so that in ten minutes we could convert from .... convert them back to their normal form and use them for batch processing or hook them together as a system and use them for something like numerical integration which had to use the whole set-up. Now the programming of the assembly as a computer was a mixture of mechanical and punched cards. All of the machines, I mean the multiplier and the accounting machine of course read cards and the multiplying machine punched cards and the summary punch punched cards. So by moving cards from one to the other, you could get any combination you wanted and one would normally arrange his computation in such a manner that there were fewer of these operations, as few as possible. Many calculations can be arranged in that way. So in this numerical integration process there were only about three card handling operations in the two minutes. So the general approach was to use the mechanical programmer for the broad outline for the problem and to use the punching on the cards for filling in the details. The numerical integration is a cyclic thing. You go through a series of say one hundred or more arithmetic operations and then you go back and replace the data you started with before by the results you just got and do it again. So this is a complete cycle and for this you always go through the complete cycle. And for this we have the control box which had a number of disks and in broad outline, there were about twenty successive steps or settings for the switch and we filed nicks in the disks to tell the different machines what to be doing at that time. And the computation was arranged so that you didn't need to use all of them if you didn't need them and the rest was controlled by the cards. X punches in various columns of the cards were helpful for instance, to change the number of differences you were using.

S. Dr. Eckert let's say after the war when you got new equipment, to what degree did this equipment in the '30's do what the later equipment did?

Did this system leave a good deal to be desired that you wanted done that was done later, or were the later machines that came in to do this work just more economical to operate?

E. Well the assembly that we had in '33 left much to be desired. It was obviously a compromise between what one could get and make work in a short time and use effectively without undue expense. We felt that at that time we should start by using reasonably simple equipment and then not spend a lot of time and money on a development program that would be much more ambitious. We got a lot of experience on that. I mean in 1933 we ran into some of the little problems that programmers and operators have had ever since, like what do you do when the machine makes a mistake? Or if a mistake occurs in the calculation, how do you get started again? Well this meant that we had written our program so that any time we got into trouble we could just go back and pick out the cards from three steps or one step or five steps previously and start over again.

S. Did you have any checking devices?

E. Yes we used the difference method so that when one looked at the record, it was obvious that a mistake occured. Because difference methods are very sensitive to errors. Now sometimes with a difference method, you wouldn't ...well if the mistake is big, you would see it the first step, the first time it printed. If it was small, you might have to go through three or four steps before you noticed it. But in every case, we found that by being able to use our results that we had just punched out as starting points, that any time you went back you merely had to pick up the right deck of cards, slip them in and start again. It has been amusing to see people struggling with this same problem with the more elaborate computers of recent years.

S. What I was really driving at was to try to analyze exactly how far this operation in its own area was coming to modern computers. I guess the way to phrase the question would be "Did you consider that operation that you started, looking back at it in perspective, as modern computing or pre-computing, pre-modern computing?"

E. Well, it was a revolutionary thing because up until that time general scientific computing always involved hand work. The numbers had to be copies. The arithmetic had to be done, at best with a desk calculator or with logarithms, and here for the first time you could do general things such as the solution of the differential equation completely automatically and you never had to read or write a number. You only had to move a few cards and look at the results to see if you approved of them. And in the mass reduction of observational data, the batch computations, these were very effective. It made it possible to do say a hundred times as much work as you could do by hand. So if you get a gain of one hundred ... a factor of one hundred in one step and you get 100 times that of another, you've got 10,000, and which is more interesting, the first hundred or the second hundred?

S. Today are the same kind of tables and calculations being made in astronomical laboratories as you were doing back then, updating and so forth?

E. One of the things that we did was to use or to make tables as part of the calculation. To make and use tables. This is an economic matter. You could compute each individual case as you go along or you can make a table and use the table. For many problems, tables are exceedingly effective. Now as I say though, it is an economic matter depending upon the computation and upon the equipment you have. At that time multiplication was expensive or general arithmetic was expensive. Table look-up was cheap. With the early computers, as soon as you got a fast electronic calculator with slow input/output, then you are bound the other way around with more memory. So that tables are expensive and arithmetic is cheap. When we went...at that time we had the punched card input and the wheels for doing arithmetic. So at that time tables were effective. Then we had punched card input and electronic arithmetic, so that at that time punched cards were the bottleneck and you got away from tables and tried to use routines in the machine. Well this depends on the configuration you have. You switch it a little bit both ways and you hear people talking about both at all times.

S. Did you ever get the feeling as this change or switch between electro-mechanical and electronic equipment took place that the tail began wagging the dog, and did you have to change your outlook to adjust to this new equipment?

E. Oh yes. But anybody who is a professional computer, this is just second nature to him. He always looks at what his tools are and he wants to find out what it does well and he's always conscious of economic balances and the law of diminishing returns, as he pursues a particular idea. So I think anybody who has been in the business for a long time will just naturally think in these terms. And when you get new equipment, the first thing you ask is if you want a speed chart of certain kinds of operations and you obviously don't limp on your worst foot. You try to use the ones that are most effective. Now I think that perhaps this is more true of people of my generation, who have come up the hard way. We are more conscious of these possibilities than someone who comes in now and learns a few tricks but doesn't worry about the effectiveness. I suppose I worry more about ... in other words,

I always want to get the most out of my equipment and sometimes perhaps I expend more effort than I should in doing this.

S. Has that effort paid off so to speak, for let's say IBM when you came to work for IBM?

E. Well it has paid off I think in a number of research projects. Because at various times along the way we've been able to get to accomplish certain things that other people didn't because of using what we had effectively. In the 30's we were doing these large jobs of data reduction and numerical integration, etc. and we were the only ones that were doing this. And at the Naval Observatory the same sort of thing happened. And with the SSEC we had a calculator that accomplished many scientific problems at a time when competition wasn't solving problems of that type.

S. Let me just go back to the pre-war period for a moment. You mentioned that no other observatory had a set-up such as your observatory.

E. Nobody, period. It was the only set-up of this type anywhere.

S. Did people from other observatories come and take a look at this?

E. Oh yes. Of course one limit to this though was that it took quite a bit of doing to get into the system. When one has an automatic computer, the machine reads data that is available for it, and if some of the other observatories had their data in other places that wasn't readily available, in order to use this equipment you would have had to get your data on cards and frequently this would have been troublesome.

S. I see.

E. And in those days you had to get it on the cards and you had to practically process the cards yourself, because for most of the computation... for the batch processing, you had to know the machines and the data and what you wanted.

S. What was the reaction of the astronomers to this? Did they feel that it was too much trouble to do that?

E. Well, one of the reactions, not only of the astronomers, but of other people, was to get similar equipment, to get punched card equipment; so that large amounts of punched card equipment were installed during the war for war work-this included Los Alamos, Aberdeen, Dahlgren, the Naval Observatory. Really a very large part of the computations that affected the war were made on IBM equipment.

S. I see.

E. The ENIAC for instance, was developed during the war but it didn't do any war work. The IBM machines though were going constantly.

They were available quickly when people needed them.

S. Were they doing the kind of calculations that you had pioneered in?

E. Well, at ~~Baldrin~~ Dahlgren and at Aberdeen (these are ballistics laboratories) and the difference between a planet and a projectile... the computations are very similar. So that it was a natural, and at the Naval Observatory and many of these other places. So in those places it was almost a direct transfer.

S. You mentioned that you went to the Naval Observatory in 1940. Were you hired there? Were you drafted?

E. No. This was not a military job. I was a Professor of astronomy at Columbia when I left, and I went to the Naval Observatory as Director of the Nautical Almanac, which had been considered one of the best and most interesting astronomic positions in the country. It was held by Nyman Newcomb, for instance, and others. So when I went there, it was not just as a temporary position. I became a permanent member and decided or intended to stay.

S. I see, so it didn't have anything to do with the mobilization and so forth. You just took another job.

E. I went there early in 1940. The Director of the Nautical Almanac is a Civil Servant and there had been Newcomb and various successors. In 1939 the place became vacant and they held a national competition and invited me to join. I went there because it was an interesting thing and here, of course, it was possible to quickly set up the same kind of computing equipment pretty much as we had before and to mechanize a very sizable computing center. The Naval Observatory had always been a center of computation. They had lots of work to do. This mechanization was quite revolutionary there.

S. They didn't have any automatic equipment to that?

E. They had no automatic equipment. Every digit was written by hand and read and written repeatedly and the data was arranged and transformed. When I arrived they didn't have a machine that would print figures automatically. They had desk calculators. As I mentioned before, many of the people there got high ratings on the exams because they were good at using logarithms. The problems they were asked to do on the exams, you could do effectively with logarithms if you could get it done in time. And if you couldn't, you didn't get it done.

S. Was it IBM equipment that went into the Naval Observatory?

E. Yes, mostly.

S. To what degree did it differ from the equipment you had developed at Columbia?

E. Well in the first place, at that time we were able to get the alphabetic accounting machine, instead of the numeric, not because we wanted to use it alphamerically but because the .... well because it provided greater flexibility. We could stick in wheels that counted by 6's instead of by 10's for a degree in seconds. And the counters were pluggable in various combinations so that we could put the 6 wheels where we wanted them and the 10's where we wanted them. So this was a very great boon in a lot of the table-making and that sort of thing that we had. We did not build a sequence calculator since we did only batch processing. On the other hand, we spent more time on the making of tables and printing methods -- automatic printing.

S. I think we're coming to the end of this reel very rapidly. Well we'll just run the tape out and perhaps we can make this a breaking point. With this new equipment that you got in, you started computing ...... End.

This is Part II of Interview TC-1 in the IBM Oral History Project on the Technology of the Computer. The date is July 20, 1967 the place, the Watson Laboratory in New York City. Lawrence Saphire interviewing Dr. Wallace J. Eckert.

S. Dr. Eckert on the last tape we finished off as you were saying you had gone to the Naval Observatory and you had been bringing in various kinds of equipment to do computing there. You were telling me about the degree to which that computing equipment was sophisticated. Perhaps you could describe the kind of equipment you had during the war and how it was improved or to what extent it started approximating modern computing equipment.

E. Well the first big problem there was mechanizing the work of the office, the Almanac Office, and a good bit of that work was the production of tables, tables for navigation. My first task was to design an air almanac that would be used for the rapidly expanding Air Force. This was designed and simultaneously I went to IBM and told about the problem of getting equipment quickly and in a very short time, astonishingly short, we had installed equipment to do the batch processing which is required in table-making and within a few months we were able to compute the data for the air almanac and by the time it had been accepted, the data was ready to go to the printer. As a matter of fact, when I arrived there the first of February, we were able to get the air almanac designed and get it approved by the Armed Forces and get it into the training camps so that the first issue started in January, '41. It was 1940 when I went there.

S. Could you just describe what kind of information was in the air almanac?

E. Well the air almanac tells the aviator very easily and quickly for any time of the day or night, where the navigational celestial objects are such as the sun and moon and three of the brighter planets at the time and a number of navigational stars. This was the first time that the user didn't have to compromise because of the trouble of making the almanac. In previous years the aviators or navigators would have liked to have had more information and less work to get it and to use it. But because of the problems in computing and constructing the tables, it was always necessary to compromise so that there was more work done by the navigator than was necessary. But with the air almanac, it was the first time when we could afford to give the navigator exactly what he wanted without worrying about how it would be produced. This almanac job was batch processing so we didn't need any of the controls that we had at the Columbia set-up. We were able to use essentially standard equipment which by that time had ~~been proven~~ some improvements. One of the principal machines of course was the alphabetic accounting machine, which permitted the use of degrees, minutes and seconds as well as decimal numbers. There was, of course, the multiplying machine and the reproducer which we used for automatic table look-up from tables on punched cards.

S. You were using standard equipment but you were doing in effect a kind of scientific computation job, ~~whereas this was~~ or at least in the scientific area. Would you consider that true computing on the standard equipment, in the result? ~~The result~~ I know of course, it wasn't ~~was~~ a computer in terms of having a real stored program. Is that the kind of thing that they would have done on a computer today? Would that have meant a difference?

E. I would say that the construction of tables indeed tends to be a systematic job which you can do by batch processing and we did batch processing on these mathematical tables on a large scale. And it was this that made ~~for~~ the success of the project. Now it's easier to use a sequence calculator frequently in batch processing but the efficieincy of the sequence calculator comes in on a different type of problem. We could do a very effective job on these mathematical tables with the equipment that we had. One had to plan it in such a way as to make use of the equipment that you had but this is part of the job. And it did revolutionize tablemaking. We were able to put out in a few weeks, pages and pages of tables with thousands of digits to a page.

S. Was this the first time it was done automatically or semi-automatically in this fashion?

E. Yes, it was automatic in the sense that nobody had to read or write a figure or look at anything. You'd have to program the machines ~~but in~~ AND put in the initial data and out would come the finished product. So in that sense it was completely automatic. Nobody intervened. Now this involved not only the arithmetic operation, but also elaborate use of mathematical tables on punched cards. We used the machines first to construct the tables. Then we would use it to consult the tables during the course of the computation.

S. The tables were stored on punched cards?

E. On punched cards and then merged with a collator and transferred from the table cards to the detail cards with the reproducer.

S. And was the program wired on the plugboards?

E. Yes.

S. Did you have one plugboard only or did you have a number of plugboards?

E. The machine of course would punch intermediate results on cards so that many of these things we would have to punch an intermediate card but not a great many. There are tricks to the tablemaking business that enable you to build up the table by means of summations and these methods are very powerful. So that there really wasn't much criticism of the equipment for this kind of work and it revolutionized the making of these tables. So that instead of having astronomers running calculators and copying figures from one sheet to another, for the first time we were able to put them on the machine and have the results come out the way you wanted it.

S. Well was this system adopted by other observatories or did we do it for all observatories around the United States?

E. Well there had been the Columbia thing which was still going and which was vastly expanded during the war and then the Naval Observatory was the second. But soon after that or... ~~well I'm not sure of the dates but at~~ approximately the same time, similar equipment was installed in other places such as Aberdeen, ~~Dolgren~~ DAHLGREN and Los Alamos. There was a rapid expansion of the use of this equipment at that time.

S. For tablemaking of different sorts.

E. Well for all kinds of computations. One would have liked to have had a sequence calculator, but there's a lot of heavy computing work in any kind of a problem that one can do by the use of this equipment if he uses a little ingenuity and of course that's always part of the fun.

S. Dr. Eckert could you just list for me the machines involved in the standard equipment so that we have it on the record.

E. Well there was an alphabetic accounting machine which had, as I said, these fractional wheels for doing decimal arithmetic. There was one or more multiplying punches, a reproducer, collator, key punches, verifier. Very quickly though, we ran into a problem. We found that for the first time it was possible to compute and prepare a printer's copy for the table without error and in large quantity. When we wanted to print these tables, however, we had to have them set in type and we found that in sending a thousand pages to the printer, he would set the type, he would have his proofreaders correct it, and these corrected proofs would come back to us and we had to proof read them again and the first year we tried this we found that even after the Printing Office had done the proofreading, our proof-readers found about an error on a page. So here was approximately a thousand errors that had to be corrected. We sent these back to the printer and he corrected those and gave us revised proofs and we had to read them. And when we did this we found that he had corrected most of them but there were still about ten errors in a thousand pages. So it was obvious that the proof-reading was now the bottleneck and here we actually were quite pleased with our automatic method of proofreading. When we prepared the copy for the printer, we had a set of punched cards of course, and by using the right kind of checking methods, these were error-free. So when the copy came back or the proofs came back, we found that instead of having a person take the responsibility for reading the proofs and saying that there were no errors in it, we had the proofs punched up on new cards and this is very simple punching and the person who did the punching of course was in a clerical classification ~~and was~~ fairly low and these people were ~~fairly~~ RATHER easy to get and they had no responsibility except to punch what they saw on the cards;, so they never had to say that the table was right. We didn't have to take their word fro it. They merely punched what they saw. And this not only would catch errors in type setting but also where there was an illegible character. So that characters were looked at by human beings but in a very quick manner and a girl could punch up in a day perhaps twenty pages of proof, whereas a good proofreader was only able to work an hour or two at the job and then he would make mistakes. So these cards were run through the reproducer or comparing device and compared column by column with the cards that were used to prepare the printer's copy. The person who ran this was a person of responsibility but not necessarily a person of scientific ability. They would run them through and whenever there was one that would not compare, it would push it to the side and then later you'd go through and make the correction. So this became a revolutionary method of proofreading as far as the mass production of tables was concerned. And it was most effective. So the first year we quickly developed this method of proofreading and we used this even after we had foolproof printing methods ~~just as I say~~ to make sure that there aren't illegible characters and so on. So that even with automatically prepared and printed tables, this method of proofreading is still worth while on something like the air almanac where we were printing 100,000 copies of it. The next thing was to try to get automatic methods of typesetting or preparing the printed material. It was too troublesome to send perfect copy to a printer and have it come back with an error a page and then go through it all again and have it come back with an error in one hundred pages and so on. So we looked at different methods and actually we devised one method which enabled us to use the alphabetic accounting machine to prepare copy which would be photographed by the printer. It wasn't too good but it was quite ~~good~~ acceptable. The trouble with things like the accounting machine generally is that the spacing in the line is too far apart compared to the height. So we found that we could run the sheet through the printer twice and print one set of columns and then print the other set the next time through and get the alignment ~~is~~ sufficiently accurate to make the pages legible. So for about a year or two this method was used in preparing the air almanac. In the meantime we went to IBM at Poughkeepsie. There was an engineer who is still in IBM by the name of Dodge who worked with me on this and we developed a table printing typewriter. This one would read punched cards and it gave you a great deal of flexibility in the spacing of the characters and so on. This machine produced as fine a mathematical table as had been produced before. The type was specially designed for this purpose and it used the proportional spacing machine so we could control the spacing in the line. We had provision for also controlling the line spacing and the alignment ~~copied with~~ by the forms. So when this machine was delivered it was really the answer to the question and that machine has been used about twenty years and recently they had another one just like it delivered so ~~they~~ now ~~[illegible]~~ they have this machine in very good order. At the moment at the Naval Observatory they are experimenting with some of these magnetic tape monotype devices or special printing devices but they still use these card operated typewriters which have done a magnificent job for a couple of decades. Well the tablemaking was and is an important part of the place.

S. What other things did you do with the machines during the war at the Observatory?

E. Well we gradually mechanized most of the observing program there so that by a reasonable number of years most of the divisions of the Observatory had put aside their pencils and were using automatic computing methods.

S. Did any foreign places come, for instance the British Observatory, did they get in on this?

E. Yes but much more slowly. We were in full operation in 1940 and it was quite a while afterwards that the other places got to do the same. Of course by now they are all mechanized. There has always been a large amount of exchange between the Almanac Offices of various nations. Actually including the enemy at times, because some of these tables are made years ahead. During World I and World War II, the computations went on and at no time were they critical at the moment but the exchange did go on during war time through neutral countries. So that material from one office would be sent to another.

S. Did you ever consult with places like Aberdeen or ~~Dalgrin~~ Dahlgren about their particular problems? I believe they worked on ballistics problems mainly.

E. Yes there were various people who came. I mean people from Los Alamos came to talk about computing machines and people from various other places came to discuss special projects they had and the mechanization of them.. This was part of the day's work.

S. Did you ever get involved with things like the relay calculator such as Aberdeen and evaluate it for possible use in your operation?

E. No. These things came along reasonably late in the war and by the time I was preparing to leave the Naval Observatory, it was obvious that the war was going to end and that our work was done, always ahead of time. The Naval Observatory publications are always well in advance so that they can be printed in the field so there seemed to be no particular urgency in revising the methods at that time.

S. Is there anything else that we should discuss about the war?

E. I think not.

S. Why don't you tell me what you did when you left the Naval Observatory. What year was that?

E. Well when I went to the Naval Observatory, I considered this a career job and had no intention of leaving. Then in 1944 Mr. Watson asked me if I would join IBM and this seemed to be exciting from the point of view of being able to do scientific research and the development of calculators for this purpose. So that made it very attractive. I left the Naval Observatory on the first of March, five years and a month after I had gone there and I've been with IBM ever since.

S. When you came into IBM, what was your assignment?

E. Well Mr. Watson said that he had been greatly interested in these developments of the use of the computing machines for scientific research and he felt that he had fine engineers and fine salesmen and systems people but he didn't have anyone in the scientific area. He felt that IBM should have people in this area and not be entirely dependent on scientists from elsewhere and so my assignment was to learn the company's problems and discuss them with people in the business and hire new people as required. It was more or less of a blank check.

S. You worked, I take it around 590?

E. I moved into 590 on the first of March and stayed there for a few months, something like three, and then we decided that we should start a laboratory. The war wasn't over yet and we weren't completely sure that it would be over. In the meantime there were problems and we felt that we should get interested in some of these. So we set up some computing equipment in Pupin by invitation and for six months or so we were doing computing experiments there and helping people from Los Alamos and some of these places where they had difficult problems. John Von Neumann was one of the people who contacted us.

S. What kind of problem did he bring?

E. Well by this time of course people were expecting great developments in computing machines because the ENIAC had been going. The IBM calculators (relay) and Bell relay machine.

S. This was 1946 I take it.

E. '45.

S. The ENIAC was going?

E. No but it was being built and it was in the air, and the Harvard machine came somewhere along in there so that there wasn't any question of building special scientific calculators and many of the questions were what should one build and how soon can you build them and where will the equipment come from. So we had this equipment at Pupin and we did a lot of experimental work and studying methods for partial differential equations and so on. Then we got a little building on 116th Street and moved in there the beginning of the year and that was the beginning of the Watson Laboratory.

S. Let me just go back to people like Noyman who were coming in I take it in '44 and '45 and discussing various things.

E. It was '45.

S. Could you tell me a little bit more about what he discussed and what the solution was, if any, to those discussions?

E. Well, first we had assembled as fast as we could equipment. We had some of the IBM engineers model equipment, experimental models and they were put into the laboratory. We had relay machines by Pete Luhn and others. We early got Halsey Dickenson's electronic multiplying machine, and we had a good assortment of standard equipment. Of course Von Neyman also was aware of the Harvard machine and the IBM relay calculators at Dalgrin and the AENIAC and we all knew that sooner or later we would have electronic calculators that would be generally useful. But in the meantime things had to go on and we used what we had for largely exploratory problems. We didn't of course, tackle the kind of problems that were done later.

S. Was Von Neyman at Los Alamos then?

E. Well he was everywhere. He went from one computing center to another and always talked about people with problems and people who were building machines and he was just sort of the leaven in the loaf in addition to being a very able man himself. Of course meantime we started acquiring a staff for the laboratory. Thomas Seeber, Groesch and a number of others, and we very quickly made the equipment we had available to people from everywhere and very early in the game we expanded our instructional facilities so people could learn how to use the equipment and the equipment was provided for using it.

S. Did Seeber come from the Harvard Computational Laboratory?

E. Not directly. He had been there at one time and then he was somewhere else when the lab opened. I don't recall where he came from but he did come here and was one of the early ones. He had been trained as an actuary now and had had experience on the Harvard computer. Thomas had had no experience with computing machines but he was recognized as one of the outstanding applied mathematicians at the time.

S. Was he teaching at Columbia?

E. Yes he has been a member of the staff of the Watson Laboratory ever since and was quickly made a Professor of Physics and still is. The beginning of the laboratory was in a way supposed to be a continuation of previous work at Columbia with close relations for a good many years, so things were quite informal. We were invited to come and occupy the building at 116th Street and to set up computing facilities there and to invite people in to use them and the intention was that and this was the big distinction I think between the Watson Laboratory and any other and that is the complete informality of it. Our agreements with the university have always been exceedingly informal. The best way to describe it is that we were invited to the campus to operate a laboratory with the understanding that if we didn't like being here or they didn't like us to be here that the situation could be terminated, but we never had detailed agreements about who would provide what or what would happen. And this has been true from 1945 till the present time.

S. Was that building owned by Columbia or did IBM buy it?

E. Both. IBM bought it and gave it to Columbia immediately and Columbia allowed us to occupy it. The building didn't cost very much to buy. It was a run down fraternity house. It cost quite a bit to make it useable.

S. The price has gone up a bit since then.

E. Well the boys ~~had used this building until [illegible] and~~ had not maintained it very well so that when we got it it was pretty well deteriorated and also to make a laboratory out of it, ~~out of a building, you have~~ WE HAD to conform to a fairly rigorous BUILDING code. So that we spent quite a bit of money in making the building suitable for a laboratory, but it worked out quite well for many years.

When this building was acquired, generally the same arrangement was made but since this was a much more expensive undertaking, the agreement was in writing but it still is just a letter saying what we were going to do. The idea is that Columbia itself has always been a very loosely integrated school in that the departments have a great deal of autonomy and the administration does not try to manage in the sense of corporation administration as it normally manages so that President Butler and President Watson decided that we were to have a laboratory but we didn't write out agreements and details on who was to do what. The idea was that we were just to be a member of the university community and the departments would become acquainted with some of the leading people here and would invite them to become members of the departments. So it was not from the President's Office down but in the other direction. So I was made a Professor of Celestial Mechanics and Thomas was made a Professor ~~of Astronomy or Physics,~~ Theoretical Physics and this tradition has been maintained ever since. At the present time there are I think seven or eight of our people who have academic appointments. Now these academic appointments don't carry a salary. One of the first policies of the laboratory was that people here are fulltime scientists. They have the privilege of coming and going pretty much as they feel they should but we have discouraged any moonlighting and we felt that if one was going to teach a course, he should teach it because it was useful to the university and to him and that he shouldn't be paid extra for it. So none of our lab members have been paid for their teaching at Columbia.

S. What kind of work did the laboratory do in those early years -- '46 and '47?

E. Well we had two questions that the company was interested in at the beginning. One was computing and applied mathematics and what is the relation of this to the computing industry, and the other is what should machines be like in order to be most useful. And to attack these two problems we hired the kind of people we did. First there were the applied mathematicians like Thomas and Seeber, etc. and for the other we at first did not intend to go into any kind of engineering research but we decided that in order to look ahead properly we really needed some good electronics people in the laboratory with us. So I went to the Radiation Laboratory at MIT -- the war-time Radiation Laboratory was being closed down. We went there and convinced some very good electronics people from that group to come join the laboratory. This was Havens, Walker and Lentz and they of course came and built up their staffs. On the mathematical end we made it a policy to keep in the laboratory the best equipment we could and to try and get better equipment experimental equipment made by various engineers, that we should learn by doing and not by talking, so the laboratory was set up as a computing service and we invited scientists and students to come and use it and of course they came from all over the world and actually carried out their research. The typical person who came had a problem that he was interested in. He would try to get instruction or talk to other people and learn how to do it effectively and then he would proceed to do it. We avoided acting as a service bureau because in the first place we felt that we shouldn't get into the commercial aspects of the business, that this would tend to interfere with our broad approach to the problem and secondly we shouldn't do .... or shouldn't solve problems for people because for the people when they can get somebody else to solve the problem for them, the tendency is not to think very carefully about it themselves. So generally we said, well you come and we'll show you the equipment and we'll show you how to proceed and you worry about your problem yourself. And we believe that one can get greater efficiency by revising the problem and fitting it to the machine rather than trying to do an efficient job of coding. In other words, the scientist has a problem. He has to state it mathematically. Then someone has to convert this into suitable arithmetic procedures and then someone has to convert these into machine programs and get the data to flow through the machine. This whole process should not be isolated and it's much better if the person who knows the problem will think about the equipment on which it is going to be done and not just pass it to somebody else and let him overwork trying to make up for a badly planned problem.

S. By the time the laboratory was in full swing, what kind of equipment did you have in here?

E. We had a full line of course of punched card equipment. It changed with time but we started off with a full line of punched card equipment. We had a number of engineering models of relay calculators. There were two that Luhn had built. One was a fast arithmetic unit that we attached to an accounting machine and made a little sequence calculator that was somewhat like the CPC that was used later. In fact it was very much that way, although it wasn't quite. The arithmetic was a little slower but the programming was more flexible. Of course the big relay calculators were the fastest things at that time. They did about six multiplications a second whereas a wheel multiplier took up ten seconds to do a multiplication.

S. When people came to the laboratory to work on problems, did they discuss it with the whole staff or were you always aware of what kind of problems people were coming in with? What kind of interaction was there?

E. Well the interaction was very close. Of course we were using punched cards and that kind of equipment is not like the modern stored program calculators where you can read in a program quickly and start to work. You had to have intimate connection with your card files in order to have efficiency. A great deal of the efficiency at that time depended on how you could effectively use card files. So people had to actually learn how to manipulate cards and push them through the sorters and the reproducers and the computers. This was a very informal place. We felt that the people who were coming to solve problems should mix with people who were learning and who were giving courses. We always had problems of our own of course, that we were interested in getting solved. So the place was more like a university laboratory than a computing center. People sat around and discussed their problems and they would wait for the machine and while one person was using one machine somebody else would be using another. So it was a very intimate arrangement.

S. When was the first time that the company made a demand on you to do something? I'm not sure if we are jumping ahead to the SSEC too quickly, but that was one request. Am I correct?

E. I don't know if it was a request. It was just sort of understood that one of the reasons that I was here was that I was interested in calculators and that the company was interested in calculators and Mr. Watson had already authorized the relay calculators and the Harvard machine and the Columbia computing set-up and so he wanted to know what IBM should be doing. It was a lot of fun very soon after coming here. We had discussions, a number of them with all the engineers in the company. This included people like Hamilton, ~~and~~ Lake, ~~and Peaty and~~ BEATTIE, Page, and ~~Luhe~~ LUHN. We would get together, Seeber and I on the one hand, and because we had both had a lot of experience with computing and knew what we would like a machine to do, and Hamilton and these people had been building machines but they didn't really know what a computer should be like. They knew how to make computers and they could make them work ~~but~~ so this was a nice arrangement. In addition of course, Hamilton had built the Harvard machine which used wheels. And now the problem was much more elaborate and the question arose about what could you do with electronic computing. Ralph Palmer and Halsey Dickenson were there. So many of these discussions went on at great length and I think were enjoyed by everybody. ~~On the one hand we were asking these inventors and engineers [illegible] well I got ahead of myself.~~ I said that Palmer and Halsey Dickenson had built electronic counters and electronic multiplying units, so that they had considerable know-how in making a machine that would do arithmetic. And we knew that the ENIAC was coming along and that it did arithmetic. But the question of how can you get everything you need in a scientific computer and get it to go at electronic speed with the equipment that was then available.... so the engineers and inventors would tell us everything they knew about components and what they could build and what they could hope to build and what the troubles would be. And we on the other hand kept telling them that the sky was the limit in what we needed, not only in doing arithmetic and fast arithmetic but in the controls and the storage and the flow of numbers, to the machine. In the batch processing business the flow is rather simple. You take a stack of cards from one machine to another and they flow through in a certain order. But with the automatic computer you want numbers to flow in all directions and this greatly aggravates your traffic problems, etc. So at this time the discussions were very stimulating. All kinds of ideas for storage and control and arithmetic, etc., input/output, the whole works were discussed. And after a number of these discussions, it was decided that there was to be an SSEC and that Hamilton was to build it and that Seiber was going to work very directly with him on this. On this end of course there was Thomas and Seiber and myself. Thomas had not had experience with machines but he was very sophisticated on applied mathematics and the kind of problems that people would like to solve if they had a machine and what kind of stores would be needed and the amount of stuff involved and the size of the problems and how they could be organized. ~~So when Seeber~~ When we finally undertook the development of the machine, Seeber went to live in Endicott where Hamilton had his laboratory and Hamilton of course had had no experience with electronics but he got some electronics people to work in his group and so finally the design was solidified. It was a compromise on ~~what we~~ how many elements of each kind an engineer was willing to guarantee would work. And on the other hand, where we could cut our demands to something that would still be useful to us and reasonable for him.

S. This was about 1946 I take it.

E. Yes.

S. Let me just ask you to summarize the inception of the SSEC. This was more or less your responsibility to do things in this area and you said that you and Seeber talked with all the engineers in the company and just decided. What made you decide that it should be an electronic machine? The fact that it was obvious that there was speed involved in electronics vs.

electro-mechanical or the fact that ....

E. Well we've mentioned before in connection with the Columbia equipment twenty years earlier, if you set yourself the task of getting a computer that will be useful and getting it in the finite time, you have to look over the state of the art and with the proper guidance, decide what are the elements available, how many of them can you hope to put together and make them work as a unit and how should you organize them and so when we were designing the astronomical thing in 1930 this same kind of reasoning had to be gone through. At that time there were wheels but they were slow and as I said then, for a scientific calculator you must have the ability to do arithmetic. You must have storage that is accessible and channels from one part of the machine to the other and you must have control of the operations, traffic control. So it's a matter of optimizing what is reasonable and at that time it was very evident -- this was in 1930 -- that for sequence calculation you need an awful lot of equipment and it's expensive. The Harvard machine of course was close to a million dollars, at least half a million in those days, and it had only the speed of the wheel. In other words, when you wanted to do a multiplication it took you ten or twenty seconds -- quite a few seconds to do it. So this is expensive. Now why does one want speed? For two reasons. One is that you'd like to get the job done and the other is that you couldn't afford to do everything you have to do

~~everything you have to do~~ at slow speed and that's one of the things about the sequence calculator that the whole machine must be there on demand while one part of it is working, so that if you have a bottleneck it is a frightfully expensive business. So the question was if you can get electronic speed and make it work, then it works many times faster than anything else and so you can afford to do it.

S. And this was one of the conceptions as you went around discussing the possibilities with engineers. Could you move into this new electronics area?

E. Yes. To what extent ....

S. Was this what you and Seeber proposed and tried to find out the feasibility of?

E. Yes, well I mean it's fairly obvious that if you wanted a scientific calculator you had to make it fast. Now let's say the wheel calculator took ten seconds say to do a multiplication and the relay calculator a second or so and the kind of electronics we had then something like twenty milliseconds, that's fifty a second. So from ten seconds or a few seconds down to ten a second with fast put relays or fifty a second with an electronic tube, it is obvious which you would like. And in those days the multiplications was sort of a bottleneck. Now on the other hand, the sad part about the electronics at that time was that you were using essentially radio techniques. Palmer and Dickenson had been using essentially the technique of radio and your pulse rate there was about 100,000 counts a second, and you had to have a lot of vacuum tubes and the number of vacuum tubes obviously had disadvantages. You know if you have ten tubes, that's all right. But when you get ten thousand, that begins to be a problem and it was felt that we certainly didn't want ten thousand if we could help it. We were willing to approach that number but ten, twenty or a hundred thousand was just out of the question. So the point of view we took was that one should use vacuum tubes on those things which will pay off most and we felt the time just wasn't ripe at that time to make an all-electronic calculator, that the demands were too much and that we had to use different kinds of elements and where you would leave off on electronics and where you would go in the next step was quite important. And we finally decided that we could afford enough vacuum tubes to provide for the arithmetic operations for the control circuit and for a small amount of storage, just enough storage to bring in the instructions, the factors that had to be operated upon to do an arithmetic operation and to do it. I don't know how many tubes there were any more, ~~six~~ TEN or ~~eight~~ TWELVE thousand, but there were plenty of them.

S. And this was on the SSEC?

E. Yes.

S. And where you left off with tubes what did you use?

E. Well things had to be compatible and we found that the IBM relays at that time were very fast, a few milliseconds, so we started with the feeling that well with electronic tubes maybe something like 20 milliseconds to a multiplication would be fine. Then how do you back this up? And the relays, not for computing now, where you use relays for computing then you have to have a lot of cycles on an arithmetic operation but for switching the relays were fine. Now we would rather not had mechanical or electro-mechanical relays but we had lots of them. And actually we had a big selection in memory that was on relays. And in this the relays were used and in fact the whole system was binary coded decimal, so that you could transfer a number over parallel channels so that one relay cycle would bring you a 20-digit decimal number. And so we had I think it was something like 300 registers of relay storage and of course if we had had this in electronics we would have just taken the vacuum tubes out. So the relays were used for switching and for fast memory and then we found that the punched paper tape of a special kind had promise. The IBM punched cards are punched in a reproducer where the card moves with 80 columns and punching is done first the 9's, then the 8's, then the 7's, etc. and we found that some of the engineers were willing to take that equipment which was in good working order and had been used in the card field and used continuous card stock instead of punched cards, and that this continuous card stock would flow through and you could punch in one line of a card a binary coded decimal number and the reproducer at the normal rate of speed is something like 40 milliseconds per line on the card. And so we were able to get these punchers and readers operating at 40 millisecond intervals and they were synchronized so that you could operate two of them and have one alternately and this would give you 20 millisecond intervals between input and output from a huge store. Now this store was rather serial and not selective. But this was a very successful method of storing. We were able to ... we had three punches so that you could be punching on one tape and then the tape would be looped back over a reading station and looped again back over another so if one of these punching and reading units had a punching station and about ten reading stations, you could read the number back ten times if you needed to at successive intervals. There were three of these and then there were a number of other of these tape units that didn't punch but would read ~~so you could use circulating ... that's~~ circular tapes, ~~on them.~~ So you would take one of these tapes and punch it up and cement the ends together and put it on as a sort of loop. There are I think about thirty of these. So there are really fifty places where you could be reading numbers out of these serial stores and by using them in pairs you could get a 20 millisecond interval which was the time of your multiplication. So that the whole thing was well integrated and compatible in that you could use this huge semi-serial store because you could select from any one of these places.

S. Physically how large was the store?

E. Oh it filled a room. Actually the operators and the people who were watching were inside the calculator. The calculator was built around the periphery.

S. Was the size of the room the size of this room?

E. Oh no it was a big showroom at 590.

S. Just for the store?

E. No the whole machine. The store wasn't too big. The store was the size of an ordinary reproducer and about twice as long. I mean the paper store. There were three of those for it.

S. And the relay stores?

E. They occupied a lot of space. I think there were 300 and they were 20 digits and it takes about ~~five digits~~ five or six relays to a digit so there were ~~thousands and~~ thousands ~~of~~ relays.

S. Well you mentioned binary coded decimals. Was it straight binary coding or was it modified?

E. I don't know. It was a code on which you didn't need to use a relay for each digit. So here was the first machine that had the possibility of doing 20 milliseconds arithmetic and had the storage facilities -- accounting selective and sequential with access in many places. But there was scarcely a problem that you couldn't program to keep the data flowing in so that you could actually keep this electronic/arithmetic unit busy.

S. Was this operating at 590?

E. Yes.

S. What were some of the first problems run on it?

E. Well the dedication problem which was put on to amuse the people who came to see it was the calculation of the position of the moon. And before that people used Brown's tables and the man could just take numbers out of the table fast enough to keep ahead of the moon. He had to compute two positions a day .... but with this we decided that by not using the tables we would get more accurate positions and the machine could compute one of these positions with complete accuracy in about eight minutes or something of that sort, so this was the demonstration calculation. This meant you were evaluating about thousand sines and cosines and multiplying and dividing coefficients, etc., etc. But here again we decided that we didn't want to charge for the use of the calculator because this would mean that whatever agency had the most money would tie up the machine. So we had problems in all areas -- optics, atomic energy, hydra-dynamics, astronomy, etc., etc. and we had a sizable group of people who coded the problems and helped get them on the machine. And I think while we're talking about the machine we should talk a little more about the coding. This machine was really a full-blown stored program calculator and I don't know how this came up but there never seemed to be much of a discussion about it between Seeber and myself. We just took it for granted that if we were going to have a machine that was computing at this rate, that you have to be able to get instructions to it. Now the Harvard machine had instructions on a paper tape and when you once had the instructions, that was that. You just played them off. The astronomy computer which preceded it allowed a certain amount of flexibility of the punched cards. You could change the program cards a bit to change the details of the thing. But we felt that this was not adequate, that you really had to be able to make choices as you went along and that the instructions should be arithmetic so that you could operate on them with a calculator. So the instructions or numbers -- you could use the calculator itself to operate on the instructions by adding and subtracting anything you wanted to do with them. And you could store. There was no difference between instructions and data as far as the storeroom was concerned. Our selective store was not big enough to have many instructions in the memory but with these tapes you could take instructions from one or twenty or thirty tapes or you could take from one and then another in various combinations, depending on the choices that you had to make and what we frequently did was to read in a strip of instructions from a tape into the relay memory and then operate on that until we were through with it and then read in another. So that we really had a full-blown stored program calculator that operated much the way the stored programs do now. And of course by getting this machine early, we got early experience on the use of stored program calculators and how you must program and how you trouble shoot and a lot of these problems that you just don't know about until you try.

S. Well who did the trying and who printed out the program? These were the first programs I take it, these tremendously long binary instructions.

E. Well when Seeber went to Endicott he was working with Hamilton on the design of the machine and on the detailed instructions of what they would be, what the choices would be, how they operations would be. Sometimes there's a choice in this. Do you want to branch on plus or branch on minus or branch on both? So he was pretty well embroiled not only in the planning but also in the detailed planning of the calculator. This was all done fairly rapidly too because we wanted to get the thing on the road and get some experience with it. Later Ken Clark went up and his job was to program under Seeber's direction and the dedication problem we decided was to be this moon calculation which was a sizable calculation. ~~One of our associates at the lab~~

(End of tape.)

#1.

This is Tape #2. of Interview TC-1 in the IBM Oral History Project on Computer Technology.

E. Well we just mentioned that Ken Clark was to go to Endicott and assist Seeber in writing programs and of course the dedication problem was to be the lunar computation. Now in getting a problem onto a machine, most people think that you just write some instructions but actually a well planned problem should start first with the problem planning, to make the computations you want to do compatible with the abilities of the machine. So Seeber, Clark and I discussed the lunar problem and just how we would do it as far as the flow of the data and the operations involved, and we did a great extent of numerical analysis and also planning of the computation so that you wouldn't have bottlenecks in the flow of the data that would make an inefficient thing. We had table look-up and that sort of thing and the tables had to be designed to be effective. Well Becky Jones was also one of the members of the Watson Laboratory staff and she worked with me on getting the problem the way we wanted it and how we thought it should be done and the actual outline for writing the program and Ken Clark did the programming, so that by the time the machine was ready to run, the program was ready to run and the dedication of the problem was debugged and it operated.

Later on after the dedication was over we ran the problem for quite a while and produced what is known as the improved lunar ephemeris from 1952 to 72 which has been the standard of comparison ever since. We took just the twenty years plus a few dates at various places for people who wanted them. Recently this ephemeris has been extended back and forward by lots of big computers. So when the machine came, Seeber was put in charge of the machine. He had a group of engineers to keep it going and he had a group of people that we hired to look after the operation of programming. As far as criticizing this calculator, I would say that the worst feature of it was that we had pushed the technology of the time about as far as one could. The result is that we had a huge amount of hardware and there were failures. So the programs had to be written so that failures wouldn't matter. In other words, we always wrote our programs so that there was a continuous running check and as soon as a mistake was made the discrepancy was put into operation again and if it didn't agree that time, it would stop and if it did agree, it would go on. Well this meant that occasional errors of a type that were checkable, accidental errors, would be corrected on the spot and the machine would go on without stopping and then of course when it did stop there was the whole business of diagnosing and correcting.

S. How frequently did it go down?

E. Well I wasn't there during much of the operation. There was always Monday morning sickness. I mean they turned the machine off on Friday night and of course when you started on Monday morning there was always systematic maintenance. So if you got it on the road again within a couple hours you were usually lucky. This is something that Rex Seeber can give you better discussion of than I. But as I say, the programs were designed so that they would be self-checking and that accidental errors would be corrected and the machine would go on. Serious machine errors would stop the machine and they would have to repair it. Otherwise the thing really had a huge store. It had elegant programming facilities. It was really a fine machine from that point of view.

S. So there were no real problems in writing that program for it?

E. ~~Well here again~~ ... you mean the one for the lunar ~~effemeris~~ ephemeris. No here again ~~sure~~ Seeber knew the machine inside and out and I knew the problem inside and out and we discussed the various possibilities of should we do it this way or that way and he had had program problems for the Harvard machine so he had a little more feeling for the program than someone else might have. So we avoided things that would be too difficult for the programmer and we finally agreed on how it should be done and Ken Clark programmed it. Becky Jones and I designed the tables and had them all ready so that the thing started off and solved the problem. It ran very well.

S. How about debugging it? Was there a fairly simple debugging routine?

E. No it was elaborate but both Rex and I had had experience in finding troubles with things. So we had learned that the main problem is to isolate the pieces and to do a piece at a time and get it straightened out and put them together. Of course it was a lot of work and Ken spent quite a lot of time trying to make it run. Later after they had a staff going and the question arose, should we reprogram it and it was reprogrammed, it gave the same answer as the previous program and wasn't an awful lot better speed. So it was a good job of programming.

S. For subsequent such problems, for each person or agency that came with a problem, did Clark program their problem or did they have their own programmers?

E. They set up a group and this included quite a few people. I mean to keep a big calculator like that going you need a lot of programming. This included Seeber and Clark and John Backus and Don Quarrels. We had about a dozen people, a fairly sizable group who came and got experience and would work with the people who had the problems and they would, when a man would come, they would discuss the problem with him and get it analyzed and usually I think they had two people working on a problem or a program so they could check each other. But some of these were very big problems, partial differential equations....

S. Of course these first problems tended to be ones in mathematics. I take it they were. Correct?

E. Well some of them, for instance the one that I had was the evaluation of the calculation of trigonometric terms and the evaluation of arguments and theories. Later we did the numerical integrations of five outer planets which was a big job. They had problems in partial differential equations, hydra-dynamics. There was not what you would call a data processing problem. These were the solutions of scientific problems.

S. Fellows like Backus and the rest, they were mathematicians essentially. Is that why they were recruited, as programmers in a way, because the problems to be programmed were complex mathematical ones?

E. Well for programming in the first place you want mathematical aptitude and there are certain characteristics which you want in a programmer, people who are careful and analytical and logical in their outlook. The training doesn't have to be mathematics. I don't know what all these people had done in the past but mathematics, engineering, physics, anything that's quantitative and rigorous.

S. Is your feeling that they should be familiar at least with the problem and know the steps through which the problem would have to go to be solved?

E. Well they would like to understand enough of the problem to make sure that the person who was promoting or proposing it wasn't overlooking something. And as I said before, frequently you can rewrite your equations, your basic equations before you start to program in such a way that it will be a saving of ten or twenty times running it. So this problem analysis of course was always done jointly and sometimes I think the people who came with the problem would go along with them under guidance.

S. Was the programming group recruited new at that time?

E. Yes. There were no programmers. That's right. Of course this group was what later became Applied Science. They went into Applied Science. The FORTRAN business came out of it.

S. When the SSEC went into operation, how did it compare with other computers that were then in operation? I believe the ENIAC was still in operation and the EDVAC.

E. The EDVAC was not. I don't recall when that came in but I would doubt if it was. The ~~A~~ENIAC was. The ~~A~~ENIAC was not a general purpose computer. It was very limited in what it could do. It was essentially a plugboard controlled type of computer with something like half a dozen words of memory. So that there was no such thing as programming or solving big problems on it. The thing that it did well was the ballistics problem which was a very simple thing and it did this of course, faster than the shell would fly which was very dramatic. The thing that would be more nearly like it would be the Bell Relay Calculators. They had paper tape facilities on them which were quite good but there wasn't anything else that would approach the electronic stored program machine.

S. So the SSEC was in effect the first general purpose stored program electronic computer.

E. Electronic in the sense that the fast operations were all done electronically.

S. Had there been any influences in the development of the SSEC that you know of from people .... or was there any work that had been done at Bell Labs or Aberdeen in the Moore School of Engineering with the ~~A~~ENIAC or that had been done in Europe by fellows like (?) Zuse?

E. I don't think so. I have never been one to run around. If I had a problem I worked on it. Here the calculator had to be built and the limitation seemed to me to be one of what kind of hardware you could use and then to use it effectively. So we solved the problems as they arose. Now as I say, Rex Seiber had been at Harvard but the programming of that wasn't very powerful.

S. What about the machine itself? For instance, did the experience that IBM had in constructing the Mark I . . . . was it Hamilton who did that or Lake. . . .

E. No Hamilton built the Mark I but Lake was involved. Hamilton may have been an assistant to Lake but Hamilton really. . . .

S. Did you ever get an inside as to what extent Hamilton's experience on the Mark I aided him in avoiding problems of making better solutions for the SSEC?

E. ~~Oh well Hamilton -- the fact that~~ He had been through the building of a big calculator, knew how to bite things off and how to organize the project. It's a huge project to build a calculator of that kind. And Frank's experience on this was invaluable because he knew how to organize his project and how to set deadlines and get them done and how to work with a large number of people and not get in each other's way, etc. And many of the techniques were the same -- the things that had to do with relays and punched paper, mechanical input/output things. The Harvard machine also was rather limited in input/output. It had a typewriter for an output. I guess it would read punched cards but that was about all. But there was a big source of experience around Endicott of course on mechanical things. Just these huge paper tape readers that used card stock would have been a major development for anybody else and the engineers there of course knew how to tackle a problem like that and how to get the things built and running and tested. Of course Hamilton didn't know any electronics. He got some young people that knew electronic circuits, but he was able to fit them into the team and get everything going. So between Seeber and the electronics boys and Hamilton with the mechanical people, it was really a powerhouse of design and construction.

S. What was Hamilton's reaction to the suggestion that it be electronic?

E. Well he knew that it made problems but still Palmer and Dickenson had made electronic arithmetic units. So they were able to find out in the house, so to speak, what the problems were and how you had to proceed. I don't think he was too happy about building in ~~six~~ TEN or ~~eight~~ TWELVE thousand vacuum tubes, and he was concerned about all the relays, but we told him that a calculator had to be a calculator and he felt ~~that he was~~ that the Harvard machine had worked well and that the next one would work.

S. So he didn't resist the idea that greater speed would be achieved with electronics?

E. Oh no there was no real question about that. The real question was can you get these circuits worked out and designed and built in large numbers. Actually I don't think the electronic part was all that bad. I think that the biggest single problem was the huge amount of stuff. Of course every unit has the probability of breaking down. Thousands of units have a lot more probability of breaking. So this was, as I see it, the big problem.

S. Did Aiken have any relationship with IBM during that period?

E. No, not that I know of. I think not. As a matter of fact, I think he was building a calculator of his own. He built a magnetic drum machine I believe somewhere along in there.

S. He was working at Harvard?

E. Yes.

S. Was his magnetic drum machine produced before the SSEC?

E. I don't believe so but I don't recall.

~~S. I recall someone telling me of some sort of interchange between T. J. Watson and Aiken around the time that the SSEC was unveiled. Do you recall anything of that nature?~~

E. No.

S. Well the SSEC ran until 1950 or so or longer than that?

E. I don't recall. I think so.

S. Solving problems all the time?

E. Yes.

S. It did one big atomic energy problem.

E. Yes I believe they called it Problem Hippo.

S. Did you follow the electronic computing business beyond the SSEC?

E. Well as I mentioned before, the electronics of the SSEC and the ENIAC and of the 601 and the 604, the circuitry there was basically radio tube type circuitry, things at 100,000 cycles.... and during the war the radar group at MIT and elsewhere were working on microseconds circuits. So when I got Havens and Walker and Lentz to come here, they came from this microsecond environment and they were convinced that nothing in the future would be slower than that. So they had nothing to do with the SSEC. They started doing electronics research under their own guidance in the laboratory and after a few years of this they went in different directions. Havens went more toward the big calculator concept and Lentz went toward miniature calculators with feedback into the personal automatic computer that Lentz designed and built and finally marketed. It had very few vacuum tubes in it.

This was pre-transistor of course. So Havens' work had to do with circuits that would be particularly suitable for a big calculator and in large numbers and he also worked on the electro-static storage tube, the Williams tube. So that after a few years of this it was obvious that the time was ripe to make electronic calculators that were all electronic with comparatively little mechanical stuff. So we discussed the possibility of making a new calculator and decided to make one for the Navy and this was what was later known as the NORC. And Havens designed and built that were in the laboratory and here again, actually in this case one of Seeber's people, Jeenel, wrote a book on programming and came to work on the NORC and here again of course Thomas and I had discussions with By about what people wanted in the way of a calculator and the different trade-offs. Very frequently in designing a calculator it is, can you do this, can you do that? So it's a discussion between the user and the maker. So Thomas and I helped on that. Jeenel of course was responsible for the specific programming system that was used. He had had experience on the SSEC. Von Neuman was a consultant for the Navy. So this was the origin of the NORC.

S. Did the Navy bid that out or did they come directly to IBM?

E. Well they knew that Havens had a lot of stuff here and we all had ideas on the computer and they wanted one so the question arose as to whether we'd build one for them.

S. What year did they come, do you recall?

E. No.

S. But the NORC was finished in about '54, wasn't it?

E. I think so. The last I heard it was still running. It was certainly the most powerful scientific calculator of its time.

S. More powerful than the 701?

E. The 701 was designed to be an all purpose calculator, and an assembly line calculator, the kind that a lot of people could use. But the NORC was designed primarily for big problems, the kind that we had solved or tried to solve on the SSEC. There were a number of very interesting electronic things in that machine. One was that it had only essentially one kind of a circuit that was used for storing and computing the multiplication of two 64-bit numbers. It took just the time it took to write the product in coded form. The numbers flowed in one end and the answer flowed out the other. So there was very fast multiplication, I think something like 30 microseconds for a multiplication. As I say, there were very few types of pluggable units. There was an automatic tester for the plugboard so that you could plug the plugboard unit into this tester and put the right plugboard in and throw some switches for that unit and the thing showed you on the oscilloscope whether it was working and doing what it was supposed to do.

So systematic maintenance was very well organized and the reliability of it was exceedingly high. The cathode ray tube store .....

S. Were we using the Williams tube?

E. Yes. However, afterward the Navy replaced the Williams tube store by magnetic core store.

S. But the NORC is still running?

E. It was the last I heard.

S. I see. And it was specifically built to do largescale mathematical problems?

E. Yes. It was very nice to code. Lots of problems. People just coded machine language without any fuss because it had very nice features. In the first place, it was decimal and the instructions or systems set-up was very good. Paul ~~Harkett~~ HERGET for instance would go down there. He would discuss a problem with somebody and he would decide he would go and do the problem. So he'd walk in and sketch out a program and punch it up and stick it on and it would run. Part of this is the fact that he is a very good programmer but part of it is the fact that the system is clear and simple.

S. Was that installed at the Bureau of Ships or the Navy Department?

E. No it's at ~~Dalgrin~~ DAHLGREN, at the Naval Weapons Laboratory. When I was last there they had it running beside a STRETCH.

S. What was the impact of the development of the UNIVAC on the operation here. Did you keep up with the development of computing outside of IBM and did that cause any reaction within the laboratory here or within the company or among the people who were using the machines in this laboratory?

E. I didn't have much to do with it. I felt that there were other people in the company who could handle the competitive aspects and that this was their business. My job was rather to make sure that some of the things we were trying to do were done properly and to get the right kind of people into the place. You see let's say when we started the lab, the first question was the role of scientific computation and the role of the electronic calculator. With the SSEC we had the general concept of the stored program calculator and how things were to run and how a group would operate them and how one could maintain them and all this. And then the NORC was a sort of a culmination of the big fast electronic calculator and then while the NORC was being designed and built, this laboratory was shifting emphasis from electronics to physics. So our principal operation became that of research and physics, largely solid state. This program took the center of the stage then.

S. Was this because we had organized to produce big computers in Poughkeepsie and so forth and therefore there was no need any longer for the laboratory to concern itself with more or less the hardware aspects?

E. Yes you see when the SSEC was being designed there were basic questions of what a calculator should be like. By the time the NORC came along, a lot of other people had ideas of what a calculator should be like and the NORC was a huge, fancy computer. By this time it was felt that, well our general policy here has not been to do things that can be done well elsewhere. We, by choice, decided that we would keep a building that could only hold about sixty people. So it is foolish for us to try to go into big calculator design and big projects. We are a research laboratory. So in the early days it was appropriate to have our mathematical operations and our small electronic group and the NORC was really stretching it a point. I mean that was really not company policy to build the NORC here. The only reason it was built here was because the people were here and they could do it more effectively. But as soon as that was done, there were fifty people on that, but they were mostly moved out of here. They were mostly brought in just for the project. So they all went to the engineering laboratory elsewhere. And in the meantime we had built up our physics group. Then of course physics grew at Yorktown. So that at the moment our biggest expansion is in the life sciences.

S. Let me just go back a bit Dr. Eckert to that switchover from electronics to physics. I recall when I first came into Research in the late 50's that people were kind of deciding whether they really wanted to go up to Yorktown to the Watson Laboratory or stay down here and I guess this was the period of switchover from physical research in semi-conductors and so forth from here up to Yorktown. Is that correct?

E. Probably yes.

S. ~~What was~~ it . . . we're getting right now really, switching from electronics, into a discussion of the beginning of components in the company. Is that correct?

E. Well ~~as I say~~, that work started here at the laboratory ~~from~~ the solid state physics ~~on up~~, although there was solid state physics started at Yorktown. But I think our general policy here is that this is a small group at a university. It should not get too big. It should not try to do things that have to be done in a big way. It is basically made up of a group of individuals with one or two or three associates and whenever an operation becomes bigger than this, then it should move. And so the people here have this choice to make, whether they want to stay and run a small thing where they are free to do pretty much as they want to except to grow. And if they want their operation to grow, it just isn't done here. So when the electronics got big, then we completed the project and that stopped. By that time of course there was lots of electronics at Yorktown and so it would have been superfluous for us to try to compete with these large developments. Also there's a tendency for people like Havens to .... something. In the meantime anybody who is a member of the staff is free to stay here. Thomas has stayed and I've stayed. A good many others have gone elsewhere. Redfield has been here a long time. Koenig came in at the beginning. On the other hand, Gunther-Mohr and Gardiner Tucker and Art Anderson and all of these people have gone elsewhere.

S. And they were working on semi-conductors and solid state devices?

E. Art Anderson worked here with Walker on a way out project called Spin Echoes. That was a phenomenon. You could take a teaspoonful of water and orient the molecules in it and store information. And Bob Walker and Art Anderson worked on that for a long time to investigate the possibilities of using it for store.

S. Did people like let's say Anderson tend to go out to San Jose to work on some other things.

E. Well he got his Ph. D. and the San Jose Lab was trying to build up the kind of thing he was good at and he went there. John Lentz proved a way and he joined the cryogenic project at Yorktown and then he came back here later to a different kind of project.

S. ~~He lectured~~ Of course during these years ~~and~~ there were little interesting things coming out of the laboratory. There was work in lasers that went on.... Thomas's work in lasers,

E. Well Havens was working on masers. That was before the days of lasers. ~~By the time~~ When By finished the NORC he decided that he would investigate microwave circuits and he did quite a number of things. One of them was this experiment, the redoing of an electronic version of the Michaelson-Morley experiment. They had a couple of masers for that. This was in the days of the gas. Bob Walker a long time ago was doing 50 microsecond circuit work .... that would be 20 nanosecond work. That was years ago. I mentioned John Lentz building a calculator that called for few vacuum tubes. Of course the microwave group that Havens developed was moved to Yorktown later. That includes Land (?) and a number of those people.

S. How did you feel personally about watching the process whereby people would come in and do relatively scholarly work or experimental work and then when it looked like there was some sort of an application for it, the people moved away more into the main stream of the practical side of development. What were your personal reactions to this rhythm in the laboratory?

E. Well I thought it was fine. My feeling was that the company could be a place where people could do just as good basic research as in a university and people who do basic research, many of them are exceedingly able people and that the company was big enough to afford having some of these people in the company and that the way to get them was to arrange a place where they could do the same kind of research that they might at a university and the way to keep a place like that is to be able to bring in new people and not have the place get too big and not get Projecitis. So we've been able here to have less Projectitis than many universities. The tendency in a university is to set up projects and grants and to build up large operating groups. But our policy was that we weren't going to do that. We were going to be more like the university than the university is. That we would not get a huge building and then have to fill it and that our people didn't have to make the decision. This was sometimes very attractive to young people. You picked a young man just out of graduate school and it's hard for him to decide whether he wants to go into industrial work or into a university. Here he can do both. He can come here and if he rang the bell on his own research, he could either stay or go to another university. We didn't feel that there was anything wrong with that. If he decided that he wanted a bigger group and in the meantime had found something that he wanted to work on and he needed twenty people, he had the opportunity of going somewhere else. So this has worked out very well I think. There have been cases where people had to move that didn't want to but by and large the people who wanted to go, wanted to. For instance, Gardiner Tucker, when he first came to the lab, when he first got his Ph. D., he knew he was interested in research management so he did research here very well for a while and then he decided that he would transfer to Yorktown. So this has been the way of life.

S. You mentioned that now the laboratories work in the area of the life sciences. When did that start?

E. Oh time flies. I think that was about three years ago.

S. What was the rationale for moving in that direction?

E. Well in the first place, the feeling that life sciences are attracting some of the brighter people at the moment. I mean there was a time when physics attracted all of us -- the sharp youngsters and now apparently the life sciences is doing this and we'd like a few of those people in the company. It's an area where there are going to be sharply important discoveries made in the next few years. It's closely connected with medicine and things of that sort and we feel that there's going to be a lot of business in medical and testing and those areas, so we would like to have knowledgability in the company. You see after the war the electronics was the big white hope and then solid state was discovered. We felt that it was not just solid state but other areas where physics might influence our machines such as the microwave business which is partly electronics but not entirely so. So at that time we felt we needed to build up physics, savvy in physics for the company.

S. Well ~~let's say~~ in ~~all~~ my few short years with the company I never realized that the Watson Lab was performing this precise function that you just described -- which is a very interesting type of rationale behind the laboratory. Since you were the one who really started it, I take it that this ~~is~~ was your conception and your policy. You mentioned a lot of things -- that you didn't want people working while they were appointed at Columbia -- you wanted to keep it kind of pure and not mix up the moonlighting let's say and teaching with working, etc. That you wanted more or less a pure scholarly sense. I take it that all of these policies that we have been discussing are basically yours.

E. Well more than that I think they have been done with my assistance and concurrence ... and of course when I first came I didn't have a Twenty Year Plan. When I first came I didn't expect even to get electronics into the lab. I thought then in terms of applied mathematics and the computational sciences and machine design and use, and that the engineering would be done elsewhere. But I felt that after a short time that our engineering and electronics was not adequate and that I needed in the house some people who were not tied up on development programs. There were people in the company who were trying to build the 604 and the 650 and the 701 and so on. I felt that we really needed the other type of electronic research that is more or less undirected. I came to this conclusion within a few months after I came here. When I first came I didn't think that we would have electronic research at the lab. So then we did and went on for a long time and then I didn't know at that time that we would go into ~~phys~~ physics either but after a while it just seemed to me that the company needed the kind of physics we could do here so we got this building and developed it. Now the life science wasn't my idea particularly but I approved of it. This was Gardiner Tucker's first whim. He felt that this was something that ought to be done and so it was decided.

S. Do you think the Watson Laboratory will continue this function in the company of drawing people into the company and permitting them to do really the more pure or unstructured experimental work in the company and to act as a kind of feeding ground as it has in the last twenty years?

E. Oh I retired as Director a year ago and I haven't seen any change in policy since I retired. But even before that I felt that if the lab essentially was going to continue in the future that I shouldn't try to master mind it any longer. So for two or three years before I retired as Director, I started shifting the responsibility on to Dick Garwin and Koenig and others, just on the feeling that I had been here for a long time and if the lab is to go on the way it should, it shouldn't go on because of me but because of other people. So essentially for the last three years I've been withdrawing from management and as far as I can tell, I haven't seen any change in policy. For many years of course, during the life of the laboratory, in the early days there were times when I had to do a great deal of selling new ideas. And then as I say, within the last few years my feeling was that I shouldn't sell these ideas. Either they were good or they were bad and this was somebody else's problem. So what I've noticed is that gradually management has taken to this idea and that it is not any longer my problem.

S. Did you encounter any resistance to this particular social research function of the laboratory along the way? I take it that originally when T. J. Watson asked you to start it you had cooperation. Were there rough spots along the way?

E. Oh yes. There are always different opinions on what the company should do with the money and when budgets get tight, then they say well why don't we close this down and do something else here instead. This is to be expected. And moreover, until this laboratory, the company had no scientific management. We had no group of scientists with scientific background. We had engineers. We had salesmen. We had lawyers and what have you, but no scientists. I think I probably was the first one who came to the company with a Ph. D. and I suppose another reason why we have more sympathy outside the lab now is because of the alumni from here. I mean if you look over the scientific and engineering management of the company now, there are about fifty people that are fairly well thought of that all started here. Our people have almost invariably made good elsewhere in the company. Havens is head of Engineering for World Trade. Art Anderson is Director of Research. Gardiner Tucker was Director of Research. Holtzberg, Reisman, Rosenheim, Walker. We have a very interesting group of alumni. And of course we also have a very distinguished group of people who came here in the early days and studied computing. Starting about 1945 and for the next ten years we trained in one program fifteen hundred people and these are the people now largely who are running computing laboratories around the country and the world.

S. Well from what you've told me Dr. Eckert, it was an excellent idea that you and Mr. Watson developed.

E. It's been a lot of fun.

(End of interview.)

From: "ARCHIVE1" <archive1@us.ibm.com>
To: "Frank da Cruz" <fdc@columbia.edu>
Sent: Monday, August 02, 2010 10:57 AM
Subject: Re: Wallace Eckert Interview

Hi Frank,

I have copied the Wallace Eckert interview and will mail it you. I could not locate any phone/office lists for the Watson Lab.

Best regards,
Stacy L. Castillo

Reference Desk
IBM Corporate Archives
Route 100/CSB
Somers, NY 10589
914-766-0612
archive1@us.ibm.com

Frank da Cruz ---08/02/2010 09:39:26 AM---Hi! Any chance I can get a copy of this?
Transcript of interview of Wallace J. Eckert by Lawrence

From: Frank da Cruz <fdc@columbia.edu>
To: ARCHIVE1/Armonk/IBM@IBMUS
Date: 08/02/2010 09:39 AM
Subject: Wallace Eckert Interview

Hi! Any chance I can get a copy of this?

Transcript of interview of Wallace J. Eckert by Lawrence Saphire. IBM Thomas J. Watson laboratory at Columbia, July 11 and July 20, 1967. Interview TC-1 in the IBM Oral History Project on Computer Technology.

I just found out about it, and it might clear up some mysteries in here:

http://www.columbia.edu/acis/history/eckert.heml

Thanks!

- Frank

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http://www.columbia.edu/acis/history/

P.S. Speaking of Watson Lab, I wonder if you have any phone / office / personnel lists from the two Watson Labs at Columbia University, 1945-1970. I confess, I'm curious whose office I'm sitting in!