The Computer

from Pascal to von Neumann

Herman H. Goldstine

To Adele

But if the while I think on thee, dear friend,
All losses are restored, and sorrows end.

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John Louis Neumann was born into a well-to-do family in Budapest on 28 December 1903, during the last socially brilliant days of that city under the Hapsburgs. His father Max, a banker, was a partner in one of the city's important private banks and was able to provide well for his children both intellectually and financially. He was ennobled in 1913 by the Emperor with the Hungarian title of *Margattai,* which young von Neumann later Germanized to *von.* His father and mother, Margaret, had three sons, John, Michael, and Nicholas, of whom John was the eldest.

While still very young, von Neumann showed tremendous intellectual and linguistic ability, and he once told the author that at six he and his father often joked with each other in classical Greek. He studied history as an avocation and became a first-rate historian. Later he was to concentrate on the culture of Byzantium, and he had a truly encyclopedic as well as profound knowledge of this as well as a number of other societies.

One of his most remarkable capabilities was his power of absolute recall. As far as I could tell, von Neumann was able on once reading a book or article to quote it back verbatim; moreover, he could do it years later without hesitation. He could also translate it at no diminution in speed from its original language into English. On one occasion I tested his ability by asking him to tell me how the *Tale of Two Cities* started. Whereupon, without any pause, he immediately began to recite the first chapter and continued until asked to stop after about ten or fifteen minutes. Another time, I watched him lecture on some material written in German about twenty years earlier. In this performance von Neumann even used exactly the same letters and symbols he had in the original. German was his natural language, and it seemed that he conceived his ideas in German and then translated them at lightning speed into English. Frequently I watched him writing and saw him ask occasionally what the English for some German word was.

His power of remembrance was a great help to his strong sense of humor because he was able to remember any story he wished. He thereby built up an unparalleled storehouse of anecdotes, limericks, and funny happenings. He loved to lighten up otherwise informal but serious discussions with just the right stories. In this he had a Lincoln-like quality. Whenever friends visited him they always tried to bring along some new stories as a sort of present to him.

He also greatly enjoyed people's company, and his house was the scene of most wonderful parties and dinners. He had a keen sense of responsibility to the temporary members of the Institute for Advanced Study and felt an obligation to introduce them socially to their colleagues. Accordingly, at least once a week he and his wife would entertain a houseful of people—ranging from young men just out of graduate school to visiting older distinguished scientists passing through Princeton. He delighted in entertaining his guests with anecdotes or relevant quotations, usually humorous, from history. In this he was superb. He had considerable ability at telling stories, particularly very long ones, so that his hearers were almost breathlessly waiting for the denouement.

The Bela Kun communist uprising in Hungary in 1919 found the von Neumann family fleeing to a place they owned in Venice. There was apparently little doubt that his father feared for the safety of his family at the hands of the communists. This experience had a great impact on von Neumann, who developed out of this a strong dislike, or even hatred, for all that communism stood for.[1]

After Bela Kun was defeated, a most remarkable period in the history of science occurred in the fragments of the Austro-Hungarian Empire. Out of the repression and intellectual sterility that were the hallmarks of Hapsburg rule there grew up suddenly most remarkable groups of scientists of whom von Neumann and Eugene Wigner were examples in Hungary, Banach in Poland, and Feller in Jugoslavia.[2] Prior to this period there had of course been other great scientists in the Austro-Hungarian countries such as Sigmund Freud, Georg von Hevesy, and Theodore von Karman, but in general Central Europe was not an ideal climate under the Haps-

[1] The von Neumanns left Hungary as soon as they could, a month after Bela Kun seized power. The Communist regime lasted 130 days; they returned almost two months after he was driven out. In von Neumann's testimony at the hearing on Oppenheimer he said, "I think you will find, generally speaking, among Hungarians an emotional fear and dislike of Russia."

[2] From the early 1950s Feller had a considerable interest in computers and was for many years a consultant to me and my successors at IBM, where he was of immeasurable importance to the development of a mathematical sciences department of first quality.

burgs and indeed both Hevesy and von Karman spent their intellectual lives outside Mittel Europa.

Whatever the reasons for the intellectual renascence, von Neumann was in all likelihood the greatest of all the remarkable figures produced during this period. This is a most difficult evaluation to make because the men of that period are by any standards outstanding. It has been more than fortunate for the United States that so many of the best of these men fled there to escape the intellectual, racial, and religious persecutions of the Nazi era.[3]

Von Neumann, who was known to almost everyone as Johnny —and to some as Jancsi—was so impressive in school that one of his teachers, Laszlo Ratz, persuaded his father to have him tutored privately in addition to his regular schooling.[4] Before he was 18 he published a paper with his tutor, M. Fekete, a well-known Hungarian mathematician. He attended the Lutheran gymnasium in Budapest from 1911 until his graduation in 1921. At this time his father made an extremely large gift to the school, which was one of the best in Hungary. It was very fortunate in having Ratz on its faculty; he was an excellent teacher, had a great influence on both von Neumann and Wigner, and finally became rector of the school.

Through the kindness of Carl Kaysen, Director of the Institute for Advanced Study, I had access to von Neumann's report cards issued while he was in this gymnasium. It is amusing to see his early strengths—he was recognized as the best mathematician the school produced—and his weaknesses. All his grades were A except for those in geometrical drawing B, writing B, music B, physical education C, and behavior sometimes A but more often B.

In 1921 he enrolled in the University of Budapest, but he spent the years 1921–1923 in Berlin where he came under the influence of Fritz Haber. From Berlin he went to the Swiss Federal Institute of Technology (Eidgenössische Technische Hochschule) in Zurich, where he had contacts both with Hermann Weyl, a superb mathematician and one of his future colleagues at the Institute for Advanced Study, and with George Polya, one of the greatest teachers of mathematics. He obtained a degree in chemical engineering

[3] The interested reader may wish to consult a charming book on some of these men. See Laura Fermi, *Illustrious Immigrants, The Intellectual Migration from Europe, 1930–1941* (Chicago, 1968).

[4] For biographical material see S. Ulam, "John von Neumann, 1903–1957," *Bull. Amer. Math. Society*, vol. 64 (1958), pp. 1–49; S. Bochner, "John von Neumann," National Academy of Sciences, *Biographical Memoirs*, vol. 32 (1958), pp. 438–457; or H. H. Goldstine and E. P. Wigner, "Scientific Work of J. von Neumann," *Science*, vol. 125 (1957), pp. 683–684.

at the Federal Institute in 1925; and the next year, on 12 March 1926—at the age of twenty-two—he received his doctorate summa cum laude in mathematics with minors in experimental physics and chemistry from the University of Budapest!

Then in 1927 he became a Privatdozent in mathematics at the University of Berlin, where he stayed three years and established a world-wide reputation by his papers on algebra, set theory, and quantum mechanics. Ulam recounts how already by 1927, when von Neumann attended a mathematical congress in Lvov, Poland, he was pointed out to the students as a "youthful genius."

It is clear that by 1927 he was already recognized as a great mathematician, and after spending 1929 in Hamburg he was invited to Princeton University as a visiting lecturer in 1930. He stayed on as a visiting professor and in 1931 became a permanent professor. Then in 1933 he went to the Institute for Advanced Study, which was then housed in Princeton's Fine Hall, the mathematics building built by Veblen in honor of Dean Henry B. Fine. Earlier we recounted how von Neumann and Wigner came to the States at Veblen's invitation (above, p. 80).

The combination of the University's and the Institute's faculties of mathematics in one building constituted one of the greatest concentrations of leaders in mathematics and physics the world had ever seen. The only parallel was the great department at Göttingen, which was by this time (1933) already much on the decline; indeed, by then a Nazi was already head of the Mathematical Institute, and Courant, Landau, Emmy Noether, Bernays, Born, Franck, Weyl, and many others were soon to leave the Institute or had already done so. Otto Neugebauer was made head of the Institute and stayed in office for exactly one day. When he refused to take the loyalty oath required by the Nazis, he left Germany.[5] Hitler's rise to power was largely responsible for the great group of celebrities at Princeton. Not that all there were refugees, but many were. The time was now up for European dominance in mathematics and physics, a dominance that had been so strong that for many years the *Bulletin* of the American Mathematical Society regularly listed the courses of lectures to be given in Göttingen.

The great leader of German mathematics during the halcyon days of Göttingen was David Hilbert (1862–1943); he had a profound influence upon world mathematics and became during his lifetime the style-setter for the entire mathematical and theoretical physics worlds. This was an unparalleled accomplishment. Hil-

[5] C. Reid, *Hilbert* (New York, 1970).

bert's role in mathematics can perhaps best be summed up by mentioning his paper at the International Congress of Mathematicians in Paris in the year 1900. In this speech, the major address of the Congress, Hilbert undertook to formulate a set of 23 problems whose "solution we expect from the future." These problems were to be in a real sense a blueprint for modern mathematics. One of von Neumann's great accomplishments was to be a partial solution of the fifth problem. Even today the unsolved ones are still at the forefront.

It is against this background that we should understand some of what follows. Among the physicists who at various times during the 1920s were members of this magic group at Göttingen were Max Born and James Franck—permanently there—P. M. S. Blackett, Karl Compton, Paul Dirac, Werner Heisenberg, Pascual Jordan, Lothar Nordheim, Robert Oppenheimer, Wolfgang Pauli, Linus Pauling, and Eugene Wigner. During this same period von Neumann was also going to Göttingen to work with Hilbert on formal logics and physics. Miss Reid quotes Nordheim, in comparing these two men, as saying that Hilbert was "slow to understand" but that von Neumann had "the fastest mind I ever met." This was in 1924.

There were a number of standard anecdotes around the mathematical community to illustrate the fantastic speed of von Neumann. One of these he asserted was not true, but it is illustrative. Hermann Weyl is supposed to have given a preliminary lecture on the profundity of the next theorem he was going to prove indicating why the proof had to be very difficult. The next day he gave this lengthy and hard proof. At the end, the story goes, young von Neumann jumped up and said, "Would you be so good as to look over the following proof?" Whereupon he wrote a very few lines and gave an entirely novel and simple proof.

Another, true incident illustrating this speed occurred in Princeton. It was von Neumann's wont to keep an open door for all visitors to the Institute, and they usually came to see him for help when in mathematical trouble. Beyond anyone else he could almost instantly understand what was involved and show how to prove the theorem in question or to replace it by what was the true theorem. On this occasion, a young man stated his difficulty, von Neumann then gave the proof in detail on the blackboard, and the student nodded, thanked him, and left. The next Saturday night at von Neumann's party the same man approached von Neumann and said that he had forgotten the proof and would von Neumann mind repeating it. This von Neumann did standing in a crowded room.

In 1930 von Neumann married Marietta Kovesi, and in 1935 they had a daughter, Marina, who has had a distinguished career not only as a wife and mother but also as professor of economics at the University of Pittsburgh. Von Neumann's marriage ended in 1937, and in 1938 he married Klara Dan. She later became a programmer for the Los Alamos Scientific Laboratory and helped to program and code some of the largest problems done in the 1950s.

It is time now to return to von Neumann's early days with Hilbert. Under the latter's influence von Neumann embarked on a program which was to have a profound influence on his later work on computers and related topics. Hilbert had become strongly involved in a great program in the foundations of mathematics. He undertook this task because there was much concern at the turn of the century about the integrity of mathematics. For centuries it had been a basic creed of mathematicians that deductive reasoning when properly used could never lead to inconsistent results. Then Bertrand Russell and Alfred North Whitehead brought out their classic.[6] This linked back to Boole in the sense that he provided the genesis for the whole subject of mathematical logics. He had set up a formal machinery for expressing mathematical thoughts and this culminated in 1905 with a very exhaustive treatise by E. Schröder.[7]

Then the modern school started with the works of Frege and Peano which lead up to Whitehead and Russell. What they attempted was stated in 1903 by Russell: "The present work has two main objects. One of these, the proof that all pure mathematics deals exclusively with concepts definable in terms of a very small number of fundamental logical concepts, and that all its propositions are deducible from a very small number of fundamental logical principles. . . ."[8] This work was intended to annihilate both the positions of Hilbert and of L. E. J. Brouwer, a Dutch mathematician. Hilbert attempted to rescue mathematics "from the stickier quagmires of classical metaphysics"[9] by separating the ideas of mathematics from their meaning; concepts such as the positive integers were no longer to have any intuitive meaning drawn from experience but were to be viewed as abstract entities obeying certain formal laws and only those. This is known as *formalism.* To make this approach valid Hilbert and his colleagues, including

[6] *Principia Mathematica* (London, 1903).

[7] *Vorlesungen Über die Algebra der Logik*, 4 vols. (Leipzig, 1890, 1891, 1895, 1905).

[8] B. Russell, *Principles of Mathematics* (New York, 1950). The first edition appeared in 1903 and "most of it was written in 1900."

[9] E. T. Bell, *Development of Mathematics* (New York, 1945), p. 557.

von Neumann, embarked on a program to prove that all mathematics was consistent; to show that under their schema the old Greek ideal would reappear. There could be no contradictions, no inconsistencies. Hilbert undertook this program as his answer both to Russell-Whitehead on the one hand and Weyl-Brouwer on the other. This was his way of cutting mathematics loose from logics where Burali-Forti, Richard, and Russell had shown there were paradoxes to trap the unwary.

The group under Brouwer had gone on a very different tack. Brouwer, and later Weyl, refused "to regard a proposition as either true or false unless some method exists of deciding the alternative."[10] The method referred to must be expressed or utilized in a finite number of steps. Otherwise, in dealing with infinite sets Brouwer and Weyl rejected the law of the excluded middle which says that if a proposition is true its contradiction must be false. This law is familiar to all of us from plane geometry where we frequently made proofs by saying "suppose the contrary were true" and then reached an absurdity. This justified the proposition for us but not for Brouwer and Weyl. Their position rendered in doubt very large areas of mathematics and caused many mathematicians great anguish, among them Hilbert, who said: "I believe that as little as Kronecker was able to abolish the irrational numbers . . . just as little will Weyl and Brouwer today be able to succeed. Brouwer is not, as Weyl believes him to be, the Revolution—only the repetition of an attempted *Putsch*, in its day more sharply undertaken yet failing utterly, and now, . . . doomed from the start!"[11]

This is the scene upon which von Neumann came in 1924. In 1927 he published his famous paper on the problem of the freedom of mathematics from contradiction. It is worthy of note that von Neumann at that time conjectured that all of analysis can be proved consistent. A few years later, in 1930, another great young mathematician and logician, Kurt Gödel, showed that certain logical structures contain propositions whose truth is undecidable within the system, that not everything can be decided.[12] His proof was very direct. He in fact produced "a true theorem such that a formal proof of it leads to a contradiction."[13] The structures where this anomalous behavior can occur are not bizarre or pathological in

[10] Russell, *Principles*, p. vi.

[11] Quoted in Reid, *Hilbert*, p. 157.

[12] K. Gödel, "Über formal unentscheidbare Sätze der *Principia Mathematica* und verwandter Systeme," I, *Monatschafte für Mathematik und Physik*, vol. 38 (1931), pp. 173–198.

[13] Bell, *Development of Mathematics*, p. 576.

character. Even arithmetic cannot be shown to be contradiction-free in the Hilbert sense.

Von Neumann remarked that Gödel was the greatest logician since Aristotle and once told a charming tale about himself. To appreciate the story fully one should know that he was inclined to work on a particular problem as long as it went forward. At the end of a day's work he would go to bed and very often awaken in the night with new insights into the problem. In this case he was busily engaged in trying to develop a proof just exactly the opposite of Gödel's and was unsuccessful! One night he dreamed how to overcome his difficulty, arose, went to his desk, and carried his proof much further along but not to the end. The next morning he returned to the attack, again without success, and again that night retired to bed and dreamed. This time he saw his way through the difficulty, but when he arose to write it down he saw there was still a gap to be closed. He said to me, "How lucky mathematics is that I didn't dream the third night!"

It was his training in formal logics that made him very much aware of and interested in a result which foreshadowed the modern computer. This was contained in independent papers published by Emil L. Post and Alan M. Turing in 1936.[14] Post taught at City College of New York and Turing was an Englishman studying at Princeton University (1936–1938). Each of them conceived of what is now called an automaton and described it in similar, mechanistic terms. The men worked independently and in ignorance of each other. There is no doubt that von Neumann was thoroughly aware of Turing's work but apparently not of Post's. We shall revert to this topic again in its proper temporal order. At this time, however, it is just worth mentioning in passing that the Post-Turing work would have delighted Leibniz, since their automata surely carry out his dream of "a general method in which all truths of the reason would be reduced to a kind of calculation." Indeed, this is exactly what their automata do.

One of the interesting things Gödel did was to designate each provable theorem by a sequence of integers with a corresponding situation for remarks about the theorem. This provides a numerical algorithm for each theorem and puts us in the field of numerical computation.

Very early in von Neumann's career he displayed the interest he

[14] Post, "Finite Combinatory Processes – Formulation 1," *Journal of Symbolic Logic*, vol. 1 (1936), pp. 103–105; Turing, "On Computable Numbers," *Proc. London Math. Soc.*, ser. 2, vol. 42 (1936), pp. 230–265.

always had in the applications of mathematics. In 1927 and 1928, at about the same time as his work on proof theory, he published several papers on the mathematical foundations of quantum theory and probability in statistical quantum theory, which showed his profound understanding of physical phenomena. Indeed he was unsurpassed in his ability to understand completely very complex physical situations. Unlike many applied mathematicians who want merely to manipulate some equations given them by a physicist, von Neumann would go right back to the basic phenomenon to reconsider the idealizations made as well as the mathematical formulation.

He possessed along with all his other accomplishments a truly remarkable ability to do very elaborate calculations in his head at lightning speeds; this was especially noticeable when he would be making rough order of magnitude estimates mentally and would call upon an unbelievable wealth of physical constants he had available.

His great interest in the applications of mathematics was to become increasingly important as time went on, and by 1941 it had become his dominant interest. This was to have the most profound implications for the computer field in particular and for the United States in general. In this connection it is worth drawing attention to a paper von Neumann wrote in 1928 on game theory. This was his first venture in the field, and while there had been other tentative approaches—by Borel, Steinhaus, and Zermelo, among others—his was the first to show the relations between games and economic behavior and to formulate and prove his now famous minimax theorem which assures the existence of good strategies for certain important classes of games.[15] In the well-known book by von Neumann and Morgenstern it is stated that: "Our considerations will lead to the application of the mathematical theory of 'games of strategy' developed by one of us in several successive stages in 1928 and 1940–1941."[16]

This typifies something we find in all von Neumann's work: a very few motifs that constantly interweave and recur, usually in unexpected but profound ways; invariably they are aesthetically pleasing. Indeed, von Neumann was one of the greatest of all mathematical artists. He had a completely sure sense of what was elegant mathematically and was always watching for these aspects. It was never enough for him merely to establish a result; he had to

[15] "Zur Theorie der Gesellschaftspiele," *Math. Ann.*, vol. 100 (1928), pp. 295–320.
[16] *Theory of Games and Economic Behavior* (Princeton, 1944), p. 1.

do it with elegance and grace. He would often say to me while we worked on some topic, "Now here is the elegant way to do this."

One of the difficulties people experienced in listening to one of his lectures on mathematics was precisely its sheer beauty and elegance. They were often so taken in by the ease with which results were proved that they thought they understood the path von Neumann had chosen. Later at home when they tried to re-create it, they discovered not the magical path but instead a harsh, forbidding forest. Be it said in their defences that von Neumann usually chose a square area roughly two feet on a side on an enormous blackboard and seemed to play the game of seeing if he could confine all his writing to this tiny area. He did this by judicious use of an eraser and made it just barely possible for his auditors to take down what he had written.

It is interesting to contrast his style in lectures with his written one. His oral style was wonderfully clear and somehow very inspiring and uplifting. His written style was highly elegant, symmetrical, and in every detail complete but often lacking in indications as to why he chose as he did among various alternatives. It was these that he introduced into his oral expositions when he shared with his auditors his supreme insights. His written style was not heavy but had a certain inevitable complexity of thought that made it sometimes difficult to comprehend. Perhaps it derived from his familiarity with German, Latin, and Greek.

His oral expression on the other hand clearly derived from his absolute mastery of idiomatic American English and his understanding of the American mind and style. The only difficulties he had in pronouncing English were those associated with "th" and "r"; but he had a delightful Hungarian accent and also certain fixed mispronunciations which he carefully preserved. One of the best of these was the way he pronounced "integer" as "integher." Once, in the author's hearing he said the word properly but then quickly corrected himself and again said it in his own style.

The story used to be told about him in Princeton that while he was indeed a demi-god he had made a detailed study of humans and could imitate them perfectly. Actually he had great social presence, a very warm, human personality, and a wonderful sense of humor. These qualities, together with his incredible mental capacity, made him a superb teacher. It has been said of him: "No appraisal of von Neumann's contributions . . . would be complete without a mention of the guidance and help which he so freely gave to his friends and acquaintances, both contemporary and younger than himself. There are well-known theoretical physicists who believe that they have learned more from von Neumann in personal conversations than from any of their colleagues. They value what they learned from him in the way of mathematical theorems, but they value even more highly what they learned from him in methods of thinking and ways of mathematical argument." [17] With real justice it can be said of him, in the words of Landor, that he "warmed both hands before the fire of life."

By the mid-1930s von Neumann had become deeply involved in the problems of supersonic and turbulent flows of fluids. "It was then that he became aware of the mysteries underlying the subject of non-linear partial differential equations. . . . The phenomena described by these non-linear equations are baffling analytically and defy even qualitative insight by present methods." [18] Thus by the beginnings of World War II von Neumann was one of the leading experts on shock and detonation waves and inevitably became involved with the Ballistic Research Laboratory, with the OSRD, with the Bureau of Ordnance, and with the Manhattan Project—all to their great good. It is not our place here to write anything like a biography of this great figure but rather to sketch in just those portions of his career that we need to explicate his role in the computer field. Instead of setting out all his activities in detail, perhaps we may be forgiven if we quote von Neumann himself speaking before the Special Senate Committee on Atomic Energy on his wartime activities and then copy Professor Bochner's brief chronology of von Neumann's career.

> Senator McMahon, Gentlemen:
>
> I assume that you wish to know my qualifications. I am a mathematician and a mathematical physicist. I am a member of the Institute for Advanced Study in Princeton, New Jersey. I have been connected with Government work on military matters for nearly ten years: As a consultant of Ballistic Research Laboratory of the Army Ordnance Department since 1937, as a member of its scientific advisory committee since 1940; I have been a member of various divisions of the National Defense Research Committee since 1941; I have been a consultant of the Navy Bureau of Ordnance since 1942. I have been connected with

[17] Goldstine and Wigner, "Scientific Work of J. von Neumann," p. 684.
[18] Ulam, *op. cit.*, pp. 7–8.

the Manhattan District since 1943 as a consultant of the Los Alamos Laboratory, and I spent a considerable part of 1943–45 there.[19]

JOHN VON NEUMANN: CHRONOLOGY[20]

1903	Born, Budapest, Hungary, December 28.
1930–33	Visiting Professor, Princeton University.
1933–57	Professor of Mathematics, Institute for Advanced Study, Princeton, N.J.
1937	Gibbs Lecturer, Colloquium Lecturer, Bôcher Prize, all in American Mathematical Society.
1940–57	Scientific Advisory Committee, Ballistics Research Laboratories, Aberdeen Proving Ground, Maryland.
1941–55	Navy Bureau of Ordnance, Washington, D.C.
1943–55	Los Alamos Scientific Laboratory (AEC), Los Alamos, N.M.
1945–57	Director of Electronic Computer Project, Institute for Advanced Study, Princeton, N.J.
1947	D.Sc. (hon.), Princeton University; Medal for Merit (Presidential Award); Distinguished Civilian Service Award, U.S. Navy.
1947–55	Naval Ordnance Laboratory, Silver Spring, Maryland.
1949–53	Research and Development Board, Washington, D.C.
1949–54	Oak Ridge National Laboratory, Oak Ridge, Tennesee.
1950	D.Sc. (hon.), University of Pennsylvania and Harvard University.
1950–55	Armed Forces Special Weapons Project, Washington, D.C.; Weapons System Evaluation Group, Washington, D.C.
1950–57	Member Board of Advisors, Universidad de los Andes, Colombia, South America.
1951–53	President, American Mathematical Society.
1951–57	Scientific Advisory Board, U.S. Air Force, Washington, D.C.
1952	D.Sc. (hon.), University of Istanbul, Case Institute of Technology, and University of Maryland.
1952–54	Member, General Advisory Committee, U.S. Atomic Energy Commission, Washington, D.C. (Presidential appointment).
1953	D.Sc. (hon.), Institute of Polytechnics, Munich; Vanuxem Lecturer, Princeton University.
1953–57	Technical Advisory Panel on Atomic Energy, Washington, D.C.
1955–57	U.S. Atomic Energy Commissioner (Presidential appointment).

[19] Statement of John von Neumann before the Special Senate Committee on Atomic Energy, Collected Works, vol. VI, pp. 499–502. He testified on 31 January 1946 concerning the pending legislation which created the Atomic Energy Commission.

[20] From Salomon Bochner, "John von Neumann," National Academy of Sciences, *Biographical Memoirs*, vol. 32 (1958), p. 447.

1956 Medal of Freedom (Presidential Award); Albert Einstein Commemorative Award; Enrico Fermi Award.
1957 Died, Washington, D.C., February 8.

Academy memberships:

Academia Nacional de Ciencias Exactas, Lima, Peru.
Accademia Nazionale dei Lincei, Rome, Italy.
American Academy of Arts and Sciences.
American Philosophical Society.
Istituto Lombardo di Science e Lettere, Milan, Italy.
National Academy of Sciences.
Royal Netherlands Academy of Sciences and Letters, Amsterdam, Netherlands.

How did von Neumann become involved in computers and computing? The answers to this question lie in his career as briefly outlined in the last section and in a facet of his make-up we have not yet mentioned. Along with all his other attributes, he had an almost insatiable interest in new ideas—sometimes! At other times—when, for instance, he was deeply committed to some intellectual pursuit—he was completely impervious to new ideas. On these occasions he would hardly bother to listen to the speaker. But in general he was extremely receptive to new intellectual challenges, and he always seemed to display a high degree of mental restlessness when he was between ideas. It seemed as if he was almost always on the lookout for new fields to conquer and he probably was. Naturally, those which appealed to him most were those that involved his great interest in applied mathematics.

Thus we are not surprised to see him moving into hydrodynamics. This subject had several aspects which certainly must have weighed strongly with him: it was and to some substantial extent still is rife with difficulties of a very great mathematical sort; a definitive advance in this field would have decisive implications in both mathematics and theoretical physics; and finally hydrodynamics was and is enormously useful to the government. All these reasons were important to von Neumann, and each must have contributed to his decision to move into this field.

Perhaps it is worth saying a few words on each of these three reasons. In the middle 1940s von Neumann and I were writing as follows:

> Our present analytical methods seem unsuitable for the solution of the important problems arising in connection with non-linear partial differential equations and, in fact, with virtually all types

of non-linear problems of pure mathematics. The truth of this statement is particularly striking in the field of fluid dynamics. Only the most elementary problems have been solved analytically in this field. Furthermore, it seems that in almost all cases where limited successes were obtained with analytical methods, these were purely fortuitous, and not due to any intrinsic suitability of the method to the milieu.[21]

An elaboration of the second reason occurs a little later in the same paper: ". . . that we are up against an important conceptual difficulty is clear from the fact that, although the main mathematical difficulties in fluid dynamics have been known since the time of Riemann and Reynolds, and although as brilliant a mathematical physicist as Rayleigh has spent the major part of his life's effort in combating them, yet no decisive progress has been made against them. . . ."[22]

To document the third reason it will perhaps suffice to quote Ulam: "Often he mentioned that personally he found doing scientific work there (Europe) almost impossible because of the atmosphere of political tension. After the war he undertook trips abroad only unwillingly."[23]

Von Neumann's knowledge of hydrodynamics was to be of inestimable value to the Los Alamos group which he joined as a consultant late in 1943. One of his earliest and most important contributions there was his work on implosions. To understand this it is perhaps desirable to say a little on what this is all about.

The big problem facing the physicists at Los Alamos was how to produce an extremely fast reaction in a small amount of the uranium isotope, U^{235}, or of plutonium so that a great amount of energy would be explosively released. This was quite another thing from the relatively slow build-up of neutrons in an atomic pile or reactor, this last having just been achieved on 2 December 1942. Many first-rate physicists were therefore proposing and examining various alternate schemes for achieving such fast reactions.

In principle, the idea was to start with material in noncritical form and somehow to transform it with extreme rapidity into a critical state. The need for speed was to prevent a very small explosion from occurring prematurely and blowing up the bomb. One scheme was to make two hemispheres of nuclear material,

[21] Goldstine and von Neumann, "On the Principles of Large-Scale Computing Machines," in von Neumann, Collected Works, vol. v, p. 2.

[22] *Ibid.*, pp. 2–3.

[23] Ulam, *op. cit.*, pp. 6–7.

each of which was subcritical but which when combined into a sphere would be critical. The two pieces were then assembled close together and at the requisite instant pushed together by means of an explosion of some conventional high explosive.

A much more sophisticated technique was to assemble a somewhat subcritical sphere of material and by conventional explosives around the sphere to compress it very rapidly with such great force that it became critical. It was this technique that was being experimented on by Seth Neddermeyer. Von Neumann in collaboration with Neddermeyer, Edward Teller, and James L. Tuck worked on this in detail. One of the big problems was the need to achieve a spherical shock wave that would push simultaneously on all points of the nuclear mass. If simultaneity were not achieved, the nuclear material would be extruded from low-pressure areas with a resultant loss of energy in the explosion. Tuck and von Neumann invented an ingenious type of high explosive lens that could be used to make a spherical wave. The idea was successful.

This was no small achievement but undoubtedly von Neumann's main contribution to the Los Alamos project was to lie in his showing the theoretical people there how to model their phenomena mathematically and then to solve the resulting equations numerically. A punched card laboratory was set up to handle the implosion problem, and this later grew into one of the world's most advanced and largest installations.

Von Neumann had an uncanny ability to solve very complex calculations in his head. This was a source of wonderment to mathematicians and physicists alike. It is possible to illustrate this quality of his by an amusing anecdote: One time an excellent mathematician stopped into my office to discuss a problem that had been causing him concern. After a rather lengthy and unfruitful discussion, he said he would take home a desk calculator and work out a few special cases that evening. Each case could be resolved by the numerical evaluation of a formula. The next day he arrived at the office looking very tired and haggard. On being asked why he triumphantly stated he had worked out five special cases of increasing complexity in the course of a night of work; he had finished at 4:30 in the morning.

Later that morning von Neumann unexpectedly came in on a consulting trip and asked how things were going. Whereupon I brought in my colleague to discuss the problem with von Neumann. We considered various possibilities but still had not met with success. Then von Neumann said, "Let's work out a few special cases." We agreed, carefully not telling him of the numerical work in the early morning hours. He then put his eyes to the ceiling and in perhaps five minutes worked out in his head four of the previously and laboriously calculated cases! After he had worked about a minute on the fifth and hardest case, my colleague suddenly announced out loud the final answer. Von Neumann was completely perturbed and quickly went back, and at an increased tempo, to his mental calculations. After perhaps another minute he said, "Yes, that is correct." Then my colleague fled, and von Neumann spent perhaps another half hour of considerable mental effort trying to understand how anyone could have found a better way to handle the problem. Finally, he was let in on the true situation and recovered his aplomb.

In any case, von Neumann had a profound interest and capability in numerical calculations, but his work in hydrodynamics would have been impossible without computers and computing. It is of course fortuitous that he linked Aberdeen and Los Alamos. It is precisely to this fortuity that we all owe so much.

Sometime in the summer of 1944 after I was out of the hospital I was waiting for a train to Philadelphia on the railroad platform in Aberdeen when along came von Neumann. Prior to that time I had never met this great mathematician, but I knew much about him of course and had heard him lecture on several occasions. It was therefore with considerable temerity that I approached this world-famous figure, introduced myself, and started talking. Fortunately for me von Neumann was a warm, friendly person who did his best to make people feel relaxed in his presence. The conversation soon turned to my work. When it became clear to von Neumann that I was concerned with the development of an electronic computer capable of 333 multiplications per second, the whole atmosphere of our conversation changed from one of relaxed good humor to one more like the oral examination for the doctor's degree in mathematics.

Soon thereafter the two of us went to Philadelphia so that von Neumann could see the ENIAC. At this period the two accumulator tests were well underway. I recall with amusement Eckert's reaction to the impending visit. He said that he could tell whether von Neumann was really a genius by his first question. If this was about the logical structure of the machine, he would believe in von Neumann, otherwise not. Of course, this *was* von Neumann's first query.

On 2 November 1944, my wife and I returned to Philadelphia and again took up residence there. This point in time formed the beginning of a long and very fruitful friendship and working relationship between ourselves and the von Neumanns that was to terminate only upon his untimely death.

Chapter 7 Beyond the ENIAC

To describe what happened next it is convenient to refer back to Babbage's Analytical Engine, to Stibitz's relay computers, to the Harvard-IBM computer, and to Post's and Turing's paper constructs. We have seen (above, p. 21) that Babbage's machine was conceived as being instructed in its tasks by a set of so-called operation cards strung together to describe the series of operations to be performed. Similarly, the other machines just mentioned all had paper tapes into which were punched holes that were a numerical code for the instructions to be effected. As early as November of 1943 Stibitz was describing how the orders were handled in a very small machine called a Relay Interpolator (RI), used for doing cubical interpolation, as follows: "To make the RI carry out a required computation, that computation is broken down into a succession of orders to the machine to memorize, read or write numbers, add, and so on. These orders are placed on a control tape with the aid of a device similar to the ordinary typewriter."[1]

Thus by 1944 there was general understanding among the group at the Moore School that the orders for a digital machine could be stored in numerical form on tape. This point is very important since it bears crucially on what happened next. My correspondence for the period August–October 1944 clearly indicates that as the two accumulator set-up came into operation the Moore School staff and I began to feel quite confident of our ability to construct the entire ENIAC as a reliably operating system without too many delays. In fact on 11 August 1944 I was optimistically writing that the ENIAC was to be ready about 1 January 1945, and that a room 20 feet by 40 feet should be provided.[2]

At the same time a general reassessment of the ENIAC's operational characteristics was being undertaken at the Moore School. This revealed a number of serious problems and deficiencies in the machine as an instrument for general-purpose scientific calculation. In particular I (and probably Cunningham as well) was

[1] G. R. Stibitz, Applied Mathematics Panel, NDRC, "A Statement Concerning the Future Availability of a New Computing Device," 12 November 1943.

[2] Memorandum, Goldstine to Simon, 11 August 1944.

bothered by the clumsiness of the mechanisms for programming the ENIAC and by the small number—20—of electronically alterable storage registers. We already had visualized doing much more elaborate calculating than solving ballistic equations.

Thus on 11 August 1944 I was writing Col. Simon in this sense:

> 2. Due however to the necessity for providing the ENIAC in a year and a half it has been necessary to accept certain make-shift solutions of design problems, notably in the means of establishing connections between units to carry out given procedures and in the paucity of high speed storage devices. These defects will result in considerable inconvenience and loss of time to the Laboratory in setting up new computing problems.
>
> 3. It is believed highly desirable that a new RAD contract be entered into with the Moore School to permit that institution to continue research and development with the object of building ultimately a new ENIAC of improved design. . . .[3]

This memorandum makes very clear that conversations were going on at the Moore School looking toward an amelioration of the operational deficiencies of the ENIAC. It is evident that by mid-August these conversations had progressed to the point where new technological ideas had emerged. In fact there is an undated report by Eckert and Mauchly, written that summer prior to 31 August, when it was sent by Brainerd to me,[4] describing a device, called a delay line, for increasing the storage capacity of the machine. This device, discussed at length below, was to be crucial to the next phase of development.

The 11 August memorandum was followed by extensive conversations at the Ballistic Research Laboratory, which resulted in a meeting on 29 August 1944 of a committee known as the Firing Table Reviewing Board and a recommendation to Simon backing my request.[5] The minutes of that meeting show that von Neumann was present. This makes it quite clear to me that von Neumann had already visited the Moore School. My records indicate that I was back on duty after my illness around 24 July 1944 and that I probably took von Neumann for a first visit to the ENIAC on or about 7 August. I recall that von Neumann's first visit was during the two-accumulator test. My travel orders show that my first business

[3] Memorandum, Goldstine to Simon, Further Research and Development on ENIAC, 11 August 1944.

[4] Letter, Brainerd to Goldstine, 31 August 1944.

[5] Memorandum, C. B. Morrey to L. E. Simon, 30 August 1944.

visit to Philadelphia after being released from the University of Pennsylvania Hospital was in the first week of August 1944. By the end of August I was urging development of a new electronic computing device that would:

a. Contain many fewer tubes than the present machine and hence be cheaper and more practical to maintain.
b. Be capable of handling many types of problems not easily adaptable to the present ENIAC.
c. Be capable of storing cheaply and at high speeds large quantities of numerical data.
d. Be of such a character that the setting up on it of a new problem will require very little time.
e. Be smaller in size than the present ENIAC.[6]

All this material makes clear that the thinking at the Moore School had advanced quite far by the end of August 1944. However, no real effort had gone into understanding how to store instructions or what they should be. The idea at that time was somehow to do this along the lines of the Stibitz machine, but nothing was formulated or thought out. Then Eckert came up with the idea that the delay line could be used for storage of information. This is in itself most important. It was fortunate that just as this idea emerged von Neumann should have appeared on the scene.

It was he who took the raw idea and perfected it. Starting in August 1944 von Neumann came regularly to the Moore School for meetings with Burks, Eckert, Adele Goldstine, Mauchly, and myself. Eckert was delighted that von Neumann was so keenly interested in the logical problems surrounding the new idea, and these meetings were scenes of greatest intellectual activity. Out of them arose very specific ideas on the types of mathematical problems needing solution and on the logical design of a new machine to handle them as well as on its engineering design.

Brainerd wrote in September:

The progress of work on the ENIAC has led to some rather extensive discussions concerning the solution of problems of a type for which the ENIAC was not designed. In particular, these discussions have been carried out with Dr. von Neumann. . . . Dr. von Neumann is particularly interested in mathematical analyses which are the logical accompaniment of the experimental work which will be carried out in the supersonic wind tunnels. . . .

[6] Draft memorandum, never sent, August 1944. This was my draft of the memorandum sent 30 August by Morrey to Simon.

> It is not feasible to increase the storage capacity of the ENIAC . . . to the extent necessary for handling non-linear partial differential equations on a practical basis. The problem requires an entirely new approach. At the present time we know of two principles which might be used as a basis. One is the possible use of iconoscope tubes, concerning which Dr. von Neumann has talked to Dr. Zworykin of the R.C.A. Research Laboratories, and another of which is the use of storage in a delay line, with which we have some experience. Such a line could store a large number of characters in a relatively small space, and would . . . enable a machine of moderate size to be constructed for the solution of partial differential equations which now block progress in certain fields of research at the BRL.[7]

The details of the new machine—which was to be called EDVAC—will be discussed later. Let us now return to von Neumann's collaboration with the Moore School group. The discussions and meetings are summarized in a first report on the new machine in March of 1945 as follows:

> The problems of logical control have been analyzed by means of informal discussions among Dr. John von Neumann, . . . , Dr. Mauchly, Mr. Eckert, Dr. Burks, Capt. Goldstine and others. . . . Points which have been considered during these discussions are flexibility of the use of the EDVAC, storage capacity, computing speed, sorting speed, the coding of problems, and circuit design. These items have received particular attention; . . . Dr. von Neumann plans to submit within the next few weeks a summary of these analyses of the logical control of the EDVAC together with examples showing how certain problems can be set up.[8]

In July Report No. 2 states that "discussions have been held at regular intervals in the Moore School to develop a logical plan for the EDVAC. These discussions continue. Dr. John von Neumann . . .

[7] Letter, Brainerd to Gillon, 13 September 1944. This letter also contained cost and time estimates for the research and development work involved. The amount proposed was $105,600; the time was to start 1 January 1945 and to go on for one year with work starting "on a small scale on October 1, 1944." On 18 September 1944 Gillon, in the Office of the Chief of Ordnance, issued an RAD Order to the Philadelphia Ordnance District for "Research and Experimental work in connection with the Development of an Electronic Discrete Variable Calculator." The contract was to be issued for the nine months beginning 1 January 1945, and the dollar amount was as indicated above.

[8] J. P. Eckert, Jr., J. W. Mauchly, and S. R. Warren, Jr., PY Summary Report No. 1, 31 March 1945. PX was the Moore School accounting symbol for the ENIAC charges and PY for the EDVAC.

has prepared a preliminary draft in which he has organized the subject matter of these discussions. This material has been mimeographed and bound. It consists of a 101-page report entitled *First Draft of a Report on the* EDVAC by John von Neumann."[9]

This work on the logical plan for the new machine was exactly to von Neumann's liking and precisely where his previous work on formal logics came to play a decisive role. Prior to his appearance on the scene, the group at the Moore School concentrated primarily on the *technological* problems, which were very great; after his arrival he took over leadership on the *logical* problems. Thus the group tended to split into the technologists—Eckert and Mauchly—and the logicians—von Neumann, Burks, and I. This was a perfectly natural division of labor, but the polarization was to become increasingly severe as time went on and was finally to disrupt the group. At the same time, tensions were also developing between the university's and Moore School's administration on the one hand and Eckert and Mauchly on the other. All these conflicts play a role in the story and will be discussed later.

Before describing von Neumann's contributions to the EDVAC—Electronic Discrete Variable Calculator—we need to explain what a delay line is, and how it is used in an electronic computer.

These devices can be built in a number of ways, but the type relevant here is the so-called ultrasonic delay line. As a general class they were developed so that electrical signals could be delayed by given predetermined amounts of time. The ultrasonic ones are very useful for achieving long delays, i.e. of several milliseconds.

These ultrasonic devices operate by transforming the electrical signal to be delayed into an ultrasonic signal in some fluid and then transforming it back again to an electrical one. The delay comes in from the fact that transmission of signals through fluids is very slow as compared to that of electricity through a wire. The speed of such a signal in mercury, for example, is 1,450 meters per second, whereas the speed in a wire of an electrical signal is comparable to that of light, 3×10^8 meters per second. Thus, by suitably fixing the length of the container of fluid, a preassigned time delay can be achieved.

The first such device was built at the Bell Telephone Laboratories by William B. Shockley, one of the co-inventors of the transistor. It used a mixture of water and ethylene glycol for the fluid. The second was built by Eckert and his associates at the Moore School

[9] Eckert, Mauchly, Warren, PY Summary Report No. 2, 10 July 1945.

for the Radiation Laboratory at MIT and its properties studied during the summer of 1943.[10] Then a group was set up at Radiation Laboratory to exploit these devices for radar applications.

As we have said, these delay lines require among other things that an electrical signal be transformed into a sonic one and conversely. This may sound quite bizarre but is a very usual thing. In fact, for very low frequencies—less than a 100 kiloherz per second—loudspeakers are examples of the first transformation and microphones of the latter. For the present application, though, extremely high frequencies are involved, and such devices are not satisfactory. Instead, the so-called piezoelectric effect is utilized.

This property—the word piezo derives from the Greek word *piezein*, to press—was discovered in 1880 by Pierre and Jacques Curie. They showed that a mechanical change in shape of a suitably cut quartz crystal would result in an electric charge being formed in the crystal, and, conversely, that an electrical charge being imposed on such a crystal would result in a mechanical change in the crystal. Such crystals can then be used to transform electrical signals into mechanical ones and vice versa. If these signals are superimposed on a suitably determined carrier, the crystals can vibrate at very high frequencies—millions of cycles per second. The way these crystals were used was to attach them to either end of a tube filled with mercury. When an electrical pulse was impressed on the crystal at the initial end of such a tube, the crystal vibrated and thereby sent a sonic wave down the mercury at a speed of 1,450 meters per second. Upon arrival at the other end, the sonic disturbance compressed the terminal crystal which then emitted an electrical disturbance, mirroring the input signal but delayed by $d/1{,}450$ seconds where d is the length in meters of the tube. Thus a tube 1.450 meters (= 4 feet 9 inches) long will give a delay of 1 millisecond.

How can such a device be used for storing information? Visualize a delay line as in Fig. 13. Further suppose the input and output

Fig. 13

[10] A. G. Emslie, H. B. Huntington, H. Shapiro, and A. E. Benfield, "Ultrasonic Delay Lines II," *Journal of the Franklin Institute*, vol. 245 (1948), pp. 101–115; see also pp. 1–23.

wires are joined together. Then in principle any pattern of sonic disturbances in the mercury will reappear periodically. This follows since upon reaching the output crystal the sonic pattern will become transformed into the corresponding electrical pattern and will then reappear at the input crystal where it will be transformed back into the original sonic pattern. The period of the phenomenon will of course be determined by the length of the column of mercury, as we saw earlier.

However, the idealization we have just made is unrealistic since friction both in the wires and in the mercury will stop the device from ever operating. To remedy this loss of energy a black box containing the order of ten vacuum tubes is needed to retrieve and reshape the pattern, as indicated in Fig. 14. To quantise what we have

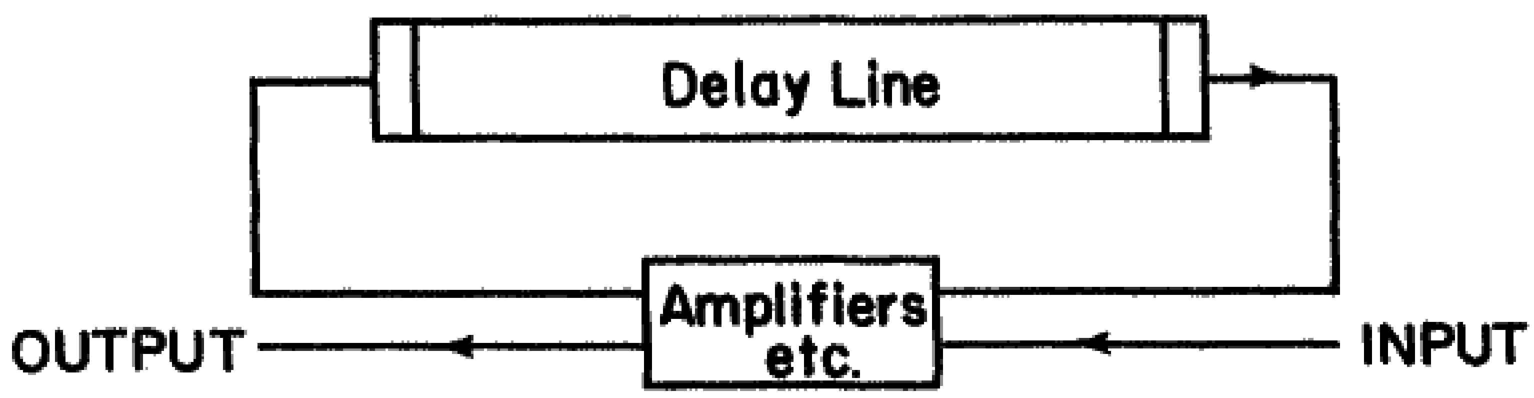

Fig. 14

described, let us suppose the binary digit 1 is represented by a pulse 0.5 microseconds wide and a 0 by no pulse. A tank 1.45 meters long can contain a pattern of 1,000 binary digits and hold it as long as the power stays on.

Let us understand the significance of this compared to what was done in the ENIAC. There a binary digit could be stored using a flip-flop, i.e. at the cost of a pair of vacuum tubes; however, since such a pair came in one glass envelope, let us say one binary digit could be stored at the cost of one vacuum tube. In the case of the delay line 1,000 binary digits could be stored at a cost of 10 vacuum tubes or less. Thus the cost of storing a binary digit dropped from 1 to 1/100 vacuum tube under the new technology. Moreover by means of suitable vacuum tube circuits called gates the recirculation can be interrupted to permit information to be read out or new information to be fed in as desired. Therefore, a pattern may be changed at will. This, then, was Eckert's great new technological invention.

In the ENIAC it was possible to store only 20 words because of the clumsy way in which binary digits—or bits, to use John Tukey's word—were stored, i.e. in vacuum tubes. For the proposed new machine using the mercury delay line it was decided tentatively to increase the storage capacity to about 2,000 words, i.e. by a factor of a hundred, thereby taking advantage of the hundred-fold drop in the cost of storage.

It is somewhat irrelevant but amusing to recount at this point a story about Alan Turing, the logician we mentioned earlier. In late 1946 or early 1947 Turing visited von Neumann and me for several weeks, during which time he argued that the mercury delay line used as a memory could not work. His argument was based on various signal-to-noise ratio considerations and seemed most convincing. In fact, he persuaded us, but fortunately experiment and experience proved him wrong.

We are now in a position to give a preliminary account of von Neumann's contributions to the EDVAC. They were contained in the 101-page draft of a report mentioned earlier which was issued by the Moore School on 30 June 1945.[11] This report represents a masterful analysis and synthesis by him of all the thinking that had gone into the EDVAC from the fall of 1944 through the spring of 1945. Not everything in there is his, but the crucial parts are. He stated that he wrote the report "in order to further the development of the art of building high speed computers and scientific as well as engineering thinking on this subject as widely and as early as feasible." [12] In a sense, the report is the most important document ever written on computing and computers.

Eckert and Mauchly in writing a progress report on the EDVAC had this to say: "During the latter part of 1944, and continuing to the present time, Dr. John von Neumann, consultant to the Ballistic Research Laboratory, has fortunately been available. . . . He has contributed to many discussions on the logical controls of the EDVAC, has proposed certain instruction codes, and has tested these proposed systems by writing out the coded instructions for specific problems. . . . In his report, the physical structures and devices . . . are replaced by idealized elements to avoid raising engineering problems which might distract attention from the logical considerations under discussion." [13]

Von Neumann was the first person, as far as I am concerned, who understood explicitly that a computer essentially performed logi-

[11] J. von Neumann, *First Draft of a Report on the* EDVAC (Philadelphia, 1945).
[12] Sworn deposition, von Neumann, notarized 8 May 1947.
[13] Eckert and Mauchly, "Automatic High-Speed Computing, A Progress Report on the EDVAC," 30 September 1945.

cal functions, and that the electrical aspects were ancillary. He not only understood this was so but he also made a precise and detailed study of the functions and mutual interactions of the various parts of a computer. Today this sounds so trite as to be almost unworthy of mention. Yet in 1944 it was a major advance in thinking. In a letter to von Neumann I said: "All . . . have been carefully reading your report with the greatest interest and I feel that it is of the greatest possible value since it gives a complete logical framework for the machine." [14]

There was an imperative need precisely at this time for a complete analysis of how a computer operates when viewed as a logical mechanism. This need developed from two new factors: the speed of the electronic device and the invention of the delay line as a storage device.

In electromechanical devices such as the Harvard-IBM machine there were 72 storage registers for numerical information, and the programs were stored in paper tapes. The speed of operation of the tapes was more or less conformed to that of the counter wheels making up the registers. There was therefore no unbalance between the numerical storage organs and those for instructional storage. In the case of an electronic machine the instructions had to be handled at the same electronic speed as the numbers or else there would have been a total unbalance. Thus a rethinking of the storage requirements both for numbers and instructions was essential. This, among other things, is what von Neumann accomplished.

Prior to von Neumann people certainly knew that circuits had to be built to effect the various arithmetic and control functions, but they concentrated primarily on the electrical engineering aspects. These aspects were of course of vital importance, but it was von Neumann who first gave a logical treatment to the subject, much as if it were a conventional branch of logics or mathematics. His account shows how he impressed his stamp on the field:

> *1.1* The considerations which follow deal with the structure of a *very high speed automatic digital computing system,* and in particular with its logical control. . . .
>
> *1.2* An *automatic computing system* is a (usually highly composite) device which can carry out instructions to perform calculations of a considerable order of complexity—e.g., to solve a non-

[14] Letter, Goldstine to von Neumann, 15 May 1945.

linear partial differential equation in 2 or 3 independent variables numerically.

The instructions which govern this operation must be given to the device in absolutely exhaustive detail. They include all numerical information which is required to solve the problem. . . . These instructions must be given in some form which the device can sense: punched into a system of punch cards or on teletype tape, magnetically impressed on steel tape or wire, photographically impressed on motion picture film, wired into one or more fixed or exchangeable plugboards. . . . All these procedures require the use of some code, to express the logical and the algebraical definition of the problem under consideration. . . .

Once these instructions are given to the device, it must be able to carry them out completely and without any need for further intelligent human intervention. At the end of the required operations the device must record the results again in one of the forms referred to above. The results are numerical data. . . .[15]

He then went on to an enumeration of the organs of the machine:

2.2 First: Since the device is primarily a computer, it will have to perform the elementary operations of arithmetics most frequently. These are addition, subtraction, multiplication and division: +, −, ×, ÷. It is therefore reasonable that it should contain specialized organs for just these operations.

It must be observed, however, that while this principle as such is probably sound, the specific way in which it is realized requires close scrutiny. . . . At any rate a *central arithmetical* part of the device will probably have to exist and this constitutes *the first specific part: CA.*

2.3 Second: The logical control of the device, that is, the proper sequencing of its operations, can be most efficiently carried out by a central control organ. If the device is to be *elastic,* that is, as nearly as possible *all purpose,* then a distinction must be made between the specific instructions given for and defining a particular problem, and the general control organs which see to it that these instructions—no matter what they are—are carried out. The former must be stored in some way—in existing devices this is done as indicated in 1.2—the latter are represented by definite operating parts of the device. By the *central control*

[15] Von Neumann, *First Draft,* pp. 1–2.

we mean this latter function only, and the organs which perform it form *the second specific part: CC.*

2.4 Third: Any device which is to carry out long and complicated sequences of operations (specifically of calculations) must have a considerable memory. . . .

(b) The instructions which govern a complicated problem may constitute considerable material, particularly so, if the code is circumstantial (which it is in most arrangements). This material must be remembered. . . .

At any rate, the total *memory* constitutes *the third specific part of the device:* M.

2.6 The three specific parts CA, CC (together C), and M correspond to the *associative* neurons in the human nervous system. It remains to discuss the equivalents of the *sensory* or *afferent* and the *motor* or *efferent* neurons. These are the *input* and *output* organs of the device. . . .

The device must be endowed with the ability to maintain the input and output (sensory and motor) contact with some specific medium of this type (cf. 1.2): That medium will be called the *outside recording medium of the device:* R. . . .

2.7 Fourth: The device must have organs to transfer . . . information from R into its specific parts C and M. These organs form its *input,* the *fourth specific part: I.* It will be seen, that it is best to make all transfers from R (by I) into M and never directly into C. . . .

2.8 Fifth: The device must have organs to transfer . . . from its specific parts C and M into R. These organs form its *output, the fifth specific part: O.* It will be seen that it is again best to make all transfers from M (by O) into R, and never directly from C. . . .[16]

We can see from the little quoted above how von Neumann gave a logically complete analysis of the structure of the EDVAC in his report. This was his first major contribution. We need not argue here whether others could or would have done this without him. The fact is that he did it. All his training in formal logics fitted him to do it, and it unequivocally bears the mark of his genius.

There has been much, quite bitter controversy about the attribution of credits for the various ideas connected with the EDVAC. It is therefore proper at this point to set out the facts as seen at the time by me.

[16] *First Draft,* pp. 3–7.

The discussions on the logical structure of this machine were very extensive and served to focus sharp attention on a myriad of problems. The whole matter was then synthesized by von Neumann in his *First Draft*. Eckert and Mauchly writing at the time said: "Those who have contributed in this way are Dr. Arthur Burks, of the Moore School Staff, Capt. H. H. Goldstine, Army Ordnance, and especially Dr. John von Neumann, consultant to the Ballistic Research Laboratory." [17] Actually, many engineering matters were also threshed out in these meetings, as we shall see.

To illustrate how the discussions went—even when von Neumann was away at Los Alamos he participated by mail—let me quote from a letter from von Neumann to me:

> Here are some further small items on which I wanted to write to you, namely:
>
> I want to add something to our discussion of feeding data into the machine, and getting results out. As you may recall, in feeding in, two types of numbers occurred: Binary integers x, y which denote positions in the memory: binaries ξ. . . . In printing only the ξ occur.
>
> Clearly the ξ ought to be typed (by a human operator) and absorbed by the machine as decimal numbers, and also printed as such. So we need here decimal → binary and binary → decimal conversion facilities.
>
> As to the x, y we had doubts. Since they have logical control functions, it is somewhat awkward to have to convert them. I argued for having x, y always in binary form, but we finally agreed that binaries are hard for handling and remembering by humans.
>
> . . .
>
> I think that we overlooked an obvious solution: That is, to handle x, y outside the machine in the octal (base 8) system. . . .
>
> Does my suggestion seem reasonable to you and to the others?
>
> There exist two recent (1944) midget pentodes which may be of interest to us: 6AK5 and 6AS6. . . .
>
> Both have sharp cutoff on the control grids. 6AK5 has inner connection between suppressor and cathode, 6AS6 however brings the suppressor out separately and has a sharp cutoff on the suppressor too: −15v for +150v on the screen. . . .
>
> I am continuing working on the control scheme for the EDVAC and will definitely have a complete writeup when I return. I am

[17] Eckert and Mauchly, "Automatic High-Speed Computing . . . ," Acknowledgements.

also working on the problem of formulating a two-dimensional, non-stationary hydrodynamical problem for the ENIAC. . . .[18]

To indicate further how things were done at this juncture here are excerpts from my reply:

We are enclosing some further tube characteristics in which you will be interested. As you will notice, one . . . is the 6AS6 which you mentioned. . . . We have also ordered the 6AK5. . . .

We all agree that the use of the octal system for handling the pairs x, y . . . is a very sound solution. . . .[19]

Or again:

The contents of this letter belong, of course, into the manuscript, and I will continue the manuscript and incorporate these things also after I get it back from you—if possible with comments on both items from you. . . .[20]

These few illustrations should serve to show the true situation: all members of the discussion group shared their ideas with each other without restraint and therefore all deserve credit. Eckert and Mauchly unquestionably led on the technological side and von Neumann on the logical. It has been said by some that von Neumann did not give credits in his *First Draft* to others. The reason for this was that the document was intended by von Neumann as a working paper for use in clarifying and coordinating the thinking of the group and was not intended as a publication. (In fact, on 25 June 1945 copies were distributed to 24 persons closely connected to the project.) Its importance was so clear however that later as its fame grew many outsiders requested copies from the Moore School or me. Through no fault of von Neumann's the draft was never revised into what he would have considered a report for publication. Indeed, not until several years later did he know that it had been widely distributed.

Hopefully having laid that ghost to rest, we return to von Neumann's contributions. First, his entire summary as a unit constitutes a major contribution and had a profound impact not only on the EDVAC but also served as a model for virtually all future studies of logical design. Second, in that report he introduced a logical notation adapted from one of McCulloch and Pitts, who used it in a

[18] Letter, von Neumann to Goldstine, 12 February 1944. The dating by von Neumann is erroneous; it should be 1945. This is evident both from the substance of the letter and from the date of the reply.

[19] Letter, Goldstine to von Neumann, 24 February 1945.

[20] Letter, von Neumann to Goldstine, 8 May 1945.

study of the nervous system.[21] This notation became widely used, and is still, in modified form, an important and indeed essential way for describing pictorially how computer circuits behave from a logical point of view.[22]

Third, in the famous report he proposed a repertoire of instructions for the EDVAC, and in a subsequent letter he worked out a detailed programming for a *sort and merge* routine. This represents a milestone, since it is the first elucidation of the now famous stored program concept together with a completely worked-out illustration.

Fourth, he set forth clearly the serial mode of operation of the modern computer, i.e. one instruction at a time is inspected and then executed. This is in sharp distinction to the parallel operation of the ENIAC in which many things are simultaneously being performed.

Fifth, he gave clear indications that some modification of the so-called iconoscope or television camera tube would be a valuable memory device. This is a foreshadowing of the project at the Institute for Advanced Study which was to be the prototype for the computers of yesterday, today, and perhaps tomorrow.

These are the things von Neumann did. They have left their mark on the whole computing world. Thus, for example, the designers of the EDSAC, the first machine in the world to use the stored program, say: "Several machines working on the same principles as the EDSAC are now in operation in the United States and in England. These principles derive from a report drafted by J. von Neumann in 1946 [*sic*]. . . . It is found that machines designed along the lines laid down in this report are much smaller and simpler than the ENIAC and at the same time more powerful. The methods by which programs are prepared for all these machines are, as might be expected, similar, although the details vary according to the different codes used. Anyone familiar with the use of one machine will have no difficulty in adapting himself to another." [23]

To recapitulate: It is obvious that von Neumann, by writing his report, crystallized thinking in the field of computers as no other

[21] W. S. McCulloch and W. Pitts, "A Logical Calculus of the Ideas Immanent in Nervous Activity," *Bull. Math. Biophysics*, vol. 5 (1943), pp. 115–133.

[22] See, e.g., Hartree, *Calculating Instruments and Machines* (Urbana, 1949), pp. 97–110.

[23] M. V. Wilkes, D. J. Wheeler, and S. Gill, *The Preparation of Programs for an Electronic Digital Computer* (Cambridge, Mass., 1951). This machine was the first electronic one to be put in operation after the ENIAC. It was built for the Cavendish Laboratory in Cambridge, England.

person ever did. He was, among all members of the group at the Moore School, *the* indispensable one. Everyone there was indispensable as regards some part of the project—Eckert, for example, was unique in his invention of the delay line as a memory device—but only von Neumann was essential to the entire task.

The thorny question of exact authorship of each idea will never be resolved satisfactorily because almost all ideas were generated in a joint mode. In fact von Neumann once summarized the situation in this regard very well by saying:

> There are certain items which are clearly one man's . . . the application of the acoustic tank to this problem was an idea we heard from Pres Eckert. There are other ideas where the situation was confused. So confused that the man who had originated the idea had himself talked out of it and changed his mind two or three times. Many times the man who had the idea first may not be the proponent of it. In these cases it would be practically impossible to settle its apostle.[24]

Let us now return to our temporal sequence and bring the story up to date. At the beginning of September 1944 the construction of virtually all parts of the ENIAC was well underway, and it was generally felt that the problem had decisively shifted from a developmental to a constructional phase. Indeed, this is why there was so much emphasis just at this moment on the EDVAC. The design engineers had little to do at this time on the ENIAC while those engaged in the actual construction were fully occupied.

It is interesting to see now how the shortcomings of the ENIAC were acknowledged at that time. In writing to Gillon, who was then in the Pacific Theatre of Operations, I said:

> To illustrate the improvements I wish to realize, let me say that to solve a quite complex partial differential equation of von Neumann's . . . the new Harvard IBM will require about 80 hours as against ½ hour on ENIAC of which about 28 minutes will be spent just in card cutting and 2 minutes for computing. The card cutting is needed simply because the solution of partial differential equations requires the temporary storage of large amounts of data. We hope to build a cheap high-speed device for this purpose. . . . The second major improvement . . . can again be illustrated on the Harvard machine. To evaluate seven terms

[24]Remarks from Minutes of Conference held at the Moore School of Electrical Engineering on 8 April 1947 to discuss Patent Matters.

> of a power series took 15 minutes on the Harvard device of which 3 minutes was set-up time, whereas it will take at least 15 minutes to set up ENIAC and about 1 second to do the computing. To remedy this disparity we propose a centralized programming device in which the program routine is stored in coded form in the same type storage devices suggested above. The other crucial advantage of central programming is that any routine, however complex, can be carried out whereas in the present ENIAC we are limited.[25]

This letter is interesting in that it shows how far developed thinking was at such an early stage. Recall that during the two prior months the two-accumulator test was the all-consuming interest of the group. Furthermore, at that time von Neumann had only just become acquainted with the project. This is an excellent example of the speed of his mind. In that same letter I said: "Now that we seem to be on the fairway as far as development goes, I feel it most important to make plans for further improvements to realize in a second machine the highly important features that seemed too difficult in the first model. Von Neumann, Eckert and I have formulated quite definite ideas along these lines. . . ."

Even during the latter part of August there was great progress on the ideas leading to the EDVAC. In an earlier letter to Gillon I wrote:

> Von Neumann is displaying great interest in the ENIAC and is conferring with me weekly on the use of the machine. He is working on the aerodynamical problems of blast. . . . As I now see the future course of the ENIAC, there are two further directions in which we should pursue our researches. After talking to S. B. Williams of Bell Telephone, I feel that the switches and controls of the ENIAC now arranged to be operated manually, can easily be positioned by mechanical relays and electromagnetic telephone switches which are instructed by a teletype tape. . . . In this manner tapes could be cut for many given problems and reused whenever needed. Thus we would not have to spend valuable minutes resetting switches when shifting from one phase of a problem to the next. The second direction to be pursued is of providing a more economical electronic device for storing data than the accumulator. Inasmuch as the accumulator is so powerful an instrument, it seems foolish to tie up such tools merely to hold numbers temporarily. Eckert has some excellent ideas on a very cheap device for this purpose.

[25] Letter, Goldstine to Gillon, 2 September 1944.

> The new torque amplifiers [this of course is a statement about the differential analyzer] are installed at Philadelphia on the integrators and also on the input tables. . . . The analyzer is operating . . . between three and four times the maximun speed ever before used with excellent results. . . .[26]

Notice that in the fortnight between the two letters the idea of the stored program seems to have evolved. Indeed, in the September letter the concept already appears in quite modern guise, whereas in the August one the author was trying to evolve an emendation of the ENIAC's decentralized controls to make it a little more useful.

All this must make clear that the fall of 1944 was perhaps the most eventful time in the intellectual history of the computer. All evidence from the correspondence of the period certainly bears this out. It also helps reinforce the comments in the previous section about the general impossibility and impracticability of sorting out authorship of ideas.

Before describing in detail the logical structure of the EDVAC and the stored program concept, the subject of the next section, we should perhaps mention a few other items.

Brainerd was so involved with administrative details in connection with the ENIAC that Prof. S. Reid Warren, Jr. was appointed to head the EDVAC project. Typical of the sorts of things happening at that time are three memoranda of the period. In April 1945 Brainerd assigned Burks to be in charge of writing the ENIAC reports and placed under him Adele Goldstine and Prof. Harry Huskey. They were charged with the tasks of producing an operating manual, a complete technical report, and a maintenance manual.[27] It is clear from the tone of Brainerd's memorandum that there were great pressures. It says: "In addition, Dr. Burks is requested to discuss with me the status of the report writing work at least once a week."

In May Brainerd wrote to a visitor from the British Scientific Office, John R. Womersley, apologizing for his delay in sending reports on the ENIAC.[28] Womersley's visit was interesting because it opened the door for modern computation to go abroad. As we shall see later, Womersley was the first in a long line of visitors; originally they came from England, then from France and Sweden.

[26] Letter, Goldstine to Gillon, 21 August 1944.

[27] Memorandum, Brainerd to Dr. Burks, Mr. Eckert, Mrs. Goldstine, Captain Goldstine, Dr. Huskey, 27 April 1945.

[28] Letter, Brainerd to Womersley, 9 May 1945.

Brainerd instituted new working rules in September to ensure that "work on the ENIAC itself [will] proceed from 8:30 in the morning to 12:30 the following morning, each day except part of Saturday and all day Sunday." [29]

It was during the spring of 1945 that the problem of where to house the ENIAC at Aberdeen began to come to the fore. In order to ensure an orderly transfer of the equipment, contract W-18-001-ORD 335 (816) was entered into in May of 1945 between the University of Pennsylvania and Aberdeen Proving Ground to move the ENIAC into the Ballistic Research Laboratory.

In the summer of 1944 I had written Simon calling his attention to the need for a room about 20 feet by 40 feet to house the machine. At first Simon proposed space in the then recently completed Wind Tunnel Building at Aberdeen. This space turned out to be inadequate, and Simon thereupon initiated a request to erect a suitable structure to house all the computing equipment of the Ballistic Research Laboratory: this included about 100 people, a set of IBM machines, the differential analyzer, the expected ENIAC and EDVAC as well as the two Bell Relay Computers.

By the spring of 1945 the Office of the Chief of Ordnance had agreed to the erection of a Computing Annex to the Ballistic Research Laboratory, and Dean Pender was requested to have his people prepare plans and specifications for locating the ENIAC in this new location. As will be seen later, this was done but much later than originally planned—not because of delays in the operability of the ENIAC but because it was not in the public interest to have the only operative electronic computer disassembled during a critical time in the life of the United States. This will be discussed in the proper place in this history.

At this same time the Moore School received the power supply transformers and chokes which had been built by an outside contractor for the ENIAC. These devices, which were essential for providing electrical power at the proper voltages, had been ordered some time before. When they arrived in the spring of 1945, all were found to be completely defective, and the entire lot was rejected.[30]

[29] Memorandum, Brainerd to Captain Goldstine, Messrs. Burks, Chu, Eckert, Sharpless, Shaw, 8 September 1945.

[30] Letter, Lt. Col. C. H. Greenall to Goldstine, 20 April 1945. This letter has as an enclosure a memorandum showing the details of the tests. An idea of the size of the equipment in question is indicated by the fact that it cost well over $10,000. Fortunately for the project, the Moore School had also ordered a duplicate set from a very small firm as a "back-up" or auxiliary set. This order was filled satisfactorily,

A number of other projects were being brought into the foreground by the impending completion of the ENIAC. For one thing there was, as seen by me, a real need for a permanent cadre of operating personnel for the machine. To this end I had John V. Holberton assigned to take charge of the operations of the ENIAC upon its completion: "Effective 1 June 1945 . . . [he] will report to Capt. H. H. Goldstine."[31] I also had two enlisted men, Cpl. Irwin Goldstein and Pfc. Homer Spence, both of whom had some technician's training in electronics, assigned to work with the ENIAC engineering group. It was the intention that they should become the service or maintenance men for the machine after it was turned over to the government. As it happened, Goldstein left, but Spence stayed on and later became an important member of the computing group at the Ballistic Research Laboratory.

To support Holberton on the programming side I assigned six of the best computers to learn how to program the ENIAC and report to Holberton. They were the Misses Kathleen McNulty, who subsequently married Mauchly; Frances Bilas, who married Spence; Elizabeth Jennings who married a Moore School engineer; Elizabeth Snyder, who married Holberton; Ruth Lichterman; and Marilyn Wescoff.

Also, a $45,000 contract was entered into between the Army and the Moore School providing a considerable amount of test equipment for the ENIAC and also ensuring the coordination of the IBM equipment with that machine.[32]

Thus the problems of housing and staffing the ENIAC upon its completion were handled. These preparations were quite satisfactory, and there was an adequate staff to maintain and operate the ENIAC both in Philadelphia and in Aberdeen after it was moved. This was particularly important since the end of the war had a very strong centrifugal effect on the staff.

One last problem was solved by establishing a committee at the Ballistic Research Laboratory under the chairmanship of L. S. Dederick, one of the associate directors, and including Haskell Curry, one of Hilbert's last Ph.D. students, and Derrick Henry Lehmer, a

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and the equipment was installed so that little real delay occurred. To give a feeling for the physical dimensions of the apparatus we might mention that the iron needed for the transformer laminations weighed over two tons. (See letter, Goldstine to V. W. Smith, 20 February 1945.)

[31] Memorandum, Lt. Col. J. M. Shackelford, Organization of the Computing Branch, 31 May 1945.

[32] Letter, T. E. Bogert to R. Kramer, 18 June 1945.

well-known computer and table compiler, who, with his wife Emma, pioneered in the use of computers for probing number theoretical problems. This committee took over the ENIAC from the Moore School and supervised its usage by the Ballistic Laboratory staff.