Biographies

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Eloge: Arthur Lee Samuel (1901-1990)

***Arthur Lee Samuel** had a rich, full, and diverse career that started in the electrical engineering era of vacuum tubes and radio and moved quite logically into the computing world, first of tubes and then of transistors, microchips, word processing, and artificial intelligence. The most significant of his many pioneering contributions to computing and artificial intelligence was the Samuel Checker Playing Program, accepted today as the world's first self-learning computer program. Samuel himself did not consider this to be his most important professional contribution, saying that it merely got the most publicity.*

At his death, on July 29, 1990, Samuel left, with his two daughters, an unpublished and unfortunately unfinished, but remarkably well written and detailed, autobiography titled A Boy from Emporia. *It is chiefly from this 180-page typescript that I have, with his daughters' permission, drawn this eloge which describes an energetic, jovial, but occasionally cantankerous and often skeptical scientist. I have tested some parts of what I have written with the persons named and included their comments where appropriate. In a few instances these disagree with Samuel's recollections, particularly in regard to some IBM organizational matters. This may be because his work was clearly more important to him, and thus better remembered, than the details of IBM's often changed administrative structure. The disagreements themselves illustrate, in a minor way, the historian's problem of distilling what actually happened out of conflicting recollections and records. In this case the differences are slight and in no way reflect discredit on Samuel or his contributions to computing.*

Otherwise unidentified quotations in the following material are from A Boy from Emporia.

Early life and education (1901-1928). Samuel was born on December 5, 1901, in Emporia, Kansas, the first child of a schoolteacher and a journeyman tailor turned furniture store proprietor. He could read, write, and figure before he started school and, having been forced out of his natural left-handedness, developed atrocious handwriting which he retained all his life. As a boy he experimented with photography, rigged telephone lines to friends' houses, and built a crystal radio receiver. At the age of 9, for reasons he could not later recall, he determined to become an electrical engineer.

In 1919 he entered the College of Emporia, which he described as "a small struggling Presbyterian school...which no longer exists." His college career, which he completed in three and a half years and into which he jammed all the mathematics, physics, and chemistry he could, was so successful that his professors wanted him to take up their specialties, while the owner of the drugstore, where he was a part-time clerk, wanted him to become a pharmacist. He remained firm in his selection of electrical engineering and furthermore decided to go to the Massachusetts Institute of Technology. Samuel recalls that MIT was then called Boston Tech (although the official history of the institute is that this name was no longer used after it relocated to Cambridge in 1916, but it was still called Tech for many years).

His admission to MIT's cooperative education course in 1923 resulted from the dramatic collision of a determined and well-organized student with a careless and disorganized college. When his letters of application to MIT received no reply, he packed a suitcase and went there, where he confronted Professor W.H. Timbie, "who seemed to be the man in charge." Timbie had filed his letters as "inactive" pending a personal interview and had never sent anyone to Emporia for that purpose. Samuel argued his way past the assertion that they were accepting no more students, pointing out that the problem was entirely due to MIT's failure to reply to his letters. Finally he was accepted and then went to Lynn, Mass., where he persuaded the General Electric plant manager to ignore his own "No Hiring" sign and take him on. On the same day he found a room for five dollars a week.

The 40 students in the EE cooperative education course were divided into two groups, one working at a GE plant while the other was at school at MIT. The groups traded places every three months. His work at GE followed the pattern of the GE Test Course in which new full-time engineering employees spent their first 18 months of employment in six different places where products were tested after manufacture and before delivery. This arrangement gave Samuel the opportunity to work with commercial electrical products; first with the electrical measuring instruments and meters made at the Lynn plant and later with the fractional horsepower induction motors made at West Lynn.

After these initial assignments, he was sent to Schenectady, N.Y., in 1924. He had asked to be assigned to the Research Laboratory but was instead put in the Vacuum Tube Laboratory, where he learned the then arcane crafts of making, evacuating, and testing tubes. He so impressed GE's Dr. Ernest E. Charleton that he was asked to return for his next two cooperative assignments to work on the development of a cesium-vapor-filled detector, the GE 200A. Charleton wanted Samuel to join GE after graduation, but instead he accepted an MIT instructorship at a starting annual salary of $1,000, the only co-op student hired by MIT that year.

Throughout his college career he was extremely short of funds and had to economize, cut corners, and work at a variety of part-time jobs to pay for his board and books. Samuel's autobiography is extremely detailed and interesting as to the nature and consequences of these economies and jobs. It shows why, throughout his life, he was always concerned with questions of personal finance. Even as an MIT instructor he had to supplement his income by working secretly as a bus boy in the company cafeteria of the Carr Fastener Co., then near MIT in Cambridge.

At MIT he, like all the new EE instructors, taught all of the undergraduate EE courses, one after another, while taking a few courses toward getting a PhD. He worked with Vannevar Bush on his differential analyzer — his first experience with this type of machine, which he said was the basis of his later interest in digital computers.

His pay raise after a year of teaching was not as big as he had anticipated, possibly because of a run-in he had with Professor Joseph W. Barker concerning a textbook's assumptions about AC motor design. Barker, a former Army major and a military-style disciplinarian, turned out to be the son-in-law of Professor Dugald C. Jackson, the head of the Electrical Engineering Department. In spite of this brief economic setback, Samuel still considered becoming a college professor, but he wanted more industrial experience at some company other than GE. He applied to the newly established Bell Telephone Laboratories (BTL) in New York City (at 463 West Street), where he started on September 1, 1928, at $42 a week. His assignment was to develop a long-life amplifier tube for use in the submerged repeaters for the projected transatlantic telephone cable.

Bell Telephone Laboratories (1928-1946). At the suggestion of his boss, Dr. J.B. Johnson, strongly seconded by the director of vacuum tube research and development and later president of BTL, Dr. Mervin J. Kelly, Samuel drafted a journal paper on his novel gas discharge tube sweep circuit for a CRT oscilloscope. Kelly's harsh criticism of his paper upset him because his writing had always been complimented at MIT and GE. Kelly suggested that he consult Dr. Karl K. Darrow, referred to in the official Bell Laboratories history[1] as a "brilliant master of exposition in mathematical physics."* In a single Socratic interview Darrow taught Samuel to write for his reader, rather than for the journal, and to say what he meant, simply and directly. Samuel credits Darrow's teaching with the development of his remarkably clear writing style. The paper, his first, was published in the September 1931 issue of the *Review of Scientific Instruments*: "A Method of Obtaining a Linear Time Axis for a Cathode Ray Oscillograph."[2]

Samuel was promoted to be head of his group, which he directed in the development of thyratrons. At about this time Kelly had a nervous breakdown, and for the three or four weeks of his absence ordered that Samuel should handle his correspondence. This gave Samuel a broad exposure to, and wide acquaintance throughout, the laboratory.

In the spring of 1930, by pure happenstance, he met a former Emporia college schoolmate, Bernice Crawford, on the street in New York — a meeting which led to their marriage a year later.

In the summer of 1930 his 15-man group at BTL was moved to the Western Electric tube factory on Hudson Street in New York City. In the following summer Kelly told him he was not a manager, relieved him of his group, and brought him back to West Street to do research on 10-centimeter microwave tubes. After working on, and abandoning, Barkhausen tubes, which turned out to be the forerunners of the Varian brothers' klystron, Samuel switched to other kinds of tubes, systematically going through all those represented in the then chaotic literature. This work led to several patentable inventions, and for the next 20 years he was granted several US patents almost every year, accumulating a lifetime total of 57. Toward the end of his life, when asked by one of his daughters what work he valued most, he mentioned his patents, although acknowledging that they were merely advances in the stream of knowledge and so no longer noteworthy.

When Kelly had his second, more serious, and longer lasting nervous breakdown, he again arranged for Samuel to handle his mail. At about this time Samuel developed some special tubes to generate the microwaves that Professor I.I. Rabi at Columbia University needed to verify some theoretical conjectures involving the magnetic moments of atoms and nuclei. Rabi acknowledged what Samuel called his "really quite minor contribution," but being thanked by name by Rabi, even then a scientific notable, improved his standing in the laboratories and in the scientific community. His standing was further improved later when Rabi received a Nobel prize for this work in 1944 and mentioned Samuel in his Nobel lecture.

Samuel continued to take courses at Columbia and finally completed all the course requirements for a PhD, but since he could not afford to leave his job for the required year of full-time residency, he never completed a dissertation.

Samuel wrote a dozen papers on microwave tubes, deliberately publishing in as many different technical journals as he could. He also accepted unpaid speaking engagements at local chapters of technical and engineering societies, engagements for which BTL paid his travel expenses.

In 1935 he conceived and patented a multicavity 10-centimeter magnetron which, while similar in some essential features, was inferior to the British-designed radar magnetron which he encountered later. Samuel remarked that this demonstrates "How it is possible to be within gunshot, as it were, of a really good idea and yet to quite miss the boat, if you will allow me to mix my metaphors."

By the late 1930s the development of the microwave klystron by the Varian brothers and Bill Hansen at Stanford University had become well known and Samuel tried to improve on it. In this he was helped by his colleague in the adjacent office, John R. Pierce, who later became director of research at BTL. Samuel claims credit for suggesting one of the pen names, J.J. Coupling, with which Pierce signed his science fiction. In the summer of 1939 Samuel went to the West Coast where he discovered that the Varian klystron had to be continuously pumped while his version was sealed off.

In early October 1940 the British Tizard Mission** to the US showed a sample of their cavity magnetron to some BTL

* Samuel noted that "the scuttle-butt around the Laboratories had it that Kelly would never have received his PhD from the University of Chicago if Darrow had not helped him to write his thesis and that Darrow, in turn, would not have received his PhD if Kelly had not helped him with his experimental work."

** In September 1940, while the Battle of Britain was at its height, and its outcome was still in doubt, a mission led by Henry Tizard, scientific adviser to Winston Churchill, came to the US for the purpose of exchanging scientific and technical information. Chief among the secret British devices brought by the mission was a 10-centimeter multicavity magnetron invented by J.T. Randall and H.A.H. Boot. All later microwave technology, from radar and satellite communications to ovens, has depended on variations of this invention.

people including Samuel. The mission had no drawings and little detailed information about the tube's performance. They agreed to leave it with BTL for three days for testing. Samuel and his associate, Vic Ronci, x-rayed the tube and, applying their own magnetron knowledge, within a week made three copies that approximated the performance of the original. The British were so delighted with the quick copies that Samuel was sure he would be given a major role in a BTL magnetron project. The official BTL history[1] describes this dramatic event in a few brief sentences without mentioning Samuel or Ronci.

In the event, Jim Fisk was put in charge and Samuel was told by Kelly what he probably already knew, that he "was both happier and more productive when he was doing his own thing rather than when he was trying to manage a group of people." He turned from microwave generating tubes to transmit-receive (TR) tubes that protected the delicate inputs of the radar receiving circuits from the powerful transmitted pulses and thus allowed a single radar antenna to be used to both transmit and receive. Starting with a British TR sample, he developed what came to be known as the "Samuel Tube," an argon-cesium gas tube remotely related to the old GE 200A. It was used in almost all the MIT Radiation Laboratory wartime radars.

Later in the war, anticipating a need for shorter wavelength radar and in defiance of the direct orders to desist given by his boss, J.R. Wilson, he developed a 3-centimeter version of his klystron. Consequently, Samuel was ready to floor his boss with a secretly finished working model when Wilson, prompted by a Pentagon visitor, at last called him in to tell him to start work on a 3-centimeter tube. This and the corresponding 3-centimeter TR tube were his last war-related tubes. Instead he worked on the modification of his 10-centimeter klystrons for the projected postwar AT&T nationwide microwave network.

At the end of the war Samuel took stock. He was a well-liked, well-known, and appreciated success in BTL, where his future was assured. He had nearly 18 years of service credit toward a pension, which in those days was not a guaranteed entitlement. Although he was an internationally recognized authority on microwave tubes, he did not want to continue in this field, for he was certain vacuum tubes soon would be replaced by something else. He was 45 and felt the need to prove himself all over again — doing what, he did not know.

The work of no other BTL group excited him. Furthermore, the BTL tradition was that an internal transfer always signified a failure and never included a raise in salary. Samuel considered himself to be grossly underpaid. His salary was $5,000 a year, about what it had been in 1941 since BTL gave no raises during the war. His family circumstances demanded more. He had to increase his income substantially. He wanted to find a neutral spot, outside BTL, where he could get an adequate salary while deciding on his next move.*

University of Illinois (1946-1949). At first he did nothing but mutter. Then, out of the blue, his hand was forced. William Everett, newly appointed head of the Electrical Engineering Department at the University of Illinois, hearing that Samuel was making noises about possibly leaving BTL, offered him the job of revitalizing the EE graduate research program. He would be a tenured full professor and could do some industrial consulting. Everett offered $7,000, which he quickly raised to $10,000. At just this time the College of Emporia offered him an honorary Doctor of Science degree which to some extent relieved his worries about taking an academic job without a PhD. Samuel always suspected that Everett had something to do with the degree offer, which cinched the deal. Kelly and others pointed out the folly of his decision to leave, but in July of 1946 he resigned from BTL.

He was 45 and felt the need to prove himself all over again — doing what, he did not know.

At Illinois he constructed a new vacuum tube laboratory, accepted an appointment as a member of the US Department of Defense Advisory Group on Electron Devices, picked up three varied consulting jobs associated with tubes, bought part of an airplane, and learned to fly. The expected flood of graduate students was slow in coming, so Samuel took up some old theoretical work on the space charge in cylindrical coordinates. This required numerical integration, which at BTL would have been done by a staff of women computers using electromechanical desk calculators — a facility which did not exist at the university. In Samuel's words, "The thing to do was to buy or build a computer."

Everett preferred buy to build, so Samuel found himself besieged by representatives of several small firms who were ready to build a computer for him if he would pay for their learning how. He comments, "Interestingly, not a single one of the five or six firms that sent people to talk with me seems to have survived." He concluded that if he wanted to have a computer, he was going to have to design and build his own, and that he probably knew almost as much about computers as anyone except for those with von Neumann at the Institute for Advanced Study at Princeton.

He set out to get someone else to take primary responsibility for the project while he had the fun of advising. First he moved to put the project into the Graduate School. When the reluctant Graduate School dean was replaced by Dr. Louis N. Ridenour, whom Samuel had known at the MIT Radiation Laboratory, the new dean immediately got him

* Samuel's words, "a neutral spot," may reflect the realities of those times as expressed in a private communication to me (1990) from Richard W. Hamming, a younger contemporary of Samuel at BTL. In response to my question about why Samuel left BTL, Hamming wrote: "In those days IBM and AT&T did not hire each other's people — so he went to a university for a period of sanitation!... At that time there was an unacknowledged peace treaty in which each did not invade the other's area. [This] is gossip, not fact that can be proved. Still they are the facts because we believed and acted as if they were true."

$110,000, saying that Samuel's estimate of $90,000 was a "bit low." Ralph E. Meagher, a new instructor in the physics department, jumped at the chance of heading the project.

Samuel visited the four places where significant computer work had been done (MIT, Harvard, the University of Pennsylvania, and the Institute for Advanced Study), arranged for weekly invited talks at the university, started graduate students working on various computer schemes and components, and personally explored ways of improving the Williams Tube, a British-designed storage system using a CRT.

By the fall of 1948 the project was running out of money, although still in the early design stages. The computer was to be asynchronous and involved a number of other novel, untried, and as yet undesigned features. Someone suggested that in order to attract attention, so that they could ask for more money, they build a cut-down version of their computer and do something dramatic with it. Claude Shannon was giving lectures on how to make a computer play chess. Samuel reasoned that if Shannon had already done this (he had not), "it ought to be dead easy to program a computer to play checkers. It happened that a world's-championship checker match was to be held in the neighboring town of Kankakee in the following spring. If we could program our computer to play checkers well enough to beat the world champion, this would attract considerable attention, and we might be able to raise the money we so badly needed." It was thus that he started on a task that would engage his attention for the next 20 years, never the basis of his livelihood but an all-consuming hobby.

He made a special trip to Chicago to talk to Shannon and after finding out the truth about the computer chess situation, decided that choosing checkers had been very wise:

> I started writing a program for a machine that did not yet exist, using a set of computer instructions that I dreamed up as they were needed. Ralph [Meagher] made a start toward redesigning the computer, but it was obvious that the design was going to require almost as much effort for a smaller machine as would be required for the full-sized machine. We were still going to run out of money long before anything actually got built.

Although the Kankakee match was never held and the checker program was not finished at Illinois, Meagher and others went on to build ILLIAC.

Samuel had never been happy with Everett's then novel but now academically commonplace expectation that he and his tube laboratory would attract enough government contracts to pay for itself and, through overhead charges, yield a net profit to the university. By January 1949 Samuel's unhappiness had turned to disillusionment:

> I decided I would have to leave the University and get another job where I would be paid to do what I wanted to do. I would not have to lead this double life of being the head of a vacuum tube laboratory in which I could take no real interest. I talked the matter over with Bernice, and she agreed with me. We decided that I should shift from being a vacuum-tube man to being a computer man whose knowledge of vacuum tubes could be put to good use. This time I was going to take the initiative and pick the job I wanted to have and not let myself be swayed by others' offers.
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> In looking around, it seemed to me that IBM was just the sort of company I wanted. IBM had entered the computer field and had built a relay computer (the Harvard Mark I), which was obsolete by the time it was finished. IBM had ample resources. The company was growing rapidly... They showed no signs of coming out with an electronic computer, and they would have to do this if they wanted to stay in business. IBM could certainly make use of my vacuum tube background. Computers were going to have to use vacuum tubes for some years. At the same time, IBM would not be wedded to vacuum tubes. It would be easier for them to shift to transistors, when these newly announced devices became well-enough developed to make this possible, than it would be for a company that had a great many years of vacuum-tube experience. Altogether, it looked like IBM was the company for me.

IBM Poughkeepsie (1949-1966). Late in the spring of 1949 Samuel called Wallace J. Eckert, Director of the IBM Watson Scientific Computing Laboratory in New York and the only IBM man he knew. Eckert put him in touch with his boss, vice president John C. McPherson, with whom Samuel had what he called "one of the strangest interviews that I have ever had." All those who interviewed him at IBM:

> went to great lengths not to reveal any detailed information as to what IBM might be doing in the computer field, but the very questions that they asked told me much more than they realized... It was perfectly obvious that they were working actively toward coming out with a product in the computer field.

Without telling him what he was to do beyond saying that it would be most interesting and would probably be in Poughkeepsie, McPherson offered him a one-year contract as a senior engineer at $16,000, which he raised to match Samuel's $20,000 counter proposal. Samuel comments that this "was just four times the salary that I have been receiving just three years before at the Bell Laboratories. Moving around certainly seemed to pay."

The offer was subject to Mr. T.J. Watson, Sr.'s personal approval. This is how Samuel describes the procedure:

> I first met with Mr. McPherson in his office and then on the stroke of the designated hour we went up to Mr. Watson's office... I was introduced to Mr. Watson...and I sat down beside Mr. Watson's desk. Mr. Watson asked me a few perfunctory questions, then

raised his eyes to Mr. McPherson, who had stood waiting by the door, and nodded slightly whereupon Mr. McPherson came over to where I was sitting and thanked Mr. Watson. I stood up and thanked Mr. Watson, I then turned and said good-bye to...Mr. McPherson, and I left the room. All very formal and perfunctory but necessary if one were to be properly hired.*

Now he was told he was to work in Poughkeepsie, under Ralph Palmeron, on some phase of the design of a very large digital computer, but the exact phase would be largely up to him to decide. After several name changes, the computer in question was finally designated the IBM 701. Details of the start of this computer, as reflected in the corporate records, can be found in Chapter 5 of the first volume of IBM's official history of its computers.[3]

Ralph Palmer introduced him to the 15 or so new employees with PhD degrees who made up the various Poughkeepsie laboratory groups and asked his advice, which amounted to this:

At the time, no very clear distinction had been drawn [by IBM] between work of a research nature that would be involved in understanding the physical principles on which a computer could be constructed and the actual design process. In fact...the drawing of such a distinction was quite foreign to IBM's traditional way of doing things. It was obvious to me that IBM would have to make this distinction if they wanted to stay in the computer business, and I would have to help them understand why this was necessary... Computers were now being made using vacuum tubes but they would soon be made using transistors. We would need to know much more about transistors than we or anybody else now knew before we could safely design a transistorized computer.

Palmer gave him one special secret task. Watson had decided that the way out of the difficulty of getting satisfactory vacuum tubes was to make their own, and Palmer had set up a small, and so far unsatisfactory, tube factory in the nearby Pickle Building. Palmer wanted specific advice about this. The whole idea of IBM starting out to make its own vacuum tubes struck Samuel as the height of folly and, after seeing the facilities, he told Palmer as much. IBM could not expect to do a better job than the existing tube industry, and the transistor, or something like it, would soon replace vacuum tubes. However, he did agree that they should keep the facilities intact as a threat to their tube suppliers and because there would be many ways that such facilities might prove useful, for example, to work on the Williams storage tubes. He proposed that they should set up and staff a solid-state research laboratory. Ralph said he would have to persuade McPherson, who would take up the matter with Mr. Watson: "Nothing could be done without Watson's approval."

After convincing McPherson, which took a bit of doing, they went to see Watson, who started the meeting aggressively by saying

"Watson soon interrupted my tale to buzz for his secretary and to ask to have his son, Young Tom, come in because he, Mr. Watson Senior, might not have time to get to the bottom of this situation."

...something to the effect that John McPherson had said that I questioned the decision that IBM was to make its own vacuum tubes. Could I explain myself? So I started with my explanation. Watson soon interrupted my tale to buzz for his secretary and to ask to have his son, Young Tom, come in because he, Mr. Watson Senior, might not have time to get to the bottom of this situation, and he might want Young Tom to follow up on the matter. So Young Tom came in and I had to start my story over again.

The whole matter was turned over to Young Tom, and Samuel spent the next few weeks in Junior's office and anteroom educating him about computers and all their components. He also explained the essential distinction between development, which involved designing the best computer possible with what existed, and research, which was done to improve one's understanding of how a computer might be built in the future. Samuel was surprised by Tom's ignorance, his willingness to admit it, and his speed in assimilating new ideas. Tom was always fearful that his father would ask questions that he could not answer. The learning phase ended when Mr. Watson, Sr., phoned to demand an accounting from his son. The Senior Watson's approval of Samuel's position followed in a telephone message from his secretary. Samuel points out in his autobiography that IBM's resulting shift in policy is discussed in Young Tom's autobiography[4] without mentioning him.

Samuel replaced the head of the tube factory with John B. Little from BTL, got Lloyd Hunter to direct the solid-state research, and set up its physical facilities in the Pickle Building. Samuel was given the title of "Manager of Research and Advanced Development," and comments that:

Perhaps a better title would have been "Manager of Research" but Mr. Watson Senior had always used the term "Research" to refer to product development and the expression "Advanced Development" had to be added to make the desired difference between what I would have meant by "Research" and ordinary product development.

* In a private communication, in response to my inquiry about this meeting, McPherson writes that "the strangeness of our interview was in part due to his relatively advanced age (47) for a new employee. I doubt that there were more than a handful of PhD's in the Poughkeepsie lab at that time."

In December 1951 Samuel was instrumental in organizing the first joint AIEE-IRE Computer Conference and in getting McPherson elected chairman.

It was in this period that Samuel managed to increase the capacity of each Williams tube from 512 to 2,048 bits and raise the mean time to failure to 30 minutes, both major accomplishments:

> For a while the [Williams storage tubes] we made were not very good. This scheme that I came up with was very, very simple and very dumb in a sense but it made all the difference in the world between making good tubes and not. We used a spark discharge tube, a spark coil for finding vacuum leaks... If you ran this over the face of a cathode ray tube, which is considered a no-no, you shouldn't do that, it would cause bright spots to glow on the face of the tube and I decided that these bright spots were probably what caused the trouble. I deliberately did this on some tubes and they turned out to be very good, much better. That is what we did, a very simple thing to do, while you were pumping the tube, you just ran this spark coil over the face of the tube and it burned off all the bad spots. That was so good that we kept it secret rather than patent it. So we could make good cathode ray tubes for Williams storage that nobody else could and it was all because I was dumb enough to try to run a spark coil along the face of the tube.

In the course of wandering around the Poughkeepsie laboratories, Samuel talked to Nathaniel Rochester, who was trying to decide on the desired word length and order structure for the IBM 701:

> Naturally enough, I looked at these matters from the view point of the checker-playing program that I had been trying to write while at Illinois. I decided that it would be well worth while for me to rewrite this checker program using the order structure that Nat was proposing for the new computer. I was a bit fearful that everyone in IBM would consider a checker-playing program much too trivial a matter, so I decided that I would concentrate on the learning aspects of the program. Thus, more or less by accident, I became one of the first people to do any serious programming for the IBM 701 and certainly one of the very first to work in the general field later to become known as "artificial intelligence."
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> In fact, I became so intrigued with this general problem of writing a program that would appear to exhibit intelligence that it was to occupy my thoughts during almost every free moment for the entire duration of my employment by IBM and indeed for some years beyond.
>
> Nat was trying to decide on the word length that was to be used in the new computer. He was wavering between 32 bits, this being a nice round octal number (2 to the fifth power) and 36 bits, which would fit in better with the number of columns that IBM regularly used on punched cards.
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> The standard punched card held 80 columns and the user could then store two computer words on each row with eight holes leftover for an identification number. The punched card also allowed for 12 rows of holes and by writing vertically on the card the user could also arrange to store each 36-bit computer word in three columns. Nat had no idea, at that time, that the computer would soon make the punched card obsolete. The punched card compatibility was, therefore, an important consideration and the 36-bit word won out.

In a private communication to me (1991), Rochester gives a somewhat differently worded explanation, suggesting that the last two paragraphs above might better be expressed like this:

> Nat was trying to decide on the word length that was to be used in the new computer. One leading candidate was 32 bits, being a power of two and likely to fit nicely in a binary computer in many, as yet undefined, circumstances. The other was 36 bits, which had immediate advantages.
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> This first IBM computer, the Type 701, had to be gotten out quickly and work properly. It had to be able to solve the problems our customers faced, be maintainable, and be usable by the programmers. There was a lot of opposition in IBM to wasting manpower and money on a computer. If this first one failed to meet any of these requirements, there might not be another.
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> IBM had a complete line of high-quality, reliable punched card machines on which programs could be keypunched, printed, and read into the computer. The results of the computation could be punched out on cards and saved or printed. To meet the schedule and the other requirements, programs had to be put on cards.
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> The 36-bit word was chosen because it was long enough to specify an instruction code and an address, and short enough so that two short instructions could be keypunched into each 80-column card with eight columns for a serial number. A programmer could remove a card from the deck holding his program, keypunch a corrected one, and get it back into the right place in the deck.

A careful comparative reading of the above two explanations reveals that there is a characteristic difference in viewpoint between a visionary like Samuel, who was happy to see the end of punched cards, and an engineer with practical, immediate problems like Rochester, who had to make a design to fit the existing conditions in the real world. Returning to Samuel's autobiography:

> When I started to rewrite my checker program, using Nat's order structure, I found that the 36-bit

word would make my coding considerably simpler, so, naturally, I was pleased when the 36-bit word won out, but I certainly did not apply any pressure on Nat to get him to choose this word length. Quite as a joke, I told a few people that I had persuaded Nat to adopt the 36-bit word because of its advantage to my checker program, and, strangely enough, some people believed me. Even today, some forty years later, I occasionally meet people who had heard and who still believe that the 36-bit word was chosen because of its advantage to my checker program.

One of my reasons for pushing ahead with writing my checker program for the new machine even before the machine was built was to have a vehicle to test the proposed instruction set for its completeness and its effectiveness as a tool for expressing the operations that one would want the computer to perform. Since Nat had already started his work on the code structure for the new machine by the time I joined IBM, he was usually well ahead of me and I seldom found the need for a command that he had not already proposed. But it did happen, and the thought that I might find the need for still other unprovided commands was an incentive that spurred me on in my coding.

I also had to worry about the fact that the experimental model that they were building did not take shape all at once, so I wrote my code in small modules that were self-contained and that could be loaded and operated separately. I did find it convenient to have one common module that I always used to specify the way the rest of the modules were to be loaded and to handle such matters as the conversion of decimal input into binary, and binary back to decimal. With time this module became, in effect, an operating-system module although it was not so called at the time. The final result of all of this was that an operational checker program could be, and was, put together from a collection of debugged modules and could be, and was, ready to run on the first experimental model of the 701 just as soon as this model became available in anything like its final form.

Writing code for the first 701 was quite a different and more difficult task from what coding is today because we did not have any of the modern-day programming tools, the assemblers and compilers that we have subsequently developed. In spite of this, I was able to get my first checker-playing program running and debugged on the 701 well before the first assembly program became operational. The program was modular. This was, indeed, 40 years ago.

Later, when Nat had written the very first assembler, I found it more convenient to write a disassembly program to disassemble the various modules of my operating checker program into new source program modules, which could then be modified and reassembled, than it would have been to start all over by writing a new set of source-program modules in the form required by this new assembly program. This disassembly program was so successful that I did a land office business in supplying it to just about all of the early 701 programmers.

Donald E. Knuth credits Samuel with the invention of hashing in connection with the construction of an assembly program for the IBM 701.[5] Samuel later said that his one contribution to Fortran was its hashing scheme for storage, apparently taken from this earlier work. Samuel also took pride in his one-card loader for the IBM 701.* In this atmosphere Samuel amused himself and others with a trick one-card loader that printed a different series of random numbers every time it was used. The loader's random number generator took its initial value from the system clock as one tenth the number of microseconds that it took the printer to come up at turn-on, a practically indeterminate variable.

"While you were pumping the tube, you just ran this spark coil over the face of the tube and it burned off all the bad spots. That was so good that we kept it secret rather than patent it."

According to Samuel, Fortran was first worked on by two groups, one at Poughkeepsie and one in New York City. When they were consolidated into one in New York, Samuel, who had been on the periphery of the Poughkeepsie group and preferred Poughkeepsie to New York, chose not to move and had no further connection with the language development.**

Peripheral professional activities, outside IBM, took a lot of his time. He continued his part-time secret work for the National Security Agency (NSA), an association he had started while at the University of Illinois and which he continued even after he retired from IBM. He also continued with the Department of Defense Advisory Group on Electron Devices, of which he was chairman for 18 years. In addition, he was one of the main technical coordinators between IBM and the US Air Force on the programming of a very large computer system to be located underground near Omaha, Nebraska. As in the case of NSA, his autobiography does not further identify the system or his work on it.

When BTL opened transistor use to licensees, IBM signed up and John McPherson immediately made what Samuel considered to be the simplistic and arbitrary decision that transistors were to be used in IBM's next computer, in spite of their then well-known unreliability. This put

* For an account of a contemporary one-card loader, and some words about the competition to hammer another instruction out of IBM 701 programs, see an article by Johnston.[6]

** At this time, Mark Halpern, also involved in Fortran, had exactly the opposite opinion of the two communities.[7]

Samuel in the awkward position of having to argue against the immediate use of transistors when he would have very much liked to see IBM take the lead but at the right time.*

Samuel comments that it was McPherson who earlier, in 1949, as vice president of engineering, had incorrectly assured the senior Watson that IBM had "the finest research organization in the world" and did not need electronic talent. The junior Watson sketches this story in his autobiography[4] (pp. 200-202), without mentioning McPherson by name.**

Samuel broadened IBM's field of interest in storage technology. He started work on ferromagnetics, and in June of 1950 hired Mike Haynes, fresh out of the University of Illinois, to work on core storage. He started Don Young to work on ferroelectric storage but when Young got discouraged, let it drop, a decision for which Samuel expressed regret in his autobiography.

In August 1952 he was given the title of Assistant Manager of the Poughkeepsie Engineering Laboratory and responsibility for activities that did not interest him. Someone else† was soon given his job, and Samuel was made Research Advisor to the Manager of the Poughkeepsie Laboratories, a job and title that suited him and allowed him to dabble in everything that was going on without taking responsibility for anything. He proposed a periodical similar to the *Bell System Technical Journal.* As a result, the *IBM Journal of Research and Development* was started, but contrary to his hopes he was not made the first editor.

"Computing Bit by Bit." At this time Samuel wrote an article for an issue of the *Proceedings of the Institute of Radio Engineers* that was devoted to digital computing. "Computing Bit by Bit or Digital Computers Made Easy"[8] was reprinted, without reference to its previous publication, by IBM as a technical publication[9] and handed out everywhere. Because of its timeliness, its qualities of truth and clarity, and its wide distribution, it was probably the most influential of all Samuel's writings. It explained computers and computing in language that was crystal clear to all electronic engineers and easily understood by lay persons. Its key paragraph states:

> We will start then by considering a digital computer as being merely an information processing device. This is the only thing that the computer does. It cannot create any new information not contained in the original source, although it may transform the input information into a very much more useful form. This is an important concept, and a full recognition of its implications will go a long way toward dispelling any mystical feelings with respect to the modern computer.

The paper also expressed Samuel's opinion as to whether computers could think as follows:

> It might be well at this point, to dispel some of the fuzzy sensationalisms of the popular press regarding the ability of existing digital computers to think. Over a hundred years ago Lord Byron's daughter, Lady Lovelace, in commenting on Charles Babbage's analytical engine, made a remark which is as true today for the modern computer as it was then. She said, "The analytical engine has no pretensions whatever to originate anything. It can do whatever we know how to order it to perform." ...The digital computer can and does relieve man of much of the burdensome detail of numerical calculations and of related logical operations, but perhaps it is more a matter of definition than of fact as to whether this constitutes thinking.

Apparently he held to this view throughout his life, leading to his identification, in 1961, by Marvin Minsky as one of the few leaders in the field of artificial intelligence who believed computers could not think and probably never would.

Advice to IBM. At IBM Samuel suggested that the corporation hire an outside person with scientific stature to be director of research. This proposal was too startling for most of the ingrown and complacent top IBM managers and met with little success until he got the idea to Tom Watson, Jr., who appointed a search committee. Samuel suggested Emanuel R. Piore, who ultimately got the job. In his autobiography Samuel tells how he thought he was involved:

> I had gone to a computer conference on the West Coast and I stopped in Phoenix, on the way home, to attend a solar-heating conference... I was sitting near the pool late one afternoon, in the rapidly decreasing area that was in the sun, when Piore and a man whose name I have forgotten came out to sit in the sun. I had known Mannie [Piore] since the days when he had just started out with RCA, so he stopped to say hello and to introduce me to his companion. They then sat down as far away as they could get and still be in the sun and continued with their conversation. I certainly had no intention of eavesdropping but I could not help but hear enough to learn that Mannie was thinking of leaving the Bureau [ed. — apparently the Office of Naval Research], that the stranger was trying to get Mannie to come to work for him, and that Mannie was not interested. So, armed with this information, I suggested Mannie to the committee. I was not party to IBM's negotiation, so I do not know whether there

* This anecdote refers to a McPherson internal decision made early in the 1950s and does not refer to the McDowell corporate policy statement of 1957 establishing the same rule (see the book by Bashe et al.,[3] p. 387). McPherson had nothing to do with this later decision.

** In a private communication (1991) to me McPherson writes "Art Samuel's comment on me, based on Tom's story in his book, compounds an unjustified conclusion that Tom made of which I was unaware until reading it in his book. I would never have made the quoted statement [quoted above], being well aware of the Bell Labs with Dr. Jewett and Mervin Kelly, presidents of the Labs, as near neighbors here in Short Hills."

† Samuel says Jerry Haddad replaced him, but Haddad, in a private communication to me (1991), indicates that it was not he.

were other people seriously considered. Some time was to elapse before the appointment of Mannie was announced, but Mannie got the job.*

The checkers program. Samuel's checkers program occupied a unique position in his life. It was apparently never out of his thoughts for long after he first worked on it in 1948. We have seen above how it was involved in the early design of the IBM 701 but, he said

> It was never once mentioned in any written record of my assignments, but when I wanted time on a computer I was always able to get it. Later when the 701 was in production on the factory floor, special arrangements were made so that I could get the continuous use of two computers during the shift from midnight to eight in the morning so that I could have two machines play each other with the two machines following different learning schemes.
>
> It wasn't until the [IBM] 704 [was developed] that I really got a lot of machine time. There were lots of machines on the factory floor that had two groups of people testing them. From seven in the morning until midnight there were two different teams testing the computers. Then from midnight to the next morning they just sat there not being used. They let me go in and start my checker program that I had fixed so it would run continuously. The main thing was keeping a record of what it was doing. I had as many as four different machines working from midnight to 7:00 a.m. playing checkers with themselves and accumulating statistics on their different playing schemes. This went on for several months, and they were glad to do it for it was a good way to test the machines, each of which was tested for about a month.
>
> The first few machines were kept for quite a long time to make sure they really would work. Because a computer is so very complicated, you just really can't test everything. But you want to test as much as you can because otherwise you ship it out and the first program they use wouldn't work for some foolish reason. We found lots of bugs, of course. Wiring mistakes. All sorts of things.
>
> No effort was made to publicize my work, but when I was asked to give talks on my checker work and even to demonstrate my program on television, I was allowed to do so.

Samuel stated that the first operating checkers program was completed for the IBM 701 in 1954. The first program with a really effective learning scheme was completed in 1955, and publicly demonstrated on television on February 24, 1956. Samuel recalled three such TV demonstrations in these words:

> Once, a Canadian crew came around with a leading Canadian checker player, a second time, an American crew brought an American player. These two programs were taped for later broadcast and I have been told of the programs having been shown in quite a few different countries. The third time, Will Rogers Jr. had an hour long morning program that was broadcast live and he interviewed me remotely, he being in the TV studio and I being at a 701. He asked me questions about the program and then we had an expert play a game that took most of the hour long program with frequent breaks when Will talked about all sorts of things as he did on all of his programs.

Samuel's checkers program occupied a unique position in his life. It was apparently never out of his thoughts for long after he first worked on it in 1948.

In spite of this publicity, he published nothing on the program until 1959. He later said that, in the early days of computing, IBM did not want to fan the popular fears that man was losing out to machines, so the company did not talk about artificial intelligence publicly. Salesmen were not supposed to scare customers with speculation about future computer accomplishments. This belief that customers were not ready to hear about machine learning, Samuel said, explains IBM's lack of public recognition of his early checkers work. On the other hand, the company did not stop him from working or talking about his checkers program and allowed him to use company facilities to run it.

IBM Europe. In the late fall of 1953, Samuel was transferred to W. Wallace McDowell's staff with the title of Research Advisor to Vice President McDowell. He described McDowell as

> A very hard-hitting man that nobody could get along with, who bawled his employees out unmercifully, and had a reputation of being a very hard man to work with. He was an MIT graduate, where he had taken course 15, which is the course taken by those who couldn't make a go of it in other engineering courses.

A feeling had developed in the management ranks of IBM that there were not many new ideas coming from the US but many were coming from England. McPherson, McDowell's predecessor, had been taken out of the management of research and engineering and had been making trips to Europe to look for novel developments. Samuel said

* In a private communication to me (1991), Piore writes that he has no current recollection of this encounter. His own autobiography[10] says that he left the Office of Naval Research, where he was chief scientist, in 1955 to be a showpiece for the defense industry conglomerate, AVCO, hung together and run by Victor Emanuel. He parted pleasantly from AVCO in 1956 to go with IBM as director of research. He says he doesn't know who nominated him, suspecting Jim Killian, Eric A. Walker, Albert G. Hill, and Mervin Kelly. Watson[4] (pp. 247-248) says it was Killian, and that he had never heard of Piore before.

that while McPherson was very good at meeting people, he wasn't very good on technical matters: "He met a lot of people and knew all their names but he didn't do a very good job finding out what they were doing."

Somebody got the idea that Samuel might do a better job; furthermore, such an assignment would be a good way to ease him out of being manager of the Poughkeepsie lab. A major part of his new assignment was to be to keep McDowell informed about what was happening in Europe in the field of computers.

After clearing things with the junior Tom Watson's brother, Arthur K. Watson, who owned Europe as a part of his World Trade presidency, the Samuels and the McPhersons embarked together on what Samuel described as "a month-long First Class Luxury tour of England, France, Germany, Sweden, Holland, and Switzerland at company expense." They set off on the Queen Mary and called on all the major universities, the government laboratories, the few industrial laboratories that were not direct IBM competitors, and, of course, the chief IBM sales offices.*

Samuel found computer work on the Continent to be very limited, but, in contrast, England was a beehive of activity and generated a feeling of excitement that he said was well expressed in *Faster Than Thought*.[12] His autobiography lists the names and accomplishments of those he visited in England. He always regretted having missed meeting A.M. Turing, who was still living at that time.

In the following year he made five or six more, shorter, fact-finding trips to Europe. On these visits he did not depend on IBM to make the interview arrangements but scheduled them himself. He tried to spend two days at each of the major institutions, giving a more or less formal lecture on the day of his arrival, describing the 701, what its order structure was, and why that structure had been chosen — and telling about his checker program, and how it worked. He knew what not to talk about, and had been enough removed from the manufacturing art so he was in no danger of giving secrets away. He could talk about indexing and hashing, things that he had a hand in doing. Even this limited release of information was a complete turnaround from the way IBM had worked in the past. As a consequence of what he did reveal in his lecture, people would feel kindly toward him, and he could spend the second day in informal discussions learning what they were doing. He admitted that this did not work well in France:

> French people were always suspicious. They would listen to me talk but they wouldn't say a thing and they didn't have anything to offer anyway. But the people in England were very cooperative, and told me lots of things, and the people in Germany were very nice.

As he dug into it, he found that so much good work was being done of a fundamental sort, especially in Germany, that following it was more than one man could do alone and adding more men from outside the countries involved would not solve the problem. This led to his idea that IBM should establish its own research laboratory in Europe to make new discoveries for IBM, to maintain ties with European universities to yield information and employees, and to keep track of European developments by being on the ground as well as in the field. He proposed this idea in writing in the fall of 1954 and was told to go ahead in January 1955. He was to select a general location, clear the matter with the local authorities, and choose the director.

England, Switzerland, and Holland, in that order, were his possible sites, England being his first choice because of the high level of British computer activity and the advantage of a common language. In February 1955 he set himself up in London to start. After a month of investigation he found that while the British were not opposed to an IBM research laboratory they insisted that it not be located near a university but in one of the New Towns that Samuel described as "the most dismal places that I have ever seen." They also took a dim view of hiring foreign nationals if there were British nationals not fully employed.

He gave up on England and tried Switzerland. Here everything was different. Fate had conspired to simplify his task. It turned out that his first choice for the head of the European laboratory, Ambros P. Speiser, was the son of the representative of the canton of Aargau to the Swiss Council of the States (Standerat), which corresponds to the US Senate. In short, the senior Speiser was a man of considerable importance.** The manager of the IBM Zurich office advised Samuel to approach the senior Speiser first, advice which Samuel followed, although he thought it an unusual cultural custom that one should first discuss an employment offer to a 33-year-old engineer with his father.

In a luncheon interview with the senior Speiser, Samuel was told that, although the Swiss preferred their own laboratories, there were no rules against foreign ones; although they preferred hiring Swiss nationals, there were no rules against hiring foreigners; and although Mr. Speiser would prefer that his son work for a Swiss company, he would not try to influence his decision: "It is up to him. He is an adult by now and I won't tell him that he shouldn't take the job."

In less than 48 hours, Samuel had most of his answers. He offered the job to Ambros Speiser without telling him of his visit with his father. Speiser had impressed Samuel on an earlier visit. He was an electrical engineer with the rank of Pivatdozentat at the Swiss Federal Institute of Technology

* In a private communication to me (1991), McPherson objects to Samuel's characterization of the trip in these terms and insists that its purpose was to get more complete information on the machines he had heard discussed at the March 1953 Computer Conference at the National Physical Laboratory in Teddington.[11] The British computers listed in the program of that meeting are Pilot ACE, EDSAC, LEO, MADAM, MOSAIC, NICHOLAS, RASCAL, and TRE. In addition to McPherson of World Headquarters, the listed IBM representatives included two from the United Kingdom and one from France.

I consider both accounts to be correct. Their differences probably reflect what each man then considered to be ordinary versus luxury traveling accommodations.

** In his autobiography, written some 35 years later, Samuel recalled that he was a member of the seven-man Swiss Federal Council, a somewhat higher position.

(Eidgenossische Technische Hochschule — ETH) in Zurich, where he was the technical director of a team of eight who were designing and constructing ERMETH (Elektronische Rechenmachine der ETH).[13] The work was being done under the overall direction of Professor Eduard Stiefel, with whom Speiser had had generous exposure to US computing six years before.

Samuel was so certain that he had succeeded that he flew home the very next day. Ambros flew to New York for the clinching interview and in May 1955 accepted the offer. Samuel commented: "Ambros proved to be an ideal director... The laboratory is well regarded in Switzerland, and three members of the laboratory's staff have received Nobel prizes."* After hiring Speiser, Samuel stopped his regular trips to Europe, making one last visit in 1956 to evaluate progress. He was impressed by the quality of those hired, about half of them being those whose names he had given to Speiser. The others were somewhat younger and if anything more promising.**

IBM Poughkeepsie revisited. After this international activity, things were slow for Samuel, and in the spring of 1957 he was able to make real progress in improving his checkers program. IBM, however, was churning. Piore and others were rearranging the research facilities and personnel. There was a confusion of announcements, assignments, and reassignments, and the creation of new research facilities. The 200-person research group that Samuel had started was now headed by Jerry Haddad. When Haddad was given another job, Piore, over Samuel's objections, installed Samuel as temporary resident manager of the Poughkeepsie Research Laboratory. Samuel devised a scheme to overcome his self-recognized limitations as a manager. He asked John Little to be his assistant, telling him that he would first discuss all specific management directions with him and leave it up to Little to actually give the orders. In that way he felt he could freely discuss scientific aspects of the work with his subordinates without their being reluctant to question his judgment or argue their points of view. Under this arrangement he said he could always pretend to himself that he was just a consultant.

The Samuels and the McPhersons embarked together on what Samuel described as "a month-long First Class Luxury tour of England, France, Germany, Sweden, Holland, and Switzerland at company expense."

One matter he handled on his own. After some effort he persuaded four of whom he considered to be totally unsuitable managers he had inherited from Haddad's reign to transfer out of research and into development.†

In November 1958, as promised, Samuel was relieved of his managerial responsibilities and given the title of Senior Consultant to the Director of Research. Piore gave him nothing to do so he was able to spend nearly full time on his checkers program. The editor of the *IBM Journal of Research and Development* had got management's permission to publish a paper on it. This first paper on the checkers program, "Some Studies in Machine Learning Using the Game of Checkers,"[14] has this abstract:

> Two machine-learning procedures have been investigated in some detail using the game of checkers. Enough work has been done to verify the fact that a computer can be programmed so that it will learn to play a better game of checkers than can be played by the person who wrote the program. Furthermore it can learn to do this in a remarkably short period of time (8 or 10 hours of machine-playing-time) when given only the rules of the game, a sense of direction and a redundant and incomplete list of parameters which are thought to have something to do with the game, but whose correct signs and relative weights are

* The recognition is now international and four members are Nobel laureates, all in physics: in 1986 Gerd Binnig and Heinrich Rohrer for the scanning tunneling electron microscope and in 1987 K. Alex Muller and J. George Bednorz for superconducting ceramics. Speiser later left IBM for the post of director of research of Brown Boveri & Cie.

** In a private communication to me (1991), Piore commented on IBM's program of establishing laboratories in Europe, describing events after those involving Samuel. I have edited his remarks into the following paragraphs:

The Zurich laboratory met IBM's goal of being close to important academic institutions, but these institutions did not welcome IBM. With a characteristic European academic suspicion of trade and commerce, they assumed that IBM's obvious purpose in establishing the laboratory was first to make money and only secondarily to advance knowledge. Piore engaged in a long program of meetings, lunches, and dinners to bring together the top scientific administrators and professors of the Technical University with IBM and other US scientific notables. Finally, Piore says, academia in Zurich began to appreciate IBM's Nobel laureates.

The governments of both England and France refused to allow IBM laboratories to locate near important academic institutions, so one was located in Hursley near Southampton and the other at Le God near Nice. Germany permitted a laboratory in Stuttgart, which grew with IBM's growth in that country. None of these achieved as much as the one in Zurich. Laboratories were established in the Netherlands and Sweden but never became viable and were later terminated.

† Probably as a consequence of the organizational turmoil at IBM, Samuel's recollections do not agree in all details with what Haddad and Piore now recall. In private communications to me (1991), Haddad points out, and Piore agrees, that he never reported to Piore and that he never headed a 200-person research group started by Samuel. In reference to the unsuitable managers, Haddad writes "I don't know what Art was referring to. Having never been in direct charge of research operations, I did not have the authority to install any research managers. I don't doubt that there were people managing some of the local lab research projects that were development type people. I would also agree that movement back and forth of such people was a good thing that IBM practices right up to the present."

It is possible that in writing this part of his autobiography Samuel inadvertently substituted Haddad's name for someone else.

unknown and unspecified. The principles of machine learning verified by these experiments are, of course, applicable to many other situations.

The program itself is described in these words:

The computer plays by looking ahead a few moves and by evaluating the resulting board positions much as a human player might do. Board positions are stored by sets of machine words, four words normally being used to represent any particular board position. Thirty-two bit positions (of the 36 available in an IBM 704 word) are, by convention, assigned to the 32 playing squares on the checkerboard, and pieces appearing on these squares are represented by 1's appearing in the assigned bit positions of the corresponding word. "Looking-ahead" is prepared for by computing all possible next moves, starting with a given board position. The indicated moves are explored in turn by producing new board-position records corresponding to the conditions after the move in question (the old board positions being saved to facilitate a return to the starting point) and the process can be repeated. This look-ahead procedure is carried several moves in advance... The resulting board positions are then scored in terms of their relative value to the machine.

In 1963 this paper was reprinted (with minor additions and corrections) in the book *Computers and Thought.*[15]

Samuel published only one other paper on his program, a chapter in the 1969 *Annual Review in Automatic Programming*. The title, "Some Studies in Machine Learning Using the Game of Checkers II — Recent Progress," is so much like that of the first two papers that it is sometimes thought to be a duplicate. It is not, for in it he discusses the more promising of the two machine learning procedures of his earlier paper, that is, the generalization learning scheme in which a preassigned list of board parameters is used. He had concluded that the other procedure, rote learning, while good in opening and end games, was less effective in its middle game playing. By this time Samuel was at Stanford University and his program had migrated to the IBM 7040.

Soon after the 1959 paper was published, he was asked to be the editor of the *IBM Journal of Research and Development* in addition to his duties as a consultant to Piore. He continued as editor until his retirement in 1966.

Editor's note: Samuel's autobiography manuscript ends here. To complete the eloge, I have abstracted material from the transcript of a tape-recorded oral interview made by Samuel with his daughter in 1983, and I have included information from his friends, family, and associates.

One of Piore's innovations at IBM, intended to establish strong relationships between IBM and an often antagonistic academic world, was to temporarily lend well-qualified and well-known senior employees to influential universities as IBM visiting professors. The loaner acted as a full-time faculty member but was continued on the IBM payroll at a level that maintained his total income unreduced. Suitable arrangements were made to take care of any extra expenses involved. Samuel, while not the first of such appointments, was an IBM visiting professor at MIT from 1963 to 1966, where, among other things, he worked on one of the text editors of the MIT time-sharing system.*

In a private communication to me (1991), Fernando J. Corbató of MIT writes as follows (slightly edited):

Samuel was indeed at MIT as a visiting professor in the mid-1960s. I believe the person that encouraged him was Professor Marvin Minsky... I am not sure if it was anything more than a courtesy research appointment... (We used to make courtesy faculty appointments but not any more.) The occasion of the appointment was the beginning of Project MAC, the large MIT effort devoted to the exploration of time-sharing as a means of supporting man-machine interaction. (Project MAC has since spawned the AI Laboratory and has itself been renamed the Laboratory for Computer Science.)

The initial time-sharing vehicle of Project MAC was the Compatible Time-Sharing System (CTSS) and it was on that machine that all the participants did their work. Samuel became persuaded of the merits of time-sharing but had a hard time shaking the habits he had developed working with large mainframes and the primitive tools of the day. Like many others, he was dissatisfied with the initial editing tools available on CTSS and quickly took to developing his own.[16] Samuel's efforts were too idiosyncratic for most and failed to develop a following. Nevertheless, he contributed to the excitement of defining what was wanted while interacting with a computer and I am sure, in the process, learned a great deal about the mechanics of the system.

It was in this period that Samuel published an article in the *New Scientist*[17] predicting what computers would be like in 1984. This article is said to be the first published prediction that personal computers would become commonplace in 20 years.

Stanford University (1966-1990). Samuel's retirement from IBM in 1966 was forced, by company policy, on account of age. One of his several job options was to participate at MIT in research on publishing automation that was sponsored by the American Newspaper Publishers Association. While they offered him an arrangement that would have more than doubled his IBM income, he turned them down. In his taped interview he said they were stodgy, didn't have enough vision, and would never allow him to do anything really radically new. He considered an appointment to the University of California at Irvine but decided instead, at the behest of John McCarthy, to accept George Forsythe's

* An inquiry to IBM elicited no further information concerning this appointment.

offer to join Stanford University's Artificial Intelligence Project as a lecturer at three-fourths of his IBM salary because Stanford had no mandatory retirement age. At that time UC Irvine could only guarantee employment until age 70 or 71. Although his duties became those of an adjunct professor, Stanford at first had no such title so he was designated a "senior research associate." His outside consulting brought his income up to what he figured he would have made with the newspaper research laboratory. In 1974 the university changed its policy regarding adjunctivism and gave him his appropriate title of Adjunct Professor of Computer Science, which was changed in the next year to Adjunct Professor, Emeritus, a position and title which he held until his death.

At Stanford he worked on speech recognition, first with Roger Reddy and then with Reddy's graduate students, supervising their PhD programs. At about the same time the Defense Advanced Research Projects Agency (DARPA) started a five-year program on speech recognition. Samuel would have nothing to do with this program because he felt the five years allowed for results was too short, but he did work on the subject under a more general program. He developed a technique of taking fast Fourier transforms (FFTs) of 10-millisecond samples of speech, recording and printing out the waveforms and the transforms. Then he developed programs that attempted to identify each transform as a consonant or a vowel or whatever, and set up a learning routine so the program would learn to recognize them. He used the signature table scheme he had recently incorporated into his checkers learning program. (He continued to work on his checkers program until it was outclassed in the early 1970s.)

By 1975 Samuel considered his speech recognition program to be about as good as any other. He thought, in his words, "it would be kind of neat" to go to some place else in his seventh year just as though he was allowed a sabbatical, which, in spite of his title, he was not. One day, while walking back from lunch at the faculty club, he fell in with a person he identified in his taped interview as the inventor of the FFTs.* Samuel told this person how he was using FFTs in speech recognition, and said that he thought he'd like to go to Australia. The person, just back from Australia, recommended him to the head of the government laboratory in Canberra. There Samuel started a speech recognition program that is still going on, but now transferred to the Australian National University.

While Samuel was in Australia, all his graduate students followed Reddy to Carnegie Mellon University and, when he returned, DARPA unwisely, in his opinion, concentrated its speech recognition support on one approach, a different one.

Stanford's time-sharing computer system had a word processing editor that Samuel said was very nice but very slow because it was written in a high-level language. After firing, for excessive temperament, the full-time employee who was revising the editor, Stanford asked Samuel to take over the revision. At Stanford he worked on it for several years and made it, in his words, "definitely superior to that on the IBM personal computer." It was used at MIT and Carnegie Mellon, and at one time was known by his initials, E.ALS. In 1983 he expected it to be used until Stanford switched computers. (In 1990 Stanford pulled the plug on what was known as "Essential E," the name of the manual Samuel had written for the system.[18])

Samuel's 1964 article is said to be the first published prediction that personal computers would become commonplace in 20 years.

On the side he got into video games. A company called Video Brain proposed to put a $500 game computer on the market to be superior to the Atari. They approached McCarthy, who, knowing that the work on the editor was ending, suggested Samuel. They wanted a "shoot-em-dead" kind of game, like Atari, but he said he could write a good checkers program, which he did, packing it down from 40K to 4K of memory. Then he wrote an even better game, a version of Othello. By the time he wrote the third game for the firm, customers could go to Atari with its shoot-em-dead games at one or two hundred dollars or to full-fledged personal computers at $10,000. Nobody wanted an intermediate version at $500, so the corporate successor to Video Brain gave up on games and went into making generic computers for the East Asian market.

Samuel felt that his games were too good; that is, they were too hard to beat. He said, "for an amateur game you want one that people can get accustomed to beating." The day his retainer arrangement with Video Brain ended, he was approached by Atari, who wanted a shoot-em-dead game, and by Apple, who offered a royalty for copies sold. He turned them both down.

By 1981 everything Samuel was working on had sort of folded up. Stanford closed the Arastradero Road laboratory and moved him into town. After a few months off the payroll, doing his own things, he was approached by an associate of Don Knuth and invited to help a graduate student debug a Pascal version of TeX, Knuth's typesetting system.**

Samuel joined the TeX project in the spring. He was then almost fifty years older than anyone else in the group, but he was a major participant in the project until it completed its work in 1985. As part of this work he corresponded with about 400 people worldwide about TeX, and handled 10 or 15 phone calls a day. Finally, following the unexpected death, after heart surgery, of his wife Bernice, in 1982, he got out from under this load and in the following year, at the age of 81, he took a three-month round-the-world cruise giving on-board lectures on computers.

During this time, in addition to the work mentioned above, he wrote software for the Livermore S-1 Multipro-

* I have been unable to determine who this was.

** TeX had been originally written for SAIL, an exotic specialty system used only at Stanford, MIT, and Carnegie Mellon.

Arthur Lee Samuel

Born, December 5, 1901, Emporia, Kan.
Early life and education, Emporia, Kan.
The College of Emporia, 1919-1923, BA.
Boston Tech (now MIT), Cambridge, Mass., 1923-1927, MS. Part-time employment in this period, General Electric at Lynn, West Lynn, and Schenectady, N.Y.
Instructor, MIT, 1927-1928.
Bell Telephone Laboratories, 1928-1946, New York City and Summit, N.J.
Professor, University of Illinois, 1946-1949.
International Business Machines (IBM), 1949-1966, Poughkeepsie, N.Y., New York City, Europe, MIT.
Senior Research Associate, Adjunct Professor, Stanford University, 1966-1990.
Died, July 29, 1990, Stanford, Calif.

cessor. He continued to demonstrate his ability to understand inadequate documentation of complicated programs, and to turn otherwise incomprehensible material into clear and attractive manuals. He was always interested in writing tutorials for beginners, as exemplified in "Computing Bit by Bit."[8] In a private communication to me (1991), Knuth writes that every user of Stanford's SAIL computer was helped by his booklets *Essential E*[18] and *Short WAITS*,[19] which provided brief introductions to the text editor and operating system.

Samuel decided to do a similar thing for TeX, telling Knuth that his first goal was to write a kindergarten primer. But after a while he found that TeX was too complicated, so he needed to go up to the first-grade level. He tried valiantly to keep the document to less than 32 pages, finally accepting a 34-page length. This classic beginners' book, *First Grade TeX*,[20] sold more than 2,600 copies in its first three years. When it was recently translated into Japanese,[21] it ran to 175 pages.

Samuel was an active computer programmer long after age forced him to give up research. Shortly after his round-the-world cruise, Parkinson's disease began to impair his walking and balance, but his mind remained alert. He continued work and travel until his death. One of his last projects involved modifying programs for printing in multiple type fonts on some of the Stanford Computer Science Department's computers. His associates consider him to have been the world's oldest active computer programmer and to hold the all-time world record for correct computer instructions written after the age of 80. He last logged in on the campus computer on February 2, 1990. In June 1990, although very frail, he went on an Alaska cruise with his daughters and a son-in-law. His final illness began after his return and he died on July 29, 1990.

Samuel took the position, said to be somewhat unique at Stanford, that artificial intelligence and expert systems are not identical, and are clearly distinguishable. He considered his own work to be artificial intelligence. He maintained that expert systems were products, the result of development work, while artificial intelligence was research, trying to find machine solutions to understanding or partially duplicating the processes of the human mind.

He is survived by a brother; two daughters, Donna Hussain and Margaret Finch; and four grandchildren. He is memorialized at Stanford by the Arthur L. Samuel Fellowship Fund, which awards a scholarship in his honor.

He was a fellow of the Institute of Electrical and Electronic Engineers and of the American Physical Society, and a member of the Association for Computing Machinery and the American Association for the Advancement of Science.

Career summary. Samuel's career and accomplishments are too diverse to be easily summarized. Before World War II, at Bell Telephone Laboratories, he was a leading designer of microwave tubes, of which his TR radar switch, the Samuel Tube, was the most widely used. At the University of Illinois he launched the ILLIAC team. He was one of those who guided IBM into computers and into real research, and he initiated its solid-state laboratory. He made a major improvement in the Williams storage tube. He invented hashing. He was chairman of the Defense Department Advisory Group on Electron Devices for 18 years. He started IBM's Zurich Laboratory and was instrumental in founding the *IBM Journal of Research and Development.*

Of all his publications, "Computing Bit by Bit"[8,9] was the most influential. IBM gave it extremely wide distribution at a time when such a clear explanation of computing was rare and badly needed. It exemplifies his ability to state difficult concepts simply, to explain hard things so they can be understood.

Clearly the accomplishment for which he is most famous is his checkers program. He lavished the most effort on it over the longest period and it is, as noted before, recognized as the world's first self-learning computer program. I will go further and claim that it is the first functioning artificial intelligence program.

Thus in spite of Samuel's own opinion, as stated to his daughters, that he considered his vacuum tube patents to be more important, the world will remember him for his Great Game.

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Joseph Clement: The First Computer Engineer

When Charles Babbage began work on his famous Difference Engine, he was in need of a professional mechanic and draftsman. He managed to arrange for the majority of his work to be done in the workshop of Joseph Clement. This arrangement continued for a number of years, essentially during the entire time that the Difference Engine was under active construction. The arrangements between Babbage and Clement are reasonably well known[1] and the story of how the two of them came to part company has been part of almost every paper written about the project. However, very little information is available about Joseph Clement himself. The purpose of this article is to attempt to show that Clement was not simply a run-of-the-mill machinist who happened to be fortunate enough to work for Babbage, and was partly responsible for the failure of the construction of the Difference Engine (an impression easily obtained from reading the majority of accounts of the project). Rather Clement was a highly respected member of the mechanical engineering community when Babbage first contacted him and Babbage actually delegated a large part of the responsibility of the actual design of the Difference Engine to Clement.

In 1990, while examining some letters in the Fitzwilliam Museum in Cambridge, I came across two letters* from Charles Babbage[2,3] to a certain George Clowes. Clowes evidently was associated with a publishing venture** because the content of the letters was Babbage's response to having been shown some proofs of an article written by a Samuel Smiles. The article concerned the life of Babbage's chief mechanic and draftsman Joseph Clement. In the first letter (August 19, 1863)

* These letters once belonged to Douglas Hartree, the early British computer pioneer, and were evidently given by him to the Fitzwilliam Museum in 1947. The author is not aware of how Hartree came into their possession.

** George Clowes was likely associated with the London publishing firm of John Murray. It was this firm that published the first edition of Smiles' book.