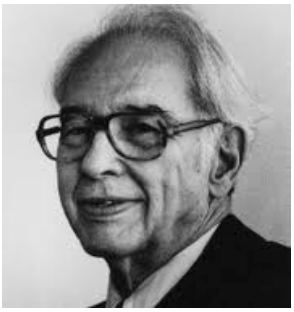
# John Vincent Atanasoff

*Born October 4, 1903, Hamilton N. Y; inventor of the Atanasoff Berry Computer (ABC) with Clifford Berry, predecessor of the 1942 ENIAC, a serial, binary, electromechanical, digital, special-purpose computer with regenerative memory.*



*Education:* BSEE, University of Florida, 1925; MS, Iowa State College (now University), 1926; PhD, physics, University of Wisconsin, 1930.

*Professional Experience:* graduate professor at Iowa State College (now University), 1930-1942; US Naval Ordnance Laboratory, 1942-1952; founder, Ordnance Engineering Corp., 1952-1956; vice-president, Atlantic Dir., Aerojet General Corp., 1950-1961.

*Honors and Awards: US* Navy Distinguished Service Award 1945; Bulgarian Order of Cyril and Methodius, First Class, 1970; doctor of science, University of Florida, 1974; Iowa Inventors Hall of Fame, 1978; doctor of science, Moravian College, 1981; Distinguished Achievement Citation, Alumni Association, Iowa State University, 1983; Foreign Member, Bulgarian Academy of Science, 1983; LittD, Western Maryland College, 1984; Pioneer Medal, IEEE Computer Society, 1984; Appreciation Award, EDUCOM, 1985; Holley Medal, ASME, 1985; DSc (Hon.), University of Wisconsin, 1985; First Annual Coors American Ingenuity Award, Colorado Association of Commerce and Industry, 1986; LHD (Hon)., Mount St. Mary's College, 1990; US Department of Commerce, Medal of Technology, 1990,[1] IEEE Electrical Engineering Milestone, 1990.

*Special Honors:* Atanasoff Hall, named by Iowa State University; Asteroid 3546-Atanasoff-named by Cal Tech Jet Propulsion Laboratory and Bulgarian Academy.

### Advent of Electronic Digital Computing[2]

#### Introduction

I am writing a historical account of what has been an important episode in my life. During the last half of the 1930s I began and, later with Clifford E. Berry, pursued the subject of digital electronic computing. Included were my conceptions of computing, and the construction both of a prototype and of what I later called the Atanasoff Berry Computer (ABC), to honor the memory of Dr. Berry's extraordinary competence. But my contacts with computing began much earlier.

The year 1913 made an impression on my mind because so much seemed to happen then. My family lived in a new house in Polk County, Florida on the premises of a phosphate mine where my father was an electrical engineer. Early that year, my father decided that his position required him to have a better slide rule than the one he had. Now, my father did not really need that new slide rule. So the new slide rule was left for me, with

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[1] Presented by President George Bush

[2] Adapted by J. V. Atanasoff from his original manuscript submitted to Annals of the History of Computing, 1983.

its book of instructions. That slide rule was my meat. In two weeks or thereabouts, I could solve most simple problems with it.

Can you imagine how a boy of nine-plus, with baseball on his mind, could be transformed by this knowledge? Baseball practice was reduced to zero and a stern study of logarithms was substituted. By the age of ten I became an expert in computing logarithms and many other mathematical and scientific problems.

Early in high school, I decided to study theoretical physics as my life's work. However, at the University of Florida I studied Electrical Engineering, since it was the most theoretical course given.

In September 1925, I started my graduate work in mathematics at Iowa State College (now University) at Ames, Iowa.

In the period between 1925 and 1928, I taught mathematics at Iowa State College, and continued graduate work in mathematics and physics. I received a master's degree in mathematics in 1926 at Iowa State College. In March 1929, I went to the University of Wisconsin to continue my work for the doctorate. I received my PhD in physics in July 1930. My thesis was titled *The Dielectric Constant of Helium.*

This was my first experience in serious computing. Such calculations required many weeks of hard work on a desk calculator such as the Monroe, which was all that was available at the time. I was also impressed that the process of approximating the solution of partial differential equations required a great many calculations, a fact that ultimately motivated my work in automatic computing.

After receiving my doctorate, I returned to Iowa State College where I became assistant professor of Mathematics. There I gave increased attention to the International Business Machines equipment located in the statistics department. While this equipment did not permit the mathematical dexterity of the Monroe, it nevertheless represented the largest calculator of the day.

As the only person in theoretical physics at ISU, I did not have much competition. I was soon made assistant professor of both mathematics and physics and then, not too much later, an associate professor in both departments. At about that time (1934), I commenced to give attention to the formal process of approximating the solution of partial differential equations.

#### State of the Computing Art

As I came to feel the basic need for more powerful means of computing, I examined, in more detail, the types of apparatus that were available. I soon determined that computers could be divided into two classes, analog and digital.

In analog computers, a number is represented by a physical quantity in the machine as measured by some system of units. A digital computer also requires some entity to represent numbers. Here, however, the representation is not a simple one-to-one relationship, but is determined by a kind of algorithm called the Hindu-Arabic number system. While historically this system used numbers to the base ten, we intend no such restriction, for in theory any whole number greater than unity can be used as the base. My own device and most modern computers use the base two.

It seemed very clear to me that the advantages of the analog approach had been largely explored and found wanting, except in some special cases not requiring relatively high accuracy. For most of the purposes of technology and science, we were left with a digital approach. But to make this fully effective would require a new and original art.

I thought I knew how a computer should work. First, the computer would have to add and subtract, and later, one could expand these operations into multiplication and division. At the time, I wondered if anyone had devised a definition of multiplication that was not based on addition, but the four elementary operations of arithmetic are interrelated and all computing theoreticians have had to go along with that fact. From the start, I was interested in carry-over; it is the crux of the digital method.

What are the characteristics with which the digital numbers are to be represented? In those days I had little precedent as to the architecture of a new digital computer of a larger size, that is, capacity. The only attempt at a machine of sufficient capacity for my purposes was the differential analyzer, an analog device which, as I have said, did not seem promising. In simple computers and even in the tabulators, the medium of representation was always mechanical, often the rotation of a shaft, and I must admit that I was inclined to follow this precept.

Even at this early stage, the principal other medium that occurred to me was an electrical state of a circuit. I had studied electrical engineering and physics, and I had also studied and experimented with electronics, then in its infancy. So it was perhaps natural that my mind turned to this medium in which I had my greatest expertise.

At an early point in my thinking about digital computers, I commenced to think about the effects of a change in the base of the number system. Now I had to visualize how a change in base would affect computing structures. At that time, I had only a very vague idea of a computing machine. I hoped that the nature of the arithmetic for the various bases would indicate which one would be most advantageous for a computer. These thoughts led to a conclusion which has stood the test of time.

In looking over the 1936 art in computing, I had become convinced that a new computer should provide for a much larger retention of data. Almost from the start, I called this “memory.” The word seemed natural to me, as I suppose that it did to others, since it is still in use today in a wide field including computers.

I now continue with a quotation from my transcript of testimony which I gave in federal court on 1971 June 15:

> “Well, I remember that the winter of 1937 was a desperate one for me because I had this problem and I had outlined my objectives but nothing was happening, and as the winter deepened, my despair grew and I have told you about the kinds of items that were rattling around in my mind and we come to a day in the middle of winter when I went out to the office intending to spend the evening trying to resolve some of these questions and I was in such a mental state that no resolution was possible. I was just unhappy to an extreme degree, and at that time I did something that I had done on such occasions-I don't do it anymore-I went out to my automobile, got in and started driving over the good highways of Iowa at a high rate of speed.
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> “I remember the pavement was clean and dry, and I was forced to give attention to my driving, and as a consequence of that, I was less nervous, and I drove that way for several hours. Then I sort of became aware of my surroundings. I had, of course, been aware of the road before, but then I became aware of where I was and I had reached the Mississippi River, starting from Ames and was crossing the Mississippi River into Illinois at a place where there are three cities, one of which is Rock Island.

> “I drove into Illinois and turned off the good highway into a little road, and went into a roadhouse there which had bright lights. It was extremely cold and I took my overcoat. I had a very heavy coat, and hung it up, and sat down and ordered a drink, and as the delivery of the drink was made, I realized that I was no longer so nervous and my thoughts turned again to computing machines.
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> “Now, I don't know why my mind worked then when it had not worked previously, but things seemed to be good and cool and quiet. There were not many people in the tavern, and the waitress didn't bother me particularly with repetitious offers of drinks. I would suspect that I drank two drinks perhaps, and then I realized that thoughts were coming good and I had some positive results.
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> “During this evening in the tavern, I generated within my mind the possibility of the regenerative memory. I called it 'jogging' at that time. I'm thinking about the condensers for memory units, and the fact that the condensers would regenerate their own state, so their state would not change with time.... During that same evening, I gained an initial concept of what is called today the 'logic circuits.' That is a non-ratcheting approach to the interaction between two memory units, or, as I called them in those days, ‘abaci.’”

During that evening in the Illinois roadhouse, I made four decisions for my computer project.

- I would use electricity and electronics as the media for the computer;
- In spite of custom, I would use base-two numbers (binary) for my computer;
- I would use condensers for memory, but “regenerate” to avoid lapse;
- I would compute by direct logical action, not by enumeration.

*I am now amazed and pleased to find that each of my four decisions relates to structures that are in use in modern computers.*

It is true that I did not invent the modern dynamic memory but this memory uses capacitors (condensers) for memory, and the refresh cycle directly derived from my jogging or regenerative ideas.

So far, the work on the computer had been done by me in my spare time. Since the trip to Illinois, I had used more than a year working mostly on jogging and logic circuits for adding and subtracting. I now felt much more confident that the project would be a success and I knew that I could not go on alone. So, early in the spring of 1939, I made an application for a grant from the dean of the graduate school. I planned to hire an assistant and to have a small budget for materials and shop work.

In selecting an assistant, I felt that I should choose an electrical engineer, since most students entering graduate work in physics did not have the mechanical or electronic skill. Soon after having these thoughts, I met Dr. Harold Anderson, a professor of electrical engineering, on the sidewalk in the center of the campus and told him of my need. He already knew of my interest in computers, and he answered in a moment, “I have your man-Clifford Berry.”

We started actual work at the beginning of the fall quarter of 1939. Our first effort was to try to prove the feasibility of this new method of computing that I had developed using theory only.

**The Prototype Computer**

Even before the fall quarter had begun, Clifford had studied my plans and we were soon involved in a discussion of how we should proceed. We both agreed that the theoretical aspect of these plans, however necessary it had been, would have to be reduced to practice. Each portion of my design would have to be built, examined and fully tested. In the end, we would compose these parts to form a prototype of a computer to see if the portions would co-act as I had planned. There were parts of the whole that were not fully designed and we would have to devise them and bring them to fruition.

As I have said, I chose condensers (or capacitors) as the element for memory, because a condenser can give a good voltage to actuate a vacuum tube, and because the vacuum tube will give enough voltage to recharge the condenser.

We next had to study how condensers would operate as memory elements, and we soon learned that almost any condenser would work. We selected paper capacitors of about 0.0015 microfarads capacity.

I suppose the reader will have gathered that I was delighted with the concept of jogging. Jogging is reminiscent of the little boy going to the grocery store and reciting, “a dozen eggs, a pound of butter,” over and over, hoping to arrive at the store before his memory has failed. Jogging may be employed when a memory element has two states and when they deteriorate over time, we can cause it to pass from one state to the other.

In the 1930s vacuum tubes were the only active element available for electronics. It was hard to get a low-voltage, direct-current source, and so we wished to use alternating current for our tubes. This limited us to heater type tubes. The only objection was that the heater types used much more power for the heat source.

Almost as soon as the prototype was completed, it began to work very well. Our visitors who understood what was going on were surprised to find so much structure giving additions and subtractions that were correct. Of course, our explanation to them had to cover base-two number theory.

There is little doubt that the prototype, which was the first electronic digital computer, was completed near the end of 1939. My memory said November 1939. Much later, during cross examination in court, counsel for defense (Sperry-Rand) showed me material which purported that this was not until early in 1940. In re-cross, material was adduced by counsel for the plaintiff (Honeywell) which showed that the first demonstration was in October 1939.

The prototype was, of course, a relatively crude device. It could just add and subtract the binary equivalents of decimal numbers having up to eight places. Nevertheless, Clifford Berry and I regarded this machine as a great success. It settled many doubts about how an electronic computer should be built:

- The device was digital, not analog like the differential analyzer;
- While the clock system was mechanical, all computing was electronic;
- For the first time, vacuum tubes were used in computing;
- The very advantageous base-2 number system was first used;
- Logic systems were first employed in computing;
- All computation was done in a serial manner;
- Capacitors (or condensers) were used as memory elements;
- A rotating drum memory contained the capacitors;
- What I called jogging (which others now call regeneration or refreshing) was first used in computation.

Clifford E. Berry and I were very pleased to have access to a method of computing with such power. Once our prototype had proved successful, we both knew that we could build a machine that could do almost anything in the way of computation.

During the early years, our computer had a title relating to the solution of large systems of linear algebraic equations. About 1968, I became aware that it would be discussed at length, and, since Berry was dead, I wanted a title that would honor him for his extraordinary ability and effort in developing this computer. Accordingly, I renamed it the Atanasoff Berry Computer ... the ABC.

In order to get started fast, I decided to take a chance and estimate the size of the machine. I knew a few dimensions of the various parts that were to go into it. I was repeatedly trying to estimate the number of vacuum tubes I would need for this digital electronic computer. Arbitrarily, we had decided that it would operate over 30 fields, and so would require 30 add-subtract mechanisms. Without very much figuring, I made an estimate of the size of the total machine and arrived at roughly the size of an office desk. So, I ordered a lot of angle iron for the frame. People saw these irons at the back entrance of the physics building and wondered about them. I heard someone replying to a question, “Oh, Atanasoff thinks he is going to make a computer out of those angle irons.”

At that time, the largest digital computer was the IBM tabulator. The one we had at Iowa State College could tabulate and sum 40 columns of the 80 that the punched card provided. But we knew there existed machines which could tabulate and add the full 80 columns, divided into ten fields of eight columns each.... We planned a machine of 3000 fields of memory, which would in fact calculate at a speed of about 30 times that of the largest IBM tabulator.

When one starts a new and strange project, one must expect that it will be in financial trouble from the start. I had received some aid from Iowa State College, and expected and did receive more from time to time. However, I knew that I should seek some other source for the funds necessary to complete the computer project.

In my requests to Iowa State College, I had used a two-page write-up, but in asking others for help, I needed something better. During the spring and summer of 1940, I used my spare time to write not only what we had done, but also what we proposed to do during the remainder of the project. This paper, finished on August 14, 1940, has been reprinted by Brian Randell in his book *The Origins of Digital Computers* (1982).

I felt that the work we were doing on computers should be patented, and so, at an early stage, I had investigated the subject of patents at Iowa State College. It seemed clear to me that there was no firm policy on the subject. I learned that the Iowa State College Research Foundation (ISCRF), whom I consulted concerning a patent application, did not regard their own patent counsel as adequate for this task. I was referred to Mr. Richard R. Trexler, a Chicago patent attorney. When ultimately ISCRF and I entered into a contract for the proceeds of a patent on my invention, they agreed, in principle at least, to use Mr. Trexler as our patent attorney.

No official pronouncement was made on the terms of a contract, or even on the necessity for a contract, until Dr. Friley, president of Iowa State College, received the letter of March 24, 1941, from President Howard Poillon of Research Corporation granting $5330, a considerable amount of money in those days. Then, from a seeming absence of any policy, the situation changed drastically and Iowa State College policy was suddenly firm. I am still not exactly sure how it all came about, but I have since seen a letter from Dr. Friley to ISCRF saying that Iowa State College should hold onto rights to the patent on my invention. I very soon heard from

ISCRE. The first words came to me verbally; in substance, I would not be allowed to use the grant until I signed a patent contract with ISCRF. I signed the contract in July 1941.

At the time I first contacted Trexler (August 6, 1940), I was finishing my manuscript on “Computing Machines for the Solution of Large Systems of Linear Algebraic Equations,” and I supplied him with a copy. This did not satisfy Mr. Trexler, so with some help from me, he drew up a rather extensive specification covering the details of the patent, and together we directed a draftsman in making the patent drawings.

At this state in my scientific career, I usually attended the winter meeting of the American Association for the Advancement of Science, which in those days was scheduled between Christmas and New Year's. So, late in 1940, I turned my car eastward with my family and after other activities, on December 26 I attended the meetings in Philadelphia for three days. My interests were rather wide, and I frequently moved between the sessions of the various societies which would meet with the AAAS. There were, of course, no papers on computing in the modern sense, but I was interested in calculation in general. As a consequence of this, I visited what I clearly remember was a more than ordinarily dusty, chalk-filled classroom assigned to Dr. John W. Mauchly for giving a paper on an application of a harmonic analyzer, which he had constructed, to some weather phenomena. After the paper was over, I advanced to the podium.

He was very enthusiastic about his analog electrical system for doing Fourier transforms, which had been the subject of his paper. We talked for most of an hour in this first meeting. In the end, we shook hands and promised to write.

Dr. Mauchly made good on his promise to write. My files do not have his first letter, but they do have a letter from me dated January 23, 1941, answering him and inviting him to come and see me. Mauchly was well pleased with my invitation and there were a succession of letters between us. My memory is that he arrived about dark on Saturday, June 14, 194 1, accompanied by his young son Jim.

My memory is very clear that he first saw the computer with his son and my family. There was no one else around, this being Sunday, and this checks with his letter saying he might spend Friday at Iowa City. The computer was covered with a sheet to prevent dust from settling on it, but I quickly removed this obstruction and he saw the ABC for the first time. The machine was in process of construction at this point.

I believe that Dr. Mauchly and his son left Ames early on Friday, June 20. Meanwhile, two-thirds of our waking time was spent talking about computers. He read my manuscript and would have liked to take a copy home with him, but I did not allow him to do that. He read all parts of this description of our machine and discussed it with me. Sam Legvold was a graduate student of mine working on another of my projects in a room next to the computer. Much later, he told the federal court in Minneapolis of Mauchly having his coat off and working with the machine, when I was otherwise employed. Mauchly took the manuscript to my home with him, and he took notes on white bond paper which I gave him, at his request.

Mauchly and I had a very cordial relationship while he was at Ames, and after he left we still corresponded, though at less frequent intervals. On September 30, 1941, he wrote a letter to me which contained the question, “. . . would the way be open for us to build an 'Atanasoff Calculator' (à la Bush analyzer) here?” In my answer of October 7, I had to tell him:

> Our attorney has emphasized the need of being careful about the dissemination of information about our device, but it does require that we refrain from making public any details for the time being. It is, as a matter of fact, preventing me from making an invited address to the American Statistical Association.

On December 7,1941, the Japanese bombed Pearl Harbor and the US was involved in that terrible holocaust. The future of everyone had to be adjusted to meet this emergency. Soon it became evident that Berry would be drafted unless he were engaged in a war-related project. Our computer work did not have such a preferred status, and although I did what I could to get him deferred, the draft board had never heard of our project. As a result, Mr. Berry started to look for a position for which he could be deferred. He was so able that he quickly found a suitable position with Consolidated Engineering Corporation of Pasadena, Calif. He married my secretary, Jean Reed, and on July 1, 1942, he left Iowa State College.

During the spring and summer of 1942, I continued to work with the ISCRF and Mr. Trexler to get the patent under way. There always seemed to be some reason why it should be put off, however, and put off it was. The patent was never applied by Iowa State College, probably due to short-term financial considerations.

**US Naval Ordnance Laboratory (1942-1948)**

I began work at the Naval Ordnance Laboratory (NOL) late in September. The NOL was the research laboratory of the Bureau of Ordnance. It had responsibility for depth charges, and mines and various other projects, as assigned. Historically, it had been located in the US Naval Gun Factory, beside the Anacostia River, and there it remained until the war was over and a new laboratory was constructed north of Washington, D.C., at White Oak, Md.

Although acoustics had not been one of my particular fields of interest, I was a theoretical physicist, and so I was put in charge of acoustical testing of mines for the Navy Ordnance Laboratory.

My first objective was to get acquainted with the subject of acoustics. My next objective was to acquire a staff. I was able to get the services of David Beecher. He had not been my major graduate student (at Iowa State College) but I knew him well, and he had on occasion, worked on the ABC. Later I was able to get both Dr. Herman Ellingson, who was on the staff of Luther College in Decorah, Iowa, and my former major student, Sam Legvold. The rest of my staff came from various other sources.

Although I do not have the date, I think that early in 1943 I was seated at my desk in my noisy, dirty space in Bld. 184, when I felt someone approaching me from the right. It was Dr. Mauchly. By some means, he had become attached to the NOL, his security clearance was satisfactory, and somehow he attached himself to my staff. The exact basis of his employment with NOL was never known to us. I introduced him to my superior, but he had no more knowledge than I. I did not know how much money he received for his consulting services and I was too busy to find out. He said he was still employed by the Moore School, but he came back once or twice a week. At first, I planned various things for him to do; later, I attached him to Dr. Ellingson's statistical group. I do believe, however, that every time he came in, he stopped by to see me briefly.

One such occasion, he told me that he and J. Presper Eckert had devised a new way to compute. I remember that I was very busy, but I asked him to tell me about it. His reply was simple: “I cannot, the subject is classified.”

The visits of Mauchly continued sporadically until the end of the war. On August 30, 1944, both Mauchly and Eckert visited me in the Gun Factory. I believe that at least Eckert did not have proper security clearance for the area and so they were escorted. They had arranged through the Army and the Navy to obtain my services on quartz transducers. Although I agreed to help them, they filled their needs elsewhere. This was the only time I ever saw J. Presper Eckert.

In a period from September, 1942 until the war ended, I was in Ames every few months, and I took the time to see some member of Iowa State College and discuss the prospect of filing a patent application on our work on computers. At various times, I also wrote to Counselor Trexler and to the ISCRF. During the first year or two, I really believed that a patent would be applied for. Much later, Mr. Trexler said he had never been authorized to proceed.

I have in my files an Iowa State College interoffice memo without signature, written, I believe, by Dean R. E. Buchanan, of Iowa State College, and it is concerned with the patent situation. The date was June 26, 1948, but I did not see it for perhaps twenty years. The author speaks of seeing me at the NOL, and I do remember seeing Dean Buchanan at about that time. He speaks of “Atanasoff's saying that the computer was probably largely obsolete.” This was clearly incorrect; I spoke, rather, of the need for moving rapidly if anything was to be salvaged from our work. The memo gives no explanation of the delay in executing the patent contract between Iowa State College and me.

About September 1945, while I was attending a staff meeting at the Gun Factory, my superior, Dr. L. H. Rumbaugh, told us that the Bureau of Ordnance was going to build a computer. Of course the research department would do the work. It was over three years since I had left computing at Iowa State College, and I had not spoken of this part of my history; it was known, however, and so I was selected to head the project.

Dr. John Mauchly came to see us for the first time in several months. In the last years of his life, he claimed that on this occasion I asked him how to build a computer. I do not remember doing this and do not believe I did; in any case, I do remember that as always we talked of many things but I heard nothing from him about computers.

The computer project seemed to be doing very well except for understaffing, but in a short time my superior and I were called to the office of the chief of research of the Bureau of Ordnance and we were told that the Bureau wished to discontinue this project. As is usual in such cases, no basic reason was given. After the conclusion of the computer project, Dr. John Mauchly no longer came to the NOL and I saw him again only once, at a much later date, when he came to my house with Laurence B. Dodds, Counsel for Sperry-Rand.

### The Ordnance Engineering Corporation (1952-1961)

Early in 1952, I was thinking of the types of experience I had encountered. I was 48 years old and I knew that if I wanted to be involved in private enterprise, the time had come; one of my friends, in fact, had told me that I was already too old. However, with a few firm friends, I incorporated The Ordnance Engineering Corporation (TOEC) in the state of Maryland.

During the first years of this corporation, we rented space in Rockville, Md. On June 15, 1954, we received a visit from Mr. A.J. Etienne, who was a patent attorney for International Business Machines Corporation. As soon as I learned that we were going to discuss patents, I called David Beecher, our vice president, into the

conference. Mr. Etienne announced his purpose by saying in substance, “If you will help us, we will break the Mauchly-Eckert computer patent; it was derived from you.”

I hesitated to reply to Mr. Etienne. I was looking back over the years that had passed since Mauchly had told me (in 1943) that he and Eckert had invented a “new method of computing, different from yours,” and I had believed him. This was the first substantial item to the contrary that I had encountered. I did not know of the patent to which Mr. Etienne was referring, that is, the Regenerative Memory patent 2,629,827, issued the previous year (1953). (The ENIAC patent, applied for in 1947, was not issued until 1964.)

I remember wondering if Etienne was correct.

In 1956, we sold the corporation to Aerojet General Corporation, with principal offices in Azusa, Calif. After our incorporation into Aerojet General (AGC), we were called the Atlantic Division. In 1959, I was made vice president of AGC and managed the Atlantic Division.

As the US changed to peace conditions after the Korean War, I used my experience in computing to shift the activity of the Atlantic Division toward the invention and manufacture of automatic equipment for parcel handling and sorting. By this time, I had found being a “corporation man” not to my liking. Accordingly, I resigned as vice president of Aerojet General Corporation early in 1961.

During these years, I was well aware of the growing interest in computing. Even before Mr. Etienne's visit, I knew that certain companies were making inquiries of anyone skilled in the art, including Dr. Clifford E. Berry and myself.

From time to time during the twenty-plus years that had elapsed after Berry had left Ames, I visited Clifford and Jean in their home in Altadena. Nearly every time that my business called me to California, I took an evening to spend with them and their family. During, say, the first two-thirds of this period, I was told of his interest in and success with his work, and of his rapid advancement with his company. I do not know exactly when this picture changed; perhaps by 1960, though, Clifford, while still excited with his work, was less satisfied with the position he held with his company. I also noticed a slight cynicism in his general attitude, which I took to be a natural consequence of aging.

Late in 1963, I was overwhelmed to get a letter from Jean Berry, saying that Clifford had resigned his position, taken a place with a company in Long Island, and had died one night at his apartment.

In late 1967, still feeling unhappy over his death, I made a personal investigation of it. I located his apartment and talked with the man who had first found him. I also visited the police of the county, since his death was not natural; he had been found in bed with a plastic sack over his head but with his bedclothes smoothly over his arms, which were by his sides. An autopsy revealed that his brain plasma had 0.12% alcohol content, which would have permitted him to drive a car in some jurisdictions. He had been taking Dilantin because of recent epilepsy, and so his blood and brain were carefully examined for this and other drugs; none was found. Suicide, said the authorities.

**Preliminaries of Litigation**

On April 26, 1967, I was visited by Mr. Allen Kirkpatrick, a patent attorney of Washington, who represented Control Data Corporation (CDC). He had learned of me by reading a book, *Electronic Digital Systems*, by R. K. Richards. Mr. Richards had seen our computer with Clifford Berry. I soon was told that Sperry-Rand Corporation was suing CDC, and was given a quick summary of the issues. Later, Mr. Kirkpatrick furnished me copies of the patents involved, and suggested that I scan their claims to find any that were developed in my own work.

During his April visit, Mr. Kirkpatrick also told me that Sperry-Rand was going to sue Honeywell (my notes say that the Honeywell suit was begun on May 26, 1967). More specifically, Honeywell was to be sued by Illinois Scientific Developments, Inc. (ISD), a subsidiary of Sperry-Rand. Kirkpatrick gave me some information on the Honeywell suit as well. At about this time, I also was contacted by Henry Hanson, D. Dennis Allegretti, Charles G. Call, patent attorneys, and Henry Halladay, trial attorney, all of whom represented Honeywell in that case. I agreed to do consulting work for both CDC and Honeywell.

Then, not long after Mr. Kirkpatrick came to see me, I was approached by attorneys George Eltgroth, Norman Fulmer, and H. Mial Dustin of General Electric. Fulmer had been at Iowa State College and had actually worked on the ABC. When these men visited me, I had partially read the ENIAC patent and I knew that certain claims were derived from my work.

Law is not my specialty, although I had spent some 2000 hours studying patent law over the years, and I append the following from hearsay. Sperry-Rand, having acquired certain patents from Mauchly and J. Presper Eckert, in particular the ENIAC patent 3,120,606 and the Regenerative Memory patent 2,629,827, felt that they had a basic patent control of the computing machine field, and sought to levy royalties on those companies in violation of the claims of their patents. I have heard these royalties were estimated at one billion dollars, all told.

To begin this legal process, Sperry-Rand chose to sue CDC over the Memory patent and Honeywell over the ENIAC patent, since both CDC and Honeywell had resisted paying royalties; Honeywell counter-sued for anti-trust violation and became the plaintiff in a combined ENIAC suit.

The Memory patent had been applied for in 1947 and was issued in 1953. The ENIAC patent had been applied for in 1947 and was issued in 1964. For some legal reasons and/or the desire of judges, the suit of Sperry-Rand vs. CDC was to be held in Baltimore and the suit of Honeywell vs. Sperry-Rand was planned for Minneapolis.

It was soon clear to everyone that these lawsuits would become monumental in computing machine history and would take some years before coming to trial.

After learning from the attorneys of Honeywell and CDC how seriously they regarded the litigation, I made a careful search of my papers. I had just moved into a new house and had a collection of boxes which had not been opened for many years. Some of these had been with me and some had been in storage. In two of these boxes, I found my files for the computing machine. These represented hundreds of items which were later used in the litigation.

I was interested, but slightly amused, that I had found myself in the mesh of the federal court system of our nation. I have always been intrigued by the law and our patent system, but jurisprudence is far removed from the fields in which I have labored. Watching these two cases unfold in our federal courts gave me a little deeper feeling of how man gets along with man.

## Formal Litigation

We use the term “formal litigation” for the two processes, deposition and trial, by which evidence is produced for the judge (or jury) to use in reaching a verdict. This part of litigation is the major part; in important cases, the records become voluminous. In the matter of computer litigation being considered here, there were only two cases at that time, the Memory case and the ENIAC case; even so, the legal documents numbered in the thousands.

In the early years while the trials were pending, there were many depositions, including those of the principal witnesses and of many other witnesses who would not attend the trial. I was deposed once for each of the two trials. While my first deposition, on the Memory case, was rather short, the second, on the ENIAC case, lasted approximately two weeks, beginning on November 11, 1968. In both cases, Dodds was counsel for Sperry-Rand.

Among the witnesses deposed (of personal interest to me) were myself, both trials; Mauchly, both trials; J. Presper Eckert, both trials; Lura Meeks Atanasoff, both trials; and Sam Legvold, both trials; as well as many other people. Clifford Berry was not alive at the time of the deposition, but in a way a letter he wrote to R.K. Richards[1] on March 22, 1963, quoted again and again in the trial, served as a statement by him of the situation when Mauchly came to Ames.

The deposition of Dr. John W. Mauchly October 11, 12, 13, 1967, was taken on behalf of the defendant, CDC, in the suit by Sperry-Rand on the so-called Memory patent 2,629,827 and certain other patents.

Mauchly spoke of his visit to see our machine. He spoke of being cordially received, but hinted that no one was willing to tell him all about the machine. He claimed that nothing worked except the motor. There was no demonstration of the action of the machine, he asserted.

He said he had no time with the machine: perhaps half-an-hour. Later: perhaps 1½ hours.

He saw one cylinder of condensers, he said. He learned that memory was retained by the use of regeneration, but said he was not told how the regeneration worked.

Both Sam Legvold (a former student and employee) and myself, on deposition and in trial testimony, stated under oath (and Berry in his 1963 letter to Richards corroborated), that when Mauchly visited the computer laboratory at Ames, he was given the full details of the project and he was given much more than anyone else; that he was given the complete current operational test of the computer; that he spent a length of time with the machine in excess of 16 hours, took his coat off to work on the machine in his shirtsleeves and held certain parts of the machine in his hands; and that he saw our current memorandum (August 1940) on the machine and took notes.

## Honeywell versus Sperry-Rand

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[1] See Biography of Clifford Berry.

This trial was held in the federal court at Minneapolis, with judge Earl R. Larson presiding. It began June 1, 1971. Knowing that I would be an early witness, I went to Minneapolis on June 7. For the moment, I spent my time listening to the court testimony and having private briefings by counsel for Honeywell.

This being my first testimony in a federal court, I took it very seriously. The total record associated with my presence on the witness stand was 1,338 pages long. Of this, the major portion was describing the computer; one of the last parts related to the period when Mauchly was working for me at the NOL, and I have covered that in my historical summary of the case.

It was late on Friday, June 25, 1971, and I was through testifying, after nine days on the witness stand. I took time to say good-bye to some of the staff, including the court reporters, who had stood by and prepared good copy in spite of technical words and low tones.

Then I looked over at the table for Sperry-Rand and there was (Counselor) Mr. Ferrill, alone, picking up some papers. I do not want to be lacking in manners, and so I advanced with my hand extended to say good-bye to him. I find it hard to describe what happened for the next few seconds; I felt that I was looked over in some minute detail. In the end, though, he reluctantly took my hand. He did not look happy; perhaps he knew better than I of the effects of my testimony.

The trial in the ENIAC case ended on March 13, 1972; it had taken 135 days or parts of days. A total of 77 witnesses were heard. The total trial transcript was 20,667 pages. After the trial ended, judge Larson took some time to reach his decision, which was finally issued on October 19, 1973. Of course, this was a decision that should have received major press coverage; the press, however, was occupied with other matters, because October 20 was the date of the Saturday Night Massacre of Watergate fame.

The decision itself comprised 248 pages of legal paper, with an appendix of 60-plus pages. The judge found 17 specific claims on the ENIAC patent invalid on a variety of grounds, including two claims taken to be representative of the subject matter derived from me by Mauchly and Eckert. He found the entire patent invalid on three grounds unrelated to actual inventorship: public use, sales, and published disclosure, all dated more than one year prior to the ENIAC patent filing date of June 26, 1947. Of greater significance to me is the fact that he also found the entire patent invalid on the ground of derivation from my prior electronic digital computer. I quote relevant passages from judge Larson's decision:

> “The subject matter of one or more claims of the ENIAC was derived from Atanasoff, and the invention claimed in the ENIAC was derived from Atanasoff. “
>
> “Eckert and Mauchly did not themselves first invent the automatic electronic digital computer, but instead derived that subject matter from one Dr. John Vincent Atanasoff.”
>
> “Between 1937 and 1942, Atanasoff, then a professor of physics and mathematics at Iowa State College, Ames, Iowa, developed and built an automatic electronic digital computer for solving large systems of simultaneous linear algebraic equations.”
>
> “This breadboard model machine, constructed with the assistance of a graduate student, Clifford Berry, permitted the various components of the machine to be tested under actual operating conditions.”

> “The discussions Mauchly had with both Atanasoff and Berry while at Ames were free and open and no significant information concerning the machine's theory, design, construction, use or operation was withheld.”
>
> “Prior to his visit to Ames, Iowa, Mauchly had been broadly interested in electrical analog calculating devices, but had not conceived an automatic electronic digital computer.”
>
> “Eckert and Mauchly did not themselves first invent 'the automatic electronic digital computer,' which Sperry-Rand and ISD contend to be the subject matter of the ENIAC patent, but instead derived that broad subject matter from Dr. John V. Atanasoff, and the ENIAC patent is thereby invalid.”

After thus spelling out the conduct of Mauchly and Eckert in regard to me and my work, judge Larson presented the other side of the picture, including a lack of action by me, and found Honeywell not entitled to antitrust damages for willful and intentional fraud on the Patent office.

In addition to his findings on the derivation of the ENIAC from me, judge Larson now ruled that the Regenerative Memory Patent No. 2,629,827 at issue in the CDC case in Baltimore, was derived from me.

### Disposition of the Two Cases

Everyone expected that judge Larson's decision would be appealed, but rather quickly it was settled by the payment of money and an agreement between the parties that each would support the judge's decision of 1973. I have been told that Sperry-Rand paid $3,500,000, sufficient to reimburse Honeywell for the cost of the trial. Thus ended this important case.

As to the Computer Memory case in Baltimore, I acted as a witness in it after the ENIAC trial was over. The trial in that case was active for only a few days; then it was abandoned from lack of interest of principals or counsel. In 1981, nine years later, some important judge insisted that the case be settled, and so it too was settled by a contractual agreement, this time between CDC and Sperry-Rand. I was told that a payment of money was made here also, by Sperry-Rand, but the amount and other terms were not disclosed. The matter was moot, because the patent had expired.

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## UPDATES

John Vincent Atanasoff died June 15, 1995. (MRW, 2012)