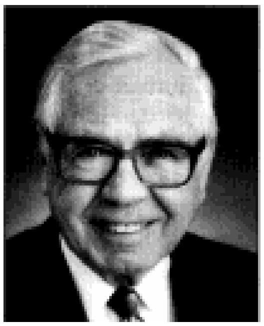
# WALTER M. CARLSON

*Born September 18, 1916, Denver, Colo.; distinguished computer engineer who combined careers in chemical engineering and computing to concern himself and his society with the future of computing and the benefits of the computer*



*Education:* BS, Chemical Engineering, University of Colorado, 1938; MS, Chemical Engineering, University of Colorado, 1939.

*Professional Experience:* manager, Operations Analysis, Engineering Service Division, DuPont Co., 1939-1963; director of Technical Information, US Department of Defense, 1963-1967; IBM Corp.: technical consultant to chief scientist, 1967-1968, marketing consultant, corporate office, 1968-1985; director, Engineering Information, Inc., 1984-1990.

*Honors and Awards:* fellow, American Institute of Chemical Engineers; ACM Distinguished Service Award, 1991.

Walter Carlson joined DuPont's industrial engineering division in October 1939, and after 15 years in process improvement and planning assignments, he set up the organization to install, operate, and program Serial #12 Univac I in August 1954. The group also provided company-wide consultation in statistics, mathematical analysis, quality control, and operations research.

In 1963, he was employed by the US Department of Defense to create the office of director of technical information. At that time, he was chairman of the Engineering Information Committee of the Engineer's joint Council, predecessor of the American Association of Engineering Societies.

In February 1967 he joined the IBM Corporation as technical consultant to the chief scientist, and in June 1968 he became a marketing consultant in IBM's corporate office. He retired from that position in 1985 while covering product development and marketing planning for storage products, printers, copiers, and application software on a world-wide basis.

## UPDATES

Walter Carlson died Aug 21, 2010.

# JOHN WEBER CARR III

*Born May 16, 1923, Durham, N.C.; numerical analyst; founder and first editor* of ACM Computing Reviews.

*Education:* BS, Duke University, 1943; MS, MIT, 1949; PhD, mathematics, MIT, 1951.

*Professional Experience:* University of Michigan: research mathematician, 1952-1955; assistant and associate professor, 1955-1959; University of North Carolina: associate professor and director, Research Computing Center, 1959-1962, associate professor, 1962-1963, professor, 1963-1966, chairman graduate group, Computing and Information Sciences, 1966-1973; Moore School of Engineering, University of Pennsylvania: professor, 1966-present.

*Honors and Awards:* president ACM, 1957-1958; ACM Distinguished Service Award 1975.

## UPDATES

John Carr died of pancreatic cancer at his home in Bryn Mawr, Pennsylvania, April 1997.

# W. W. CHANDLER

*Born December 1, 1913, Bridport, Dorset, England; died September 11, 1989, London, England; a member* of *the General Post* Office *team that developed, installed, and maintained the Colossus machines for the Government Code and Cipher School at Bletchley Park during World War Il.*

*Education:* BSc, London University, 1938.

W.W. Chandler began his career as an apprentice telephone engineer with Siemens Bros. in 1930. He joined the British Post Office Research department in 1936 and obtained a BSc degree from London University in 1938 by private study. Prior to World War II he worked on long-distance signaling and dialing systems of the Post Office telephone network. During the war he was responsible for the installation and maintenance of the Colossus machines at Bletchley Park. After the war he helped develop and install the MOSAIC computer for the Radar Establishment at Malvern and later worked on optical character recognition for the Post Office.

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## UPDATES

# Jule G. Charney

*Born January 1, 191 7, California; died June 16, 1981, Boston, Mass.; with John von Neumann, first introduced the electronic computer into weather prediction in 1950.*

*Education:* PhD 1946.

*Professional Experience:* leader, Meteorology Group, Princeton, 1948-1956; Alfred P. Sloan Professor of Meteorology at the Massachusetts Institute of Technology, 1956-1981.

Jule Gregory Charney, Alfred P. Sloan Professor of Meteorology at the Massachusetts Institute of Technology, died June 16, 1981, at the Sidney Farber Cancer Institute. He was the leading world figure in meteorology ever since he and John von Neumann first introduced the electronic computer into weather prediction in 1950.

Charney was born on January 1, 1917, in California, to Russian immigrants. His original graduate studies at UCLA were in mathematics, but he changed to meteorology in about 1942. The basic principle of numerical weather forecasting is to express the physical laws of atmospheric hydrodynamics and thermodynamics that can be numerically solved by the computer as a step-wise marching process in time.

As a concept this was not new in 1950—it had been outlined in some detail 30 years earlier by the Englishman Lewis F. Richardson (1922). Richardson's test calculation done "by hand," under difficult frontline conditions in World War I, gave very erroneous results, however.

As early as May 1946, von Neumann had envisaged meteorology as a major component of his newly formed Electronic Computer Project at the Institute for Advanced Study (Goldstine 1972; Platzman 1979). Charney's 1946 doctoral thesis had suggested to him that the large-scale circulations in the atmosphere could only be analyzed in a physically appealing and mathematically tractable way, if certain specific approximations were used to distinguish those circulations from sound waves and gravity waves of higher frequency (Charney 1947). After being exposed to von Neumann's hopes for numerical meteorology in an August 1946 meeting (Platzman 1979), Charney spent most of the following year in Oslo. There he extended his ideas and arrived at the "quasi-geostrophic prediction equations" (Charney 1948). These equations predicted only the slow large-scale motions and were free of the sensitivity to high-frequency motion that had plagued Richardson.

On Charney's return he joined von Neumann at Princeton as leader of the Meteorology Group. He then set about answering a series of critical technical questions such as: How important are friction and heating? From how large an atmospheric volume must one have data in order to make a 24-hour forecast for the US? What is the simplest formulation that might have some predictive skill?

The first computations were made in 1950 with the ENIAC and were gratifyingly successful (Charney, Fjörtoft, and von Neumann 1950; Platzman 1979). Similar research was quickly started in other countries, and more elaborate and accurate formulations were used at Princeton as soon as the new IAS computer was ready in 1952 (Goldstine 1972).

With Charney's help, the US Weather Bureau, Air Force, and Navy established in 1954 a joint Numerical Weather Prediction Unit in Suitland, Maryland, for routine daily prediction of large-scale atmospheric flow patterns and weather. The Weather Bureau, also with intellectual encouragement from Charney, soon started a specifically research-oriented group, the Geophysical Fluid Dynamics Laboratory, to use computers for basic atmospheric and oceanic research. Nowadays, computers are used for weather prediction at the 1- to 4-day range in all of the larger industrial nations and many smaller countries. This success has revolutionized other types of meteorological research as well, by emphasizing both the possibility and the responsibility to see that the consequences of hypotheses about the atmosphere are examined quantitatively. The computer has to a marked extent become for meteorologists the equivalent of the laboratory for physicists and chemists.

It must be admitted that these developments would have occurred eventually in the absence of Charney's personal insight. After all, Princeton was not the only center of computer development in the late 1940s, and Charney's quasi-geostrophic equations were being developed independently at that time in England, Norway, and the Soviet Union. It is very doubtful, however, that the first meteorological use of electronic computers would have been as successful elsewhere as it was under Charney and von Neumann. Their immediate success was a profound stimulus to the postwar development of atmospheric science.

In 1956 Charney left IAS to become professor of meteorology at MIT. A stream of major contributions in dynamic meteorology and oceanography came from him in the ensuing 25 years, including studies of the generation of the Gulf Stream, vertical propagation of hydrodynamic energy in the atmosphere, large-scale wave instability, formation of hurricanes, and hydrodynamic effects on desert climate. In the mid-1960s his clear view of the atmosphere as a single physical system, expressed in a report of the National Academy of Sciences (Charney et al., 1966), led to the extraordinary international effort in 1979 known as the Global Weather Experiment. Charney communicated his infectious enthusiasm for understanding the atmosphere and ocean to many students and collaborators, but his inspiring insights will be difficult to match.[1]

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## UPDATES

Portrait inserted, MRW, 2012.

# HAROLD CHESTNUT

*Born November 25, 1917, Albany, N. Y; first president of the International Federation of Automatic Control (IFAC).*

*Education:* BSEE, MIT, 1939; MSEE, MIT, 1940.

*Professional Experience:* manager, Systems Engineering and Analysis, 1940-1966; Information Science Laboratory, 1966-1967; Systems Engineering and Analysis, 1967-1971; consulting systems engineer, Research and Development Center, General Electric Co., 1971-present.

*Honors and Awards:* DEE., Case Western Reserve University, 1966; D Eng., Villanova University, 1972; National Academy of Engineering; president, IFAC, 1957-1959; fellow, Instrument Society of America; president, American Automatic Control Council, 1962-1963; fellow, AAAS. .

## UPDATES

Harold Chestnut August 29, 2001, in Schenectady, New York. Portrait inserted, MRW, 2012.

# DOV CHEVION

*Born April 16, 1917, Lodz, Poland, died October 5, 1983, Jerusalem, Israel, Israeli educator and pioneer computer scientist. Dov Chevion was a giant among his fellow men and women. He was respected for his strength of leadership, for his commitment toward worthy goals, and as the spokesman in the computer field for Israel for over two decades. He was particularly concerned with education, an area in which he made major contributions.*

Dov was born on April 16, 1917, in Lodz, Poland. He was educated in the gymnasium in Lodz and immigrated to Palestine in 1935. He attended the Hebrew University in Jerusalem, where he studied philosophy, mathematics, and physics. His student days were interrupted by World War II, after which he worked in the field of statistics for the British government. He became active in the computer field in the late 1950s and, with Aaron Gertz, was responsible for teaching and training hundreds of computer and communication systems designers, computer analysts, and programmers who now hold senior positions throughout Israel. He left his mark of accomplishment on the computer user community of his country and those other countries that were privileged to benefit from his teaching.

### IPA Contribution

In the early 1960s Chevion, together with some professors of the Weizmann Institute and Aaron Gertz, planned and founded the Information Processing Association of Israel (IPA). He served as chairman of the board from 1966 to 1976 and president from 1976 to 1982. Early in 1983 he was given the tide of Honorable President of IPA for life.

He helped create the first International Jerusalem Conference on Information Technology UCIT) in 1971 and was deeply involved in those that followed in 1974 and 1978. He was an active advisor for JCIT IV, held in May 1984, until his death in October 1983. Just before his death, he was awarded the IPA Certificate for his lifelong efforts for the development of computing in Israel. The Israeli government awarded Chevion the Kaplan Prize in 1972 and 1973 for teaching the blind to work with computers. He always employed blind people in his computing center.

### IFIP Participation

IPA became a member of the International Federation for Information Processing (IFIP) in January 1964, and Chevion was its first representative. He served until 1978, when he was elected an individual member for a 3-year term. He was an IFIP trustee in the periods 1965-1967, 1970-1973, and 1973-1976, and a vice president from 1967-1970. As the chairman of the Future Policy Committee from 1967-1969 and a member of that committee from 1979-1981, he helped focus attention on the development of IFIP as the most important international federation in the computer field. He served as a member of the following IMP committees: Tenth Anniversary Committee 1969-1970, Statutes and Bylaws Committee 1969-1981, IFIP Committee for International Liaison 1969-1983, Technical Committee 3 (Education) 1971-1979, Education Policy Committee 1973-1974, Activities Planning Committee 1975-1981, and Committee for Liaison with SEARCC 1976-1981

Chevion was chairman of Working Group 3.1 (Informatics Education at the Secondary Education Level) from 1966-1968 and chairman of a Nominations Committee for IMP trustees. In 1974 he received the Silver Core award for services rendered to IFIP.

Chevion was a personal contributor as well as an active IFIP proponent of each of the World Conferences on Computer Education and was chairman from 1973 of the steering committee for the Second World Conference on Computers in Education, which took place in 1975. In addition, he organized, participated in, and led Israeli lecture teams on computer education to many countries in Central and South America. In recognition of his singular contribution he was appointed honorary professor of computer science by the University of Sao. Paulo in Brazil.

Obviously, Chevion was one of IFIP's most active contributors and an outstanding organizer of many important IFIP activities, reflecting, in particular, his devotion and dedication to education about computing and information processing. He was responsible for developing computer curricula for schools, teacher training, and information booklets. These efforts, plus his ability to get people to work together, will be acknowledged for a long time.

### IPA Fellowship Program

In November 1965 Chevion suggested a joint fellowship program between one of the Auerbach corporations and IPA to increase the knowledge, experience, and productivity of the people working in the field of computer technology in Israel. Young computer professionals agreed to accept a position for one year in the US so they could gain technical experience, after which they would return to Israel to share their knowledge.

After providing the success of the fellowship program, IPA was able to expand the program to five other US companies in the data processing field. A total of 30 fellows have participated in the program to date.

### Final Days

In 1977 Chevion stimulated the General Assembly's interest in governmental and municipal data processing to the extent that he was named chairman of an IFIP task group to investigate the topic. The first IMP conference on the Impact of New Technologies on Information Systems in Public Administration in the 1980s was held in Vienna in February 1983. Chevion presented a paper entitled “International Cooperation as a Vehicle of Information Technology in Public Administration.” He returned home from the conference and underwent major surgery in March. He died in Hadassah Hospital on October 5, 1983, with his family present.[1]

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[1] From Auerbach 1985.

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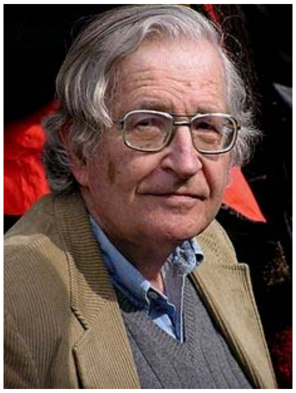
## UPDATES

# Noam Chomsky

*Barn December 7, 1928; mathematical linguist who is responsible for the hierarchy of grammars that bears his name; recipient of the 1988 Kyoto Prize in Basic Science.*



Chomsky's work on the syntax of languages coincided neatly with the early development of programming languages and thus his work found ready application to the more formal style of artificial language than those of his original interest—natural languages. The recognition of a hierarchy of syntactic forms each properly subsetted inside the next and each representing a particular language style also matched some of the lower levels of programming language elements. The Chomsky hierarchy places regular (or linear) languages as a subset of the context-free languages, which in turn are embedded within the set of context-sensitive languages also finally residing in the set of unrestricted or recursively enumerable languages. By defining syntax as the set of rules that define the spatial relationships between the symbols of a language, various levels of language can be also described as one-dimensional (regular or linear), two-dimensional (context-free), three-dimensional (context sensitive) and multi-dimensional (unrestricted) relationships. From these beginnings, Chomsky might well be described as the “father of formal languages.”

Like Edmund Berkeley, Chomsky became embroiled in the peace movements of the 1960s and so divided his efforts between his linguistic studies and his social concerns. His publications since 1960 have been divided between the two subjects.

**QUOTATIONS**

“The fundamental aim in linguistic analysis of language L is to separate the grammatical sequences which are sentences of L from the ungrammatical sequences which are not sentences of L and to study the structure of grammatical sequences.” (1957)

“The notion grammatical cannot be identified with meaningful or significant in any semantic sense. Sentences (1) and (2) are equally nonsensical, but any speaker of English will recognize that only the former is grammatical:

(1) Colorless green ideas sleep furiously.
(2) Furiously sleep ideas green colorless.” (1957)

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## UPDATES

Portrait inserted, MRW, 2012.

# ALONZO CHURCH

*Born June 14, 1903, Washington, D. C.; mathematical logician, creator of the Lambda Calculus who contributed the Church-Rosser theorem to the study of computer science.*

*Education:* AB, Princeton University, 1924; PhD, mathematics, Princeton University, 1927.

*Professional Experience:* national research fellow in mathematics, Harvard University, 1927-1928; University of Göttingen and University of Amsterdam, 1929; Princeton University, assistant professor, professor, 1929-1967; UCLA, professor, philosophy and mathematics, 1967-present.

*Honors and Awards:* DSc, Case Western Reserve University member, 1969; National Academy of Science.

## QUOTATIONS

"Our subject is logic—or as we may say more fully, in order to distinguish from certain topics and doctrines which have (unfortunately) been called by the same name, it is formal logic." (Introduction to Formal Logic, 1956)

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Church, Alonzo, "Introduction to Mathematical Logic." Princeton University Press, Princeton, 1956.

## UPDATES

Alonzo Church died August 11, 1995. Portrait inserted, MRW, 2012.

# ARTHUR C. CLARKE

*Born December 16, 1917, Minehead, Somerset, UK; science fiction writer who "invented" HAL in the movie* 2001: A Space Odyssey. *Originator of the concept of communications satellites.*[1]

*Education:* Huish's Grammar School, Taunton, 1927-1936; BSc, first class honors, physics and mathematics, King's College, London, 1946-1948.

*Professional Experience:* Auditor, H.M. Exchequer and Audit Department, 1936-1941; Royal Air Force: Instructor, No. 9 Radio School, then Flight Lieutenant, Ground Controlled Approach Radar[2] 4 1941-1946; assistant editor, *Physics Abstracts*, IEE, 1949-1950; self-employed author, 1950-present; chancellor, University of Moratuwa, Sri Lanka, 1979-present.

*Honors and Awards:* honorary fellow, British Interplanetary Society; honorary fellow, American Astronomical Association; academician, World Academy of Art and Science; honorary fellow, International Academy of Astronautics, 1960; Stuart Ballantine Medal, Franklin Institute, 1963; fellow, Franklin Institute, 1971; DSc (Hon.), Beaver College, Pennsylvania, 1971; Aerospace Communications Award, American Institute of Aeronautics and Astronautics, 1974; honorary fellow, American Institute of Aeronautics and Astronautics, 1976; Bradford Washburn Award, Boston Museum of Science, 1977; fellow, King's College, 1977; DSc (Hon.), University of Moratuwa, 1979; Engineering Award, Academy of Television Arts and Sciences, 1981; fellow, Institute of Robotics, Carnegie Mellon University, 1981; Marconi International Fellowship, 1982; honorary fellow, Institute of Engineers, Sri Lanka, 1983; Centennial Medal, IEEE, 1984; foreign associate, National Academy of Engineering, 1986; Charles A. Lindbergh Award, 1987; associate fellow, Third World Academy of Sciences, 1987; Hall of Fame, Society of Satellite Professionals, 1987; DLitt, University of Bath, 1988; fellow, International Aerospace Hall of Fame, San Diego, 1989; fellow, International Space Hall of Fame, Alamagordo, N.M., 1989; R. A. Heinlein Memorial Award, National Space Society, 1990; honorary life president, UN Association of Sri Lanka, 1990; honorary fellow, Ceylon College of Physicians, 1991; International Science Policy Foundation Medal, 1992; freeman, Town of Minehead, 1992.

Arthur C. Clarke was born in the small Somerset town of Minehead, not far from Exmoor, the site of the story of *Lorna Doone*, in 1917. He was educated at Huish's Grammar School, Taunton. Clarke entered H.M. Exchequer & Audit Department in 1936, and served in the Royal Air Force during World War IL While operating the prototype Ground Control Approach radar system, he conceived the basic theory of communication satellites, and published the concept in 1945.

After demobilization, he took a first class honors degree in physics and mathematics at King's College, London, which later elected him as a fellow. From 1948 to 1950 he was assistant editor of *Physics Abstracts*, a publication of the Institution of Electrical Engineers. Twice he was chairman of the British Interplanetary Society-1946-1947, and 1950-1953.

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[1] See Wireless World, October 1945.

[2] Developed by MIT Radiation Laboratory.

Since 1954 his interest in underwater exploration has taken him to the Great Barrier Reef of Australia and the Indian Ocean; he is now a director of the Colombo-based “Underwater Safaris.”

He has published more than 70 books and made many appearances on radio and television, most notably with Walter Cronkite on CBS during the NASA Apollo missions. His 13-part “Mysterious World” and “Strange Powers” television programs have been seen worldwide, and reappear frequently on PBS in the US.

He is a council member of the Society of Authors, a vice president of the H.G. Wells Society, and a member of many other scientific and literary organizations. He was nominated for an “Oscar” for the screenplay of 2001: A *Space Odyssey*.

Clarke has lived in Sri Lanka for the past 30 years, and in 1979 was appointed Chancellor of the University of Moratuwa by President Jayewardene. The university, near Colombo, is the location of the government-established Arthur C. Clarke Centre for Modern Technologies, specializing in communications and computers. He is also chancellor of the International Space University.

In 1989 H.M. Queen Elizabeth awarded him a CBE for “services to British cultural interests in Sri Lanka.” On returning to the UK in 1992 for his 75th birthday celebrations, he was made the first Freeman of his hometown, Minehead, Somerset.

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## UPDATES

Arthur C. Clark died 19 March 2008. Portrait inserted, MRW, 2012.

# JOSEPH CLEMENT

*Born 1779, Great Ashby, Westmoreland, UK; died 1884; Babbage's chief mechanic for the Difference Engine.*

**Introduction**[1]

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 work-shop 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 (Hyman 1984) 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. Clement was not simply a run-of-the-mill machinist who happened to be fortunate enough to work for Babbage, and who 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 he was a highly respected member of the mechanical engineering community when Babbage first contacted him and when 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 (August 19, 1863 and August 26, 1863) to a certain George Clowes[2] .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.[3] The article concerned the life of Babbage's chief mechanic and draftsman Joseph Clement. In the first letter (August 19, 1863) Babbage offers some corrections to the proofs and says "he is too busy to do more but will pass the proofs on to Mr. Wilmot Buxton, who fully comprehends the subject."[4] In the second (August 26, 1863) Babbage begins:

> I enclose a letter from Mr. Buxton which it may be interesting to Mr. Smiles to see. The substance of it or any extracts are at his service but I have not permission to publish the writer's name. I wish it to be returned to Dorset St.

and then continues to give a summary of his dealings with Mr. Clement. We conclude that these letters relate to a book called *Industrial Biography* written by Samuel Smiles in 1863 (Smiles 1882).[5] It contains not only a chapter on the life of Joseph Clement, quite evidently the one for which Babbage had been given the proofs, but

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[1] From Williams 1992.

[2] 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 does not know how they came into the possession of Hartree.

[3] 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.

[4] Mr. Wilmot Buxton was Babbage's friend and, after Babbage's death, produced a biography which has only recently been published. It was not unusual for Buxton to deal with matters relating to Babbage's engines; for example, he was the person who explained the working of the first Difference Engine when it was displayed in the Exhibition of 1862.

[5] Smiles first wrote Industrial Biography in 1863. It subsequently had at least sixteen other editions published in both Britain and America. There also exist at least two editions in Spanish. Further information on Smiles can be obtained from examining his autobiography and the work of Timothy Travers.

also an appendix consisting of a lightly edited version of the letter (August 26, 1863) I had seen in the Fitzwilliam Museum. The book is typical of the Victorian writings designed to inspire the populace with successful tales of hard-working individuals who have made their own way in life, and, in what follows, I have attempted to eliminate the most obvious inspirational tales and concentrate on what I believe to be the more factual information.[1]10 Unfortunately, Smiles, who occasionally mentions the papers Clement left behind, does not give any information as to the location of any papers or relics.

**Joseph Clement (1779-1844)**

Joseph Clement was born in Great Ashby in Westmoreland (northern England) in 1779. He came from simple stock; his father was a weaver by profession, with a strong interest in nature. It is reported that his hobby was a beetle and insect collection and that he set up a number of bee hives near his loom so that he could watch their activities without leaving his work. Joseph appears to have inherited at least some of his mechanical ability from his father, as Old Clement was known to have a lathe that he used both for recreation and for turning items, such as bobbins, that he needed in his profession. While no portrait appears to exist of Joseph Clement, he is described as a heavy-browed man without any polish of manner or speech. He had a very heavy North Country accent, developed in his youth, which never left despite his living most of his adult life in other areas of Britain. Although he did attend the village school, it was only to master the rudiments of reading and writing, and he was almost illiterate as an adult. Indeed John Herschel (the son of the great astronomer and himself an astronomer of note), a friend of Babbage, actually comments on the impression that Clement gave in matters of business: while Babbage was away in Europe, Herschel was looking after the business side of the construction of the Difference Engine and wrote to Babbage saying (December 22, 1827):

> ... C. told me he must have £150, which he (after much hesitation and what at first I took for reluctance, but which I fancy to be the mere consequence of his singular slowness of thought) put into writing in the following form which is a curiosity at least as great as the Engine will be when done.
>
> This is to certify that eight men have been employed on Mr. Babbage's calculating machine this five weeks past. I have likewise got a man to assist me in the drawing. Joseph Clement December 5th 1827. I want £150 to enable me to go on.

After his short schooling, Clement worked with his father at the weaver's trade, but the increasing mechanization of that industry soon led to his seeking alternate employment. From age 18 to 23 he apparently worked as a thatcher and then a slater in the area around his village. As this trade did not occupy him full time, he became friends with the village blacksmith; together, they produced a lathe which Clement used to manufacture various instruments such as flutes, clarinets, and Northumberland bagpipes. Clement's cousin, a watch and clock maker who had spent some time in London, lent him some books on mechanics and he used these as a resource guide to construct a microscope for his father's use in his hobby of collecting insects, and a reflecting telescope. Anyone with this level of talent was not likely to stay as a part-time village thatcher and, sometime in 1805, he left to take a job in the nearby town of Kirby Stephen, where he was employed in the construction of looms at a wage of 3 shillings 6 pence per day. He apparently lived with his employer, a Mr. George Dickinson, to whom he sold the telescope for the sum of £12. In late 1805 he moved to Carlisle and, in

[1] Unless otherwise noted, all personal information concerning Clement in this paper is based on the contents of the 1882 edition of Industrial Biography (Smiles 1882).

1807, to Glasgow. While in Glasgow he happened to meet a Mr. Peter Nicholson, a writer of popular woodworking books at the time. Mr. Nicholson lent Clement one of his drawings of a power loom and Clement, although he had no training, copied it so expertly that Nicholson had difficulty in telling the original from the copy. The writer was so impressed that he gave Clement a series of free lessons in technical drawing. After a year in Glasgow, Clement again moved, this time to Aberdeen, where he was employed by Leys, Masson & Co. in the construction of power looms. Clement evidently found the tools inappropriate, for he constructed a turning lathe with a sliding mandrill[1] and guide screw,[2] and a device for correcting errors in the guide screw. He also produced a special tool holding device, the slide rest, which, although not unknown earlier, was one of the earliest in Britain. During this time his wages rose from one and a half guineas per week to three guineas (1 guinea = 21 shillings), showing that his employers were quite pleased with the quality of work he was able to produce. Not content with simply advancing his employers' stock of tools, he evidently enrolled in the Marischal College in Aberdeen where he attended at least one course in Natural Philosophy during the 1812-1813 term. Later in 1813 he took his savings, which had amounted to almost £100, and moved to London. After spending a few weeks in an ordinary machine shop, he moved to one of the most famous engineering firms of the day, that of Joseph Bramah, famous for engineering feats ranging from hydraulic presses that could lift 1,144, tons, to the construction of continuous-process papermaking machines. Clement was placed in charge of the tools in the shop and quickly distinguished himself in not only improving the tools, but in organizing and improving the flow of work. On April 1, 1814 Bramah and Clement signed an agreement in which Clement was to be the chief draftsman and superintendent of Bramah's main works at Pimlico. Unfortunately Bramah died on December 9, 1814, and his sons, who returned from college to take over the business, did not see Clement as indispensable as had, apparently, their father. The contract was broken and Clement joined the firm of Maudslay and Fields as their chief draftsman. Finally, in 1817, he struck out on his own and set up a small shop in Prospect Place, Newington Butts, where he advertised himself as a mechanical draftsman and manufacturer of small machinery requiring first class workmanship.

Some indication of Clement's talent can be taken from the fact that he often produced the illustrations of mechanical machinery in the *Transactions of the Society for the Encouragement of Arts*. Between 1817 and 1832 his tiny signature can be found in the corner of many of the best drawings of such complex items as theodolites, complicated drawing machines, and lathes. In the process of his technical drawing work, he often had to produce both paper drawings and copper engravings of perspective views of circles, ellipses, and other complex items.

In 1827 the Society of Arts again gave him their Gold Medal, and in 1828 their Silver Medal, for the invention of several improvements to lathes, the most important of which was a device to change the speed of rotation of the work as the tool came closer to the axis of rotation, as it might when turning the surface of a large flat disk. Another of his successes was the development of a very large and accurate planing machine by means of which the surfaces of metal plates of large dimension could be finished to a fine tolerance. Although he never attempted to patent this device, a full description was published (Varley 1832) but, perhaps because of his

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[1] A mandril, now spelled 'mandrel' is the spindle running through the center of the headstock. It is turned by the driving pulley to which it is keyed and at one end holds the work with a faceplate or chuck. The meaning of 'sliding mandril' is not clear. It may imply a two-part spindle in which the inner cylinder, keyed to rotate with the outer cylinder, can be slid in and out, toward and away from the work. This might well be an example, as suggested by the editor of this Department, of Babbage simply making up terms to express his thoughts whereas Clement would have been in the habit of using the accepted terminology of the mechanical engineering practice of his day.

[2] The guide screw, also 'feed screw' or 'lead screw,' is a long screw with rectangular threads mounted parallel to the main axis of the lathe by which the tool holder on the slide rest is moved longitudinally. If used to cut screw threads, its pitch must be precise and uniform.

limited skill at writing, it was not done by Clement himself. Another of his inventions which, at the time, was highly regarded, resulted from his need to produce large accurate drawings for Babbage's projects. He therefore designed and constructed a special drawing table which, because of its size, required an intricate and adjustable set of supports.

His list of inventions and improvements is impressive, but none has had more impact than his attempt, started in 1828, to produce screws and bolts with standard diameters and with threads of a standard shape and pitch-essential elements for interchangeability. Although his campaign in this area did not immediately result in success, his best journeyman at the time, Mr. (later Sir) Joseph Whitworth, was the man who ultimately established the standard Whitworth thread, which dominated British machine practice for almost 175 years until replaced by the metric standard. Whitworth left Clement's shop when the demand for work on Babbage's Difference Engine came to an end. He moved to Manchester and within a few years became the foremost precision machinist in Britain. In later years, Charles Babbage even had some correspondence with Whitworth, who had offered to undertake the actual construction of his Analytical Engine, but this was never taken beyond the initial stages of discussion. As Hyman (1984) speculates, Whitworth, having seen the effects of the Difference Engine project upon the advancement of the tool-making industry, may well have thought it worth undertaking the construction of the Analytical Engine just because of the potential 'spin-offs' it might produce. When Clement died in 1844, the business was continued by his nephew. The new owner was evidently not as good a workman as his uncle had been and the business gradually faded away.

### Clement-Babbage Relationship

In 1823, when the Government grant of £1500 made it feasible for Babbage to think seriously about constructing his Difference Engine, he needed a proper draftsman and mechanic. In one of the letters mentioned earlier (August 26, 1863 ) he states:

> At the commencement of the Diff. Engine I wanted a person to assist me in the drawings and afterwards if necessary to construct the Engine. The late Sir Isambard[1] Brunel recommended Mr. Clement as likely to suit me. Mr. Clement then possessed one lathe small but very good having also an excellent screw. His workshop at that time was his front kitchen. The lathe had also a very valuable slide rest rather too large for it. His small stock of tools were all excellent of their kind. As soon as any part of the Diff. Engine was contrived and drawn, I proceeded to ask Clement what was the perceived mode of making it. The plans then in use were tedious and demanded the skill of the very best workmen. Now as I required an identity amongst hundreds and even thousands of similar parts the then existing methods were insufficient. I suggested special tools and by the aid of Clement's admirable skill and availing myself of his practical knowledge arranged the plan of several new machine tools. So far was the 1st Diff. Engine from not [sic] having a printing apparatus that I well remember the discussion I had with Clement on its first suggestion. I remarked that I required what I should call a coordinate machine which I explained to Clement. I then sketched two slides at right angles to each other and proposed to place these vertical slides at the back of a lathe having a cylindrical mandrel. This plan was adopted and at once gave us the command of the use of circular cutters, drills, and saws over every part of the plane. Copper plates were inscribed by steel figure punches on this machine. Again I required for making the bolts the vertical motion of an horizontal plane upon a slide rest. I proposed inclined planes as the

[1] Note that this reference is to Sir Mark Isambard Brunel, not his son, Isambard Kingdom Brunel, although by the time this letter was written they were both dead.

> simplest method but Clement preferred four screws driven by a central wheel which probably was the best of the two plans. Under the demands of the Diff. Engine the number of machine tools increased and Mr. Clement converted a large building at the back of his house into a workshop. The improvements in tools are mainly due to the stronger system of iron framing now employed; circular cutters, slides, slotting, and even planing machines, adapted to ornamental turning may be found drawn and described in several of the older authors in that subject.

It would appear that Babbage, either through the haze of years or because of a lasting feud, could not accurately remember his first dealing with Clement nor the state of Clement's establishment. This letter makes it look very much like Clement was a poor man who happened to have the luck of being recommended to Babbage. The fact that Clement possessed only one small lathe was not an indication that his business was not a success, but rather than he was in a specialized shop dedicated to high quality workmanship. We have seen how, in earlier years, he had actually made several lathes of very high quality and was fully used to constructing any tool that he needed. Again, Babbage's remark that his workshop was in his front kitchen would also imply a certain lack of success and sophistication. Clement was, as amply demonstrated by Herschel's remark above, not a sophisticated man, but he was, certainly successful and well respected in his craft when Babbage first met him. The fact that he was good enough to have already won his Society's Gold Medal (an honor of which Babbage, who received the Gold Medal of the Astronomical Society in 1824, was always very proud) seems to have been overlooked in any consideration of Clement's contributions to the construction of the Difference Engine. The latter part of this letter seems to be, other than the first few sentences, in accordance with what must have been the actual working relationship. Babbage appears to give Clement credit for some of the advances in the machine tools and methods of construction and the description of their working relationship (essentially Babbage suggesting a special tool and Clement agreeing that one was needed but making it to his own design) rings true.

In fact Babbage completely abdicated the office of design engineer for the Difference Engine and, at least for a time, left entirely in Clement's hands the technical details of how it was to be implemented. During the time that Babbage went off on his trip to Europe, leaving Herschel in charge of the Difference Engine project, the two friends wrote several letters back and forth. The majority of the contents of these letters concern the changes that Clement had made to the detailed design of the Difference Engine. Herschel spends a lot of effort, including drawing diagrams, trying to explain to Babbage these modifications. Herschel, whatever his qualifications in other areas, was not good at explaining complex mechanical questions and, at the end of one letter (December 22, 1827) is forced to admit:

> On reading this description I see that it is unintelligible and what is worse that I can't mend it. I had it all there before my mind when I left Clement, and last night when he called here I rehearsed it to him, and now I am puzzled, but you will see the principles from this....

Again, when describing some of the work that had taken place, Herschel tells Babbage about some new drawings containing (February 12, 1828):

> ... in part a new scheme of Clement's who says you left it to him to plan as well as he could.

Babbage's reply shows that he had complete confidence in Clement (May 9, 1828):

> I left Clement so well acquainted with all the mechanical actions of the machine that I have not the least fear of his making such changes as he may think necessary.

Later, in the same letter, he again demonstrates the fact Clement had been given a free hand when he indicates that Clement had made very major changes to the mechanical motions:

> The plan for locking the axes in their places until the wheels are ready to be bolted seems to me better than rollers which latter I never intended and know not how they got into the drawings.

Although not absolute proof, these quotations imply that Clement had a much larger hand in the actual detailed design of the Difference Engine than has previously been appreciated. The one other major factor in the Babbage-Clement relationship was the fact that, when the working relationship deteriorated to the point where they parted company, Clement took all the tools and drawings that he had made for the Difference Engine project and left Babbage without the means of continuing. There is no doubt that Clement had a legal right to take the tools, but the fact that he did so has caused him to have a lot of bad press. It would appear that both Babbage and Herschel were not unaware of this potential problem. In the letters between Herschel and Babbage, mentioned above, are a number of remarks which seem to indicate a growing unease with Clement's activities. Just prior to Christmas 1827, Herschel wrote to Babbage about the progress of the project (December 22, 1827):

> I went over to him [Clement] to see how he gets on, at the early part of the week. He has been making a new drawing to show the effect of certain alterations he recommends (which I shall explain presently) I am so new to the scale of daily weekly and monthly progress that such work ought to make, that I will confess to you it seemed to me not to have got on too much since you left.

A few weeks later he again writes to Babbage (February 12, 1828)

> Clement ... tells me he keeps 10 men constantly at work on the Engine. I saw 7 there in the workshop and one man working on the drawings when I last called. He seems to have been chiefly at work on the drawings ... he has drawn upon me for £200 more making in all £350 since you left England. Verily it makes not much show, but I am a sad novice in matters of workmanship and I suppose the secret is that good work is not cheap.

Having received no answer from Babbage,[1] Herschel, apparently getting more concerned about several aspects of the project, writes again (April 10-17,1828):

> Clement has drawn on me for £200:0:0 more making in all £550 since you left. It may be that £550 worth of work is done, but I confess it makes marvelous little show for the money.

Babbage eventually wrote back (May 9, 1828):

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[1] It is not surprising that Babbage had not yet answered as Herschel had simply sent the letters to Rome and Florence knowing that these cities were on Babbage's route. Babbage did receive the letters, but only after a delay.

> I have just received yours of the 10-17 April. The accounts you gave me of the progress of the machine are by no means discouraging as to the work done and the drawings. It is a species of work which makes but little show. As to the actual steel and brass cut up for my use I fear it is little and that Clement is spending much time in making tools. This is to a certain extent necessary and requires considerable supplies of money but I should wish you incidently [sic] if possible to find out whether it is not Clement's intention to make me pay for the construction of these tools and then to keep them as his own property. From the multitude he is making it looks so.

Babbage had obviously become more than just a little suspicious. Considering this early suspicion, and the fact that Babbage had access to the best of advice from very knowledgeable friends and associates, it is interesting that he let the problem develop to such an extent that it eventually put him out of business.

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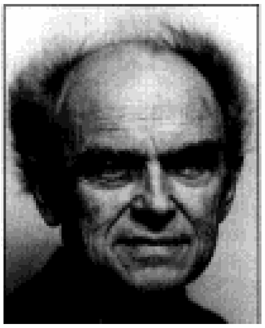
## UPDATES

# Richard F. Clippinger

*Born 1913, East Liberty, Ohio; computing laboratory staff member, Aberdeen Proving Ground, who converted the ENIA C to a stored program computer using its read-only hand-set function tables.*



*Education:* PhD, mathematics, Harvard University, 1940.

*Professional Experience:* ballistic research laboratory, Aberdeen Proving Ground, 1944-1952; Raytheon Computer Laboratory (later Datamatic Corporation, and later still EDP Division of Honeywell), 1952-1976.

Clippinger went to the Ballistic Research Laboratory at Aberdeen Proving Ground in 1944. There he invented and developed the closed-chamber firing range, which rivaled the wind tunnel for measuring forces on a supersonic model. At Aberdeen he also worked in the development of numeric methods for solving ordinary and partial differential equations on the ENIAC, EDVAC, and ORDVAC. In 1952 he joined the Raytheon Computer Laboratory, which became Datamatic Corporation in 1954 and the EDP division of Honeywell in 1956. He was in charge of software development for the Honeywell 800 family until 1959 when he supervised the development of the FACT business language compiler by Computer Sciences Corporation. He became Honeywell's representative to CODASYL when it was created.

He chaired the ANSI and ISO language-standardization committees, and retired from Honeywell in 1976. Currently he consults on doing color graphics on a Macintosh Quadra.

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## UPDATES

Dick Clippinger died 24 December 1997.

## John Cocke

*Born May 25, 1925, Charlotte, N. C.; computer scientist who specializes in compiler optimizations techniques.*

*Education:* BS, mechanical engineering, Duke University, 1946; PhD, mathematics, Duke University, 1956.

*Professional Experience:* IBM research, 1956-1993; fellow, IBM, Yorktown Heights.

*Honors and Awards:* IEEE Computer Society Pioneer Award, 1989; ACM/IEEE Eckert-Mauchly Award, 1985; ACM Turing Award, 1987; National Medal of Technology, 1991; National Medal of Science, 1994.

Burnout has never bothered John Cocke, the inventor of reduced-instruction-set computer (RISC) technology. His interest in all parts of the computer business and his ability to "always find something a little different" to engage his attention have led to some 22 patents. Besides those for RISC technology, his patents cover logic simulation, coding theory, and compiler optimization. Inventing is something Cocke does with great enthusiasm. A self-motivator, he does not feel acceptance or acclaim are important for motivation. In fact, he enjoys discovering his mistakes because "that is when you learn something." But he recalls no major failures in his career.

Cocke's approach to solving problems is not guided by rules or any particular philosophy. He feels that solutions come through continuous work and does not remember ever having had dramatic flashes of inspiration. Claiming that he is clumsy at using a keyboard or a mouse, he prefers a pencil and paper or a blackboard. He also told *IEEE Spectrum* that he is more diagram—than word-oriented.

His most productive period, Cocke feels, was when he was about 35 and "wildly interested in computers." At that time, he had the opportunity to work in the laboratories of IBM Corp. with such luminaries as Frederick P. Brooks Jr., now Kenan Professor of Computer Science at the University of North Carolina, Chapel Hill. Cocke describes those days of freedom in thinking, when there were few known procedures at IBM, as energizing, but, ever self-effacing, said he was 'Just there to learn."

Cocke's keen intellect is combined with an avid curiosity and an ability to totally immerse himself in a technical challenge, according to colleagues. "The smartest man I ever met, " said Joel S. Birnbaum, vice president and general manager of the information architecture group at Hewlett-Packard Co., who was once interviewed for a job by Cocke at IBM and subsequently worked there. Lewis M. Branscomb, director of science technology and public policy at Harvard University, Cambridge, Mass., and a former IBM chief scientist, describes Cocke as "one of the very few people I know whose IQ is higher than his blood cholesterol level."

Cocke's interest in inventing was sparked at an early age by an uncle's comment that you could catch a bird by putting salt on its tail. But his attempts to develop an effective salt sprayer failed. He also experimented early on with a device to wash windows. This invention worked. By moving an electromagnet on the inside of a window, he was able to cause a piece of iron attached to a cloth to move in tandem on the outside. Another of his first experiences with "inventing" was a hydraulic pipe wrench. Its jaws were opened and closed by pressing a button each time. But he discovered that such a wrench had already been patented in 1890.

Born May 25, 1925, in Charlotte, N.C., Cocke was the youngest of three sons. His father was chairman of the board of a local power company. Because Cocke was not a good student in grammar school, his mother had to have him tutored. When he got to high school, he did a little better by studying general science and physics, courses he took to avoid taking Latin, a subject he considered “too difficult.” The mathematics he felt he could handle without a lot of study, he told *Spectrum*.

He fared even better at Duke University, Durham, N.C., where he received a bachelor's degree in mechanical engineering in 1946. His courses in engineering and physics were selected because “they were easier” than art courses, which he felt would have been “too difficult memorizing hundreds of paintings and painters.”

As a student at Duke, he had been in the US Navy's V-12 program and was called back into the Navy in 1952. In the interval from 1946 to 1952, he held several jobs, including one with a heating and air conditioning company and another with General Electric Co.'s high-voltage laboratories.

Cocke returned to Duke in 1954. While there, he took a summer job at Patrick Air Force Base in Florida, where he designed a Monte Carlo program to determine the optimum number of aircraft required for delivering supplies to the Bahamas. After receiving a doctorate in mathematics from Duke, he joined IBM in 1956. The decision to go with IBM at that time, he said, was a lucky one: it put him where the action was in computer development.

Over the years his work habits have changed. When he was younger, he arrived at work late and stayed late, principally to have access to a computer, a scarce resource in those days. He often stayed up all night, he said, so satisfying was it to get a lot done. Now, because he needs “to sleep at night,” his work hours are more routine.

In his younger years, too, Cocke used to ski, play golf, and unicycle. He has never been a game-playing type, he told us. He prefers, for example, to speculate on how to build a chess-playing machine, rather than to play chess itself.

Cocke's successes have been recognized at the highest levels. In September President Bush named him a recipient of the 1991 National Medal of Technology “for his development and implementation of Reduced Instruction Set Computer (RISC) architecture that significantly increased the speed and efficiency of computers, thereby enhancing US technological competitiveness.”

In 1987 Cocke received the Turing Award from the Association for Computing Machinery-the group's highest honor for technical contributions in computing. In 1990 he was the first to receive the US $100,000 IBM John E. Bertram Award for sustained technical excellence. In making the award, IBM chairman John F. Akers said, “John has that rare ability to understand and synthesize both hardware and software concepts, optimize the design of both, and produce a unique synergy.”

In 1991, a group of Cocke's colleagues held an all-day symposium celebrating his 35th year with IBM. Some attendees also participated in a videotape, “John Cocke: a retrospective by friends.” On the tape, Abraham Peled, now IBM's Research Division vice president and director of computer sciences, remembered being interviewed by Cocke. “John asked what my thesis topic was,” he said. “After I had talked for about 5 minutes on the topic-digital signal processing—he went to the blackboard and more or less wrote out a major part of my thesis. It was a rude awakening. "[1]

In the announcement of the award of the National Medal of Science in 1994, the National Science Foundation attributed the award to "his contributions to computer science in the design and theory of compilers and for major advances in the theory and practice of high-performance computer systems. RISC machines are the essential building blocks for today's high-performance parallel machines. Cocke's thinking and technical leadership has been widely credited for setting the tone for these developments. The RISC concept is a stunning unification of hardware architecture and optimization compiler technology and John Cocke had the total mastery of both fields to have made the RISC breakthrough."

## QUOTATIONS

In reflecting on his career, Cocke is self-critical: "Things have always taken too long," he said.

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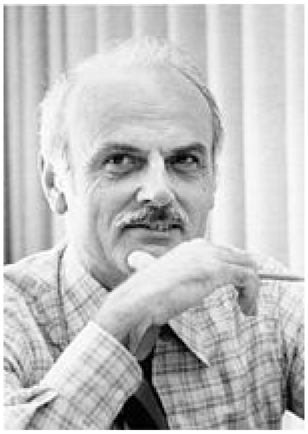
John Cocke died July 16, 2002. Portrait added (MRW, 2012).

[1] From Jurgen 1991.

# Edgar Frank Codd

*Born August 19, 1923, Portland, UK; invented the first abstract model for database management, as a whole undertaking, including retrieval, manipulation, logical integrity constraints, views and view updatability, and the management of distributed databases with distribution independence; recipient of the 1981 ACM Turing Award.*



*Education:* BA and MA, mathematics, Oxford University, 1948; PhD, communication sciences, University of Michigan, 1965.

*Professional Experience:* captain, Royal Air Force, 1942-1945; instructor, mathematics, University of Tennessee, 1949; IBM Corp.: mathematician/programmer, SSEC,16 1949-1951, designer, IBM-701 and IBM-702, 1951-1952, designer, STRETCH (IBM-7030), 1957-1959, creator, STEM, 1957-1961, developer, Relational Database Model, 1970-1981, retired, 1984; Computing Devices of Canada,1953-1957.

*Honors and Awards:* fellow, British Computer Society, 1974; fellow for life, IBM, 1976; Turing Award, ACM, 1981; member, National Academy of Engineering, 1983; fellow, American Academy of Arts and Sciences, 1994.

Codd joined IBM in June 1949 after a short stint at the University of Tennessee, and began his professional career in the computer industry as a mathematician and programmer for the SSEC[1] in New York City. As IBM moved into the computer field more solidly he worked on the logical designs of the IBM 701, initially named the "Defense Calculator," and the IBM 702, which was the first machine designed for business use rather than scientific computations. After four years in Canada he returned to IBM at the time of the development of the STRETCH system (IBM 7030) and created the first multiprogrammed control system capable of managing the interleaved and concurrent execution of programs designed independently of each other-STEM.

On leave from IBM for four years, he completed his PhD at the University of Michigan and presented a thesis on the topic of a self reproducing computer consisting of a large number of simple identical cells, each of which interacts in a uniform manner with its four immediate neighbors. Codd reported this work in a book entitled *Cellular Automata* published by Academic Press in 1968.

Returning to IBM after the announcement of System/360, but at the beginning of the push for the development of a universal language which would match in software the basic concepts of the 360 line of hardware, he backed the IBM laboratory in Vienna (Zemanek, Lucas, et al. 1965) to create a formal definition of the language PL/l. This language became known as the Vienna Definition Language (VDL).

He began work in 1969 on the relational model for database management, a project which he continued to promulgate for the next 12 years within IBM, although the corporation was less than enthusiastic about the work. Eventually, in 1982, IBM announced the availability of SQL/DS, a database management system (DBMS) based on the relational model, intended for mid-size systems. The following year a system for large scale computer systems, DB2, also based on the relational model, was released.

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[1] Selective Sequence Electronic Computer.

Since retirement from IBM in 1984 at the age of 61, Codd has established two companies to provide world-wide lecturing and consulting services to vendors and users of database management systems, and continues to write technical papers in response to ill-conceived criticisms of the relational model.

In a private communication, Codd provided an outline of what he considered to be his 10 major technical contributions to the field:

- multiprogramming;
- self-reproducing computers;
- the relational model for database management, version 1;
- the Rendezvous project for the casual user of a Relational DBMS;
- the Tasmanian version of the Relational Model RM/T;
- a system for managing Bill-of-Materials applications;
- 12 rules for distinguishing RDBMS from non-relational DBMS;
- the relational model for database management, version 2;
- 12 rules for Repositories;
- developed the new model for DELTA for business specification and management.

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## UPDATES

Ted Codd died April 18, 2003 (MRW, 2012)

# ARNOLD A. COHEN

*Born August 1, 1914, Duluth, Minn.; an employee of Engineering Research Associates (ERA) from its inception in 1946, and a founder of AFIPS; his IEEE Fellow citation read: "For pioneering achievement on computers and storage devices, and sustained service to the profession in this field. "*

*Education:* BEE, University of Minnesota, 1935; MS, University of Minnesota, 1938; PhD, physics, University of Minnesota, 1947.

*Professional Experience:* development engineer, electron tubes, RCA Corp., 1942-1946; computer development engineer, technical director, Engineering Research Associates Inc., (ERA),[1] 1946-1971; assistant dean, Institute of Technology, University of Minnesota, 1971-1979; senior fellow, Charles Babbage Institute, 1980-present; member, board of directors, Charles Babbage Foundation, 1980-present.

*Honors and Awards:* national chair, IRE Professional Group-Electronic Computers,[2] 1960-1962; member, board of directors, AFIPS, 1960-1965; member, scientific advisory board, National Security Agency, Ft. Meade, Md., 1960-1974; Valuable Invention Citation, Minnesota and American Patent Law Association, 1962; fellow, IEEE, 1964; member, advisory board, Chemical Abstracts Service, 1969-1972; IEEE Centennial Award, 1984.

Cohen joined RCA in 1942, working in gaseous electron tube development, largely for military use, later for other applications. His first contact with the computer field came when he joined ERA in 1946. He was first assigned to information storage problems, under ONR sponsorship, initially analyzing the feasibility of storing information on beams of charged particles. Then, he worked on the development of selectively alterable digital storage on magnetic drums; the patent that resulted turned out to be basic in the field. He then led system design of the ATLAS I magnetic drum computer for the National Security Agency (a later commercial version was called the ERA 1101). ATLAS I, which was delivered in December 1950, is believed to be the first stored-program electronic digital computer actually shipped to a customer site. Cohen was also responsible for the ATLAS II system design (commercialized as the Univac Scientific 1103 after the Remington-Rand merger in 1952). ATLAS II had both CRT and magnetic drum storage. Variations on these systems, including a magnetic core memory at an early date, were built and delivered for specific requirements, mostly military. The commercial 1103A which followed incorporated powerful new system features, in addition to core and drum storage.

An important aspect of ERA's business was to conduct digital systems work for outside customers, in response to requests for proposals, or as unsolicited proposals. Cohen had responsibility for much design and development in these areas. One early effort (1949-1950) led by him was a design study for IBM that called for a magnetic drum computer with punched-card input and output, for commercial applications. Although this work resulted in a massive system patent that was assigned to IBM, a parallel design program within IBM prevailed, ultimately producing the IBM 650.

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[1] Cohen served in this position in several successor companies, through to Sperry Rand Corporation, St. Paul MN.

[2] Now IEEE Computer Society

ERA was acquired in 1952 by Remington-Rand, which had a development group of its own in Connecticut, and which had in addition picked up Eckert-Mauchly Corporation in 1950. This brought together, as the Remington-Rand Univac division, three development and manufacturing locations, and an assortment of marketing groups. Cohen continued to be actively engaged in various phases of technical management, including system planning, government relations, and marketing support.

In 1971 Cohen joined the staff of the dean of the Institute of Technology at the University of Minnesota, as assistant dean for industry and professional relations. Upon retirement from the dean's staff in 1979, he became active in helping to form the Charles Babbage Institute for the History of Information Processing, which has since become a part of the University of Minnesota. He has been a senior fellow at CBI and is a member of the Charles Babbage Foundation's board of directors.

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## UPDATES

Portrait added (MRW, 2012)

## Harvey Cohn

*Born December 27, 1923, New York City; mathematical researcher with innovative uses of computers in number theory, particularly for algebraic number theory and modular junctions, specific computations involve class numbers, genus, and systems of modular equations (using computer algebra).*

*Education: BS,* mathematics and physics, City College of New York, 1942; MS, applied mathematics, New York University, 1943; PhD, mathematics, Harvard University, 1948.

*Professional Experience:* Wayne State University (Detroit) 1948-1956; Washington University (St. Louis), 1956-1958, director of Computer Center, 1957-1958; University of Arizona, 1958-1971, head of Mathematics Department, 1958-1967; distinguished professor chair at City College of New York and the Graduate Center (CUNY), 1971; Stanford University, 1953-1954; member, Institute for Advanced Study, Princeton, 1970-1971; lektor, University of Copenhagen, 1976-1977.

*Awards:* Putnam Prize Fellowship at Harvard, 1946; Townsend Harris Alumni Award, 1972.

In my Townsend Harris high school years in New York, 1936-1939, I found myself honored as a mathematics prodigy but troubled by the idea that the honor might be of dubious value in earning a living. At the College of the City of New York, 1939-1942, my feelings were accentuated. The gospel of the purity of mathematics was echoed dogmatically but yet defensively by teachers whom I otherwise admired greatly. Their attitude seemed encapsulated in the famous “Mathematician's Apology” of G.H. Hardy, which they recommended to me to read.[1]

This book appalled me as the ultimate in snobbery for its theme that “if real mathematicians do no good they also do no harm.” Indeed Hardy used “real” provocatively to mean “pure” as compared with “trivial” which meant “applied.” He seemed to say that a gentleman never works with his hands, and felt this arrogance expressed the academic workings of the British class system, which Americans were sanctimoniously taught to disdain, particularly since they had not yet formalized their own academic class privileges.

At that time some of the best mathematical brains, nationally, were in the academic engineering departments, and they were effective rivals of the pure mathematicians. At City College, it was necessary to go to the Electrical Engineering Department to find a course in matrices or in applications of complex analysis, and to the Physics Department to learn of the applications of differential equations. This was not unusual; it was their purity, their avoidance of real applications, that gave mathematics departments their sense of class.

I never accepted the folklore of the mainstream mathematician “purists,” who viewed non-appreciation of their calling as a form of ennoblement, as though it were some idealistic political cause. On the other hand, I did not believe the mathematical “crowd-pleasers,” who said that everything abstract shall eventually be applied. (They proved to be largely correct, but I understood then they were making very uninformed guesses couched in generalities which can only be proved right, but never proved wrong). I ended up as an applied mathematician in spirit, specializing in number theory, which looked very “applied” since the interest created by a theorem concerning numbers at that time lay in its numerical examples.

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[1] Hardy, G.H., *A Mathematician’s Apology*, Cambridge Univ. Press, Cambridge, UK, 1940, pp. 139-143.

The advent of computing still took time. First I had an exciting two-year adventure, 1942-1944, with Richard Courant, which included an MS in applied mathematics at New York University in 1943. Courant had only an embryonic “Applied Mathematics Group” (quickly renamed “Graduate Center of Mathematics”), which was unpretentiously housed in a corner of the Judson Women's Dormitory at Washington Square. It was the forerunner of the present lavish Courant Institute in Weaver Hall. The classrooms were in the main buildings, but the library was housed at the dormitory and furnished mainly with Courant's own books and reprints, one of many aspects of a “zero-budget” operation. He, with K.O. Friedrichs and J. J. Stoker, formed a triumvirate, but, also because of budget, they had to be supported in part, not as mathematicians but as engineering teachers. Also present was Donald A. Flanders, whom I was to later meet at the Argonne National Laboratory. Courant obviously was thinking ahead in terms of his world-renowned Institute at Göttingen, which he himself had largely created but had to abandon when the Nazis took power. Not surprisingly, he did have a computing center, consisting of a bank of desk calculators in search of users. To look busy, he employed his wife Nina and for director gave a start to the career of Eugene Isaacson, who stayed on to become a more meaningful director of the computing center for the Courant Institute.

The environment seemed mathematically felicitous, particularly to see a mathematician of Courant's stature so lacking in pretension of “purity.” Courant reminded us continually that Gauss had always tested results numerically with extravagant precision (like 40 decimals), and indeed that Gauss gained fame by tracking asteroids numerically as they disappeared behind the sun and reemerged. This numerical work inspired some of Gauss's deepest research in number theory and function theory. Clearly there had to be a moral here for aspiring mathematicians. Numerical work was also an important aspect of Courant's public relations campaign to obtain government support from the Office of Scientific Research and Development (OSRD). He was not at all modest about the eternal relevance of his famous work[1] With K.0. Friedrichs and H. Lewy on *numerical solutions* of partial differential equations. He also made the ethnically generous point that the British mathematicians should not be put down as mere “problem solvers,” compared with French and German “theorists,” since the British had a special instinct for numerical answers, as he saw in such work as that of Rayleigh and Southwell. One of his favorite associates was a very cheerful self-styled “Scotsman” J.K.L. MacDonald, from Cooper Union, who carried asymptotics to a higher state of the art. He developed techniques to estimate Bessel functions for “medium-sized” values, not being content with the ordinary infinite behavior. MacDonald's proclivity for numerical tricks seemed to go well with his love of gadgets, which he collected and even built. When he died in a private airplane crash during World War II, I felt he had tried one gadget too many. Looking back, I must acknowledge Courant's prescience of the early hold the British would have on computation within the next five years because of their skill with numerical computations. I thought of Richard Courant's endeavor as a “shoe-string” operation and such efforts seemed to occur very often in academic computing. What was lacking by most entrepreneurs, including me, was an “irresistible force” to overcome the “immovable administration.”

After service in the Navy (1944-1946) as an electronic (radar) technician's mate, I felt I had acquired an understanding of electronics of the future. Unquestionably the “Captain Eddy” (Navy) training program was the start for many of my generation who became attracted to computing. (The total effect of this program on American computer skills might prove comparable with the Fulbright Program if an accounting were made.)

The automation used even in the earliest tracking radar was like science fiction. Some of my immediate Navy classmates whose names I noticed later in programs of computing meetings include James Butler, Fernando Corbató, and Saul Padwo (and there must have been many others).

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[1] Courant, R., K.0. Friedrichs, and H. Lewy, “Über die partiellen Differenzengleichungen der mathematischen Physik,” Math. Annalen, Vol. 100, 1928, pp. 32-74.

Even after I returned to "pure" number theory at Harvard for my PhD (in 1948 as a Putnam Fellow), I must have been influenced greatly by Norbert Wiener's doctrines of cybernetics and the Second Industrial Revolution.[1] In fact his work was talked about more by laymen than mathematicians, who tended to look instead to John von Neumann, who was writing in more technical terms on automata.[2]22 But for whatever motivation, I felt machines had to "think" at least in routine fashion. (Maybe it was negative of me to want machines to think; maybe I did not consider mathematicians to be doing as much creative thinking as they pretended.) At any rate, I was beginning to become obsessive on this matter of "thinking."

From the wonders of naval radar, I looked to fancy electronics. The only machines I knew of in any functional detail beyond desk calculators were "analogue computers," and some of Derrick H. Lehmer's "garage-made" models for number theory, which were used in prime factorization.[3] All I saw, however, were mathematicians "riding hobby horses." I had heard enough about the electronic ENIAC to be unimpressed by the Harvard Mark 1, which gave itself away with its noise background as a mere relay machine.

Far from "thinking," Mark I was routinely calculating tables of Bessel functions, or something similar, when I saw it in operation.

## Wayne State University, Detroit (1948-1956)

My first academic position was in the Mathematics Department at Wayne (now Wayne State) University in Detroit (1948-1956), which was upgrading the curriculum to a PhD program by getting research oriented faculty even from as far away as Harvard. In view of the many recalcitrant non-research faculty members in all departments, I soon realized that Wayne was an unfortunate choice for me, except that computing unexpectedly happened to me in the form of two unlikely persons for Wayne. One was Arvid Jacobson, who pursued applied mathematics with literally religious zeal for what he called "industrial community service." His career was extraordinary. He spoke with an accent acquired from a Finnish-speaking community in northern Michigan. He became involved with, but later renounced, Communism, after a disastrous imprisonment in Finland. He then returned to Christianity with messianic zeal, always speaking of "spiritual leadership and community service" with a combination of religious and secular meanings. Since he was known to (and cited as a Communist by) Whittaker Chambers, his past came back to haunt him in the McCarthy era, but his many friends (including me) were adamant in vouching for him. His outlook was scarred by his past suffering. He saw his enemies in Detroit as an "Anglo-Saxon clique," and carried this paranoia to the point of explaining his vote for Eisenhower over Stevenson in 1952.

Jacobson worked in the automobile industry before obtaining his PhD at the University of Michigan in his forties. He knew most of the industrial research people in Detroit. Then came his calling. I must say I had to believe in him after I saw his miracles. He got a completely indifferent university whose administration feared progress to accept the largess of Detroit industries which feared mathematics (two characteristic fears). He formed his Industrial Mathematics Society (IMS) somewhat before the (now) better-known SIAM. Arvid soon became ready for computing. He first got Vannevar Bush's original Differential Analyzer, scrapped by MIT, to

[1] Wiener, N., Cybernetics, John Wiley and Son, New York, 1948.

[2] von Neumann, J., Collected Works, Vol. 5, General and Logical Theory of Automata (1948), Macmillan, New York, 1963, pp.288-328.

[3] Lehmer, D.H., "A Photo-Electric Number Sieve," Amer. Math. Mo., Vol. 40, 1933, pp. 401-406.

Wayne as a preliminary but ineffective start. Ultimately, however, he brought the Burroughs UDEC (Universal Digital Electronic Computer) to Wayne University. This was the first stored program machine I saw (1953). It had 1,000 ten-digit words of drum storage, did about 30 multiplications per second, and had all of 10 instructions (one per digit). The UDEC was loaded by paper tape with punched holes, sensed mechanically not optically. The machine was so slow that even the small problems I had in mind were not practical for it because of normal down times of vacuum tube computers, possibly every half hour.

Also, coincidentally with UDEC, Wayne University acquired Joe Weizenbaum. His friendship was very valuable to me. From him I learned what a stored program was, and I soon learned what a Turing Machine was. Now I felt that I knew the meaning of a “giant brain.” Joe Weizenbaum was a psychologist with an amazing fondness for mathematics. He went on to fame at MIT (with ELIZA).

Arvid Jacobson stayed on at Wayne to perform the further miracle of attracting rather prominent persons to serve as director of the computing program (through industrial donations). They served in succession, all leaving dissatisfied.

Among the names I can recall are Harry Huskey (who had been associated with Derrick H. Lehmer at Berkeley), Elbert Little (who was an industrial consultant), and, after I left (1956), Wallace Givens, who had been at the Oak Ridge National Laboratory, but who did not want to continue living in the South as a matter of political choice. In the long run, sadly, Arvid Jacobson quit in despair, and went into private consulting in 1959. He summed up his career as “pushing a great big sponge.” The last straw, I am told, was a donnybrook involving what should have been a routine but predictably unpleasant problem, choosing a new machine to replace the IBM-650, which followed the UDEC. (The IBM-650 remains the great performer in this memoir.) At the same time as the UDEC was set up at Wayne University, in 1953, John Carr set up the MIDAC at the University of Michigan, and got it operating very quickly. (It was a faster machine by a factor of ten; it was a miniaturized version of the National Bureau of Standards SEAC, and was also better supported, by the Air Force). The MIDAC even had an *optical* tape reader. I had the distinction in 1953 of being the first one to run a “real” program (not an exercise) successfully on that machine. The program was in pure mathematics, on cubic units, and I was ecstatic to find that Derrick H. Lehmer (whom I had not yet met) encouraged me to publish it in 1954 in *Mathematical Tables and Aids to Computation (MTAC)* (Cohn, 1954), the forerunner of today's *Mathematics of Computation.*

Thus in 1953 1 began my career as a professional parasite, using anybody else's machine, which was always better and bigger. This was easier then than now, since most managers of machine installations were easily flattered and very obliging, and bureaucracy then was not as intimidating as today (with batches, accounts, and priorities). I used many machines unknown today, such as MIDAC, UDEC, ORDVAC, EDVAC, SEAC, Univac, CDC-3600, and George (the Argonne Laboratory machine). Ironically, for my first 18 years of research using computers, the “home” facilities were not adequate until in 1971, when I achieved my current position.

I also learned of the potentially seductive appeal of computing. In 1955 I was not alone in wanting to leave Wayne University, nor was I averse to taking a nonacademic job (although my wife, Bernice, who claimed telepathic foresight, insisted I never could!). The three papers I wrote about using computers seemed to have had more weight than my ten papers in pure mathematics. I was interviewed by several computation laboratories, including the Ballistic Research Laboratories (Aberdeen, Md.), Argonne National Laboratory, Burroughs Research Laboratory (Philadelphia), IBM (Columbia), and Oak Ridge National Laboratory, but the MIT interview stands out in my mind. That was the job I really wanted. Philip Morse was a man whose work (including textbooks) at MIT in mathematical physics and operations research (in World War II) was legendary, and I was overwhelmed that he and Jay Forrester wanted to arrange a joint appointment for me with his laboratory and the MIT Mathematics Department. In the interview, he made me aware of the current interest in speeding up Monte Carlo and simulation computations; he felt number theory was important for randomness research.

When the Mathematics Department would not come up with its share, Morse discussed a job, using “soft” money, which was outside grants and would not lead to tenure. This still interested me, but clearly he would not offer it because he knew that I had achieved tenure at Wayne University and was becoming “established” in mathematics (despite my obvious dissatisfaction).

The most active period of my involvement with computers was about to begin. In 1953 1 had a small US Army Ordnance grant to do algebraic number theory. It was not large enough to cover computing, so I obtained access to the computing facility at the US Army's Ballistic Research Laboratories at Aberdeen, Maryland. In 1949 I had corresponded with John Giese, one of the mathematicians at Aberdeen, on a problem in aerodynamics using numerical methods I had learned while taking my master's degree with Courant much earlier. Giese's connection with the laboratories seemed a logical lead to me, and it led me to his colleague Saul Gorn. Saul had a PhD from Columbia in algebra, and quickly took an interest in the problem which led me there. It was the discovery of integral solutions in *a, b, c* to the equation

$$n(a3+b3+c3) + m(a^2b+b^2c+c^2a) + k(ab^2+bc^2+ca^2) + labc = 1$$

with specially selected integral parameters *n, m, k,* I chosen so as to make the cubic polynomial factorable in the real domain. If the problem looked messy and limited in appeal, the method of solution involved an even messier three-dimensional version of continued fractions. The details later appeared in a paper jointly with Saul Gorn (Cohn and Gorn, 1957).

It was this unlikely problem which made me appreciate Saul Gorn's vision of computing beyond number crunching.

He was looking to create a machine-independent programming system. His logistics were very simple. Since our problem in number theory was approved by the Army, he could obtain the time to work on his “universal code” using our problem as the pretext. It became his *cause célèbre* as well as mine. In the summer of 1954, he programmed ORDVAC at Aberdeen's Ballistic Research Laboratories to translate this program from universal code to internal code and to run it successfully. These were heady days of self-realization. We were not upset to find that Peter Swinnerton-Dyer had done a similar calculation simultaneously at Cambridge University, England, by a different method, because he wrote his program only in more direct code and he had a less automated program. His work was never published. Saul had now shown new potentialities for mathematics to evolve its own language.

During that same summer (1954) I worked at the AEC computing facility of the Courant Institute (NYU). This was sponsored by Remington-Rand with personal support of its honorary president, former General Douglas MacArthur, for whom Courant had great praise and with whom he claimed to have had very profitable discussions. (Since Courant was famous for speaking tentatively and mumbling subliminally, we in the laboratory amused ourselves trying to reconstruct the dialogue and imagining the strain on the dignified and redoubtable general.) The installation was built around the Univac and very much influenced by Grace M.

Hopper. I now became aware of a larger movement to regard computers as thinking machines and accordingly to regard "thinking" as a concept with different degrees of depth.

Grace Hopper set the moral tone with such maxims as: *If you do it once, it is permissible to do it by hand; if you do it twice, it is questionable as to whether or not to do it by hand; but if you do it more than twice, it had best be done by machine.* Her Univac B-2 Compiler, indeed, meant that translation to machine (assembly) code and the assignment of memory would never again have to be done by hand. She even refined the compiler to sense systematic typographical errors and correct them (with the author's permission each time). She was also one of Saul Gorn's spiritual leaders. Some of the speakers at the Courant Institute that summer told of efforts at MIT to produce automated algebra (not yet called MACSYMA). I was delighted to find computing beginning to mean something "cybernetic." This computing group at the Courant Institute was independent of Courant (I saw him only once) but it clearly was part of his master plan. Yet I knew the computer would not be permitted to be the tail that wags the dog. Courant was determined to keep numerical analysis dominant over machine usage. Combinatoric Computing was represented by George Dantzig and David Fulkerson, who were summer visitors. The only other guest I recall from the summer of 1954 was Wallace Givens. I had first met him at the Oak Ridge National Laboratory in 1953 and I was destined to keep running into him. He had just finished his famous work on the computation of eigenvalues.[1]

He had a sobering thought on it later on. He said that aside from its mathematical value, his method has demonstrably saved so much in machine time that if he does nothing else, he will still have deserved whatever salary he is paid. This idea was vicariously comforting to me too, and it was also symbolic of the new age, representing the capacity of the machine to amplify man.

In the summer of 1956, preparatory to leaving Wayne University, I worked at the National Bureau of Standards in Washington, D.C. It was my great opportunity to learn what computing people thought, particularly about one another. Having been influenced by both Saul Gorn and Grace Hopper, I felt my interest would have a cutting edge. I was even more fortunate in having the sponsorship of John Todd and Olga Taussky (who shared my interests in algebraic number theory). John Todd was the guru of traditional numerical analysts, who were looking only to make their well-established skills more effective by enlisting the computer. More than that, he was aware of the special groups of computer abstractionists. Although he was distant from them, he did not disparage ideas which he did not share. That was not true of everyone on the NBS staff, however.

The bureau had inherited an active group of famous Works Project Administration (WPA) table-makers from the 1930s who seemed to set the tone. I remember Irene Stegun, Ida Rhodes, and Henry Antosiewicz particularly. Phrases were bandied about which were largely derogatory to computing machines and were too numerous to recount. A small sample would include: "programming is garbage," "a machine is just a big slide rule," "we don't teach flowcharts," "programmers just don't understand error estimates," or best of all, "don't trust a subroutine you didn't write." Obviously "computer science" was oxymoronic there. I was disappointed but not surprised to also find that Saul Gorn and Grace Hopper were not considered relevant either. The members of John Todd's and Olga Taussky's crew included mathematicians who functioned independently of computing, such as Philip Davis, Everett Dade, and Morris Newman. They tried to be "political centrists" on computing but they must have felt themselves caught in the middle when I argued and sometimes harangued about computer progress.

[1] Givens, W., "Numerical Computation of the Characteristic Values of a Real Symmetric Matrix," Oak Ridge Nat. Lab. Report 1574, 1974.

I was still gratified enormously that John and Olga appreciated me sufficiently to have me represented in both the theorist's and user's side of computing in one of John's handbook-type books (Cohn 1962, 1962a). John also wanted me to join his group permanently later on despite my offbeat attitudes toward the role of computers. I could not accept his offer because of my growing instinctive fear of the nonacademic world. Although computing was no more secure in the academic world, I felt more comfortable with its irrationalities, and the university did have an irrational love-hate relationship with the computer!

The pattern seemed to be nationwide, maybe worldwide. Computer science did not exist the way mathematics did (with a 2,500 year history) and had no obvious home academically. Even more so, the computer scientist, whether in mathematics or engineering, was regarded as a *nouveau riche* technician whose pay unfortunately came out to be insufferably high compared with the prevailing salary scales.

### Washington University, St. Louis (1956-1958)

My next permanent job began September 1956 at Washington University in St. Louis in the Mathematics Department.

The chairman, Holbrook MacNeille, was a capable administrator and a former executive director of the American Mathematical Society. He was intent on helping to prepare the university for computing. The then current Computer Center consisted of a leased IBM-650 housed in an old shack (formerly a student theater) and run by the Industrial Engineering (IE) Department. This arrangement was based on a promise to run it “efficiently” in terms of producing some university income (which was just about forbidden under the “noncommercial” terms of the IBM academic discount). The IE Department members were known, however, to be using the machine only to produce unrecorded consulting income for themselves. This was doubly bad news because they usually took program decks and manuals away with them on house calls, leaving the laboratory denuded. The provost had known of this, too, and had made those IE Department members promise to resign from the center as soon as a better arrangement could be made. He then asked me if I would take over the Computer Center to make it academically responsible. Although I felt Washington University was a respectable institution, the Computer Center was not then an attractive proposition. (Among other things, the building was like a barn with an unfinished interior and wooden partitions which made the occupants feel like cows, and there was no washroom.)

Before I could give an answer, a climactic incident in my life occurred in February 1957, the last one linking me to my mentor Saul Gorn. He was now at the Moore School of the University of Pennsylvania, trying to form a Computer and Information Sciences Department, the first of its kind. He recommended me for the position of head of the department; this time the appointment was to be supported by Mathematics. I went to Philadelphia for an interview, which seemed like a triumphal entry as I knew many members of the Mathematics Department, chiefly Hans Rademacher's number theory group at the University of Pennsylvania, who were involved in related research interests and would welcome my presence. Clearly I was to be made the offer, but a lingering doubt arose somehow in my mind. Likewise Saul also had doubts about the adequacy of the support. In fact, he pointed out to me that the only support available was a one year grant from NSF, with no supporting funds promised by the university. I verified that the administration would promise nothing more to me, even as an outside candidate, than to Saul. We were both used to computing centers not having the respect of colleagues in more established disciplines. We naturally wondered if nonacademics such as the administration would respect us as little.

The answer to our fears came a day after I returned to St. Louis, when I received a call from the telephone operator in Philadelphia asking me to accept a collect call from the vice president of the University of Pennsylvania! I sensed the usual disrespect for computing personnel and declined to accept charges until the VP offered not one but three improbable reasons why he had no phone credit. The phone call continued with a discussion of salary, starting lower than my salary at Washington University and working its way up to parity with very little effort on my part. I suppose I disappointed Saul Gorn by declining the offer. He took it himself, mainly to get the program started, and tried to get me to come as a visitor, but I had changed jobs too recently to accept something temporary. He stepped down as head after a year, and I saw him only a few times afterwards for pleasant reminiscences.

The result was that I could now accept the position of director of the Computer Center at Washington University rather honorably, since Brook MacNeille would support me as much as possible and accept me back in Mathematics if I later wanted to quit. (The administration still would not install a minimal $300 washroom!) I served as director from May 1957 to July 1958 but with no previous illusions. I accepted the fact that the position was for image rather than development. Ominously, the center remained administratively in Engineering.

Things started badly for my directorship. Right off, IBM (which was silent when the Computer Center was mismanaged) now started to enforce its rule against nonacademic income for the university. This led to an ugly incident.

The Bemis Bag Company of St. Louis was starting to work with IBM on computing, and one of their staff formally invited our laboratory to set up a demonstration with a practical problem in linear programming. It was a classic allocation of differently priced paper stocks to various end products subject to supply restrictions. The problem was so classic that we even happened to have the right program deck for the IBM-650. We were already rejoicing over the prospect of outside money for the laboratory when IBM intervened with Bemis, telling them we were not allowed to do it because the problem was “commercial” not “academic.” The IBM staff high-handedly brought the Bemis group to their headquarters to do the demonstration without telling us.

Thus ended our first and only outside job.

The lack of income was not alleviated even by meager “legitimate” income from grants; most grants that used the laboratory were tiny and seldom in sciences, so big money was clearly absent.

The provost and his advisers soon reacted in shock at the failure of income. Everyone was very “forgiving” of me, although they announced that the Computer Center had incurred a loss of $80,000 and would have to be discontinued. (This figure involved “creative bookkeeping” since that was more than twice the budget!) Moreover, they came up with the desperate idea of replacing the IBM-650 Computer Center with some cheaper (short-lived) IBM desk machine with an external paper tape for programs but no internal memory. The machine and center would then go back into obscurity in the Industrial Engineering Department. It seemed I had no say.

At this point, however, the Russians came charging to the rescue like the Red Army Cavalry. They sent up not one but two Sputniks, the second one with a dog. I quickly tried (with no success) to find a relevant scientific program to run, but had to resort to an exercise program to keep the lights flashing for the many visitors.

Nevertheless, our IBM-650 became a prime newspaper, television, radio, and newsreel photo opportunity of St. Louis. One of our programmers, Pat Zwillinger, even brought along Athena, her racing greyhound, to pose standing with her paws on the console of the machine, looking at the flashing lights and eagerly panting for a message from the Russian canine in space. The St. Louis *Post Dispatch* failed to use that picture, preferring some serious ones with us humans. There were also laudatory newspaper editorials, which now served to make the center impregnable. Some of my more political friends solicited statements of support from Sen. Stuart Symington of Missouri and Rep. Melvin Price of Illinois, both with influential congressional committee memberships. We also received unsolicited offers from some of the financial angels of Washington University to put up a building to replace our shack (I assume with a washroom). This was dangerous in itself since the financial angels are not supposed to talk directly with faculty. Edward U. Condon, of Bureau of Standards fame, was chairman of Physics, and he insisted on lending his professional weight to the cause of having a scientifically competent laboratory. (His influence was considerably augmented by his public stand against McCarthyism.)

I was not surprised to be asked finally to continue as center director but was more than disappointed at the *reduced budget. I* kept thinking that my idol, Courant, could sell ice to Eskimos, while I could not sell computing, with two Sputniks raging overhead. I now knew that I was not enough of a promoter to deal with the situation. MacNeille did attempt to rescue the situation by trying to get the Computer Center moved administratively into Mathematics, where it would have a larger budgetary base, but this idea was premature, and the Mathematics Department was not impressed. Also, the National Science Foundation could not help much because the Sputniks had created demands far beyond their budget for the present year. I must have appeared unsportsmanlike, but I asked to be relieved of the position of director in April 1958.

I also resigned from Washington University at that time, taking solace in the fact that no one wished me to leave, but that a better position was awaiting me at the University of Arizona in the fall of 1958.

1 left for my next job with very fond memories of my colleagues and my staff at Washington University, which also included programmers Alan V. Lemmon, Joe Paull, David Tinney (all part-time students), and one full-time technical supervisor, Robert Carty. It is to my eternal regret that I did not keep up with my enthusiastic staff, whom I also regarded as cherished friends. Those on my staff in a sense were typical of the coming generation of computing. At the age of 35, I knew only the mathematical stereotypes for the younger generation, so I was still in for an education on what species of man was evolving under the influence of computer science. Fortunately the more distinctive species of hackers and nerds were not fully evolved at the time. Alan V. Lemmon was an undergraduate mathematical prodigy who came to the Computer Center to inquire about computers reluctantly, and only because one of his professors sent him. He timidly confessed ignorance about machines and programs. I gave him an IBM-650 Programming Manual to look at. The manuals in those days were written in real English prose, so I felt it was not unfair to ask him to read it through (it was only about 50 pages) and to come back when he understood the program on the last page.

That program was depicted on an IBM card which clears the memory except for the zero-cell and the accumulators. He came back the next day with several cards, one of which cleared all memory and accumulators and others which did special tricks like labeling the memory so that in a memory dump, the unused cells could be identified. He now works for the GTE (General Telephone and Electronics) Laboratories in Waltham, Mass. Not all neophytes were that startling, but the cases of “genetic computer readiness” in the younger generation are so common today that I can believe in Lamarck's adaptive theory of evolution. Bob Carty was a much different stereotype. He was a totally rigid personality, a strict observing Catholic, who saw in machines a kind of model of law and order. He had been a Marine and an FBI agent, and even then was an auxiliary sheriff; also, he believed in the rights of gun owners and had his own arsenal, including many types of handcuffs. He was a very serious and effective worker with no special compulsion for polemics or evangelism. Yet he was very social and made a large number of political contacts in our behalf. Bob left when I did, in 1958, to work in a supervisory capacity at the Wright Air Force Computing Center in Dayton, Ohio, but I lost track of him subsequently. I often think of him today when I observe that programming and software systems are very attractive as exercises in logic to a variety of rigid religious groups, who would feel considerably less comfortable with the humanistic scientific culture which both motivated computing and made it technically possible.

Joe Paull was originally a chemical engineer, and I must remark that there were a remarkable number of chemical engineers who switched to computing in the early stages (maybe second only to mathematics). The flood tide of physicists as machine users with money was yet to come. Patricia Zwillinger had been an honors student of mathematics at Wellesley, and her motivation for continuing in mathematics was largely due to computing. She and her husband had an overriding interest in animals, however, which caused them to move west. David Tinney had little motivation for any career. He was raised in comparative luxury (his father was Calvin Tinney, the humorist). His motivation for that brief period of the Computer Center also came from computing.

It was hard to resist the attraction of consulting. I worked for the IBM SBC (Service Bureau Corporation) in St. Louis for about six months from October 1956 to April 1957 as part of my enthusiastic search for what "real people" do in everyday computing. Surprisingly, it had some very interesting moments. It gave me the opportunity to visit large commercial and industrial computing installations, which only whetted my appetite for "more and more." I learned that most businesses claimed to have an "inventory routine," which did not function, owing to the noncooperation of their accounting departments. (This was not unlike the behavior of pure mathematicians, who feared a similar degradation of their role.) The most delightful moment was an official house call that a group of us from SBC made on Oscar Johnson, a shoe tycoon in St. Louis. He greeted us at the door of an impressive mansion in a private enclave with two sports cars and two Afghan hounds the size of ponies sitting on his driveway. He invited us in for drinks in a dining room with Queen Anne chairs, which he boasted were still safe to sit on.

He had been using the SBC to gather data for his "programmed trading" in the stock market. His method was (typically) the following: He had the SBC gather data daily on each of hundreds of favorite issues and print out to *four-decimal accuracy* results of computations in four-column format.

$$\text{NAME OF ISSUE} \qquad \frac{a}{b} \qquad \frac{b}{a} \qquad \frac{a}{b} \times \frac{b}{a}$$

Here *a* and *b* were data taken from the financial pages. If I recall, *a* was *balance shares* (shares bought minus shares sold) and *b* was total shares traded, all arranged in order of *current yield*. He noticed, of course, that for some mysterious reason the last column was close to 1.0000, undoubtedly proving the stability of the market, but seldom *precisely 1.0000*. For high figures like 1.0002 he would buy and for low figures like 0.9997 he would sell, but (I give him credit) he did use some additional judgment. I suppose it was my duty to tell him politely that he was starting with a random criterion, but I held back because of the rejoinder he would surely be too polite to make: "How many sports cars, Afghan hounds, and Queen Anne chairs do you have?" The problem for the men from SBC was far from mathematical, although I believe they knew enough algebra. The problem was to keep him from getting his own IBM-650 and giving up their service. I believe they succeeded by stressing that his basement, however attractive, would require remodeling with a possible change of decor for the heavier air conditioning.

One other SBC problem, which stands out in my mind for its pioneering value, was an on-line use of the IBM-650 for scheduling the ecological use of water power for generating electricity. The idea was to use power during the dead hours of the night to pump water uphill into reservoirs so as to ensure availability during the heavy usage of the day. This very successful program was written first in flowchart form by an enthusiast in the power company named Estil Mabuse. The SBC easily wrote a program for him facilitated by his clear thinking. He apparently just learned about programming ad hoc. He told me later that he put in so much overtime playing with the program that it threatened his marriage.

I also acquired a more professional consulting arrangement with the AEC Laboratory at Argonne, Illinois. It lasted from 1956 to 1969 and it permitted me to see the evolution of computing at a very dedicated and active organization. The original director of the mathematics division who initiated the arrangement was Donald A. Flanders. Later directors were William Miller and Wallace Givens (again), who symbolized the evolving discipline. “Moll” Flanders was a member of Courant's original crew before going to the Oak Ridge National Laboratory. He had an old-fashioned gentle character, which seemed to make him find automation menacing.

He lived in the world of hand computation. We used George, which happened to be Argonne's proprietary (binary) machine. The normal usage was to enter data in decimal form (called “binary coded decimal”), which the machine converted to binary for internal purposes. But Moll converted input and output from decimal to binary and back by hand, using tables, obviously suspicious of an untraditional operation. Nevertheless, he gave the laboratory the high-level mathematical character which served as an asset later on. He had his own problems of depression and erratic behavior, which endangered his clearance from time to time. The fact that his brother, Sen. Ralph Flanders of Vermont, was a leading and outspoken opponent of McCarthyism made the concerns of security look somewhat political. Yet his personal problems were real, culminating in suicide in 1956.

Bill Miller, his successor, was a computational nuclear physicist from Purdue. He was a supreme administrator, with a healthy agenda for all kinds of growth, service, and expansion. His professional input was important, with the emerging role of physics in computing, but there were many users in physics who were lacking in “cybernetic” instincts and overly endowed with money for machine time. Several times the machines were tied up overnight by physicists who looked for errors by running their program in “single-cycles,” one instruction at a time, or otherwise expressed, at less than one-millionth its running speed. Bill ultimately left in 1962 to become a vice president of Stanford University. I felt he should have eventually become president of Stanford University, but he became the target of student anger over the Vietnam War. This was an especially cruel blow for him because he was honestly sympathetic and liberal-minded. He did become president of the Stanford Research Institute.

Wallace Givens took over as next director of the mathematics division. He had a full-time appointment now at Northwestern University in Evanston, Illinois, but he had a flexible leave arrangement which lasted indefinitely. His work in numerical analysis and his knowledge of hardware made him the director the most in tune with the functioning of the mathematics division of the laboratory.

The bread-and-butter work was with other science divisions at the Argonne National Laboratory but the mathematical program was not restricted to servicing. There was a large number of inspiring visitors in pure and applied mathematics sometimes only distantly related to computing, but all computer-progressive. My work in number theory led to at least one research paper a year, always using the computers. All in all, I was able to enjoy an environment with a broad, healthy vision of computing to offset the usual narrow academic vision. Some of my closer associates were James Butler, Joe Cook, Bert Garbow, and Bob Buchal. I knew them less well than my crew at Washington University, but I recall things that again are somewhat characteristic of the personnel at computing laboratories. Jim Butler and Burt Garbow had not gotten their BS degrees, but were “redeemed” through their ability at computing. Joe Cook, on the other hand, was a very sophisticated mathematical physicist with a PhD at the University of Chicago under Irving Segal. He had an aversion toward academic stereotypes, which prevented him from obtaining a university position for which he would have been uniquely qualified and, I believe, in great demand.

All of these associates (and others at the Argonne National Laboratory) were involved in imaginative research projects going beyond what usually arises in purely academic context. The most imaginative work I did there, however, would be trivial for a PC today, namely the representation of the three-dimensional boundaries of a four-dimensional object in two-dimensional cross section, all printed by character pixels (like '*') on an unsophisticated line-printer (Cohn 1965). I only wish that the university computing centers I knew personally were that well supported and could deal in ideas as visionary as the Argonne National Laboratory. My impatience was undoubtedly reflected in a talk I gave at a meeting of the American Mathematical Society in June 1963 on computing. The invitation came from John Todd, who chaired the meeting. From sentiments expressed at the NBS in 1956, he knew I looked upon computing as more than the sum of individual tricks.

Instead of simply boasting of the wonders, I asked for a more sober distribution of effort toward new areas which could stimulate development. The title, “Purposeful and Unpurposeful Computing,” conveyed the message (Cohn 1963). This was my last public venture at influencing computing. From that point on, it would all be private, through my own research, or through my influence as a mathematician.

### Later Positions (after 1958)

By the time I took my next job as head of mathematics at the University of Arizona, Tucson, in August 1958, 1 knew I was not a good promoter. My moment was past. I could serve the cause of computing only as an outsider doing mathematics and building up a compatible PhD department. Happily, I did not appreciate the main difficulty immediately, namely the low salaries. I tended to discount this because the Southwest was so beautiful (and cheap) as a place to live. I did not count on the major difficulties in recruiting created by the ego of most professional mathematicians expressed in the appeal of the “established” departments with which I had to compete.

Nevertheless, the creation of the new Mathematics Department in Tucson, Arizona, was my only great administrative glory. Of course, one factor was working in my favor, which would not have been present in computing. This was the major university in Arizona and, whether the administration liked it or not, they needed a Mathematics Department. It soon became the largest department in the university.

The location was no hindrance in getting money. Our senator Carl Hayden was chairman of the Senate Finance Committee. With or without his influence, one never knows, we obtained many departmental grants from the government.

Also, Arizona was fortunately notoriously behind in its social thinking; a department head was a boss, not a chairman of the board, so he could act decisively when necessary. The problems with the administration arose only when computing entered, because it was an innovative budget concept and involved equipment.

I did like the idea that the West was known for healthy, independent thinking, but unfortunately this meant I had to contend with skeptical administrators who did not think anything called “Mathematics” could be trusted to do anything useful; in fact to them “Applied Mathematics” was a type of engineering (if it existed at all).

Although I had the largest departmental budget in the university, I could not get permission from the administration for even the smallest computer. In fact, we did get a free Teletype connection to the GE BASIC Mainframe in Phoenix in 1965.

It was installed without university funds; only the phone bill was charged. The administration would not let the Mathematics Department pay the phone bill, but Southwest Bell assumed the money *had* to be there, so they let us run up some $3,000 over two years before the service was terminated. Computing was officially a small activity of the Engineering College. The engineering dean was very mathematically oriented and had made no objections to the participation of the Mathematics Department in computing. The reluctance came from my own dean of Liberal Arts, who possibly also thought Mathematics was too big a department for his comfort. My attempts to play up the future destiny of computers were stagnant for a while, but suddenly they became counterproductive. The engineering dean was replaced in 1965 by a fortunately short-tenured one who had experience only in physics laboratory supervision. He got the novel idea to counter the engineers' (and his own) fear of mathematics by removing the source of fear.

This involved a catalogue proposal to reduce the Engineering mathematics requirements to advanced placement calculus (in high school) supplemented in college by a short calculus course taught not in the Mathematics Department but in the Computing Center (taught by engineers). My skepticism, of course, was predictable.

To show mathematics courses were superfluous, the engineering dean gave a demonstration to an “impartial committee” appointed by the Faculty Senate, purporting to show how calculus problems were “solved” by computer *without* mathematics courses. I had the privilege of observing with the committee. This demonstration was a procedure more reminiscent of convention centers than universities.

It included signs in large letters on tripods explaining even to the most ignorant exactly what problem was being solved at each time, and even signs announcing when each problem had been solved. The only things missing were the models in miniskirts carrying the signs. The main operation was a charade of students looking on a shelf for the appropriate deck of IBM cards for each problem and loading them into, you guessed it, the IBM-650.

In today's world of terminals, interactive demonstrations are quite legitimate and very much in vogue, but they are scarcely advertised as freeing engineers from the study of college mathematics.

I politely sat through this sideshow, trying to look dignified, and interrupting only to remark that obviously somebody has to do mathematics for the programs. This was so silly to say that I could scarcely refrain from laughing, but this select committee voted that a “new method had just been unveiled which will make the University of Arizona famous.” The few votes favorable to mathematics came from such unlikely sources as deans of agricultural engineering and architecture, not from the heavy mathematics users and surely not from the non-science departments. Of course I had to react. I quickly gathered data from colleagues who were on national engineering evaluation committees and I sent word to the university president privately that we might become famous by having the Engineering College disaccredited. The president (who was experienced enough to consider the source of his advice as well as the rhetoric) assured me it was never necessary for me to have worried. He soon made a statement to the faculty senate that “an engineer has to have a lot of mathematics to do his arithmetic,” and he summarily canceled out the calculus-without mathematics notion before its popularity could swell to even more gigantic proportions. This did not improve my political popularity with my bewildered colleagues. After I stepped down as head of the department (in 1967) and was no longer a public figure, the Mathematics Department was quietly permitted to start what later became a decent computation laboratory in mathematics. In fact there was a simultaneously created university computer laboratory in its own building.

Over and over again, I saw that the fruits of the innovators were enjoyed, unappreciatively, by the succeeding generations. *The dream is not for the dreamer*.

I spent a delightful summer vacation in 1963 teaching at John Green's Institute for Numerical Analysts at UCLA. Considering that the University of California at Los Angeles was central to the development of computers (SWAC) as well as curricula in numerical analysis, I expected computing to be treated better than at the National Bureau of Standards. Again, however, the emphasis was on using computers to make numerical analysts look better, not to inspire any new attitudes in mathematics. My only cybernetic soulmate turned out to be Charles Coleman, whom I had briefly met at the National Bureau of Standards in 1956. He had also become so engrossed in computing that he did not complete his bachelor's degree at the University of Virginia. He worked later for IBM at Yorktown Heights. I did not engage in polemics at UCLA, as I had experience with the intransigence of numerical analysts, but I thought to myself that computing might have been better respected by American academics if it were not so “American” looking. To be a great scientist even in 1963, it was not necessary to have an “accent,” but it helped.

I came to my present chair appointment at my alma mater City College, now part of CUNY, in 1971. My main purpose is to supervise doctoral students and to teach computing as numerical methods in analysis for undergraduates and as number theory or cryptography for graduates, including computer scientists. In any case, I am now on the sidelines enjoying the achievements of others, with more time to write papers using computers.

I am involved with many computing laboratories, owing to the multiple bureaucratic structure enjoyed by the City University, but when “bad things” happen to the laboratories, they are not my problem. I have no longer any input into sources of computer power and destiny. My encounters with computing still continue but in increasingly satisfactory form from the scientific viewpoint. Machines are faster and programming aids (and even programming assistants) are available. The NSF (National Science Foundation) also helps to make computing relatively easy for me now.

## Epilog

Looking back, probably I am pretending to have had a mutual relationship to computing like that of James Boswell to Samuel Johnson. In fact it may have been more like the rooster to the sunrise. I am not modest, however, about having pioneered in the intensive use of computers in an innovative way in a large number of classical mathematical problems. Therefore my presentation is that of a mathematician who uses computing rather than who serves as a creator of hardware, software, or systems. There are many such, but I also claim to have been an early "true believer" who felt that computers are more than devices to aid mathematicians, but rather devices which *must* change the nature of mathematics. To use the classical analogy, when rational mechanics was introduced in the seventeenth century to an environment of scientists using geometry, this was not just another application of geometry, but the entrance cue for *calculus*. In retrospect it may be said that the introduction of computers has been an entrance cue for many fields which seem to vary with fashion, like *information* theory, *complexity* theory, *knowledge* theory, and so on, and others which surely shall follow. I am not advocating any one of these viewpoints. To the contrary, I make no choice because my involvement was a search for the future which I did not fully understand, nor do I now. I must count on others to find it.[1]

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[1] An enlarged version of Cohn 1994.

## Leslie John Comrie

*Born August 15, 1893, Pukekohe, New Zealand; died December 11, 1950, London, England; developer in 1930 of mathematical tables for the British Nautical Almanac Office using punched-card bookkeeping machines and who, in that decade, established the first commercial calculating service in Great Britain.[1]*

*Education:* BA, chemistry, University College, Auckland, New Zealand, 1915; MA, chemistry, University College, Auckland, New Zealand, 1916.

*Professional Experience:* assistant professor, mathematics and astronomy, Swarthmore College, Swarthmore, Pa., 1923-1924; assistant professor, astronomy, Northwestern University, Evanston, Ill., 1924-1925; deputy director, Nautical Almanac Office, 1925-1931, superintendent, 1931-1936; founder and director, Scientific Computing Service, 1936-1950.

Leslie Comrie had begun his professional life as a chemist in Pukekohe and Auckland, New Zealand, when World War I broke out, during which he served in France in the New Zealand Expeditionary Force. He was invalided out of the force, having lost a leg; he turned to a past interest in astronomy, and became a research student at St. John's College, Cambridge, in 1918 with a University Expeditionary Force scholarship. While there he developed a means for the application of new computational techniques to the problems of spherical astronomy, which he carried forward into his later appointment as the deputy superintendent of the *Nautical Almanac,* for the British Navy (the Admiralty) in London. Later as superintendent he modified the almost one-hundred-year-old *Almanac* and introduced the concept of a standard equinox" (Comrie 1926 and 1929).

He left the Admiralty in 1934 to found the Scientific Computing Service, where he was able to better apply his ideas of mechanical computation for the preparation of mathematical tables. Using card processing systems he prepared the way for the electronic computer, which he would observe shortly before his death in 1950.

In May 1946 Leslie Comrie returned from a visit to the US with a copy of von Neumann's *First Draft of a Report on the EDVAC.* Maurice V. Wilkes, later developer of the EDSAC at the University of Cambridge, was given the opportunity of one night in which to read and digest the document which described the stored-program computer concept. Wilkes "recognized this at once as the real thing, and from that time on never had any real doubt as to the way computer development would go. "[2]27

Wilkes has credited Comrie with providing some of the enthusiasm for mechanical computational techniques which would affect the Cambridge Mathematics Laboratory and later the Computer Laboratory.

In 1982 the *Annals of the History of Computing* provided the following anecdote about Leslie Comrie:

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[1] See Wilkes' comments on Cornrie's contributions to the construction of tables in his biography of Charles Babbage: Wilkes, M.V., "Babbage's Expectations for His Engines," Ann. Hist. Comp.)., Vol. 13, No. 2, pp. 141-146.

[2] See also the biography of Maurice Wilkes.

Comrie, who has lost a foot [sic] during World War I, was in the habit of unscrewing his artificial limb when he came home in the evening to relax and listen to the BBC. On one of those evenings he heard an announcement on the radio that the Works Project Administration (WPA) was to be ended; with it, he realized, would come the demise of the Mathematical Tables Project (MTP). Without stopping to put on his artificial limb, he hopped to the nearest telegraph office and wired President Roosevelt to save the MTP.

There was no way to verify the story, Ira Rhodes said, but a few days later their loft in New York City was visited by a group of government people who gave the appearance of not really knowing what they were doing there. Shortly thereafter, the MTP, instead of ceasing to exist, was made part of the National Bureau of Standards of the US Department of Commerce.[1]

## QUOTATION

Sadler (1980) said: "Comrie, with no claim to be a mathematician, had the clarity of mind, tenacity of purpose, scientific courage, and immense energy that enabled him, by using essentially simple and direct methods, to obtain practical solutions to many problems that defied theoretical analysis. But he was inclined to impatience with those who did not share his devotion to perfectionism, and this led to some difficult personal relationships."

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Portrait added (MRW, 2012).

# Larry L. Constantine

*Born February 14, 1943, Minneapolis, Minn.; 1960s pioneer of disciplined, structured software design methodologies.*

*Education:* SB, management, Massachusetts Institute of Technology, 1967 (plus graduate study); certificate, Family Therapy, Boston Family Institute, 1973.

*Professional Experience:* staff consultant, programmer/analyst, CEIR, Inc., 1963-1966; president, Information & Systems Institute, Inc., 1966-1968; faculty member, IBM Systems Research Inst., 1968-1972; independent consultant, information system design, 1969-1976; assistant clinical professor of psychiatry, Tufts University, School of Medicine, 1973-1980; independent consultant, organization development, 1975-1978; assistant professor of human development and family studies, University of Connecticut, 1983-1987; clinical supervisor, adolescent and family intervention, LUK, Inc., Fitchburg, Mass., 1984-1986; consulting supervisor and family therapist, private practice, 1973-1991; independent consultant, software development, 1987-1992; principal consultant, Constantine & Lockwood, Ltd., 1993-present.

*Honors and Awards:* Research Award, Society for Family Therapy and Research, 1984.

Constantine began his work on the "invention" of structure charts, first used in essentially their modern form in 1966 and "published" in course material most of which later appeared in Yourdon and Constantine (1975). The direct precursor was Jim Emery's hierarchy charts of 1962.[1]

A year later (1967) Constantine introduced data flow diagrams based on Martin and Estrin's "data flow graphs." This was first used, in its modern form, as an analysis and design tool by Constantine and his company later that year and first made widely accessible in the *IBM Systems Journal* piece (1974).

The concept of coupling and cohesion developed by Constantine in the mid-1960s was first published in 1968 and repeated in the 1974 publication. These two measures have been the subject of more than 100 studies and are at the heart of a number of software quality and complexity metrics (including Card and Aggresti's intrinsic complexity metric). They have also demonstrated their resilience as fundamental intellectual constructs in that they have carried over into metrics for object-oriented software.

A significant event in the structural revolution, a sort of "coming out" for what would become structured analysis and design, was the first (and only) National Symposium on Modular Programming, sponsored by the Information and Systems Institute in July 1968. This conference gathered such luminaries as Mealy, Morenoff and McLean, Yourdon, Vincent, Aron, and Constantine, to discuss issues in modular system architecture and development methods long before these were *au courant.* This conference saw the first more or less complete summary of structured design.

It was also at this conference that Conway's Law (*"the structure of a system resembles the structure of the organization that developed it"*) was named by George Mealy.[1] Also named at the conference was Mealy's

1 Emery, J, "Modular Data Processing Systems Written in COBOL," *Comm. ACM*, Vol. 5, No. 5, 1962.

Law: "*There is an incremental programmer who, when added to a project, consumes more resources than are made available.*"

The first publication using the term "structured design" appeared in May 1974 (Stevens, Myers, and Constantine 1974) and generated more reprint requests than any other in the journal's history. It has been widely cited and reprinted as one of the seminal works of software engineering. In a more widely accessible forum it gave a more complete explanation of coupling and cohesion and presented structure charts and data flow diagrams in modern form.

The concept was further expounded in a pair of editions of books entitled *Structured Design: Fundamentals of a Discipline of Program and Systems Design*, published successively by Yourdon Press and Prentice-Hall in 1975 and 1979.

## QUOTATION

Constantine has laid his claim to pioneer status by stating:

"I know there is an understandable ACM and academic bias in this, but if you are going to include publication of Knuth, "Art of Computer Programming," [in a listing of landmark articles on software engineering] how can you ignore a landmark like *Structured Design*, one of the most successful books in the history of computing, still in print in original form some 17 years after first publication? I may never have the academic legitimacy to even be considered for a Turing Award, but the methodology is the most widely practiced today and the book is used in colleges and universities around the world.

"You should also consult Paul Ward's series on the history of structured analysis in *American Programmer*. He documents Ross's role with SADT and the advent of SA by DeMarco, Gane, and Sarson. These should be represented in the chart also.

"It seems to me a disservice to reduce the entire thread of structured methods and the structural revolution to one letter to the editor (even though it did appear in the *Comm. ACM.* As a footnote to history, although I have been cited dozens of times in *Comm. ACM, I* was never able to get published there."

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## UPDATES

Portrait added (MRW, 2012).

## Lynn Conway

*Born January 2, 1938, Mt. Vernon, N. Y; Xerox Parc (Palo Alto Research Center) researcher who with Carver Mead created a radically new way of designing* chips.

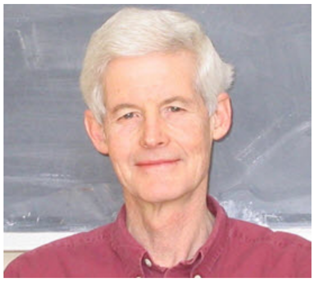
*Education:* BS, Columbia University, 1962; MSEE, Columbia University, 1963.

*Professional Experience:* member, research staff, computer architecture, IBM Corp., 1964-1969; senior staff engineer, Memorex Corp., 1969-1973; Xerox Palo Alto Research Center: member, research staff, Digital Systems Architecture, 1973-1977; manager, LST Systems Area, 1977-1980; research fellow and manager, VLSI System Design Area, 1980-present

### UPDATES

A complete description of Lynn Conway is available here. (MRW. 2012)

## Stephen A. Cook



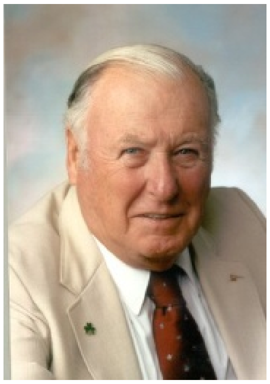
*Recipient of the 1982 ACM Turing Award for "contributions to the Theory of Computational Complexity, including the concept of nondeterministic, polynomial-time completeness."*

### UPDATES

A description of Cook and his work can be found on the ACM Turing Award website here. (MRW, 2012)

## James William Cooley

*Born September 18, 1926; with John Tukey, creator of the fast Fourier transform.*



*Education:* BA, arts, Manhattan College, 1949; MA, mathematics, Columbia University, 1951; PhD, applied mathematics, Columbia University, 1961.

*Professional Experience:* programmer, Institute for Advanced Study, Princeton University, 1953-1956; research assistant, mathematics, Courant Institute, New York University, 1956-1962; research staff, IBM Watson Research Center, 1962-1991; professor, electrical engineering, University of Rhode Island, 1991-present.

*Honors and Awards:* Contribution Award, Audio and Acoustics Society, 1976; Meritorious Service Award, ASSP Society, 1980; Society Award, Acoustics Speech and Signal Processing, 1984; IEEE Centennial Award, 1984; fellow, IEEE

James W. Cooley started his career in applied mathematics and computing when he worked and studied under Professor F.J. Murray at Columbia University. He then became a programmer in the numerical weather prediction group at John von Neumann's computer project at the Institute for Advanced Study in Princeton, New Jersey.[1] In 1956, he started working as a research assistant at the Courant Institute at New York University, New York. Here he worked on numerical methods and programming of quantum mechanical calculations (Cooley 1961). This led to his thesis for his PhD degree from Columbia University.

In 1962 he obtained a position as a research staff member at the IBM Watson Research Center in Yorktown Heights, New York. Here he worked on numerical methods for solving ordinary and partial differential equations, solutions of hole-electron diffusion equations for semiconductors, and numerous other research projects. He collaborated with Fred Dodge, a neurophysiologist, in research in neurophysiology including modeling of electrical activity in nerve membranes and in heart muscle (Cooley and Dodge 1966).

With John Tukey, he wrote the fast Fourier transform (FFT) paper (Cooley and Tukey 1965) that has been credited with introducing the algorithm to the digital signal processing and scientific community in general.

Cooley spent the academic year 1973-1974 on a sabbatical at the Royal Institute of Technology, Stockholm, Sweden. He gave courses on the FFT and its applications there and in several other locations in Europe and worked on new versions of the FFT and on number theoretic Fourier transforms.

In 1974 Cooley started collaboration with S. Winograd and R. Agarwal on applications of computational complexity theory to convolution and Fourier transform algorithms (Agarwal and Cooley 1977).

Around 1985 he worked with a group that programmed the elementary functions for the new IBM 3090 Vector Facility. He and the same group also produced the Digital Signal Processing subroutines for the Engineering and Scientific Subroutine Library (ESSL) for the IBM 3090 Vector Facility and, later, for the new IBM RS6000 computer (Agarwal and Cooley 1987).

[1] See the biography of Jule Charney.

Cooley retired from IBM in 1991 and joined the faculty of the Electrical Engineering Department of the University of Rhode Island as director of the computer engineering program.

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## UPDATES

Portrait changed (MRW, 2012)

## Allen W. M. Coombs

*Born October 23, 1911, Bristol, Gloucestershire, UK; early computer engineer responsible for the construction of the Colossus computer system designed by Alan M. Turing and others.*

*Education:* BSc, Glasgow University, 1932; ARTC,[1] Strathclyde University, 1932; PhD, Glasgow University, 1936.

*Professional Experience:* engineer, research branch, British Post Office (now British Telecom), 1936-1973.

From 1936 to 1973, Coombs was employed by the British Post Office (now British Telecom) on various projects in its research branch, most of them still classified. He spent some of the World War II years in association with the project to design and implement the Colossus computer for Bletchley Park for the government code and cipher school together with Thomas Flowers.

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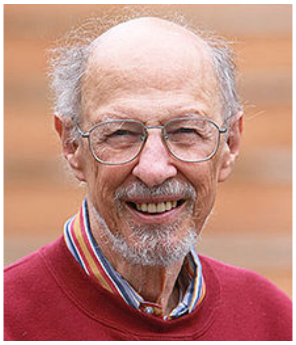
### UPDATES

[1] Later designated ARCST (Associate of the Royal College of Science and Technology).

# Fernando Jose Corbató

*Born July 1, 1926, Oakland, Calif.; creator of the Compatible Time-Sharing System (CTSS), the first general-purpose interactive system.*



*Education: BS,* physics, California Institute of Technology, 1950; PhD, physics, Massachusetts Institute of Technology, 1956.

*Professional Experience: US* Navy, electronics technician, 1943-1945; research associate, MIT Computation Center, 1956-1959; assistant director for programming research, 1959-1960; associate director, 1960-1963; deputy director, 1963-1966; group leader in the computing systems research group, Laboratory for Computer Science , 1963-1972; co-head, systems research division, 1972-1974; co-head, automatic programming division, 1972-1974; associate head, Department of Computer Science and Engineering, 1974-1978, 1983-present; Cecil H. Green Professorship, 1978-1980.

*Honors and Awards: W.W.* McDowell Award, IEEE, 1966; Harry Goode Memorial Award, American Federation of Information Processing Societies, 1980; IEEE Computer Society Pioneer Award, 1980; ACM Turing Award, 1990.

When the war broke out in 1941 (for the US) Corbató's high school went into long hours and he saw a chance to get out in a hurry. He went to UCIA as a student, with the threat of the draft looming over his head. People came by the school who were concerned about the ability of the Navy to maintain and repair the incredible amount of electronic equipment they were getting. There was a program called the Eddy Program, and they gave Corbató the opportunity to join the program and get an education as an electronic technician. One of the benefits, of course, was one did not get drafted, or get assigned to be a cook, or something worse. So Corbató enlisted at the age of 17 in the Navy and went through a year-long program as an electronic technician. He got exposed to some of the earliest and largest electronic systems then deployed in the Navy-mostly radar, loran, and sonar systems—both on land and on ship. He did not realize until later how important that was.

After the war he got a chance to go back to college, this time at Cal Tech. Since everyone wanted to be a physicist in those days, Corbató read to be a physicist following his undergraduate studies with graduate work at MIT, still with the intention of undertaking a career as a physicist. He got his doctorate in physics, but along the way got exposed to digital computers by the late Philip M. Morse [later to be director of the MIT Computation Center]. In 1951 Morse recruited Corbató to take on a new research assistantship in the use of digital computers. The people who signed on with this initial ONR-sponsored research assistantship program were exposed to punched cards in the spring, and in the summer they were introduced to Whirlwind. The Whirlwind was just barely operational; it had approximately 1,000 words of 16-bit custom-built electrostatic memory that had a mean time between failures (on a good day) of about 20 minutes. Jay Forrester was very prescient in recognizing that the Williams tubes (Manchester University) were flaky in design, and he had actually gone back basically to first principles to design a very elaborate mosaic storage tube. Somewhat similar in storage retention (charge retention) to Williams tubes, it was a tour de force in electronics to make it work. A group headed by Patrick Youtz built them in a basement at MIT.[1]

The people who operated Whirlwind recognized the need to have unclassified work and be able to use it in an open way. So the deal they worked out was that approximately three to four hours a day were made available

[1] Wildes and Lingren 1986.

for that purpose. In the morning were the times when the non-classified personnel were permitted to use the machine. This meant that general users worked all day and all evening preparing Flexowriter tapes, which were the input medium, and then took a quick shot at the machine the next morning. If one were really fast, the user might get two shots in during each two-hour period. Most times it was one shot a day, which could be very frustrating, but on the other hand, the machine was not that big, and the programs were not that huge.

It was a personal computer; there were many things about it that got lost in the next few years, such as graphical display output and even audio output—there was a probe put on the circuit of the accumulator which gave the users a “signature” of each program they were running. The users actually could hear where the program was, could sense the loops, and could even get the tempo of how the program was running. Occasionally people recognized from the audio that they had a program that was misbehaving, because it did not sound as they thought it should at that point.

After Corbató got his doctorate in physics in 1956, Phil Morse recruited him into the newly formed Computation Center, which he had established that year. The focal point of the new center was an IBM-704; Morse had worked out a package deal with IBM, where in return for the use of the machine on campus for one shift, MIT would also make it available to a cooperative group of New England colleges for the second shift. The third shift would be retained to be used by IBM's own local scientific office. Time was also provided for a group in Cambridge, formed and directed by Martin Greenberger, that computed the orbits for the first USSR Sputnik.

Corbató played various roles in the center, ranging from initially supervising a research assistantship program to later becoming an assistant director, associate director, and finally deputy director of the Computation Center.

In the spring of 1961 when the IBM-704 had been replaced by an IBM-709, Corbató started up a project which eventually developed the Compatible Time-Sharing System (CTSS) with just a couple of the key staff people—Marjorie Daggett (who was then Margaret Merwin) and Bob Daley. By November 1961 they were able to demonstrate a really crude prototype of the system. They had eked out 5K words of the user address space from the standard operating system and inserted a tiny operating system that managed four typewriters. Backup storage was achieved by assigning one magnetic tape drive per typewriter.

This system could operate effectively as long as no other user wanted an “all-the-core-memory” type job to run under the Fortran (EAP) monitor system. This system could coexist with that kind of an operating system and could run jobs. So it could run compatibly; it could run while ordinary batch work was being run on the IBM-709. It used the same language systems as the batch system; thus, using CTSS meant not having to start programming over again, as had happened in most system and machine changes up to that time. Even early versions of LISP ran under CTSS.

The IBM-7090 replacement hardware arrived in the early spring of 1962, supporting the necessary interrupt capabilities which had not been available in the earlier machine. Corbató et al. presented a paper at the 1962 Spring Joint Computer Conference (Corbató et al. 1962), which suggested that they were running CTSS on IBM-7090 hardware on the basis that this would be fact by the time the paper was presented. In fact, it was not operational until the fall of that year.[1]

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[1] Editor's note: A prime example of why historians cannot even always trust the primary sources of information on an event.

That paper was the first description of a working time-sharing system. It stated:

> ... it is best to give a ... precise interpretation to time-sharing. One can mean using different parts of the hardware at the same time for different tasks, or one can mean several persons making use of the computer at the same time. The first meaning, often called multiprogramming, is oriented toward hardware efficiency in the sense of attempting to attain complete utilization of all components. The second meaning of time-sharing, which is meant here, is primarily concerned with the efficiency of persons trying to use a computer. Computer efficiency should still be considered, but only in the perspective of the total system utility.
>
> An experimental time-sharing system has been developed. This system was originally written for the IBM-709 but has been converted for use with the 7090 computer.
>
> The 7090 of the MIT Computation Center has, in addition to three channels with 19 tape units, a fourth channel with the standard Direct Data Connection. Attached to the Direct Data Connection is a real-time equipment buffer and control rack designed and built under the direction of H. Teager and his group. This rack has a variety of devices attached but the only ones required by the present systems are three Flexowriter typewriters. Also installed on the 7090 are two special modifications (i.e., RPQ's): a standard 60-cycle accounting and interrupt clock, and a special mode which allows memory protection, dynamic relocation and trapping of all user attempts to initiate input-output instructions.
>
> In the present system, the time-sharing occurs between four users, three of whom are on-line each at a typewriter in a foreground system, and a fourth passive user of the background FAP-MAD-MADTRAN-BSS Monitor System (FMS) used by most of the Center programmers and by many other 7090 installations.
>
> Significant design features of the foreground system [for the user] are [that he can]:
>
> 1. Develop programs in languages compatible with the background system,
> 2. Develop a private file of programs,
> 3. Start debugging sessions at the state of the previous session, and
> 4. Set his own pace with little waste of computer time.
>
> The foreground system is organized around commands that each user can give on his typewriter and the user's private program files which presently (for want of a disk [sic] unit) are kept on a separate magnetic tape for each user.
>
> The commands are typed by the user to the time-sharing supervisor (not to his own program) and thus can be initiated at any time regardless of the particular user program in memory. For similar coordination reasons, the supervisor handles all input/output of the foreground system typewriters. Commands are composed of segments separated by vertical strokes; the first segment is the command name and the remaining segments are parameters pertinent to the command. Each segment consists of the last six characters typed (starting with an implicit six blanks) so that spacing is an easy way to correct a typing mistake. A carriage return is the signal which initiates action on the command. Whenever a command is received by the supervisor, “WAIT” is typed back followed by “READY” when the command is completed. (The computer responses are always in the opposite color from the

> user's typing.) While typing, an incomplete command line may be ignored by the “quit” sequence of a code delete signal followed by a carriage return. Similarly after a command is initiated, it may be abandoned if a “quit” sequence is given. In addition, during unwanted command type-outs, the command and output may be terminated by pushing a special “stop output” button.
>
> Although experience with the system to date is quite limited, first indications are that programmers would readily use such a system if it were generally available. It is useful to ask, now that there is some operating experience with the IBM-7090 system, what observations can be made. An immediate comment is that once a user gets accustomed to [immediate] computer response, delays of even a fraction of a minute are exasperatingly long, an effect analogous to conversing with a slow speaking person. Similarly, the requirement that a complete typewritten line rather than each character be the minimum unit of man-computer communication is an inhibiting factor in the sense that a press-to-talk radio-telephone conversation is more stilted than that of an ordinary telephone. Since maintaining a rapid computer response on a character-by-character basis requires at least a vestigial response program in core memory at all times, the straightforward solution within the present system is to have more core memory available. At the very least, an extra bank of memory for the time-sharing supervisor would ease compatibility problems with programs already written for 32,000-word IBM-7090's.
>
> In conclusion, it is clear that contemporary computers and hardware are sufficient to allow moderate performance time-sharing for a limited number of users. There are several problems which can be solved by careful hardware design, but there are also a large number of intricate system programs that must be written before one has an adequate time-sharing system. An important aspect of any future timeshared computer is that until the system programming is completed, especially the critical time-sharing supervisor, the computer is completely worthless. Thus, it is essential for future system design and implementation that all aspects of time-sharing system problems be explored and understood in prototype form on present computers so that major advances in computer organization and usage can be made.

After the success of CTSS Corbató continued the development of time-sharing systems under Project MAC, which was headed by Robert Fano. While Project MAC initially used CTSS as its basic system, it was superseded by Multics operating on GE computers in the late 1960s, and Multics was marketed by Honeywell Information Systems for many years after GE sold its Computer Division in 1968.

In 1991 Corbató received the ACM Turing Award: “For his work in organizing the concepts and leading the development of the general purpose large-scale time-sharing system and resource-sharing computer systems CTSS and MULTICS.” His major research interests continue to be in time-sharing systems, automatic programming, and knowledge-based application systems.

## QUOTATION

Regarding the November 1961 primitive CTTS: “. . . we were just trying to get a demonstration system going to convince people that it was a good idea. A lot of people did not understand what it meant to interact. [That] was amazing.” (Lee and Rosin 1992)

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## UPDATES

Portrait changed (MRW, 2012)

# Louis Couffignal

*Born 1902, died 1966; French computer pioneer who designed and attempted to construct an early innovative system.*

*Education:* DSc, mathematical sciences, University of Paris, 1938.

Couffignal's career began in secondary school education, teaching mathematics and mechanics at the Ecole des Élèves Ingenieurs Mecaniciens, belonging to the Ecole Navale de Brest. At that time the wisdom of providing schools with calculating machines was being debated in the French scientific journals; the arguments for and against put forward by the opposing sides remind one of those used more recently concerning the place of computer science in secondary education. Interested in the problems of calculation, Couffignal decided to write a thesis on the subject, under M. d'Ocagne's supervision. This was the first time that the theory of calculating machines had been proposed as the subject of a doctorate thesis in mathematical sciences in the University of Paris; the thesis was submitted in March 1938 and was greeted by d'Ocagne as the start of a new line of research that promised "consequences with a breadth of scope one could not yet assess."

After d'Ocagne's death in September 1938, Couffignal, an established expert in numerical calculation, was to take on greater responsibilities. He had applied for the chair of applied mathematics at the Conservatoire National des Arts et Metiers, but had been ranked second, after A. Saint-Lague, by the Academie des Sciences.

However, the course of events was to favor Couffignal. After the Munich agreement, it became steadily clearer that war was approaching; mobilization of scientific, technical, and industrial resources was begun and the plans of the Centre National de la Recherche Scientifique Appliquée (CNRSA) started to be put into effect in October 1938.[1] Henri Laugier, director of CNRSA, asked a number of leading individuals to draw up a report on the state of research and the resources available in various fields. Couffignal was asked to report on the application of mathematics to scientific and technical research, and this marked the beginning of his administrative career.

One of Couffignal's hopes was that he would be able to build a calculating machine of his own design. He had become secretary of CNRS's specialist committees, secretary general of the CNRSA committee dealing with inventions, and was appointed director of the laboratory for calculation and mechanics in the Institut Poincaré, where he found himself alongside laboratories in which there was a certain amount of calculating machinery and some computing staff. He had the task of organizing his laboratory.

Couffignal was to find his plans held up by the events of the start of the war and the occupation of France, especially as it seemed for a time that the survival of the CNRS, which had replaced CNRSA in 1939, was threatened by the attitude of the Vichy government. Couffignal collaborated with the new director of CNRS, suggesting that the Inventions Committee should be disbanded as it had failed to achieve its objectives—in his view, because of the opposition of the ministers for the various armed services and boycotting by the CNRS

[1] The CNRSA was established in 1938 to consolidate all sources of funding for scientific research. In October 1939 the name of the organization was changed to Centre National de la Recherche Scientifique (CNRS), and it took on a wide range of objectives in fostering pure and applied sciences.

directorate. This committee was later replaced by the Inventions and Patents Committee, of which Couffignal became and remained secretary general. His involvement in many of the CNRS committees made it possible for him to observe what was being done in France in scientific research. He was appointed inspector general for technical education, and found time to write a book, *Histoire de la machine à Calculer*, which had the distinction of being awarded the Binoux Prize of the Academie des Sciences, usually reserved for works on the history and philosophy of science.

Building his own machine remained his ambition and thanks to his position at the heart of CNRS, he now saw a possibility of achieving it. In 1938 the Institut Poincaré's Statistical Laboratory, directed by Borel and Frechet, had obtained a grant of FF 100,000 for the construction of a calculating machine that was to work in binary, and had gone as far as signing a contract with the Outillage R.B.V. Company to produce an electromechanical device comprising an Ellis-type adder and two converters, binary/decimal and decimal/binary. In December 1939 General Desmazieres, who had recently assumed responsibility for artillery tables, suggested to Couffignal that he should turn his laboratory into a center for artillery calculations and should equip it with a powerful machine. At that time Couffignal had arranged a contract, also with Outillage R.B.V., worth FF 80,000, for the construction of an electromechanical linking of a Sanders-Octoplex 10-column accounting machine to a Monroe A-1-213 calculating machine. This link was to enable any number produced by either machine to be transferred to the keyboard of the other, and all operations would be controlled automatically by means of a perforated tape.

The events of the war delayed the fulfilling of these contracts; further, the board of Outillage R.B.V., most of whom were Jewish, was disbanded and it was not until the beginning of 1942 that a new board was constituted. With the advance of the German armies some of the company's machines were scattered and some were seized by the occupying forces. The new management told Couffignal that they would not be able to fulfill the 1938 and 1939 contracts because there was no possibility of acquiring a Monroe machine, and in the course of these discussions an idea developed: why not combine the two projects into a single new one? For the same total sum, FF 180,000, the company would build a machine of the Sanders type and add to it a “calculating mechanism” working in binary. To make this possible, the statistical laboratory would have to give up its grant and Couffignal. set about persuading its director, Frechet, of the virtue of this idea. After some procrastination Frechet agreed, subject to certain conditions that he presented to Jacob, the director of CNRS: he would transfer his grant of FF 100,000 to Couffignal provided that the latter guaranteed him two-thirds of the time to be under the sole control of the statistical laboratory. Frechet insisted on a rigorous accounting of the machine's working time.

Couffignal was a member of the committee for Mechanics and Applied Mathematics; in the autumn of 1944, at the suggestion of Dautry, the Minister of Reconstruction, this committee began discussion of the formation within CNRS of a foundation to be called the Centre d'Etudes Superieures en Mecanique (CEMA) whose capital would be in the form of government stock and shares in national industries, SNCF for example. The latter would be able to call on the foundation to undertake various investigations or to provide training for young recruits. In November 1944 the view was formed that computing services should form the kernel of CEMA and accordingly Couffignal was asked by the Mathematics Committee to make a survey of the tables of numerical functions available in France. At the same time he kept in touch with the activities of a scientific mission to Germany in so far as these concerned bringing back machinery for calculation. Awareness of this mission's activities led many laboratories to put in requests for calculating machines, and also for slide rules that could not be bought in France; CNRS was swamped with demands and was unable to satisfy all of them.

Couffignal again saw a possibility of building his own machine; these aspirations led to the regrouping which was to give birth to the Institut Blaise Pascal. Couffignal obtained the grants he needed for the building of his machine, and at a meeting of the CNRS board on May 6, 1947, Peres proposed that a contract should be drawn up with the Logabax Company for the design and construction of a universal computing machine.

The sequel is well known in France: the machine was never finished. Meanwhile the mechanical computing section of the Institut Blaise Pascal performed calculations for scientists, using the classical methods of the interwar years. When the first commercially produced computers appeared, priorities began to change, the emphasis going to training in programming electronic computers. In 1957 a decision of the director of CNRS abolished the post occupied by Couffignal at the Institut Blaise Pascal; the man who in 1938 had been heralded as the one who would revolutionize the subject of automatic computation was removed from the field unnoticed.

Couffignal's unvarying aim was to reorganize, on rational scientific lines, the computing bureaus that were operating up to the start of World War II. He felt that one of the first uses of his machine should be to automate the accounting processes for postal checks. He always envisioned a computer center as a laboratory for constructing tables, nomograms, and charts to be used as aids for rapid approximate calculation, "useful calculations" in his definition. Conditioned by this outlook, he always believed that the real problem was interconnecting classical machines: he never understood that the speeds made possible by the new electronic machines changed the problem of automatic calculation completely.

Might this be attributed to a certain French insularity? It is difficult to answer that question. Certainly in Couffignal's case there were gaps in his mathematical awareness. In 1938, he published a note in *Comptes-Rendus* claiming that he had designed a machine for proving theorems in logic; the consequences of the famous Gödel Theorem (1931) seem never to have crossed his mind, and he made no reference to the "Turing Machine" of 1936. This contrasts strongly with his usual practice of claiming priority, that he had had the same idea earlier, without giving any details of his work.

The seriousness of the decision to entrust Couffignal with the task of building an electronic computer is shown by its consequences. As scientific activities expanded, the need for computers became ever more pressing; in the face of this rising demand the decision taken was to wait for Couffignal's machine, and when the failure of that project became evident those with the needs turned to commercial products, and especially to foreign manufacturers. Precious time for training and education in computer techniques had been lost.

A last point needs to be mentioned. The decision to build the machine seems to have been based more on Couffignal's reputation than on any rational evaluation of his project. Further, there was no regular monitoring of progress, which would have shown that it was running into the sands, and consequently the decision to halt it in time to minimize the damage was not taken. This problem of project evaluation is still with us. Whoever is entrusted with the decision must pronounce equally on the skills needed for carrying out the project. As we have noted, the necessary expertise in electronics was, effectively, available in France at this time; should not the first course have been to approach those who held this expertise? The point to make here is that the history of this project raises the question, "to what extent did its failure give rise to a kind of mutual distrust between research workers and engineers, between research and industrial laboratories?" No doubt those who lived through the events will have an answer to the question.[1]

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[1] 36Extracted from Rammmi 1989.

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## UPDATES

Portrait added (MRW, 2012)

# Perry O. Crawford

*Engineer who worked on Whirlwind and other early military computers.*

In his oral interview with Christopher Evans, J. Presper Eckert mentions that he had considered building a memory device which was composed of a magnetic disk-I had gotten the idea of using disks for memory, digital memory, from a master's thesis written by Perry Crawford at MIT. He had not built any such disks; it was just speculation."

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## UPDATES

# Seymour R. Cray

*Control Data Corporation (CDC) designer responsible for the CDC 6600, which was perhaps the first modern supercomputer and subsequently leading designer of supercomputers for his own corporation.*

*Honors and Awards:* IEEE Computer Society Pioneer Award, 1980; ACM/IEEE Eckert-Mauchly Award, 1989.

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Seymour Cray died October 5, 1996 as a result of severe head injuries received in an automobile accident (MRW, 2012). Portrait inserted (MRW, 2012)

# Kent K. Curtis

*NSF director who got the foundation going in the field of computer science, who got researchers going and founded (and expanded) many university computing centers through innovative funding opportunities.*

Curtis studied mathematics, physics, and music at Yale, Dartmouth, and Berkeley. He did scientific programming for the Lawrence Berkeley Laboratory of the University of California, where he was head of the Division of Mathematics and Computing from 1957 until 1967. He joined the National Science Foundation in 1967 and became head of the Computer Research Section of the Division of Mathematical and Computer Sciences at a time when the foundation was to be very influential in the development of university and college computing centers. He took leaves at the Atomic Energy Commission and the Courant Institute of Mathematical Sciences at New York University, and served as a consultant to the Department of Energy, other federal agencies, and the Swedish Technical Development Union. He was one of the founders and first president of VIM, the user's group for CDC6600 computers.

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## UPDATES

The New York Times published the following obituary on December 20, 1987:

> Kent K. Curtis, chief scientist of the National Science Foundation, died Thursday of cancer at Sibley Memorial Hospital in Washington, D.C. He was 60 years old and lived in Washington.
>
> Born in Charles City, Iowa, he joined the foundation in 1967.
>
> He is survived by his wife, Herta Kley Curtis of Washington; four daughters, Sandra of Berkeley, Calif., Greta of San Rafael, Calif., Celia and Katherine, both of Washington; a son, Christian of Berkeley; two brothers, James of Missoula, Mont., and Mark of Bethesda, Md., and a sister, Cora Hayes of Des Moines.
>
> (MRW, 2012)

## John H. Curtiss

*Born December 23, 1909; died August 13, 1977, Port Angeles, Wash.; man of many talents; first and always a mathematician, but also a highly able administrator, musician, and tennis player; during the years 1946-1953 he was at the National Bureau of Standards (NBS) and played a vital role in the development, procurement, and widespread application of computers in the US.*

*Education:* MS, statistics, Northwestern University, 1930; MS, statistics, University of Iowa, PhD, Harvard University, 1935.

*Professional Experience:* instructor, mathematics, Johns Hopkins University, 1935-1936; mathematics faculty, Cornell University, 1936-1943; Lt. Commander, Bureau of Ships, US Navy, 1943-1946; National Bureau of Standards: assistant to the director, E.U. Condon, 1946-1947; chief, National Applied Mathematics Laboratories (later the Applied Mathematics Division), 1947-1953; visiting lecturer, Harvard University, 1952; executive director, American Mathematical Society (AMS), 1954-1959; professor of mathematics, University of Miami, Coral Gables, 1959-1977.

John Curtiss was born on December 23, 1909, into an academic environment. His father, D.R. Curtiss (1878-1953), was professor of mathematics at Northwestern University, and president of the Mathematical Association of America in 1935-1936; he wrote a standard introduction to complex variable theory (1926), still in print. His uncle, Ralph H. Curtiss, was professor of astronomy at the University of Michigan.

After graduating with highest honors from Northwestern University in 1930, John Curtiss obtained an MS degree in statistics under H.L. Rietz at the University of Iowa, then one of the leading centers in the Midwest for mathematical training. Two of Curtiss' fellow graduate students there were S.S. Wilks and Deane Montgomery, who went on to distinguished mathematical careers at Princeton University and the Institute for Advanced Study, respectively.

John Curtiss then went to Harvard University, where he earned his PhD in 1935 under Professor J.L. Walsh. His first job after obtaining the doctorate was an instructorship in mathematics at Johns Hopkins University in 1935-1936. In 1936 he joined the mathematics faculty at Cornell University, where he taught until entering the US Navy in January 1943. He was stationed in Washington, D.C., with the quality control section of the Bureau of Ships until April 1946, when he was discharged with the rank of Lt. Commander.

He immediately joined the NBS as an assistant to the director, E. U. Condon, and was initially responsible for statistical matters. On July 1, 1947, he was appointed chief of a new division of the NBS initially called the National Applied Mathematics Laboratories; later the designation Applied Mathematics Division (AMD) was used. John Curtiss remained at the NBS until mid-1953, except for a semester as visiting lecturer at Harvard University in 1952. He spent a year at the Courant Institute of New York University, and was executive director of the American Mathematical Society (AMS) in Providence, R.I., from 1954 to 1959. In 1959 he became professor of mathematics at the University of Miami, Coral Gables, where he worked intensively on one of his first areas of interest: approximation theory in the complex domain. One by-product of this period was a graduate text on complex variable theory (1978). He died of heart failure at Port Angeles, Wash., on August 13, 1977, while en route to the AMS summer meeting in Seattle.

While no one can recall ever seeing John Curtiss at the console of a computer—he always said that "I was involved in the salt mines of computing"—his interest in numerical analysis was considerable. He wrote one paper on numerical algebra (1954b) and edited the proceeding of an important symposium (1956). Although the main body of his work on approximation theory is peripheral to practical computation, as a statistician he was deeply interested in "Monte Carlo" methods. One paper on this subject (1950), delivered at an IBM conference in 1949, was much acclaimed, and another (1954a) was translated into Russian. He gave courses on numerical algebra at NYU in 1953-1954 and on numerical analysis at the University of Miami. During his time in Providence he made a careful analysis of the book-sales policies of the AMS.

John Curtiss was quite sure that the nascent computing fraternity had to become a national society with publications of its own if it were to develop appropriately and be able to exert influence. Thus, he helped enthusiastically in the organization of the Eastern Association of Computing Machinery (ACM), which dropped the regional adjective from its title in 1948. He was the first president in 1947 and always encouraged his staff to participate in the work of this and other professional organizations. Of these, Franz L. Alt, Harry D. Huskey, and George E. Forsythe later became presidents of ACM and Thomas H. Southard was president of SIAM. Curtiss also saw that publications were supported, in particular *Mathematical Tables and Other Aids to Computation* and *The Pacific Journal of Mathematics.*

It is appropriate to point out here that John Curtiss' contributions to the development of modern numerical mathematics cannot be overestimated. He realized that any experienced pure mathematician could find attractive, challenging, and important problems in numerical mathematics, if the person chose to do so. From the death of Gauss in 1855 to 1947, the field of numerical mathematics was, with a few exceptions, cultivated by nonprofessional mathematicians whose real interests lay elsewhere. Accordingly Curtiss recruited professionals from far and near to take part in the programs he had envisaged. Indeed, nearly all of his recruits contributed significantly to his program.

John Curtiss once documented the remarkable success of the operation he planned by counting the papers presented at the 1952 International Congress of Mathematicians by various organizations. NBS was in the middle of the top seven; the others were the University of California at Berkeley, the University of Michigan, the University of Chicago, the Institute for Advanced Study, Harvard University, and the University of Pennsylvania—all organizations founded long before the Applied Mathematics Division of NBS.

For further details of the numerical mathematics programs at the NBS see Lowan (1949), Blanch and Rhodes (1974), and Todd (1975).

## The Prospects

NBS was no stranger to computing equipment. It had been responsible for the Mathematical Tables Project of the Works Progress Administration in New York since 1938. This group was supported during World War II by the Applied Mathematics Panel of the Office of Scientific Research Development and from 1946 by the organization that later developed into the Office of Naval Research (ONR). Led by Arnold N. Lowan, the group included Milton Abramowitz (later chief of the Computation Laboratory), Ida Rhodes, Gertrude Blanch, Herbert E. Salzer, and Irene A. Stegun.

While John Curtiss was first concerned with statistical matters within NBS, he soon had a national responsibility. Several incidents led to this. In 1945, Eckert and Mauchly, who were largely responsible for the ENIAC, approached the Census Bureau (which, like NBS, was part of the US Department of Commerce) with the suggestion that a computer could facilitate its work—in the coming 1950 census, for example. This suggestion was discussed by the Science Committee of the Department of Commerce, which asked NBS for technical advice. The final agreement (April 1946) was that the Census Bureau would transfer funds to NBS, which would select a suitable computer and purchase it. The Army Ordnance Department also transferred funds to the electronics division of NBS for the development of computer components.

At this time Condon instructed Curtiss to survey the federal needs for computers and for a national computing center. This investigation had its source in ONR, and Rear Admiral H.G. Bown suggested that NBS and ONR should jointly establish such a center, to develop as well as use computers. Funds for this purpose were transferred in September 1946. In other countries, similar plans were being considered (see Todd 1975, p. 362).

Curtiss' investigation led to a broadening of the program. He realized very early, for example, that the mathematics needed to exploit the new computers had also to be developed, an opinion shared by Mina Rees of ONR (1977). The program he formulated was described in the prospectus issued in February 1947. The AMD was to have four sections:

1. Institute for Numerical Analysis (INA)—to be a field station at UCLA.
2. Computation Laboratory (CL).
3. Statistical Engineering Laboratory (SEL)
4. Machine Development Laboratory (MDL)

The last three were to be in Washington, D.C. The nucleus of the CL was to be the Lowan group. The program of the AMD was to be guided, within NBS operations, by the Applied Mathematics Executive Council, consisting of representatives of various federal agencies and some outside experts. Later the title of this group was changed to Applied Mathematics Advisory Council (AMAC). A total staff of about 100 was contemplated, and of that only about 30 were on the NBS payroll when the AMD was founded.

The AMD came into being on July 1, 1947. The prospectus had been a remarkable document insofar as there was little need to change its contents as time passed. Curtiss undertook a massive recruitment program to implement the aims of the AMD. He was highly qualified for this activity, with his outgoing personality and many academic contacts. Fortunately, too, the time was opportune for such an expansion, because many mathematicians were being demobilized from their World War II activities—a-number of them fresh from some experience with applied mathematics.

The soundness of the original structure of AMD is clear from the fact that despite a succession of NBS directors and several reorganizations, the organization was essentially unchanged for 25 years, apart from the transfer of the MDL activities elsewhere.

### Procurement Problems

By late 1946, once certain legal situations were resolved, NBS had funds available for two computers. The first was the Univac for the Census Bureau, contracted for with the Eckert-Mauchly organization in 1946, and the second was the NBS computer (financed by ONR), contracted for with the Raytheon Company early in 1947.

The terms of these contracts were discussed by various committees, by advisers (notably, G. Stibitz), and by the AMAC. There were many complications, both administrative and technical. During these discussions the one Univac became three: one for the Air Comptroller and another for the Army Map Service were added.

It is appropriate to mention here the division of responsibilities between the Applied Mathematics Division and the Electronics Division of NBS. The AMD was responsible for the logic design of the computers and their suitability for the jobs envisaged, and for initial liaison with the contractors. The Electronics Division was responsible for the soundness of the design of components and for all engineering matters. Once the development was complete, the divisions were to share the liaison and documentation duties.

The chief of the MDL from 1946 was E.W. Cannon, who succeeded John Curtiss as chief of the AMD in 1953. Ida Rhodes, originally with the Mathematics Tables Project, was active in ensuring the suitability of proposed designs and later in educating the coders and programmers.

Early in 1948, as it became clear that none of these machines would be completed on schedule, two enormously significant events took place. The Air Comptroller, while awaiting the delivery of the Univac, realized that a small “interim” computer to be developed at NBS would provide useful experience. This led to SEAC, discussed below. The Air Materiel Command wanted two computers, one for Wright Field and one for INA, but no supplier could be found. Consequently, it accepted a proposal for a modest machine to be developed at INA by Harry D. Huskey. This led to SWAC.

To end this historical sketch, the Census Univac was completed early in 1951 and was dedicated on June 16, 1951. The Univac for the Air Comptroller was completed in February 1952, and the one for the Army Map Service was completed in April 1952. The Raytheon machine for NBS was never completed, but a related machine was delivered to the Naval Air Missile Test Center at Point Mugu, Calif. in 1952.

**SEAC**

The NBS Interim Computer, later called SEAC (Standards Eastern Automatic Computer), was constructed for the Air Comptroller by a group in the NBS Electronics Division (led by S.N. Alexander), beginning in the fall of 1948. The MDL collaborated in the design, and it was agreed that as soon as the computer became operational it would be moved to the CL. At that time John Todd was the chief of the CL, and they had a considerable group (led by Alan J. Hoffman) working for the Air Comptroller on linear programming, a subject just being developed by G.B. Dantzig and his associates.

In about 15 months SEAC became productive. On April 7, 1950, with help from R.J. Slutz, Todd ran his first program: solving the Diophantine equation $ax + by = 1$. Actually $a$ and $b$ were originally taken to be the largest pair of consecutive Fibonacci numbers that fitted into the machine (<244); this was chosen to give the slowest Euclidean algorithm. The day before, Franz Alt had run a factorization program using a small sieve.

SEAC was dedicated on June 20, 1950. Originally it had a 512-word delay-line memory, but 512 words of electrostatic memory were added. The original Teletype input/output was supplemented by magnetic wire.[1]

On a visit to Los Alamos in 1951, after Todd had described (perhaps too enthusiastically) the current state of CL operations, the laboratory authorities there decided that SEAC was just the thing they needed for their weapon-related computations. Accordingly they preempted SEAC, providing their own crew (for security reasons and to educate them in the use of computers). Even less time was then available for developmental work, and pleas to move the machine to the CL, where they now realized how odd minutes could be used effectively, were rejected on the grounds that the delicate equipment might not survive the trip. John Curtiss finally negotiated with the AEC for the construction of a cinderblock building abutting the SEAC building, and those in direct contact with the machine moved into the new structure. A few years later the machine was moved to the CL, where it operated until it was retired on April 23, 1964.

## SWAC

Earlier, we noted the origin of the Air Materiel Command machine, later called SWAC (Standards Western Automatic Computer). This project began from scratch in January 1949, and the first Williams tube machine to be completed in the US was dedicated on April 7, 1950. Just as the British ACE was designed by a mathematician (A.M. Turing), the SWAC was designed by Harry D. Huskey, who was trained as a mathematician. It was built among and for mathematicians.

There was a rather long period of debugging but in due course all troubles were overcome and SWAC became a reliable machine with many significant accomplishments. When the INA operation closed in 1954, SWAC was transferred to UCLA and remained in operation until 1967.

## Conclusion

John Curtiss, with the full support of NBS director E.U. Condon, and with modest encouragement from various federal agencies, accelerated the progress of the US toward a preeminent position in the construction of computers and their exploitation for scientific computations. We have already mentioned the use of SEAC by the AEC; rocket and comet orbits were computed on commercial equipment at INA (Herrick 1973), and perhaps the first automatically computed earth-moon trajectory was done on SEAC (Froberg and Goldstein 1952).

Had Curtiss been able to stay at NBS and had support for INA been continued, there is no doubt that the mathematical development would have kept up with the enormous achievements of the engineers. Those who remained did what they could to carry on the work for which Curtiss laid solid foundations. Those who were with him at NBS enjoy getting together and recalling the exciting times of 1946 to 1953 and were grateful to have had the privilege of working with a fine American mathematician.

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[1] For more technical information, see NBS (1947, 1950, 1951, 1955), Greenwald et al. (1953), Shupe and Kirsch (1953), and Leiner et al. (1954).

**Postscript--Some Personal Reminiscences-John Todd**

My own first contact with John Curtiss was a letter dated early in 1947, enclosing the prospectus and inviting me to consider joining INA. During World War II, I had been active in organizing an Admiralty Computing Service in Great Britain. Later, with my colleagues A. Erdelyi and D.H. Sadler, I suggested the formation of a National Mathematical Laboratory, later established as a division of the National Physical Laboratory. During that time my wife, Olga Taussky, and I had many contacts with American mathematicians stationed in or visiting Europe, especially H.P. Roberston, H.M. MacNeille, G. Baley Price, R. Courant, and J. von Neumann. They were aware of my activities, and I was in correspondence with members of the Applied Mathematics Panel. Some of these people probably suggested my name to John Curtiss. We arrived in New York on a troop ship late in September 1947. Our first contact with the computer world outside Washington was at the Aberdeen Meeting of ACM on December 11-12, 1947.

John Curtiss was a bachelor who enjoyed fast cars and plenty of good food and drink. In introducing us to Washington society he asked me to arrange a sherry party in his apartment. I provided sherries of varying quality and served them according to his evaluation of the guests, reserving the Bristol Cream for the director. For the benefit of many of the visitors to INA he compiled a list of restaurants labeled according to the civil service gradings P1 to P8.

He did not find the civil service regime too convenient, and much of his activity was spent maintaining contacts with other agencies, often after regular hours. He dictated a diary late at night; a transcription was circulated to his staff the next day so that we were aware of what commitments he had made.

He was not happy on planes and did not travel to Europe until 1976. A letter from him dated May 11, 1976, from the Mathematical Research Institute at Oberwolfach, is addressed to me as “The Savior of Oberwolfach.” (A British naval officer, G.E.H. Reuter, and I were able in 1945 to prevent the dissolution of an institution that has since made great contributions to mathematics, including formal languages, complexity theory, many aspects of numerical analysis, and for instance computerized tomography.) He complained about staying in “magnificent old fire traps” and characterized one of the famous London clubs as “the awfullest fire trap of all, but interesting.” He indicated two remembrances of England, the first “an infinite series of near-head-on collisions,” and the second musical: “I recently got a record of Elgar's organ music at Colston Hall, Bristol, which we inspected amid chaotic preparation for a Salvation Army Choral concert. Then we heard the Sonata itself, included by coincidence in a noon recital in Hertford Chapel in Oxford (and played too slowly).”

My last meeting with John Curtiss was at the 1976 Los Alamos Research Conference on the History of Computing. He said then that he thought that historians, so far, had not fully appreciated the contribution of the National Bureau of Standards in the field. I hope this essay will begin to put things in balance.[1]

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**UPDATES**

# Ole-Johan Dahl

*Born October 12, 1931, Mandal, Norway; with Kristen Nygaard, developer of the SIMULA programming language, which introduced classes and inheritance into the field of programming languages.*

*Education: MS,* numerical mathematics, University of *Oslo, 1957.*

*Professional Experience:* Norwegian Defense Research Establishment (NDRE), 1952-1963; Norwegian Computing Center, 1963-1968; professor, computer science, University of Oslo, 1968-present.

Dahl worked with the Norwegian Defense Research Establishment (NDRE) from 1952 to 1963 in computing and programming under Jan V. Gatwick. From 1956 onwards his main activity was software development. His master's thesis ("Numerical Mathematics," 1957, University of Oslo) addressed the representation and manipulation of multidimensional arrays on a two-level store computer. His main contribution at the NDRE was a high-level programming language, MAC, used locally during the 1960s (first specification was dated 1957; the implemented version was modified as a result of the Algol effort). In 1963 he joined the Norwegian Computing Center for full-time work on SIMULA, and in 1968 he became a professor of computer science, then a new discipline at the University of Oslo. His main research during recent years has been in the areas of program architecture, specification tools, and verification techniques.

From 1964 to 1976 he was the Norwegian delegate to IFIP Technical Committee 2 (Programming Languages), and from 1970 to 1977 he was a working member of IFIP Working Group 2.2 (Language Definition). He has been a member of IFIP Working Group 2.3 (Programming Methodology) since its founding in Oslo in 1969.[1]

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[1] Adapted from the biography in Wexelblat, Richard L., ed., *History of Programming Languages*, Academic Press, New York, 1981, Chapter 9.

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## UPDATES

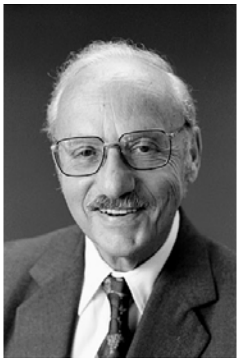
Dahl received the ACM Turing Award (joint with Kirsten Nygaard) in 2001 for ideas fundamental to the emergence of object oriented programming languages. (MRW, 2012)

Ole-Johan Dahl died June 29, 2002 after a long battle with lymphatic cancer. (MRW, 2012).

Portrait changed. (MRW, 2012)

# George Bernard Dantzig

*Born November 8, 1914, Portland, Ore.; inventor of the Simplex method of linear programming.*



*Education:* AB, University of Maryland, 1936; MA, University of Michigan, 1937; PhD, mathematics, University of California, Berkeley, 1946.

*Professional Experience:* junior statistician, US Bureau of Labor Statistics, 1937-1939; statistician, US Air Force, 1941-1945; chief mathematician, US Air Force Hq. Comptroller, 1945-1952; research mathematician, Rand Corp., 1952-1960; professor of engineering science and chairman, Operations Research Center, University of California, Berkeley, 1960-1966; professor of operations research and computer science, Stanford University, 1966-present.

*Honors and Awards:* DSc (Hon.): Israel Institute of Technology, 1973; University Linkoping, Sweden, 1975; University of Maryland, 1976; Yale University, 1978; Exceptional Meritorious Service Medal, War Dept., 1944; National Medal of Science, 1975; Von Neumann Theory Prize, Operations Research Society and Materials Science Society, 1975; Applied Mathematics and Numerical Analysis Prize, National Academy of Science, 1976; member, National Academy of Science; fellow, American Academy of Arts and Science; fellow, Operations Research Society; fellow, Institute for Management Science (president, 1966).

## QUOTATION

"For a short period of time Dantzig almost deliberately tried to avoid discovering the simplex method that made him famous. But logic triumphed over doubt. In a few months of brilliant, concentrated effort in the latter half of 1947, Dantzig conceived the inclusive framework into which the scattered pieces fitted and added a critical missing piece, the simplex method of solution. With that, linear programming was born." (Dorfman 1984)

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## UPDATES

George Dantzig died on May 13, 2005 in Stanford, California. (MRW, 2012)

Portrait inserted (MRW, 2012)

# Charles H. Davidson

*Early user's group leader who created one of the earliest "load-and-go" compilers for Fortran.*

Davidson did his undergraduate work at the American University in Washington, D.C., and received both his MS and his PhD from the University of Wisconsin-Madison, all in physics. His thesis in 1952 involved the design and programming of a digital computer being built in the Department of Electrical Engineering there. In 1961 he organized the Engineering Computing Laboratory in the College of Engineering, and served as its director until 1981. Davidson played an active role in the early days of the 1620 Users Group, serving as chairman of the Midwest Region, and as the representative of its successor organization, Common, to the ASA Fortran standardization committees. In 1961 he supervised the design and development of FORGO, the first load-and-go Fortran compiler. He has participated in many activities within ACM, including serving two terms on the council, and as chairman of SIGCAS for four years. He recently retired as professor of electrical and computer engineering, professor of computer sciences, and assistant to the director of the Madison Academic Computing Center for Instructional Computing.

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## UPDATES

# Donald W. Davies

*Born June 7, 1924, Treorchy, Wales; worked on the Pilot AGE, one of the first operational stored-program computers, and later was responsible for the concept of packets in network communication.*

*Education:* BS (Honors), physics, Imperial College, London University, 1943; BS (Honors), mathematics, Imperial College, London University, 1946.

*Professional Experience:* National Physical Laboratory, 1947-1984; private consultant, 1984-present.

*Honors and Awards:* John Player Award, British Computer Society, 1974; distinguished fellow, British Computer Society, 1975; John von Neumann Award, John von Neumann Society, Budapest, 1983; DSc (Hon.), Salford University, 1983; Commander of the British Empire (CBE), 1983; fellow, Royal Society, 1987.

Donald Davies graduated in physics with first-class honors from Imperial College, University of London, 1943, at the age of 19. Thereafter his wartime work was mainly related to the application of numerical mathematics to fluid flow and diffusion problems on an industrial scale. In 1947 he graduated from Imperial College with a second first-class honors degree in mathematics.

After his second graduation he joined the team of scientists at the National Physical Laboratory, which built the ACE Pilot Model, then the fastest of the three pioneer digital computers in the UK. He designed the input/output equipment, and the arithmetic and logic units. He used the computer, among other applications, to simulate road traffic control and to optimize the settings of controllers, and to simulate warning and escape systems in coal mines.

From 1955 to 1965 he was a project leader for a number of research projects at NPL. He played a leading role in the design of the full-scale ACE computer. His projects included such diverse topics as machine translation from Russian into English, and the development of the Cryotron, an early superconducting logic and storage device. His interest and involvement with the commercial application of computers began during this period when he led a group investigating the choice of magnetic characters for checks for the London Clearing Banks.

Davies was the first project leader of the Advanced Computer Technology Project for the UK Ministry of Technology—a precursor of the 1980s Alvey Project. Among its successes were the ICL Distributed Array Processor (DAP) and the Context Addressable File Store (CAFS).

In 1965 Davies pioneered new concepts for computer communications in a form to which he gave the name "packet switching." He introduced this concept to the UK Post Office (at that time the equivalent of a PTT) in 1966 and to the CCITT and the US Advanced Research Projects Agency (ARPA) in 1967. The design of the ARPA network (ArpaNet) was entirely changed to adopt this technique.

In 1966 Davies became head of the computer science division of NPL. He initiated research in data communications at NPL including the building of a packet switched local network (completed in 1971), and simulation studies of flow control, congestion, and routing in networks. He wrote and lectured widely to promote the concept of a data communication system with well-defined interfaces and protocols. At CCITT, on behalf of the UK Post Office, he helped to formulate some of the X recommendations for data communication services.

Other research of the computer science division at this time was in pattern and speech recognition, CAD in architecture, human factors, and office systems that used the local network.

In 1978 he was given an "individual merit" post as deputy chief scientific officer, allowing him to lead a research group without the management tasks of a division. He chose as his specialty the security of data in networks. The group developed the application of cryptographic methods to the practical needs of network security, especially the use of asymmetric (public key) cryptography. Consulting work under contract to financial institutions and others provided the practical experience.

Since leaving NPL in 1984, Davies has provided consultancy to financial institutions on high value payment systems (SWIFT and CHAPS), ATMs, and EFT/POS. He has advised suppliers and users of secure systems of many kinds, including mobile telephone and direct broadcast satellites.

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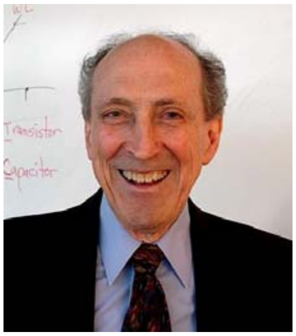
[1] Known in the US as "Tic Tac Toe."

## UPDATES

Portrait added (MRW, 2012).

## Robert H. Dennard

Born 1932, Terrell, Texas; National Medal of Technology winner for IBM invention of the basic, one-transistor dynamic memory cell used in virtually all modern computers.



Education: BS, electrical engineering, Southern Methodist University, 1954; MS, electrical engineering, Southern Methodist University, 1956; PhD, Carnegie Institute of Technology, 1958.

Professional Experience: IBM: Research Division, 1958-present; Thomas J. Watson Research Center, Yorktown Heights, 1963-present.

Honors and Awards: member, National Academy of Engineering, 1984; IBM fellow, 1979; fellow, IEEE, 1980; National Medal of Technology, 1988; IEEE Cledo Brunetti Award, 1982; Industrial Research Institute Achievement Award, 1989; Harvey Prize, Technion, Haifa, Israel, 1990; DSc (Hon.), State University of New York, Farmingdale, 1990; six IBM Outstanding Invention and Outstanding Contribution Awards; two IBM Corporate Awards.

Robert Dennard joined the IBM Research Division in 1958, where his early work included the study of new devices and circuits for logic and memory applications, and the development of advanced data communication techniques. Since 1963 he has been at the IBM Thomas J. Watson Research Center, Yorktown Heights, New York, where he has been involved in research and development of microelectronics from its inception.

Starting in the mid-1960s, he was a leader in the development of IBM's N-channel MOSFET[1] devices and technology. This work led to the first IBM MOSFET memory products in the early 1970s, and a number of his circuit and device innovations were in the products. In 1967, he invented the one-transistor DRAM memory cell and obtained a basic patent for IBM. In the early 1970s, he led a group working on scaling MOSFET devices to smaller dimensions, and in 1974 published, with his colleagues, the historic paper on MOSFET device scaling theory. In the 1970s he also worked on yield models for integrated circuits, which led to development of the theoretical base for word and bit line redundancy in DRAMs. In recognition of his contributions, he was appointed an IBM fellow in 1979. Through the 1980s to the present, he has continued to be a leader in the IBM efforts to miniaturize devices and integrated circuits. This work has culminated in the demonstration of sub-0.1-micron MOS devices. He has also been a leader in setting directions for IBM technology research and development.

1. Dr. Dennard has received six IBM Outstanding Invention and Outstanding Contribution Awards, and two IBM Corporate Awards. The invention and contribution awards were for:
   1. MOSFET device design,
   2. the MOSFET technology and device design manual used throughout the company in the early days of MOSFET development,
   3. the one-transistor DRAM memory cell,
   4. word and bit line redundancy,
   5. MOSFET scaling theory, and
   6. 1-micron latch-up-free CMOS technology development.

[1] MOSFET—metal-oxide-semiconductor field effect transistor.

The corporate awards were for the 1-transistor memory cell and for the MOSFET scaling theory.

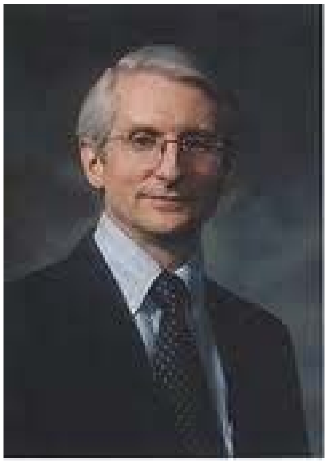
## UPDATES

Dennard has also received the following awards: IEEE Edison Medal (2001); The Franklin Institute Benjamin Franklin Medal (2007); Charles Stark Draper Prize (2009); IEEE Medal of Honor (2009); Honorary DSc from Carnegie Mellon University (2010).

Photo added (MRW, 2012)

## Peter J. Denning

*Born January 6, 1942, New York City; computer scientist whose work on virtual memory systems helped make virtual memory a Permanent Part of modern operating systems.*



*Education:* BEE, Manhattan College, 1964; SM, electrical engineering, MIT, 1965; PhD, electrical engineering, MIT, 1968.

*Professional Experience:* assistant professor, electrical engineering, Princeton University 1968-1972; Purdue University: associate professor, 1972-1975; professor of computer sciences, 1975-1984, head, Computer Sciences Department, 1979-1983; Research Institute for Advanced Computer Science (RIACS), NASA Ames Research Center, Mountain View, Calif.: founding director, 1983-1990, research fellow, 1990-1991; associate dean for computing and chair of the Computer Science Department, School of Information Technology and Engineering, George Mason University, 1991-present.

*Honors and Awards:* teaching award, Princeton University, 1971; ACM Service Award, 1974; two best paper awards: "The Working Set Model for Program Behavior," *Comm. ACM*, May 1968; and "Operating Systems Principles and Undergraduate Computer Science Curricula," *Proc. Spring Joint Computer Conference,* 1972; fellow, IEEE, 1981, for "contributions to the understanding of virtual memory systems and the development of the working set concept"; fellow, American Association for the Advancement of Science, 1984, "for outstanding contributions to computer systems development and computer security, and for service to the profession and to his professional society"; doctor of law, Concordia University, 1984; doctor of science, Manhattan College, 1985; ACM Distinguished Service Award, 1989; Centennial Engineering Award, Manhattan College, 1992.

Peter J. Denning is associate dean for computing and chair of the Computer Science Department in the School of Information Technology and Engineering at George Mason University. He took up this appointment in August 1991.

Denning was the founding director of the Research Institute for Advanced Computer Science (RIACS) at the NASA Ames Research Center in Mountain View, Calif. He served in that capacity from 1983 to 1990, when he stepped down and became research fellow until August 1991.

Before accepting the RIACS assignment, Denning was head of the Computer Sciences Department at Purdue University; where he was a professor of computer sciences (1975-1984) and an associate professor (1972-1975). He was an assistant professor of electrical engineering at Princeton University (1968-1972). Denning was one of the four cofounders of the CSNET, which began with NSF support and evolved into the first fully self-supporting community network; CSNET is a predecessor of the NSFNET and the NREN. He has worked closely with NASA on computational science and on the high-performance computing and communications program.

Denning's primary research interests are computer systems architecture, parallel computation, operating systems, performance modeling, and organizational informatics. He has published over 230 papers and articles since 1967. His work on virtual memory systems helped make virtual memory a permanent part of modern operating systems. His book with E. G. Coffman, Jr., *Operating Systems Theory,* was published by Prentice-Hall in 1973 and is still widely used today. His book with Jack Dennis and Joseph Qualitz, *Machines, Languages, and Computation,* was published by Prentice-Hall in 1978. His edited collection, *Computers Under Attack: Intruders, Worms, and Viruses,* published by Addison-Wesley in fall 1990, is a best-seller.

Denning was the president of the Association for Computing Machinery (1980-1982). He has participated actively in the ACM since 1968, where he has served as chairman of the Special Interest Group on Operating Systems (SIGOPS) (1969), chairman of the Board on Special Interest Groups (1970-1974), member-at-large of Council (1974-1978), and vice president (1978-1980). In June 1984 he retired from Council after 14 years of service. He served as chairman of the Task Force on the Core of Computer Science (1986-1988), as chair of the ACM Editorial Committee, and member of the Publications Board (1986-1992). He was elected chair of the Publications Board in 1992.

Denning served as editor-in-chief of the *Communications of the ACM* (1983-1992), which under his guidance has become the leading technical magazine in computing. During this period, he radically altered the character of the journal from a research publication to an up-to-date communication for practitioners. He continues as a contributing editor of the *Communications.* He is an associate editor of *Acta Informatica.* He was consulting editor for computer science for the MIT Press, editor-in-chief of ACM's *Computing Surveys,* and editor of the Elsevier/North-Holland Series on Operating and Programming Systems. He was writer of the column, “The Science of Computing,” in each issue of *American Scientist* from January 1985 through October 1993.

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Denning, Peter J., *Computers Under Attack: Intruders, Worms, and Viruses,* Addison-Wesley, Reading, Mass., 1990.

## UPDATES

## John H. Dessauer

*Born May 13, 1905, Aschaffenburg, Germany; died August 12, 1993, Rochester, N. Y; directed research and engineering at Xerox Corporation for 33 years, having been the major influence in persuading the tiny Haloid Company to acquire Chester Carlson's electrostatic photographic reproduction process and develop it into the technology which was the basis of xerography and the Xerox success.*

*Education:* BS, Institute of Technology, Munich; MS, Institute of Technology, Aachen; PhD, Institute of Technology, Aachen.

*Professional Experience:* Agfa Ansco, Binghamton, N.Y, 1929-1935; director of research, director of research and engineering, director of research and advanced engineering, executive vice president, member and vice chairman of the board of directors, Xerox Corp.,[1] 1935-1970; retired, 1970.

Dessauer was born in Aschaffenburg, Germany, on May 13, 1905. He first studied liberal arts at the Albertus Magnus University in Freiburg and then chemical engineering at the Institute of Technology in Munich, where he obtained the equivalent of a bachelor of science degree. He received his master's and doctoral degrees in engineering sciences at the Institute of Technology in Aachen, both magna cum laude. He emigrated to the US in 1929, and first joined the research department, and later the photographic paper manufacturing department, of Agfa Ansco in Binghamton, N.Y In 1935 Dessauer joined the Rectigraph Company in Rochester, which shortly afterwards was acquired by the Haloid Company, a small manufacturer of copying cameras and silver-halide photographic papers. It was one of several firms that in the World War II era offered wet-process machines and material to make photocopies of documents. Dessauer established Haloid's research department.

Early in 1945 Dessauer, looking for new product opportunities, came upon an article from the July 1944 issue of *Radio News* which described Chester F. Carlson's electrophotography process. He later recalled, “It was as if lightning struck when I read that article. What came to mind first was that it could be used for reproducing documents and letters.” After some preliminary investigation, Dessauer and the new young vice president of Haloid, Joseph C. Wilson, visited the Battelle Memorial Institute in Ohio where Carlson gave a manual and very messy demonstration of his novel copying process. The two Haloid executives were impressed, saw the problems and the possibilities, and as of January 1, 1947, took a limited license. It was an offer that had been turned down by dozens of others, including GE, RCA, IBM, and Remington-Rand, which later became major computer companies.

Carlson's copier concept was embryonic and far from commercial viability. It took Dessauer's fervent conviction of its potential value, which he demonstrated in enthusiastic leadership, to resolutely and persistently attack the thousands of problems in the poorly understood technologies of photoconductivity and electrostatics that had to be overcome.

A corporate obituary had this to say of him [Bickmore et al. 1993]:

> The uncertainties of pioneering the unproven technology required great faith, perseverance, and courage. “JD,” as he was affectionately known to the technical community, embodied all of those qualities in abundance.

[1] Rectigraph Co., Rochester, N.Y, acquired by Haloid Co., which became Xerox.

> He skillfully assembled and managed a team of young, creative engineers and scientists, and guided the multi-functional efforts needed to solve problems ranging from solid state physics, to optics, to the chemistry of polymers and pigments, and to mechanical design. He instilled a spirit of teamwork in R&D groups by recognizing the individual skills within the groups, thus avoiding intra-team rivalries. Dessauer's conduct at staff meetings was formal and somewhat authoritarian in approach; yet he showed a ready smile and a personal warmth that inspired trust.
>
> Dr. Dessauer also maintained effective interactions with the business staff. He viewed himself as a “transducer” between the technical and business communities, and, as he wryly noted, at times this required him to act as a “filter.” On the one hand, he shielded the technical staff from unrealistic business pressures; on the other hand, he reassured the business staff that seemingly insurmountable technical problems could be solved. His overall appreciation of both technical and business issues allowed him to plan and direct the R&D work in a manner consistent with business realities-the needs of customers were foremost objectives in all technical activities.

In 1949 Haloid made its first abortive attempt to market a product based on xerography, a Haloid-invented name. The Xerox (with a capital “X”) Model A Copier (also called the “Ox Box”), was really three machines with one operator who had to execute 39 manual steps in three minutes and transfer a flat, dirty, heavy metal plate from each machine to the next for every single copy to be made. It worked, but nobody wanted to lease it.

This market failure shook even Dessauer's confidence, but although the Model A failed as an office copier, a redesigned version found success producing paper master plates for high volume offset duplicating. This fortuitous application supplied cash for further development. The Model A also showed that a successful office copier had to be child-simple in use.

In 1955 the Xerox Copyflow was tried out on the public. It was semiautomatic and made continuous copies on ordinary paper. It lacked the necessary simplicity. It was not what was needed.

Finally, in 1959, almost 14 years and $75 million after Dessauer first looked at Carlson's process, Haloid Xerox Inc., as it had now become, offered the revolutionary 914 copier, so named because it could copy sheets as large as 9 by 14 inches) This, the first automatic plain-paper office copier, swept all other copiers from the market, including Mimeograph, Photostat, ThermoFax, and Verifax. The 914 and its many successors, copies, and clones, foreign and domestic, became as indispensable as the telephone and changed office life and office practices forever. “Xerox” became a household term and, in spite of the corporation's protests, is now a lowercase addition to the languages of the world as a generic noun and verb.

In that same year Dessauer became executive vice president. In this role he directed an explosive expansion in R&D personnel and facilities. He gave thought to the nurturing and management of creative scientists. Appreciating that peer recognition could be a powerful motivator, he made sure that key contributors had opportunities to present their findings at technical seminars. He was also willing to let creative scientists fail occasionally, for in his view, the only unacceptable behavior was *inaction*. Recognizing that creative people may not be managers, he instituted a “dual ladder” promotion policy to reward valuable technical and scientific employees without requiring that they become administrators.

He, as well as Wilson, always felt deeply about the social responsibilities of corporations, and turned this concern into social awareness supported by action that became a mark of identity for Xerox.

Dessauer continued as executive vice president of Xerox until 1968, when he relinquished the office to an executive from outside Xerox. His replacement, Jack Goldman of Ford, was said to have been surprised to find that the concentration of Xerox's management and research and development, under Dessauer's direction, had been so narrowly focused on xerography that they had very little understanding of the world of digital technology [Bickmore et al. 1993] except, perhaps, that involved in digital imaging.

A member of the board of directors from 1946, Dessauer was vice chairman of the board and executive vice president in charge of the Research and Advanced Engineering Division from 1966 to 1970, when he retired. He participated in the corporate decision process that led to the 1969 purchase of "$900 million in stock of the computer vendor" Scientific Data Systems (SDS), and in the next year to the creation of the Xerox Palo Alto Research Center (PARC). The purchase of SDS is now seen as the first of several ill-advised and abortive efforts by Xerox to buy its way into computing, while PARC, which pioneered in digital imaging, is now recognized as having essentially invented the personal computer, "windows," Ethernet, and the laser printer, all of which were largely ignored by Xerox but accepted and exploited with great success by its competitors.

After retirement, Dessauer set up an office near his home in Pittsford, N.Y., from which he gave financial assistance to charities. He was a trustee, board member, and adviser to several charitable and educational institutions. He was a fellow of the New York Academy of Sciences and of the American Institute of Chemists, a member of the National Academy of Engineering, and in the last year of his life, was made an honorary member of the Society for Imaging Science and Technology. Dessauer held eleven patents. He co-edited *Xerography and Related Processes*, published in 1965, the first technical textbook about the subject. In 1971 Doubleday published his now out-of-print autobiography, My *Years with Xerox: The Billions Nobody Wanted.*[1]

## QUOTATIONS

At the time of Dessauer's death, as part of a brief press release, Paul A. Allaire, Xerox chairman and chief executive officer, eulogized, "No history of the commercial development of the xerographic process or Xerox Corporation would be complete without early and prominent tribute to the many contributions made by John Dessauer. All of us at Xerox owe a great deal to him for our jobs, our company, and our industry. The world is a different place because of the part he played in making the office copier a vital element in propelling all of us into the information age."

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[1] By Eric Weiss.

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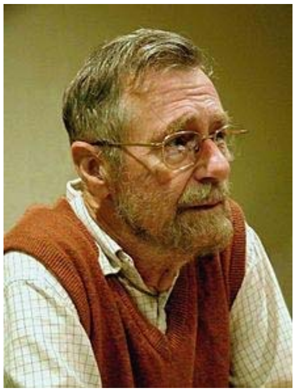
## UPDATES

John H. Dessauer died August 12, 1993. (MRW, 2012)

## Edsger W. Dijkstra

*Born 1930, Rotterdam, The Netherlands; Leading critic of programming without a mathematical proof of correctness and condemner of the infamous GOTO; recipient of the 1972 ACM Turing Award.*



*Education:* MS, mathematics and theoretical physics, University of Leiden, 1956; PhD, computing science, Municipal University of Amsterdam, 1959.

*Professional Experience:* professional programmer, Mathematisch Centrum, 1952-1962; professor of mathematics, Eindhoven University of Technology, 1962-1984; research fellow, Burroughs Corp., 1973-1984; Schlumberger Centennial chair, computer sciences, University of Texas at Austin, 1984-present.

*Honors and Awards:* fellow, Netherlands Royal Academy of Sciences; distinguished fellow, British Computer Society, 1972; ACM Turing Award, 1972; IEEE Computer Society Pioneer Award, 1980.

Dijkstra, recipient of the 1972 ACM Turing Award, is known for early graph-theoretical algorithms, the first implementation of Algol 60, and the first operating system composed of explicitly synchronized sequential processes. He is also credited with the invention of guarded commands and of predicate transformers as a means for defining semantics, and programming methodology in the broadest sense of the term. In recent years he has been deeply involved in the applications of mathematical proof techniques to programming and the development of programs from mathematical axioms.

At the 1972 ACM Turing Award ceremony M. Doug McIlroy read the following citation:

> The working vocabulary of programmers everywhere is studded with words originated or forcefully promulgated by E.W. Dijkstra: “display,” “deadly embrace,” “semaphore,” “go-to-less programming,” “structured programming.” But his imprint on programming is more pervasive than any catalog of jargon can indicate. The precious gift that this Turing Award acknowledges is nothing less than Dijkstra's style-his approach to programming as a high intellectual challenge; his eloquent insistence and practical demonstration that programs should be composed correctly, not just debugged into correctness; and his illuminating perception of problems at the foundations of program design. He has published about a dozen papers, both technical and reflective, among which are especially to be noted his philosophical addresses at IFIP (1962, 1965a), his already classic papers on cooperating sequential processes (1965b, 1968a), and his memorable indictment of the go to statement (1968b). An influential series of underground letters by Dijkstra have recently been published in monograph on the art of composing programs (1971).
>
> We have come to value good programs in much the same way as good literature. And right at the center of this literary movement, creating, and reflecting patterns no less beautiful than useful, stands E.W. Dijkstra.

His interests focus on the formal derivation of programs and the streamlining of the mathematical argument. His publications represent only a minor fraction of his writings—he writes, in fact, so much that he cannot afford the use of time-saving devices such as word processors. He owns, however, several fountain pens, three of which are Mont Blancs, for which he mixes his own ink. His writings, which include technical papers, trip reports, and essays on various topics, are distributed in an informal distribution tree to many colleagues. The latest is “numbered” EVVD1070, which is an indication of how prolific he has been.

In 1964 at a WG 2.3 meeting in Baden bei Wein, van Wijngaarden was showing how to get rid of GOTOs by replacing them with recursive procedure calls that never return. Dijkstra's reaction to this academic result was to spend the evening deriving programs that had no GOTOs in the first place. On the terrace during next morning's coffee break, and throughout the day, he peddled his new style, pointing out that many programs became simpler in the process and that none got harder. He invented a LOOP-EXIT statement to solve some structural problems. In less than 24 hours, Dijkstra had converted a sterile academic exercise into a movement that would shake the field when the eruption came later in 1968 with his famous letter on “the GOTO considered harmful.[1] Most people regard his 1968 letter as the start of the eliminate-the-GOTO movement; few realize that it began as a reaction to an academic talk in 1964.

Perhaps the measure of this man is best expressed by those he has influenced, and who more influenced and better placed than his students? This influence has been caused by his particularly perceptive and brilliant mind, his intense desire to be professionally honest, a discipline that is unequaled, and a way with the pen (in both form and content) that others would kill to attain. His ability to make a decision on technical grounds and then to put it into practice is unrivaled. He seems to have been endowed with all the good qualities one would like to see in a scientist, and he has taken care to sharpen them. On the occasion of his 60th birthday, the University of Texas organized a celebratory symposium on the “Frontiers of Computing” primarily staffed by his disciples. This was an occasion to provide insights of the person behind the facade of the “professor.” Even Dr. W.S. Livingston, vice president and dean of graduate studies of the University of Texas at Austin, could not resist relating his “Dijkstra experience”:

> In 1983 Dr. Dijkstra was being interviewed by the University of Texas at Austin to determine his suitability for appointment to the distinguished position. Someone decided that it was my task as vice president (even though I am by trade a political scientist) to conduct an interview on behalf of the University administration. I was not quite certain just what topics we might discuss, but Professor Dijkstra soon solved that problem. After some very short preliminaries he stood up and provided me with a lecture on his thoughts on the subject, striding up and down in my office. It is not at all clear to me just who interviewed whom.

Tony Hoare, himself a Turing Award winner and pioneer (but not a student of Dijkstra's), told of their first meeting, which exemplifies the discipline of programming that Dijkstra espoused (see Dijkstra 1976):

> The first time I visited Edsger in Eindhoven was in the early Seventies. My purpose was to find out more about the THE operating system,[2] which Edsger had designed. In the computing center at which the system was running I asked whether there was really no possibility of deadlock. “*Let's* see” was the answer. They then input a program with an infinite recursion. After a while, a request appeared at the operator's console for more storage to be allocated to the program, and this was granted. At the same time they put a circular paper tape loop into one of the tape readers, and this was immediately read into buffer file by the spooling demon. After a while the reader stopped; but the operator typed a message forcing the spooler to continue reading. At the same time even more storage was allocated to the

[1] Dijkstra, Aug. 1968.

[2] Dijkstra, May 1968.

recursive program. After an interval in which the operator repeatedly forced further foolish storage allocations, the system finally ground to a complete halt, and a brief message explained that storage was exhausted and requested the operator to restart operations.

So the answer was YES; the system did have a possibility of deadlock. But what interested me was that the restart message and the program that printed it were permanently resident in expensive core storage, so that it would be available even when the paging store and input/output utilities were inoperative. And secondly, that this was the very first time it had happened. I concluded that the THE operating system had been designed by a practical engineer of high genius. Having conducted the most fundamental and far-reaching research into deadlock and its avoidance, he nevertheless allocated scarce resources to ensure that if anything went wrong, it would be recognized and rectified. And finally, of course, nothing actually ever did go wrong, except as a demonstration to an inquisitive visitor.

David Gries remembered that Dijkstra's main contributions have been in programming methodology, and that he was one of the founders of IFIP Working Group 2.3 in 1970. On the other hand the remembrance indicated that Dijkstra is not infallible:

WG 2.3 met in a log hotel overlooking Oslo during the week that man landed on the moon. During that summer week, Edsger, slightly short of breath while climbing a steep hill during an outing, said he did not believe that programming as a field of research would last another ten years-fifteen at the outside. Wad Turski says it is a pity that he did not challenge Edsger with a bet at the time, for he would have won. Turski was hesitant to bet because he had just lost a case of cognac: almost a decade earlier, at a New Year's party in Moscow, Turski bet a Russian scientist that man would not set foot on the moon before December 31, 1969, so Wad had just lost that bet by five months!

An unsubstantiated anecdote illustrates to what lengths people go to get the upper hand on Edsger:

After Carel Scholten had built one of the early computers at the Mathematical Centre [Mathematisch Centrum, Amsterdam], Edsger claimed that nobody could write a shorter routine than his for some problem, and he offered a free meal to whoever could beat his routine (quite a bold bet for a Dutchman). He lost his bet, because Carel Scholten secretly added an instruction to the machine just so that he could write a shorter program! Thus, Edsger lost his one and only bet!

Dijkstra watchers, be they students of his lectures, or lecturers who have had him in the audience, are often perturbed by his lecturing and listening activities. Students are irritated by his habit of pausing between sentences to think about what he is to say next. Asked about it on one occasion he pointed out that English is not his native language and he picked up the habit early in his using the language. Doug McIlroy (Bell Telephone Laboratories) recalled the penury of a speaker who finds Dijkstra in his audience:

As the speaker drones on, Edsger will become displeased at something, or begin thinking about something the speaker said. The body will rise, the sandals will come off, and the walking at the back of the room will begin. The unsuspecting new lecturer will continue blithely on. A more experienced lecturer will suspect and begin to worry. If he can contain himself, Edsger will wait until the end of the lecture, but sometimes he just has to interrupt. A snort will erupt, the nostrils will flare, the chin will elevate, and out will come an inspired, amazingly logical and eloquent, commentary. Both parties will emerge pleased, one for having vanquished stupidity, the other for having evoked the commentary and

> for the understanding they have gained. In the long run, this supreme effort of abrasion has polished the understanding of both.

McIlroy recalls only once that an eruption went supercritical. Unfortunately, the verbal outburst was saved for the end, and when it came, it lacked all divine inspiration: *"This stuff makes me sick!"* he thundered. Understanding was nonetheless polished, and two years later Dijkstra had taken up the topic himself.

David Gries (Cornell University) was one of the recipients of "on-line" coaching during a lecture:

> My own experience with lecturing before Edsger took place in Marktoberdorf. It rests on the fact that in some languages (notably Fortran), the equality symbol and the assignment symbol are the same, and many people say *"x equals e"* when they mean *"store the value of e in x,"* or *"x becomes e."*[1]
>
> I was lecturing along, when I said *"x equals e" meaning* an assignment of e to x. From the back of the room came a loud *"becomes," and* then a stunned silence. Finally, I gathered my wits and said, "Thank you, Edsger, for correcting me. If I make the same mistake again, stop me." Twenty minutes later, I made the same mistake, and again from the back of the room came *"becomes" "Thanks, I won't make the mistake again,"* I said, and to this day I haven't!

Without doubt Edsger Dijkstra, for all his technological contributions, epitomized by many to be the "GOTO" letter in the *Communications of the ACM,* is one of the "characters" of the field. He is difficult to predict. The titles of the two lectures he gave on accepting the ACM Turing Award and the SIGCSE Education Award are typical of this proclivity: "The Humble Programmer" and "On the Cruelty of Really Teaching Computer Science."

## QUOTATIONS

"The question of whether computers can think is just like the question of whether submarines can swim." (Attrib.; posted on the CMU Board, December 1986)

I would require of a programming language that it should facilitate the work of the programmer as much as possible, especially in the most difficult aspects of his task, such as creating confidence in the correctness of his program. This is already difficult in the case of a specific program that must produce a finite set of results. But then the programmer only has the duty to show (afterwards) that if there were any flaws in his program they apparently didn't matter . . .' ("On the Design of Machine Independent Programming Languages," *Ann. Rev. in Auto. Prog., Vol. 3)*

"For the absence of a bibliography I offer neither explanation nor apology." *(A Discipline of Programming)*

"Program testing can be used to show the presence of bugs, but never to show their absence." *(Structured Programming, 1969 NATO* Conference)

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[1] More precisely one should say "store the representation of the value *e* in the memory location associated with the name *x*." Ed.

"Suffering as I am from the sequential nature of human communication ......

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## UPDATES

Edsger W. Dijkstra died August 6, 2002.

Dijkstra also received the following awards: an honorary doctorate from the Athens University of Economics and Business (2001) and the NEC Foundation Computer and Communications Prize (2002).

Portrait added. (MRW, 2012)

## Philip Henry Dorn

*Born August 14, 1930, New York City; died June 8, 1993, New York City; software pioneer, champion of computing, skilled industry analyst, respected consultant, gifted writer and speaker, and friend and supporter of people in the world of computing and beyond.*

*Education:* BA, political science, Princeton University, *1952*.

*Professional Experience: US* Army, 1953-1956; Personnel Laboratory, Inc., 1956-1958; System Development Corp. (SDC), 1958-1961; General Motors Research Laboratories (GMR), Warren, Michigan, 1961-1965; Computer Applications, Inc., New York City, 1965-1966; Technical Services Group, Union Carbide Corp., New York City, 1966-1972; The Equitable Life Assurance Society, 1972; Dorn Computer Consultants, Inc. 1972-1993.

Philip Henry Dorn, software pioneer, champion of computing, skilled industry analyst, respected consultant, gifted writer and speaker, and friend and supporter of people in the world of computing and beyond, died suddenly while returning by taxi to his New York City home from a ballet performance on June 8, 1993. He was 62 years o1d.[1]

Dorn was born on August 14, 1930, and brought up in New York City, in Manhattan. He attended the Lawrenceville School in New Jersey, from which he graduated in 1948. Discouraged by his parents from pursuing studies in engineering, he earned a BA in political science from Princeton University in 1952. He then entered the law school at Stanford University. At Stanford he met Sue Bricker, a native of the Pacific Northwest, who became his wife in 1955 and was at his side when he died.

Dorn was drafted out of law school in 1953, and, while serving in the Army, he decided that law was not for him. In 1956, following his discharge and marriage, he took a customer-liaison position with a firm, Personnel Laboratory, Inc., that specialized in testing candidates for employment. This enabled him to support his family, but he became restless and intellectually dissatisfied.

In 1958, with his wife's encouragement, he answered an employment advertisement placed by System Development Corporation (SDC). In those days, when most people had no concept of computers, SDC would simply seek people who were intelligent and intellectually curious—which described Dorn perfectly. He told the interviewer that he had no background in mathematics or engineering, but his score on a screening examination was off the top of the scale, and he was offered a position.

SDC had major responsibilities for programming the SAGE air defense system on AN/FSQ-32 computers. [See the Special Issue on SAGE, *Ann. Hist. Comp., Vol.* 5, No. 4, Oct. 1983.] There was no ready supply of programmers; computer science departments did not exist, and few universities even offered serious courses in programming. So SDC ran its own school, offering its employees introductory courses in “bits, bytes, ones, and zeros” and advanced courses in air defense applications.

[1] Rosin 1994.

Dorn excelled in both the introductory and advanced courses and, upon completing the latter, became one of SDC's instructors. His teaching colleagues found him delightful to work with. But he also had very high standards. If Dorn respected a colleague intellectually, their relationship could be very positive; but woe to a lesser person who let Dorn get the upper hand.

Although he enjoyed his work and the opportunity to play tennis year-round, Dorn was not cut out for Southern California-in particular, he did not drive a car-so in 1961 he joined the staff at General Motors Research Laboratories (GMR) in Warren, Michigan.

At this time, an IBM-7090 computer was installed at GMR to support development of a system for vehicle body design. The resulting system, DAC-1, was the first industrial computer-aided design system and the system that pioneered the use of computer graphics in industry. [See Krull, F., "The Origin of Computer Graphics within General Motors" *IEEE Annals of the History of Computing, Vol.* 16, No. 3, 1994, pp. 40-56.]

Along with a colleague, Phyllis Cole, Dorn's role in the DAC-1 project was to develop the disk-based memory system, including the design of a storage access method. The work began before any disk drive was available for the IBM-7090, so Dorn and Cole simulated the storage system they were designing, first using tape drives, and then with an IBM-1401 that had a very early disk drive. Dorn wrote and checked out the first customer-developed programs for the IBM-1301 disk drive at IBM prior to delivery of that hardware to GM.

Dorn's professional relationship with Cole was typical of the way he interacted with women throughout his career; he treated them equally with men. Women whom he respected intellectually, and there were many, became trusted colleagues and friends. He also encouraged women, telling one who expressed doubts, "You are not a woman manager—you are a manager!"

While DAC-1 was the most exciting experience in his entire career-and Michigan was closer to home than California-Dorn was an inveterate Manhattanite. So in 1965 he joined Computer Applications, Inc., in New York City, where he co-directed implementation of the Indexed Sequential Access Method (ISAM) for OS/360 under a contract with IBM.

In 1966 Union Carbide Corporation formed a corporate-level Technical Services Group in New York City, and Dorn was one of four people selected to staff it. This group established policies and plans for third-generation computing in Union Carbide, and its members served on related committees and projects in data centers throughout the corporation. Here Dorn honed his skills as a technical consultant. He had a reputation for offering an opinion on everything, but that opinion was formed on the basis of what he heard and read-and he always knew everyone else's opinion. When the Technical Services Group was broken up a few years later, its members were assigned to various Union Carbide data centers. Dorn, of course, stayed in New York.

While at GM, Dorn had become involved in SHARE, then known as the "IBM user group for large, scientific computers."[1] During his 12-year formal relationship with SHARE, he managed the SHARE PL/I Project and its Systems Division, served as SHARE vice president and, in 1969-1970, as president. After his presidency, he served another term on the SHARE Board as past president, and he continued to consult for the SHARE management long after he was affiliated with a SHARE member corporation.

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[1] See Armer, Paul, "SHARE-A Eulogy to Cooperative Effort," *Ann. Hist. Comp.*, Vol. 2, No. 2, Apr. 1980, pp. 122-129.

The 1960s were years of significant turmoil in SHARE. Among other major issues, IBM announced and delivered its System/360 and OS/360, and in 1969 IBM announced its decision to “unbundle” its software and sell it separately from its hardware products. Dorn took a leading role in developing SHARE's response to this announcement, which resulted in vendors other than IBM participating in SHARE and SHARE's incorporation as a tax-exempt organization. An attorney retained by SHARE during this period credits Dorn with teaching him to write with a precision that, he says, still distinguishes him from most other lawyers.

Dorn left Union Carbide in late 1972 and, after a short stint with the Equitable Life Assurance Society, began independent consulting under the rubric Dorn Computer Consultants, Inc. He set up an office in his Manhattan cooperative apartment, and began to serve clients in the US and abroad. In addition to his areas of technical expertise, he offered services in organizational studies, installation audits, product planning and marketing, and evaluation and selection of hardware and software.

As a consultant Dorn also capitalized on his talents as a writer and editor. He was a member of the *Datamation* editorial advisory board and contributed regularly to that publication. He was a regular columnist for data processing publications in Denmark, Finland, Iceland, Japan, New Zealand, and Australia. He was a reviewer for the *Annals* and *ACM Computing Reviews,* a referee for *Communications of the ACM,* and, for a while, edited the News and Notices Section of the *Annals.*

Dorn was also an effective speaker. In his first lecture in Iceland, which filled the largest room in the university, he introduced his audience of data processing managers to spreadsheets. He warned them to “embrace the coming personal computer revolution or lose control.... Departments will find ways to buy personal computers even if not budgeted to do so.” This prediction was made in 1979, well before introduction of the IBM PC. Dorn was the most frequent speaker at the annual Scandinavian Norddata Conference, at which he addressed overflow audiences for 15 consecutive years.

One of Dorn's more recent activities was to serve as a charter member of the Harvard Business School History of MIS Project, which is led by James L. McKenney, a colleague from their days at SDC. This project, some of whose reports appear periodically in the *Annals,* has as its goal to show how information technology, especially software, has transformed industries. [See Carlson, Walter M., “Transforming an Industry Through Information Technology,” *Ann. Hist. Comp., Vol. 15,* No. 1, 1993, pp. 39-43.] Dorn was a strong force in the project and was an extraordinary and most critical editor-making English mean what it says.

Dorn's relationship with New York City was an abiding one. He was a regular attendee at the ballet, concerts, and New York Ranger hockey games-and he never did drive an automobile. His other interests included the history of the Civil War and the art of the Inuit people. He was also a frequent visitor to the Museum of Modern Art, where his wife Sue has been deputy director for development and public affairs since 1987.

Dorn's public image was as an outspoken, opinionated pragmatist, but those who knew him well benefited from a very different, private personality. To his many friends, he was warm, loyal, and caring. He was a source of personal support to those who needed it—phoning daily to friends suffering the death of a spouse, serving as surrogate father to children whose fathers had died, sending personal notes of praise for work well done, and generously providing free advice and counsel to professional colleagues in difficulty. This aspect of his character is also reflected in obituaries that have appeared in computer industry publications throughout the world, for example, *ComputerWorld Denmark,* June 11, 1993, *Information Week (USA),* June 14, 1993, and *ComputerWorld Australia, July 16,* 1993.

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## UPDATES

## Stephen W. Dunwell

*Born April 3, 1913, Kalamazoo, Mich.; died March 21, 1994, Poughkeepsie, N. Y; IBM engineer primarily responsible for the development of the first supercomputer—STRETCH, IBM-7030.*

*Education:* BS, electrical engineering, Antioch College, Ohio.

*Professional Experience:* IBM Corp., 1933-1976; Lt. Colonel, Army Security Agency, 1941-1945.

*Honors and Awards:* Legion of Merit, 1945; IBM Outstanding Invention Award; IBM fellow, 1966; Computer Pioneer Award, IEEE Computer Society, 1992.

Stephen W. Dunwell was born April 3, 1913 in Kalamazoo, Mich. His first contact with electronics came in 1928, when, while in high school, he designed, built, and operated an amateur radio station. He later attended Antioch College, Ohio, where he majored in electrical engineering. As part of a cooperative program with IBM, he entered the IBM student engineering program for graduate engineers in 1933 in Endicott, N.Y, and joined the company on a full-time basis the following year.

As a demonstration to IBM management of the possibilities for use of electronics in punched-card machines, he designed and built a machine which sorted marked cards. Also during that period, he designed and built the switching device used by Dr. Wallace Eckert of Columbia University for his experiments in the use of punched-card machines for the computation of the lunar orbit. In 1938, Dunwell was transferred to the IBM world headquarters in New York City, where he worked on the specification and design of future IBM products. During World War II he received a direct commission to the Army Security Agency, whose mission was cryptography and code-breaking, using IBM machines with attached relay calculators. He received the Legion of Merit for this work, and returned to IBM at the end of World War II with the rank of Lt. Colonel.

At IBM Poughkeepsie he was involved in the specification and design of a number of calculators, including the IBM-502-A, the 603 and the 604, and the Card-Programmed Calculator, known as the CPC. This was followed by work on the stored-program computers including the IBM-650, the Tape Processing Machine, and the IBM-702 and 705 (commercial data processing machines). Then, in 1958, he became the director of Project STRETCH, the stretching of transistor technology for both commercial and scientific applications. The project had three objectives: (1) provide components for commercial transistorized computers, (2) combine in one machine both scientific and commercial capabilities, and (3) establish the ground rules for the design of future IBM computers. Among the 22 ground rules so established were the 8-bit byte, a standard interface to peripheral equipment, and automatic error correction.

While the IBM-7030 (the actual “STRETCH” Machine) never became a commercial success, the transistors, the circuits, the packaging, the cooling, the design automation system, the diagnostic techniques, and the design ground rules that were developed for the project became the model on which IBM's successful 7080 and 7090 lines were based. Dunwell received an IBM Outstanding Invention Award for patents relating to STRETCH. In 1966 he was made an IBM fellow, giving him the freedom for pursuing any topic of research or development that he was interested in.

Between 1966 and 1976, when he retired from IBM, Dunwell had produced COURSEWRITER, the first time-sharing software marketed by IBM, and led a program which put in place a worldwide computer time-sharing network offering computer-assisted instruction for the education of IBM field engineering. This network was the basis for the current RETAIN network, which provides for the exchange of engineering information, and the HONE network, which provides for the exchange of sales information.

After retirement, Dunwell and his wife, Julia McClure Dunwell, rescued and, for three years, operated Poughkeepsie's historic Bardavon 1869 Opera House, until it could be turned over to a professional management team. In 1980 they established a computer time-sharing company and laboratory to search for a universal computer language capable of replacing all computer languages now in use. This kept them busy until his death.[1]

## UPDATES

Portrait changed (MRW, 2012)

[1] From the press release for the presentation of the IEEE Computer Society Computer Pioneer Award, 1992.