Assigned: 2 Feb 07 Topic: What is a computer? (part I)

Contents

O'Connor & Robertson 1998, "Charles Babbage"

Simon, Herbert A., & Newell, Allen (1958), "Heuristic Problem Solving: The Next Advance in Operations Research", Operations Research 6(1) (January–February): 1–10.

Ensmenger, Nathan (2004), "Bits of History: Review of A.R. Burks's Who Invented the Computer? The Legal Battle that Changed Computing History", in American Scientist 91 (September–October): 467–468.

Charles Babbage

Born: 26 Dec 1791 in London, England
Died: 18 Oct 1871 in London, England

Both the date and place of **Charles Babbage**'s birth were uncertain but have now been firmly established. In [1] and [12], for example, his date of birth is given as 26 December 1792 and both give the place of his birth as near Teignmouth. Also in [18] it is stated:-

> *Little is known of Mr Babbage's parentage and early youth except that he was born on* 26 *December* 1792.

However, a nephew wrote to *The Times* a week after the obituary [18] appeared, saying that Babbage was born on 26 December 1791. There was little evidence to prove which was right until Hyman (see [8]) in 1975 found that Babbage's birth had been registered in St Mary's Newington, London on 6 January 1792. Babbage's father was Benjamin Babbage, a banker, and his mother was Betsy Plumleigh Babbage. Given the place that his birth was registered Hyman says in [8] that it is almost certain that Babbage was born in the family home of 44 Crosby Row, Walworth Road, London.

Babbage suffered ill health as a child, as he relates in [4]:-

> *Having suffered in health at the age of five years, and again at that of ten by violent fevers, from which I was with difficulty saved, I was sent into Devonshire and placed under the care of a clergyman (who kept a school at Alphington, near Exeter), with instructions to attend to my health; but, not to press too much knowledge upon me: a mission which he faithfully accomplished.*

Since his father was fairly wealthy, he could afford to have Babbage educated at private schools. After the school at Alphington he was sent to an academy at Forty Hill, Enfield, Middlesex where his education properly began. He began to show a passion for mathematics but a dislike for the classics. On leaving the academy, he continued to study at home, having an Oxford tutor to bring him up to university level. Babbage in [4] lists the mathematics books he studied in this period with the tutor:-

> *Amongst these were Humphry Ditton's 'Fluxions', of which I could make nothing; Madame Agnesi's 'Analytical Instructions' from which I acquired some knowledge; Woodhouse's 'Principles of Analytic Calculation', from which I learned the notation of Leibniz; and Lagrange's 'Théorie des Fonctions'. I possessed also the 'Fluxions' of Maclaurin and of Simson.*

Babbage entered Trinity College, Cambridge in October 1810. However the grounding he had acquired from the books he had studied made him dissatisfied with the teaching at Cambridge. He wrote [4]:-

> *Thus it happened that when I went to Cambridge I could work out such questions as the very moderate amount of mathematics which I then possessed admitted, with equal facility, in the dots of Newton, the d's of Leibniz, or the dashes of Lagrange. I thus acquired a distaste for the routine of the studies of the place, and devoured the papers of Euler and other mathematicians scattered through innumerable volumes of the academies of St Petersburg, Berlin, and Paris, which the libraries I had recourse to contained.*

> *Under these circumstances it was not surprising that I should perceive and be penetrated with the superior power of the notation of Leibniz.*

It is a little difficult to understand how Woodhouse's *Principles of Analytic Calculation* was such an excellent book from which to learn the methods of Leibniz, yet Woodhouse was teaching Newton's calculus at Cambridge without any reference to Leibniz's methods. Woodhouse was one of Babbage's teachers at Cambridge yet he seems to have taken no part in the Society that Babbage was to set up to try to bring the modern continental mathematics to Cambridge.

Babbage tried to buy Lacroix's book on the differential and integral calculus but this did not prove easy in this period of war with Napoleon. When he did find a copy of the work he had to pay seven guineas for it - an incredible amount of money in those days. Babbage then thought of setting up a Society to translate the work [4]:-

> *I then drew up the sketch of a society to be instituted for translating the small work of Lacroix on the Differential and Integral Calculus. It proposed that we should have periodical meetings for the propagation of d's; and consigned to perdition all who supported the heresy of dots. It maintained that the work of Lacroix was so perfect that any comment was unnecessary.*

Babbage talked with his friend Edward Bromhead (who would become George Green's friend some years later- see the article on Green) who encouraged him to set up his Society. The Analytical Society was set up in 1812 and its members were all Cambridge undergraduates. Nine mathematicians attended the first meeting but the two most prominent members, in addition to Babbage, were John Herschel and George Peacock.

Babbage and Herschel produced the first of the publications of the Analytical Society when they published *Memoirs of the Analytical Society* in 1813. This is a remarkably deep work when one realises that it was written by two undergraduates. They gave a history of the calculus, and of the Newton, Leibniz controversy they wrote:-

> *It is a lamentable consideration, that that discovery which has most of any done honour to the genius of man, should nevertheless bring with it a train of reflections so little to the credit of his heart.*

Two further publications of the Analytical Society were the joint work of Babbage, Herschel and Peacock. These are the English translation of Lacroix's *Sur le calcul différentiel et intégral* published in 1816 and a book of examples on the calculus which they published in 1820.

Babbage had moved from Trinity College to Peterhouse and it was from that College that he graduated with a B.A. in 1814. However, Babbage realised that Herschel was a much more powerful mathematician than he was so [12]:-

> *He did not compete for honours, believing Herschel sure of first place and not caring to come out second.*

Indeed Herschel was first Wrangler, Peacock coming second. Babbage married in 1814, then left Cambridge in 1815 to live in London. He wrote two major papers on functional equations in 1815 and 1816. Also in 1816, at the early age of 24, he was elected a fellow of the Royal Society of London. He wrote papers on several different mathematical topics over the next few years but none are particularly important and some, such as his work on infinite series, are clearly incorrect.

Babbage was unhappy with the way that the learned societies of that time were run. Although elected to the Royal Society, he was unhappy with it. He was to write of his feelings on how the Royal Society was run:-

> *The Council of the Royal Society is a collection of men who elect each other to office and then dine together at the expense of this society to praise each other over wine and give each other medals.*

However in 1820 he was elected a fellow of the Royal Society of Edinburgh, and in the same year he was a major influence in founding the Royal Astronomical Society. He served as secretary to the Royal Astronomical Society for the first four years of its existence and later he served as vice-president of the Society.

Babbage, together with Herschel, conducted some experiments on magnetism in 1825, developing methods introduced by Arago. In 1827 Babbage became Lucasian Professor of Mathematics at Cambridge, a position he held for 12 years although he never taught. The reason why he held this prestigious post yet failed to carry out the duties one would have expected of the holder, was that by this time he had become engrossed in what was to became the main passion of his life, namely the development of mechanical computers.

Babbage is without doubt the originator of the concepts behind the present day computer. The computation of logarithms had made him aware of the inaccuracy of human calculation around 1812. He wrote in [4]:-

> *... I was sitting in the rooms of the Analytical Society, at Cambridge, my head leaning forward on the table in a kind of dreamy mood, with a table of logarithms lying open before me. Another member, coming into the room, and seeing me half asleep, called out, Well, Babbage, what are you dreaming about?" to which I replied "I am thinking that all these tables"* (*pointing to the logarithms*) *"might be calculated by machinery."*

Certainly Babbage did not follow up this idea at that time but in 1819, when his interests were turning towards astronomical instruments, his ideas became more precise and he formulated a plan to construct tables using the method of differences by mechanical means. Such a machine would be able to carry out complex operations using only the mechanism for addition. Babbage began to construct a small difference engine in 1819 and had completed it by 1822. He announced his invention in a paper *Note on the application of machinery to the computation of astronomical and mathematical tables* read to the Royal Astronomical Society on 14 June 1822.

Although Babbage envisaged a machine capable of printing out the results it obtained, this was not done by the time the paper was written. An assistant had to write down the results obtained. Babbage illustrated what his small engine was capable of doing by calculating successive terms of the sequence $n^2 + n + 41$.

The terms of this sequence are 41, 43, 47, 53, 61, ... while the differences of the terms are 2, 4, 6, 8, .. and the second differences are 2, 2, 2, ... The difference engine is given the initial data 2, 0, 41; it constructs the next row 2, (0 + 2), [41 + (0 + 2)], that is 2, 2, 43; then the row 2, (2 + 2), [43 + (2 + 2)], that is 2, 4, 47; then 2, 6, 53; then 2, 8, 61; ... Babbage reports that his small difference engine was capable of producing the members of the sequence $n^2 + n + 41$ at the rate of about 60 every 5 minutes.

Babbage was clearly strongly influenced by de Prony's major undertaking for the French Government of producing logarithmic and trigonometric tables with teams of people to carry out the calculations. He argued that a large difference engine could do the work undertaken by teams of people saving cost and being totally accurate.

On 13 July 1823 Babbage received a gold medal from the Astronomical Society for his development of the difference engine. He then met the Chancellor of the Exchequer to seek public funds for the construction of a large difference engine. The Royal Society had already given positive advice to the government:-

> *Mr Babbage has displayed great talent and ingenuity in the construction of his machine for computation, which the committee thanks fully adequate to the attainment of the objects proposed by the inventory; and they consider Mr Babbage as highly deserving of public encouragement, in the prosecution of his arduous undertaking.*

His initial grant was for £1500 and he began work on a large difference engine which he believed he could complete in three years. He set out to produce an engine with [3]:-

> *... six orders of differences, each of twenty places of figures, whilst the first three columns would each have had half a dozen additional figures.*

Such an engine would easily have been able to compute all the tables that de Prony had been calculating, and it was intended to have a printer to print out the results automatically. However the construction proceeded slower than had been expected. By 1827 the expenses were getting out of hand.

The year 1827 was a year of tragedy for Babbage; his father, his wife and two of his children all died that year. He own health gave way and he was advised to travel on the Continent. After his travels he returned near the end of 1828. Further attempts to obtain government support eventually resulted in the Duke of Wellington, the Chancellor of the Exchequer and other members of the government visiting Babbage and inspecting the work for themselves. By February 1830 the government had paid, or promised to pay, £9000 towards the project.

In 1830 Babbage published *Reflections on the Decline of Science in England*, a controversial work that resulted in the formation, one year later, of the British Association for the Advancement of Science. In 1834 Babbage published his most influential work *On the Economy of Machinery and Manufactures*, in which he proposed an early form of what today we call operational research.

The year 1834 was the one in which work stopped on the difference engine. By that time the government had put £17000 into the project and Babbage had put £6000 of his own money. For eight years from 1834 to 1842 the government would make no decision as to whether to continue support. In 1842 the decision not to proceed was taken by Robert Peel's government. Dubbey in [6] writes:-

> *Babbage had every reason to feel aggrieved about his treatment by successive governments. They had failed to understand the immense possibilities of his work, ignored the advice of the most reputable scientists and engineers, procrastinated for eight years before reaching a decision about the difference engine, misunderstood his motives and the sacrifices he had made, and ... failed to protect him from public slander and ridicule.*

By 1834 Babbage had completed the first drawings of the analytical engine, the forerunner of the modern electronic computer. His work on the difference engine had led him to a much more sophisticated idea. Although the analytic engine never progressed beyond detailed drawings, it is remarkably similar in logical components to a present day computer. Babbage describes five logical components, the store, the mill, the control, the input and the output. The store contains [4]:-

> *... all the variables to be operated upon, as well as all those quantities which had arisen from the results of other operations.*

The mill is the analogue of the cpu in a modern computer and it is the place [4]:-

> *... into which the quantities about to be operated upon are always bought.*

The control on the sequence of operations to be carried out was by a Jacquard loom type device. It was operated by punched cards and the punched cards contained the program for the particular task [4]:-

> *Every set of cards made for any formula will at any future time recalculate the formula*

> *with whatever constants may be required.*
>
> *Thus the Analytical Engine will possess a library of its own. Every set of cards once made will at any time reproduce the calculations for which it was first arranged.*

The store was to hold 1000 numbers each of 50 digits, but Babbage designed the analytic engine to effectively have infinite storage. This was done by outputting data to punched cards which could be read in again at a later stage when needed. Babbage decided, however, not to seek government support after his experiences with the difference engine.

Babbage visited Turin in 1840 and discussed his ideas with mathematicians there including Menabrea. During Babbage's visit, Menabrea collected all the material needed to describe the analytical engine and he published this in October 1842. Lady Ada Lovelace translated Menabrea's article into English and added notes considerably more extensive than the original memoir. This was published in 1843 and included [7]:-

> *... elaborations on the points made by Menabrea, together with some complicated programs of her own, the most complex of these being one to calculate the sequence of Bernoulli numbers.*

Although Babbage never built an operational, mechanical computer, his design concepts have been proved correct and recently such a computer has been built following Babbage's own design criteria. He wrote in 1851 (see [7]):-

> *The drawings of the Analytical Engine have been made entirely at my own cost: I instituted a long series of experiments for the purpose of reducing the expense of its construction to limits which might be within the means I could myself afford to supply. I am now resigned to the necessity of abstaining from its construction...*

Despite this last statement, Babbage never did quite give up hope that the analytical engine would be built writing in 1864 in [4]:-

> *... if I survive some few years longer, the Analytical Engine will exist...*

After Babbage's death a committee,whose members included Cayley and Clifford, was appointed by the British Association [12]:-

> *... to report upon the feasibility of the design, recorded their opinion that its successful realisation might mark an epoch in the history of computation equally memorable with that of the introduction of logarithms...*

This was an underestimate. The construction of modern computers, logically similar to Babbage's design, have changed the whole of mathematics and it is even not an exaggeration to say that they have changed the whole world.

Article by: *J J O'Connor* and *E F Robertson*

October 1998

MacTutor History of Mathematics
[http://www-history.mcs.st-andrews.ac.uk/Biographies/Babbage.html]

Operations Research

January - February 1958

HEURISTIC PROBLEM SOLVING: THE NEXT ADVANCE IN OPERATIONS RESEARCH*

Herbert A. Simon and Allen Newell

Carnegie Institute of Technology, Pittsburgh, Pennsylvania, and
The Rand Corporation, Santa Monica, California

THE IDEA THAT the development of science and its application to human affairs often requires the cooperation of many disciplines and professions will not surprise the members of this audience. Operations research and management science are young professions that are only now beginning to develop their own programs of training; and they have meanwhile drawn their practitioners from the whole spectrum of intellectual disciplines. We are mathematicians, physical scientists, biologists, statisticians, economists, and political scientists.

In some ways it is a very new idea to draw upon the techniques and fundamental knowledge of these fields in order to improve the everyday operation of administrative organizations. The terms 'operations research' and 'management science' have evolved in the past fifteen years, as have the organized activities associated with them. But of course, our professional activity, the application of intelligence in a systematic way to administration, has a history that extends much farther into the past. One of its obvious antecedents is the scientific management movement fathered by Frederick W. Taylor.

But for an appropriate patron saint for our profession, we can most appropriately look back a full half century before Taylor to the remarkable figure of Charles Babbage. Perhaps more than any man since Leonardo da Vinci he exemplified in his life and work the powerful ways in which

---

* Address at the banquet of the Twelfth National Meeting of the Operations Research Society of America, Pittsburgh, Pennsylvania, November 14, 1957. Mr. Simon presented the paper; its content is a joint product of the authors. In this, they rely on the precedent of Genesis 27:22, "The voice is Jacob's voice, but the

fundamental science could contribute to practical affairs, and practical affairs to science. He was one of the strongest mathematicians of his generation, but he devoted his career to the improvement of manufacturing arts, and—most remarkable of all—to the invention of the digital computer in something very close to its modern form.

The spirit of the operations researcher, his curiosity, his impatience with inefficiency in any aspect of human affairs, shows forth from every page of Babbage's writing. I give you just one example:

> Clocks occupy a very high place amongst instruments by means of which human time is economized: and their multiplication in conspicuous places in large towns is attended with many advantages. Their position, nevertheless, in London, is often very ill chosen; and the usual place, half-way up on a high steeple, in the midst of narrow streets, in a crowded city, is very unfavourable, unless the church happen to stand out from the houses which form the street. The most eligible situation for a clock is, that it should project considerably into the street at some elevation, with a dial-plate on each side, like that which belonged to the old church of St. Dunstan, in Fleet-street, so that passengers in both directions would have their attention directed to the hour.[1]

I have mentioned Babbage as the inventor of the computer. Since Babbage and the computer are going to be the heroes of my talk tonight, I should like to tell you a true story, culled from Babbage's writings, about the history of the computer. I like this story because it illustrates not only my earlier point about the many mutual relations of the professions in our field, but also because it gives the underdogs like myself—trained in 'soft' fields like economics and political science—something we can point to when the superior accomplishments of the natural sciences become too embarrassing for us. As you will see, this story shows that physicists and electrical engineers had little to do with the invention of the digital computer—that the real inventor was the economist Adam Smith, whose idea was translated into hardware through successive stages of development by two mathematicians, Prony and Babbage. (I should perhaps mention that the developers owed a debt also to the French weavers and mechanics responsible for the Jacquard loom, and consequently for the punched card.)

The story comes from a French document, which Babbage reproduces in the original language. I give it here in translation:

> Here is the anecdote: M. de Prony was employed by a government committee to construct, for the decimal graduation of the circle, logarithmic and trigonometric tables which would not only leave nothing to be desired from the standpoint of accuracy, but which would constitute the most vast and imposing monument of calculation that had ever been executed or even conceived. The logarithms from 1 to 200,000 are a necessary and essential supple-

> he associated with himself three or four experienced collaborators the longest reasonable expectation of the duration of his life would not suffice to complete the undertaking. He was preoccupied with this unhappy thought when, finding himself before a bookstore, he saw the beautiful edition of Adam Smith published in London in 1776. He opened the book at random and chanced upon the first chapter, which treats of the division of labor and where the manufacture of pins is cited as example.
>
> Hardly had he perused the first pages when, by a stroke of inspiration he conceived the expedient of putting his logarithms into production like pins. He was giving, at this time, at the Ecole Polytechniques, some lectures on a topic in analysis related to this kind of work—the method of differences and its applications to interpolation. He went to spend some time in the country and returned to Paris with the plan of manufacture that has been followed in the execution. He organized two workshops which performed the same calculations separately, and served as reciprocal checks.[2]

It was Prony's mass production of the mathematical tables, in turn, that suggested to Babbage that machinery could replace human labor in the clerical phases of the task, and that started him on the undertaking of designing and constructing an automatic calculating engine. Although the complete absence of electrical and electronic components, and his consequent dependence on mechanical devices, robbed him of full success in the undertaking, there is no doubt that he understood and invented the digital computer—including the critically important idea of a conditional transfer operation.

It would be hard to imagine a more appropriate illustration of the unexpected ways in which human knowledge develops, and of the contribution of all the sciences and arts to this development that is so characteristic of operations research and management science.

AS WE TURN our gaze now from past to future, I should like to outline my main thesis quite bluntly. Operations research has made large contributions to those management decisions that can be reduced to systematic computational routines. To date, comparable progress has not been made in applying scientific techniques to the judgmental decisions that cannot be so reduced. Research of the past three years into the nature of complex information processes in general, and human judgmental or heuristic thinking processes in particular, is about to change this state of affairs radically. We are now poised for a great advance that will bring the digital computer and the tools of mathematics and the behavioral sciences to bear on the very core of managerial activity—on the exercise of judgment and intuition; on the processes of making complex decisions.

Let me spell out this thesis, first describing the present situation in operations research as I see it, then indicating why I think this situation is

THE RAPID GROWTH of operations research over the past two decades has brought to industry and government an important kit of tools for grappling with the complexities of managing large organizations. These tools have been collected from the far corners of the intellectual world—from mathematics, from statistics and probability theory, from econometrics, from electrical engineering, and even from biology. Such exotic techniques as linear programming, queuing theory, servomechanism theory, game theory, dynamic programming, marginal analysis, the calculus of variations, and information theory are now at work helping to solve practical problems of business operation.

Skeptical—and sensibly skeptical—managements have come to see that, even if not all the blue-sky claims for the new approaches have been backed by solid fact, there is a large core of valid technique and application. The tools have produced tangible results in a substantial number of demonstration installations, and the question is less and less 'Are they here to stay?' and more and more 'How and where can we use them effectively?' The traditional areas of production and inventory control, of scheduling, and of marketing research are undergoing a substantial and rapid evolution.

Having observed this important change, we can note with equal accuracy that large areas of managerial activity—it would be correct to say most areas—have hardly been touched by operations research or the advances in management science. Operations research has demonstrated its effectiveness in dealing with the kinds of management problems that we might call 'well structured,' but it has left pretty much untouched the remaining, 'ill structured,' problems.

The trouble, as executives are fond of pointing out to operations researchers, is that there are no known formal techniques for finding answers to most of the important top-level management problems. Nor do these problems seem to be of the same kind as the more tangible middle-management situations in which existing operations research techniques have been most effective. Unarmed with formal techniques, operations researchers have to resort to the same common sense and human cleverness that has served managements these many years. Executives still find a vast sphere of activity in which they are secure from the depredations of mathematicians and computers.

Let me try to make a little more precise this distinction between well-structured and ill-structured problems that today establishes the jurisdictional boundary beyond which formal tools do not reach.

A problem is well structured to the extent that it satisfies the following criteria:

1. It can be described in terms of numerical variables, scalar and vector quantities.

2. The goals to be attained can be specified in terms of a well-defined objective function—for example, the maximization of profit or the minimization of cost.

3. There exist computational routines (*algorithms*) that permit the solution to be found and stated in actual numerical terms. Common examples of such algorithms, which have played an important role in operations research, are maximization procedures in the calculus and calculus of variations, linear-programming algorithms like the stepping-stone and simplex methods, Monte Carlo techniques, and so on.

In short, well-structured problems are those that can be formulated explicitly and quantitatively, and that can then be solved by known and feasible computational techniques.

What, then, are ill-structured problems? Problems are ill-structured when they are not well-structured. In some cases, for example, the essential variables are not numerical at all, but symbolic or verbal. An executive who is drafting a sick-leave policy is searching for words, not numbers. Second, there are many important situations in everyday life where the objective function, the goal, is vague and nonquantitative. How, for example, do we evaluate the quality of an educational system or the effectiveness of a public relations department? Third, there are many practical problems—it would be accurate to say 'most practical problems'—for which computational algorithms simply are not available.

If we face the facts of organizational life, we are forced to admit that the majority of decisions that executives face every day—and certainly a majority of the very most important decisions—lie much closer to the ill-structured than to the well-structured end of the spectrum. And yet, operations research and management science, for all their solid contributions to management, have not yet made much headway in the area of ill-structured problems. These are still almost exclusively the province of the experienced manager with his 'judgment and intuition.' The basic decisions about the design of organization structures are still made by judgment rather than science; business policy at top-management levels is still more often a matter of hunch than of calculation. Operations research has had more to do with the factory manager and the production-scheduling clerk than it has with the vice-president and the Board of Directors.

I am not unaware that operations researchers are often called in to advise management at top levels and regarding problems of the kinds I have called ill-structured. But I think we all recognize that when we are asked by management to advise on such decisions, we are asked because we are thought to possess a certain amount of experience and common sense, and not because of any belief that our specialized tools, mathematical or otherwise, have much to do with the task at hand. I think most of us as operations researchers, and those in which we are performing as general management consultants. And I am sure that most of us look forward to the day when our science will enable us to handle with appropriate analytic tools those problems that we now tackle with judgment and guess.

The basic fact we have to recognize is that no matter how strongly we wish to treat problems with the tools our science provides us, we can only do so when the situations that confront us lie in the area to which the tools apply. Techniques are the arms and hands of science, and the reach of a science is measured by their range. The telescope made sunspots and Jupiter's moons a part of Galileo's science, just as particle accelerators and the mathematical machinery of quantum mechanics bring the interior of the atom within the reach of the nuclear physicist.

In dealing with the ill-structured problems of management we have not had the mathematical tools we have needed—we have not had 'judgment mechanics' to match quantum mechanics. We have not had the engines—no executive centrifuges. We have had only the rudiments of experimental techniques for observing organizational behavior in the laboratory, although we have made great strides in the last decade in developing these.

IF OUR SCIENCE, then, is to be coextensive with the field of management, we must have the tools and techniques that will extend its range to that whole field. I think there is good reason to believe that we are acquiring these tools and techniques at this very point in history.

Even while operations research is solving well-structured problems, fundamental research is dissolving the mystery of how humans solve ill-structured problems. Moreover, we have begun to learn how to use computers to solve these problems, where we do not have systematic and efficient computational algorithms. And we now know, at least in a limited area, not only how to program computers to perform such problem-solving activities successfully; we know also how to program computers to *learn* to do these things.

In short, we now have the elements of a theory of heuristic (as contrasted with algorithmic) problem solving; and we can use this theory both to understand human heuristic processes and to simulate such processes with digital computers. Intuition, insight, and learning are no longer exclusive possessions of humans: any large high-speed computer can be programmed to exhibit them also.

I cannot give here the detailed evidence on which these assertions—and very strong assertions they are—are based. I must warn you that examples of successful computer programs for heuristic problem solving are still very few. One pioneering effort was a program written by O. G.

SELFRIDGE and G. P. DINNEEN that permitted a computer to learn to distinguish between figures representing the letter O and figures representing A presented to it 'visually.'[3] The program that has been described most completely in the literature gives a computer the ability to discover proofs for mathematical theorems—not to verify proofs, it should be noted, for a simple algorithm could be devised for that, but to perform the 'creative' and 'intuitive' activities of a scientist seeking the proof of a theorem. The program is also being used to predict the behavior of humans when solving such problems. This program is the product of work carried on jointly at the Carnegie Institute of Technology and the Rand Corporation, by Allen Newell, J. C. Shaw, and myself.[4]

A number of investigations in the same general direction—involving such human activities as language translation, chess playing, engineering design, musical composition, and pattern recognition—are under way at other research centers. At least one computer now designs small standard electric motors (from customer specifications to the final design) for a manufacturing concern, one plays a pretty fair game of checkers, and several others know the rudiments of chess. The ILLIAC, at the University of Illinois, composes music, using I believe, the counterpoint of Palestrina; and I am told by a competent judge that the resulting product is aesthetically interesting.

Let me summarize as concretely as possible my assessment of the present and future state of the art and theory of heuristic problem solving. As of the present—1957:

1. Digital computers can perform certain heuristic problem-solving tasks for which no algorithms are available.
2. In doing so, they use processes that are closely parallel to human problem-solving processes.
3. Within limits, these machines learn to improve their performance on the basis of experience (not merely by memorizing specific patterns of successful behavior, but by reprogramming themselves in ways that parallel at least some human learning procedures).

On the basis of these developments, and the speed with which research in this field is progressing, I am willing to make the following predictions, to be realized within the next ten years:

1. That within ten years a digital computer will be the world's chess champion, unless the rules bar it from competition.
2. That within ten years a digital computer will discover and prove an important new mathematical theorem.
3. That within ten years a digital computer will write music that will be accepted by critics as possessing considerable aesthetic value.
4. That within ten years most theories in psychology will take the form of

computer programs, or of qualitative statements about the characteristics of computer programs.

It is not my aim to surprise or shock you—if indeed that were possible in an age of nuclear fission and prospective interplanetary travel. But the simplest way I can summarize the situation is to say that there are now in the world machines that think, that learn, and that create. Moreover, their ability to do these things is going to increase rapidly until—in a visible future—the range of problems they can handle will be coextensive with the range to which the human mind has been applied.

What are the implications of this development? They are of at least three rather distinct kinds:

1. There will be more and more applications of machines to take the place of humans in solving ill-structured problems; just as machines are now being more and more used to solve well-structured problems.

2. There will be applications of machines to tackle ill-structured problems of such magnitude and difficulty that humans have not been able to solve them. (This is parallel to current applications of computers to the numerical solution of partial differential equations that lie beyond the capacity of hand methods.)

3. The research on heuristic problem solving will be applied to understanding the human mind. With the aid of heuristic programs, we will help man obey the ancient injunction: Know thyself. And knowing himself, he may learn to use advances of knowledge to benefit, rather than destroy, the human species.

In estimating the rates at which these developments will come about, it may be instructive to turn to a close analogy in the field of atomic energy. The implications of atomic energy are also threefold: (1) the generation of power to replace and augment power from conventional fuels; (2) the production of hitherto unrealizable concentrations of power (the primary peaceful application being thus far to the study of the interior of the atom); and (3) the use of radioactive materials as tracers for the study of physical and biological processes. The main point in drawing the analogy is that in both cases—computers and atomic energy—the usefulness of the first application hinges on economic calculations, while the significance of the other two rests mainly on their technical feasibility.

Atomic fuels will replace conventional fuels only when the capital costs per unit of energy-generating capacity are competitive with the capital costs of conventional plants. Computers for heuristic problem solving will replace executives only when the costs per unit of problem-solving capacity are competitive with the costs for executives. In neither case is it easy to make a forecast with available data, but it seems highly probable in both cases that the changeover, if it comes, will come gradually.

A substantial impact of heuristic problem solving on research (either in allowing us to tackle more difficult problems than humans now can, or in informing us how talented humans solve problems) is probably more imminent. Here—as in the parallel cases for atomic energy—the question will be very little 'How much will it cost?' and very much 'Can we do it?' It is neither a trivial nor a costless process to transfer from a productive scientist to a student the heuristic programs that make the former a powerful problem solver. To do this generally takes some twenty years of educational effort, and the undertaking is frequently unsuccessful. To reproduce in another computer a problem-solving program that has been learned and been proved effective by a first computer is a trivial matter. When machines will have minds, we can create copies of these minds as cheaply as we can now print books.

If what I have said still seems distant and speculative to you, I would like to recall to you again the precedent of Charles Babbage, who, always standing on the realities of the present saw the importance also of peering into the future and forecasting its shape.

> Perhaps to the sober eye of inductive philosophy, these anticipations of the future may appear too faintly connected with the history of the past. . . .
>
> Even now, the imprisoned winds which the earliest poet made the Grecian warrior bear for the protection of his fragile bark; or those which, in more modern times, the Lapland wizards sold to the deluded sailors;—these, the unreal creations of fancy or of fraud, called, at the command of science from their shadowy existence, obey a holier spell: and the unruly masters of the poet and the seer become the obedient slaves of civilized man.
>
> Nor have the wild imaginings of the satirist been quite unrivalled by the realities of after years: as if in mockery of the College of Laputa, light almost solar has been extracted from the refuse of fish; fire has been sifted by the lamp of Davy; and machinery has been taught arithmetic instead of poetry.[5]

PERHAPS this is an appropriate point to bring my speculations to a close and to summarize briefly the course of my argument. Up to the present time, operations research and the management sciences have been largely limited, by the nature of their tools, to dealing with well-structured problems that possess algorithmic means of solution. With recent developments in our understanding of heuristic processes and their simulation by digital computers, the way is open to deal scientifically with ill-structured problems—to make the computer coextensive with the human mind.

The energy revolution of the eighteenth and nineteenth centuries forced man to reconsider his role in a world in which his physical power and speed were outstripped by the power and speed of machines. The revolution in heuristic problem solving will force man to consider his role in a world in which his intellectual power and speed are outstripped by the intelligence of machines. Fortunately, the new revolution will at the same time give him a deeper understanding of the structure and workings of his own mind.

It is my personal hope that the latter development will outstrip the former—that man will learn where he wants to travel before he acquires the capability of leaving the planet.

REFERENCES

1. Charles Babbage, *On the Economy of Machinery and Manufacturers*, p. 45.
2. *Ibid.*, p. 193. [Quoted by Babbage from a *Note sur la publication, propose par le gouvernement Anglais des grands tables logarithmiques et trigonometriques de M. de Prony*, (1820)].
3. O. G. Selfridge, "Pattern Recognition and Modern Computers," and G. P. Dinneen, "Programming Pattern Recognition," both in *Proceedings of the 1955 Western Joint Computer Conference*, IRE.
4. "The Logic Theory Machine," *IRE Transactions* **IT-2,** 61–79 (September 1956); "Empirical Explorations of the Logic Theory Machine" and "Programming the Logic Theory Machine," *Proceedings of the 1957 Western Joint Computer Conference,* IRE; "The Elements of a Theory of Human Problem Solving," *Psych. Rev.*, in press.
5. Charles Babbage, *op. cit.*, p. 389.

American Scientist online
THE MAGAZINE OF SIGMA XI, THE SCIENTIFIC RESEARCH SOCIETY

see list of all reviews from this issue: September-October 2003

COMPUTER SCIENCE

Bits of History

Nathan Ensmenger

Who Invented The Computer? The Legal Battle That Changed Computing History. Alice Rowe Burks. 463 pp. Prometheus Books, 2002. $35.

In the mid-1930s, a professor of physics and mathematics at Iowa State College named John Vincent Atanasoff began work on a machine capable of solving complex sets of linear algebraic equations. In doing so, he and his graduate-student assistant Clifford Berry explored many of the techniques and technologies that later became widely adopted in electronic computing: the use of binary arithmetic based on logical rather than counting principles; periodically regenerating rotating drum memory; the separation of memory and arithmetic units; the automatic coordination of operations through a centralized "clock." Although the Atanasoff-Berry Computer (ABC) was never fully completed, and Atanasoff himself soon moved on to other projects, the ABC nevertheless represented a pioneering milestone in the development of the modern computer.

Just how pioneering a milestone it was has been a subject of considerable controversy, however. Overshadowed by larger, more visible wartime computing projects such as the ENIAC, the accomplishments of Atanasoff and Berry went largely unnoticed for decades, even within the electronic computing community. In fact, information about their work on the ABC did not become widely available until Atanasoff found himself at the center of a high-profile legal dispute involving patent rights to the electronic computer (Berry had earlier committed suicide).

click for full image and caption

At stake in the case was the Sperry Rand Corporation's claim to patent rights (based on work done on the ENIAC machine by John W. Mauchly and J. Presper Eckert) and millions of dollars in potential licensing fees; at the heart of a legal challenge by rival computer manufacturer Honeywell, Inc., was a 1941 visit that Mauchly made to Iowa to observe Atanasoff's progress on the ABC. Suddenly the question of who invented the computer became more than merely academic, and in 1973 Federal District Judge Earl Larson delivered a surprising decision: The true inventor of the computer was Atanasoff, not Mauchly and Eckert. (Why only Atanosoff, and not also Berry, is a question that has never been satisfactorily addressed.)

Despite Judge Larson's decision, Atanasoff remains a relatively obscure and controversial figure, even within the history of computing literature. In this book Alice Rowe Burks attempts to restore Atanasoff to what she believes to be his proper role as the inventor of the modern computer. Making extensive use of transcripts of the trial, as well as many other published sources and firsthand reminiscences (including those of her husband, Arthur Burks, who was one of the principal designers of the ENIAC), she defends Judge Larson's decision and argues that Atanasoff deserves credit not only for developing the first true electronic computer, but also, through his influence on John Mauchly, for having an "immediate and enduring" effect on the subsequent history of computing. Although she stops short of accusing Mauchly outright of stealing Atanasoff's ideas (albeit just barely), she strongly implies that Mauchly and others (including most professional historians of computing) have deliberately denied Atanasoff his true role as the father of modern electronic computing.

Burks makes a convincing case that Atanasoff has been unfairly disregarded in much of the literature on the history of computing. She also clearly reveals that Sperry Rand's attempt to patent the electronic computer was both misguided and mishandled. For various reasons, including but not confined to Atanasoff's claims to priority, the case was doomed to failure from the very beginning.

The problem with Burks's book, however, is that it provides a convincing (and at times overly detailed) answer to what is fundamentally the *wrong question*. Although it might sometimes be legally necessary to identify a single inventor of a particular technology to determine patentability, debates about who was first rarely serve a useful role in understanding the historical development of technology. As Michael Williams suggests in a recent volume edited by Raúl Rojas and Ulf Hashagen called *The First Computers* (note the crucial use of the plural), any particular claim to priority of invention must necessarily be heavily qualified: If you add enough adjectives, you can always claim your own favorite. Atanasoff's ABC machine was the first computer as Burks defines the computer, but there are other plausible definitions of what constitutes a "true" computer, and therefore other defensible answers to the question of who was first. Ironically enough, in her zeal to redress the wrongs done to Atanasoff, Burks defines the history of computing solely in terms of the ABC and the ENIAC, and she therefore fails to acknowledge the contributions (and claims to priority) of other pioneering machines, such as the Colossus and the Zuse Z3.

Although Burks provides some new and useful information about the contributions of Atanasoff, it is difficult to recommend this book to anyone but the most dedicated scholar of the history of computing. In its single-minded focus on the question of priority it loses sight of the bigger issues. It is also marred by its polemical tone and the author's obvious contempt for John Mauchly. The book is overly long to begin with, and almost half of its more than 400 pages are devoted to elaborate descriptions of the author's squabbles with other historians. The general reader would better served by a broader and more balanced book such as *Computer: A History of the Information Machine*, by Martin Campbell-Kelly and William Aspray (1996), which considers the many developments—technological, economic, scientific and social—that have contributed to the shaping of the modern computer.—*Nathan Ensmenger, History and Sociology of Science, University of Pennsylvania*

Article Tools

printer friendly request classroom permission e-mail this article

Related Letter to the Bookshelf
Who Invented the Computer?, by Alice Rowe Burks

Of Possible Interest
BOOK REVIEW: Battling Blazes
BOOK REVIEW: Aeronaut with a Heart of Gold
BOOK REVIEW: Astropolitics
BOOK REVIEW: An Education in Irrationality

BOOK REVIEW: Misters Wizard

Related Sigma Xi Links
SciStacks Media Resources: Earth Sciences
SciStacks Media Resources: Mathematics
Distinguished Lecturer: Diana Rhoten
Distinguished Lecturer: Gretchen Kalonji
SciStacks Media Resources: Physics & Astronomy