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## 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

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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.<sup>[1]</sup>

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 supplement to this work. It was easy for M. Prony to convince himself that even if

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 Polytechnique, 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.<sup>[2]</sup>

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 going to change drastically.

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 can distinguish pretty clearly between the cases in which we are working

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.

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