the discovery, by Richard Brent of the Australian National University, that the computation of the first N terms of any power of a power series is no harder than squaring the power series. To obtain this result, Brent changed the representation of the power series by taking its logarithm. He then solved the problem in its new form and changed back to the original representation by taking the exponential. Brent showed that these changes of representation can be computed with relatively few operations. The first N terms of both the logarithms and the exponential can be computed with no more steps than are needed to multiply two Nth degree polynomials.

The most recent, and to some mathematicians the most surprising, result on manipulating power series involves the speed of self-composition. Composition is a complicated operation that involves taking the power series of a power series—that is, letting one power series serve as the variable, x, that is raised to powers in the other power series. Self-composition is composing a power series with itself. Repeated self-composition is of considerable practical importance, occurring in applications that include difference equations, numerical analysis, and the study of dynamical systems. By changing the representation of self-composition problems, Brent and Traub were able to show that any number of self-compositions can be done as quickly as a single composition.

It is too soon to tell how great an impact these new algorithms will have on day-to-day computer calculations. But the intellectual impact of these algorithms is already apparent. As Borodin explains, previously no one even considered looking for fast algorithms to manipulate power series. It was generally assumed that the naïve way to do these calculations was the only way. The recent results, then, provide encouraging evidence that slow manipulations need not always be accepted.

—Gina Bari Kolata

The 1978 Nobel Prize in Economics

For his contributions to our understanding of decision-making, particularly in organizations, and for numerous other contributions to social science, Herbert A. Simon has been awarded the Nobel Prize in Economics for 1978. It is an appropriate tribute to an exceptional figure in contemporary science. Since I have known Simon as a colleague, collaborator, and friend for 25 years, it is natural for me to be asked to describe his work and its place in social science. But I do it with some hesitation. The canvas is too large for the brush, and not reliably passive. I recall telling a friend once that the only commentary it would be safe to write about Herb Simon would be an epitaph, because that would be the one comment on his work to which Herb would not reply. For once, however, the pleasure of honoring him overcomes a recognition that I do it inadequately.

Herb Simon is an economist, psychologist, political scientist, sociologist, philosopher, computer scientist, and a not-bad tetherball player. The number of disciplines with which he has been associated and the creativeness of even his minor efforts sometimes obscure the intellectual coherence of his major work. Although he has written many things and almost everything has stimulated important work, Simon's major professional life divides into two periods. The first is the period from 1947 to 1958, when he focused on decisions, particularly in organizations, but also wrote extensively on a variety of problems in the modeling of behavior. This is the work that is best known in economics, political science, and sociology. The second period is from 1958 to 1978, when his concerns shifted to human problem-solving and artificial intelligence. This is the work, much of it done in collaboration with Allen Newell, that is best known in psychology and computer science. Although the two audiences tend to be different, the two periods show a common enthusiasm for trying to connect the behavioral study of intentional action and the engineering design of intelligent systems.

Simon's deep concern for the engineering of intelligence is not always explicit, but it is persistent. His interest in organizational decision-making was tied to an interest in improving decision-making through information technology. His interest in understanding human problem-solving was tied to an interest in artificial intelligence. He has sometimes been seen as overly rationalistic by behavioral students of human choice, and as overly behavioral by economists and other enthusiasts for rational models of human action; but both sets of comments are misleading. He studies reason's limitations in the name of reason. Implicit in much of the research is a belief that improvement in the design of intelligence requires an understanding of human behavior. He is an insightful theorist of thinking, deciding, problem-solving, and choosing. But he studies human behavior not simply because of curiosity about how people behave, although he has that, but more because of an interest in, and affinity for, the perfection of intelligence. Like B. F. Skinner, with whom he shares almost nothing else, he is an unrepentant knight of the enlightenment. Not Freud, but Descartes.

It is a sweet fate and a tribute to the power of Simon's intellect that this man, whose most unwavering characteristic is commitment to the intelligence of rational discourse and to the technology of reason, should receive the Nobel Prize for his provocative explications of some of the ways in which human beings and human institutions are often intelligent without being, in the usual sense, rational. Simon's major contributions to the economics of decisions are found in a small number of works published between 1947 and 1958: *Administrative Behavior* (1947), *Models of Man* (1957), and *Organizations* (1958). In those works, and the articles from which they were drawn, he outlined some ways in which economic theories of the firm and other theories of rational choice might be revised. The specifics were important, but the impact of the work was less through the details than through the basic reformulations they reflected.

In company with most economists, Simon began with the assumption that human choice behavior was intendedly rational. That is, he assumed that decision-makers had a set of criteria known to them in advance of their actions, and that they made choices by measuring estimates of the consequences of alternative actions against the criteria. What Simon added was an awareness of the informational and computational limits on rationality within human institutions. Where most theories of rational choice assumed that all relevant alternatives were known, Simon suggested that alternatives had to be discovered through search and that typically only a relatively few alternatives were considered. Where most theories assumed that information on the consequences of alternatives was

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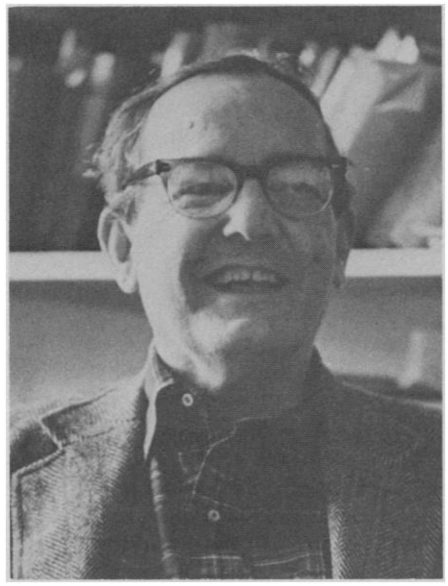
known, at least up to a probability distribution, Simon suggested that information had to be sought through some kind of search. Where most theories assumed that decision-makers optimized—that is, looked until they found the best alternative from the point of view of their preferences—Simon suggested that decision-makers "satisficed"—that is, chose the first alternative that was "good enough."

The perspective was behavioral. Simon argued that the rationality demanded of human beings by classical theories of rational choice was not observed in actual human behavior and was inconsistent with what was known about human capabilities for processing information. The argument was narrow. Most rational theories of choice already assumed that choice was constrained by factors of availability, cost, technology, time, and the like. Simon added the idea that the list of constraints on choice should include not only external factors in the environment but also some properties of human beings as processors of information and as problem-solvers. He called attention to human limits on memory and computing power, viewing them as obvious restrictions on full rationality. Thus, he initiated a string of related developments by others that have come collectively to be called a theory of limited, or bounded, rationality. In a proper sense, these developments comprise not a theory but a collection of behavioral complications for conventional theory. The number of such complications has grown considerably since 1958, but Simon's formulation remains the core.

Simon focused on three aspects of bounded rationality. The first was the extent to which information was sought through search in response to a problem rather than simply given. He assumed a search process stimulated by a failure or a need, and characterized by working backward from a desired outcome to a set of antecedent actions sufficient (but not necessary) to produce it. The second was the conception of preferences. He proposed substituting two-valued utility functions for the more complete preference orders familiar to decision theory. Alternatives were assumed to be judged sequentially and to be defined as either satisfactory or unsatisfactory, with much less attention to finer discriminations within those categories. The third was the importance of ordinary rules of behavior. Decisions were seen as the result of combining premises and rules that were modified through a long-term process only marginally affected by a current choice.



Herbert A. Simon

The standard metaphor for the theory reflects both the simplicity of the ideas and the memories of a Wisconsin boyhood. Consider a farmer confronting a haystack and deciding what to do with it. To make a decision in purely classical form, he would want to know (among other things) all of the contents of the haystack, all possible uses of each of the contents and combinations of them, and the probability distribution over all possible consequences of each. Simon observed that few farmers behave in such a way. A more typical farmer reduces the size of the problem. He notices that his shirt needs a button, and considers looking for the sharpest needle in the haystack. That seems a difficult thing to do, so he decides to look for any needle good enough to sew a button. But then he remembers an old family rule that shirts needing buttons should be hung in the laundry. So he does that. Simon's great contribution was to point out that decision-making in economic organizations is more like hanging a shirt in the laundry than looking for the sharpest needle in the haystack.

The metaphor has been durable and the ideas fruitful. For example, a simple extension of the original provides an interpretation of how organizations use organizational slack to absorb fluctuations in their environment. It appears to be true that organizations are often able to survive relatively abrupt, unfavorable shifts in environmental conditions. They apparently discover some hitherto unidentified economies, even though to all appearances they were previously operating in an efficient (that is, optimal) manner. Such a result is common, but it is not clear why an organization discovers economies under conditions of adversity, but fails to do so under conditions of plenty. At least, such a result is not the obvious prediction if we assume that organizations optimize. On the other hand, suppose a firm has a goal (a sales goal, profit goal, or other) that specifies what level of performance is satisfactory. Suppose further that this goal adapts to experience in such a way that it is some weighted average of past performance. If performance is a function partly of an exogenous environment and partly of search activity on the part of the firm, fluctuations in the environment will be transformed into fluctuations in the performance of the firm in a way that depends on the rate of adjustment of goals to performance and the productivity of search activity. Now suppose that search productivity depends on the amount of slack in the organization. During times in which the environment is favorable, goals are easily achieved, and search activity is modest. Slack (in the form of unexploited opportunities, undiscovered economies, simple waste, and so on) accumulates. Such slack becomes a reservoir of search opportunities, and the size of that reservoir affects the productivity of subsequent search. The net result is that environmental fluctuations are dampened by the internal decision process. Such a process can be used to interpret both the resilience of organizations during bad times and their apparent sluggishness in exploiting opportunities during good times; the interpretation appears to do somewhat less violence to our observations of organizational life than do other alternative explanations.

The example illustrates a conspicuous feature of many models built on a bounded rationality base: they tend to make choices substantially more history-dependent than do more conventional theories of the firm. It is possible, of course, to transform most history-dependent theories into theories of expectations; and this option has been exercised in much of microeconomic theory. Such a procedure conserves the theory and is, consequently, not lightly to be rejected; but conservation of received theory has generally not seemed as vital to behavioral economists as it has to others. In fact, no one has yet been able to reconcile the spirit and specifics of Simon's ideas and the main thrust of microeconomics in a way that is congenial to both. As a result, Simon's direct influence on the main body of microeconomic theory has been modest. Despite the attention given his ideas by other key economists, a student in the United

States can easily take several courses in microeconomics without hearing of him. For such purposes, his ideas have been "economized" into a general consideration of search and information costs, into a more elaborate consideration of expectations, or into a brief footnote on the possibility that the classic portrayal of choice may not necessarily describe what happens at the individual firm level.

Simon's ideas on decision-making have had their primary direct impact in economics among those concerned with embedding economic theory into a behavioral understanding of actual decision processes within the firm. Those effects can be illustrated with a few examples, drawn from a longer list. The key con-

Speaking of Science

Weather Modification: A Call for Tougher Tests

Since the inception of weather modification in 1946, various techniques have been enthusiastically adopted, but have all too often failed in the end to modify the weather. A case in point is rainmaking. In the early 1950's, some experts began to take a dim view of the then current penchant for rainmaking, believing that the rainmakers did not have a full appreciation of the complexities of the atmosphere.

Since then, it has become obvious that cloud seeding—that is, the addition of enough particles to clouds to promote condensation of water vapor—can produce complicated effects. For example, rainmaking may fail both because of too little or too much seeding. Rainfall can actually be reduced by seeding under some conditions. Both increases and decreases in rainfall may extend far beyond the area directly seeded, some researchers now believe. Reliably detecting these effects is made particularly difficult by the limited ability of experimenters to predict how a cloud would have behaved if it had not been seeded.

A recent report* to the congressionally mandated Weather Modification Advisory Board from its Statistical Task Force concludes that researchers have not always coped well with this sort of complexity. "The inherent difficulties of the situation and the well-founded need for completely anchored conclusions," the report says, "have not been taken seriously enough." A strict evaluation of seven rainmaking experiments of the past 5 years left only one that the task force finds statistically convincing. This experiment, the second of two successful Israeli experiments, appeared to achieve a 15 percent increase in rainfall. The other experiments yielded results that fell short of statistical significance or their interpretations were confused because of their questionable design.

When considering techniques to affect other kinds of weather, the panel notes that the major American effort to suppress destructive hail production, modeled after reportedly successful Soviet experiments, was terminated 2 years early for lack of positive results. On the other hand, they describe as wholly successful two earlier attempts to increase mountain snowfall, and consequently spring runoff, in Colorado.

In contrast to the reserved tone of its task force's report to it on past experiments, the WMAB, in a report† to the Secretary of Commerce, expresses optimism about the future of weather modification. Citing a broader basis for its conclusions, the WMAB declares that, with more money and some hard work, significant modification of many kinds of weather seems to be probable in the next two decades. It estimates that snow in the mountains and rain in the High Plains and Midwest could be increased by 10 to 30 percent by the late 1980's. Hail reduction, up to 60 percent in some kinds of storms, could be realized by the 1990's. While more specific in its hopes and expectations, the report reflects the optimistic tone of previous requests for increased funds made in 1973 and 1966 (National Academy of Science committees) and in 1957 (a presidential committee).

The WMAB finds encouragement today in the suggestive results of experiments conducted in Florida and southern California that were positive but were not judged to be statistically convincing. These results, plus an increasing understanding of cloud physics and an estimation of the impact of their recommended increased funding, led the WMAB to go beyond its task force's conclusions, according to Harlan Cleveland, chairman of the WMAB.

The statistical task force of the WMAB does not proscribe future experiments with the weather, but it does conclude that researchers must exercise greater caution in designing and evaluating experiments if the results are to be convincing. Toward this end, the panel provides a demanding "guidebook" of standards and procedures that may become de rigeur in the field. It contains a strict procedural regimen, including statistical requirements compared by some with those that have been adopted for the best clinical investigations in medicine.

Such a regimen must continue to include thorough randomization of seeding operations, the report concludes. In addition, an exploratory phase, in which likely situations susceptible to seeding are identified, must precede a confirmatory phase, in which the highest standards of experimental design are imposed in order to answer a single question. Too little data have sometimes been used in the past to answer too many questions, the task force says. Even the appearance of possible subjective influence must be avoided. The task force also recommends making the details of experiments available within 1 year after completion of fieldwork, and serious consideration should be given to parallel, independent analyses of results.

The statistical task force concludes that "in view of the great importance of enhancement [of rainfall] if it exists, . . . it is quite reasonable to go forward" with experimental cloud seeding, but only if investigators adhere to the highest standards. While philosophical differences may exist concerning the status of weather modification, most WMAB members welcome the development of the guidelines and agree that they will probably become a standard for all proposed work in the field. Some researchers believe it is the only way a concensus will ever be reached.

—Richard A. Kerr

**The Management of Weather Resources* (Superintendent of Documents, U.S. Government Printing Office, Washington, D.C., 1978), vol. 2.
†*Ibid.*,vol. 1.

cepts were elaborated and extended by some of Simon's colleagues at Carnegie Tech as part of an effort to sketch a behavioral theory of the firm, an effort that stimulated subsequent work both in the United States and Europe on pricing, capital investment, innovation, and internal resource allocation. The work of Jacob Marschak and Roy Radner on a theory of teams was a parallel effort more in the tradition of economic theory, but it drew on Simon's work and, in turn, influenced the subsequent development of limited rationality theories. Radner's analysis of satisficing has identified a number of situations in which optimal search or decision strategies involve satisficing rules. In a series of articles exploring natural selection models in economics, Sidney Winter has connected the idea of stable rules in decision procedures to an evolutionary theory in which rules are reproduced through survival and growth of firms. Winter and Richard Nelson have used similar ideas in their work on a theory of growth, viewing economic action as more rule-determined and less choice-determined than is typical of most such theories. Oliver Williamson has based important parts of his work on a new institutional economics on ideas of transaction costs that are considerably influenced by Simon's work during the period 1947 to 1958. Most books on business administration give Simon major credit for reformulating the view of decision-making that characterizes that field, and Peter Keen has recently summarized in an article for *Management Science* some of the many ways in which Simon's work has influenced management, information, and decision sciences.

I have emphasized the work on decision-making, partly because it is the part of Simon's work that I know best and partly because I believe it to be his most important contribution to economics. But there is a third reason. What makes Simon almost unique among economists who have worked on problems of choice is his pervasive importance outside economics. His ideas are in major works on decisions done in political science, sociology, and psychology. Graham Allison, in his study of the Cuban missile crisis, used a set of ideas about organizational choice that were heavily influenced by Simon's work. He showed how both Soviet and American decision-making during the crisis was difficult to understand as a simple case of strategic rationality. Charles E. Lindblom's work on the relation between governmental and market decision-making, including his very influential paper on "The science of muddling through," developed an appreciation of the intelligence of bureaucratic and political decision-making that supported and extended Simon's work. James Q. Wilson in his work on police, J. P. Crecine and Aaron Wildavsky in their work on public budgeting, John Steinbruner in his work on French bureaucracy, Johan P. Olsen in his work on Norwegian bureaucracy, and recent Soviet work by D. Gvishiani and others all depend on an understanding of Simon's work. Indeed, it is hard to find a major study of bureaucratic decision-making in recent years that does not use his ideas, and their influence has spread to studies of the courts by Martin Shapiro and to studies of Congress by Lewis A. Froman.

The work on behavioral decision theory by Ward Edwards, Amos Tversky, Daniel Kahneman, and their colleagues goes considerably away from the Simon framework, but it traces some of the key questions to his original formulations. Recent investigations of decision-making under conditions of ambiguity (where goals and technology are unclear), particularly in educational and public institutions, are also clearly influenced by the original ideas. Decision-making by rule, bounded rationality, satisficing, and the other central concepts that Simon outlined more than 20 years ago continue to illuminate studies made by many different disciplines in many different kinds of organizations. For example, in the December issue of the *Administrative Science Quarterly* there will be a study showing how such ideas correctly predict some empirical aspects of the adaptation of a university to adversity; in the current issue of the *Bell Journal of Economics* there will be several articles showing the pervasive influence of these ideas in economic studies of decision-making. In most months, it would be possible to say essentially the same thing about almost any major journal in social science that is concerned with decision-making or organizations.

The Swedish Academy properly emphasized Simon's giant role in the development of an understanding of bureaucratic decision-making in both the private and public sectors, but it could hardly have been ignorant of some other aspects of his career in science. His writings are a collection of prolegomena. In economics he has made significant studies of the size distribution of firms, executive compensation, and the employment contract, as well as one of the more fundamental considerations of the identification problem in econometrics and one of the early foundations of causal modeling. In each of these efforts he has developed new perspectives and new methods for exploring them.

And there is more. Although a prize in economics does not directly record it, Simon's creativity in economics is matched by his creativity in other social sciences. There are several contemporary economists who write insightfully about politics, sociology, or philosophy. Gary Becker, James Buchanon, and Anthony Downs are examples. For the most part, their writings exhibit creative disciplinary chutzpah, using economic analysis to illuminate a problem in politics, marriage, or ethics. Simon's writings in several disciplines are distinctive for the way in which they speak to each discipline in the language of that discipline. His work on causal order is written for an audience of philosophers; his work on small groups takes some familiar studies in sociology and social psychology and addresses them in the terms used in those fields; his articles on power consider the concerns of political scientists in the language of political science; his work on human problem-solving is a fundamental exercise in psychology; and his work on artificial intelligence is often basic computer science. He is an interloper, but not an imperialist, and his development of mathematical models of social groups and computer models of thought substantially changed the direction of research in domains of the social, behavioral, and computer sciences that are distant from economics, administration, organizations, and decision-making. Indeed, his record exhibits such breadth and versatility that it would clearly be pretentious were it not so distinguished.

A leading American economist once asked me why Simon stayed at Carnegie-Mellon University in the face of innumerable attractive opportunities to go to universities of grander repute. The question was casual, the occasion innocent, and my response lighthearted: Perhaps, I said, he wanted to generate one more bit of data for the proposition that decision-makers seek satisfactory, rather than optimal, alternatives. The answer may have a particle of sense in it, but I do not think it is what Herb would have said. I suspect he would have said that a discipline that finds ordinary behavior surprising probably ought to spend a bit more time looking at ordinary behavior, and a bit less time contemplating its theories. And I suspect he would be right.

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