puzzling that the concept of "settlement units" (neighborhoods) that Rothschild has proposed elsewhere (pp. 29–37 in *Living in Cities*, E. Staski, Ed.; Society for Historical Archaeology Special Publication Series, no. 5, 1987) does not appear in this analysis.

Still, this work is a building block. It raises important questions and proposes a methodology and scale of analysis that make sense for the analysis of archeological data and the creation of historical ethnography. The need for further archeological work in the city is made abundantly clear. A particularly important, if not quite realized, aspect of the approach developed here is the promise to integrate residents' "imaging" of their neighborhoods with social and economic developments within the city and with urban geography's principles of placement. Such work, as it continues, will increase our understanding of urban places. By examining the historical context of the growth of New York, this work also focuses attention on the most interesting potential of historical archeology in this country: exploring the peculiar development of American culture.

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Bounded Economics

Models of My Life. HERBERT A. SIMON. Basic Books, New York, 1991. xxx, 415 pp. + plates. $26.95. Alfred P. Sloan Foundation Series.

Herbert Simon has ranged widely: psychology, artificial intelligence, organization theory, and economics. My own competence is only in the last, and this means that I will do only very partial justice to his achievements. But there is a unifying theme that applies to all of his work. That theme is the problem-solving agent (or groups of agents). How do individuals and institutions make decisions, and what guides them? How far can the problem-solving person be mimicked by a machine program?

Orthodox (that is, neoclassical) economic theory is quite unconcerned with the process by which decisions are reached. The agent is rational, and this means that when he chooses he chooses that action to which no other action that is feasible is preferred. (The agent is endowed with a preference ordering over the set of all possible choices he could conceivably make, since feasibility of choices changes with market conditions.) Like Venus from the waves, decisions arise spontaneously from the interaction of preferences and feasibility.

Simon has no difficulty in persuading one that this is not only implausible but also often impossible. In a world of neoclassical agents no one would play chess. It would be as uninteresting as a game of noughts and crosses. The reason why chess is played is that our human (and machine) computational powers are far too weak to perform the process of backward induction on the complex chess tree. Quite generally, neoclassical theory endows agents with computational powers they cannot possess. But it also endows them with more information than they can either acquire or store. To know your market opportunities you must know very many prices. You must also be fully informed of the nature of the goods you choose between. Think of a second-hand motorcar!

Simon's answer is to replace the rational with the boundedly rational agent. The latter follows a route of "procedural rationality," which is a name for a sensible way of solving the problem of choice. But how do you know when you have solved it? According to Simon you are content to meet your aspiration level—you satisfice. Both procedural rationality and aspirations are matters for empirical research, for example, computer simulation, and, unlike rationality, not the stuff for axioms. This kind of research in many settings has been at the center of Simon's work and of his many contributions.

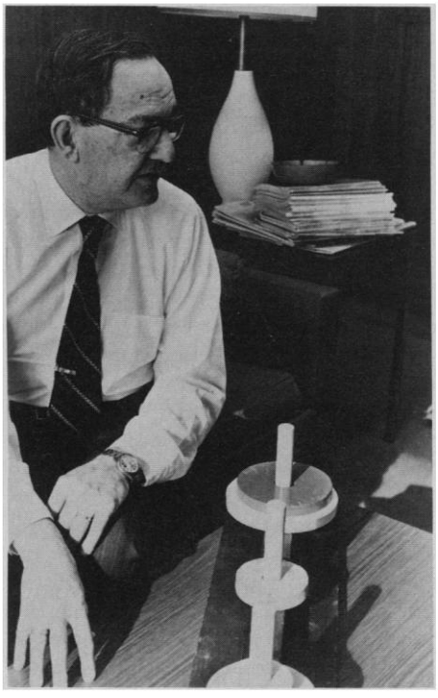
I have a strong feeling that Simon is indeed on the right track, but I must also confess that I find his arguments often incomplete and not sufficiently deep. He has done many splendid things in his chosen endeavor, but he may, if I may say so, have "satisficed" in the extent of his thinking.

Economists are very used to being told by their students and others that "people don't behave like that." One answer given by Milton Friedman and his many followers is that assumptions don't matter, only predictions do. If they are not falsified then one has a good theory. This charter for the intellectually lazy can be criticized at all sorts of levels, but it suffices to remark that no economic theory whatsoever has ever been regarded as falsified by all reasonable and knowledgeable people. Data and statistical inference don't deliver that kind of answer. Even experiments in economics are inconclusive because they *are* experiments and not decisions in real situations.

However, there is another answer. Let Simon be right but notice that "aspiration levels" are not only partly social but also endogenous to the economic process. Firms with low aspiration levels will fare badly or be driven out by those with higher ones. Observing others will lead to some learning, and much that has been learned will not be lost. Specialists, seeing a profit, may arise to provide computational and information services. Firms have managers and consultants and experts in operational research. Dispersed information can be aggregated—partly by observing prices. In short we only have half a theory when aspiration levels and computational ability are taken as given. One could instead tell an evolutionary story with the elements noted above and, who knows, it might lead to a plausible account of a distribution of survivors of whom most act rationally.

I myself doubt that this will be so, or plausibly so. Nonetheless, without exploring the interaction of agents one has not finished the argument. It is one of the reasons that account for the comparative neglect of, and sometimes hostility to, Simon's work, which he notes frequently in this book. No one gives up a beautiful and fully developed theory for half of one. But there is something else. It is not at all clear that Simon's critique is very damaging to the traditional work of mundane economics. For instance, when the hourly wage rises and is expected to stay higher, the worker is better off and would like more leisure, and at the same time leisure has become more expensive. There are, economists say, two contrary effects, an income and a substitution effect. This is captured quite precisely in the language of the rational agent. We know what we would like to measure. How are we to tackle the same question *à la* Simon? Even if it is true that our worker has not calculated his optimum actions over his lifetime there is no good ground for arguing that the economist's procedure is not the best at present for understanding what is involved in the rise in wage. In short, the virtues of the theory are that it organizes our thinking rather precisely and it leads us to ask the right questions.

But in all of this economists must understand what they are about, and there is much evidence that many do not. In particular they are reluctant to consider the neoclassical theory as a first step that will not yield certainty and that must be modified in many ways—not least by Simonesque considerations. Simon himself wants to abandon the



"Research on problem solving, using the Tower of Hanoi puzzle as the laboratory task, 1969." The Tower of Hanoi is "a puzzle of Chinese origin involving a pyramid of disks impaled on one of the vertical pegs. The task is to move the pyramid of disks impaled to one of the other pegs, moving only one disk at a time and never placing a disk atop another that is smaller than it. . . . If chess plays the role in cognitive research that Drosophila does in genetics, the Tower of Hanoi is the analogue of *E. coli*, providing another standardized setting around which knowledge can accumulate." [From *Models of My Life*]

crutch afforded by theory even for first steps, and the consequence sometimes seems to be elevated commonsense on a computer. Orthodox economists on the other hand never let go of the crutch. To a European both these failings seem peculiarly American. In America economists, sociologists, and the like call themselves scientists. This is much rarer in Europe. The name matters because it signals an intellectual attitude—in particular that in due course theory and facts will be as transparent as they are in physics and yield similar certainties. I have considerable doubts, more or less for the same reasons as Simon gives for his theory: the order of complexity is too high. Social "scientists" for a long time will have to be "boundedly scientific." The claim to be scientists leads many orthodox economists to attempt to fit quite foolish models to data—their substitute for the "scientific" experiment—while it leads Simon to miss the intellectual possibilities of "wrong" theories. Sometimes it also leads him to a quite shallow "scientism" (see p. 190).

This is an autobiography, and we learn something about Simon the man. He is evidently widely read, a great linguist, a workaholic, and very decent. There are fascinating accounts of university politics, of the first tentative steps to artificial intelligence, of his experiences at the Cowles Foundation, and of his travels. He has thought very widely over many areas, and his contributions are clearly important. For my taste he takes too much pride in "gongs"—that is, signs of external recognition, from election to the National Academy of Sciences (as first social scientist) to the Nobel Prize. Of course it is perfectly natural to feel pride, but perhaps it is nicer to keep it to oneself. But this is a small failing.

The judgment I reach at the end of my perusal of the book is that Simon could have been a great scholar but has only been a very good one. That is because he has been somewhat too ready to "satisfice" rather than to strive to reach beyond his grasp.

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An Alternative to Associationism

The Organization of Learning. C. R. GALLISTEL. MIT Press, Cambridge, MA, 1990. xii, 648 pp., illus. $45. Learning, Development, and Conceptual Change. A Bradford Book.

In two books, separated by 10 years, Charles Gallistel has sought to loosen the grip of the British empiricist philosophers on the study of learning by relating learning to the natural history, motivational structure, and neurobiology of particular species. In the first book, *The Organization of Action*, Gallistel brought together a set of important historical papers about the control of behavior that outlined the elementary units of behavior—reflexes, servomechanisms, and oscillators. He then considered the rules of interaction for these units and their organization into larger hierarchical control structures. In essence the first book provided information about how organisms function as a prerequisite to a realistic theory of behavior and learning.

In the present book Gallistel makes more explicit his alternative framework for the study of learning. He specifically rejects the basic assumption of associative theory that temporal contiguity between events is a necessary and sufficient determinant of learning. He replaces it with computed correlations of events in time stemming from a representational conception of learning based on complex functional isomorphisms between the environment and neurophysiological mechanisms. In this approach, the animal is assumed to record the position and timing of each environmental event veridically; thus, changes in behavior with experience are assumed to reflect changes in the statistical certainty of event correlations rather than changes in the strength of associations and their neural basis. In chapters on operant conditioning (based on expected overall rates of reward) and Pavlovian conditioning (based on stimulus correlations in time) he reviews the well-recognized difficulties with unidimensional associative accounts that explain learning by the simple pairing of a single stimulus (or response) with reward.

Though the sound of "computational," "representational," and "isomorphic" strung together in a single sentence may delight avid followers of cognitive science and send behaviorally oriented researchers scrambling for the exits, Gallistel is fairly cautious about how he uses these terms. "Computational" simply means computable; "representational" means there is a relatively rich adaptive correspondence (isomorphism) between the encoding process (the way the brain operates) and the processes and stimulus relations in the external environment.

Admittedly, the idea of a functional isomorphism between the environment and its internal representation is not new, nor does it entail Gallistel's conceptual approach. For example, traditional learning theorists have viewed the result of operations such as response-contingent reward as isomorphic with physiological changes in connections between neurons. Also, there are points in the book when Gallistel seems to use the concept of isomorphism to invoke a primitive realism (things in the environment must correspond to things in the head). Still, his particular implementation of a richer correspondence between environment and mechanism is a welcome alternative to traditional attempts to compress the complex determinants of learned behavior into the strength of a single associative bond, or even more recent efforts involving multiple associative bonds.

This is an ambitious work, neither glib nor superficial. The book has enough detail