"Modeled boatmen straddle a tule boat on top of a Lambayeque-Chimu bottle. Another tule boat is rendered in low relief under an arch of interconnecting anthropomorphized waves." [From McClelland's paper in *The Northern Dynasties*; Museo Bruning de Lambayeque; photograph by Donald McClelland]

of political propaganda. Conrad thus addresses issues broader than the culture history of the North Coast and presents a dynamic model of statecraft that should generate much interest among colleagues working with complex societies elsewhere.

Further insight into the way the Chimu viewed their political landscape is provided by Theresa Topic, who argues, following Zuidema, that the stages of conquest visible in the accounts represent categories of social identity rather than individual military campaigns. She also suggests that since the coastal dynastic histories do not mention the highlands the Chimu regarded that area as very different from their own in terms of climate and economic and social organization. This is an important point, since other contributors contend that the coast and highlands were characterized by a shared, deep-seated, and long-lived Andean cultural pattern ("*lo andino*"). This is particularly evident in the contributions of the four ethnohistorians, Moseley's essay, Kolata's provocative analysis of Chan Chan's urban concept, and Cordy-Collins's and Richardson *et al.*'s papers, which specifically address the issue from the perspective of Central Andean–North Andean interaction.

The book is also striking for various revisions of previously widely accepted positions, one of them a major reversal of the conclusions reached by the Chan Chan–Moche Valley Project, a large-scale archeological investigation carried out between 1969 and 1974 and directed by Moseley and Mackey with the participation of various of the authors in the present volume and others. Paradoxically, Moseley and Kolata overthrow what was considered one of the best fits between archeology and the dynastic histories: the number of ciudadelas (royal palaces) and Chimu kings. They now call into doubt the sequential ordering of the ciudadelas (and rulers) at Chan Chan, the sprawling capital city of the Chimu, and argue instead that the ciudadelas were paired—a reflection of the structural duality they hold to characterize Chimor (see also Rostworowski and Netherly). Until there is an unambiguous chronology for the capital, however, the newly proposed pairing as well as the ciudadela sequence will remain debatable.

With its increased database (most notably Shimada's tour-de-force discussion of Sicán and radiocarbon dates, Donnan's masterful comparative analysis of the Chotuna and Dragón friezes and Chotuna and Chornancap excavations, and Richardson *et al.*'s new data from the far North Coast) and pioneering integration of ethnohistory, archeology, and the Andeanist structural perspective, *The Northern Dynasties* is a valuable contribution to Andean archeology and to the comparative study of ancient statecraft.

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The Ordering of a City

New York City Neighborhoods. The 18th Century. NAN A. ROTHSCHILD. Academic Press, San Diego, CA, 1990. xvi, 264 pp., illus. $44.50.

One of the motivations for this portrait, by a historical archeologist, of 18th-century New York is a fascination with the modern city and the processes that created it. Origin questions are appealing at many levels, no less when the temporal scale is one of mere centuries and the spatial scale is that of a city and its neighborhoods.

Rothschild's aim is to consider the interplay of ethnicity and economics in the formation and persistence of neighborhoods, especially as New York was "rationalized" through the 18th century. The historical and archeological methodology is meant to examine the large and complex city by analyzing its subunits; that is, by focusing on the way city-dwellers themselves experienced the city. The author claims many audiences for her analysis of urban structure: urban planners, cultural geographers, historians, and cognitive psychologists. Though she does not explicitly target anthropologists or historical archeologists, they will also find useful data in the book.

There are some interesting findings that come out of the detailed and painstaking documentary research. Changing ethnic strategies are apparent in the spatial analyses: Those for whom ethnicity became less economically useful tended to abandon it as a means of self-identification, at least as a determinant of residential location. Newcomers and those upon whom ethnicity was imposed, particularly Jews and free African-Americans, continued to live in residential clusters defined by ethnicity. As ethnicity at least partly gave way to economic considerations, occupation rather than wealth tended to be reflected in residential clusters. Higher-class (merchant) segregation indicates exclusionary residential choice, whereas spatially similar segregation of semiskilled workers would seem to result from a different process, as much of the residential clustering came about as a result of church leases. Occupational segmentation became increasingly important through the 18th century but began to give way to the class structure that would dominate in the 19th century. As in other cities, massive economic changes occurred as wealth became more concentrated through the 18th century.

Unfortunately, the weakest part of the book is the integration of the archeological data. Faunal remains (of meals) are examined because documentary record on food choice is "almost nonexistent." Availability seems to be the most important factor affecting the composition of these remains; they can be correlated in a statistically significant way with neither ethnicity nor class. One unexpected conclusion of the analysis of food cost is that 18th-century meals were private affairs, unconnected to public status displays. Because historical and archeological data in other colonial cities indicate that dining was socially particularly important in the 18th century, such a suggestion demands further comparisons between data sources both within sites and between cities.

There are some frustrating aspects of the book. It is careless in places; for example, although city wards are an essential part of the descriptions, it is difficult to find a map on which the wards are identified by name. It is clear that the book is not written primarily for an archeological audience, yet archeologists will expect some data that are not available and non-archeologists will find some of the data description confusing. It is

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