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Formal Models and Formalist Economic Anthropology: The Problem of Maximization

Stuart Plattner

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FORMAL MODELS AND FORMALIST ECONOMIC ANTHROPOLOGY: THE PROBLEM OF MAXIMIZATION

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Harold Schneider's work has been an important force in the development of economic anthropology. His hard-boiled insistence that we see things for what they are, and not deny the economic quality of exotic behaviors merely because the analogous behavior in our own society is not economic, places him in a valuable tradition in anthropology. His insistence that people generally act rationally in pursuit of their own interests is a healthy antidote to any lingering intellectual miasma caused by a belief in "culture-bound" natives practicing exotica. Because of this and more, we appreciate his work.

In his recent book Economic Man he gives a strong picture of formalist economic anthropology.[1] Among other things, the book provides a nice fifty-page summary of selected concepts in microeconomics, some of which are then used as a theory of social exchange to reanalyze some well-known cases in the literature; it includes a good discussion of the relevance of economic anthropology; and in it Schneider slings a few more barbs at the substantivist camp. This list has not been exhaustive, and this essay is not meant to be a complete review of the book. Here I will mainly react to what I think is the key issue in his argument: the use of maximization as a basic assumption for a model of human behavior.

Schneider claims that he has the answer to our economic anthropological problems, and tells us that

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[1]Some of the ideas presented here were discussed at a seminar chaired jointly by myself and John Bennett, at the Department of Anthropology, Washington University, on April 17.

> anthropology can profit by opening its mind to [microeconomic] analytic methods, not only in economic anthropology but more generally as well. Economic man is . . . in all of us, he is a part of all of us, but he is an abstraction from us and not the whole of any of us . . . (p. ix, square bracket insert mine).

Yet methinks he doth protest too much, and demonstrate too little. We all stand more or less in awe of the mathematical power of microeconomics--at least as a normative discipline. But even economists, and famous ones at that, admit that their models are brutally simplified pictures of empirical reality (e.g., Mansfield, 1970:15-16; Samuelson, 1964:737). The empirical point of the game is to show how the abstract model relates to the complex, noisy reality we must deal with. There are precise rules about this, yet even Nobel prize-winning economists use the word "art" when they talk about it (Samuelson, 1964:739). Thus we should adopt a Missourian "show-me" attitude towards Schneider's claim that the microeconomic model is applicable to all sorts of transactional behavior. Especially since the claim is not substantiated with empirical demonstrations in the strict sense, where ethnographic data is actually used in a microeconomic model. Most of the examples in <u>Economic Man</u> are actually <u>ex post facto</u> rationalistic sorts of analyses where microeconomic analogs are merely identified in ethnographic cases.

Microeconomic theory, insofar as it is a unified body of concepts rather than a claim to a domain of research, is the normative study of efficient production, exchange, and consumption in defined contexts. The theory specifies the optimal combinations of inputs to some process which creates an output valued along a single dimension, such as money or utility. Thus in production the model shows how to combine various factors (such as land, labor, capital) in order to efficiently produce an output (e.g., corn). In exchange or consumption the model delineates the levels of different goods necessary to yield the optimal level of profit or satisfaction. Efficiency, here, refers to the largest quantity of output per unit of input, or the smallest quantity of input consistent with a given level of output.

Schneider claims that this model has universal applicability and exhorts us to use it. Now all theories, being phrased in general terms, can be used for more than one case--otherwise they are trivial. What is needed is the specification of the limits to the theory. We must know what it <u>can</u> interpret so that we have some idea of what it <u>cannot</u> handle, since a theory that explains everything explains nothing. What is sorely missed in the book is the explication of particular attributes which any empirical situation must have in order to satisfy the basic assumptions of the microeconomic models. This would be a significant service to economic anthropology. Without it, we must conclude that Schneider's urgings about microeconomics have not lifted the formalist-substantivist controversy out of the non-empirical mire it has floundered in since its inception.

My main intent here is to criticize the use, by other anthropologists as well as Schneider, of maximization as a basic assumption in models of human behavior. I have no quarrel with the use of maximization as a normative concept, but do have doubts about its validity in descriptive models. The importance of this question increases as more and more formal models, based on microeconomics, are used in anthropology.[2]

Is what Schneider says about microeconomics valid? Is the field really based on the "primal" assumption that "actors make decisions aiming to maximize their utility. . ."? (p. 35) Is it true that

> The famous and much-maligned economic man is a greedy fellow, seeking always to improve his position with respect to value taken in some general sense (utility). Is this assumption justified? Is it something peculiar to economics, a crutch upon which it leans in contrast to the harder sciences, which can do without it [sic] as they deal only with observable and measurable reality?" (p. 35)

This is a complex question and deserves to be examined at length. To begin, we must ask whether the assumption that people maximize as a strategy is empirically justified, that is, whether it describes observable behavior. If it does not, we must ask whether it should, or whether the model can do without it. If it should then prove to be dispensable, what concept should replace it? And lastly, given all of that, what should we conclude if a microeconomic model is found to apply to some "really" ethnographic data, what will that tell us about

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[2]Here I must clarify a confusion in the book between a "formal" approach to data analysis and the "formalist" approach to economic anthropology. Schneider does not distinguish between the two, as for example in the following: "formal method only works if men are seen as entirely self-oriented" (p. 21). Now, this cannot really be, since formal models (as sets of deducations, from explicit assumptions, related to each other in logically sound structures), are obviously applicable to all sorts of reality. Formal models of exchange and production are constructed without any mention of microeconomics--ecologists and psychologists do it all the time. The validity of the microeconomic formal model is certainly a separate issue from the value of formality in theorizing. So Schneider uses the wrong word, and says "formal" when he means "micro-economic."

the real world from which the data was drawn? I will discuss this last question in light of a strange thing about this book. Although Schneider exhorts us in the strongest terms to use microeconomics wherever we can in analyzing our data, he ignores the only published case that I know of where a microeconomic model is used to statistically analyze ethnographic data. This is the article by Benton Massell, a mathematical economist, published first in 1963 and republished by Schneider and LeClair in 1968. In the light of its complete absence from this new book, it is astounding to note that the article consists of a production function analysis of Schneider's own Turu data, done by Massell because he though that Schneider was trying to do verbally what could be done econometrically (personal communication).

I must first note that microeconomic analysis always takes the parameters of the economic environment as given, or exogenous to the model. Thus the maximum efficiency situation for a particular firm with set amounts of capital, labor, ability, and so forth is calculated by the economic analyst, given the culturally set definitions of a "normal" work day, rate or work, level of income, etc. This solution is completely culture bound and situation specific. When unions succeed in lowering the work-week to 25 hours, or banks succeed in raising the interest rate to 20%, then a new maximization point must be reached. It does not matter that a model can only be used to predict if particular values can be given for the parameters, since this is one of the attributes of a good empirical model. But as cross-cultural observers we are concerned with explaining how the parameters get set. Is the society "maximizing its utility" in some sense by changing the definition of a normal work week from 40 to 25 hours? Can the same be said about the interest rate, or about international relations as they affect trading patterns? If the claim is "yes," then by what mechanisms does this process work? The maximization position which microeconomic theory describes is always purely local, and wholly dependent upon the workings of the (exogenous) cultural system to provide the parameter values. The analysis is usually normative, in the sense of showing how an observed process deviates from some optimal state; rather than descriptive, meaning that it shows how an observed process developed and functions.

Is maximization a valid empirical hypothesis? That is, does it describe behavior that we can observe in the real world? Few economists think that it does. To quote from a recent work on the subject,

> "There is probably unanimous agreement in the economics profession today that theoretical analyses of profit-maximizing behavior are not to be taken as a literal account of the processes by which firms make significant economic decisions . . . .Given the methodological consensus of decision processes, it is not surprising that there exists nothing that could be regarded as a coherent orthodox view of how firms actually make decisions." (Winter, 1971:240)

In the context of microeconomic theory, a maximizing decision requires the decision maker to calculate and compare marginal values (revenue, costs, products) which are extremely subtle ontological entities. There is an ongoing field of research in psychology which examines the relation between statistical inferences made by average people, and the corresponding optimal inference as would be made by a "statistical man" (Peterson and Beach, 1967). While it is premature to summarize the results of a new area of research, I believe it can be said that statistical entities like the parameters of regression equations are not normally well estimated by people. Indeed, Herbert Simon, one of the leading researchers in the area of behavioral decision making, long ago flatly claimed that the computational difficulties involved in solving the maximization equations in a complex process are beyond the abilities of human beings (1955). No evidence has come forward since then, that I know of, that would lead us to alter this view.

Perhaps empirical realism (the quality of being observable in the real world) is not necessary for the maximization concept to be used as an axiom of the theory. After all, if the deducations drawn from the maximizing axiom lead to empirically valid predictions, if "individual firms behave <u>as if</u> they were seeking rationally to maximize their expected returns" (Friedman, 1953:21), then what does it matter if we happen to know that practically no one firm <u>actually</u> does "seek rationally to maximize?" As Schneider says, the maximization axiom allows us to "solve equations" and to predict things through the "establishment of equilibrium points" (p. 35). In fact Schneider, following Friedman and Chomsky, goes even further and links the use of "mystical" assumptions to the "great success" of advanced sciences such as physics (pp. 35-37).

This sort of notion, that a theory may actually be superior for the unreality of its assumptions, has been impishly labelled the "F-Twist" by Professor Samuelson in his superb critique of it (1963, 1964, and especially 1965). Essentially he says that unreality in assumptions can be nothing but a weakness in any theory which aims to explain reality. Sometimes, in the development of a field, an unrealistic assumption may prove useful in allowing the construction of a model which proves empirically valid in its predictions. This usefulness should not blind the practitioner to the fact that ignorance is always a sad fact and never a blissful state in science. The actual debate that Samuelson's papers contribute to quickly left this rather obvious point to focus on more difficult general questions of the relationship between observable reality and the statements made by scientific theorists. The papers in the debate contain an important and clear statement of the issues and are worth examining. (I benefited from reading Massey, 1965; Nagel, 1963; Samuelson, 1963, 1964, 1965; and Simon, 1963.)

If maximization is too unrealistic to be part of the general microeconomic theory, what concepts can take its place? So far as I can tell, there have been two main streams of discussion on this point, concerning the theory of the firm. One is based on a model of natural selection, and the other on observational studies of natural decision-making.

The natural selection approach can be summarized as follows: in essence it does not matter what decision rule a firm uses so long as it exists in a competitive environment. For if and when a firm becomes more efficient than its competitors, for any reason including chance, it has a higher probability of remaining in business than its less efficient peers. Over time, a process of natural selection can insure that the firms which remain in business are the more efficient ones.

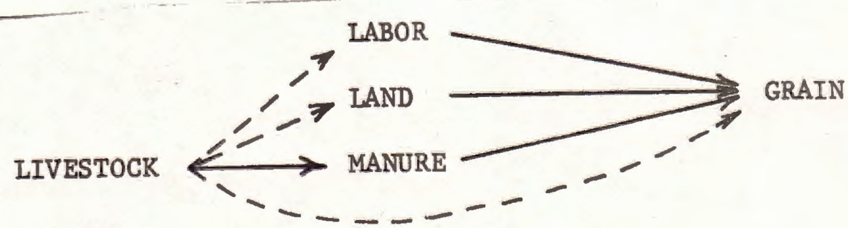
Analogs of the biological model of natural selection are commonly found in social science and usually have face validity, due to the logical power of the general model. The problems with specific models usually concern the isolation of the behavioral analog to genetic inheritance. Winter notes that a random decision rule is not appropriate even logically, as the model must be applicable to firms over the long run and randomness does not imply consistency over time. If the firm's behavior is not consistent, success today says nothing about success tomorrow and thereafter, even in an unchanging environment. Thus a decision rule that is somewhat stable most be postulated. In that case, however, its implications for short-run predictions must be accepted: "The very continuity that makes evolutionary adjustment possible in the long run may produce short-run responses to changed conditions that are significantly maladaptive" (Winter, 1971:245). This sort of notion is familiar to economic anthropologists acquainted with the ecological explanations of over-production in primitive societies, such as the yam mounds of the Trobriands. The excess production in the short run is seen as an adaptation to long run extreme variations in productivity caused by environmental changes. The strategy that produces huge rotting piles of yams at the end of most harvest seasons also produces the "Liebig-ian" minimum necessary for the survival of the group in seasons where nature does not act normally.

Winter stipulates that decisions are governed over the long run by routine applications of simple decision rules, which are only examined and changed when the normal functioning of the firm is upset.

This is a kind of "satisficing" decision rule, as proposed by Simon in his classic paper on "A Behavioral Model of Rational Choice" (1955). Here he argued that most actors seem to examine alternatives sequentially and accept solutions to their problems that merely surpassed some threshold, rather than examine all alternatives concurrently in a search for a unique maximizing solution. Cyert and March use these sorts of decision rules to develop a behavioral theory of the firm in their landmark book (1963). One of the studies they describe is a computer simulation of the pricing behavior of a department in a large department store. The program was built upon satisficing rules, and accounted for the observed pricing behavior to an extremely close degree (in three sorts of tests the model predicted 88, 95, and 96 percent of the prices). Thus Schneider should not tie his belief in the fruitful yields economic anthropology will harvest by cultivating the microeconomic orchard, to maximization. Alternatives exist in the literature which do not force us to deny one of the main strengths of ethnography, our common-sense familiarity with the realities of every-day life.

What then, does it mean when a microeconomic model is used to analyze ethnographic data? A model "explains" data insofar as it "fits," meaning that its assumptions are relevant to the empirical situation and that it statistically accounts for a significant proportion of the variance. Let us examine what to my mind is an excellent case: Massell's production function analysis of Schneider's own Turu data (both articles are conveniently reprinted in LeClair and Schneider, 1968). This is a good test because the analyst is a professional economist and so could be expected not to misinterpret microeconomic theory. We can focus on the main issue of the meaning of the analysis and not be misled into quibbles about anthropological mis-translations of economic concepts.

Schneider portrays the Turu men as economic actors interested in gaining wealth, and able to act strategically in order to do so. Their primary goal is capital in the form of livestock; however the stock is easily and frequently exchanged for grain. Wives produce grain, and apply manure in the process as the necessary fertilizer. Thus the system can be diagrammed as follows, where solid lines are physical production causalities, and dashed lines denote exchanges (i.e., livestock physically create manure which physically contributes to create grain; while livestock can be directly exchanged for grain as well).



A Model of Turu Grain Production

Massell describes this process by a production function relating quantities of grain to a set of factors of production consisting of fertilizer (a proxy for capital, or livestock), land, and women (a proxy for labor). Having measures of these variables for 29 homestead "farm-firms" in Schneider's data, Massell fits the model to the data statistically in two ways: by the common Cobb-Douglas form of logarithmically transformed variables, and by a simple linear form, both estimated by multiple regression. The Cobb-Douglas function yields the following estimates:

$$\ln(Q) = .673 + \underset{(.158)}{.523} \ln(K) + \underset{(.174)}{.267} \ln(N) + \underset{(.321)}{.330} \ln(L)$$

$$R = .785,\ F = 13.4$$

where ln denotes a natural logarithm, Q is grain, K is a fertilizer index, N is labor, L is land, and the numbers in parentheses under the coefficients are the standard errors. The value of R is not very high, but certainly not insignificant when it is remembered that data based on individual observations usually has more scatter than data based on aggregates. Massell notes that the coefficient for capital is significantly greater than zero at the .01 level, while the coefficients for labor and land are not significant at the .05 level. He then goes on to observe some facts about the data.

First, the coefficients sum to one, which is evidence of constant returns to scale. The scale condition for firms in equilibrium is precisely unity, since then it does not pay to increase or decrease scale. Massell does not explicitly infer from this that the Turu homesteads are akin to competitive firms, but this is the reason that returns to scale have theoretical interest.

He goes on to compute the marginal value products of the factors of production (i.e., the monetary value of the physical marginal products), and compares them to the monetary cost of the factors. This comparison is at the heart of production analysis. It reveals the economic structure of the production function by comparing the yields of units of capital spent on each factor. Under conditions of perfect competition firms will keep buying factors of production until a dollar spent on any factor yields the same revenue as a dollar spent on any other factor. This is the equilibrium position, as otherwise the firm could make more revenue by buying more of some, and less of other factors.

In this light it is noteworthy that a shilling spent on a cow yields .155 shillings worth of grain, one spent on labor yields the same (.150) and a shilling's worth of land yields almost two shillings (1.95) in return. Massell concludes that

> Given the institutional factors which help determine the price of land, it is quite understandable, in purely economic terms, for a man not to want to sell land. *More-over the community has set the relative prices of cattle and women in such a way as to reflect their relative marginal contribution to grain output*. . . .The question arises, can the results of the regressions be accepted as evidence of the economic "rationality" of the Turu? . . .one might argue that the pricing of women relative to cattle (and refusal to sell land at the institutionally fixed price) lends support to [this] thesis. (P. 449-450 in LeClair and Schneider; italics, and square bracket insert in place of the word "his", added by S.P.)

Finally, he notes that any "rationality" applies to the aggregate and not necessarily to the individual farmsteads. This is so because each farm could increase profits by buying more of some and less of other factors until "the relative marginal productivies equal the relative factor prices" (*ibid*, p. 450).

The prices of factors of production in a competitive economy are theoretically set by the working of supply and demand through the market. Massell implies, on the basis of his analysis, that an analogous market for wives and cattle exists in Turuland. Now, a market must display certain characteristics, relating to the flexibility with which resources flow between opportunities, to be an efficient mechanism for price-setting. In particular, the things offered for exchange must be homogeneous, so that a buyer can potentially buy anything from anyone, not restricted by his personal relationship with the seller; the participants should be similar in size or share of the market, so that every participant's decision to buy or sell is independent of any other decision; resources should be able to enter, leave, and substitute for each other freely; and participants should have good (or perfect) knowledge of offers to sell or buy and production possibilities. Finally, participants should be motivated to economize, in the "weak" sense of generally preferring more to less wealth.

The market for wives in Turuland is not perfect in the technical sense. In the competitive model, factors move into and out of different productive enterprises in response to differences in profitability--but do Turu men do the same with grain production? Do they obtain wives freely from any sector of the economy? Obviously not. By what process, then, have the Turu come to value wives in such an "economizing" way? In what sense has the microeconomic model of the production function explained the reality of Turu grain farming?

This is not the place to answer that question. I hope that Schneider will do so some day, for I think that the answer will be significant to us. I want to cast doubt on the validity of Massell's analysis, not because I know it to be mistaken, but because I think we need an explicit discussion of the issues it raises. Specifically the fit between the assumptions of the model he uses and the empirical situation he applies it to must be examined. I do not raise these questions maliciously, but on the basis of some very significant research recently done by Hugh Gladwin (n.d.).

Gladwin's work is in the tradition of Simon's (op. cit) proposals for alternative, more behaviorally sensible, microeconomic decision rules. Gladwin demonstrates precisely how a commonly used model of decision making is invalid, even when it is "successfully" fitted to a specific body of data. The difference Gladwin focuses on is between hierarchical and "trade-off" rules. In a hierarchical rule, especially the "lexicographic" (cf. Quinn, 1971; Tversky, 1969) rule he uses, the criteria at any level of a decision process have complete logical priority over subsequent levels. For example, in buying a house, no buyer will purchase a house if it costs more than some amount, no matter what its value on other important criteria such as location and appearance. The trade-off model presupposes that the decision-maker may accept a house with a price over his preferred limit if it is very high in location- or appearance value. Thus he trades off negative values on some criteria for positive values on others, in the process achieving a high level on some underlying criteria such as "utility." Regression models are based on trade off processes, while hierarchical or tree models are not.

Gladwin does what Simon, Cyert and March should have done: he constructs a model of decision-making that is hierarchical, produces a body of data with it, and then analyzes this data with the (inappropriate) trade-off model. He thus is investigating the power of our analytic procedures to detect invalid models. The decision he studies is house-buying, and he constructs a computer program to simulate the process using a lexicographic rule. The simulation program produces "buyers" with various amounts of income and preferences with respect to location and appearance; for each buyer a list of "houses" is prepared which vary on the three dimensions of cost, location, and appearance. Each buyer searches the available houses for one which satisfies his criteria; but all buyers use the same lexicographic non-trade off decision rule. In this way the program produces a sample of buyer-house pairs, or purchased houses.

Gladwin then analyzes this data <u>as if</u> he did not know the model which caused it. He uses the seemingly sensible notion that a house's saleability is some function of its cost, appearance, and location acting together, i.e., that buyers trade off one criterion for another. This model, which we know to be incorrect, is statistically fitted to the data with multiple regression. If we did not know the model which produced the data, we would be led to accept the trade off model as an "explanation." Knowing the truth (the value of creating your own data!) we see that it is merely a curve fitted to the data. This may be used for correlation but it is in no sense an explanation.

In his conclusion Gladwin points out that the difference between a true model and a false one may not show up until the basic structure of the empirical situation changes. When some change occurs, the false model will be incapable of dealing with it, while the valid one will be productive of new analyses and understandings.

This may apply rather directly to Massell's analysis of Turu grain production, and potentially to any analysis of data which takes Schneider's advice and uses the microeconomic model (and, of course, it applies to any other model). The moral is simple: empirical analysis must consist of more than curve-fitting if it is to explain the world. The models used must have empirically sensible assumptions.

So as to be as clear as possible, let me state my conclusions: I agree completely with Schneider about the importance of formal analysis (by my definition), because anthropology must deal with data if it is to explain the world we study, and formal methods are superior to informal methods. In addition, and just as with any sort of analysis, the theory used must be relevant to the empirical reality it is proposed to explain. Thus the researcher has the responsibility of choosing the most formal method of analysis that is appropriate to the problem, and must also empirically justify the model as thoroughly as possible.

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