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The Idea of "Advancement" in Theories of Social Evolution and Development

Mark Granovetter

State University of New York at Stony Brook

Implicit in most theories of social evolution, modernization, or development is the idea that systematic rank ordering of societies, on some dimension of problem-solving capacity, is feasible. This paper argues that, in our present or foreseeable state of knowledge, such rank orderings are not empirically meaningful. When the comparison is based on efficiency (the ability to solve current problems) the insuperable difficulty is similar to that encountered in what economists call "interpersonal comparison of utility." When the criterion is flexibility in dealing with possible future problems, uncertainty of prediction in system environments is a crucial obstacle which, even in principle, cannot be overcome. Evidence is cited from nonlinear models in mathematical ecology to support this argument. It is claimed that this critique is related to but independent of previous arguments against developmental theories.

The idea of "evolution" is one of the most durable in the history of social theory (see Nisbet 1969). Implicit in theories of social evolution are a number of assumptions. One is that the most important cause of social change lies in the necessity for societies to adapt to their surrounding physical and/or social environments. Because the range of "problems" posed by these environments is so broad, and so many different "solutions" to them can be imagined, evolutionary theory is embraced by those with highly diverse orientations—from those who favor material causes (as, e.g., Harris 1977; Sahlin and Service 1960) to those identified with the causal primacy of ideas and values (Parsons 1964, 1966; Bellah 1964).

Another common assumption in theories of evolution, as well as in those of political and economic modernization and development, is that one can make clear-cut rank orders among social systems on a dimension of "advancement," defined by systemic problem-solving ability. This assumption

1 I am indebted to Howard Aldrich, Ivan Chase, Ronald Cohen, Shmuel Eisenstadt, Robert Nisbet, Charles Perrow, James Rule, Michael Schwartz, Robert Simon, Charles Tilly, and anonymous referees for their helpful comments; to Rada Dyson-Hudson and Roland Soong for their guidance, respectively, in ecology and mathematical models; and to the Center for Advanced Study in the Behavioral Sciences in whose stimulating atmosphere an earlier draft of this paper was written. The center's support was made possible in part by grants from the Andrew Mellon Foundation and the National Science Foundation.

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requires a corollary belief that one can see clearly what the unit of analysis is which adapts to exigencies or solves problems. The conception of societies as coherent systems whose performance can be clearly tracked over time in relation to some distinguishable external environment is one which dovetails well with systems theory in the social and biological sciences. This accounts for the close intellectual affinity among evolutionism, developmentalism, functionalism, and systems ideas.

This paper is addressed mainly to the enterprise of rank ordering societies: I argue that meaningful rank orders on a dimension of problem-solving capacity cannot be constructed in our present or foreseeable state of knowledge. The argument does not depend on attacking systems-theoretic ideas (though it is somewhat strengthened if one accepts such attacks) or on attributing hidden value judgments to proponents of advancement measures. Nor does it require any denial that, as Lenski points out, "the presence of long-term trends is clearly evident—. . . growth in numbers, . . . increasing division of labor. . . . increased production of goods and services, growth in the power of the state, and increased bureaucratization, to name a few of the most obvious" (1976, p. 554). I will argue instead that even if one grants all these points (which antievolutionists sometimes challenge) the attempt to rank societies will fail because of difficulties of comparison and prediction inherent in the task.

MEASURES OF ADVANCEMENT: EFFICIENCY

Broadly speaking, advocates of intersocietal ranking on a dimension of problem-solving ability can be divided into those whose ranking addresses a society's ability to solve current problems (i.e., its "efficiency"), and those stressing capacity to deal with future ones—some not easily foreseen—so that concepts such as "flexibility," "versatility," and "adaptive capacity" come to the fore. I begin with a discussion of "efficiency."

It is not surprising that considerations of efficiency should be important in a tradition which stresses problem solving. In Parsons's paradigm of evo-

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2 Such a bias is vigorously denied by most contemporary evolutionists. Dole asserts, for example, that "the term progress is used to refer only to forward movement as opposed to reversal, to an advance measured by specified objective criteria" (1973, p. 250); Lenski adds that the term indicates only "specific directional trends (as in the expression, 'the progress of a disease,' or as Hogarth used the term in his famous series of prints, The Rake's Progress)" (1976, p. 561). Yet the emphasis of many evolutionists on the problem-solving capacity of societies, while it may differ from the crude value judgments of Social Darwinism, seems altogether consistent with Enlightenment aspirations toward the possibility of rationalizing and perfecting social arrangements. Given a choice between two societies, one of which was better able to "solve its problems" than another, one may wonder how many evolutionists or development theorists would prefer the "less advanced." But this consideration will not enter further into the argument; the logic or power of a theory is not a result of its underlying value assumptions.
volutionary change, part of the process consists of "adaptive upgrading": the social system and its members become "more productive than before, as measured by some kind of output-cost relationship" (1966, p. 22). The solution of current problems is an immediate and pressing difficulty; hence it is more plausible as a mechanism of social change than some movement toward generalized flexibility in dealing with future problems would be. Lenski and Lenski argue, in fact, that current efficiency may be illusory—specifically, that many individuals and societies "have voluntarily, even eagerly, adopted more efficient tools and techniques and new ways of doing things, because they believed the gains would outweigh the costs. And they were right—up to a point. . . . [They] had no way of knowing that their technological advance would lead to such an increase in population that all the gains would be wiped out in just a few generations . . . ." (1978, p. 245).

That productive efficiency is the mainspring of evolutionary development is a recurrent idea in 20th-century social thought. The connection between efficiency and development has frequently been made by means of the idea that the level of efficiency determines the amount of economic "surplus" produced, beyond subsistence needs. This surplus is viewed as the raw material out of which advanced civilizations are constructed, since its size determines the number of administrators, scientists, and other nonproducers of economic goods that can be supported. As early as 1899, Veblen observed that for a leisure class to exist, "subsistence must be obtainable on sufficiently easy terms to admit of the exemption of a considerable portion of the community from steady application to a routine of labor" ([1899] 1973, p. 25). In his later work, an explicit connection was made between the "surplus" and the notion of evolutionary stages ([1914] 1964, chap. 4).

The archeologist V. Gordon Childe sketched the evolutionary argument in a more complete and influential form, popularizing the notion that human history ought to be seen as a series of revolutions: chiefly, the Neolithic revolution, defined by the invention of agriculture, which catapulted men out of the "savagery" of hunting and gathering economy; the urban revolution, "which ushers in civilization and initiates the historical record" (1942, p. 25); and the industrial revolution. He attributes the first and third of these to technological developments which increased the efficiency of production. Writing of the technical changes involved in the Neolithic revolution, Childe leaves his readers in no doubt about whether it constituted an "advance": "the escape from the impasse of savagery was an economic and scientific revolution that made the participants active partners with nature instead of parasites on nature" (1942, p. 48).

Childe's discussion of the concept of "surplus" is central, but theoretically elliptical. The fullest theoretical development of connections among efficiency, surplus, and evolution has been provided by Lenski (1966, 1970),
and elaborated in Lenski and Lenski (1974, 1978). In this account, socio-cultural progress "refers to technological advance" (1970, p. 101) and societies are ranked along the dimension of "overall technological efficiency, i.e., the value of a society's gross product in international markets divided by the human energy expended in its production" (Lenski and Lenski 1974, p. 46).

Given this view, it is natural to classify and rank order societies according to their technological base. Lenski arranges them from least to most advanced as follows: hunting-gathering, horticultural, agrarian, and industrial societies—with the first three further subdivided into "simple" and "advanced" (1974, p. 96). The ordering is said to be one of advancing technology, and ipso facto, efficiency. Implicit in the assumption that efficiency determines the level of surplus is the idea that a society always produces the maximum possible surplus; without this assumption, efficiency could constitute only a necessary but not a sufficient condition for surplus, and other factors influencing productive decisions would have to be considered.³

Together, these assumptions imply that the less surplus is produced the more work must be required for subsistence. Zero surplus would thus mean that all the available labor time is needed merely to produce subsistence. Correspondingly, Lenski asserts that hunters and gatherers, having the most technologically primitive productive system, "live close to the subsistence level for much of the year . . . there is no sustained economic surplus, and life is often an alternation of periods of feast and famine, or abundance and shortage, with the latter usually more frequent" (1966, p. 97). Members of simple horticultural societies, unlike those "of hunting and gathering societies . . . are not compelled to spend most of their working hours in the search for food and other necessities of life, but are able to use more of their time in other ways" (1966, p. 121).⁴

Recent field studies of hunting and gathering peoples suggest that this bleak picture is exaggerated. Lee, for example, in a detailed input-output analysis of Bushman food production, indicates that "despite their harsh environment, [they] devote from twelve to nineteen hours a week to getting food" (Lee and DeVore 1968, p. 37). Moreover, 35% of the population, not included in this computation, does not work at all because of age or other handicaps (Lee 1969, p. 67).

As evidence on hunting and gathering populations accumulates, it seems increasingly less likely that many ever lived on the edge of subsistence,

³ Pearson (1957) offers a telling critique of the concept of "surplus." Subsequent comments of Harris (1959) and Dalton (1960) are also of considerable interest.

⁴ This pessimistic view of hunting and gathering life is moderated somewhat in Lenski and Lenski (1974, pp. 138–39), but no conclusion is drawn as to the relevance of this change for the evolutionary argument.
without leisure. Sahlin's comments that it "will be extremely difficult to correct this traditional wisdom. Perhaps then we should phrase the necessary revisions in the most shocking terms possible: that this was, when you think of it, the original affluent society" (quoted in Lee and DeVore 1968, p. 85). Whether or not one wants to put the matter so strongly, hunting-gathering productive techniques do appear quite efficient, by the measure of output (food) per unit of labor input.

Recent research on systems of agriculture has also called into question the assumption that "primitive" means "inefficient." Consider the world's most primitive agriculture, called "slash-and-burn," "swidden," or, more broadly, "shifting cultivation." The cultivators wait for dry weather and enter a wooded area where they slash, cut, and then burn the vegetation. Using simple digging sticks, they then plant their crop in the burned-out area. Without fertilizer or plow, the land is exhausted of nutrients after one or two seasons, and the group moves along to another wooded area, repeating the process. Anywhere from five to 25 years later, when the original plot has, by normal ecological succession, reverted to forest, it will again be set ablaze. What could be more simpleminded? At first glance, who would suppose that such a system could be efficient?

The traditional assumption of primitive inefficiency has been negated only by anthropologists with an interest in human ecology and energy flows, who have carefully measured work and energy inputs against agricultural returns. One of the most important results of these careful studies has been to show that simple classifications of agricultural systems, usually along some single dimension, such as technology, are typically misleading, since the combinations of crops, technology, cultivation practices, soil types, climatic variations, and terrain are almost endless; each of these variables is important, as are their interactions.

Despite the hazards of generalization, it does now seem clear that under many, but not all, conditions, "primitive" slash-and-burn systems of shifting agriculture provide adequate and reliable diet with only a modest input of work and considerable time left over for leisure. Those practicing technically more advanced methods frequently work more hours for the same return (Boserup 1965; Allan 1965; Conklin 1961; Rapoport 1971; Spooner 1972; Polgar 1975).

Why, then, have analysts made, for so long, an equation between efficiency and technical complexity? The answer lies in the ambiguity of the concept of "efficiency." Students of comparative social structure have long recognized the strong correlation between technical complexity and the size and density of population sustainable within a society. Since advanced division of labor, urbanization, specialization, and the other benchmarks of "civilization" are highly correlated with size and density, it was natural to assume that productive methods which could support so many more
people per square mile were more efficient in every sense. But this assumption obscures a crucial distinction between efficiency of production per acre of land cultivated and efficiency per unit of human labor time. In economic terms, the different factors of production must be disaggregated, so that the possibility of increasing efficiency in the use of one (land) concurrent with decreasing efficiency in the use of another (labor) can be considered.

Hunting-gathering and slash-and-burn agriculture are labor-efficient techniques but yield less food from a unit of land than more "advanced" methods, so that population density must be lower. The peak population density supportable by hunting and gathering generally stands in the neighborhood of one person per square mile (Lee and DeVore 1968, p. 11; Lee 1972b). In shifting agriculture, given that most land must be fallow in any given year, the allowable density is also low but almost always higher than for hunters—ranging from four to a hundred times higher (Allan 1965; Boserup 1965). The rank ordering of societal types presented by Lenski and others, then, stands up well as one of land efficiency but not of labor efficiency. Until the point in the typology where industrialization enters the productive process, the two are negatively correlated. Thus, a surplus beyond subsistence in "primitive" societies appears not to be prevented by a shortage of available labor time. There is also some evidence that even available land is not used as fully as possible within the existing technology. Lee's study of Bushman hunter-gatherers, for example, showed that their population density was stabilized "well below the level that could be supported by available resources" (Lee 1972a, pp. 349–50). Moreover, under the most favorable conditions, the output of slash-and-burn systems can be quite high, in some cases able to support population densities comparable with those of industrial societies (Rapoport 1971; Bronson 1972, pp. 210–11).

Attempts to measure and compare efficiency of food production, then, must take account of the fact that there is more than one factor of production—there are both land and labor. Putting the matter in this form makes it clear that we have simply rediscovered a commonplace of classical economics. In more complex technologies we would have to add capital as a third factor.

This suggests that the microeconomic theory of production may help us understand the questions of efficiency. For a particular product, economists mean by "efficiency" how close a producer comes to getting the maximum output from a given set of resources, or, equivalently, whether for a given output, that combination of factors of production has been used which incurs the lowest costs (see, e.g., Ferguson 1972, pp. 133–245; or Scitovsky 1971, pp. 155–65). In nonmonetized economies this puts us in trouble immediately since land and labor have no "prices" and are thus incommensurable. If land is not privately owned, and hence not alienable, it is even harder to imagine how to measure its cost.
Some analysis is possible if we note that "cost," in modern economics, refers to the valuation of that which must be forgone in order to have something (see also Homans 1974, p. 31). It follows that costs are not fixed by the technical situation but by a group's preferences among various alternative uses of its resources and time. Thus, when we cannot assign money costs to land or labor we can still ask what must be done in order to bring these resources into the productive process. In the case of land, the cost depends on the circumstances and location of the parcel in question. In any type of society some labor cost is incurred in bringing new land into use. For hunters and gatherers this may mean only that the group has to travel farther in the course of its daily round (Cohen 1975, pp. 86–87); but this involves forgoing whatever other activity might have been undertaken with that time. In agricultural systems we need to consider land preparation costs. If land must be wrested from neighboring groups, other costs are incurred, and Harner has even suggested indexing the scarcity of land by the "'price' in human life that populations are willing to pay to seize and defend land" (1975, p. 135). An argument of Sahlin's can be recast as the assertion that the basic social structure of some groups may develop in such a way as to reduce the costs of acquiring new land (see his "The Segmentary Lineage: An Organization of Predatory Expansion" [1961]). One need not agree to either proposal to recognize that competition for land introduces costs of complex types.

This analysis of costs might lead us to expect that hunters and gatherers would prefer technical methods which yield more food per man-hour to those which extract more per acre of land, at the expense of additional labor, since the former yields more leisure, the latter only additional food and land in use. Much of the recent discussion of the transition to agriculture has consisted exactly of asking why labor-efficient methods would be sacrificed for land-efficient ones, given the likely preference for leisure.

One natural though still controversial answer is that population pressure forced this change (Boserup 1965; Spooner 1972; Polgar 1975). With additional mouths to feed and difficulty in adding new land, the cost of a longer workweek declines because leisure, under such circumstances, becomes less valued. Another possibility is that some groups conceived a need for products not locally produced and thus had to produce a "surplus" for trade; in this case leisure becomes less valued because it is associated with a shortage of the newly desired items. Bronson suggests that labor efficiency might have been sacrificed, on occasion, because of locational constraints; the presence of a rich fishing lagoon might, for instance, induce a hunting-gathering group to remain in one spot rather than move in the usual way, forcing more intensive land use and a shift toward agricultural techniques (1975, p. 65). Concentrations of political power could have similar effects:
a group might need to produce a surplus to pay as tribute. The costs of not doing so would mount as the relevant power became more effective.

In all these cases, choices can be explained only by understanding the specific valuations placed by members of a particular group on labor and the production of various commodities, as compared with leisure or other uses of time. This is not fundamentally different from what the modern economist does in computing efficiency: he reads off the cost of the various factors of production from their market prices. These prices were set (in competitive markets at least) by the supply and demand for the factors, which depend also on a complex set of highly subjective value choices made by large numbers of people. If the market mechanism works, the economist need not study those choices directly because they will be summed up in factor prices; the unwary may thus view an efficiency calculation as objective, with the illusion that no subjective preferences have intervened. The student of simpler societies has the more difficult problem, in the absence of price information, of following shifting valuations of different factors and commodities and the corresponding day-to-day choices.5

Now let us suppose what is unlikely: that the arduous task of tracking these valuations in a large number of societies can be accomplished. Can we then make efficiency comparisons? Consider one product, produced in two countries, with the same technology (i.e., with identical production functions). But suppose the costs of productive factors differ—the cost of labor is higher in one country because workers will not come to work if there is a soccer game on television, thus shifting the supply curve of labor to the left. If producers in both countries select that combination of labor, land, and capital which minimizes their costs for the desired level of production, then both have achieved maximum efficiency in this limited sense. Is there any sense in which the country with lower labor costs has, more broadly speaking, a more efficient economic system?

The answer to this question depends on whether the country with lower labor costs is getting more from this saving than the other gets from its love of soccer. More generally, any economic system produces a variety of

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5 Even when price information is available, however, efficiency calculations which might seem straightforward can be highly controversial. Fogel and Engerman, in *Time on the Cross* (1974), argued that slave production of cotton in the South was more efficient than free production of other crops, such as wheat, in the North. Their assertion raised a storm of protest from economists and historians. One objection was that the apparent advantage was an artifact of a long boom in the demand for cotton, which was ending around the time of the Civil War. This is relevant because, in order to compare the output of different crops, Fogel and Engerman used the money value of output rather than the physical amount (see Wright 1976). Another objection is that not all inputs of land are equal in quality, and to assume that land prices capture these differences implicitly argues not only a perfectly competitive national land market, but also one whose prices are at equilibrium values (David and Temin 1976). These two articles make many other objections (for a response to these see Fogel and Engerman [1977]).
goods and services, and overall efficiency judgments must take all of them into account. So the question is how to decide which of two societies gets more out of its total package of goods and services.

It will be helpful to start with a simpler problem—that of deciding which of two individual consumers gets more from the goods and services he consumes. Economists refer to a consumer's rank ordering of preferences among all possible combinations of commodities as his "utility function" (e.g., Henderson and Quandt 1971, chap. 2). Nineteenth-century economists believed that such preferences could be represented with cardinal, not merely ordinal, measurement; this position is now widely abandoned. That is, while we may know that the first consumer prefers a vacation in Bermuda to one in Philadelphia, we do not know how much more "utility" he gets from one than from the other—only that it is preferred. Because we cannot assign numbers to these preferences, we also cannot say whether this consumer gets more "utility" from his Bermuda vacation than does the other, or even whether he gets more from Bermuda than the other does from Philadelphia. Though we may think we know in extreme cases, no consistent procedure appears possible with which systematic comparisons can be made across a range of alternatives for even two individuals (see, e.g., Dorfman 1964, pp. 72–75).

Because these "interpersonal comparisons of utility" cannot be made, it follows that if our two consumers are in situations in which neither finds his utility at a maximum, given the possible outcomes, we are unable to say which is "closer" to that maximization. Similar difficulties arise in comparing two possible allocations of goods within the same system. This is why the study of "welfare economics" proceeds with the restrictive criterion of "Pareto efficiency" or "optimality." "An allocation is Pareto-optimal if production and distribution cannot be reorganized to increase the utility of one or more individuals without decreasing the utility of others. . . . Since individuals' utility levels cannot be compared, changes which improve the positions of some individuals but cause a deterioration in those of others cannot be evaluated in terms of efficiency: the net effects of the moves may or may not be beneficial" (Henderson and Quandt 1971, pp. 255–56).

If two states of the same system cannot be compared as to efficiency, neither can different systems, even with the exact same distribution of utility functions in each population, be compared in this way. Suppose we give the comparison every chance by a radical simplification: let each member of a society have exactly the same utility function (an updated version of Durkheim's "mechanical solidarity"). We can then think of such a function as embodying the "society's" rank ordering in importance of current problems and of the desirability of possible solutions. In more economic terms, this assumes uniform perceptions within the society of the costs of productive factors and of the benefits of the output. Even this simplification only
returns us to the two-person comparison already judged impermissible, since
we would still have to judge how "close" each society had come to maxi-
mizing its particular utility function.

In the context of hunters and gatherers, for example, suppose one group
sacrifices labor efficiency so as to increase total production because it has
developed an insatiable craving for cinnamon, only available in trade for
a surplus of its own food products. Is such a group less efficient than one
which achieves a higher output per man-hour, hence more leisure but no
cinnamon? To answer this question, we must assess how the benefit one
group gets from cinnamon compares with that which the other gets from
leisure. But this requires exactly the fruitless interpersonal comparison of
utilities described above. I conclude that if by "efficiency" one means how
well a society solves its current problems, no consistent procedure is avail-
able for comparing societies on this dimension. 6

MEASURES OF ADVANCEMENT: ADAPTIVE CAPACITY

My argument to this point has concentrated on a system's ability to meet
current priorities and problems. But a substantial part of the evolutionary
and developmental literature stresses instead the advent of flexibility in
dealing with possible future problems. This criterion of advancement ap-
ppears in a number of disciplines. In anthropology, the emphasis relates
closely to the metaphor of species adaptation, as taken over from the study
of biological evolution. Sahlins and Service, for example, divide the study
of "cultural evolution" into that of "specific" and "general" evolution, of
which only the latter involves advancement. Specific evolution is the study
of adaptation to particular circumstances; general evolution the process of
increasing adaptability. An "advanced" culture is "freer" of its environ-
ment than one which displays lower "all-round adaptability" (Sahlins and
Service, 1960, pp. 12–44). In this conception, efficiency in solving current
problems not only is not the proper measure of advancement but may even
indicate a kind of backwardness, because being too well adapted to some
particular environmental niche may make it all the more difficult to face
a change in circumstances.

In sociology, Parsons stresses the concept of "adaptive capacity," which
"includes an active concern with mastery, or the ability to change the en-
vironment to meet the needs of the system, as well as an ability to survive

6 Problems caused by implicit interpersonal comparison of utility may be more per-
vasive in the social sciences than we recognize. Lukes, for example, makes a persuasive
case that power ought to be defined as follows: "A exercises power over B when A
affects B in a manner contrary to B's interests" (1974, p. 34). But such a definition
then requires us, if we are to compare the power of two individuals, to compare the
effects of each on the interests of those over whom each has power. This appears to
me to require the same sort of procedure I have argued against in the present paper.

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in the face of its unalterable features. Hence the capacity to cope with broad ranges of environmental factors, through adjustment or active control, or both, is crucial. Finally, a very critical point is the capacity to cope with . . . uncertainty . . . and unpredictable variations” (1964, p. 340).

In political science, Karl Deutsch refers to the “‘learning capacity’ of a system,” going on to say that “it has become quite possible to estimate in advance the problem-solving capacities of an electronic calculator. . . . It should be possible to estimate, although far more roughly, the problemsolving capacity of a government or a society, as well as to estimate its ability for innovation . . . that is, its capacity to put a new solution actually into operation” (1966, pp. 164–65). In a similar vein, Almond comments, in an essay on “political development,” that “the problem of measuring performance is relatively simple for the economist. . . . Surely this capacity to measure and evaluate performance is one of the principal goals of political theory.” He goes on to assert that we “may not be far from a capacity to compute ‘adaptiveness’ or ‘versatility’ scores for political systems.” An adaptability score “would enable us to compare the performance of political systems according to their versatility in responding to pressures of different kinds without significantly altering their institutions and processes. . . . We are in the beginning stages of a discipline of ‘polimetrics,’” which will make such measurement possible (1970, pp. 292, 299, 301).

If adaptive capacity is the criterion of advancement, is the argument of the previous section still appropriate? It could be argued that a society’s rank ordering in importance of current problems and of the desirability of possible solutions might place a higher value on outcomes which not only solve current problems but increase future flexibility. While others may be skeptical, economists have no doubt that individuals are capable of carrying out, implicitly, extraordinarily complex discounting procedures in allocating their time, energy, and money during a career (see, e.g., the voluminous output on the theory of human capital, esp. Becker [1964]; Mincer [1974]). But even if societies are sufficiently farsighted, the difficulty of making intersocietal comparisons of utility remains unchanged.

Those in the functionalist tradition may object, however, that this difficulty arises only because I have conceived societal “problems” to be specified subjectively, by each society’s preferences, rather than given objectively by the environment. I do see the matter this way; in Eisenstadt’s words, “The goals and needs of social systems . . . do not announce themselves in some easily understood form, like so many unpaid bills” (1971, p. 53). I want to go on here to argue, however, that even if problems were conceded to be objectively given, the concept of adaptive capacity would still be extremely difficult to give empirical meaning.

I begin this second line of argument by reviewing some proposals to
measure adaptive capacity. Marshall Sahlin, in the tradition of Leslie White (1959), asserts that the best operational definition is the extent of energy transformation "involved in the creation and perpetuation of a cultural organization" (Sahlin and Service 1960, p. 35). Direct measurement of such transformation seems impractical to Sahlin, but he asserts that "there are good structural criteria. As in life, thermodynamic achievement has its organizational counterpart, higher levels of integration. Cultures that transform more energy have more parts and subsystems, more specialization of parts, and more effective means of integration of the whole. Organizational symptoms of general progress include the proliferation of material elements, geometric increase in the division of labor, multiplication of social groups and subgroups and the emergence of special means of integration: political, such as chieftainship and the state, and philosophical, such as universal ethical religions and science" (Sahlin and Service 1960, pp. 35–36).

The keynote here is the successful integration of complexly differentiated and specialized parts of a social and/or cultural system. Among evolutionary sociologists a similar emphasis can be found. Parsons argues that increases in adaptive capacity occur when and if four developments take place: (1) differentiation occurs; (2) "adaptive upgrading" (i.e., an increase in efficiency) results; (3) the problems of integration posed by the differentiation are overcome; and (4) "value generalization" occurs, legitimating the changes and thereby (in Parsons's view) insuring their stability (1966, pp. 21–24).

This paradigm makes differentiation a necessary but not sufficient condition for the development of adaptive capacity and hence for evolution of societies to "higher" levels. Some sociologists who stress the adaptive potential of differentiation have discussed the difficulties posed by differentiation and the possibility that they are not overcome. Eisenstadt has charted these carefully for political systems (1964a, 1964b, 1968); Smelser has implicated differentiation and resulting attempts at integration as major causes of social disturbances and movements (1963, pp. 112–15). In this respect, progress has been made over Durkheim's unwillingness to see conflict resulting from the division of labor as anything but a malady of temporary importance, resulting from the fact that "the interests in conflict have not yet had the time to be equilibrated ([1893] 1932, p. 370)."

But still more needs to be said about the connection between differentiation and adaptive capacity. Consider the shift from subsistence to commercial agriculture—seen in some evolutionary analyses as a type-case of advancement (e.g., Parsons 1971, chap. 4). A simple illustration of how such differentiation may reduce adaptive capacity emerges from an examination of regional patterns of famine in preindustrial Europe. Historical demographic studies suggest that actual starvation rarely occurred in re-

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gions of subsistence farming, since the variety of crops produced insured that a failure of some, from adverse weather, would nevertheless leave some others intact. In the more “advanced” areas of commercial agriculture, however, largely confined to a single crop, agricultural disaster was total when it occurred (Laslett 1971, p. 113).

This case could be viewed as a simple failure of integration: the famine-stricken regions were presumably poorly integrated into national systems of food distribution, and insurance schemes to compensate victims of such emergencies were not yet at hand. But this instance also has broad ecological implications, as it illustrates the tendency of differentiation to reduce ecological predictability by making it dependent on the success of integrative processes.

This tendency is central, since for measures of adaptive capacity to have meaning, a substantial measure of predictability is required. The intent of distinguishing adaptation from adaptive capacity is to avoid counting as “advanced” a society so perfectly adapted to its specific environment that minor changes would be disastrous for it. Thus, Sahlins comments that cultures “higher in general evolutionary standing [need not be] necessarily more perfectly adapted to their environments than lower” (Sahlins and Service 1960, p. 26), and Service advances the “law of evolutionary potential”: “The more specialized and adapted a form in a given evolutionary stage, the smaller its potential for passing on to the next stage”; or, put differently, “Specific evolutionary progress is inversely related to general evolutionary potential” (Sahlins and Service 1960, p. 97). His argument continues in Hegelian fashion: most societies stagnate by virtue of closer and closer adaptation to their environment, so that progress must continue elsewhere, in societies which have the “privilege of backwardness” and the negative role model of the stagnating societies. He cites as an example Veblen’s analysis of how Imperial Germany surpassed Britain in industrial capacity at the turn of the 20th century.

But this example should give us pause. Did Imperial Germany have high adaptive capacity? A contemporary observer might have thought so; subsequent events would have rendered this judgment dubious. The problem is that accurate measurement of adaptive capacity requires us to know with some confidence what a society’s likely future environmental exigencies are. No society is well prepared for all possible problems, and one which is best prepared for those which are least likely can hardly be scored high on adaptive capacity. Thus if a society spends billions preparing for a war which cannot occur, or undergoes extensive differentiation, thereby becoming well-suited to face unlikely environmental fluctuations, it pays the costs of these transformations but will reap few or no benefits.

It follows that societies which have previously been highly successful in handling their problems do not necessarily have high adaptive capacity
now since conditions may change, and the features which enabled them to cope so well may no longer be apt. But even more can be said: a society which has done well or badly in the past need not have done so because its adaptive capacity was respectively high or low. If we index adaptive capacity by success, then the argument is merely circular, and the concept has no independent definition. If it is to have meaning it must be possible for societies with low adaptive capacity to have been successful because the exigencies which were most likely did not, for some reason, occur, and conversely, for societies with high adaptive capacity to fail upon being confronted with unlikely difficulties for which they were unprepared.

Consider hunting and gathering societies, now nearly extinct as a societal type. Anthropologists increasingly agree that such societies were well adapted to their environments, in which they were the dominant form for at least 40,000 years. They were displaced by more highly organized agricultural and industrial societies which presented them with conditions beyond their ability to manage. Did this show low adaptive capacity? To so judge requires the assumption that the rise of agriculture and industry was more or less inevitable. If, on the other hand, this rise were seen as the result of improbable events, it would follow that the hunters and gatherers actually were highly "advanced." If the most likely outcome for them was a stable environment, their excellent adaptation would have been indistinguishable from adaptive capacity; the distinction is sensible only when substantial environmental changes are likely.

PREDICTABILITY IN SIMPLE ECOLOGICAL MODELS

Because a high level of environmental predictability is important for judgments of adaptive capacity, it is useful to survey briefly the levels of predictability available in simple ecological systems. Theoretical (biological) ecology is much less well developed than empirical work, but some recent developments are of interest here. Theoretical models generally attempt to predict changes over time in the population size of the various species located in a particular ecosystem. While one might consider other ecological variables, this "is the approach pioneered by Volterra . . . and followed by most ecologists since" (Maynard Smith 1974, p. 3).

Although it would be foolish to attempt any comprehensive summary of a complex body of work, one general conclusion seems safe to extract: even though an ecosystem may be very simple, with dynamics governed by uncomplicated relationships, it is often the case that very little predictability of population sizes over time is practical. The reasons have nothing to do with problems of measurement but are instead matters of principle. In two important articles, May (1976) and May and Oster (1976) describe models of one-species systems. Their models are extremely simple, but non-
linear. May notes that for biological populations "there is a tendency for
the variable X [population size] to increase from one generation to the
next when it is small, and for it to decrease when it is large" (1976, p.
460). This commonplace ecological observation demands a nonlinear model.
Such a model can be as simple as the first-order difference equation,
\[ X(t+1) = aX(t) \left[ 1 - X(t) \right] \]
de picting the situation in which popula-
tion size would increase in each generation by a factor of \( a \) (the birth rate,
for example), but this rate of increase is reduced in each generation by
the factor \( 1 - X(t) \). Thus the effective rate of increase, \( a[1 - X(t)] \),
approaches zero as the population size comes closer to some limiting value,
here represented by 1, indicating ecological constraints, and increases as
population size moves away from that value.

May comments that the "elegant body of mathematical theory pertaining
to linear systems . . . and its successful application to many fundamentally
linear problems in the physical sciences, tends to dominate even moderately
advanced University courses in mathematics and theoretical physics. The
mathematical intuition so developed ill equips the students to confront the
bizarre behavior exhibited by the simplest of discrete nonlinear systems,
such as [the equation above]. Yet such nonlinear systems are surely the
rule, not the exception, outside the physical sciences" (1976, p. 467).

The "bizarre behavior" is described by a developing branch of mathe-
matics known as "bifurcation theory." The results can be summarized as
follows: Suppose population size over time for some species is governed by
a process represented in the above equation, and the exact local conditions
are given by the particular value of \( a \). We can then observe what predicted
ultimate population sizes are associated with different local conditions by
systematically varying \( a \) and computing, for each \( a \), the equilibrium value,
if any, of \( X \). Over some range of values of \( a \), the population does reach a
stable equilibrium size as \( t \) increases. But if \( a \) lies just beyond this range,
the stable solution "bifurcates" into two solutions; population size osci-
llates from one to the other in each successive generation. If we now
increase \( a \), another point is reached beyond which the two solutions bifurcate
into four, each describing the population size reached every fourth genera-
tion (a "stable cycle of period four"). As we continue to increase \( a \), suc-
cessive bifurcations occur, so that after \( n \) bifurcations we have \( 2^n \) solutions.
But as \( n \) increases, smaller and smaller change in \( a \) is required before the
next bifurcation occurs. Beyond a "critical value" of \( a \), there are no stable
cycles whatever, and the behavior of population size over time becomes,
for all intents and purposes, "chaotic" and indistinguishable from "random
noise".\(^7\)

\(^7\) To avoid confusion I should stress that in this paragraph I am comparing the dif-
ferent time trajectories of \( X \) associated with various values of the parameter \( a \), rather
than describing the behavior of \( X \) as \( a \) varies over time. The reader may verify the
This phenomenon, May observes, has "disturbing practical implications. It means, for example, that apparently erratic fluctuations in the census data for an animal population need not necessarily betoken either the vagaries of an unpredictable environment or sampling errors: they may simply derive from a rigidly deterministic population growth relationship . . ." (1976, p. 466). This possibility is important because it shows that an understanding of complex systems may be elusive for reasons entirely unrelated to the usual sociological complaints that "there are so many factors to take into account," or that measurement is inevitably inexact. Neither difficulty is present in this argument.

"Alternatively," May continues, "it may be observed that [beyond the critical point of the parameter] arbitrarily close initial conditions can lead to trajectories which, after a sufficiently long time, diverge widely. This means that, even if we have a simple model in which all the parameters are determined exactly, long term prediction is nevertheless impossible" (1976, p. 466). In meteorology, this has been called the "butterfly effect": "even if the atmosphere could be described by a deterministic model in which all parameters were known, the fluttering of a butterfly's wings could alter the initial conditions and thus . . . alter the long term prediction" (May 1976, p. 466). If such difficulties arise in even the very simplest ecological models, we might expect them more readily in complex ones. May and Oster report that this appears to be the case, though complex models are less well developed; in fact, it appears that the extent of nonlinearity required for chaotic outcomes declines with the complexity of the system, so that one may find "chaotic dynamical behavior . . . for a high-dimensional system with almost imperceptible nonlinearity" (1976, p. 590).

While the conclusions reached here are most compelling for the biological context described, the models and procedures are so general that I believe they foreshadow important limits on predictability of social and economic systems as well. With a different set of models, for example, I have recently argued that in situations of collective behavior, where preferences are distributed in a nonlinear way (as one would, in general, expect), prediction of outcomes can be extremely difficult if there are even minor fluctuations in the preference distribution. As in the models reported here, this is the

statements here with a calculator or a simple interactive computer routine, by computing successive values of \( X(t) \) for the equation in the text, for various values of \( a \). For \( a \) between 1 and 3, \( X \) quickly settles down to the stable equilibrium \( 1 - (1/a) \). Between 3 and 3.57, the values oscillate among various solutions, just two for much of this range, then four, and eight, etc. Beyond 3.57 the pattern of the values becomes "chaotic," resembling random noise. (Note that in these simulations, \( X \) must have an initial value less than 1.0, otherwise the population becomes extinct.) It is important to say that the work of May (1976) and of May and Oster (1976) goes far beyond this particular equation, embracing a very general class of models, many of which have a secure history in the empirical literature.
case even though the dynamics of the model are extremely simple (see Granovetter 1978, pp. 1428–33).

SOME PREDICTABILITY PROBLEMS IN THEORIES OF SOCIAL EVOLUTION

Judgments of adaptive capacity impose severe predictability requirements. At a minimum one would have to be able to say what ecological and energetic problems a society will ultimately face as a result of its present system of extracting energy from its environment. While such knowledge would be a necessary condition for the desired measurement, it would probably not be a sufficient one, as one would also need some confidence about what political, economic, and military courses of action would be followed by other societies. For my purpose, it is enough to argue here that the ecological judgments alone are highly impracticable.

Hunting and gathering is the simplest type of extractive system ever organized by human society. Earlier denounced as a simpleminded form of "parasitism" by, for example, Gordon Childe (1942, p. 48), it has recently attracted a good deal of nostalgic, almost romantic attention as a way of life in which man is in fundamental harmony with nature. Yet such systems gave rise to the agricultural societies which displaced them. To judge whether this displacement reflected low adaptive capacity requires us to say whether the change was highly probable or not. Childe and his followers had assumed that agriculture was adopted because hunting and gathering was hopelessly inefficient, and its practitioners were therefore delighted that a new and better way had been invented. Had this been true, the adoption would have seemed highly likely, since so much of the food gathered—especially in the Middle East—consisted of seed grains that it would be unlikely for the individuals involved not to realize eventually that new plants would grow in places where the seeds had been dropped. But with the failure of the inefficiency assumption, attention has shifted from the actual "invention" of agriculture to the motivation for practicing it; here a prominent argument has been that this occurred where local populations outstripped their carrying capacity, thus requiring a more land-efficient food technology (Boserup 1965; Spooner 1972; Polgar 1975).

Suppose this is correct. Were such population swings inevitable? From the point of view of theoretical biology, hunting and gathering systems are special cases of what are called "predator-prey" models, in which the population size of each species is determined in part by that of other species of which it is either predator or prey (see, e.g., Wilson and Bossert 1971, pp. 127–38). May's models, described in the previous section, can be seen as one-species cases of models with the same aim: to predict the size of the population over time. Hunting and gathering systems are far more
complex because the number of animal species is substantial and plant species enter in as well. May demonstrates that multispecies predator-prey models, with their increased complexity, are even more likely than his one-species cases to generate results which appear “chaotic” and are thus unlikely to permit simple statements or adequate predictability of population stability or size over time (1976, pp. 466–67). It follows that we are unlikely even in principle, to be able to answer the question posed about the adaptive capacity of hunters and gatherers, even if the answer depends on so straightforward a matter as accounting for changes in local population sizes and densities.

More technically advanced modes of production have correspondingly more complex impacts on their environments than simpler ones. Even, for example, if one could devise a useful way to index agricultural productive efficiency, the method used would have to be discounted for future effects of present methods before it would indicate adaptive capacity. The use of methods which permanently degrade the soil structure of an area may be efficient in the short run, for example, but not “advanced” in this broader sense. We are accustomed to suppose that modern methods of farming will produce efficiently on any soil, for an indefinite period. But some soils are so fragile that they can be farmed effectively and without damage only by methods which make minimal alterations in the ecological system—such as slash-and-burn techniques. This is said to be especially true of some African soils (Allan 1965, pp. 72–74). Allan, however, in his exhaustive treatise on African agriculture, stresses that knowledge of African soils is meager and systematic study is only beginning (1965, p. 12).

In 1935, Paul Sears published a book whose dramatic title, Deserts on the March, heralded his argument that much of the world’s desert resulted from early attempts to farm fragile soil structures too intensively, by “advanced” methods (Sears 1935). Geologically, desert is an end point in certain successions of soil structure and could be brought on by overintensive cultivation. After such a point there is no practical way to restore the soil, except via the most recent technological developments—as in the Negev—and then only at great cost. Whether such a succession actually led to existing deserts remains controversial.

Just as currently efficient methods which reduce future productivity must be pinpointed, our assessment of methods which are currently inefficient but enhance future results must also be adjusted accordingly. The necessary discounting procedures become less and less conceivable as we extend our purview further in time from the particular system considered. Even to apply such procedures over a 50- or 100-year period generally requires a level of ecosystem theory not yet available. And this does not even take into account the necessity of predicting fluctuations in weather, trade patterns, and a variety of other outside influences.
As we approach industrial society, complexity increases in two ways. The intrinsic complexity of systems of production increases over that found in the agricultural world. In addition, the systemic autonomy characteristic of simpler systems diminishes. With increasing social and economic differentiation, it becomes increasingly difficult to view ecosystems as being self-contained. The anthropologist Rapoport, for example, compares simpler to more advanced agricultural systems and comments that problems in the latter are typically dealt with by infusions of energy from outside the system. He points out that a modern farmer may even expend more energy in the gasoline consumed by his farm machinery than is returned by the crop he raises. The same non-biological power sources make it possible to provide the world agricultural community with the large quantities of pesticides, fertilizers, and other kinds of assistance that many man-made immature ecosystems require in order to remain productive. Moreover, the entire infrastructure of commercial agriculture—high-speed transportation and communications, large-scale storage facilities and elaborate economic institutions—depends on these same sources of non-biological energy. . . . [Modern ecosystems] are subject not only to local environmental stress but also to extraneous economic and political vicissitudes. They come to rely more and more on imported materials. . . . National and international concerns replace local considerations. . . . [1971, p. 132]

These comments, written before the "energy crisis," nevertheless give an apt description of the problems arising from dependence of advanced agriculture on petroleum imports from a few regions, and of the resulting unpredictability of ecosystem adaptation.

A different instability argument, made by the geochemist Harrison Brown, rests on the unpredictability of international relations. Brown begins by asserting that, given our present level of technology, energy sources will be available almost indefinitely. But this "given" is crucial. When industrial civilization was established, his argument goes, it was made possible by the existence of easily available fuel sources—petroleum and rich ores near the surface of the ground—which could be obtained without either energy-demanding extractive machinery or refining methods. Current energy is obtained only by virtue of our far more sophisticated equipment, which digs much deeper and refines metals out of much cruder ores than earlier. If, by some catastrophe, Brown argues, this vast technical superstructure were ever destroyed, industrial civilization could never be rebuilt, because without this equipment, energy sources could never again be tapped in sufficient quantity to do so (Brown 1956).

Brown wrote in the 1950s, when the threat of nuclear war seemed far more imminent than now. Such a war is the kind of catastrophe he had in mind. Under present conditions we might be more likely to imagine sabotage, on a large scale, as the threat. If we imagine that the destruction
Brown speaks of has probability near zero, the argument has no effect on our assessment of the adaptive capacity of industrial civilization. If we think there is some chance of such events, the argument may be a telling one for the proposition that our "advanced" type of society is merely a brief, unstable equilibrium. Lee and DeVore put the case colorfully:

It is still an open question whether man will be able to survive the exceedingly complex and unstable ecological conditions he has created for himself. If he fails in this task, interplanetary archeologists of the future will classify our planet as one in which a very long and stable period of small scale hunting and gathering was followed by an apparently instantaneous efflorescence of technology and society leading rapidly to extinction. "Stratigraphically," the origin of agriculture and thermonuclear destruction will appear as essentially simultaneous. [1968, p. 3]

I argue only that we are in no position to make judgments about the probability of such events with any confidence. It follows that questions of adaptive capacity must remain moot.

EVOLUTION, DEVELOPMENT, AND SYSTEMS THEORY

At the outset, I suggested that the enterprise of systematically ranking societies usually entails a view of them as coherent, well-defined entities—a view consistent with functionalism and systems theory in the social sciences. Bendix has pointed out that this orientation in theories of development and social evolution is not new:

From the vantage-point of Europe in the late eighteenth and early nineteenth centuries, both revolutions and much of the social change that followed appeared as phenomena that were internal to the societies changing. This mode of explanation goes back to influences emanating from Plato and characteristics of Western philosophy down to the present. . . . [The] intellectual tradition of Europe and the specific historical constellation at the end of the eighteenth century encouraged explanations of social change which emphasize the continuity and interconnectedness of changes within society, a tendency which was reinforced when modern nationalism came into its own. As a result a certain lawfulness was attributed to the social structure, while the relative autonomy of government and the impact of external factors upon every society were ignored or minimized. [1967, pp. 324–25]

The present paper makes two arguments that would remain pertinent even if societies were self-contained, coherent systems: (a) intersocietal comparisons of utility cannot be made meaningful and (b) the nature of the systems in question does not lend itself to useful predictions of future problem-solving ability. But to the extent that one accepts criticisms of the systems conception, both arguments gain additional force. Utility comparisons are not even conceivable if the units which attempt to maximize
utilities are ill defined. Predictability problems, already severe in simple ecological systems, become all the more intractable when one adds the element of dependence on forces outside the system. In the previous section, I described some predictability problems for ecological systems, in the order of increasing complexity of solution. I noted that difficulties of prediction are compounded by the fact that increases in social and economic differentiation make it harder to view attempts at problem solving as self-contained. It is ironic that the very element—differentiation—which is often cited as a measure of adaptive capacity, can be seen instead as making the concept impossible to give empirical content by virtue of the reduced predictability which accompanies it.

In recent thinking on development, modernization, and evolution, attacks on systems ideas have multiplied. In an essay provocatively entitled "The Disintegration of the Initial Paradigm of Studies of Modernization," Eisenstadt comments that "the criticisms of the structural-functional and systematic approaches in sociology which have developed from within studies of modernization have concentrated mostly around two major themes. . . . The first such theme was the denial . . . of the closed systemic interrelations between different aspects of a society, of the assumption of the necessary convergence of development of modernization in all institutional spheres of society. . . . These criticisms coalesced with a more general theoretical emphasis which stressed the relative autonomy of different institutional spheres . . ." (1973, pp. 107–8).

Attacks on the assumption of system coherence are complemented by those on that of self-containment. Bendix's 1967 statement, "Tradition and Modernity Reconsidered," was one of the earliest systematic broadsides (Bendix 1967, esp. pp. 326–27). Nisbet's attack on theories of development describes the notion of self-containment as one aspect of the "abuse of a metaphor." He indicts Rostow's conception of developmental stages in these terms:

[Rostow] seeks to endow England with the same kind of self-containment . . . [with] which Marx had endowed, not any historical nation, but capitalism. But this, all too plainly, simply will not hold water. For it is utterly impossible to extract from the myriad events, forces, impacts and historical contacts of modern economic history, of which those in the British Isles are but an aspect, any self-contained national entity with its own dynamics, its principles of development, and its "stages of growth." . . . Making all allowances for the vivid meaning that England, or any other nation, has to its citizens and for the continuing political and cultural identity that it may be seen to have had for some centuries, it is not possible to deal with any of the major changes of England save in terms of incessant historical interaction of the English—traders, merchants, artisans, scholars, artists, as well as statesmen—with peoples and ideas and forces of one kind or other which cannot conceivably be localized in England. [1969, pp. 355–56]
This general theme has been taken up increasingly by revisionist writers on development, particularly those who stress the dependence of "less developed countries" on more powerful economic entities (see, e.g., Cockcroft, Gunder-Frank, and Johnson 1972). Its most influential recent expression has been Wallerstein's treatment of "world-system" theory (1976). Historical research has also moved in a similar direction. William McNeill's important *Rise of the West* (1967) rests on an explicit denial of the possibility of explaining large-scale trends without giving a central role to the relationship among social systems at any point in time, and especially to the migrations of population from one area to another, frequently related to difficult-to-predict ecological changes, territorial skirmishes, and technological developments.

In a symposium on new approaches to the study of the development of national states in Western Europe, Tilly points out that this development, far from being the result of internal forces alone, was inextricably tied up with war among political units, and that the act of building a military, if successful, "produced arrangements which could deliver resources to the government for other purposes. (Thus almost all the major European taxes began as 'extraordinary levies' earmarked for particular wars, and became routine sources of governmental revenue.) . . . War made the state, and the state made war" (1975, p. 42). Moreover, "commercialization facilitated the flow of revenues to the governments. Regions (or periods) of minimal trade blocked governmental efforts to extract resources and carry on expensive tasks" (p. 72).

Nor should we assume beforehand that the units of analysis are straightforward. "The Europe of 1500 included some five hundred more or less independent political units, the Europe of 1900 about twenty-five. Comparing the histories of France, Germany, Spain, Belgium and England . . . for illumination on the processes of state-making weighs the whole inquiry toward a certain kind of outcome which was, in fact, quite rare" (Tilly 1975, p. 15). Thus, much of our research agenda must involve the way in which this early set of patchwork boundaries was ultimately reduced to the simpler map of the present day. But this task requires us to abandon the idea that boundaries, at any time, delimit well-defined, self-contained units.

**DISCUSSION**

Theories of evolution, development, and modernization have been attacked many times on a variety of grounds. (For a good summary see Eisenstadt [1973, chaps. 1 and 5].) They have been accused of being ethnocentric, value-biased, teleological, imperialistic, and unilinear and of making unreasonably sharp distinctions between tradition and modernity. The present
argument is different from any of these. It attacks a usually implicit aspect of the phenomenology underlying such theories: the view that systematic comparisons and rank orderings of social units are possible on the basis of relative success in problem solving.

The attack has proceeded on two grounds. First, ranking societies according to current efficiency ultimately reduces to a task equivalent to the fruitless interpersonal comparison of utilities no longer attempted by economists. Second, ranking societies according to their flexibility or adaptability requires a level of prediction of future system problems which is unlikely, in principle, to be achieved.

Neither argument depends on asserting that the systems in question are too complex for proper analysis, that equilibrium states (or problem solutions) are difficult to define, or that the units of analysis are internally incoherent or inseparable from outside forces. Instead, the arguments are based on a more direct theoretical dissection of the requirements of the rank ordering attempted. The other assertions yield a more equivocal attack, since one must argue in every case the extent to which they are applicable; the theoretical dissection, in contrast, implies not that the attacked procedures are inappropriate in some cases but rather that they simply cannot be made sense of even in principle.

What, then, is left in the systematic study of social change? Almost everything, I would argue. The emphasis on rank ordering of societies has directed an imbalance of attention to taxonomy at the expense of dynamics. This imbalance is most explicit in Parsons’s work: "... some sociologists insist that only 'dynamic' analysis has any scientific standing. ... But I am saying that the use of available ... evidence to order structural types and relate them sequentially is a first order of business which cannot be bypassed" (1966, p. 111). While this position is perhaps at one extreme, it is not essentially different from that of many other evolutionists, whose strategy is to devise a taxonomy of societies and organize the investigation of dynamics in the framework of that taxonomy: that is, the study of dynamics is seen to consist of asking how societies "progress" from one "type" to another. This narrowing of the field of attention makes it more difficult to address questions which are not clearly related to movement within the chosen taxonomy. It also leads to neglect of any substantial social change which does not move societies into "new" evolutionary stages. Worse, whatever criterion has gone into the taxonomy of advancement generally forecloses the investigation of dynamics by assuming from the outset that the forces which distinguish between less and more "advanced" are already understood. Thus a taxonomy based on technological level makes it hard to avoid technological deterministic interpretations of change; one based on levels of differentiation compels the motion that differentiation is the
mainspring of change; one based on strength of political integration inclines one to search for political forces as central.

If the language and substance of ideas of "advancement" can be abandoned, the study of social change can proceed to more detailed understanding of the dynamics of change. In a more limited way, there is no reason why evolutionist interpretations cannot still be pursued. Writers who believe that adaptation to the ecological and social environment constitutes the main cause of change can follow this theme without the baggage of advancement to weigh them down. This corresponds in fact more closely to the situation in evolutionary biology, where the comparison of species rarely involves any notion of relative "advancement," but instead a study of the mechanisms and parameters of evolutionary change. The fact that one may not be able to predict the future success of a current adaptation of some species to its environment has never constituted an obstacle to progress in biological thinking, since the need for such prediction has not been felt.

Consider, for example, some comments of the biologist George Gaylord Simpson on explaining the extinction of species:

Narrowness of adaptation means that the organisms range over a smaller variety of environmental conditions. The adaptation is therefore more likely to be affected by environmental change than in the case of organisms that tolerate or thrive in a wider environmental range. Under relatively constant conditions or with a general deterioration of the whole range of both, the more narrowly adapted animals have the advantage, but with more specific change the more widely adapted animals are less likely to become extinct. . . . In this, as in so much of evolution, there is a balance, and neither broad nor narrow adaptation has become the general rule. . . . Extinction in which specialization . . . is involved is usually said to be due to overspecialization, but this way of putting things confuses the issue. . . . Overspecialization is just specialization that has become disadvantageous because the environment has changed. Precisely the same characters may be highly adaptive at one time or in one group and inadaptive at another time or in another group. . . . [The] sad fact is that explicit assignment of immediate causes to particular instances of extinction is almost always unconvincing. [1953, pp. 298–303; emphasis supplied]

Thus evolutionary biologists are reluctant to make sweeping statements about what features of species make them more or less likely to survive, since that outcome is not the result of species characteristics but of a complex and generally unpredictable interplay between these and environmental changes. This does not prevent evolutionary biology from investigating and proposing "lawlike" regularities concerning species adaptation; but these involve statements about what kinds of conditions a species would be (or was) best suited to face, and thus, contingent predictions of the form: if the following changes occurred, the outcome for this species would be such-and-such.

This sort of prediction is similar in form to those of most scientific enter-
prises and seems to me a useful model for the study of social change. Rather than assert that society A is more efficient or adaptable than B, sociologists can more fruitfully say that under specified conditions, the outcome will be of one kind for A and another for B. The difficulties of constructing systematic sets of such statements are sufficiently bracing to provide us an agenda for the next hundred years, without adding tasks which are, in principle, beyond our grasp.

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