

ECLECTICISM AND ITS ALTERNATIVES

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Since the breakdown of the functionalist theoretical hegemony in the 1960s, many sociologists have retreated into defenses of eclectic positions. That eclecticism has become a popular position among sociologists can be seen in numerous ways. For one thing, the vast majority of textbooks, particularly those for the introductory course, strongly proclaim the superiority of an eclectic position, usually arguing for combining functionalism, conflict theory, and symbolic interactionism in order to generate more adequate and complete explanations. Although it might be objected that the content of textbooks is heavily determined by publishers' marketing strategies, these strategies are based purely on what sociological instructors are willing to use for their classes. That so many instructors use eclectic texts for their classes clearly suggests that a sizable proportion of the discipline is comfortable with eclecticism, if not fully in support of it. It is inconceivable that such books could be so widely adopted without at

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least a minimal commitment to eclecticism on the part of most of their adopters.¹

There is considerable evidence however for the popularity of eclecticism outside the textbooks. Many sociologists (and closely related social scientists like anthropologists) have produced scholarly works that advocate eclecticism, sometimes vigorously (e.g., Stinchcombe, 1968; Goody, 1973, 1976; Service, 1969; Dahrendorf, 1959; Klass, 1980; Naroll, 1973; Warshay, 1975; cf. Harris, 1979). For instance, one of the premier sociological theoreticians of the last four decades, Robert Merton (1981:1), has advocated a type of eclecticism under the heading of theoretical pluralism, claiming that pluralism represents "the cognitively appropriate state of sociology at large." The well-known contemporary theorist Jonathan Turner (1986:vii) has declared that "today, theory is more eclectic, and this is all to the good." Especially vigorous scholarly defenses of eclecticism have been made by the prominent theoreticians Arthur Stinchcombe and Ralf Dahrendorf. In attempting to assist sociologists in constructing good social theories, Stinchcombe (1968:4) suggests the following as a general philosophical guideline:

I have a firm conviction that some things are to be explained one way, some another. . . . Some things are to be explained by personality dynamics, some things by their consequences, and some things by ecological causes. Some personality theories are true, some functional theories are true, and some ecological theories are true. . .

Thus the point of view of this book is deliberately eclectic. The reader will find no cases of one "approach" contradicting or conflicting with another "approach." If one approach does not work for explaining a particular phenomenon, the theorist should try another . . .

In a similar vein, Dahrendorf (1959:159), in commenting on what to do with mutually exclusive theories, has said: "in sociology (as opposed to philosophy) a decision which accepts one of these theories and rejects the other is neither necessary nor desirable." Dahrendorf goes on to suggest that there are some problems that are only interpretable in terms of the one theory, and other problems that are only interpretable in terms of the other theory.

Although numerous sociologists and other social scientists neither advocate nor passively accept eclectic formulations, there seems little doubt that eclecticism is significantly endorsed at various levels of sociological discourse. It is difficult to ascertain just how widespread eclectic arguments are, but they certainly seem to be common enough to demand systematic and careful scrutiny. This paper is an effort at such scrutiny. It argues that eclecticism is a seriously flawed perspective that should be abandoned by social scientists. Eclecticism suffers from several crucial defects. By accepting the basic notions of diverse theoretical perspectives, eclecticism

generally leads to hopelessly self-contradictory arguments that generate far more confusion than insight. In addition, it runs strictly counter to the widely accepted scientific aim of simplicity. Eclectic arguments increase rather than decrease the number of explanatory concepts and principles we must rely on, and they militate against efforts to see the world as fundamentally unified and coherent. Finally, eclecticism is antithetical to what I take to be the basic mechanism of rationality and progress in science, namely, theory testing and evaluation in a comparative context. To test theories against data is also to test them against other theories, and rational theory choice amounts to selecting not a uniquely correct theory but the theory that fares better than its rivals. The adoption of eclecticism makes this crucial scientific undertaking impossible, and thus it thoroughly undermines that which makes science uniquely successful.

To argue against eclecticism, however, is not to argue, as many seem to think, for dogmatic loyalties and commitments to particular points of view, nor is it to deny value in more than one theoretical approach. The other basic purpose of this paper is to argue that there are three basic alternatives to eclecticism, any one of which is preferable to it. First, scientists can commit themselves to a particular approach and conduct their research exclusively within it. To do this is not in itself evidence of dogmatism; neither can we conclude from this that the scientists in question believe their approach to be the only valuable one among many. These issues are simply analytically separate from the question of theoretical loyalty and commitment. I argue that this first approach is the most common one in science, and that a major reason for the success of science is that this approach is so widely employed. A second alternative to eclecticism involves scientists' accepting one theoretical approach and conducting most of their research within it, yet at the same time conducting some of their research within the framework of another approach. The second approach is not accepted by the scientists, but only *pursued* by them. Although this may look simply like a version of eclecticism, it will be argued that it is not. Finally, scientists may engage in theoretical synthesis, which involves producing new theories out of the reworking and recombining of old ones. This too is not eclecticism, and I shall have much to say about the differences between synthesis and eclecticism and the epistemological acceptability of the former.

The arguments to be developed refer mainly to the social sciences, sociology in particular. Yet they are meant to apply fully to all of the sciences. In fact, a major point of this discussion is that natural scientists seldom if ever advocate eclecticism and that they regularly engage in the aforementioned alternatives to it. Given the extraordinary success of natural scientists, they must be doing something right, and therefore it would seem that they have much to teach us in these matters.

ECLECTICISM AND ITS PROBLEMS

Meaning of Eclecticism

Part of the debate over eclecticism is less a substantive debate than a confusion regarding terms, a confusion about just what eclecticism means. Careful definition of this concept is therefore essential. Eclecticism is *the a priori doctrine that multiple theoretical approaches must be used jointly in order to arrive at acceptable explanations*. In advance of any empirical analysis eclectics generally assume that multiple theoretical perspectives will in fact be essential to explain what they wish to explain. The foregoing definition also tells us that different theoretical approaches are "used jointly" to provide explanations. This means that they are used more or less mechanically without any real effort to integrate them or to assess the logical implications of adhering to one view while simultaneously endorsing another. This mechanical juxtaposition of diverse theoretical approaches is taken to be the essence of eclecticism.

Eclecticism is a rather diversified doctrine, and eclectics come in various forms. An especially vigorous form of eclecticism holds that any particular problem must be explained from many different angles and therefore that a diversity of approaches need to be brought to bear on it. This form of eclecticism maintains that each approach provides partial insight into the nature of the problem, and that when all the approaches are used in combination, we get a complete (or more nearly complete) understanding of it. A more modest, and probably more common, form of eclecticism holds that different approaches need to be applied to different problems. For instance, some problems are judged to be amenable to explanation in terms of Approach A, whereas other problems will not be amenable to Approach A but will require Approach B, and so on. Those who advocate eclecticism in this sense often emphasize the "complementarity" of different theories. A famous example of this complementarity argument comes from Ralf Dahrendorf's (1959:163) recommendation about how to join together functionalism and conflict theory:

Now I would claim that, in a sociological context, neither of these models can be conceived as exclusively valid or applicable. They constitute complementary, rather than alternative, aspects of the structure of total societies as well as of every element of this structure. We have to choose between them only for the explanation of specific problems; but in the conceptual arsenal of sociological analysis they exist side by side. Whatever criticism one may have of the advocates of one or the other of these models can therefore be directed only against claims for the exclusive validity of either. Strictly speaking, both models are "valid" or, rather, useful and necessary for sociological analysis. We cannot conceive of society unless we realize the dialectics of change, integration and conflict, function and motive force, consensus and coercion

...

Dahrendorf goes on to tell us that the kinds of problems functionalism is really useful for are those of consensus and stability. This is one "face" of society and it requires understanding in terms of one particular model. By contrast, the other "face" of society is that of conflict, disagreement, and coercion, and to understand this the conflict model is necessary and functionalism is of no real use.

Robert Merton's (1981:iv) advocacy of eclecticism is also of this complementarity type. Basic to his conception of theoretical pluralism is the notion "that though theoretical orientations may differ in their cogency when directed toward the same problems, their very difference of perspective typically leads them to focus on different rather than the same problems. As a result, the theories are often complementary or unconnected rather than contradictory."

In addition to saying what eclecticism is, we should also point out what it is not. It is clearly not simply a doctrine that declares value in more than one theoretical tradition. Presumably the vast majority of scientists, including those who are most rigidly attached to one particular theoretical tradition, believe this. The point is not whether one believes there is value in more than one approach, but rather the concrete consequences that flow from such a belief. Eclecticism only results when those who perceive value in several approaches advocate the unintegrated joining of these approaches as a basic explanatory logic.

Closely related to the foregoing consideration is the strong tendency of some social scientists to confuse eclecticism with a toleration for theoretical diversity or the actual existence of such diversity. Norbert Wiley (1985:206, 207), for instance, in discussing the current theoretical interregnum predicts a "creative eclecticism" as the dominant theoretical focus of sociology in the very near future. He suggests that eclecticism (his word) provides an important atmosphere for creativity and theoretical growth, and that the hegemony of any particular approach tends to stifle such growth. He notes, for instance, that "the great turn-of-the-century classics were written in an atmosphere of theoretical combat and diversity." The confusion Wiley has between eclecticism and diversification is also obvious in his remark that, "at present, the eclecticism still wavers, but I think that the diversified and pluralistic departments are beginning to do the most creative work . . ."

One can readily agree (within limits) with Wiley's point about the positive consequences of diversity (and toleration for it), but that is not what the problem of eclecticism is all about. Eclectics and noneclectics alike can tolerate diversity, but noneclectics favor theoretical combat (to use Wiley's own phrase), whereas eclectics favor an imposed theoretical peace.

A final point must be made in regard to the level at which eclecticism applies. Basically, it seems to apply at the level of what have been called

paradigms (Kuhn, 1970), *research programs* (Lakatos, 1970), *research traditions* (Laudan, 1977), or *research strategies* (Harris, 1979). These are highly abstract, global networks of ontological and epistemological assumptions, theoretical concepts and principles, and interlocking theories. Therefore, an eclectic is one who attempts to use side by side two or more of these global complexes. However, there is a sense in which eclecticism also applies at the level of the smaller theories within these global complexes. Thus, if one attempts to show that both the functionalist and conflict theories of social stratification are mutually complementary and enlightening, then one is being an eclectic at the level of particular theories rather than at the level of theoretical traditions or global complexes. Nonetheless, most of our present discussion concerns global traditions rather than specific theories.

Some Familiar Arguments for Eclecticism

Dogmatism As the Only Alternative

Perhaps the most commonly advanced argument in favor of eclecticism is that the only real alternative to it is dogmatism. The issue is sometimes even posed in Manichean terms of "open-minded eclectics" versus "rigid dogmatists" (cf. Klass, 1980:7-8). The issue of dogmatism, however, is simply a red herring. The concern about dogmatism has no logical bearing on the question of the acceptability of eclecticism or its alternatives, and the only reason it seems to have been drawn into the discussion is the considerable confusion as to what dogmatism means. Properly understood, dogmatism is a form of thinking in which the thinker rigidly clings to certain ideas and refuses to modify them when confronted by contrary information (or refuses even to examine new information). As the social psychologist Milton Rokeach (1960) pointed out a quarter of a century ago, dogmatism refers to the structure or form of thought, not its content. Logically, dogmatism can be associated with any particular viewpoint.

Rokeach's notion of dogmatism suggests that it is a complete misapplication of the concept simply to assume that scientists who proclaim a commitment to a given theoretical tradition are thereby dogmatists. The issue in question is the particular way in which scientists regard these commitments. If they unbendingly refuse to modify their judgments in light of compelling arguments or evidence to the contrary, then they are indeed dogmatists. However, if they are like probably the majority of theoretically committed scientists, then they regard their commitments as provisional and subject to modification given sufficient cause.

The notion that noneclecticism is dogmatism therefore rests on a con-

flation of dogmatism and commitment. Beyond this, however, there is an interesting irony here, which is that eclectics are logically subject to the same charge they hurl at others, namely, they too are dogmatists. Because dogmatism refers to the structure of thinking rather than to its content, there is no reason in principle why one cannot be a dogmatic eclectic. In fact, they probably abound in the social sciences. It is difficult for critics of eclecticism not to delight in the irony that eclecticism is a particular theoretical position that argues against the desirability of scientists' holding particular theoretical positions (cf., Harris, 1979).

Comparative Completeness of Eclectic Theories

Another argument commonly recited in favor of eclecticism is that only under its auspices can we produce explanations with the degree of completeness or thoroughness that scientists desire. Any individual theory constitutes only a partial understanding of reality, and therefore several theories must be used to reach truly complete understanding.

This argument suffers from two main flaws. The first is that it tends to assume in advance that theories only give partial understandings. Although this is often (if not usually) the case, to state it as an a priori judgment rather than as a matter for empirical consideration is inappropriate. To know whether any particular theory only gives a partial understanding, or to know just how large or how small this partial understanding is, is a question that is best settled empirically.

The second and more serious difficulty with the completeness argument is that it suffers from a sense of misplaced priorities. Surely we want to say that completeness of explanation is an important goal. Yet this goal cannot be placed ahead of other important goals that would actually be sacrificed in order to achieve it. Science has many goals or aims, some of the most important of which are consistency and coherence, and both of these aims clearly have a priority over completeness. The aim of consistency is that of producing theories that have no logical contradictions among their elements. Obviously those that do are fatally flawed. The aim of coherence is to produce explanations that demonstrate a fundamental unity among the phenomena being studied in order to show how seemingly disparate phenomena are fundamentally intertwined, so that the explanation given for one is in principle the same as that given for the others. Can the goals of consistency and coherence be achieved under the rubric of eclecticism? It is a major argument of this paper that they decidedly cannot. Therefore, although completeness is an important scientific goal, we will have to work toward a level of completeness that is compatible with the prior aims of consistency and coherence.

Feyerabend and Pluralism

An interesting version of eclecticism has been set forth by the philosopher Paul Feyerabend (1975). In the context of a highly relativistic philosophy of science, Feyerabend makes a plea for what he calls "pluralism" or "theoretical proliferation." "There is no idea, however ancient and absurd that is not capable of improving our knowledge," declares Feyerabend (1975:47).

Feyerabend's eclecticism is an especially extreme version inasmuch as he does not even limit his plea for pluralism to science. Indeed, he suggests that we should consider any views drawn from anywhere, including astrology, myth, voodoo, and religion. Feyerabend's argument for pluralism cannot be understood or assessed apart from his more general philosophy of science, which he refers to as "anarchistic." Simply put, his argument is that all of the methodological rules that scientists have set forth as guidelines to rational scientific work not only have been useless to the real acquisition of knowledge, but also have actually constituted serious obstacles to scientific advance. Feyerabend believes that all of the great scientific discoveries have been possible because the scientists who made them broke the rules and followed their natural inclinations. Because of this, Feyerabend suggests, we should get rid of all methodological rules in science and follow the principle of "anything goes." This principle is obviously what is guiding Feyerabend's advocacy of pluralism. Because we never know where truth may be lurking, and because there are no reliable guides to finding it, the only thing we can do is to marshal all our resources, including entertaining all possible theories, scientific, non-scientific, and antiscientific.

Feyerabend's reading of the history of science is an extraordinary one and the conclusions he draws from it are entirely unwarranted. Although one can point to instances in the history of science where scientists broke methodological rules, and even to instances where the breaking of rules was necessary to scientific advance, it does not follow that the history of science generally (or even significantly) evolves this way. (In fact, Feyerabend cites only two or three examples of this rule-breaking behavior, Galileo being his favorite example.) By and large, historians and philosophers of science have read the history of science in a radically different way. Most of them, particularly the philosophers, have conceived it to be a highly rule-governed and rational activity. Science, in other words, has been anything but "anything goes."

To follow Feyerabend's recommendations would be to make scientific practice extremely cumbersome and inefficient. The classical model of scientific practice is the so-called hypothetico-deductive method. As is well known, this method involves using global theoretical complexes as

a means of deducing working hypotheses, which are then examined empirically. To use this model efficiently, one cannot follow up every conceivable lead. Because the vast majority of these leads will not produce anything of value, to follow this strategy is to waste enormous amounts of time and energy. A better alternative, and the one generally followed in science, is to adopt the theoretical tradition that seems at the time most useful and to follow it as intensively as possible. This, I argue, and not Feyerabend's anarchism, is the key to scientific progress today and has been the key to it in the past. Later in this discussion the reasoning behind this assertion will be made plain.²

Some Arguments Against Eclecticism

Although we have already noted in passing some difficulties with eclecticism, let us now consider the problems of eclecticism in a much more detailed and systematic way.

Eclecticism and the Logical Consistency of Theories

The most important criteria we have for theory choice in science are those of consistency, simplicity, scope, conformity with empirical data, and ability to generate novel predictions. Of these criteria, none is more important or fundamental than that of logical consistency. For our explanations to be regarded as even minimally sound, we understand at the outset that their elements must be logically compatible with each other—that there can be no logical contradictions among the concepts and propositions that are part of our theories.

This criterion of theory acceptability is the most fundamental of all, because the other criteria of the adequacy of a theory obviously depend on it. In other words, theories that are logically inconsistent or contradictory cannot possibly hope to achieve empirical success, comprehensiveness, novelty of prediction, and so on. This is perhaps the most basic reason for objecting to eclecticism, namely, that it generally leads to explanations that do not satisfy the criterion of logical consistency. It is generally the case that some or all of the theories that eclectics are drawing upon have fundamentally opposing metaphysical assumptions and theoretical concepts and principles. Think, for example, of simultaneously using such radically disparate theoretical traditions as functionalism and conflict theory, ethnomethodology and sociobiology, or in anthropology, cultural materialism and French structuralism. The problems of consistency are bad enough when only two such perspectives are being used at a time, but imagine the level of confusion that is being produced if more than two are being drawn upon. Indeed, in such cases eclecticism can

lead (and sometimes actually has led) to grotesque creations whose incoherence is so extreme that the only reason it seems to go unnoticed is that the eclectic theorist in question has not been seriously concerned with the problem of internal consistency. As Harris (1979:290) has aptly put it, "eclecticism results in theories that do not link up with or interpenetrate each other and that are often mutually exclusive."

Harris suggests that some eclectics seem actually to be aware of the logical incompatibility of the theories they are advocating, but nonetheless they do not see this as a particular problem. Such an awareness seems apparent in Morton Klass's eclecticism. In the first chapter of his book on the origins of the Indian caste system, Klass (1980) devotes several pages to discussing the debate between Marvin Harris and Harry Pearson over the nature of the concept of an economic surplus and the fundamental character of precapitalist economic institutions (cf., Pearson, 1957; Harris, 1959). Harris takes a view that emphasizes the rationally calculative character of economic behavior in precapitalist societies, whereas Pearson is an advocate of Karl Polanyi's substantivist approach to economic anthropology, an approach that holds that precapitalist economic behavior is shaped by nonrational cultural ideals and values. Obviously these two views of precapitalist economic behavior are fundamentally opposing. Although Klass is aware of this, it does not stop him from attempting to use them both. As he tells us with disarming candor (Klass, 1980:18):

How then can I set out to explore and explain a case of socioeconomic change and development—even evolution—while deriving from one approach based upon fundamental and causal "rationality" and simultaneously from another approach that challenges and discards that "rationality" as neither fundamental nor causative?

I can only do it by begging the question. Shameful, but there it is: the typical eclectic waffle. . .

It is likely though that most eclectics are unaware or only partially aware of the problems of inconsistency and contradiction in their theories. This seems to be the case, for instance, with George Foster's (1967) analysis of poverty in the Mexican village of Tzintzuntzan, a case analyzed carefully by Harris for its logical flaws. Although Foster seems unaware of the inconsistency of his argument, he does seem to have a sense that something is amiss because he has considerable difficulty deciding what the crucial causal variables are. Hence his argument shifts back and forth and continually undermines itself.

It is clear that Foster is committed to eclecticism because he warns against relying exclusively on materialist explanations and declares the necessity of explanations that take into account many other kinds of factors, particularly ideational phenomena—beliefs, values, and norms. Fos-

ter seems to want a broadly based analysis that combines materialism and idealism.

At one point Foster tells us that the major reason for the poverty of the peasants of Tzintzuntzan is a mental complex, which he calls the "Image of Limited Good." This image is a complex of beliefs, values, and norms that holds that success can come only to a few and then only at the expense of others. Foster argues that this image prevents the peasants from struggling to get ahead and thus keeps them at their low level of economic development.

However, having stated this idealist interpretation of Tzintzuntzan poverty, Foster then undermines his argument and replaces it with a materialist alternative. He suggests that it is not really the image that is holding the peasants back, but their objective lack of economic possibilities. But Foster is not done. In a later article (Foster, 1974) in which he responds to the criticism that he has undervalued the role of economic opportunities in his study, he suggests that economic opportunities are indeed key variables, but not simply in and of themselves. What is important, he suggests, is not mere objective possibility, but the peasants' *perception* of economic possibility. It is really the negative perception of economic potential on the peasant's part, rather than the objective character of that potential, that is holding him back. On the basis of this continual shift of Foster's argument, it is very difficult to know just what his theory is. However, this is typical of the kind of confusion that results when one starts out with eclectic commitments.

At this point in the argument, some eclectics may wish to assert that, properly conceived, eclectic arguments need not lead to contradiction at all. Those who defend a complementarity version of eclecticism might wish to argue that contradiction can be avoided if we simply compartmentalize our theories and use them to explain different things rather than the same thing. Although it must be conceded that this argument is perhaps a way of getting around the contradiction thesis, it leads to another problem of its own, which is virtually as severe.

Eclecticism and the Aim of Simplicity

A classic aim of science for centuries has been the achievement of simple theories. In this paper we shall think of the simplicity of theories in two senses: *simplicity as economy* and *simplicity as unification*. Simplicity as economy is the aim of desiring those theories that require the fewest number of concepts and principles. What is at stake is not the clarity of these concepts and principles, their familiarity to us, or even necessarily the brevity with which they can be stated. Undoubtedly these are important notions, and they are perhaps included in some scientists' notions of sim-

plicity, but they are not under consideration here. What is intended is simply the aim of, other things being equal, choosing the theory that requires the smallest number of basic elements.

Simplicity as economy cannot be achieved at all within an eclectic framework. In fact, quite the opposite is the case. By drawing on different theoretical traditions, eclectics must draw on several different sets of concepts and principles, and therefore it is obvious that eclecticism maximizes rather than minimizes the number of basic elements to be relied on. Consider, for example, Dahrendorf's eclecticism discussed earlier. Dahrendorf sets forth highly abstract accounts of the integration and coercion models of society, reducing each to four general propositions. To be eclectic in regard to these models requires utilizing eight propositions, but to be non-eclectic here requires using only four. Although this is a very simple example, it should make the point nonetheless, for the same basic logic applies to the side-by-side utilization of other combinations of theories. Moreover, it is also obvious that the greater the number of theoretical traditions that are being drawn upon, the farther we depart from simplicity as economy.

Simplicity as unification is a similar, yet importantly different, notion. It is the aim of developing theories that maximize the number of phenomena being explained and of simultaneously demonstrating a fundamental underlying interconnectedness, unity, or coherence to these phenomena. The aim of simplicity in this sense is to generate theories that depict an underlying orderliness or unification to the world. It is, in short, the aim of showing that the world is really simple rather than complex, or at least that it has a greater simplicity than is readily apparent. Developing theories that meet the criterion of simplicity as unification means developing theories that link the explanation of A to that of B, of B to that of C, and so on, rather than developing separate theories for each of A, B, C . . . n.

The ideal of simplicity as unification has been well stated by the British philosopher Nicholas Maxwell (1974a:140–141):

According to the view to be advocated here—a view which may be called “aim oriented empiricism”—the fundamental aim of pure science is to discover more and more about an underlying simplicity, coherence, unity, harmony, order, beauty, or *intelligibility* which we conjecture to be inherent in the universe (or inherent in the phenomena we are investigating). According to aim oriented empiricism there is, in other words, inherent in the basic aims of science a wild metaphysical *conjecture*—namely, that the world (or the domain of phenomena under investigation) is intelligible. . .

Maxwell points to Einstein and Poincaré as outstanding exemplars of the aim of simplicity as unification. Both thinkers seemed almost obsessed with the notion of the intelligibility, coherence, or underlying beauty of

nature and of the necessity of developing theories to capture that. Moreover, Maxwell suggests that aim-oriented empiricism has been the classic mode of scientific practice since the scientific revolution of the seventeenth century. As he puts it (1974b:265–266):

That science persistently seeks simplicity, unity, coherence, order, lawfulness, harmony, beauty, explanatoriness or intelligibility in *some* sense of these terms, can hardly be doubted by anyone . . . Even the most casual glance at the history of physics, let us say, reveals the enormous importance that notions such as these have played in influencing the direction of research, and in influencing evaluations of contributions to knowledge. For example, the hope that nature may ultimately turn out to be simple, coherent, unified or intelligible can be seen to lie behind the seventeenth century idea that all natural phenomena arise as a consequence of the arrangements and motions of a few different sorts of elementary corpuscles, interacting only by contact; the same hope lies behind the Faraday-Einstein idea that everything is made up of one unified field; and it lies behind Wheeler's idea . . . that there is in the end only curved empty space with changing topological features . . .

Some of the greatest contributions to science are precisely contributions which "unify" apparently diverse phenomena (often against a background of entirely different metaphysical blueprints): there is, for example, Newton's unification of the motion of terrestrial and astronomical bodies by means of his laws of motion and law of gravitation; Maxwell's unification of electricity, magnetism, and optics (further unified by the special theory of relativity); Einstein's unification of gravitation and geometry. Again, there is the discovery of the relatively few chemical elements, from which all the millions of diverse chemical compounds can be built up; there is Mendeleev's discovery of a pattern in the properties of the elements; and the twentieth century discovery that all matter is built up of just three types of particles—electrons, protons, and neutrons. There is the tremendous unification achieved by quantum theory—a few relatively simple physical postulates sufficing in principle to predict and explain all the vast diversity of physical and chemical properties of ordinary matter. And there is the discovery that all natural phenomena can be understood in terms of just four (or even possibly only three) kinds of forces or interactions . . .

The implications for eclecticism of Maxwell's argument are readily apparent: eclecticism directly and profoundly undermines the achievement of simplicity as unification. Eclecticism does not treat the world as simple and unified, but as complex and disjointed. One sees this sentiment constantly expressed by defenders of eclecticism. Things are not so simple, they will say, or some things are to be explained one way and other things another, or sometimes material forces are operative, sometimes ideational forces are, and sometimes it is human biology that makes a difference. Eclectics believe in an intelligibility to the world alright, but it is an intelligibility that is extraordinarily complex and diverse. Moreover, it is a kind of intelligibility that has nothing in common with the sort that Maxwell speaks of, and that has fascinated the greatest scientists like Einstein and Darwin.

The negative influence of eclectic doctrines on the achievement of sim-

plicity can be better appreciated through a concrete illustration. An excellent example of a work that departs markedly from simplicity in both senses is the first volume of British sociologist Michael Mann's *The Sources of Social Power* (1986). In this work Mann attempts nothing less than a detailed account of many of the major historical transformations in human societies from about 10,000 years ago up to the beginnings of the Industrial Revolution. Mann's approach is more Weberian than anything else, but in the most causally pluralistic Weberian way, and with other theoretical notions thrown in as well. It is in essence a type of eclecticism. Mann starts out by accepting a general evolutionary account of historical development through the origin of agriculture, but he suggests that this was the end of general social evolution. From that point on, a broad evolutionism is abandoned in favor of an emphasis on historical uniqueness. This emphasis is applied to analyses of several agrarian civilizations, and with special vigor to the development of the modern Western world. In regard to the latter, Mann combines ideological, religious, political, military, technological, economic, and other variables to produce an explanation of the rise of the West that is extraordinary for its complexity and its failure to produce a unified understanding. Mann's commitment to a deep causal pluralism is explicitly restated in the final chapter, which deals with patterns of agrarian historical change. Here Mann (1986:523) declares that there is no general pattern and no general formula for understanding the multitude of threads that history reveals:

It will be evident by now . . . that this volume cannot support a general "historical materialism." . . . I cannot agree with Parsons. . . when he says, "I am a cultural determinist . . . I believe that . . . the normative elements are more important for social change than . . . material interests." Normative and other ideological structures have varied in their historical force . . . Nor is it true, as Spencer and other military theorists asserted, that military power was the decisive tracklaying agency in extensive preindustrial societies . . . Political power seems to have attracted fewer enthusiasts. But they would be equally stymied by the comings and goings of political power.

To the extent that simplicity is an intellectual virtue, Mann's work can only be judged negatively. After finishing his book one experiences a kind of intellectual exhaustion, both from trying to recall all the myriad pieces of the argument and from trying to see any sort of general principles that govern human social life. One tries the latter in vain.

Maxwell suggests that to achieve the goal of simplicity as unification, a basic metaphysical blueprint is required. This means the articulation of a coherent research program—a basic set of ontological assumptions and theoretical concepts and principles—and the following of it as thoroughly and systematically as possible in the conduct of research. Maxwell believes

that such a blueprint is especially crucial for the social sciences. I will presently attempt to show why I believe Maxwell is essentially correct.

Eclecticism and the Strategy of Comparative Theory Assessment

A viewpoint once widely accepted among philosophers of science was Karl Popper's (1959) *falsificationism*. Basically, Popper held that good science was a matter of putting forth bold conjectures and then submitting them to the most rigorous possible empirical tests, that is, attempting to falsify or disprove them. Those theories that emerged unscathed were provisionally accepted until better ones came along.

One major problem with Popper's argument is that the criterion for falsification it proposes is unrealistically stringent (Lakatos, 1970). Theories cannot be falsified simply by comparing them with empirical data, for scientists are generally unwilling to throw out even highly unsatisfactory theories in the absence of a good alternative. Imre Lakatos (1970) has therefore proposed that the testing of theories can only meaningfully take place in a comparative context of other theories. Theories are tested not only against the data, but also against other theories as well, and the goal is not to choose a single correct theory (or an unfalsified theory), but rather the one that holds up better than its competitors. Larry Laudan (1977:71) has expressed essentially the same notion:

... *the evaluation of theories is a comparative matter*. What is crucial in any cognitive assessment of a theory is how it fares with respect to its competitors. Absolute measures of the empirical or conceptual credentials of a theory are of no significance; decisive is the judgment as to how a theory stacks up against its known contenders. . .

Lakatos' and Laudan's arguments about the comparative context of theory choice should not be taken merely as scientific ideals, but as actual descriptions of what largely happens in science. At least this is what basically happens in the natural sciences, where eclecticism seems rare or non-existent. This occurs to a substantial extent in the social sciences too, except where eclecticism is practiced. Moreover, where eclecticism prevails, comparative theory evaluation cannot possibly occur because the eclectic has already decided in advance that all theories are partially valid interpretations that should be used jointly rather than scrutinized for their relative merits.

Think of what would have happened, for instance, if eclecticism had been adopted as a peace-keeping device in contemporary cosmological theory. Proponents of the big bang cosmology and their rivals, the steady-state theorists, would have agreed that each camp had part of the truth and the camps should combine forces in order to give a more complete

understanding of the universe (or complementarity eclectics would have argued that the big bang theory explains some aspects of the universe, whereas the steady-state theory explains other aspects). The result would have been theoretical contradiction and chaos on a grand scale, and speaking more directly to the point under consideration here, there would have been no possibility for the big bang cosmology to have emerged as a reigning interpretation, making sense out of many empirical phenomena and paving the way for many ingenious research efforts. Scientific progress would have been stopped dead in its tracks.

Consider also what actually has happened in sociology because of a failure to understand the crucial importance of comparative theory assessment (Wiley's "theoretical combat"). Earlier we noted Dahrendorf's advocacy of a complementarity version of eclecticism in which functionalists would study social order and conflict theorists would explore conflict and change. Although Dahrendorf himself has not actually been a practitioner of this type of eclecticism, being a major contributor some years ago to a conflict model of society, he has nonetheless become famous for recommending that the sociological discipline as a whole practice it as a means of achieving greater explanatory completeness. His advice has been taken seriously, coming to be perpetuated as standard textbook wisdom to millions and millions of undergraduates. The two leading introductory textbooks of the last decade, for instance, those by Robertson and by Light and Keller, are strong advocates of a complementarity eclecticism (Robertson, 1981:vi-vii; Light and Keller, 1985:19). To the credit of these authors, they do not advocate complementarity eclecticism in a wholly uncritical fashion, and they do sometimes engage in a superficial sort of comparative assessment of the relative explanatory power of theories. Yet on the whole the complementarity eclecticism shines through, and the message that is therefore conveyed is that maximizing the virtues of all approaches takes priority over "letting the best theory win."

Why Eclecticism?

Eclecticism seems to be a rare if not actually nonexistent occurrence in the natural sciences. At least one has great difficulty recalling any real cases of its serious advocacy. However, it is common today in sociology and in some other social sciences like anthropology (cf., Harris, 1979). It seems that the reason for this is that we are living through a period of immense crisis and confusion in these two social sciences, a period of crisis probably greater than they have ever experienced before. This crisis is one that is simultaneously ontological, epistemological, methodological, and theoretical. With so many contending voices, many people simply despair of any basic consensus being formed around one major perspective

or research tradition. They throw in the towel and declare value in all the traditions. This seems a natural and predictable, if highly unfortunate, occurrence. In this sense then, eclecticism is really a defensive rather than an offensive strategy, a means of groping through crisis but not really of overcoming it. As Harris (1968:284) has suggested, "eclecticism is certainly the path of least resistance through the frequently strident polemics of the system-mongers."

But why is there this response in the social sciences and not the natural sciences? After all, the natural sciences have suffered through periods of great crisis (these are commonplace, for instance, in twentieth century physics).³ The answer, I think, is that the natural sciences deal with more manageable phenomena and have methods and instruments more capable of providing definitive answers to difficult questions. In other words, they have material means of resolving crises that are not really available to the social sciences or not available to the same extent.⁴

Crises in science, even in social science, *are* resolved, and the current crisis of sociology will also be resolved. To understand how this might occur, we need to consider the alternatives to eclecticism, because it is only through one of them that crisis and confusion can be overcome.

BEYOND ECLECTICISM

An Alternative to Eclecticism: Commitment to a Research Tradition

The most obvious alternative to eclecticism is commitment to a particular research tradition and the full-scale use of it as a guide to research. This is clearly the most common form of scientific practice, particularly in the natural sciences.

The best-known advocate of the notion that strong commitment to a research tradition is a fundamental prerequisite for scientific success is Thomas Kuhn (1970). Kuhn argues that the basic unit guiding scientific research is the paradigm, a constellation of ontological, methodological, and theoretical notions represented by an outstanding, prototypical piece of research (an exemplar) and associated with a concrete body of researchers interested in a particular research specialty. Most scientific research, Kuhn asserts, is paradigm-based puzzle-solving activity. Representatives of the paradigm take its basic elements for granted and attempt to apply it to a wider and wider range of empirical problems. Kuhn believes that this is the only way in which progress in science can occur. Only by having a strong commitment to what the world is really like can researchers pose and solve empirical problems in any sort of meaningful way at all.

Without endorsing any of the particulars of Kuhn's view of the nature

of science, and without agreeing with Kuhn that the commitments of scientists must really be dogmatic ones, we can nonetheless agree with the spirit of his view about the importance of commitments. Commitments are crucial, because without them scientific research would be a grossly inefficient and confusing activity in which the rate of progress would be intolerably slow. Only through such commitments can we know what kinds of questions are most in need of answering, what sorts of basic theoretical premises are most apt to yield productive answers to those questions, and what kinds of methods will be optimal for generating the answers.

Moreover, there is very good evidence that Kuhn's description of science (as opposed to his prescription as to how it has to work) is close to the mark. Although scientists are probably not as dogmatic as he suggests or as inclined to take as much for granted as he seems to think, they do seem to behave in ways remarkably like his characterization. This is borne out in Ian Mitroff's (1974) interviews with various scientists participating in the Apollo moon missions. The scientists that Mitroff talked to noted again and again the intense commitments of scientists to particular points of view as well as the importance of these commitments for scientific advance.

The traditional view of achievement in science is increasing empirical success. Science moves forward inasmuch as it provides more answers to more empirical questions, that is, it increases its empirical content. This is clearly the Kuhnian view, and it is clear that Kuhn believes that the role of commitments is to increase empirical success. The same view is also associated with rival philosophies of science, like that of Popper. Nicholas Maxwell provides us with another reason for desiring commitment and definitive theory choice, one that derives from his aim-oriented empiricism. Maxwell believes that we can choose to commit ourselves to a given perspective not only on the grounds of its empirical success, but also in terms of its ability to understand the world as a coherent and unified whole. In fact, Maxwell's (1974b:257) first methodological rule of science is:

Other things being equal, choose that aim, that blueprint, which is the most intelligible, simple, coherent, harmonious, explanatory, unified, beautiful. (In part, at least this will mean choose that blueprint which promises to lead to the development of the most intelligible, simple etc. testable scientific theory.)

The implication of Maxwell's argument is that only by a deep kind of commitment (actually a metaphysical commitment in this case) can we promote the proper aims of science.

Defenders of eclecticism often point out that any particular research tradition provides only a partial understanding of the phenomena we seek

to explain. Commitment to any single one, therefore, dooms us to incomplete views of the world. There is no denying the accuracy of this statement, but that is no cause for any particular concern. Scientists clearly recognize and accept the incompleteness of their theories. Moreover, the eclectics' way of achieving greater completeness, as we have seen, comes at much too high a price.

As illustration of the virtues of strong theoretical commitments, consider three well-known theoretical strategies in the social sciences. Marvin Harris is renowned among anthropologists and many comparative sociologists for his role as the chief architect of the strategy that he has named cultural materialism (cf., Harris, 1968, 1974, 1977, 1979; Ross, 1980). This approach attempts to explain the fundamental similarities and differences in human societies past and present as responses to the material conditions of human existence. It divides societies into infrastructural, structural, and superstructural components; enunciates the principle of infrastructural determinism, which holds that causation is probabilistically generated from the infrastructure; assumes the crucial importance of an *etic* (or scientifically detached) epistemology; rests on a few key assumptions about human nature; and presents a nonreified model of social life as the aggregate result of individual choices made under various kinds of material constraints. This approach is extraordinary for its simplicity, both in the sense of economy and of unification, for its relative ease of application to numerous research questions, and for the sheer quantity of studies that it has generated. Many of this approach's proponents, Harris especially, have shown an exceptionally vigorous commitment to it, and this commitment seems to have paid excellent dividends. Cultural materialism has generated an extremely large number of highly consistent research findings, and it shows every sign of continuing empirical robustness in the future.

Immanuel Wallerstein's (1974, 1979, 1980) world-system approach has the same basic virtues. It attempts to understand the evolution of the modern world in terms of a conception of capitalism as a world economy divided into core, peripheral, and semiperipheral segments that interact in certain ways. The world economy is structured according to a logic in which powerful groups located in the core act so as to generate large economic surpluses and appropriate them for themselves, producing high levels of wealth concentration within the core and low levels elsewhere, especially in the periphery. The relations between the three segments of the world economy change over time because of such things as shifts in world market opportunities and constraints, changing balances of material resources, and changing political features of states. Like Harris, Wallerstein has generated a large following and his approach has shown an

extraordinary capacity for generating interesting research questions and provocative answers.

A third theoretical strategy worth considering is one that is older, more general, and undoubtedly better known, at least to sociologists: the *exchange* approach of George Homans (1974, 1983, 1984). Homans has shown a vigorous commitment over many decades to this approach, which he believes is generally applicable to all forms of social behavior. He has suggested that social behavior can be understood in terms of a very small number of basic propositions drawn from behavioral psychology and elementary rational choice economics. Hardly notable for any sympathy to an eclectic position, Homans has eschewed all forms of societal functionalism of the Radcliffe-Brownian or Parsonian variety and has tried to show that more individualistic forms of functionalism of the Malinowskian or Mertonian variety are easily translated into the reward-cost propositions of exchange theory (Homans, 1983). He has also been especially vigorous in his intellectual condemnation of the "culture-vultures," social scientists who explain behavior as being what it is "because of the culture." For Homans, this kind of explanation suffers from a severe form of question begging: one must still explain why the culture itself became what it did. In this regard, Homans's conception of what social norms are and how they originate and change is most illustrative of his general position. In commenting on various types of norms governing kinship relationships, he says (1984:159): "Members of families living in a uniform environment, with a simple technology and division of labor, tend to evolve the same general kind of family organization . . . [W]hat many families do in fact becomes in time what every family ought to do. That is, it becomes a norm, and the elder generation teaches it to the younger, thus reinforcing what the tendencies inherent in the social organization originally brought about." In other words, the statistically most common forms of behavior characteristic of aggregates of individuals, behavior that has itself been selected by those individuals because of its rewarding qualities, eventually comes to be codified in the mind as what is right and proper. When the cost/benefit ratios of such behavior change, new forms of behavior, and hence new norms, will emerge.

Beyond its elegant simplicity, a great virtue of Homans's approach is its wide applicability to diverse forms of social behavior. Homans himself has applied his elementary propositions to such questions as why William the Conqueror never invaded Scotland; why people conform to norms; why economic markets operate the way they do; and why some primitive societies prefer matrilineal cross-cousin marriage whereas others prefer the patrilineal variety (Homans, 1967; Homans and Schneider, 1955). Followers of Homans have applied his approach to other diverse features of social life (cf., Hamblin and Kunkel, 1977). Moreover, it is interesting

that Homans' basic notions, so long ridiculed by sociologists as unacceptably reductionist, have recently been moving rapidly forward under the heading of rational choice theory (cf., Elster, 1979; Popkin, 1979; Hechter, 1983). The proponents of this model are trying to show the very large amount of social behavior that can be explained through reliance on it, including behavior that is seemingly altruistic and selfless.

Harris, Wallerstein, and Homans have all been severely criticized for oversimplifications, and numerous pieces of evidence have been set forth as falsifications of (or at least grave limitations upon) their models. It is undeniable that their models *are* oversimplifications, but that is precisely the purpose of scientific models. The best ones must necessarily oversimplify in order to be useful as research tools, and in this regard these three models are exemplary, for their research impact has been truly outstanding. Compare their economical and unified models to that of Mann discussed earlier. Mann would claim that his eclectic model more closely resembles the real world of human experience; if so, it does so at an extraordinary cost. Time will tell the extent to which Mann's approach generates a large research following, but a fair prediction would be that it will not. A map as large as the geographical area it represents may be accurate, but it would scarcely be of much practical use.⁵

An Alternative to Eclecticism: Acceptance versus Pursuit of Research Traditions

Full-scale commitment to any given research tradition is the most obvious and most common alternative to eclecticism. Another alternative is considerably less well known and much less frequently practiced. This alternative requires an understanding of Laudan's (1977) distinction between the *acceptance* of research traditions and their *pursuit*. When scientists accept a research tradition, they believe the theories generated within it to be true, or at least they proceed as if the theories are true. To accept a research tradition is also to employ it as a major guide to research. Pursuit of a research tradition, however, involves suspension of any commitment to the truth-value of the tradition. When scientists pursue a research tradition, they do not believe it to be true (nor do they treat it as if it were true, but simply believe that it has some interesting features and perhaps considerable promise. It is employed to some extent as a guide to research on the grounds of its perceived possibilities.

Laudan argues that the history of science is filled with instances in which scientists pursue but do not accept research traditions. Indeed, he believes that almost all new research traditions emerge in this manner. Moreover, he argues that scientists frequently pursue a research tradition while si-

multaneously accepting another, and do so even in instances in which the traditions are mutually incompatible.

Thus, we see that scientists can, and frequently do, entertain the merits of more than one approach at a given time, but it is crucial to see that this in no sense entails eclecticism. For eclecticism to be involved, scientists would actually have to be conducting research in two or more research traditions, *all of which they accepted*.⁶ In the situation Laudan has identified, the mutual inconsistency or logical contradictoriness of the theories in question is not problematic inasmuch as only one of the theories is actually being accepted. Neither are the other objections to eclecticism (violation of the aim of simplicity and obviation of the strategy of comparative theory assessment) relevant because the situation is a temporary one. In working within a research tradition that they do not accept, scientists must eventually decide to abandon it because it has not fulfilled its promise, to embrace it and give up their former tradition (the one they had accepted), or, as will be shown presently, they can attempt to fuse elements of the traditions into a new tradition, which they then accept.

A sociological example of what we have been discussing can be drawn from my own work. I accept and work within the tradition of cultural materialism, yet I pursue a strongly opposing research tradition, sociobiology. Although I do not accept sociobiology, I believe it has some interesting features worth attending to. I think it extremely unlikely that I would give up cultural materialism and accept sociobiology, but I think it possible for a certain amount of synthesis to take place between the two. Any possible synthesis seems most likely to take place in the arena of human reproduction, a phenomenon given high priority by sociobiology as well as being strongly emphasized by cultural materialism and by the grandfathers of cultural materialism, Marx and Engels (although interpreted very differently by these latter traditions).⁷

An Alternative to Eclecticism: Theoretical Synthesis

The final alternative to eclecticism involves the genuine synthesis of different theoretical traditions. It is crucial to understand just what is meant by synthesis and how it differs from eclecticism. Synthesis involves the selection of elements from different research traditions and their recombination and fusion into a novel research tradition that is similar to its parents, yet notably distinct from them. Synthesis involves, in other words, taking portions of Traditions A, B, and C and uniting them into a new tradition, D, a tradition with assumptions, concepts, and principles all its own. For research purposes, A, B, and C are abandoned and D becomes the basis for research efforts. Synthesis is fundamentally different from eclecticism, for the latter does not involve the recombining or mixing of

elements together into something new. Rather, it involves a mechanical juxtaposition of the elements of different research traditions, a placing of them side by side. If synthesis is a chocolate layer cake, then eclecticism is certain unmixed quantities of flour, eggs, water, sugar, and cocoa. The taste in each case is obviously very different.

Laudan has suggested that there are two somewhat different types of theoretical synthesis and that both have been common in the history of all the sciences. The first type of synthesis involves preserving the basic elements of two or more research traditions intact but grafting them onto one another. Laudan points to the grafting of parts of subtle fluid theory onto parts of Newtonianism in eighteenth-century natural philosophy as an example of this form of synthesis (Laudan, 1977:104): "While undermining the presuppositions of neither of its predecessors, the amalgamation suggested important new lines of research, and put scientists in a position to deal with empirical and conceptual problems which neither of the ancestor traditions alone could resolve satisfactorily."

The second kind of synthesis is more radical. It involves rejection of many of the elements of the ancestor traditions and the ultimate abandonment of those traditions. This is the type of synthesis implied in our foregoing definition of synthesis, and it seems clearly the most frequent and most important type of synthesis in science. Examples of this process in the history of natural science include the eighteenth-century natural philosopher Roger Boscovich's theory of mutual forces, which synthesized elements of Newtonianism and Leibnizianism, Daniel Bernoulli's effort to synthesize parts of Cartesian and Newtonian physics, and the efforts of the geological followers of Hutton to draw together vulcanist geology and caloricist heat theory (Laudan, 1977).

Synthesizing work has also been common in the social sciences. The most famous example of a theoretical synthesis is probably that of Marxian historical materialism, which is an extraordinarily selective recombination of Hegelian dialectical philosophy, British political economy, and French socialist thought. Examples from contemporary sociology and anthropology are also relatively easy to cite. In anthropology, one of the most interesting syntheses in recent decades is cultural materialism, an integration of certain elements of historical materialism, cultural ecology, and evolutionary theory. Notable features of cultural materialism include its rejection of the dialectical element of historical materialism, its broadening of the material base to include demographic and ecological variables, and its extension of the range of applicability of its concepts and principles to a much wider variety of societal types (cf., Harris, 1979).

In modern sociology noteworthy synthetic efforts have been undertaken by Gerhard Lenski (1966), Randall Collins (1975, 1985), Anthony Giddens (1984), and Talcott Parsons (1937). As is well known, Lenski has attempted

to synthesize the functionalist and conflict theories of stratification into a third theory, which he has referred to as an evolutionary theory. Lenski is highly explicit about what his theory shares with its predecessors and how it differs from them. He notes that it shares with the functionalist theory the notion that human nature is basically self-seeking, the belief that stratification is inevitable, and a nominalist conception of social class. By contrast, it leans in the direction of the conflict theory in emphasizing the importance of struggle in society, in stressing the role of coercion in maintaining systems of inequality, and in noting the degree to which inequality generates social conflict. In other respects it is like neither of the predecessor theories. Lenski is also well aware of the difference between synthesis and eclecticism, and emphasizes that his is a true synthesis, not mere eclecticism.

Collins' effort at synthesis is attempted at a more global level than is Lenski's, that is, it is carried out at the level of whole research traditions. Collins is basically arguing for a type of conflict theory, but one that grafts onto it certain of the insights of other very different traditions. Collins interprets Weber as a conflict theorist and elaborates a type of conflict perspective that draws heavily upon the ideas of both Marx and Weber. However, he adds to these ideas certain specific elements from strongly opposing traditions, especially the Durkheimian tradition and contemporary ethnomethodology. He rejects functionalism in general, but argues that despite Durkheim's functionalist errors he gives us a key insight with his general theory of social rituals. Goffman is seen as a perpetuator of this aspect of the Durkheimian tradition, and his theory of interaction rituals is used liberally. Finally, although he regards ethnomethodology as an unacceptable perspective overall, Collins believes it provides an important antidote to the hypostatizations and reifications of the functionalists and other system-oriented theorists. Its concentration on the details of everyday social interaction is seen as a crucial means of understanding the micro-behaviors that constitute the essence of social life.

Giddens' extensive synthesizing efforts have finally culminated in what he calls *structuration theory* (Giddens, 1984). Giddens has been highly critical of all the major theoretical traditions of sociology, but it has been his major task to show that some approaches err by exaggerating social structure and the structural determination of individual behavior (e.g., Marxism and functionalism), whereas others err in terms of exaggerating how individuals create or construct society (e.g., phenomenology and ethnomethodology). Giddens wants an approach that brings out the virtues of these polar approaches while neutralizing their weaknesses. Structuration theory is said to be this approach. Its central feature is its stress on the dialectical interplay of agency and structure—how society is made by

individuals but within the context of material and nonmaterial resources and human meanings and rules.

Let us finally consider perhaps the most famous synthesizing theorist in all of contemporary sociology: Talcott Parsons. It is well known that Parsons really embarked on his intellectual career by trying, in his massive work *The Structure of Social Action* (1937), to synthesize the works of Alfred Marshall, Vilfredo Pareto, Emile Durkheim, and Max Weber. Parsons correctly noted that these thinkers were in many ways strikingly different. When all were considered together, they could be analyzed in terms of the degree to which utilitarianism, positivism, and idealism were differentially embodied in their works. However, Parsons thought he had discerned a definite tendency in each thinker to move in the same characteristic theoretical direction toward the end of his career. Thus, despite their differences, they were all said to be converging, and quite independently so, on the same basic set of ideas. This set of ideas was what Parsons called the *voluntaristic theory of action*. Parsons saw it as basically his job to complete the synthesis essentially begun by the four classical thinkers themselves. That Parsons (1949:720) was engaged in synthesizing work, or at least quite consciously saw himself as doing such work, is clearly evident from a passage in which he says that the voluntaristic theory of action is:

. . . a new development of theory . . . It is not, of course, a creation *ex nihilo* but was arrived at by a gradual process of critical reexamination of certain aspects and elements of the older systems . . . Though every one of its major groups of elements had some place in at least one of the other traditions as something more than part of a residual category, this is not true of the system as a whole looked at as a specific total structure of conceptual elements. The completed structure is at some vital point incompatible with each of these older systems.

In his later works Parsons moved much more in the direction of a strongly systemic functionalism, and after about the mid-1960s he began to show a major concern with social evolution approached, of course, from a strongly functionalist perspective. Yet in many ways these works too can be seen to have shown a strong concern for synthesizing diverse theoretical elements into a comprehensive scheme. Certainly this is how Parsons (1977:70–71) has viewed his own work. In a personal history of the development of his theoretical ideas written near the end of his career he says that:

. . . the theme of convergence did not stop with the cases intensively examined in *The Structure of Social Action*, but has continued to be a major theme of my whole intellectual career. The conviction of some kind of convergence between socioeconomic and biological thinking played an early part. Perhaps above all my concern

with Freud made salient both the problem of convergence between social system theory and personality theory, and, gradually, the extent to which this was actually present. Of course such convergence often had to be teased out from what at face value were incompatible positions. The analytical distinction between personality and organism has been indistinct in most psychological thinking . . . but . . . it seemed to me to be a case of convergent patterns within a framework of analytical distinctness. Similar considerations have operated in the field of the relation between social and cultural systems, for which I was primed above all by Weber, but also by my many associations with cultural anthropologists. In a sense, perhaps the most extensive convergence of all has seemed to occur under the umbrella of the cybernetic conception, with its many associations and ramifications.

A crucial point that must be emphasized about synthesizing work is that it ultimately allows us to carry out scientific research in the same hypothetico-deductive manner that we would be following if we exclusively relied on a single research tradition. Our syntheses constitute new traditions to serve as basic guides in problem identification, methodological choice, and theory formulation. The main distinctiveness of synthesis is that it more explicitly recognizes the value in several research traditions and attempts to maximize that value, while at the same time not falling prey to eclecticism.

It is essential to stress that synthesizing work in all the sciences is a very challenging task. As Alex Callinicos (1985:135) has pointed out, "Too often attempted syntheses amount merely to banality, incoherence, or eclecticism." Indeed, although they are fundamentally different in principle, there can be a very fine line in practice between synthesis and eclecticism. Many efforts at synthesis are not truly successful or are only partially successful, owing to both the difficulty of the task and the abilities of the theorist. Gerhard Lenski's attempted synthesis, for instance, seems to be only partially successful. Although in many respects it has the character of a genuine synthesis—after all, Lenski does produce a new theory with a new name and with particular principles of its own—careful scrutiny of his work will show that it has some characteristics of eclecticism. It is still a somewhat unintegrated mixture, part functionalism and part conflict theory. (His more general attempt at an evolutionary synthesis [Lenski and Lenski, 1982] appears to suffer from the same problem.) It is notable that Lenski's analysis of preindustrial societies is very much like a conflict theoretical one. Yet in his analysis of industrial societies, and especially capitalist ones, the conflict emphases of the theory largely evaporate and his arguments look more or less like those of the functionalists. This virtually brings Lenski's theory back into the realm of a complementarity version of eclecticism.*

Before closing this discussion of synthesis, it seems worth asking whether a synthesis so broad that it could actually unify all of sociology, or even all of the social sciences could ever be achieved. After all, even

our best syntheses currently fall far short of this mark, and micro- and macro-theorists have for the most part ignored each other. I would venture a nervous "yes" in answer to this question, even to the extent of suggesting that the micro- and macro-realms will one day be brought together in a unified theory. Such a massive synthesis will certainly be extraordinarily difficult to achieve, but I do not think it will be impossible, and there are already some promising lines of development in this regard. My own preferences would be to see work directed toward integrating many aspects of Marxism (including world-system theory), the conflict elements of Weber, sociobiology, elements of rational choice and exchange theory, and cultural materialism. Such an integration would need to be closely informed by a proper understanding of the renewed concern for "agency" that Giddens and others talk so much about, and certain carefully elucidated evolutionary notions not only can be brought into this picture, but also are actually crucial to it (cf., van Parijs, 1981).

The most difficult part of such a synthesis would involve uniting micro- and macro-concerns, but even here I think optimism is warranted, for serious and promising work exploring the microfoundations of macrosociology has been underway for several years in the work of Collins (1981), Hechter (1983), and others. (Giddens's concern with "agency" and with overcoming dualisms of action and structure is also relevant here.) One problem here is that the kind of microfoundation suggested by Collins (largely ethnomethodological) is different from that suggested by Hechter (rational choice). However, this difficulty is probably not as serious as it might initially appear, and the most important consideration is the fact that micro-macro linkages are now being seriously explored far more than they ever were before.

In short, much work is yet to be done, both within the micro- and macro-camps as well as between them, but recent theoretical work suggests that a major unifying synthesis is indeed a realistic possibility.⁹

CONCLUSION: ECLECTICISM AND THE CONTEMPORARY PHILOSOPHY OF SCIENCE

In attempting to show the pitfalls of eclecticism and the alternatives to it, not only has this paper assumed that sociology is a science (or at least has many strong features of science), but it has also made certain basic philosophical assumptions about the best way to conduct science. By and large, these assumptions are closer to the philosophical model of science presented by Laudan (1977, 1984) than they are to other models. Laudan assumes that theory choice in science is not merely a matter of comparing theory and data or of picking out a uniquely correct theory, but of choosing

the theory (or research tradition) that is better than its rivals. (A better theory for Laudan is one that solves more problems, not one that is true or closer to the truth.) Obviously Laudan's model cannot tolerate eclecticism, for at least two of the reasons we have given (logical inconsistency and violation of the strategy of comparative theory assessment).

Attaching my arguments rather closely to Laudan's model does not however affect in any significant way the conclusions of this paper, for no major philosophical model of science of the last half-century (or earlier for that matter) tolerates eclecticism. The two other major models besides Laudan's are those of Popper (1959) and Kuhn (1970). Popper's model would have to oppose eclecticism even more strongly than Laudan's. For Popper, good science is a matter of attempting to falsify theories, and the theories we accept are those that have most thoroughly withstood falsifying efforts. Because eclecticism's aim is to save as much as possible of prevailing theories, it is diametrically opposed to Popper's exhortations to try to kill our theories off. Falsificationism and eclecticism cannot possibly coexist without massive logical contradiction.

What then of Kuhn's model, a model that has been enthusiastically endorsed by many social scientists, sociologists especially? For Kuhn, most science is paradigm-driven normal science. Outside of scientific revolutions, Kuhn cannot conceive of science being conducted in any other way. A Kuhnian view of science could not even remotely endorse eclecticism as a meaningful approach, for eclecticism precludes the kind of commitment (indeed, dogmatism) that Kuhn believes to be essential to progress in science.

In conclusion, because all prevailing philosophical models of science stand steadfastly opposed to eclecticism, such a stance should be abandoned by sociologists. However, to eschew eclecticism does not mean to embrace dogmatism or to devalue all elements of all research traditions save one. Indeed, one can be perfectly open-minded and recognize value in several points of view and still have several solid options available for avoiding the eclectic trap.

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NOTES

1. Before the current theoretical interregnum and the widespread popularity of eclecticism, textbooks were generally not eclectic. When functionalism was hegemonic, most textbooks were functionalist.

2. Although Feyerabend's advocacy of pluralism in its extreme form is indefensible, a substantially milder version of it need not evoke any strong objection, and Wiley's position discussed earlier is basically of this sort. One can certainly agree that certain types of strong intellectual hegemony can have stultifying consequences for intellectual progress, and that diversity can contribute in an important way to intellectual progress when it stimulates "theoretical combat." In fact, later in the paper a strong argument for such "theoretical combat" (phrased in terms of "comparative theory assessment") is made. Yet despite all this, it must again be emphasized that encouragement of diversity is something quite different from eclecticism.

3. Although natural scientists seldom if ever become eclectics, they do sometimes work in more than one research tradition at the same time. Yet they do this in a way that does not qualify as eclecticism, a point to be explained in the next section. Moreover, it appears to be during periods of greatest confusion and crisis that natural scientists behave in this way (cf. Laudan, 1977).

4. Another reason why sociologists and other social scientists so frequently endorse eclecticism might well involve the general attractiveness that relativistic arguments have for so many social scientists. Although relativism and eclecticism cannot be equated, they are close cousins, and they often go together. My impression is that sociologists and anthropologists are the social scientists who most frequently espouse eclecticism. (Economists seem the least likely to do so.) The members of these disciplines have shown much positive regard for the epistemological relativism of Thomas Kuhn (1970), and often erroneously assume that his relativism implies the necessity of eclecticism. (An error of this sort is made by Cardwell and Kalab, 1986.) Moreover, these disciplines originated and still espouse at least to some extent another kind of relativism—cultural relativism—which they often promote as an adequate moral philosophy. It may also be worthy of mention that the recent popularity of eclecticism in sociology and anthropology coincides with a general cultural climate in which relativisms of all kinds are widely endorsed, and the theoretical positions of sociologists and anthropologists have always been notably influenced by wider cultural conditions.

5. Despite some of their obvious differences, it seems worthwhile to note that the models of Harris, Wallerstein, and Homans are all strikingly similar in being rooted in what might be called an *interest* model of social behavior: people generally behave so as to maximize the extent to which they can achieve their interests while minimizing the costs involved in doing so. This is the whole foundation of Homans' model. However, the same fundamental notion underlies Harris and Wallerstein's approaches as well. Harris (1979:60), for instance, has said that, "the selection processes responsible for the divergent and convergent evolutionary trajectories of sociocultural systems operate mainly on the individual level; individuals follow one rather than another course of action, and as a result the aggregate pattern changes." Harris even goes so far as to suggest the fundamental kinds of needs

humans have and therefore the kinds of interests they will be following (a rather serious omission, it seems, in Homans' work). It is true that Harris has occasionally lapsed into functionalist arguments of one kind or another, but this is not especially characteristic of his work. Wallerstein, too, has been charged with a strongly systemic functionalism, and he often speaks of the conditions necessary to keep the world system from collapsing. However, this seems more of a particular kind of language than the essence of the analysis: the core of his analysis focuses on economic actors who are maximizing their interests.

Because of the strong similarities between these three models, their synthesis would be highly conceivable, a point I expand on at the end of the paper.

6. It is conceivable that eclectics could deny that they actually accept any of the traditions they use, claiming only that they find features interesting and useful. Such a claim would be exceptionally difficult to defend, however, not to say disingenuous. Eclectics do accept the different traditions they draw upon in the sense that they believe them to be at least partially valid models of explanation. They are not suspending any judgment about their value. Pursuit without acceptance, however, involves precisely such a suspension of judgment. To pursue a theoretical model without accepting it means to tinker with it with the thought in mind that it could eventually turn out to be worthy of acceptance.

7. It is difficult to think of other actual examples of simultaneous acceptance and pursuit in the work of social scientists, but even if there are few of them (or even none), the point remains that this is an intellectual practice that can be highly recommended for future work. Social scientists could seriously consider it as a way of getting around the difficulties of eclecticism in circumstances in which dogmatic commitments to a particular approach seem unwarranted and in which genuine synthesis seems far off in the future.

8. The degree to which attempted syntheses actually are successful can, undoubtedly, still be fruitfully debated. I would argue that synthesizing efforts like those of Giddens and Harris are among our best examples of true syntheses. In both structuration theory and cultural materialism we have theorists who have developed novel concepts and principles by drawing upon a number of older theoretical traditions. In both cases the older traditions have lost most of their former identity and have been submerged into some larger theoretical entity. In both cases there is an explicit set of recommendations for how the novel concepts and principles should guide research efforts. Even if there would remain an unsynthesized residue of eclecticism in these formulations, this is as close to genuine synthesis as we have come thus far.

Collins' work perhaps does not have quite the feel of genuine synthesis in the way that Harris and Giddens' work does, and it therefore seems more understandable that the term eclecticism is occasionally applied to it. In principle, however, Collins is doing a kind of grafting synthesis in which the major element is a Weberian version of conflict theory.

Parsons' integrative efforts have for the most part produced genuine syntheses, but they are still not quite on precisely the same level as those of Harris and Giddens, and for two basic reasons: in Parsons' early work he was basically claiming only to be completing a synthesis already half done by the original thinkers themselves; and the charge that Parsons never had a theory at all but only an elaborate conceptual scheme cannot be ignored in considering the rigor of his synthesizing efforts. These problems aside, Parsons' work is otherwise highly characteristic of synthesis.

If we contrast these efforts with those of Dahrendorf or of textbook accounts like those of Robertson, then the difference between eclecticism and synthesis is drawn into sharp relief. Neither Dahrendorf nor Robertson is attempting anything like a fusion of disparate traditions. Dahrendorf has recommended mechanical juxtaposition of functionalism and conflict theory, and Robertson has practiced what Dahrendorf preached.

Undoubtedly one could think of examples of theoretical efforts that are much less clear-cut than the aforementioned ones in terms of whether synthesis or eclecticism is at stake.

Such actual borderline examples, however, even if they turned out to be numerous, would not obviate the distinction made here. Synthesis is fundamentally different *in principle* from eclecticism, and synthetic efforts are to be applauded *pari passu* to the extent to which theoretical fusion and integration result.

9. The recent work of Jon Elster (1982, 1985), which tries to link Marxism and methodological individualism through game theory, is another major example of the possible lines along which a synthesis of micro- and macro-levels could run. Elster insists, properly in my opinion, that explanations of macro-phenomena must always have microfoundations, that is, they must always be ultimately rooted in the aims and interests of individuals. Although Giddens would say it differently and go further, I take this to be the real significance of his stress on human agency.

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