PROGRAMS, THEORY, AND METATHEORY[1]

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PREFACE

In the January 1985 issue of the *American Journal of Sociology* we published a paper, "Do Sociological Theories Grow?", concerned primarily with the roles we believe theoretical research programs play in the growth of theoretical knowledge in sociology. This paper has generated a good deal of comment. The *Journal*, in fact, has accepted two critiques of "Do Sociological Theories Grow?", and our response to them. This exchange will appear in a forthcoming issue of the *Journal* (probably July 1986).

The exchange focuses on such basic questions as the interrelatedness of theory, metatheorv, and observations, and the adequacy of program analysis in exploiting the intellectual tradition of sociology. Discussion of these issues is of interest to ail sociologists. We wish to encourage more such discussion.

Because of the importance of these matters we are issuing our response to these critiques as a technical report. This response is somewhat longer than that published in *AJS*. We invite your comments, both privately and as participants in a public debate of interest to sociologists generally.

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INTRODUCTION

Analysis of the ways in which theoretical knowledge in sociology grows or develops is a large and complex task. "Do Sociological Theories Grow?" was an attempt to contribute to the performance of that task. In that article we distinguished three levels of theoretical activity: orienting strategies, unit theories, and theoretical research programs. We argued that theoretical growth is best analyzed at the level of programs, in part because orienting strategies are comparatively stable and because the analysis of growth at the level of unit theories focuses primarily on questions of empirical adequacy. We then showed that an analysis at the level of programs yields at least five different patterns of theoretical growth: elaboration, proliferation, competition, variation, and integration. The first three of these patterns, we claimed, generate entire programs of activity, while the latter twa usually occur only at advanced stages of programs based on the other three patterns.

Some challenges to our analysis have been raised. A few of these challenges reflect basic differences in our approaches to sociology. Many more involve misunderstandings based on ambiguities or differences in emphasis in our original article. We appreciate this opportunity to clarify and expand our discussion of some issues that have arisen from our analysis.

Our comments are organized around several major issues: (1) the way in which the structures of theoretical research programs are determined, (2) the role that such programs play in the analysis of theoretical growth, (3) the interrelatedness of theory, metatheory and observations, and (4) the adequacy of program analysis in exploiting the intellectual tradition of sociology.

Before considering these substantive issues, howsver, we find it necessary to make two comments: one on the nature of some of the statements in Maines and Molseed's remarks and the second on their use of technical terms.

First, we note that their remarks are punctuated throughout with statements imputing motives and intentions we are supposed to have, attributing attitudes toward others ws are supposed to hold, and inferring character traits ws presumably exhibit in our work. Thus, for example, we are said to behave "arrogantly"; we are told that the "obsessive discoverer's complex" applies to our work with its accompanying "disdain for prior work not of their clique thinly veiled"; and we are said to display the quality of "rigidity" in our work. Our response to these statements is to call attention to their repeated occurrence (as if repetition would give them reality) and to invite the reader to read our paper.

Second, we observe that Maines and Molseed make frequent use of technical-sounding terms in analyzing our work. They claim that it is "inconsistent," that it is "imprecise," that our definitions are "vague and specious," that we "engage in ontological debate," and that our account of theory growth is "tautological"—these among many other things. Some of the time, the meanings of their technical terms are unclear. Some of the time, they apply these terms without reasoned argument, or with an argument whose validity ws believe to be in question. But almost all of the time, these technical-sounding judgments are delivered in an apodictic manner. We shall not stop to analyze Maines and Nblseed's usage of each of these "technical" terms. Later in our discussion, however, we shall briefly consider two exanples of their technical-sounding judgments, one involving the idea of "tautology" and the other the idea of "ontology."

Now, on to substantive matters.

DETERMINING THE STRICTURE OF A PROGRAM

In our article illustrated the analysis of linear programs (i.e. ones in vMch the primary mode of development is theory elaboration) with two examples from the bargainirig literature. Both programs—conflict spiral and deterrence—use key ideas from Ihibaut and Kelley (1959) regarding interdependence in social relationships. Both focus specifically on the role of threats in bargaining. However, the two programs develop somewhat different accounts of just what that role is.

In the conflict spiral program Deutsch and Krauss (1960) elaborate Ihibaut and Kelley*s more general analysis by specifying a "face-saving" mechanism governing the use of threats. Any threat by A causes a loss of face for B, which prompts B to issue a counterthreat to A; the result is a spiral of conflict. Shomer, Davis, and Kelley (1966) then elaborate Eeutsch and Krauss by distinguishing threat from actual harm or punishment; only the latter, they argue, always generates the conflict spiral. By contrast, in the deterrence program Itorai and Tedeschi (1969) elaborate Ihibaut and Kelley by specifying a "subjective expected utility" mechanism governing the use of threats. Each credible threat by A increases the utility of compliance for 8; as a result, B is deterred from issuing a counterthreat to A. Ttedeschi et al. (1972) elaborate Horai and Tedeschi by enumerating a set of structural and situational conditions (e.g. attractiveness, prestige) that affect the subjective expected utility of compliance; the higher the utility, the greater the deterrent effect of compliance. Finally, Bacharach and Lawler (1981) integrate both conflict spiral and deterrence principles in a single formulation v*hich specifies tne conditions under which each effect occurs. Basically, their argument is that deterrence occurs when the stakes in the bargaining are relatively low; as the stakes increase, the likelihood that threat will prompt counterthreat increases, thus inducing a conflict spiral.

Maines and Molseed have challenged our analysis of these programs. Their first criticism is that the deterrence principles do not constitute a theory. This claim is based on a misunderstanding of what may be considered a theory. Theories are minimally composed of a set of concepts and a set of theoretical assertions and principles that involve these concepts. Often much of the structure that these concepts and assertions comprise is left implicit: concepts are vaguely defined or left undefined altogether; assertions are vaguely stated or ambiguously related to each other. Nevertheless, implicit theories are still theories. They may simply be regarded as theories which are not fully explicated. Such is the case with both deterrence and conflict spiral. In much of the relevant literature large parts of the theoretical structure are left implicit. It is only in Bacharach and Lawler's work that a relatively clear and complete explication of both theories becomes available.[2]

Maines and Molseed also criticize our characterization of relations between the theories in the conflict spiral and deterrence programs. Our analysis is in error, they argue, because some of the relations we specify are not reflected in citation analyses or statements of authors' intentions. This challenge too is based on a misunderstanding, in tnis case of the nature of the theoretical relations that characterize a program. All of the relations we consider—elaboration, proliferation, competition, variation and integration—are abstract and analytical. They have to do with the structural features of theories. Our specification of different relational types is based on similarities in such features as the conceptual and propositional apparatus of the theories, the problem focus or explanatory domain of the theories, and the predictions made about the data and observations relevant to the theories. For example, if one theory T(l) uses principles very similar to those of another theory T{2) in addressing a similar sociological problem, but makes predictions which are more comprehensive, precise, rigorous, or empirically accurate, then T(2) is an elaborant of T(1).

Thus, to determine vrtiat theoretical relation (if any) exists between two specific theories, one must first identify the structural features we have outlined and then evaluate how similar each of those features are. Other kinds of information, such as citations or the expressed intentions of authors, may provide clues to the relations between theories, but they are not the basis upon which the relations between theories are specified.

Consider first citation analyses. Such analyses do not, of course, directly reveal theoretical structure. Furthermore, particular citations may be present or absent for many reasons other than theoretical relatedness. Expressly stated intentions of the author are subject to many of the same limitations. Authors are not always explicit, consistent, or sure of their intentions in constructing a theory. Even when the author's intentions are clear, they may not accurately reflect vtfiat he or she has accomplished or how others will use the theory; abstract, analytical linkages between theories may be independent of the intent of the theorist. Since determinations of theoretical relatedness are based on the analysis of structural features of theories, citations and expressed intentions are at best only suggestive.

In the bargaining programs theoretical relations are based on several structural features. First, there is a similarity in problem focus. In both programs theories include as at least part of their explanatory domains the role of threats in determining the outcome of bargaining relationships. Second, there is a similarity in conceptual and propositional apparatus. In both programs theories incorporate the key notion of "interdependence of behavioral outcomes" from Thibaut and Kelley (1959). These features are central to all the linkages we specify in the two bargaining programs. They are the most important (though certainly not the only) bases for our claim that Thibaut and Kelley (1959) is related to Deutsch and Krauss (I960) and to Horai and Tedeschi (1969).

Other structural features determine what kind of relationship exists between these theories. Deutsch and Krauss (1960) and Hsrai and Tedeschi (1969) each introduce propositional mechanisms (face-saving and subjective expected utility respectively) that permit more specific predictions about the consequences of threats in bargaining situations than is possible using Thibaut and Kelley (1959). This greater determinacy or precision of predictions (along with the similarities in problem focus and conceptual apparatus) makes both theories elaborants of Ihibaut and Kelley.^

Maines and Molseed's misunderstandings lead them astray more than once. Perhaps the most important instance is in tneir challenge to our claim that Bacharach and Lawler (1981) integrate Tedeschi's wark with conflict spiral theory. Maines and Molseed have misrepresented some of our statements here.^ The more important issue, however, is that structural analysis shows that the work of Tsdeschi and his associates does fall within the scope of the Bacharach and Lawler theory (and is therefore part of the integration Bacharach and Lawler perform).

First, the conceptual apparatus of both theories is similar. For example, the key explanatory mechanism in Tedeschi's theory is subjective expected utility. Bacharach and Lawler use that idea to generate one of their core assumptions and to derive four core propositions from that assumption. Clearly, part of Tedeschi's theoretical apparatus has been incorporated in Bacharach and Lawler's theory. Second, the set of conditions with which Tedeschi's theory is concerned constitute a narrower and simpler subset of the conditions with vtfiich Bacharach and Lawler's theory is concerned. Specifically, Tedeschi's theory focuses on bargaining situations in which only one party has the capability of threatening or punishing the other. Bacharach and Lawler sought to expand that concern to deal with multiple party capabilities. Third, using the same theoretical mechanism, they saw that the behavior exhibited in these more complex situations differs significantly from behavior in the simpler situation. Thus, in broadening the scope of the theory to deal with more complex situations they were (unsurprisingly) required to generate different predictions. If, however, Bacharach and Lawler decided at some point to deal with the less complex situation, they could easily derive Tedeschi's predictions simply by excluding the second party's capabilities from consideration.^

Thus, most of Maines and Molseed's challenge is based on erroneous assumptions about our analysis. They misunderstand its structural character and they ignore or misrepresent evidence which is consistent with it.

THEORY RELATIONS AND THEORY GROWTH

Questions have also been raised about the general adequacy of our analytic framework in accounting for theory growth. Maines and Molseed, for example, charge that our "version" of the "notion" of theoretical research programs is meaningful only in terms of our use of cases to illustrate its applicability, and that, because we apply that framework to some of our own work, our account of theoretical growth in that work is "tautological."

What sense (if any) the label "tautological" has in this context is not clear. It is just possible that Maines and Molseed do not understand the logic of our analysis. That analysis consists of two parts. First, we *explicate*, in *terms* of abstract and *general* *criteria*, what we believe to be the major types of relations that obtain between theories. We use these relations to describe the structure of major types of research programs: the linear program, the branching program, and the competing program. The second part of our analysis involves claims that the actual relations that obtain between the particular theories we consider (including some that we have developed) satisfy our abstract criteria for different types of theory growth.

CUr analysis may be challenged on at least two grounds. First, our explication of different relations between theories and the different ways in which theories grow might be challenged by identifying structural relations between theories ws have not considered or by providing alternative explications of the relations between theories we have considered. One could certainly present a reasonable argument on these counts, and we would be interested in seeing what alternative explications are put forth. Second, our analysis might be challenged by showing that, given our criteria for different types of theory relations, the actual relations that exist between the theories we analyze (including some we have developed) fail to satisfy these criteria.

Maines and Molseed do not challenge our explication of the different types of theory relations. Nor do they even attempt to show that the relations that exist between some of the theories we have developed fail to satisfy the relevant criteria. It may be that Maines and Molseed believe our account of growth in our own theories is "tautological" because, say, increasing the generality and deductive power of these theories were goals in developing them (goals which of course have to be realized). If that is the case, Maines and Molseed either misunderstand the two-part nature of our analysis or are using the term "tautology" (which is, after all, a standard term in logic) in an inappropriate manner.

Seicfrnan's criticisms are more specific; he believes we have excluded much theoretical growth that is central in the historical development of the discipline and included other growth that is either "decidedly of a low-level" or not in fact growth at all. Unfortunately, Seicknan never explicates the criteria he is using to evaluate theoretical growth. Although he does present examples of cases he believes we have inadequately analyzed, he says nothing about <u>how</u> or <u>why</u> our analysis fails. At one level, then, there is very little in Seidman's comments for us to respond to. Nevertheless, some clarification is in order.

First, we do not contend that theoretical development is excluded at the metatheoretical level. We believe there are several ways in which metatheoretical growth can occur. One of the most important ways is through the theories and programs a strategy spawns. Strategies change and develop in large part, we suggest, because they generate smaller, narrower, more focused theoretical entities which are more successful than their counterparts at this more limited level. After all, that is what strategies are about. They tell us how to build theories. It makes sense, therefore, to judge change and development in a strategy in terms of the success of its products.

In our view, then, each kind of theoretical activity plays a different role in the development of theoretical knowledge. Consider, for example, Davis and Moore's functional theory of stratification. Seidman suggests that this theory is more than just a set of empirically testable propositions; it is also a conceptual framework that gives coherence and meaning. While we basically agree here, we would state the point in the opposite manner: Whatever else is involved in Davis and Moore's theory, there is also a set of interrelated propositions, at least one of which can be subjected to empirical test.^ It is the comparative success (or lack of success) in the Davis and Moore of developing these propositions that is central in the evaluation of the functionalist strategy within which the Davis and Moore theory is formulated."^

Seidman believes we have included too much in our scheme. We do not agree. Our choice of relational types is based in part on an assumption that other criteria besides empirical evaluation are involved in accounting for theoretical growth. Thus, for example, we regard proliferation as a type of theoretical growth because it opens up new explanatory domains. Proliferation increases the breadth of a program in that it can render accounts of different kinds of phenomena using the same family of concepts and assertions. Of course, empirical evaluation is important, as in any theoretical move. However, expansion of the range of application of a program's core ideas is equally important.

We regard variation as a type of growth because it refines our knowledge of phenomena already within the range of application of a program. When a theory provides a basic and general account of some social phenomenon, any of a number of different kinds of specific explanatory mechanisms may be involved. Variation is enormously important in enabling the theorist to explore the implications of these different explanatory mechanisms; it allows the theorist to make close comparisons of subtle differences in thinking about the phenomenon to be explained. Just as proliferation increases the breadth of a program, variation increases its depth.

Competition represents growth because it highlights aspects of a phenomenon not dealt with in the original theory. It increases our appreciation of the complexity of the phenomenon to be explained. Further, because our understanding is more complete, competition often focuses debate and helps to establish explanatory boundaries between theories (based on the aspects of the phenomenon dealt with by each theory)

Seidman apparently accepts our characterization of elaboration as a type of theoretical growth. Ihis is not surprising, since elaboration is the pattern most widely understood and accepted by sociologists. Che of the major points of our analysis, however, was to show that elaboration is not the <u>only</u> form of theoretical growth.

Seme of our examples of elaboration are criticized by Seidman as less than compelling. In particular, he challenges our claim that Dahrendorf constitutes an elaborant of Marx. We believe our characterization is the most reasonable one. Dahrendorf increases the precision of Marx's theory in moving from a dichotomous to a continuous conception of class interest. Hs increases the scope of Marx's theory with his incorporation of multiple bases for class division (not just the economic basis). Both of these clearly constitute elaborations of Marx's argunent.

Although Seidman's analysis is very brief, he seems to see Dahrendorf as either a variant or a competitor of Marx ("Dahrendorf's theory...is an alternative to Marx's notion...."). While we remain convinced that our characterization is the most reasonable one, we believe the case is sufficiently ambiguous that we would be interested in more detailed arguments for a different characterization. It is interesting that, even if we are wrong in our characterization of the relation between Dahrendorf and Marx, our typology yields plausible alternatives that are consistent with our critic's character ization.^

Finally, Seidman also seems to accept our characterization of integration as a type of theoretical growth. He apparently has missed the argument, however, that integration is part of a system of relations. The form that integration takes depends directly on the type of relation that exists between the integrated theories: variation, proliferation, or competition. Thus, to fully understand the nature of different types of integration (which is one of the important consequences of our analysis), one must also understand the nature of variation, proliferation, and competition as types of growth—the very relations Seidman questions.

THEORY, METATHEORY, AND OBSERVATIONS

Our critics invoke what they seem to believe are well-established postpositivist arguments in challenging our analysis. Even "testability" as a criterion for assessing theory and theory growth is brought into question, presumably (according to Maines and Molseed) because there is debate among sociologists on its "relative importance," because "theory constructs its evidence" and because testability is "problematic." Furthermore, our critics appear to argue that in our analysis we are espousing some form of traditional positivism. These issues are of importance to sociologists (over and above this exchange) and merit some discussion.

Note first tnat there are many forms of postpositivism. Fbr example, the positions of Hanson (1969), Feyerabend (1962), Kuhn (1970) (whose Weitanschaungen views of science and theory are, according to Suppe, 1977, already "on the wane,"), Lakatos (1970), Tbulmin (1972) and Shapere (1977)—postpositivists all—differ considerably. Fbstposivitism is not all of one cloth. In addition, the postpositivist statements cited by Seidman are neither universally accepted arguments nor established truths. There exists a body of critical literature concerned with explicating these arguments (i.e. what do they really mean?), evaluating them, and in some cases challenging them (see Scheffler, 1967, Achinstein, 1968, and Suppe, 1973).

We regard our views of theory and science as postpositivist. At the same time, however, we reject the extreme relativism and subjectivism associated with certain postpositivist positions. The most coherent way to respond to our critics is to present our postpositivist views on the issues that have been directly and indirectly raised by than. (For related discussions of these matters, see Wagner, 1984, and Zelditch, 1985.)

Theory, data and testability. It is obvious that theory and data are related. Theory can be involved in determining what data is relevant, for what social situations the data is to be collected, what the data means, and, in conjunction with auxiliary assumptions, what the observational contexts for collecting data are. These different relations between theory and data, however, are quite complex; they depend on such things as how well developed the theory is (e.g. primitive or sophisticated), what the nature of the observational situation used by the researcher is (e.g. relatively unconstrained or highly controlled), and *hat the theorist's research focus is (e.g. the development of the theory or its application to a particular social situation) .

At present we are far from having a deep understanding of these matters. We do not believe, however, that our understanding is particularly illuminated by such general statanents as "observation is theorv-laden" or "theory constructs its evidence." Such statements tell us little about the complex relations that different types of theory may have to different types of observations and data.

Further, these statanents can easily be construed to say more than intended. Theory does not determine the outcome of data. Theory does not determine, say, that a particular pattern of numerical values, expected to support a specific prediction based on the theory, will in fact occur or be observed. If theory had this relation to data, there would be no need for observation. The theoretical enterprise would be complete with the formulation of the theory and the derivation of its implications. At sane stage, then, some one or more predictions of a viable theory is expected to confront data, data whose outcome is not dictated by the theory. It is in this sense that testability is a criterion for assessing any scientific theory. What the researcher does (e.g. the "relative importance" he or she attaches to the outcome of any specific test of a theory, particularly when the observations are anomalous) is a *separate* issue from the argument that testability is a criterion for scientific theory.[10]

Theory and *anomalous data*. Anomalous data (i.e. data that does not accord with the predictions of a specific theory) presents problems to which the theorist can respond with any of several theoretical moves. Che can question the design, execution and observational techniques involved in the relevant study. Che can introduce *ad hoc* hypotheses.[11] One can live with the anomalous data without making any changes in the theory.[12] One can make refinements, changes and modifications in the theory (e.g. eliminate a simplifying assumption, modify an existing concept or assertion, or add a new concept or principle to the formulation). The action chosen depends not only on the relation of the anomalous data to the theory, but also on the relation of the theory to other theories: Does there exist or can one construct a theoretical *variant* that will resolve the problems raised by the anomalous data? Does there exist or can one develop a theoretical integration that will account for the anomalous data?

These are the theory-to-theory relations we explicate in our analytical scheme. Such a scheme should help us understand what theoretical moves the theorist can make, given anomalous data. Theory change may not be the first move the theorist undertakes; theory change may also be undertaken in the absence of any need to deal with data and observations (e.g. as when some concept or principle is changed to simplify the theory) . This does not mean that the theorist is unresponsive to data and observations. In fact, the postpositivist emphasis on what wb think of as "theories in progress" has made us aware of the many *different* ways in which data and observations affect the development of theory. This perspective has made us particularly sensitive, for example, to the importance of "early successes" (i.e. corroborative observational outcomes) in promoting theory development in a particular direction, to the importance of observations and data that are relevant to a theory but are not yet within its explanatory domain in developing new extensions of the theory, and, of course, to the importance of anomalous data as a basis for modifying and refining theory. Thus, "testability" is seen to play, not a less important role in theory development, but a more complex and varied role than that delineated in different versions of traditional positivism.

In our view, to understand the rationality of the theorist's particular response to observations and data, we must understand the structure of theoretical research programs (i.e. the ways in which unit theories are connected to each other through such relations as proliferation, elaboration and variation). It is within this context that theoretical moves are made and growth occurs.

Metatheory and theory. Our critics take sharp exception to our separating unit theories from metatheory. Ihey argue that theory is "embedded" in metatheory, that theory "articulates" metatheory, that values and faith "construct" theory.[13] In fact, as we suggested earlier, we believe that metatheoretical elements play a crucial role in the construction of unit theories. Broad-based (and at times metaphorical) conceptions about the nature of the social actor, conceptual schemes and franeworks, and theoretical directives (anong other elements) guide the theorist in constructing theories. At the same time, we argue that such metatheoretical elements, which orient the theorist in the construction of the theory, should be distinguished from the product of that construction—the theory proper. To make clear our thinking on this matter, we briefly describe in terms of an example from our own work what we believe to be the relation of particular types of metatheoretical elements to unit theories.

As sociologists interested in social interaction, we hold some very broad and general conceptions about the nature of the social actor. Anong other notions these include the idea that the social interactant is an information processing actor. Also, in any particular situation of action, the actor can be confronted with different types of information that may be of significance to him or her. Further, we believe that the way the actor processes different types of information is crucial in determining the *way* he or she behaves in a given social situation. Ch the basis of these very broad and general conceptions, in conjunction perhaps with other metatheoretical notions and codified empirical information that is relevant to the particular domain of our concern (e.g. status, justice, social control), we can pose certain theoretical questions.^ what kind of information becomes salient to actors in a given type of situation? How do actors organize this salient information? How is the organized information translated into behavior? Under what conditions does the actor's behavior maintain or erode the structure of organized information in the situation? In answering these questions for a particular domain we formulate concepts and theoretical assertions and principles. The specific set of concepts and theoretical principles, as well as the assertions that are implied by these concepts and principles, constitutes our unit theory.

What is the relation between our broad-based conceptions about the nature of social actors and our unit theory? First, the broad-based conceptions, in conjunction with other elements, orient us in constructing a particular type of cognitive and behavioral theory. They are, however, distinct from that theory. For within these conceptions we can construct a large number of different unit theories for a given domain, say status, that are in accord with these same conceptions. We can also construct within the same broad-based conceptions different theories for different interpersonal processes.

In comparison to the unit theories that are formulated within them, these broad-based conceptions tend to be relatively stable. Although they have undergone some change and they have become more clearly articulated over time, there has been considerably less change in these ideas than in the unit theoretical products that have been produced in accord with them. Furthermore, these conceptions (and other such metatheoretical elements) are evaluated indirectly and in terms of criteria different from those used to evaluate unit theories. In the final analysis they are evaluated in terms of their utility as framework ideas, orienting principles, and directives in producing different types of unit theories. It is the unit theories, however, that are evaluated in terms of such criteria as empirical adequacy, generality, or explanatory power relative to other available unit theories.

In our original article we did not undertake the task of developing a refined analysis of the relation of the metatheoretical elements that are involved in orienting strategies to unit theories. Such an analysis would require (1) distinguishing the different types of elements in orienting strategies (e.g. values, broad-based conceptions about social actors and the social order, theoretical directives which are either substantive or methodological, and conceptual frameworks), (2) describing the different types of relations between these elements (since ve doubt that they can be ordered on a single dimension), and (3) describing the relations between these types of elements and init theories.

These are issues that will require study and analysis, lb fully understand these issues, it will be necessary for sociologists to study and analyze different examples and cases of theory construction as it actually occurs in different substantive areas. The aphoristic statements of our critics, however, do not constitute such analysis. At best such statements obfuscate the existing distinctions between unit theories and metatheories. At worst they suggest that we cannot distinguish further between unit theories and the metatheoretical elements of orienting strategies. They suggest also that such terms as "articulates" or "embedded" in fact tell us all there is to know as to how these elements are related to each other. We think that sociologists can do considerably better than that.

GLOBAL THEORIES AND THE INTELLECTUAL TRADITION OF SOCIOLOGY

Finally, our critics accuse us of narrowiess, of (in Seicinan's terms) "intellectual emasculation" of the discipline. This is apparently because our critics believe that our approach (again in Seicinan's terms) "excludes from consideration those global theories or overarching paradigms which are at the center of the structure and dynamics of sociology."

We have no desire to exclude "global" theories from our sociological concerns. However, we do not believe it is appropriate to assume that "global" refers to a single kind of theoretical entity.

Insofar as "global" refers to broad theoretical schools or traditions, we believe they involve a mixture of different types of theoretical elements. Thus, we do not think in terms of a single symbolic interactionist theory but of theories within the symbolic interactionist tradition (e.g. Scheff's labelling theory of mental illness). Similarly, we do not think in terms of a single functionalist theory but of, say, Davis and Moore's theory of stratification as an element of the functionalist tradition. We do not believe it is fruitful to treat these broad theoretical schools as if they represented unitary theoretical formulations.

Insofar as "global" refers to some broad and perhaps vaguely formulated theory, we are interested in capturing the ideas, insights and principles that are involved in such formulations. Vfe think this can be best accomplished by making the concepts in these formulations explicit and by articulating the theoretical principles they contain. Evaluation, refinement and theoretical development are thereby enhanced. (See TUrner, 1982, and Stinchcombe, 1963, for instances of this kind of enterprise.)

Finally, insofar as "global" refers to the metatheoretical elements that are involved in theory building, we are concerned writh understanding the different types of elements that are involved in orienting strategies and how they affect our theoretical formulations (as we believe they do). Our objective is not to deny the operation of metatheoretical elements but to recognize their different forms and to understand their roles.[1]®

We are not interested in eliminating the intellectual tradition in sociology but, on the contrary, in profiting by it and using it!

FOOTNOTES

1. We gratefully acknowledge the contribution of Edward J. Lawler, Barry Markovsky, Henry Walker, Murray Webster, Jr., and Morris Zelditch, Jr. to our consideration of the issues discussed in this paper.

2. In support of their claim, Maines and Molseed cite Bacharach and Lawler's (1981:117-118) statement that "one could even argue that, at present, there is no theory of deterrence." This quote is taken out of context. The relevant paragraphs show that Bacharach and Lawler see both deterrence and conflict spiral as implicit theories, only partially explicated in prior literature. What they are saying at base is that a reader will not find the core proposition they state presented neatly somewhere in prior work. All that exists are fragments they are synthesizing. Nevertheless, coherent theoretical ideas are present that at least implicitly comprise a theory. (This interpretation is also supported by recent statements in Lawler, 1985a,b.)

3. Maines and Molseed are very selective in their review of the relevant literature. They ignore in an obvious manner information from citations that is consistent with our structural analysis. For example, although Deutsch and Krauss (1960) do not cite Thibaut and Kelley (1959), a summary of their work published two years later (Deutsch and Krauss, 1962) does. So too does Deutsch's 1973 book The Resolution of Conflict. Clearly, Deutsch and Krauss were aware of and made some use of Thibaut and Kelley's work (although, of course, one must use information other than citations to determine what the nature of that usage was). The same is true with respect to the work of Tedeschi and his associates. While the Horai and Tedeschi (1969) paper does not cite Thibaut and Kelley, later work does. In fact, Tedeschi's 1973 book

Conflict, Power and Games (with Schlenker and Bonoma) has at least ten citations to Thibaut and Kelley.

4. We indicate that Tedeschi and his associates have developed an account of part of the deterrence process. We state that Bacharach and lawler integrate deterrence theory (including much of the Tedeschi work) with conflict spiral theory. In other words, we claim that Ttedeschi's work contributed to the integration, not that it is the only (or even the single most important) deterrence element in that integration.

5. Maines and Molseed base their challenge here primarily on a quote from Bacharach and Lawler vrf-iich states that Tedeschi's work is outside the scope of their theory. Maines and Molseed have, in fact, misrepresented Lawler*s position on this matter. Lawler believes it is useful to distinguish the scope of Tedeschi's theory from the focus of his research. The Tedeschi theory does not explicitly state its scope and presumably could be used to deal with bilateral threat as well as unilateral threat. However, almost ail of the Tedeschi "line of research" deals exclusively with unilateral threat. It is Tedeschi's line of research, more directly than the range of problems to which his theory is applicable, that Lawler sees as outside the scope of the Bacharach and Lawler theory. See Lawler, 1985b.

6. Although a discussion of this issue would take us far afield from our immediate concerns, ve want to make it clear that we do not accept the idea that each of the propositions or principles in a theory must be directly testable.

7. Of course, the evaluation of a strategy depends on the success of many different programs of theory, not just one. Progress in the exchange strategy, for example, depends on progress in the conflict spiral program, the deterrence program, the equity program, and many, many others.

8. Seidman argues that variation and competition are best understood as instances of Kuhnian preparadigmatic conflict. We do rot accept Kuhn's sharp distinction between paradigmatic and preparadigmatic status. We believe that multiple paradigms often exist even in mature science, Note that, although The Structure of Scientific Revolutions is clearly a path-breaking work, Kuhn's depiction of the growth of scientific knowledge has been subject to a great deal of critician. See, for example, the papers in Lakatos and Musgrave (1970).

9. Of course, this probably would not satisfy Seicfcian, since he does not see either of the other possible characterizations as representing patterns of growth.

10. Although his views are a mixture of different postpositivist positions, Kuhn does argue for testability as a criterion for assessing the growth of a scientific theory. He argues that, if we compared an early theory with a relevant later theory, we could design a list of criteria that "would enable the uncommitted observer to distinguish the early from the more recent theory time after time." This list would include: "accuracy of predictions, particularly quantitative predictions, the balance between esoteric and everyday subject matter, the number of different problems solved" and also "such values as simplicity, scope, and compatibility with other specialities." See Kuhn (1970:206).

11. There are, of course, limits to the use of the strategy of ad hoc hypotheses, limits which depend on the relation between the relevant theory and the anomalous data. With Popper, one wonders about the structure and status of the set of ad hoc hypotheses that would be necessary, given Newton's dynamics and theory of gravitation, to account for observations such as "the orbit of some planet is approximately rectangular" or "the velocities of some planets decrease (rather than increase) when approaching their perihelion."

For a relevant and interesting exchange between Lakatos and Fbpper on this issue, see Fbpper (1974).

12. lb live with anomalous data or observations does not mean that these observations have been forgotten or expunged and that the status of the theory is unchanged. Such anomalous data can continue to be salient to the theorist, and may even come to play a key role in the development and corroboration of a new theoretical formulation. See Pais' (1982) description of the role that accounting for the precession of Mercury's perihelion played in the development of relativity theory.

13. Msines and Molseed attempt to demonstrate the inseparability of theory from metatheory with an illustration from our own work. In particular, they characterize a debate on the relative merits of status characteristics theory and a "styles of behavior" theory by Lee and Ofshe as an "instance of an ontological debate" at the level of theory (see Lee and Ofshe, 1981; and Berger and Zelditch, 1983). The basic facts of this case are straightforward. Berger and Zelditch offer a methodological critician of an experiment carried out by Lee and Ofshe. Among many other criticisns is the observation that one stimulus manipulation that Lee and Ofshe associate with styles of behavior, the degree of formality of dress, could easily be associated with the status variable they used, differences in occupational status. (Is this the ontological move?) Berger and Zelditch then present an alternative theoretical formulation to that given by Lee and Ofshe. This alternative leads to predictions that differ from those of Lee and Ofshe and which are subject to experimental test. (See Ridgeway et ai., 1985, for further information on the Berger and Zelditch argument; and TUziak and Moore, 1984, for recent experimental findings bearing on this debate.)

"Chtology," as it is commonly understood, deals with questions of "the order and structure of reality in the broadest possible sense..." (Angeles, 1981; see this reference also for related definitions). We do not believe this term is applicable to the debate we have described. That debate consists of (1) a methodological criticisn of an experiment, (2) an alternative theoretical formulation, and (3) predictions differing from those of Lee and Ofshe that can be subjected to experimental test. Maines and Molseed are either using an idiosyncratic definition of ontology vdnich makes it applicable to an unlimited set of considerations or they have failed to understand the nature of this debate.

14. Metatheory-to-theory and theory-to-data relations are but one side of the story. Data and observations, which can be organized and conceptualized as abstract empirical generalizations, can play a crucial role in defining theoretical problems and in constructing theoretical formulations that constitute solutions to these problems. While such generalizations are not, of course, formally induced, they are constructed from factual bases of information consisting of observations and data.

15. Such study will probably show that the relation between metatheory and unit theories is <u>not</u> unidirectional. Nat only do metatheoretical elements influence the construction of unit theories, but metatheoretical elements (e.g. conceptual frames, problem solving strategies) also are developed from the theorist's experiences in constructing such theories. See Berger, Wagner, and Zelditch (1985).

16. We have <u>never</u> claimed that attention to or understanding of the classics is wasted effort. It is perhaps by now not surprising that Maines and Molseed's quote to that effect is taken out of context. In fact, the source from which this quote is taken argues that, because we spend so much of our time debating issues about the classics that are unresolvabie in any direct sense, we often do not build the theoretical structures suggested by the classics. What is largely wasted effort then is <u>not</u> our attention to the

classics, but the way in v^ich ws attend to them. lb see how this argunent was actually presented, see Wagner, 1984: Chapter 2.

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