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Reflections on Classes

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Reflections on *Classes*

By Erik Olin Wright

Editors' Introduction

Karl Marx, like August Comte, thought the study of society to be properly a "science." And yet, as increasing numbers of sociologists have repudiated Comte's positivist tradition in the past decades, Scientific Marxism has lost ground to hermeneutic and other traditions. It has become, almost, a term of insult among critical leftists who equate it with some crude and naive structural determinism. Seen in this context, Erik Olin Wright's *Classes* is an ambitious project—not just to deal with the Marxist problem of the continued existence of the middle classes, but also to refurbish the somewhat tarnished reputation of Scientific Marxism in an era in which the very meaning of "science" is debatable and often debated.

In what follows, the editors, in cooperation with other graduate students in the Berkeley Department of Sociology put a series of methodological questions to Wright, who has just joined the faculty here. Michael Burawoy, also a member of the faculty at Berkeley, provides a critique of Wright's methodology and presents an alternative. Wright then presents a short rebuttal. (Methodology is considered here at an almost meta-methodological level: not so much "how to," but instead the *study* of "how to.") The background for much of this discussion is found in the post-positivist methodological inquiries of the last three decades. To attempt a summary of this literature in 25 words or less, we might say that authors such as Thomas Kuhn, Imre Lakatos, Paul Feyerabend, and Michel Foucault—plus others too numerous to list—have tried to cast doubt on the once-unproblematic relations between theories and facts.

Kuhn, for instance, has argued that scientists work within paradigms which limit the types of evidence they may consider. Though Kuhn himself did not apply his theory of scientific paradigms to the *social* sciences, sociologists have appropriated his ideas to explain, and occasionally to justify, the blinders which every sociological tradition forces them to wear. Lakatos has adopted the notion of research programmes, and has added the optimistic argument that science can be assured of eternal progress: the programmes which become dominant do so because they have dramatic success at uncovering and explaining facts which the earlier programmes had not even looked for. Feyerabend has called into question the very

idea of an independently existing fact: in his view, facts are created by theories. One cannot use a telescope to discover facts about a star, for instance, until one holds the belief that stars are susceptible to accurate examination by telescopes. And Foucault has called attention to the power dimension involved in the scientific production and analysis of facts. The object of study is often, first, objectified, and second, subjected to analysis for the sake of control. Here, evidence is created, and it is created for the purpose of subjugation.

This thumbnail sketch has touched on several points which will be put to Wright in the following questions. And Wright is particularly qualified to discuss these issues: though survey research is his primary method of analysis, he has displayed in *Classes* a sensitivity to more theoretical issues in methodology. An entire section of his book (Chapter Two) is devoted to explicating the limits of the Marxist paradigm within which he intends to work. Wright recognizes the problems in deriving theories directly from facts (p. 20), and he makes explicit his methodological stance: "that empirical adjudications are always between rival concepts or propositions, not directly between a proposition and the 'real world' as such." (p. 189)

Though *Classes* shows clearly the attention Wright has paid to the relation between facts and theories, some issues remain about the relation of one theory to another. For this reason, we will start with a series of questions on this subject, before moving on to questions about the proper use of scientific evidence and about the extrascientific implications of the scientist's method.

[the editors]

I. THEORY VS. THEORY

1.1 *In Chapter Two of Classes you list six "conceptual constraints" within whose limits the Marxist must operate. But other Marxists, certainly, would come up with different lists. For instance, one school might emphasize the importance of class struggle in determining class consciousness. Others might take ideology as a separate factor. And so on, as you admit (p. 27). By what criteria do you choose your Marxism?*

To answer this question I need to first very briefly review the context in which I elaborated the list of conceptual constraints on the concept of class structure within Marxist social science. In order to study anything, we need concepts—the categories within which we ask questions, observe the world, organize our possible explanations. A

radical empiricist would claim that the only fundamental constraint on the formation of concepts is the way the world is. All anti-empiricist methodologies argue, in various ways, that our concepts are also constrained (and in some versions, exclusively constrained) by the theories within which they function. These theories, in turn, are constructed by linkages of various sorts among the very concepts which the theory constrains.

The central task of *Classes* is to solve a problem of concept formation: how to produce an adequate concept for the “middle classes.” If one adopts an anti-empiricist methodological stance towards the process of concept formation, then it is essential to specify the theoretical conditions which any legitimate concept of the middle class must fill (where, by “legitimate,” I mean that the concept is capable of functioning in the theory in question). Thus the attempt at elaborating a list of conceptual constraints.

My claim in Chapter Two of *Classes* is that the following six constraints on the concept of class structure are common to most varieties of Marxist theory:

1. Class structure imposes limits on class formation, class consciousness and class struggle.
2. Class structures constitute the essential qualitative lines of social demarcation in the historical trajectories of social change.
3. The concept of class is a relational concept.
4. The social relations which define classes are intrinsically antagonistic rather than symmetrical.
5. The objective basis of these antagonistic interests is exploitation.
6. The fundamental basis of exploitation is to be found in the social relations of production.

The first two of these constraints define what *explanatory* tasks “class structure” is meant to accomplish; the last four specify interconnected properties of this concept if it is to accomplish these tasks. If one were to ask, “what makes a Marxist concept of class structure ‘Marxist?’”, the answer would be, “the concept conforms at least to these six conceptual criteria.”

I am not claiming, it should be emphasized, that these six conceptual criteria define what is Marxist about Marxist theory in general, but simply what is Marxist about the concept of class structure. And I am also not saying that all Marxists would *limit* the conceptual constraints on class concepts to these six criteria—additional constraints would undoubtedly be present in certain traditions of Marxism. There may even be some additional constraints which all Marxists share, although I have not been able to figure out what these might be.

In your question you point out that "other Marxists would come up with other lists. For instance, one school might emphasize the importance of class struggle in determining class consciousness. Another might take ideology as a separate factor." This is undoubtedly true, but the issue is not whether other Marxisms would emphasize additional factors, but whether they would reject any of these constraints. Do any Marxists deny that class structures must be defined relationally, that these relations are antagonistic and exploitative, and that exploitation is rooted in the social organization of production? All that is being claimed is that these constraints are in fact common to Marxist conceptualizations of class structure, and therefore any *Marxist* concept of the "middle class" must, at a minimum, conform to these criteria.

Now, three kinds of arguments could be raised against this particular list. First, it could be argued that there are no common criteria that unite the diverse concepts of class structure across all Marxisms. Some Marxisms, indeed, might even reject the concept of class structure itself. This is a reasonable objection, but it really amounts to a rejection of the claim that there is any conceptual unity whatsoever among self-styled "Marxist" theorists, at least around the concept of class. It implies that the word "Marxist" has been appropriated by radically incommensurate theories. This criticism does not, however, undermine the legitimacy of the inventory of conceptual constraints as such, but merely its identification with some historical usages of the label "Marxist."

Second, it could be argued that all varieties of Marxist "theory," like most other existing social theories, are so far from constituting coherent, systematic scientific paradigms, that it is impossible to specify meaningful conceptual constraints on any process of concept formation. Social theories, it could be argued, are more or less chaotic collections of terms, intuitions and specific explanations rather than coherent abstract systems of thought. Even Marxism, which has aspirations to be such an abstract framework, contains so many disjointed and contradictory elements that it is best thought of as a loosely coupled discourse than a coherent scientific system of concepts. If this is correct, then the elaboration of a list of conceptual constraints such as the list which I propose should be viewed primarily as an attempt at *producing* order within the theoretical space of Marxism rather than simply *discovering* the underlying order which already exists.

Finally, one could accept the legitimacy of the enterprise of constructing a list of formal constraints on the concept of class within Marxism, and yet argue that this particular list is not a proper specification of these constraints. This could, of course, be a valid criticism, but the burden in such a criticism is showing what

alternative set of constraints are constitutive of the Marxist theory of class. I continue to believe that as a matter of *empirical generalization* about “actually existing Marxisms,” these criteria are broadly common to Marxist concepts of class structure and that most of these criteria are shared by Marxist theorists who in other respects would sharply disagree on theoretical issues. Contrary to what you suggest in your question, I believe that *Marxist* theorists who emphasize ideology and class consciousness still believe that class *structures* are constituted by antagonistic exploitative relations rooted in production.

To assert that virtually all theorists who would call themselves “Marxist” as a matter of fact explicitly or implicitly operate under these conceptual constraints does not mean, of course, that specific Marxist theorists would not quibble with some of the details of these six criteria. Some theorists would certainly object to the expression “historical trajectories of social change” in the second constraint on the grounds that this suggests, perhaps, a unilinear, deterministic path of historical development. They would agree that class structures define fundamental qualitative lines of demarcation between types of societies that have occurred in history, but they would reject any strong claims about these types being arrayed in any logically ordered temporal sequence, as suggested by the expression “trajectory.” Other theorists would question the claim that class structures impose *limits* on class formation and class struggle in the first criterion. Such limits, many Marxists have argued, are imposed by the totality of social relations, not simply class relations. While all Marxists would agree that class struggles do operate within some kind of social relationally imposed limits (struggles are not just a matter of subjective will on the part of people), and they agree that class relations are *part* of the limit-imposing process, many would not want to simply assert that class structures as such impose these limits.¹ And certainly there would be intense debate over the precise *content* to be put on the terms in any of these criteria: “relational” in constraint #3, “antagonistic” in #4, “exploitation” in #5 and “production” in #6. The point is not that there would be complete agreement on all of the details of these criteria or on the meanings of all of the concepts contained within them, but that they in practice define the conceptual terrain upon which debates over the theory of class structure are waged within Marxism.

What I have said so far concerns the methodological standing of these six criteria for class structural concepts. The last sentence in your question, however, raises a broader issue: “by what criteria do you choose *your* Marxism?” While I may be correct that most Marxists in fact would accept these six constraints on the concept of class structure, this does not answer the question about the criteria I use to justify my general theoretical posture within Marxism. Much of my

discussion of the remaining questions you have posed will, in effect, constitute an answer to this broader question, but I will state in abbreviated form my basic position here.

All theoretical choices derive their meaning from the "contrast space" in which they occur. "Choosing" a variety of Marxist theory is a contrast with alternative Marxisms, and the criteria implicit in the choice depend, in part at least, upon which alternative is being considered.² As I see it, my particular brand of Marxism is a result of a sequence of three basic choices within the array of historically available Marxisms. Each choice involves different criteria.

Choice 1. Scientific vs. "nonscientific" (perhaps: anti-scientific) Marxism. I do not pose this initial choice as scientific vs. critical Marxism (as does Gouldner, for example), because I believe that scientific Marxism is a variety of critical theory: it attempts to provide the scientific foundations for a nonarbitrary immanent critique of capitalism. The first choice, therefore, is not between science and critique, but directly a choice over the status of Marxist theory as a scientific project. What do I mean by this? Fundamentally I mean that the task of Marxist theory is to produce *explanations* of *real* phenomena that exist in the world independently of the theory.³ Whether or not imperialism is a real cause of deepening underdevelopment in parts of the Third World depends upon how capitalist penetration actually works, not upon the categories of the theory of imperialism. Whether or not the sexual division of labor around childrearing is a real cause for the reproduction of male domination depends upon how mechanisms in psycho-sexual development actually work, not upon the discourses of our theories of psycho-sexual development. Whether or not we have *knowledge* of these mechanisms of underdevelopment and reproduction of male domination, however, depends upon the availability of adequate explanatory theories (I will discuss the problem of "adequacy" in answers to subsequent questions), but the mechanisms themselves have a real existence independent of this knowledge.

The first choice among Marxisms, therefore, is whether or not one wishes to embark on the difficult path of actually producing explanations of the world. The alternative is to restrict one's efforts to producing *descriptions* of the world, *interpretations* of the world, or *philosophical commentaries* about the world. There may be no guarantees of success in this explanatory enterprise, or even of knowing with certainty whether or not one has been successful (i.e. there is no absolute way of knowing when one has produced knowledge), but the first criterion for my choice of a type of Marxism is that it attempts to produce explanations.

Choice 2. Analytical vs. dogmatic Marxism. This is, undoubtedly, a highly contentious way of posing the second choice. By analytical

Marxism I mean this: the heart of all scientific theory is the dual process of elaborating concepts and deploying them in the construction of theories. Analytical Marxism insists on the necessity of laying bare the assumptions that underlie these concepts and spelling out as clearly and systematically as possible the steps involved in linking them together within a theory. "Dogmatic" Marxism, in contrast, defends its use of concepts through a variety of other forms of argumentation: citations from canonical textual authority (typically through Marxiological argument); arguments based on ulterior political justifications (a particular concept is rejected *simply* because it is politically "undesirable" without further argument); appeal to vague and imprecise abstractions whose content is never systematically elaborated (such as the common use of "dialectics" to defend Marxist concepts). To be analytical in this sense does not imply a commitment to particular substantive positions, but to the importance of breaking down concepts, making explicit and systematic distinctions, defending the fine points of definitions, etc.⁴

Choice 3. Empirical (but not empiricist) vs. theoristicist Marxism. I believe, for reasons which will become clearer in my responses to subsequent questions, that in order to have any confidence that the explanations produced within Marxist theory are in fact explanatory of anything, they must be produced in articulation with empirical research agendas. Analytical precision and coherence alone does not ensure explanatory power. Neither, of course, does empirical research alone. For Marxist explanations to advance, the two must be combined. The word "combined" is fraught with difficulties and ambiguities, but these difficulties are not so severe as to make theoretical advance impossible. In any event, this ambition is embodied in the third dimension of choice.

The list of six constraints on the concept of class structure can't be viewed as somehow methodologically derived from these three choices over the type of Marxism which I pursue. These six constraints all involve substantive claims about class theory, and substantive claims can never be logically derived from methodological principles.⁵ Nevertheless, the effort at producing such a list can be seen as motivated by these general methodological commitments. This list is meant to specify in an analytically explicit way what class structure is meant to explain.

*

1.2. In Chapter Five you "adjudicate" between your tri-axial, exploitation-centered theory of class and other theories of class. Your theory wins, but the theory belonging to the person setting the terms of debate always seems

to win. Isn't this like playing prosecutor and judge at the same time—and is there any way to adjudicate fairly between rival theories?

First, a point of clarification: the adjudication at issue in chapter five is actually between alternative *concepts* of *class structure* rather than alternative theories of classes. Of course, the presupposition of concept-adjudication is that these concepts fit into some general theory, but in this particular case I made the assumption that the contending concepts all fit into the *same* general theory. This is important, because the task of concept adjudication within a common general theory is much easier (if still often difficult) than the adjudication between contending general theories. When concept adjudication occurs *within* a general theory there is an agreement about what the concept in question is meant to explain (this is what it means to say that they are contending concepts within the same theory); the debate is over the appropriate elaboration of the concept for it to accomplish this explanatory task, not over the object of explanation itself.

A theorist engaged in the task of adjudication may be prosecutor and judge at the same time, but she or he is not also the jury. The jury is the intellectual community engaged in the theoretical debate in question, and the “verdict” of such juries involves examining the cases presented by contending attempts at adjudication. As I point out in my methodological discussion of the problem of adjudication, the results of such adjudication are usually ambiguous: some concepts appear more coherent theoretically, but less consistent with observations than their rivals; some concepts appear consistent with some theoretical constraints, but not others; etc. It is because of these ambiguities that debates over given conceptual definitions can go on and on. But this does not mean that such debates can never be resolved, that certain rivals can never be eliminated.

Within the constraints of a given theory, adjudication can be fair or unfair, honest or dishonest. Dishonest adjudication occurs when the theorist surveys a range of alternative empirical results and only reports those that are consistent with the desired verdict. Dishonest adjudication occurs when concepts are operationalized in ways that privilege the desired outcomes. But such biases are not inherent in the adjudication process. In the case of the conceptual adjudication in *Classes*, there are a number of results which I present which run counter to the conceptual position I am trying to defend. And in a recent paper on the transformation of the American class structure, I show that an earlier adjudication between Marxist and post-industrial theories of changes in class structures is not supported by evidence from the 1970s.⁶ In any case, the real safeguard to fairness is not the scholarly integrity of the investigator, but the openness

of the challenges from alternative views and the intellectual capacity of the “jury” to juggle the ambiguities of contending adjudications.

*

1.3. *All academics, it is probably fair to say, try to come up with something new. But Classes, with its reconceptualization of classes and its appropriation of statistical procedures, seems to be quite a break from the Marxist tradition in which you place yourself. Do you feel that you are founding a new subtradition within Marxist thought—and if so, what are the implications of such a position?*

My work in *Classes*, and my earlier work in empirical class analysis, are by no means the first examples of relatively sophisticated uses of statistical analyses by Marxists.⁷ Nor does my preoccupation with sorting out the underlying assumptions and logic of key concepts within Marxism, in this case the concept of class, represent a novel innovation in Marxist theory. What is probably true, however, is that *Classes* and the earlier work of which it is an extension are relatively unusual in trying to do both of these: to aspire to analytical precision in the elaboration of concepts and statistical rigor in empirical investigation.

The biographical roots of this particular gestalt are to be found in the intellectual and academic context in which I first seriously engaged both Marxism and sociology. As a radical intellectual in the early 1970s I was an enthusiastic participant in the renewal of Marxist theory, first in terms of the problem of the state and subsequently the problem of classes. But I was also an enthusiastic budding academic and wanted Marxist ideas to have an impact within sociology as a discipline. As a missionary proselytizer I wanted to “save sociology” from the sins of bourgeois thought as well as to “save Marxism” from the sins of dogmatism.⁸ The joining of statistical methods with conceptual rigor seemed the most powerful way of accomplishing these two goals.

Does this combination constitute the basis for a new subtradition within Marxism? If it is part of a subtradition, I would not characterize this so much as the joining of *quantitative techniques* and conceptualization, but of systematic empirical research and conceptualization. I do not in any way privilege quantitative analysis over qualitative data as bases for empirical investigation. The kinds of data used to engage empirical problems should be strictly determined by the questions being asked and the evidence needed to discriminate between alternative answers. What is characteristic of the empirical research in this “new subtradition,” then, is not so much its reliance on statistical procedures as such, but its stress on the importance of

formulating explicit causal models of *variations* in the theoretical objects of the research. The actual research can take forms as diverse as quasi-experimental designs of comparative qualitative case studies, as in Burawoy's work in industrial sociology, or multivariate quantitative data analysis. The critical point is that the causal models (or what I have called in a more Marxian voice "models of determination") are explicit and that they are deployed to explain variations.

*

II. THEORY AND EVIDENCE

2.1. You argue that concepts are constrained by theoretical frameworks (p. 20) and that data are constrained by "real mechanisms in the world" (p. 58). But while you elaborate the conceptual constraints, the empirical restraints on data remain unclear. What are these "real mechanisms" and how do they constrain the data?

To claim that data are constrained by real mechanisms in the world is to reject the idealist claim that "facts" are *entirely* produced by "discourses." A radical idealist view of data is based on three correct theses:

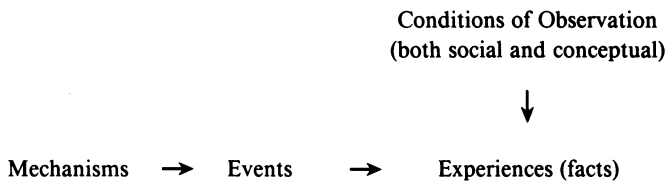
1. Our theories determine what questions we ask.
2. Our conceptual frameworks determine the categories in terms of which we make our observations and thus determine what we can see.
3. There is therefore no such thing as theory-neutral or concept-neutral facts.

From these correct premises, however, an unjustified conclusion is drawn: facts are wholly constituted by theories. While concepts may determine what we *can* see (the range of *possible* observations), it does not follow from this that they determine what we *do* see (the actual observations within that range). The "transcendental realist" argument against idealism is that within the range of possible facts determined by our concepts, real mechanisms in the world, mechanisms that exist independently of our theories, determine our actual observations.⁹ It is in this sense that data is constrained by the world, not just by our theories of the world.

A realist claim of this sort is based on a distinction between three domains of "reality," which Bhaskar calls the domain of the *real*, of the *actual* and the *empirical*, to which correspond three ontological categories: *mechanisms*, *events* and *experiences*.¹⁰ Bhaskar argues that mechanisms should be seen as generating events, and these events, in conjunction with various conditions of

perception/observation, in turn generate our experiences (i.e. observed “facts”). In a simple way this can be diagrammed as follows:

Figure 1
Logic of Production of Facts in a Realist Philosophy of Science



The claim that experiences are not identical to events and mechanisms is the basis for the rejection of empiricism; the claim that experiences are shaped by mechanisms and events (and not entirely explainable by conditions of perception) is the basis for the rejection of radical idealism. One of the pivotal consequence of this position in the philosophy of science is that it helps to explain how factual anomalies are produced within theories. If theories were entirely self-confirming, if they determined the actual experiences of the observer, then anomalies would not occur: theories could produce facts entirely consistent with the theories. Observational anomalies are possible because the real mechanisms in the world that exist independently of our theories shape our actual observations.

Question 2.1 asks “what are these ‘real mechanisms’ and how do they constrain the data?” The answer to that question, of course, depends entirely upon the substantive problem under consideration. The real mechanisms in the formation of class consciousness are different from the real mechanisms in the production of economic crisis. And the specific ways in which they constrain data also varies with substantive problem. It is the central task of scientific theories—at least if one adopts a realist perspective on theory construction—to try to understand these mechanisms.

Let me give a specific empirical example of these issues to try to add further clarity to the problem of the interaction of real mechanisms and conceptual categories in the production of “data.” Let us look at the problem of class formation, specifically at the formation of what might be termed ideological class coalitions. Class *structures* can be viewed as a relational terrain upon which multiple possible class formations can be historically created. One of the tasks of class analysis is to study the process by which these possibilities are actualized. One kind of data that is relevant to observing class

formations is the distribution of ideologies across various categories in the class structure. When the people in different class locations share similar ideological configurations, we can say that they are part of a common ideological class coalition. Now, to explore this set of issues several critical conceptual tasks have to be accomplished: we must abstractly specify what we mean by class structure and by ideology; we must operationalize these abstract concepts into observational categories; and we must gather observations using those categories based on those abstractions. For argument, let us suppose that we have adopted the class structural framework advocated in *Classes*. This implies that the class structure can be represented as a multidimensional matrix of locations determined by the distribution of exploitation-generating assets. Figure 2 indicates how I will represent this matrix for present purposes.¹¹

Figure 2
Matrix of Class Locations within the
Class Structure of Contemporary Capitalism

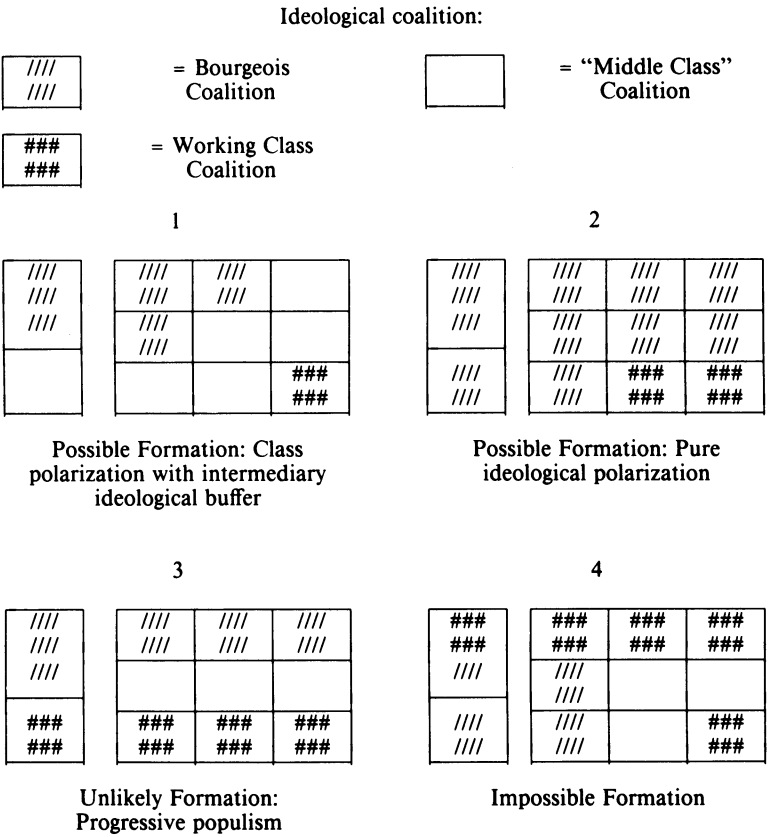
OWNER		WAGE LABORER			
		Skill Assets			
		+	0	-	
Capi- talist	Expert Manager			Nonexp. Manager	+
					0
Petty Bourg.	Nonmgr. Expert			Worker	-

Org.
Assets

Now, on the basis of the logic of this conceptualization of class structure, it is possible to specify a range of possible ideological class formations that could be built on this structural foundation. Several of these are illustrated in Figure 3.

Let us suppose that after we elaborate and operationalize our concepts and conduct our observations, we obtain an empirical map similar to Model 1: that is, workers and capitalists are ideologically polarized with an ideological buffer “middle class” coalition in between. The realist framework for understanding the production of these data implies a particular agenda for someone who is skeptical about the interpretation of these “facts.” The burden on such a critic is to propose an alternative explanation for the results, for the “experiences” represented in the empirical map. The critic has a *double* task: first, to elaborate an alternative account of underlying

Figure 3
Formable and Unformable Class Formations
in Contemporary Capitalism



mechanisms, and second, to explain how, with those alternative generative mechanisms, these results are produced by the conceptual framework of the observer. That is, the critic needs to present a model of the *conditions of possibility* for these observations given an alternative theory of generative mechanisms.

For example, let us suppose someone objects to this asset-based exploitation model of the relation between class structure and class formation and argues that ideological formations are not the result of such mechanisms at all, but of the strategies of political parties. Parties, of course, operate under theories, and if party leaders *believe* that something like Model 1 in Figure 3 above explains ideological proclivities, then they may adopt strategies which in fact produce these results. Party strategies may generate self-fulfilling prophecies:

if the leadership of socialist parties believes that only workers are amenable to socialist ideas, and organize their mobilization drives accordingly, then only workers will be prosocialist ideologically. But—it might be argued—workers actually have no greater inherent predisposition to accept such ideas than do people in any other class category. The same argument holds for the strategies of parties supporting pro-capitalist ideologies or any other kind of ideology. The distribution of ideologies in a population, then, would not be the result of any inherent or natural susceptibilities of people in different class locations to particular ideologies but of the intersection of the diverse strategies of various parties (and other ideology producing institutions).

The implication of this alternative view is that *if* we could find a political environment in which a socialist party tried to mobilize capitalists and managers and workers, whereas procapitalist parties mobilized experts and petty bourgeois, then in fact the pattern represented in Model 4 in Figure 3 could occur. The only reason it does not occur empirically is because parties falsely believe that people in different “objective” locations are likely to be more responsive to certain ideologies than to others.

This criticism is framed in terms of the requirements of realist theory of science: it not only poses an alternative mechanism, but explains the conditions of possibility for the empirical observations. Figure 4 illustrates the explanatory shift represented by this criticism.

Figure 4
Competing Models of Consciousness Formation

Initial Model:

limits

Class structure → patterns of ideological class formation

Alternative Model:

Empirical map of
class structure

↗

Ideologies of Class → Party strategies → ideological class formation

The initial theory posed a simple relationship between class structure and class consciousness: class structure imposes limits of possibility on ideological class formation. The challenging model asserts that the empirical association between class structure and ideological formations is spurious: ideologies of class explain *both* the empirical

map of class structure and patterns of ideological class formation (via the intervening mechanism of party strategies). Furthermore, this kind of realist critique of the initial model poses a quasi-experimental design for adjudicating the contending claims: what we need to find is a society with the same basic class structure but with parties targeting radically different kinds of people for recruitment. Of course, the fact that it is impossible to conduct the experiment means that it will be very difficult to resolve the debate. Indeed, this is partially why debates in social science are often so protracted. But the criticism still recognizes both the existence of real mechanisms and the mediation of conceptual frameworks in the production of “facts.”¹²

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2.2. Classes presents a Mertonian balance between theory and empirical research. In fact, the book seems to be an exemplary model of “logical positivist” scientific inquiry. Is this an accurate description of your methodological views?

It is often quite unclear to me exactly what methodological prescriptions (virtues or sins) are being subsumed under the rubric “positivism.” If positivism is simply the view that theory and empirical research need to be “balanced” in some kind of systematic interaction, then indeed I would describe my work as “positivist”—in contrast to both theoreticism and empiricism.

“Positivism,” however, is generally taken to mean not just a “balance” between theory and empirical research, but a particular way of understanding the relationship between the two. As discussed by Bhaskar, positivism is generally associated with the view that “empirical invariances are necessary for laws” and that “the conceptual and the empirical jointly exhaust the real.”¹³ A transcendental realist perspective on theory construction rejects the identification of empirical invariances (constant conjunctions of events) with laws. In its place the more complex understanding reflected in Figure 1 above is adopted: underlying generative mechanisms are seen as producing events which in turn, in conjunction with observational mechanisms, produce experiences (the domaine of the empirical). Empirical regularities are thus always the result of the operation of at least two ontologically distinct mechanisms: the mechanisms of observation and the mechanisms producing the events. This implies that unless the scientist adopts a strong theory of observation, it will be impossible to distinguish between empirical regularities produced by the observational mechanisms from regularities produced by underlying mechanisms in the phenomenon under study. It is in this sense that theories are a *precondition* for understanding empirical regularities—

and thus “laws”—rather than simply a generalization of observational regularities.

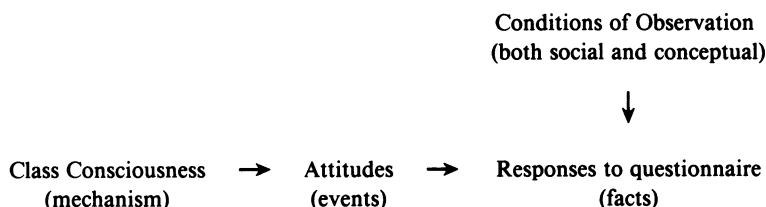
This perspective on science is not an esoteric doctrine. It is in fact the implicit stance of most real scientific practice. The search for spurious empirical relations, the insistence on the distinction between simple correlation and causation, the treatment of “laws” as “laws of tendency” (and thus their effects being empirically blockable by countervailing mechanisms) rather than “empirical invariances”—all of these are at the heart of good scientific practice.¹⁴ Positivism may have been the predominant current in the philosophy of science, but it is not the implicit philosophy of the actual practice of science.

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2.3. You use a series of eight questions to determine survey respondent's class consciousness, which you are then able to manipulate statistically as a quantitative variable (pp. 146-147). To do so you must presume that class consciousness is something susceptible to measurement. How do you justify such a presumption?

Class consciousness is *not* susceptible to measurement. Class consciousness is a concept that specifies a set of mechanisms; what is “measurable” (observable) are the effects of this mechanism. *If* class consciousness is a real mechanism—if this concept actually designates something real in the world—then it must generate events (this is what it means to be a mechanism), and if it generates events, then in conjunction with our observational procedures, these events can generate “facts.” That is, consciousness can be placed within the ontological framework of Figure 1 in the following manner:

Figure 5
Class Consciousness, Attitudes and Questionnaire Responses



The “facts” gathered in a survey, therefore, are removed from class consciousness in a double sense: first, these facts are removed from consciousness because the attitudes which they reflect are not identical to class consciousness as such; and second, they are

removed from consciousness because they are determined by the observational conditions of the survey as much as they are determined by the attitudinal "raw materials" which they attempt to measure.

Two central aspects of these observational conditions affect the nature of the facts embodied in the survey. First, and most obviously, there is the formulation of the questions themselves. The questions in any survey reflect the conceptual frameworks of the observer: not only the topics, but the subtle aspects of wording are shaped by the conceptual presupposition of the designer of the questionnaire. The range of possible "facts" from a survey is thus determined, in part, by the conceptual constraints imposed in the survey design. Second, and less obviously, the administration of a survey to a respondent is a social process, and this process also affects the translation of attitudes (the events produced by consciousness) into data. To just illustrate the point, many respondents experience the survey encounter as a kind of "examination" and are therefore concerned about giving the "correct" answer (in spite of being explicitly told that "there are no right answers; we are interested in your opinions"). The answers, therefore, may have less to do with attitudes rooted in class consciousness than with attitudes towards testing and authority.

It is sometimes argued that the social relational context of survey interviews is so powerful that it destroys any possibility of treating survey responses as measures of the attitudinal events produced by class consciousness.¹⁵ The only observations that are capable of reflecting the events produced by consciousness are unobtrusive observations, either from direct participation in the social struggles embodying class consciousness or from the spontaneously produced texts and records of those struggles. While the problem of the conceptual framework of the observer would still be present in such "naturalistic" observations (as they are in all observations), the social interactions of the observer with the people under study would less pervasively influence the "facts."¹⁶

These are serious objections to using survey data as measures of consciousness-producing events. If one adopts a radically empiricist approach to theory construction in which theories are no more than inductively arrived at generalizations from the data, then the kinds of measurement distortions discussed above would be very damaging. Unless one had reason to believe that the distortions caused by the measurement procedure are random, then any descriptive generalizations built on those observations could not be extended beyond the context of attitudes-in-interviews. Ironically, perhaps, it is within a narrowly positivist philosophy of science that the distortions of survey methods would most seriously undermine the

usefulness of surveys as a strategy of empirical research.

If one adopts a realist approach to science, however, the problems of observational distortions in survey research do not necessarily invalidate research using such data. The data from a survey are not used to generate inductively arrived at descriptive generalizations, but to construct quasi-experimental designs for testing various theoretically elaborated causal models. In such a context, a critic of using survey data has to do more than simply demonstrate distortions in the measurements. These distortions have to be such as to produce systematic biases in the results *relative to the theoretical model under investigation*. Distortions can simply scramble results, or they can reduce the strength of the empirical predictions of the model, or they can produce strong empirical correlations completely contradicting a given theoretical model.¹⁷ The distortions generated by the method of observation need not reinforce the theory of the observer—there is no *a priori* reason to assume that the biases have the character of a “self-fulfilling” prophecy (i.e. theories need not produce observational biases that are self-validating of the theory). As in any theoretical criticism within a realist framework, therefore, a critic of the measurements in a survey must explain the “conditions of possibility” for the empirical relations generated by the data.

In the specific context of the research reported in *Classes*, there is little reason to believe that the measurement distortions would tend to artefactually produce associations between exploitation-centered concepts of class structure and the empirical measures of class consciousness. So long as the data are being used to adjudicate between theoretically specified models, therefore, the burden is on a critic to show that the results favor one model over another because of the measurement strategy rather than because of the real mechanisms in the theory.

One of the implications of these comments is that survey research in general, and perhaps research on such concepts as class consciousness in particular, is mainly useful for studying well-specified theories rather than for making novel “discoveries” about the world. It is much harder to have confidence in highly counter-intuitive, unexpected results, in results which correspond to no theoretical model, than in results which are strongly supportive of one or another existing explanation. Of course, descriptive anomalies and counter-intuitive results in survey research, like in any other kind of research, can provoke the construction of new theories. But because of the seriousness of the problem of observational distortions, it is always necessary to treat descriptive surprises with particular suspicion in surveys.

*

III. ROLE OF THE SCIENTIST

3.1. Politics come last in Classes, literally: the subject is discussed only in a six-page postscript. Does your scientific method preclude considerations of ethical and political issues, or is there some other explanation for the relative absence of such consideration?

While it is true that the only place in the book where I explicitly engage “politics” is in the final few pages, I do not think that this implies that the rest of the book is unconcerned with political and normative issues. Indeed, the preoccupation of the book with the problem of exploitation reflects ethical concerns: to characterize the generative mechanisms of class relations in terms of exploitation is bring questions of justice and oppression into the heart of the concept of class. Similarly, a range of substantive discussions in the book are centrally preoccupied with political issues: the discussion of the historical trajectory of forms of exploitation and the successive eliminations of forms of exploitation, the discussion of class alliances and class formation, the discussion of the relationship between class structure and state structure, and so on. It is hard for me to see why these discussions are seen as somehow nonpolitical.

Still, most of the book is concerned with clarifying conceptual issues and not with politics as such. Whether this signals a “relative absence” of political discussion or not, it is certainly the emphasis of the book. This emphasis, I think, is related to my “scientific method”: if one adopted a more empiricist approach to concept formation and theory construction, there would be little call for such elaborate attention being paid to the nuances of the concept of class. If the definitions of concepts are treated simply as heuristic conventions, then there is no need to specify and justify the theoretical presuppositions of a given definition or to attempt to adjudicate between rival definitions. The fact that so much of the book is concerned with concept formation in this sense is a consequence of the underlying method.

I do not, however, see these methodological concerns as in tension with normative and political interests. The reason for worrying about how best to conceptualize the “middle class” is because inadequate concepts impede the construction of adequate theories, and inadequate theories impede our explanations of social and political problems. Marx is famous for saying that the point is not merely to interpret the world, but to change it. The methodological premise of my work is that in order to effectively change the world, one must understand it.

*

3.2. Survey research and statistical analysis are methods of social analysis well-entrenched in the American sociological community, which is not known for its rebelliousness. Is this conformist method at odds with a revolutionary theory and praxis?

Any method for generating explanations of real mechanisms is necessarily in a creative tension with revolutionary "praxis," and in that sense could be seen as "at odds" with revolutionary activity. Revolutionary praxis requires deep and absolute commitments, a suspension of skepticism, a willingness to believe in the viability of historical alternatives to the extent that one is willing to risk one's life for their achievement. Scientific debate, on the other hand, requires perpetual skepticism, a constant questioning of certainties, an insistence on the provisional character of all explanations and on the problematic status of all predictions for the future. Revolutionary militancy requires true believers; scientific method rejects the possibility of absolute truth.

The tension is therefore not between something called "conformist method" (whatever that means) on the one hand, and a harmonious couplet "revolutionary theory and praxis" on the other.¹⁸ The tension is fundamentally between revolutionary theory itself (understood as the scientific theory of revolutionary transformation) and revolutionary praxis. The tendency within the Marxist tradition for revolutionary theory to be transformed into revolutionary ideology reflects this tension. Marxism as Ideology provides certainties. It has a ready explanation for everything. Its rhetoric, at least in certain historical situations, is powerful in campaigns of mobilization. When Marxism becomes an Ideology in this sense, it is no longer at odds with revolutionary praxis and commitment, but it also ceases to be a scientific theory capable of producing new explanations and understandings of the world.

The contrast being drawn here between "Ideology" and "scientific theory" is, needless to say, a controversial one. Many radicals want to argue that science is *no more than* a form of ideology. If ideology is defined as any and all systems of ideas embodied in the subjectivities of actors, then of course, by definition, all scientific theory must be "ideology." This is equivalent to simply saying that science is a form of thought, which is hardly a bold insight. If, on the other hand, we use the term "Ideology" to designate a particular structure of thought, a particular mode of cognition in which ideas are ordered authoritatively in terms of some closed system of principles and are not subject to any internal principles of auto-critique and revision in light of "empirical" evidence, then it is no longer trivial to say that science is "just" ideology. This is the sense in which I believe there is a deep tension between revolutionary

theory (science) and revolutionary praxis: revolutionary praxis needs revolutionary ideology, but revolutionary theory, to remain scientific, must constantly challenge all ideology.¹⁹ While I believe that it is certainly the case that scientific practices (like all practices), and thus the theories produced by those practices, are influenced by ideology, often pervasively, I do not believe that scientific theories are reducible to ideology.

Question 3.2 above implies that a method of data gathering (surveys) and analysis (statistics) can be used for only certain kinds of theories (anti-rebellious conformist theories). As a matter of historical record, it is worth noting that neither Marx nor Lenin held such views: Marx was involved in survey research (a survey of Belgian workers) and was certainly willing to use the forms of statistical data analysis available in his time, and Lenin did not hesitate to engage in quantitative data analysis where appropriate. This, of course, does not settle the methodological question, for both Marx and Lenin may simply have uncritically used “conformist” methods themselves.

What would have to be true about a theory for survey data and statistical analysis to be intrinsically inappropriate? Survey data is simply data gathered by asking people questions about themselves—about their work, about their biographies, about their ideas. A census, for example, is no more than “survey research” on the whole population. If knowing the distribution of the population into different occupations is relevant for a theory, then survey data are appropriate. There are two bases upon which one could categorically reject survey data. First, on strictly methodological grounds it can be argued, as suggested in the discussion of question 2.3 above, that the interviewing relation so powerfully deforms responses to survey questions, that they cannot be treated as measures of the salient “events” in a theory. Particularly when the attempt is to measure “attitudes,” the resulting data, it could be argued, is simply an artifact of the interview and is thus useless in investigating important theoretical problems. Second, apart from the problem of measurement distortions, it could be claimed that the mechanisms postulated in the theory are unconnected with the subjective reports of individuals about their lives, even if those reports could be accurately recorded. Data obtained from the reports of subjects would thus not provide access to any of the events generated by the mechanisms of the theory.²⁰

The first of these objections has some plausibility, but it has nothing to do with a tension between survey methods and “revolutionary theory”; it would constitute a tension between survey methods and any explanatory theory. The second objection, on the other hand, has no plausibility. While it may be the case, as Marxists have always argued (in good realist fashion) that many of the key

mechanisms of social determination operate “behind the backs” of actors, it is certainly not the case that Marxist theory insists that subjects have no knowledge of any of the events produced by those mechanisms. This does not mean, of course, that survey data is necessarily the best kind of data for answering Marxist questions, but simply that it is not inherently proscribed by the explanatory principles of Marxism.

What about quantitative methods of data analysis? For a theory to be logically incompatible with statistical analysis, none of the mechanisms postulated in the theory can produce quantitative variations—either in the sense of variations in *degree* along some dimension or variations in the *probability* associated with some event. While it may certainly be the case that it is difficult to measure such quantitative variations on the conceptual terrain of Marxist theory, there is no inherent reason why quantitative analysis is incompatible with the causal processes postulated in Marxist theory. Again, as with the issue of survey data, this does not mean that quantitative analysis has a privileged standing within Marxist theory. Indeed, a good case can be made that because of the impossibility of adequately measuring the appropriate quantitative variations, statistical research is generally unsuitable for many of the central questions Marxists ask. The point is merely that there is no *inherent* incompatibility between Marxism as a scientific theory of society and such techniques of observation and data analysis.

I think that the suspicion many Marxists have of quantitative methods comes, at least in part, from the common practice among sociologists (and other social scientists) to invert the proper relationship between method and substance. Many sociologists begin with a bag of technical tricks and then ask: “What questions can I address with these methods?”. Many dissertations are motivated not by passionate engagement with the substantive theoretical issues in some subfield of sociology, but by a desire to apply some elegant technique. Students are forced to invest a lot of time and energy into learning these techniques (at least in some academic programs) and thus have an interest in using them in their research. Much quantitative research is thus methods-driven rather than theory-driven.

Methods-driven research tends to produce work of relatively marginal theoretical contribution. This is not a logical necessity—one could begin with a method and still ask interesting and important questions. But in practice, methods-drive research usually reflects a general disengagement of the researcher from theory as such. The most profound theoretical problems and debates tend to be quite distant from the practical matters of research and thus considerable theoretical work is necessary to translate those issues into researchable questions. If one begins with methods, then it is unlikely that

this theoretical work will be accomplished. The result is that methods-driven research tends to be preoccupied with exceedingly narrow problems, relatively unconnected with enduring theoretical debates.

Marxists—and other theoretically minded social scientists for that matter—thus have good reason to be suspicious of research strategies that put methods at the center stage and ask only those questions which are answerable with a limited set of techniques. But it does not follow from this that they need be suspicious of quantitative methods as such.

In fact, there are many debates within the Marxist tradition that can really only be resolved through quantitative study. For example, in recent years there has been an important debate over the problem of whether or not the technological and organizational changes in advanced capitalism have the effect of “deskilling” the labor force. This has important theoretical and political implications. It is possible through intensive case studies to document clear instances where such deskilling has occurred. And it is possible to elaborate a theoretical argument for why the underlying mechanisms of capitalism tend to generate such deskilling effects. But, as many theorists have also recognized, there are countervailing mechanisms for reskilling, and plenty of empirical examples where this has in fact occurred. The only way to assess the relative causal weights of these tendencies and counter-tendencies and thus the cogency of the overall deskilling argument is to attempt to measure skills, their transformations over time, and their relationship to various technical and organizational characteristics of work. Such research could involve “surveys” (gathering data from individuals about their work) and would surely involve quantitative analysis (both of the temporal changes in skills and of the correlations between changes in skills and changes in the technical and organizational properties posited in the theory). Again, this does not mean that quantitative methods have any kind of privileged status within Marxism, but simply that there are theoretical questions for which such methods are essential for producing serious empirical answers.

*

3.3. Given the respect and prestige which quantitative science commands in the United States, and especially in the academic community, can your method be considered a strategy for winning support for Marxism?

I first embarked on doing quantitative research deploying Marxist concepts in the early 1970s. From the start, this research had three general objectives. First, and foremost, I hoped that the

research would contribute to the reconstruction of Marxist theory itself. Debates within Marxism have always tended to be highly abstract and conceptual, and while historical and qualitative data is often deployed within those debates, rarely had the empirical side of the debates been played out in the form of systematically testing formal causal models. I saw such research as essential if Marxism was to advance as a social science, and quantitative research was one way of doing this.

Secondly, I did hope that by adopting a research strategy that deployed sophisticated design and techniques, Marxism would seem more respectable among non-Marxists, and that this would expand the institutional space for all sorts of Marxist work within the academy. Marxism is often viewed as a purely ideological theory (in the sense discussed in question 3.2 above) incapable of framing its propositions as “testable hypotheses” about the world. In part this characterization is itself ideologically motivated by anti-Marxists, but it has to be acknowledged that dogmatism within the Marxist tradition has also contributed to this intellectual image. “Multivariate Marxism” (as my research strategy has sometimes been dubbed) was one way of combatting this image.

Finally, I had some hopes that the research itself would actually convince some people of the theoretical virtues of Marxism. Not only did I have hopes of creating more tolerance for Marxist work among non-Marxists, I had the fantasy that by sheer intellectual energy and empirical power my research would convert some of the opposition.

It has now been ten years since I published my first “Multivariate Marxist” paper with Luca Perrone, “Marxist Class Categories and Income Inequality”.²¹ What is my assessment of these grand objectives?

In terms of the contribution of the quantitative research I have pursued on Marxism as such, so far the direct results have been relatively modest. Mostly, the data analysis has served to lend moderate support to particular theoretical arguments about class structure and its effects, but frequently—as chronically occurs in this game—the results are ambiguous, troubled by noise and weak correlations and thus fail to provide compelling adjudications between rival arguments. There have, of course, been some interesting surprises. I had not expected, for example, to find such pervasive and often dramatic interactions between class and gender. My expectation had always been that class mechanisms would more or less have the same empirical effects for women as for men, but this is simply not the case.²² But overall it remains the case that the direct empirical pay-offs of the research have, so far at least, not been spectacular.

Indirectly, however, the research has had significant effects. One of the virtues of quantitative research is the way in which it forces explicit definitions and operationalizations of concepts. It is much more difficult to use vague and unspecified categories when you have to defend a particular strategy of measuring them. Furthermore, in having to formally specify the causal model underlying the analysis, one is forced to engage a range of theoretical issues about the connections among concepts which otherwise might remain unelaborated. In short, being forced to operationalize both concepts and theories can contribute significantly to their abstract reformulation and clarification.

Let me give two examples from my recent work to illustrate this. Perhaps the central practical task in my research has been the operationalization of the concept "class structure." In my earlier work, one of the key elements in this concept was the category "semi-autonomous employees," a class location which I described as occupying a contradictory location between the working class and the petty bourgeoisie. As it turned out, it was exceedingly difficult to operationalize this category, to provide explicit criteria which could be used without producing anomalies (e.g. an airline pilot being more proletarianized than a janitor). These operational classification anomalies were one of the central spurs to the reconceptualization of class structure represented in *Classes*.

A second example concerns the analysis of class consciousness. In a recent data analysis, I initially wanted to study "class mobility effects" on ideology and constructed a range of models to pursue this task. In the course of operationalizing class consciousness and specifying the causal models, it became clear that a unidimensional measure of class consciousness was completely inadequate. Since "mobility" effects tap a temporal dimension of the lives of individuals, it occurred to me that it would be worth trying to rethink the problem of measuring consciousness in terms of what could be called the "temporal" dimension of consciousness—whether the consciousness in question had a forward or backward time horizon. In the end this led to an argument which characterized *class identity* as temporally backward and *subjective class interests* as temporally forward, and this in turn suggested a particular pattern linking these temporal dimensions of consciousness to biographical class trajectories and to current class location. I doubt very much if this reconceptualization would have been provoked in the absence of the operational tasks of quantitative research.

What about the second general objective of my research—creating more institutional space for Marxists in the academy? There is no doubt that there are more radical intellectuals in faculty positions in sociology departments today than fifteen years ago and that

more Marxist and other "critical" sociological work is published in major journals. Perhaps ironically, while in recent years Marxism as a theoretical framework has lost considerable support among radical intellectuals, it has gained at least some credibility as a contending and legitimate perspective within sociology in general. I do not, however, believe that this trajectory in the academic fortunes of Marxist theory has been primarily a result of the existence and successes of quantitative "Multivariate Marxism" as such. Qualitative historical and comparative research by Marxists has done at least as much to legitimate academic Marxism in American sociology as quantitative research. Symptomatic of this was the publication in 1982 of the special supplement, *Marxist Inquiries: Studies of Labor, Class and States*, to the *American Journal of Sociology*. Only three of the nine contributions to the volume were quantitative, and neither of the editors of the supplement, Michael Burawoy and Theda Skocpol, use quantitative techniques in their own research.

While quantitative research has been part of enlarging the influence of Marxism within American sociology, there is little evidence that it has played the decisive role in this expansion. What I think is more important has been the general seriousness with which Marxist and other radical sociologists have pursued systematic research in general, regardless of the technologies deployed in that research.²³ The quality of this research both in terms of the conceptual framing of the questions asked and the practical execution of the empirical strategies has often been exceptionally high, and this has impressed many critics of the radical theoretical orientation of this work.

I do not want to suggest that this expansion of the institutional space for Marxist and radical scholarship is simply the result of dispassionate appreciation by mainstream sociologists of the intellectual quality of the work. The acceptance of radical scholarship required the creation of a political climate of relative intellectual pluralism and tolerance, and this depended in many instances on struggle. Nevertheless, in the context of such struggles for recognition, the general quality of research, whether qualitative or quantitative, by Marxist and radical sociologists was important.

Finally, has the quantitative "respectability" of my research actually converted anyone to Marxism? I originally had visions of glorious paradigm battles, with lances drawn and the valiant Marxist knight unseating the bourgeois rival in a dramatic quantitative joust. What is more, the fantasy saw the vanquished admitting defeat and changing horses as a result.

What has been striking over the past decade is how little serious debate by mainstream sociology there has been in response to the outpouring of neo-Marxist research. I have generally been unable to

provoke systematic responses to my research among mainstream sociologists, of either a theoretical or empirical kind. A similar silence seems to be the general response to the quantitative research of most other radical scholars. The main effect of my research on the mainstream, as far as I can tell, is that certain “variables” are now more likely to be included in regression equations. What I envisioned as a broad theoretical challenge to “bourgeois sociology” backed up by systematic empirical research has resulted in the pragmatic appropriation of certain isolated elements of the operationalized conceptual framework with little attention to abstract theoretical issues.

It is now clear to me, as perhaps it should have been from the start, that support for Marxism as a social theory is not primarily a question of a belief in its analytical and explanatory power. It is primarily a political question. The production of systematic and rigorous research, therefore, could not in and of itself “convert” anyone to Marxism. For one thing, on the basis of research alone no one would be convinced of the importance of the questions being asked. For another, the distance between the conceptual framework of any general theory and the concrete results of an empirical study is too great for anyone to be convinced of the virtues of the former simply because of the empirical power of the latter. And finally, the results of quantitative analyses in sociology, if presented honestly, are always so messy and filled with ambiguities and inconsistencies, that without any other reasons for adopting a particular set of theoretical commitments, the results by themselves could never convince someone to abandon one general framework for another.

It might be concluded from these observations that quantitative research, and perhaps all empirical research, is *irrelevant* to extending the influence of Marxist theory. This would be, I believe, a false conclusion. While politics may be at the heart of the explanation for why intellectuals adopt particular theoretical perspectives, I do not think that political motivations are a sufficient explanation. The fact that Marxist theory has become an exciting and productive terrain on which to ask questions and pursue research is also important. While the actual results of this research in and of themselves do not convince anyone to become a Marxist, the fact that Marxist research produces results—produces new knowledge—is essential if Marxism is to be a contending theory within social science.

Footnotes

1. It should be noted in this regard that the statement in constraint #1 is not that only class structures impose limits on class struggle, but simply that they do impose such limits. I find it hard to imagine that any

Marxist who uses the concept of class structure would reject this relatively weak claim.

2. In what follows I am *not* discussing the criteria involved in my choice of Marxism over either non-Marxist social theory in general or "post-Marxist" radical social theory in particular, but rather the criteria involved in choosing among Marxisms. The choice of Marxism as such involves other issues.
3. This does not mean that the theories we produce are not *in* the world as well as *about* the world. Marxist theory itself produces effects in the world once it is embodied in ideologies, in political programs, in sociology curricula. This does not, however, pose fundamental problems to social theory, so long as one believes that the effects of theory on social processes can themselves be theorized (explained). That social theory must be reflexive—explaining both its own production and its own effects—does not imply that it cannot also be scientific.
4. The expression "analytical Marxism" has been identified with what is sometimes called "rational choice Marxism." This identification is unjustified. While it is certainly true that rational choice Marxists are analytical, and equally true that analytical Marxists are often drawn to rational choice theory because of its clarity and precision, there is no necessary relationship between the two, and many analytically oriented Marxists reject rational choice theory as an adequate way of building theories of society. For a discussion of rational choice Marxism, see Alan Carling, "Rational Choice Marxism," *New Left Review* #160, 1986. For an anthology of recent work by self-styled analytical Marxists, see John Roemer (ed.), *Analytical Marxism* (Cambridge: Cambridge University Press, 1986). For a critique of the methodological individualist aspirations of certain analytical Marxists, see Elliott Sober, Andrew Levine and Erik Olin Wright, "Marxism and Methodological Individualism," *New Left Review* #162, 1987.
5. I strongly agree with Barry Hindess and Paul Q. Hirst's arguments in *Marx's Capital and Capitalism Today* (London: Routledge and Kegan Paul, 1977, chapter 4), on this point: substantive theoretical claims about the world cannot be derived from epistemological doctrines. Such doctrines may make it possible to make certain substantive claims, but substantive claims require specific arguments about mechanisms, causes, processes, and these cannot be logically inferred from methodological principles.
6. See Erik Olin Wright and Bill Martin, "The Transformation of the American Class Structure: 1960-1970," *American Journal of Sociology*, July, 1987.
7. To cite just a few other examples, see Michael Reich, *Racial Inequality* (Princeton University Press, 1981); Roger Friedland, "Class Power and Social Control: the War on Poverty," *Politics & Society*, 6:4, 1976; Gosta Esping-Anderson, *Politics Against Markets* (Princeton University Press, 1984); Adam Przeworski and John Sprague, *Paper Stones* (University of Chicago Press, 1986).

8. Michael Burawoy, in a personal communication, suggested the tension in my work between these two missions of salvation.
9. I am following Roy Bhaskar, *A Realist Theory of Science* (Sussex: The Harvester Press, 1978) in adopting the expression "transcendental realism." This position is contrasted both to what Bhaskar calls "transcendental idealism" (the view that facts are wholly constituted by concepts) and what he calls empirical realism, or what is generally called simply empiricism (the view that there is an identity between facts and mechanisms).
10. Bhaskar, *ibid.*, p.56.
11. This is a slight simplification of the elaboration in Chapter 3 of *Classes*, since the distinction between capitalists and small employers has been dropped.
12. The example given above is from debates in class theory. I could equally well have chosen an example from gender theory. Much traditional gender analysis has argued for "natural" differences between the sexes: men are more aggressive, women are more nurturant, etc. A realist feminist critique would argue that some unspecified mechanism (patriarchal culture or male domination, for example) explains the conditions of possibility for the empirical observations of the traditional model. Again, the quasi-experimental design for definitively establishing the realist feminist thesis—the observation of gender differences in the absence of male domination—makes these debates particularly difficult to resolve.
13. Bhaskar, *op. cit.*, p. 20.
14. The relation between mechanisms, events and experiences in Figure 1 supports the treatment of explanatory laws as laws of tendency. Since the world is an "open system" in which countless mechanisms are operating simultaneously, it is always possible that a given mechanism is present, but its empirical effects are blocked by the operation of some other mechanism. This means that the presence of a given mechanism is not sufficient to produce the empirical consequence; it simply produces tendencies, tendencies whose realization depends upon a range of other conditions. This is precisely why experiments are so important in science: by adding theoretically controlled causes to the natural world—the causes imposed by the experimenter—a law of tendency can be observed as producing empirical invariances. These invariances between mechanism and experience (observation), however, are *consequences of the experiment*: they do not occur in nature.
15. For a defense of such arguments, see Gordon Marshall, "Some Remarks on the Study of Working Class Consciousness," *Politics & Society*, 12:3, 1983.
16. One could also, of course, reject the basic model in Figure 5 by arguing that the events produced by consciousness are not attitudes—discursively accessible opinions of individuals—but actions (practices). The implicit model would then be that the underlying subjective mechanisms designated by "consciousness" directly shape the practices of actors without affecting their consciously held opinions on anything.

Such a model, I would argue, is more appropriately called a model of class *unconsciousness* than *consciousness*. The point of talking about “consciousness” is that we believe that social practices should be viewed as *intentional* actions rather than just “behaviors.” And if intentions are important in explaining actions, then it is important to study the various aspects of the discursively accessible subjective states that frame the formation of intentions: the preferences of actors, the views of alternatives courses of actions, the theories people hold of the consequences of different choices, etc. These are basically what “attitudes” are meant to designate.

17. The chronically low explained variances in regression equations predicting attitudes reflects, I think, the pervasiveness of noise in such data. Many respondents literally answer the questions randomly: they do not listen to the interviewer, they are distracted by other concerns, they simply want to get the interview over and say whatever pops into their head without reflecting on the question. The result is that the *explainable* variance in a survey question (that part of the total variance that is systematically generated by any underlying mechanisms) is much lower than the empirical variance.
18. The expression “conformist method” is highly tendentious in this question. It is clearly meant to impugn the *motivations* for adopting the method (conformism) rather than to constitute a serious evaluation of the method.
19. It is very important in this discussion not to get bogged down in the problem of how best to deploy words. If one insists on using the term “ideology” in the encompassing sense of all subjectively constituted systems of thought, then the tension discussed here can be reframed as a tension between two types or aspects of revolutionary ideology: revolutionary scientific ideology and, perhaps, revolutionary religious ideology. Revolutionary ideologies often become a kind of secular religion, at least in so far as in certain historical settings Marxism involves “sacred” texts, talmudic scholars, anointed priests and rituals which affirm ultimate meaning. As a motivating revolutionary ideology, Marxism shares with traditional religions a preoccupation with telos and ultimate meanings. While god is replaced as the wellspring of that telos by “history” or “class struggle,” the cognitive processes in defending the vision of that telos are not so different from theology.
20. Thus, for example, one might reasonably argue that survey research is incompatible with certain types of psychoanalytic theories, at least in so far as none of the salient events in the theory are discursively accessible to the subject.
21. *American Sociological Review*, 1977, 42:1 (February).
22. These issues are discussed in two recent and, as yet, unpublished papers: “Individual and Families in the Class Structure” and “Temporality, Class Structure and Class Consciousness.”
23. In this context I couple “Marxist” sociology with other “radical” sociology. Most mainstream sociologists do not make the distinction between self-conscious Marxism and more general radical/critical

perspectives. Theda Skocpol, for example, continues to be viewed as a Marxist sociologist by many (perhaps most) American sociologists in spite of her repeated insistence that her work is in dialogue with Marxism but not Marxist.

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