

ysis tends to restrict investigations to problems that are easily quantifiable; rational choice theory tends to direct attention to those problems of strategic interaction that can be formally modeled within the repertoire of game theory models. Such potential restriction on the domain of inquiry imposed by the choice of scientifically rigorous methods poses serious threats to the political vitality of radical thought.

These risks need to be acknowledged, and resisted. But to respond to them by refusing to build enclaves of radical scholarship within leading universities robs Marxism of the capacity to play an effective role in the academy; and to cope with these risks by rejecting these analytical and scientific methods altogether undermines the ability of Marxism to enhance its theoretical understandings of the world in ways which will enable it, once again, to play an effective role in politics as well.

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## Marxism as Social Science

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In 1989, the *Berkeley Journal of Sociology* invited a number of people to comment on an exchange between myself and Michael Burawoy which had been published in the 1987 issue of the journal.<sup>1</sup> The original exchange revolved around the scientific and theoretical status of my book *Classes*. In an interview with a group of graduate students at the University of California, Berkeley, which was published as the opening item in the dialogue between myself and Burawoy, I had defended the attempt in that book of pursuing Marxist questions with quantitative research techniques. Burawoy criticized my position, arguing that my vision of science was inattentive to the social conditions for the production of knowledge and that this had especially critical implications for my aspirations to produce a science that was faithful to Marxism as an emancipatory theory. I then replied, defending a version of scientific realism and arguing that academic Marxism, isolated to some extent from the pressures of popular struggles, had the potential of making certain distinctive kinds of contributions to knowledge relevant for emancipatory projects of social change.

The 1989 *BJS* symposium on this earlier exchange included commentators who were much more hostile to the whole enterprise of trying to build a serious Marxist social science. In this chapter, I engage two themes that emerged in different ways in a number of the contributions to the symposium: first, the claim in several of the essays that the exchange between myself and Michael Burawoy was simply a rehash of the old-fashioned debate between "scientific" and "critical" Marxism; and second, the claim that the preoccupations of our debate are largely

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1. See Erik Olin Wright, "Reflections on Classes," and Michael Burawoy, "The Limits of Wright's Analytical Marxism and an Alternative," in the *Berkeley Journal of Sociology*, vol. XXXII, 1987. This exchange was subsequently reprinted as chapter 2 in my book, *The Debate on Classes*, London 1989.

irrelevant given the broader supersession of Marxism as a plausible perspective for any kind of radical social theory, scientific or otherwise. With respect to the first claim, I will argue that our debate does not revolve around a polarized opposition between science and critique, but rather concerns two contrasting emphases in the construction of Marxism as a science. With respect to the second issue, I will argue that Marxism remains a productive, knowledge-producing theoretical tradition and that its relevance is not diminished by the proliferation of a range of post-Marxist radical theoretical approaches. I will not primarily engage these themes in the form of a point-by-point discussion of the various contributions to the symposium, but will rather take a few of the claims of these contributions as a point of departure for a more general discussion of the issues.

### Marxism as a Social Science

Ben Agger writes:

The controversy over Marxism's scientificity, raging for over half a century, has been settled: Lukács, the Frankfurt school, the Parisian existential-Marxists all vanquish the Engels/Stalin model of a "dialectic of nature" . . . Quantitative Marxism is nothing new, except to Quantitative non-Marxists; it merely refurbishes the stagnant "dialectical materialism" used by socialist authoritarians to justify one political perversion or another. . . . Whichever side we are on – positivist Marxism or Western Marxism – all of this has been said before.<sup>2</sup>

This characterization of the issues in my exchange with Burawoy rests on an unjustified identification of contemporary aspirations for Marxism to be a social science with previous uses of the rhetoric of "science" within Marxism as an ideology of intellectual domination, particularly by elites within political parties. This treatment of the issues reflects a deep irony in the use of the word "science" within the Marxist tradition: Marxists who have most stridently insisted on the scientificity of Marxism have often adopted theoretical practices which are quintessentially anti-scientific. "Scientific Marxism" often functioned more like a secular theology than a scientific discipline: Marxism became Marxology; classical texts were canonized; and the central arguments of the "science" were impervious to transformation.

2. Ben Agger, "Is Wright Wrong (Or Should Burawoy Be Buried?): Reflections on the Crisis of the 'Crisis of Marxism'," *Berkeley Journal of Sociology*, vol. XXXIV, 1989, p. 187.

This kind of theoretical practice has nothing to do with "science" in any philosophical tradition, and certainly has nothing to do with "positivism."<sup>3</sup> This is not to say that all substantive theses contained within such pseudo-scientific Marxism were necessarily false, but simply that the theoretical practices and methods which were deployed to defend those theses were often profoundly anti-scientific.

One of the striking features of some of the current efforts to reconstruct Marxism as a social science is precisely the commitment to take its scientificity serious. It would, of course, be arrogant to insist that before the present renaissance in Marxist theory, there was no awareness of the anti-scientific character of much of what passed for Marxist science. Indeed, when Marx himself declared "I am not a Marxist" he was in part affirming the need for a genuinely scientific practice. Nevertheless, throughout much of the history of Marxism, the claims to scientificity have been largely ritualistic and hardly congruent with the actual practices. Whatever else might be our differences, both Michael Burawoy and I are committed to the view that Marxism should aspire to the status of a science and that it should take that aspiration seriously. Neither of us treat Marxism primarily as a hermeneutical practice for understanding the meaning of social practices, nor as a strictly philosophical practice for cultural critique. Both of us believe that Marxism should aspire to produce explanations, and that any given explanation we produce may turn out to be wrong. Contrary to Agger's suggestion, this position has nothing to do with Stalinist "scientific Marxism."

Now, affirming a commitment to "science" leaves unspecified exactly what is meant by "science," and this is, of course, a hotly contested issue in philosophy. Michael Burawoy, at least, believes that on the question of how science itself should be understood we are in significant disagreement, while I think that our positions are not in fact so divergent.

As I argued in the original exchange in the *Berkeley Journal of Sociology*, I adopt what is generally described as a *realist* view of science. This involves the following basic view of the scientific enterprise: science attempts to identify the underlying *mechanisms* which generate the empirical phenomena we experience in the world. Our ability to gain

3. Many commentators on Marxism seem to believe that since positivist approaches to science often seek invariant "laws" in the phenomena they study, and since orthodox Marxism also speaks of the "laws of history," it must therefore be the case that this kind of Marxism is guilty of positivist scientific practice. It may be guilty of positivist *rhetoric*, but the theoretically rigid orthodoxies of "scientific socialism" are completely antithetical to science, whether positivist or anti-positivist. One of the hallmarks of this anti-scientific practice is the way in which citations of Marx's (or Lenin's or someone else's) texts displace the systematic analysis of data and development of arguments as a way of defending theoretical positions.

knowledge of these mechanisms is complicated by two properties of the relationship between our observations of the effects of the mechanisms – our “experiences” – and the mechanisms themselves. First, we live in an open system in which many mechanisms are simultaneously operating. This means that the effects of one mechanism may be counteracted by another. There is not, therefore, an invariant relationship between the existence of a mechanism and empirical manifestations of its effects.<sup>4</sup> Secondly, our observations of anything are simultaneously shaped by mechanisms internal to the process of observation itself (which include such things as our systems of classification and description, as well as our technologies of observation) and mechanisms which directly generate (cause) the phenomenon in question.<sup>5</sup> Because of this duality, even apart from the problem of the complexity of living in an open-system, it is impossible ever to inductively discover truths about mechanisms *simply* by generalizations from pure empirical “facts,” since those facts are necessarily shaped by the observation process itself. And this, in turn, implies that in order for observations to be intelligible, they must be embedded in theories about these mechanisms.

Thus, I reject the view of naïve empiricism that we can observe the world without categories already embedded in theories. Observations cannot be theory-neutral, and therefore our theories can never be simple inductive generalizations from pre-theoretical “facts.” But I also reject the anti-realist view that our observations are wholly constituted by the categories of thought, by the discourses we use in describing the world. Scientific theories attempt to construct explanations based on real mechanisms that exist in the world independently of our theories, even though our observations of those mechanisms and their effects depend in part upon the theories themselves.

I believe that, his protestations notwithstanding, in practice Burawoy accepts a view of scientific practice that is essentially congruent with

4. It also follows from this complexity that the empirical observation of an hypothesized effect cannot be taken as definitive proof of the existence of the proposed mechanism, since it is possible that two (or more) distinct kinds of mechanisms could generate the same empirical effects (experiences). Loïc Wacquant’s claim in his essay in this symposium that the kind of realism I propose is guilty of the “fallacy of affirming the consequent” is incorrect: scientific realism does not imply that observations of effects constitute definitive proof of the operation of a given mechanism, since multiple mechanisms are always present in open-systems.

5. Dick Walker, in his essay, “In Defense of Realism and Dialectical Materialism: a Friendly Critique of Wright and Burawoy’s Marxist Philosophy,” in the 1989 *Berkeley Journal of Sociology* symposium, states that the control of “observational error” (his words) was not an important issue in scientific realism. I do not think that this is correct: mechanisms internal to the process of observation are among the most important “contingent” mechanisms implicated in the use of data (experience) to evaluate claims about causes.

these views. There is nothing inherently anti-realist about Burawoy’s adoption of Lakatos’s approach to the development of science in terms of research programs and theoretical cores. In my judgment, Burawoy’s central concern in his discussions of the philosophy of science is giving an account of the process through which scientific *questions* are productively generated: these emerge from the puzzles posed by research programs as they encounter anomalies in the world, where an anomaly is understood as a set of observations which are in some way or another inconsistent with the existing theories of the program.<sup>6</sup> A program is progressive if it is capable of recognizing such puzzles and generating new explanations in a non-arbitrary way for dealing with them, where to be “non-arbitrary” means that the new propositions are in some sense derived from the internal principles of the “core” of the program rather than added on in an *ad hoc* manner. This Lakatosian view of the *question*-generating machine of science is entirely consistent with the realist claim that the objective of science is to produce *answers* to these questions that take the form of explanations revolving around the identification of effect-generating mechanisms.

Where then do we differ? Two issues seem to me to be especially important: the first concerns the kinds of puzzles with which we are preoccupied, and the second concerns our response to a set of dilemmas posed by the sociology of knowledge.

While both of us accept the importance of working within a research program with a relatively consistently articulated theoretical “core,” and both of us believe that the ultimate theoretical objective is producing explanations rooted in mechanisms consistent with that core, we are preoccupied with rather different kinds of puzzles. My work has been concerned mainly with puzzles generated by the internal logic of the *concepts* of the core of Marxism, whereas Burawoy has been concerned with puzzles generated by *predictions* of the core. Thus, above all, I have worried about the concept of class structure, trying to accommodate the non-polarized reality of “middle classes” within a conceptual framework built around a polarized concept of class. The middle class is a puzzle or anomaly within such a conceptual field, and I have proposed various ways of providing a non-arbitrary conceptual solution. This is not, as Loïc Wacquant asserts in his essay in the symposium, an attempt to “solve on paper a question which is not resolved in reality”; rather, it is

6. Perhaps a slightly more precise way of saying this is that *if* one wants one’s research to have a cumulative effect on knowledge, then the questions which drive the research need to be generated by such puzzles. Much research, of course, may not self-consciously be tied to the puzzles of a research program. The result is that the specific knowledge generated by the research is unlikely to add to any body of theoretical knowledge.

an attempt to generate a conceptual repertoire that gives precision to the ways in which it is not "resolved" in reality.<sup>7</sup> This is what the concept of "contradictory locations" attempts to do. Burawoy, in contrast, is directly concerned with the puzzles posed by the failure of classical Marxist predictions of a revolutionary working class in advanced capitalism. He has engaged in a wonderfully rich array of empirical investigations of workers acting under different constraints in different times and places in order to figure out why it is that workers in capitalism are so consistently non-revolutionary.

Another way of putting this is that my work has revolved around the "independent variables" of the Marxist core – the central explanatory concepts, especially class itself – whereas Burawoy's work has revolved around the central "dependent variable" of Marxism – especially patterns of class formation. My intuition has been that I could not effectively embark on the task of confronting in a serious way the explanation of class formation until I got the conceptual apparatus used in those explanations straightened out; Burawoy's assumption is that the only way to straighten out such concepts is to launch headlong into the dirty work of generating explanations themselves.

This brings me to the sociology of knowledge point. Both of us, I think, accept the very general point that in one way or another the kind of knowledge we produce is shaped by the social constraints within which we live. And both of us believe that bourgeois academic institutions impose enormous pressures on scholars to produce knowledge that does not pose threats to existing forms of oppression and exploitation. As Marxists wanting to produce a particular kind of knowledge – knowledge that, we hope, will contribute in some way to emancipatory possibilities – we thus have somehow to situate ourselves in a social setting in such a way that these pressures are to some extent counteracted. Where we differ, I think, is in our view as to how to accomplish this.

Burawoy feels that the only viable strategy to counter the academicizing forces of intellectual work in a university is to be directly engaged in the world and struggles of the people one studies. By being a participant

7. See Loïc Wacquant, "Social Ontology, Epistemology and Class: On Wright's and Burawoy's Politics of Knowledge," *Berkeley Journal of Sociology*, vol. XXXII, 1987, p. 174. Contrary to Wacquant's characterization of my position, at this level of abstraction I do not think that my views differ at all from Pierre Bourdieu's. Bourdieu's distinction between "classes on paper" and "real classes" is exactly parallel to my distinction between class structure (determined by the patterns of objective relations and resources) and class formation (socially constituted groups organized within a class structure around these relations and resources). The real struggles of actors over social identities and categories are, in these terms, struggles over class formation; but – as Bourdieu also argues – the probabilities of success and stability in such struggles for class formation are determined by the underlying class structure itself.

observer in the world of ordinary workers, one's identity is partially unhinged from the normative coordinates of the academy. This engagement accomplishes two critical tasks. First, it provides a methodological vehicle for generating the necessary kinds of data to answer the questions about which Burawoy is most interested – questions about the limits and possibilities of radical working-class formation. As a participant observer actively engaged in the world he studies, Burawoy in effect conducts a long series of mini-experiments, making daily conjectures about what will happen if he does X and then "testing" these hypotheses on the shop floor. Second, and particularly important in the present context, this kind of engaged fieldwork provides the social setting for sustaining a commitment in a serious way to the questions themselves. Participant observation thus serves a double purpose: it partially counters the pressures of the academy, enabling a radical scholar to ask the right questions, and it provides a social setting for obtaining the data necessary for producing the best answers.<sup>8</sup>

I am not convinced that, at least at this moment in history, the institutional pressures on conformity to bourgeois values are so strong that Burawoy's solution is the only one possible. In any event, for better or worse, I have certainly adopted a different strategy. Instead of weakening my ties to the academy, I have tried to counter the pressures by creating within the academy a dense network of radical scholarship which openly and consistently affirms emancipatory values. As a graduate student I helped found the Union of Marxist Social Scientists and the West Coast Socialist Social Science Conference; as a faculty member at the University of Wisconsin I have helped build the Class Analysis and Historical Change Program; and, more recently, with a number of colleagues I have helped found a left-wing research center, the Havens Center for the Study of Social Structure and Social Change. Each of these projects of institution-building was self-consciously designed to generate an environment of intellectual accountability in which norms of political radicalism would have a legitimate place.

It would be absurd to argue that this strategy of creating institutional enclaves of radical social thought inside established universities can effectively eliminate pressures for conformity. But neither does personal engagement with workers in factories and unions dissolve the pressures

8. To state this point in a slightly different way, I believe that Burawoy's commitment to qualitative methods in general and participant observation in particular does not directly follow from any general philosophical beliefs about the nature of social science, but rather from a particular substantive view about the social structural conditions for the production of knowledge on the one hand, and a view about the nature of the decisive mechanisms responsible for explaining variations in class formation on the other (i.e. mechanisms that are embedded in the lived experiences of workers within production).

from grant agencies and tenure committees. I suppose that the model of building institutional enclaves of radical scholarship within which different values have effective power is like the strategies of "advanced democracy" or "non-reformist reforms" advocated by democratic socialists in the 1970s: this strategy acknowledges that reforms are achievable within existing institutions that allow for genuinely new possibilities of action but which, nevertheless, remain compatible with the reproduction of the institutions themselves. The strategy of constructing an alternative form of intellectual accountability through personal engagement outside the university, on the other hand, implies that such reforms are ultimately illusions which, far from creating an institutional space within which radical intellectuals can effectively work, have the effect of co-opting those intellectuals and neutralizing their radicalism.

In the end I do not know which is the more effective strategy. Both seem plausible; both contain real risks.

### Theoretical Cores and Eclecticism

Judith Stacey and Linda Collins strongly affirm the virtues of theoretical eclecticism:

Our commitment to comprehending and resisting domination and injustice in their all too variegated guises leads us to greater theoretical eclecticism than Wright or Burawoy. Having passed through different stages of Marxist political discourse along our individual paths to our current research and politics, each of us has lost interest in the terms of their *BJS* debate. Our work on gender, race and community requires different, perhaps less rooted, anchors than Marxism alone provides.<sup>9</sup>

Similarly, Loïc Wacquant asks why should we restrict our effort at theory construction to Marxism as such:

If the end-purpose of class theory is to explain the structure, formation and trajectory of classes as historical forces, and if we believe that the social universe comprises complex structures wholly or partly independent of our knowledge of them, why should we *a priori* limit our investigations of them by holding on to Marxist tenets, however defined, rather than launch into an all-out search using the full gamut of theoretical resources at hand?<sup>10</sup>

Eclecticism is certainly an appealing doctrine of intellectual practice.

9. Linda Collins and Judith Stacey, "Salvation or Emancipation? Reflections on the Wright/Burawoy Exchange," *Berkeley Journal of Sociology*, vol. XXXIV, 1989, p. 52.  
10. Wacquant, "Social Ontology, Epistemology and Class," p. 167.

Sticking to a strong, integrated theoretical core often gets social theorists into trouble, for the world is complex and ambitious theories always try to push the limits of simplification. It is much easier to respond to each new complexity with a patchwork of relatively unconnected conceptual elements than to constantly strive for coherence and theoretical integration. There is no doubt that much of the time an eclectic approach will be less vulnerable to charges of dogmatism, oversimplification, rigidity, and other academic sins. But is eclecticism a desirable universal principle for building theories and advancing knowledge?

Imagine the following two possible worlds of theoretical debate on social questions. In the first world, most theorists are committed to one or another of a series of well-articulated, systematic theoretical paradigms (or research programs, to use the Lakatosian expression favored by Burawoy). Each of these paradigms is built around a core of central questions, assumptions, concepts, and general theses. To be committed to such a paradigm does *not* mean that one is "dogmatic" in the cognitive sense of rigidly adhering to a set of ideas that are immune from challenge; but it does imply a certain stubbornness in the defense of core elements of a paradigm, a reluctance to abandon ship whenever it springs a leak. On occasion, these divergent approaches may attempt to explain the same empirical phenomena, and when this happens the theorists working within these different approaches may engage in sustained debate; but often, they are asking different questions, and thus do not directly confront each other.

In the second world, theorists all take an eclectic stance towards such paradigms. Theorists are not committed in general to developing and defending coherent, conceptual cores in their theories, but rather believe that the best way to build our understanding of social phenomena is to use whatever concepts and arguments seem most appropriate for each specific empirical problem without worrying about the compatibility of such concepts with some overarching "framework." This does not imply that the theorists in question are opposed to explanation as such, but simply that they reject the methodological view that it is desirable to embed explanations of specific phenomena within more general explanatory principles.

In which of these worlds is our knowledge of social phenomena most likely to advance? In which world is there a better chance that our learning process will have a cumulative character to it? I think knowledge is more likely to advance in a cumulative way in a world with many theorists committed to systematic, tightly organized paradigms, than in a world made up exclusively of eclectics. This does not imply that I would prefer a world made up entirely of theorists who accept my own perspective. I think that it is much healthier to work as a Marxist

sociologist in a world with radical feminist sociologists who are committed to the pervasive explanatory centrality of gender and reject class analysis altogether, and with neoclassical economists who affirm the centrality of economic processes but deny that they should be theorized in class terms, than to work on an intellectual terrain inhabited entirely by Marxists (let alone entirely by Analytical Marxists!).

I believe that the intellectual terrain with the greatest vitality for exploring social questions is likely to be an intensely pluralistic one, in the sense that no single approach overwhelmingly dominates intellectual life; but on this pluralistic terrain, knowledge is more likely to advance if the different perspectives each attempts, self-consciously, to elaborate their underlying assumptions, to formulate their concepts in as coherent a way as possible, and to develop a systematic set of general theses using these concepts and assumptions. This is hard work. Eclecticism might itself be one of the theoretical approaches contending on this terrain, but it would be parasitic on the more tightly organized theoretical paradigms, since there must be something from which to pick and choose in order to construct eclectic explanations. Eclecticism would thus not be a privileged methodological stance, but simply one strategy among several.<sup>11</sup>

This general view of the conditions under which knowledge is likely to advance is not equivalent to epistemological relativism. To defend the virtues of a pluralistic theoretical terrain is not to argue that all theoretical positions are equally valid. Evidence and argument can be brought to bear to "adjudicate" among rival explanations *when the rivals attempt to explain the same empirical phenomena*. The point is that the outcome of such contestation of rival perspectives is more likely to produce a cumulative trajectory of knowledge, rather than simply a fragmented resolution of a specific debate, if the positions in dispute are built around systematic, coherent theoretical frameworks.

To argue that it is desirable to have an intellectual environment within which some people are committed to elaborating a distinctively Marxist approach does not imply that in the concrete analysis of specific problems. Marxists should refuse to consider causal processes that fall outside of a Marxist framework. As argued in the previous chapter, *Marxism* is largely sex-blind in so far as its core categories are defined independently of gender relations, but this does not imply that when *Marxists* analyze a

11. The premiss of the argument advanced here is that the kind of realist explanation outlined earlier is possible. One can, of course, deny the very possibility of realist social knowledge altogether, and thus deny the possibility of knowledge having a "cumulative" character. Perhaps all we can do is *describe* social phenomena from particular cognitive points of view, rendering them meaningful to us because of our normative concerns. If this is so, then there would be no particular virtue in trying to construct general explanatory theoretical frameworks around a core of coherently integrated concepts and assumptions.

concrete empirical problem their analyses must be sex-blind. Marxists can also be feminists, even if Marxism and feminism cannot be melded into a unified theoretical framework, at least given the current state of knowledge.<sup>12</sup> If we want to understand the absence of solidarity in a working-class community, for example, it may be necessary to study both the sorts of mechanisms specified in Marxist theory – forms of organization of the labor process, mechanisms of hegemonic incorporation by capitalism, fragmentation of labor markets, etc. – as well as mechanisms unspecified by Marxism as such, such as gender relations or ethnic divisions. The importance of the *question* itself is driven by Marxism, which places working-class formation at the center of its analysis; and the theoretical *relevance* of these non-class mechanisms is established by their articulation to the class-based mechanisms. In the actual deployment of class analysis to investigate concrete empirical problems, therefore, Marxism is compatible with a kind of anchored eclecticism.<sup>13</sup>

An analogy with medical science may help to clarify these arguments about the relationship between Marxism, feminism, and other frameworks of emancipatory social theory.<sup>14</sup> In medicine it is often useful to distinguish the *clinical* practice of medicine from the *scientific* practice of medicine. A scientific endocrinologist does research on the structure and processes of the body's endocrine system. In developing the general theories of this system and its functioning, an endocrinologist will draw on, for example, the genetics theory, but only in so far as this is relevant for answering the questions posed by the study of hormones. This in no way implies that the only theoretical relevance of genetics in general is its bearing on the endocrine system, but simply that this is the relevance for the explanatory purposes of an endocrinologist. In clinical medicine, a physician tries to diagnose a concrete illness in a concrete human body in order to cure it (i.e. to paraphrase Marx, understand the symptoms in order to transform them). For this task the physician draws on the available theories of a wide variety of illness-producing mechanisms, not for the purpose of advancing our scientific understanding of those mechanisms, but for the purpose of solving a particular concrete prob-

12. By a "unified framework" I mean a theoretical structure within which class and gender are two components logically integrated through some overarching principle rather than simply added together. The attempts at formulating such integrated frameworks of which I am aware are quite unpersuasive and typically have the character of either subsuming gender under class analysis or class under gender analysis.

13. The best Marxist historical and sociological research, from Marx's own historical writings to contemporary Marxist scholarship, has always adopted this kind of flexible strategy, linking a wide array of non-class factors to the class and economic causes that are at the core of Marxist theory.

14. This analogy is developed at some length in Erik Olin Wright, Andrew Levine, and Elliott Sober, *Reconstructing Marxism*, London 1992, pp. 180–82.

lem. In this sense, in the practice of clinical medicine the physician engages in a certain kind of eclecticism by drawing on the diverse theories of different biological mechanisms. The development of those theories themselves, however, proceeds in a much less eclectic manner.

Now, it may be the case that certain of these mechanisms might in general be much more "important" than others in the sense that they are implicated in many more kinds of diseases or more powerfully shape the course of development of diseases. Just as Marxists have often made strong claims about the general centrality of class in the production of social "illness," some medical scientists have argued, for example, that the immune system is really the key to understanding disease processes across a wide spectrum of quite distinct illnesses. If this were true, it might provide a justification for focusing our research energy on deepening our theoretical understanding of that kind of mechanism, and it might suggest to a physician that in beginning a diagnosis for certain illnesses, a good place to begin would be to analyze the immune system.

It is also possible that because of explanatory zeal and professional interests, a proponent of a particular disease-production mechanism might exaggerate its explanatory scope and importance, in the extreme case elevating a single kind of mechanism to the status of the central determinant of all illness. At times, Marxists have certainly been guilty of such explanatory imperialism when they claim that class is the most important cause of everything. Combating such explanatory imperialism is an important task of science. But arguing for a more precisely formulated specification of the explanatory reach of class analysis does not imply that class is simply one factor in a long list of social causes, no more important than any other in explaining the major dilemmas and contradictions of contemporary society. Class may not be *the* central cause of *everything* social and still be *a* central cause of *most* social phenomena of interest to radicals. And, so long as it is recognized that class is a pervasive social cause, then social clinicians interested in diagnosing specific empirical problems with an eclectic tool kit have an interest in the preservation and development of Marxism as the social science of class analysis.