

En Wright, Erik Olin y Burawoy, Michael, *The Debate on Classes*. London (Reino Unido): Verso.

## Reflections on Classes.

Wright, Erik Olin y Burawoy, Michael.

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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 corre-

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 alternative courses of actions, the theories people hold of the consequences of different choices, etc. These are basically what "attitudes" are meant to designate.

lations completely contradicting a given theoretical model.<sup>17</sup> 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 (that is, 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.

### 3 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*

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.

*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 to 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 non-political.

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.<sup>18</sup> 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,

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.

must constantly challenge all ideology.<sup>19</sup> 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

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.

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.<sup>20</sup>

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

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.

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 logical necessity—one could begin with a method and still ask interesting and important questions. But in practice, methods-driven 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 quanti-

tative 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 combating 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.”<sup>21</sup> 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

21. *American Sociological Review*, 1977, vol. 42, no. 1 (February).

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.<sup>22</sup> But overall it remains the case that the direct empirical payoffs 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 “semiautonomous 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 (for example, 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

22. These issues are discussed in “Women in the Class Structure,” *Politics and Society*, vol. 17 no. 1, 1989, pp. 35–66; and “Temporality and Class Analysis: A Comparative Study of the Effects of Class Trajectory and Class Structure on Class Consciousness in Sweden and the United States,” *Sociological Theory*, vol. 6 no. 1, 1988.

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, uses 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.<sup>23</sup> The quality of this

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.

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.

# THE LIMITS OF WRIGHT'S ANALYTICAL MARXISM AND AN ALTERNATIVE

*Michael Burawoy*

## 1 The Context of Our Disagreement<sup>1</sup>

I had just launched myself into the job market in 1975 when Erik Wright, a Berkeley graduate student but unknown to me at the time, called to tell me that a letter of defamation had arrived from a very prominent Chicago sociologist. It accused me of the worst sins of left sectarianism and, so Wright said, had ruined any chances of my getting a job at Berkeley. I had better put a stop to its circulation to other departments. We met three months later when he and a number of other Berkeley graduate students together with a select group of faculty and even the odd staff member organized an undercover interview to resurrect my candidacy. In the end they were successful. In those days students' sense of political efficacy—a dwindling legacy of the New Left—was enhanced by the dramatic political split in the department. It was a time of Marxist renaissance, particularly in Berkeley around *Socialist Revolution*, *Kapitalistate* and the *Berkeley Journal of Sociology*. Since then Marxism has become a little more established within sociology and 1950s style red-baiting and black-listing is more of an anachronism. But there is no room for complacency. The long struggle against Wright's appointment here last year might have succeeded had the department been less unified in his defence. The denial of tenure to radical thinkers or even their expulsion from academia are a continual reminder of the seamier side of life in the liberal university. Nevertheless, it is true that the political context, both within and outside the university, has changed over the last twelve years and we must ask what sort of Marxism it calls for.

Twelve years ago Wright was finishing his dissertation where he developed his now celebrated reconceptualization of class structure of advanced capitalist societies. His project was to reformulate Marxist

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1. In this paper I have followed Wright in not appealing to authorities and texts to defend my arguments and in using examples from my own research to illustrate an alternative methodology. I'd like to thank Carol Hatch for her biting comments on a late draft.

notions of class so as to come up with a class map that took into account the differentiated character of contemporary capitalist class structures without losing the Marxian idea of class polarization. It was, of course, more than a retheorization of class. It used Michigan Survey data to map the contours of the American class structure and to demonstrate that a Marxian notion of class can more powerfully explain income inequality than theories relying on human capital or status attainment. It combined conceptual rigor with empirical adjudication that has become the hallmark of Wright's work. At that time I too was completing my dissertation which by a coincidence neatly complemented his own, in that it examined the microdynamics of class within a single capitalist firm. We were both committed to what was then called "Marxist structuralism" and our intellectual mentors were Louis Althusser, Etienne Balibar, and Nicos Poulantzas.

On reflection my commitment to this Marxist science was thrust upon me by the Chicago sociology department, dominated by a narrow-minded hostility to Marxism. To demonstrate that Marxism, for me sharply distinguished from sociology, could more effectively explain social phenomena became a survival strategy. Wright, on the other hand, from the beginning was a deep-seated believer in the virtues of science. Just as Wright's move to Madison only reinforced his commitment to the pursuit of science as universal truth, the atmosphere at Berkeley stimulated my own doubts about Marxist science, at least as I had been practicing it. We have never argued out our differences in print. I am, therefore, delighted that the *Berkeley Journal of Sociology* should have celebrated Wright's return to Berkeley by asking him to elaborate some of the assumptions underlying his Marxism. I'm even more delighted to have this opportunity to respond.

## 2 Science and Revolution

Let us go to the heart of the matter. Of all Wright's claims I find the one that science and revolution are antithetical the most disturbing. Certainly the unity of science and revolution, has traditionally been seen as the core of Marxism and symbolized by the lives of all the great Marxists: Marx, Engels, Trotsky, Luxemburg, Lukács, Lenin, and Gramsci. Wright comes to a different conclusion. "Revolutionary militancy requires true believers; scientific method rejects the possibility of absolute truth . . . 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." These characterizations of revolution and science strike me as odd. Revolutionary activity requires true believers to be sure but it also requires a willingness to change one's views, to adopt new strategies at critical junctures. Was not a certain revolutionary skepticism the secret of Lenin's success and that it was sometimes found wanting the secret of Trotsky's ultimate demise? Equally, as I shall be at pains to show, the skepticism of the scientist is ineffective without passionate commitment not just to the scientific enterprise but also to a given theoretical framework. A certain dogmatism is necessary to discipline and channel the readiness to abandon one set of beliefs for another. Without dogmatism there is only chaos.

Given then that the opposition of science and revolution is far from obvious, indeed arbitrary, what are we to make of Wright's insistence on that opposition? It permits a shift of commitment away from revolution toward science. As individuals we have to make a choice, he seems to be saying, either we take the high road of science with its inherent skepticism toward final truth or we take the low road of revolution with a religious commitment to a mobilizing ideology.<sup>2</sup> Society also has to make a choice: at the extremes we have the repression of science—the Stalinist solution—and on the other side we have the repression of revolution—the liberal solution. Wright appears to be more inclined to opt for the latter.

By presenting, what I believe to be a false antithesis, he readjusts the relationship between truth and politics. In his Berkeley days, he 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" (p. 44). There was an unquestioning faith that truth would serve the Marxist cause, adopted on political and moral grounds. Now, the tables are turned and the ultimate grounding of Marxism is its truth. If it is not true then it is not politically defensible. For Wright to call himself a Marxist is to believe that Marxist theories are true or more precisely the closest approximation to the truth. They are the most faithful maps of the world we possess. Should feminism

2. Wright puts to rest any doubt where he stands in the choice. In note 19 he writes, "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 defending the vision of that telos are not so different from theology."

demonstrate a greater "truth," produce better explanations, then he would transfer his commitment.<sup>3</sup>

It is no longer Marxism per se that is emancipatory but its truth, its correspondence with the "real" world. Wright believes that the pursuit of an autonomous science, what we used to call "theoretical practice" after Althusser, is a necessary weapon of emancipation. Bhaskar supplies the rationale: "the essential movement of scientific theory will be seen to consist in the movement from the manifest phenomenon of social life, as conceptualized in the experience of the social agents concerned, to the essential relations that necessitate them. Of such relations the agents involved may or may not be aware. Now it is through the capacity of social science to illuminate such relations that it may come to be 'emancipatory'. But the emancipatory potential of social science is contingent upon, and entirely a consequence of, its contextual explanatory power."<sup>4</sup> As scientific knowledge approaches a cognitive appropriation of the real mechanisms it will be a more effective instrument of emancipation.

When social science shows that pre-existing ideas are false and at the same time necessarily generated by real mechanisms, then we have a critique of ideology as false consciousness. This is how Bhaskar can claim that scientific Marxism is also critical theory.<sup>5</sup> Establishing the discrepancy between a "scientifically proven" reality and the commonly accepted reality becomes a road to emancipation. It is a form of idealism at odds with the materialist theses that ideas cannot be abstracted from the context of their production and that they have a social force only when they resonate with the interests of actors. It is no accident that a

3. As I shall argue below there is no danger of demonstrating the superiority of feminism over Marxism or *vice versa*. Such broad frameworks with their very different problematics are incommensurable. The corollary is that Marxists who abandon Marxism because it is false are rationalizing a rejection based on other, usually political or moral, grounds. So I predict that Wright will always be a Marxist, despite his skepticism.

4. R. Bhaskar, *The Possibility of Naturalism* (New York: Humanities Press, 1979), p. 32.

5. Here Wright departs somewhat from Bhaskar. When Wright says that "scientific Marxism is a variety of critical theory" he means to restrict the standpoint of critique to tendencies immanent in reality. Those tendencies are discovered scientifically by penetrating appearances to real mechanisms. By identifying what could be with what ought to be he avoids arbitrary or utopian moral critique. But what if there are a multiplicity of tendencies? What happens if those tendencies point only to a bleaker future? Can one even decide what those tendencies are? For example, Wright has a fascinating theory of history in which the movement from one epoch to another entails progressive elimination of forms of exploitation. This is indeed a bold conjecture but is there any reason to believe it apart from its aesthetic and optimistic qualities? Could one even justify it on scientific grounds? Is it not a morality wrapped in the guise of science, a morality that is grounded in extra-scientific assumptions? Anyway Wright is just not clear about the relationship between normative and scientific claims.

thorough going realism ends up as a form of idealism. They are natural bedfellows. Endowing the pursuit of "truth" with an emancipatory function justifies the eclipse of the material forces of revolution.

Nor is it difficult to understand why it might be appealing to bracket revolution in order to give science undiluted attention. These are not revolutionary times here. It is just difficult to be a revolutionary in the United States today without also being isolated as a lunatic. But that is no reason to make a virtue out of a necessity, to celebrate the surrender of revolutionary goals in the name of science, to adapt to the exigencies of the day.

Furthermore, the demise of radical groups and movements has also forced Marxism to retreat behind academic walls, so that we are tempted to adopt other academics as one's reference group. Here there is the danger that Marxism be reduced to an ideology of intellectuals whose professional interests masquerade as the interests of all. It has always been difficult to be a Marxist in the United States; it is particularly difficult when conservative politics are ascendent. So there are no easy responses to the pressures corroding Marxist ideals. We require, in my view, a grounding to Marxism at odds with an autonomous science pursuing explanations of real phenomena that exist in the world independently of theory. This view of science plays into the hands of those who would wash them of revolution. We have to seek an alternative.

Is Bhaskar's science then the only possible Marxist science? My claim is that not only is it not the only form of science but it has no privileged position among the different sciences. In the brief comments that follow I want to argue that the realist view of science is fundamentally flawed on its own terms so that we need not be shy about picking an alternative which holds greater promise for the unity of science and revolution. I develop such an alternative which comprehends knowledge as produced and validated through transformative practices. This applies no less to scientific knowledge which advances through the generation and then solution of anomalies that emerge through engagement with the world.

### 3 Is a Realist Social Science Possible?

Bhaskar's book is called *The Possibility of Naturalism*. He asks what must be true about the social world if we are to study it as we study the natural world. He passes lightly over the problems of knowing that social world. Yet these problems appear to turn the possibility of naturalism into its impossibility. And I think Wright's work demonstrates this conclusion.

Wright sets up a model in which "facts" are doubly determined, on the one side by real mechanisms acting independently of our knowledge and on the other side by the social and conceptual context of the production of knowledge. That "facts" are shaped by social practices and theoretical frameworks sets up an insuperable barrier to any direct apprehension of those real mechanisms. We can never be sure what in the data is the result of the mechanisms we seek to comprehend and what is due to the "distorting" influence of the scientific process itself. Furthermore, because the mechanisms cannot be directly comprehended, any set of observations which seem to refute a conjectured mechanism can be countered by postulating an additional alternative mechanism. The inescapably provisional character of any claims about the existence of particular mechanisms leads in two directions: the abandonment of a realist approach to science or the willingness to live with an acute and chronic uncertainty. Wright prefers the quicksand in which we must be prepared to relinquish our views at any moment. His commitment to a realist approach to science is the source of his celebration of skepticism.

Since we cannot apprehend real mechanisms directly there is no way of knowing whether a postulated mechanism corresponds to reality. The best we can do is try and show it is nearer "reality" than some other rival postulate. Let us consider Wright's example of the adjudication of his own and Poulantzas's concept of class (*Classes*, Chapter 5). Here he shows that those people who fall into Poulantzas's middle class but into Wright's working class are more like the agreed-upon workers than the agreed upon middle class in terms of their income and their class consciousness (as measured by Wright's variables). The results clearly favor Wright's concept. But he is very cautious in drawing the conclusion that the mechanism corresponding to his concept is nearer "reality" than Poulantzas's.

First, alternative mechanisms might be postulated that explain the apparent superiority of his class concept. Perhaps some other mechanism is at work which would explain why the people in the disputed category should be closer to workers than the middle class. Wright examines two—gender and trade union membership. When, first, men and women, and then trade union members and non-trade union members, are separated out his earlier conclusions are if anything strengthened. But one can always think of further possible mechanisms to take into account, for example, size of employing enterprise. It might be that those of Wright's workers who Poulantzas regards as petty bourgeois (essentially those who are unproductive or mental laborers) are to be found in large firms which promote greater working class consciousness.

A second source of uncertainty lies in the production of the data. Namely, it is possible that the mode of questioning, and the questions asked predispose the people in the disputed category to give answers more like workers than middle class. For example, research shows that survey respondents are sensitive to the order in which questions are asked. Wright's questions concerning the class position of respondents, which come before the attitude questions, highlight issues of autonomy and subordination in the workplace which would lead unproductive and mental laborers to identify with the working class. A survey that emphasized more the productive/unproductive and mental/manual divisions might give results more consonant with Poulantzas's concept.

Finally, there is the uncertainty of barking up the wrong tree. That Wright's concepts do better than Poulantzas's is not incompatible with the view that they are both wide of the mark and that a third very different set of concepts drawn perhaps from a different theory, say Weberian status groups, does much better in explaining the distribution of income and attitudes. The top of the mountain may be enshrouded in clouds so that Poulantzas and Wright don't realize they are scrambling around in the foothills. So the first task must be to try and discover how far they are from the summit by dropping their rivalry and staring around. They should start by comparing theories and not concepts. Of course, Wright did in fact do precisely this in his dissertation turned book, *Class Structure and Income Determination*. There he claimed to show that human capital theory was less successful in explaining income inequality than his own class theory based on contradictory class locations. But, as he himself admits, such an adjudication is fraught with even more uncertainties than the adjudication with Poulantzas. Given the infinite array of alternative mechanisms they could draw upon and the different ways of conceptualizing and thus measuring income inequality, human capital theorists would have no difficulty in countering with a model that did better than Wright's. And it is just as possible that a theory of status groups could be made to do better than both.

All I want to suggest here is that the process of adjudicating among concepts or theories in terms of their capacity to comprehend mechanisms which exist independently of our knowledge but whose existence we can only apprehend through that knowledge is not just a hazardous but a futile task. Wright is only too well aware of its pitfalls but still feels that through skepticism and honesty we can approach the truth.

#### 4 Adjudication versus Puzzle Solving

It is not just that adjudication in the hunt for real mechanisms is futile but it comes at great cost. Let us return to the example of Wright versus Poulantzas. How might Poulantzas have responded? Poulantzas's agenda was to understand how advanced capitalism survived the revolutionary temper of the working class in Western Europe. One answer concerned the character of the state. A second answer concerned the development of a class alliance between the old and the new petty bourgeoisie. This theory is not refuted by demonstrating that it explains less of the variance in attitudes than Wright's concept of class for two reasons. First, he is concerned with countries such as France (not the United States) where there had been intense class struggle and second, he would insist on more activist conceptions of class conflict (including strikes, participation in social movements, etc.). Of course Wright recognizes this problem, that to adjudicate between two concepts they have to have the same explanatory task. That's why he sets up his six strictures on the Marxist concept of class. Yet even if all Marxists abide by those strictures, their agendas in formulating the concept of class may still differ.

In order to adjudicate between two theories the imposition of homogeneity of explanatory task is even more severe. To adjudicate between Marxist class categories and human capital theory one has to assume that the goal of each is to explain income inequality.<sup>6</sup> This is a dubious proposition since human capital theory is concerned with the efficient allocation of resources whereas Marxism is concerned with the transcendence of capitalist inequalities. In a sense different theories are climbing different mountains. The agendas are different and therefore in Wright's terms they are incommensurable. If it is feasible at all, adjudication can only take place between concepts that have been designed to explain the same phenomena. This can only take place within a single paradigm and even then within a very constricted conceptual space.

Two conclusions seem to follow. First, the choice between theories and often between concepts cannot be reduced to a process of "empirical adjudication." Extra-empirical considerations necessarily enter. In other words the antithesis between analytical and dogmatic Marxism is a false one. Contra Wright, it is necessary to defend the "use of concepts through a variety of other forms of argumentation," including

6. Wright recognizes that income inequality is only one concern, and a minor one at that, of both human capital theory and Marxism and that he is not in fact adjudicating between the two theories.

“arguments based on ulterior political justifications” (p. 25). To deny the necessity of dogma is in fact to subvert the analytical process 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” (p. 25). I challenge him to defend his commitment to Marxism in terms of its capacity to produce more “truthful” explanations of real phenomena than say Weberian theory. I am not seeking to substitute political criteria for empirical ones. The claim is weaker: empirical considerations are a necessary but not a sufficient criterion in theory choice.

My second conclusion concerns the effect of empirical adjudication on the development of theory. Homogenizing the explanatory tasks of concepts in order to make them compete involves suppressing the problems for which they were designed. Analytical Marxism as Wright defines it has little to say about the problems of Marxism. The essential task is to define the concepts clearly so that their explanatory power can be measured. We hear little about the abiding anomalies of Marxism or of Marxist theories of class: the defeat of the working class in the West and the East, the failure of Western working classes to fulfill their revolutionary mission and of the Eastern working classes to inaugurate a classless society.

Rather than define Marxism first and foremost by the strictures it imposes on the concept of class I would define Marxism by a historically evolving sequence of anomalies, misfits between two theoretical constructs: anticipations and facts. Where Wright sees the growth of knowledge as a process of adjudication through which we achieve ever closer representations of reality I view it as solving puzzles and in the process creating further puzzles. Theories or rather the sequence of theories which compose what we call research programs can be compared on the basis of their puzzle solving capacity or the generation of new facts, anticipations some of which are corroborated. This is not to deny the importance of adjudication between rival concepts or theories but rather to say that the adjudication takes place with respect to the prior elucidation of particular puzzles.<sup>7</sup> The task is not to decide how the world “really is” which seems to be Wright’s (in my view impos-

7. Progressive research programs are ones in which puzzle solving leads to the prediction of new phenomena, some of which are corroborated. Degenerate research programs tend to patch up anomalies without generating any new knowledge. Thus, where Wright sees adjudication very much as the capacity to explain what we already know and argues that prediction is futile, I would argue the opposite. It is easy to provide *ad hoc* theories to explain what we already know to be the case but it is quite difficult but not impossible to develop theories that successfully predict hitherto unknown and unexpected phenomena. This is the real test of theoretical advance. The great Marxists, including Marx, Luxemburg, Lenin, Trotsky, and Gramsci were all endowed with great prophetic powers.

sible) agenda but to decide why the world doesn’t conform to Marxist expectations!

## 5 Positivist Knowledge versus Practical Knowledge

The argument above adds up to the following: the realist view of science is strong in stating its ontological premises but weak in dealing with the epistemological problems it raises.<sup>8</sup> We can never be sure we are approaching those real mechanisms and the attempt entails a necessary separation of science and revolution as well as the repression of the fundamental problems which define Marxism. The realist ontology does not sit comfortably in a contemporary Marxist chair so we should look around for an alternative which permits the unity of science and revolution at the same time as promoting the progressive development of the Marxist research program—a program that keeps up with the puzzles history continually generates while constructing new belts of theory to solve those puzzles and stimulate new ones. The rest of the paper elaborates such an alternative ontology.

Part of the realist world view is to see only one alternative to itself, namely idealism, that the world is an ideological creation. By acknowledging that the facts are socially produced as well as being determined by mechanisms existing independent of us, Wright is already making a concession to idealism. Indeed, this is precisely what makes his process of adjudication so constraining. Instead of such a strained amalgam of realism and idealism I would propose to go beyond both to embrace a different theory of knowledge. In this perspective the world is neither external to us waiting to be mapped nor is it a figment of our imagination but exists in an inseparable relationship to us. The world does not exist outside our relationship to it. We cannot separate ourselves from the world we study. We create and recreate that world and in the process develop our knowledge of it. There is no way we can catapult ourselves out of our self-made prison. Rather, we have to learn to live within it. There is no archimedean standpoint of objectivity. Or as the young Kolakowski once wrote, “in all the universe man cannot find a well so deep that, leaning over it, he does not discover at the bottom his own face.”<sup>9</sup>

8. By “ontology” I mean theories about the nature of the world, about what exists, and by “epistemology” I mean theories of how we can know about the world.

9. The feminist critique of positivist science as male science—the critique of constituting the world as other in order to “master” it—leads to the same alternative ontology as outlined here. Of course, the convergence of ontologies does not preclude a fundamental divergence of theoretical and political agendas.

These two ontologies give rise to different accounts of the relationship between theory and practice. The one that Wright defends, which we can call a *positivist knowledge*, sees cognition as reflecting the world. Valid knowledge seeks to copy an external world, which is viewed as a pre-existing entity. This view can be found in the philosophical writings of Engels and Lenin. Both regard political practice as a means of *verifying* theories that seek to capture the real mechanisms that govern the world. Here theory and practice are connected but separate. The alternative perspective, that of *practical knowledge* to be found in Marx's early writings and most succinctly in his *Theses on Feuerbach*, regards cognition as an instrument of adaptation. Consciousness is born of practical needs that develop through our relationship to the world—a world that is constructed through interaction with it. Here political practice is the *basis of knowledge*, theory and practice are inseparable.

Knowledge, in this framework, is a function of engagement with the world. The more thoroughgoing, radical that engagement, other things being equal, the more profound our understanding. In seeking to transform the world we learn about the forces resisting transformation. In this sense, science requires revolution. It is no accident that the most profound and prophetic Marxist thought has come from reflections of those most deeply engaged in revolutionary activity. It is not a passive organization of pre-given data designed to maximize the explained variance that leads to understanding the forces operative in the world but engaging the world, challenging it, putting it on trial.

If science thrives on revolution, does revolution profit from science? That's a more difficult question that we can begin to answer only by following a further implication of our theory of knowledge. If knowledge is produced through engagement with the world, different engagements produce different accounts of the world which are equally valid so long as they conform to certain technical requirements, such as agreement with commonly accepted experiences.<sup>10</sup> This necessarily introduces a certain relativism in which theories correspond to constellations of interests. The production of knowledge is therefore an inescapably political process.

Let us take an example from Wright's reflections. "Whether or not imperialism is a real cause of deepening underdevelopment in parts of the Third World depends on how capitalist penetration actually works, not upon the categories of imperialism" (p. 24). This sounds reasonable

10. Because human beings share a great deal in the ways they appropriate the world, so our knowledge of the world must conform to certain common experiences, which are then constituted as facts.

but is it? We have already argued how difficult it is to ascertain those "real clauses" precisely because our data are so colored by our "categories" and because any postulated mechanisms can be saved from refutation by the conjecture of another one. But there's more to it.

Within Marxist theory a seemingly endless and fascinating debate has unfolded as to the causes of underdevelopment. On the one hand there are those who stress the transfer of surplus from peripheral to core countries (satellite to metropolis). This is the premise of Frankian "development of underdevelopment," of Cardoso's dependency theory, of Wallerstein's world systems theory, of Amin's unequal development theory. On the other hand there are those who stress the mechanisms through which dominant classes in the Third World countries pump surplus out of the direct producers. This is how Lenin explains the backwardness of Russia. It is the basis of the theory of modes of production. The underlying premise is that the class character of Third World societies inhibits development. The debate seems as irresolvable as it is ferocious. At stake are the interests of two different classes or intellectuals who represent those classes, the interests of the dominant classes in the Third World who seek to blame not their own domination but external forces for economic backwardness and the interests of subordinate groups who point to the class character of the society in which they live.

But it is obviously more complicated. It is not only that perspectives on the Third World are intimately tied to the class interests *there*. They are also tied to constellations of interests in the imperial power. Radicals in the United States, working on their home terrain, aim their blows at the United States administration's involvement in Third World countries and so they are naturally led to embrace some form of dependency or world systems approach. This speaks to the political reality in which they have to operate. The point is this: to adjudicate between these two theories of underdevelopment is not only scientifically futile but ignores the entrenched interests defending each position. By not acknowledging that theory is deeply rooted in interests, adjudication does violence to the very reality it seeks to explain.

I am reminded of the most insane project I have ever undertaken. Twenty years ago I became interested in the role of education in economic development. For a set of largely arbitrary reasons I decided to study "the problem of the medium of instruction in Indian universities." I wanted to know whether from an educational point of view it would be more effective for Indian students to learn in their regional language, in Hindi or in English. There were different arguments arraigned on each side of the debate and I was going to undertake a scientific adjudication! I planned to administer a comprehension test to

economics students in different colleges in different parts of India. Half a class would take the test in English and the other half the same test but in the regional language. Believe it or not I actually carried out this research in four states, chosen for their different language policies in education.

However, I was quickly shaken out of my naivety when I discovered there was no way to isolate the educational question from political issues. The struggle over the medium of instruction, involving demonstrations, riots and strikes in many states, was a struggle over regional autonomy and the class and regional distribution of opportunities for upward mobility. Only from the haven of a distant university could one imagine reducing the problem to an adjudication of the educational effectiveness of those different policies! When I wrote up the research I relegated the results of my enormous scientific labors to a two-page appendix. By ignoring the constellation of interests in the struggle, adjudication not only violates reality, not only condemns itself to irrelevance but, as we shall see, can unwittingly become an instrument of domination.

## 6 From the Standpoint of Politics

We can now pose two questions. First, how should academic Marxists enter the political fray? That is, how should science enter practice? Second, how should the political fray enter the academic terrain? That is, how should practice enter science? This section addresses the first question while the following section addresses the second.

For knowledge to have an effect, that is become a social force it has to resonate with the relevant practices. So that if social scientists want to shape the world they must work very closely with those whose interests they seek to defend. As the following research underlines, this involvement should embrace not simply the production of knowledge but should extend to its dissemination.

Fifteen years ago I completed a study of the localization of the labor force in the *Zambian Copper Mines*. At the time of independence in 1964 Zambia was a prototype of the enclave economy. Ninety-five percent of the country's export earnings came from copper production, controlled by two multinational corporations—the gigantic South African based Anglo American and the British Company, Roan Selection Trust. Zambia was the fourth biggest copper producer in the world. The mines were run by white managers, engineers, and administrators. A strict color bar prevailed, in which no blacks had any authority over any whites. In the colonial era the mining companies,

trying to capitalize on cheap black labor, had tried to “advance” blacks into higher positions by fragmenting tasks hitherto monopolized by whites. “Africanization” had been slow because of resistance from trade union and staff associations representing white employees and where it did take place it never breached the color bar, but rather shifted its position. In the years after independence several reports on *Zambianization* appeared. Figures were presented showing that indeed *Zambians* were moving into higher level positions and that the number of expatriates was declining albeit at a slower rate. What was the story behind these figures?

In 1968 I took up a position as a research officer in the mines' personnel research unit. My hidden agenda was to study the companies' strategy to the new post-colonial regime. *Zambianization* of mine employees became the focus of the study. I spent one and a half years working for top management in the personnel field, followed by two and a half years of further research while I was a student at the University of Zambia. In opposition to the “neo-colonial” explanations of underdevelopment, stressing the subordination of the *Zambian* economy to international economic forces, I chose to focus on the perpetuation of class relations from the colonial era. I argued that all the attention given to the *Zambianization* of the labor force concerned the movement of blacks into higher positions and distracted attention from the unchanging class and racial order of the organization. *Zambianization* forecasts had been fulfilled but without undermining the color bar. Where *Zambians* were promoted into white positions, a new higher position would be created into which the displaced white would be moved. Alternatively an entire department might be *Zambianized* and at the same time stripped of its previous functions which would be handed over to a new body made up of whites. Naturally *Zambian* workers and expatriate managers blamed the helpless *Zambian* successors for the inevitable lapses in organizational efficiency.<sup>11</sup>

Why was no one doing anything about this? Did the government know? Of course they did but their interests, I argued, were to ensure that copper mines continue production. They did not want to rock the boat by insisting on more orderly *Zambianization* which would have scared expatriates. Furthermore, they were quite content to have expatriates running the mines because they were politically weak. Had *Zambianization* proceeded from the top down this might have provided an alternative base of power from which to threaten the government.

11. As I have since learnt, these strategies are quite common in US organizations where women or blacks are “advanced” into higher positions in the name of affirmative action.

The trade unions had been largely muzzled and the workers had been pacified with wage increases.

With the manuscript complete I went to the mining companies, who still knew nothing of this research, to seek publication. They were dumbfounded and told me I must be out of my mind to think that they would let me publish an independent assessment of such a sensitive political topic as Zambianization, let alone one with such controversial conclusions. I protested. 'Was I factually incorrect?' How could I be since I defended my case studies with company "facts" collected while I had been an employee. No, the problem was my interpretation. I got annoyed, threatening to publish anyhow so they compromised by sending me to the government.

Two years previously the mines had been nationalized, cementing the apparently cozy relationship between state and corporation. I went to see the officer responsible for Zambianization on the mines. This newly created position was occupied by an expatriate who had left the mines. It was my fortune that he wanted to clean up the Zambianization program. He read my manuscript and quizzed me endlessly and finally said he thought it was terrific. Why? Because it was an "objective" scientific account. Oh, yes? Well, he said, you criticize the mining companies, the government, the expatriates, the black trade unions and even the workers. So? So it must be objective. Because it culled lots of statistics as well as offering in-depth insider analysis, because it took a stance against everyone, because it would be published by the Institute for African Studies, and because I was an academic it had all the trappings of objectivity and therefore could be that much more effectively used against the mining companies. Science is mobilized not in the abstract interest of truth but in the concrete interest of domination.

This is even clearer if we continue the story. Following publication and the commotion the report stirred the mining companies used it as a weapon against the mine managers to trim their bloated organizational structures. A study blatantly hostile to the mining companies was used by them to advance their profits by streamlining the Zambianization process.

It was profoundly naive to think that by casting out an indictment of the most powerful forces in society, they would be forced to compromise their interests. It is not enough to work with oppressed groups in the research process, the collaboration has to continue in the process of dissemination. Because we don't control the balance of political forces our work can always be used in unintended ways. This danger can only be minimized by continuing engagement and collaboration with "progressive forces" to the bitter end. This, of course is fraught with problems. It is not always simple to identify which are the progressive

forces. Nor does one necessarily want to let their world view dominate one's own. Nevertheless, the alternative of staging politics from the academic pedestal is a risky venture.

Whatever its other outcomes engaging those one studies does compel recognition of one's interests as an intellectual, that politics is an inseparable part of every aspect of knowledge production. In light of the above accounts it is difficult to sustain the distinction between an interest-propelled process of discovery and an interest-free process of validation. The role of "observer" is no less entangled with contextually defined interests than the role of "participant." But by not engaging the world one studies one can eclipse the constellation of interests around the scientific process. Thus, although Wright's methodology leaves no space for systematically reflecting on the significance of engagement, this is not to deny that he is actively engaged. But it is an active engagement with a restricted group of academics, who share a common interest—the suppression of their interests as academics. They become neutral arbiters in the search for truth. Those "studied" partake neither in the production nor the consumption of knowledge so the interests of the academic remain unchallenged and hidden.

A strange dualism emerges from the realist perspective in which the scientist is the dispassionate seeker after truth whereas the beliefs of those being studied reflect their class, race, or gender. Paradoxically, Wright does recognize that intellectuals have interests too—he has written about them. And when he tries to explain why his theory has not drawn greater support, he writes, "support for Marxism as a social theory is not primarily a question of belief in its analytical and explanatory power. It is primarily a political question" (p. 45). He, on the other hand, founds his own commitment to Marxism precisely on its analytical and explanatory power. It is not simply that this puts him in a different (implicitly superior) category from almost every one else but the basis of that difference is illusory. Wright's own relentless rigor and honesty demonstrate the impossibility of demonstrating the scientific superiority of Marxism.

It is a trite observation that as academics we are no less subject to institutional pressures, hierarchies, and interests than anyone else. In the preface to *Classes* Wright declares that changes in his institutional position may have affected his work but he has tried to be self-reflective and to minimize the negative effects. Here Wright is uncharacteristically unrigorous. Indeed positivist knowledge has to be unrigorous about its own determination if it is to uphold its claims to universalism. Practical knowledge, on the other hand, compels and provides a basis for being rigorous about the determination of science by the context of its production, validation and consumption. Science is no less socially

determined than any other knowledge. Within the university political struggles—be they over a nuclear weapons laboratory, unionization of clerical workers or teaching assistants, affirmative action, academic freedom, or the curriculum—are not a distortion of true knowledge production. Political struggles do not contaminate the pursuit of truth, they are the pursuit of truth. Science is a political process. But what sort of science is this? Let us now look at the theory–practice couplet from the standpoint of the development of theory. Let us focus on the advancement of Marxism as a body of knowledge.

## 7 From the Standpoint of Science

Wright recognizes a double determination of facts, on the one side by “real mechanisms” and on the other side by the conceptual apparatus and the interaction of subject and object. The elucidation of real mechanisms, I have argued, is an impossible task so I begin at the other end of the determination equation—knowledge shaped by the social and theoretical context of its production. What he regards as an annoying distortion, impeding our grasp of real mechanisms, I regard as the defining character of knowledge.

But, how do we choose between systems of knowledge deriving from different social and theoretical contexts? First, theories must explain commonly agreed experiences as well as being internally consistent. Conformity to such empirical controls and to technical rules still leave competing theoretical systems. Marxists have often argued that the superiority of Marxism rests in it being the “world view” of the proletariat—whose privilege it is to emancipate humanity. When the working class “betrays” this privilege by not embracing Marxism intellectuals deem it guilty of false consciousness. This is the very opposite of a knowledge based on engagement. It is not my solution.

The choice of a particular system of knowledge involves political, moral and aesthetic judgments as well as an empirical judgment. Objective knowledge cannot be reduced to knowledge in pursuit of empirical reality but stems from commitment to the theoretical framework one adopts to examine that reality. It is the anchor within Marxism that allows me to make sense of the experiences produced through engagement in the world, to turn those experiences into knowledge that can then be mobilized to advance Marxism. And by the advance of Marxism I mean the solution of the anomalies at the center of Marxism, the development of what I earlier called a progressive research program.

What do I mean by engagement with the world? I mean actively participating in the lives of those one studies. In historical work I am

arguing for a dialogue with the past from the standpoint of involvement in the present. It means making the fact that we are simultaneously participant and observer in society an inescapable reality. That involvement grounds one in the real empirical interests of those being studied—the participants—at the same time that Marxism provides the necessary lens through which to interpret what one experiences through this engagement—a lens which also has an associated set of interests.

When I began working in a South Chicago machine shop, the overwhelming experience was how hard people were working—harder than seemed warranted by the ostensible rewards. From this emerged the question of how consent was organized on the shop floor. I asked my fellow workers why they worked so hard and they laughed at me, saying they weren't working hard at all. They were getting away with murder. How effortlessly they seemed to have accepted management's norms of hard work. What an effective means of exploitation! Perhaps, then, all the talk about the role of state, family and ideology to explain the dampening rather than deepening of class struggle is unnecessary—consent is manufactured at the point of production. Was this true of other factories, other capitalist countries, other periods of history, socialist societies? This was my second question. I conjectured that in socialist countries interests are organized very differently in the workplace. Working in a Hungarian steel mill I was struck by the way in which the organization of production systematically engendered opposition to socialism for failing to live up to its claims. Paradoxically it is in state socialism that workers, although hostile to socialism, actually act in defense of its principles.

The process of discovery is simultaneously a process of validation. Each moment of each day on the shop floor becomes a trial of one's conjectures, hypothesizing that people will react in particular ways to given situations and trying to resolve the anomaly when they don't. This can take place on a very micro level of everyday interactions or it can take place in a “social drama,” sometimes provoked by one's own presence. For example, my friend János (who had spent time with managers in their offices and on the shop floor) and I wrote a paper which argued that in a socialist enterprise there is a bimodal distribution of functions among managers. Top managers have to bargain with the state for resources, subsidies, production profiles, etc. while shop floor management has to retain a lot of autonomy in order to adapt the exigencies of shortages. It means that the layers of middle management have no clear function and their redundancy is reflected in disruptive interference on the shop floor backed up with punitive sanctions, particularly fines.

According to the rules of the Hungarian Academy of Sciences if

János wants to publish case study material he must first submit the paper to the enterprise concerned. Well, we did and the Academy was informed that this was not an objective report and was damaging to the company. A representative of top management said that he actually would have liked the analysis had it been of another company but since they wanted to export steel to the West such an article in English wouldn't be to their advantage. [It would be difficult to hide the identity of the steel mill without losing essential detail in our exposition.] The paper was handed to middle management for their comment. It was ritually condemned at a meeting of thirty managers from the plant. We then received sets of written comments on our paper from middle managers, supposedly refuting what we had said. As far as we were concerned these comments only further convinced us that middle managers didn't know or didn't want to admit they knew the true character of work organization. One manager, who had been away when it all blew up, commented in front of another manager that he thought that the analysis was fine. The following day he was furiously berated in a meeting of managers, "We don't need people like you around here." Handing back one's work to those one studies is a way of learning one's interests as a scientist. But it is also a means of validating and developing one's explanations. Though don't expect anyone to like you for it. Don't expect any rosy consensus in the name of truth.

But does this mean we should rule out the use of surveys? The more the survey is abstracted from the context in which it is carried out the more skeptical I am about its use. Surveys that are administered to a community in which the sociologist is already involved are more meaningful than national samples that ride rough shod over diverse contexts. The results of surveys are often more revealing in the effects their use engenders than in the abstract responses to their questions. Let me offer a last example.

While studying for my MA at the University of Zambia I became actively involved in the newly established student Sociological Association. Colonialism had left Zambia with only a hundred university graduates and so the university was grooming a new elite. We thought that one of the functions of the Sociological Association could be to regularly tap student opinion. We did this with considerable success, using questions ranging from domestic and international political issues to the quality of campus food. In 1971 there was a demonstration outside the French Embassy over rights granted to South Africa to manufacture Mirage jet fighters. A battle with the police ensued, many students were arrested and one lay in hospital with a bullet in his thigh. Rather than endorsing student support for government anti-apartheid policy, President Kaunda admonished students and told them to leave

matters of foreign policy in his hands. Piqued at this rebuff the executive of the student union signed an open letter to President Kaunda, accusing him of "hypocrisy" and "inconsistency." In response the ruling party organized massive demonstrations of solidarity with the President. Threats to invade the campus by party youth brigades led students to mobilize themselves behind their union executive and barricade themselves in. But the defenses were not strong enough to stop the military, paramilitary and riot police marching on to the campus at 4 a.m. and herding us out into a field at gunpoint. The student executive was expelled and the university was closed down for six weeks.

When classes resumed I and another student decided to run another survey of student opinion. Among other matters we were asking students for their opinion about the closure, who should make foreign policy, the nature of democratic politics, etc. Fielding the survey and publicizing the results generated a furious battle, revealing the true factions on campus and their connections to outside forces. The social and political structure of the student body was laid bare by the sociological investigation, not in its empirical results which revealed little, but through its social and political effects. From this perspective the social context is not a contaminating influence, a barrier to discovering those underlying mechanisms, but becomes the very object of investigation by examining the consequences of applying the survey instrument.

Don't get me wrong. I am not claiming that there is anything "revolutionary" in these examples. What is essential is that the methodology they embody is not incompatible with revolutionary commitment. I am only trying to defend a perspective that *can* be both scientific and revolutionary. Indeed it is a method in which revolutionary activity can give rise to the greatest advance in science. Revolutionary activity is activity that challenges the status quo in the most radical way and therefore reveals most vividly the lines of interests, the constellation of social forces resisting and promoting change. At the same time, the more revolutionary the intervention in society, the more it threatens to transform social and political structures and the more necessary is a science to guide it. At a moment when societal structures are at their most fluid, expanding the range of possible development, we also require a flexible theory to guide us through the uncertainty.

## 8 Conclusion

I can now return to my original question: what sorts of Marxism are appropriate to the present period? By claiming that theory too was a practice, "theoretical practice" cut the umbilical cord uniting Marxist

theory to practical activity. It was designed to create a breathing space for intellectuals within the French Communist Party. It was taken over by some American academics to win a place for Marxism within the university. This was at a time when one could still talk about socialism in the United States with a straight face, when *Socialist Review* was still called *Socialist Revolution*, when the tide of popular struggles had not completely receded from the campus.

Theoretical practice now justifies a rigorous science at odds with radical politics. Political quiescence has cut Marxists adrift within the university to find a new equilibrium, one that is shaped by interests within the liberal university. Professionalization threatens to reduce Marxism to an ideology of intellectuals whose interests are systematically concealed by the veil of neutrality surrounding the pursuit of science. Venturing beyond the narrow community of Marxist academics and engaging people with other interests has two benefits, apart from the possibility of directly affecting change. It makes us aware of our interests as academics and it fosters the solution and generation of anomalies that define the Marxist research program. When the mountain doesn't come to Mohammed, Mohammed must go to the mountain.

### Postscript

As ever committed to dialogue and truth Wright read the penultimate draft of the above essay. After making detailed criticisms he concluded as follows:

The irony in much of your commentary is that many of the criticisms you raise could be raised equally by someone committed to a realist philosophy of science. I found nothing in your discussions of Zambia and Hungary or your comments on adjudication with Poulantzas unreasonable, but I also do not see them as representing some radical methodological alternative. Above all, aside from the specific issue of whether or not one should ever try to bring evidence to bear in arguments in favor of one theory (or concept) over another, I do not believe that there are great consequences in practice from our differences.

I have two comments.

First, examples chosen to illustrate features of one ontology can obviously be understood from within an alternative ontology. Nevertheless, from the same account our different frameworks draw different conclusions. Wright relegates the issues I raised in the Hungarian and Zambian examples to the province of "the sociology of knowledge"—how scientists discover knowledge, how participants respond to scientific

knowledge. He sharply distinguishes the context-dependent *production and consumption* of knowledge from the context-independent *validity* of that same knowledge. I, on the other hand, am arguing that the very criteria distinguishing truth from falsehood are themselves contextually shaped. By withholding the academic context within which the correspondence theory of truth gains acceptance he gives it an aura of universality. Realist ontology and the correspondence theory of truth are thereby presented as natural and inevitable. Rival ontologies such as the one explored here and rival epistemologies such as the consensus view of truth (in which truth is what we agree to be true) or the pragmatic view (in which truth is what works) are not so much as mentioned. And so the consequences of his analytical Marxism (the inability to ever know whether one is approaching truth, the opposition of science and revolution, and the emphasis on adjudication at the expense of the examination of anomalies that face Marxism) are presented as the unavoidable if unpleasant facts of scientific life when in reality (!) they are products of a particular ontology and a particular epistemology.

Second, I leave it to the reader to decide whether there are significant differences in the way we practice Marxist science. But to the extent that there are similarities, this may not be because holding divergent theories of knowledge makes no difference but because we don't follow our prescriptions. On the basis of realism and a correspondence theory of truth alone I don't believe Wright would be able to defend his commitment to Marxism. In my own case, in order to persuade academic audiences of the validity of Marxism, I adopted their criteria of truth and falsehood. It was a defensive maneuver whose implications I have only slowly and dimly begun to recognize. While I think it imperative to continue the war on that front and here Wright's work is critical because it is consonant with conventional sociological practices, nevertheless the more urgent task is to try and practice the methodology I have proposed in this essay. To save sociology or to save Marxism—that is the question!

# REPLY TO BURAWOY

*Erik Olin Wright*

I would like to briefly discuss three issues raised in Michael Burawoy's comments: the tension between commitment and skepticism in science and revolution; the relationship between knowledge and emancipation; and the relationship between a realist ontology and scientific knowledge.

## 1 The Tension Between Commitment and Skepticism

From early in its history, Marxism has been troubled by tendencies for Marxist theory to degenerate into Marxology. Debates over theoretical issues are often waged through an idiom of interpretation of *texts* of Marx (or Lenin or Stalin or Mao, depending upon the historical context). Scientific writings become viewed as sacred texts in which authoritative readings become the criteria for truth. This is the sense in which Marxism can become more like a theology than a science. Any defense of the harmonious "unity of theory and practice" needs to be able to account for the strength of this tendency within the Marxist tradition.

In my comments I suggested that this tendency reflects an inherent *tension* (not polar opposition, but tension) between the psychological states required for revolutionary practice and scientific activity. The former, I said, required absolute commitment; the latter, perpetual skepticism. Since Marxism was simultaneously the ideological foundation of revolutionary movements and the theoretical foundation of a social science, it embodies this tension and, in certain times and places, one mode or the other dominates.

The rhetoric with which I expressed this tension was perhaps somewhat overdrawn. It is certainly excessive to say that revolutionary movements require "true believers" instead of open-minded activists prepared to learn from their mistakes. The image I conveyed was of the revolutionary as fanatic, and while tendencies toward fanaticism may be inherent in revolutionary movements, revolutionary commitment itself does not entail fanaticism. And, as Burawoy points out, it is equally essential for the advance of Marxist science that theoreticians have passionate commitments. No one would ever be a Marxist simply from a dispassionate weighing of the evidence and argument in its favor, without extra-scientific commitments.

To say that scientists need deep commitments and revolutionaries need open-mindedness, however, is not to deny that a tension between these modes of thought exists. It may be true, as Burawoy suggests, that without dogmatism (or at least some degree of dogmatism) there is only chaos, but this does not eradicate the tension between the intellectual rigidity encouraged by dogmatism and the intellectual flexibility required to learn new things. What I described as the tension between revolutionary practice and revolutionary theory, therefore, may equally be a tension within the practice of revolutionary theory, but it remains a tension nevertheless.

## 2 Knowledge and Emancipation

Burawoy poses an interesting contrast between the view that "truth would serve the Marxist cause" and the view that "the ultimate grounding of Marxism is its truth." He believes that there was a time in which I believed the first of these statements, but that I am now committed to the second. Thus, if I came to believe that Marxism was not true—or, perhaps more precisely, that some alternative theoretical perspective was closer to the truth—then I would abandon Marxism.

Burawoy is correct in this conclusion: I would abandon Marxism if I came to believe that it was false relative to a rival theory *that attempted to explain the same things*. But the initial contrast he draws between truth serving the "Marxist cause" and Marxism being grounded in truth is a misleading one and leads to misinterpretations of the implications of this conclusion. I would reformulate the contrast by saying that I believe that truth serves the cause of *emancipation* (where emancipation is understood as the elimination of oppression, exploitation, domination) and that "Marxism" is the name I give to the emancipatory theory which I believe is closest to the truth. Thus while Burawoy is correct in saying that if I came to believe that Marxism was false (relative to a rival) I would indeed abandon it, this would not in any sense imply an abandonment of the moral and political cause of emancipation itself. Emancipatory interests are central to defining the kinds of questions I think are important to ask and thus the explanatory objects that a scientific theory to which I am committed should address. My commitment to these *questions* remains grounded in moral and political concerns, but my commitment to the specific concepts and explanations embodied in Marxist *answers* to these questions is based on my assessment of their truth relative to rivals.

This raises the issue of the relationship between emancipatory goals and scientific knowledge. Burawoy quotes Bhaskar as saying "the

emancipatory potential of social science is contingent upon, and entirely a consequence of, its textual explanatory power." This, Burawoy says, is a form of idealism for it sees ideas as having a social force (an emancipatory potential) independently of the interests of actors. There are two comments I would like to make on this issue.

First, I agree entirely with Burawoy that the social efficacy of ideas is contingent upon the ways in which they resonate with interests. "Truth" *per se* has no effects. Contrary to Burawoy, however, I do not think that there is anything whatsoever inherent in a realist perspective on knowledge that implies that ideas should have an autonomous social effectivity. A realist would say that in order to understand the effects of ideas we must study the real mechanisms through which ideas work in the world. These mechanisms would include a range of psychological mechanisms (through which ideas are "heard" and incorporated into cognitive and motivational structures) and institutional mechanisms (through which ideas are disseminated). It is entirely plausible that as a general "law" in the sociology of knowledge we might decide that ideas have social impact only when they resonate with the interests of actors. This claim about the conditions for ideas to have causal weight in the world, however, does not follow logically from any epistemological premises. It is a substantive claim about how the world works, not an *a priori* philosophical assertion.

Second, I cannot defend rigorously my assertion that scientific truth in fact has emancipatory potential (when this truth "resonates with interests"). It could be the case, for example, that distortions and lies aid the cause of emancipation more than knowledge. It might be the case that people need illusions of grandeur, exaggerated beliefs in their historical efficacy, confidence in the ultimate triumph of communism, in order to engage in the practical sacrifices needed to accomplish even limited emancipatory transformations. A scientific analysis which convinced people that historical materialism was false (that is, that communism was not inevitable) might thus in fact reduce the chances for even partial emancipation. Since I believe that partial emancipation is preferable to no emancipation, it might therefore be the case that defending strong historical materialism (which I believe to be a false theory) could serve the cause of emancipation (or the "Marxist cause" if you prefer). Such a situation would pose a serious moral and intellectual dilemma for me: would I support ideas which I believed to be false when I also believed them to be emancipatory?

If the interests served by particular ideas become the essential criterion for their "truth," the above dilemma of believing certain ideas to be simultaneously false and yet emancipatory would be reframed as a conflict between two interests held by a theorist: their interests as aca-

demics (which leads them to adopt realist-type criteria for truth) and their interests as Marxists (which leads them to hold emancipatory criteria for truth). The theorist thus simply has to choose which of these interests is more important: is it more important to be an academic or a Marxist? As Burawoy says, "to save sociology or to save Marxism—that is the question."

This seems to me to be an unsatisfactory resolution of the dilemma. When I say that strong historical materialism is false what I mean is that I can marshal evidence and argument which, if presented to a revolutionary *who was willing to listen and to rationally weigh the issues*, would lead that revolutionary to reject strong historical materialism. It would not necessarily lead the revolutionary to reject revolutionary goals, but it would lead to a discrediting of the theory that such goals will inevitably be achieved because of the fettering of the development of the forces of production by capitalist relations of production. Of course, some people will not "listen" and rationally consider the evidence—they are dogmatic in a way that destroys rather than complements skepticism—and thus discredited ideas can remain durably believed. Nevertheless, it seems much more plausible to *explain* this by the analysis of various social and psychological mechanisms of belief formation—cognitive dissonance, wishful thinking, pressures to conform to institutionalized ideologies, etc.—than by the global epistemological claims that truth has meaning only in terms of interests and thus different truths are no more than direct expressions of different interests.

### 3 Realist Ontology

Burawoy's critique of scientific realism rests on a critique of its ontological position (that real causal mechanisms exist independently of the theorist) and its epistemological position (that we are capable of distinguishing the relative truthfulness of rival claims about the world). These two issues are joined, for the ability to adjudicate between rival explanations of the same theoretical object hinges on the existence of a "real world" independent of our thought, since it is this independence that makes the various strategies of adjudication plausible (experimental and quasi-experimental designs, criteria of internal consistency of concepts and data, etc.). Adjudication may still be fraught with difficulties, and in many cases it may prove impossible to decisively marshal evidence and arguments to differentiate rival explanations of the same phenomena, but nevertheless if the realist ontology is correct, adjudication becomes at least possible in principle.

Burawoy rejects the realist ontology by saying that in his perspective, "the world is neither external to us waiting to be mapped nor is it a

figment of our imagination but exists in an inseparable relationship to us. The world does not exist outside our relationship to it" (p. 59). Particularly in the context of social science, there is a deep ambiguity in the collective personal pronoun used in this statement. Is the claim that the social world does not exist outside of *my* individual relationship to it, or that it does not exist outside of the relation of *people* in general to it? The latter statement seems to me eminently reasonable: the theoretical objects of social science are constituted by the relations among people and their practices, and thus the social world does not exist independently of our collective relationship to the world.

The former statement—that the social world does not exist independently of my personal relation to it—does not make sense to me. I believe that apartheid exists, that workers are exploited and that the US government is supporting right-wing movements around the world independently of my individual relationship to any of these particular social phenomena. I could, of course, be wrong about any of these beliefs, but whether or not apartheid, capitalist exploitation or support of right-wing movements exists is independent of me.

Furthermore, with the exception of a radically idealist epistemology, in *all* of the alternative epistemological positions mentioned by Burawoy—consensus views of truth, pragmatic views of truth, realist views of truth—the belief that the social world exists independently of my individual relationship to it would be considered "true." Burawoy's preferred epistemology is what he terms the consensus view of truth "in which truth is what we agree to be true" (p. 70). It would certainly be the consensual view of human beings in general (and certainly of human beings in modern capitalist societies of whatever class) that the social world exists independently of each individual person, and thus the realist ontology would be consensually validated. It is one thing to say that each person does not exist independently of the social world (since we are all constituted as persons within social interaction) or that the social world does not exist independently of people in general; and quite another to say that the social world does not exist outside of my individual relationship to it.

A realist ontology does not logically entail a realist epistemology—the view that real mechanisms exist in the world independently of our theories and our individual relation to the world does not imply the view that we are capable of differentiating the relative truthfulness of claims about those real mechanisms. But a realist ontology does imply that our descriptions of the world, and the theories we construct using these descriptions, are constrained *both* by the effects of these real mechanisms and by the concepts which we use to analyze them. This double constraint at least opens up the possibility for scientific adjudications between rival concepts and explanations of the social world.

## Classes and Class Analysis

*Guglielmo Carchedi*

By transporting the concept of capitalism from its production relations to property relations and by speaking of simple individuals instead of speaking of entrepreneurs, he moves the question of socialism from the domain of production into the domain of relations of fortune—that is, from the relation between capital and labor to the relation between poor and rich.<sup>1</sup>

In recent years E.O. Wright has emerged as an influential sociologist of the left. In his most recent work, *Classes*, he undertakes a complete reformulation of his theory of social classes.<sup>2</sup> In this process of reconceptualization, Wright touches upon a number of issues which lie at the core of Marxist analysis. A review of *Classes* is thus an important occasion to clarify the issues and assess the consequences of choosing among the different, alternative, formulations of those issues. Thus, the importance and significance of the following discussion goes much further than the assessment of this work.

There are two dimensions to this work. A "biographical" one, which deals with the reasons for Wright's theoretical shift and with a comparison between his previous and his new conceptualization. In this review I shall not dwell on it. Rather, in what follows I shall focus on the second dimension, that is, on the characteristic features of Wright's new approach. Wright has a clear and immediate style of writing. He can present complex issues in an attractive way. He also undertakes a difficult task, that of providing empirical evidence for a Marxist theory of class. He should be given credit for this and the difficulty of the task should not be forgotten in assessing his results. But, unfortunately, and

1. R. Luxemburg, *Reform or Revolution*, in M.-A. Waters (ed.), *Rosa Luxemburg Speaks* (New York: 1970).

2. E.O. Wright, *Classes* (London: Verso, 1985).