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into account, and assuming that all married couples pool their money incomes, Fuchs shows that women's economic position compared to that of men stayed about the same in the 1960–86 period, and that white, young (under 44), well-educated, unmarried women fared far better in this period than their sisters.

It is not exactly news that the household division of labor between men and women is inequitable. But, more crucially, showing that this involves real "costs" to women does not prove that the dynamics of the labor market do not also produce highly inequitable results for women independent of the impact of gender-asymmetric family commitments. In this book the latter thesis is assumed but never demonstrated. Certainly there are also gaps in the evidence advanced elsewhere in support of employer-focused theories of gender inequality at work; but the claim that women's own choices shape the labor market is at least as problematic. Fuchs' is a highly selective and partisan reading of what is now a vast literature on this topic. He presents himself as a sympathetic commentator who wishes something could be done to improve women's economic well-being. However, in my judgment, his ability to contribute to that goal is deeply impaired by his blind faith in employers and in the marketplace, and by his unwillingness to examine seriously their role in shaping patterns of gender inequality.

Macrosociology: Social Change, Social Movements, World Systems, Comparative and Historical Sociology

A World Political Economy


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The last twenty-five years have seen a dramatic transformation of macrosociology. Randall Collins (1986, p. 1346) has justly called this period the Golden Age of historical and comparative sociology, which has surpassed in sophistication and impact the achievement of the generation of Weber and Hintze that was both the model and the base of the current work. A correlate of this development was a profound change—induced by sometimes acrimonious and politicized debates—in the prevailing theoretical models and paradigms, a change that can be described negatively as moving away from conceptual formalism, questioning stylized evolutionary sequences, making the master concept of (a single) society less central, and suspending functionalist system premises. Both structural functionalism and Marxist theories were undercut by these developments or else had to be revised drastically.

At the same time, much new work returned to a more inclusive vision of the social sciences, neglecting the boundaries separating economics, political science, sociology, and history from one another. Especially in studies of Third World development there was an explicit return to the "political economy" conceptions that had dominated social science from Adam Smith to Karl Marx and John Stuart Mill and that persisted in the work of Max Weber. As in the case of Europe's capitalist development in the eighteenth and nineteenth centuries, the institutional structure of Third World countries had to be made problematic if economic growth and stagnation and the overall process of development were to be understood. These internal requirements of development studies came together with new impulses from revised and more empirically oriented Marxist positions and with the continued impact of revised and more theoretically oriented Weberian ideas.

The result is a renewal of a political economy vision that seeks to identify historically significant actors in the context of constraining and enabling macrostructures. States, classes, and powerful corporate actors are central to this perspective, as are technology, the social organization of production, markets, and power relations between and within nations. A strong tendency, grounded in Weber's work, is to consider states and politics as explanatory factors relatively independent of the role of economic relations and economically based dominance.
It is within these roughly sketched developments that the multivolume project of Robert Cox and Jeffrey Harrod must be located. In addition to the two volumes under review, two more will deal with the situation of established workers in capitalist economies and with production relations in noncapitalist countries. Cox's _Production, Power, and World Order_ presents the overall design. It claims to offer "a new approach to the study of power by identifying the connection between production, the state, and world order," as the cover announces. It can also be read as an attempt to codify the political economy approach into a theoretical framework, which then is further explored and put to the test in the subsequent volumes. Judged against these standards, the volumes published thus far represent a substantial achievement, but they are not without serious problems and flaws.

The theoretical framework focuses on _historical structures_ rather than actors; yet in contrast to other forms of structuralism, it insists that these structures are not givens but are constructed and transformed by collective human action. Cox distinguishes three levels of structure: modes of the social relations of production, forms of state, and structures of world order. Intricately interrelated with each other, none can be understood in isolation from the others.

The most salient innovation of Cox and Harrod lies in the concept of _mode of social relation of production_, which is different—more differentiated and more empirically sensitive—from the classic Marxian concept of mode of production. Cox and Harrod distinguish twelve such types of social relations of production. Defined by the power relations among workers, employers, and intervening states, these types range from subsistence agriculture, household work, and peasant agriculture controlled by money lenders, merchants, and landlords, through "primitive" labor exchange between day laborers and casual employers, to various forms characterized by degree of worker organization, state involvement, and employer dominance. The latter include "enterprise labor markets" without unions, "bipartite" and "tripartite" labor relations (organized workers and employers confronting each other directly or with state mediation, as well as "enterprise corporatism" and "state corporatism." Two forms—communalism and central planning—are unique to the noncapitalist political economies of "redistributive development" (Cox's name for today's "really existing socialism"). Several of these forms of production relations coexist with each other in the same society or social formation, with one or a few dominating the other, subordinate, modes. And it is these historically variable configurations that shape decisively the particular character of different societies.

This central concept of production relations is extensively developed, but the discussion leaves readers with a number of open questions. Though the conceptualization explicitly focuses on internal power relations, the different modes of social relations of production are actually defined largely in labor market terms, neglecting—if not ignoring completely—technology and even the immediate social relations at the workplace. Thus Cox can, for instance, say of Stalinist industrialization that "the organization of production and the hierarchy of command within capitalist production were reproduced within the social relations of central planning" (p. 96).

The modes of social relations of production are introduced as "monads, as self-contained structures each with its own developmental potential and its own distinctive perspective on the world" (Cox, p. x). This definition is immediately qualified as a heuristic decision, but the idea of self-contained production relations informs the structure of the project as a whole, with the remaining three volumes being concerned each with a specific group of "monad-modes." At odds with the very conception of _historically contingent_ structures, this decision has no particular utility other than justifying the organization of the project—itself not a matter beyond objection.

The conception of "monad-modes" also relates to the thorny question of causal priorities. This is a matter of some ambiguity in the two volumes at hand. Does a view of society "from the standpoint of the variety of forms of social relations of production" (Harrod, p. 19) imply the old Marxian substructure-superstructure conception? No. Cox asserts boldly that "new modes of social relations of production become established through the exercise of state power," and that "states undoubtedly act with a certain auton-
omy” (pp. 105 and 399). Yet the state is conceived here as including the specific state apparatus as well as historic blocs of social class forces dominating society. The state is in fact seen as the political and legal aspect of the overall system of domination. This conception is joined to the claim, reminiscent of Poulantzas’s structuralism, that the actions of the autonomous state are “consistent with the economic project of the [dominant] class as a whole” (Cox, p. 149).

Unlike many neo-Marxian structuralists, Cox emphasizes the importance of the world order. A hegemonic world order limits state autonomy through military as well as financial constraints, while a breakdown of world hegemony increases the freedom of state action. The development of national welfare states was in Cox’s view made possible by such a breakdown of hegemony, and was not just the result of changing internal class relations.

If this summary suggests a distinctive set of causal patterns that goes beyond both a simple production determinism and a bland assertion of continuous interaction among different levels of power, Cox ends his book with a formula on the role of the state that blurs critical distinctions by emphasizing what he views as essential in the last instance:

Class forces, and [external] financial and military constraints . . . all derive from production. They are different forms of power into which the accumulated results of the production process have been transformed. In being so transformed they have become divorced from the production process to become forces that can either maintain or change production relations. In defining the parameters of the state these forces mark both the dependence of the state on production and its dominance over the development of productive forces and production relations. (p. 400)

Cox, after an initial conceptual discussion, offers historical sketches of the emergence of the liberal world order, the era of rival imperialisms, and the establishment of “Pax Americana.” These historical discussions conclude with a series of propositions spelling out the central role of the “state,” its structures being shaped by internal class forces and a constraining world order and shaping in turn the pattern of dominant and subordinate modes of production relations. This theoretical armory was to guide analyses of current developments since the world economic crisis of 1973–74 and explorations of future possibilities, with which the book concludes. But in the interpretation of current events the theoretical propositions blur into assertions about tendencies, and the outlook into the future is rather speculative and little constrained by the eleven propositions developed earlier.

Harrod offers, after his own brief discussion of the conceptual framework, a series of deft and often illuminating discussions of six modes of production relations, each concluding with an exploration of possible change. The six modes have in common a weak position of the worker: subsistence farming, peasant-lord relations, primitive labor markets, enterprise labor markets, self-employment, and household social relations. They are particularly important in, but by no means confined to, the Third World.

Many of the basic ideas of this ambitious project are both powerful and promising of further results. There is an awareness that technology as well as the division of labor respond to power and the interests of the powerful, and are not driven by “objective” efficiency considerations—an argument that I see as central to any understanding of social structure and of social change. In a manner reminiscent of Weber, Cox alternates in his historical studies between the identification of mutually reinforcing functional interrelations in a persistent pattern—say in the modern welfare state—and a causal analysis that avoids functionalist fallacies by focusing on the genesis of the pattern in question out of harsh conflicts.

Cox’s and Harrod’s mode of analysis is quite clearly Marxian, but it is in no way dogmatically closed. In fact, a commitment to respond to evidence-based arguments is evident on every page. Yet when the discourse turns theoretical, it tends to blur the line between metaphysical premise and empirically challengeable proposition, as is evident in the quoted passage about production as the ultimate base of all power. This tendency is not in the best interest of the project they have undertaken.

I close with two suggestions, both, in a broad sense, methodological. While one can hardly expect ever to resolve arguments about
what is the case “in the last instance,” more specific propositions—even about as large issues as class power, state power and autonomy, transnational relations, and changes in the division of labor—should be subjected to careful comparative historical research addressing sharply defined theoretical questions. Such a strategy may à la longue settle many issues that today are seen as parts of eternally contestable metatheoretical and quasi-philosophical positions.

The other suggestion seems equally compatible with the basic thrust of Cox’s and Harrod’s project. It may be useful to revise their strategy of emphasizing structures at the expense of collective actors. State apparatuses, as distinct from overall structures of domination, class organizations standing in complex relations to underlying class interests, and other large corporate actors, need more specific and conceptually complex attention. Balancing in this way attention to actors and structures is particularly incumbent on those who consider states, classes, and transnational power constellations as central categories of macrosociology.

Other Literature Cited


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Touched by the watershed cycle of social movements of the 1960s, researchers of the movements of the 1970s and 1980s have struggled to create theoretical alternatives to dominant paradigms in both Europe and the United States. Here, responding most directly to a pervasive psychological reductionism and key macroanalyses shaped by anticomunist ideology, observers of that burst of collective action have crafted a new perspective, now widely known as resource mobilization. In Europe, “the idea of the social movements as historical agents marching toward a destiny of liberation, or crowds in the grip of suggestion and under the control of a few agitators” (Melucci, p. 330) was displaced as close observers of escalating collective action tinkered with the outlines of a “new social movements” approach to understanding it. International Social Movements Research showcases the fruits of systematic empirical research accomplished within each of the emergent paradigms by scholars from both sides of the Atlantic knowledgeable about and sensitive to both.

Editors Bert Klandermans, Hanspeter Kriese, and Sidney Tarrow convened a meeting in Amsterdam in June 1986, which included movement researchers from Italy, West Germany, the Netherlands, and the United States; the papers included in this volume were presented there. The focus of the collection is explicitly comparative, focusing mostly on the student, environmental, women’s, and peace movements. Klandermans and Tarrow summarize the substantive focus for which they strove in the conference and the volume, saying that “we emphasize the process of mobilization—that is, how the potential for social movements which emerges from the social and political structure of advanced capitalist democracies is translated into social and political action” (p. 3). They have produced a coherent collection of superior quality. And while the comparative promise of the separate papers remains implicit—few of them attend to a movement beyond the boundaries of a single nation—the editors have provided an introductory essay by Klandermans and Tarrow and a concluding essay by Kriese that bring the extensive research findings into some comparative focus.

The twelve contributions are arranged rather tidily into three sections, on recruitment networks, consensus mobilization, and the construction and careers of social movements. The first section “centers on the networks, support structures, and subcultures that form the bases of . . . mobilization potential. . . . Hanspeter Kriese, Karl-Deiter Opp, and Mario Diani and Giovanni Lodi discuss each of these aspects . . . [and] Micromobilization is the subject of both Doug McAdam’s and Donatella della Porta’s contributions” (p. 31). The papers in the second group define and utilize the notion of