



Review: What Makes Peasants Revolutionary?

Reviewed Work(s):

Peasants, Politics, and Revolution: Pressures toward Political and Social Change in the Third World by Joel S. Migdal

Agrarian Revolution: Social Movements and Export Agriculture in the Underdeveloped World by Jeffery M. Paige

"Hegemony and the Peasantry," by James C. Scott

Peasant Wars of the Twentieth Century by Eric R. Wolf

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What Makes Peasants Revolutionary?

Theda Skocpol

Joel S. Migdal, *Peasants, Politics, and Revolution: Pressures toward Political and Social Change in the Third World*, Princeton, Princeton University Press, 1974.

Jeffery M. Paige, *Agrarian Revolution: Social Movements and Export Agriculture in the Underdeveloped World*, New York, Free Press, 1975.

James C. Scott, "Hegemony and the Peasantry," *Politics and Society* 7(3) (1977): 267-296.

Eric R. Wolf, *Peasant Wars of the Twentieth Century*, New York, Harper and Row, 1969.

The centrality of peasants in modern revolutions was underscored for the first time in contemporary U.S. scholarship by Barrington Moore, Jr. in *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World*. Eloquently, the opening sentences of Moore's chapter on "The Peasants and Revolution" declared that the "process of modernization begins with peasant revolutions that fail. It culminates during the twentieth century with peasant revolutions that succeed. No longer is it possible to take seriously the view that the peasant is an "object of history," a form of social life over which changes pass but which contributes nothing to the impetus of these changes."¹ Published in 1966, *Social Origins* in fact proved uncannily prescient and timely in its emphasis on the revolutionary potential of the peasantry. When Moore's great opus was in preparation during the 1950s and early 1960s, neither Marxism nor orthodox social science paid much heed to the roles of agrarian classes in "the making of the modern world." The peas-

antry, especially, was spurned as the repository of conservatism and tradition, of all that needed to be overcome by a revolutionary bourgeoisie or proletariat or by a modernizing elite. But once the United States became tragically engaged from the mid-1960s in a military effort to halt the Vietnamese Revolution, U.S. scholars quite understandably became fascinated with the revolutionary potential of the peasantry—especially in the Third World. The questions addressed by Moore—“What kinds of social structures and historical situations produce peasant revolutions and which ones inhibit or prevent them?”²—were immediately relevant for an entire nascent genre of research and theorizing on peasants and revolution.

The first major contribution to this new literature, written in the heat of the movement against U.S. involvement in Vietnam, was Eric R. Wolf's 1969 book on *Peasant Wars of the Twentieth Century*. Studies undertaken by younger scholars emerged during the middle 1970s: Joel S. Migdal's *Peasants, Politics, and Revolution*, Jeffery M. Paige's *Agrarian Revolution*, and James C. Scott's article on “Hegemony and the Peasantry.”³ In contrast to the “Old World” frame of reference predominant in Moore's *Social Origins*, the works of Wolf, Migdal, Paige, and Scott share a principal focus on Third World revolutions, the natural result of the preoccupations of the Vietnam era out of which they developed.⁴ Nevertheless, variations of method and substantive focus are immediately noticeable. Eric Wolf seeks to generalize inductively about peasant-based revolutions on the basis of in-depth histories of six twentieth-century cases: Mexico, Russia, China, Vietnam, Algeria, and Cuba. Joel Migdal elaborates a systematic theory of how imperialistic modernizing forces impinge on peasant villages and how peasants, in turn, are likely to respond economically and politically. This theory is illustrated with bits and pieces of secondary evidence gleaned from fifty-one published village studies in Asia and Latin America, as well as with primary evidence from Migdal's own field experiences in Mexico and India. James Scott's ideas on peasant revolutions are widely but impressionistically based, illustrated by examples from publications on revolutions ranging from the seventeenth-century English case to the recent Chinese and Vietnamese revolutions. Finally, Jeffery Paige's study is the most methodologically elaborate. It combines a quantitative analysis of agrarian movements between 1948 and 1970 in 135 agricultural export sectors, with in-depth historical accounts of agrarian movements in three countries: Peru, Angola, and Vietnam. All of this evidence is used to test an explicit formal theory of rural class conflict in the contemporary underdeveloped world.

The works of Wolf, Migdal, Paige, and Scott cry out for discussion as a set. By looking at these works together, by weighing the relative merits and common limits of their arguments, we can readily lay bare the state of current knowledge on peasants and revolution and indicate the unresolved issues and

the potentially fruitful paths for future research. I shall review and evaluate what these scholars have to say in answer to three major questions: (1) Which peasants are most prone to revolution, and why? (2) What roles do political and military organizations play in peasant-based revolutions? (3) Does capitalist imperialism create conditions for peasant-based revolutions—and, if so, how? These organizing questions will take us to the heart of the basic arguments of the four authors, and into the thick of the often sharp differences among them.

Which Peasants Are Most Prone to Revolution, and Why?

Our excursus through the recent literature on peasants and revolution starts with a question that will eventually reveal itself to be too narrowly framed. All the same, it is a revealing place to begin because three of our authors split sharply in their answers. Eric Wolf and James Scott argue different variants of one polar position—that the peasants most prone to revolution are village-dwellers who possess landed property. In contrast, Jeffery Paige argues that smallholding peasants are normally conservative and quiescent, whereas propertyless laborers or sharecroppers—cultivators who earn income from wages not land—are more likely to become revolutionary.

The key issue in explaining revolutions for James Scott is whether or not a lower class has the cultural and social-organizational autonomy to resist “the impact of hegemony ruling elites normally exercise” (p. 271). Despite their localism and traditionalism, precapitalist peasant smallholders, sharecroppers, or tenants are in Scott’s view unusually likely to enjoy such autonomy. Their village- and kin-based social networks promote local communal solidarity, even as their world-views and values are inherently in tension with dominant-class culture. Moreover, the immediate processes of economic production are directly controlled by the peasants themselves. “If this analysis is accurate,” Scott concludes,

it implies that we are often likely to find the strongest resistance to capitalism and to an intrusive state among the more isolated peasantries with entrenched precapitalist values. While the values that motivate such peasantries are thus hardly socialist values in the strict, modern use of that word, their tenacity and the social organization from which they arise may provide the social dynamite for radical change. The situation of immigrant workers and landless day laborers . . . may well seem more appropriate to strictly socialist ideas, but their social organization makes them less culturally cohesive and hence less resistant to hegemony. (p. 289)

Wolf tends to agree with Scott about the kinds of peasants most likely to

become involved in revolutions, but he provides a different analysis of the reasons why. Impressed by the obstacles that poverty and vulnerability to repression can place in the way of political involvement by peasants, Wolf argues that most poor peasants and landless laborers are unlikely initiators of rebellion. They are usually closely tied to or dependent upon landlords and cannot rebel unless outside forces intervene to mobilize and shield them. In contrast, much greater "tactical leverage" to engage in rebellion is normally possessed by landowning "middle peasants," by smallholders or tenants who live in communal villages outside direct landlord control, and by peasants (even poor ones) who live in geographically marginal areas relatively inaccessible to governmental authorities. As Wolf concludes: "Ultimately, the decisive factor in making a peasant rebellion possible lies in the relation of the peasantry to the field of power which surrounds it. A rebellion cannot start from a situation of complete impotence" (p. 290). Thus, for Wolf, the crucial insurrectionary capacities possessed by (especially) communal, property-holding peasants are not cultural as Scott would have it, but lie instead in the material and organizational advantages their situation offers for collective resistance against outside oppressors.

Jeffery Paige approaches the issue of which sorts of peasants are most prone to revolution from a different angle. Instead of speaking of "peasant villages" facing outside forces, Paige organizes his argument strictly around class relations between lower-class laboring "cultivators" and upper-class "noncultivators" who appropriate surplus income from agricultural production in established capitalist enclaves of underdeveloped countries. Paige hypothesizes about the political effects of different combinations of upper-class and lower-class income from agricultural production, and he insists that patterns of agrarian class conflict are dependent on the characteristics and situations of both the lower classes and the upper classes taken together. For example, Paige argues that agrarian revolution is potentially "on the agenda" only when upper-class noncultivators derive their income from land rather than capital, because only then are the upper classes forced by their structural position to refuse incremental, "reformist" concessions. This turns class conflicts with cultivators into a "zero-sum" game, in which control of property and state power are inherently at issue. Even if one cannot fully accept Paige's argument about the political consequences of upper-class sources of income,⁵ the class-relational logic of the analysis is exemplary: Whether peasants become revolutionary or not depends as much on the interests and capacities of their class opponents as it does on the interests and capacities of the peasants themselves.

Agrarian conflict also depends for Paige upon the likely political behavior of different kinds of lower-class cultivators—and on this matter he arrives at opposite conclusions from those of Scott and Wolf. When smallholding peas-

ants who derive their livelihood from land ownership are dominated by a landed upper class, Paige holds that the “normal” result will be an absence of overt conflict. Rather, propertyless laborers are the cultivators prone to make revolutions against landed upper classes. Landholding peasants are, in Paige’s view, mutually isolated and economically competitive among themselves, averse to taking risks, and strongly dependent upon rich peasants and landed upper classes. Wage-earning cultivators are just the opposite, and are thus structurally inclined to give solidary, deliberate support to revolutionary political movements.

Nothing better illustrates the contradictory lines of reasoning embodied in the theories of Wolf and Paige than the contrast in their writings on the rural social basis of the Vietnamese Revolution. Wolf locates the areas of strongest support for the Communist-led revolution disproportionately in northern and central Vietnam, in certain mountainous regions populated by ethnic minorities, and (during the 1960s) in those areas of southern Vietnam differentially populated by smallholding peasants. Wolf reasons that these were areas where peasants could safely provide solidary support, relatively free from French, landlord, or U.S. repressive power. In southern Vietnam, by contrast, village communities were unstable and therefore more difficult for the Communists to organize. Moreover, landlords and their allies were quite strong in the South.

Disagreeing sharply with Wolf, Paige makes an analytic distinction between areas of Vietnam that provided strong “spontaneous” social support for revolutionary socialism, versus the more geographically marginal areas where Communist forces flourished during the military phases of the revolution. Using this distinction, Paige argues that the earliest and most historically consistent agrarian support for the Communists was centered in the export-oriented, rice sharecropping areas of the Mekong delta in southern Vietnam—precisely where the cultivating lower strata consisted of workers without secure landholdings, paid in crop shares, and condemned to common low status in an export economy dominated by large landholders, creditors, and merchants. The southern Vietnamese sharecroppers, Paige reasons, had a strong interest in, and capacity for, collective revolutionary action, whereas smallholding peasants in central and northern Vietnam were dominated by village notables and divided against one another by competition for land and village resources.

The available answers to our first organizing question—which peasants are most prone to revolution, and why?—are thus strikingly contradictory. Who is right? Or, to put it another way, whose way of posing the issues and developing an explanation is more valid and fruitful? If explicit, thorough reasoning and methodological sophistication were sufficient to ensure correctness in social science, our vote of confidence would automatically go to Paige, for his

Agrarian Revolution is an unusually meticulous piece of scholarship. Nevertheless, I believe that Paige's argument is open to serious question—especially on the issue of which peasants are most prone to revolution.

As we have already noted, Paige predicts that in an agrarian system where both the upper classes and the lower classes derive their incomes from land, there should normally be “little or no peasant political activity” (p. 41). However, Paige is quickly forced to note that “periodic uprisings have been a constant part of manorial economies from the German peasant wars to the Bolivian revolution of 1952” (p. 42). These historical realities “do seem to contradict the principle that peasants should lack the coherent political organization necessary to oppose the landlords” (p. 42). But there is no real contradiction, claims Paige, because “peasant rebellions in commercial hacienda systems depend on the weakening of the repressive power of the landed aristocracy, the introduction of organizational strength from outside the peasant community, or both” (p. 42). Even when such facilitating factors intervene, the peasants still are not truly revolutionary, Paige asserts. His reasoning has several steps:

Many peasant revolts which occur when a landed upper class has been critically weakened are little more than simultaneous land rushes by thousands of peasants bent on obtaining land that they may legally regard as theirs. . . . (pp. 42-43)

The land seizures, in turn, may destroy the rural class structure and end the political power of the landed upper class. . . . (p. 45)

[Nevertheless] even after the landed class has been weakened to the point that it can be liquidated by widespread peasant land seizures, the peasants themselves still lack the internal political organization to seize state power. . . . (p. 43)

In fact the peasants are seldom the beneficiaries of the political changes they set in motion. . . . [R]eform or socialist parties . . . [provide] the political organization and opposition to the landed elite that the peasants themselves . . . [cannot] sustain. It is, therefore, usually those parties that fill the political vacuum left by the departure of the landlords. (p. 45)

I have quoted at such length because it is very important to note what Paige is doing here. In the course of elaborating a theoretical category in which “little or no peasant political activity” is expected, Paige recounts evidence of revolts and (in effect) revolutionary overturns of landed upper classes and states that support them. By the end of the discussion, Paige offers a formulation that is really quite different from the theoretical prediction of no political activity with which he began; he states instead that the “characteristic forms of political behavior in systems in which both the upper classes and the lower classes are dependent on income from land *are, alternatively, political apathy*

or agrarian revolt” (p. 45, emphasis added). Although he never says so explicitly, Paige surely realizes that under appropriate military conditions such “agrarian revolts” can have truly revolutionary consequences. But supposedly his original theoretical expectations are not contradicted, because the peasants are “only” revolting for immediate objectives (such as driving out the landlords and taking their property!) and are not deliberately trying to reorganize national politics, and because the revolts of the peasants can only spread and achieve lasting results when military breakdowns or outside organizers intervene to help the peasantry.

Nothing said by Paige in all of this maneuvering to deal with the revolts of landed peasants is wrong historically. But it should not escape the reader’s attention that Paige has set formidably high standards for an agrarian cultivating class to qualify as truly “revolutionary.” Our curiosity should be thoroughly aroused: Do wage-earning cultivators meet these standards in Paige’s theoretical discussion and empirical accounts?

For Paige, the “typical form of social movement in systems dependent on landed property and wage labor is revolutionary. Such movements involve not only violent conflict over landed property and direct attack on the rural stratification system, but also a coherent political effort to seize control of the state by force . . . [L]ong guerilla wars are the likely result” (p. 58). Yet Paige carefully distinguishes between two subtypes in his revolutionary category. In the first subtype, where migratory labor estates are involved, the “workers themselves are too divided to provide the coherent political organization necessary for armed insurrection” (p. 68). Thus, “only in colonial areas where the estate system has not completely eliminated the power of the indigenous landed classes can a revolutionary nationalist movement occur” (p. 70). In such cases, organized nationalist parties and armies created by indigenous elites can intervene to organize the migratory laborers who, otherwise, like peasants “on a commercial hacienda . . . [are] incapable of providing the organizational strength to oppose the power of the landlords” (p. 70). Obviously, for this subtype, Paige fails even to assert that there are autonomous revolutionary dynamics among the wage-earning cultivators themselves. A critic can justly point out that the differences in political behavior between smallholding peasants and migratory laborers seem to depend not so much on the income sources of the cultivators as upon the larger societal and political contexts within which these agrarian lower classes are located.

But when we arrive in *Agrarian Revolution* at discussions of the truly revolutionary landless cultivators—above all, sharecroppers in systems of decentralized wet-rice agriculture—then military factors and organized political parties suddenly take on very different roles from those they play in “agrarian revolts” or merely “nationalist” revolutions. *Socialist* revolutions are, for Paige, genuinely class-based affairs. To be sure, organized ideological parties

are also involved in these revolutions—namely, Communist parties and ideologies. But Paige boldly maintains that these parties bear a unique relationship to sharecropper tenant supporters: “Areas of tenancy have shown a pronounced attraction to left-wing, particularly Communist, ideologies and a surprising potential for powerful political organization. . . . *Unlike the politics of peasants dependent on individual subsistence plots, these political affiliations are internally generated, not introduced by outside urban-based parties*” (p. 62, emphasis added). “In the case of decentralized sharecropping systems the organization is based on a Communist party *organized from within* the worker community” (p. 70, emphasis added). In short, Paige would have us believe that parties involved in organizing smallholders or migratory laborers come to them “from without,” whereas Communist parties organizing rice sharecroppers somehow emerge “from within” as pure expressions of cultivators’ class interests and their conscious revolutionary determination to overthrow landlords and the state.

A moment’s reflection will reveal the unbelievability of Paige’s bizarre theoretical treatment of revolutionary communism in Vietnam and in other rice sharecropping systems. Asian communist parties, like all modern political parties from reformist to socialist to nationalist, have been created and led by urban-educated middle-class people.⁶ In no sense are they the autonomous organizational creations of agrarian lower classes. Sometimes these parties have operated in the countryside primarily as political mobilizers, without deploying their own military forces. At other times—especially during armed guerilla struggles for revolutionary power—Asian communist parties have combined political and military mobilization of peasants and workers. Invariably, communist parties *come to* agrarian lower classes in search of their support for national political objectives that go well beyond the immediate goals of the vast majority of the peasants, whether smallholders or sharecroppers. In Vietnam, the Communists had anticolonial, nationalist objectives as well as the “revolutionary socialist” goals exclusively stressed by Paige.⁷ And the survival of the Vietnamese Communist party from 1930, let alone its ultimate victories in northern and then southern Vietnam, is simply incomprehensible as the product of anything less than widespread social support among many different kinds of Vietnamese peasants, not to mention Vietnamese workers and middle classes.

Paige’s climactic argument about the social basis of Vietnamese communism refers to southern Vietnam in the early to mid-1960s—and here peculiarities and contradictions abound. For Vietnam as a whole, Paige dismisses nonsharecropping areas of Communist strength as indicators of military presence rather than political appeal. Yet for southern Vietnam he uses (pp. 329-33) as his indicator of Communist political appeal an index of the geographical locations of assassinations of village notables and chiefs by the

(Communist-led) National Liberation Front. But assassinations, surely, are an expression of combined political and military struggle. Even more important, assassinations would logically seem to reveal those localities where Communists were contesting for control, not the places where such control was already securely possessed. Indeed, Paige's own historical discussion reveals a well-known fact about politics in southern Vietnam: It was always an arena of uphill struggle for the Communists, and not only because of French and then U.S. military strength. Even in those localities with Paige's theoretically appropriate, protorevolutionary class relations, the Communists had to compete tooth and nail for power, not only with local repressive organizations controlled by landlords, but also with two powerful nonrevolutionary sects, the Cao Dai and the Hoa Hao.⁸ The Hoa Hao, according to Paige's own data, did just as well among the rice sharecroppers as the Communists. This sect was ultimately eclipsed only after the assassination of its leader and much governmental repression; until then it was much more truly a spontaneous peasant organization than the Communists ever were. Yet Paige's theory cannot make sense of the Hoa Hao. For the theory predicts only "socialist revolutionary" politics for the rice sharecroppers of the Mekong Delta.

In sum, Paige's arguments about the political capacities of landholding versus landless peasants do not hold up in the face of critical scrutiny. Paige theoretically posits a kind of "revolutionary socialist" agrarian lower class that probably does not exist in reality. Certainly Paige provides no valid evidence that cultivators in this category can organize themselves self-consciously to attack class relations and the state, for the presence among the sharecroppers of communist slogans and activities is his prime empirical indicator of "revolutionary socialism."⁹ A close, skeptical reading of *Agrarian Revolution* suggests that *either* peasant smallholders or landless laborers can end up playing important parts in revolutions. Given that this is hardly the conclusion entailed by Paige's model, one is forced to conclude that "income sources" in the abstract are not valid predictors of the political interests and capacities of agrarian classes.

In *Social Origins*, Barrington Moore presents a distinction between contrasting sorts of local community solidarity: "conservative solidarity," in which peasant smallholders, tenants, or laborers are dominated by rich peasants or landlords who control the resources and organizational levers of village society, versus "radical solidarity," in which peasants themselves share resources and run village organizations that can be set in opposition to landlords or the state.¹⁰ Paige's attempt to derive degrees of solidarity among cultivators directly from their sources of income in land versus wages blinds him analytically to the possibility of "radical solidarity" among smallholding peasants.¹¹ Yet in sociohistorical situations where such solidarity has existed (for example, Russia and Mexico), communities of landholding peasants have

been collectively able and willing to revolt against landlords and the state. Thus, James Scott and Eric Wolf are correct to argue that communities of peasant smallholders have at times fuelled revolutionary overturns of dominant classes and the state.

This brief detour into Barrington Moore's *Social Origins* suggests the kind of analytic approach necessary to improve upon Paige: a social-structural approach that looks closely at institutionalized economic and political relations between landed upper classes and agrarian lower classes, on the one hand, and institutionalized relations among the peasants themselves, on the other. Much more than James Scott, Eric Wolf resembles Moore in using such a social-structural approach. Scott may be right in some of his assertions about the revolutionary potential of peasant communities, but his primarily cultural approach leads him to romantic, ahistorical assertions about "the peasantry" in general. To read Scott is to get the impression that all "peasant villages" are basically the same: communal, subsistence-oriented, nonexploitative, culturally in tension with "outside" dominant classes, and economically on the defensive against encroaching capitalism or imperialism. But as demonstrated by Paige's astute and detailed analysis (pp. 285-300) of the villages of central and northern Vietnam, exploitative and competitive internal divisions and class tensions can readily exist within subsistence-oriented villages with communal resources. Imperialist pressures can exacerbate internal divisions and exploitation.¹² And as the comparative-historical investigations of both Moore and Wolf document, the structural variations of class and community arrangements within agrarian societies are very great. These variations, in turn, determine different landlord and peasant responses to capitalism and different patterns of agrarian politics from case to case. Scott's cultural approach cannot descriptively handle—let alone explain—such variations of structures and outcomes.

Eric Wolf, however, is sensitive to the full range of social-structural and political issues that must be taken into account to explain peasant-based revolutions. Although there is nothing rigorous about his answers, Wolf inquires about peasants' property-holdings, about their relations to one another and to landlords, and—perhaps just as important—about their relations to the state and to organized political and military forces challenging state power. In these final emphases Wolf goes beyond even Moore. Wolf's notion of "tactical mobility" for the peasantry encompasses many of the same concerns addressed by Moore's discussion of conservative versus radical forms of village solidarity. Yet Wolf is alluding to more than whether peasants are collectively solidary and free from tight control by landlords. His concept also inquires into the relative freedom of peasants from state repression, either by virtue of their "marginal" geopolitical location or as a result of the intervention of armed revolutionaries to shield the peasants.

In a sense, Wolf's explanatory approach is too complex and vague to be more than a set of analytic pointers. It tells us to pay attention to political and military as well as socioeconomic relationships. It also suggests that we must examine more than the situation of the peasantry and the agrarian economy alone, if we are to understand peasant participation in revolutionary transformations. Taking heart from these pointers, we move on to examine directly how broader political and economic forces are implicated in peasant-based revolutions.

What Roles Do Political and Military Organizations Play in Peasant-Based Revolutions?

This is a good time to bring up some of the key arguments of Joel Migdal's *Peasants, Politics, and Revolution*. By highlighting the centrality of political organizations in revolutions, Migdal achieves among our authors a unique angle of vision on how and why peasants become revolutionary.

Wolf, Scott, and Paige alike tend to envisage revolutions as (in one way or another) "made by" class forces. Certainly Paige strives mightily for pure economic and class reductionism. Reformist, socialist, nationalist, and Communist parties abound in his empirical data and illustrative historical accounts, but such parties are never there as independent variables, only as indicators of economically determined political conflicts. Agrarian income sources and class relations are supposed to explain reforms, revolts, and revolutionary movements. Despite his sharp differences with Paige, James Scott also belittles the causal importance of political organizations in peasant revolutions. Scott grants that a "revolution to be successful may . . . require a disciplined party or army in addition to an aroused peasantry" (p. 292), because only such extra-peasant forces can provide "the coordination and tactical vision" (p. 294) necessary to overcome peasant disunity and achieve national state power. Still, Scott celebrates the indispensable revolutionary force of autonomous peasant violence. He maintains that "the spontaneous action of the peasantry in many revolutionary movements . . . has forced the issue and mobilized its would-be leadership" (p. 295), adding that "more often than not it has been the autonomous . . . action of the peasantry that has *created* the revolutionary situations . . ." (pp. 295-296). Institutionalization of peasant politics, argues Scott, is very likely to undercut revolution:

There is . . . no a priori reason for assuming that the outside leadership of the peasantry will be more militant than its clientele. . . . In fact, one would expect that the more organized, the more hierarchical, and the more institutionalized a peasant . . . movement becomes . . . the more likely it will become woven into the established tapestry of power. (p. 296)

In contrast, Joel Migdal asserts that the peasant revolutions of the twentieth century have been propelled by armed revolutionary parties that have directly mobilized peasant support. Such “revolutionary movements,” Migdal points out, “are created by the impetus of those from outside the peasant class . . . [T]he participation of peasants in revolutionary organizations is preceded by the development of an organizational superstructure by students, intellectuals, and disaffected members of the middle class” (p. 232). To be sure, peasants must also be involved in the revolutionary process. Yet for Migdal the issue is not how agrarian class relations themselves generate revolutionary movements, nor how peasant spontaneity creates revolutionary situations and prods radical elites to make revolutions. Rather Migdal seeks to explain how social exchanges between revolutionary parties and local peasant populations can be established—exchanges so stable and mutually rewarding as to account for sustained peasant support and “participation in institutionalized revolutionary movements” (p. 228).

Underlying Migdal’s approach to peasant revolutionary involvement is his strong belief that twentieth-century peasant revolutions differ fundamentally from revolutions and revolts in previous times:

In the last fifty years, peasants in certain areas have engaged in prolonged national struggles to change the system of government and the distribution of power. These movements have not been based on a sudden burst of violence after frustration has built as was often true of the spasmodic, anomic peasant rebellions of past centuries. Rather, peasants in these cases have engaged in long drawn-out revolutions in a variety of institutionalized ways—as political cadres, as disciplined soldiers, as loyal suppliers of food, money, and shelter, and as active and passive members of a host of revolutionary organizations and groups. (p. 226)

“Why,” Migdal wonders, “has the character of . . . [peasant] participation changed from the more eruptive, anomic qualities of the French Revolution . . . and the Russian Revolution to the organized aspects of the Chinese and Vietnamese Revolutions?” (p. 227). Migdal never answers this question very satisfactorily. His book argues at length that peasants in the twentieth-century Third World face an unprecedented economic crisis due to pressures from imperialism. Participation in organized revolutionary movements that offer programs to address local peasant problems is said to be one way that peasants can try to cope with the unprecedented crisis. But Migdal never compares, for example, prerevolutionary French and Russian peasants to Chinese and Vietnamese peasants. He does not show that the economic difficulties faced by these two sets of peasants were different in ways that could explain “anomic” versus “institutionalized” forms of revolutionary participation.

Even if Migdal fails to explain adequately the reasons why peasants have

historically participated in revolutions in different ways, he still points toward a distinction that needs to be made. The distinction is *not* really between twentieth-century and pre-twentieth-century peasant-based revolutions. Migdal is mistaken to argue that peasants participated in the French and Russian Revolutions as “eruptive” masses of “anomic” frustrated individuals; on the contrary, peasants in those revolutions were well organized at local levels and pursued their goals in a very determined, sustained fashion over a period of years.¹³ The same can be said for the village-based supporters of Emiliano Zapata in the Mexican Revolution, which also fits the same overall pattern as the French and Russian cases.¹⁴ The pattern of these revolutions has been one of the breakdown of the old-regime state, followed by widespread local peasant revolts that undercut landed upper classes and conservative political forces. Organized revolutionaries have then consolidated new state organizations, not by politically mobilizing the peasantry, but rather by more or less coercively imposing administrative and military controls on the countryside.

A contrasting pattern of peasant-based revolution is exemplified by the Chinese, Vietnamese, and, perhaps, Cuban revolutions, and by the revolutionary anticolonial movements of Portuguese Africa.¹⁵ Here peasants have been directly mobilized by organized revolutionary movements, either before (Cuba, Portuguese Africa) or after (China, Vietnam) the collapse of effective state power in the preexisting regime. Because of this direct mobilization, peasant resources and manpower have ended up participating in the building of new-regime social institutions and state organizations. Peasant participation in this revolutionary pattern is less “spontaneous” and autonomous than in the first pattern, but the results can be much more favorable to local peasant interests, because during the revolutionary process itself direct links are established between peasants and revolutionary political and military organizations.¹⁶

Once we make the distinction between these two alternative scenarios for peasant-based revolutions, many apparent disagreements among scholars about such issues as “which peasants are revolutionary” and “what roles are played by organized political forces” tend to dissolve. Basic explanatory questions can also be sorted out in terms of their applicability to one pattern or the other. It should be clear that autonomous peasant villages are more likely to play a pivotal role in the first revolutionary pattern, where widespread local revolts accelerate the downfall of the old regime and *indirectly* condition the consolidation of the new regime. Without being willing to call them “revolutionary,” Paige describes instances of this pattern under his category of “agrarian revolt.” Moreover, much of what Wolf has to say in his “Conclusion” and virtually all of what Scott has to say in “Hegemony and the Peasantry” fits this first pattern of peasant-based revolution. By contrast, Joel Migdal deals mainly with the second pattern, as does Paige in his “revo-

lutionary nationalism” subcategory.

When peasants are directly mobilized into revolutionary politics (according to the second pattern), then autonomous villages are not causally important. What is more, many different kinds of peasants—subsistence smallholders in marginal areas, landless laborers or tenants, even solidary villages of peasants alone, or else villages of landlords and peasants together—can potentially be mobilized by revolutionary movements. There has been too much of a tendency in the literature to suppose that the adherence of peasants to organized revolutionary movements must be explained by the economic interests and social circumstances of the peasants themselves. Even Joel Migdal succumbs to this tendency when he argues (pp. 229-30) that peasants undergoing the most rapid, disruptive exposure to newly penetrating market forces will be the ones most likely to respond to organized political movements that offer solutions to their market-induced woes. But there is no reason at all to suppose that peasants in “traditional” social structures are free from experiences of poverty, class exploitation, and political insecurity.¹⁷ There is no reason why organized revolutionary movements, once on the scene, cannot appeal to many different kinds of agrarian cultivators, including “traditional” ones. This certainly was what the Vietnamese Communists succeeded in doing. In mountainous areas, they mobilized minority ethnic groups, peasants, and notables together by appealing to their fears of ethnic exploitation. In northern Vietnam, they mobilized peasants by displacing the French and by pushing aside within the communal villages the exploitative landlords and the Confucian notables. And in southern Vietnam they mobilized peasants—including Paige’s favorites, the rice sharecroppers—by seizing and redistributing large land holdings and by organizing local associations to support peasant livelihood and defend their possession of the redistributed land.¹⁸

Insofar as the occurrence—or success—of peasant-based revolution depends upon the direct mobilization of peasants by revolutionary movements, then the sheer availability and viability of such movements becomes decisive—just as much as the condition of the peasants themselves. Migdal, in fact, correctly points out that a crucial “factor determining the probability of peasants’ participation in revolutionary movements is the degree to which revolutionary leadership appears, with an organizational framework capable of absorbing peasants and then expanding power through their recruitment” (pp. 231-32). Moreover, given that “revolutionary movements are created by the impetus of those . . . outside the peasant class” (p. 232), Migdal admits that “exogenous factors,” beyond the scope of his analysis of peasant villages as such, “determine in which countries such outside revolutionaries will appear and where they will provide a high degree of revolutionary leadership in those countries in which they do appear” (p. 235).

Perhaps the most important questions to ask about the emergence and

growth of institutionalized revolutionary alliances between peasants, on the one hand, and political parties and armies, on the other, refer not to the peasants themselves, but to the circumstances that produce organized revolutionaries and allow them to operate effectively in the countryside. Under what social-structural and world-historical conditions have nationalist or communist parties, or both, emerged and become willing and able to address themselves to rural populations? Have colonial situations been more amenable to this development than neocolonial situations? How have variations in colonial situations and processes of decolonization helped to produce or inhibit the formation of agrarian revolutionary alliances? What social-structural, historical, and (even) cultural factors can help us understand why Asian communists have been more willing to attempt peasant mobilization than have, say, Latin American communists or communists in Moslem countries? Answers to questions such as these may turn out to explain more about the occurrence of peasant-based revolutions of the second pattern than any amount of investigation of the peasant situation as such.¹⁹ For impoverished and exploited peasants in many places may *potentially* be amenable to revolutionary mobilization—if a revolutionary organization can establish itself with some minimal security in the countryside, and *if* its cadres can address peasant needs successfully. But this process cannot begin to get underway unless such a revolutionary leadership emerges in colonial or national politics, finds itself unable to achieve power in the cities alone, and proves militarily and politically capable of operating in the countryside.

Once a political movement is in contact with the countryside, there may be only some possible policies that will “work” to mobilize the peasantry, given, on the one side, the constraints faced by revolutionaries and, on the other side, the specific features of local class, community, and political arrangements among the peasantry. In Joel Migdal’s book, reformist, conservative, and revolutionary political organizations are treated as if they make the same kinds of appeals to the peasantry—namely, the offering of “selective” economic incentives to individuals and small groups. To some extent this may be true. Yet compared to nonrevolutionary politicians, revolutionaries may offer distinctive kinds of benefits to peasants, and they certainly demand more costly kinds of support from peasants in return. Nonrevolutionary politicians are well advised to offer modest, economic benefits to particular individuals and subgroups, playing them off against others *within* the peasantry. Revolutionaries must attempt to stimulate demand for, and then supply, more *collective* benefits (even if just at local levels). *Class* benefits, such as redistributed land or local political power, can tend to unite peasants against landlords. *Security* benefits, such as village defense against counterrevolutionary military forces, can also broadly unite peasants.

Insofar as revolutionaries can organize and lead peasants by providing such

benefits, they can, in turn, profit from the willingness of peasants to act together in defense of the collective benefits. Then, on the basis of such willingness, the revolutionaries can ask for major sacrifices of resources and manpower from the peasantry—in order to sustain the extra-local party and army organizations that are indispensable to win national state power. Thus, Joel Migdal is undoubtedly right to analyze the process of institutionalization of a peasant-based revolutionary movement as an *exchange* between revolutionary politicians and peasants. But he could have suggested good reasons why this revolutionary exchange—much more than reformist or conservative exchanges—probably has to take place on the basis of collective benefits for the peasants.²⁰

If the above points are valid, then we can understand the kind of dilemma faced by organized revolutionaries if and when they attempt to operate in the countryside. The revolutionaries must discover or create among the peasantry the demand for collective benefits. They must be able to supply the relevant benefits with great sensitivity to the specific features of local political and social arrangements. All of this must be done without getting themselves and their initial supporters killed or driven away. And not until *after* such delicate and dangerous political work has been completed can the revolutionaries expect to benefit greatly from widespread peasant support.

It is hard to imagine the successful institutionalization of such social exchange between peasants and revolutionaries except in places and times unusually free from counterrevolutionary state repression. Marginal, inaccessible geographical areas are the most suitable places for the process to begin, but for it to spread and succeed, no doubt “exogenous” events must intervene to drastically weaken the existing state power. Just as such developments in the realm of the state must occur to create a revolutionary situation in the first pattern of peasant-based revolution, so must they occur in the second pattern to facilitate the institutionalization of peasant participation in the organized revolutionary movement. In both patterns of revolution, defeats in wars and international military interventions are the most likely ways for existing state power to be disrupted—opening the way either for autonomous peasant revolts or for appeals by organized revolutionaries to peasant support in the countryside.

Our second question—What roles do political and military organizations play in peasant-based revolutions?—has brought us far from the immediate circumstances of the peasantry. State power, it turns out, plays a decisive role in limiting the possibilities for emergence and success of such revolutions. Moreover, organized (political and military) revolutionary movements play crucial roles in peasant-based revolutions, but in alternative possible ways. Either they consolidate revolutionary new regimes separately from, and in necessary tension with, the peasantry. Or they directly mobilize peasant sup-

port to defeat counterrevolutionaries and consolidate the new regime. Peasant participation is a pivotal arbiter of revolutionary success in both patterns, yet—ironically—peasants are politically autonomous collective actors only in the pattern where developments in the realms of the state and organized national politics go on “above their heads.” In the other pattern—Joel Migdal’s pattern of “institutionalized” peasant revolution—organized revolutionary movements are the key collective actors, as they struggle politically to bridge the gap between peasants and the national state.

Having come this far from the peasantry itself, we must now in a sense step back still further—into the sphere of the world political economy. For our third, and last, organizing question directs our attention to the great emphasis placed by our authors on capitalist imperialism as a world-historical impetus to peasant-based revolutions.

Does Capitalist Imperialism Cause Peasant-Based Revolution—and, If So, How?

In *Social Origins of Dictatorship and Democracy*, Barrington Moore explained peasant revolts and revolutions by looking, first, at the structural vulnerability to peasant insurrections of different kinds of premodern agrarian sociopolitical orders. Then he investigated how, in “the process of modernization itself,”²¹ different degrees and forms of agricultural commercialization could enhance or preclude possibilities for peasant revolts against landed upper classes. Like Moore, Wolf, Migdal, and Paige seek to generalize about the macro-structural and world-historical contexts that promote peasant-based revolutions. Yet whereas the relevant context for Moore consisted of variously structured agrarian states undergoing commercialization and industrialization in alternative possible ways, the macro-historical context for Wolf, Migdal, and Paige is envisaged in global rather than cross-national terms. In one way or another, each of these authors stresses *imperialistic Western capitalism* as the fundamental promoter of peasant revolutions. The (not insignificant) differences among them have to do with exactly how this world-historical force is conceived and the specific ways in which it impacts upon, or creates, potentially revolutionary peasant forces.

For Eric Wolf the “peasant rebellions of the twentieth century are no longer simple responses to local problems. . . . They are . . . parochial reactions to major social dislocations, set in motion by overwhelming societal change” (p. 295). The agent of change is “a great overriding cultural phenomenon, the world-wide spread and diffusion of a particular cultural system, that of North Atlantic capitalism” (p. 276). Wolf sees the spread of North Atlantic capitalism primarily as the impingement of market economics upon precapi-

talist societies in which “before the advent of capitalism . . . social equilibrium depended in both the long and short run on a balance of transfers of peasant surpluses to the rulers and the provision of a minimal security for the cultivator” (p. 279). Intrusive capitalism has upset the prior balances: Peasant populations have increased markedly, even as peasants have lost secure access to their lands and been transformed into “economic actors, independent of prior social commitments to kin and neighbors” (p. 279).

Simultaneously, there has occurred “still another—and equally serious—repercussion . . . a crisis in the exercise of power” (p. 282). For the spreading market has created more distant and exploitative relationships between peasants and their traditional overlords, whether tribal chiefs, mandarins, or landed noblemen. And it has also created partial openings for new kinds of elites: entrepreneurs, credit merchants, political brokers, intellectuals, and professionals. Out of this disequilibrated transitional situation peasant revolutions have sometimes emerged. Specifically, they have happened when a political fusion has occurred between armed organizations of one “marginal” kind of new elite—the “new literati” of intellectuals and professionals—and “the dissatisfied peasants whom the market created, but for whom society made no adequate social provision” (pp. 288-89). Thus, peasant revolutions are one possible resolution of the profound societal disequilibria caused for pre-industrial populations, elites and peasantries alike, by the worldwide expansion of North Atlantic capitalism.

Joel Migdal’s vision of the forces at work to prompt potential revolutionary involvement by peasants in the Third World is not greatly different from Eric Wolf’s, but there are two distinctive nuances to Migdal’s approach. First, in contrast to Wolf’s broad focus on society as a whole, Migdal looks more narrowly and in greater depth at “peasant villages” as such. Migdal’s basic argument is that peasants in the twentieth-century Third World have been undergoing a disruptive economic transition from predominant “inward orientation”—marked by subsistence agriculture and strong communal and patronage controls—to greatly increased “outward orientation”—marked by the substantial involvement of individual peasants and households with extra-local “multiplier mechanisms: markets, cash, and wage labor” (p. 87). According to Migdal, traditional peasant villages remained “inwardly oriented” in order to give their members assured, minimal security in the face of exploitative overlords and uncertain ecological conditions. When recurrent crises did strike, moreover, traditional peasants attempted to protect themselves through greater self-exploitation and reliance upon patrons or communal ties. Only an extraordinary crisis of unparalleled impact and continuity could push peasants into greater “outward orientation” in their economic behavior.

Like Wolf, Migdal sees the roots of this crisis in the worldwide expansion

from the eighteenth century of the capitalist-industrial West. Yet—and here is the second distinctive feature in Migdal’s argument—he especially highlights the *political mediation* of that expansion. Migdal speaks of “imperialism” rather than of “capitalism” or “markets” as the prime force promoting changes within and between nations. And he portrays disruptive changes—such as population growth following from public health programs, and increased market penetration due to tax impositions, transportation improvements, and legalized land transfers—as resulting primarily from increases in state controls over formerly locally autonomous peasant villages. “Imperialism,” says Migdal, “caused a reorganization of societies’ centers, enabling them to achieve new levels of efficiency in the transfer of wealth from the peripheries. Direct colonial rule or indirect imperial domination led to vast increases in the state’s power through more effective administrative techniques. Bureaucracies became more complex and coherent and, as a result, were able to penetrate rural areas on a much broader spectrum than previously” (p. 92). Because of the increasingly “outward” economic orientation that many peasants have been forced to adopt in response to the changes wrought by strengthened states, Third World peasants have found themselves at the mercy of extra-local economic conditions that leave them insecure or exploited within the national society and world economy. As a result, Migdal argues, they become potential supporters of political parties and movements, from conservative to reformist, or revolutionary.

Predictably, Jeffery Paige understands the global forces promoting what he calls “agrarian” revolutions differently from Wolf and Migdal. What interests Paige is not the external impact of “North Atlantic capitalism” upon precapitalist agrarian societies, but the new kinds of economic enclaves—agricultural export zones—created within underdeveloped countries by world markets in agricultural commodities. Indeed, according to Paige, “the economy of the typical underdeveloped country can be described as an agricultural export sector and its indirect effects” (p. 2). In the newly formed commercial zones lie the seeds of contemporary agrarian revolutions. Without completely discounting the involvement of other social and political forces in recent revolutions, Paige maintains that the “relationship of the rural population to the new forms of class cleavage and class conflict introduced by the agricultural export economy is essential in understanding the origin of . . . agrarian unrest in the developing world” (p. 3). Class conflicts in export agriculture have come to the fore since World War II, because political conditions have been propitious: “The strength of colonial and imperial political controls long prevented the political expression of these conflicts, but with the decline of colonial power in the postwar era, the commercial export sectors of the underdeveloped world have become centers of revolutionary social movements” (p. 3).²² In sum, whereas Wolf and Migdal see peasants in the contemporary

Third World reacting to encroaching world capitalism, with their local revolts or attachments to national political movements sometimes producing revolutions as part of this reaction, Paige sees agrarian cultivators reacting from within the capitalist world economy to overthrow landed upper classes heretofore dependent upon colonial or imperial state coercion for their survival.

The differences between Wolf and Migdal, on the one side, and Paige on the other, may seem worthy of extended discussion and adjudication, but I propose to step quickly around them. For if the arguments of parts one and two of this essay are valid, then both camps may be saying partially correct things about the ways in which globally expanding capitalism (or "imperialism") has helped to cause peasant-based revolutions. The historical record shows that peasant-based revolutions have (alternatively or simultaneously) received support *both* from peasants economically or politically threatened by newly penetrating capitalist forces and from agrarian cultivators involved in export-agricultural production. In Vietnam, for example, the revolution gained support from northern peasants resentful of French colonial controls, and also from southern peasants set in opposition to the great landlords who dominated the export-oriented rice economy. The Vietnamese Communists were able to sink roots in both groups, drawing from them resources to wage prolonged revolutionary war.

More interesting than the disagreements among Wolf, Migdal, and Paige are the shared features of the ways they think about the role of imperialist capitalism in promoting peasant-based revolutions. Despite their considerable differences, all three authors emphasize imperialism's *commercializing* influence upon agrarian societies and peasant life. Through this emphasis upon agrarian commercialization the views of these primarily "Third World"-oriented authors end up meshing well with the more "Old World"-oriented analysis of Barrington Moore. Capitalist commercialization either develops endogenously as Moore portrays it, or it is imposed from without as Wolf, Migdal, and Paige suggest. Commercialization promotes peasant-based revolution by creating new social strata prone to revolution (as Paige would have it). Or it arouses peasants to defensive revolts by intensifying exploitation and weakening traditional dominant strata (as Moore, Wolf, and Migdal would have it). Thus, commercialization—perhaps endogenously generated, perhaps induced by imperialist capitalism—is envisaged as promoting peasant-based revolutions especially because of its effects on agrarian class relations and peasant communities.

But even if everyone seems in cozy agreement about the prime causal role (if not the exact forms and effects) of capitalist commercialization, there is still room to doubt whether such commercialization is a necessary cause, or even an essential concomitant of peasant-based revolution. Take the Chinese Revolution, undoubtedly socially based in the peasantry. Although scholars

disagree, many believe that Chinese agriculture was not on the whole any more commercialized in the first half of the twentieth century than it had been for centuries before.²³ Certainly the northern areas of China, where the Communists eventually developed their deepest ties to the peasantry, were not highly commercialized relative to other parts of China; nor had these areas experienced significant “modernizing” changes. The most important changes for the worse experienced by Chinese peasants between 1911 and 1949 were huge increases in taxation and violations of physical security. These woes were due to intense civil warfare followed by foreign military invasions by the Japanese. By addressing the issues of taxes and security, and by transforming long-standing local political and class relations between peasants and landlords, the Chinese Communists were able to mobilize peasant support for their revolutionary acquisition of state power. In all of this there is no indication that increased agrarian commercialization—whether endogenously generated or due to imperialist penetration—was the decisive cause of peasant involvement in the Chinese Revolution.

With the strong emphasis on capitalist imperialism as a promoter of increased agrarian commercialization, another aspect of imperialism has been relatively neglected. Expanding North Atlantic capitalism has, since its inception, had enormous impact upon interstate relations and upon the politics of lagging countries. In the second section of this essay, we established that suspensions of state coercive power have been necessary to every successful peasant-based revolution, and that revolutionary political parties willing and able to mobilize the peasantry have been central to many such revolutions. Therefore, it obviously stands to reason that imperialism may have helped to promote peasant-based revolutions not simply because of its economic effects on peasants, but also because of its effects on states and organized politics.

What sorts of effects on states and politics? Both the Chinese and the Vietnamese revolutions point to relevant ones. In the Chinese case, defeats in wars and steady encroachments on Chinese sovereignty by Western capitalist nations and by Japan pushed the Manchu rulers into reforms that led to conflicts with the landed gentry. Out of these conflicts grew the “Revolution of 1911” and the subsequent dissolution of the Imperial state. In turn, foreign ideologies and models of party organization facilitated the emergence of revolutionary movements among educated urban Chinese. Finally, during World War II, military conflicts between the Chinese Kuomintang government and the Japanese, and between the United States and Japan, opened the geopolitical space needed by the Communists to mobilize the peasants of North China for social revolution and military victory.²⁴

In Vietnam, French imperialism conquered and colonized the country. The direct effects on the peasantry were very great—mediated in the North especially by colonial tax exactions (as Migdal’s theory would emphasize) and

mediated in the South especially by export-oriented agriculture (as Paige's theory would emphasize). Yet the Vietnamese Revolution also grew out of the impact of colonialism upon the politics of indigenous middle-class Vietnamese, who became modern-educated, yet were denied important elite posts in the French-dominated colonial state. Nationalist and revolutionary political movements were the predictable result. Thereafter, the progress, even survival, of these movements depended upon a weakening of French power—and that came only with the interimperialist military rivalries of World War II. The Japanese captured colonial Vietnam and in 1945 displaced the Vichy French administrators, only themselves to face defeat soon thereafter at the hands of the United States and Britain. The disruptions—and ultimate vacuum—of state power during World War II gave the Vietnamese Communists an ideal opportunity to claim the nationalist mantle, to assert sovereignty on the heels of the departing Japanese, and finally to mobilize Vietnamese peasants (especially in the North) to resist France's attempt to reimpose colonial control.²⁵

Thus, the military and political reverberations of imperialist expansion contributed crucially to the emergence and success of the Chinese and Vietnamese revolutions. Without the breakdown of the imperial and colonial regimes, without the emergence of organized revolutionary parties, and without the openings created for them by interimperialist military rivalries, the peasants of China and Vietnam could not have been mobilized for revolution. And given the local agrarian structures of China and Vietnam, the peasants could not have become revolutionary in the absence of direct mobilization.

In my own book, *States and Social Revolutions*, I analyzed the causes and outcomes of three revolutions—the French, Russian, and Chinese—also discussed by Barrington Moore in *Social Origins*. My approach placed much greater emphasis than Moore's on the relationships among states, and on relationships between state organizations and social classes, including the peasantry. Capitalist development figured in my analysis more as a motor of interstate competition, and as a propellant of changing relations between states and classes, than it did as an agent of commercialization and market penetration. Imperialism has been seen as promoting peasant-based revolutions primarily through the effects of agrarian commercialization in Third World countries. Yet the impacts of globally expanding capitalism on states and politics in the Third World may have been equally or more important—the touchstone case of Vietnam suggests as much. Perhaps, therefore, future analyses of the role of capitalist imperialism in causing and shaping peasant-based revolutions in the Third World could profit from taking the kind of state-centered approach used in *States and Social Revolutions*.

Capitalism's global expansion has, to be sure, encroached upon and remade traditional agrarian class relations. Yet that expansion has also been accompanied by colonization and decolonization, and by a continuation of the inter-

state military rivalries that marked capitalism's European birthplace even in feudal times. Peasant-based revolutions—in which peasant revolts or mobilization become pivotal in intertwined transformations of class and state structures—have not only grown out of capitalist agrarian commercialization. Such revolutions have emerged more invariably out of occasionally favorable political situations, shaped in large part by the interstate dynamics of the modern world-capitalist era. For these dynamics have, at crucial conjunctures, weakened indigenous or colonial state controls over the peasantry. Moreover, they have often allowed, even impelled, revolutionary political movements to forge new relationships with the mass of the peasantry. Only in favorable political circumstances such as these has the insurrectionary potential of peasants—whether traditionalist or commercializing, landed or landless—actually been able to propel revolutionary transformations.

Conclusion

“Before looking at the peasantry,” Barrington Moore wrote in *Social Origins*, “it is necessary to look at the whole society.”²⁶ His point is amplified by Michael Adas in the introduction to *Prophets of Rebellion*, a recent comparative investigation of millenarian peasant-based protests against European colonialism: “When I first conceived this study,” writes Adas, “I intended to focus specifically on peasant protest, but as I gathered evidence it became clear that elite groups played key roles in the genesis and development of these movements.”²⁷ The burden of this review of recent scholarship on peasant-based revolutions has been that, here as well, peasants are only part of the story. Too close a focus on peasants themselves, even on peasants within local agrarian class and community structures, cannot allow us to understand peasant-based revolutions.

A holistic frame of reference is indispensable, one that includes states, class structures, and transnational economic and military relations. Ironically, of the four students of peasants and revolutions whose works have been reviewed here, only Eric Wolf—the one who wrote earliest and least “theoretically”—comes close to a suitably holistic analysis. Since Wolf—and since Barrington Moore's *Social Origins*—the tendency among scholars has been to look more narrowly (if also, often, more systematically) at peasants and agrarian economies, seeking broad theoretical generalizations about peasant politics from that level of investigation alone. Much of value has been learned about agrarian class relations and peasant communities. But an integrated explanation of peasant involvement in revolutions, from the eighteenth-century French Revolution to the anticolonial revolutions of the mid-twentieth century, has not yet been achieved. No doubt such an explana-

tion can only be developed in conjunction with explanations of other forms of peasant-based political protest (and its absence or failure) in various epochs of world history. Yet as we move forward, we will do well to keep in mind a basic truth: During all the centuries of peasant existence from ancient to modern times, the forms of revolt open to peasants, as well as the political results conceivably achievable by peasant protests, have been powerfully shaped by the stakes of political struggles, domestic and intersocietal, ongoing within the ranks of the dominant strata. "Peasant revolutions" are not at all an exception to this enduring truth. They are, indeed, its fullest and most modern expression.

NOTES

1. Barrington, Moore, Jr., *Social Origins of Dictatorship and Democracy* (Boston: Beacon Press, 1966), p. 453.

2. Ibid.

3. Scott also has a book on *The Moral Economy of the Peasant: Rebellion and Subsistence in Southeast Asia* (New Haven: Yale University Press, 1976), but it analyzes only peasant rebellions and is very cautious (cf. p. 194) in what it suggests about peasant-based revolutions. The discussion of Scott's views in this essay will be based exclusively on his essay about revolutions.

4. I don't mean to say that there is no overlap in the historical cases of peasant-based revolutions analyzed by Moore versus Wolf, Migdal, Paige, and Scott. Actually there is considerable shared interest in Russia and, especially, in China. But divergent angles of vision are involved: Moore approaches his cases as historically established "agrarian bureaucracies," analyzable in the same terms as Western European agrarian states, whereas the others tend to treat their cases as countries that have fallen under the sway of Western capitalist imperialism. For all four authors writing since Moore the Vietnamese Revolution is the touchstone, whereas for Moore Russia and China together were.

5. Actually, Paige's notion that the simple fact of income from capital gives upper classes room to make reformist concessions to farmers or laborers is very dubious. Market conditions can exert severe constraints on possible concessions. Moreover, the line between reforms and structural changes is not always easy to maintain. Paige's own discussion of Malayan rubber plantations (pp. 50-58) illustrates the inadequacies of his basic theory.

6. On the social origins of the leaders of "modern" Vietnamese political movements, see Wolf, *Peasant Wars*, pp. 178-81. Their cultural and intellectual roots are thoroughly explored in Alexander B. Woodside, *Community and Revolution in Modern Vietnam* (Boston: Houghton Mifflin, 1976).

7. On the enormous importance of nationalism in Vietnamese communism, see John T. McAlister, Jr., *Vietnam: The Origins of Revolution* (Garden City, N.Y.: Doubleday Anchor, 1971), and John Dunn, *Modern Revolutions* (Cambridge: Cambridge University Press, 1972), chap. 5. Quite inappropriate in my opinion, Paige tries to draw a firm distinction between communally oriented "nationalist" revolutionary movements such as the União das Populações de Angola (UPA) and class-oriented "socialist" revolutionary movements such as Vietnamese communism. But the Vietnamese Communists were effective precisely because they combined nationalism with class-based appeals to the peasantry! And, indeed, it is hard to see how any successful revolutionary movement in the Third World could avoid having strong nationalist overtones.

8. On these sects see John R. McLane, "Archaic Movements and Revolution in Southern Vietnam," in *National Liberation: Revolution in the Third World*, ed. Norman Miller and Roderick Aya (New York: The Free Press, 1971), pp. 68-101; and Samuel L. Popkin, *The Rational Peasant: The Political Economy of Rural Society in Vietnam* (Berkeley and Los Angeles: University of California Press, 1979), pp. 193-213. Popkin is especially good in explaining how the Cao Dai and Hoa Hao appealed to southern peasants and thereby made themselves formidable organizational competitors to the Communists.

9. See Paige's statistical analysis of "world patterns" in *Agrarian Revolution*, chap. 2. On pp. 94-96 he indicates that the presence of communist or Trotskyist parties at protests in export agricultural zones was a key indicator of "revolutionary socialist events."

10. Moore, pp. 475-76.

11. Paige posits (pp. 32, 37) that only conservative (i.e., landlord-dominated) solidarity should occur when a landed upper class coexists with a landed peasantry.

12. See also the excellent discussion in Popkin, pp. 133-70.

13. See Theda Skocpol, *States and Social Revolutions: A Comparative Analysis of France, Russia, and China* (Cambridge and New York: Cambridge University Press, 1979), chap. 3.

14. See Wolf, chap. 1.

15. On the leftist revolutionary movements in Portuguese Africa (Angola, Guinea, and Mozambique) see Basil Davidson, "African Peasants and Revolution," *The Journal of Peasant Studies* 13 (April 1974): 269-90. On peasants in the Cuban Revolution, see Wolf, chap. 6.

16. James Scott suggests that revolutionary outcomes are likely to be better for the peasantry if their revolts are autonomous and spontaneous in relation to the revolutionary leadership. But the most clear-cut instance of this was the Russian Revolution where, in large part *because* of the extreme spontaneity and local autonomy of the peasant revolts, the peasants in the end faced the coercive extension of Bolshevik state power into the countryside. Chinese peasants, by contrast, benefited after 1949 from the fact that the Chinese Communists had found it necessary to mobilize their direct political support in order to achieve national power in the first place.

17. In *Rational Peasant* Samuel Popkin effectively underlines the insecurities and exploitation built into "traditional" agrarian structures and points out that peasants have good cause to attempt to "remake" these structures through revolts or participation in political movements.

18. On the organizing activities of the Communists in Vietnam, see Popkin, *Rational Peasant*, pp. 213-42; Douglas Pike, *Viet Cong: The Organization and Techniques of the National Liberation Front of South Vietnam* (Cambridge: Massachusetts Institute of Technology Press, 1966); and Jeffrey Race, *War Comes to Long An: Revolutionary Conflict in a Vietnamese Province* (Berkeley and Los Angeles: University of California Press, 1972).

19. I have not seen systematic investigations of such questions, but useful speculations can be found here and there in Dunn, *Modern Revolutions* and in Gérard Chaliand, *Revolution in the Third World: Myths and Prospects* (New York: Viking Press, 1977).

20. Popkin, chaps. 5-6 has some very insightful things to say about the collective mobilization of peasants.

21. Moore, p. 459.

22. Although Paige does not realize it, this comment about political conditions since World War II introduces explanatory variables into the picture that his ahistorical and apolitical theoretical model cannot handle. Moreover, the validity of Paige's statistical analysis of "world patterns" of agrarian politics between 1948 and 1970 is called into serious question once we realize that, during this time, decolonization was happening in Asia and Africa, but U.S. neocolonial hegemony over Latin America remained quite firm. Many of Paige's findings about revolutionary movements versus agrarian revolts may reflect not so much, as he argues, the inherent political potentials of migratory and sharecropper estates versus commercial haciendas, as they may reflect the internationally conditioned differences between African and Asian politics versus Latin American politics during the period after World War II.

23. See Mark Elvin, *The Pattern of the Chinese Past* (Stanford, Cal.: Stanford University Press, 1973); Dwight H. Perkins, *Agricultural Development in China, 1368-1968* (Chicago: Aldine, 1969); and G. William Skinner, "Marketing and Social Structure in Rural China," three parts, *Journal of Asian Studies* 24 (November 1964): 3-43; 24 (February 1965): 195-228; and 24 (May 1965): 363-99.

24. See Skocpol, *States and Social Revolutions*, pp. 67-80, 147-54; and chap. 7.

25. See McAlister, *Vietnam: Origins of Revolution*, parts IV-VIII; and Dunn, *Modern Revolutions*, chap. 5.

26. Moore, p. 457.

27. Michael Adas, *Prophets of Rebellion: Millenarian Protest Movements Against the European Colonial Order* (Chapel Hill, N.C.: University of North Carolina Press, 1979), p. 25.