

“RESOCIALIZING” LATIN
AMERICAN BANDITRY:
A Reply

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I was initially inspired to track Latin America's elusive “social bandits” when, in the midst of my own long-term investigation of rural protest and consciousness in late-nineteenth- and early-twentieth-century Mexico, I was charged with reviewing Rich Slatta's provocative, state-of-the-art collection, *Bandidos: The Varieties of Latin American Banditry*.¹ The volume was a revelation in several respects. First, I was struck by the fact that while Slatta and his colleagues had produced informative, empirically rich studies of banditry and related forms of rural social action in a variety of postindependence regional contexts, the work of these revisionist *banditólogos* seemed largely uninformed by the broader, post-Hobsbawmian discussions of peasant social action and mentality now under way for Europe, Latin America, and other developing areas.² Second, I discovered that like the work of Eric Hobsbawm (but unlike some of the best recent comparative literature), these revisionist studies of Latin American banditry never really grapple with the thorny methodological problem of defining banditry, which entails an effort to sort out social and legal connotations.

Finally, I was struck by Slatta's unrelenting preoccupation with disproving Hobsbawm's “model.” No matter that the recent scholarship on Latin American bandits (in Slatta's collection and elsewhere) reflects a significant range of thematic concerns and interpretive nuances,³ or that (as Christopher Birkbeck correctly suggests) Hobsbawm's venerable the-

1. *Bandidos: The Varieties of Latin American Banditry*, edited by Richard W. Slatta (New York: Greenwood, 1987). My initial review appeared in *Inter-American Review of Bibliography* 38, no. 2 (1988):223–25.

2. Here and in my essay “On the Trail of Latin American Bandits: A Reexamination of Peasant Resistance,” I am defining *peasants* in the broad sense: as rural cultivators from whom an economic surplus is extracted in one form or another by nonproducing classes. When appropriate, I employ other terms to characterize the various structural and cultural differentiations encompassed by such a broad construction of peasantry.

3. Contrary to Birkbeck's contention, the review of the literature on Latin American banditry that appears in my essay goes well beyond that literature's concern with Hobsbawm's thesis.

sis is not really a “testable” model and is often more subtle than Slatta represents. In both *Bandidos* and the present comment, Slatta promotes an unabashed revisionism. His final verdict in *Bandidos* is unequivocal: “The close ties of class and camaraderie that theoretically bind social bandits and peasants together do not surface in the Latin American context.”⁴ Similarly, his comment again takes aim at the conceptual nub of Hobsbawm’s argument, “the peasant-bandit link . . . that makes banditry ‘social,’” which Slatta contends “is largely absent, exaggerated, or mythical” for Latin America.

Much of the revisionist critique of Hobsbawm is compelling, and I said as much in my article. In fact, the first half of my essay is largely a ringing endorsement of the efforts of Slatta and his colleagues. We have reached a point in bandit studies where, in a certain sense, we are all revisionists now. Clearly, despite Hobsbawm’s strategic caveats, the old master and particularly his less sophisticated disciples have often been rather cavalier in attributing “social content,” let alone class solidarity, to the diverse operations of bandits over the centuries—attributions based more often than not on problematic literary and oral sources. Certainly, a line must be drawn among the poorer classes between “avengers” and genuine thugs, who prey on the have-nots more than they threaten the haves. If no distinction is made, Billy Jaynes Chandler and Slatta correctly warn, historians will find themselves on a slippery slope where all banditry might ultimately be deemed social, “involving as it does, relations between people.”⁵

And yet, the danger exists that the new revisionism—or at least Slatta’s more vocal and reified version of it—may go too far in attempting to “desocialize” Latin American banditry. Such an outcome would be ironic because in their collective critique of Hobsbawm, the revisionists argue compellingly that historians should not be reductionist in interpreting Latin American criminality, that banditry is a complex, multivariate phenomenon governed by sociopolitical, cultural, and ecological determinants. In a fundamental way, their recent scholarship challenges Hobsbawm’s monochromatic conceptualization of the countryside, which was inspired primarily by his familiarity with the more traditional peasantries of Europe, particularly Mediterranean Europe. By contrast, the revisionists have sketched a Latin American social matrix that is considerably more heterogeneous and complex. Their work, augmented strategically by other recent contributions to Latin American rural historiography,⁶ has

4. Slatta, *Bandidos*, 192.

5. Billy Jaynes Chandler, “Brazilian *Cangaceiros* as Social Bandits: A Critical Appraisal,” in Slatta, *Bandidos*, 109; compare Slatta’s remarks in the present comment.

6. For example, see two recent collections: *Resistance, Rebellion, and Consciousness in the Andean Peasant World, 18th to 20th Centuries*, edited by Steve J. Stern (Madison: University of Wisconsin Press, 1987); and *Riot, Rebellion, and Revolution: Rural Social Conflict in*

demonstrated that such a varied landscape of agrarian structures and social relations embraced diverse groupings of rural cultivators. It has also begun to suggest that the differentiated peasantry had recourse to a wide range of social options, including a broad spectrum of modes of contestation.

It is therefore unfortunate that much of the new revisionist literature on Latin American banditry, while documenting the relationships that individual bandits forged with elite actors and correctly encouraging Hobsbawm's followers to attend to such linkages, has tended to dismiss "the peasant connection," particularly the rural population's attitudes toward bandits and perceptions of them. My article suggests an explanation for this tendency: revisionists have not been sufficiently critical in reading authoritative sources, such as administrative and police reports and criminal cases, let alone attempting a necessary deconstruction of the discourses embedded in them. Because these official sources, the core of bandit scholars' documentation, are predominantly discourses of power and social control, they rarely focus on the social questions relating to group composition and motivation that need to be answered to determine whether a particular gang or individual was truly an exponent of popular protest (a "social bandit"). Consequently, despite important exceptions,⁷ the revisionists have made their principal contribution to an "elite historiography" of Latin American banditry, a history of highly visible individual bandits and their incorporation into, or subordination by, the world of power and interests.

Hobsbawm, for his part, has consistently asserted in a provocative general manner the primacy of bandits' connection with the peasantry. He has not, however, empirically documented either the substance or the mental realm of that partnership. Thus in order to write a more organic and "popular" history of Latin American banditry, scholars must begin, as Catherine LeGrand recently suggested, "to integrate the lower sectors back into bandit studies by going beyond the simplistic dichotomy between elite collaboration and peasant rebellion that some students of banditry, intent on demolishing Hobsbawm, are posing."⁸

My article draws on several currents in the global literature on peasant social action and mentality to build a conceptual framework that might enable us to conceive of peasants as the subjects of their own history and place them at the center of bandit studies without marginaliz-

Mexico, edited by Friedrich Katz (Princeton, N.J.: Princeton University Press, 1988).

7. See particularly my discussion of the work of Andeanists Erick Langer, Benjamin Orlove, and Deborah Poole, Colombianists Gonzalo Sánchez and Donny Meertens, and Cubanist Louis Pérez in "On the Trail." Each of these authors integrates banditry into broader social contexts.

8. Catherine LeGrand, review of Slatta's *Bandidos* in the *American Historical Review* 93, no. 4 (Oct. 1988):1145.

ing elites. While my proposal is informed by recent work in European social history (the British and French historiography of social crime)⁹ and Latin American agrarian history (such as recent contributions by Alan Knight, Steve Stern et al., and Eric Van Young),¹⁰ it is particularly influenced by two vital currents (Peter Singelmann calls them “middle-range theories”) in peasant studies. The first is the work of Ranajit Guha and the “subaltern studies” school of Indian historiography on the “common forms” of peasant consciousness that underwrite protest and insurgency and can often be teased out of official discourse and “popular” folkloric sources.¹¹ The second current consists of the efforts of James Scott and other Asianists (and Africanists) to reconstruct material, symbolic, and discursive patterns of “routine” or “everyday peasant resistance.”¹² Each of these challenging approaches has potential pitfalls, which are addressed in my essay. Nevertheless, by creatively blending critical approaches to Marxist theory (particularly the work of Gramsci) with judicious borrowings from the post-Marxist turn in cultural studies, the exponents of these currents have already begun to influence cutting-edge Latin Americanist scholarship on peasant resistance and consciousness.¹³ Still, if the skepticism registered in the comments by Slatta and particularly Birkbeck provides any indication, the reception of these new approaches among mainstream historians and criminologists is likely to be a rocky one. Slatta caricatures the subalternists’ appropriation of post-Marxist concepts and methods by linking them to the chaos and excess accompanying the poststructuralist “descent into discourse.”¹⁴ Birkbeck, in classic positivistic fashion, dismisses as relativistic and unproductive virtually any attempt to identify resistance or political motivation in peasant behavior. I will respond to their objections in greater detail further on.

My article, which is informed by such comparative literature, seeks

9. See my discussion of the work of E. P. Thompson, Douglas Hay, Peter Linebaugh, Richard Cobb, and others in “On the Trail.”

10. Alan Knight, *The Mexican Revolution*, 2 vols. (Cambridge: Cambridge University Press, 1986); Stern et al., *Resistance, Rebellion, and Consciousness*; Eric Van Young, “Mentalities and Collectivities: A Comment,” in *Rebellions in Mexican History*, edited by Jaime E. Rodríguez (Los Angeles: Latin American Center, University of California, Los Angeles, forthcoming).

11. For the main works of Guha and the subalternists, see “On the Trail,” nn. 65 and 67; see also *Selected Subaltern Studies*, edited by Ranajit Guha and Gayatri Chakravorty Spivak (New York: Oxford University Press, 1988).

12. The principal works of James Scott, Michael Adas, and other Asianist resistance scholars are cited in “On the Trail,” nn. 44 and 97. For recent Africanist contributions, see *Banditry, Rebellion, and Social Protest in Africa*, edited by Donald Crummey (Portsmouth, N.H.: Heinemann, 1986).

13. Stern et al., *Resistance, Rebellion, and Consciousness*; also see the contributions of Daniel Nugent, Ana María Alonso, and María Teresa Koreck cited in “On the Trail,” n. 66.

14. For a trenchant critique of poststructuralism *a ultranza*, of a discourse theory that would substitute language for history, see Bryan D. Palmer, *Descent into Discourse: The Reification of Language and the Writing of Social History* (Philadelphia: Temple University Press, 1990).

to address two broader themes pertaining to Latin American banditry that have yet to be treated adequately either by Hobsbawm's disciples or by the revisionists. First, I inquire more centrally into the relationship between banditry and the law. Specifically, I am concerned with the way in which different groups in society perceive deviance and define (or socially construct) criminality and how it provides a window on forms of social control and popular resistance in the countryside. Second, I build on the existing patchwork of efforts in revisionist studies to sketch out some of the factors that determine an ecology of rural protest and accommodation.¹⁵ (Two such factors would be the village's links with external loci of power and the cultural resources that are accessible within local peasant societies.) Such an exercise is essential if we are to gain a more systematic understanding of how banditry and other strategic peasant options reflect the dynamic larger social environment. Here I take pains to distinguish acts of protest and resistance from whatever peasants merely do to survive, an exercise admittedly more easily achieved at the theoretical level than at the empirical level. In the process—and without falling into the teleological trap that ensnared Hobsbawm, wherein forms of peasant resistance are understood in essentialist terms (such as “prepolitical” versus “modern”) rather than in historical terms—I suggest something of a continuum of social forms and mentalities of popular protest, which Scott has described as “ranging all the way from petty individual acts focussed on the here-and-now to highly organized, durable movements of broad ideological purpose.”¹⁶

I conceive of my proposal as a contribution, rather than a demurrer, to the new scholarship on Latin American banditry. My intention was to promote a research agenda that is more conceptually and methodologically integrated into the broader concerns of comparative studies of the countryside. Peter Singelmann has grasped both the synoptic and socially contextualized dimensions of my perspective in observing that it “places our understanding of social banditry into broader settings of peasant societies and their responses to social disorder or transforma-

15. Here again, Birkbeck fails to acknowledge a theme that is featured prominently in my essay (see particularly pp. 19, 25–33).

16. James Scott, “Resistance without Protest and without Organization: Peasant Opposition to the Islamic *Zakat* and the Christian Tithe,” *Comparative Studies in Society and History* 29, no. 3 (July 1987):419. In Slatta's comment and in his recently published essay, “Banditry as Political Participation in Latin America,” he characterizes banditry as “a weaker strategy” to be employed “only when other tactics . . . were not available.” See “Banditry as Political Participation,” *Criminal Justice History* 11 (1990). Do I really misrepresent him, then, in suggesting that he views banditry as “a tactic of last resort”? In my essay, rather than attaching a priori value to these social forms of action (such as “weaker” or “stronger”), I contextualize them. Thus like Scott, Adas, Stern, and other students of resistance, I regard all these forms as strategic options, whose actualization depended on both historical circumstances and the cultural resources that a peasant community possessed.

tion. . . . What Joseph proposes, as he links the models of Guha and Scott, reflects . . . less a plea for a new paradigm than an invitation to perceive the possibilities and the limits offered by each model in order to come closer to the complexity of real social worlds.”

Yet Singelmann also appreciates that the construction of a general conceptual framework enabling us to decode raw data in different historical contexts and for specific groups within a given social context is relatively less problematic than a causal explanation of discrete historical cases. I would concur with him that “*Why* particular groups of bandits . . . emerged and became transformed or dissolved can be explained only by an intersection of theoretically connected, complex sets of variables and by historical research examining their particular combinations in given cases as well as the unique aspects of human action that can be ‘explained’ at best through retrospective induction.” As I suggest in my article and elsewhere,¹⁷ the political behavior of particular groupings of bandits and peasants is typically overdetermined, the product of multiple and complex social and individual origins.

Singelmann and I stand much in agreement on these questions, and I am grateful for his efforts at underscoring theoretical points implicit in my proposal and for his thoughtful extension of my analytical framework to social groups and phenomena beyond the historical parameters of the Latin American countryside. I will therefore devote the remainder of my reply to examining more fully the positions taken by Slatta and Birkbeck.

Aspects of Slatta’s comment, when read together with his just published “Banditry as Political Participation in Latin America,” suggest something of an evolution in his thinking about the political *and social* dimensions of banditry since *Bandidos* was published in 1987. In fact, his comment betrays a certain cognitive dissonance that deserves closer analysis. Although Slatta refuses to budge from his original anti-Hobsbawm position and again emphatically rejects the “peasant-bandit link,” reading between the lines reveals that a hint, if not a voluntary recognition, of the social aspects of banditry is gradually seeping into his work. In his comment and his recent article, Slatta essentially revalues the significance of banditry in the service of the rural poor. Whereas previously he viewed banditry primarily as elite collaboration or as a weaker strategy of adaptation on the part of the poor, he increasingly acknowledges its strategic

17. See, for example, Gilbert M. Joseph and Allen Wells, “El monocultivo henequenero y sus contradicciones: estructura de dominación y formas de resistencia en las haciendas yucatecas a fines del Porfiriato,” *Siglo XIX* 6 (July–Dec. 1988):215–77; and Joseph and Wells, “Seasons of Upheaval: The Crisis of Oligarchical Rule in Yucatán, 1909–1915,” in *The Revolutionary Process in Mexico: Essays on Political and Social Change, 1880–1940*, edited by Jaime E. Rodríguez (Los Angeles: Latin American Center, University of California, Los Angeles, 1990); compare Van Young, “Mentalities and Collectivities.”

uses "as a type of mass political participation" or as resistance with an accompanying "political consciousness." For example, Slatta writes, "The rural masses, who possess a lesser quantity of law, resort to other forms of resistance, including banditry," or, explicitly invoking James Scott, he affirms that "banditry can be a 'weapon of the weak.'"¹⁸

Yet paradoxically, while Slatta enthusiastically appropriates the rubrics of Scott and other students of "routine" or "everyday forms of peasant resistance," he consistently refuses to recognize their agrarian underpinnings, the social context in which they were developed and take on meaning. A more than passing familiarity with Scott's construct reveals that it rests foursquare on notions of peasant moral economy. Thus Scott argues with some force that rustling, theft, and other offenses often grouped together in standard legal definitions of banditry tapped into "deep subcultures of resistance."¹⁹ Indeed, Scott contends that such routine forms of resistance were underwritten, legitimated, and celebrated by vital informal networks and structures of community and ritual that obtained even in stratified peasant villages and other rural settlements where class contradictions were rife. Even if Slatta is not as sanguine as Scott regarding the strength of such solidary bonds in the highly differentiated peasant communities and tightly controlled plantation populations that characterized Latin America's diverse rural past (and it is clear from Slatta's rejection of class explanations that he is not), the borrowing of Scott's construct would seem to carry with it an obligation to assess its clear implications for a social interpretation of bandit phenomena.

How, then, can one explain Slatta's reluctance to accept the social underpinnings of the resistance literature, which he otherwise seems to endorse? Why are words like *peasant* and *agrarian* so noticeably absent from his writing as he continues to refine the variants of his "political" typology of Latin American banditry and resistance? Even several of Slatta's fellow contributors to *Bandidos* (Erick Langer, Louis Pérez, Gonzalo Sánchez, and Donny Meertens) offer regional case studies that concentrate more on social movements and their agrarian matrices than on the careers of highly visible brigands. These scholars document close ties between bandits and rural communities, and other contributors (such as Chandler) at least recognize that given the proper historical circumstances, no "insurmountable barriers" stand in the way of such ties.²⁰ Slatta, by contrast, seems trapped in a paradox. He seems quite willing to acknowledge the participation of individuals or small groups of peasants

18. These quotations are drawn from Slatta's present comment and his article, "Banditry as Political Participation."

19. James Scott, *Weapons of the Weak: Everyday Forms of Peasant Resistance* (New Haven, Conn.: Yale University Press, 1985), 300.

20. For a discussion of these authors' works, see "On the Trail," 17 and n. 60.

in everyday forms of resistance (in which he includes banditry). Moreover, he recognizes the collective participation of peasants at the other end of the spectrum in broader insurgent movements (participation invariably branded as "banditry" by the state). Why, then, is he so reluctant to accept even the potential for peasant-bandit solidarity in those intermediate operations or liminal contexts where its presence can often be documented or reasonably inferred from official sources?²¹

Two mutually reinforcing explanations for Slatta's refusal to acknowledge the social dimensions of banditry might be adduced. First, it is possible that while avowing a theoretical interest in "conflict criminology," which mandates a social construction of the law and a healthy skepticism for official characterizations of peasant behavior,²² Slatta remains in practice something of a hostage to the authoritative sources and the traditional methods of analysis that he uses. Second, it may be that Slatta has become too invested in his debate with Hobsbawm to let go of it. Such an outcome would be unfortunate because, as I argue at some length in my article, the debate has increasingly become a sterile taxonomic one that proceeds at the expense of more interesting thematic and methodological issues attending the social history of banditry.

If Slatta wants to take issue with an insistence on class ties as the *sine qua non* of "social" banditry, he will again receive no argument from me. I would agree with him that "what united people behind outlaw gangs *more often* were kinship, friendship, and region—not class."²³ More to the point, the majority of students of such phenomena would also concur, possibly even Hobsbawm himself. Slatta correctly points up the elusiveness of Hobsbawm's argument, which I examine with some care in my essay. To be sure, Hobsbawm emphasizes social bandits' ties with the poor. At the same time, however, he clearly appreciates what revisionist scholars and the historical bandits themselves knew only too well: that bandits' long-term profit and survival also meant forging larger alliances that transcended the peasant community and entailed relationships with

21. See, for example, Ranajit Guha, *Elementary Aspects of Peasant Insurgency in Colonial India* (Delhi: Oxford University Press, 1983), 76–108; Michael Adas, "From Footdragging to Flight: The Evasive History of Peasant Avoidance Protest in South and Southeast Asia," *Journal of Peasant Studies* 13, no. 2 (Jan. 1986):64–86; and Joseph and Wells, "Seasons of Upheaval."

22. Despite Slatta's skepticism regarding my (modest) use of Foucault, note the nice conceptual fit between Slatta's discussion of "conflict criminology" in "Banditry as Political Participation," and my treatment of labeling and deviance in "On the Trail," 21–22 and nn. 147–48. Also see Slatta and Karla Robinson, "Continuities in Crime and Punishment: Buenos Aires, 1820–50," in *The Problem of Order in Changing Societies: Essays on Crime and Policing in Argentina and Uruguay*, edited by Lyman Johnson (Albuquerque: University of New Mexico Press, 1990), 19–45, especially 38–40.

23. The italics are mine. This is not to say that class ties were not controlling in certain historical contexts. For example, see Knight, *The Mexican Revolution*, 1:123–26, 352.

the elite.²⁴ Alan Knight's work on revolutionary Mexico and Pat O'Malley's research on late-nineteenth-century Australian rural unrest are particularly helpful in focusing attention on the relational, circumstantial nature of social banditry. They argue that it could be articulated on the basis of class, clientelist, or regional ties. Both emphasize the potential for social bandits to galvanize rural communities of heterogeneous class composition "where there exists a commonly shared experience of . . . exploitation."²⁵ At the same time, both Knight and O'Malley question Hobsbawm's contention that communal unity depended on the existence of a solidary "traditional peasantry."

Thus the way that Slatta invokes class becomes something of a red herring. In the process, alternatives are overdichotomized: either we accept Hobsbawm's complete, ready-made package of "social banditry" (depicted as exclusively class-driven and "pre-political" to boot), or we entirely discard social interpretations, allegedly to engage in a more profitable discussion of distinct varieties of "political banditry." I will grant that here and in his recent article, Slatta has honed his typology, taking greater pains to distinguish his "guerrilla-bandits" and "political bandits" from each other, from Hobsbawm's "social bandits," and from common thugs.²⁶ Nevertheless, many of the difficulties that I discussed in the article remain. Readers will have to decide for themselves whether Slatta's new political variants significantly improve on Hobsbawm's earlier construct. Despite its obvious defects (and unfortunate vulgarization by critics and disciples alike), it still sheds a good deal of light on the dynamic, often contradictory dimensions of the politics of banditry. Slatta takes issue with charges of "hair-splitting" that I and others have made,²⁷ but considering the example that he himself raises, has he really broken new ground conceptually or merely christened Hobsbawm's haiduks with the good Spanish name of *guerrilla-bandits*?

More important, I am skeptical about the real worth of such consuming exercises in classification. Slatta himself admits that "taxonomic debate can be overdone." I would suggest that the time for such typolo-

24. For example, compare Hobsbawm's discussion in the revised edition of *Bandits* (New York: Pantheon, 1981), especially chapter 6, with that in Anton Blok's classic revisionist treatment, *The Mafia of a Sicilian Village, 1860–1960* (Oxford: Basil Blackwell, 1974), chapter 5, or with that in Phil Billingsley's more recent *Bandits in Republican China* (Stanford, Calif.: Stanford University Press, 1988).

25. Knight, *The Mexican Revolution*, 1:122–26, 352–55; and Pat O'Malley, "Social Bandits, Modern Capitalism, and Traditional Peasantry: A Critique of Hobsbawm," *Journal of Peasant Studies* 6, no. 4 (July 1979):489–501, quotation 492.

26. Slatta, "Banditry as Political Participation."

27. Compare my critique in "On the Trail," 12–13, with those of Judith Ewell in her review of *Bandidos* in *The Americas* 45, no. 1 (July 1988):131–33, and Arnold Bauer's review in the *Journal of Social History* 22, no. 3 (Spring 1989):562.

gies of banditry and peasant social action has likely passed. Such an approach, so popular in studies of social change in previous decades, has distinct limitations and potential liabilities. While typologies can be helpful initially in directing data-gathering and theory construction, they soon become constricting. Invariably the danger arises that classification gives way to reification, as historical actors and phenomena are extracted from dynamic social contexts and converted “for heuristic purposes” into ideal types. Indeed, the critique that *Bandidos* contributor Dretha Phillips made of Hobsbawm’s original typology might now with some justification be applied to Slatta’s new-and-improved model: “The problem . . . is that the data do not fit these discrete types. . . . Thus we face the situation of trying to explain the exceptions rather than the rule, of trying to decide which of the behaviors of any particular bandit is more salient . . . , of trying to fit real-life complexities into simplistic, unidimensional types and wondering why our explanations actually explain so little.”²⁸

Unfortunately, to make matters worse, typologies beget typologies, and the comparative literature on agrarian societies and social movements has witnessed a proliferation of such models over the past several decades. Now, as Slatta urges scholars to construct new “typologies of the various models of bandit behavior”²⁹ and the prospect of interminable taxonomic debate looms before us, I would propose that we first adequately address the nature of the category of “banditry” itself, particularly the circumstances that surround its application and perception in different regions and different sectors of society. Only by “resocializing” banditry, contextualizing both the label and the variety of strategic peasant options that it signifies, will we begin, as Singelmann aptly observes, to “establish a trail in the labyrinth,” to approximate the complexity of real social worlds.

28. Dretha Phillips, “Latin American Banditry and Criminological Theory,” in Slatta, *Bandidos*, 187. For example, how would Slatta begin to type the complex actions and motivations of Maya villagers that Allen Wells and I have traced in both official and popular sources in our research on Yucatán during the late Porfirian and early revolutionary periods (circa 1908–1920)? Here were campesinos who operated at various times as individuals, in small informal groups, or in larger insurgent bands, depending on the possibilities and options that circumstances provided. Members of several communities on the frontier of henequen export production who were tenaciously defending the last remnants of their agrarian patrimonies, these individuals first came to our attention in the records of the Porfirian criminal courts, where they were charged as thieves and rustlers by the *hacendados* for whom they periodically labored. Later, come the Revolution, they were redefined, if only temporarily, by the winning elite faction as revolutionaries, even though their activities remained much the same as before. Finally, when some of them parted company with the new order then being consolidated by the (not very) Revolutionary State, their activities once more became problematic and they were again labeled as bandits and thieves. Historians can find support in the activities and (court-mediated) testimonies of these campesinos to justify classifying them as everyday resisters, guerrilla-bandits, political bandits, and social (or antisocial) bandits. Nevertheless, none of these ideal types brings us very close to the complex behavior and perceptions of these historical actors or promotes a more nuanced understanding of their social context.

29. Slatta, “Banditry as Political Participation.”

Getting at the complex ways that human beings initiate social action, complying with or defying ruling conventions and norms in the process, is admittedly no easy task. It becomes that much more difficult when we rarely have the unmediated testimonies of the actors themselves. Slatta, while open in principle to a broadening of conceptual frameworks and methods, expresses some skepticism regarding resistance scholars' use of discourse analysis, preferring "a more modest, incremental approach, building on existing Latin American evidence."³⁰ I would contend that the existing documentation, while rich, is overwhelmingly an official discourse that will only begin to yield its subtle meanings when scholars know how to read it more profitably. For such a task, we do require "a proper blueprint." In my essay, drawing on a sophisticated comparative literature that successfully grounds discourses in their material contexts,³¹ I make some modest proposals as to how Latin American social historians can begin to develop the methodological tools needed to decode the kinds of popular knowledge embedded in authoritative sources. Only then can we truly make some sense of the aspirations and moral criteria that inform social behavior.

Birkbeck, by contrast, would dispense with all inquiry into peasant consciousness or any attempt to evaluate the "social" or "political" content of banditry. He contends that exercises such as Hobsbawm's, Slatta's, or my own, rather than establishing a trail through the labyrinth of peasant politics and mentalities, ultimately go nowhere, "petering out in a swamp of conceptual relativity."³²

Whereas Slatta, Singelmann, and I indicate our interest in broadening the discussion and definition of rural social history, Birkbeck would

30. Slatta's invocation of *Time on the Cross* as emblematic of the kind of methodological worst-case scenario to be avoided at all costs is rather ironic. Some of the harshest critics of that book and of cliometrics are social historians who allow discourse analysis a place in historical interpretation but insist that it be rooted in its social context. Bryan Palmer, for example, in *Descent into Discourse*, characterizes *Time on the Cross* as the "fetishization of method" (p. 52).

31. In addition to Palmer and the "subaltern studies school," American historian Joyce Appleby provides insights on how a contextualized approach to discourse can profitably inform strategies of historical interpretation. See Appleby, "One Good Turn Deserves Another: Moving beyond the Linguistic," *American Historical Review* 94, no. 5 (Dec. 1989): 1326-32.

32. Birkbeck is correct to locate my work (and Slatta's) within a broader tradition, the social historiography of rural crime, in which Hobsbawm has been a pioneer. Thus my conceptualization of peasant resistance is meant to extend and add nuance to Hobsbawm's arguments rather than to reject his line of inquiry. But here, as elsewhere, Birkbeck tends to exaggerate or misrepresent my argument. Despite his statement to the contrary, I have been careful to specify why I find Hobsbawm's thesis to be "constricting." First, Hobsbawm's failure to define social banditry, coupled with his typological approach, has generated an increasingly arid debate over classification of bandit phenomena. Second, although Hobsbawm provocatively asserts the primacy of peasant-bandit solidarity, he never really explores the mental realm of that presumed partnership.

limit discourse to those social phenomena that are “readily observable,” “clearly identifiable,” and capable of being “precisely defined.” Obviously, banditry’s inherent “social ambiguity” presents a serious problem for him: fraught as it is with intimations, but rarely with clear-cut documentary proof, of class struggle and resistance, let alone a well-marked peasant consciousness, “social” or “political” banditry is not easily assimilated into traditional criminology’s empirical orientation.

It should be clear that I share some of Birkbeck’s concern regarding the historical inquiry into “consciousness” and the attending search for intentionality.³³ Yet the fact that intentions are not always articulated and must on occasion be inferred from social behavior and read into sources does not mean that individuals lack consciousness, nor does it “contradict” my attempt to distinguish peasant and elite discourses on phenomena such as banditry.³⁴ It does point up the problem of how historians can reliably interpret available sources. Ethnographers try to get at intentions in a way that avoids Birkbeck’s “quagmire” of subjectivity. They cross-check the statements of individuals with their actions (“beliefs” with “practices”), noting differences and similarities between individuals in the same or different social positions, observing carefully the context in which statements were generated, how they were phrased, and so forth. This task is invariably complicated for historians by gaps and ellipses in both “official” and “popular” sources of documentation. Nevertheless, while this complexity makes historical epistemology more rigorous, it does not render it impossible, much less invalidate the enterprise. Surely Birkbeck appreciates that history is ultimately an interpretation, a calculation of probabilities. As Guha has expressed the point in his characterization of official sources, “Precisely because history is the verbal representa-

33. I discuss these problems in some detail in “On the Trail” (see especially pp. 18–22). Although an explicit definition of consciousness does not appear in the essay, it should be clear that I operationalize the concept along much the same lines that E. P. Thompson does in his classic treatment of English workers: “The consciousness of a worker is not a curve that rises and falls with wages and prices; it is an accumulation of a lifetime of experience and socialization, inherited traditions, struggles successful and defeated. . . . It is this weighty baggage that goes into the making of a worker’s consciousness and provides the basis for his [political] behavior.” Thompson as cited in Peter Winn, *Weavers of Revolution: The Yarur Workers and Chile’s Road to Socialism* (New York: Oxford University Press, 1986). Stern wrestles with the concept provocatively in his introductory essay to *Resistance, Rebellion, and Consciousness*.

34. Of course, it would be a mistake to conceive of pristine, “authentic” popular sources or discourses on banditry neatly juxtaposed with their official counterparts. As cultural theorist Stuart Hall has observed, “Popular culture [exists] in a continuing tension (relationship, influence and antagonism) to the dominant culture.” Thus discursive relations between elites and subaltern groups proceed dialectically, in a “state of play.” From time to time, elements of official discourse may be selectively incorporated into popular discourse to mediate self-understanding and communication. See Stuart Hall, “Notes on Deconstructing ‘the Popular,’” in *People’s History and Socialist Theory*, edited by Raphael Samuel (London: Routledge and Kegan Paul, 1981), 227–40, quotation 235.

tion by man of his own past, [it] is by its nature . . . full of hazard. . . . The historical discourse is the world's oldest thriller."³⁵

Although Birkbeck hints at being sensitive to social constructions of banditry and resistance, he ultimately opts for the more narrow legal definition because it is "easier to formulate" than the "political definition" with which Slatta, Singelmann, and I wrestle. Herein lies the problem. For banditry is only less problematic if one uncritically accepts the state's exclusive definition of it as crime or deviance. In calling for "a study of forms of crime, in this case banditry, in relation to structures of social control," rather than the inquiry into forms of social action (and consciousness) in relation to structures of domination that I propose in my article, Birkbeck limits himself to examining only those categories that the state recognizes. In the wake of so much innovative work by ethnohistorians on social memory and the manner in which material and ideological variables shape peasant consciousness and the larger realm of popular culture,³⁶ Birkbeck's proposal is not only restrictive but anachronistic—an eerie attempt to put the genie back in the bottle. At the same time, Birkbeck endorses the traditional official discourse on banditry that scholars such as Guha, Scott, Stern, and I have set about to demystify and deconstruct.

Scholars should make explicit their guiding assumptions. I would subscribe to the notion that "the essence of the historical is the long and extensively varied socio-cultural interplay between ruler and ruled, between the elite . . . and the subaltern."³⁷ A recent avalanche of literature in area studies on peasants and slaves suggests that modes of peasant resistance and self-defense were an integral part of that complex historical dynamic (although not the entire story). Beneath Birkbeck's preoccupation with definition (some of which I share), "readily observable" phenomena, and the ordering of dependent and independent variables appears to lie a deep-seated reluctance to countenance a proposal that would place challenges to the prevailing order, as well as the subjects who made those challenges, at the center of the discussion. To do so, Birkbeck would contend, is to assume that which needs to be demonstrated—and, I would respond, *cannot* be demonstrated through traditional empirical examination of official sources. Birkbeck's call for an "ecology of banditry" translates into a study of social deviance inquiring into the variables that give rise to banditry and other violent forms of criminal behavior. My own

35. Ranajit Guha, "The Prose of Counter-Insurgency," in Guha and Spivak, *Selected Subaltern Studies*, 37–84 (quotation, 55).

36. For example, see the essays in Stern, *Resistance, Rebellion, and Consciousness*, the works cited in "On the Trail," nn. 66 and 138, and Ana María Alonso, "The Effects of Truth: Representations of the Past and the Imagining of Community," *Journal of Historical Sociology* 1, no. 1 (Mar. 1988):33–57.

37. Edward Said, "Foreword," in Guha and Spivak, *Selected Subaltern Studies*, vi.

examination of the problem would proceed rather differently, inquiring into the meanings that peasants (as well as elites) attach to banditry and other strategic options that permit peasants to defend their fundamental interests and reproduce themselves.

Where should bandit studies go from here? Birkbeck, seizing on one of the broader questions raised by Hobsbawm, would ask how banditry contributes to larger “observable changes” in society. This route, however, has already been rather well traveled, both by Hobsbawmians and revisionist proponents of an elite historiography of banditry.³⁸ While I am not opposed to incorporating the study of banditry into discussions of larger historical themes and trajectories (for example, the development of political organizations and the transformation of economic systems),³⁹ a case can certainly be made for giving priority to an agenda that would establish bandits and peasants as the subjects of their own history. By focusing on the internal organization of the rural sector (but without neglecting its links to external sources of power), some of the best revisionist work has demonstrated how an interest in bandits furthers knowledge of rural communities and vice versa. For example, in addition to documenting a rather diverse set of social backgrounds for Latin America’s brigand leaders, social historians are beginning to reassess the make-up of bandit gangs. Ethnohistorical research on rural communities in the Andes and Mexico has revealed the active participation of older small-holding peasants with dependents in a variety of bandit operations, thus challenging Hobsbawm’s notion that bandits were young and unattached. Such studies have raised a host of new questions about the role of families, gendered forms of domination, and wider rural-urban networks of kinship and patronage in banditry.⁴⁰ Only when banditólogos investigate these leads will we be able to speak with any real sophistication about forms of peasant social action and the cultural and mental realms that underwrite them. In the meantime, by commissioning this forum, the editors of *LARR* have showcased the diversity of sources, interpretive frameworks, and theoretical orientations that presently inform the historical study of Latin American bandits and rural society.

38. See, for example, Knight, *The Mexican Revolution*, and the revisionist works cited in “On the Trail,” nn. 13–16.

39. But note the subalternists’ discussion of the dangers that attend assimilating the complex history of peasant protest into larger, encompassing projects, causes, and ideologies. For example, see Guha, “The Prose of Counter-Insurgency,” in Guha and Spivak, *Selected Subaltern Studies*, 70–84.

40. This new research is discussed and referenced in “On the Trail,” 34–35 and nn. 141–45.