

LATIN AMERICAN BANDITRY
AS PEASANT RESISTANCE:
A Dead-End Trail?

Christopher Birkbeck
Universidad de los Andes, Venezuela

Despite the length and detail of Gilbert Joseph's study of Latin American bandits, his argument is relatively simple: previous research has focused too narrowly on Hobsbawm's model of the "social bandit." That model has become constricting, and it is time to reexamine bandits within the larger themes of peasant resistance and peasant consciousness, drawing on recent work in other areas of the world, particularly Asia. In opposition to this argument, I suggest that Joseph's proposal for reexamining banditry merely continues the debate initiated by Hobsbawm, that study of the "social" or "political" content of banditry is unlikely to be productive, and that other dimensions of banditry offer viable and interesting research agendas.

Hobsbawm's work is problematic because it refers to an amorphous category of behavior. Social banditry is defined by two criteria. First, "social bandits . . . are peasant outlaws whom the lord and state regard as criminals, but who remain within peasant society, and are considered by their people as heroes, as champions, avengers, fighters for justice, perhaps even leaders of liberation. . . ." ¹ Second, "It would be unthinkable for a social bandit to snatch the peasants' (though not the lord's) harvest in his own territory, or perhaps even elsewhere. Those who do therefore lack the peculiar relationship that makes banditry 'social.'" ² Thus social banditry requires a certain kind of behavior and a certain social image of the bandit, neither of which is precisely defined.

Hobsbawm proposes the term *social banditry* because he is interested in studying "prepolitical" movements that may have political consequences. He is not asking whether all bandits are social, but how social banditry contributes to political change. Absence of a clear definition conflates the two questions, however, because one strategy for defining social banditry may be to evaluate the contribution of bandits to political change. Thus the real issue raised by Hobsbawm is the question of what

1. Eric J. Hobsbawm, *Bandits* (London: Pelican, 1972), 17.

2. *Ibid.*, 18.

constitutes “social,” “prepolitical,” or “political” activity. Subsequent discussion of his work is not so much a “test” of his model³ as an attempt to give fuller consideration to this problem by using case studies of bandit groups. It is in this manner that the material on Latin American bandits reviewed by Joseph is to be read.

Joseph’s study, like most research on Latin American bandits that attempts to evaluate the political content of behavior, offers a confusing picture. Banditry is seen variously as “economic gain rather than prepolitical protest,”⁴ or as “more opportunistic than patriotic or communally solidary,”⁵ or as a “surrogate form of popular protest.”⁶ But economic gain might also be prepolitical protest, just as opportunistic activity might also be a surrogate form of popular protest, which is surely what leads Hobsbawm to acknowledge that “the crucial fact about the bandit’s social situation is its ambiguity.”⁷ Hence the reader reaches the middle of Joseph’s article in some confusion. When is banditry “social” or “political”?

The second half of the article is largely an attempt to answer that question, although the author seems to be unaware of this point. For Joseph, “continued focus on the Hobsbawm thesis . . . has become constricting . . . it is time to get on with exploring the broader issues related to the social history of rural crime.” For me, the discussion of peasant consciousness and routine resistance continues to address the fundamental issue raised by *Bandits*. Joseph has not moved beyond Hobsbawm; he has merely extended his line of inquiry. Rather than showing why Hobsbawm’s thesis may be “constricting,” Joseph seems to be following him down a dead-end trail.

Joseph’s proposals for moving beyond Hobsbawm are developed in two parts. First, he suggests that we follow the lead of “subaltern studies” and examine peasant consciousness as revealed in both elite and peasant discourses on phenomena such as banditry. Aside from the numerous methodological doubts and queries that this proposal raises, one is left with the larger question concerning what constitutes “consciousness” and how it is to be identified or characterized. Indeed, the author’s second proposal—to apply Scott’s ideas on “routine resistance”—emphasizes the “inarticulate” nature of peasants’ intentions and the need to “infer intention . . . from the social behavior itself.” Thus the second proposal would seem to contradict the first.

3. “Testing” Hobsbawm’s model is supposedly the objective of Slatta’s book. See *Bandidos: The Varieties of Latin American Banditry*, edited by Richard W. Slatta (New York: Greenwood Press, 1987), 2.

4. *Ibid.*, 8.

5. Paul J. Vanderwood, “Nineteenth-Century Mexico’s Profiteering Bandits,” in Slatta, *Bandidos*, 11–31.

6. Alan Knight, *The Mexican Revolution*, 2 vols. (Cambridge: Cambridge University Press).

7. Hobsbawm, *Bandits*, 87.

Both proposals, however, wrestle with the problem of identifying *resistance*, *insurgence*, or *defiance*, terms that all suggest a challenge to the prevailing political order. In the end, because Joseph provides no clear answer, the reader continues to repeat Joseph's own question: "How do scholars know when class struggle lies at the heart of the matter?" Furthermore, I would suggest that there is no clear answer because class struggle is not easily defined or identified.

Let us take just one example. On 6 June 1926, the Brazilian bandit Lampião entered the village of Caraibas, Alagoas, burned buildings and corrals, robbed the more affluent male visitors of their money and clothing, and kidnapped the most prominent local rancher, releasing him a day later after the ransom was paid.⁸ The implications of this incident for the balance of class struggle are hard to determine. First, class struggle arguably involves different dimensions, including the intentions of Lampião and his band, the perceptions of his activities by others, and the material consequences of the raid. Moreover, these dimensions may not coincide. Thus Lampião's intention may simply have been to obtain money while the material consequence was that income was redistributed from richer to poorer.

Second, it is hard to measure such dimensions. Joseph recognizes the problem of gauging intentions but consider also the material consequences. Lampião appropriated income from the rich, but attacking this village may also have strengthened the position of nearby ranchers. More than advancing the claims of the poor, his raid may have contributed to conflicts within the ruling class. Third, evaluating the political content of the raid seems doomed to subjectivity. The poor villagers may simply have looked on Lampião with fear. Some analysts might argue that this response lowered their awareness of the political implications of the raid, but others might say that fear itself contributed to a deepening distrust of the prevailing political order because of its inability to control banditry. Thus these dimensions can easily be woven into different interpretations of the incident as either reinforcing or challenging the existing social order.

Unless we are provided with a clear and operational definition of class struggle, the path of inquiry taken by Joseph (and Scott) seems to peter out in a swamp of conceptual relativity. Moreover, there is now no horizon, for this kind of debate applies to all human behavior, not simply to banditry or peasant activities. Conceptual confusion weakens the overall research strategy suggested by Joseph, which is to treat resistance (including banditry) as a dependent variable and ask what determines the

8. Billy Jaynes Chandler, *The Bandit King: Lampião of Brazil* (College Station: Texas A & M Press, 1978), 76–78.

form of such resistance. If resistance cannot be clearly identified, however, there is no dependent variable.

The only way to get out of this quagmire, I suggest, is to put the analytical focus in reverse. It seems much easier and somewhat more instructive to ask how different kinds of behavior contribute to observable political changes in, for example, political opinions, political organization, and systems of government or in the distribution of wealth. This approach enables scholars, if they wish, to retain the concept of banditry as an independent variable, and it leads to the rather better question posed by Hobsbawm: "What part, if any, do bandits play in . . . transformations of society?"⁹ Precisely this approach has been taken by Alan Knight in his study of the Mexican Revolution,¹⁰ and his answer could well be summarized in Hobsbawm's own words: "When banditry . . . merges into a large movement, it becomes part of a force which can and does change society."¹¹ Nevertheless, as Anton Blok reminds us, at other times bandits may also work as active agents in upholding the existing order.¹²

The foregoing discussion suggests that examining banditry as a political phenomenon may not be very enlightening or even interesting precisely because the real object of study becomes the broader political forces that use banditry and other violent tactics for their various ends. Moreover, banditry has more dimensions than the political and can be studied in a number of different ways. Here again, I take issue with Joseph, for I do not think that research on Latin American bandits has been entirely or even principally concerned with Hobsbawm's model. For example, only part of the material presented in Slatta's *Bandidos* (notably the work by Linda Lewin and Billy Jaynes Chandler) deals with Hobsbawm, and to a large extent it is Slatta himself who, in the introduction and conclusion, attempts to develop a critique of Hobsbawm as a way of imposing a common theme on what are otherwise disparate, although interesting, contributions. In fact, many studies of Latin American banditry contain so much descriptive material that they touch on a wide number of analytical (and to some extent theoretical) issues.

A detailed discussion of other fruitful lines of inquiry on bandits is beyond the scope of this commentary. I would like, however, to mention one promising area of study that parallels, but does not coincide with, the research agenda suggested by Joseph. This approach would entail developing an "ecology of banditry" that seeks to understand what social,

9. Hobsbawm, *Bandits*, 24.

10. Knight, *The Mexican Revolution*.

11. Hobsbawm, *Bandits*, 29.

12. Anton Blok, "The Peasant and the Brigand: Social Banditry Reconsidered," *Comparative Studies in History and Society* 14, no. 4 (Sept. 1972):494-503.

economic, political, and geographical conditions produce and maintain banditry as one type of violent behavior. This theme appears in many studies of Latin American banditry but is especially prominent in the work of Sílvia Duncan Baretta and John Markoff,¹³ Alan Knight,¹⁴ and Amaury de Souza.¹⁵ Apart from providing valuable insights into rural violence, such research also meshes with a broader level of reflection on the relation between forms of crime and forms of social control. A fine example of this kind of work is Mary McIntosh's historical study of the organization of crime in England.¹⁶

Thus banditry reappears as a dependent variable, but without the problems of definition raised by Joseph's proposal. Indeed, although I agree with him that the concept has not been well defined, I think that a criminological definition of banditry is easier to formulate than the political definition he is looking for. In contrast to Joseph, I would argue not for a study of forms of resistance in relation to structures of domination but for a study of forms of crime, in this case banditry, in relation to structures of social control.

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13. Sílvia R. Duncan Baretta and John Markoff, "Civilization and Barbarism: Cattle Frontiers in Latin America," *Comparative Studies in Society and History* 20 (1978):587-620.

14. Knight, *The Mexican Revolution*.

15. Amaury de Souza, "The *Cangaço* and the Politics of Violence in Northeast Brazil," in *Protest and Resistance in Angola and Brazil: Comparative Studies*, edited by Ronald H. Chilcote (Berkeley and Los Angeles: University of California Press, 1972).

16. Mary McIntosh, *The Organisation of Crime*, (London: Macmillan, 1975).