



The Production of Hindu-Muslim Violence in Contemporary India by Paul R. Brass

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which test the very different prediction that “[t]wo broad structural conditions, the distribution of income among individuals and the mobility of assets, determine both the type of political regime and the extent of political violence in any country in the long run” (p. 65). The obvious risk Boix runs is not so much of running directly from preferences to outcomes as of portraying elite strategies as resulting only from elite consideration of the costs of toleration alone, and not also the costs of repression. Later in the book, the latter reappear sporadically.

That said, Boix’s creative and valuable quantitative analyses consistently demonstrate correlations in expected directions. He persuasively concludes that “per capita income, as employed in the modernization literature in postwar samples, is simply a proxy for other more fundamental factors” (p. 92). His qualitative sections permit intriguing intranational comparisons, whose findings are consistent with the quantitative results.

Boix considers how his model’s predictions might be affected by economic growth, social mobility, trade openness, and variation in democratic decision rules. The last substantive chapter uses his assumptions to consider a truly wide-ranging battery of issues revolving around the politics of redistribution across centuries and continents. Along the way, he improves the core model of Mancur Olson’s (2000) *Power and Prosperity* in order to move several large-scale discussions in truly exciting directions.

Any such far-reaching book must contain shortcomings. The foremost concerns how Boix situates his own core model in preexisting theorizing. His explicit engagement with Barrington Moore’s (1966) *Social Origins of Dictatorship and Democracy*—which details both their overlap and Boix’s improvements—is exemplary (p. 40). Similarly, explicit engagement would have been highly appropriate in the case of others, especially Rogowski. A second shortcoming concerns the model’s domain of applicability. The notion of “a complete theory” suggests one that transcends a key and frustrating limitation of the most prominent and influential traditions of theorizing about regime outcomes. That theorizing consistently speaks best to the left/right, class-based regime contestation characteristic of Western Europe and Latin America. But it remains unclear to what extent Boix’s model overcomes this limitation. Most obviously, state socialist systems are difficult to analyze in terms of contestation over private property and income, which remain central to Boix’s analysis. It seems no coincidence that he repeatedly excludes the state socialist countries from his quantitative analysis and does not otherwise address transitions from the communist subtype of authoritarianism.

Neither of these shortcomings nor other minor ones, however, can overshadow how much Boix has accomplished in *Democracy and Redistribution*. He combines into a single analysis factors that others had treated in isolation. He shifts selected assumptions in highly fruitful directions. He tests resulting predictions in a range of very impressive ways. And he demonstrates, with his consideration of extensions of the model, the analytic dexterity and imaginative reach for which his research is already justifiably well known.

The Production of Hindu-Muslim Violence in Contemporary India. By Paul R. Brass. Seattle: University of Washington Press, 2003. 448p. \$50.00.

— Irfan Nooruddin, *The Ohio State University*

In this book, Paul Brass, one of America’s foremost scholars of Indian politics, draws on four decades of fieldwork to make an important contribution to a rapidly growing literature on the causes of Hindu-Muslim riots in India, the persistence of which is justly considered an embarrassment to the world’s largest democracy.

The book centers on the politics of riots in the North Indian city of Aligarh. Focusing on a single city over 38 years allows Brass to conduct a diachronic analysis of riots in a single site, an ideal research design for understanding the persistence of riots. The key to the book’s importance is its provocative argument. Essentially, Brass implicates the political order of North India in fomenting Hindu-Muslim riots for electoral gain (p. 6). Riots, he explains, are not spontaneous eruptions of primordial hatreds but “dramatic productions, creations of specific persons, groups, and parties operating through institutionalized riot networks within a discursive framework of Hindu-Muslim communal opposition and antagonism that in turn produces specific forms of political practice that makes riots integral to the political process” (p. 369). Once riots end, the process enters its final stage of generating “post hoc interpretations, analyses, and explanations that are in no way scientific or adequate to yield satisfying causal statements, but rather themselves contribute to the persistence of riots” (p. 369).

The argument thus has three main components. First, there exist in some North Indian cities (why not in the South is a question Brass does not address) what Brass calls “institutionalized riot systems.” In these systems, “known persons and groups occupy specific roles in the rehearsal for and the production of communal riots” (p. 32). These people stoke tensions between Hindus and Muslims (p. 118) and, when the political context is right, unleash violence staged to appear spontaneous (p. 258). Second, riots are political instruments used prior to elections to “consolidate one community or the other or both at the local, regional, and national levels into a cohesive political bloc” (p. 34). Lastly, the naming, discussion, and analysis of riots in India is political, feeding into what Brass calls “blame displacement.”

The book’s organization reflects this tripartite argument. Following an opening section providing an overview of the terrain, it is divided into five additional parts. Part II describes the demographic, caste, and communal composition of Aligarh and recounts some of its more calamitous riots. Part III considers effects of spatial and economic distribution on riots (the chapter on the geography of riots is particularly interesting). Part IV presents evidence on the relationship between elections and riots, while Part V develops the “blame displacement” thesis. Part VI summarizes Brass’s conclusions concerning the persistence of Hindu-Muslim conflict in Aligarh.

Any book of this size and scope is bound to raise questions and here I consider five, several of which present opportunities

for future research. First, Brass's argument appears limited to North India as he makes no mention of the South. As such, the reader is left wondering why South India has been immune to the vicious electoral strategies that have allegedly resulted in such violence in the North.

Second, the culpability of the police and civil administration in failing to prevent riots and, indeed, in being full participants in the killing of Muslims is thoroughly documented in this book. For instance, in discussing Aligarh's November 1978 riots, Brass claims that "most of the people killed . . . were shot by the police" (p. 96). About ten days after this phase of rioting, "the district magistrate and senior superintendent of police were replaced" (by whom we are never told) and "under [their] strict control, no further deaths occurred" (p. 96). Yet Brass offers no explanation for such observed variation in police and civil administration responses.

Third, Brass insists that "communal riots have preceded and have led to intensification of interparty competition" but not the other way around (p. 220). But the most plausible argument here is endogenous. That is, where elections are expected to be keenly contested, the incentive to use riots to polarize and mobilize voters increases, which is consistent with Brass's finding that "riots have often occurred in close temporal proximity to elections" (p. 231).

Fourth, Brass regrettably fails to use his rich data from Aligarh to comment directly on the other recent explanation of Hindu-Muslim violence in India (Ashutosh Varshney, *Ethnic Conflict and Civic Life*, 2002). While Brass makes clear his disdain for Varshney's work (see p. 419, note 44, for an almost ad hominem attack), he never deals with the "civic life" argument as a possible alternative explanation.

Finally, and most disturbingly, the tone of the book suggests a lack of objectivity on the part of the author. Brass clearly sees Muslims as the victims of Hindu aggression in India, but the line between identifying certain political parties claiming to represent Hindus and all Hindus as the culprits is blurred. Consider this long quotation, which is jarring in its pejorative nature: "In India . . . the two communities that are seen to be at war or prone to intergroup violence are also associated with distinctive approaches to history. . . . In India, it is the Muslims who have a true historical consciousness. . . . Most Hindus, by contrast, cannot and do not try to separate what others consider mythology from history. . . . Their own dates for their origins, their books, their monuments tend to be fantastic, not credible, said to have arisen in eras that all schoolboys in the West know to have been pre-historic, even pre-Homo sapiens" (pp. 382–83). Such claims, for which no evidence is given in support, are irrelevant to his central argument, and their inclusion serves only to distract.

These criticisms notwithstanding I remain convinced that Paul Brass has written an important book with an original and compelling argument for the how, when, where, and why of Hindu-Muslim riots in modern India. To my knowledge, this is the first explanation to tackle all four questions simultaneously, which fact alone should ensure that it is widely read and a staple on South Asia and comparative ethnic conflict graduate syllabi.

Creating Cooperation: How States Develop Human Capital in Europe. By Pepper D. Culpepper. Ithaca, NY: Cornell University Press, 2003. 272p. \$35.00.

— Gary Herrigel, *University of Chicago*

David Soskice and Peter Hall's agenda-setting edited volume *Varieties of Capitalism* appeared in 2001, and much of the work that appeared as chapters there is now beginning to appear as the first books of assistant professors. Pepper Culpepper's superb, theoretically literate, and empirically well researched comparative monograph is one of these. It is a good example of the interesting ways in which those working within the Varieties of Capitalism (VOC) research program are pushing the theoretical boundaries of the rational choice-based institutional approach developed by the two senior mentors. In fact, Culpepper's account of industrial-training reform initiatives in France and eastern Germany has genuine drama in this regard. He finds (some) successful cooperative reform among decentralized social actors in areas where the preconditions for such cooperation, at least from the rationalist point of view of VOC, were inauspicious. To account for this, he boldly ventures into the enemy territory of constructivist political economy for conceptual guidance. In the end, Culpepper claims to have returned to the rationalist camp safely, but the tension generated by the illicit encounter underlies much of the book's argument and makes for extremely compelling reading.

The occasion for this drama is the efforts in both eastern Germany and France during the 1990s to implement reforms in their industrial-training systems modeled after the highly successful western German dual system. In the latter system, independent small, medium, and large firms cooperate together in the training of apprentices. Part of the apprenticeship occurs at the expense of the firm in shop-floor training programs and part at the expense of the state in vocational school classrooms. The remarkable feature of the West German system is that firms incur the training expense and participate in this system despite the fact that the opportunity exists for other firms to free ride and "poach" skilled labor trained at someone else's expense. In the VOC view, such decentralized cooperation is possible because strong secondary associations and the state protect the system by sanctioning free riders when they emerge. The result is a "high skill equilibrium" where firms voluntarily produce labor with highly portable and general skills and, moreover, see the market advantages of pursuing production strategies that rely on that kind of labor (and thus create further demand for it). It is fair to say that such decentralized cooperation is a kind of poster child for the nonmarket cooperation that the institutional design of coordinated market economies (CMEs) makes possible.

The problem posed by efforts to replicate this system in east Germany and France, according to Culpepper, is that the institutional preconditions and actor capacities that make the system successful in west Germany were either not in place at all (in France) or only newly in place (in east Germany). In particular, because the proposed system was so new, firms did not rightly see its benefits (Culpepper calls this "analytic