The Micropolitics of Social Violence

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Review Articles

THE MICROPOLITICS OF
SOCIAL VIOLENCE

By CHARLES KING*

Mark R. Beissinger. *Nationalist Mobilization and the Collapse of the Soviet State*. Cambridge: Cambridge University Press, 2002. 503 pp.
Ashutosh Varshney. *Ethnic Conflict and Civic Life: Hindus and Muslims in India*. 2nd ed. New Haven: Yale University Press, 2002. 382 pp.

THE Peloponnesian War was a contest between rival alliances, but it also involved what would now be called an internationalized civil war. In 427 BC a bloody dispute erupted on Corcyra, an island in the Ionian Sea. A small group of citizens conspired to sever the existing alliance between the Corcyraean city-state and Athens, and to restore the island's traditional link with Corinth. Both sides sought not only to defeat their enemies but also to wipe out anyone, women and children included, who might be identified with the opposing group. Yet the Corcyra affair, Thucydides says, had little to do with differences over foreign policy. The most prosaic of disputes—over unpaid debts, personal slights, or simple jealousy—were translated into the exalted language of alliance politics. "War," he concludes, "is a stern teacher."[1] In times of social upheaval, the ability to wrap one's own ambitions in the mantle of justified violence may be the only thing that separates perpetrators from victims. The good pupils become the former; the poor ones become the latter.

Sorting through the confusing array of motives, interests, and post hoc rationalizations that accompanies social violence has become a major subject in mainstream political science. It was one of the central scholarly and policy problems of the "post–cold war period," the long

decade that stretched from 1989 to 2001. Its attendant themes—ethnic conflict, peacekeeping, nation building—remain important today, although often under a different set of monikers: terrorism, counterinsurgency, postconflict reconstruction.[2] Perhaps more than in any other field of research, comparative politics and international relations have found common ground in trying to understand why people kill each other in large groups outside the context of a declared interstate war.

The debates of the 1990s over the causes of and responses to substate violence were significant and wide ranging.[3] There were empirical ones about whether civil wars were increasing in number and whether conflicts grounded in "identity" were more common than in the past; there were theoretical ones about the role of state structures, elite machinations, and rational calculations in group violence.[4] Others had a policy dimension, such as the efficacy of population transfers and partition, and when and how the United States or international organizations should intervene to halt civil wars and genocide.[5] New generations of graduate students were trained to think across the domestic–international divide. New journals and funded research programs flourished.

But in profound ways, these debates were also culs-de-sac—in a literal, not a pejorative, sense: they offered a route into a new research area but little place to go once one got there. There were too few connections to long traditions of scholarly theorizing about group mobilization and

[2] For a fascinating report on the return of "counterinsurgency" to the policy lexicon, see Peter Maass, "Professor Nagl's War," *New York Times Magazine*, January 11, 2004.

[3] For a middecade survey, see Steven R. David, "Internal War: Causes and Cures," *World Politics* 49 (July 1997). For updates, see Nicholas Sambanis, "A Review of Recent Advances and Future Directions in the Literature on Civil War," *Defense and Peace Economics* 13, no. 2 (2002); and the special issue of *International Studies Review* 5 (December 2003).

[4] See Roy Licklider, "The Consequences of Negotiated Settlements in Civil Wars, 1945–1993," *American Political Science Review* 89, no. 3 (1995); idem, "Early Returns: Results of the First Wave of Statistical Studies of Civil War Termination," *Civil Wars* 1, no. 3 (1998); Ibrahim Elbadawi and Nicholas Sambanis, "How Much War Will We See? Explaining the Prevalence of Civil War," *Journal of Conflict Resolution* 46 (June 2002); and the special issue of the *Journal of Conflict Resolution* 46 (February 2002), edited by Paul Collier and Nicholas Sambanis and based on the World Bank's civil war modeling project.

[5] See Stephen John Stedman, *Peacemaking in Civil War: International Mediation in Zimbabwe, 1974–1980* (Boulder, Colo.: Lynne Rienner, 1991); Roy Licklider, ed., *Stopping the Killing: How Civil Wars End* (New York: New York University Press, 1993); I. William Zartman, ed., *Elusive Peace: Negotiating an End to Civil Wars* (Washington, D.C.: Brookings Institution Press, 1995); Milton J. Esman and Shibley Telhami, eds., *International Organizations and Ethnic Conflict* (Ithaca, N.Y.: Cornell University Press, 1995); Michael E. Brown, ed., *The International Dimensions of Internal Conflict* (Cambridge: MIT Press, 1996); Barbara F. Walter and Jack Snyder, eds., *Civil Wars, Insecurity, and Intervention* (New York: Columbia University Press, 1999); Chaim Kaufmann, "Possible and Impossible Solutions to Ethnic Civil Wars," *International Security* 20, no. 4 (1996); Barbara F. Walter, *Committing to Peace: The Successful Settlement of Civil Wars* (Princeton: Princeton University Press, 2002); Stephen John Stedman, Donald Rothchild, and Elizabeth M. Cousens, eds., *Ending Civil Wars: The Implementation of Peace Agreements* (Boulder, Colo.: Lynne Rienner, 2002).

collective violence. Rather than linking up with these established literatures, much of the new research either began from scratch or focused mainly on how theories of international relations might be retooled to explain what appeared to be a new wave of ethnic conflict.[6] As a result, some of the discussions—over the role of external guarantors of peace agreements and the commitment problems of belligerents, for example— pushed the study of social violence into the same paradigm-level debates that have characterized the American study of international relations.

This article examines two extraordinary books that help move scholarship in new and creative directions. Although these studies do connect with similar rethinking about the nature of civil wars within the security studies subfield,[7] they represent more than simply a new generation of research on large-scale social violence. Rather, they turn mainstream theorizing about social violence back toward its roots in problems of social order, state-society relations, and mobilization. They resist the monocausal temptations of research drawn from a single theoretical paradigm, while nevertheless developing clear and sometimes elegant models of collective violence. Most importantly, they break down the intellectual wall that grew up in the 1990s between the study of something called "ethnic conflict" or "nationalist violence" and a long line of work on collective action in political sociology and cognate fields. In the end, these books, along with the work of other scholars surveyed below, point to an exciting reconsideration of how political scientists ought to approach some of the most brutal and tragic manifestations of political power.

Section I examines how scholars have normally divided up the existing literature on social violence, particularly on ethnic conflict. This division actually mischaracterizes scholarly traditions in the field and can have undesirable consequences for how research programs are structured. Section II considers the original contributions of new books by

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[6] This trend began with an influential article by Barry Posen, "The Security Dilemma and Ethnic Conflict," *Survival* 35, No. 1 (1993).

[7] Several authors have questioned some of the key assumptions of the early 1990s security studies literature, such as the power of ascriptive identities in civil wars and the distinction between "old" and "new" forms of political violence. See, for example, John Mueller, "The Banality of 'Ethnic War,'" *International Security* 25 (Summer 2000); Nicholas Sambanis, "Partition as a Solution to Ethnic War: An Empirical Critique of the Theoretical Literature," *World Politics* 52 (July 2000); Stathis N. Kalyvas, "'New' and 'Old' Civil Wars: A Valid Distinction?" *World Politics* 54 (October 2001); Mats Berdal and David M. Malone, eds., *Greed and Grievance: Economic Agendas in Civil Wars* (Boulder, Colo.: Lynne Rienner, 2000); Paul Collier and Anke Hoeffler, "Greed and Grievance in Civil War" (Washington, D.C.: World Bank, January 2001); Stuart Kaufman, *Modern Hatreds: The Symbolic Politics of Ethnic War* (Ithaca, N.Y.: Cornell University Press, 2001); Charles King, "The Benefits of Ethnic War: Understanding Eurasia's Unrecognized States," *World Politics* 53 (July 2001); James D. Fearon and David D. Laitin, "Ethnicity, Insurgency, and Civil War," *American Political Science Review* 97 (February 2003).

Mark Beissinger and Ashutosh Varshney, particularly with respect to the reflexive nature of both violent and nonviolent mobilization and the role of formal civic associations as inhibitors of violence. Section III draws out the common theoretical and methodological positions in these books and in related scholarship. This expanding body of literature represents what might be called a micropolitical turn in the study of social violence: a concern with uncovering the precise mechanisms by which individuals and groups go about trading in the benefits of stability for the inherently risky behavior associated with violence—and how, as Thucydides knew, they often do it at the expense of people whom they previously called friends and neighbors. This final section also assesses what such a turn might mean for research methods and theory making in comparative politics and international relations as a whole.

I. The Genealogy of "Ethnic Conflict" Research

In the now considerable literature on ethnic conflict, writers usually identify at least four theoretical positions: essentialism, instrumentalism, institutionalism, and constructivism.[8] Essentialism claims that social identities—religious, linguistic, ethnic—are key to explaining the onset and duration of violent conflict. These identities are durable, if not perennial, and disputes that involve identity might be expected to be more contentious than those over political power, natural resources, or ideology. Instrumentalism holds that identities themselves are less important than the particular political ends they serve. Since identities can be manipulated by political elites, research should concentrate on how they are wielded, not on their content. Institutionalism focuses attention on the formal and informal constraints that channel social identities and either facilitate or inhibit group confrontation. Constructivism examines the process by which identities are formed. Any social identity is made, not begotten, and perpetrating a violent act can itself be an intrinsic part of the process of transforming a latent identity into one that is politically salient.

    The responses to each of these ideal-typical approaches are well rehearsed. Essentialism posits the timeless existence of what are plainly

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[8] These are the labels that Varshney uses (p. 23), but for similar surveys, see Charles A. Kupchan, ed., *Nationalism and Nationalities in the New Europe* (Ithaca, N.Y.: Cornell University Press, 1995); Walter (fn. 5); Kaufman (fn. 7); and Kanchan Chandra, "Introduction: Constructivist Findings and Their Non-Incorporation," *APSA-CP: Newsletter of the Organized Section in Comparative Politics of the APSA* 12 (Winter 2001). Other terms include primordialism (for essentialism), modernism (for instrumentalism), structuralism (for institutionalism), and postmodernism (for constructivism). The middle two are sometimes made subsets of constructivism.

protean identities and simply assumes, rather than explains, the link between who one is and what one does. Instrumentalism attributes too much power to the machinations of unscrupulous elites and portrays the masses as pawns in a vast mobilizational conspiracy. Institutionalism rarely shows precisely how institutional constraints are meant to work and, in any case, has little to say about where particular social institutions come from in the first place. Constructivism is intuitively right that social identities can be shaped, but it rarely offers an account of why identities take the shape they do (and why this fact should even matter in explaining mobilization and violence).

This is the standard way in which the now substantial political science literature on ethnic conflict, civil wars, and related themes characterizes its own past. There is nothing inherently wrong with dividing up previous scholarship in this way, of course. Marking off any "school" usually tucks diverse thinkers into procrustean beds, and it is possible to find scholars who have argued versions of each of these positions (although poor Clifford Geertz is ritually cited, unfairly, as the only known essentialist).[9] But this quadripartite vision of the past is, in fact, scholarly genealogy as fictive kinship. It impels researchers to frame their work in response to an intellectual ancestry that is either wholly phantom or ancillary to the core concerns of the study of social violence. It is problematic in three major senses.

First, it uncritically fuses the literature on nationalism with literatures on ethnicity and collective violence. Many of the signature conflicts of the 1990s—Bosnia, Kosovo, Chechnya, and others—involved protagonists who self-consciously used the "national" label in describing their goals and grievances. That usage was a reflection of the indigenous way of speaking about social identities in eastern Europe and Eurasia; "nationalities," especially in the communist period, were what in any other context would simply be called "ethnic groups." But this language had an effect on scholarship. If nationalities were mobilized and coming to blows, the natural place to look for conceptual clarity seemed to be the literature on nationalism. This inclination was reinforced in the vocabulary adopted by journalists, politicians, and others outside academia to describe the major post–cold war conflicts and

[9] Geertz's edited volume is usually given as the essentialist urtext; Geertz, ed., *Old Societies and New States: The Quest for Modernity in Asia and Africa* (New York: Free Press, 1963). Most statistical analyses of ethnic violence are implicitly essentialist in the way that data are coded. See, for example, Nicholas Sambanis, "Do Ethnic and Non-Ethnic Civil Wars Have the Same Causes? A Theoretical and Empirical Enquiry (Part 1)," *Journal of Conflict Resolution* 45 (June 2001). For a spirited defense of the essentialist line, see Stephen Van Evera, "Primordialism Lives!" *APSA-CP: Newsletter of the Organized Section in Comparative Politics of the APSA* 12 (Winter 2001).

their belligerents. Slobodan Milosevic, for example, was an "ultrana-tionalist," but the Rwandan genocide was essentially about "ethnicity."

The problem is that the classic literature on nationalism actually talks across, not directly to, the phenomena of social mobilization and collective violence. Many of the greats—Ernest Gellner, Anthony Smith, and Benedict Anderson, for example—were concerned mainly with the emergence of the nation as an idea, the development of modern national identities out of the congeries of clan, religious, and local identities that preceded them.[10] Their work has more to do with how movements that embodied the national idea arose after the eighteenth century and less with the complex interaction of state institutions, competing social affiliations, and individual desires that are usually at play in modern ethnocultural movements, much less the even more complex dimensions of social violence.[11]

The misuse of the nationalism literature also explains why "identity" has been such a frequent theme in recent research on ethnic conflict. By linking up with a literature that privileges the national idea, social scientists have naturally focused on the quality of belief and self-conception as a key variable in explaining mobilization and violence. Indeed, for all their putative differences, the four major schools of thought identified above are all, at base, about the nature of identity, whether primordial, manipulatable, constrainable, or protean. An almost obsessive concern with this variable also led to an overstatement of the differences between older, putatively ideological conflicts of the cold war period and the allegedly "identity-based" conflicts that came after.[12]

Second, this quadripartite view of the field casts as mutually exclusive a set of theoretical approaches that have never been genuinely at odds. Even if we allow that major writers on nationalism and ethnicity might fit into one or another of these camps, differences between them are really about the questions they ask, not the answers they propose. Benedict Anderson, for example, has been concerned with exploring

[10] Gellner, *Nations and Nationalism* (Oxford: Blackwell, 1983); Smith, *National Identity* (London: Penguin, 1991); Anderson, *Imagined Communities*, 2nd ed. (New York: Verso, 1991). For a survey of the development of nationalism studies, see Charles King, "Nations and Nationalism in British Political Studies," in Brian Barry, A. H. Brown, and Jack Hayward, eds., *The Study of Politics: The Twentieth Century British Contribution* (Oxford: Oxford University Press, 1999).

[11] A reassessment of Gellner and his relationship to other theorists is provided by John A. Hall, ed., *The State of the Nation: Ernest Gellner and the Theory of Nationalism* (Cambridge: Cambridge University Press, 1998).

[12] For representative statements of the "new wars" position, see Hans Magnus Enzensberger, *Civil Wars: From L.A. to Bosnia* (New York: New Press, 1994); Samuel P. Huntington, *The Clash of Civilizations and the Remaking of World Order* (New York: Simon and Schuster, 1996); and Mary Kaldor, *New and Old Wars: Organized Violence in a Global Era* (Stanford, Calif.: Stanford University Press, 1999). For an important critique, see Kalyvas (fn. 7).

the "modular" nature of the national idea, particularly its export from Europe to other parts of the world in the nineteenth and twentieth centuries. But he might just as well be cast as an institutionalist, insofar as he has stressed the role of censuses, cartography, and other formal conventions in cementing particular conceptions of the nation. Likewise, Donald Horowitz, in his influential *Ethnic Groups in Conflict*, was interested in elucidating the political pathways for managing conflict and avoiding violence in multiethnic settings.[13] It should be no surprise, then, that he stresses the design of political institutions, even though he might equally be labeled a constructivist when it comes to the question of where identities come from.

Imagining the theoretical landscape in this way—as a set of clear antagonists battling over the same conceptual terrain—fit remarkably well with the American tradition of international relations, the subdiscipline that witnessed an upsurge in writing on nationalism and intrastate violence in the 1990s. Ernest Gellner and Anthony Smith, for example, had carried on a long debate about whether nationalism was based on universal, durable sentiments or on the exigencies of modernization— a debate that fit, with some necessary trimming, into the mold of the dispute between neorealists and liberal institutionalists. Benedict Anderson had argued that identities could be shaped in unexpected ways and that their content could in turn have causal power, something that might be cast as constructivism *avant la lettre*. As with most fictive kinships, however, this is a backward projection of current templates onto an otherwise unconnected scholarly literature. The danger is that representing the scholarly past in this way can end up settling into the same paradigm-level debates that have sometimes bedeviled international relations.

Third, this vision of the field marginalizes the scholarly literatures that are in fact most potentially helpful: work on social mobilization and violence in general. It is now common for scholars to embrace the constructivist view that no social identities—not even ethnic ones—are primordial. Yet in the literature on ethnic conflict and civil wars, one frequently finds citations to classic work on the origins of ethnonational identity, ethnic political parties, ethnic voting behavior, ethnic minorities policy, ethnicity and economic development, and related topics— the implication being that the study of something we call "nationalism" or "ethnic conflict" falls naturally within this intellectual family. However, many of the research problems that have intrigued students of

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[13] Horowitz, *Ethnic Groups in Conflict* (Berkeley: University of California Press, 1985).

ethnic conflict and civil wars over the last decade or more are already well represented in other literatures.

The relationship between identities and interests; the relative power of institutions, resources, and opportunities in facilitating mobilization; the function of atrocities and extreme violence; and the role of political entrepreneurs—all have been vigorous subjects of debate in cognate fields, from political sociology to anthropology and history.[14] From the 1960s forward scholars in these fields have developed progressively more nuanced approaches to the study of social mobilization and collective violence. Early studies that focused on the imponderable workings of "the crowd" were supplanted by macrolevel structural explanations.[15] These in turn gave way to greater appreciation for microlevel studies of opportunities, resources, framing, and social networks.[16] Today, running parallel to—and thus largely unconnected with—the literature on ethnic conflict and civil war are exciting projects for bringing together macro- and microlevel approaches, structure and intentionality, under a single "contentious politics" rubric.[17] Yet it is rare to find any of this work cited in the research on collective violence that emerged in the 1990s. By focusing on the adjective rather than the noun, scholars of "ethnic conflict" have by and large cut

[14] For recent surveys, see Robert D. Benford, "Framing Processes and Social Movements: An Overview and Assessment," *Annual Review of Sociology* 26 (August 2000); Francesca Polletta and James M. Jasper, "Collective Identity and Social Movements," *Annual Review of Sociology* 27 (August 2001); Marc Edelman, "Social Movements: Changing Paradigms and Forms of Politics," *Annual Review of Anthropology* 30 (October 2001); and Mark Mazower, "Violence and the State in the Twentieth Century," *American Historical* Review 107 (May 2003); Jeff Goodwin and James M. Jasper, eds., *Rethinking Social Movements: Structure, Meaning, and Emotion* (Lanham, Md.: Rowman and Littlefield, 2004).

[15] The classic text is Gustave Le Bon, *The Crowd: A Study of the Popular Mind* (reprint; Atlanta: Cherokee Publishing, 1982), but for a survey of this literature, see J. S. McClelland, *The Crowd and the Mob: From Plato to Canetti* (London: Unwin Hyman, 1989). For other approaches, see William Kornhauser, *The Politics of Mass Society* (Glencoe, Ill.: Free Press, 1959); Neil J. Smelser, *Theory of Collective Behavior* (Glencoe, Ill.: Free Press, 1963); Ted Robert Gurr, *Why Men Rebel* (Princeton: Princeton University Press, 1970).

[16] See Charles Tilly, *From Mobilization to Revolution* (Reading, Mass.: Addison-Wesley, 1978); Doug McAdam et al., eds., *Comparative Perspectives on Social Movements: Political Opportunities, Mobilizing Structures, and Cultural Framings* (Cambridge: Cambridge University Press, 1996); Sidney Tarrow, *Power in Movement: Social Movements and Contentious Politics*, 2nd ed. (Cambridge: Cambridge University Press, 1998); Doug McAdam, *Political Process and the Development of Black Insurgency, 1930–1970*, 2nd ed. (Chicago: University of Chicago Press, 1999); Steven M. Buechler, *Social Movements in Advanced Capitalism: The Political Economy and Cultural Construction of Social Activism* (Oxford: Oxford University Press, 2000); Karen Barkey and Ronan Van Rossem, "Networks of Contention: Villages and Regional Structure in the Seventeenth-Century Ottoman Empire," *American Journal of Sociology* 102 (March 1997).

[17] See Ronald Aminzade et al., *Silence and Voice in the Study of Contentious Politics* (Cambridge: Cambridge University Press, 2001); Doug McAdam et al., *Dynamics of Contention* (Cambridge: Cambridge University Press, 2001); Charles Tilly, *The Politics of Collective Violence* (Cambridge: Cambridge University Press, 2003).

themselves off from the literatures of which they should naturally be a part.

Having an appreciation for this alternative intellectual genealogy is important. It admits a whole body of scholarship that has normally been sidelined. It situates the study of ethnic conflict within a tradition that, unlike the study of nationalism, asks the same kinds of social scientific questions that are of most interest to scholars in the field today. And it allows for the emergence of a real consensus on basic concepts and analytical tools that paradigmatic debates between "essentialists" and "constructivists" does not. The two books by Beissinger and Varshney demonstrate that linking up with these older research traditions can lead in profitable directions.

II. Reflexivity, Tides, and Associations

It is tempting to think of collective violence as anomalous, episodic, and irrational. The predominant image is one of a crowd running wild, consumed by the elemental passions of the group, lost in a bewildering mix of hatred, fear, and exhilaration. That may well describe a particular type of violence—the kind known in some Southeast Asian societies as *amok*, whence the term in English—but it is hardly the norm. Violent episodes are, if not predictable, then certainly patterned forms of social interaction, even when they involve seemingly inscrutable bonds of culture and kinship. They have a certain life cycle that begins with precipitating events such as persistent prejudices or rumors, progresses through a brief burst of bloodletting, passes through a lull, then rapidly escalates into a series of massive deadly attacks. Deescalation happens gradually, either because of an intervention by the forces of order or because of simple fatigue on the part of the perpetrators of violence.[18] That cycle seems to hold in many forms of mass violence, from street riots to massacres in the context of a civil war.[19]

Some organization is usually involved in collective violence, but the picture of receptive masses whipped up by an unscrupulous leader is not quite true to life. As Donald Horowitz has pointed out, violence is in

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[18] Donald L. Horowitz, *The Deadly Ethnic Riot* (Berkeley: University of California Press, 2001), chap. 3.

[19] Patterning can be seen in two unusual types of collective violence, one in which the victim is single and the perpetrators multiple (lynchings), another in which the perpetrator is single and the victims multiple (suicide terrorism). Robert A. Pape, "The Strategic Logic of Suicide Terrorism," *American Political Science Review* 97 (August 2003); Stewart E. Tolnay and E. M. Beck, *A Festival of Violence: An Analysis of Southern Lynchings, 1882–1930* (Urbana: University of Illinois Press, 1995).

reality closer to a pickup game.[20] It certainly requires some minimally qualified activists to get things going. Beyond that, however, there are a host of other facilitating conditions that have little to do with the organizational skill or capacity of those who might have originally had an interest in fomenting disorder. There must be social norms that either allow for the prospect of violence or, more frequently, at some level condone it. Assuming that all rational people must condemn violence overlooks the relatively common condition of "the moral mass murder," instances in which social violence is generally approved, if not overtly supported.[21] There must also be a set of accepted social rules governing how the violent game is played: who is a legitimate target; the level of violence that can be meted out, from destruction of property to murder; and what counts as a sufficient condition for escalating from one level to the next.[22] And critically, as in a pickup game, there usually need to be lots of young men with nothing better to do.

These factors are difficult to sort through, especially in contexts in which previous instances of violence produce echoes in the present. Violent behavior can become routinized, even ritualized, and putative root causes can become illusory.[23] The victims had it coming because of their past treachery, they were in collusion with the enemy, we just did it to them before they did it to us: all are common modes of justification for the actual perpetrators, as well as for the wider society of which they are a part. That violence begets violence is intuitively true—this is Thucydides' point about the Corcyra affair—and it is also a seductive aperçu. But it is also an unsatisfying place to end up. What precisely does the reflexivity of violence mean, and how can one even begin to study it without simply bracketing the past?

Mark Beissinger points toward some answers. To say that *Nationalist Mobilization and the Collapse of the Soviet State* is theoretically and empirically rich would be an almost criminal understatement. It is the fruit of more than a decade of careful data collection and analysis, a

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[20] Horowitz (fn. 18), 266.

[21] Ibid., 366.

[22] On the role of violent contexts and norms, see Jan T. Gross, *Revolution from Abroad: The Soviet Conquest of Poland's Western Ukraine and Western Belorussia* (Princeton: Princeton University Press, 1988); and Timothy Snyder, *The Reconstruction of Nations: Poland, Ukraine, Lithuania, and Belarus, 1569–1999* (New Haven: Yale University Press, 2003). Gross's widely read *Neighbors: The Destruction of the Jewish Community in Jedwabne, Poland* (Princeton: Princeton University Press, 2001) examines the power of context in a particular instance of violence. For a critical rejoinder, see Antony Polonsky and Joanna B. Michlic, eds., *The Neighbors Respond: The Controversy over the Jedwabne Massacre in Poland* (Princeton: Princeton University Press, 2003).

[23] Stanley J. Tambiah, *Leveling Crowds: Ethnonationalist Conflicts and Collective Violence in South Asia* (Berkeley: University of California Press, 1996).

truly monumental contribution to our understanding of the final years of the Soviet Union and the place of social mobilization and collective violence in its collapse.

Beissinger has assembled the most extensive list available of mobilizational episodes in the Soviet Union from the late 1980s through the early 1990s—marches, demonstrations, protests, strikes, riots, pogroms, civil wars—based on multiple-source coding of events reported in more than 150 western and local newspapers and other periodicals: to be precise, from January 1987 through December 1992, 6,663 protest demonstrations and 2,177 incidents of mass violence, plus a few others from the pre-perestroika years. No other researcher has yet had at his disposal as detailed a catalog of the accelerating street politics of the late Gorbachev period and the rising tide of popular unrest that attended the demise of the Soviet Union.

The word "tide" is not just a metaphor. It is part of Beissinger's core argument: that the shape of protest activity in the late 1980s and early 1990s cannot be understood, much less modeled, without taking account of the reflexive power of mobilization itself. The organizers of demonstrations and even average participants were acting within a particular knowledge environment. They knew of mobilizational episodes and state responses in other parts of the Soviet Union. They were often in direct contact with and emboldened by activists from other republics and regions. Their calculus of costs and benefits, such as it was, was demonstrably influenced by their assessment of what had succeeded and failed in other circumstances. Any single protest was thus a wave in a much larger period of "tidal politics."

It was the very context in which individual events took place that accounts for how over time the impossible came to be seen as inevitable: an uprising by the people in a political system that was self-defined as a people's democracy; interethnic violence within a country premised on the "friendship of peoples"; the swift disappearance of the world's largest state. The bounds of the politically imaginable expanded because, as Beissinger says, history "thickened" in the late Gorbachev period. Mobilizational events were chronologically clustered, a feature graphically clear from the data set. These individual events were not only the key arenas of contention between mobilized groups and the state; they were also the crucibles in which the solidarity that bound together those mobilized groups was formed.[24]

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[24] For a fascinating analysis of these processes in eastern Europe, see Padraic Kenney, *A Carnival of Revolution: Central Europe, 1989* (Princeton: Princeton University Press, 2002).

Pace the book's title, the literature on nationalism is secondary in Beissinger's work, as it should be in this sort of analysis. Instead, the general literatures on social mobilization and collective violence (not to mention the literatures on event analysis and statistical methods) are front and center. But this also points to the book's most consistently frustrating feature: it continually buries the lead. Beissinger insists on framing as a study of "nationalism" what is in fact a profound rethinking of the constitutive power of the event in creating group solidarity and the role of clustered events in facilitating group mobilization.

Structural features matter, of course, and Beissinger investigates systematically how a combination of resource endowments, formal political structures, and political opportunities could produce a mobilizational outcome. Yet if any particular Soviet ethnic group lacked one of these structural advantages, there was always a ready and fungible substitute: the mere knowledge that other groups had already mobilized effectively. Through a detailed series of case studies, Beissinger shows how otherwise structurally disadvantaged groups—with small populations, no clear history of grievances, no institutional resources—experienced a rapid broadening of the bounds of their mobilizational horizons. In the context of tidal politics, being poorly endowed turns out not to be an obvious obstacle.

Having an appreciation for how actors themselves understood their environment allows Beissinger to get at two of the most pressing questions about the nature of the Soviet collapse. First, why were some ethnic groups "early risers"—early and eventually successful mobilizers against the Soviet center—and others relatively passive until the center failed to hold? And, second, why did some groups engage in almost universally peaceful protest, even in the face of extreme reactions by the state, while others turned to violence?

Until the breakup of the Soviet Union, the standard way of answering the first question was to point to the power of identity. The Soviet Union was, after all, a land of "captive nations," as the ideology of the West had it, which would ultimately yearn to breathe free. At the highest level of abstraction, that was certainly true. The Soviet Union ended and fifteen new countries, each one named for one of the fifteen constitutive republican nationalities of the Soviet federation, emerged out of its ruins. But it is worth remembering that those who made this argument before the late 1980s were relatively few, and those who did almost universally bet on the wrong horse. The greatest threat to the Soviet system was thought to be the "Muslims" of Central Asia, the various ethnic populations that, in fact, turned out to have the lowest

levels of mobilization. The common response today, more than a decade on, is to focus on structure, particularly the formal institutional resources upon which mobilized ethnic groups could draw—a republic-level parliament, party apparatus, and newspapers, among other things.[25]

Structural conditions certainly mattered. All things being equal, having your own republic and being numerically larger, more urbanized, and less linguistically assimilated to Russian were good things for would-be mobilizers. Yet while these facilitating conditions might explain the onset of mobilization, they do not explain the fact of mobilization. For less well endowed groups, there were certain benefits to backwardness. They could learn from the experience of the early risers, avoid costly mistakes, and engage in complex mobilizational activity in a short period. Over time, the "causal role of event-specific processes" (p. 130) grew relative to the power of structural conditions.

Violence, too, was part of the mobilizational mix. Beissinger shows that the involvement of an ethnic group in an episode of collective violence produced a 3.1 percent increase in the incidence of public demonstrations by that ethnic group in the following week (p. 142). Those groups that failed to mobilize at all—very small minorities within the Russian Federation and, by and large, Central Asians—were saddled with inauspicious structural conditions or had local leaders who actively blocked the tidal influences coming from other parts of the Soviet Union.

The second question, about the use of violence, is trickier. Overall, the collapse of the world's largest state was unexpectedly peaceful, with probably under two thousand people killed and perhaps another thirteen thousand injured in interethnic violence. (The post-Soviet wars in Chechnya and elsewhere are another matter, where perhaps two hundred thousand people have died—but this is still an order of magnitude lower than in places such as Sudan and Afghanistan). During the period of collapse, from 1987 to 1992, violence came in waves, in several senses. It started in particular regions then moved to others. It involved large numbers of people in some periods and far fewer in others. It began with the use of less sophisticated weapons, literally, sticks and stones, and then after 1991 rapidly escalated to the use of heavy artillery.

Once again, however, structure seems to be a poor explanation for the variability of violence, across both space and time. In Beissinger's model, structural factors—a previous historical experience of mass vio-

[25] Valerie Bunce, *Subversive Institutions: The Design and Destruction of Socialism and the State* (Cambridge: Cambridge University Press, 1999).

lence, various demographic features, institutional resources, being "Islamic"—turn out to be weak predictors. For example, groups that had the highest levels of previous violent conflict with the Soviet state within living memory, Baltic groups and ethnic Germans, engaged in virtually no violent activity. And even the Chechens became involved in mass violence only after the Soviet Union was long dead.

Instead, violence seems to have emerged from three rather different sources. It could erupt as a reaction to an initial use of force by the state. It could be a strategy pursued by ethnic leaders on the back end of the mobilizational cycle, as a way of raising the stakes at a time when peaceful protests were winding down. Or it could arise, after the end of the Soviet Union, as part of the contentious politics associated with defining borders and new political institutions within the successor states. The tragic irony is that a mobilizational cycle that was relatively peaceful led to devastating wars in some of the new political systems that it ultimately produced.

Beissinger has written an elegantly theorized account of the power of contingency. When a particular event occurred, in relation to others within the same mobilizational cycle, is as critical as the structural conditions that might have facilitated it. Ashutosh Varshney's work on India takes things in a complementary direction. As in the Soviet cases, social mobilization and collective violence involving India's two largest communal groups—Hindus and Muslims—is not equally distributed geographically or temporally. Since 1947 some Indian states have experienced recurrent episodes of communal rioting with high casualties; others have remained relatively calm. Even within high-violence states, such as Uttar Pradesh and Bihar, there is a marked diversity from one city to another. In cities where the relative size of the communal populations and other structural variables are similar, some are violence prone—that is, there has been a consistently high incidence of intercommunal rioting—while others have seemed generally immune. (A third category consists of locales, such as Gujarat, where violence is rare but intense.) Varshney has been able to identify this basic puzzle by creating his own original data set derived from a systematic coding of riots reported in the *Times of India* from 1950 to 1995. Just assembling the data set, as in Beissinger's work, is a hugely important task. The landscape is uncertain without it, and depending on the level of analysis—the country, the state, the city, perhaps even the neighborhood—what constitutes an interesting and researchable question looks radically different.

The city seems to be the lowest level that the available data can reach, and it is also a level with a sufficient degree of complexity to en-

sure that some large-scale social processes are at work, something be-
yond, for example, violence prompted by a family feud or a stolen car
in an individual village or neighborhood. Varshney's central question,
then, is how to explain city-level variation in the incidence of inter-
communal rioting. The answer, in brief, is that low-violence cities have
strong associational ties between the Hindu and Muslim communities.

It is one thing to interact on a daily basis with members of another
communal group, to buy your newspaper from a Muslim, your flowers
from a Hindu, and your food from a Sikh. Indeed, this is what most
people mean when they talk about long histories of intercommunal
concord or refer nostalgically to periods of cross-cultural exchange in
diverse societies, even in those that are eventually torn apart by war.[26]
But these informal contacts are not good enough. They are ephemeral,
nonbinding, and not necessarily intergenerational. Associations, by
contrast, are durable, and they have ancillary qualities that turn out to
be critical when exogenous shocks threaten social order. They provide
channels of communication between elite groups in the ethnic commu-
nities. They raise the stakes for those who would upset the peace. They
bring together—and, indeed, even create—interest groups that do not
readily emerge from everyday interactions. Associations are how the
strategic decisions of elites become concretized, and they can have a
major effect on the durability of communal peace.

But arguing that differences in associational life map differences in
communal violence is a correlation without an explanation, and it is
here that Varshney's argument is most intriguing. As it turns out, levels
of associational engagement mirror longer-term patterns of communal
interaction, but those patterns were not bequeathed to particular cities
merely by social structure (Hindu-Muslim demographics, levels of
wealth, and so on) or by an imponderable "history." Rather, they, too,
were the products of political action, in this case during the period of
the all-India national movement from the 1920s to the 1940s.

Elites in different cities chose different responses to the politics of
mass mobilization during these decades, creating what Varshney calls a
"master narrative" about the nature of intercommunal relations. In
some, the master narrative became one of caste, with Hindu and Mus-
lim elites cooperating against low-caste Hindus. In others it became

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[26] There is now an entire literature on the nostalgia of ethnic peace. See, for example, Robert J.
Donia and John V. A. Fine, Jr., *Bosnia and Hercegovina: A Tradition Betrayed* (New York: Columbia
University Press, 1994); Maria Rosa Menocal, *The Ornament of the World: How Muslims, Jews, and
Christians Created a Culture of Tolerance in Medieval Spain* (Boston: Little, Brown, 2002); Norman
Davies and Roger Moorhouse, *Microcosm: A Portrait of a Central European City* (London: Jonathan
Cape, 2002).

one of communal identity, with Hindu leaders reaching across caste lines to mobilize against an indigenous Muslim dominant class. In the former, the choices of elites encouraged cooperation across the Hindu-Muslim divide, a form of cooperation cemented in the creation of bicommunal associations, from trade unions to business alliances. In the latter, intercommunal differences were infused with political significance, and the salience of ethnic lines as political dividers discouraged the establishment of lasting associations. Since independence, the first road has led to relative peace, the second to deadly ethnic riots.

There is a certain practical optimism here, and its lessons are important. To reduce the chances of violence, encourage intercommunal contacts—but make sure that those contacts find expression in associations. In times of social crisis, remembering the kind member of an ethnic minority who used to repair your shoes becomes a thin foundation for intercommunal peace. School textbooks that show Germans as hardworking, Jews as frugal, and Russians as jolly—a project, incidentally, sponsored by the United Nations Development Program in a multiethnic district of Ukraine—will not do the trick. Rather, elites at all levels must be bound together in repeated, patterned, and formal interactions.

Ultimately, however, the argument is perhaps less optimistic than one might think. It is not about how to secure social peace but rather is about the trade-offs involved in pursuing a particular brand of it. This is a book specifically about relations between Hindus and Muslims and the role of civil society in structuring them. Yet the concomitant of strong associational linkages across these two large communities has sometimes been conflict along other axes. The cities of Calicut and Lucknow, for example, emerge as models of Hindu-Muslim concord. But both have experienced recurrent conflict, sometimes brutally violent, along lines of caste (low-status versus high-status Hindus) and sect (Shia versus Sunni Muslims).

That fact does not diminish the power of the associational argument in explaining the Hindu-Muslim relationship, but it does lead one to wonder whether the price of concord along one social cleavage might be violence along another. Yes, the master narratives in these two cities are different from those in which Hindu-Muslim rioting has been the norm, but both also have narratives built around other, equally divisive visions of social life. It seems a stretch to describe a dense set of associational ties between Hindus and Muslims as "civic life" when those ties do not also seem to have had any carryover to the city as a whole. But this may well be the crucial, sobering point of Varshney's engaging study: in any social setting with multiple poles of allegiance, multiple

sets of grievances, and multiple exogenous shocks, peace is always a relative condition.

III. A Micropolitical Turn

In a recent survey of comparative politics, David Laitin identified a "new consensus" among comparativists.[27] He argued that the most influential new work seeks to unify three methods: survey techniques and large-N data analysis to identify broad patterns and develop hypotheses; microlevel anthropological and historical digging to uncover evidence; and explicit, perhaps formal, theorizing to link hypotheses and evidence by specifying causal mechanisms. Something similar seems to be going on in the study of social violence. It may be too much to speak of a micropolitical turn in the field, but in the books under review, as well as in the exciting work of several other writers, there are clear trends—toward a reconsideration of the scholarly traditions on which work on social violence should draw and toward an eclectic approach to what constitutes cutting-edge methods.

There seem to be at least four characteristics of this emerging research program: a stress on engaging violence at analytical levels far below the nation-state; an attentiveness to how discrete episodes of violence are defined; a skepticism about the utility of labels applied to conflicts from the outside; and a commitment to finding ways of incorporating the voices of participants into the analysis. Each of these has important implications for methods and theory building in comparative politics and international relations in general.

Disaggregating the Case

Beissinger and Varshney, in their different ways, both call for disaggregation. Episodes of social violence, whether riots or atrocities committed during civil wars, may well be patterned, but they do not occur uniformly across time or space. There are lulls and peaks. Violence comes to different cities, towns, and neighborhoods at different times. It plays itself out differently in various social contexts, even within a series of violent events that are lumped together as a single ethnic conflict or civil war. Disaggregation thus has two important advantages. It expands the number of cases and hence the number of observations available for large-N work; and it provides added nuance to our under-

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[27] Laitin, "Comparative Politics: The State of the Discipline," in Ira Katznelson and Helen V. Milner, eds., *Political Science: The State of the Discipline* (New York: W. W. Norton, 2002).

standing of the diversity of violent outcomes within the dominant unit of analysis, the nation-state. Recent work by Elisabeth Wood on El Salvador and Stathis Kalyvas on the Greek civil war moves things in these directions.[28]

Disaggregation can work in another way. Much of the literature has treated violence as merely the highest stage of mobilization. Get enough people mobilized enough—or, to use a technical expression, get them mad as hell—and you are likely to end up with someone, usually lots of people, getting killed. However, as Rogers Brubaker and David Laitin have argued, there is no reason to assume that mobilization and violence are naturally linked.[29] True, the former can sometimes lead to the latter. A strike can turn into a riot; a march can become a pogrom. Some of the same mechanisms are no doubt also at work in violent and nonviolent mobilization; killing en masse, as much as going on strike, is still a collective action problem. Yet there are certain features of social violence that have no clear analogues in the process of mobilization. How victims are selected, why atrocities occur, and how personal revenge intersects with group goals are all themes that are critical to understanding both the variability in and the life cycles of violent episodes.

Doing research on these questions demands an extreme sensitivity to microlevel social interactions, phenomena that can be studied only through detailed, almost ethnographic work. Consider the question of choosing victims. Groups and individuals are specifically targeted, often with surprising care, even in the midst of what seems an otherwise chaotic event.[30] But knowing precisely whom to kill, maim, or run out of town can be problematic, and perpetrators often have an array of techniques for sorting out friend from foe. Skin color may matter, but then humans have an infinite capacity for parsing gradations of skin tone; it is rarely a case literally of black and white. Linguistic ability can also be a criterion, but in environments of multiethnic interaction and multilingual repertoires, how one speaks is a slippery desideratum. Frequently, targeting seems to be based on more subtle characteristics of the victim: occupation, clothing, perceived social status, the football team he supports, all of which can convey important information about religion, social status, ethnicity, or other traits. Even eyewear can matter: in Romania in 1990 rioting miners, encouraged by the government,

[28] Wood, *Insurgent Collective Action and Civil War in El Salvador* (New York: Oxford University Press, 2003); Kalyvas, "The Logic of Violence in Civil War" (Book manuscript).

[29] Brubaker and Laitin, "Ethnic and Nationalist Violence," *Annual Review of Sociology* 24 (August 1998).

[30] Stathis N. Kalyvas, "The Paradox of Terrorism in Civil War," *Journal of Ethics* 8, no. 1 (2004).

were known to attack people wearing glasses, a sure sign that the target was an "intellectual" and therefore a supporter of the embattled prodemocracy movement there. None of this will be readily apparent, however, without carving off specifically violent acts from the broader process of group mobilization.[31]

INTERROGATING THE VIOLENT EVENT

Violent events are often clustered spatially and temporally. Existing research practice has been to treat the cluster itself—something called "the Bosnian war" or "the Rwandan genocide"—as the only serviceable dependent variable. Cases, in other words, have become coterminous with conflicts. But violence does not come in prepackaged units. Violent events are themselves constructed as part of the process of social violence; they are wrapped up in the constitutive power of collective action. Even at the lowest level of aggregation, the individual violent episode, bounding the case can still be frustratingly difficult. Previous instances of violence may be invoked as rallying points. What outside observers see as discrete phenomena may be, in the minds of participants, multiple iterations of the same dispute. Violent events, in other words, are not natural kinds.

The rhetorical battle for control over defining the event can thus be as much a part of the contestation as violence itself. Anyone who has spent time in violent settings, from societies plagued by sectarian discord to an English football match, can understand how difficult it is to distinguish successive iterations of violence from one another, both analytically and causally. Slicing into the complex narrative of first causes and iterated grievances can provide a cross-sectional image of a conflict at one point in time, but it can also be misleading. Any single episode of violence may be part of an intricate web of meanings connected with previous events and acting as precipitants for those to come. But things can also work in the opposite way. Participants themselves may devise very clear ways of marking off one episode from another. That is why in societies where interfamilial feuding is common, there are also usually social rules for deciding how to terminate a violent dispute—whose blood and how much of it must be spilled in order for a wrong to be righted, for example.[32] The alternative would be an endless spiral of revenge, precisely the condition that complex feuding norms are meant

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[31] For a recent effort to tackle this issue, see Scott Straus, "The Order of Genocide: Race, Power, and War in Rwanda" (Ph.D. diss., University of California, Berkeley, 2004).

[32] Christopher Boehm, *Blood Revenge: The Anthropology of Feuding in Montenegro and Other Tribal Societies* (Lawrence: University Press of Kansas, 1984).

to forestall. The point is that where any instance of collective violence begins and ends, whether it is a single riot or an entire civil war, can be determined only from within the cognitive landscape of those who are engaged in it. Marking off events as discrete by fiat of the researcher will not do the trick.

What constitutes an analytically singular event is thus both a conceptual and an empirical question, part of what Horowitz and Kalyvas have both called, in slightly different senses, the "ontology" of violence.[33] But how exactly does one go about ordering the varied and often contradictory versions of who did what to whom?

One technique is simply to rely on press reports in local languages, as Beissinger does, and to make sure that those reports come from many different, mainly indigenous sources. That, at least, takes one as close as possible to the action without requiring a multisource account of every killing. Beissinger's careful event analysis protocol (included as an appendix to his book) is a model for how one might think clearly about the problem of bounding the violent event.

Another is to write an ethnography of event making, to examine systematically the various meanings attached to violent episodes and to explore the ways in which one is marked off from another. That approach is less amenable to quantitative analysis and may produce only a *Rashomon*-like series of multiple stories. But focusing on the construction of meaning itself can provide a valuable corrective to the idea of the violent event as a naturally occurring species.[34]

A third is represented by what Horowitz has called a "near-miss strategy": doing enough microlevel work to know under what conditions a case that looked to be heading toward large-scale, mass violence instead turned into something smaller, a lynching, for example.[35] This is a technique much preached but rarely practiced. It is not quite enough to work at extremely high levels of aggregation, to ask why Yugoslavia's end was violent but Czechoslovakia's was not. Rather, follow-

[33] Horowitz (fn. 18), 56; Kalyvas, "The Ontology of 'Political Violence': Action and Identity in Civil Wars," *Perspectives on Politics* 1 (September 2003).

[34] The best recent application of this technique is Paul R. Brass, *Theft of an Idol: Text and Context in the Representation of Collective Violence* (Princeton: Princeton University Press, 1997). A brilliant model—although one involving the killing of cats rather than people—is Robert Darnton, "Workers Revolt: The Great Cat Massacre of the Rue Saint-Severin," in Darnton, *The Great Cat Massacre* (New York: Vintage, 1984). See also Sudhir Kakar, *The Colors of Violence: Cultural Identities, Religion, and Conflict* (Chicago: University of Chicago Press, 1996); and Liisa H. Malkki, *Purity and Exile: Violence, Memory, and National Cosmology among Hutu Refugees in Tanzania* (Chicago: University of Chicago Press, 1995).

[35] Horowitz (fn. 18), 478.

ing through on this strategy would involve narrowing the research focus, both spatially and temporally, and giving greater attention to cases that really seemed, but for a few key variables, to be heading in the same awful direction.

These techniques would certainly dampen scholarly ambitions, but that might not be a bad thing. They would cause researchers to take very seriously the bounding of both cases and events. They would remind us to be honest about what we are really studying: not violence *tout court*, but one small, bracketed space on a scale of behaviors running from murder to total war. Knowing with some certainty why a massacre did not escalate to genocide is not nearly as attractive as saying why one country is war torn and another peaceful. But it is probably closer to science.

PROBLEMATIZING LABELS

When "ethnic conflict" joined the mainstream of comparative politics and international relations in the early 1990s, there was a tendency to look uncritically at the labels applied to violent episodes. Actors were categorized according to ascribed identities—usually ethnic, but also sometimes religious or linguistic—and typologies were developed to sort conflicts into their respective analytical boxes.

There are two obvious problems with this way of proceeding. One is what might be called the implicit teleology of ascriptive difference. It is often too easy for labels to masquerade as causes; to declare a conflict "ethnic," say, usually rests on a set of assumptions about the roots of the conflict and the unusual levels of violence said to characterize it. But emphasizing social identities can blind researchers to the mechanisms that are at work in shaping them, often in the middle of violence itself.[36] Violence raises the stakes of defection by presenting both perpetrators and victims as threatened; it makes it more difficult to move across interidentity boundaries. As one example, in the "lynching era" in the U.S. South—from the early 1880s to the early 1930s—one-fifth of all lynchings were intraracial, whites killing whites and blacks killing blacks. The highest incidence of these within-group attacks occurred before the period when new racial laws had reestablished the clear social boundaries that had been eroded by the Civil War and Reconstruction. Lynching was thus not only an abhorrent form of intergroup

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[36] For further exploration of this point, see Rogers Brubaker and Frederick Cooper, "Beyond 'Identity,'" *Theory and Society* 29, no. 1 (2000).

violence but also a method of in-group policing.[37] Violence does not always make identity, of course, but it can certainly push a particular identity to the top of one's repertoire.

Another problem is that the way participants themselves label a conflict is often an essential part of the contentious event, not analytically prior to it. Acquiring the power to define a hegemonic discourse about a conflict is a goal self-consciously pursued by belligerents. The aim is, in part, to convince outsiders of the rightness of one's own cause and the perfidy of others, to demonstrate that the opposite side is composed only of ethnic militants, fanatical hard-liners, terrorists, separatists, and so on. But it is also to control the entire vocabulary that observers and participants use when they speak about the origins of the dispute, the identities of the belligerents, and what might count as a legitimate form of conflict termination. Labeling, in other words, is a political act.

Social identities morph. People switch sides. Labels change. None of this, however, is to argue for a postmodern rejection of analytical categories altogether. On the contrary, labels should be taken even more seriously than they normally are. What they mean, how they are used, and why some stick and others do not should be part of the raft of research questions that one asks, both of people in the middle of conflict and of the scholars who study them. In the 1990s claims about "nationality" or "ethnicity" became a central component of the way many belligerents talked about the wars they were waging. But figuring out why and how that discourse emerged is a project very different from investigating why there is more "ethnic conflict" in the world now than there was in the past. The former problematizes the label; the latter simply embraces it.

The causative power of naming is evident even today. It would not be surprising to find that, a few years hence, political science data sets show a marked increase in the incidence of something called "terrorism" beginning in 2001 and rising steadily through the early 2000s. There will no doubt be significant discussion about how to explain such an upsurge: whether it came about as a reaction to unipolarity or globalization, an outgrowth of state weakness and authoritarianism, or a reflection of postmodern angst and fundamentalist nihilism. Yet just as one might now be skeptical about whether a natural category called "ethnic conflict" began to grow after the end of the cold war, one might

[37] E. M. Beck and Stewart E. Tolnay, "When Race Didn't Matter: Black and White Mob Violence against Their Own Color," in W. Fitzhugh Brundage, ed., *Under Sentence of Death: Lynching in the South* (Chapel Hill: University of North Carolina Press, 1997).

be equally skeptical about whether a natural category called "terrorism" has now taken its place. Coding is never divorced from the particular context in which it occurs, whether the "coder" is in the middle of a civil war or the middle of a political science department. An appreciation for this context ought to be a more explicit part of research design.[38]

Theory Building as Sense Making

The new micropolitics of social violence is explicitly theory focused. Beissinger and Varshney develop broad hypotheses about political and social behavior and then test them using an array of sophisticated empirical tools. But what is perhaps most appealing about these books, and the emerging literature of which they are a part, is an implicit argument about what constitutes theory in the social sciences.

Contemporary political science privileges a particular notion of what theory is: a set of careful propositions meant to link cause and consequence. There is debate, of course, about the epistemological status of such propositions, but those debates take place within a paradigm in which theory is conceived as a mainly positivistic statement concerned with explanation. This view is remarkably out of step with most of the other social sciences, all of which have vigorous theoretical discussions that deal with issues beyond the narrow goal of explanation. One need only have a conversation with an anthropologist or a historian to understand that the realm of theory is both broader and richer than the discipline of political science has come to understand it—involving such varied enterprises as clarifying concepts, honing analytical categories, and reflecting critically on one's own research practice.

The intriguing subtext in much of the new microfoundational work on social violence is a call for theory building as sense making: a multifaceted understanding of what constitutes theoretical work, grounded in the goal of integrating the self-conscious perspectives of participants themselves. Varshney, for example, is careful to elucidate the multiple interpretations of violent acts and to caution against broad generalizations disconnected from the particular vision of rationality in which these acts are imbedded.[39] Beissinger likewise focuses on the social environment in which mobilization takes place, an environment infused with the knowledge about what other people in structurally similar

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[38] On designing a research project that takes this point into account, see David Laitin and Daniel Posner, "The Implications of Constructivism for Constructing Ethnic Fractionalization Indices," *APSA–CP: Newsletter of the Organized Section in Comparative Politics of the APSA* 12 (Winter 2001).

[39] Ashutosh Varshney, "Nationalism, Ethnic Conflict, and Rationality," *Perspectives on Politics* 1 (March 2003).

situations have done or are likely to do. Most explicitly, Stathis Kalyvas has demonstrated that multiple methods—from large-N data collection to participant interviews and careful archival work—can yield a far more complex picture not only of the interests and intentions of violent actors but also of the durable social meanings with which their acts are invested.[40] The goal of this type of work is not to reduce social behavior to individual calculation (although a kind of soft rationalism is implicit)[41] but rather to understand why a set of otherwise puzzling behaviors might, from the vantage point of those who perform them, make sense.

In practical terms, theorizing these microlevel processes entails two things, one conceptual and one empirical. First, it involves thinking hard about how to operationalize fluidity. If identities really are constructed, as most people seem to believe, how and why are they constructed as they are? And, more important, why does that fact even matter for how one studies mobilization and violence? Demonstrating that labels, identities, and social categories change over the course of a conflict or even within the context of a single violent event is an important first step. Far too little work has been done just uncovering this phenomenon in particular cases. The next task, however, is to link those changes with social behavior by treating the fact of fluidity as both dependent and independent variable: to investigate whether there are patterns of identity change within violent contexts and, if so, what accounts for them; and to examine what this says about who wins and loses in instances of large-scale killing.[42]

Second, it implies embracing the full panoply of available empirical sources as the acceptable purview of political science and to use those sources in ways consonant with the best practices of other disciplines. If we use archives, we must use them properly: reading systematically, using accepted archival notation, and being suitably critical about the textual evidence they contain. If we use interviews, we must conduct them with an appreciation for the kaleidoscopic nature of memory and sensitivity to the potential costs to our interviewees, not only in terms of their time but the potential threats they may face to their livelihood

[40] Kalyvas, "The Logic of Violence in Civil Wars," *APSA-CP: Newsletter of the Organized Section in Comparative Politics of the APSA* 14 (Spring 2003).

[41] For examples of applications to specific cases, see Stathis N. Kalyvas, "Wanton and Senseless? The Logic of Massacres in Algeria," *Rationality and Society* 11, no. 3 (1999); idem, "Red Terror: Leftist Violence during the Occupation," in Mark Mazower, ed., *After the War Was Over: Reconstructing the Family, Nation, and State in Greece, 1943–1960* (Princeton: Princeton University Press); King (fn. 7).

[42] Kanchan Chandra at MIT has been coordinating a multiple-researcher project looking at precisely these issues. See the project website at web.mit.edu/kchandra/www/caeg/.

and personal security.[43] If we use press reports, we have to handle them with the care, skepticism, and cross-checking of the best historians. Being even more explicit about the empirical substance of our work, not just the elegance of its manipulation, is crucial.

In short, we need to consider carefully what constitutes *evidence* in research on social violence, not just the reified category of *data*, which political science has come to use for the stuff of what it studies. Data carry with them the seductive promise of their own objectivity. Evidence, as any trial lawyer knows, does not. The new wave of qualitative methodologists has called for broadening and deepening discussions about problems of research design and argument,[44] but really integrating the results of these discussions into research practice has to be a goal. How to conduct an interview, how to use an archive, how to write systematic field notes, and how to "read" complicated social relationships must become as much a part of good method (and methodological education) as statistics and formal modeling. In an area as fraught with human suffering as the study of collective violence, being careful about how we engage both perpetrators and victims should be a priority. The stakes, after all, are rather high.

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[43] In one recent tragic example, in January 2004 protestors destroyed several priceless manuscripts at the Bhandarkar Oriental Research Institute near Bombay, simply because an American researcher had thanked several of the institute's scholars in the acknowledgments of his book. The protestors deemed the book offensive to Hindus. Martha Ann Overland, "Hindu Protestors Attack Prestigious Research Institute in India," *Chronicle of Higher Education*, January 23, 2004, A41.

[44] Alexander L. George and Andrew Bennett, *Case Studies and Theory Development in the Social Sciences* (Cambridge: MIT Press, 2004); Henry E. Brady and David Collier, eds., *Rethinking Social Inquiry: Diverse Tools, Shared Standards* (Lanham, Md.: Rowman and Littlefield, 2004).