Explaining Violence Against Non-Combatants in Civil War

Jeffrey Kopstein

Rivalry and Revenge: The Politics of Violence during Civil War. By Laia Balcells. New York: Cambridge University Press, 2017. 285p. $99.99 cloth, $29.99 paper.

Violence as a Generative Force: Identity, Nationalism, and Memory in a Balkan Community. By Max Bergholz. Ithaca: Cornell University Press, 2016. 464p. $35.00 cloth.

Inquiry into the logic of violence within civil wars is now one of the most theoretically rich fields within political science. The gruesome details of civil war violence have long captivated observers as examples of humans at their very worst, from Thucydides' account of the Corcyrean civil war in 427 BC, which produced intracommunal conflict so vicious that "words changed their meaning," to the Rwandan genocide at the end of the twentieth century that pitted neighbor against neighbor in unspeakable acts of cruelty (Fujii 2009). Responding to the documented shift in violence globally from interstate to intrastate conflict after the Cold War, some of the most challenging recent work in the field has focused on why combatants target non-combatants during civil wars.

The primary methodological innovation of the past two decades in this research program has involved a shift from macro-level explanations for patterns of civil war violence to accounts that draw on meso- (community) and micro- (individual) level data (Finkel and Straus 2012; King 2011). Within civil wars, some towns and villages become sites of pogroms, sexual violence, and mass shootings, while others remain relatively quiescent. What accounts for this variation? Moving to the communal level allows us to hold national-level causes constant, to leverage local variation to support or refute rival hypotheses, and to limit the pitfalls of relying on macro-level indicators that "may aggregate local cleavages in misleading ways" (Kalyvas 2006, p. 370).

The books under review make important and original contributions to this literature. Both focus on the communal and individual determinants of violence against non-combatants and remind us that violence is jointly produced by a combination of forces external and internal to the community. But Laia Balcells is a political scientist and Max Bergholz is a historian, and it should not surprise us that they approach the question of violence from different vantage points, with different tools, with different ambitions, and different expectations.

Among political scientists, the standard accounts of violence within civil wars highlight economic (Zhukov 2016), organizational (Weinstein 2006), and/or military factors (Kalyvas 2006) to account for the pattern of civilian deaths. Laia Balcells, however, directs our attention to something different: 1) political divides that antedate the war, sometimes by years, and 2) the emotion-driven desire for revenge unleashed as the conflict intensifies. Taken together, these factors, she maintains, account for the spatial and temporal patterns of civilian deaths better than the available alternatives. The conflicts she studies are important in their own right—the Spanish Civil War (1936–39) and the protracted civil conflict in Cote d'Ivoire (2002–11)—but also provide her with the data she needs to develop and test a theory that is applicable well beyond these cases.

Good social science starts with clear concepts, and Balcells tells us that different kinds of civil wars should produce different patterns of violence. The focus of *Rivalry and Revenge* is what she terms "conventional civil wars" (p. 24) in which the front lines are clearly drawn, the war is waged from stable positions, and the outcome is determined in major battles. In both Spain and Cote d'Ivoire, two sides competed for military control over territory, and once they controlled territory they controlled the local population.

Balcells conceives of two kinds of violence that are distinguished by how they are produced. *Direct* violence is carried out at close quarters, usually with light weaponry (guns, knives, and machetes), and is jointly produced by armed groups and civilians. That is, for armed groups to carry out direct violence against enemies in territory they have captured, they require the cooperation of local

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activists who can identify the enemy and sometimes take part in the violence themselves. In the Spanish Civil War, Republicans and Nationalists "looked alike" to armed groups entering villages and cities from the outside and were therefore much more easily fingered by locals. *Indirect* violence, by contrast, can be administered through aerial bombardment or artillery in areas where armed groups have no control. But this lack of immediate territorial control does not mean that it cannot be selective. Forces loyal to Franco, for example, frequently bombed neighborhoods and cities controlled by Republican units.

Having zeroed in on these two kinds of violence, Balcells then turns to the conditions under which direct violence is likely to occur. Importantly, she first notes that violence is costly, especially when it is face-to-face, because the victims, their families, and their friends will likely never forgive or forget. In those communities, however, where the two sides approach parity in electoral support in the years of political competition before the civil war, local elites might opt for the selective administration of violence against their political enemies during the war in the hope of future political dominance. If so, they will help armed groups identify their enemies and kill them. Conversely, they will be less likely to do so where they are already politically dominant or where the opposition is so overwhelming that selectively executing opponents is unlikely to secure political advantage in the future. As the war drags on, however, and more and more blood is let, we can expect direct violence to be less determined by the political calculus of local elites than by the desire for revenge for previous outrages. Politically driven direct violence occurs at the beginning of wars and revenge-driven atrocities transpire later.

Balcells tests these propositions primarily in Spain but also in Cote d'Ivoire using an impressive array of data on local-level voting returns and noncombatant deaths. In both cases, she convincingly shows that the pattern of civilian deaths in areas already controlled by a combatant side hews closer to the logic of political rivalry (in the short run) and revenge for previous killing (in the long run) than it does to the logic of its theoretical alternatives. In Spain, direct violence was most likely to occur in cities and villages where the Left and the Right engaged in closely fought electoral contests during the 1930s. In Cote d'Ivoire, rearguard territories become deadly after the presidential elections of 2010.

Beyond the impressive statistical exercise, Balcells presents some important communal-level narrative accounts showing how the mechanism that she identifies either produced or prevented direct violence. The quantitative data themselves are from the meso (community), level but Balcells claims to be offering a methodologically individualist account for the violence. Therefore, the analysis really does hinge on whether the reader is convinced by the narrative accounts showing how political parity between rivals produced direct violence during the civil war in one place and less keen rivalry produced quiescence in others. She does not find a smoking gun. Indeed, it might be unreasonable to expect to find local elites saying, in effect, "Let's turn in those Republicans and that way in a future democracy we will exercise political dominance," or "Let's not point them out to Franco's soldiers since there are so few of them"; perpetrators and rescuers do not usually leave this kind of evidence for social scientists to find. But barring lots of that kind of evidence, we are left with a highly innovative correlational exercise.

This is very good social science, and Balcells convincingly addresses competing hypotheses. If violence were the product of a "security dilemma" at the local level, prewar politics would not tell us where direct violence occurs at the outset of the war. If the degree of dominance by leftists or rightists in Spain determined the spatial variation in local noncombatant violence, then we would not expect those cities and villages with the tightest electoral races in the late 1930s to become the sites of executions. If the logic of "military control" shaped the pattern of violence, Balcells would not have found both militarily contested and uncontested areas to be susceptible to direct violence. And finally, unlike much of the literature highlighting violence among insurgent groups as a substitute for tight discipline, it is local-level political competition that explains violence *across* armed groups, rather than any internal organizational differences within the groups themselves.

There is, of course, a disturbing implication of this book. Democratic conflict and mass politics can, under the right circumstances, produce horrific results down the line when all of the other arrows are pointing in the right direction, that is, when a country finds itself in a civil war. As Balcells notes, "When individuals are politicized, they are more likely to be perceived as threatening by armed groups, which might then devote resources to their persecution. Thus, the book might be uncovering a 'dark side' of both political mobilization and competition" (p. 190). Democracy can be dangerous.

Balcells draws her cases from conventional civil wars and not from genocides, where the intention of one side is not to dominate but to exterminate the other. But she also understands that her theory and evidence have important implications for the study of ethnic conflict. She explicitly refers to aspects of the Holocaust, the Rwandan genocide, and the Yugoslav wars of the 1990s as relevant comparisons.

Max Bergholz's beautifully written and closely argued *Violence as a Generative Force* extends the study of micro-level violence to World War II Yugoslavia. Bergholz is a historian, but one who uses much the same literature and vocabulary as Balcells; he is well versed in social science but

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less committed to theory building and testing than is Balcells.

Bergholz's point of departure is a massacre of 2,000 Bosnian Muslims, including many women and children, carried out by Serb insurgents in the town of Kulef Vakuf on September 6–8, 1941, in revenge for anti-Serb violence committed by Croats and Muslims. In the summer of that year, Kulen Vakuf and the surrounding towns in northwestern Bosnia had been incorporated into the newly created Independent State of Croatia (NDH), led by the fascist Ante Pavelić. Although the region had long been the object of Croat ambitions for a "restored" state within "historic" borders, the question quickly arose as to what to do with the region's Muslims and approximately one million Serbs. Muslims could be declared Croats, creating a clear demographic majority for the titular ethnicity, but what about the Serbs? Conversion, resettlement, and execution were preferred methods.

Croat plans for ethnic purity would be difficult to realize, however. Serbs in this region were mountain dwellers, and many remained armed from their service in the Yugoslav military. They could resist. The Croat state defined itself as an ethnic state. Its local agents enacted this identity through the seizure of assets, violating Orthodox Church property and arresting and murdering locals defined as "Serb." Whatever vague sense of national and ethnic belonging was present before 1941 quickly solidified in the guerilla war that took shape.

Bergholz follows Rogers Brubaker in questioning the "naturalness" of ethnic groups. He writes: "The establishment of the NDH suddenly empowered opportunists and a few extremists to kill on an ethnic axis. This violence triggered a rapid process of antagonistic collective categorization, whereby many former neighbors suddenly viewed one another as dehumanized parts of abstract collectivities" (p. 197). Violence generated ethnic identity at least as much as ethnic identity generated violence. It is an important insight, but one is not always certain how far Bergholz wants to take it. Alongside tales of intercommunal cooperation and amity (and class divides as strong as ethnic ones), he provides plenty of evidence of ethnic division and violence even before 1941.

If "ethnicity" did not cause violence, however, what did? Why did massacres (frequently involving torture, dismemberment, and the public display of corpses) occur in some villages but not in others? It is on this question that Bergholz's study is comparable with Balcells's study on Spain. This was civil war. But Bergholz does not find the same factors at work as does Balcells. On the question of "direct violence," he argues that its logic had little to do with prewar voting patterns: "[A]nalysis of prewar voting patterns in the wider Kulen Vakuf region does not suggest clear linkages to wartime behavior" (p. 197). Elections tended to be ethnic censuses, and those communities that became violent in 1941 were no more or less electorally competitive in the prewar era than those that remained quiescent.

Bergholz does leave open the possibility that further disaggregated work will unlock some sort of electoral connection: "In the Kulen Vakuf region, the election results in some villages suggest that significant intra-ethnic splits existed (among Orthodox and Muslim residents, for example, but more qualitative micro-level research would be needed to determine whether these intra-communal voting differences were of significance with regard to local intercommunal relations and, if so, how" (p. 197). And he does point to the local factors inducing "restraint," such as a strong local communist presence, a finding consonant with what others have found in World War II Poland and Romania. But ultimately, prewar structures do not account for the variation: "This is because the dynamics of violence and events on the ground quickly assume a much greater level of importance" (p. 198).

Revenge rather than rivalry therefore makes up Bergholz's main story line, and it is here where the overlap with Balcells is the strongest. Whatever accounts for the onset of violence at the beginning of the NDH regime, within two months the cycle of violence and counterviolence had taken over. Small, even idiosyncratic, decisions within an already violent landscape—local commanders threatening revenge seekers with death, the willingness to kiss the cheeks of a terrified Muslim man in front of fighters seeking revenge, or the death of a local advocate of intercommunal harmony—could tip a community into restraint or gruesome violence.

Both of these studies point us to the theoretical frontiers of micro studies of violence within civil wars. The first frontier is demarcating the threshold where violence moves from the systematic ("direct," in Balcells's usage) to the random. Balcells and Bergholz both emphasize that violence against non-combatants in civil wars occurs in specific places, and each of these places has its own political backstory. That backstory becomes the crucial context that determines where and how direct violence initially begins. At the same time, although both tell us that these histories can be thought of as exogenous to the war (though clearly more so for Balcells than for Bergholz), once the carnage begins, as both books amply document, the violence takes on a life of its own. Just when this happens, where the threshold lies, precisely when the logic of rivalry tips into one of revenge remains a question for future research. Understanding why some civil wars last longer than others, an important debate within political science, may have something to do with the move from "rational" politically driven rivalry violence to emotionally driven revenge killings (Fearon 2004).

The second frontier for micro studies of violence is one I have already touched upon: the relationship between meso-level factors and micro-level motives. Although the shift in the literature from the national to the community

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and individual level permits us to hold the macro-level factors constant in accounting for patterns of violence, any consideration of contextual factors, no matter how low the level of aggregation—whether at the county, village, or even neighborhood—still leaves unresolved the theoretically and empirically thorny issue of individual determinants of violent behavior, that is, connecting the meso-level factors to individual motives. Balcells presents community-level results that are *consistent* with certain individual-level motives (the prospect of future political advantage), but the actual evidence for these individual motives is mixed. Bergholz, focusing with tremendous detail on a small number of communities, is able to provide a huge amount of information on motives for the onset of violence, but no single set stands out, and, on his own account, the array of factors behind the onset of violence defies generalization.

Human subjectivity is a mess—a jumble of reason, passions, and habit—and perhaps nowhere more so than during civil war. The dizzying capacity of humans *in extremis* for unpredictable acts of rationality and rage, kindness and cruelty, empathy and indifference constitutes the theoretical frontier for explaining violence against non-combatants in civil war. Historians find it unreasonable to theorize parsimoniously about this; social scientists cannot resist. Bergholz tells us that the violence in summer and fall 1941 was situational and multicausal. Balcells provides us with one set of factors that made it easier, more attractive and logical, and therefore more likely in some communities for individuals to assist those wanting to kill their neighbors. But even this logic of political calculus yields, as both authors' studies convincingly show, to a terrible emotional calculus of revenge. Those interested in sorting out the complex and tragic mix of passions and interests unleashed by the violence of civil war will be well served by starting their journey with these books.

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