

# Inside Insurgencies: Politics and Violence in an Age of Civil War

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**Understanding Civil War: Evidence and Analysis.** Edited by Paul Collier and Nicholas Sambanis. 2 vols. Washington, DC: World Bank Publications, 2003. 800p. \$60.00; \$40.00 Vol. 1, \$40.00 Vol. 2.

**The Logic of Violence in Civil War.** By Stathis Kalyvas. New York: Cambridge University Press, 2006. 508p. \$70.00 cloth, \$27.99 paper.

**Inside Rebellion: The Politics of Insurgent Violence.** By Jeremy M. Weinstein. New York: Cambridge University Press, 2006. 428p. \$70.00 cloth, \$26.99 paper.

**Insurgent Collective Action and Civil War in El Salvador.** By Elisabeth Jean Wood. New York: Cambridge University Press, 2003. 328p. \$70.00 cloth, \$25.99 paper.

## Introduction

“Inside Insurgencies”?<sup>1</sup> An odd title for a review of four books that deal with one of the most wide-ranging, violent, and protracted forms of contentious politics the world has known—civil wars. Should we not care more about their impact on citizens at large, their effects on national politics, and their creation of instability in the international system than on their interior lives? But think of the conflicts among communists, anarchists and others in the Spanish Republic: They inhibited the republic’s capacity to resist the assaults of Franco’s forces. No adequate understanding of that country’s civil war could have excluded these “internal” relations.

Not only do these four books fill in gaps in the “large-n” studies that preceded them; they also reveal the tremendous variations in the patterns of violence, goals, and actors in civil war insurgencies and expose the dynamics of civil wars. Path dependency, past histories of violence, the endogeneity of political culture to the process of civil wars, the plasticity and variety of alliances—all these and more fill in the gaps in the studies that preceded these books. They portend a new and more exciting stage in our understanding of this form of civil wars and illuminate three important features of their dynamics:

- First, they go beyond the quantitative studies of civil wars that became popular around the turn of the

new century and which have reached a plateau in their capacity to inform or enlighten.<sup>2</sup>

- Second, each book uses both detailed narrative and extensive comparison to expose the dynamics of organized violence and counterviolence.
- Third, these books promise to help us understand the complex relations between militarized insurgencies and the communities in which they operate.

In recent years, as I will show in Part I, a rationalist-derived set of studies attempted to infer the incentives for participation in insurgency from aggregate data sources. These four books go further: digging deeper into historical contexts; taking seriously the variety of dispositions of the people they encounter; relating their struggles to their structural situations; focusing on the interactions among insurgents, governments, and host communities; and demonstrating how the conflicts themselves produce new incentives, alignments, and outcomes.

All of the authors under review have taken a case-study turn. But the term “case study” does not signify a particular method. These books employ broad batteries of methods that match the boldness of their claims. Some are quantitative—if not in the number of cases they examine then in the logic of the inferences they draw; others are decidedly narrative, while still others build on different combinations of narrative, quantitative evidence, and formal modeling. In what follows I will first focus on why there has been a (re)turn to case-study methods. I will then examine two large issues that these books help to raise:

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- First, how do individual incentives for violence intersect with larger political and organizational processes?
- Second, how can we go beyond static correlations between sociodemographic variables and civil war violence to study the mechanisms and processes present within insurgencies?

All four studies provide rich evidence about both of these issues. But for ease of comparison and clarity of argument, I will center my discussion of the first issue on Jeremy Weinstein's and Stathis Kalyvas's books and focus my discussion of Elisabeth Wood's and Paul Collier and Nicholas Sambanis's books on the second.

### I. Why a Return to Qualitative Methods?

These books must be seen against the background of the tradition of quantitative studies of political violence that grew out of Ted Robert Gurr's work on conflict in the 1960s and 1970s. They also draw on types of data and reasoning that grew out of the quantitative study of international conflict that developed at the University of Michigan in the 1980s, and on the new tradition of large-n civil war studies from the late 1990s on. A rapid review of this background will help us place the achievements of these four books and underscore their departures from previous studies.

#### A. *The Gurr-reat Tradition!*

Starting from a simple social-psychological model of "civil strife" in 1968, in his early work Ted Robert Gurr carried out quantitative analyses of a variety of forms of conflict through national-level measures of protest and rebellion. His work was animated by an interest in the individual-level variable of "relative deprivation," but he mainly drew on aggregate national sociodemographic data to operationalize his major variables. While Gurr gave some attention to political determinants—for example, what he called "the balance of institutional support and coercive control"<sup>3</sup>—in *Why Men Rebel* he was mainly interested in the determinants of collective contention that could be cross-nationally correlated with measures of conflict.

In part in response to criticism of his early work,<sup>4</sup> Gurr later shifted to a more politically attuned specification of the causes of violence.<sup>5</sup> He also narrowed his focus from "conflict" in general to conflicts involving minorities,<sup>6</sup> from where it was one short step to the quantitative civil war tradition of today. Gurr did much more than this: He founded an entire school of "conflict studies" that took the study of contentious politics beyond the largely narrative traditions that were current in the 1960s.<sup>7</sup>

#### B. *From COW to Civil War*

How did that new tradition get under way? In the decades following Gurr's pathbreaking work, international relations scholars used cross-national quantitative data to examine the correlates of international conflict, including internal wars, as one of their measures.<sup>8</sup> Scholars interested in civil wars adopted the Correlates of War (COW) project's logic and its measure of civil war (e.g., a numerical threshold of conflicts producing 1,000+ deaths). They regressed these data on sociodemographic but also against political covariates, again mainly through national profiles. The research design was Gurr-like but the dependent variable had become internal wars and not all forms of conflict.<sup>9</sup>

Inspired by the COW scholars' procedures, economists Paul Collier and Anke Hoeffler, apparently oblivious of their debt to Gurr, applied them vigorously to civil wars.<sup>10</sup> Accepting COW's numerical definition, Collier and Hoeffler saw two main models of civil war origins: "greed and grievance." They wrote that "the political science and economic approaches to rebellion have assumed both different rebel motivations—grievance versus greed—and different explanations—atypical grievances and atypical opportunities."<sup>11</sup>

Unfortunately, they could measure only the individual-level incentives as they posited aggregate data. "Greed" was operationalized with proxies for what they first called "opportunity" and now call "feasibility,"<sup>12</sup> and grievances were measured with secondary measures of democracy, economic growth, and ethnicity. These measures of grievance showed very little effect on the propensity for civil war. This was a solid challenge to researchers accustomed to thinking that grievances were at the heart of civil war insurgencies.

For example, Collier and Hoeffler found significant correlations between civil wars and high levels of primary commodity exports, large populations, low levels of secondary education, low economic growth, low per capita income and the presence of previous civil wars—all of which they assigned to the "opportunity" column.<sup>13</sup> In the "grievance" column, they found only that the lack of democracy was significant, that inequality was insignificant but with the expected positive sign, and that ethnic and religious fractionation were surprisingly unimportant.<sup>14</sup> When they combined both sets of proxies into a full model, they found that a model that focused on the "opportunities" for rebellion performed well in predicting civil war onset, whereas objective indicators of "grievances" added little explanatory power.<sup>15</sup> At the turn of the new century, but with more attention to politics, James Fearon and David Laitin both criticized and built on Gurr's work.<sup>16</sup> Proceeding from a similar numerical-violence definition of civil war and from similar microeconomic premises as Collier and Hoeffler, they too found that primary exports—and especially oil—were highly correlated with

civil war outbreaks. Their other major conclusions were that civil wars are most likely to emerge 1) in mountainous territories, 2) in countries governed by weak but non-democratic governments—“anocracies”; and 3) where there is political instability. They also found that civil wars are not statistically correlated with ethnicity—which, like Collier and Hoeffler, they measured as ethno-linguistic fractionalization, or ELF.<sup>17</sup>

The Collier/Hoeffler and Fearon/Laitin work has enhanced our understanding of outbreaks of internal political violence in many times and places, encompassing relatively long time periods (1960–99 in Collier and Hoeffler’s original work, 1945–99 in Fearon and Laitin’s); facilitating comparison with economic trends and with different types of regime; and tracking changes in the number of internal insurgencies in relation to the number of international conflicts (by general agreement, the ratio has declined). Both sets of studies employ a vigorous correlational logic, are couched mainly at the national level, and use aggregate-data proxies to make inferences about the incentives of participants.<sup>18</sup>

Yet there are major lacunae in this tradition as it developed in the 1990s:

- *First*, much of the data that was used to measure the incidence of civil wars was overaggregated. In treating the number of deaths from internecine violence as its measure of the dependent variable, researchers sidestepped a more substantive definition of their key term and ignored the analytical distinction between civil wars as a whole and violence in civil wars.<sup>19</sup>
- *Second*, there were operationalizations of convenience in the measurement of the independent variables. For example, Collier and Hoeffler used as their proxy for diaspora support of insurgency only the number of emigrants living in the United States; the number of diaspora members living nearby or in refugee camps might have produced somewhat different findings. Even more surprising, Fearon and Laitin used measures of economic development as a proxy for state strength. Both groups of scholars relied on the degree of ethnic fractionation of a society as a proxy for the potential for ethnic conflict.
- *Third*, there were problems of unit heterogeneity. Unit heterogeneity refers mainly to whether the same variable has different effects in different subsets of the data—either over time or between types of regime. While both studies included temporality in their models (e.g., the Cold War vs. the post–Cold War period), neither dealt with the potential differences between ethnic versus nonethnic civil wars.<sup>20</sup> Collier and Hoeffler’s finding that ethnic fractionation does not predict civil wars could well be the result of failing to divide their samples of cases into those in which there

is a high degree of ethnic tension and those in which ethnicity is not salient.<sup>21</sup>

- *Fourth*, the use of national-level statistics to look for the causes of civil war made it difficult to tap the subnational centers of insurgency where civil wars emerge and develop. Land inequality or ethnic conflict in a region in which insurgency erupts may be washed out statistically if other regions in the country are relatively equal in landholdings or are populated by only one ethnic group. Only arduous and uncertain collection and analysis of subnational statistics for insurgency and its correlates make it possible to deal with the subnational sources of insurgency and its shift in scale to the national level.<sup>22</sup>
- *Fifth*, none of these studies gets inside the mechanisms or processes of civil war insurgencies. For example, although they demonstrate a correlation between mountainous terrain and civil war, Fearon and Laitin had little to say about the mechanisms that produce more civil wars in (some) mountainous countries than in (most) relatively flat ones or about which mechanisms are at work where. A number of complementary or competing mechanisms could explain the correlation they found between mountains and insurgency (e.g., social pressure to conform, topographic protection from governmental forces, the social solidarity of rural communities). Without a more contextualized analysis, we are unlikely to understand the dynamics of contention in mountainous territories or understand which of these mechanisms are at work in deserts or plains. (Think of Iraq!)

Most fundamentally, by hiving off civil wars from other forms of contention, quantitative scholars of civil wars risked reifying the category of civil war and downplaying the relationship between insurgencies and “lesser” forms of contention. Escalation to civil war from nonviolent contention or from less lethal forms of violence; transitions from civil wars to post–civil war conflict; co-occurrence between core conflicts in civil wars and the peripheral violence they trigger—none of these was exhaustively examined in these studies.

These large-n quantitative analyses were not intended to be the last word in civil war studies, however, and because of them we now have a better understanding of measurement problems, ontology, the problems of aggregation, the importance of mechanisms, and the limits of pure variable-based analysis of dynamic processes. Moreover, some of the original investigators are producing new and better measures of their variables and extending them further in time.<sup>23</sup> Others are taking up their findings through statistical studies of specific issues. For example, Christian Davenport, David Armstrong, and Mark Lichbach have developed models to show how alternative paths of civil

war develop out of nonlethal conflict.<sup>24</sup> David Cunningham has studied how veto players affect civil war duration.<sup>25</sup> Sambanis, who cut his intellectual teeth on the Collier-Hoeffler data set, is disaggregating separatist civil wars to the regional level,<sup>26</sup> while he and Annalisa Zinn are examining the relationship between low-level violence and protest and escalation to fully fledged civil wars,<sup>27</sup> a problem also being studied by Lichbach, Davenport, and Armstrong.<sup>28</sup>

Nevertheless, there are inherent gaps in large-n studies of insurgency that even the best measures and the longest time series cannot resolve. As Jeremy Weinstein nicely puts it, “The quantitative data that exist . . . have limited the questions scholars have been able to ask about civil war.”<sup>29</sup> In the last few years, some of the very authors who initiated the quantitative tradition have begun to produce innovative qualitative studies. The foundations of a first bridge were laid by Sambanis in his 2004 article in this journal. In recent years, Laitin has called insistently for the use of “random narratives” to complement the quantitative analyses that he and Fearon carried out.<sup>30</sup> As Collier and Sambanis put it in the introduction to their edited volume: “quantitative and qualitative research designs are often (mistakenly) considered as substitutes rather than complements in political science.”<sup>31</sup> Thus, we are seeing a growing turn and, in one case, a *re*-turn<sup>32</sup> to qualitative case methods that complement the contributions of the quantitative tradition. That takes us to the four books under review in this article.

## II. Weinstein and the Social Organization of Violence

Jeremy Weinstein’s fine monograph, *Inside Rebellion*, takes up the following puzzle: whereas some communities are victims of indiscriminate attacks from insurgent groups, others suffer far less violence. Observers of civil wars have sometimes noted the high level of such “collateral damage” in civil war situations; but Weinstein’s book is the first, to my knowledge, to propose a theory that posits violence as the result of simple predation on the part of insurgents and the result of rebellion in the name of a cause.

Weinstein studied four civil war situations in three countries on two continents, using his knowledge of English in Uganda, Portuguese in Mozambique, and Spanish in Peru to interview a range of political and military leaders, combatants, and civilians in capital cities and in the countryside.<sup>33</sup> In addition to his semistructured interviews, Weinstein and three field assistants built local histories of the insurgencies in two different regions—one closer to the center of rebel control and the other on the fringes. They also studied the internal dynamics of each group from internal and police documents and built an events database charting patterns of political violence over time during each war.<sup>34</sup> These data sources array into a neat

paired comparison of two types of insurgencies that produced two path-dependent trajectories toward different degrees and types of violence against civilian populations.

Weinstein “builds on the insight that recruitment strategies depend a great deal on the incentives that are likely to motivate individual participation. . . . High commitment individuals are *investors*. . . . Low commitment individuals are *consumers*. . . . Individuals are rational in that their actions reflect deliberate decisions designed to maximize payoffs.”<sup>35</sup> In its catholicity and its embedding in organizational contexts, Weinstein’s book is typical of the new generation of rational choice institutionalism.<sup>36</sup> But there can be few authors who would cite with equal approval—and within two pages!—work as different as that of James Scott, Mancur Olson, Samuel Popkin, Craig Jenkins, this reviewer, Doug McAdam, and Charles Tilly.<sup>37</sup>

Beyond the breadth of his citations, Weinstein goes beyond his rationalist roots in three important ways: First, in his willingness to learn from the work of the “resource mobilization” school of social movement studies,<sup>38</sup> he focuses on the *organization* of rebellion. Second, rather than plumping for either a “greed” or a “grievance” interpretation of insurgency, he deduces two major types of insurgency from his case studies:

- *Opportunistic rebellions*: where participation involves fewer risks, short-term gains are more likely, and low-commitment participants resemble *consumers*, whose commitment to the organization is weak and who expect to be rewarded immediately for their involvement.<sup>39</sup> The modal participant is an opportunist—greed trumps grievances.
- *Activist rebellions*: where participation is risky, short-term gains are unlikely, and high-commitment participants resemble *investors* dedicated to the cause of the organization and willing to make costly investments of time and risk-taking in return for the promise of rewards in the future.<sup>40</sup> The modal participant is an *investor* and grievance trumps greed.

Third, he views these differences in initial endowments, the contrasting organizations they produce, and their respective incentives to potential participants as path dependent, producing two different relations to the resident population: resource predation and indiscriminate violence in the opportunistic type; cooperative relations with the resident population and selective violence in the activist type.

Weinstein combines the progression from resources to organizational type to incentives to violence neatly in Chapter 1. He argues that

differences in how rebel groups employ violence are a consequence of initial conditions that leaders confront. Factors that raise or lower the barriers to organization by insurgent leaders—in particular whether material resources to finance warfare can be easily mobilized without civilian consent—shape the types of individuals who elect to participate, the sorts of organizations

that emerge to fight civil wars, and the strategies of violence that develop in practice.

He infers from this that

rebel groups that emerge in environments rich in natural resources or with the external support of an outside patron tend to commit high levels of indiscriminate violence; movements that arise in resource-poor contexts perpetrate far fewer abuses and employ violence selectively and strategically.<sup>41</sup>

This is both a theoretically parsimonious and an aesthetically symmetrical argument, as Weinstein illustrates in a summary graphic showing how each of the two types of organizations deals with five major organizational challenges faced by all insurgent organizations: recruitment, control, governance, violence, and resilience.<sup>42</sup> But his insistently paired comparison and his determinedly path-dependent argument raise questions that he either ignores or dismisses in asides:

- Paired comparison can be a useful discipline by forcing the analyst to go back and forth between contrasting cases, but it can also act as an analytic cage, distracting the analyst from exogenous factors or unmeasured variables. One such variable in Weinstein's model is the strategy of the state, which is sidelined as "only one part of the larger landscape from which groups emerge,"<sup>43</sup> and pops up only intermittently in the narratives. Where states deny resources to opportunistic groups, have they no discretion to turn into activist organizations? And where activists win access to resources from the state, will they have incentives to turn into opportunists?
- There is no apparent effect of participation on the socialization of recruits in Weinstein's model. What, for example, is the effect of training rituals? Of the experience of solidarity and combat? Of the view of the enemy that comes from seeing him through the sights of a rifle? Although Weinstein fills in his developmental types with care and sensitivity, once particular insurgencies attract distinct types of recruits, the process of insurgent collective action seems to have little effect.
- Are greed and grievance-based rebellions as distinct as Weinstein proposes? What of rebellions that begin with idealistic aims, but bang up against the brick wall of state repression or trudge through the molasses of unresponsive citizens; are they condemned to disappear or do they turn to the indiscriminate violence more typical of opportunistic rebellions?<sup>44</sup>
- Faced by these different patterns of resource predation, are local residents no more than inert objects, faced by the more or less selective violence that results from Weinstein's two types of organization? Do they not, in some cases, respond to violence with counterviolence, with civic militias, and by taking advantage of the atmosphere of violence around them to

settle scores against their neighbors? Do local participants—faced by different macro-organizations of insurgency—lack all autonomy?

- Weinstein has very little to say about the social and ideological contexts of these insurgencies. For example, one of his groups (RENAMO) was an overtly ethno-regional organization, while another (FRELIMO) was deeply steeped in Marxist and nationalist rhetoric. He has little to say about ethnic dynamics, which were central to the two Africa cases. RENAMO's greater level of violence may even have been due to the different nature of ethnic groups in northern Mozambique than those in southern Uganda.

Weinstein's use of organizational logics is a precious addition to the repertoire of how we study civil wars, but it should not be allowed to displace the importance of the social and ideological contexts of insurgency. These questions take us to the second study under review, which is deeply embedded in the context of the Greek civil war—Stathis Kalyvas' *The Logic of Violence in Civil War*.

### III. Kalyvas and the Ontology of Violence

In his great *minestrone* of a book, Stathis Kalyvas presents an impressive array of methodological innovations, ranging from a microanalysis of the Greek civil war to comparative analyses, both within Greece and elsewhere, from interviews to archival research, and from the micro level of why family *x* denounced family *y* to the macro level of the military strategy of the occupying forces. If nothing else, his book is a methodological tour de force, but it is much more than that: Forswearing numerical definitions of civil war for a definition of "armed combat within the boundaries of a recognized sovereign entity between parties subject to a common authority at the outset of the hostilities,"<sup>45</sup> Kalyvas offers a key distinction between civil wars and civil war *violence*. This makes it possible for him to compare and relate the strategies and actions of the war's core cleavage and its central participants to the variety of types of peripheral violence that occur on the ground and make civil wars so savage.<sup>46</sup>

Not everyone will be able to absorb the abundant diet that Kalyvas proposes. Those who wish to know everything that he knows about civil wars (a great deal!) will want to study Chapters 1–3 with care; those who want to understand his theoretical model and the comparative evidence for it should attend carefully to Chapters 4–8, where he carefully distinguishes among five different degrees of rebel hegemony and the effect of these differences on the degree of selective or indiscriminate violence; and those who want to examine his discoveries from the Greek civil war will want to read Chapters 9–10. The conclusion, drawing heavily on his contribution to this journal<sup>47</sup> warrants attention from all. I will focus mainly on the

relationships he finds between centers and peripheries in civil wars.

Like Weinstein's, this book can usefully be compared to the "greed versus grievance" dichotomy that Collier and Hoeffler posited, although Kalyvas uses that language only toward the end of the book.<sup>48</sup> However, while Weinstein's book turns on a *horizontal* paired comparison between two forms of organization of insurgency—activism and opportunism—Kalyvas's book is structured around the *vertical* relationship between centers and peripheries within civil wars: that is, between the central ideological/political cleavage at the macro level and the congeries of local conflicts and violence that it either triggers or adapts to.

Like Weinstein, Kalyvas both begins from a rationalist baseline<sup>49</sup> and refuses to reduce his actors to people who join insurgencies either to satisfy their greed or to right their grievances. Instead, he works with two parallel models of civil war organization: a *Hobbesian* model "stressing an ontology of civil wars characterized by the breakdown of authority" in which violence is privatized, and a *Schmittian* model, which "entails an ontology of civil wars based on abstract group loyalties and beliefs," which "stresses the fundamentally political nature of civil wars and its attendant processes."<sup>50</sup> Kalyvas finds both Hobbesian and Schmittian elements in the civil wars he studies, and—most importantly—finds that the peculiar dynamic and extreme brutality of civil war violence results from their interaction, rather than from properties of their participants, their histories, or their environments. In fact, one of the puzzles the book seeks to answer is that vicious violence often takes place in territories with no prior history of conflict.

This is a complicated argument, true to the "vast complexity, fluidity, and ambiguity one encounters on the ground."<sup>51</sup> The book has many strengths:

- Kalyvas discerns that the idea of a central cleavage, which dominates macro-level studies of civil wars, may not be at all central to the motives of the peripheral actors whose violence it triggers. Conversely, peripheral violence, which dominates much of the historical and anthropological literature, cannot be understood apart from the political opportunities and threats posed by the central core conflict.
- Rather than as an outcome of civil wars, Kalyvas sees violence as a process linking core cleavages and peripheral actors. That leads him to the mechanisms that are central to this process. Uncertainty and preemption play a role, but rather than being linked by the same cleavage in core and periphery, which would depend on the existence of common preferences, core and peripheral actors are connected despite the differences in their motives and alignments.
- The central mechanism linking center and periphery is not a single overarching cleavage but a set of *alli-*

*ances* between people who may have little in common.<sup>52</sup> Of course, alliance is only one mechanism. It is, however, "the most overlooked mechanism of cleavage formation and articulation"—at least in the civil war literature. Its central role in Kalyvas's book is that it allows him to go beyond a simple incentives model to a more *interactive* one that focuses on the relations between actors: central and peripheral, combatant and noncombatant.

If Kalyvas' book has a problem, it is that it reifies violence: The interaction between center and periphery focuses on violence; violent core participants have different reasons for committing violence than peripheral ones. But what of the relationship between violent and *nonviolent* forms of contention in the development of their alliances? Do all forms of contention give way to violence once it is triggered by the core cleavage? Do peripheral participants invariably choose violence once it begins to percolate outward from the core of the civil war conflict? Or can they—as many participants in Northern Ireland's conflicts did at the height of the troubles—continue to engage in routine, if still contentious, forms of politics?<sup>53</sup>

Early in his book, Kalyvas dismisses the relevance of work on *nonviolent* conflict with a simple syllogism: Since "contentious politics" takes place in peacetime, and since peace and war are fundamentally different, that tradition of research on non-violent conflict cannot help us to understand civil wars.<sup>54</sup> But we find many mechanisms in civil wars that parallel similar mechanisms in other forms of contentious politics. Think of the mechanism of brokerage;<sup>55</sup> it links actors in nonviolent as well as in violent conflicts. And if this is the case, then Kalyvas unnecessarily limits his framework by focusing only on civil war violence.

These two books are remarkable for putting flesh on the skeletal bones of much discussion. Both recognize that a wide variety of motives animates participants in civil wars; this leads to two books that center on the dynamics of contention in civil wars. Both utilize a rich combination of methods, avoiding the relentless quantifying that characterizes the large-n tradition. And both transcend the "greed versus grievance" debate by distinguishing among the ways in which different forms of insurgent organization (Weinstein) and different degrees of rebel control (Kalyvas) affect whether participants employ selective or indiscriminate violence. However, neither book examines very carefully the dynamics of civil war: what kinds of noncivil war contention they come from and how they evolve internally. This takes us to the second pair of studies under review.

## IV. Wood: The Endogeneity of Civil Wars

Elisabeth Wood's *Insurgent Collective Action and Civil War in El Salvador* illustrates just how wide a range of methods

can be trained on civil wars. The book begins with a classical structuralist account of popular insurgency. “The Salvadoran civil war,” Wood writes,

was, at the macro level, a struggle between classes. The long-standing oligarchic alliance of the economic elite and the military led to a highly unequal society in which the great majority of Salvadorans were excluded from all but the most meager life opportunities.<sup>56</sup>

Her book ends with a formal model of insurgent collective action in the Appendix. In between, and for 265 dense pages, she adapts the methods of historical ethnography, supported by comparative analysis, to the study of the micro level of political insurgency. Not only that: Unlike both classical scholars of revolutions and formal modelers, Wood carried out her research in intimate contact with the Salvadoran civil war, where ethnographic methods based on interviews with insurgents and government supporters were, to put it mildly, hard to employ (see Chapter 2).

Wood’s book is based largely on interviews—many of them repeated over several years—with activists, insurgents, opponents, and elites—in five areas of El Salvador. But it is also based on careful secondary analysis of surveys and an ingenious use of mapmaking by her respondents, a process that both helped her to plot the depth of the changes in the communities she studied and to glimpse the depth of the changes in their political culture (see Chapter 7).<sup>57</sup> Their human quality alone warrants the effort of surfing the Web to find these maps, but they also demonstrate the significant changes in land tenure that resulted from the insurgency and the agrarian reforms it produced.

The book shows that there is no necessary contradiction between informed case studies and good comparative analysis. Wood did most of the research for *Insurgent Collective Action* before publishing a paired comparison, *Forging Democracy From Below: Insurgent Transitions in South Africa and El Salvador* (2000). South Africa provided her with a comparative benchmark for the case study work she had done in El Salvador, a model that other scholars can fruitfully adopt.<sup>58</sup> The first book made the argument that democratic transitions are driven by collective action from below; in this book, she shows how such action was sustained in the face of state violence.

In the middle and late 1970s, some Salvadoran *campesinos* had joined a wave of nonviolent protests, while a very few collaborated with the guerilla organizations that would eventually join the Farabundo Martí National Liberation Front (FMLN). But unrest and violence deepened when a coalition of landlords and military hard-liners began a campaign of brutal and indiscriminate repression. That led more residents of the case-study area to join the FMLN and many more to support it covertly.<sup>59</sup> The implicit model that emerges from this story is that the repression of legitimate and deeply felt grievances leads to

support for armed insurgency when superior armed force is used indiscriminately.

Wood finds little direct correlation between the degree of inequality of peasants in different zones that she and others have studied in El Salvador and the degree of support for the insurgents. No standard economic or social measures predicted which areas supported the guerillas and which either remained neutral or lent support for the government.<sup>60</sup> As she concludes: “Many *campesinos* ran extraordinary risks to support the insurgency over many years . . . despite the absence of benefits contingent on their participation.”<sup>61</sup>

If the conventional material benefits (e.g., “greed”) could not explain participants’ willingness to take high risks, what did? Wood argues that “an emergent insurgent political culture was key to generation and sustaining the insurgency despite its high costs.” She stresses how the peasants who joined the insurgency “came to interpret insurgency as justified by the injustice of existing social relations and state violence, and to interpret its costs, even the highest of them, as meaningful sacrifices.”<sup>62</sup>

Wood is not the first to argue that political culture matters, or that the attribution of injustice is necessary to produce collective action.<sup>63</sup> What is unusual about her work is that she applies these ideas to a type of mobilization—civil war insurgencies—that have most often been understood in broadly structuralist terms, and that she specifies the links between grievances and outcomes through a series of processes and mechanisms that she infers from the data and not through mechanical correlations. These processes she calls *participation*, *defiance*, and *the pleasure of agency*. She sees them as supported by two path-dependent aspects of the civil war: *local past patterns of violence* and *proximity to insurgent forces*.<sup>64</sup> Many participants, she argues, came to value participation per se; some activists who had suffered at the hands of authorities were driven by feelings of moral outrage; others claimed authorship for the successes of their actions. The two path-dependent aspects—past state violence and proximity to insurgent forces—set these processes in motion.<sup>65</sup>

Like many of her cohort, Wood begins from the premise that individuals decide whether to participate in collective action on the basis of its anticipated costs and benefits,<sup>66</sup> citing Thomas Schelling’s foundational 1978 work on collective action as a coordination game. But her viewpoint shares little with Mancur Olson’s conventional formulation of the collective action problem, because the processes she specifies “are *intrinsic* to the process of participation itself”: *process-regarding*, *other-regarding*, and *endogenous* to the course of the war.<sup>67</sup> Wood’s conception of the realization of the potential for mobilization differs from rational choice models “in that the likely outcomes of participation are not evaluated in terms of conventional self-regarding and outcome-oriented preferences” and take place, at least in part, in the course of the

conflict.<sup>68</sup> Rather than focus only on the *emergence* of civil war insurgencies or on static comparisons between greed and grievance, Wood looks ahead from conflict emergence to the processes endogenous to mobilization.

### V. Collier and Sambanis: Connecting the Dots Through Case Studies

Paul Collier and Nicholas Sambanis's embrace of qualitative methods in their two-volume edited work *Understanding Civil War* sits at the opposite pole of intimacy from Wood's monograph. Enlisting the cooperation of 16 coauthors, the editors' effort puts flesh on the bones of what had been, for Collier and Hoeffler, a largely quantitative exercise. True, in his spirited 2004 article in this journal, Sambanis had already made a powerful case for a qualitative turn in civil war studies; in this volume, we gain both comparative breadth and historical depth as he and Collier ask their authors to produce "thick" descriptions of 22 cases to "help identify the causal mechanisms through which the independent variables in the Collier-Hoeffler (CH) model influence the risk of civil war onset."<sup>69</sup> Even if the voices of their co-authors are not entirely in tune with those of the editors, Collier and Sambanis's book provides strong evidence that the triangulation of quantitative and qualitative methods will lead to a deeper understanding of civil war. In his conclusion, Sambanis examines with disarming candor how well the CH model emerges from its confrontation with the range of case studies in the volumes he and Collier have edited.

Collier and Sambanis's book presents us with a different relationship among theory, case studies, and comparative analysis than Wood's. Where Wood delved deeply into the motivations, cultures, and sufferings of insurgent *campesinos* in two small case-study areas of El Salvador, *Understanding Civil War* consists of 18 case studies of internal insurgencies by authors whose work ranges across Europe, Central Asia, and Africa, followed by a spirited and self-critical conclusion by Sambanis. Where Wood began from rational choice premises but felt obliged to radically revise them in interpreting her data, Collier and Sambanis cheerfully model rebellion "as an industry that generates profits from looting." And where Wood proceeded ethnographically through interviews and mapmaking, offering a formal model for illustrative purposes only in an Appendix, they base their case selection on a formal economic model and use the cases to develop the theory further and "add context and texture to the basic insights" of the original model.<sup>70</sup>

Collier and Sambanis are no ethnographers: The particular virtues of their study lie in three of its features: *first*, their selection of a panoply of talented country experts; *second*, their ability to convince (most of) their authors to take the original CH work seriously enough to relate it to their papers; and *third*, exhaustively confronting the lacu-

nae and imperfections in the large-n tradition as these emerged from its confrontation with the case studies (see Sambanis's conclusions, which appear in identical form in both volumes).

*1. Case Study Selection and Design.* Space limitations prevent me from describing the rich diet of country cases in detail. Because of the high susceptibility of Africa to internal strife, the studies in Volume 1 merit close attention, especially Annalisa Zinn's chapter on Nigeria and Macartan Humphreys and Habaye ag Mohamed's comparison of Senegal and Mali. Because its setting is counterintuitive (e.g., not an "anocracy" but a highly developed, democratic system in the West), Douglas Woodwell's chapter on Northern Ireland in Volume 2 is a lively challenge to a generalization that the large-n tradition supports. Because of its deliberate focus on the dynamics of contention, Kalyvas and Sambanis's chapter on Bosnia deserves close reading, too.

The selection of the cases, identically justified in the introduction to both volumes, is difficult to fault. The authors are all experts in their own right. Moreover, selecting on both the independent and the dependent variables, Collier and Sambanis "included mostly countries that had experienced at least one civil war, but also high-risk countries that did not have a war."<sup>71</sup> They thus find useful information both on cases that the CH model explained well and on those that the model predicted poorly, either by predicting wars that failed to occur or by failing to predict some that did. The latter cases serve their purpose of refining and correcting the findings of the CH model particularly well. For example, in Kenya, despite low income, intense ethnic antagonisms, and a coup attempt, there was no civil war. This may be explained by how clientelism—absent from the original CH model—effectively dampened the prospects for insurgency (see Vol. 1, Chapter 5).

*2. Taking Collier/Hoeffler Seriously.* Several of the large-n findings of the original Collier-Hoeffler study are questioned or modified by the case evidence. Two in particular deserve special attention: the extent and meaning of "resource predation" as a motive for insurgency and the role (and measurement) of ethnic conflict in causing civil wars.

Both Collier and Hoeffler and Fearon and Laitin found that country dependency on primary commodity export was significantly correlated with civil war. Diamond predation in Sierra Leone and oil revenues in Biafra resonate with this generalization. But some of the case study narratives "show that those natural resources were neither a motive for the war nor a means to sustain rebellion."<sup>72</sup> The correlation between primary commodity resources and insurgency that emerges from the large-n studies could either have been the result of the motives for insurgent



action or of a resource to sustain insurgency, once the insurgents went to war for ideological reasons. In other words, resource predation might be the result of pure “greed” or only a side product of “grievance.” The lesson is clear, and Sambanis draws it repeatedly in his concluding chapter: Resource predation is not a correlation but a *mechanism*, and the correlation between the presence of natural resources and civil war is compatible with several alternative mechanisms.

The issue of ethnicity is somewhat different. With the use of large-n national statistics, ethnicity can be specified either as ethnic fractionation or as ethnic dominance.<sup>73</sup> Both the Collier/Hoeffler and Fearon/Laitin studies found that ethnic fractionation was not important in triggering civil war, but several case studies question this finding, Ali Abdel Gadir Ali, Ibrahim A. Elbadawi, and Atta El-Batahani’s Chapter 7 on Sudan in Volume 1 being the most striking. Of course, both in Biafra and in the Sudan, oil politics and—in the latter case—race complicate the equation. But more important is how ethnicity is specified and operationalized in relation to insurgency. Where ethnic *fractionation* may even contribute to peace by increasing the costs of coordinating a rebellion across ethnic lines, as Collier and Hoeffler argue, ethnic *dominance* can increase the risk of civil war, a finding that is supported by several of the case studies. We are clearly going to need more direct measures of the potential for ethnic conflict.

Why ethnic dominance should increase the chances of civil war while ethnic fractionation does not leads us to the mechanisms that link ethnicity to violence. One such mechanism might be *mutual recognition* between members of the same, and of different, ethnic groups, a distinction that made Mali more prone to ethnic conflict than Senegal (see Chapter 9 in Volume 1). Another mechanism is the *fear of victimization or exclusion*, especially when ethnic divisions overlap with class cleavages<sup>74</sup>—as in the Northern Irish case examined by Woodwell (see Volume 2, Chapter 6). A third is *civil-society segmentation or interaction*: Where ethnic differences are bridged by instrumental interdependencies among ethnic groups, their interactions undercut political entrepreneurs’ calls for ethnic mobilization.<sup>75</sup> But the operationalization of this “civil society” mechanism would have required Collier and Hoeffler (and Fearon and Laitin as well) to have collected difficult-to-standardize direct measures of civil society.<sup>76</sup>

*3. Confronting Mechanisms and Processes.* More generally, the case studies in *Understanding Civil War* underscore the difficulty of specifying the mechanisms and processes that lead to civil war through large-n correlational analysis. In his conclusion, Sambanis is nothing if not candid about this: “The difficulties associated with distinguishing between rival mechanisms on the basis of limited quantitative results are becoming clearer,” he begins.<sup>77</sup> His solu-

tion is to confront the quantitative findings with the findings in the case studies and to speculate about the mechanisms that could explain them.

Given the different metrics and the different degrees of contextuality in large-n and small-n studies, this goal is ambitious enough for civil war *onset*. But identifying the operative mechanisms is even more daunting when we turn to the process of *escalation*, for example, how and when nonviolent protest and low levels of violence escalate into civil war. Sambanis observes that such transitions are poorly understood. He writes that “there is currently no overarching theory of political violence that explains how societies transition from one form of violence to another.”<sup>78</sup> His theoretical question has a methodological underpinning: Most large-n studies contrast civil wars with *noncivil* war situations; perhaps a more relevant comparison is between situations of high levels of contention that do not lead to civil wars and those that do.

Neighborhood and spillover effects complicate the issue further—a possibility that worries the rulers of Saudi Arabia, Jordan, and Turkey as they contemplate Iraq. If neighboring states, nearby diasporas, or movement missionaries instigate insurgency, it could spread even to countries whose internal covariates would predict no civil war. Far from responding only to predominantly domestic conditions, civil wars may be part of regional cycles of violence, as the chapters on Lebanon, Bosnia, and the Caucasus suggest, Chapters 3, 7, and 9, respectively, in Volume 2.

What produces the escalation of episodes of contention into civil wars? On the basis of the case studies in these two volumes, Sambanis hazards a hypothesis, “that government repression increases opposition and, if repression is incomplete, it can lead to violence.”<sup>79</sup> This is entirely compatible with Wood’s interpretation from El Salvador: “Unrest and violence deepened,” she writes, “when a coalition of landlords and military hard-liners brutally derailed a reformist government’s attempt at a limited agrarian reform along the coastal plain.”<sup>80</sup>

Reflecting on the convergence of Wood’s and Sambanis’ conclusions, we see the benefits of triangulating different forms of data analysis:<sup>81</sup> Sambanis’s perusal of a number of case studies from around the world resonates with Wood’s paired comparison and ethnographic case study from El Salvador. The repression of legitimate and deeply felt grievances produces support for armed insurgency when superior armed force is used indiscriminately.<sup>82</sup> Whether Wood’s conclusions and Sambanis’s speculations can best be replicated through better large-n comparative studies or through more focused case studies remains to be seen.

## VI. Conclusions

We learn a great deal from these four studies about the variations among civil wars, the relationship of central

and peripheral actors to one another, and the dynamics of conflict within civil wars. In these books we find archival research, interview methods, formal models, quantitative evidence, ethnographic mapping, and a good dose of reflection and interpretation. We also learn that civil wars vary enormously: Some are strongly ideologically driven, and others seem to center on resource predation; some are ethnically structured, while others seem to cross ethnic lines; and some seek separate fates for their supporters while others fight over control of central states. If there is a “master model” of civil wars, we still do not know what it is.

I think it would be wrong to conclude from these studies, however, that future scholars should abjure large-n quantitative studies in favor of soaking and poking. For a start, many of the questions examined by these authors were put on the research agenda by the large-n studies that preceded them. In addition, they all use quantitative methods, either independent of their qualitative approaches or in combination with them. Their advantage over the large-n generation of studies examined in Part I is that they work harder to contextualize their findings and carry out careful smaller-n comparisons that underscore the relevance of internal relations and pinpoint key mechanisms that drive them. In so doing, these studies will inform the generation of large-n studies that is sure to follow.

How so? For one thing, they show how much definitions matter.<sup>83</sup> Except for Collier and Sambanis, whose book is tied to the earlier numerical criterion of civil wars, none of the authors surveyed here adopted the COW definition—and even Sambanis, in the conclusion to *Understanding Civil War*, seems uneasy with this criterion. Scholars are going to have to think hard and long about whether to use a definition that is standardized but may be purely nominal, or one that is contextualized but less precise, and which may require them to disaggregate the very concept of civil war.

Second, subnational spaces matter a great deal, if only because they help us to avoid the flattening effect of using national aggregate statistics to study civil conflicts<sup>84</sup> and allow us to study how center/periphery dyads map onto civil wars.<sup>85</sup> In his forthcoming work, Sambanis has made heroic efforts to collect and analyze subnational data to overcome this problem, but territorial lines will need to be drawn for the purpose of analysis, not determined by the preferences of census takers. The lines that divide, say, mountainous areas from the plains may not map onto the divisions between the areas in which different ethnic groups are dominant.

Third, all of these studies demonstrate the argument that Kalyvas and Wood make most forcefully: that it is not quantities but *interactions* that are the key to the dynamics of violence in civil wars. The aggregate properties of populations in civil war episodes offer hints of crucial interactions, but they have not yet provided a firm handle

on who is killing whom or who allies with whom across which political or territorial divides. For that to happen, scholars will have to pay more deliberate attention to the mechanisms and processes that drive contention.

We can go further: Should these qualitative studies be seen as way stations—albeit nourishing ones—on the road between the last wave of large-n studies and the next? Clearly, case studies can serve as a useful corrective for the approximations of the large-n studies and offer hypotheses that can be tested on larger populations of cases. But I think qualitative research has an independent contribution to make as well.<sup>86</sup> For example, qualitative studies can help in making a choice among competing mechanisms, can explicate the links between contexts and forms of insurgency, and can identify key actors and alliances in the process of civil war emergence and duration.

This question is related to whether we want “civil wars” to become a subspecialty distinct from other forms of mass violence and from nonviolent forms of contention. It was a step in the right direction to distinguish civil wars from other forms of civil strife. But unless we are careful to map their relations to social movements and other forms of contention, this new tradition will risk becoming a specialty that guards its borders and ignores findings from research on adjoining forms of contention. This means that we should try to embed our studies of civil wars in the larger episodes of contention from which they emerge and to which they may eventually give way. Social movement scholars have been working on this problem for decades<sup>87</sup> and have recently begun to broaden their horizon from movements to entire episodes of contention.<sup>88</sup> It is a welcome sign that students of civil war are beginning to do the same.<sup>89</sup> Here is how Charles Tilly—no stranger to studies of violence—puts it:

... collective violence occupies a perilous but coherent place in contentious politics. It emerges from the ebb and flow of collective claim making and struggles for power. It interweaves incessantly with nonviolent politics, varies systematically with political regimes, and changes as a consequence of essentially the same causes that operate in the nonviolent zones of collective political life.<sup>90</sup>

A microscopic focus on civil war violence that ignores both its predecessors and its correlates in routine politics risks forgetting what Kalyvas observes elsewhere in his book about conflict’s “deep structure.” “The challenge,” Kalyvas writes, “is to specify this ‘deep structure’ in terms that are general enough to accommodate the appropriate analysis without falling into the trap of maximum extension and conceptual stretching.”<sup>91</sup>

There is, finally, the question of how countries get out of insurgencies. We know that one of the best predictors of civil wars is a history of violent conflict; we also know that the factors that produce a propensity for civil war onset are not the same ones as those that produce civil war prevalence.<sup>92</sup> What are the factors and the mechanisms

that are likely to produce postwar civil peace? Power sharing? Vigorous peacemaking by international institutions? Or simply stalemate and exhaustion on the part of the internal antagonists? In this era of intractable civil wars, this may well be the next phase in civil war studies.

## Notes

- 1 I am grateful to Lars-Erik Cederman, Jaikwan Jung, Stathis Kalyvas, Mark Lichbach, Nicholas Sambanis, Charles Tilly, Nic Van de Walle, Jeremy Weinstein, and Elisabeth Wood for help with an early version of this article and to The Jazzman for giving me the opportunity to write it.
- 2 Needless to say, practitioners of this method would not agree and believe that better measures and longer data sets will fill the theoretical lacunae in their work: See the new analysis of the causes of civil war with improved data extending to 2004 by Paul Collier and his collaborators (2006).
- 3 Gurr 1970, 330–33; 232 ff.
- 4 Snyder and Tilly 1972.
- 5 Compare Gurr 1968 with Gurr 1986 and Gurr, ed., 1980.
- 6 Gurr et al. 1993 and Gurr 2000.
- 7 Needless to say, Gurr was not alone. Though also drawing on quantitative data, Charles Tilly founded a tradition that was far more embedded in political and historical contexts than was Gurr's work. For a review of Tilly's trajectory up to the mid-1990s, see Tarrow 1996.
- 8 Small and Singer 1982.
- 9 See Sambanis 2004 for a review.
- 10 Collier and Hoeffler 2000. One of my informants, who shall remain nameless, observed that "economists only recognize that work exists if it was written by other economists." Thirteen years after the publication of Gurr's *Minorities at Risk*, Collier et al. tranquilly continued to ignore his work and their (indirect) debt to it (Collier, Hoeffler, and Rohner 2006).
- 11 Collier and Hoeffler 2000, 2.
- 12 Collier, Hoeffler, and Rohner 2006.
- 13 Collier and Hoeffler 2000, summarized in Collier and Sambanis 2003, vols. 1 and 2, pp. 9–12.
- 14 Collier and Hoeffler 2000, 10; Collier and Sambanis 2003, vols. 1 and 2, p. 13.
- 15 Collier and Hoeffler 2000, 11; Collier and Sambanis 2003, vols. 1 and 2, p. 17.
- 16 Fearon and Laitin 2003. See their "Collaborative Project: 'Minorities at Risk'. Data Base and Explaining Ethnic Violence." NSF Grant Proposal by James D. Fearon and David D. Laitin. They also offer detailed descriptions of how they constructed their variables and additional tables of analysis at [www.stanford.edu/~jfearon/papers/addtables.pdf](http://www.stanford.edu/~jfearon/papers/addtables.pdf).
- 17 Fearon and Laitin 2003. In a careful critique and reanalysis of civil war data from Africa and Eurasia, Cederman and Girardin argue that, however well measured, "ethnic fractionation" does not measure what is central to ethnic conflict, which, for them, is exclusion from positions of power. For their critique and an alternative proposed measure that produces positive results for the correlation between ethnicity and civil war, see Cederman and Girardin 2007; for a response, see Fearon, Kasara, and Laitin 2007).
- 18 See the review in Kalyvas 2007.
- 19 Ibid., 20; Collier and Sambanis 2003, vol. 1, pp. 323–24.
- 20 Collier and Sambanis 2003, vol. 1, pp. 324–25.
- 21 Kalyvas 2007. Fearon and Laitin (2003) provided separate models for ethnic and nonethnic civil wars (Models 2 and 4 in Table 1 on p. 84 of their 2003 article), which essentially showed similar correlations. In their newer work, Collier, Hoeffler, and Rohner (2006, 14) find a significant effect of ethnic fractionation on civil war risk, "in the most ethnically diverse societies, most notably in Africa," findings which differ from the earlier findings.
- 22 Brubaker and Laitin 1998, 447; Kalyvas 2007, 49; Sambanis 2004.
- 23 Collier, Hoeffler, and Rohner 2006; Fearon, Kasara, and Laitin 2007.
- 24 Davenport, Armstrong, and Lichbach 2006.
- 25 Cunningham 2006.
- 26 "A Theory of Separatist Civil War," book manuscript in progress.
- 27 Sambanis and Zinn 2003.
- 28 Lichbach, Davenport, and Armstrong 2004.
- 29 Weinstein 2006, 366.
- 30 Fearon and Laitin 2005.
- 31 Collier and Sambanis 2003, vol. 1, p. 2.
- 32 Before turning to large-n analyses of civil wars, David Laitin did distinguished work as a political ethnographer in Somalia (1977), Nigeria (1986), and the former Soviet Union (1998).
- 33 See Table A.1 on p. 358 of Weinstein 2006 for a summary of the distribution of his interviews.
- 34 Weinstein 2006, Appendix A.
- 35 Ibid., pp. 8, 9, 40.
- 36 Katznelson and Weingast 2005.
- 37 Weinstein 2006, 8–9.
- 38 Ibid., pp. 46 ff.
- 39 Ibid., pp. 9–10.
- 40 Ibid., pp. 8–9.
- 41 Ibid., p. 7.
- 42 Ibid., Fig. 0.1, p. 12.
- 43 Ibid., p. 15.
- 44 Ibid., p. 259.
- 45 Kalyvas 2006, 5.
- 46 Ibid., p. 20.

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- 47 Kalyvas 2003.  
48 Kalyvas 2006, 376.  
49 Ibid., p. 13.  
50 Ibid., p. 376; see also Kalyvas 2003, 475–76.  
51 Kalyvas 2006, 10.  
52 Ibid., pp. 381–86.  
53 Tilly and Tarrow 2006, chap. 8.  
54 Kalyvas 2006, 22.  
55 Tilly 2003 and 2005.  
56 Wood 2003, 11.  
57 Wood's maps are reproduced in black and white in the book itself, but have been made available in color at [www://us.Cambridge.org/features/wood](http://www://us.Cambridge.org/features/wood). Do not miss them; her hard work and the publisher's repay careful reflection!  
58 Take one example of one who did not: A student of peasant communism in southern Italy failed to place his work in the context of other communist-led peasant movements before he rushed into print. Had he done so, he might have recognized more of the mechanisms he encountered in the field as typical of rural insurgencies elsewhere. Mea culpa; compare Tarrow 1967 and 2006.  
59 Wood 2003, 2.  
60 Ibid., pp. 17–18.  
61 Ibid., p. 224.  
62 Ibid., p. 225.  
63 See the classic historical studies by Thompson (1966) and Moore (1966). In the social movement field, a tradition of cultural analysis from Alberto Melucci (1989) and William Gamson (1992) to Snow et al. (1986) and Klandermans (1992) has focused on how people turn grievances into mobilization.  
64 Wood 2003, 231.  
65 Ibid., pp. 232–39.  
66 Ibid., p. 267.  
67 Ibid., p. 240.  
68 Ibid., p. 267.  
69 Collier and Sambanis 2003, vol. 1, p. 1.  
70 Ibid.  
71 Ibid., vols. 1 and 2, p. 21.  
72 Ibid., vol. 1, p. 309.  
73 But see Cederman and Girardin 2007 for an alternative method.  
74 Collier and Sambanis 2003, vol. 2, p. 181.  
75 Varshney 2002.  
76 Collier and Sambanis are well aware of the “omitted variable” problem (vol. 2, p. 328) but are perhaps a bit optimistic about the ease with which such variables as civil society interaction across ethnic groups can be included in large-n cross-national analyses. For an early attempt to do so in a small-n cross-national study, see Varshney 2001.  
77 Collier and Sambanis 2003, vol. 1, p. 307.  
78 Ibid., vol. 1, p. 323; also see Sambanis and Zinn 2003.  
79 Collier and Sambanis 2003, vol. 1, p. 318; also see Lichbach 1987.  
80 Wood 2003, 2.  
81 Tarrow 2004.  
82 Wood 2003, 2.  
83 Sambanis 2004.  
84 Kalyvas 2007.  
85 Buhaug, Cederman, and Rød 2006.  
86 Tarrow 2004.  
87 Among others, see della Porta and Tarrow (1986), Koopmans (2004), Mueller (1999), Oliver and Myers (1998), and Tarrow (1989).  
88 McAdam, Tarrow, and Tilly 2001; Tilly and Tarrow 2006.  
89 Davenport, Armstrong and Lichbach 2006; Lichbach, Davenport, and Armstrong 2004; Sambanis and Zinn 2003.  
90 Tilly 2002, p. 238.  
91 Kalyvas 2006, 9.  
92 Sambanis 2004, 849.

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