16 Promises and pitfalls of an emerging research program: the microdynamics of civil war

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The study of civil war ranks among the most notable developments in political science during the last decade. Several important papers have been published in this period and the field has witnessed an important shift toward cross-national, large-N econometric studies (e.g. Collier and Hoeffler 2004; Fearon and Laitin 2003), following a previous shift from the case-study format to that of theoretically informed studies (Wickham-Crowley 1992; Skocpol 1979; Scott 1976; Eckstein 1965).

However, despite these advances much remains to be understood. On the one hand, the conceptual foundations of our understanding of civil wars are still weak (Kalyvas 2001; 2003; Cramer 2002); on the other hand, econometric studies have produced very little in terms of robust results – the main one being that, like autocratic regimes (Przeworski *et al.* 2000), civil wars are more likely to occur in poor countries. The problems of econometric studies are well known: their main findings are incredibly sensitive to coding and measurement procedures (Hegre and Sambanis 2006; Montalvo and Reynal-Querol 2005; Sambanis 2004b); they entail a considerable distance between theoretical constructs and proxies (Cederman and Girardin 2007; Fearon *et al.* 2007) as well as multiple observationally equivalent pathways (Kalyvas 2007; Humphreys 2005; Kocher 2004; Sambanis 2004a); they suffer from endogeneity (Miguel *et al.* 2004); they lack clear microfoundations or are based on erroneous ones (Cramer 2007; Kalyvas and Kocher 2007b; Gutiérrez Sanín 2004); and, finally, they are subject to narrow (and untheorized) scope conditions (Wimmer and Min 2006).

In response to these problems, a new research program has emerged: the microdynamics of civil war. It calls for the systematic collection of data at the subnational level and its sophisticated analysis. Compared to the macro level, a subnational focus offers the possibility of improving data quality, testing microfoundations and causal mechanisms, maximizing the fit between concepts and data, and controlling for many variables that can be held constant. Inspired by pioneering research on political violence and contention – whether quantitative (Tong 1991; Greer 1935), ethnographic (Wood 2003; Brass 1997), or mixed-method (Wilkinson 2004; Gould 2003; Varshney 2002; Petersen 2001; Tarrow 1998; Tilly 1978) – this research program is presently in full bloom.[1]

These studies address a variety of questions (e.g. patterns of conflict, dynamics of violence, logic of recruitment and displacement, processes of demobilization, effects of civil war), focus on several units of analysis (e.g. events, localities, organizations, individuals) and geographical regions (e.g. Colombia, Greece, Sierra Leone, Rwanda), spring from different disciplines (e.g. political science, sociology, economics), and rely on diverse methods (e.g. individual surveys, human rights datasets, archival research, GIS or Geographic Information Systems), but they do share a distinct outlook, namely the systematic application of social scientific methods at a level of analysis and on a type of problem where such methods did not traditionally enjoy much purchase.

Clearly, this is a very exciting development which has already begun to deepen our understanding of civil conflict and mass violence. At the same time, however, like any such program, it entails certain compromises. The most obvious ones are the sacrifice of a measure of external validity to gain more internal validity and the exclusion of those macro processes that cannot be analyzed at the micro level. These compromises are often accompanied by a pronounced lack of clarity on scope conditions, and a tendency, sometimes, toward reckless extrapolation from the micro to the macro level.

Besides these somewhat inherent "structural" compromises, the incipient microdynamics literature is characterized by recurrent flaws that can be addressed and corrected. Here, I focus on two such flaws. The first one is the use of overaggregated variables. One would not expect this to be a problem in micro-level research, yet it turns out to be. A combination of insufficient theorization, superficial engagement with the case at hand, and reliance on off-the-shelf datasets leads to the use of variables that are insufficiently or inadequately disaggregated. These practices lead to the reproduction of problems encountered in the macro-literature such as the absence of clear microfoundations, the distance gap theoretical constructs and proxies, and the inability to

[1] A very partial listing would include recent works such as: Arjona and Kalyvas (2006); Barron *et al.* (2004); Blattman and Annan (2007); Deininger (2004); Deininger *et al.* (2004); Guichaoua (2007); Humphreys and Weinstein (2006); Weinstein (2007); Verwimp (2003; 2006); Kalyvas (2006); Kalyvas and Kocher (2007a; 2007b); Kocher (2004); Restrepo *et al.* (2004); Straus (2006); Trejo Osorio (2004), and Viterna (2006). Much more work is currently underway.

adjudicate between observationally equivalent causal mechanisms. The second flaw is the frequent omission of what turns out to be a key factor shaping the dynamics of civil war, namely territorial control. I show why territorial control matters and demonstrate how its omission biases the results of econometric analysis.

I proceed in two steps. First, I discuss three recent papers that in many ways exemplify the micro-level turn – at least in its more "economistic" manifestation. These papers use extensive subnational data to quantitatively analyze the dynamics of the civil war in Nepal. Also known as the Maoist insurgency, this conflict, presently in remission, lasted for about ten years (1996–2006) and cost over 13,000 lives. In spite of their important contributions, these papers suffer from the problems described above. Second, I turn to my own quantitative study of the dynamics of violence in the Greek Civil War and replicate it in order to show how insufficient disaggregation and the omission of territorial control from the analysis bias the results – and hence how these problems may be likewise biasing the results presented in the three papers on Nepal.

The Maoist insurgency in Nepal: a critical review

The first paper to focus on the Nepal insurgency using district-level data was published in 2005 by S. Mansoob Murshed and Scott Gates. Their dependent variable, *conflict intensity*, is proxied by fatality data. These data, collected by Shobha Gautam and published in 2001 by the Institute of Human Rights Communication Nepal (IHRCON), include the number of homicides in each of the seventy-five districts of Nepal from the start of the war, in 1996, up to 2001. From this dataset, Murshed and Gates generate a single fatality indicator per district for the entire period. They then find that the intensity of the conflict, as measured, is a function of two types of inequality: asset inequality (proxied by landlessness) and horizontal income inequality (proxied by a human development index for each district which includes life expectancy, education, and road density). Districts with high values of landlessness and low values of the HDI are found to have experienced higher fatality rates. An additional finding is that hilly terrain is associated with higher levels of violence. Finally, the main negative finding is that the presence of natural resources (which are not plentiful to begin with in Nepal) is associated with lower fatality rates. The authors conclude by suggesting that inequality causes civil war, a claim associated with the "grievance theory" of civil war onset, which has not fared well in the cross-sectional econometric literature.

The second paper, published in 2006 by Alok K. Bohara, Neil J. Mitchell, and Mani Nepal, also uses violence as a proxy for conflict intensity, but pays some attention to violence *per se*, and especially the relation between incumbent and insurgent violence. The paper focuses on three causal variables: poverty, political participation, and social capital. The data on violence come from a different source than those used in the previous paper: the Informal Sector Service Center (INSEC), a human rights NGO which published them in 2003. Violence is much more disaggregated: by actor (state and insurgents) and by time (the models are run on three time periods: 1997–2001, 1997–2002, and 1997–2003). The main findings are that geography (hilly terrain) and rurality (captured by population density) are powerful predictors of violence, whereas participation in elections and "social capital," as proxied by the number of civic organizations, reduces the levels of violence. Ethnicity and, contrary to the previous paper, poverty are found to have no effect on the levels of violence. Furthermore, insurgent and incumbent violence seem to interact with each other.

Finally, the third paper by World Bank economists Quy-Toan Do and Lakshmi Iyer, initially circulated in 2006 and revised in 2007, relies on the same INSEC dataset but takes better advantage of its rich data. The authors normalize the district-level fatality counts, disaggregate them into four yearly counts (1999, 2002, 2003, 2004), and analyze data on insurgent abductions. The most important predictors of violence in this paper are geographical factors, namely elevation and forested areas (which explain about 25 percent of the total variation), and poverty. Prewar poverty levels are also a significant predictor of violence, whereas sociopolitical variables such as the prevalence of advantaged castes, and caste or linguistic polarization, do not significantly increase the intensity of the conflict. Some variables, such as caste polarization, literacy rates, or infrastructure, matter in some specifications but not others. The authors conclude that development indicators are more robust predictors of conflict compared to political factors such as caste and ethnicity (described in the paper as "diversity factors"). Of particular importance is the fact that the disaggregation of the fatality data into yearly counts yields an important finding: there is considerable variation in the significance of the key causal factors across time. For example, whereas poverty and literacy are significant until 2003, they cease to be significant in 2004, a fact suggesting that the war is moving from more to less poor areas; in other words, war is a dynamic rather than a static process.

Clearly, all three papers go a long way toward improving our understanding of the dynamics of civil war. Nevertheless, there are problems. While all three papers converge in ascribing causal significance to geographical factors, especially hilly terrain, they diverge in their assignment of causal significance for almost every other variable, i.e. poverty, class, caste and ethnic polarization, participation, or social capital. Furthermore, the interpretation of their findings is loose. The reason, I contend, is related to the fact that while the empirical focus of these papers is unmistakably turned toward the micro level, their conceptual and theoretical focus is almost exclusively derived from the macro-level literature. These papers refer almost exclusively to the macro-literature on civil war onset, as if empirical findings on violence at the district level in Nepal could be extrapolated to civil war onset at the cross-national level and vice versa. In other words, there is a mismatch between their micro-level empirical focus and their macro-level conceptual and theoretical focus. This mismatch is reflected in five specific problems: (a) problematic proxies, (b) observational equivalence, (c) endogeneity, (d) overaggregation, and (e) omitted variable bias.

Problematic proxies

At its heart, the problem of the distance between concepts and empirical proxies is theoretical. A common problem in many studies on civil war, aptly illustrated by the three Nepal papers, is the conflation of two related, yet distinct, concepts: war/conflict on the one hand, and violence on the other.

Although war is defined as collective violence, the dynamics of violence and the dynamics of war are analytically distinct – and more so in civil wars compared to interstate ones. An obvious example of the perils of such a conflation is the use of fatality counts as a proxy for the intensity of a conflict. In a civil war, the absence of fatalities from a particular location may be indicative of two distinct (even opposite) states of the world: either a total state monopoly of violence (which is what most studies assume is the case) or a total absence of the state. This is the case for two reasons: primarily because the absence of the state in a civil war context does not necessarily signify the prevalence of anarchy understood as the absence of any order; and secondarily because, *contra* Hobbes, anarchy may not necessarily entail mass violence. A common state of affairs is instead the emergence, in a country undergoing a civil war, of an alternative state, one fully controlled and administered by rebels in so-called liberated zones – or "base areas" to use Maoist parlance. Such zones are often violence-free because violence there is off the equilibrium path (Kalyvas 2006).[2] The emergence of such zones is a testament of the state's inability to defeat the rebels and is compatible with a view of a civil war as being intense. In other words, the absence of violence from a specific area cannot be assumed to indicate the reduction of a conflict's intensity.

This point holds an additional implication. Insofar as the emergence of rebel-held zones is typically the crucial factor that fuels the expansion of the war in neighboring areas, the intensity of the conflict in these districts cannot be attributed exclusively to factors that are *only* present in that district. However, all these papers share the fundamental assumption that the conflict in a given district is *solely* related to factors that are district-specific.

Observational equivalence

A second problem is observational equivalence. Consider the finding that poverty is positively correlated with high fatalities, which points to several observationally equivalent mechanisms, each with different theoretical and policy implications. It may be, for instance, that insurgents recruit poor individuals who join to seek justice (the "grievance theory" of civil war); or that individuals in poor areas (which is not the same as saying poor individuals) see joining as a job in places where job opportunities are nonexistent (the "greed theory" of civil war); or that the government invests more resources to prevent the conflict in wealthier areas which is why the conflict is "fully expressed" in the rural countryside; or that the opportunity costs of violence are lower for the state in poor areas; or that people in poor areas may be less able to protect themselves from violence, including its "collateral damage" dimension. Of the three Nepal papers, Do and Iyer's stands alone in recognizing several possible observational pathways, though it acknowledges its inability to distinguish between them. The other two papers are much less forthcoming in recognizing this problem.

Endogeneity

Endogeneity is a significant problem in all three studies, most notably in Bohara *et al.*'s paper. For example, they use turnout in the May 1999

[2] I have also argued that high levels of contestation are associated with low levels of violence, which undermines proxying conflict by violence even more. See Kalyvas (2006).

elections to measure participation and find that it predicts lower rates of fatalities, which is to say that districts with low turnout in these elections have lower levels of violence. Obviously, with an ongoing war and with the Maoists openly denouncing the political process, the incidence of lower turnout in conflict-affected areas should not be surprising. However, there are exceedingly strong grounds to believe that the direction of causality flows from the conflict to turnout rather than the other way around. The same is likely the case for their indicator of social capital which is based on an index of social organizations compiled in the Nepal Living Standard Survey (NLSS) conducted in 1995–1996, again while the war was ongoing. It is not clear that organizations can operate freely in the midst of war, yet this would not mean that the presence of associations inhibits conflict. Note that this is also a measurement problem as the conflict must have affected the process of data collection; it is unlikely that the enumerators of the NLSS could have gotten truthful (or any) information from conflict areas.

Last, endogeneity is clearly a problem in the case of the negative correlation between violence and government economic development allocations (GRANT). Bohara *et al.* acknowledge so much when they quote evidence that the Maoists obstructed the implementation of hundreds of development projects. Do and Iyer are, once more, more forthcoming in this respect since they recognize that the effect of poverty may be endogenous to the conflict insofar as the state is likely to expend a greater effort to prevent rebel activity in richer areas or, alternatively, the opportunity costs of violent repression are lower in poorer areas. Likewise, they speculate that the effect of polarization they find may actually be due to the fact that the state is perhaps more repressive in areas where society is extremely polarized.

More generally, civil war is a deeply "endogenous" process (Kalyvas 2006), meaning that behavior, beliefs, preferences, and even identities can be altered as a result of the conflict and its violence. This is why the use of prewar lagged variables to address endogeneity concerns creates more problems than it solves. For example, Murshed and Gates rely on sociopolitical variables collected either in or before 1996, the year the war started. Their assumption is that year-to-year fluctuations in violence can be causally traced exclusively to prewar causes. However, this assumption flies in the face of evidence from many conflicts that (a) the war generates "contagion" effects independent of prewar factors, and (b) these variables (e.g. road density, human development index) change in the course of the conflict, partly as a result of the war.

Overaggregation

The turn to micro-analysis is testament to the need for disagreggation (Brubaker and Laitin 1998). It is, therefore, rather surprising to observe that micro-level studies may also suffer from overaggregation. In fact, this problem is made much more obvious precisely by the adoption of a micro-focus.

I identify two problems of overaggregation. The first one concerns the unit of analysis. All three papers adopt a district-level analysis. While Nepal's seventy-two districts provide a number that is high enough for statistical analysis, it is important to keep in mind that these are large areas averaging 1,948 square kilometers with a population of 309,000 (in 2001). Obviously such a size is likely to conceal significant internal heterogeneity. Furthermore, the decision to run the analysis exclusively at the district level is dictated less by the coding level of the independent variables (which are often collected at the community or even household levels and must be aggregated up at the district level) and more by the coding level of the dependent variable. In other words, this is an inefficient use of data dictated by the availability of an "off-the-shelf" dataset. The alternative would have been a deeper engagement with the case at hand which would have led to the collection of data at multiple levels of aggregation and, thus, allowed for a more sophisticated research design.

This brings up the second, and more serious, problem of overaggregation, that of violence. The analysis of the dynamics of violence requires at least three levels of disaggregation: (a) by actor (incumbent and insurgent violence), (b) by temporal period (e.g. monthly, yearly), and (c) by type of violence (homicides versus other types of violence; combatant versus noncombatant fatalities;[3] and selective versus indiscriminate violence).

Murshed and Gates do not disaggregate at all: they use one fatality data point per district for the entire February 1996–July 2001 period; Bohara *et al*. introduce a measure of disaggregation: they distinguish between insurgent and incumbent violence and test for three time periods. Finally, Do and Iyer go further by introducing yearly violence data for both sides and disaggregating the insurgent violence into killed and abducted.

The absence of disaggregation impacts on the analysis as can be seen by its effect when it is actually introduced: Bohara *et al*. find significant

[3] Bohara *et al*. (2006) point out that close to two-thirds of the total fatalities in Nepal are thought to be noncombatants.

differences in the violence produced by the two rival sides: ethnic heterogeneity reduces Maoist violence but not its state counterpart. Likewise, Do and Iyer find, among others, that poverty and literacy rates are not significantly associated with Maoist violence before 2002 but are significantly associated with state deaths in 1999; they also find that poverty explains killing but not insurgent abductions, but that literacy levels do (they speculate that insurgent violence may move from poor to wealthier areas over time). Not coincidentally, common to all three papers is the absence of any direct evidence about the belligerents: who they are, what are their goals and strategies, how they think, and so on. What allusions there are to these matters, and they are not numerous, tend to be inferred in a very indirect way from the data analysis.

All three papers fail to recognize the distinction between selective and indiscriminate violence, which is based on the level at which "guilt" (and hence, targeting) is determined. Violence is selective when targeting requires the determination of individual guilt and it is indiscriminate when targeting is based on guilt by association or collective guilt. States and rebels engage in a variable mix of selective and indiscriminate violence, primarily as a function of their degree of local knowledge (Kalyvas 2006). Note that, though indiscriminate violence is often associated with mass killing, this distinction is independent of the scale of targeting: selective violence can be massive while indiscriminate violence can be limited. The Phoenix Program in Vietnam was massive despite being selective in intent, whereas more recent violence in Kosovo was rather limited yet indiscriminate, as targeting was ascertained primarily by group membership.

The failure to disaggregate violence is deeply problematic from a theoretical perspective, as violence is an inherently *interactive* and *dynamic* process: the violence of one side impacts on the violence of the other side, while the war evolves and changes as the political actors strategize in response to evolving constraints and their rivals' strategy.

Omitted variable bias

A last problem flows from the failure to recognize the impact of territorial control on violence. I have formulated and tested a theory of selective violence (Kalyvas 2006) where the level of territorial control is the main explanatory variable, summarized as follows.

Most civil wars are fought as asymmetric or "irregular" wars – sometimes referred to as "wars without fronts." Irregular war is defined by the twin processes of segmentation and fragmentation of sovereignty: segmentation refers to the division of territory into zones that are monopolistically controlled by rival actors; while fragmentation refers to the division of territory into zones where the rivals' sovereignty overlaps. Put otherwise, civil wars are political contexts where violence is used both to challenge and to build order. The type of sovereignty or control that prevails in a given area affects the type of strategies followed by political actors. Political actors want to capture popular support ("collaboration") and deter noncollaboration ("defection"). The degree of control determines the extent of collaboration instead of the other way around, because political actors who enjoy substantial territorial control can protect civilians who live on that territory – both from their rivals and from themselves – giving civilians a strong incentive to collaborate with them, irrespective of their true or initial preferences. In this sense, collaboration is endogenous to control. In the long run, military resources, best proxied by geography, generally trump prewar political and social support in spawning control: incumbents tend to control cities, even when these cities happen to be the social, religious, or ethnic strongholds of their opponents, while the insurgents' strongholds tend to be in inaccessible rural areas, even when rural populations are opposed to them. However, the military resources that are necessary for the imposition of control are staggering and are usually lacking. The rival factions are, therefore, left with little choice but to use violence as a means to shape collaboration. The use of violence is bounded by the nature of sovereignty exercised by each political actor and, generally, must be selective rather than indiscriminate.

Political actors maximize territorial control subject to the (local) military balance of power. Territorial control in the context of irregular war requires the exclusive collaboration of individual civilians who, in turn, maximize various benefits subject to survival constraints. Irrespective of their preferences (and everything else being equal), most people prefer to collaborate with the political actor that best guarantees their survival. However, collaboration is much more uncertain in areas of fragmented sovereignty where control is incomplete. Because of its value for consolidating control, it is here that the premium on selective violence – that is, violence against defectors – is particularly steep. Selective violence, however, requires private information, which is asymmetrically distributed among political actors and civilians: only the latter may know who the *defectors* (i.e. those who collaborate with the actor's rival) are – and they have a choice: to *denounce* them or not. Put otherwise, selective violence is the result of transactions between political actors and individuals: it is *jointly* produced by them.

The likelihood of violence is a function of control. On the one hand, political actors do not want to use violence where they already enjoy high levels of control (because they do not need it) and where they have no control whatsoever (because it is counterproductive, since they are not likely to have access to the information necessary to make it selective). Instead, they want to use violence in intermediate areas, where they have incomplete control. On the other hand, individuals want to denounce only where it is safe for them to do so. This is the case in areas of full control (where political actors do not need their information) but not in areas of low control (where they are likely to face retaliation). This argument specifies the exact geographical space where violence is most likely to occur as a result of the intersection between the logics of the two sets of actors: it is where control is neither too skewed toward one side or the other, nor too balanced. It is, in other words, where the organizational demand for information meets the civilian supply of information via denunciation. Outside this space, violence is less likely: political actors may demand information but individuals will fail to supply it (or veto its application), and individuals may supply information but political actors will not need it. In short, the model predicts, rather ironically, that strategic political actors will not use violence where they need it most and, likewise, strategic individuals will fail to get rid of their enemies where they are most willing to denounce. The outcome, in other words, is suboptimal for both.

I operationalize control on a five-zone continuum, from zone 1 (total incumbent control) to zone 5 (total insurgent control). The main prediction is that the distribution of selective violence is likely to be bimodal, concentrating in zones 2 and 4. Incumbents will be most likely to resort to selective violence in areas where they exercise hegemonic, though not total control (which I call zone 2) and insurgents most likely to resort to the same type of violence in similar areas on their side (which I call zone 4). Areas of total control (zone 1 for incumbents and zone 5 for insurgents) will be largely free of violence (though not of repression). Areas of complete contestation and parity where both sides are simultaneously present in equal force (zone 3) will be free of violence. Figure 16.1 provides a graphic depiction of these predictions.

Figures 16.2 and 16.3 display the actual distribution of both selective and indiscriminate violence in the Argolid, a Greek region that experienced a civil war in 1943–1944 which I studied intensively in order to test these predictions. It is easy to note the multiple differences in the distribution of these two types of violence as well as the difficulty of distinguishing a clear pattern, at least for selective violence.

Figure 16.4 shows the observed distribution of selective violence as a function of control. Note the similarity in the shape of the distribution compared to Figure 16.1.

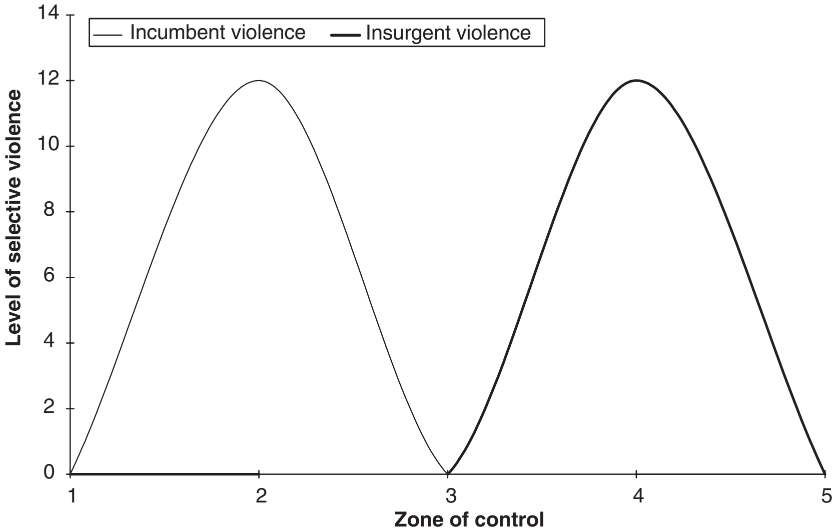

Figure 16.1 Predicted levels of selective violence as a function of territorial control

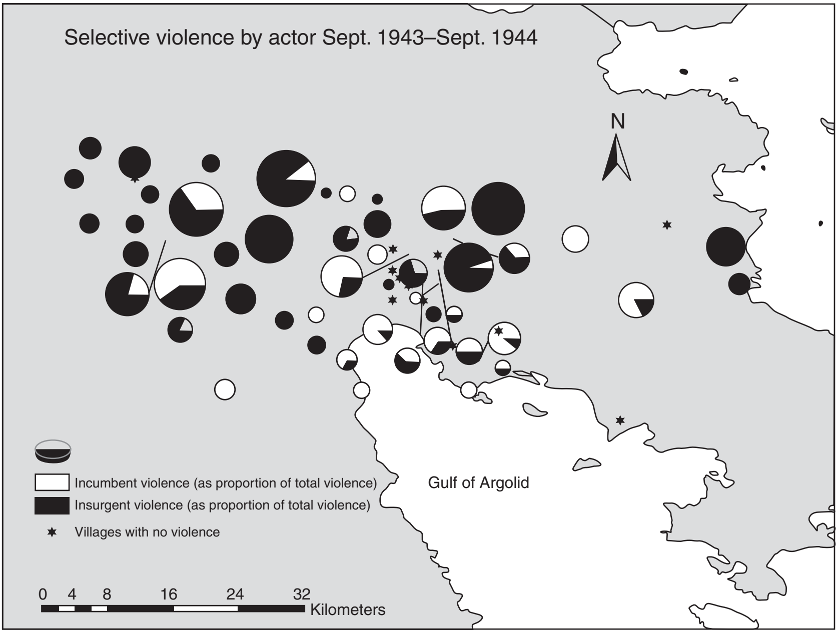

Figure 16.2 The spatial distribution of selective violence: Argolid, Greece, 1943–1944

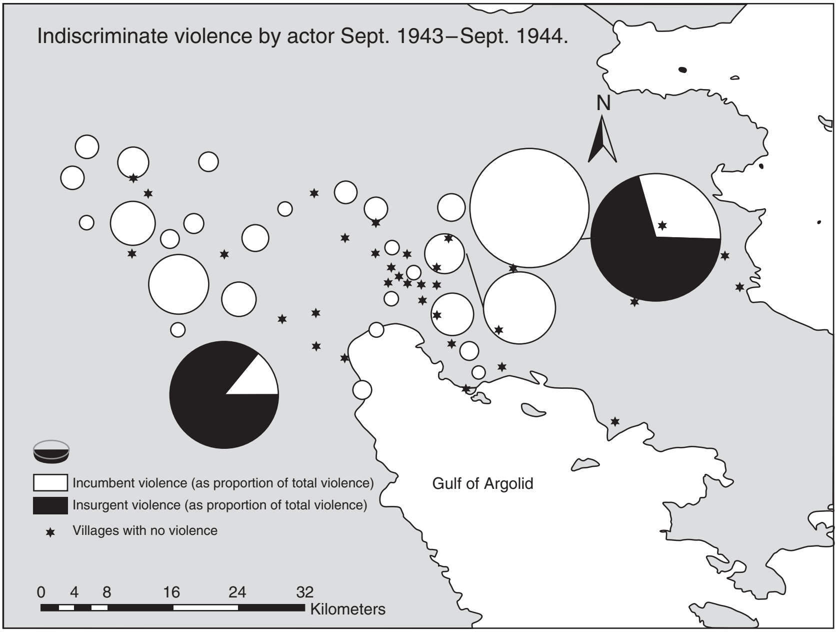

Figure 16.3 The spatial distribution of indiscriminate violence: Argolid, Greece, 1943–1944

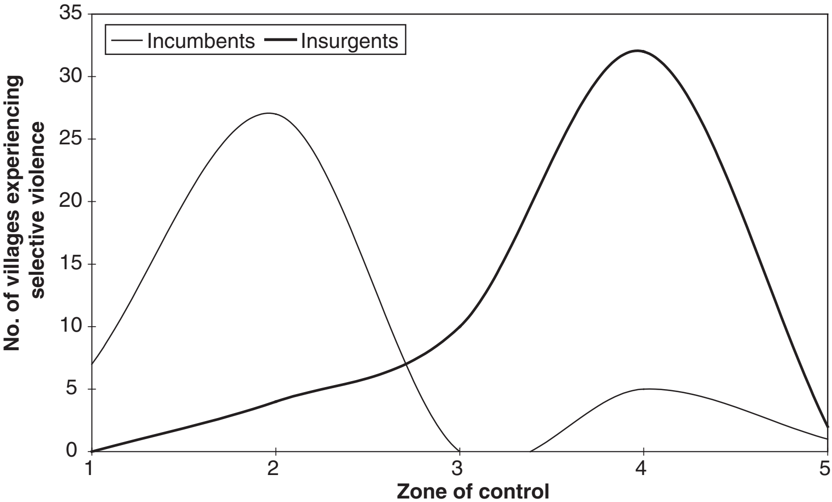

Figure 16.4 Observed levels of selective violence across control zones: Argolid, Greece, 1943–1944

Table 16.1. *Basic descriptive statistics, Argolid 1943–1944*

		No. of homicides	% of total violence	% of population
	Total violence	725		1.61
Type of violence	Selective	366	50.48	0.96
	Indiscriminate	359	49.52	0.94
Actor	Incumbents	353	48.69	0.92
	Insurgents	372	51.31	0.97
Time period	t_1	66	9.10	0.16
	t_2	295	40.69	0.77
	t_3	168	23.17	0.44
	t_4	196	27.03	0.50
Location	Plains	152	20.97	0.40
	Hills	450	62.07	1.18
	Mountains	123	16.97	0.32

Table 16.1 displays the basic descriptive statistics. Note the geographical concentration of violence: 62.07% of all homicides are located in hilly terrain, compared to 20.97% in the plains and 16.97% in the mountains. The parallels with the most robust finding of the three Nepal studies are striking.

Last, Table 16.2 includes the results of a multivariate test using these data, with selective violence as the dependent variable and a set of independent variables that includes territorial control.[4] Note that what

[4] These are OLS regressions estimating the *intensity* of selective violence, coded as the number of deaths per village. The model was estimated for the four time periods based on major shifts in control. The main explanatory variable is a dummy variable whose value is 1 if a village is located in zone 2 or 4. Control variables include the following: village population (as recorded in the 1940 census – logged); educational level (measured as the per-capita number of village children attending secondary school in 1937–1939), intended to capture a variety of hypothesized effects in both directions, including civilizing effects, political moderation which may reduce violence and rising expectations, or political extremism which may increase it; altitude (logged meters), intended to capture rough terrain which may have a positive effect on violence given that insurgencies are more likely to take place in such areas; distance from the closest town (logged travel minutes from the closest town in 1940), intended to capture the geographic ability of the two sides to access a particular locale and provide credible opportunities and sanctions; litigiousness (the total number of trials in the civil and penal courts of the region during the period 1935–1939, logged), intended to capture social polarization as well as the absence of social capital which should reduce violence; and a three-scale ordinal GDP proxy, intended to capture wealth and opportunity costs. See Kalyvas (2006) for more details on coding and specification.

Table 16.2. *Determinants of the intensity of violence (no. of homicides per time period), Argolid 1943–1944, OLS regressions (robust standard errors and p values in parentheses)*

Dependent variable: selective violence (number of homicides)	t1	t2	t3	t4
Control zone 2 and 4 (dummy: 1 when control zone is 2 or 4)	1.29* (0.66) (0.056)	3.62*** (1.06) (0.001)	3.28** (1.39) (0.022)	10.06*** (3.54) (0.006)
Population (1940) (log)	0.24 (0.22) (0.27)	1.28** (0.59) (0.034)	1.42* (0.73) (0.056)	1.05 (0.81) (0.198)
Education level (high-school students per capita)	−0.11 (0.21) (0.600)	0.37 (0.52) (0.477)	0.38 (0.42) (0.365)	0.52 (0.46) (0.257)
Altitude (meters) (log)	−0.22 (0.22) (0.335)	0.96*** (0.28) (0.001)	0.63** (0.33) (0.044)	0.51 (0.49) (0.298)
Distance from closest town (in minutes) (log)	0.36 (0.62) (0.561)	0.36 (0.68) (0.597)	−1.47* (0.76) (0.066)	0.14 (0.53) (0.789)
Prewar conflict (court suits per capita 1935–1939) (log)	0.11 (0.17) (0.513)	−0.67** (0.33) (0.046)	−0.47 (0.48) (0.334)	0.69 (0.43) (0.110)
GDP proxy (interval variable; wealthiest village = 3)	0.32 (0.36) (0.379)	−0.09 (0.42) (0.815)	−1.13** (0.50) (0.028)	0.61 (0.46) (0.192)
Constant	−2.09 (2.96) (0.485)	−17.65 (6.7) (0.011)	−3.46 (5.58) (0.538)	−9.11 (7.17) (0.209)
Observations	61	61	61	61
R-Squared	0.265	0.372	0.328	0.543
Prob > F	0.0258	0.0172	0.0062	0.1357

*p<0.10; **p<0.0.05; ***p<0.01 (two-tailed test) (p-values in parentheses)

looked like a powerful effect of geography disappears in a multivariate setting that includes a measure of territorial control.

How robust is this finding? I provide two additional pieces of evidence from two very different conflicts: Colombia and Vietnam. The impact of territorial control on the patterns of recruitment can be seen in Table 16.3 and Figure 16.5 which use data from a survey of demobilized combatants in Colombia (Arjona and Kalyvas 2006).

Table 16.3. *Recruitment as a function of control, Colombia*

		Joined the guerrillas	Joined the paramilitaries
Who ruled one year prior of joining?	Guerrillas	75.36	24.63
	Paramilitaries	2.98	97.02
	Neither	30.67	69.29
	Both	50	50

Source: Arjona and Kalyvas (2006)

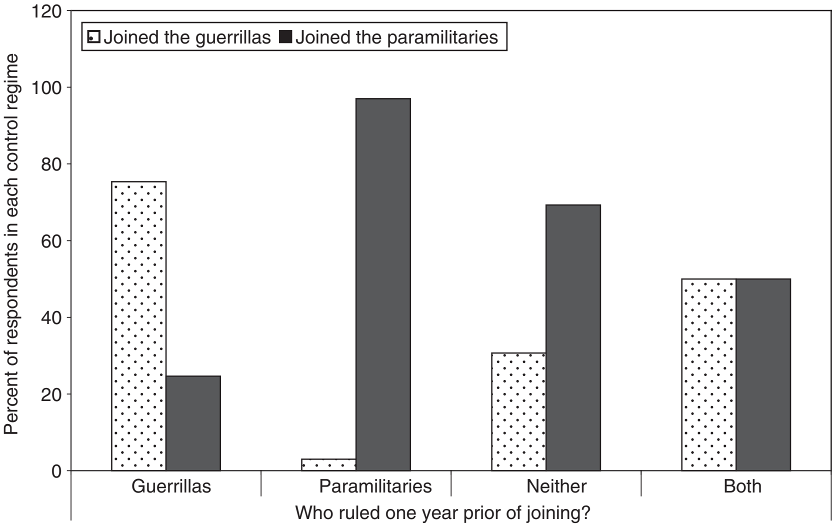

Figure 16.5 Recruitment as a function of control, Colombia

These data are based on a survey question that asks ex-combatants who had joined either the guerrillas or the paramilitaries, where they used to live one year prior to joining. Ex-combatants who lived in an area ruled by the guerrillas a year prior to joining were 50% more likely to join the guerrillas than the paramilitaries; those who lived in areas run by the paramilitaries were 95% more likely to join them as opposed to the guerrillas; those who lived in areas ruled by neither actors (i.e. primarily ruled by the state) were about 40% more likely to join the paramilitaries; and last, when control was divided between these two actors, joiners split in half. These data strongly suggest, in other words, that territorial control

Figure 16.6 Vietcong selective violence as a function of territorial control

is clearly associated with recruitment. A multivariate test assigns causal significance to this finding (Arjona and Kalyvas 2007).

Last, Figure 16.6 is derived from an analysis of the Hamlet Evaluation System, a data-collection system initiated by the US during the Vietnam war which includes information on territorial control at the hamlet level, as well as data on Vietcong selective violence (though not state selective violence) (Kalyvas and Kocher 2006). Data from a random monthly cross-section (July 1969) covering 10,479 hamlets and measuring Vietcong selective violence across five zones of controls show a distribution very similar to what I found in the Argolid: insurgent violence is much more likely in zone 4 compared to everywhere else.

A replication

What this evidence suggests is that omitting territorial control from an analysis of violence is likely to bias the results. Rather than just assert this point, I replicate the Greek test using an overaggregated version of the variable for violence and omitting the variable for territorial control. I then compare the results to the "correct" model which includes the properly disaggregated variable for violence and the variable for territorial control.

How were these data collected in the first place? The choice of Greece was motivated by practical concerns – the ability to conduct wide-ranging research in a rural context combining archival and field components. Within Greece, I selected the prefecture (*nomós*) of Argolid, located in the northeastern part of the Peloponnese peninsula in southern Greece. This choice was dictated by the discovery of an important judicial archive that included information about most of the 725 homicides that occurred during the civil war (1943–1944) in every village of the two major counties of the Argolid – an area including sixty-one villages with a total population of 45,086 inhabitants and two towns with a population of 20,050, in 1940. In most cases I was able to reconstruct the identity of the perpetrator and the victim, the links between perpetrator and victim, the time and location of the homicide, the way it was carried out, the links between this homicide with anterior and subsequent instances of violence, and the justifications (if any) that were given or that can be inferred about it. In addition to the Argolid judicial archive, I consulted the archives of the Greek Communist Party, the Greek army, the British Foreign Office, and the (British) Special Operations Executive (SOE).[5] Beyond archival sources, I relied on tens of published and unpublished memoirs, autobiographies, and local histories as well as 116 taped interviews covering all the villages via a "snowball sampling" technique. These interviews were not restricted to "victims." I interviewed both women and men, active participants (in various capabilities: low-level and middle-level political and military cadres, soldiers, collaborators of various shade) and nonactive participants, mostly peasants. I also interviewed perpetrators, victims, their relatives, and bystanders. Finally, I talked to leftists, rightists, politically uncommitted people, as well as people who shifted political identities over time. By conducting archival and field research simultaneously, I was able to cross-check facts between different types of written sources, between individuals of different political orientation, between individuals with different war and postwar experiences, and between individuals from neighboring communities.

In short, reconstructing the process of violence in a war that took place in the 1940s required an extremely labor-intensive assemblage of multiple sources. Because of the fragmentary character of the sources, I had to proceed like an archaeologist and "gather discrete and disparate traces of the past and assemble them in order to shed light on the circumstances and background of what we otherwise can only know from a haunted

[5] In fact, there were SOE operatives in the Argolid during 1943–1944 whose reports complement other local sources. Last, I made limited use of German military and military justice archival material that focuses on the military situation of the Argolid.

Table 16.4. *Replications*

Model:	1	2	3	4	5	6	7	8	9	10	11	12
Dependent variable	Totviol	Totviol	Indviol	Selviol	Incviol	Insviol	Incviol	Insviol	Indviol (t4)	Selviol (t4)	Indviol (t4)	Selviol (t4)
Population (log)	14.036***	12.799***	12.434**	4.151***	8.026***	8.559**	7.833**	8.191**	2.99	1.071	2.993	1.051
	[3.70]	[3.48]	[2.6]	[3.05]	[2.82]	[2.46]	[2.59]	[2.28]	[1.06]	[0.94]	[1.05]	[1.30]
Education	2.699	2.167	0.723	1.163	2.048**	−0.161	1.965**	−0.319	0.571	0.237	0.535	0.522
	[1.45]	[1.20]	[0.39]	[1.02]	[2.06]	[−0.11]	[1.89]	[−.23]	[0.78]	[0.53]	[.74]	[1.15]
Altitude (log)	5.542***	6.513***	4.049*	2.128***	3.149**	3.028**	3.301**	3.316**	0.335	0.342	0.313	0.517
	[2.76]	[3.09]	[1.9]	[3.35]	[2.03]	[2.33]	[2.22]	[2.31]	[0.73]	[0.58]	[.69]	[1.05]
Dist. from town (log)	4.792	5.399	7.585	0.161	1.35	6.396*	1.445	6.577**	2.726	0.420	2.761	0.141
	[1.51]	[1.64]	[1.53]	[.12]	[0.74]	[1.74]	[.76]	[1.80]	[1.07]	[0.82]	[1.06]	[.27]
Court litigation (log)	−1.639	−2.464	1.882	−1.124	0.609	0.148	0.479	−0.096	−0.779	0.376	−0.820	0.694
	[−0.84]	[−1.08]	[0.82]	[−1.34]	[0.51]	[0.09]	[.32]	[−.05]	[−.93]	[0.58]	[−.93]	[1.63]
GDP	2.558	1.652	5.749	0.187	1.676	4.26	1.534	3.991	1.538	0.111	1.474	0.613
	[1.07]	[0.68]	[1.6]	[.20]	[1.13]	[1.51]	[.92]	[1.31]	[1.03]	[0.22]	[1.01]	[1.32]
Mean control		−3.827					−0.598	−1.136				
		[−1.30]					[−.33]	[−.46]				
Disaggre-gated control											−1.271	10.068***
											[−.79]	[2.85]
Constant	−139.038***	−127.872	−135.529	−35.998	−69.614	−101.913	−67.867	−98.596	−39.266	−8.391	−39.175	−9.114
	[−3.84]	[−3.68]	[−2.47]	[−2.53]	[−2.82]	[−2.45]	[−2.58]	[−2.32]	[−1.07]	[−0.86]	[−1.07]	[−1.27]
Observations	61	61	61	61	61	61	61	61	61	61	61	61
R-Squared	0.44	0.45	0.33	0.29	0.30	0.34	0.30	0.34	0.12	0.05	0.12	0.54

Note: Robust t statistics are in brackets.
*Significant at 10% **Significant at 5% ***Significant at 1%

memory" (Geyer 2000, 178). At the same time, however, this type of engagement with data collection allowed me to properly disaggregate the data and code the key variable of territorial control. Though restricted to a single region, this dataset is, therefore, ideal for the purposes of comparing the results of a "correct" test to deficiently specified alternatives.

A different way of describing this exercise is as follows: suppose that I did not conduct this extensive data-collection effort, but instead stumbled upon a count of fatalities compiled by historians or a local NGO – like the authors of the three Nepal papers. What would an analysis based on such data look like?

To begin with, had I relied on historical data, it is highly likely that I would have undercounted two key variables: the level of selective violence (which turns out to reach 50.48% of all homicides) and the level of insurgent violence (which accounts for 51.31% of the fatalities). The reason is that the historical record has preserved the more visible large indiscriminate massacres rather than the individualized targeted killings. Furthermore, it has also privileged the more visible (and politically more blameworthy) violence of the incumbents rather than the violence of the insurgents. Last, I would have been completely unable to distinguish between selective and indiscriminate violence or to disaggregate violence by time period.

I estimated eleven models to fit these different scenarios which I compare to the correct model (model 12; Table 16.4). The dependent variable in model 1 (total violence or **Totviol**) is the aggregate total count of fatalities. Like the paper by Murshed and Gates (2005), it is a single indicator (for each village). This model does not include the independent variable for territorial control. Two variables are significant: altitude (like in all the Nepal models) and population. Model 2 is similar, save for the introduction of an aggregate measure of control (i.e. one that averages control over the entire 1943–1944 year). The same variables (altitude and population) remain significant and aggregate control fails to attain significance (in fact, its sign is opposite from predicted). The dependent variable in models 3 and 4 are the counts of indiscriminate and selective fatalities respectively (**Indviol** and **Selviol**) aggregated, again, by actor and time. Population and altitude remain significant in both models, but their effect goes down in model 4. Models 5 and 6 disaggregate violence by actor and focus respectively on insurgent and incumbent violence (**Insviol** and **Incviol**). Population, education, and altitude are all positively correlated with incumbent violence whereas population, altitude, and distance from the closest town are positively correlated with insurgent violence – though the statistical significance in all specifications is lower compared to the previous models.

Models 7 and 8 have the same dependent variable but introduce the aggregate measure of control which is not significant. The same variables stay significant. Last, models 9–11 are the most "disaggregated": they disaggregate violence by actor and into selective and discriminate, and also introduce temporal aggregation.[6] I omit the measure for territorial control from models 9 and 10. No variable is significant and the R-Squared collapses. A disaggregated measure for control is introduced in model 11 where the dependent variable is indiscriminate violence. No variable is significant in that model either which also has a very low R-Squared. Last, model 12 is the "correct" one. The dependent variable is selective violence disaggregated by time period but not by actor (as the theoretical prediction applies to all actors). In this model, the control variable is large and statistically significant, and the R-Squared is the highest.

Conclusion

The conclusion is straightforward. Had I relied on insufficiently disaggregated or "mis-aggregated" data and omitted a measure of territorial control, the analysis would have produced biased results. Clearly, disaggregation is essential, as is the inclusion of a measure for territorial control.

There is no doubt that collecting data at that level of disaggregation is not easy. However, the alternative is to use highly aggregated data and omit the crucial control variable with the results I have suggested. Alternatively, one could compensate for the lack of these variables in creative ways. Such an example is contained in a recent paper by Humphreys and Weinstein (2006) that uses survey data from Sierra Leone to estimate a model of civilian abuse by armed groups. Having coded no variable for control, they rely on a substitute called "dominance," which records the estimated size of a unit relative to the estimated total number of troops in the zone. This measure, however, is highly problematic as any student of insurgency and counterinsurgency would easily surmise: the ability of an armed group to control a particular locality is only partly a function of the raw numbers of combatants. Control is a function of the distribution of these troops across an area with specific geographical features, combined with the number, commitment, and distribution of civilian supporters across the same area.[7] In short, when it comes to coding territorial control there is no

[6] For simplicity, I use here one cross-section, the fourth time period (t4) which covers the period of August–September 1944.

[7] This paper also fails to distinguish between selective and indiscriminate violence. Again, lack of appropriate coding is justified by a dubious argument whereby this distinction is "blurred" (Humphreys and Weinstein 2006, 444). The entire exercise is quite

easy alternative to either direct and careful data collection using all available sources, or prior coding by the insurgents or counterinsurgents themselves, when they do leave extensive archival material behind.

To sum up, although this essay fully endorses the current "microdynamics of civil war" research program, it calls for a deeper engagement with cases, careful and detailed collection of fine-grained data, and thorough theorization. Unless these conditions are fulfilled, the current turn to the micro level will miss the opportunity to live up to its promise.

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