Using Case Studies to Expand Economic Models of Civil War

By Nicholas Sambanis

This article draws on a comparative case study design to refine formal-quantitative models of civil war, expanding them to highlight political processes that lead to civil war. It uses 21 case studies of civil war onset and avoidance to show the shortcomings in prominent rationalist models of civil war that rely heavily on economic variables. These shortcomings include measurement error, unit heterogeneity, model misspecification, and lack of clarity about causal mechanisms. Additionally, the greed/grievance distinction that underlies the economic models is misguided. This article analyzes civil war not as a discrete phenomenon, but rather as one phase in a cycle of violence. Economic models of civil war, however, rely on theories that cannot distinguish effectively between civil war and other forms of political violence. To explain civil war, we must explain why various and often conflicting micro-level motives combine to produce political violence with the characteristics that we attribute to civil war. If we cannot understand why we get civil war instead of other forms of organized political violence, then we do not understand civil war.

About 140 civil wars around the world since 1945 have killed approximately 20 million people and displaced 67 million.[1] Despite this massive scale of human misery, the academic community did not pay much attention to the problem of civil war until very recently. Two important papers—one by Paul Collier and Anke Hoeffler (henceforth CH), and the other by James Fearon and David Laitin (henceforth FL)—have generated much interest in the question of why civil wars occur.[2] These papers present the counterintuitive finding that civil wars are not caused by ethnic division or political grievances, but by the opportunity structure for the organization of rebellion or insurgency.[3] Both papers use macro-level data to test hypotheses about civil war that are based on ideas about micro-level behavior (e.g., why individuals would join an insurgency).[4] They identify a set of statistically significant correlations between civil war onset and a number of explanatory variables.

The already significant gap between the micro-level theories and their macro-level implications is magnified when the micro-macro relationships are studied solely through cross-national statistical analyses. Such studies often overlook information about the causal pathways that link individual or group behavior with the outbreak of civil war. In this article, I argue that by combining statistical and case study work we can better understand the political processes that lead societies to civil war. I also emphasize the importance of focusing on the macro level in order to explain how individual episodes of violence are organized in the form of a civil war.

Given the significance of the CH and FL models, I begin with them to demonstrate how a comparative case study design can be combined with a formal-quantitative approach to build a better-specified theory of civil war.[5] I use cases to develop—not test—theory and to qualify the causal inferences that we can draw from a quantitative model of civil war. I draw on the Case Study Project on Civil Wars, a set of 21 case studies on civil war onset and war avoidance[6] that systematically apply the CH model.[7] (I was the primary investigator from the summer of 2001 until the project's completion in the spring of 2004.) Two online supplements to this article include details about the project's research design and summaries of the cases.[8]

The case studies make several contributions to our understanding of civil war:

- They help us identify a number of causal mechanisms through which independent variables in the CH and FL models influence the dependent variable—i.e., the risk of civil war onset. It quickly becomes clear that the CH model's distinction between "greed" and "grievance"

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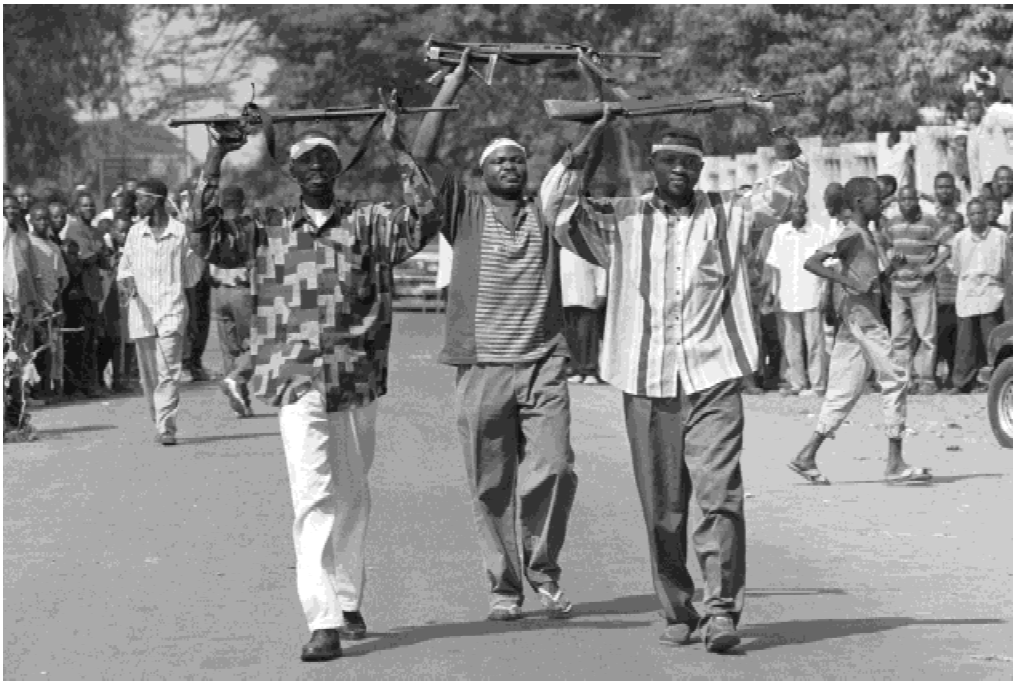

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as competing motives for civil war is illusory, because greed and grievance are usually shades of the same problem.[9]

- They question assumptions and premises of the quantitative studies and make clear that CH and FL are often right for the wrong reasons yet also wrong for the wrong reasons. (In other words, the cases identify mechanisms that are different from those underlying their theories, both where the statistical models make good predictions and where they make bad predictions).
- They sometimes point to a poor fit between the empirical proxies and the theoretically significant variables—i.e., they identify measurement problems in the statistical studies. Later in this article, I offer some examples and suggest ways to improve the connection between theory and data.
- They help us identify new variables that might explain civil war but are omitted from CH and FL (e.g., external intervention,[10] or diffusion and contagion from violence in the "neighborhood").[11] Adding these variables to quantitative models might reduce the risk of omitted-variable bias and facilitate inductive theory building.[12]
- They highlight interactive effects between variables in the statistical models and help us identify exogenous and endogenous variables by presenting narratives of the series of events and the processes that led to civil war.
- They suggest substantial unit heterogeneity in the data, as the mechanisms that lead to civil war seem to differ substantially across different sets of countries and types of civil war.

This last observation informs a new theoretical contribution made in this article. I argue that it is not as useful to view civil war outcomes as the result of deep-seated and hardly changing structural conditions as it is to observe the links among different forms of political violence and to analyze the dynamics of conflict escalation and the transition from one form of violence to another.

It is accepted practice in the literature to pool events of civil war without exploring whether, in fact, they all result from the same causal process.[13] Although civil wars are thought to be different from other forms of political violence, current models of civil war do not allow us to make that determination, and it is important to establish whether there are such differences. If civil wars have different causes than do other forms of violence, then the policy interventions designed to prevent them should differ from those meant to ward off terrorism or other violence. For example, perhaps we will eventually find that riots and minor armed conflict occur more frequently than civil wars when there are sharp drops in a country's economic growth or when intense price shocks reduce real incomes in primary commodity–dependent countries.[14] Perhaps these conditions also lead to military uprisings, or coups, if soldiers are not well paid and if military elites are not controlled by political elites. Prevention or intervention policies should then be designed to stabilize prices, ensure that the troops are paid, and gradually replace the leadership of the army to establish better military-civilian relations. By contrast, these factors may not be as important in preventing separatist civil war, which may be a greater concern for some countries (e.g., countries with large and territorially concentrated ethnic groups).[15] Separatist violence may be more strongly associated with regional inequalities in the distribution of income and may be more likely to occur in peripheral regions bordering countries that are governed by ethnic kin of the majority population in the region.[16] Thus, policy interventions should involve an increase in the state's police presence in those regions to prevent the organization of insurgency,[17] or they may involve redistribution to reduce regional disparities. However, if a country faces a higher risk of a popular revolution because of widespread class inequities, then redistribution should take a different form (it should not focus on regions, but rather on reducing interpersonal inequality). If our research shows that genocides are likely to take place in the context of civil wars in countries that have autocratic systems and ethnic elites with exclusionary ideology,[18] then the international community could develop a strategy for early intervention in those civil wars to prevent genocide.[19]

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If civil wars are not unique, however, then by analyzing them in isolation we may be getting inefficient (or even biased) statistical estimates, because we are arbitrarily restricting our analysis to a subsample of the data. Either way, to explain civil war, we must construct a theory that tells us why conflict escalation leads to civil war as opposed to other kinds of violence.

It is not my intention here to pit CH against FL and pronounce one the winner. Both models are useful starting points as I try to outline a new way of analyzing the causes of civil war. By focusing on the distinction between process and outcome, I offer a way to reconcile quantitative and qualitative research designs, which political scientists often (mistakenly) consider as mutually exclusive rather than complementary approaches.

A Primer on the State of Civil War Theory

Civil war in both CH and FL is defined as armed conflict between the government of a sovereign state and one or more organized groups that are able to mount effective resistance against the state. More details go into these definitions (e.g., a threshold of the number of deaths) and the definitions used in the two studies differ slightly, but these differences do not influence their key empirical results much.[20]

The CH model of war onset is based on the idea of an individual's tradeoff between production and appropriation.[21] According to CH, people decide to become rebels after weighing the economic opportunity cost of violence against its expected utility. War is an inefficient way to settle disputes because it is costly and reduces the net value of rents available to the state. Yet war occurs, because of "three interacting determinants: *preferences*, *opportunities*, and *perceptions,*" and because it is hard to negotiate credible peaceful settlements without the use of force.[22] Preferences for private gain ("greed," in the CH model) lead to political violence if there are opportunities to rebel. Defined in this way, rebels are indistinguishable from bandits or greedy war entrepreneurs.[23] Grievance, in CH, is simply rhetoric used to legitimize a person's decision to engage in appropriation rather than production. The instrumental cause of civil war is the availability of loot, combined with the opportunity to organize an insurgency.

According to the CH model, rebellion is sustained through looting of natural resources, extortion of local populations, and support from ethnic diasporas. Insurgency is less likely when the state is strong or when economic opportunity costs of rebellion are high. State strength, not well theorized in CH, is approximated by the country's economic strength (gross domestic product per capita). The expectation is that richer states will be bureaucratically more efficient and will have the resources needed to defend themselves against a rebellion. Additionally, strong states can reduce the available rebel labor supply by decreasing the net expected gains of rebellion. Rebel labor supply should also decline as the economic opportunity costs of rebellion increase (time devoted to production rather than appropriation pays more in rich states than in poor ones). In sum, a country's economic opportunity structure—its income level and growth, as well as its economic system—determines the "supply" of insurgency for a given level of insurgency "demand."

The FL model shares much of the same logic. FL rejects primordialism, nationalism, modernization, and other grievance-based explanations in favor of an opportunity-structure argument that explains civil war as a function of state strength (also measured by GDP per capita) and the technology of insurgency (rough terrain in which to hide and navigate, diaspora funding, et cetera). FL dismisses CH's resource-predation argument—that is, FL finds no support for linking primary commodity exports to war onset. Although FL finds that oil-dependent states are more prone than others to civil war, the study interprets this as a weak-state effect, since oil dependence frequently causes weak state structures. The FL model is state-centric; it includes a host of variables that measure state capacity, such as political instability, new state formation, and regime type. However, many of the differences between CH and FL are mainly interpretive, since the models often use the same (or closely related) proxy variables to test different hypotheses about civil war onset.

Regarding opportunity structure for rebellion, CH and FL resemble earlier important works.[24] But what is new is the quantitative empirical testing of their models, which suggests several important and robust results. Both CH and FL find that GDP lowers the risk of civil war: CH takes this to mean that poverty exacerbates risk, while FL interprets it as support for the state-weakness theory. While FL sees no significant association between high economic growth and civil war risk, for CH, high economic growth and high levels of secondary-school enrollment (particularly among males) lower risk by increasing opportunity cost. Both CH and FL find that countries with mountainous terrain and large amounts of external financial support have higher risk of civil war; but they disagree on other ways of financing rebellion.[25] According to both models, civil war is more likely in populous countries. However, CH claims this is because large populations are more likely to include aggrieved groups, whereas FL attributes it to states' having a harder time controlling large populations.[26] Finally, both CH and FL find that democracy does not significantly reduce the risk of civil war and that ethnic fractionalization does not increase it—although, according to CH, ethnic *dominance* increases risk. CH and FL seem to agree that countries in the middle of the democracy-autocracy spectrum and those with political instability are more prone to civil war.[27]

CH and FL get their results from pooled logit analysis of time-series cross-sectional data covering roughly the same set of countries and much of the same time period.[28] Both models analyze war onset, but CH drops observations of ongoing war, while FL codes such periods as *0*'s so as not to leave out war onsets that occur in countries with an ongoing civil war.[29]

With this telegraphic review in mind, let us now turn to the case studies.

Case Study Methodology

Most researchers who work with quantitative methods are averse to doing case studies. They presume that case studies cannot test or inspire theory because they suffer from selection bias, omitted variable bias, endogeneity, and measurement error.

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However, these problems are also frequently present in quantitative studies. In what follows, I address these concerns by describing the methodology of the Case Study Project on Civil Wars, arguing that well-designed case studies can actually help reduce the presence of such problems in quantitative analyses of civil war.

The project's main purpose was to improve causal inferences drawn on the basis of the CH model. CH was tested statistically, so we did not need to retest it using case studies. In fact, such retesting was not even desirable, because statistical identification associated with case study designs is widely known to be problematic.[30] Thus, each case study tried to complement our theory, refine our empirical measures, explore interactions among explanatory variables, identify endogenous and exogenous variables in the model, and identify causal mechanisms underlying the theory.

To achieve these goals, we used a structured-focused comparison design, exploring the fit of the CH theory to each case. Country experts wrote narratives that addressed a common set of questions about the onset and organization of rebellion. Since all narratives responded to the same questions, our research design allowed us to draw generalizations.

Unit of observation

As in the CH and FL statistical tests, the unit of observation for the case studies is the country—or, more accurately, several periods of peace and war within each country. Some countries have had recurrent wars. The authors discuss all periods of war, exploring the connections among these events. In effect, each case study provides us with several observations. For example, the Indonesia study focuses on patterns of war and peace in Aceh over eight five-year periods; it therefore yields eight observations (two of war and six of no war). The Nigeria study analyzes several periods that included two wars, while the Ivory Coast study examines three periods, each with no war but with a different structure of war risk.[31]

Case selection

Given that we knew the values of the dependent variable for each case and we had a sense of which variables were on average significant predictors of outcomes, we did not pick cases with a view to predicting outcomes. Rather, we selected them with two goals in mind: first, to maintain sufficient variation among the independent variables,[32] and second, to include cases that the model predicted well *and* ones that it predicted poorly.[33] Among cases predicted poorly by the model, we considered both periods of war onset not predicted by CH and periods of peace in countries for which CH predicted high risk of civil war.[34] The case studies helped us sort out which of these poor predictions failed for idiosyncratic reasons and which ones resulted from possible flaws in the theoretical model, the operational measures used in quantitative analysis, or the estimation method.[35] Where CH generated accurate predictions, the case studies primarily aimed to trace the mechanisms linking the independent variables to the dependent variable, so we could assess whether the theory underlying the CH (and related FL) tests of hypotheses corresponded to the empirical measures used. Our selection criteria and the large number of cases in our project allowed us to build theory inductively; the new theory could later be taken back to the data for further hypothesis testing.

We sought to ensure some variation in the independent variables, though our case-selection mechanism was not sophisticated. We picked democracies and autocracies, countries with long and short histories of violence, high and low levels of ethnic fragmentation, and high and low natural-resource dependence.[36] Choosing a random sample of cases was not necessary, since we did not intend to use them to test the theory.[37] Random selection from the CH or FL data sets might have resulted in a sample that predominantly included cases of no war, given that civil war is relatively rare. It could also have resulted in a sample with no significant variation in the independent variables. We could have avoided the first (but not the second) problem by sampling more heavily on cases of war, but nonlinearities that may have been present in the data could have complicated the sampling process.[38] For example, civil wars occur predominantly in poorer countries, so a case-control design that selected three no-war cases for every case of war[39] would have included far too many middle-to-high-income countries to allow us to disentangle the relationship between the dependent variable and other independent variables at different levels of income. Properly controlling for this problem would have entailed a highly stratified selection rule and a sample size even larger than the one we had in our project. So we opted instead for a design that would offer theoretical insights into the complex pathways and sequence of events that lead to civil war. These insights, at a later stage, could help us retest the theory with quantitative methods.

A comparison of Chechnya and Dagestan (both parts of Russia) in our case studies offers an example of the complicated causal path linking political instability to civil war. Both regions were harmed by the political instability that followed the collapse of the Soviet Union. In Chechnya, political institutions fell at the hands of Chechen nationalists, led by General Dzhokhar Dudaev, who pursued secession on behalf of the Chechens. In Dagestan no titular majority group existed, and the national independence movement acted through preexisting political institutions that represented 54 regional soviets, or parliaments. Only 39 soviets favored secession, and political elites decided that secession was too costly.[40] Thus, the effects of political instability on civil war risk in Chechnya and Dagestan were filtered through the type of regional political institutions available in each case and were conditioned by the regions' ethnic composition as well as by nationalist elites' actions. The helpful perspective on sequence offered by these cases can lead us to refine our hypotheses in quantitative models (e.g., some variables might be endogenized or considered in interaction with others).

Unit heterogeneity

The CH and FL tests are based on a pooled sample of all civil wars, reflecting a strong assumption of unit homogeneity.[41] If this assumption is violated, it can bias causal inferences from

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the model. Case studies are a good way to test the validity of the homogeneity assumption—for instance, by exploring the organization of rebellion.[42] If there is substantial unit heterogeneity in the data, then tests should be restricted to particular regions or ranges of key variables across which outcomes differ significantly. For example, we should use slightly different models to analyze the risk of civil war in Africa or in the Middle East, or we should analyze the likelihood of civil war only in low-income or heavily resource-dependent countries.[43] We did not adopt such an approach in selecting cases, because both CH and FL put forth propositions that can be widely generalized, so we sought to have broad representation roughly corresponding to the geographic incidence of civil war.[44]

Identifying causal mechanisms

Case studies can identify interactions between variables and establish a chronological sequence of events that helps map out the pathways linking the independent variables to the dependent variable. This also makes it easier to deal with the problems of endogeneity and selection that likely affect the results of both CH and FL. Several plausible causal mechanisms can be identified by the case studies, though testing the significance of these mechanisms and rank-ordering them is probably best done by going back to the statistical models.[45]

Mechanisms are, in effect, variables that operate in sequence.[46] They can also be outcomes: depending on the level of aggregation at which a theory is built, different outcomes become intervening variables, connecting a set of variables to other outcomes. Consider the theory that HIV causes AIDS and AIDS causes death. HIV infection can be an independent variable in a regression on mortality rate. Contraction of AIDS is the mechanism through which HIV usually leads to death. But at a finer level of specificity, we could discern a number of competing mechanisms that link HIV to death (e.g., pneumonia, cancer, viral infections). Finally, HIV is itself a mechanism through which actions (e.g., needle sharing among intravenous drug users) might cause death. It is therefore up to the analyst to determine the level of aggregation at which he or she formulates a theory; and according to that level of aggregation, one could proceed to identify the mechanisms linking independent variables to the dependent variable. There could be an almost infinite regression toward more micro-level cause-and-effect relationships, but each step down the ladder changes the real focus of the theory (in our example, we go from a theory of how HIV causes death to a theory of how cancer causes death). This infinite regression to more remote causes ultimately becomes irrelevant, as more distant causes will explain less and less of the variance in the outcome that we are trying to explain.[47]

Thus, we come back to the distinction between micro- and macro-level research on civil wars that I made earlier. It is true, as argued by Stathis Kalyvas in an earlier issue of this journal, that motives for violence vary wildly at the micro level and that micro-level and macro-level determinants of civil war often do not directly correspond to one another.[48] But analysis at different levels of social conflict will necessarily reveal different causal patterns. The interesting question is how and why disparate private motives for violence are disciplined into a single organizational form, such as civil war. The disjuncture between micro-level actions and macro-level identities that Kalyvas expertly demonstrates implies one of two things: the variation at the micro-level is irrelevant to the question of civil war onset, or civil war is such an aggregate concept that it is not useful as an analytical category. Supposing the latter, if civil war includes coups, riots, gang violence, crime, and genocide, are we right in analyzing civil war as distinct from all these other forms of violence? If what we are trying to explain is the outbreak of civil war, then the process of interest is that by which divergent incentives and myriad personal calculations generate civil war rather than another type of violence. The theories proposed by CH and FL *assume* that civil war is a distinct category of violence and try to explain when and where civil wars are likely to occur. (This assumption is clear in their arguments but is also revealed by the fact that they test their models only on data of civil war, not on data on all forms of violence.) But these theories can partially explain many forms of violence—even organized crime—and are not specific to civil war. We must therefore consider a wider array of both micro- and macro-level theories, including ones that explain how emotions, ideology, revenge, or coercion can interact to produce collective action that culminates in a civil war.

Ultimately, it is the interaction between micromotives and macrostructures that determines the expression of violent conflict. Different organizational forms (e.g., civil war) are at the same time outcomes of such a micro-macro interaction *and* mechanisms that can explain violence at higher levels of aggregation. Thus, terrorism, coups, and riots may be leading indicators of civil war (and may precipitate civil war), yet we must explain why in some countries we observe those forms of violence without also observing violence escalation into civil war. A general theory of political violence must explain how and why we shift from one form of violence to another, and it must analyze civil war as part of a dynamic process.[49] At the same time, if we do not know why a civil war instead of some other type of violence occurs, then we do not understand civil war.[50]

There is potentially a hierarchy of mechanisms that explain how civil war starts, but such a hierarchy might be hard to establish through case studies. However, even without rank-ordering these mechanisms, we can identify patterns that keep coming up in the cases.[51] These patterns can be explored further in statistical analysis. The narratives presented in the case studies show dynamic interactions over time between explanatory variables in the CH model, and they help identify mechanisms and variables that may be more commonly found in civil wars than in other types of violence. Later in this article, I focus on a few variables and processes that might explain civil war: I consider the effects of state repression, the impact of neighborhood effects, and external intervention in the process of conflict escalation leading to civil war.

Measurement Error and Causal Inference

A discussion of the CH and FL empirical results must start with an assessment of the consequences of measurement error

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in these data sets. Although CH and FL take much care in coding cases and collecting data, in several instances they use problematic proxy variables. This point, borne out by several cases, casts some doubt on several causal inferences that might be drawn from the CH and FL findings.

Economic variables: GDP, growth, education

The key measures of opportunity cost in the CH model are GDP per capita, secondary education (among males), and economic growth. Consistent with the CH theory, many countries in the Case Study Project on Civil Wars had low or declining income in their prewar years. In Sierra Leone, real per capita income was barely more than $900 before the war started, down from $1,400 in the 1970s. In Indonesia, the East Asian financial crisis caused income to fall by 9.8 percent in the province of Aceh in 1998, right before a war started. The oil and gas sector, which accounted for 65 percent of Aceh's GDP, shrank by almost one-fourth during the financial crisis.[52] In Nigeria, recession in the late 1970s caused unemployment to double to more than 20 percent before the onset of the Maitatsine rebellion. In Yugoslavia, income dropped and unemployment soared after the liberal reforms of 1989, just two years before the first of several wars in the former Yugoslavia.

But GDP per capita can also be used as a proxy for state strength, as in the FL model. It is unclear, therefore, how to interpret the negative correlation between GDP per capita and civil war. CH's use of GDP adjusted by purchasing power parity is consistent with the opportunity-cost argument; yet FL's constant-dollar GDP figures are more in line with the state-strength argument, since they describe the overall size of the economy. However, both uses of GDP are questionable. The size of an economy probably correlates with country size or population size and may not indicate the extent of a state's control over its territory. For the CH opportunity-cost argument, unemployment levels might have been a more direct measure, because they would have indicated potential rebel supply. In prewar Yugoslavia, income per capita was two or three times that of war-affected countries in our sample, but unemployment surged and in some regions reached 40 percent of the adult population. Using unemployment rates (especially region-specific rates) in the quantitative analysis might help distinguish the CH opportunity-cost theory from the FL state-strength theory—hypotheses that are now conflated by their similar use of GDP as a proxy variable.

The interpretation of GDP as a measure of state strength is also problematic in a number of cases. Consider the United Kingdom (Northern Ireland insurgency) and Kenya (Rift Valley ethnic violence). In the United Kingdom, the FL theory could be used to explain (at least partially) why the "Troubles" and their aftermath did not escalate into a larger war. The strength of the British state may have forced the insurgents to adopt a strategy of low-level urban violence and terrorism.[53] Yet this explanation omits other plausible factors, such as the role of civil society and public opinion in the United Kingdom and neighboring Ireland. A more intense campaign by the Irish Republican Army (IRA) and a more indiscriminate and forceful response from the British army would have caused negative reactions from civil society on both sides. In an established democracy like Britain, war-fighting tactics such as the ones used by the Russian state in the war in Chechnya (for instance, bombing the capital, Grozny) are not viable—indeed, they are unthinkable. But it is hard, on the basis of the FL model, to disentangle the impact of the state's strength (defined in terms of military and economic capacity) from the consequences of the state's liberal-democratic characteristics.

The problem lies with the poor analytic value of the empirical proxy used to measure state strength. The case of Kenya is instructive. Here, we have a weak economy with a weak civil society, but a strong authoritarian state. The state's ability to repress its opponents was unconstrained until the recent liberalization in Kenya. Despite strong ethnic antagonism, significant electoral violence, and a coup attempt in August 1982, no civil war has occurred in Kenya, mainly as a result of the state's strength and authoritarianism.[54] The mechanism of exercising state control was corruption.[55] The government used local police forces to violently repress local opposition groups that could not be bought off; it rewarded government supporters with gifts of public land. Our case study on Kenya shows that a low GDP is not necessarily a good measure of a state's capacity to prevent civil war. Additionally, we cannot even distinguish between the effects of state strength and those of absent or weak civil-society institutions. Clearly, to sort out the relative significance of these explanations, we must return to large-N data analysis. But case studies help us identify plausible mechanisms and hypotheses to test.

Consider next the CH argument about education. Several countries seem to fit the CH model well, especially in Africa (there were virtually no educated Congolese at the time of their independence and right before the start of their first civil war). But regional disparities may weaken the underlying theory linking education to violence. Other civil war countries—Cyprus, Yugoslavia, Georgia, and Russia—had high education rates. Lebanon had one of the highest in the Arab world, with adult literacy at 60 percent (compared with 15 percent in Iraq) in the 1950s and 1960s, and school enrollment at 76 percent in the 1950s.[56] Saudi Arabia, by contrast, had a secondary schooling rate of 4 percent (males enrolled), but no war.

To make sense of these contradictions, we need an explanation of *how* schooling might influence civil war risk. A close-up look at what is being taught in schools might help. In many countries, the curriculum is the primary way of inculcating children with nationalist ideology. It is not surprising that CH does not focus on this micro-level mechanism, since the model dismisses the significance of nationalist ideology as a motive for civil war. But others have shown a causal link between nationalist education in schools and the persistence of nationalist ideology.[57] This might explain cases such as Lebanon, where sectarian education fueled war by nurturing ideologies of intolerance. Indeed, a recent study on terrorism found that Hezbollah recruits in Lebanon have been drawn from the ranks of the highly educated, and this pattern also seems to hold cross-nationally.[58] If economic opportunity cost is the

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mechanism through which education influences the decision to join a civil war, then why does this mechanism not apply to the decision to join a terrorist organization? This puzzle suggests that a refinement of the theory is necessary to distinguish civil war from terrorist violence.

Turning to economic growth, several of our countries seem to be perfect examples of the CH argument. In Sierra Leone, economic growth was negative before the start of the war in 1991.[59] Yugoslavia's growth rate declined 15 percent to 20 percent between 1988 and 1992, fueling social unrest.[60] Economic growth was also negative in the five years before civil war broke out in Senegal, Mali, Azerbaijan, and other countries in our sample. However, the relationship between economic growth and civil war is complex. While CH posits a linear relationship, there are undeniable dynamic effects between growth and civil war.

First, the effects of economic growth may be channeled through other variables. Even rapid growth may (indirectly) cause civil wars. In Indonesia, it indirectly reinvigorated GAM—the rebel Free Aceh Movement—because it resulted in the expansion of the extractive resource industries in the region and an increase in the number of migrants, leading to land seizures in Aceh.[61] Thus, government policies implemented in high-growth periods exacerbated the risk of war. This example seems to suggest that internal migration is a mechanism for war outbreak; but migration was the result of a deliberate government policy of repression, so the cause of violence was state repression, not migration (or growth) per se.

Second, quantitative studies of civil war fail to account for the effects of low-level violence that typically precedes war, reducing both income and growth by reducing investment and encouraging capital flight. This is particularly true for studies using data sets that code civil war onset during the year that deaths cross the 1,000 threshold (as is common in the literature), even though armed conflict may have been occurring for several years. Quantitative models should therefore consider modeling the endogeneity of economic growth to armed conflict. Political violence in Caucasian states caused massive declines in income. Georgia's GDP per capita dropped from approximately $3,670 in 1991 to somewhere between $777 and $913 in 1997.[62] In Azerbaijan, GDP fell from around $4,400 per capita in 1985 to around $400 in 1996, and then rose to $510 in 1999. In the Democratic Republic of Congo (DRC)—one of the most war-ravaged countries, with up to five civil wars—income per capita in the late 1990s was half of what it had been at the time of independence in 1960. In Burundi, another country with recurrent civil wars, GDP per capita fell by half in the 1990s, from $211 in 1991 to $110 in 1999.[63] If at least some of these declines were attributable to escalating violence that led to civil war, then we have a feedback effect that should be properly modeled in quantitative studies of war onset.

In response to these drastic changes in economic conditions, the CH theory predicts that the risk of civil war would increase as income fell. This is consistent with evidence that the risk of war recurrence is far greater immediately after the end of a war than several periods later.[64] Declining income can be the mechanism through which time at war increases the risk of future wars. But if we interpreted GDP per capita as a measure of state strength, we would reach a similar conclusion, as falling GDP would imply loss of strength, which would increase the risk of a new war. This logic might suggest a specification change in the CH model: adding an interaction term between GDP and ongoing war to a model of civil war onset would measure such an effect. Furthermore, the CH model neglects the effect that ongoing civil war has on the risk of a new war. The authors drop ongoing periods of war from their analysis, ignoring the feedback effects mentioned above. If we used the FL coding of the dependent variable, we could add such an interaction term to control for the potentially differential effects of some variables during periods of war as compared with periods of peace. For instance, as argued in the case study on Georgia, a war's depletion of national income may discourage a long war duration because the available "loot" shrinks, making war unprofitable.[65]

In short, a close examination of the cases reveals the need to refine our empirical measures so we can explain the relationship between economic variables and war onset. The same is true with respect to the CH and FL arguments on resource dependence.

Resource predation and primary commodity exports

The resource-predation hypothesis is a cornerstone of the CH "greed" (or "opportunity") model of civil war. DRC is perhaps the best example of this argument. Five Congolese rebellions originated in the resource-rich regions of Katanga, Kivu, and Kasai.[66] DRC, formerly Zaire, produces 50 percent of the world's cobalt, 30 percent of its diamonds, 20 percent of its copper, and sizable amounts of gold and tin—and most of this production is concentrated in the eastern provinces. Mineral exports equal 25 percent of the country's GDP.

However, testing the resource hypothesis is difficult, because CH uses the ratio of primary commodity exports to GDP—a very distant measure of resource dependence. CH finds maximized risk of war onset when primary commodity exports constitute 25 percent to 32 percent of GDP (depending on whether GDP or education is included in the model). While this is a useful result (for one thing, it suggests that diversifying a country's economy may reduce the risk of civil war), the proxy includes agricultural commodities that are not easy to loot. A more targeted test would consider the exports individually and focus on easily lootable usual suspects, such as diamonds, timber, or gold.[67]

Testing the resource-predation argument is also difficult because, as the case studies show, civil wars in resource-dependent countries often have nothing to do with natural resources. The Maitatsine rebellion in Nigeria in the 1980s took place in an oil-dependent country, but the rebellion was not financed by natural-resource rents. Rather, the rebels—drawn from the ranks of the homeless, the unemployed, and the politically exiled—were recruited through ideological indoctrination and Koranic teaching. They used primitive weapons; their limited finances came from beggars' hoards, small-scale thefts, and profits from sales of charms and medicines.[68] For

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predicting civil war in Senegal in the 1990s, the FL model seems more appropriate than the CH model. Senegal reexports oil, so although it does not produce a significant amount, it is coded as an oil exporter; the country also depends on primary commodity exports. However, "subscriptions" to the movement, not resource extraction and looting, initially financed its civil war.[69] Exploitation of the cannabis and cashew nut crops in Casamance, Senegal, helped the rebels pay for the war once it had started, while the army extorted timber from the region.

In some countries, resource predation is a motive for civil war even though the CH data set codes them as having low primary commodity exports. Oil was a key cause of the 1967 Biafran rebellion in Nigeria. There had been no demands for self-determination of the eastern regions before the discovery of oil. But once oil was found, Odumegwu Ojukwu, governor of the eastern region, mandated "that oil revenue be paid to the regional treasury," and demands for independence grew.[70]

Using the country/year as the unit of analysis makes it hard to avoid spurious correlations in CH and FL. Consider Azerbaijan, which had a nationalist war between Azeris and Armenians from 1991 to 1994.[71] The country is dependent on exports of oil and natural gas, amounting to 23 percent of GDP in 1999 and 91 percent of total exports in 2001.[72] On the surface, this observation seems consistent with CH and FL, since both models predicted a high risk of civil war in Azerbaijan partly because of its oil dependence. But war occurred in Nagorno-Karabakh, a region bereft of natural resources, with a small economy based on agriculture and food processing. Because econometric tests are based on country/year analysis, they could not establish the fact that this conflict was entirely unrelated to oil.[73]

Another important question left unanswered by CH and FL is whether natural resources create motives for war, opportunities to sustain wars, or both. Although case studies cannot uncover hidden motives, they can trace the sequence of events that betrays them, and they can analyze public rhetoric and actions of rebel leaders. In many countries with no natural resources—Bosnia, Lebanon, Burundi,[74] Georgia, Mozambique—we nonetheless observe predatory behavior by rebels.[75] Theft from houses and small businesses, car jackings, extortion, and kidnappings for ransom are all ways rebel groups can finance rebellion. So it is not natural resources per se, but opportunities for looting, that are widely used to sustain insurgency. Some resources cannot be exploited unless the rebels have gained control of the territory (this is the case with oil). One would therefore expect resource predation to be especially important for sustaining rebel organizations once the violence has started. Conflict escalation is likely to occur, then, in oil-dependent countries with secessionist ethnic groups. The state cannot afford to lose revenue from oil exports or to let the rebel groups grow by taking control of oil fields.

Unit Heterogeneity: Ideology, Ethnicity, and the Organization of Civil War

As the previous section of this article makes clear, the data from the Case Study Project on Civil Wars demonstrate that the risk of civil war is not spread evenly across countries. Rich, industrialized countries are virtually risk-free. Middle-income countries have low or declining risk. Poor countries face the most risk. While the case studies' quantitative models control for differences in development, the reason that countries have different proclivities to civil war might have more to do with the way other independent variables, such as ethnicity and democracy, behave at various levels of income.

Take the United Kingdom, where a high level of economic development contributes to the CH model's estimate of a 2 percent probability of civil war from 1970 to 1974, an estimate that is three times lower than the population average. However, a war did break out in Northern Ireland in 1971, despite high levels of income.[76] Secondary school attendance there was high (in 1970, Northern Ireland was among the top 13 countries in the world), as was its income level (not far from Britain's), and there were no natural resources.[77] But in a sense, all of that may be irrelevant if the civil war was motivated by religious difference and fueled by repressive government policies. The opportunity-cost argument does not apply well to "volunteer forces" such as the Irish Republican Army (IRA) that are more concerned with ideology than with looting.

A key reason that CH does not predict this case well may be the fact that the model pools all types of civil war (as does FL).[78] Thus, CH assumes that the opportunity-cost argument applies to all civil wars, even wars that are ethnicized. But ethnicization of a war and other aspects of the organization of violence might be related to the causes of violence. For example, Miriam Lowi asserts that economic decline and demographic pressures led to the emergence of Islamist protest in Algeria in 1992.[79] But several periods of serious economic decline in that country did *not* produce Islamist backlash—e.g., under Houari Boumedienne (1965–1978). Rather than economic failure alone, the country's bankrupt political system (leading to illegal intervention in the 1991 elections)—*combined* with increased economic woes—may have caused the Islamist protest in Algeria.

Some analysts classify wars fought by ethnic groups as ethnic wars.[80] Others argue that ethnicity is just a cover for economic motives,[81] personal hostilities,[82] criminality,[83] or an assortment of other objectives that are not truly ethnonationalist at their core.[84] But even though many conflicts can become ethnicized after they start, and ethnic mobilization can be used by political elites to support nonethnic rebellions, it may be significant that ethnicity is mobilized in some wars but not in others. After all, many rebel groups are organized within ethnoreligious parameters (in Burundi, recruitment follows tribal lines;[85] in Lebanon, recruitment and alliance patterns follow religious lines[86]). A common-sense definition of *ethnic war* is a war fought between ethnic groups over issues that relate to ethnicity. It does not matter whether ethnic identity can be manipulated by elites pursuing private goals; the fact that ethnicity lends itself to manipulation and can be used to motivate collective action is in itself significant. If there is *anything* special about ethnic ties, then wars that are possible only through the mobilization of ethnic identities should be viewed as distinct from other wars and may have different causes. Wars over

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self-determination, for example, should be usefully distinguished from popular (class-based) revolutions. (Different variables—for example, regional inequality or the territorial concentration of ethnic groups—may explain wars over self-determination while being less relevant for other types of war or other forms of violence, such as terrorism or coups.)

To avoid making false inferences about the causes of civil war, before we pool all civil wars, we must establish that there is no significant unit heterogeneity (i.e., no differences between ethnic and nonethnic war).[87] To better predict civil war, we must also explore another type of heterogeneity: the differences across forms of political violence. Assume, for a moment, that coups have different causes from civil wars but that genocides or politicides have similar causes. Current practice is to include coups in civil war lists when the total number of deaths is high enough and when the state loses some lives, and to exclude genocide unless it takes place during civil war (but not right before or right after it).[88] If many coups are categorized as civil wars, then our inferences about the causes of civil war will be conflated with results about the causes of coups.[89] By contrast, if genocides or politicides are excluded from our data, there will simply be more uncertainty in the estimates, because of the observations lost. If we assume, however, that both coups and genocides are different from civil war, as is commonly argued in the literature, then we can consider whether coups (both failed *and* successful) are causes of civil war. In that case, we would want our data sets to distinguish a coup's "coup" phase from its "civil war" phase, but neither our data nor our definitions are currently fine-tuned enough to do so for all cases. Genocides might in fact require that we invert the equation, since they are often consequences of civil war.[90] If so, only a model or narrative explaining how these two outcomes fit together could show how we shift from civil war to genocide. These hypothetical examples demonstrate that, rather than studying civil war by way of a static model and ignoring the differences between civil war and other violence, we can gain leverage by identifying and explaining the differences both within the category of civil war and between civil wars and other types of political violence.

Along the same lines, in order to grasp the dynamics of civil war, we must study the growth of rebel organizations. One pattern identified in our case studies is that most insurgent groups do not have war-fighting capital (troops and weapons) from the outset; they start small.[91] In Colombia, the National Liberation Army grew from 30 men in 1965 to 270 in 1973, and then to 4,500 in 2000.[92] In Azerbaijan, the NK rebels grew from under 1,000 in 1988 to 21,000 between 1992 and 1994.[93] In Aceh, GAM started with 24 members in 1976 and, by 2001, had 2,000 to 3,000, plus a militia of 24,000; it came to control 80 percent of Aceh's villages.[94] In Georgia, the South Ossettian force went quickly from 300 to 400 volunteers in early 1990 to 1,500 full-time fighters and 3,500 reservists.[95] The war in Mali started when a small group of Libyan-trained fighters belonging to the Mouvement Populaire de Libération de l'Azawad killed four people and took control of a dozen rifles, which they gradually used to increase their ranks and military strength.[96] Based on these examples, one could argue that to get to civil war (as opposed to smaller-scale violence) violent groups need time to grow. A factor that facilitates growth is external assistance as well as the state's inability to effectively repress the insurgency in its early stages.

The government's actions during the early stages of rebellion are often a critical determinant of escalation to war. A civil war (but potentially, also, other violence) is likely if the government uses repressive tactics that reduce the effectiveness of nonviolent protest while not being sufficiently strong to end all protest.[97] In several countries, we have observed conflict escalation that transformed preexisting nonviolent political groups, political parties, or ethnic organizations into violent rebels. In Burundi, the army was "a permanent threat" that caused much violence.[98] Hutu groups from the 1960s and 1970s participated in the violence of 1990, although new ones also formed. In Lebanon, most militias were associated with a preexisting political party or religious group.[99] In Northern Ireland, a clearly defined ethnic base supported insurgency, since the IRA recruited predominantly from the (mostly working-class) Catholic community. The violent wing of the IRA splintered off a nonviolent movement. In the DRC, ethnicity formed the basis of five rebellions. The Lunda, Ndembu, and Yeke groups, for example, led the Katanga secession and Shaba wars, while the 1996–1997 Kabila rebellion drew its first recruits from the Banyamulenge.[100]

The fact that preexisting ethnic organizations are effectively mobilized to support violence in some cases, but not others, might offer the basis for making a distinction between ethnoreligious and other wars. If, however, ethnoreligious affiliation is widely used to mobilize support for rebellion in different types of wars, establishing such a typology may be difficult[101]—unless we focus on wars over self-determination. (Such "ethnic wars" are of the ideal type discussed earlier: they are fought between ethnic groups *over* ethnicity.) If some countries are at greater risk of ethnic war than of nonethnic war (perhaps as a function of their demographic characteristics), and if ethnic wars are caused by different combinations of factors than are nonethnic wars, then there is both analytical and policy interest in establishing a typology of ethnic war. Case narratives can help us develop such a typology by offering details on the patterns of recruitment as well as on the issues over which the war is fought.

The case studies highlight yet another feature of rebel organization during civil war that economic models have overlooked: forced recruitment of soldiers. Coercion was widely practiced by the guerrillas of the (Communist) Democratic Army of Greece from 1947–1949,[102] for instance, and by the Lord's Resistance Army in Uganda in the 1990s. In Burundi, rebel groups purchased Kenyan street children at the price of $500 for 150 boys.[103] In Mozambique, the Front for the Liberation of Mozambique (FRELIMO) used repression, imprisonment, reeducation, and indoctrination to increase its forces, while the Mozambican National Resistance (RENAMO) "used force at every point for almost every purpose."[104] This pattern creates problems for the economic opportunity-cost theory, since many recruits do not have the luxury of deciding independently to join the rebels.[105]

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A crucial question is whether coercion and material gain together are enough to motivate political violence. To answer this question, we need to go beyond CH and FL. Roger Petersen's research, for example, associates violence with emotional responses to structural change.[106] This perspective appears to negate the presumption of CH and FL that everyone is a potential rebel, given the opportunity. Rather, Petersen might say, a rebel (at least one who is not coerced) should have an emotional makeup that differs from that of someone committed to nonviolence.

I want to emphasize that emotional and economic theories of civil war can be complementary, not mutually exclusive (at least inasmuch as emotions are controllable by thought). The relationship between the two is especially clear in Robert White's analysis of insurgency in Northern Ireland. White explains the Catholic Nationalists' switch from peaceful protest to violence as a reaction against the initiation of internment and violent acts committed by the state.[107] Most of the protesters interviewed by White were outraged by government repression. But those who turned to violence tended to be working class, unemployed, or in school; by contrast, those who continued to believe that violence was not the answer tended to be older, employed, middle-class people with college degrees—in other words, people who faced higher opportunity costs if they fought.

Many emotions (e.g., resentment toward injustice or fear of repression) are clearly consistent with economic models of war, since some of the models' variables, such as political instability and economic decline in an ethnically fractionalized society, could elicit emotions in favor of insurgency. Moreover, emotions can lead people to pursue private goals, like greater security or more wealth. However, other emotions (e.g., rage) are inconsistent with economic models because they generate violent acts that are not necessarily selective in their targets. Perhaps rage is more closely associated with terrorism, as terrorism is an example of usually nonselective violence;[108] civil war, however, has been shown to be selective.[109]

One way to combine the emotional and economic theories is to view emotion-based explanations as focusing on the demand side of the equation and economic models as focusing on the supply side. As we develop more of the demand side, it becomes obvious that ideology and psychology cannot be ignored as explanations of civil war. It may be the case, however, that ideology, ethnicity, and emotions play different roles in different forms of violence. To determine whether this is true, we must look first at what causes violence and then at how violence takes specific forms.

Reconceptualizing the Dependent Variable

I have been arguing that we cannot understand the causes of civil war without looking both below civil war (at individual-level violence) and around it (at different forms of organized political violence). Case studies provide useful insights on the similarities and differences among coups, wars, and politicides, and on the pathways that lead from one to another.

Recurrent cycles of violence

For many countries caught in a conflict trap, civil war is a phase in a cycle of violence. By isolating civil war in quantitative studies, we choose to focus on an event rather than a process, and we discard a lot of useful information that explains how we end up having a civil war.

It is common to see violent anticolonial movements give way to civil war (Algeria in 1962, Mozambique in 1976, West Papua in the 1960s, Sudan in the late 1950s), or to see civil wars grow out of international wars and occupations (Greece in the 1940s; Yugoslavia in the 1990s, with an almost 50-year delay since the intra-Yugoslav fighting during World War II; and, more recently, Iraq).[110] Civil wars can be very bloody coups (as in Costa Rica in 1948, Bolivia in 1952, and Argentina in 1955) or international wars fought by proxy in a third country (Lebanon from 1979 to 1991, the DRC from 1998 to 2002). They can be born out of riots. The 1947 riots in India had both the organizational complexity and the level of destruction usually found in a civil war; bloody spikes of riots and pogroms have marred Indian history ever since. Sometimes, as in Rwanda, civil wars are indistinguishable from intercommunal fighting, since all politics seems to be colored by ethnic divisions. In Burundi, nationalist strife in the 1950s led to ethnic violence after independence had been obtained; this continued with massacres in 1972 and 1988, and culminated in a civil war in the 1990s. Nigeria went from a massive civil war in Biafra in the late 1960s to relative peace in the 1970s, but then experienced ethnic rioting and massacres and a second bout of civil war in the 1980s. Though Nigeria has not had a civil war in the 1990s, widespread rioting has killed 10,000 people. Other countries—such as Cyprus, which was embroiled in violent conflict from the late 1950s until the mid-1970s—have gone from anticolonial movements to coups, to civil war, to interstate war. DRC has seen every imaginable form of political violence since the 1960s, including a possible genocide in the Kivu area in the 1990s.

Indeed, the reality of civil war is messy, and our decision to code a period of violence as a civil war (instead of a riot, a coup, or politicide) hinges on rather vague criteria.[111] In many definitions of civil war, a distinguishing feature is effective resistance—i.e., the state must also suffer. But no coding rule to date specifies the time period over which the state must incur its deaths. If it suffers 100 deaths in the first year of a four-year conflict that causes 30,000 total deaths (with 29,900 on the side of the opposition), the entire four years might qualify as civil war. But when state deaths are concentrated in a short period of time, is it not more accurate to code one phase of the conflict as civil war and the other as politicide?

The example of Cambodia illustrates the difficulty of distinguishing between civil war and politicide. In Cambodia, civil war from March 1970 to April 1975 killed, on average, 122,500 people per year. Most data sets code a politicide that followed immediately after, killing an average of 347,500 people per year from April 1975 to January 1979. A second civil war is coded from 1979 until 1991.[112] The killing fields in Cambodia (1975–1979) are thus excluded from civil war lists,

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even though this period of extreme violence was closely linked to the war. But fighting (which included Vietnamese troops) was ongoing around the border during these years. Because of the low quality of the data, we cannot establish with certainty that 100 or so troops from the stronger side were not killed during that period; if we were certain about this, we would code an ongoing civil war in Cambodia along with politicide in 1975–1979. More importantly, we cannot understand the onset of the new conflict in 1979 as merely a function of Cambodia's GDP per capita or natural resource dependence—the killing fields are an obvious missing link here.

Lack of attention to the relationship between civil war and other forms of violence hurts models like CH and FL, which code 1975–1979 in Cambodia as a period of "peace" (i.e., no war). Such periods of "peace" can hide much violence. By modeling the proximity of other forms of violence, we can indirectly model the complex links among past riots, coups, and interstate wars—all of which add to the risk of a future civil war by destroying property and human capital, undermining economic incentives, increasing levels of intergroup hostility, and accumulating conflict-specific capital. Revising the peacetime measure, for instance, would allow the CH model to make better predictions.[113] Beyond this, we can follow a two-pronged approach: First, if our data do not allow us to make clean distinctions between forms of violence, we should not throw away cases for not meeting arbitrary definitions, but rather analyze all events of political violence as an aggregate category. (The theory must be stated at a higher level of aggregation in that case—it will be a theory of political violence.) Second, we can try to improve our definitions and data coding, and develop models that explain different forms of violence as well as the transition from one form to another. This dynamic approach has considerable appeal, as political violence may itself be understood as a part of a process of state evolution, and states at different stages of their development might be at risk of different forms of violence.

Crime, grievance, and politics: The organization of violence

Another interesting link identified by the case studies is one between criminal and political violence. In Colombia, for example, 90 percent of the regions in 1995 with the highest homicide rates also had active guerrilla groups, whereas these groups were active in only 54 percent of the country's total number of regions. Furthermore, 70 percent of these high-homicide regions had substantially high drug trafficking (as compared with 23 percent of regions nationally).[114]

Criminal and political violence are both favored by state weakness. Mafias are organizations designed for extortion, smuggling, and drug trade, but they can also provide security and authority in areas where the state has no monopoly over the means of violence.[115] Organized crime thus flourished with the decline of the Soviet state's strength. In the 1990s, Russia's inability to maintain the prison population led to mass releases, which led to more crime.[116] Haphazard privatizations increased the amount of loot, spurring the formation and growth of criminal gangs, much in the same way that rebel organizations have grown in Sierra Leone and other resource-rich failing states.

Looting is also a common way of supporting both political and criminal violence. The form of violence that emerges—Mafia or rebel group—may be determined by the type of available loot and the means necessary to appropriate it. If ordinary crime or corruption is sufficient to acquire the desired amount of loot, then it could prevail as the organizational mode of looting. If large-scale looting is needed and if economies of scale in looting can be exploited, then organized crime will flourish and can coexist with a weak state. If appropriation of loot requires control of the state or control of territory (as in the case of oil deposits), then rebel organizations, particularly secessionist groups, will likely grow. Rebellion may not be necessary if control of the state can be achieved indirectly through the exploitation of ethnic or kinship networks, turning the state into a source of rents for the group.

In sum, organized violence is the result of four interacting factors: the demand for loot, the demand for political change, the opportunity to mobilize criminal or insurgent groups, and the mechanisms (relational, emotional, cognitive, or environmental) that characterize claim making and resource extraction. And there are important links between political and criminal forms of violence: while a strong state can deter the escalation of a conflict to violence, criminal and political violence can reinforce each other and thereby undermine a state's authority and capacity. Consider Sierra Leone, where criminal activity accumulated violence-specific physical and human capital, and war diverted the state's attention from fighting crime.[117] Over time, the rebels and criminals became indistinguishable from one another: the Revolutionary United Front (RUF) recruited and supported illicit diamond diggers while fighting against the state. Similarly, in Colombia, guerrillas provided protection for drug cartels, which in turn financed the rebellion. War economies create constituencies that benefit from war, and violence is sustained by the same logic of profiteering that supports criminal activity. Over time, low levels of crime and violence can take control of a state.

In cases where resources are insufficient to create much public support for rebellion or where economic gains do not trump emotional or ideological motives for violence, terrorism may occur. Terrorism can also grow where large-scale rebellion is likely to be crushed by a strong state (terrorism might be considered an "incomplete" civil war). It can feed off civil war, and vice versa. In Egypt, terrorism against Westerners was the direct result of government suppression of the al-Gama'at al-Islamiyya, an insurgent group. The Israeli-Palestinian conflict (some have called it a civil war since the first intifada, in 1987) was at the heart of international terrorism before the Oslo Accords of 1997. Kidnappings in Colombia provided a means for rebels to finance their insurgency. Today, Chechen terrorism in Russia is an outgrowth of the Russo-Chechen war. In fact, many Chechen terrorists are "heroes" from the Abkhaz war against Georgia (including Shamil Basaev, the Chechen prime minister, who fought in Georgia in 1992 and 1993). As one form of violence feeds

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into another over time and across space, we become less able to study each form in isolation.

Theory Building and Omitted Variables

Just as various forms of violence in the same country can influence the risk of civil war onset, civil wars themselves may have such an effect on neighboring countries. The CH and FL models disagree on this point. While CH finds that a country's civil war risk increases if its neighbors have had a civil war in the previous year, FL finds no such evidence. Both models, however, seriously underestimate the international dimension of civil war. Several case studies point to ways a "neighborhood" influences a country's war risk.[118]

Contagion and diffusion

As our cases show, such influence can be seen in the significant demonstration (diffusion) effects of civil war. A good example is the Aceh rebellion in Indonesia, where an independence movement simmered for decades and a brief civil war was quickly suppressed in 1991. The war reignited in 1999 when, in a climate of political instability and economic recession, East Timor's referendum on independence emboldened Acehnese resistance. The onset of mass protest in favor of independence in Aceh can be traced to November 1999, soon after the September 1999 referendum in East Timor.[119] In Senegal, the Casamance independence movement was influenced by the ideology of the independence struggle in Guinea-Bissau.[120]

Another sign of neighborhood influence, direct contagion (the spillover of war across borders), is abundantly evident in the cases under review. In the Casamance conflict, Guinea-Bissau was used as a venue for cross-border raids in Senegal, a market for goods, and a source of arms. Yugoslavia's wars (Croatia in 1991 and from 1992 to 1995; Bosnia from 1992 to 1995; and Kosovo from 1998 to 1999) all were shaped by the rival irredentist nationalisms of Greater Serbia and Greater Croatia, and many of the same groups were active in each war. In the former Soviet republics, wars clustered around the Caucasus in the early 1990s, "benefiting" from the region's accumulation of war-specific physical and human capital.[121] Sierra Leone's civil war did not start until Charles Taylor's Liberia provided sanctuary to the rebels; it was sustained by international crime networks that engaged in arms-for-diamonds trade. The civil wars in the African Great Lakes region are perfect examples of contagion. Burundi's and Rwanda's recurrent wars influenced each other as well as fighting in DRC, ultimately pulling in Uganda and Zimbabwe. In both Burundi and Rwanda, wars have occurred between the same two ethnic groups: the Hutu and the Tutsi. The Rwandan social revolution of 1959 caused a transfer of power from a Tutsi monarchy to a Hutu majority, leading to massacres of Tutsi and massive refugee movements, some to Burundi. Tutsi groups in Burundi feared a similar development, since the Hutu were also the majority there and the Tutsi sought to consolidate their power over state institutions, especially security forces.[122] This ongoing ethnic conflict was at the core of seven episodes of civil war in the two countries.

Quantitative studies that point to significant neighborhood effects of civil war have a hard time distinguishing among many possible diffusion and contagion mechanisms.[123] A prominent instrument of contagion is cross-border ethnic kin.[124] We see this in some of the cases. In Macedonia, the risk of civil war in the 1990s increased when ethnic Albanians in Kosovo actively supported Albanian independence across the border by organizing armed opposition to the Macedonian government.[125] Indeed, some civil wars are better understood as regional communal conflicts. The wars in Burundi and Rwanda are really wars between Hutus and Tutsis in the Great Lakes region, with significant temporal and spatial dependence connecting civil war outbreaks in these two countries. Another contagion mechanism is the flow of refugees from one country to another. A large number of refugees from Burundi and Rwanda to the Eastern Congo threatened the ethnic-demographic balance of the Kivu region, contributing to conflict among natives, migrants, and refugees.[126] An even more common neighborhood factor is the cross-border trade in small arms.[127] The end of a civil war creates a surplus of small arms and lowers their price in the neighborhood.

The examples above suggest that if we want to predict where and when a civil war will occur, we can no longer afford to ignore the temporal and spatial dependence of various forms of political violence.

External intervention and internationalized civil war

External intervention is another form of international influence that many case studies have identified as a key factor in civil war onset. In Mozambique, DRC, Burundi, Georgia, and several other countries, external economic and military assistance was critical in both inciting and supporting rebellion. Jeremy Weinstein and Laudemiro Francisco make a powerful argument that Mozambique's civil war was largely the result of South Africa's intervention. When FRELIMO became the new government in Mozambique, it offered safe haven to all African liberation movements and threatened the country's neighbors, Rhodesia and South Africa. FRELIMO's opposition in Mozambique, RENAMO, initially had a small base of support and amounted to a proxy war against Zimbabwe African National Liberation Army (ZANLA) guerrillas. The level of violence dropped markedly in 1979, when Rhodesian support for ZANLA stopped after the collapse of the Ian Smith regime.[128] RENAMO became incorporated into the South African Defense Forces, from which it acquired supplies as well as logistical and technical support, accounting for its tight, centralized structure.[129]

Mozambique's experience is not unique. The third Congolese war—the Shaba rebellion—was the result of an invasion by Congolese expatriates from Angola.[130] Yugoslavia's ethnic conflict in Kosovo rose to the level of civil war only after NATO's military intervention. And earlier, the Bosnian Serbs and Croats would probably not have had sufficient military resources to wage war in Bosnia without the support of Serbia and Croatia, respectively.[131] In Georgia, Abkhazian resistance could not have been organized or sustained without direct Russian

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assistance.[132] Similarly, the Lebanese war cannot be understood as distinct from the multiple external interventions and counterinterventions by the United States, Syria, and Israel. Most of the local factions in Lebanon represented a foreign government's interest. In Sierra Leone, persistently high levels of poverty, slow economic growth, low levels of education, and high dependence on natural resources did not cause a civil war until soon after the onset of Liberia's civil war in 1989. Charles Taylor offered Foday Sankoh, leader of Sierra Leone's RUF, a base from which to mount a rebellion. Sankoh received his insurgency "schooling" in Libya.[133] In this and many other wars in Africa, but also in countries as far away as Indonesia, Libya's Muammar Gaddafi proved exceptionally meddlesome.

Despite these observations, we have no quantitative research to date on the relationship between external intervention and civil war onset. This suggests that an important variable may be omitted from the CH and FL models. If intervention occurs with high frequency and ends up being statistically associated with civil war onset, and if the intervention is also correlated with any of the independent variables in CH or FL, then the CH and FL parameter estimates will suffer from omitted variable bias. It would be useful to test for such bias by adding a variable measuring intervention (or the expectation of intervention) to the models and re-estimating them.

Conflict escalation and civil war

External intervention is itself usually the result of an escalating pattern of conflict. To understand the conditions under which intervention will lead to war, we need to analyze process and not simply focus on outcome. Case studies can give us a better sense of the dynamics of conflict escalation by presenting a sequence of events—a series of actions and reactions—linking several independent variables together in a process that culminates in war. They can help us determine whether civil wars occur with little warning, like earthquakes, or with much buildup, like volcano explosions.

Some of the most useful insights from the case studies discussed here come from analyses of the dynamics of conflict in countries where civil war did not occur despite a large number of risk factors. Consider Nigeria since the mid-1980s. The risk of war outbreak there was among the highest in the world, according to both CH and FL. Nigeria has seen a lot of ethnic rioting over the past 20 years, but not war. Because the state's response has not been indiscriminately violent, antistate violence by ethnic rioters did not escalate. (By contrast, the state's reaction during the Biafran secession in 1967 was swift and overwhelming, since Nigeria felt threatened by demands for secession by an oil-rich region.) In the 1990s, a rebellion by the Ijaw did not grow into a civil war. Because the Ijaw used violence mostly against other communities and oil companies, the Nigerian government did not feel sufficiently threatened to respond forcefully. In fact, it granted concessions to the Ijaw. Whenever the Ijaw targeted the police or other government institutions, however, the government responded with decisive force. It is generally easier to gain concessions from the state if those concessions do not threaten state security; if they do, the potential for further violence is heightened.

A strong state can afford to be accommodating or repressive, at low cost.[134] But even accommodating policies may not effectively curb opposition if the state is weak and therefore cannot uphold its end of the bargain. In Aceh, for instance, the new democratically elected government's decentralization laws of 1999 were not credible. Because the government did not control the military and because Indonesia relied on Aceh for oil and gas exports, there was a significant risk that the government would renege on its promises of fiscal autonomy for Aceh.

If accommodation does not work, repression is usually next; but as our case studies show, government repression typically leads to more opposition and violence.[135] In Burundi, the government excluded Hutus from elite positions and inflicted violence on their leaders. Over time, this repression led to fewer educational opportunities and less economic power for Hutus—but it eventually backfired, resulting in a large-scale Hutu rebellion that entailed Hutu coup attempts, Tutsi countercoups, Hutu massacres of Tutsi in 1965 and 1972, and the involvement of the army and ethnic militias. The lack of democratic governance and the collapse of political and judicial institutions meant that there was no source of legitimate authority that could break the cycle of violence.[136]

A strategy of incomplete repression is likely to do more harm than good, while complete repression by strong states can eliminate the threat of war. However, in the case of the Muslim Brotherhood (one of Nigeria's many rebellious groups), war was avoided through selective repression. The Brotherhood had backing from Iran, was guided by an antigovernment ideology, and used violent tactics. It called for an Islamic state, but the arrest of its leader decapitated the movement in its early stages. Selective repression worked because it was applied quickly. Nondemocratic states can use selective repression more easily than democratic ones can to reduce the risk of conflict escalation. Thus, a government's likelihood of using repression or accommodation—and the effects of these approaches—may be determined by state capacity *and* regime type combined.

The conflict escalation potential of incomplete repression strategies may explain why democratization increases civil war risk.[137] A democratic or democratizing regime cannot easily use repression, because the state's enforcement apparatus becomes weaker as its activities become more transparent. The state is therefore less able to root out opposition in its early stages. During Indonesia's authoritarian period, state repression obliterated the GAM, the main rebel group in Aceh, until the early 1990s. But then a period of incomplete democratization caused friction between the state and the military, leading to incomplete repression strategies in Aceh. Since the state lacked control of the army, human rights abuses in Aceh went unpunished; this undermined the government's credibility and elevated popular grievance, which the GAM capitalized on to mobilize public support and increase its ranks of fighters.[138] In Senegal—a democratic country—large-scale expropriations of indigenous land in Casamance began in 1979, and a systematic

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denigration of Casamançais followed through the imposition of Wolof in education, the media, and the administration. The protests of December 1982 and 1983 triggered a harsh reaction from the state, helping to radicalize the movement as some of the protesters sought refuge in the forests and created the maquis (rebel bases). Protesters who were imprisoned in Dakar started organizing the political wing of the party.[139]

These examples of growing discontent because of failed democratization or incomplete repression suggest that most cases of civil war are better explained by the "volcano" (or escalation) theory rather than by the "earthquake" theory. Northern Ireland perhaps best illustrates how civil war can result from a slow but steady escalation of protest. The pivotal event was the march in Derry/Londonderry on October 5, 1968, when Royal Ulster Constabulary forces assailed protesters, leading to efforts at partial appeasement of Catholics with British intervention, and a package of political concessions to the Northern Ireland Civil Rights Association.[140] The unsubstantial reforms came late and caused extreme negative reactions by Unionists. Political instability and protest led to Terence O'Neill's resignation in 1969 and a victory for extremists, paving the way to the "Battle of the Bogside," which marked the start of the Troubles on August 12, 1969, and the development in 1970 of the Provisional IRA (PIRA). The PIRA abandoned the strategy of "abstentionism" that had been used up to that point—something akin to the peaceful protest of the U.S. civil rights movement—in favor of a radically militant stand against Protestants and the British, transforming a disorganized sectarian protest into an organized campaign for political violence.

Another important piece of the puzzle is that escalation potential varies across subnational regions and is greater in regions whose "special status" privileges have been revoked, as in Casamance (Senegal), Kosovo (Yugoslavia), Aceh (Indonesia), and elsewhere. In DRC, the Loi Fondamentale overturned longstanding legislation on minority rights and was seen as a precipitant to war. A series of nationality laws designed to "protect" the local population in the Kivu region led the Transitional Parliament to strip Banyarwanda and Banyamulenge of their Congolese nationality in April 1995. The Banyamulenge refused to leave and turned to Rwanda for help. Rwanda's intervention led to the massacre of Hutu refugees in DRC.[141]

Escalation risks are also a function of what goes on in nearby countries. Conflict was brewing all over the neighborhood for years before the Lebanese civil war erupted. The power of Palestinian organizations in the country grew after the 1967 Arab-Israeli War heightened anti-Israeli emotions and Palestinians forged alliances with Lebanese groups. The civil war can actually be traced back to 1968, when armed conflict broke out between rival Lebanese groups and between the government and Palestinian groups that wanted to use Lebanon as a stage for action in Israel.

These examples suggest that civil wars do not erupt without warning. The state and challengers go through a process of conflict escalation, often involving external influences. The process leads to civil war as the result of extreme demands by the challengers or repression by the state. The CH and FL models' logic of opportunity structure applies here, too, but it is only a part of a more complicated picture. Both repression and economic opportunity figure prominently as explanations of insurgency in this comment from John Garang, leader of the Sudan People's Liberation Army:

> The burden of neglect and oppression by successive Khartoum clique regimes has traditionally fallen more on the south than on other parts of the country. Under these circumstances, the marginal cost of rebellion in the south became very small, zero or negative; that is, in the south it pays to rebel.[142]

Conclusion

In this article, I have tried to make a substantive contribution to the literature on civil war by relying on a methodological approach that combines quantitative and qualitative research. I have briefly reviewed two major contributions to the field: the CH and FL models of civil war. These models make fundamentally sound propositions, but their scope could be expanded and their fit to the data improved.

Elaborating on the interplay between statistics and case studies has led me to consider the relationship between micro-level and macro-level explanations of civil war. Economic models such as those reviewed here offer one of several possible explanations of violence, but they do not explain why violence will take the form of civil war. I have argued that micro-level explanations must be aggregated to make sense of civil war (though I have not presented sufficient evidence to prove this claim). That is, micro-level theories explain violence *in general*, but it is hard for them to distinguish among the many different motives behind violence in civil war as opposed to violence in a riot, a coup, or a genocide. Macro-level theories, by contrast, are most helpful in explaining why, for a given set of private motives, violence is organized in the form of civil war or something else. Process-driven explanations, best explored through historical narratives that focus on the dynamic interaction among actors and between actors and opportunity structures, can elucidate particular outcomes and cases.

One difficulty is that this discussion presumes clear definitions of the various forms of political violence. If our concept of civil war is muddy, then it will be hard to explain. In the quantitative literature, this problem is addressed empirically, by running the same model on a few different definitions of civil war: if the results are the same, the model is accepted. But if there is no substantive difference between, say, coups and civil war (at least with respect to the variables typically included in quantitative models), then all competing definitions of civil war can be wrong together. Thus, to understand the interaction between micro-level and macro-level analysis with a view to explaining civil war, we must first establish and measure the differences across forms of political violence and identify the "ontology" of civil war. Ultimately, civil war cannot be defined only or even predominantly at the micro level. It refers to a specific organization of violence and is therefore a macro-level phenomenon. For civil war to have meaning as a distinct category, there must exist a combination of micro-level motives and macro-level structures that is unique to civil war.

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Case studies can help map the processes that link individual motives and actions to broader outcomes. In so doing, they may identify plausible mechanisms or potentially missing variables that can feed into theories of civil war onset. Case studies may point to causes of particular wars by systematically comparing several periods within each country, holding several variables constant across periods. But if too much is changing in tandem, then the cases can only be used to highlight processes. Either way, case studies offer theoretical inspiration, context, and texture that improve formal and quantitative models.

This article demonstrates one way to combine quantitative and qualitative methodologies: cases selected after model estimation can be used to identify measurement error, explore the exogeneity and homogeneity assumptions in the quantitative model, identify potentially omitted variables, and discuss causal mechanisms. This knowledge can help us to refine a theory and retest it in new quantitative analysis, improving our causal inferences from the model. As our cases suggest, the role of external assistance (direct intervention or provision of cross-border sanctuaries), neighborhood effects, and escalation processes may be important theoretical additions in explaining how political violence takes the form of civil war. Adding these variables and others to existing models may let us distinguish civil war from other forms of violence.

The Case Study Project on Civil Wars has also pointed to ways in which concepts and operational definitions of variables used in statistical analysis should be improved. Quantitative studies of civil war aim primarily at explanation, not prediction (though the two are obviously related). But most of the variables included in these models have few time-varying covariates and can pick up mostly cross-sectional variation, so their capacity to predict the timing of civil war onset will be limited. Thus, these models should at least make the correct predictions for the right reasons. We have seen that use of poorly measured and poorly conceived empirical data sometimes leads CH and FL to make the right predictions, but for the wrong reasons. If large-N studies make incorrect assumptions about causal paths, they will lack explanatory power. Moreover, the inferences drawn from these models cannot yet inform policy except in a very general and indirect way. We know that by increasing GDP per capita, we will *somehow* reduce the risk of civil war, but a more targeted policy intervention might be both more effective and easier to implement.

Some of the cases under review have challenged the unit-homogeneity assumption that underlies current quantitative work. This should prompt analysts to test for fixed effects by country, region, or period. Furthermore, periods with no civil war often hide important social conflicts and violence—a fact that is not properly modeled in quantitative studies. The path dependence of violence implies that we must model the transitions across different forms of political violence, such as riots, genocide, civil war, and terrorism. To better measure differences across forms of violence, we need to go back to theory-building and statistical tests. In such tests, the unit of analysis cannot always be the country and year. Narrowing down the analysis to the subnational level (the largest administrative region below the state) might reduce the risk of finding spurious correlations between natural-resource dependence proxies and the onset of civil war. Moreover, various units of analysis might be required to answer questions about the causes of different forms of violence. When analyzing the risk of secession, focusing on the subnational region is more profitable than focusing on the country, and it allows us to introduce new variables to the model, such as precise measures of interregional inequality, that might help explain the risk of secession in particular regions but may be unrelated to other forms of violence.

So my message in this article is not only that economic models of civil war must be expanded by bringing politics back in. In addition, I have outlined the need to combine several theories of violence and use both qualitative and quantitative methods to cumulatively construct a theory that establishes the boundaries of the concept of civil war. Instead of modeling simplistic distinctions between "greed" and "grievance," our theory-building efforts should be redirected toward understanding how different forms of violence are organized during escalating conflict. Such a theory is both more intellectually satisfying and more policy-relevant, as different interventions must be designed to address the risks and consequences of various forms of violence. To develop models to guide our policies, we must proceed interactively, complementing statistical inference with in-depth case knowledge.

Notes

1 These figures are based on Doyle and Sambanis 2003. Other data sets list different numbers of civil wars; data on deaths vary widely.

2 Collier and Hoeffler 2001.

3 *Rebellion* is CH's term; *insurgency* is FL's term.

4 In this article, the terms *micro* and *macro* are not used to distinguish microeconomic from macroeconomic models, but rather to set individual-level incentives and actions (the "micro" level) apart from social movements and opportunity structures (the "macro" level).

5 See Ballentine and Sherman 2003, a book of case studies that makes this same point.

6 Throughout this article, I cite revised drafts of case studies originally presented as part of the Case Study Project on Civil Wars in New Haven, Connecticut, in April 2002. In each instance, I include a reference listing for the relevant version of the paper, not necessarily the original. For the Caucasus study, I include both the 2002 and the 2003 versions, because revisions were extensive. See Zürcher et al. 2002 and Baev et al. 2003. (Please note that these are two versions of the same study; the authors changed the order of their names.)

7 The Case Study Project on Civil Wars began in the spring of 2000. Teams of country experts wrote the case studies; in most instances, an author from the country under consideration worked with an author from a U.S.-based institution. The following countries were included: Algeria, Azerbaijan, Bosnia, Burundi, Colombia, Democratic Republic of Congo, Georgia, Indonesia, Ivory Coast,

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Jamaica, Kenya, Lebanon, Macedonia, Mali, Mozambique, Nigeria, Russia, Senegal, Sierra Leone, Sudan, and United Kingdom (Northern Ireland). Some of these countries had more than one civil war. The following case studies were commissioned but never completed: Afghanistan, El Salvador, Moldova, Somalia, Sri Lanka, and Uganda.

8 The first guidelines given to authors are posted at www.yale.edu/unsy/civilwars/guidelines.htm. (More detailed instructions were given at two conferences—in Oslo, Norway, in June 2001, and in New Haven, Connecticut, in April 2002.) Go to pantheon.yale.edu/~ns237/index/research.html#Cases for a summary assessment of the models used in each case. The cases will be published in an edited volume (date to be determined).

9 The distinction was proposed in early versions of the CH model. It corresponds to the distinction between grievances and opportunity structures in the FL model.

10 External intervention is likely partially determined by other variables in the model. Thus, if it were added to the model, the estimation method would probably have to be adjusted and an instrumental-variables approach used. Such an approach might also be more appropriate for the current versions of the CH and FL models, since they include variables that may not be purely exogenous.

11 This is not simply an exercise intended to maximize the R^2 in quantitative models, though who would mind a higher R^2 if it could be achieved by adding significant, exogenous variables to the model?

12 There is not room in this article to present new quantitative tests of an expanded civil war model or even to present a complete new theory of civil war. This is the subject of a book-length manuscript that I am writing.

13 Exceptions are Sambanis 2001, Sambanis 2002, and Sambanis 2003b, in which I explore differences between ethnic and revolutionary civil wars and between civil war and politicide or genocide.

14 Horowitz 2001 casts doubt on the hypothesis that price shocks are associated with ethnic riots and refers to an inconclusive literature on the presumed association between price shocks and food riots. I have no evidence to add to the debate; I use this only as an example of a possible association that might differentiate forms of violence.

15 For a theory and evidence on the relationship between ethnic violence and the territorial concentration of ethnic groups, see Toft 2003.

16 See Sambanis and Milanovic 2004 for a theory of separatist conflict that focuses on regional differences in income and ethnic composition as determinants of the demand for self-determination.

17 See Herbst 2000 for a relevant argument. Herbst explains that state formation in Africa has evolved into a concept of a "core" territory that the state must control to remain viable. Peripheral regions are often excluded; and the military, weakened because of its competitive relationship with the state, allows political conflicts to turn into insurgencies much more easily in peripheral regions than in the "core" territories.

18 Harff 2003 identifies the significance of the elite ideology argument. Public statements should make identification of these elites relatively straightforward. All genocides also seem to occur in autocracies. Out of 244 country-years of politicide from 1960 through 1999 in Harff's data set, only Sudan from 1965 through 1968 and 1986 through 1988 and Guatemala in 1996 can be labeled deep democracies with a net democracy-autocracy score of 7/10 or higher in the Polity IV database (see Marshall and Jaggers 2000). And these are cases of civil war, not genocide, which is usually understood as intentional targeting of an ethno-religious group with the intent to destroy that group.

19 A concrete discussion of policy interventions that better target civil war as compared to other forms of political violence must await the results of empirical studies that identify differences in the causal paths leading to different forms of political violence.

20 On the definition and measurement of civil war and empirical differences that result from different definitions, see Sambanis 2003a.

21 The theoretical underpinnings of this idea are modeled by Grossman 1991; Hirshleifer 1989; Hirshleifer 1995; Konrad and Skaperdas 1999.

22 Hirshleifer 1995, 172. The credibility argument is developed for interstate war (Fearon 1995) and civil war (Skaperdas 2001).

23 See Collier 2000. Collier and Hoeffler write, "On the literal greed interpretation the extortion of primary commodity exports will occur where it is profitable, and the organizations which perpetrate this extortion will need to take the form of a rebellion." Collier and Hoeffler 2001, 3.

24 For example, Gurr 2000, chapter 3.

25 For instance, CH focuses on the looting of natural resources as both a motive for war and a means to sustain it. FL agrees with the hypothesis but contradicts CH's empirical findings. CH finds that diaspora funding only influences war duration and recurrence, not the risk of initial war onset. Both models use a dummy variable denoting the Cold War period as a proxy for external support from superpowers, though who receives such support (the government or the rebels) is not specified—and this factor should influence civil war risk.

26 China and the Soviet Union (under Stalin) are obvious exceptions to FLs' argument, though there are also many examples of unruly small countries (Cyprus, Georgia, Azerbaijan, and others).

27 These results were first reported by Hegre et al. 2001 and can also be found in the literature on social movements.

28 FL analyzes data per year from 1945 through 1999; CH analyzes five-year panel data from 1960 through 1999.

29 CH finds a highly significant negative relationship between time at peace and the risk of civil war onset, but FL does not.

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30 Ragin 1987.
31 On the Ivory Coast, see Azam and Koidou 2003. On Indonesia, see Ross 2003. On Nigeria, see Zinn 2003.
32 On this selection rule for qualitative research designs, see King et al. 1994.
33 Including cases of no war resembles John Stuart Mill's *indirect method of difference*. See Ragin 1987. Each case study effectively includes no-war comparisons—i.e., periods of war and no war in each country—and the authors explain the transition across these two states.
34 Even where it generates false negative or false positive predictions, the model is still technically correct because, strictly speaking, it does not predict war, but rather the probability of war. Thus, war may not have happened in high-risk countries for reasons that remain outside the model. But the same is true for "accurate" statistical predictions: war might occur in high-risk countries for reasons that the model does not consider.
35 Of course, all of this assumes that the case studies themselves are relatively free of measurement error and other biases. This is one of the reasons why commissioning experts to write the cases may be superior to simply summarizing existing case studies. Ultimately, we can check the usefulness of the case studies when hypotheses and model refinements based on the cases are taken back to the data for further testing.
36 All selection criteria were controlled for in the CH model, and the fact that the case studies analyzed long periods in each country's history ensures over-time variation in the independent variables.
37 Fearon and Laitin (2004) are currently engaged in a similar case study project, based on a random selection of cases.
38 Nonlinearities imply that the theorized linear relationship between the dependent variable and the independent variable does not apply to the entire sample. If ethnic identity matters in different ways in developed and less-developed countries (cf. Horowitz 1985), then adding interaction terms is one way to properly explore conditional effects. When such effects are present, a stratified sampling method is correct if cases are used for hypothesis testing.
39 A case-control design was used by Esty et al. 1995 in their study of state failure.
40 Baev et al. 2003.
41 "Two units are homogeneous when the expected values of the dependent variables from each unit are the same when our explanatory variable takes on a particular value." King et al. 1994, 91. In other words, in a sample that includes homogeneous observations, if GDP (or any other explanatory variable) changed by, say, 10 percent and all other variables in the model were held constant, the expected risk of war should change by an equal amount in all cases.
42 Ragin 1987. This is an exploratory, not formal, test of the assumption.
43 These are sometimes called "within-systems relationships." See Przeworski and Teune 1970, 57–9; Ragin 1987.
44 Collier and Hoeffler (2002a) show that there are no statistically significant patterns in civil war onset in different regions. There is, however, a heavier representation of African wars in our sample.
45 This is because of the well-known "degrees of freedom" problem in small-N studies—i.e., the problem of having too few observations of outcomes to estimate the effects of several independent variables.
46 Mechanisms have been defined as a "delimited class of events that alter relations among specified sets of elements in identical or closely similar ways over a variety of situations." McAdam et al. 2001, 24.
47 Consider another medical example. Assume that obesity causes clogging of the arteries, which can lead to a heart attack, which can cause death. One could therefore say that heart attack is the mechanism through which obesity causes death. But assume, also, that obesity is the result of bad eating habits, which are at least partially the result of socialization at home by one's parents. Therefore, it is theoretically consistent to argue that obesity is the mechanism through which bad parenting causes death. That statement, however, has very little prima facie credibility. Put more formally, bad parenting should have little power as an explanatory variable in a model that tries to describe death as a function of a person's physical characteristics.
48 Kalyvas 2003 focuses on variation in micro-level actions to challenge the idea that the ontology of a civil war can be established on the basis of macro-level identities. In my analysis, ontology is a broader concept, referring to civil war as an analytically and empirically meaningful category that is distinct from other forms of political violence.
49 Developing such a theory is the subject of my ongoing work.
50 A counterargument might be that once the preconditions for some political violence are established, the organizational form of violence is entirely contingent. If that is true, then focusing on civil wars as a distinct outcome is not meaningful, and we must analyze all political violence as a whole. Empirical testing of such a theory must be based on a data set that pools all events of political violence, not just civil wars.
51 It is true that the process of using the case studies to identify variables to build theory is not systematic in the sense that there is no rule specifying that a variable must appear *x* number of times before we should consider adding it to the statistical model. But the same might be said of any process of theorizing: how many times must a theorist who is developing a deductive model of a phenomenon observe the phenomenon before he/she decides to model it? It is important that case-study authors who suggested rival explanations of outcomes in their countries did so while being guided by the CH model. But in

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selecting variables from the case studies to develop the theory further, it may be possible for several, potentially conflicting, theories to be developed, depending on the reader's interpretation of the case studies. In the end, these competing theories can be sorted out empirically when we take them back to the data.

52 Ross 2003.
53 See Woodwell 2003 regarding the deterrent effect of the Royal Ulster Constabulary's strength of 13,500 members.
54 CH and FL do not code a war in Kenya. Other data sets (Doyle and Sambanis 2003) code a war from 1991 to 1993 because of the state's indirect involvement in the violence, but this is an ambiguous case and could also be classified as intercommunal violence.
55 Kimenyi and Ndung'u 2002.
56 Makdisi and Sadaka 2002.
57 Darden 2002.
58 Krueger and Malečková 2003. The authors present micro-level results but also cross-national results consistent with this finding.
59 Davies and Fofana 2002.
60 Kalyvas and Sambanis 2003.
61 Ross 2003.
62 All former Soviet states had drastically falling growth rates during the collapse of the Soviet Union. It is unclear how much of the growth decline in Georgia, Azerbaijan, and Chechnya was a result of the war and how much was a result of Soviet collapse.
63 Ngaruko and Nkurunziza 2002.
64 CH finds that the risk of war is 50 percent greater immediately after the previous war ends than in other time periods.
65 Baev et al. 2003.
66 Ndikumana and Emizet 2003.
67 In a more recent paper, CH presents such a disaggregated test. See Collier and Hoeffler 2002b.
68 Zinn 2003. This war is omitted from many data sets, but it meets the case studies' definitional criteria (ongoing violence from 1980 to 1984 that caused 5,646 deaths).
69 Humphreys and ag Mohamed 2003. The Mouvement des Forces Démocratiques de Casamance, led by veterans from the Senegalese army, staged protests in 1983 for independence of the Casamance region. Violence increased in the late 1980s and turned into a civil war in the 1990s.
70 This may explain the CH model's false negative prediction of the Biafran war. Nigeria's primary commodity export share of GDP increased to 38 percent between 1990 and 1994. It was 12.3 percent from 1965 to 1969, much lower than the highest risk level. Zinn 2003.
71 The conflict had actually started earlier, in 1988, when Azerbaijan was still part of the Soviet Union.
72 Zürcher et al. 2002.
73 Ibid., 63.
74 Burundi has a high ratio of primary commodity exports to GDP because of coffee exports. No study has argued that control of coffee production is related to the Burundi war.
75 All of these countries had lower resource dependence than the population mean.
76 Some data sets do not code a civil war in the United Kingdom. Even though the 1,000-aggregate threshold of deaths has certainly been exceeded, fighting has been sporadic, which has led to a slow accumulation of deaths in violence that some label as terrorism.
77 At the same time, if the CH model had accounted for high unemployment among Catholic men, the probability estimate for a rebellion among Catholics would have increased because of easier rebel recruitment, consistent with CH's predictions.
78 In Sambanis 2001, I first made the argument that the trade-off between the economic costs of rebellion and the gains of political and cultural freedom may be different in pure ethnic conflicts as compared to nonethnic conflicts.
79 Lowi 2003.
80 Licklider 1995; Sambanis 2002.
81 Collier and Hoeffler 2001.
82 Kalyvas 2002.
83 Mueller 2001.
84 Brubaker and Laitin 1998.
85 Ngaruko and Nkurunziza 2002.
86 Makdisi and Sadaka 2002.
87 Another test of the homogeneity assumption would be to see whether explanatory variables (e.g., democracy) have different effects on the risk of civil war during different periods (e.g., pre- and post-Cold War).
88 See Sambanis 2003b for a more formal analysis of the differences between genocide and civil war.
89 More formally, the coefficient estimates for explanatory variables in the civil war model may be biased.
90 In Harff's (2003) list of politicides and civil wars, I could find only one case (Chile, 1973–1976) that took place outside civil war.
91 The exception to this rule is when the national army becomes split between warring groups, as in Bosnia.
92 Sanchez et al. 2003.
93 Baev et al. 2003, 23–4.
94 Ross 2003.
95 Baev et al. 2003.
96 Humphreys and ag Mohamed 2003.
97 See Sambanis and Zinn 2003 for empirical evidence in wars fought over self-determination.
98 Ngaruko and Nkurunziza 2002, 36.
99 Makdisi and Sadaka 2002.
100 Ndikumana and Emizet 2003.
101 Kalyvas 2002; Fearon and Laitin 2003.
102 Glenny 2001.
103 Ngaruko and Nkurunziza 2002.
104 Weinstein and Francisco 2002.
105 The FL model is less compromised by this finding, because its focus on state strength allows it to claim

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that a strong state should be able to prevent forced rebel recruitment.
106 Petersen 2002.
107 White 1989.
108 Krueger and Malečková 2003.
109 Kalyvas 2003.
110 On the Greek civil war, see Kalyvas 2000. On the link between Yugoslavia's civil wars and memories of atrocities since World War II, see Glenny 2001. Iraq in 2004 can be coded as a civil war, with the United States occupying the government.
111 On the complexities of defining and measuring civil war, see Sambanis 2003a.
112 Singer and Small 1994.
113 The CH model under-predicts the risk of civil war onset in Burundi, Indonesia, Lebanon, and Sierra Leone, because of the narrow measurement of the "peacetime" variable.
114 Rubio 1999; Sanchez et al. 2003.
115 Gambetta 1993.
116 Andrienko and Shelley 2003.
117 Davies and Fofana 2002.
118 On neighborhood effects, see Brown 1996; Lake and Rothchild 1998; Sambanis 2001.
119 Ross 2003.
120 Humphreys and ag Mohamed 2003.
121 Baev et al. 2003.
122 Ngaruko and Nkurunziza 2002.
123 See Sambanis 2001 and Gleditsch 2003.
124 Recent empirical work at the dyadic level suggests that the presence of common ethnic groups across national borders increases the risk that domestic ethnic conflicts will become internationalized. See Woodwell 2004; see also Gurr 2000.
125 Lund 2003.
126 Ndikumana and Emizet 2003.
127 This was one of the most common observations in most of our case studies.
128 Weinstein and Francisco 2002.
129 Intervention cannot succeed without local support. In Mozambique, FRELIMO's failed socialist agricultural policies, intense repression, and southern political dominance combined to create a favorable climate for external agitation to civil war.
130 Ndikumana and Emizet 2003.
131 Kalyvas and Sambanis 2003.
132 Baev et al. 2003.
133 Davies and Fofana 2002.
134 Gurr 2000.
135 Lichbach 1987 and Tarrow 1989.
136 Ngaruko and Nkurunziza 2002.
137 This result is identified in FL, but not in CH. See also Snyder 2000.
138 Ross 2003.
139 Humphreys and ag Mohamed 2003.
140 Woodwell 2003.
141 Ndikumana and Emizet 2003.
142 John Garang de Mabior on the founding of the Sudan People's Liberation Army (SPLA) and the Sudan People's Liberation Movement (SPLM). Quoted in Ali et al. 2003, 1.

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