

## **Expanding Economic Models of Civil War Using Case Studies**

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### **Abstract**

This paper draws on a comparative case study design to refine and expand formal-quantitative models of civil war. I draw on twenty-one case studies of civil war onset and avoidance to discuss shortcomings in prominent economic models of civil war (i.e. models that rely heavily on economic variables). These shortcomings include measurement error, unit heterogeneity, model misspecification, and lack of clarity about causal mechanisms. The combination of quantitative and qualitative research generates new insights. I argue that the greed-grievance distinction that underlies the economic models is misguided and that civil war should not be analyzed in isolation, as a discrete phenomenon, but should rather be explained as one phase in a cycle of violence. Economic models of civil war are based on micro-level theories that cannot distinguish effectively between civil war and other forms of political violence. I argue that the concept of civil war as a distinct category must be based on a unique mapping of micro-level motives to macro-level structures. If we cannot understand why we get civil war as compared to other forms of organized political violence, then we really do not understand civil war

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## 1. Introduction

More than one hundred-forty civil wars around the world since 1945 have killed approximately 20 million people and displaced 67 million.<sup>1</sup> Despite this massive scale of human misery, the academic community had not paid much attention to the problem of civil war until very recently. Two important papers by Collier and Hoeffler (2000, henceforth CH) and Fearon and Laitin (2003, henceforth FL) have generated much interest in the question of why civil wars occur. These papers present the counterintuitive finding that civil wars are not caused by ethnic division or political grievances. Rather, civil war is a function of the opportunity structure for the organization of rebellion or insurgency.<sup>2</sup> Both papers use macro-level data to test hypotheses about civil war that are based on ideas about micro-level behavior and identify a set of statistically significant correlations between civil war onset and a number of explanatory variables.

The gap between micro-level behavior and macro-level explanation is large. It is magnified when the micro-macro relationships are studied solely through cross-national statistical analyses. What is often lost in such studies is information about causal pathways that link outcomes with causes. In this paper, I will argue that a combination of statistical and case study work is necessary to better understand the processes that lead societies to civil war. In so doing, I will argue that, despite large amounts of “noise” in micro-level data about violent behavior in civil war, we can still make useful inferences about the organization, causes, and consequences of violence at the macro level, but to do so, we cannot rely on a single methodological approach.

Given the importance of the CH and FL models, I use them as my starting point 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. I use cases to develop – not test – theory and to qualify causal inferences drawn from quantitative data analysis. I draw on a set of twenty-one case studies of civil war onset and war avoidance that systematically applied the CH model.<sup>3</sup> Two online supplements to this paper include summaries of the cases and information about the project’s research design.<sup>4</sup>

The case studies make several contributions. First, they help us identify several causal mechanisms through which independent variables (IVs) in economic models influence the dependent variable (DV) –i.e. the risk of civil war onset. It quickly becomes clear that the distinction between “greed” and “grievance” that has been proposed in recent scholarship on civil war is illusory as greed and grievance are usually shades of the same problem.<sup>5</sup> Second, the cases reveal that the CH and FL models are often right for the wrong reasons (also wrong for the wrong reasons). At the heart of this problem is the mis-measurement of key variables. I offer some examples and suggest ways to improve the fit between the theory and the data. Third, the cases help us identify new variables that might explain civil war but are omitted from CH and FL. An example of such a variable is external intervention. Adding such variables to quantitative models might reduce the risk of omitted variable bias and facilitate inductive theory-building.<sup>6</sup> Fourth, the cases suggest substantial unit heterogeneity. This observation informs a new theoretical contribution of this paper. I will argue that focusing on civil war outcomes as the result of deep-seated and hardly-changing structural conditions is not as useful as a theory that is sensitive to the links across different forms of political violence and analyzes the dynamics of conflict escalation.

Current practice in the literature is to pool events of civil war without exploring if they in fact constitute homogeneous observations.<sup>7</sup> At the same time, civil wars are thought to be different from other forms of political violence. I will argue that current models of civil war do not allow us to make that determination. If civil wars are different from other violence, then the policy interventions needed to prevent them will differ from interventions designed to prevent terrorism or other violence. We need to understand how and when we are likely to get a civil war as compared to another form of violence so as to develop appropriate interventions. If civil wars are not different, then by analyzing them in isolation we may be getting biased or inefficient statistical estimates, as we arbitrarily restrict our analysis to a sub-sample of the data. Either way, to explain war outbreak, we must develop a process-driven explanation of conflict escalation.

It is not my intention here to weigh CH against FL and pronounce one the winner. Nor will I spend much time discussing these papers. They are useful starting points as I try to outline a new approach to 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 are often (mistakenly) considered as substitutes rather than complements in political science.

The paper is organized in six sections, beyond the introduction. Section 2 briefly summarizes the CH and FL models to give non-specialists some context within which to evaluate my arguments. Section 3 presents the case study project's research design. Sections 4 through 6 use examples from the cases to discuss problems of measurement error, unit heterogeneity, and omitted variables in the quantitative studies. I focus on mechanisms that link explanatory variables to civil war by explaining aspects of the

process of conflict escalation. Section 7, the conclusion, sums up my arguments and points to a new direction in the study of civil war.

## 2. A Primer on the State of Civil War Theory

The CH model of war onset is based on the idea of an individual tradeoff between production and appropriation.<sup>8</sup> According to CH, people decide to become rebels after weighing the economic opportunity cost of violence against the expected utility of violence. War is inefficient from a Coasian perspective because it is costly and reduces the net value of rents available to the state. Yet, we observe war due to “three interacting determinants: *preferences*, *opportunities*, and *perceptions*” and because it is hard to negotiate credible peaceful settlements without the use of force.<sup>9</sup> 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.<sup>10</sup> Grievance, in this model, is simply rhetoric that is used to legitimize a person’s decision to engage in appropriation rather than production. The instrumental factor explaining the onset 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 the looting of natural resources, extortion of local population, and support from ethnic diasporas. Insurgency is less likely when the state is strong or when the economic opportunity costs of rebellion are high. State strength is not well theorized in the model and is approximated by the country’s economic strength (GDP per capita). The expectation is that relatively richer states will be bureaucratically more efficient and will have the resources needed to

defend themselves against a rebellion. Since insurgency is more likely as the supply of rebel labor increases, strong states can reduce the available labor supply by decreasing the net expected gains from rebellion (by reducing the probability that the rebellion will be successful). Rebel labor supply should also decline as the economic opportunity costs of rebellion increase and in richer states, time devoted to production (rather than appropriation) pays more than in poor states. In sum, a country's economic opportunity structure (income level and growth, and economic structure) determines the "supply" of insurgency for a given level of insurgency "demand."

The FL model shares much of the same logic. FL reject primordialism, nationalism, modernization, and other grievance-based explanations for an opportunity structure argument that explains civil war as a function of state strength (also measured by GDP per capita) and by the technology of insurgency (rough terrain for the rebels to hide, diaspora funding). FL dismiss CH's resource predation argument. Although FL find that oil-dependent states are more prone to civil war, they interpret this as a weak-state effect, since oil dependence frequently causes weak state structures. The FL model is more state-centric and includes a host of variables that measure state capacity (political instability, new state, regime type). However, the differences between CH and FL are mainly differences of interpretation, as the same proxy variables are used to test macro-level hypotheses that are consistent with the micro-level logic underlying both models.

The concept of an opportunity structure for rebellion in CH and FL is in fact very similar to earlier important works, such as Gurr's work on transient risk factors of rebellion.<sup>11</sup> What is new here is the quantitative empirical testing of the models, which suggests several important and robust results. Both CH and FL find that GDP lowers the

risk of civil war, which CH take to mean that poverty exacerbates the risk of civil war, while FL interpret as evidence supporting their state weakness theory. In contrast to CH, FL find 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 the risk of war by increasing its opportunity cost, though FL disagree with these findings. There is more agreement on the question of technology for insurgency. Both CH and FL find that countries with mountainous terrain and high degrees of external financial support have higher risk of civil war. They disagree on other ways of financing rebellion, as CH focus on the looting of natural resources as both a motive for war and a means to sustain war. Both find that civil war is more likely in populous countries, though they disagree on the interpretation of this result. CH argue that large populations are more likely to include aggrieved groups, whereas FL argue that states have a harder time controlling large populations.<sup>12</sup> 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, while ethnic dominance increases war risk according to CH. Both seem to agree that countries in the middle of the democracy-autocracy spectrum and countries with political instability are more prone to civil war.<sup>13</sup>

CH and FL get their results from pooled logit analysis of panel data covering roughly the same set of countries and much of the same period.<sup>14</sup> Each model is estimated on a slightly different dependent variable. Both analyze war onset, but CH drop observations of ongoing war, while FL code these periods as “0’s” so as not to drop war onsets that occur in countries with an ongoing civil war.<sup>15</sup> With this telegraphic review in mind, let us now turn to the case studies.

### 3. Case Study Methodology

Most researchers who work with quantitative methods are averse to doing case studies. Their presumption is that case studies cannot test theory because they suffer from selection bias, omitted variable bias, endogeneity, and measurement error.<sup>16</sup> In this section, I address these concerns in the context of describing our project's methodology. I will argue in favor of combining case studies and quantitative analysis.

The project's main purpose was to improve causal inferences drawn on the basis of the CH model. The CH model was tested statistically, so it was not necessary to re-test it using case studies. Testing a large number of hypotheses using case studies would have been problematic, due to the well-known problem of statistical identification associated with case study designs.<sup>17</sup> Thus, the cases tried to complement our theory, refine our empirical measures, and identify causal mechanisms underlying the theory.

Since both CH and FL test a micro-level logic of rebellion against macro-level data, it is likely that their empirical findings do not correspond to a fully specified causal theory. Case studies can help us tease out causal mechanisms and explore interactions among independent variables. To do so systematically, we used a structured-focused comparison design, exploring the fit of the CH theory to each case. Country experts were asked to write narratives that addressed a common set of questions about the onset and organization of rebellion. Since all narratives addressed the same questions, our research design allows us to make comparisons across cases and draw generalizations.

*Unit of observation:* As in the CH and FL statistical tests, the unit of observation for the case studies was the country – or, more accurately, several periods of peace and

war within each country. Some countries had recurrent wars and the authors discussed all periods of war, exploring the connections between these events. In effect, most case studies provide us with several observations. For example, the Indonesia study focuses on patterns of war and peace in Aceh over eight five-year periods and can therefore be considered as a study with 8 observations (2 observations of war and 6 of no-war). The Nigeria study analyzes several periods that included two wars, while the Ivory Coast study analyzes three periods, each with no war, but with different structure of war risk.

*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 these outcomes, we did not pick cases with a view to predicting outcomes. Rather, we picked cases with two goals in mind: first, we wanted to maintain sufficient variation among the independent variables (IVs);<sup>18</sup> and, second, we wanted to include both cases that the model predicted well and cases that the model predicted poorly.<sup>19</sup> Both false positives and false negatives were considered, i.e. cases of war onset that were not predicted by the CH model and cases (periods) of peace in countries that the CH model predicted high risk of civil war. The cases help sort out which of these poor predictions were due to idiosyncratic reasons and which were due to systematic flaws in the theoretical model, operational measure, or estimation method. In those cases where the CH and FL models generated accurate predictions, the main function of the cases was to trace the mechanisms linking the IVs to the DV so we could better link the micro-level and macro-level dimensions underlying the CH and FL theories. These selection criteria and the large number of cases in our project allow us to build theory inductively and the new theory can later be taken back to the data for further testing.

We picked democracies and autocracies; countries with long and short histories of violence; high and low levels of ethnic fragmentation; high and low natural resource dependence; but did not apply a selection criterion that was not already controlled for in the CH model. Selecting a random sample of cases was not necessary, since we did not intend to use the cases to test the theory. Random selection from the CH or FL datasets might have resulted in a sample that predominantly included cases of no-war, given that civil war is relatively rare. It could also result in a sample with no significant variation in the IVs. We could have avoided the first (but not the second) problem by sampling more heavily on cases of war, but non-linearities that may be present in the data could have complicated the sampling process.<sup>20</sup>

The presence of complex pathways linking the IVs to the DV is sometimes best studied in case studies.<sup>21</sup> For example, it seems to be the case that civil wars occur predominantly in poorer countries. In that case, a case-control design that selected, for example, three no-war cases for every case of war<sup>22</sup> would have included far too many middle-to-high-income countries to allow us to disentangle the relationship between the DV and other IVs at different levels of income. Properly controlling for this problem in the context of a case study design would entail a highly stratified selection rule and a sample size even larger than the one we had in our project.

A comparison of Chechnya and Dagestan (both parts of Russia) offers an example of the complicated causal path linking political instability to civil war. Both regions were adversely affected by the political instability that followed the collapse of the USSR. In Chechnya, political institutions collapsed at the hands of Chechen nationalists, led by General Dzhokhar Dudayev, who pursued secession on behalf of the Chechen majority. In

Dagestan, no titular majority group existed and the national independence movement acted through pre-existing political institutions that represented 54 regional Soviets, or parliaments. Only 39 Soviets were in favor of secession and political elites decided that secession was too costly.<sup>23</sup> Thus, political instability had different effects in these two cases as they were filtered through regional political institutions and were conditioned by the level of ethnic composition and by actions taken by nationalist elites.

*Unit Heterogeneity:* The CH and FL tests are based on a pooled sample of all civil wars, which is based on a strong assumption of unit homogeneity.<sup>24</sup> If this assumption is violated, it can bias causal inferences from the model. Case studies are a good way to test the validity of the homogeneity assumption.<sup>25</sup>

If there is substantial unit heterogeneity, then tests should be restricted to particular regions or ranges of key variables across which outcomes differ significantly.<sup>26</sup> Such an approach is useful to analyze “within-systems relationships.”<sup>27</sup> We did not adopt such a method in our case selection and sought to have broad geographic representation roughly corresponding to the geographic incidence of civil war since both CH and FL put forth widely generalizable propositions.<sup>28</sup>

*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 IVs with the DV. This also makes it easier to deal with the problems of endogeneity and selection that likely affect the results of both CH and FL. More plausible mechanisms can be identified by case studies than were identified by the statistical tests, though testing the significance of these mechanisms and rank-ordering them is probably better done by going back to the statistical models.<sup>29</sup>

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.”<sup>30</sup> I would argue that mechanisms are, in effect, variables that operate in sequence. Mechanisms can also be outcomes: depending on the level of aggregation at which a theory is built, different outcomes become intervening variables, which connect a set of variables to other outcomes. Consider the theory that HIV causes AIDS and AIDS causes death. HIV infection can be an IV 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. sharing needles among intravenous drug users) might cause death. It is therefore up to the analyst to determine the level of aggregation at which s/he formulates a theory and, according to that level of aggregation, one could proceed to identify the mechanisms linking IVs to the DV. There could be an almost infinite regression towards 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.<sup>31</sup>

Thus, we come back to the distinction between micro-level and macro-level research on civil wars. It is true, as argued by Stathis Kalyvas in an earlier issue of this journal, that there is wild variation in motives for violence at the micro-level and that there is often not a direct correspondence between micro-level and macro-level

determinants of civil war (i.e. a war over ideology might be fought both by ideologues and criminals, each fighting for a different reason).<sup>32</sup> But analysis at different levels of social conflict will necessarily reveal different causal patterns. The interesting question is how and why different private motives for violence are all disciplined into a single organizational form or expression – such as civil war. This implies either that the variation at the micro-level is irrelevant to the question of war onset, or that civil war is such an aggregate concept that it is not a useful analytical category (if civil war includes coups, riots, gang violence, crime, genocide, etc). If what we are trying to explain is the outbreak of civil war, then what is of interest is the process through which divergent incentives and myriad personal calculations are all channeled towards the same outcome. Given that the micro-economic theories proposed by CH and FL could be consistent with violence of various forms, and not just civil war, it is important to consider a wider array of micro-level theories, including how emotions, ideology, revenge, or private material interest interact to produce collective action that culminates in a civil war.

Ultimately, it is the interaction between micro-motives and macro-structures 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, it may be that terrorism, coups, or riots eventually lead to civil war. To understand civil war, this implies that we have to understand which particular mapping of micro-motives and macro-structures led to civil war as opposed to other forms of violence. If we cannot know why we get civil war as compared to other violence, then we really do not understand civil war.

There is potentially a hierarchy among mechanisms that explain civil war, but such a hierarchy might be hard to establish through case studies. But even without ordering these various explanations by exploring their frequency in the data, we can use case studies to explain how the variables in the CH and FL models lead to civil war. In the cases, our attention frequently shifts from individual mechanisms to processes, which are understood as either families of mechanisms or dynamic interactions over time between explanatory variables. I provide examples of this in the paper. Given space constraints, I explore only on one process – the escalation of violence— to demonstrate how state repression can lead to civil war. I also explore the impact of neighborhood effects and external intervention in the process of conflict escalation and civil war onset.

#### 4. 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 in these datasets. CH and FL use a number of proxy variables that the case study project frequently found to be problematic. This casts doubt on some of the CH and FL causal inferences based on their empirical results.

##### *Economic Variables: GDP, Growth, Education*

The key measures of opportunity costs in the CH model are GDP per capita and secondary education. Many countries in our project had low and declining income in the years preceding the start of the war. In Sierra Leone, real per capita income was just over \$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% in the province of Aceh in 1998, right before a war started. The oil and gas sector, which accounted for 65% of Aceh's GDP

contracted by almost a fourth during the financial crisis.<sup>33</sup> In Nigeria, recession in the late 1970s caused unemployment to double to more than 20% 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 former Yugoslavia. These patterns are in line with the CH opportunity cost argument.

However, 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. The Purchasing Power Parity (PPP)-adjusted measure of GDP used by CH is more consistent with their opportunity cost argument, while the constant-dollar GDP figures used by FL are more consistent with their state strength argument, since they describe the overall size of the economy. But the government's size should also be correlated with country size or population size and need not denote the degree to which the state penetrates the society and controls its territory. For the CH opportunity argument, unemployment levels would have been a more direct measure of potential rebel supply. In pre-war Yugoslavia, income per capita was two or three times the average for war-affected countries, but unemployment surged and, in some regions reached 40% of the adult population. Using unemployment rates (especially region-specific rates) could help distinguish the CH opportunity cost argument from the FL state strength argument that are now conflated by the use of GDP as a proxy variable.

The interpretation of GDP as a measure of state strength is relevant to several cases. Woodwell's study of the war in Northern Ireland is a good example. He describes a protracted insurgency that remained low-intensity precisely because it took place in a highly developed country. According to Woodwell, part of the reason that the

“Troubles” and their aftermath did not escalate into a larger war had to do with the strength of the British state, which forced the insurgents to adopt a strategy of low-level urban violence and terrorism.<sup>34</sup> A larger insurgency would have triggered massive retaliation from the British government.

What this explanation probably leaves out is the role of the civil society and public opinion in the U.K. and neighboring Ireland. The state strength argument should probably be conditioned by the strength of civil society in each country. A more intense campaign by the 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 like the ones used by the Russian state in the war in Chechnya (i.e. bombing Chechnya’s capital, Grozny) are not viable – indeed they are unthinkable. But it is hard, on the basis of the FL model to disentangle the effects of British state strength and implications of the state’s liberal-democratic characteristics.

A comparison with another strong state – Kenya—might help disentangle these effects. Here, we have a weak economy, a weak civil society and a strong authoritarian state. The state’s ability to effectively repress its opponents is unconstrained and this could explain how the state was able to undercut opposition in its early stages. Despite strong ethnic antagonism, significant electoral violence, and a coup attempt in August 1982, no civil war has occurred in Kenya due mainly to the state’s strength and authoritarianism.<sup>35</sup> The mechanism of exercising state control over Kenyan territory was corruption.<sup>36</sup> The government used local police forces to violently repress local opposition groups that could not be bought off and rewarded government supporters with

gifts of public land. The problem here is that a low GDP is not a good measure of the Kenyan state's capacity to prevent a civil war.

Turning to the CH argument about education, data on schooling seem to fit the CH model in many countries, especially in Africa (consider that there were virtually no educated Congolese at the time of independence and right before the start of the first civil war). But there may be a regional effect at play here, since in other countries –Cyprus, Yugoslavia, Georgia, Russia— schooling rates were high. Lebanon, had a long, bloody civil war, as well as one of the highest education levels in the Arab world with a 60% adult literacy rate (compare to 15% in Iraq) in 1950s-60s and a school enrollment ratio of 76% in the 1950s.<sup>37</sup> Saudi Arabia, by contrast, had a schooling rate of 4%, but no war.

What is missing here is an explanation of *how* schooling might influence civil war risk. CH simply focus on the correlation between schooling and economic opportunity. While this might be a reasonable argument for some countries, it need not apply to countries with rapidly declining economic growth and rising unemployment. More importantly, the CH schooling argument is missing a close-up look at what is being taught in schools. In many countries, the curriculum is the primary mechanism of inculcating children with nationalist ideology. It is not surprising that CH do not focus on this micro-level mechanism because they dismiss the importance of nationalist ideology as a significant motive for civil war occurrence. But a detailed theoretical and historical argument has been developed that demonstrates a close correlation between nationalist education in schools and the persistence of nationalist ideology.<sup>38</sup> That argument might go a long way towards explaining cases such as Lebanon, where sectarian education fueled the war by nurturing ideologies of intolerance. Indeed, a

recent study on terrorism found that Hizbollah recruits in Lebanon were drawn among the ranks of the highly educated and this pattern also seems to hold cross-nationally.<sup>39</sup> Given the proximity of events of terrorism and civil war in Lebanon, it is incumbent upon theorists of civil war to explain the contrasting results on education.

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.<sup>40</sup> Yugoslavia's growth rate declined 15-20% from 1988-1992, fueling social unrest.<sup>41</sup> 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 model 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, rapid growth indirectly reinvigorated the GAM – the Acehenese rebel movement—because it led to the expansion of the extractive resource industries in the region and an increase in the number of migrants, leading to land seizures in Aceh.<sup>42</sup> Thus, government policies that were 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 *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 those studies using

datasets that code civil war onset during the year that deaths cross the 1,000 threshold (as is common in the literature), while armed conflict might have already been occurring for several years. Thus, quantitative models should consider modeling the endogeneity of economic growth to civil war. Political conflict and violence in Caucasian states caused massive drops in income. Georgia's GDP per capita dropped from approximately \$3,670 in 1991 to somewhere between \$777 and \$913 in 1997.<sup>43</sup> In Azerbaijan, GDP fell from around \$4,400 per capita in 1985 to around \$400 in 1996 and rose to \$510 in 1999. In the 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 was at the time of independence in 1960. In Burundi, another country with recurrent civil wars, GDP per capita has fallen by half in the 1990s, from \$211 in 1991 to \$110 in 1999.<sup>44</sup> If at least some of these declines were due to the process leading to the onset of 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 to economic conditions, the CH theory would predict that the risk of civil war would increase as income fell. This argument is consistent with evidence that the risk of war recurrence is far greater immediately after the end of a war than several periods later.<sup>45</sup> Declining income can be the mechanism through which time at war increases the risk of new wars in the future. If we interpreted GDP per capita as a measure of state strength, we would reach a similar conclusion, as declining GDP would imply declining strength, which would increase the risk of a new war. This suggests a specification change for the CH model: adding an interaction term between GDP and ongoing war to a model of civil war onset would be able to measure such an effect. Of course, the effect of ongoing civil war on the risk of a new war

breaking out in the country is neglected in the CH model, as the authors drop ongoing periods of war, ignoring the feedback effects mentioned above. If we instead 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 to periods of peace. A relevant argument put forth in the case study on Georgia is that the depletion of national income due to the war may have discouraged a long war duration as the available “loot” shrunk, making war unprofitable.<sup>46</sup>

The case studies point to the need to refine our empirical measures so as to identify causal pathways that explain the association between income level and war. Such refinement is even more necessary to sort out the argument on resource dependence.

#### *Resource Predation & Primary Commodity Exports*

The resource predation hypothesis is a cornerstone of the CH greed model of civil war. The DRC is perhaps the best example of this argument. Five Congolese rebellions originated in the resource-rich regions of Katanga, Kivu, and Kasai.<sup>47</sup> Zaire (DRC) has 50% the world’s cobalt production, 30% of the world diamond production, 20% of copper production; and sizable deposits of gold and tin and most of this is concentrated in the Eastern provinces. Mineral exports equal 25% of GDP.

However, empirical tests of the resource hypothesis are limited by the fact that CH measure resource dependence as the ratio of primary commodity exports over GDP. Using this proxy, CH find that the risk of war onset is maximized when the share of primary commodity exports to GDP is around 25%. 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 disaggregate the components of the primary commodity exports and focus on easily lootable usual suspects such as diamonds, timber, or gold.

The case studies uncover a serious problem for large-N testing of the resource predation argument. Often, we find resource-dependent countries with civil wars that, upon close inspection, had nothing to do with these resources. The Maitatsine rebellion in Nigeria in the 1980s took place in an oil-rich and oil-dependent country, but the rebellion was not financed by natural resource rents. The rebels were in fact recruited through ideological indoctrination and Koranic teaching, drawn from the ranks of the homeless, refugees and unemployed. They used primitive weapons and their limited finances came from hordes of beggars, small-scale theft and profits from sales of charms and medicines.<sup>48</sup> In Senegal, the FL model's prediction of civil war in the 1990s seem correct, given that the country is coded as an oil exporter. But Senegal does not have a significant oil production and merely re-exports oil. Senegal is dependent on primary commodity exports, but resource extraction and looting were irrelevant for the onset of the war, which was initially financed by "subscriptions" to the movement.<sup>49</sup> Exploitation of Casamance's cannabis and cashew nut crops helped the rebels finance the duration of the war *once it had started*, while the army extorted timber from the region.

Elsewhere we find countries that are coded in the CH dataset as having low primary commodity exports –Nigeria in 1960— but resource predation was still a motive for civil war. Oil was a key motive for the Biafran rebellion of 1967. There had been no demands for self-determination of the Eastern regions before the discovery of oil, but once oil was found, Ojukwu, the governor of the Eastern region, "demanded that oil revenue be paid to the regional treasury" and demands for independence grew.<sup>50</sup>

These examples of spurious correlations in CH and FL are caused by using the country year as the unit of analysis. A good example is Azerbaijan, a country with a nationalist conflict between Azeris and Armenians. Azerbaijan is dependent on exports of oil and natural gas, amounting to 23% of GDP in 1999 and 91% of total exports in 2001.<sup>51</sup> On the surface, this would seem to confirm the CH and FL models, as they predict a high risk of civil war in Azerbaijan. But the war occurred in Nagorno-Karabakh (NKR), a region bereft of natural resources, with a small economy based on agriculture and food processing. The fact that the conflict was entirely unrelated to oil could not be established because econometric tests are based on country-year analysis.<sup>52</sup>

Another important question that is left unanswered by CH and FL is if natural resources create motives for war or simply opportunities to sustain wars, or both. Case studies can better trace the sequence of events that betray motives and can introduce us to the public rhetoric and actions of rebel organizations. In many cases (Bosnia, Lebanon, Burundi,<sup>53</sup> Georgia, Mozambique), we observe predatory behavior and looting by the rebels even though these countries have no natural resources.<sup>54</sup> Small theft, looting houses and small businesses, car-jackings and extortion, kidnappings and ransom are all ways through which rebel groups can finance rebellion. Looting in these cases seems to be a mechanism to sustain rebellion and can hardly be called a motive for the violence, given its relatively modest scale. Some resources cannot be exploited unless the rebels have gained control of the territory (this is the case with oil). One would therefore expect that resource predation would be especially important for sustaining rebel organizations once the violence has started. This makes conflict escalation likely in oil-dependent countries with secessionist ethnic groups as the state cannot afford to lose

export revenue from oil exports nor can it afford to let the rebels take control of oil fields as this would allow their organizations to grow.

The preceding discussion suggests that we need better measures of economic variables and that causal inferences from economic models with reference to the link between resources and war would be more accurate if we focused our studies on the sub-national level.

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

The risk of civil war may not be spread evenly across countries. Rich, industrialized countries are virtually risk-free. Middle-income countries have low and declining risk. Poor countries have the highest risk. But these differences across levels of development may be due to the way other IVs behave at different levels of income.

Take the example of the UK, where a high level of economic development explains why the CH model generates a 2% probability of civil war in the UK from 1970-74, an estimate that is three times lower than the population average. However, a war did break out in Northern Ireland in 1970-71 despite high levels of income.<sup>55</sup> In Northern Ireland, secondary school attendance is high (top 13 in the world in 1970), as is income (not far from Britain's) and there are no natural resources.<sup>56</sup> But, in a sense, all that is irrelevant to the outbreak of the war, which was motivated by religious difference and fueled by repressive government policies. The opportunity cost argument does not apply very well to "volunteer forces" such as the IRA that are not concerned with looting.

The cause of this classification failure in the CH model may lie in the fact that CH (and FL) have pooled all types of civil war in their quantitative studies.<sup>57</sup> CH assume that

their simple opportunity cost argument should apply to all civil wars. Even where wars are ethnicized or dressed in a religious garb, as in Algeria, if the underlying motives are economic, the opportunity cost argument should apply. But it is an open question if ethnicization is epiphenomenal or substantively important as a motive for war. The economic argument is at times forced upon some wars while other explanations may also be plausible. In Algeria, for example, Lowi argues that economic decline and demographic pressures led to the emergence of Islamist protest.<sup>58</sup> But Algeria had several periods of serious economic decline in its history. Under Boumedienne (1965-78), economic growth dropped rapidly and unemployment and corruption increased, yet we saw no Islamic backlash. Rather, a bankrupt political system, leading to the illegal intervention in the elections of 1991 –combined with increased economic woes— may explain Islamic protest better than economic failure alone.

How would one know if protest is genuine or manufactured, instrumental or epiphenomenal? One way to peer into the motives of rebel groups is to analyze their organization. Our case studies found that rebel groups were often organized along ethno-religious lines. In Burundi, rebel recruitment follows tribal lines;<sup>59</sup> in Lebanon, recruitment and alliance patterns followed religious lines.<sup>60</sup> Some analysts use this fact as evidence that the war is an ethnic war.<sup>61</sup> Others argue that ethnicity is just as a cover for economic motives,<sup>62</sup> personal hostilities animosities,<sup>63</sup> criminality,<sup>64</sup> or an assortment of other motives that are not truly ethno-nationalist at their core.<sup>65</sup> But, even if many conflicts can become “ethnicized” after they start and even though ethnic mobilization can be used by political elites to support non-ethnic rebellions, there is an empirical basis on which to argue that not all civil wars have the same causes and that “pure” ethnic wars

are different from other wars. A common-sense definition of “ethnic” war is a war fought between ethnic groups over issues that relate to ethnicity. It does not matter if 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 something (*anything*) special about ethnic ties, then wars that are only possible by mobilizing ethnic identities should be distinct from other wars. Wars over self-determination, for example, should be usefully distinguished from popular revolutions and coups that escalate into civil war.

If different types of civil war have different causes, and if different forms of political violence are also different from civil wars, then our current practice of pooling all civil wars may lead us to make false inferences about the causes of civil war. Assume, for a moment, that coups have very different causes from civil wars but that genocides or politicides have similar causes. Current practice is to include coups in civil war lists if the death toll is high enough and if the state suffers some deaths; and to exclude genocide unless it takes place during civil war (but not right before or right after it).<sup>66</sup> If a sufficiently large number of coups are included in the list, then the coefficient estimates for a number of explanatory variables in the civil war model may be affected (biased). By contrast, if genocides or politicides are excluded, the costs will simply be loss of efficiency in the estimates, which will be less accurate due to the loss of several observations. Now, if we assume that both coups and genocides are different from civil war, as is commonly argued in the literature, then it may be the case that coups belong on the right-hand-side of a civil war equation (coups often lead to civil wars). In that case, our datasets must be able to distinguish the “coup-like” phase of a coup from the “civil

war-like” phase, but neither our data nor our definitions are fine-tuned enough to allow us to do that. Genocides may in fact require that we flip the equation around, as they are often the consequences of civil war.<sup>67</sup> In this case, only a model or narrative that explains how these two outcomes are nested together would be able to explain how we transition from civil war to genocide. It is important, I would argue, to understand not only how different forms of violence are organized, but also under what conditions we are likely to see one form leading to another. This would add dynamic perspective to our models.

To understand the dynamics of civil war, we must also study the growth of rebel organizations. One pattern that our case studies identified was that most rebel organizations did not have ready access to war-fighting capital (troops and weapons) from the outset. This would only occur if the national army became split between warring groups (as in Bosnia), in so-called “spontaneous” wars.<sup>68</sup> In most cases, insurgent groups started small. In Colombia, the National Liberation Army (ELN) grew from 30 men in 1965, to 270 in 1973, to an army of 4,500 in 2000.<sup>69</sup> In Azerbaijan, the NK rebels grew from under 1,000 in 1988 to 21,000 in 1992-94.<sup>70</sup> In Aceh, GAM started with 24 members in 1976 and grew to a fighting force of 2-3,000 with a 24,000-strong militia in 2001, controlling 80% of Aceh’s villages.<sup>71</sup> In Georgia, South Ossetian fighters grew quickly from 300-400 volunteers in early 1990 to 1,500 full-time fighters and 3,500 reservists.<sup>72</sup> The war in Mali started when a small group of Libyan-trained fighters belonging to the *Mouvement Populaire de Libération de l’Azawad* (MPLA), killed four people and took control of a dozen rifles, which they used to gradually increase their ranks and military strength.<sup>73</sup>

Understanding how rebel organizations develop is a critical component of developing intervention strategies, as these must clearly be different according to the stage of development of the rebel group. In some cases, the risk of war increases with the number of political non-violent organizations that oppose the government.<sup>74</sup> Our case studies reveal that oftentimes, pre-existing nonviolent political organizations, political parties or ethnic group organizations were used as a springboard for the organization of rebellion. In Burundi, the army was “a permanent threat” that allowed mass mobilization for violence.<sup>75</sup> Hutu groups from the 1960s and 1970s were mobilized in the violence of 1990, but new ones were also formed after the violence started. In Lebanon, most militias were associated with a pre-existing political party or religious group.<sup>76</sup> In Northern Ireland, a clearly defined ethnic base supported the insurgency, as the IRA recruited predominantly from the (mostly working class) Catholic community. The violent wing of the IRA was a splinter group from a nonviolent movement. In the DRC, ethnicity formed the basis of five rebellions. The Katanga secession and Shaba wars, for example, were led by the Lunda, Ndembu, and Yeke groups, while the 1996-97 Kabila rebellion drew its first recruits from the Banyamulenge.<sup>77</sup> If ethno-religious affiliation is used widely to mobilize support for rebellion, then it may be hard to clearly identify a typology of ethnic war,<sup>78</sup> unless we focus on wars over self-determination, which are close to the ideal-type ethnic war: a war fought between ethnic groups over ethnicity.

The case studies also shed light on the financing of rebel organizations. It is true that rebel financing was usually obtained in the ways suggested by the CH model: through the extraction of rents from local populations, direct and indirect taxation, looting and crime, resource predation and trade, or by support from foreign governments or

diaspora populations. But many case studies point to something that economic models have overlooked: forcible recruitment of soldiers. While financing is still necessary, many rebel organizations coerce participation in the movement. Forced recruitment was widely practiced by the EAM-ELAS guerillas in Greece in the 1940s;<sup>79</sup> by the LRA in Uganda in the 1990s, and in many other cases. In Burundi, rebel groups purchased Kenyan street children at the price of \$500 for 150 boys.<sup>80</sup> In Mozambique, Frelimo used repression, imprisonment, re-education and indoctrination to increase its forces, while Renamo “used force at every point for almost every purpose.”<sup>81</sup> This pattern creates problems for the economic opportunity cost theory, since many recruits do not have the privilege to make an independent decision to join the rebels.<sup>82</sup>

Are coercion and material gain sufficient reasons for an ordinary citizen to join a rebel organization? We need to go beyond CH and FL to find non-economic explanations of the micro-foundations of ethnic violence. Roger Petersen’s research would suggest that use of violence is linked to emotional responses to structural change.<sup>83</sup> This would seem to negate the presumption of CH and FL that everyone is a potential rebel, given the opportunity. Rather, rebels (at least not the ones who are coerced) should have an emotional makeup that is different from those still committed to non-violence. It is critical, I would argue, to consider emotional and economic theories as complements, not substitutes. In a study of the Northern Ireland insurgency, White explains the Catholic Nationalists’ switch from peaceful protest to violence as a reaction to violent acts committed by the state and the start of the policy of internment.<sup>84</sup> Most of the protesters who were interviewed by the author were outraged by government repression, but those who resorted to violence tended to be members of the working-class,

unemployed, or students. By contrast, those who continued to believe that violence was not the answer tended to be older, employed, middle-class people with university degrees facing higher opportunity costs for engaging in violence.

Emotions (e.g. resentment towards injustice or fear of repression) are clearly consistent with economic models of war, since interactions of some of the variables included in economic models (e.g. political instability and economic decline in an ethnically fractionalized society) could elicit emotions that can support insurgency. Moreover, there are instrumental uses of emotions that can lead people to pursue private goals such as greater security or more wealth. Other emotions (rage) are inconsistent with economic models, as they generate violence that Kalyvas calls “Schmidtian” – i.e. not selective— violence. This sort of violence is directed against targets that are perfectly substitutable and might characterize terrorism more than it characterizes civil war, where violence has generally been shown to be selective.<sup>85</sup>

One way to combine these theories is to consider 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 the demand-side of the equation, it will become obvious that ideology and psychology can no longer 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 if this is true, we must look both more broadly and more narrowly at civil war: i.e. we must consider more broadly what causes political violence (as a more naturally occurring category than civil war) and, more narrowly, at how political violence is organized and how we transition from one form of violence to another, ending up at civil war.

## 5. Re-Conceptualizing the Dependent Variable

In the rest of the paper, I argue that a deeper understanding of the causes of civil war can be attained by looking both above and below civil war (at political violence more generally) and around it (at different forms of organized political violence). Case studies provide useful insights on both the closeness of different forms of violence (coups, wars, politicides), but also on their differences and the pathways that lead from one to another.

### *Recurrent Cycles of Violence*

Many countries are caught in a conflict trap and 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. Our case studies suggest the need to re-conceptualize civil war.<sup>86</sup>

It is common to see violent anti-colonial movements give way to civil war (as in 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 occupation processes (as in Greece in the 1940s, Yugoslavia – with a 50-year delay, and Iraq, more recently).<sup>87</sup>

Civil wars are sometimes very bloody coups (as in Costa Rica in 1948, Bolivia in 1952, or Argentina in 1955); or they are international wars fought by proxy in a third country (as in Lebanon 1979-1991; or the DRC from 1998-2002). In other countries, civil wars are born out of riots, as in India, where the 1947 riots could easily be compared to both the organizational complexity and level of destruction usually found in a civil; war.

Bloody spikes of riots and pogroms mar Indian history ever since. In other countries, civil wars are indistinguishable from inter-communal fighting, since all politics seems to

be colored by ethnic divisions, as in Rwanda. In Burundi, nationalist strife in the 1950s led to ethnic violence after independence continuing into the 1970s and 1980s, culminating in a civil war in the 1990s. Nigeria transitioned 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 have transitioned from anti-colonial movements, to coups, to civil war, to inter-state war, as in Cyprus, which was embroiled in violent conflict from the late 1950s until the mid-1970s. The DRC has seen every imaginable form of political violence since the 1960s with genocide in the works in the Kivu area in the 1990s.

The decision to code a period of violence as a civil war as opposed to a politicide, riot, or coup, hinges on rather vague criteria.<sup>88</sup> One distinguishing feature of civil war in many definitions is effective resistance – i.e. the state must also suffer. FL use a threshold of 100 state deaths to distinguish a civil war from a politicide, so they code, for example, Argentina from 1973-77 as having had a civil war since somewhere from 100-400 deaths on the state side occurred in that period. But most of the violence actually took place after a coup installed a new authoritarian government that purged the political opposition in Argentina. More to the point, no coding rule to date specifies the time period over which the state must incur its deaths in order to establish effective resistance. The state may suffer 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) and this would qualify as a four-year civil war in the FL dataset. But if state deaths are concentrated in one short period, shouldn't different phases of the event be distinguished, coding one phase a civil war and

another a politicide? In Cambodia, civil war from March 1970 to April 1975 killed on average 122,500 people per year. A democide/politicide followed immediately after and killed on average 347,500 people per year from April 1975 to January 1979. Another war is coded by the Correlates of War Project as starting in 1979.<sup>89</sup> The period of the killing fields in Cambodia (1975-79) is typically excluded from civil war lists. But this is not a period of peacetime and we cannot understand the onset of the new war in 1979 as merely a function of Cambodia's GDP per capita and natural resource-dependence in the preceding 5 year period. The genocide is an obvious missing link here. Thus, we must either develop models that can account for the links across different forms of political violence, or, in the absence of such models, we should not arbitrarily throw data away because they do not meet the criteria set by somewhat arbitrary definitions of civil war. One way to avoid these problems is to look beyond civil war at all large-scale organized political violence. Another way is to analyze the transition across different forms of political violence. Political violence may itself be understood as a part of the process of state evolution.

Lack of attention to the inter-relationship between civil war and other forms of violence hurts the CH model since CH find that prior war increases the chances of a future war. The mechanisms through which this occurs are not specified in the model, but a variable measuring peacetime between events of civil war is highly significant. Yet, I have argued above that periods of "peacetime" in the CH model can hide much violence. If CH modeled the proximity of other forms of violence, this could be an indirect way to model the complex links between past riots, coups, interstate wars, all of which can add to the risk of a future civil war by destroying property and human capital,

undermining economic incentives, increasing levels of inter-group hostility, and accumulating conflict-specific capital. Revising the peacetime measure should allow the CH model to make better predictions.<sup>90</sup>

*Crime, Grievance, and Politics: The Organization of Violence*

Another interesting link identified by the case studies is one between criminal and political violence. Civil wars can degenerate into organized crime, as in the case of Russia and Sierra Leone. In 1995 in Colombia, 90% of the regions with the highest homicide rates also had active guerilla groups, whereas these groups were active in only 54% of the total number of regions; and 70% of these high-homicide regions also had substantially higher drug trafficking (as compared to 23% of regions nationally).<sup>91</sup>

Criminal and political violence share a common causal link in state weakness. Mafias are organizations designed for extortion, smuggling, and drug trade, but they can also be thought of as organizations that provide security and authority in areas where the state has no monopoly over the means of violence.<sup>92</sup> Thus, we see that organized crime flourished with the decline of the Soviet state's strength. In the 1990s in Russia, the state's inability to maintain the prison population led to mass releases of convicted criminals, increasing crime.<sup>93</sup> Haphazard privatizations increased the amount of "loot" over which criminals could fight, spurring the formation and growth of criminal gangs, much in the same way that we observed the growth of rebel organizations in Sierra Leone and other resource-rich failing states.

Looting is also a common dimension of political and criminal violence. The form that violence will take (a mafia or a rebel group?) may be determined by the type of available "loot" and by the necessary means to appropriate it. If ordinary crime or

corruption is sufficient to acquire the desired amount of loot, then common crime 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. If appropriation of loot requires the control of the state's apparatus or the control of territory (as, for example, in the case of oil deposits), then we are likely to observe the growth of rebel organizations, particularly secessionist organizations. Rebellion may not be necessary if the control of the state can be achieved indirectly, through the exploitation of ethnic or kinship networks that turn the state into a source of rents for the group.

These various forms of organizing violence will result from the interaction of demand for loot and power and the supply of opportunity to organize criminal or insurgent groups. A strong state can deter the escalation of violence. But criminal and political violence together can undermine state authority and capacity by creating production externalities that help each other grow. In Sierra Leone, criminal activity accumulated violence-specific physical and human capital and war diverted the state's attention from fighting crime.<sup>94</sup> Over time, the rebels and criminals were indistinguishable from one another as the RUF recruited illicit diamond diggers and continued their operations while fighting against the state. The same occurred in Colombia, as guerillas provided protection for drug cartels and drug cartels 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. Thus, we can observe a pattern through which low levels of crime and violence slowly undermine the state and, over time, take over the state.

In those 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, we can observe terrorism. Terrorism can also grow where large-scale rebellion is likely to be crushed by a strong state. It can also feed off civil war and vice-versa. In Egypt, terrorism against Westerners was the direct result of government suppression of the Gamaat Islamiya, an insurgent group. The Israeli-Palestinian conflict (a civil war since the first Intifada of 1987) had been at the heart of international terrorism before the Oslo accords of 1997. Kidnappings in Colombia are a direct consequence of the civil war and a means for the rebels to finance their insurgency. Chechen terrorism in Russia today is the outgrowth of the Russo-Chechen war. Interestingly, many of the Chechen “terrorists” were “heroes” of the Abkhaz war against Georgia, as Basaev and other Chechens were bused to Abkhazia by Russians in 1992-93. As one form of violence feeds into another over time and across space, we become less able to study each form in isolation and we must instead focus on the complexity of these events.

## 6. Theory-Building & Omitted Variables

If other forms of violence in the same country can influence the risk of civil war onset, it may also be possible for civil wars in neighboring countries to have such an effect. The CH and FL models disagree on the likely effects of neighborhood civil war. While CH find that a country’s civil war risk increases if its neighbors have had a civil war in the previous year, whereas FL find no such evidence. Both seriously underestimate the international or “neighborhood” dimension of civil war. Several case

studies in our project point to various ways in which the neighborhood influences a country's war risk.<sup>95</sup>

### *Contagion and Diffusion*

Several of our cases point to significant demonstration (diffusion) effects of civil war. A good example is the Aceh rebellion in Indonesia, where an independence movement was simmering for decades and a brief civil war was quickly suppressed in 1991. The war re-ignited 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.<sup>96</sup> In Senegal, the Casamance independence movement was influenced by the ideology of the independence struggle in Guinea-Bissau.<sup>97</sup> This diffusion effects was coupled with contagion, Guinea-Bissau was used as a location for cross-border raids in Senegal, a market for goods, and a source for arms.

Evidence of regional contagion effects (direct spillover of war across borders) abounds in the case studies. Yugoslavia's wars (Croatia in 1991 and 1992-95; Bosnia in 1992-95, and Kosovo in 1998-99) all were influenced 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.<sup>98</sup> Sierra Leone's civil war was sustained by international crime networks that were engaged in arms-for-diamonds trade and did not start until Taylor's Liberia provided sanctuaries to the rebels. The civil wars in the African Great Lakes region are

perfect examples of contagion. Recurrent wars in Burundi and Rwanda spilled across their borders and influenced each other as well as the DRC, ultimately pulling in Uganda and Zimbabwe. In both Burundi and Rwanda, the wars have occurred between the same two ethnic groups – the Hutu and Tutsi. The Rwandan social revolution of 1959 caused a transfer of power from the 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 as the Hutu were also the majority and Tutsi sought to consolidate their power over state institutions, especially security forces.<sup>99</sup> This recurrent ethnic conflict crossed borders and lasted through time, being at the core of around seven episodes of civil war in the two countries.

Quantitative studies that point to significant neighborhood effects have a hard time distinguishing between many possible diffusion and contagion mechanisms.<sup>100</sup> Our case studies suggest that a key mechanism for the spread of civil war is the presence of ethnic kin across the border.<sup>101</sup> In Macedonia, for example, the main risk of civil war in the 1990s came from ethnic Albanians who were actively supporting independence in neighboring Kosovo and were responsible in organizing and supporting Albanian armed opposition to the Macedonian government across the border.<sup>102</sup> Indeed, some civil wars are better understood as regional communal conflicts. For example, 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 mechanism for the trans-nationalization of civil war is refugee flows. A large influx 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.<sup>103</sup> An even more common neighborhood factor is the cross-border trade in small arms.<sup>104</sup>

These examples 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*

An international dimension of civil war that many case studies have identified as a critical factor in civil war onset is external intervention. In Mozambique, the DRC, Burundi, Georgia, and several other countries, external economic and military assistance was critical in both inciting and supporting rebellion. Weinstein and 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 its 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 ZANLA guerillas. The level of violence dropped markedly in 1979, when Rhodesian support for ZANLA stopped after the collapse of the Smith regime.<sup>105</sup> RENAMO became incorporated in the South African Defense Forces, from which it acquired supplies, logistical and technical support, accounting for its tight, centralized structure.<sup>106</sup>

Mozambique's experience is not unique. The third Congolese war – the Shabba rebellion— was the result of an invasion by Congolese expatriates from Angola.<sup>107</sup> Yugoslavia's ethnic conflict in Kosovo only rose to the level of civil war after NATO's military intervention. And, earlier, it is doubtful that the Bosnian Serbs and Croats would

have had sufficient military resources to wage war in Bosnia without the support of Serbia and Croatia, respectively.<sup>108</sup> In Georgia, Abkhazian resistance could not have been organized or sustained without direct Russian assistance.<sup>109</sup> Similarly, the Lebanese war cannot be understood as distinct from the multiple external interventions and counter-interventions by the US, Syria and Israel. Most of the local factions represented a foreign government's interest in Lebanon. In Sierra Leone, persistently high levels of poverty, slow growth, low levels of education, and high dependence on natural resources had not caused 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 "schooling" in insurgency in Libya.<sup>110</sup> In this and many other wars in Africa (Mali, Chad), but also in countries as far away as Indonesia, Libya's Quaddafi 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 IV's in the CH or FL models, then the CH and FL parameter estimates will suffer from omitted variable bias. It is possible 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 the process leading to war, and not simply focus on the outcome. Case studies can give us a better sense of the dynamics of conflict escalation, presenting a sequence of events, a series of actions and reactions that link several IVs together in a process that culminates in war. They can help us establish if civil wars occur suddenly with little warning –like earthquakes— or slowly with much buildup – like volcano explosions.

Some of the most useful insights from our case studies came from analyses of the dynamics of conflict in countries where civil war did not occur despite a large number of risk factors. Consider the example of 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 during this period, but not war. The state's response has not been indiscriminately violent in this case. This has kept in check anti-state violence by ethnic rioters. By contrast, the state's reaction during the Biafran secession in 1967 was swift and overwhelming. It is generally easier to gain concessions from the state if those concessions do not threaten state security. But demands for secession of an oil-rich region like Biafra were threatening to the Nigerian state, in which case violence escalation could have been expected. By contrast, 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, so the government did not feel sufficiently threatened to respond forcefully and granted concessions to the Ijaw. Whenever the Ijaw targeted the police or other government institutions, the government responded with decisive force. Comparing these cases we find that escalation potential is influenced by the interaction of the intensity of insurgent claims and the form of protest.

A strong state can afford to be accommodative or repressive at low cost.<sup>111</sup> But even policies of accommodation need not be effective in curbing opposition, if state legitimacy is weak. An illegitimate government cannot credibly commit to upholding its end of the bargain. In Burundi, the political exclusion of Hutus from elite positions led, over time, to lower educational opportunities and economic power for Hutus. Political repression of the Hutu population and violence against Hutu leaders backfired into coup attempts by the Hutu in 1965 and 1972 and counter-coups by the Tutsi, leading to massacres of Tutsi by Hutus in 1965 and 1972 and a large-scale Hutu rebellion, more massacres 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.<sup>112</sup> Similarly, in Aceh, the new democratically elected government's decentralization laws of 1999 were non-credible given that the government did not control the military and that Indonesia was dependent on oil and gas exports from Aceh, thereby increasing the risk that the government would renege on its promises of fiscal autonomy for Aceh.

If accommodation does not work, repression is usually next, but a lesson from our case studies is that government repression typically leads to more opposition.<sup>113</sup> In particular, 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. In the case of the Muslim Brotherhood, another one of Nigeria's many rebellious groups, war was avoided through the use of effective repression. The Brotherhood had backing from Iran and a clear anti-government ideology and used violent tactics. It supported a religious movement wanting an Islamic state, but the arrest of its leader decapitated the movement

in its early stages. Selective repression was effective because it was applied quickly. This is a mechanism through which non-democracies, which can use selective repression more easily, can reduce their risk of conflict escalation. Thus, state capacity may interact with regime type to determine the likelihood of using repression or accommodation.

The conflict escalation potential of incomplete repression strategies may explain why democratization increases civil war risk.<sup>114</sup> A democratizing regime cannot use repression with as much ease as the state's enforcement apparatus is weakened by having its activities become more transparent. The state is therefore less able to root out opposition before at its early stages. Indonesia offers a good example. During the country's authoritarian period, state repression had obliterated the GAM – the main rebel group in Aceh until the early 1990s. A period of incomplete democratization caused friction between the state and the military and led to incomplete repression strategies in Aceh. Lack of control of the army by the state allowed human rights abuses in Aceh to go unpunished, undermining the government's credibility and increasing popular grievance, which the GAM capitalized on to mobilize public support and increase its ranks of fighters.<sup>115</sup> In Senegal – a democratic country— large-scale expropriations of indigenous land in Casamance began in 1979 and a systematic denigration of Casamançais followed through the imposition of Wolof in the media, education & 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) and those protesters who were imprisoned in Dakar started organizing the political wing of the party.<sup>116</sup>

These examples of growing discontent due to failed democratization or incomplete repression suggest that the “volcano” (or escalation) theory of civil war can better explain most cases than the “earthquake” theory. Northern Ireland perhaps best exemplifies how civil war can result from a slow, but steady escalation of protest. The pivotal event was the October 5, 1968 march in Derry/Londonderry, when RUC forces assailed the protestors, leading to efforts at partial appeasement of Catholics with British intervention, and a package of political concessions to NICRA.<sup>117</sup> The reforms came late and were not substantial and caused extreme negative reactions by Unionists. Political instability and protest led to O’Neal’s resignation in 1969 and a victory for extremists, leading 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). PIRA abandoned the strategy of “abstentionism” that had been used up to that point – something akin to peaceful protest in the US civil rights movement- in favor of a radically militant stand against Protestants and the British, transforming a disorganized sectarian protest into an organized political violence campaign.

Another important piece of the puzzle is that escalation potential varies across sub-national regions and is greater in regions whose “special status” privileges were revoked, as in Casamance (Senegal), Kosovo (Yugoslavia), Aceh (Indonesia) and elsewhere. In the DRC, the *Loi Fondamentale* overturned long-standing legislation on minority rights and was seen as a precipitant to war.<sup>118</sup> A series of nationality laws designed to “protect” the local population in the Kivu region, led in April 1995 the Transitional Parliament to strip Banyarwanda and Banyamulenge of their Congolese

nationality. The Banyamulenge refused to leave and turned to Rwanda for help.

Rwanda's intervention led to the massacre of Hutu refugees in the DRC.<sup>119</sup>

Escalation risks are also a function of what goes on in the neighborhood. In Lebanon, 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 increased anti-Israeli emotions and Palestinians forged alliances with Lebanese groups. The civil war can be actually be traced back to 1968-69, 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 either as the result of extreme demands by the challengers or due to 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. This quote from John Garang, leader of the SPLA, highlights the link between state repression and economic opportunity:

“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.”<sup>120</sup>

## 7. Conclusion

In this paper, I have tried to make both a substantive and a methodological contribution to the literature on civil war. I begun by reviewing two major contributions to field – the CH and FL models of civil war. These models are sound in their

fundamental propositions, but their scope could be expanded and their application to the data improved. By drawing on a set of comparative case studies, I attempted to demonstrate ways in which qualitative and quantitative methods can be combined to build better empirically testable theories of civil war.

Elaborating on the interplay between statistics and case studies led me to consider the interplay between micro-level and macro-level explanations of civil war. The economic models reviewed here offer one of several possible micro-level explanations of political violence. I have argued that micro-level explanations must be aggregated to make sense of civil war. Micro-level theories explain the use of political violence, broadly speaking, at an individual level. But it should be hard to distinguish, at the micro-level, among people who choose to use violence in a civil war as opposed to those who choose to use violence in a riot. Macro-level theories, by contrast, will be most helpful in explaining why, for a given set of private motives, violence is organized in the form of civil war. Process-driven explanations that focus on the dynamic interaction among actors and between actors and opportunity structures will explain particular outcomes and cases. These are posited as hypotheses for development in further work.

One difficulty is that this discussion presumes that there exist clear definitions of the various forms of political violence. If our concept and operational definition of civil war is muddy, then it will be hard to explain it. In the quantitative literature, this problem is addressed empirically, by running the same model on a few different definitions of civil war and, if the results are the same, then the model is accepted. But if there is no substantive difference between, say, coups and politicides and civil war (at least with respect to the variables that are typically included in quantitative models), then all

competing definitions of civil war can be wrong together. Thus, to understand the interaction between micro-macro level of analysis with a view to explaining civil war, we must first establish the differences across forms of political violence and identify what Kalyvas calls the “ontology” of civil war. But, ultimately, the ontology of civil war cannot be defined at the micro-level alone. War refers to a specific organization of violence and as such, it is a macro-level phenomenon. For civil war to have meaning as a distinct category, there must exist a mapping of micro-level motives and macro-level structures that is unique to civil war.

The case study project helps in clarifying both concepts and operational definitions. First, it suggests ways through which the empirical measures used in quantitative tests could be improved. Since the quantitative models that we currently use are not predictive models (consider the fact that they have few time-varying covariates and can pick up mostly cross-sectional variation), they should make predictions for the right reasons – i.e. they should have analytical value. The fact that large-N studies make incorrect assumptions about causal paths implies that inferences drawn from statistically significant results cannot yet inform policy except in a very general and indirect way (i.e. increasing GDP per capita will *somehow* reduce the risk of civil war).

Second, case studies help identify a host of independent variables that can explain civil war onset but are currently omitted from economic models. I focused on two examples of such variables – neighborhood effects and external intervention. Taking the models back to the data, we can test the statistical significance of these variables.

Third, the cases challenged the unit homogeneity assumption that underlies current quantitative work on civil wars. This should prompt quantitative analysts to test

for country-, region-, or period-fixed effects. More importantly, we found that periods with no civil war often hide important social conflict and, sometimes, violence, and that this is not properly modeled in quantitative studies. The path-dependence of violence implies that we must model the transition across different forms of political violence, such as riots, genocide, civil war, and terrorism. To get a better measure of the differences between these forms of violence, we need to go back to formal modeling and statistical tests. For such tests, the unit of analysis cannot always be the country-year. If we used as our unit of analysis the sub-national region (e.g. largest administrative region below the state) we might be less likely to find spurious correlations between the natural resource dependence and the onset of civil war. Moreover, different units of analysis might have to be used to answer questions about the causes of different forms of violence. To analyze the risk of secession, focusing on the sub-national region seems more profitable than focusing at the country-year and it can allow us to introduce new variables, such as inter-regional inequality, that might explain secession but cannot be defined at the country-year level.

So, the message of this paper is not simply that economic models of civil war must be expanded by bringing politics back in. Rather, I outlined the need to combine several theories and use both qualitative and quantitative methods to cumulatively construct a broader theory of political violence. By looking closely at actual civil wars, we can best identify the boundaries of the concept and credibly argue that the distinction between greed and grievance that has been employed in the recent literature is a false one. Instead of modeling such simplistic, misguided distinctions, our theory-building efforts should be redirected towards understanding how different forms of violence are

organized while building models that can explain the conditions under which conflict will escalate into one form versus another. Such theory is both more intellectually satisfying and more policy-relevant, as different interventions must be designed to address the risks and consequences of riots versus coups, genocides versus civil wars. To develop models to guide our policies, we must proceed interactively, complementing statistical inference with in-depth case knowledge.

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<sup>1</sup> These figures are based on Doyle and Sambanis (2003).

<sup>2</sup> Rebellion is the CH term and insurgency is FL's term. Both refer to civil war.

<sup>3</sup> The project started in the spring of 2000. The author was primary investigator from the summer of 2001 until the project's completion (spring 2004). The cases were written by teams of country experts, usually matching an author from the country under study with an author from a US-based institution. The following countries were included: Algeria, Azerbaijan, Bosnia, Burundi, Colombia, Georgia, Democratic Republic of the Congo, Jamaica, Indonesia, Ivory Coast, Kenya, Lebanon, Macedonia, Mali, Mozambique, Nigeria, UK (Northern Ireland), Russia, Senegal, Sierra Leone, and Sudan. Some of these countries had more than one civil war. The following case studies were commissioned but never completed: Afghanistan, El Salvador, Moldova, Uganda, Somalia, and Sri Lanka.

<sup>4</sup> The cases are currently being prepared for publication in an edited volume. See the guidelines given to authors: <http://www.yale.edu/unsy/civilwars/guidelines.htm>. More detailed instructions were given in two conferences, in Oslo, Norway, in June 2001 and New Haven, CT, in April 2002. The second supplement (64 single-spaced pages) presents a summary assessment of the model's fit to each case and can be accessed online here: <http://pantheon.yale.edu/~ns237/index/research.html#Cases>.

<sup>5</sup> The distinction was one proposed in early versions of the CH model. It corresponds to

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the distinction between grievances and opportunity structures in the FL model.

<sup>6</sup> Space constraints preclude the presentation of new quantitative tests of an expanded civil war model. This is the subject of a book-length manuscript by the author.

<sup>7</sup> An exception is Sambanis (2001, 2002, 2003), who explored differences between ethnic and revolutionary civil wars; and between civil war and politicide or genocide.

<sup>8</sup> The theoretical underpinnings of this idea are modeled by Grossman (1991, 1995); Hirschleifer (1989, 1995); Konrad and Skaperdas (1999).

<sup>9</sup> Hirschleifer (1995, 172). The credibility argument is developed, in the context of interstate war, by Fearon (1995) and for civil war, by Skaperdas (2001).

<sup>10</sup> See Collier (2000). Collier and Hoeffler (2001, 3) write that “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.”

<sup>11</sup> Gurr (2000).

<sup>12</sup> China and the USSR (under Stalin) are obvious exceptions, though there are also many examples of unruly small countries (Cyprus, Georgia, Azerbaijan, and others).

<sup>13</sup> These results were first reported by Hegre et al (2001).

<sup>14</sup> FL analyze data with annual frequency from 1945-1999 while CH analyze five-year panel data from 1960-1999.

<sup>15</sup> CH find a highly significant negative relationship between time at peace and the risk of civil war onset, but FL do not find such a result.

<sup>16</sup> These problems are also frequently present in quantitative studies.

<sup>17</sup> Ragin (1987, 49).

<sup>18</sup> On this selection rule, see King, Keohane, and Verba (1994; 141; henceforth KKV).

<sup>19</sup> Including cases of no-war resembles Mill’s *indirect method of difference*. See Ragin (1987, 41).

<sup>20</sup> Non-linearities imply that the theorized linear relationship between the DV and IV 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. If such effects are present, then a stratified sampling method should be used, if cases are used for hypotheses testing.

<sup>21</sup> This is true unless the data quality and quantity is good enough to apply sophisticated statistical methods that can analyze nested and multi-stage models.

<sup>22</sup> A case control design was used by Esty et al 1995 in their study of state failure.

<sup>23</sup> See Baev, Kohler, and Zurcher (2003, 39-40).

<sup>24</sup> “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, Keohane, and Verba 1994, 91).

<sup>25</sup> Ragin (1987, 49). This is an exploratory, not a formal test of the assumption.

<sup>26</sup> Przeworski and Teune (1970); Ragin (1987, 48).

<sup>27</sup> Przeworski and Teune (1970, 57-59).

<sup>28</sup> In addition, Collier and Hoeffler (2001) show that there are no statistically significant patterns in civil war onset in different regions.

<sup>29</sup> This is due to the well-known degrees of freedom problem.

<sup>30</sup> McAdam, Tarrow, and Tilly (2001, 24).

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<sup>31</sup> 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 was the mechanism through which obesity causes death. But assume, also, that obesity is the result of bad eating habits, which is 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 and, put more formally, bad parenting will have little explanatory power as an explanatory variable in a model that tries to explain death as a function of a person's physical characteristics.

<sup>32</sup> Kalyvas focuses on that variation to discuss the "ontology" of civil war by which he refers to our ability to know what the civil war was about. He argues that we cannot label a war "ethnic" or "ideological" because those committing the violence have many and often conflicting motives. In my paper, ontology refers to the meaning of civil war as a category that is meaningful and distinct from other forms of political violence.

<sup>33</sup> Ross (2003, 27).

<sup>34</sup> Woodwell (2003, 16-17) also notes the deterrent effect of the Royal Ulster Constabulary's strength of 13,500 members.

<sup>35</sup> CH and FL do not code a war in Kenya. Other datasets (Doyle and Sambanis 2003) code a war in 1991- 93 due to the state's indirect involvement in the violence, but this is an ambiguous case and could also be classified as inter-communal violence.

<sup>36</sup> Kimenyi and Ndung'u (2002, 12).

<sup>37</sup> Makdisi and Sadaka (2002, 6).

<sup>38</sup> Darden (2002).

<sup>39</sup> Krueger and Maletckova (2003).

<sup>40</sup> Davies and Fofana (2003).

<sup>41</sup> Kalyvas and Sambanis (2003).

<sup>42</sup> Ross (2003, 15-18).

<sup>43</sup> All former-USSR states had drastically falling growth rates during the collapse of the USSR. It is unclear how much of the growth decline in Georgia, Azerbaijan, and Chechnya was due to the war and how much to the collapse of the Soviet state.

<sup>44</sup> Ngaruko and Nkurunziza (2002, 5).

<sup>45</sup> CH find that the risk of war is 50% greater in the period immediately after the previous war ends as compared to other periods.

<sup>46</sup> Baev, Kohler, and Zurcher (2003).

<sup>47</sup> Ndikumana and Emizet (2002).

<sup>48</sup> Zinn (2003, 13). This war is usually omitted from many datasets, but it meets the definitional criteria as ongoing violence from 1980-1984 caused 5,646 deaths.

<sup>49</sup> Humphreys and Mohamed (2003, 10).

<sup>50</sup> This may explain the CH model's false negative prediction for the Biafran war. Nigeria's primary commodity export share of GDP increased to 38% in 1990-94. Zinn (2002, 10).

<sup>51</sup> Zurcher, Kohler, and Baev (2002, 63).

<sup>52</sup> Zurcher, Kohler, and Baev (2002, 63).

<sup>53</sup> Burundi has a high ratio of primary commodity exports to GDP due to coffee exports. No study has argued that control of coffee production is related to the Burundi war.

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- <sup>54</sup> All of these countries had lower resource dependence than the population mean.
- <sup>55</sup> Some datasets do not code a civil war in the UK. Even though the 1,000 aggregate threshold of deaths has certainly been exceeded, fighting has been sporadic, leading to a slow accumulation of deaths.
- <sup>56</sup> At the same time, high unemployment among Catholic men – if this had been accounted for in the CH model – would have increased the probability estimate for a rebellion among Catholics due to easier rebel recruitment.
- <sup>57</sup> Sambanis (2001) first made the argument that in pure ethnic conflicts there is a tradeoff between the economic costs of rebellion and political and cultural freedom.
- <sup>58</sup> Lowi (2003).
- <sup>59</sup> Ngaruko and Nkurunziza (2002, 31).
- <sup>60</sup> Makdisi and Sadaka (2002, 9-10).
- <sup>61</sup> Licklider (1995); Sambanis (2002).
- <sup>62</sup> Collier and Hoeffler (2000).
- <sup>63</sup> Kalyvas (2002).
- <sup>64</sup> Mueller (2001).
- <sup>65</sup> Brubaker and Laitin (1998).
- <sup>66</sup> See Sambanis (2003) for a more formal analysis of the differences between genocide and civil war.
- <sup>67</sup> In Harff's (2003) list of politicides and civil wars, I was able to find only one case (Chile 1973-76) that took place outside the context of a civil war.
- <sup>68</sup> Kalyvas (2003, page).
- <sup>69</sup> Sanchez, Solomon, Formisano (2003, 11).
- <sup>70</sup> Baev, Kohler and Zurcher (2003, 23-24).
- <sup>71</sup> Ross (2003, 24).
- <sup>72</sup> Baev, Kohler, and Zurcher (2003, 23).
- <sup>73</sup> Humphreys and ag Mohammed (2003, 2).
- <sup>74</sup> See Sambanis and Zinn (2003) for empirical evidence in wars over self-determination.
- <sup>75</sup> Ngaruko and Nkurunziza (2002, 36).
- <sup>76</sup> Makdisi and Sadaka (2002, 7-8).
- <sup>77</sup> Ndikumana and Emizet (2003, 3, 36).
- <sup>78</sup> See Kalyvas (2002); Fearon and Laitin (2003).
- <sup>79</sup> Glenny (2001).
- <sup>80</sup> Ngaruko and Nkurunziza (2002, 31).
- <sup>81</sup> Weinstein and Francisco (2002, 4).
- <sup>82</sup> The FL model is less affected by this finding since its focus on state strength allows it to claim that a strong state should be able to prevent forced rebel recruitment.
- <sup>83</sup> Petersen (2002).
- <sup>84</sup> White (1989).
- <sup>85</sup> Kalyvas (2003a, 475); Krueger (2003) argues that terrorist violence is random in the sense that any member of an opposition group can be equally targeted for violence.
- <sup>86</sup> On this point, see, also, Kalyvas (2003b).
- <sup>87</sup> On the Greek civil war, see Kalyvas (2000). On the link between Yugoslavia's civil wars and memories of atrocities during intra-Yugoslav fighting in World War 2, see Glenny (2001).

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- <sup>88</sup> I discuss elsewhere the complexities of defining and measuring civil war. See Sambanis (2003).
- <sup>89</sup> Singer and Small (1994)
- <sup>90</sup> The CH model under-predicts the risk of civil war onset in Burundi, Indonesia, Lebanon, and Sierra Leone, due to the narrow measurement of the “peacetime” variable.
- <sup>91</sup> Rubio (1995). Sanchez, Solimano, and Formisano (2002).
- <sup>92</sup> Gambetta (1993).
- <sup>93</sup> Andrienko and Shelley (2003).
- <sup>94</sup> Davies and Fofana (2003).
- <sup>95</sup> Brown (1996); Lake and Rothschild (1998, 5); Sambanis (2001).
- <sup>96</sup> Ross (2003, 3, 18-20).
- <sup>97</sup> Humpreys and Mohamed (2003)
- <sup>98</sup> Zurcher, Kohler, and Baev (2003).
- <sup>99</sup> Ngaruko and Nkurunziza (2002, 2).
- <sup>100</sup> See Sambanis (2001) and Gleditsch (2003).
- <sup>101</sup> 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 (2003). See, also, Gurr (2000, 92).
- <sup>102</sup> Lund (2003).
- <sup>103</sup> Ndikumana and Emizet (2002, 25).
- <sup>104</sup> This was one of the most common observations in most of our case studies.
- <sup>105</sup> Weinstein and Francisco (2002, 12-14).
- <sup>106</sup> 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.
- <sup>107</sup> Ndikumana and Emizet (2003).
- <sup>108</sup> Kalyvas and Sambanis (2003).
- <sup>109</sup> Zurcher, Kohler, and Baev (2003).
- <sup>110</sup> Davies and Fofana (2003).
- <sup>111</sup> Gurr (2000, 82).
- <sup>112</sup> Ngaruko and Nkurunziza (2003, 12).
- <sup>113</sup> An important paper offering evidence of this dynamic is Lichbach (1987). See also, Tarrow (1989), for an explanation of how state repression and accommodation can be linked to the radicalization of social movements.
- <sup>114</sup> This result is identified in FL, but not CH. See, also, Snyder (2000).
- <sup>115</sup> Ross (2003, 23-30).
- <sup>116</sup> Humpreys and ag Mohammed (2003, 8, 42, 52).
- <sup>117</sup> Woodwell (2003, 10-11).
- <sup>118</sup> Ndikumana and Emizet (2003, 14).
- <sup>119</sup> Ndikumana and Emizet (2003, 20-24).
- <sup>120</sup> John Garang De Mabior on the Founding of the Sudan’s People Liberation Army (SPLA) and the Sudan’s People Liberation Movement (SPLM). Quoted in Ali, Elbadawi, and El-Batahani (2003).