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### CIVIL WAR

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### **ABSTRACT**

Most nations have experienced an internal armed conflict since 1960. The past decade has witnessed an explosion of research into the causes and consequences of civil wars, belatedly bringing the topic into the economics mainstream. This article critically reviews this interdisciplinary literature and charts productive paths forward. Formal theory has focused on a central puzzle: why do civil wars occur at all when, given the high costs of war, groups have every incentive to reach an agreement that avoids fighting? Explanations have focused on information asymmetries and the inability to sign binding contracts in the absence of the rule of law. Economic theory has made less progress, however, on the thornier (but equally important) problems of why armed groups form and cohere, and why individuals decide to fight. Likewise, the actual behavior of armed organizations and their leaders is poorly understood. On the empirical side, a vast cross-country econometric literature has aimed to identify the causes of civil war. While most work is plagued by econometric identification problems, low per capita incomes, slow economic growth and geographic conditions favoring insurgency are the factors most robustly linked to civil war. We argue that microlevel analysis and data are needed to truly decipher war's causes, and understand the recruitment, organization, and conduct of armed groups. Recent advances in this area are highlighted. Finally, turning to the economic legacies of war, we frame the literature in terms of neoclassical economic growth theory. Emerging stylized facts include the ability of some economies to experience rapid macroeconomic recoveries, while certain human capital impacts appear more persistent. Yet econometric identification has not been adequately addressed, and there is little consensus on the most effective policies to avert conflicts or promote postwar recovery. The evidence is weakest where it is arguably most important: in understanding civil wars' effects on institutions, technology, and social norms.

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# 1 Civil war and the study of economics

Internal civil conflict has been commonplace during the past half-century, a fact that, until recently, escaped the notice of most economists. *Civil wars*, or those internal conflicts that count more than 1,000 battle deaths in a single year, have afflicted a third of all nations. Counting *civil conflicts*, or those that count at least 25 battle deaths per annum, increases the incidence to more than half.<sup>3</sup> This internal warfare is not just extremely common, it is also persistent. Figure 1 displays the cumulative proportion of all nations experiencing wars and conflicts since 1960. Twenty percent of nations have experienced at least 10 years of civil war during the period.

The proportion of countries embroiled in civil conflict at a single point in time increased steadily through the last half of the 20<sup>th</sup> century, peaking in the 1990s at over 20% (see Figure 2). In sub-Saharan Africa, the world's poorest region, nearly a third of countries had active civil wars or conflicts during the mid-1990s.<sup>4</sup> The prevalence of war prompted scholars to ask a simple question: why there is so much civil war in the world? A decade later, observers began to ask where some of the civil wars had gone; there were "just" 32 active conflicts in 2006, the result of a steady decline in conflict from a peak of 51 in 1992.

The outbreak of internal wars is commonly attributed by poverty. Indeed, the correlation between low per capita incomes and higher propensities for internal war is one of the most robust empirical relationships in the literature. Figure 3 illustrates the relationship between per capita

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<sup>3</sup> These definitions come from the well-known UCDP/PRIO dataset developed by Gleditsch, Wallensteen, Eriksson, Sollenberg, and Strand (2002) and extended in Harbom and Wallensteen (2007). UCDP/PRIO defines conflict as "a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths." As noted below, the definition and coding of civil war is contested, but our main points are robust to alternative approaches.

<sup>4</sup> The proportion of individuals directly affected by war violence is lower than suggested by this figure, as many armed conflicts are confined to sub-regions. Yet even individuals living in largely peaceful regions can be adversely affected by insecurity, the public policy changes, and economic consequences of a civil war.

income (percentiles) and civil war using a non-parametric Fan regression; countries towards the bottom of the world income distribution – many in Africa – have several times more wars than those in the top quartile, while middle income countries still face considerable conflict risk.

Yet claims of a direct causal line from poverty to conflict should be greeted with caution. One reason is that this line can be drawn in reverse. Conflicts devastate life, health, and living standards. A chilling example is the Democratic Republic of Congo, where surveys suggest millions may have died as a result of the recent civil war, primarily due to hunger and disease (Coghlan, Ngoy, Mulumba, et al. 2007). Although the accuracy of mortality figures in such war zones is open to question, estimated mortality figures for Rwanda, Angola, and Sudan are likewise shocking. Massive loss of life inevitably affects the economy. Warfare also destroys physical infrastructure and human capital, as well as possibly altering some social and political institutions. Moreover, internal wars are contagious; refugee flows, disease, lawlessness, and the illicit trades in drugs, arms, and minerals have generated “spillover” effects into the countries neighboring conflict zones. Some have argued that the destructive consequences of internal warfare may be so great as to potentially be a factor in the growing income gap between the world’s richest and poorest nations (Collier, Elliot, Hegre, et al. 2003).

A seeming paradox, however, is that warfare is also sometimes credited for the technological and institutional development that underpins Western economic prosperity. Both internal and external wars are commonplace in European history. Several scholars have claimed that interstate wars and wars of territorial conquest served a critical role in enabling the development of strong and capable government institutions in Europe (e.g. Acemoglu and Robinson 2006; Ferguson 2002; Tilly 1975; Tilly 1992). The evidence on institution-building and internal warfare is limited, but cases of stronger states emerging out of contemporary civil wars also exist in East

Africa and Southeast Asia (Slater 2005; Weinstein 2005a).

It seems clear to us that civil war ought to be central in the study of international economic development. Yet leading development economists have too often overlooked it; for instance, two respected and widely taught undergraduate development economics textbooks (Ray 1998; Todaro 1999) do not contain the words “war”, “conflict” or “violence” in their subject index. Moreover, a 2007 survey (by the authors) of 63 development economics course syllabi in leading U.S. universities reveals that only 13% of undergraduate courses and 24% of graduate courses mention any of these topics at all.<sup>5</sup> Over the past decade, however, many economists and other social scientists have worked to better understand the causes and the economic legacies of internal warfare, often in collaboration with political scientists and other scholars. This article’s main goal is to summarize this progress and help chart a productive path forward. As befits an emerging field, this article focuses as much on what we cannot say today as what we know.

Unfortunately, our survey deals too briefly with important topics such as civil war endings and duration, post-war reconstruction, and the emergence of peaceable institutions, in part because these sub-literatures are still largely in flux. We must also neglect related forms of violence – interstate war, terrorism, coups, communal violence, political repression and crime – to keep this article a reasonable length. This is a pity because the distinction between civil wars and other forms of political instability has largely been assumed rather than demonstrated.<sup>6</sup>

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<sup>5</sup> The survey included 38 undergraduate and 25 graduate syllabi. We searched for online syllabi for undergraduate institutions ranked in the top 50 of the U.S. News and World Report college rankings (2007a), and for Ph.D. economics programs ranked in the top 25 of either the National Research Council (1995), Chronicle of Higher Education (2007), or U.S. News and World Report economics PhD (2007b) ranking. Details are available upon request.

<sup>6</sup> Related literatures investigate the logic and organization of terrorism, including: self-selection and screening of terrorist recruits (Buono de Mesquita 2005); why radical religious clubs specialize in suicide attacks (Berman and Laitin 2008); how terrorist organizations use bureaucracy to align the asymmetric preferences for violence among leaders and operatives (Shapiro 2008); the economic logic of hostage-taking and government response (Sandler and Enders 2004); the splintering and ideology of terrorist groups (Buono de Mesquita 2007); the logic of suicide missions (Gambetta 2005); and why terrorists employ roadside bombs (Hanson 2008). The line between rebel and ter-

Our principal conclusions challenge researchers to focus on new questions, econometric methods, and data. First, beginning with the origins of conflict, we argue that existing theory is incomplete. Central theoretical problems remain unresolved, including the sources of armed group cohesion amid pervasive collective action problems. Moreover, we have yet to develop persuasive arguments for non-traditional mechanisms—myopic or selfish leaders, for example, or the role of ideology and identity in reducing free-riding within armed groups. As a consequence, too little empirical work is motivated by (and explicitly derived from) formal models.

Second, the leading existing theories remain untested. Simple contest models—ones that link conflict to geographic conditions that favor insurgency, or ones where poverty triggers political violence—have been tested often. Yet one of the most dominant rational explanations for civil war, conflict as the result of commitment problems that prevent socially desirable agreements between fighting sides, has barely been examined.

Third, theories seldom specify the empirical predictions that can test between competing accounts. What, for instance, is the alternative to purely rational theories of warfare? “Irrational” warfare? In fact, there are several plausible alternatives: rational actors who do not internalize the social costs of war; maximizing actors with systematic defects in decision making or expectations formation; strategic interactions between multiple actors within coalitions; idiosyncratic war (described below); and so forth. Theory will lead to better empirical testing if and when it better specifies the empirical predictions that distinguish between models.

Fourth, further cross-country regressions will only be useful if they distinguish between competing explanations using more credible econometric methods for establishing causality. Up to

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rorist groups is blurry, and many of the lessons we draw may apply to terrorism. Further theoretical work laying out the analytical distinctiveness of civil wars versus terrorism and other forms of political violence would be useful.

now this literature has been enormously provocative but has faced equally important limitations: convincing causal identification of key relationships is rare; robustness to alternative specifications or assumptions is seldom explored; country-years are often assumed to be independent units in time and space; measurement error is rarely addressed; an absence of evidence about particular effects has often been interpreted as evidence of absence; and theories of individual or armed group behavior are tested at the country level despite obvious aggregation difficulties. It would be easy to conclude that the cross-country literature has been exhausted, but that would go too far. We highlight new macro-level research that addresses some of these challenges head on.

Fifth, we believe the most promising avenue for new empirical research is on the sub-national scale, analyzing conflict causes, conduct, and consequences at the level of armed groups, communities and individuals. We refer mainly to the blossoming number of microeconomic statistical studies of armed conflict and combatants, as well as the integration of quantitative evidence with case and historical analysis. The empirical microeconomic work sometimes employs more credible research designs, yet so far the results are scattered and many findings may be context-dependent. More studies are exactly what is needed. In our view, the most interesting directions for research include the internal organization of armed groups, rebel governance of civilians, the strategic use of violence, counter-insurgency strategy, and the roots of individual participation in violent collective action. Each is ripe for the concerted application of contract theory and mechanism design and insights from behavioral economics and industrial organization.

Sixth, we argue that researchers ought to take a more systematic approach to understanding war's economic consequences. An episode of civil conflict, not its absence, is the norm in most countries, and that war may be a nation's singular historical event. Yet what those effects imply for long-run economic development is unclear. This article also attempts to bring a unifying

growth theory framework to the study of war's economic legacies. The bulk of existing evidence focuses on war's impacts on factors of production—population and capital—and finds that rapid recovery along these dimensions is possible. War's impacts on human capital (including education, nutrition, and health), however, are often more persistent. Like the 'causes' literature, research into 'consequences' is beginning to benefit from better micro-level data and greater use of experimental or quasi-experimental variation. Viewed through the lens of economic growth theory, however, there remain more gaps than solid conclusions in our understanding of postwar recovery. Both theory and evidence are weakest in assessing the impact of civil war on the fundamental drivers of long-run economic performance—institutions, technology, and culture—even though these may govern whether a society recovers, stagnates, or plunges back into war.

Finally, in pursuit of all these objectives, much is to be gained from collecting new data. We conclude our review with recent examples and priorities for data development.

We share a title with a useful recent survey by Collier and Hoeffler (2007) but have different goals. Collier and Hoeffler focus in-depth on a set of core macroeconomic questions. Our piece brings in a broader range of research questions and approaches, including an overview of the large conflict literature in political science, and a critical but hopeful view of the new applied microeconomic work in conflict, probably the single most promising research frontier in our view. We also focus on the theoretical and econometric limitations of existing work. We believe a number of the arguments in this article are novel or have never before been assembled in a single place, including the discussion of specific directions for future research.

The rest of the paper is organized as follows. On civil war causes, Section 2 surveys theoretical advances and Section 3 covers the large empirical literature. Section 4 tackles the growing literature on civil war's economic consequences. The final section summarizes key lessons and



policy implications, and suggests strategies to sustain intellectual progress in this emerging field.

## 2 Theories of armed conflict

Newspaper reports, historical accounts, and econometric work overflow with explanations for conflict: ancient hatreds incite violence; oil wealth breeds separatism; trade shocks trigger insurrections; income inequality leads to class warfare. Surveying the vast literature on civil war, one feels caught in a complex web of root and proximate causes (not to mention endogeneity). In this context, the principal contribution of formal economic theory has been to clarify and systematize this tangle of material explanations. Models from both economics and political science have reduced varied accounts of civil war onset to a few common logics, each of which can be approximated in a parsimonious framework of self-interested, wealth-maximizing groups or individuals. We first review the seminal theories of civil war, then other influential branches of the theoretical literature, and wrap up this section with our views on promising directions.

### 2.1 Insurrection as competition for resources

Models of armed conflict depart from the assumptions of standard economic theory in at least three ways: property rights are neither well-defined nor automatically protected, contracts between parties cannot be enforced, and rulers can be replaced by means other than the ballot box. In this lawless setting, predation and defense are alternatives to directly productive activities.

The contest model, the workhorse of the formal conflict literature, originated with Haavelmo (1954), and was popularized by Hirshleifer (1988; 1989), Garfinkel (1990), and Skaperdas (1992). It considers two competing parties, a rebel group and a government, and analyzes each side's allocation of resources to production versus appropriation; Garfinkel and Skaperdas

(2007) summarize the permutations and mechanics of two-party contest models embedded in a general equilibrium framework, while production is modeled in the standard manner, appropriation is modeled using a “contest success function” where inputs (e.g. guns,  $G$ ) translate into a probability of fighting side 1 winning,  $p_1$ , and consuming the opponent (side 2’s) economic production in addition to their own. Following Hirshleifer (1989), the most commonly used formulation in theoretical applications is presented in equation 1, where  $G_1$  refers to side 1’s weapons,  $G_2$  refers to 2’s weapons, and  $m$  captures the effectiveness of weaponry in determining the victor:

$$(1) \quad p_1(G_1, G_2) = \frac{G_1^m}{G_1^m + G_2^m}$$

Contest models boast at least one robust prediction: the odds of winning increase with the relative effectiveness of that side’s fighting technology. Technology is defined broadly in this literature, including any factor that influences effectiveness, from skillful revolutionary leaders, to access to firearms and training, rugged terrain, or bases on foreign soil. As we will see below, this prediction receives broad empirical support in the success of rebel movements, contributing to the popularity of the contest approach.

Contest models often treat rebels and rulers as unitary actors. Grossman (1991) departs slightly, considering the case of a single ruler and many citizens, each of whom can either produce or predate.<sup>7</sup> Grossman’s move from unitary actors to representative households (assumed unable to coordinate their activities) does not greatly change the conclusions of the contest model, but it does highlight the importance of the individual participation problem: armed group leaders must motivate citizens to soldier for their side. One immediate insight is that participation in soldiering rises as the opportunity cost of fighting falls.

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<sup>7</sup> Another approach considers a rebel leader who competes with the incumbent for citizen support (Grossman 1999).

These models thus predict that poverty lowers individual incentives for maintaining order, as soldiering increases with the relative returns to fighting versus production. Can this prediction account for the cross-country correlation between poverty and civil war? In fact, the theoretical connection between income and armed civil conflict is not so clear cut. In contest models the winning party consumes the resources of both the state and the losers. On the one hand, the greater the national wealth (whether from taxes, assets like natural resources, or external transfers), the more there is to fight over and thus, in standard formulations, the greater the equilibrium effort devoted to fighting rather than producing (e.g. Garfinkel and Skaperdas 2007; Grossman 1999). Yet the absence of resources – natural or otherwise – makes production less individually attractive than fighting, but also means there is a smaller pie to fight over. Fearon (2007) notes that these opposing wealth effects cancel out in some cases: if state revenues are drawn entirely from taxes on citizen incomes, then income could have no effect on equilibrium levels of conflict with risk-neutral agents. Positive or negative income effects could result, though, if utility or revenue collection have nonlinear functional forms.

Dal Bó and Dal Bó (2004) model these potentially opposing effects in a two-sector model of the economy. In the capital-intensive sector, an income shock increases the value of controlling the state without increasing wages and the opportunity cost of fighting; the opposite is true of a shock to the labor-intensive sector. Thus in the first case conflict risk increases, while in the second it falls. Besley and Persson (2008a) use a related framework to model the impact of import and export commodity prices on government revenues and rents, as well as on labor incomes. They conclude that terms of trade volatility in either direction may stimulate armed conflict: increasing import prices increase conflict risk by suppressing the real wage while higher export prices lead to greater conflict risk by boosting the size of the government revenue pie tempting

armed groups. A notable aspect is the authors' attempt to link these sharp theoretical predictions to cross-country evidence, and we return to their empirical findings below. Both papers also suggest that the distribution of income and wealth—whether across individuals or sectors—is central in explaining the economic incentives for rebellion.<sup>8</sup> Civil war seems more likely when state wealth is easily appropriated or divorced from the citizenry, as with some natural resource wealth and foreign aid flows. We revisit this issue below.

## 2.2 Why fight? Information asymmetry and incomplete contracting

One drawback of the typical contest model is that insurrection is never fully deterred; arming and fighting always occurs in equilibrium. There is typically no decision to fight: arming and fighting are one and the same. This prediction of ever-present conflict is unsatisfying since political competition over power and resources is ubiquitous while violent conflict is not. Thus we turn to the determinants of compromise (and its breakdown).

Creating and arming organizations is costly and wars are destructive and risky. Thus a fundamental question is why wars ever occur at all. If the competing groups are rational, both should prefer a bargained solution to destructive conflict.

The possibility of bargaining under the threat of violence is embedded in leading theories of political and institutional development.<sup>9</sup> Acemoglu and Robinson (2001; 2006), for instance, develop a model of elites competing with the poor for control of the state. Elites accommodate the poor by extending the voting franchise in periods when the poor can credibly threaten to revolt,

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<sup>8</sup> See Benabou (2000) and Banerjee and Duflo (2003) on inequality and economic performance across countries.

<sup>9</sup> Bargaining models of conflict proceed from microeconomic theories of bargaining where parties have the option of resorting to costly conflict if bargaining breaks down (see Kennan and Wilson 1993 for a comprehensive survey). Union-firm wage negotiation and pre-trial legal settlement in wealthy countries have been the two most studied cases. Conflict models, however, do not assume that contracts will be enforced once signed, further complicating the negotiation. Posen (1993) presents a theoretical international relations perspective on ethnic conflict.

and there is no violent conflict on the equilibrium path. Boix (2003) develops a related model, where conflict outbreaks depend on shifts in the military capacity of a revolutionary challenger. Fearon (1995) famously outlined three reasons why bargaining could fail, leading to inter-state war. First, leaders may not always behave rationally—decisions might be based on emotion, or leaders may not fully calculate benefits and risks (bounded rationality). Second, leaders may be fully rational but not internalize the full cost of conflict because of political agency problems. Third, leaders might be rational and internalize costs, but find war unavoidable nonetheless.

Almost all theoretical work focuses on this third case. Fearon highlights three mechanisms consistent with “rational war”: (i) *asymmetric information*, including private information about military strength, and the strategic incentive to misrepresent it to potential opponents;<sup>10</sup> (ii) *commitment problems*, especially the inability of the parties to commit to deals in the absence of a third-party enforcer; and (iii) *issue indivisibilities*, whereby some issues do not admit compromise. We will follow the literature and focus attention on the first two.<sup>11</sup>

### *Information asymmetries*

War can occur when one side overestimates its ability to win, or underestimates its opponent’s strength (Powell 2002). But asymmetric information is generally insufficient cause for war. After all, if both parties have an incentive to make a deal, they should also have incentives to gather information and communicate their strengths (Fearon 1995). For asymmetries to cause war among rational actors, accurate disclosure of information must also be impaired. An incentive to

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<sup>10</sup> This argument appears to originate with Brito and Intriligator (1985).

<sup>11</sup> Issue indivisibilities are considered a relatively minor explanation in most cases. However, Hassner (2003) argues that the indivisibility of sacred spaces may be one reason for the persistence of conflict between Israelis and Palestinians or Hindus and Muslims in India. Powell (2006) argues, however, that indivisibilities are merely a special case of the commitment problem; if commitment were possible, both sides would prefer a lottery that awards the contested indivisible prize with the same odds as fighting, thus avoiding war’s destruction.

misrepresent one's strength is the most commonly theorized mechanism, such as when a state exaggerates its strength and engages in (inefficient) war in order to deter future opponents from insurrection.<sup>12</sup> To take an interstate war case as an example, Saddam Hussein's exaggeration of Iraq's stock of weapons of mass destruction in 2002 could be seen as an effort to mislead opponents and deter invasion—an effort that, nevertheless, failed.

Such accounts are plausible but likely offer only half an explanation. For one, relative military strength should reveal itself quickly on the battlefield. Information problems thus provide a particularly poor account of the many prolonged civil conflicts (Fearon 2004; Powell 2006).

Most models also assume just two actors but civil wars are seldom so simple. Esteban and Ray (2008) develop a multiplayer contest, where each has imperfect information about the others' costs of conflict. With four or more players, Pareto-improving social decision-making is impossible and conflict ensues. Thus information asymmetries may be even more hazardous than the basic two-player models would suggest. Ray (2009) goes on to argue that conflict may be unavoidable even in a world with complete contracts. Societies divide along multiple lines—by class, geography, religion, or ethnicity, and while society can arrange a set of transfers that avoids a conflict along any one division, it may be impossible to find an arrangement that simultaneously prevents conflicts along all divisions simultaneously.<sup>13</sup>

Also promising are recent attempts to integrate asymmetric information with other theoretical mechanisms. Chassang and Padro-i-Miquel's (2008a; b; 2009) work incorporates such asymme-

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<sup>12</sup> For instance, Walter (2006) shows that ethnic groups are more likely to seek self-determination if a government has acquiesced to earlier autonomy demands by other groups. A government that takes such future externalities into account might find it worthwhile to fight a costly war today to prevent secession, thus possibly heading off future conflicts. The relationship also suggests that war may not always be accurately modeled as a two-player game.

<sup>13</sup> Motivated by warlord politics in Tajikistan and Georgia, Driscoll (2008) models bargaining between a president and multiple challengers.

try into a contest model employing a global games logic.<sup>14</sup> Their key insight is that transient economic shocks increase the immediate incentives to fight but not the discounted present value of victory. The model thus implies that in dire economic circumstances groups predate upon one another since they have less to lose than in periods where the returns to production are higher. Yet conflict is also possible in better economic times as asymmetric information on the true economic conditions, and first-strike advantages on the battlefield, combine to generate mutual fears of preemptive attacks. The framework is notable for its testable predictions: armed conflicts should follow negative economic shocks; higher and less volatile national incomes are associated with less conflict; and expected future income growth reduces the risk of war today.

Dal Bó and Powell (2007) also show that asymmetric information can lead to war in the context of pervasive commitment problems without relying on global games. The government has better information on its wealth than challengers. As in Chassang and Padro-i-Miquel, they show that government attempts to buy off the opposition (and avoid conflict by offering them a share of the pie) fail in periods of lower economic activity, since the challenger fears that the government is low-balling them. Commitment problems could further restrict the government's ability to secure peace by incorporating the challenger into a power-sharing government (since this would provide the challenger with a stronger position for future aggression). Baliga and Sjöström (2004) develop a related imperfect commitment model, in which private information about each fighting side's propensity to arm can lead to arms races with probability close to one.

### *Commitment problems and incomplete contracting*

The most intriguing theories of civil war focus on the cases where credible commitments to

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<sup>14</sup> Global games are associated with the work of Carlsson and van Damme (1993) and Morris and Shin (1998).

peace or redistribution cannot be made even with complete information—that is, at least one side faces an incentive to renege once a settlement is reached (Walter 1997). Such circumstances include military scenarios with a first-strike advantage, and instances where waging war today can prevent one’s opponent from gaining military strength in the future.

Powell (2006) shows formally that each of these commitment problems is rooted in a single phenomenon: large shifts in the future distribution of power. For a leading example, consider a temporarily weak government that is attempting to “buy off” a strong rebel group with transfers to secure peace. When the state returns to relative strength – perhaps because of a rebound in economic activity, foreign aid or commodity revenues – it will be tempted to renege on its earlier bargain, thus limiting the amount it can credibly promise to the rebel group today. If this time-consistent but more modest transfer is less than what the rebels can gain by fighting today, they will wage war now to lock in the highest possible payoff.

Similarly, a commitment problem arises when one party can permanently alter the strategic balance of power by waging war now (Garfinkel and Skaperdas 2000; McBride and Skaperdas 2007; Powell 2006). If going to war weakens or even eliminates a rebel group for all time, the state will gain a peace dividend since it no longer needs to spend on arms to deter future conflict. Thus the state has reason to wage bloody but short conflicts if peace deals are not credible.<sup>15</sup>

The commitment problem directly suggests that civil war is more likely to occur when there are limits to conflict resolution and contract enforcement. Since formal legal and state institutions presumably help to enforce commitments inter-temporally, societies with weak government

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<sup>15</sup> This approach suggests that the likelihood of war is affected by each side’s valuation of the future versus the present. The risk of future retaliation—the “shadow of the future”—should deter sides from conflict. When future returns depend on present success on the battlefield, however (i.e., first-strike advantages), the shadow the future can increase the incentives for conflict. It can also lead to war if a peace settlement is costly in terms of expected future defensive arming. Greater inter-temporal discounting increases the likelihood of war in the first case and decreases it in the second and third (McBride and Skaperdas 2007; Skaperdas and Syropoulos 1996).



institutions and few checks and balances on executive power should empirically be those most likely to experience violent civil conflict (e.g. Fearon and Laitin 2003; La Ferrara and Bates 2001; Skaperdas 2008). This relationship may partially explain the widespread occurrence of lengthy civil wars in sub-Saharan Africa, a region notorious for its weak state capacity and limited legal infrastructure (Bates 2001; Bates 2008; Herbst 2000).

Yet weak institutions and the absence of a third-party enforcer alone are not sufficient cause for civil conflict. The theory implies that conflict is at least twice conditional: first on weak institutions, and second, on future shifts in relative power across the fighting sides. Future empirical models must begin to take these issues into account more seriously in testing.

In terms of policy, the theory suggests that enforcement of contracts by the international community can potentially substitute for weak domestic institutions (Walter 1997). Interventions might include armed peacekeepers, the provision of guaranteed financial transfers to rebels by outside international agencies, and the threat of punishment (including trade sanctions, asset freezes, and bombing) if either side reneges on the peace deal.

External interventions could also have the opposite effect, however, and prevent an ongoing war from reaching a credible peace agreement. For instance, the recent prosecution of Charles Taylor (former warlord and President of Liberia) and indictment of Joseph Kony (head of Uganda's Lord's Resistance Army) by international courts could make post-war power-sharing deals for rebels less credible in the future, and thus extend current civil wars if the rebels have no guarantee that putting down arms will shield them from prosecution in The Hague. On the other hand, the possibility that an international indictment could be dropped appears to have been one of the primary incentives for Kony to agree to a ceasefire and begin negotiating peace in the first place. We discuss the scattered empirical evidence on international interventions below.

## 2.3 The micro-foundations of group conflict

Contest models and rationalist theories of civil war rely upon groups behaving as unitary actors, strong assumptions considering the well-known problem of collective action (Olson 1971). To understand the causes of war, we must also understand how groups form, cohere, and persuade their members to risk their lives.

### *Civil war and the participation problem*

Classic solutions to the collective action problem use ‘selective incentives’ to motivate participation, with material and pecuniary incentives the focus of most models (e.g. Grossman 1999). Such incentives include wages, opportunities to loot, promises of future reward, or physical protection from harm. Economic inequality provides a possible motive for conflict to the extent that seizure of the state brings material gains to the victors (Fearon 2007).

A literature on agrarian revolutions in the 1960s and 1970s argues instead that inequality motivates participation in rebellion not for private gain, but because it generates frustration over inequality or the destabilization of traditional social systems (Davies 1962; Gurr 1971; Paige 1975; Petersen 2001; Scott 1976). By these accounts, poverty, income inequality, and unmet economic expectations may indeed be the root causes of conflict, but the more proximate explanations are better described as grievances. Rather than deny material motivations, these accounts provide an alternative set of mechanisms for individual participation in rebellion.

Related non-material incentives are thought to be common within armed groups. Several studies argue that a leader’s charisma, group ideology, or a citizen’s satisfaction in pursuing justice (or vengeance) can also help solve the problem of collective action in rebellion (e.g. Roemer 1985; Wood 2003b). These unconventional incentives have typically been the subject of sociol-

ogy (Etzioni 1975; Olson 1971). A convenient way of modeling such sentiments in a rational framework is as ‘goods’ of inherent value that individuals consume by fighting.<sup>16</sup>

What these micro-level approaches often ignore, however, is that fighting is not the only means by which individuals and groups can pursue political and economic change. Walter (2004) argues that the absence of a non-violent means for achieving change is also often necessary to incite rebellion. Non-violent political alternatives could be incorporated into the decision framework facing citizens, leaders, and armed groups, generating testable predictions about the relationship between political institutions and the likelihood of civil conflict.<sup>17</sup>

Such diverse selective incentives—pecuniary or not—are easily embedded in a principal-agent framework. A leading example is Gates (2002), who models how rebel leaders can use material incentives alongside ethnic appeals to motivate citizens to join and exert effort in the rebellion (i.e., to satisfy the participation and incentive compatibility constraints). His model emphasizes how incentives and methods of recruitment vary with ease of supervision; the greater the distance—whether geographic or social—between the leader and the recruit, the more difficult is supervision and punishment, and the more likely that material incentives (e.g. looting) will need to be offered to recruits to secure their cooperation.

Threats and punishments can also be used as selective incentives. Coercive recruitment is especially common in African insurgencies where, in the absence of a shared social basis for mobilizing rural support, rebel leaders resort to the only tool at their disposal (Mkandawire 2002). Chwe (1990) and Beber and Blattman (2008) model the use of coercion and pain in a principal-

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<sup>16</sup> The approach closely parallels two literatures. One is a branch of the voting literature that suggests the collective action problem inherent in participation in democratic elections (where the odds of affecting the outcome are infinitesimal but voting has concrete costs) is overcome by the value some individuals place on the act of voting itself (Dhillon and Peralta 2002; Feddersen 2004). A literature on revolution emphasizes the inherent value individuals place on retaliation against an unjust state (Fanon 1968) or ethnic solidarity (Mueller 2000), to give two examples.

<sup>17</sup> Ellman and Wantchekon (2000) is a useful step in this direction.

agent setting, and identify the conditions (and agent types) where it is optimal for armed group leaders to threaten pain instead of offering rewards. This consumption approach to non-pecuniary incentives is analytically convenient and yields useful insights, such as the rationale for using coercion on low productivity recruits (especially children). Yet these models are unlikely to capture the complex individual motivations underlying participation in armed groups, however, and thus constitute an important area for further research.

### *The formation of competing coalitions*

The models reviewed assume that rebel and government groups exist and are actively engaged in combat. They do not tackle the issue of how competing groups form and why they cohere. An emerging literature based on the non-cooperative theory of endogenous coalitions explores the distributional basis of group formation. These models typically assume that group action is more efficient than individual action, providing citizens with an incentive to join forces. These models also allow for conflict within each group over the distribution of their joint surplus, conflict that can be costly for the individual. Stable groups are those that have low-cost mechanisms for distributing the gains, such as property rights norms. The size of stable groups depends on the relative effectiveness of groups at managing both inter-group and intra-group conflict (e.g. Bloch, Sánchez-Pagés, and Soubeyran 2006; Garfinkel 2004). This approach is a promising source of micro-foundations for the commitment problems discussed above, since the institutions that allow for within-group cooperation may also mitigate inter-group conflict.

Relaxing the unitary actor assumption could also expand the range of rational explanations for armed conflict. Information problems within groups could lead to bargaining breakdowns (just as was the case for asymmetric information across groups). Field generals have incentives to mis-

lead civilian leaders about the capability of their military forces if they hope to keep the fighting going for longer than citizens would like (to keep military budgets at high levels, for instance).

Alternatively, the possibility that groups might split could exacerbate commitment problems: signing a peace deal with a rebel group leader is of limited value if hard-liners are able to secede and continue fighting.<sup>18</sup> The existence of splinter factions may explain the reluctance of fighting sides to enter into peace talks and cause such talks to fail. Stedman (1997) argues that the greatest risk for peace negotiations comes from such ‘spoilers’: “leaders and parties who believe that peace emerging from negotiations threatens their power, worldview, and interests, and use violence to undermine attempts to achieve it.” (p.5). Ray (2007; 2009) and Acemoglu, Egorov, and Sonin (2009) discuss coalitional stability when such deviations and counter-deviations can occur.

### *Ethnic groups and conflict*

Ethnic nationalism is popularly viewed as *the* leading source of group cohesion and (by extension) inter-group civil conflict; of 709 minority ethnic groups identified around the world, at least 100 had members engage in an ethnically-based rebellion against the state during 1945 to 1998 (Fearon 2006). But why do ethnic groups themselves form, cohere, and sometimes engage in such violence? A full review of the literature on the formation of ethnicity and ethnic conflict is beyond the scope of this paper, but an outline of the main ideas merits discussion.<sup>19</sup>

‘Primordialist’ arguments stress the deep cultural, biological or psychological nature of ethnic cleavages, whereby conflict is rooted in intense emotional reactions and feelings of mutual threat (Horowitz 1985). Economic models that assume individuals prefer to mingle with co-ethnics (or

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<sup>18</sup> For example, Kalyvas (2000) argues that internal divisions between moderates and radicals within Algeria’s Islamist FIS party, and the moderates’ inability to make binding policy commitments to reassure anti-Islamist elements of the national army, contributed to the outbreak of civil war there after FIS won the 1991 national election.

<sup>19</sup> For overviews of ethnic mobilization and violence see Laitin (2007), Fearon (2006), and Bates (2008). On ethnic divisions and economic performance, see Alesina and La Ferrara (2005) and Bardhan (1997; 2004).

share political preferences) might be construed as primordialist in nature (Alesina, Baqir, and Easterly 1999; Alesina and La Ferrara 2000; Esteban and Ray 1999). There are clear parallels to the models of group formation discussed above: co-ethnic preferences can augment intra-group mechanisms of communication and cooperation, while inter-ethnic animosities may exacerbate information and commitment problems.<sup>20</sup>

Even if ethnic identities are not primordial and inter-ethnic animosities are absent, ethnicity may still facilitate strategic coordination and enforcement. Ethnic groups often exhibit dense social networks and low cost information and sanctioning, and may have identifiable characteristics that allow outsiders to be excluded from public goods (Caselli and Coleman 2006; Fearon and Laitin 1996; Miguel and Gugerty 2005). Fearon and Laitin (1996) show that better within-group cohesion can facilitate peace deals between ethnic groups. Alesina and La Ferrara (2005) also speculate that ethnically homogenous groups possess a production advantage that augments their incentives to associate. Bates (1986) argues that shared language and customs facilitate organization. Finally, Esteban and Ray (2008) suggest that ethnic alliances have a distinct advantage over class alliances in mobilizing for conflict. While class and ethnic groups both possess shared social identities, only ethnic groups exhibit within-group economic inequality: inequality allows the rich to supply conflict capital (e.g., guns) while the poor supply conflict labor.

Finally, ‘modernist’ theories stress that ethnic conflict arises when groups excluded from social and political power begin to experience economic modernization (Bates 1986; Gellner 1983)—a situation that parallels Powell’s (2006) account of shifts in future power leading to bargaining breakdowns today.

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<sup>20</sup> Alternatively, as with the grievances discussed above, ethnic violence might have inherent utility value. In the extreme case, we could even reject the rationalist assumption entirely that opposing ethnic groups prefer to reach a peaceful solution. However, we believe the goals of formal economic theory here should go beyond simply assuming that a taste for violence drives civil conflict, to uncover the deeper economic, political and social factors at play.

## 2.4 Challenges and areas for further work

Most real-world disputes are settled, even among antagonistic ethnic groups. Thus the theoretical apparatus described above is plausible: conflict is rooted in endemic competition for resources across groups, with bargained solutions occasionally breaking down because of commitment or information problems. Persuasive though this framework may be in many circumstances, there remain many challenges and areas for further theoretical investigation.

### *Disentangling competing accounts*

Existing formal theories of conflict yield falsifiable predictions, but few articulate the precise empirical tests that would distinguish among alternative mechanisms. Income volatility is one example. In the theories we consider above, a negative aggregate income shock is associated with an increase in armed conflict in various models, including those that emphasize the diminished opportunity costs of soldiering (Gates 2002), weaker state repressive capacity (Fearon and Laitin 2003), or the role of asymmetric information (Chassang and Padro-i-Miquel 2008a). Meanwhile, a negative aggregate income shock is associated with a *decrease* in conflict risk in models that stress capturing the state and its revenues as a prize (e.g. Garfinkel and Skaperdas 2007; Grossman 1999). Finally, income volatility in either direction could inhibit credible bargaining and commitments if it is associated with rapid shifts in power across groups (Powell 2006), or gives rise to worse information about current economic conditions (Chassang and Padro-i-Miquel 2009, Dal Bo and Powell 2007). Few theories model more than one of these dynamics or identify the empirical predictions that will adjudicate among competing accounts.

One exception is Dal Bó and Dal Bó (2004), who distinguish between shocks to different economic sectors. This theory is an improvement over single-sector models, yet even so, alterna-

tive mechanisms and interpretations are still possible. For instance, if higher capital-intensive good prices fail to increase conflict, it might be because greater state capacity (associated with higher government revenue) dominates the state-as-prize effect. If civil war is the result of a bargaining breakdown, there are good theoretical reasons to believe that events such as price shocks have differential effects on civil conflict depending on the local institutional setting, the number of already existing armed groups, and the future shifts in power across political groups likely to result. Future theoretical work should follow the lead of this paper in helping us pinpoint the empirical patterns that distinguish between alternative mechanisms.

### *Understanding grievances*

At present, the economic motivations for conflict are better theorized than psychological or sociological factors. Individual preferences in existing models typically include only material rewards and punishments. One key implication is that we have not derived the falsifiable predictions that distinguish between material and non-material theoretical accounts. Yet the greater degree of existing theory on economic factors does not imply that researchers should discard non-economic explanations for conflict. Take the role of economic inequality, for example. The unequal distribution of resources can generate material incentives for a relatively poor group to seize control of the state. More than one historical account, however, emphasizes citizens' emotional and ideological outrage over inequality as a prime motivation for engaging in violent collective action.<sup>21</sup> While the reduced-form prediction that greater economic inequality leads to armed conflict is unchanged in either case, the relationship could be interpreted as evidence of either "greed" (economic motivations) or "grievance."

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<sup>21</sup> Barrington Moore (1993), for instance, has argued that Nazi fascism and anti-capitalist rhetoric stirred anger in German peasants over the perceived control of resources by a supposedly hostile Jewish elite. A similar dynamic was at work during the anti-Tutsi genocide in Rwanda in 1994 (Straus 2006).



Cramer (2002) critiques the conflict literature for its tendency to use such reduced-form empirical relationships to buttress economic interpretations, arguing that the underlying relationships between economic, social, and psychological factors are far more complex. He stresses Gramsci's (1971) definition of 'economism': presenting causes as immediately operative that in fact only operate indirectly, and thus overstating proximate causation. Understanding these complex relationships is crucially important for preventing armed conflicts. Innovative ways of modeling and measuring individual political grievances are required to make progress on this agenda. Yet in the end, our measures may fail to capture the relevant variation. Grievances are fluid and the case literature points to the evolution of identities and norms during wartime (Wood 2003a).

Recent behavioral and experimental economic research argues that notions of fairness and grievance are salient in individual decision-making. There is growing lab evidence that individuals have a taste for punishing social norms violations and are willing to incur non-trivial private costs to do so. This willingness to punish unfair behavior appears to have neural-physiological underpinnings (de Quervain, Fischbacher, Treyer, et al. 2004) and is consistent with preferences for equity (Charness and Rabin 2002; Fehr and Schmidt 1999). Choi and Bowles (2007) argue that altruistic preferences favoring one's in-group may have conferred an evolutionary advantage. Such within-group social preferences could reduce the local collective action problem inherent in mobilizing armed groups by lowering the cost of sanctioning free riders.<sup>22</sup>

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<sup>22</sup> The taste for violence may differ from the taste for punishing others monetarily, but the experimental economics literature has not to our knowledge carried out similar research on individual preferences for inflicting violence on others. Given the inherent human subjects issues, observational data on perpetrators of violence is a more promising avenue here. Sociological and psychological understandings of inter-personal violence contrasts sharply with the rational choice approach we emphasize in this article. Sociologist Randall Collins (2008) argues that most inter-personal and combat violence is characterized by a short and confused belligerent "haze": actors are emotionally overwhelmed with tension and fear, and violence is perceived as the resolution of this fear. Meanwhile, some public health and psychology evidence suggests that much violence is "shame-induced" (Gilligan 2000).

### *Disaggregating institutions*

The commitment problem is a persuasive explanation for civil war. Unfortunately, we have a poor understanding of the specific political and legal institutions capable of enforcing commitments and facilitating compromise between competing groups. Some theories emphasize the importance of market promotion and tax levying (Besley and Persson 2008b), and others property rights and the rule of law (Garfinkel 2004). Others emphasize the role of international institutions and the threat of external intervention, others the internal legitimacy of the state (e.g. the rule of a minority ethnic group, whether in Tutsi Rwanda or apartheid South Africa, could be particularly destabilizing). Meanwhile, Powell (2006) emphasizes institutions that help manage rapid shifts in power, an example of which might be the ability of elites to extend or retract the democratic franchise, as in Acemoglu and Robinson (2001; 2006). The ‘institutions’ concept needs to be better disaggregated and tested to be useful in the civil war literature.

Weingast (1997), Bates (2008) and Bates, Greif and Singh (2002) argue that the incentives and constraints facing leaders are crucial, and in particular that rulers loot the state when the long-term costs of doing so are low. Institutions shape these costs as well as the ruler’s time horizon and discount rate. Paradoxically, institutions that extend a ruler’s horizon, such as the elimination of term limits or the weakening of political competition, may increase the incentives for supporting political order, so the ruler can extract rents over a longer time. Conversely, Bates (2008) argues that international pressure for African states to democratize in the 1990s increased disorder, since it shortened leaders’ horizons at the same time that foreign aid flows were reduced; with few institutional checks on their power, African rulers had incentives to predate.

These arguments meld with a growing comparative politics literature on state failure and ‘warlordism’ in the late 20<sup>th</sup> century. In a study of civil war in Liberia, Sierra Leone, and Guinea,

Sawyer (2004; 2005) emphasizes the large spoils from power combined with the absence of checks and balances on the executive as the primary cause of war in those nations. Reno (1999) also examines the internal dynamics of ‘warlord states’ in Sierra Leone, Nigeria, the Democratic Republic of Congo, and Liberia. Like Bates, Reno argues that, in the presence of resource wealth, a weakened state, less foreign aid, and pressures for economic liberalization, strongmen found it optimal to de-institutionalize the state and formal bureaucratic mechanisms in favor of a parallel “shadow state” under their own control.

### *The conduct and organization of civil war*

Another important area of study is the conduct of rebellion, investigating what factors and initial conditions influence a group’s formation, recruitment strategies, fighting tactics, and internal organization. One goal is to describe the logic of civil war and violence—a reaction to the view, popularized by journalism and some international relations scholars, that the brutal violence that characterizes much of modern civil warfare is a product of illogical barbarism unrestrained by economic, political or social structures (e.g. Kaldor 1999; Kaplan 1994).

Rebel groups are large, self-sustaining indigenous organizations in societies where effective organizations (including private firms) are rare; understanding the glue that holds them together should be a top research priority. One strand of recent research applies contract theory to theories of recruitment. Some of these, already discussed above, focus on how armed groups motivate recruits to fight (e.g. Beber and Blattman 2008; Gates 2002; 2004). Weinstein (2005b; 2007) develops a theory linking a rebel group’s social and economic endowments to its composition and tactics. He argues that groups rich in material resources are flooded with opportunistic joiners with little commitment to the civilian population, while armed organizations with ideological

“resources”, like a strong sense of common identity, tend to attract more committed soldiers.<sup>23</sup>

Armed group cohesion is the subject of a large body of work in military sociology and history. Rather than focusing on economic incentives, this literature emphasizes the powerful role of group socialization and social identity in generating solidarity, commitment, and a willingness to risk one’s life (see Kenny 2008 for a review). Influential organizational devices include the creation of new identities among recruits and unit solidarity (Gabriel and Savage 1979; Shils and Janowitz 1948) and systems of command and control (Rush 1999; 2001; Van Creveld 1983). This emphasis on organization-level dynamics in state militaries contrasts with the emphasis on individual-level motives often used to explain participation in non-state groups; we believe both literatures could gain from an exchange of perspectives. Two such crossovers are Gutiérrez Sanín (2008) who examines Colombian paramilitary and guerrilla groups, and Wood (2008), who discusses how armed groups have constructed identities and reconfigured social networks in El Salvador, Peru, Sierra Leone, and Sri Lanka.

Fearon (2007) asks why we tend to see the sustained survival of many small and lightly-armed guerrilla groups, each with little chance of capturing political power (Congo, Sudan and Uganda are countries where this has been true). He constructs a contest success function with decreasing returns to scale for rebels over some size range—in other words, above some size, each additional rebel increases the probability the rebel group is detected, denounced, or destroyed by the government, and this effect outweighs the fighting benefits of greater size (at least up to some point).<sup>24</sup> Powell (forthcoming) is perhaps the best articulated formal attempt to get

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<sup>23</sup> The contrast between the Revolutionary United Front (RUF), funded through diamond mining and smuggling, and the community-supported Civilian Defense Forces (CDF) in Sierra Leone’s recent civil war provides an illustration of this divergence. Smith, Gambette and Langley (2004) show that the RUF was much more likely to commit human rights abuses against civilians than the CDF. See Keen (2005) for a careful discussion of the Sierra Leone civil war.

<sup>24</sup> The sensitivity of results to such functional form assumptions calls out for more research investigating the micro-

inside the black box of armed groups' fighting strategies. He models optimal military spending across potential targets (e.g., cities or fighting units) by a government fearing rebel attack, and is able to decompose such spending into a defensive effect, a deterrence effect, and a cost effect.

Finally, other models help to explain rebel violence directed at civilians.<sup>25</sup> Azam (2002; 2006) formalizes a strategic logic whereby an armed group engages in looting to reduce the returns to non-military labor effort for potential recruits (thus making them more likely to join the group), while simultaneously generating spoils to reward existing recruits. The logic of violence against civilian populations is the subject of a growing literature in political science (see Kalyvas 2006 for a review). In work based on a comparative study of irregular civil wars (i.e., guerrilla wars) in the past century, Kalyvas (2006) argues that rival sides prefer to use selective rather than indiscriminate violence to punish “defectors,” or civilian enemies and informers. In the absence of information, both sides rely on local collaborators to denounce defectors. Kalyvas argues that selective violence – including violence related to private, not political motives – is most widespread in zones where each side holds significant force but lacks full control.<sup>26</sup> Recent studies examine the use of sexual violence by armed groups (Cohen 2008; Wood 2006; 2009).

This collection of theories just scratches the surface of the recruitment of fighters and organization of civil warfare. This area remains one of the most promising and understudied areas in the literature on conflict, and is ripe for the application of advances in contract theory, corporate finance, behavioral economics and industrial organization (Tirole 1988; 2006). New evidence to motivate and test these theories is discussed in section 3.

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foundations of contest success functions.

<sup>25</sup> See Straus (2007) for a review the related literature on the perpetrators of violence.

<sup>26</sup> In contrast, according to Kalyvas, the most heavily contested zones are likely to be relatively peaceful because denunciations will be deterred by the likelihood of immediate retribution.

### *Departures from the rational model*

As we discuss below, existing empirical models of conflict have limited explanatory and predictive power. We can draw at least three possible conclusions from their relatively weak performance. First, the determinants of war could be understood within standard rational choice frameworks but simply difficult to measure. In this case our prime focus as researchers should be to improve data and measurement, and to some extent this is already happening.

Second, war could have idiosyncratic causes, attributable to chance, singular circumstances, or unsystematic ‘irrational’ behaviors by leaders encompassing errors in decision-making, personality defects, and so forth (Gartzke 2003). Such an account is not inconsistent with formal theory. Models are seldom intended to be deterministic but rather to describe general tendencies. Civil war outbreak is a relatively rare event and thus it is conceivable that the basic formal logic is right but at least some civil wars are in fact costly mistakes. Indeed, the historical literature is replete with leaders’ passions, fallibility, and ideology; historians often attribute war and peace to the attributes of individuals like Hitler or Gandhi. Some possibilities for incorporating these issues into formal models already exist. For instance, uncertainty over whether an opposition leader is an ‘irrational’ type would affect strategies in models of asymmetric information.

Third, wars could have determinants that are outside the simple rational framework, but systematically so. Some obvious explanations are still consistent with rational models, such as a leader’s failure to internalize the full social costs of war—a possibility raised by Fearon (2004) and recently modeled by Jackson and Morelli (2007) in the context of inter-state war. They show it only takes one “biased” leader (in the sense that their returns to war differ from their citizens’) for war to break out between opposing states.

A related possibility is that leaders are vulnerable to systematic errors in decision-making,

such as overestimating their chance of winning (overconfidence), time-inconsistent preferences, or other types of predictable ‘irrational’ behavior. In such cases, formal models of conflict may be fertile ground for application of advances in theoretical behavioral economics. Efforts to incorporate psychological factors and misperception into international relations theory include Jervis (1976) and Levy (1997), but, to the best of our knowledge, these insights have yet to be applied to formal models of civil war.

New empirical evidence suggests that political leaders often do matter. Jones and Olken (2007) compare successful to failed assassination attempts, and find that the unexpected assassination of leaders tends to enflame low-scale conflicts and diminish high-intensity conflicts. Similarly, the unexpected death of rebel leader Jonas Savimbi is widely viewed as the event that directly ended Angola’s war, so much so that Guidolin and La Ferrara (2007) use his death in an event study of war’s termination on diamond company stock returns. While we think that economic theory should probably refrain from pinning too much on personalities, the econometric evidence just cited means that leadership cannot be entirely ignored.

This example on the role of leaders suggests that certain determinants of conflict outside standard models are observable and testable, and thus could be a basis for new theory. Furthermore, an important possibility seldom discussed in the recent formal theoretical literature is that complex, unsystematic, and difficult to observe forces may greatly influence the outbreak of war, and make a single general economic theory of civil war impossible to craft. The need for intellectual humility is taken for granted by many civil war scholars (e.g. Cramer 2007). Even if observable structural factors remain important, the existence of other influences will complicate empirical testing, especially in the statistical analysis of relatively rare events. We now turn to the evidence, where the implications of these and other estimation challenges are discussed.

### 3 Evidence on the causes of conflict

The correlates of war are by now well-established. Civil war is more likely to occur in countries that are poor, are subject to negative income shocks, have weak state institutions, have sparsely populated peripheral regions, and those with mountainous terrain. Ultimately, empirical work should aim to distinguish which of the competing theoretical mechanisms best explain the incidence, conduct, and nature of civil war, but this goal is still far from being realized.<sup>27</sup> We have limited evidence on the relative influence of the commitment problems and information asymmetries so central to formal theory. In many cases it is still not clear which of the above correlates actually cause war and which are merely symptoms of deeper problems, and we have yet to solve the puzzles of participation, collective action, and group cohesion laid out above.

#### 3.1 Cross-country evidence

Cross-country regressions dominate the conflict literature, and no discussion of civil war empirics is complete without reference to the seminal contributions of Collier and Hoeffler (1998; 2004) and Fearon and Laitin (2003). Collier and Hoeffler ignited interest among economists – and heated disagreement among scholars in other fields – with a simple argument: political grievances are universal but the economic incentives to rebel are not, and these latter factors are often decisive. Scores of papers have followed in their footsteps, most sharing a handful of traits: a dependent variable that indicates war onset or incidence, proxy variables representing possible causes of conflict, and a regression-based test of these competing determinants.

Collier and Hoeffler broadly root their empirical model in a contest model of conflict, and

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<sup>27</sup> Other recent reviews of the empirical literature, often spanning economics and political science, include Collier and Hoeffler (2007), Humphreys (2003), Justino (2007), Kalyvas (2007), Sambanis (2002), and Wood (2003a).



find several variables with robust, positive correlations with conflict incidence. First, slow current economic growth is associated with conflict, as is the proportion of natural resources in total exports. Higher levels of secondary school attainment in the population, in contrast, are associated with a lower risk of civil war. Meanwhile, a country's ethnic fractionalization, income inequality, and democracy are not statistically significant predictors of conflict risk conditional on these other factors. Collier and Hoeffler conclude that economic forces, primarily the ability to organize and finance a rebellion (as captured in their economic growth and schooling variables, and the ability to exploit natural resources) most strongly predict whether civil war occurs.

Fearon and Laitin (2003) take a closely related cross-country approach. Their core regression, while not derived explicitly from theory, became the standard formulation for most cross-country work that followed. It resembles the logit specification in equation 2, where the dependent variable,  $CW_{it}$ , is an indicator for the onset of civil war in country  $i$  in year  $t$ ;  $y_{i,t-1}$  is lagged per capita income; and  $X_{it}$  is a vector of  $K$  population, geographic, political controls (including democracy measures) and social variables (including ethnic and religious fractionalization):

$$(2) \quad CW_{it} = \Lambda(\beta_0 + \beta_1 CW_{i,t-1} + \beta_2 y_{i,t-1} + X_{it}' \beta_K + \varepsilon_{it}).$$

Like Collier and Hoeffler, Fearon and Laitin find, first, that conditions favoring insurgency, like rough terrain, increase the likelihood of civil war, and second, that proxies for political 'grievances' (e.g. ethnic and cultural diversity) have little predictive power. Yet Fearon and Laitin also argue that proxies for state institutional capacity and strength – most importantly, per capita income – are robust predictors of civil war. They conclude that war is engendered by weak central governments and environmental conditions favoring insurgents.

How do these two papers reach different conclusions with similar data and econometric techniques? Most importantly, the two studies attach different interpretations to key variables like per

capita income. Collier and Hoeffler link it to the opportunity costs facing potential rebels, while Fearon and Laitin emphasize its correlation with state capacity. Yet neither of these two “pure” interpretations is entirely justified given the evidence at hand. The link between income levels and armed conflict is theoretically complex, and finer-grained data—say, on incomes that revert to the state versus the citizenry, or actual longitudinal measures of state capacity—is required to distinguish between these interpretations.

Second, the authors differ in how they code civil wars. Three main coding approaches dominate the literature. Some examine conflict *onset* (dropping years of ongoing war from the analysis and focusing solely on the peace-to-war transition) while others use conflict *incidence* (including all years of war in the analysis). Others still examine conflict *duration*. These three approaches can be applied to at least four different civil war datasets, to create twelve possible dependent variables. These datasets do not always agree on the coding of war and correlation coefficients across datasets range from 0.96 down to 0.42 (with an average of 0.68).<sup>28</sup> What drives such diversity? Sambanis (2004) examines the four datasets and finds five main differences: (i) in thresholds of violence required to be defined as a civil war; (ii) the definitions of war beginnings and endings; (iii) in their treatment of ‘internationalized’ civil war (where there is some involvement by outside parties); (iv) in their treatment of related forms of conflict (e.g. communal violence or state repression); and (v) the underlying data sources they draw from.<sup>29</sup>

A third source of inconsistent results lies in the somewhat ad hoc empirical models typically

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<sup>28</sup> See Sambanis (2004). The four most common datasets are: the Correlates of War (COW) (Singer and Small 1994), Fearon and Laitin’s (2003) dataset, PRIO/Uppsala (Gleditsch, et al. 2002), and Sambanis’ dataset (2004).

<sup>29</sup> The coding of civil wars and conflicts remains problematic. Increases in conflict intensity are generally not captured, except by the 25 and 1000 death thresholds in the PRIO/Uppsala data. Some civil ‘conflicts’ may include low level violence that by other criteria would not be considered a war (e.g. crackdowns by federal police on drug gangs). Cramer (2007) reviews other challenges, including: a large-country reporting bias (where it is easier to meet the battle death threshold); the emphasis on battle deaths (omitting, for instance, civilian killings, refugee movements, and state repression); and difficulties of differentiating conflict lulls from true termination of conflict.

used. In this way the cross-country conflict literature mirrors the earlier debate assessing the causes of economic growth, where there was also little agreement on the correct econometric specification (e.g. Levine and Renelt 1992). Authors vary in their use of annual versus five-year periods, corrections for time dependence, the treatment of ongoing war years, the appropriate estimator for rare events, the use of country fixed effects, and so forth.<sup>30</sup>

Fourth, estimates are sensitive to the explanatory variables employed. Hegre and Sambanis (2006) test the sensitivity of estimates to changes in the conditioning set, using the approach popularized in Sala-i-Martin (1997), and identify a few robust correlates of civil war onset: low per capita income, slow income growth, rough terrain, large population size<sup>31</sup>, recent political instability, small government militaries, and war-prone neighbors. Yet many of these variables are plausibly endogenous, biasing other estimates in unknown directions.

Finally, the country level of analysis has inherent limitations. Individual- and group-level conflict factors, such as poverty or ethnic hostility, are imperfectly tested at the national level (Sambanis 2004). In such cases, cross-country evidence (or the absence of evidence) should be regarded with caution. As we discuss in the next section, micro-level data is likely to yield more convincing answers to the fundamental theoretical questions. In our view, other analytical tools, from case studies to historical analysis, will also remain useful.

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<sup>30</sup> On time-dependence and dynamics see (Beck and Katz 2004; Beck, Katz, and Tucker 1998); on logistic regression with rare events data (King and Zeng 2001). For other issues, see the discussion in Hegre and Sambanis (2006).

<sup>31</sup> The finding on population size suggests a possible link between population pressure – with its resulting resource scarcity and environmental degradation – and civil conflict, a theory that dates back to at least Malthus (for a review see Homer-Dixon 1999; Kahl 2006). This population pressure hypothesis is related to the hypothesis that poverty increases armed civil conflict risk, where rapid population growth could be one driver of lower per capita income. The link between population and conflict is complex, however, and could represent several causal factors (as well as systematic measurement error arising from the coding of battle deaths). See Gleditsch (1998).

## *Recent cross-country empirical advances*

Recent cross-country research focuses on improving causal identification and measurement.

**The search for exogeneity.** The correlations of civil conflict with both low income levels and negative income shocks are arguably the most robust empirical patterns in the literature cited above, but the direction of causality remains contested. Even the use of lagged national income growth (as in earlier studies) does not eliminate this concern, since the anticipation of future political instability and conflict can affect current investment behavior and thus living standards.<sup>32</sup> Another way of saying this is that there are likely to be permanent fixed differences between countries that are correlated with their income levels, economic growth rates, and civil war.

To address this concern, several papers seek to isolate exogenous variation in income. In sub-Saharan Africa, where most households rely on rain-fed agriculture, falling rainfall and drought cause large reductions in income. Miguel, Satyanath and Sergenti (2004) use annual rainfall growth as an instrument for income growth. The second stage estimation equation is as follows, where  $CW_{it}$  is civil conflict prevalence (or onset in some specifications) in country  $i$  in year  $t$ ,  $g_{it}$  denotes per capita income growth,  $X_{it}$  is a vector of population, geographic, political controls and social variables,  $\alpha_i$  is a country fixed effect (capturing time invariant characteristics that relate to violence, growth, or both), and  $\delta_i year_t$  denotes a country-specific time trend:

$$(3) \quad CW_{it} = \alpha_i + X'_{it}\beta + \gamma_0 g_{it} + \gamma_1 g_{i,t-1} + \delta_i year_t + \varepsilon_{it}$$

They use annual country rainfall growth rates (current and lagged one year) as instrumental variables for the per capita income growth terms in equation 3. There is a reasonably strong first stage relationship in the sub-Saharan Africa sample, but it is weaker in other world regions,

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<sup>32</sup> For a discussion of this theoretical point see Chassang and Padro-i-Miquel (2008a; b).

where much less economic activity relies on rain-fed agriculture, making Africa the natural region for the application of this approach. In the IV specification they find that a 5% drop in income growth increases the likelihood of a civil conflict in the following year by up to 10 percentage points, or nearly one half. This main effect is not substantially dampened in countries with stronger democratic institutions, greater ethno-linguistic fractionalization, or oil exporters.

This analysis highlights the role that income shocks play in generating armed conflict in Africa. Unfortunately, this econometric strategy once again does not allow the authors to definitively pin down a unique causal mechanism: rainfall shocks may provoke conflict because they lower the opportunity cost of fighting among rural populations (those most affected by weather shocks), or because crop failure also reduces government revenues and state capacity, or both.<sup>33</sup>

Besley and Persson (2008a) exploit international commodity price movements to investigate civil war causes. Consistent with the predictions of their theoretical framework (summarized above), rising import prices lead to greater conflict, which they argue is due to a drop in real wages. Export price increases are also associated with increased civil war prevalence, since growing government revenue makes seizing the state increasingly attractive.<sup>34</sup> However, Bazzi and Blattman (2008) reexamine the effect of trade shocks on civil war using an expanded and more disaggregated database of international commodity prices and country trade shares. Bazzi

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<sup>33</sup> There may also be other violations of the exclusion restriction unrelated to economic factors, for instance if rainfall directly affects the costs of fighting. Moreover, the authors only study one type of economic shock, mainly affecting the rural sector; variation in national income induced by changes in industrial production or foreign aid could conceivably have different impacts. Future work should also examine the possibility that droughts lead to violence between settled and nomadic groups, a salient issue missed in the existing civil war data.

<sup>34</sup> Bruckner and Ciccone (2007) use terms of trade shocks (driven by commodity price movements) as an instrument for national income. They find a large effect of adverse income shocks on conflict risk among undemocratic African countries. This finding differs from Miguel Satyanath, Shanker and Sergenti (2004), who do not find any statistically significant interactions between income shocks and political institutions. Commodity price shocks, while exogenous, are again plausibly not a valid instrument for income; these shocks could affect conflict via government instability (due to collapsing revenues) or by heightening inequality. In this case, a reduced form approach is less vulnerable to bias than the instrumental variable one (Bazzi and Blattman 2008). Political instability in even moderate-sized commodity producers could affect the world price, making commodity price shocks less exogenous than rainfall.

and Blattman argue that the Besley and Persson results, along with previous results showing a relationship between trade shocks and political instability (e.g. Brückner and Ciccone 2007; Deaton and Miller 1995) are sensitive to the definition of conflict and to specification. Unlike rainfall, these shocks show a less consistent relation to conflict, whether they are experienced mainly by farmers (i.e., agricultural commodities), the government (minerals and energy), or in the aggregate. Nor do trade shocks robustly predict political instability in Africa when interacted with governance quality, ethnic fractionalization, or other common explanatory variables, casting doubt on the oft-cited relationship between trade volatility and civil conflict.

More important than generating any single result, however, these papers illustrate the advantage of quasi-experimental econometric approaches for distinguishing correlation from causation. Indeed, future cross-country empirical work should achieve more credible causal inference by focusing on a single, or small number of, exogenous conflict determinants and plausible instruments for them rather than running horse races between many endogenous variables.

**More detailed and theoretically-motivated measurement.** Recent developments in the literature on natural resources and conflict illustrate the value of better measurement.<sup>35</sup> Leonard and Straus (2003) emphasize the importance of enclave production, which has little connection to the productivity of most citizens (and therefore may be less vulnerable to civil war violence). More accurate data have been compiled on oil production and reserves (Humphreys 2005), while others have done the same for primary and secondary diamond deposits (Gilmore, Gleditsch, Lujala, and Rød 2005), and mineral rents (Hamilton and Clemens 1999). Ross (2006) finds that these new and improved measures of underlying hydrocarbon and diamond deposits are strongly associated with more civil conflict while older natural resource measures (based on actual pro-

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<sup>35</sup> For a review of the literature see Ross (2004b; 2006) and Humphreys (2005).

duction or exports) show less robust correlations. These findings are consistent with the contest model's prediction that insurgencies flourish in resource rich regions because of the existence of more rents to fight over and the availability of easy finance, which are also echoed by some case studies (Le Billon 2001; 2005; Ross 2004a).

There remains a need for better measures of political grievances, institutional quality and even poverty. Consider political grievances first. Much has been made of the weak cross-country association between armed conflict and grievance proxies, including economic inequality and ethnic fractionalization (e.g. Hegre and Sambanis 2006; Laitin 2007). This weak association is surprising given the robust negative relationship between economic performance and some social divisions, as well as popular perceptions of their centrality in driving conflict (Alesina and La Ferrara 2005; Alesina and Perotti 1996; Easterly and Levine 1997). However, if risk factors like inequality and ethnic fragmentation are measured with considerable error, or if their relationship to conflict is conditional on particular institutional or historical contexts, then we should not be surprised that their statistical association with conflict is weak. A similar case could be made about the existing and generally crude measures of state capacity.

A more fundamental concern is that the existing proxies are theoretically inappropriate. National income per capita, for instance, may not capture the relevant aspects of poverty that drive fighting, such as the proportion of rural male youth living close to subsistence income. Most measures of ethnic and religious divisions are used principally because they are straightforward to calculate, rather than because they are theoretically convincing. Indices of ethnic fractionalization have been questioned as a meaningful proxy for ethnic tensions (e.g. Posner 2004a; b). Here we have again seen some progress in measurement. Esteban and Ray (1999) propose that a bimodal distribution of preferences or resources—"polarization"—is linked to greater conflict risk.

Montalvo and Reynal-Querol (2005) create an empirical measure of polarization and find support for Esteban and Ray's theory: while fractionalization is not correlated with civil conflict, polarization predicts civil war incidence. More recently, measures of ethnic *dominance*—effectively indicators of minority ethnic rule—have been explored; Cederman and Girardin (2007) find that minority ethnic rule is associated with increased risk of war, although once again this result may not be robust (Fearon, Kasara, and Laitin 2007).

Another area of measurement concern is income inequality. Some case studies suggest that 'horizontal' inequality—inequality that coincides with ethnic or other politically salient cleavages—is a particularly important driver of civil conflict (Sambanis 2005; Stewart 2001). Yet more work is necessary to code these inequalities, as the existing data remains fragmented and its sensitivity unexplored (Besancon 2005; Gurr and Moore 1997; Østby 2005). Even with better measures, it remains difficult to say whether it is the extent of inequality or its context (factors such as state strength or the ideological climate) that matter most (Cramer 2003; 2007).

The finding that many civil conflicts are fought partially along ethnic lines alone is insufficient to make the case that ethnic-based grievances are driving the fighting. An alternative explanation, for example, is that the costs of organizing a rebellion (and collective action more generally) are simply lower within ethnically homogeneous groups. Heightened ethnic tension during a civil war might then be a result of the fighting rather than its cause.

Both the cross-country and case evidence highlight the susceptibility of states with weak institutions to civil war. In particular, partly democratic societies (called *anocracies* in political science) have emerged as prime incubators of civil conflict. By this argument, violent collective action occurs because dissidents are free enough to organize but non-violent political activism is typically ineffective (Fearon and Laitin 2003; Hegre, Ellingsen, Gates, and Gleditsch 2001).



Yet recent work suggests such findings must be taken with caution. For instance, democracy and anocracy measures, commonly based upon the Polity IV dataset (Marshall and Jaggers 2006), explicitly use civil war and political violence in the coding of the data, thus mechanically correlating democracy and conflict by definition (Vreeland, forthcoming). These findings highlight the need for better measures of state institutions and less reliance on existing data.

Reviewing the case literature, Sambanis (2005) suggests several possibilities awaiting empirical exploration: considering new versus established democracies separately; the mass popular inclusiveness of political institutions; the geographic concentration of power; and the degree of state control over a country's geographic periphery. Leonard and Straus (2003) also emphasize the importance of direct taxation and institutions of personal rule.<sup>36</sup> Several of these institutional characteristics have yet to be carefully defined and measured; where they exist, moreover, they have not been tested against the alternatives.

Finally, while a degree of measurement error in both dependent and independent variables is an unavoidable hazard of cross-country work, few of the papers we reviewed weigh its consequences on their estimates. The implications of measurement error ought to be discussed with the same attention as endogeneity concerns. Fortunately, instrumental variables estimation addresses attenuation bias due to classical measurement error, and this is one promising way forward.

**Integration with case studies.** Historical-political analysis is the most time-honored approach to the study of civil conflict. New efforts to integrate case analysis with cross-country statistical work look to build on the strengths and minimize the weaknesses of both approaches.

The clearest example comes from the study of peacekeeping interventions. Ironically, the

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<sup>36</sup> In the former case, Leonard and Straus attempt to develop a measure of state strength for Africa using direct taxes relative to national income in the early independence period. Such worthwhile efforts should be extended.

study of peace has given us some of the best evidence we have for the rationalist roots of war. Doyle and Sambanis (2000; 2006) review the evidence on United Nations peacekeeping missions and find that they are associated with a higher likelihood of peace two years after the end of the war. Fortna (2004a; 2008) examines the duration of peace with and without peacekeepers and reaches a similar conclusion. Both recognize the limitations of econometric analysis when missions are selective and heterogeneous. Fortna shows, however, how nearly all observable determinants of peacekeeping interventions point to the U.N. selecting the hardest, rather than easiest, cases and thus if anything her analysis may be underestimating peace-building effectiveness.

More revealing, however, is the insight their cases bring to theories of conflict. Doyle and Sambanis, and Fortna, conclude that interventions are effective because peacekeepers (i) change the economic incentives of the armed groups away from warfare; (ii) monitor and enforce compliance with the peace agreement; and (iii) facilitate communication between sides, reducing information asymmetries. Agreeing to a foreign intervention, furthermore, is a means for both sides to credibly signal a commitment to peace. If keeping the peace requires that external actors resolve information asymmetries and commitment problems, it seems likely that their absence contributed to war in the first place. The evidence is far from conclusive, but unlike most cross-country regressions, these case-based studies specifically grapple with rationalist theories of war.

Multi-country case studies are also generating new hypotheses and illuminating some of the causal dynamics driving civil conflict (Arnson and Zartman 2005; e.g. Collier and Sambanis 2005a; 2005b; Laitin 2005; Walter and Snyder 1999). Generalizable or not, a single case can illustrate possible causal mechanisms, generate new hypotheses for testing, and stimulate innovative data collection. While this case literature is diverse and impossible to summarize in full, a number of influential patterns and mechanisms stand out, including: the conflict-provoking ef-

fects of commodity price shocks on fragile economies (a claim with only mixed cross-country empirical backing); the central role of external financing to sustain insurgencies (including providing cross-border territory for camps, markets for extracted resources, and military aid); the pervasiveness of earlier state repression; persistent ethnic or elite class dominance; and the emergence of insurgencies in peripheral regions where central government control is weak.<sup>37</sup>

**Beyond borders.** Another promising direction is investigating civil conflict causes beyond the nation-state. One of the more novel approaches is taken by Wimmer and Min (2006), who use fixed geographic territories as the unit of analysis (rather than the more recent nation-state) over two centuries. They suggest that the likelihood of civil and interstate wars has been highest during the two massive institutional transformations that shaped the modern world: the 19<sup>th</sup> century incorporation of most of Africa and Asia into European empires, and mid-20<sup>th</sup> century formation of nation-states in those regions. Many wars, they argue, have been fought to determine states' governing structures, and so are most likely to occur when these institutional principles are in flux due to external geopolitical forces.<sup>38</sup>

More commonly, researchers looking beyond borders explore spillovers (or “contagion”) from neighboring countries. Hegre and Sambanis (2006) find that war in a geographically contiguous country is a robust predictor of armed civil conflict. Gleditsch (2007) finds that the presence of trans-boundary ethnic groups increases conflict risk, while having stronger democracies in the region and more interregional trade are both associated with less civil war. Salehyan and

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<sup>37</sup> For instance, Zinn (2005) studies the Nigerian case, where many factors contributed to civil war risk but there was no full-blown conflict in the 1980s and 1990s. The federal structure of Nigeria may have helped diffuse ethnic rivalry at the center. This argument echoes that of Horowitz (1985) who, in his seminal contribution to the study of ethnic conflict, argues for federalism as an institutional reform that changes the locus of political conflict from the center to an increasingly large set of smaller conflicts in different federal states.

<sup>38</sup> Note that Alesina, Easterly, and Matsuseki (2006) find that artificial states created largely during the 19<sup>th</sup> century wave of European colonization are no more likely to experience civil or interstate warfare than other countries.

Gleditsch (2006) provide evidence for another potential source of conflict contagion: refugees. Refugee flows can ease arms smuggling, expand rebel social networks, and provide a new pool of rebel recruits. Looking beyond borders may also change our perspective on the role of ethnic ‘grievances’. Diasporas, whether in neighboring countries or farther afield, driven by ethnic or religious sentiments often play a major role in rebel finance.

The empirical salience of these and other international issues in driving domestic civil conflicts (including the role of foreign aid, Cold War interventions, and cross-border raids) highlights an important limitation of the existing theoretical work on armed conflict causes, namely its almost exclusive focus on the internal armed groups’ decision of whether or not to fight. This is an important direction for future formal theoretical work, and will likely draw heavily on the existing international relations literature.

**Conflict duration and termination.** Researchers have also studied war duration and termination.<sup>39</sup> For instance, Fearon (2004), proceeds inductively, sorting cases by length and looking for salient patterns. He finds that short wars are disproportionately initiated by coups and popular revolutions, or arose from the breakup of the former Soviet Union and anti-colonial wars, all of which seek political control of the central government. Meanwhile, autonomy-seeking peripheral region insurgencies and ‘sons-of-the-soil’ movements (fought by the local majority against immigrants) tend to last much longer.

Researchers commonly use a proportional hazard model to analyze conflict duration, employing a variety of economic and social variables to assess the role of greed, grievance and other factors in the length of civil wars (e.g. Collier, Hoeffler, and Soderbom 2004). Others have introduced more sophisticated methods, including competing risk models (de Rouen and Sobek

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<sup>39</sup> Licklider (1993) provides an influential early collection of case studies.

2004). One finding is that ethnically fragmented and polarized countries experience longer conflicts (Elbadawi and Sambanis 2000; Montalvo and Reynal-Querol 2007a). Cunningham (2006) finds that conflicts where multiple groups (“veto players”) must approve a settlement are longer, because there are fewer mutually acceptable agreements, information asymmetries are more acute, and shifting alliances create incentives to hold out, complicating negotiations.

These duration analyses have been useful but suffer from many of the same challenges as the onset and incidence literature: divergent results using different datasets; endogenous explanatory variables; and heroic interpretations of proxy variables. Non-linear hazard models also come with additional identification assumptions, and thus are sensitive to measurement error, repeated and contemporaneous conflict events, and the unit of time used (Gates and Strand 2004).

**A typology of conflict.** Researchers have analyzed civil war as a single phenomenon by assumption, leading some political scientists to ask whether the heterogeneity in types of civil war should be explicitly incorporated into empirical models. Are the 1967 Biafran separatist conflict in Nigeria, Nepal’s Maoist insurgency, and the long-running insurgency in Colombia all examples of the same phenomenon, or should we study them separately?

In response, several papers have begun to explore new civil war “typologies”. Some have segregated wars by scale, distinguishing between ‘conflicts’ of 25 to 1000 battle deaths per year, versus ‘wars’ of more than 1000 battle deaths (Gleditsch, et al. 2002). Others, like Sambanis (2001), explore whether ‘identity’ (i.e. ethnic and religious) wars have different causes than ‘nonidentity’ wars. Kalyvas (2005; 2007) and Balcells and Kalyvas (2007) suggest an alternative typology based on war origins and conduct, identifying four main classes: ‘conventional wars’ (featuring regular armies and defined front lines); ‘symmetric non-conventional wars’ with regular armies fighting peripheral or rural insurgencies; ‘symmetric irregular wars’, fought between

weak national armies and insurgents; and the least common, ‘urban wars’.

Such sub-classifications are difficult to test statistically, as they may only increase the volatility and sensitivity of empirical results (especially because it subdivides an already uncommon event into smaller sub-categories where there may not be statistical power to demonstrate the validity of a typology). To the extent that rare events limit the statistical power of such analysis, typology research will need to lead with theory or case studies rather than regression analysis.

A further concern is that a generally-accepted approach (and theoretical justification) for sub-classification will prove elusive; the type of civil conflict that occurs – for instance, a center-seeking versus autonomy-seeking civil conflict – is endogenous to state repressive capacity, rebel organizational competence, underlying political grievances, and the likelihood of military success using that strategy relative to others.

Cramer (2007) challenges civil war scholars further, asking why civil wars are analyzed as phenomena distinct from other forms of political violence—communal riots, state massacres, and coups d’état. Neither the theoretical nor empirical case has been settled for how to most usefully classify political violence into different categories. Moreover, the lines between wars, conflicts, coups and communal violence are sometimes ambiguous, potentially leading to errors of measurement. Exploring these categorical assumptions is an interesting area for future analysis, one that could soon be more convincingly tackled with new data on non-state armed conflicts and ‘one-sided’ state violence (Eck and Hultman 2007; Mack and Nielsen 2008; UCDP 2008).<sup>40</sup>

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<sup>40</sup> The Uppsala Conflict Data Program (UCDP) defines non-state armed conflict as the use of armed force between two organized groups, neither of which is the government of a state, which results in at least 25 battle-related deaths in a year. They define one-sided violence as the use of armed force by the government or by a formally organized group against civilians which results in at least 25 deaths in a year, excluding extrajudicial killings (UCDP 2008).

### *Further challenges and paths ahead for cross-country empirical work*

Despite the empirical difficulties, we do not believe that the cross-country regression should be abandoned entirely. But the path forward looks different than the one already traveled. Existing empirical models are too rarely rooted in formal economic theories of conflict, regression functional forms are too often ad hoc, the selection of proxies is driven by the variables easily at hand (or online), and their inclusion justified by informal arguments. As noted above, there is good reason to believe that the relationships between civil conflict and income shocks, ethnic diversity and political grievances should be conditional ones, evident primarily when interacted with other contextual variables, and the theorizing and testing of these potential interactions is a logical next step for cross-country research.

As this literature continues to advance, there are a handful of best practices to maintain. First, relentless robustness and specification checking. Second, a focus on causal identification via the use of a single or small number of exogenous instrumental variables. Third, the generation of new data on conflict risk factors and triggers, including better measures of political grievance and poverty among key population subgroups, and various dimensions of state institutions and capacity. Fourth, where measurement error persists, explicit attention to its ramifications.

Although deriving policy implications is not the main goal of this survey, there are some implications of this literature worth speculating about. The empirical relationship between violence and low and falling incomes found in the cross-country literature suggests that implementing insurance schemes to protect poor societies from negative income shocks could be fruitful in reducing the risk of civil conflict. A number of authors have recently proposed reforms to the design of foreign aid and to national agricultural policies to help blunt aggregate income shocks and thus help avoid future rounds of bloodshed (Collier and Hoeffler 2002). One possibility is

expanded regional drought insurance for farmers. A variant is foreign aid contingent on objective conflict risk indicators (e.g., weather) – what Miguel (2007) calls “rapid conflict prevention support” – to bolster local economic conditions when the risk of political violence is high.<sup>41</sup>

This example illustrates the potential value research could have in informing policies to prevent civil war. Yet while we observe a poverty-conflict link in the data, too little is known about the precise identity of the actual perpetrators and organizers of violence, so the question of *which* poor to target with assistance to head off violence has yet to be decisively answered. It also remains an open question whether interventions that change incentives for government and rebel leaders would be more cost-effective than efforts to target the pocketbooks of potential rebel recruits. A clearer understanding of rebel recruitment and organization is necessary for such assessments. These issues (and others) are beginning to be addressed in the emerging applied microeconomic research on civil war.

### 3.2 Micro-level empirical evidence on the causes of civil war

The analysis of household and regional data is a growing, and perhaps the most promising, new direction of empirical research. Three questions have been of greatest interest so far: (i) the roots of individual participation in armed groups; (ii) the role of internal geography in influencing where and when civil conflicts are fought; and (iii) the organization and conduct of conflict.

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<sup>41</sup> Targeting this aid towards the social groups most likely to participate in armed violence – for example, by funding temporary job creation for unemployed young men, or crop insurance for farmers – and in bolstering state capacity might be most effective in preventing armed civil conflicts from occurring in war prone countries, most importantly in sub-Saharan Africa. Several African countries, most notably Botswana, have already successfully implemented similar national drought insurance programs including public works employment, and these could serve as models (Valentine 1993); Moore and Jadhav (2006) discuss a related large-scale rural public works employment program successfully implemented in Maharashtra, India. Adverse climate change would make the need for insurance against weather shocks even more pressing. See Hendrix and Cullen (2007), Kevane and Grey (2008), and Lobell, Burke, et al. (2008) for a range of perspectives on the issue of weather, climate change and conflict in sub-Saharan Africa.



## *The decision to rebel*

Individuals are the natural unit of analysis for understanding how armed groups mobilize civilians to fight and contribute resources to their cause. The black box assumptions made in theoretical models on group cohesion and origins need better justification. In response, the issue of collective action is the subject of a growing empirical literature.

The largest body of evidence comes from case studies of 20<sup>th</sup> century rebellions. Several offer evidence consistent with models of self-interested actors seeking to maximize material payoffs. For example, Lichbach (1994; 1995) illustrates how successful social movements offer selective material incentives to joiners. Popkin (1979; 1988) finds that political entrepreneurs developed mechanisms to directly reward peasant rebellion in Vietnam. Weinstein (2007) illustrates how in Mozambique, Sierra Leone, and Peru rebel fighters were remunerated via looting of civilian property and drug sales. Material incentives may also be non-pecuniary. Where violence against civilians is commonplace, joining an armed group has in many cases been a path to relative safety (Goodwin 2006; Kalyvas and Kocher 2007; Lichbach 1995; Mason and Krane 1989). The prestige associated with martial success may also be valued in and of itself.

Yet echoing our discussion above, material incentives are not always a factor in the individual decision to fight, leading some scholars to instead argue that moral, ideological, or ethnic grievances mainly facilitate collective action. Scott (1976) and Wood (2003b) argue that moral outrage led people to rebel against deprivation during economic modernization in Southeast Asia, and over government abuses in El Salvador, respectively. In neither case were selective material incentives apparent.<sup>42</sup> Another literature documents how ethnic and social identities have been

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<sup>42</sup> Some recent work on terrorism reaches similar conclusions. Krueger and Maleckova (2003) claim terrorists' primary motive is passionate support for their cause and feelings of indignity or frustration, and that poverty and educa-

used to identify, reward, and sanction free-riders, thereby providing selective social incentives to participate (Moore 1993; Ostrom 1990; Petersen 2001; Weinstein 2007).

A small but growing number of recent papers employ within-country regional data to explore the factors that predict violence and rebellion, and most find strong associations with local economic conditions. In Indonesia, Barron, Kaiser and Pradhan (2004) find positive correlations between village-level communal violence and local unemployment, economic inequality and natural disasters. Using data gathered from newspaper reports, Chen (2005) finds that areas of high baseline religiosity experienced more social violence in the aftermath of the Indonesian financial crisis. In Nepal, Murshed and Gates (2005) find a strong correlation between district-level civil war deaths and low living standards. Using the same conflict outcome measure in 75 Nepalese districts, Do and Iyer (2007) find that conflict intensity is strongly positively related to the presence of mountainous and forested terrain, as well as higher local poverty and lower literacy rates, but is only weakly related to caste diversity. Macours (2008) uses different data to argue for another dimension to Nepalese recruitment: Maoist insurgents appear to have targeted the districts with the fastest recent growth in income inequality for recruitment.<sup>43</sup>

These studies are informative and pioneering but many suffer from challenges of data quality and endogeneity (limitations the authors are sometimes the first to note). Further data selection bias worries are introduced when conflict data are assembled from Western, English-language news reports. Moreover, individual motivations and decisions are difficult to infer from district-level aggregate data; just as in the cross-country literature, there is too often a tendency to make

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tion play a secondary role. Bueno de Mesquita's (2005) formal model allows terrorist organization leaders to sort recruits by quality, possibly leading to the positive selection of terrorists documented by Krueger and Maleckova.

<sup>43</sup> One of the drawbacks to reviewing a burgeoning literature is that it is sometimes difficult for the outsider (and reviewer) to readily reconcile contrasting results from different dataset and papers on the same country. The variety of recruitment and violence patterns found in Nepal alone is testament to this fact, and we look forward to more cross-dataset comparisons and weighing of alternative hypotheses for Nepal, as well as other conflicts.

deep behavioral claims from simple cross-sectional correlations. The location of fighting might also reflect armed groups' strategic considerations (i.e., the location of important government military targets) rather than the underlying socioeconomic conditions in those areas. Finally, there remains the possibility of reverse causality; for example, in a single cross-section, conflict could contribute to poverty directly, as well as being driven by poverty itself. Even panel data is not immune to these concerns, due to the economic changes driven by anticipated future conflict or other omitted variables. Nevertheless, the sub-national approach is a useful step forward.

A recent study by Dube and Vargas (2007) overcomes some of these concerns, employing exogenous price shocks and detailed panel data on civil violence – guerilla and paramilitary attacks, clashes with government military, and civilian casualties – across over one thousand Colombian municipalities. Consistent with their theoretical model (which builds on Dal Bó and Dal Bó 2004), they find that an increase in the international price of Colombia's leading labor-intensive export commodity, coffee, significantly reduces violence in coffee-producing regions, while an increase in the international price of an important capital-intensive export good, petroleum, increases violence in regions with oil reserves and pipelines. In an important validation of their theoretical model, they then use rural household surveys to show that the positive coffee shock affects labor market outcomes in the hypothesized way, boosting rural incomes and thus presumably raising the opportunity cost of participating in rebellion. A limitation of the study is its lack of data on actual individual recruitment into rebel groups or paramilitaries.

To the extent the patterns observed in Indonesia, Nepal and especially Colombia are causal, the most likely interpretation is that higher individual opportunity costs lower the probability of participation in armed groups. We are hopeful that increased use of innovative micro datasets will yield a more complete view of the poverty-conflict relationship including elucidating the

exact mechanisms.<sup>44</sup> For instance, it remains unclear empirically whether it is usually the poorest who actually fight in rebel groups, and none of these studies tells us by what means collective action problems are overcome in forming and running armed groups.

A handful of individual- and household-level studies are beginning to answer these central questions. Working in post-genocide Rwanda, Verwimp (2003; 2005) built a panel dataset based on a pre-genocide agricultural survey sample. He finds that both poor wage workers and land renters were disproportionately represented among genocide perpetrators, and that they appear to have been motivated by interest in the property of landlords (who were disproportionately victims), suggesting a class-based interpretation.<sup>45</sup> In Sierra Leone, Humphreys and Weinstein (2008) collect post-war data on combatants and non-combatants from the same villages, and find that retrospective poverty measures (e.g. mud housing), low education, and rebel promises of material rewards are robustly correlated with recruitment into armed groups. However, proxies for political exclusion, such as supporting a national opposition political party, did not predict participation. Unexpectedly, they also find that the empirical determinants of voluntary and forcible recruitment were similar, suggesting that rebel leaders might be employing selective rewards and punishments strategically. Alternatively, it could point to the limitations of postwar self-reported data on the rebel participation decision, since respondents have strong incentives to lie about the nature of their recruitment and wartime behaviors (claiming abduction even if they in fact volunteered to fight), to escape social disapproval or even legal prosecution.

Taken together, these studies suggest that material incentives are influential in driving killing even in the most brutal civil wars and in genocide—supposedly the quintessential act of irra-

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<sup>44</sup> For example, Justino (2008) surveys the emerging micro-level evidence and suggests that household poverty interacts with vulnerability (or risk of exposure) to violence in complex ways.

<sup>45</sup> Andre and Platteau (1998) find related results. See also Straus (2006) on the Rwandan Genocide.

tional hatred. Proxies for political grievances perform far more poorly at predicting individual behavior than economic factors in these cases. Existing data on political grievances are admittedly quite coarse and may not adequately account for context specificity, but this provides evidence against the view that political grievances are always decisive determinants of participation in armed groups. We again return to the need for detailed micro-data – ideally, individual panel data collected comparably across countries – on political attitudes and grievances, incomes and labor market opportunities, as well as local government budgets and security capacity in order to more conclusively disentangle competing explanations for participation in civil conflict.

The above examples also call attention to the importance of measuring the incentives offered by armed organizations. A robust assessment of competing explanations for rebel recruitment would require data on the individual characteristics of rebel participants and non-participants, as well as the recruitment “offers” received by both types of individuals, both offers taken and those refused. These data will obviously be extremely challenging to collect, especially retrospective data given the high and selective mortality experienced during wartime, and will likely require greater coordination between researchers, governments and humanitarian aid donors.

### *Internal geography*

Like recruitment, geographic patterns of conflict within states are best explored using sub-national data. To this end, researchers in organizations like the International Peace Research Institute of Oslo (PRIO) and the University of Uppsala have begun to construct and analyze sub-national conflict datasets. Early results are largely consistent with the existing cross-country and case evidence on the role of geography. Buhaug and Rød (2006), for instance, disaggregate conflict and geographic country data into 100 kilometer by 100 kilometer grids within Africa, and

find that separatist conflicts are more likely to occur in sparsely populated regions near national borders, at greater distances from the capital (where political control by the central government is likely costlier), and in the vicinity of petroleum fields, where the rents of political power for secessionists are presumably highest.<sup>46</sup> The existence of easily lootable resources in the context of a bitterly poor society also drove violence in Sierra Leone's war: there are significantly more armed clashes within chiefdoms containing greater diamond wealth (Bellows and Miguel 2008). Angrist and Kugler (2008) similarly find that an increase in world cocaine prices led to increased civil conflict violence in coca growing regions of Colombia relative to other parts of the country.

### *The organization and conduct of warfare*

Another direction is empirical research on the fighting factions themselves. Theoretical progress in this area accelerated some years ago, and we now have a host of theories of rebel and terrorist organization, several of which we reviewed above, as well as extensive case evidence.<sup>47</sup>

Systematic evidence on armed group organization and action, however, has lagged behind. Economists and political scientists have begun to conduct surveys of ex-combatants in Burundi, Colombia, Liberia, Sierra Leone, and Uganda (Annan, Blattman, Carlson, and Mazurana 2008; Arjona and Kalyvas 2008; Humphreys and Weinstein 2004; Mvukiyehe, Samii, and Taylor 2008; Pugel 2006), some still works in progress. These surveys explore who the combatants were, where they came from, and their pre-war experiences. Comparisons across armed groups, or to civilians, suggest motivations for joining an armed group or committing particular acts of war.

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<sup>46</sup> A closely related approach examines the geographic features of civil wars, such as the size of the conflict zone or the distance from the capital. Buhaug and Gates (2002) build a database of battle sites and find that wars of secession and wars with an ethnic or religious dimension tend to be fought in the peripheries. Larger conflict zones are associated with border zones, the presence of natural resources, and peripheral conflicts. While interesting, it remains to be seen what such associations can add to a theoretical understanding of civil war.

<sup>47</sup> For case studies on African guerrilla movements, see Clapham (1998) and Bøås and Dunn (2007).

Such data have limitations, however: they are self-reported; they are based on a sample of survivors; and they can seldom move beyond descriptive analysis of who joins and why. Isolating exogenous variation in recruitment, tactics, or exposure to violence is crucial for drawing firm conclusions. For instance, to understand the determinants of civilian abuse, Humphreys and Weinstein (2006) construct military unit-level measures of discipline and civilian abusiveness that are exogenous to the individual respondent. However, the sample size is such that, unfortunately, there are seldom more than one or two observations per unit. Beber and Blattman (2008) exploit data combatants and exogenous constraints on rebel recruitment to understand the logic of coercive child recruitment in northern Ugandan. Unpopular and short of funds, the rebel Lord's Resistance Army had just one means of gaining recruits: abduction, followed by the constant threat of punishment against new recruits. The likelihood of receiving a firearm and self-reported dependability was increasing in age, while loyalty and length of stay fall in age. From a rebel perspective, the intermediate age group – young adolescents – were the most attractive recruits, suggesting that coercion and child soldiering may go hand in hand.

Deliberately indiscriminate violence against civilians may be another source of exogenous variation. Kalyvas (2006) documents 100 studies and 45 historical cases where state violence against noncombatants provoked greater insurgent violence as a response. In a recent micro-empirical study, Lyall (2009a) examines the effect of Russia's purposefully random shelling of Chechen villages on insurgent activity. This paper, one of the few to arguably study truly random violence, comes to the opposite conclusion: insurgent attacks in the village and its neighbors decline after shelling.<sup>48</sup> However, he is reluctant to draw conclusions about how violence in a par-

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<sup>48</sup> Lyall (2009b) compares "sweep" operations by Russian and pro-Russian Chechen forces during the war in Chechnya (2000-2005). Co-ethnics appear to be better at counter-insurgent operations: comparing sweeps in pairs of similar settlements, insurgent attacks increase after Russian sweeps but decline after Chechen operations.

ticular place translates into long-term outcomes there, and the generality of this result is unclear.

Counterinsurgency is a topic of major current interest in the wake of the U.S.-led wars in Iraq and Afghanistan. Military analysts and commanders have written extensively on theories and lessons learned in the field (e.g. Clutterbuck 1966; Galula 1964; McMaster 2008; Nagl 2002; Petraeus 2006; Sepp 2005).<sup>49</sup> Researchers have begun to investigate the efficacy of recent United States counterinsurgency operations. Berman, Shapiro and Felter (2008) find that the disbursement of small-scale reconstruction funds by U.S. field military commanders in Iraq is correlated with lower levels of insurgent attacks. Of course, selection on unobserved traits, or regression to the mean, could be driving this apparent effect; for this reason, a number of experimental and quasi-experimental evaluations of counter-insurgency spending are presently underway.<sup>50</sup>

Some recent work finds that armed groups respond strategically to new information. Iyengar and Montan (2008) develop data on insurgent attacks and media coverage in Iraq, and find an ‘emboldenment’ effect of new information about U.S. withdrawal intentions on the pace of insurgent attacks. They use this evidence to show that insurgent organizations are sophisticated strategic actors, but while doing so also illustrate the existence of asymmetric information between the warring parties. This work advances our understanding of the empirical relevance of the information asymmetries that are so prominent in theoretical work.

### *Next steps*

There are four main limitations to this new applied micro-empirical literature. First, the necessary datasets are expensive, hard-won, and often require a mix of luck and ingenuity. Hence they

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<sup>49</sup> Biddle, Brown, Isaac and Kalyvas (2008) review military counterinsurgency research in political science.

<sup>50</sup> Of note is the U.S. Defense Department’s Minerva Research Initiative, which in cooperation with a consortium of university researchers, is supporting rigorous impact evaluations of development and military programs to build peace in Iraq, Afghanistan, the Caucasus, Philippines, and elsewhere (Princeton University 2009).



are too few in number. Second, sufficient attention has often not been paid to measurement issues, research design, and econometric identification. Too many researchers have rushed to collect micro-data without adequately preparing a research design in advance, or testing the assumptions required for causal inference. Third, it remains to be seen whether and how micro-level results will test conflict theories. In particular, while counter-insurgency studies have inherent military value, it is not always clear how they relate to broader theoretical debates. Fourth, it remains to be seen how micro-level insights from one war generalize to other contexts, and thus can be useful for policymakers elsewhere. At this early stage, this is primarily an argument in favor of increasing the number of micro-empirical studies on the causes and conduct of civil war.

## 4 Economic legacies of civil conflict

People living in zones of war are maimed, killed, and see their property destroyed. They may be displaced, or prevented from attending school or earning a living. A growing empirical literature estimates the magnitude of these effects of war on later income, poverty, wealth, health, and education.<sup>51</sup> Each of these outcomes has implications beyond the individual, however. To the extent that these costs are borne unequally across groups, conflict could intensify economic inequality as well as poverty. The destruction (and deferred accumulation) of both human and physical capital also hinder macroeconomic performance, combining with any effects of war on institutions and technology to impact national income growth.

Understanding the economic legacies of conflict is also important to the design of post-conflict recovery. If war itself further aggravates factors that enhance the risk of civil conflict –

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<sup>51</sup> Justino (2007; 2008) also surveys this emerging literature. Many of the datasets and working papers are being shared via research groups such as the Households in Conflict Network (<http://www.hicn.org>).

poverty, inequality, and social discord – then it could partially account for war re-occurrence.

Indeed, the aggregate effects of armed conflict, and its threat, are considerable. Rodrik (1999) argues that outbreaks of social conflict are a primary reason why national economic growth rates lack persistence and why so many countries have experienced a growth collapse since the mid-1970s. A number of cross-country growth studies link measures of political instability to large negative effects on national savings, investment, income and growth.<sup>52</sup> Cerra and Saxena (2008) find that output declines six percent in the immediate aftermath of a civil war. Quantitative case evidence supports this cross-country relationship: Abadie and Gardeazabal (2003) find that terrorist violence in the Basque region of Spain has significantly reduced economic growth there relative to neighboring regions. The effect on poverty can be dramatic. In Rwanda, 20% of the population moved into poverty following the genocide (Justino and Verwimp 2006). Civil wars may also have negative growth spillovers on neighboring countries (Murdoch and Sandler 2004).

An economic growth theory framework is useful for analyzing the consequences of conflict. If conflict affects economic performance, it must be because it affects a factor of production (physical capital, labor, or human capital), the technology, institutions, and culture that augment these factors, or prices (e.g. costs of capital). The growth framework also clarifies the possible nature of the impacts, not only on income levels and economic growth in equilibrium, but also out-of-equilibrium dynamics such as the speed of convergence.

The framework we use to organize our discussion is based on neoclassical models of growth with human capital (e.g. Lucas 1988; Mankiw, Romer, and Weil 1992). Alternative frameworks, however, can generate radically different predictions regarding the likely impact of violence—

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<sup>52</sup> See Barro (1991), Alesina, Ozler, Roubini, and Swagel (1996), Alesina and Perotti (1996), and Svensson (1998). The political instability-growth relationship may be partly endogenous but some argue that the association is likely to persist even after better accounting for this bias (Gyimah-Brempong and Traynor 1999).

and, in particular, the destruction of capital—on economic performance. To illustrate, a one-time shock to capital has no effect on equilibrium income or growth in a neoclassical model, but persistent effects are possible in poverty trap, endogenous growth, and vintage capital models (e.g. Azariadis and Drazen 1990; Barro and Sala-i-Martin 2003; Gilchrist and Williams 2004). The relative degree of physical and human capital destruction also matters: recovery could be faster under highly asymmetric destruction – say, extensive physical capital destruction when human capital remains largely intact – since the relative abundance of one type of capital raises the marginal product of the scarce type, spurring on investment (Barro and Sala-i-Martin 2003).

Given the proliferation of plausible theoretical perspectives, empirical evidence is essential. Yet assessing the economic consequences of civil war is complicated by a central identification problem: war-torn countries are different than peaceful ones (as detailed in sections 2 and 3). Poor post-war economic performance could reflect the declining economic conditions that contributed to armed conflict in the first place, in addition to any direct impacts of war. Similar endogeneity concerns arise in assessing impacts on government performance and institutions, and even individual-level outcomes (to the extent different types of individuals are targeted for violence or recruitment into armed groups). The existing empirical literature on post-war economic recovery is only beginning to seriously address these issues, and as a result the conclusions we can draw about the consequences of war, and the appropriate post-war policy responses, are more limited than in the case of civil conflict causes discussed above.

#### 4.1 Physical capital and investment

Evidence from interstate wars suggests that the postwar evolution of physical capital often behaves as predicted by the neoclassical model, namely, rapid recovery to equilibrium levels.

One set of studies examines the impact of U.S. bombing on later outcomes at the city or regional level. Although they generally lack detailed information on local physical capital levels, in Japan (Davis and Weinstein 2002) and Germany (Brakman, Garretsen, and Schramm 2004) in World War II, cities that were heavily bombed quickly recover in population back to prewar trends, such that 20 to 25 years post-war city populations are indistinguishable from cities that were left untouched by bombing. In the Vietnam War, which combined external intervention and a civil war, Miguel and Roland (2005) find similarly rapid local population recovery from bombing.

These cross-region results echo the consensus from the cross-country literature on the rapid recovery of postwar economies (Organski and Kugler 1977; 1980; Przeworski, Alvarez, Cheibub, and Limongi 2000). Indeed, a recent study of the output response to alternative crises—including currency crises, banking crises, civil war, and sudden shifts in executive power—finds that while civil wars cause the steepest short-run fall in output (six percent on average), only in the case of civil war does output rebound quickly, recovering half of the fall within a few years, while output drops are more persistent for financial crises (Cerra and Saxena 2008). While such event studies conceal a great deal of heterogeneity in experiences, and suffer from obvious omitted variable bias concerns, none of these results appear consistent with poverty trap models of economic growth such as those recently advanced by Sachs (2005).

Nevertheless, there are reasons to be cautious in generalizing these experiences. These studies cannot rule out the possibility that the economic devastation caused by civil war prevent some countries from achieving durable peace. Countries with successful postwar recovery economic are also more likely to collect systematic economic data, introducing possible selection bias: war-torn countries where the economy and institutions have collapsed (e.g. Congo and Somalia) lack good data, while those that recover (Vietnam) have data. This could bias the cross-country

estimates of war's economic impacts towards zero as the most destructive wars exit the sample. Civil wars are also often localized and fought with small arms and munitions, so they do not necessarily see the large-scale destruction of capital caused by bombing, creating some separation between the evidence we have and the contexts of greatest interest.

Yet even in civil conflicts without large-scale bombing, capital can sometimes be depleted in devastating ways. First, household assets may be stolen or destroyed. Mozambicans, for instance, are thought to have lost 80% of their cattle stock during their civil war (Bruck 1996), while many in northern Uganda lost all of their cattle, homes and assets (Annan, Blattman, and Horton 2006; Gersony 1997); cattle and other farm assets often represent most of a rural household's savings. As of yet, however, there is still limited systematic panel data on the implications of such asset loss on long-run household welfare. Second, countries at war are likely to see massive flight of mobile forms of capital, since foreign assets offer higher relative returns at lower risk (Collier 1999; Collier, Hoeffler, and Pattillo 2002). The same factors could lead to such low levels of new investment that the existing capital stock quickly deteriorates.<sup>53</sup>

The neoclassical growth model prediction that the capital stock should return to its steady state level once the fighting stops – implying relatively high returns and rates of investment that decline as the equilibrium is approached – supposes that underlying institutions and technology are largely unaffected by the fighting, and that military spending, the returns to capital investment and the cost of capital similarly return to pre-war levels. Yet any political or economic uncertainty following war is likely to decrease expected returns, increase relative risk, and possibly shorten investment horizons, thus reducing investment and raising the cost of capital. Collier

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<sup>53</sup> Rising military spending can also crowd out government infrastructure projects and other public goods. A World Bank report estimates that average military spending in poor countries rises from 2.8% of national income in peace to 5% at war (Collier, et al. 2003). Cross-country evidence suggests that such military spending is growth-retarding due to the shift away from productive investment (Knight, Loayza, and Villanueva 1996).

(1999) argues that adverse effects on the cost of capital are sometimes persistent empirically.

Foreign financial aid and other international interventions could conceivably play an important role in rebuilding infrastructure and replenishing household assets in these cases. There is also anecdotal evidence from countries like Sierra Leone and Liberia that the role of the international community was decisive in shifting expectations about future conflict risk (Collier 2007). Collier and Hoeffler (2002) suggest that increased foreign aid is likely to reduce civil conflict risk, and find some modest reductions in the likelihood of conflict for aid recipients, working through the channel of faster economic growth. Yet the non-random placement of both civil conflicts and foreign aid means we cannot necessarily interpret these statistical relationships as causal. De Ree and Nillesen (2006), however, examine aid disbursements and civil conflict risk in sub-Saharan Africa using a more sophisticated instrumental variable econometric strategy that exploits exogenous changes in donors' overall foreign aid budgets, and they find that a 10% increase in aid to an African country reduces conflict risk by 6%. Taken together, these two studies suggest that postwar foreign aid may play a key role in solidifying the transition to peace.<sup>54</sup>

Future advances could come from disaggregating such post-war aid flows and investigating the role of specific activities (such as peacekeeping) on reducing risk and promoting economic recovery. Recently Collier, Chauvet and Hegre (2008) attempt just such a cost-benefit analysis of alternative aid and military interventions. Such calculations are highly dependent on parameters for which we currently have limited data (e.g., the growth effects of avoiding a civil war, the selection of countries into different interventions) and thus have to be interpreted with caution.

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<sup>54</sup> An emerging literature examines the role that postwar demilitarization programs (usually funded by aid donors) could play in securing the peace, although there is little quantitative evidence on the effectiveness of these programs.

## 4.2 Life, labor and human capital

Wars most obviously kill and maim people, both directly and indirectly through famine and disease. Conflict victims are overwhelmingly civilians, and indirect deaths are seen disproportionately among the poor, women, children and the elderly.<sup>55</sup>

The short-run impact of war is clearly disastrous, but there is mixed evidence on how long the economic effects on human capital and quality of life persist. In the study of Vietnam bombing mentioned above, local living standards and human capital levels also converged rapidly across regions after the war, leaving few visible economic legacies 25 years later (Miguel and Roland 2005). This empirical finding echoes the cross-country literature showing rapid post-war economic recovery and argues against poverty trap type models. An innovation is the attempt to address the endogeneity of bombing. Miguel and Roland instrument for bombing intensity using distance from the arbitrarily settled North-South Vietnamese border (on the 17<sup>th</sup> parallel north latitude).<sup>56</sup> A limitation of this paper – and others that examine differences across sub-national units over time – is its inability to credibly estimate the aggregate national economic impact of war damage; for that, cross-country studies may be more convincing.

A new and rapidly growing microeconomic literature finds more persistent negative war impacts on individual human capital, especially in African cases. Using panel data on child nutrition, Alderman, Hoddinott and Kinsey (2004) find that young children who suffered from war-related malnutrition in Zimbabwe are significantly shorter as adults, which may affect their lifetime labor productivity. In a related paper, Akresh, Bundervoet and Verwimp (forthcoming) ex-

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<sup>55</sup> Wars are thought to have directly caused 269,000 deaths and 8.44 million disability-adjusted life-years (DALYs) in 1999 alone, with twice again this number of deaths and DALYs estimated in 1999 due to the lingering effects of wars between 1991 and 1997 (Ghobarah, Huth, and Russett 2003; 2004).

<sup>56</sup> Districts located near the border were subject to more fighting, cross-border raids, artillery shelling, and bombing.

exploit variation in the timing of armed clashes in the Burundi civil war to estimate impacts on child nutrition, and find that children who lived in a war-affected region have sharply lower height-for-age than other children, with an average drop of roughly 0.5 standard deviations. Turning to a Central Asian setting, adolescent Tajik girls whose homes were destroyed during that civil war are less likely to obtain secondary education, again with likely adverse effects on later wages and life chances (Shemyakina 2006). The validity of these studies, all of which use difference-in-differences methods, relies on the assumption of similar underlying human development trends in the war-affected and peaceful regions of these countries, something that is challenging to convincingly establish with the limited time horizons of most datasets.<sup>57</sup> Moreover, as in the bombing studies, these studies may underestimate war's overall impacts to the extent that even those in largely peaceful regions were also adversely affected by civil war disruptions.

Turning to combatants, it appears that the interruption of human capital accumulation is one of the most pervasive impacts of military service. Studies of U.S. and European veterans of the Vietnam and Second World Wars find large and persistent falls in earnings and higher mortality (Angrist 1990; 1998; Angrist and Krueger 1994; Hearst, Newman, and Hulley 1986; Imbens and van der Klaauw 1995). These patterns are echoed by new evidence from developing countries. Blattman and Annan (2007) use exogenous variation in rebel recruitment methods—namely, the near-random forced recruitment in certain rural areas—to estimate its impact on adolescents and young adults. These youth are more likely to have persistent injuries, accumulate less schooling and work experience, are less likely to be engaged in skilled work, and earn lower wages as adults. Psychological trauma and community rejection, meanwhile, is concentrated in the small

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<sup>57</sup> An unexpected spillover effect of war on the human capital of neighboring countries comes from Montalvo and Reynal-Querol (2007b) who, using civil wars as an instrumental variable, argue that for each 1,000 refugees there are between 2,000 and 2,700 additional cases of malaria in the refugee-receiving country.



minority that experienced the most violence.<sup>58</sup> The conclusion that emerges is that military experience is a poor substitute for civilian education and labor market experience. In settings where a large share of male youth actively participated in the fighting, aggregate economic impacts could be quantitatively important.

This emerging applied microeconomic literature only scratches the surface of the range of possible civil war impacts on the economy and society. More evidence is required on the educational, employment, and health impacts of conflict on armed group participants and civilians, including internally displaced people. The leading question is not whether wars harm human capital stocks, but rather in what ways, how much, for whom, and how persistently—all crucial questions for understanding war's impacts on economic growth and inequality, as well as priorities for post-conflict assistance. To our knowledge, no rigorous evidence yet exists on which types of programs are most effective at overcoming war's adverse legacies on human capital.<sup>59</sup>

### 4.3 War, Institutions, and Society

The steady state to which a post-conflict society returns is a function of the fundamental determinants of growth: technology, institutions, and social organization. The rapid return to pre-war levels of labor and capital in Germany, Japan, and Vietnam noted above suggests that these determinants were not diminished by war (or, if they were, they likewise recuperated quickly).

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<sup>58</sup> A somewhat different pattern is observed by Humphreys and Weinstein (2006; 2007), who find that increases in Sierra Leone fighters' exposure to violence are correlated with lower post-war community acceptance, but also find that violence has little correlation with labor market success.

<sup>59</sup> Other potentially important topics awaiting systematic empirical analysis include: the role of war-related emigration (especially of the skilled) on later economic growth, the general equilibrium effects of death and emigration on labor markets, and civil war's effects on the prices of land, capital, and labor. Steele (2007) includes a review of the determinants of population displacement, while Kondylis (2007; 2008) presents quasi-experimental evidence on some of its economic impacts. To the extent that the sudden death of sizeable shares of the working age adult population affects relative prices, fertility and investment decisions, civil war could have impacts on living standards reminiscent of the HIV/AIDS epidemic in sub-Saharan Africa along the lines argued in Young (2005).

Unfortunately, we have little systematic quantitative data with which to rigorously judge claims about the evolution of institutions during and after civil wars. A sizable literature has sought to identify the specific institutional factors that matter most for economic growth—including property rights (Acemoglu, Johnson and Robinson 2001), social capital and cohesion (Knack and Keefer 1997), rational bureaucracies, and work ethics, to name a few – but which of these are affected by civil war (not to mention how much and under what circumstances) remains a matter of speculation. The social and institutional legacies of conflict are arguably the most important but least understood of all war impacts.

The historical evidence (described above) that war enables the development of capable government institutions in Europe may not generalize to civil war cases. In the three countries that experienced U.S. bombing, the wars were fought against (largely) foreign armies, and hence could rally citizens and renew government motivation and legitimacy. In civil war, government may lose legitimacy, while victors and vanquished (and victims) are condemned to coexist in the same society, potentially exacerbating political and social divisions.

Yet even the waging of internal war need not always be uniformly destructive to institutions. The effort to control the nation's peripheries, and the extension of national control down to the community level are essential state responsibilities. Successful states do so through a variety of means, including the use of force (Tilly 1982; Weber 1965). Hence internal warfare could hypothetically generate state-building rather than institutional disintegration. Yoweri Museveni's violent takeover of the Ugandan state, for instance, hinged in part on his ability to organize citizen councils from the village up to the national level—councils that became the basis for post-war administration and (especially by regional standards) a relatively strong state (Weinstein 2005a)

Indeed, there is some cross-country evidence that wars that end in outright military victory for

one fighting side lead to a more stable peace and possibly stronger state institutions (Fortna 2004b; Toft 2008), although once again the omitted variables correlated with outright military victories make interpretation of this pattern difficult. A draw, a negotiated agreement, or a cease-fire is, by this logic, an unstable equilibrium bound to unravel. This belief has led some scholars to argue that the international community should “give war a chance” (Herbst 2000; Luttwak 1999). In an indication of how unsettled the literature currently is, some other recent case study and statistical analysis, on the other hand, indicates that aggressive peacekeeping leads to more lasting peace (Fortna 2008; Mack and Nielsen 2008; Sambanis 2007). Easterly (2008) argues against this view, however, pointing out that there are as many examples of failed foreign interventions (e.g., Darfur, Democratic Republic of Congo, Somalia) as successes (Sierra Leone).

One reason why research has produced few definitive answers so far may be that there is no simple, general relationship between civil war and institutions; impacts may depend on why a war started in the first place, how it is fought, and how it ends.<sup>60</sup> The unpacking of these complex relationships is perhaps the most pressing area for future empirical research in this area. While the war termination literature tends to focus on the durability of peace agreements, a useful next step is to focus on patterns of institutional change during and post-conflict. While the analysis of these relationships is clearly wrought with omitted variable bias concerns, the search for innovative research designs should continue. Beber (2008), for instance, uses the higher likelihood of mediated bargaining occurring during the summer months (when foreign politicians and diplomats are freer to broker deals) to identify the gains to mediation in inter-state conflicts.

A final perspective on the institutional and social impacts of war comes from an empirical literature on post-war political participation and accountability. Of the preliminary data available,

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<sup>60</sup> The literature on war termination in particular is vast, and is outside the scope of this review.

some of the results are quite counter-intuitive, especially an apparent causal link between war violence and productive citizenship postwar. A recent micro-study finds that war victimization increases later individual political mobilization and participation in local collective action in Sierra Leone, which the authors interpret as a result of the psychological legacies of individual violence exposure (Bellows and Miguel 2006, 2008). Former combatants in Uganda were also more likely to vote and become local leaders (Blattman 2008). Likewise, psychologists find that the victims of violence are in general resilient (Masten 2001), and that exposure has even led to greater political activism among groups such as Jewish Holocaust survivors (Carmil and Breznitz 1990) and Palestinian victims of bombardment (Punamäki, Qouta, and El Sarraj 1997). Together, these findings begin to challenge the notion that the motivations, institutions, and social norms that promote local level collective action are necessarily harmed by civil war.<sup>61</sup>

#### 4.4 Remaining Challenges

Viewed through the lens of economic growth models, the existing empirical literature on civil war impacts still looks spotty. Macroeconomic studies indicate that the short-run output effects of armed conflict can be large, but more work is needed to examine the underlying effects on factors of production and relative prices. The early signs suggest that population and physical capital can fully recover after war, perhaps as quickly as within two decades. That recovery, however, appears to be contingent on the preservation, or even the improvement, of political stability and institutions, as was the case in Japan, Germany, and Vietnam. Yet what the key institutions are, and which domestic policies and external interventions can help maintain their stabil-

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<sup>61</sup> Yet findings are far from uniform. Miguel, Saiegh, and Satyanath (2008) argue that civil wars may shape national socio-cultural norms towards violence. They find that European soccer league players from countries with histories of civil war commit significantly more violent yellow and red card fouls (conditional on player characteristics).

ity, are still poorly understood. Even theoretically compelling patterns could be spurious, driven by omitted variables rather than causal impacts.

The microeconomic literature is even less systematic at present, although in our view it holds great promise. Many factors appear to be adversely affected by civil war in at least some cases. Those who participate in wars, or simply live through them, often suffer from persistent injuries, lose out on education, and see a permanent decline in their productivity and earnings. But understanding which impacts are more profound and persistent than others; which disproportionately strike the poor; and how those effects can be contained by local institutions and economic policies is still largely unexplored.

Without firm answers to these questions, policymakers and foreign aid donors have often taken a scattershot approach to postwar programs. The subject of post-conflict recovery policy is vast and is largely outside the scope of this review, but most of that literature comes in the form of best practices summaries, case studies, and other literature produced by international aid organizations, governments, and NGOs. Academic research remains limited, and where it exists, tends to focus on high-level analysis (e.g., the relationship between aggregate foreign aid and national economic growth) as to be largely unhelpful to those seeking more specific programmatic solutions. Given the many possible omitted variables involved in the timing of foreign interventions, related to both domestic and international political factors, establishing the causal impact of armed intervention on long-run political and economic outcomes has been elusive.

An obvious answer is to call for more data collection, continued searching for natural experiments or actual field experiments, and more rigorous impact evaluations of post-conflict programs. Judging by the rise of research organizations like the Households in Conflict Network (HiCN), increased funding by the World Bank's Development Economics Research Group, and

the growing literature, this call for more and better analysis is already beginning to be answered.

## 5 Discussion and future directions

Armed conflict is finally moving into the research mainstream in development economics. This article has attempted to survey this flourishing inter-disciplinary field, describe its more robust findings, and point the way forward in a way that is useful for both those new to the field as well as those already actively working within it.

Some of the core insights are worth re-stating here. First, there has been considerable progress in the formal modeling of the political economy of civil war during the past two decades, with insights on the individual decisions, institutional features, and economic conditions that promote violent conflict. Commitment problems – either across the two sides to a conflict, or among factions within a fighting side – are currently viewed as the leading rationalist theoretical explanation for civil war, especially for long-duration civil wars, although certain types of information asymmetries may also play a role. Disentangling the relative contributions of the various commitment problems and information asymmetries proposed in the theoretical literature is a top priority for empirical research. Developing new explanations—possibly challenging the current modeling assumptions of unitary armed groups, or even rationality—is also likely to be fruitful.

Second, a variety of theoretical models predict that low incomes, weak state institutions, and social divisions may contribute to the onset of civil wars, and these issues have been the focus of most empirical treatments. The most robust empirical finding in the existing literature is that economic conditions – both low income levels and slow growth rates – contribute to the outbreak of civil wars and conflicts in less developed countries. This finding has found support at both the cross-country and the micro levels, although the correct interpretation of these patterns in terms

of underlying theoretical mechanisms remains contested. A smaller literature suggests that economic factors are decisive in driving individual participation in armed groups. However, the theoretical and empirical conflict literatures have too often run along parallel paths, informing each other, yes, but seldom directly intersecting; greater efforts need to be made to identify and test the precise empirical implications of the leading theoretical frameworks.

In contrast, the empirical evidence that social divisions, political grievances, and resource abundance are drivers of violence remains weaker and more controversial. The existing literature tends to measure non-material factors crudely, and empirical tests rarely attempt to capture the nuances of a social phenomenon as complex as civil war, making it impossible to decisively reject that non-material factors are playing some role. Further research using better data is needed to firmly settle the question of what role political grievances play in driving civil conflicts.

There is also an emerging literature on the economic legacies of war. At this point the macro literature and newer micro literature have produced somewhat contradictory findings, although they can potentially be reconciled by appealing to the divergent outcomes they consider (the macro literature focuses on economic growth and population, while the micro literature mainly on human capital). Future work must clarify how the nature of the conflict (internal versus international) as well as the political, social, and institutional context affects its long-run economic impacts, and just as importantly, must more seriously address the many omitted variables that could simultaneously drive the outbreak of wars and affect post-war economic recovery. The neoclassical economic growth framework usefully highlights the most important gap in our knowledge: the impacts of internal war on institutions, technology, social norms and culture. Progress on these issues is critical for crafting appropriate postwar recovery policies, a major economic policy issue in conflict-prone regions, including sub-Saharan Africa.

Throughout this discussion, a key lesson that emerges is the important role that new data sources have played in enabling research progress. The development of the PRIO/UCDP civil conflict database has propelled the cross-country conflict literature forward. Disaggregated data on U.S. bombing patterns allowed Davis and Weinstein (2002) to carry out their seminal study on Japan. The increasing number of longitudinal household-level datasets in less developed countries have made the new micro studies on war impacts possible. Some of this data collection has required remarkable ingenuity and courage on the part of the investigators, notably, the collection of data on civilians and combatants from ongoing or recently concluded wars.

Yet much more work remains to be done. In our view, a major goal of civil war researchers within both economics and political science in the coming years should be the collection of new data, especially extended panel micro-data sets of economic conditions and opportunities. Ideally, these efforts would also be coordinated, publicly shared and comparable, in a similar fashion, say, to the World Bank's Living Standards Measurement Study (LSMS) or the Demographic and Health Surveys (DHS) program.<sup>62</sup>

Data collection is of course inherently difficult in "hot" conflict zones. But even in many post-conflict settings where conditions are closer to normal, statistical agencies simply return to the status quo ante in terms of survey instruments, and fail to valuable collect retrospective conflict experience data. To illustrate from the authors' own experiences in Liberia, Sierra Leone and Uganda, neither the government statistical agencies nor the international donors financing reconstruction there had plans to systematically include questions on war experiences, victimization, or participation in the national census or other representative household surveys conducted

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<sup>62</sup> For LSMS, see <http://go.worldbank.org/IPLXWMCNJ0>. For DHS, <http://www.measuredhs.com>. The institutional basis for such coordination may already exist in the Households in Conflict Network (<http://www.hicn.org>).



at conflict's end. Where national surveys of war experiences were conducted, they tended to focus exclusively on combatants. Closer cooperation among government data collection agencies, development organizations, and researchers will be required for the systematic and comparable data needed to make further progress.

A few specific data collection directions appear particularly promising. First, more detailed information on rebel organization and decision-making would be useful. Some civil conflicts, like those in Congo and Sudan, feature a dozen or more active armed groups. Pooling data from several such settings could allow for a relatively large sample analysis of armed groups—or “cross-rebel regressions”—establishing patterns useful to applied theorists working on rebel organizations. While the proliferating number of surveys of ex-combatants (described earlier) will improve such analysis, so far these datasets have been selective: most focus on non-state actors and nearly all on Africa. The risk is that our understanding of civil war will be driven by a subset of conflicts rather than a more globally representative sample. New data from Afghanistan, Colombia, Indonesia, Iraq, and Nepal (discussed above) is starting to fill in the gaps.

Second, and perhaps easiest to collect, are follow-up household surveys in post-conflict settings integrating retrospective information on a wide range of economic behaviors and experiences during the war, along the lines of the data work in Akresh, Bundervoet and Verwimp (forthcoming) in Burundi, Bellows and Miguel (2006, 2008) in Sierra Leone, Blattman and Annan (2007) in Uganda, and Verwimp (2005) in Rwanda. The Burundi and Rwanda surveys deserve special mention. In both cases the authors identified a pre-war national household survey, located the original (archived) surveys, and tracked down the sample households again after the war. Such intellectual entrepreneurship should be expanded and rewarded in the profession.

Third, we need to improve measures of political attitudes and grievances and test their asso-

ciation with actual behaviors. One example stands out. Habyarimana, Humphreys, Posner and Weinstein (2007; forthcoming) identify theoretically distinct mechanisms that link ethnic diversity to trust, cooperation and public goods provision, and then run experimental games to compare the explanatory power of the distinct mechanisms in a representative sample of 300 subjects from a Kampala, Uganda slum neighborhood. Experimental economics lab research in similarly interesting less developed country populations, especially in conflict and post-conflict societies, could potentially also help shed light on the individual decision to participate in violence, or on the resolution of collective action problems within armed groups.

Fourth, at the macro level, we encourage the development and synthesis of data on additional forms of political instability and violence. Political repression figures prominently in theories of conflict and cooperation (e.g. Acemoglu and Robinson 2006; Besley and Persson 2009), and yet we have a limited sense of its use or effectiveness. As noted above, the distinction between civil war and these other phenomena has been asserted rather than demonstrated. If we are interested in the struggle between groups for national power, it is not obvious that we should ignore coups; communal violence could similarly shed light on participation in violent collective action.<sup>63</sup>

Civil wars and conflicts arguably inflict more suffering on humanity than any other social phenomenon. Now they are emerging as central to many countries' political evolution and possibly as key impediments to global development. We hope this article will promote the incorporation of these topics into graduate and undergraduate courses in both economics and political science, and stimulate further research. As we've discovered, and to recast Bob Lucas' famous phrase, once you start thinking about civil war, it's hard to think about anything else.

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<sup>63</sup> New data are already emerging on leadership transitions (Goemans, Gleditsch, and Chiozza 2009) and on one-sided violence, non-state armed conflict, coups, and human rights abuses (Mack and Nielsen 2008).

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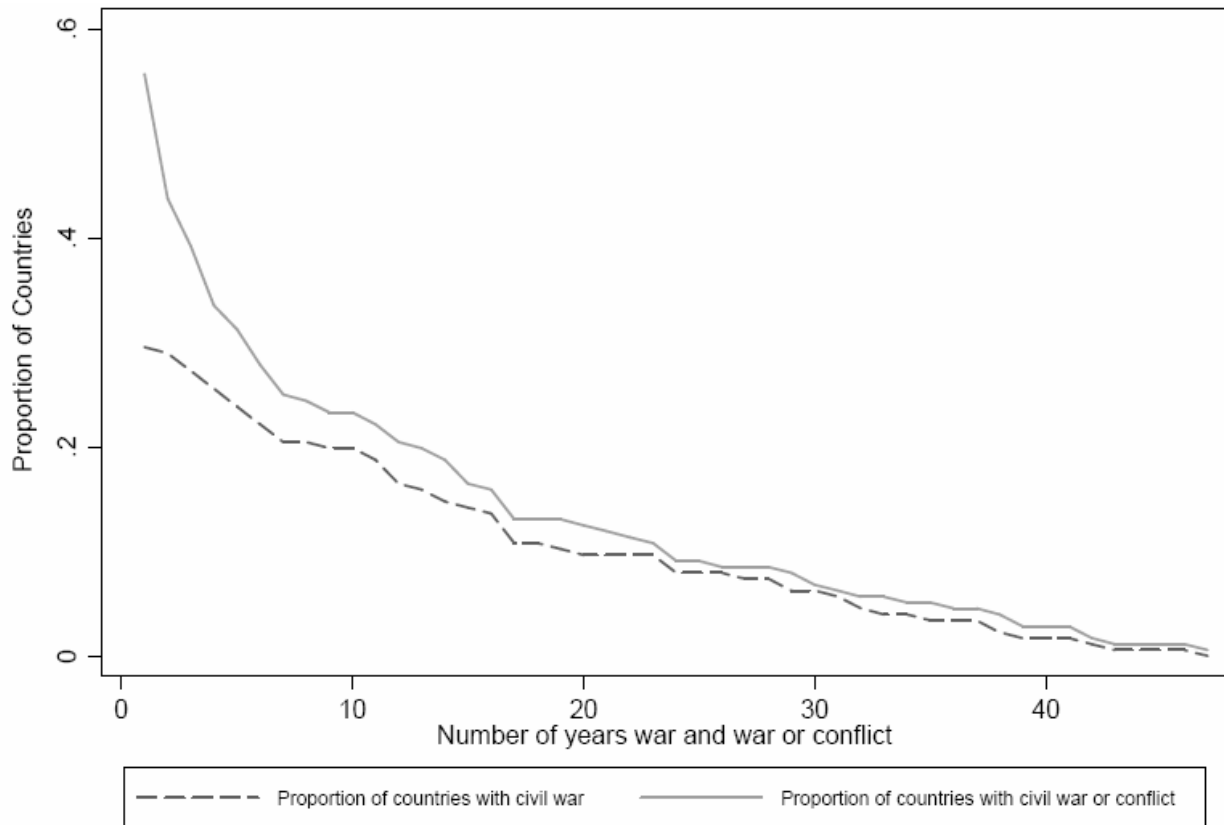
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**Figure 1: The distribution of civil war or conflict years across countries, 1960-2006**



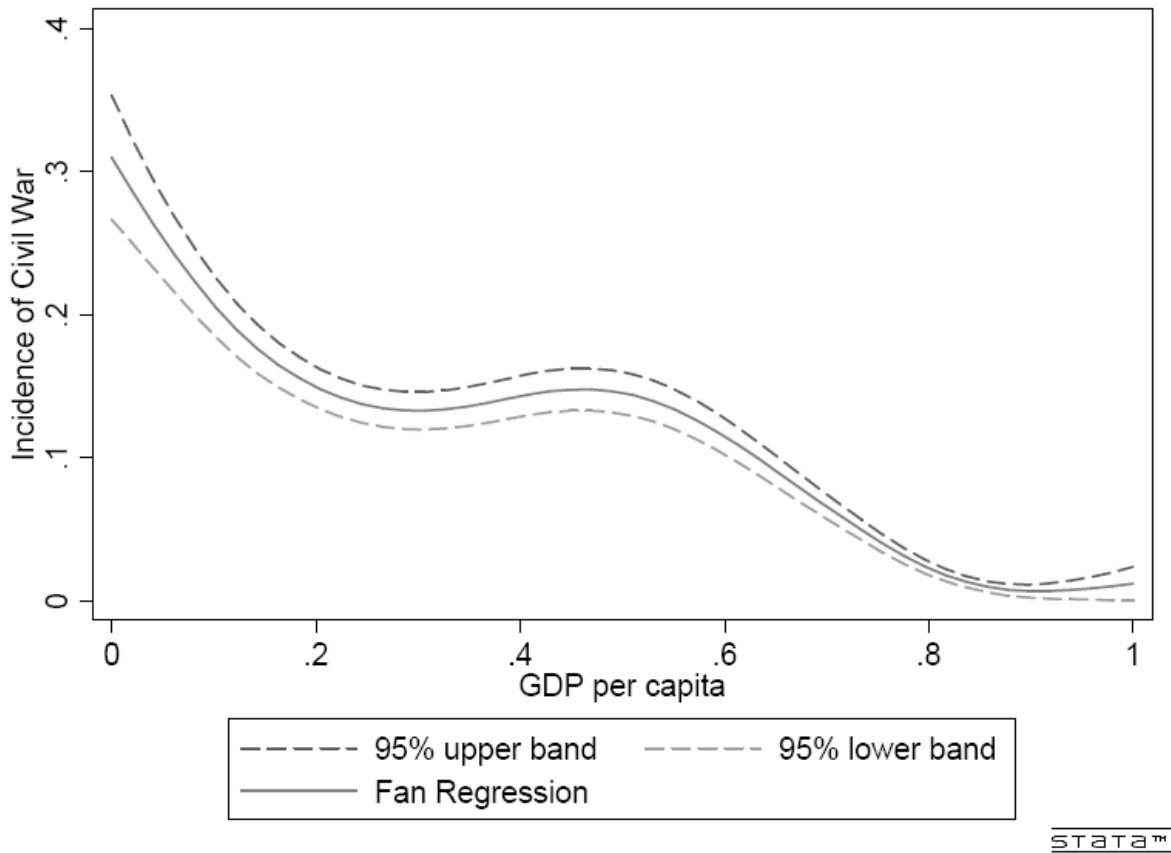
**Sources:** Data based on UCDP/PRIO armed conflict database database. Civil wars are those internal conflicts that count more than 1,000 battle deaths in a single year. Civil war or conflict includes cases with at least 25 battle deaths in a single year.

**Figure 2: Proportion of countries with an active civil war or civil conflict, 1960-2006**



**Sources:** Data based on UCDP/PRIO armed conflict database (Gleditsch, et al. 2002). Civil wars are those internal conflicts that count more than 1,000 battle deaths in a single year. Civil war or conflict includes cases with at least 25 battle deaths in a single year.

**Figure 3: Incidence of civil war by country income per capita, 1960-2006**



**Sources:** Figure displays the results of a Fan regression of the incidence of civil war on GDP per capita percentiles (bandwidth=0.3, bootstrapped standard errors). Population and GDP data are drawn from the World Development Indicators (World Bank 2008). Civil war incidence is drawn from the UCDP/PRIO armed conflict database (Gleditsch, et al. 2002; Harbom and Wallensteen 2007).