

Understanding Civil War: a New Agenda

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The papers in this Special Issue of the *Journal of Conflict Resolution* are drawn from the first round of a research project on civil war initiated by the World Bank.² Civil war is pertinent to the World Bank because it occurs disproportionately in low-income countries and evidently further reduces income. Hence, it is of concern for an organization whose mission is poverty reduction. Since the World Bank is a financial institution, the instruments over which it has influence are primarily economic. One focus of the project has therefore been to investigate the extent to which civil war might have economic causes, as well as the more evident economic consequences. However, the project's objective is wider than to help guide the Bank's own activities. In the international community, many policies towards conflict have to date been guided by little more than rules of thumb derived by practitioners from their experience. Policy has not rested on a solid foundation of research-derived knowledge. The ultimate goal of our research project is to stimulate the research community into providing these foundations.³

If conflict has a substantial economic dimension, economists can make a positive contribution to conflict analysis. Most contributors to this volume are economists, but this does not presuppose that economics is more important—or even as important—as political science in generating better research foundations for policy. Rather, it is an effort to rectify what we perceive as the relative neglect of economics in the study of conflict and, correspondingly, the integration of conflict in the study of economics.

Conflict policy and analysis can be divided into three areas: prevention, settlement, and post-conflict recovery. The papers in this collection discuss questions of relevance to all three areas: they attempt to empirically discover the factors that cause conflict; they deal explicitly with the problem of reaching a settlement of civil war; and

they analyze the problem of securing credible commitments to a settlement, an issue that bears directly upon the problems of maintaining a post-settlement peace.

In considering the causes of conflict, we draw on Hirshleifer's (1995) distinction between preferences, opportunities, and misperceptions. This distinction can also be applied to the difficulties of reaching a settlement and maintaining post-war peace.

Let us start with the causes of conflict. Understanding the causes of observed conflict is not necessarily synonymous with understanding its motivation. Motive may or may not be more decisive than opportunity for action. At one extreme, some societies are structured so that particular groups suffer exceptional exploitation and this in turn provokes rebellion. Understanding the exceptional circumstances of exploitation would provide both the motivation for conflict and its explanation: conflict would be explained by well-grounded preferences and this has been the basis for much work on conflict analysis within political science. By contrast, some of the economics literature has assumed that the motivation for rebellion is not grievance but greed – the extortion of economic rents on a grand scale by quasi-criminal rebel groups. On this analysis motivation is divorced from explanation. Presumably, greed is a vice found in most societies, so what distinguishes rebellious from peaceful societies cannot be motivation—or at least not simply motivation—but rather opportunity, which makes rebellion profitable only in some societies. The remaining explanation for conflict is misperception. Both grievance and opportunity for rebellion can be misperceived by potential rebels. At one extreme, rebels with a legitimate grievance may misperceive or underestimate the opportunities available to them for action. At the other extreme, plenty of opportunity for rebellion may exist in societies where groups' grievances are

objectively unrelated to their circumstances. Whether any of these groups resorts to large-scale violence may depend on the groups' perceptions of the extent of their grievance as well as upon the opportunities to do so—the presence of a charismatic leader, the availability of finance, or the military weakness of the government. Grievance can be the constitutive grammar of conflict or simply its discourse, with no more explanatory power as to the determinants of observing violence than either perception or opportunity. Of the papers in this issue, that by Collier and Hoeffler applies to Africa an econometric model based on a theory stressing the importance of opportunities for rebellion. Reynal-Querol's paper develops a rival econometric model stressing objective grievance as the cause of violence.

Once they have started, conflicts may endure because of misperceptions (or private information) about relative strength. Conflict initiation may similarly be the result of an information failure and ongoing conflicts may turn into wars of attrition based on a lack of common information about likely outcomes of the war. Alternatively, wars may persist because of the lack of opportunity to reach a settlement. In the absence of what economists call a 'commitment technology' there cannot be a time-consistent credible settlement to a civil war that is Pareto-improving. Returning to the status quo ante implies that rebel groups must demobilize and disarm, which provides the government with an opportunity to violate the terms of a settlement with little risk of reprisal. Similarly, the rebels may be unable to commit to prevent the entry of new rebel groups seeking to occupy the same viable niche in a war economy. Two papers in this volume address problems of time-inconsistency. Jean-Paul Azam, and Garance Genicot and Stergios Skaperdas discuss settlements that are rational during the conflict, but

become irrational once the violence stops. The two papers explore some of the conditions that generate new incentives for violence in post-war transitions.

Finally, an enduring conflict may be the direct result of antithetical and irreconcilable preferences. Hatred and ideology can motivate parties to use violence even if this is costly for all. Resorting to violence may be easier if the people within a community who inflict the most harm are those who receive the least harm in return (e.g. leaders, aristocrats, adult males). The weakest elements of society are in no position to restrain the strongest—the perpetrators of violence. Thus, conflict might arise in communities where *intra*-community political institutions are incapable of restraining a minority with a taste for violence and cannot enforce the majority's preferences for peace. In this volume these concerns are explored in Scott Gates's paper, which investigates the circumstances in which a rebel organization will persist, collapse or win; and in Garance Genicot and Stergios Skaperdas' paper, which considers when it will be rational to devote significant resources to negotiating a settlement rather than to fight a war. Patrick Regan's paper is also relevant to this discussion in that it considers the impact of external partial intervention to the duration of war and the potential for a conflict settlement. Such interventions can be seen as lowering the costs of negotiation, or changing the balance of power, thereby influencing parties' perceptions and opportunities (for a given set of preferences).

Once violence is initiated, it may follow a path-dependent process. It is an empirical regularity that the risk of war recurrence in post-war societies is higher than the risk of onset of a new war in countries with no prior war history. The causal links are not clear in this case: it may be that the same underlying conditions that caused the first war

also cause subsequent wars; or the heightened risk may be due to the effects of previous wars on a country's society and its political economy.⁴ We do observe, however, that civil wars generate a conflict trap. Hatred and other rebellion-specific capital accumulate during war, making further conflict more likely. The economy deteriorates, making resource-driven rebellion more viable. In this collection of papers, James Murdoch and Todd Sandler, and Brock Blomberg and Gregory Hess examine the latter possibility empirically and offer insightful views of the dynamics of civil war.

Let us consider the papers in turn. The first two papers in the volume focus on the causes of conflict, using econometric modeling and reaching somewhat different conclusions. Collier and Hoeffler (CH) apply to Africa an econometric model of civil war onset. The model is based on a set of global empirical patterns and a theory of civil war developed in Collier and Hoeffler (2001). The results may be interpreted in several ways. The interpretation chosen by the authors is that opportunity is much more important as a cause of conflict than is objective grievance. It is an open question whether opportunity is linked to war because it facilitates greed-driven rebellion or because it allows the pursuit of redressing objective grievances. In effect, rebels groups in the CH model are equally interpretable as for-profit or non-profit organizations. Applying the model to Africa, the authors find no basis for African exceptionalism. Africa is distinctive only because some of its pertinent characteristics are atypical, but not because behavioral relationships are different. Their analysis of the rise in the incidence of war in Africa is radically different from common perceptions. They suggest that the rising incidence of civil war is fully explained by Africa's distinctive economic performance – negative growth rates, and hence cumulatively lower levels of income, and

rising dependence on primary commodity exports. They find that Africa's distinctive social structure – high ethnic and religious diversity – is a source of stability, rather than a source of risk: globally, diverse societies are safer than homogenous societies.

The model places economic policy towards growth and diversification at the top of the conflict prevention agenda, giving social and political considerations a lower priority. Both the model and its interpretation can be challenged in several ways, but the most telling would be a direct assault upon the econometric results. In this collection of papers, the nearest to such a challenge comes from Reynal-Querol. Two of her results are of potential importance. First, she finds that religious polarization is significant in explaining ethnic civil war, but her results are based on a more limited data set than that of CH.⁵ The variables in her model are also not sufficiently close to those in the CH model for direct comparison. However, the emergence of rival results from the analysis of large data sets is a useful first step in determining the robustness of any result. Reynal-Querol's also finds that the structure of democracy is an important determinant of ethnic war onset. This challenges a controversial result from the CH model –that, controlling for other variables in the model, the level of democracy has no effect on conflict risk. In political science, it has long been argued that this relationship is non-monotonic in that both deep democracy and extreme repression can lower the risk of rebellion as compared to intermediate or transitional regimes. CH do not find this effect. Reynal-Querol goes deeper than previous analyses and argues that the level of democracy does not capture the importance of political institutions in preventing civil violence. She models aspects of the political system and argues that the key distinction is not that between a democracy and an autocracy, but rather between the differing structures of democracies.

Specifically, consociational democracies –proportional representation systems that produce coalition politics– reduce conflict risk relative to first-past-the-post, winner-take-all systems. This result echoes the one grievance variable that CH find to be significant, namely the higher risk that comes from ethnic dominance (the largest ethnic group having 45-90% of the population).

While the Reynal-Querol result is plausible, and potentially opens up the important territory of constitutional design in conflict-prone societies to rigorous analysis, it is also fragile. Most democracies are high-income countries and high-income countries have a very low incidence of civil war. Thus, civil war is very rare in both consociational democracies and other democracies. The data set might be enlarged by taking the analysis back well before 1960, using the data sets recently produced by quantitative economic historians such as Angus Maddison (2001). Reynal-Querol's analysis would also benefit from further development of a theory of political exclusion that explains why the different political systems, which she considers in this paper, can be ordered along their level of inclusiveness in ways that matter to preventing ethnic conflict. Several intervening variables –such as the strength of local or regional governments, the independence of the judiciary, the ethnic affiliation of parties—should also be important in determining the level of effective political participation in any system. Similarly, the effects of religious polarization (as captured by the two indices used in this paper) may be moderated by controlling for the degree of secularism of different societies – the political implications of religious divisions must be different in secular states as compared to theocracies. More research into these questions is clearly necessary. Reynal-Querol's paper is a springboard to a promising research agenda that

goes beyond the use of blanket proxy variables and distinguishes between various types of war, using more refined measures of potentially significant variables.

Our focus now shifts from war onset to war duration as we turn to Patrick Regan's paper. Regan brings international relations into the study of civil war by analyzing the impact of external intervention on civil war duration. He uses an important new data set on civil war interventions (military or economic) and asks whether or not they have the desired effect (of ending the violence). Regan utilizes duration models to estimate econometrically the hazard of continuing war and finds that interventions usually prolong civil war.⁶ Only interventions that clearly favor the government appear to shorten conflict. This again tends to suggest that the rules of thumb on which practitioners have based their interventions has been seriously deficient. Regan's analysis echoes Richard Betts' (1994) earlier argument that no external intervention can be impartial and that the international community or interested third parties should intervene in favor of the strongest party to end the war quickly. This analysis has three possible problems: First, it does not tackle the normative implications of a policy rule of intervention in favor of the government or of the strongest party irrespective of the perceived justice of the rebellion. Second, even if we separate these normative considerations from the positive implications of intervention, given the usual informational asymmetries during war, it is often difficult for third parties to gauge the relative strength of combatants and hence to know which side they need to assist to end the war quickly. Third, our data limitations do not allow us to gauge the economic and human costs of external military intervention. Certainly, if intervention prolongs war, it should increase these costs, ceteris paribus. But it precisely the relationship between

external intervention and all other explanatory variables that we do not fully understand so we do not yet know the conditions under which intervention is more likely to occur.⁷ This makes it harder, in turn, to appreciate the full gamut of consequences of external intervention. Fourth, Regan's data require further refinement and the analysis omits variables that other researchers have identified as significant in determining civil war duration. These concerns notwithstanding, Regan's work on external intervention is the gold standard in the field and further improvements in his data set will add to our understanding of the links between external and internal dimensions of civil war.

The next two papers are also empirical, but focus upon the economy. Blomberg and Hess investigate the two-way relationship between war risk and economic performance using MCMC estimations. They find that recession increases the risk of conflict; a result that is consistent with Collier and Hoeffler's finding that slower economic growth increases risk. Blomberg and Hess expand Collier and Hoeffler's analysis by adding a new variable—external conflict—to their model. This specification also emphasizes the links in practical applications of the fields of international relations, comparative politics, and development economics, as Blomberg and Hess find that the conjunction of recession and external conflict is the cocktail that maximizes the risk of internal violence conflict. Worse, the authors find that conflict worsens the economy, so that there is the potential for a trap—a cycle of economic deterioration and repeat conflict.

The cost of violent civil conflict in terms of economic growth is also the theme of Murdoch and Sandler's paper. Like Blomberg and Hess, they also find that conflict reduces growth, but they add an important extension: spillover effects to neighboring countries and the broader region. Murdoch and Sandler utilize spatial econometric

techniques to identify and measure such spillovers. Indeed, a civil war in one country has negative economic externalities for other countries over a very considerable radius; and the larger the shared space (border) between any two countries, the larger the spillover effects of civil war. Note that Murdoch and Sandler introduce civil war into a growth model that already controls for the rate of investment. Hence, one obvious route by which conflict will reduce growth, namely by lowering investment, is not incorporated into the model. Further channels through which war may affect growth (e.g. human capital) are not fully explored except in the construction of interaction terms between, for example, a civil war variable and human capital. Nevertheless, Murdoch and Sandler discover that in addition to any effect of war on the rate of investment, civil war has negative spillover effects by reducing the efficiency with which resources are used. For example, war might induce neighbors (and their neighbors) to increase military expenditure, thereby diverting labor from more productive uses. An important finding also concerns the effects of different levels of war on patterns of economic growth – predictably (but interestingly) these effects are greater the greater the magnitude of the war. The authors carefully distinguish between the short-term and long-term effects of civil war on growth and find those effects to be contained mostly in the short term. However, intense civil wars have deeper long-term effects – perhaps mirroring the consequences of destroying so much human capital and foregoing so much investment.

Turning from the empirical to the theoretical papers, we now get a sense of why people join rebel movements and why we observe war given that war is costly for all.

The paper by Scott Gates focuses on the question of rebel recruitment. If the Collier-Hoeffler model is broadly correct, this is precisely the sort of theory that needs to

be supplied to explain the empirics. The CH model emphasizes the viability of the rebel organization as the decisive explanatory factor in civil war and one of the key components of viability is the organization's ability to recruit members and prevent their defection. Differential ability to recruit is the interpretation that Collier and Hoeffler place on three of the variables in their model. They interpret the level and growth of income as proxy variables for the earnings that would be foregone by joining a rebel organization. They interpret the otherwise puzzling finding that socially diverse societies have a lower risk of conflict as reflecting the need for social homogeneity within a rebel group: a diverse society thus offers a more limited recruitment pool, controlling for population size. Gates models the recruitment problem, first treating the rebel organization in isolation, and then introducing a government and even a rival rebel organization. He rightly stresses that one of the defining peculiarities of a rebel organization is that being extra-legal, it cannot rely upon normal means of contract enforcement. Gates' analysis rests on the proposition that other things equal a rebel is more likely to defect as his 'distance' from the leader grows. Distance can be defined culturally (as with ethnic identity), ideologically, or geographically. To avoid defection, the leader must compensate the rebels either financially, or in some other way and this is costly. Gates' analysis is pertinent both to the initiation of war and its duration. For war initiation, the central issue is to identify the circumstances under which when rebel leaders can recruit enough labor to make their organization viable. For war duration, the key question is under which conditions the soldiers of one army might be prevented from defecting into the other army. This paper is therefore an excellent bridge between the

macro-econometric studies of civil war and the micro-economic foundations of individual behavior that underlies the large-N econometric studies.

The final two papers are the ones most evidently in the tradition of the formal conflict resolution literature. At last, there is a general equilibrium with at least two parties and both papers address a central question in the rational choice literature on war: if war is costly and Pareto inefficient, then why do we ever observe war? Why don't the parties try to reach an agreement short of war that would leave them both better off?

Azam presents a model in which the main activity of both government soldiers and rebels is the looting of economic resources. In effect, both parties target civilians. Consistent with the CH model, if external financing is available, rebels will be less likely to loot (this is a view of a 'non-profit' rebel organization that loots simply to remain operational). Azam then focuses on the potential for a Pareto-improving bargain that bans looting and finds that, without side-payments, such a bargain is unlikely. Yet the provision of side-payments is complicated due to a time consistency (or enforcement) problem: how can the rebel group trust the government to provide side-payments once the military threat has receded? The revenue that the rebel group receives from looting constitutes a bound to the negotiating range, to which a government may not be able to credibly pre-commit. Azam considers the case in which the government represents a rich majority confronting a poor minority –his context being ethno-regional disputes in Africa. In order to pre-commit, the government needs some constitutional device not provided by winner-take-all democracy. In effect, this takes us back to the favorable effect of consociationalism in Reynal-Querol's paper which seems preferable to winner-take-all democracies and to the problem of 'ethnic dominance' identified by Collier and

Hoeffler (2001). Azam is effectively asking how to deal with ethnic dominance, given that this is the circumstance in which even consociational politics will not work? If the largest ethnic group is sufficiently large not to have to build a coalition with other groups, how can we secure political cooperation and equal representation?

Azam's proposed solution to this commitment problem is that the army should act as a constitutional guarantor. To do this, it needs to over-represent the minority group. He sees this as pertinent to the conditions of West Africa, where quite commonly a poor northern minority is over-represented in the army and secures transfers from a richer south which prevents it from resorting to violence. Note, however, that in one of the most serious West African civil wars—the Biafran war in Nigeria in the late 1960s—it was precisely the prospect of this south-to-north transfer that triggered rebellion. In this case, the prospect of power and resource transfers caused a war that was initiated not by the poor north, but rather by the richer south. In effect, the rich region feared that the transfers produced by majority rule would be so large that the outcome of such an arrangement would not be preferable to war. The notion of the rich region as the minority may be more applicable than Azam's assumption that it is the poor region that is the minority. Natural resources may be concentrated in a relatively small part of the country so that there is one favored region. It is possible that the favored region would rebel to pre-empt redistribution. This small modification could still be portrayed within Azam's framework, but it would reverse the policy conclusion—the army should in this case guarantee an upper limit on redistribution, rather than a lower limit. War would then be the result of too much redistribution from the rich to the poor. It is straightforward to

see which of these scenarios is empirically more likely to occur and we invite more empirical research to test the implications of Azam's theoretical framework.

Genicot and Skaperdas also develop a model in which resources can be devoted to production or fighting, but they add negotiation as a third resource-using activity. This model captures the reality of civil war settlements in which the parties frequently shift between strategies of war and negotiation. Here, the slow accumulation of institutions develops restraints that can guarantee an agreement, but these institutions are themselves costly to produce and so require a conscious decision to utilize resources for institution-building, rather than for production or fighting capacity. Genicot and Skaperdas' analysis is insightful and generates two important results that explain why civil war occurs in the first place. First, they find that with lower income, the incentives to invest in institution-building decline. As a result, the conflict equilibrium of high military spending is more likely to occur. Secondly, they show that for a territory of a given size, the higher the number of actors the lower will be the investment in institution-building and the higher the expenditure on the military option. Thus, for example, a low-income country with many ethnic groups willing to consider the conflict option, would, according to the model, have a markedly lower chance of reaching and maintaining a negotiated settlement of conflict. It is perhaps why in such a context Azam is driven to invoking the government army itself –which for Genicot and Skaperdas is a resource devoted to conflict– as the institution that will guarantee the settlement.

It would be useful to consider the implications of forging alliances among parties to a conflict. As the number of parties rises, the number of politically-relevant factions (or alliances) need not increase proportionately, as cross-cutting alliances may form that

influence the number of active parties on any one issue dimension. In the international relations literature, scholars have long since considered the implications of polarity on system stability and, by extension, war. Doyle and Sambanis (2000) have extrapolated from that literature and considered the effects of the number of factions on the likelihood of post-civil war peacebuilding success, finding that this relationship is not linear. Further theorizing about the number of parties, the propensity to form alliances, and their impact on the decision to invest in conflict management would be a welcome extension to the Genicot and Skaperdas study.

A related important point is that, for both Azam, and Genicot and Skaperdas, the number and identity of groups that might resort to conflict is given exogenously. However, in the circumstances in which conflict is profitable, the ‘Machiavelli Theorem’ (Hirschleifer, 1994) by which no profitable opportunity for conflict will be passed up, the existence of at least one rebel group is endogenous. If an existing group is bought off, another will replace it. In such a circumstance there is no point in negotiating a settlement because the rebel group cannot credibly commit to prevent competitors replacing it (Collier, 2000). Equivalently, if subjective grievance is sufficiently common, if one group is satisfied by side-payments, the viable niche for a not-for-profit grievance group will be filled by some other group, and so negotiation is again pointless. In either of these cases, preemptive negotiation between groups is not possible because the groups are not defined. Even if the society organized itself so that the distribution of resources was completely equal between persons, the incentive to rebel would remain. This goes back to the fundamental distinction between opportunity and objective grievance. The more important is objective grievance as a cause of conflict, the more reasonable it is to

treat groups as exogenous –the groups are, in effect, endogenous to the grievances but the grievances are exogenous. Redistributing to redress the grievance is therefore likely to be an effective means of conflict prevention.

Hence, the opportunity/objective grievance distinction informs the appropriate strategy for conflict prevention in two ways. If disputes are generated by opportunities rather than by objective grievances, it is at least doubtful whether the subjective grievance which may motivate the rebellion can be accommodated by redressing grievances. More fundamentally, even if one group is successfully accommodated, other groups will enter to fill the niche. By contrast, if objective grievance is the cause of the conflict, unless the grievance is addressed it may be impossible to sustain peace regardless of the resources devoted to reducing the viability of rebellion. A low-viability strategy might ensure that a rebellion fails, but each failed rebellion may be rapidly replaced by another induced by the same grievance.

In conclusion, the papers collected here demonstrate the potential for the formal analysis of civil war to contribute to policy across a wide range. However, as is evident from our brief review of potential explanations of conflict initiation, persistence and repetition, many questions remain unanswered even at a theoretical level. The shortage of quantitative analysis able to distinguish between theories is even more apparent. Until the profession has a body of well-specified theory, substantiated both by quantitative analysis and the more informal evidence from case studies, the world of policy practitioners will continue to rely upon rules of thumb.

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² Most of these papers were first presented at a March 2000 conference on the “Economics of Civil Wars” co-sponsored by the World Bank Research Department and Princeton University’s Center for International Studies. Authors were invited to submit revised drafts for this issue. All papers were reviewed by two anonymous referees and the special issue editors. A subset of submitted papers was accepted for publication.

³ The project covers civil violence and crime broadly defined, but has focused on civil war as the most destructive form of violent civil conflict. For information on the project and related publications, see <http://www.worldbank.org/research/conflict/index.htm>.

⁴ The latter hypothesis is tested empirically for all post-civil war transitions since 1945 in Doyle and Sambanis (2000). The authors find a significant positive correlation between the level of war-generated hostility and the likelihood of civil war recurrence within five years of the end of the first war.

⁵ Reynal-Querol follows Sambanis (2001) in separating ethnic/religious from other types of civil war. Sambanis (2001) argued that different types of civil war have different causes and found that political variables (lack of democracy) were significantly correlated with ethnic war onset. Thus, Sambanis argued that civil war events should not be pooled without fully exploring and accounting for such differences. Reynal-Querol’s research design builds on Sambanis’ (2001) result by considering the causes of ethnic war onset, whereas the Collier and Hoeffler analyze the entire population of civil wars.

⁶ Elbadawi and Sambanis (2001), using Regan's data to test a different model focusing on the link between intervention, ethnic fragmentation and autocracy, also find a significant positive association between military intervention and civil war duration.

⁷ A first cut at this problem is Elbadawi and Sambanis (2001) who endogenize the intervention variable and explore the consequences of expected intervention on civil war duration. They are constrained by the paucity of data on interventions that originate prior to the coded initiation of a civil war –making it difficult to gauge the degree to which interventions were also causes of war onset as well determinants of war duration.