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Aiding and Abetting: How International Humanitarian Assistance Can Inadvertently Prolong Conflict and How Combatants Respond

A dissertation submitted in partial satisfaction of the requirements for the degree Doctor of Philosophy

in

Political Science

by

Neil Narang

Committee in charge:

Professor David A. Lake, Chair
Professor Erik Gartzke
Professor Peter Gourevitch
Professor Miles Kahler
Professor Craig McIntosh

2011

The Dissertation of Neil Narang is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

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Chair

University of California, San Diego

2011

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complicated puzzle always impressed me. I am lucky to have received his help and encouragement throughout graduate school. I am also grateful to Peter Gourevitch for encouraging me to think about my project in terms of the big picture. His comments were always helpful and he always made my work better. Finally, Craig McIntosh served as a constant reminder – through his comments at seminars and workshops – that I should strive first and foremost to be a good scientist. He taught me that there are often powerful limitations to the inferences social scientists can make given our data.

I was also fortunate to have great colleagues in the political science department at UCSD. I will be forever indebted to my fellow graduate student and friend Brad LeVeck. As a colleague, Brad read every paper, cover letter, research statement, and application I put together. He even listened to me give practice talks over the computer from thousands of miles away. As a friend, Brad was there to listen when the program got especially tough my second year, and he was there to give advice whenever life decided to intervene. Leigh Bradberry and Chris Milliron also deserve special thanks. Leigh was like a sister to me through the program – always checking to make sure I was getting out and watching tennis. I also benefited from observing the friendliness and grace with which she handled the entire dissertation process. Chris was the most supportive roommate I could have asked for during our second year of graduate school. As the pressures of comprehensive exams set in, Chris served as a constant reminder to laugh and stay balanced. The rest of my fellow graduate students, including Alan Bolar, Chris Dawes, Justin Gottschalk, Molly Hamilton, Paula Jacobson, Sarah Knoesen, Derek Liu, Ellen Moule, Ben Tang, and Andrew Waugh also deserve thanks for helping me complete this project by providing valuable conversations and opportunities to socialize.

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Can Humanitarian Aid Inadvertently Prolong Conflict? A Theory and Evidence from Panel Data

Forgotten Conflicts? Political Priority versus Need in the Allocation of Humanitarian Assistance

Humanitarian Aid, Bias, and the Durability of Peace after Civil War

Securitizing International Security: How Unreliable Allies Effect Alliance Portfolios

Tying Hands and Sinking Costs using Alliance Terms as Risk Premia

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ABSTRACT OF THE DISSERTATION

Aiding and Abetting: How International Humanitarian Assistance Can Inadvertently Prolong Conflict and How Combatants Respond

by

Neil Narang

Doctor of Philosophy in Political Science

University of California, San Diego, 2011

Professor David A. Lake, Chair

The provision of humanitarian assistance has rapidly become a core component of modern peacebuilding and post-conflict reconstruction. Yet, despite the normative appeal of providing humanitarian assistance to the victims of violent conflict, aid workers and analysts frequently claim that humanitarian assistance can inadvertently prolong war. If such claims are true, the very treatment that the international community has been employing to address the consequences of violent conflict may actually be prolonging war and increasing the amount of suffering over time. To date, however, the evidence to support these claims is mostly anecdotal, and a satisfying theoretical link between humanitarian aid and the duration of war has yet to be specified

This dissertation explores the link between humanitarian aid and the duration of war both theoretically and empirically through a series of four papers, with each paper serving as a chapter. I show how humanitarian aid can inadvertently prolong civil war when disbursed during conflict (Chapter 3) and how humanitarian aid can undermine

peace when disbursed in the aftermath of civil conflict (Chapter 4). I also show that – as a result of these effects – combatants strategically respond to aid provisions by violently attacking aid workers (Chapter 5). In each paper, I test propositions econometrically using observational data to estimate the relationship between the level of aid and political violence at the macro and micro levels.

Chapter One
Introduction: Humanitarian Aid and the Duration of Civil War

The provision of humanitarian assistance has rapidly become a core component of modern peacebuilding and post-conflict reconstruction. In just the two decades since the end of the Cold War, the amount of international humanitarian aid reported through the OECD Development Assistance Committee (DAC) from member and non-members states increased nearly 1400-percent in real terms, from $796 million USD in 1989 to well over $11 billion USD in 2008. The overwhelming majority of these resources have been allocated across conflict and post-conflict areas to provide basic resources like food, shelter, and medical supplies to victims of violent conflict. For example, in the 1990's, the international community provided billions of dollars in humanitarian relief to assist refugees and internally displaced persons (IDPs) in places like Rwanda and Bosnia. And since 2001, this strategy has continued more intensely in Afghanistan, Iraq, and Sub-Saharan Africa. By one estimate, 80 percent of all social services like healthcare and education in Afghanistan today are provided through contracts with international aid organizations (Cohen et al. 2009).

Yet, despite the normative appeal of providing humanitarian assistance to the victims of violent conflict, aid workers and analysts frequently claim that humanitarian assistance can inadvertently prolong war (Anderson, 1999; Luttwak, 1999; Terry, 2002). During the Rwandan Civil War, for example, western journalists and international watchdog organizations joined the Rwandan government in accusing the United Nations High Commissioner for Refugees (UNHCR) and its aid contractors of indirectly fueling

the conflict by assisting Hutu war criminals in competition with the state (Gourevitch, 1999). In Bosnia, aid workers and international observers argued that safe- zones created to provide relief services inadvertently prolonged fighting and resulted in the death of nearly 20,000 people in and around the aid enclaves (Woodward, 1995). Now, in Afghanistan, President Hamid Karzai has consistently ridiculed the oversupply of foreign assistance for aiding insurgents in competition with the state (Cohen, Kupcu & Khanna, 2008).

If such claims are true, the very treatment that the international community has been employing to address the consequences of violent conflict may actually be prolonging war and increasing the amount of suffering over time. To date, however, the evidence to support these claims has been mostly anecdotal (Borton, 1998; Shearer, 2000; Terry, 2002). That is, existing research has failed to determine how robust the empirical relationship is or what underlying factors help explain variation in the effect across cases. This is surprising given that the empirical relationship is the biggest issue at stake for policy makers and practitioners debating whether the provision of humanitarian assistance is an effective peacebuilding and post-conflict reconstruction strategy in conflict and post-conflict areas. Moreover, a satisfying theoretical link between humanitarian aid and the duration of war has yet to be specified (Humphreys, 2005; Ross, 2004a; Ross, 2004b). This is important because without a theoretically coherent model of the interaction, it is impossible to identify the conditions under which the tendency for aid to prolong war will be more or less acute and adjust policy accordingly.

This dissertation explores the link between humanitarian aid and the duration of war both theoretically and empirically. I aim to address three primary research questions. First, can humanitarian assistance inadvertently prolong conflict when administered as a peacebuilding strategy during civil war, like it appears to have done during the Bosnian Civil War? Second, can humanitarian assistance inadvertently undermine peace when administered as part of a post-conflict reconstruction strategy in the aftermath of civil war, like it appears to have done following the Rwandan Civil War? And finally, how, in theory, does humanitarian aid affect the incentives of competing parties such that they might prefer to fight rather than accept a negotiated settlement that avoids the costs of war?

To investigate these questions I draw on existing bargaining models of war to show how humanitarian assistance can inadvertently prolong conflict. Specifically, I argue that humanitarian assistance can prolong war when administered during an ongoing conflict by decreasing the informational value of fighting. Dynamic bargaining theories of war suggest that opponents fight in order to reduce uncertainty in a less-manipulable forum than the bargaining table by observing their opponent's costs of war and risk of military collapse over time (Filson & Werner, 2002; Filson & Werner, 2007; Powell, 2004; Slantchev, 2003; Slantchev, 2004; Smith & Stam, 2004; Wagner, 2000). In these models, war is treated as a costly learning process in which sides signal their strength by enduring and imposing the costs of war. It follows that if war is not costly, sides learn nothing from engaging in it, and the less costly war becomes, the longer fighting will last as conflicts remain marked with uncertainty (Powell, 2004). International humanitarian

assistance is explicitly designed to mitigate the costs of war. Paradoxically, this implies that treating conflicts with greater levels of humanitarian assistance may prolong violent conflict by slowing down the accrual of information that allows opponents to converge on more congruent estimates of relative strength and coordinate expectations about what each is prepared to accept in a settlement. Critically, however, I argue that this effect should only hold provided opponents are somewhat uncertain about the advantage gained from international assistance.

In the post-conflict context, I once again draw on bargaining models of war to explain how humanitarian assistance can inadvertently undermine peace when administered as a post-conflict reconstruction strategy in the aftermath of civil war, and to explain why relief provisions appear to be associated with renewed conflict after some wars and not others. My theoretical mechanism follows from what I identify to be a fundamental contradiction in the global humanitarian aid model: although the principles of humanitarian assistance dictate that aid be distributed in accordance with need while remaining neutral with respect to the political stakes, these principles are prone to contradiction in the post-conflict context where need is often correlated with opponents' performance in the previous contest. In these cases, I argue, humanitarian assistance is likely to be biased towards the conflict-loser and – as a result – aid can create a revisionist party with the incentive to renegotiate the post-conflict settlement. Importantly, however, I expect these effects to be highly conditional. I hypothesize that aid is most likely to create a revisionist party after decisive military victories where one side suffered a disproportionate share of the costs and thus exhibits a greater level of

humanitarian need to be targeted by aid providers. Conversely, I expect that the effect of aid on the durability of peace after stalemates and relatively close victories will be far less significant, as competing parties are likely to exhibit similar levels of need, which in turn causes them to receive relatively similar levels of humanitarian assistance, thus leaving the post-conflict distribution of power relatively unaffected.

I test the empirical implications of my theoretical mechanisms in both strategic settings: first, when aid is disbursed as a peace building strategy during civil war, and second, when aid is disbursed as a post-conflict reconstruction strategy in the aftermath of civil war. In both cases, I focus on the unintended consequences of humanitarian assistance as it relates to the duration of civil war and the durability of peace.

In general, I find strong support for my arguments in both strategic contexts. After controlling for factors known to be correlated with the duration of civil war and durability of peace that are also significant predictors of the level of humanitarian assistance disbursed in each civil war and post-civil war year (to control for selection bias), I show that the level of humanitarian assistance appears to be associated with the continuation of civil war. Importantly, however, I show that this perverse side effect is also highly conditional. That is, the tendency for aid to prolong conflict appears to be mediated in predictable ways by the strategic context in which the humanitarian assistance is provided. The evidence suggests that when humanitarian relief is provided during ongoing civil wars, aid appears to prolong conflict only to the degree that it adds uncertainty to the bargaining process. Empirically, I show that this is more likely when aid provisions are disbursed in peripheral regions of states and are thus more difficult to

observe by a government. And when humanitarian relief is provided to the victims of violent conflict after civil wars, the evidence suggests that aid can undermine peace only when it is substantially biased such that it creates a revisionist party with the incentive to renegotiate the post-war settlement. Empirically, I show that this is more likely after decisive military victories where one side suffered a disproportionate share of the costs and thus exhibits a greater level of humanitarian need to be targeted by aid providers.

Overview of the Dissertation

This dissertation explores the link between humanitarian aid and the duration of war both theoretically and empirically through a series of four papers, with each paper serving as a chapter. I show how humanitarian aid can inadvertently prolong civil war when disbursed during conflict and how humanitarian aid can undermine peace when disbursed in the aftermath of civil conflict. I also show that – as a result of these effects – combatants strategically respond to aid provisions by violently attacking aid workers. In each paper, I test propositions econometrically using observational data to estimate the relationship between the level of aid and political violence at the macro and micro levels.

In Chapter 2, I explore the allocation of humanitarian assistance across conflict areas. Despite a principled commitment to assist people in need equally, the allocation of humanitarian aid across conflict and post-conflict states shows remarkable variation that is not easily explained by differences in the level of need. This paper attempts to explain "forgotten conflicts" by analyzing the determinants of humanitarian assistance to civil

war and post-civil war states. Using cross-national panel data on humanitarian aid provisions, I show that the most important determinants of international humanitarian assistance are not always demand-side factors measuring humanitarian need – as the principals of humanitarian action would dictate – but rather supply-side factors that effect donors' willingness and ability to provide humanitarian assistance. Specifically, I find that although humanitarian assistance provided to ongoing civil wars is significantly more humanitarian than strategic in its allocation, aid to post-conflict recipients tends to target conflicts where donors perceive important strategic and political interests. Substantively, the strategic interests of the largest donors explain roughly 25-percent more of the variation in humanitarian aid giving to post-conflict states compared to humanitarian factors measuring recipient need. The results suggest that one important explanation for why some conflicts are essentially ignored or gradually neglected over time is that the strategic interests of donors come to dominate humanitarian concerns after civil wars terminate.

Exploring the allocation of humanitarian assistance across conflict areas also serves a second important function in the context of the larger project. In all likelihood, the provision of humanitarian aid across conflict and post-conflict states is not random. Aid organizations and donor governments are likely to make allocation choices based on where humanitarian assistance is most needed or where it is likely to be most effective. This non-random selection raises an important concern for estimating the impact of humanitarian assistance on the duration of civil war and the duration of peace after a civil war. If the amount of humanitarian assistance disbursed in any period is determined

based on observable indicators of humanitarian need that are also correlated with the duration of war or duration of peace, then any relationship between the amount of humanitarian assistance and these outcomes observed in the data may be the result of selection bias – wherein conflicts that have an underlying likelihood of continuation also have a higher propensity to be treated. To account for the bias introduced by strategic selection, Chapter 2 pays particular attention to the underlying factors known to be correlated with the duration of civil war and the durability of peace that might also be significant predictors of the level of humanitarian assistance over time. In the second stage, I control for these factors when analyzing the impact of humanitarian assistance on the duration of war (Chapter 3) and the duration of peace (Chapter 4) in order to limit bias in the statistical estimates.

In Chapter 3, I draw on bargaining models of war and deduce that humanitarian assistance can inadvertently prolong conflict by decreasing the informational value of fighting. In these models, war is treated as a costly learning process wherein sides signal their strength by enduring and imposing the costs of fighting. Paradoxically, international humanitarian assistance is explicitly designed to mitigate the costs of war. This implies that conflicts treated with greater levels of humanitarian assistance may actually last longer than those that receive less assistance. Critically, however, the model suggests that this effect should only hold provided opponents are at least somewhat uncertain about the advantages gained from international assistance. If each side could directly observe the degree to which aid mitigated their opponent's costs of fighting, they would factor this into the terms of their settlement offers over time and converge on an

agreement just as quickly as if no relief were provided. I therefore predict that aid should prolong conflicts only when it is difficult to observe, and this is more likely when the conflict is occurring in peripheral areas of the country. I demonstrate these effects empirically using newly compiled cross-national panel data on the amount of humanitarian aid allocated in every civil war year since 1945. I find that civil wars treated with higher level of humanitarian assistance not only last longer, they are also significantly less prone to settlement at any given moment during the war. Consistent with an informational mechanism, I find that the level of government uncertainty over the provision of assistance mediates the tendency for aid to prolong conflict.

In Chapter 4, I argue that humanitarian assistance can undermine peace when used as a post-conflict reconstruction strategy in the aftermath of civil war. Although the principles of humanitarian assistance dictate that aid be disbursed in accordance with need alone, while remaining neutral to the political stakes, these principles have unique implications in the post-conflict context, where need is often correlated with opponents' performance in the previous contest. In these cases humanitarian assistance is likely to be biased towards the conflict-loser. Using a crisis bargaining framework, this paper describes a simple logic for how humanitarian aid can inadvertently undermine peace by creating a revisionist party with an incentive to renegotiate the post-war settlement. As the theory predicts, I find that post-conflict states treated with higher levels of humanitarian assistance exhibit shorter spells of peace and are significantly more likely to experience renewed conflict at any given moment. Consistent with a bias-mechanism, I find that this effect is more acute after conflicts that ended with a decisive victory.

Specifically, for every 1-unit increase in the log-value of humanitarian aid disbursements after a decisive military victory, the instantaneous risk of peace ending with a second civil war more than doubles.

Finally, in Chapter 5, I explore why warring parties direct violence against humanitarian aid workers. In the period from 1997 to 2010, there were a total of 1026 acts of major violence reported against aid workers involving 2129 victims and resulting in 880 fatalities. To date, there has been no systematic attempt to analyze the determinants of aid worker attacks. In this chapter, I argue that the occurrence and variation in attacks against aid workers across civil wars is partially the result of within-group collective action problems. Although warring factions have a long-term collective incentive to exercise restraint in their relationship with humanitarian aid, they are also composed of individual members with private incentives to hijack and divert aid for personal gain. Using data on the organizational characteristics of rebel groups, I demonstrate that the likelihood of abuse depends on two important factors related to groups' willingness and ability to solve this collective dilemma: first, whether sufficient collective incentives exist for armed groups to exercise restraint in anticipation of future punishments (or rewards), and second, whether factions exhibit strong centralized control to identify and sanction opportunistic defections among individual members. With respect to groups' willingness to solve the collective action problem, I find that the likelihood of an aid worker attack is 118-percent lower in civil conflicts where a rebel group maintains a political wing – and is therefore more sensitive to the possibility of alienating the civilian population – compared to conflicts in which a rebel group does not

maintain a political wing. And with respect to groups' ability to solve the collective action problem, I find that the likelihood of an aid worker attack decreases roughly 66-percent when moving from the lowest level of rebel group centralization to the highest level of rebel group centralization in the data.

Chapter Two
Forgotten Conflicts: Need versus Political Priority in the Allocation of Humanitarian Assistance to Civil Wars

The principals of humanitarian action dictate that aid donors and humanitarian agencies respond to crises in proportion to the level of need in all situations. Yet, despite a principled commitment to assist people in need equally, the allocation of humanitarian assistance across civil war and post-civil war states shows remarkable variation that is not easily explained by differences in the level of need. For example, recent conflicts in Kosovo, Bosnia, Iraq and Afghanistan have received the lions-share of international humanitarian assistance over the last two decades, while equally destructive conflicts in Somalia, Sierra Leone and East Timor have gone relatively neglected by donors and aid organizations alike.

What explains these "forgotten conflicts"?[1] Or, rather, why is it that one or two emergencies tend to receive adequate levels of funding, while others are left to struggle on with little or no humanitarian assistance? Policy makers and aid-practitioners often implicate foreign policy interests, or the lack thereof, as the main reason for this variation. In this view, many humanitarian emergencies are either ignored or gradually neglected over time because they provide no compelling reasons for action beyond

[1] The terms "forgotten emergencies" and "forgotten conflicts" generally refer to conflict areas – typically civil wars – that the international community has essentially ignored or gradually neglected over time (Oxfam, 2000; Smillie & Minear, 2003; Smillie & Minear, 2004).

humanitarian need (Oxfam, 2000; Smillie & Minear, 2003; Smillie & Minear, 2004). To the degree that such claims are true, they do not bode well for the overall humanitarian enterprise. If the humanitarian imperative and its associated principals of neutrality and impartiality are what they purport to be, then aid donor and humanitarian agencies must respond in proportion to need in every situation where people are suffering from a lack of life-sustaining resources. If, on the other hand, the provision of humanitarian aid is equally susceptible to the political priorities and strategic interests of donor governments, then it should claim no special status in relation to other foreign policy tools more openly aimed at advancing the foreign policy interests of donors.

To date, researchers have typically focused on explaining variation in aggregate levels of Official Development Assistance (ODA) by measuring the relative impact of "humanitarian" versus "strategic" factors on the allocation of total foreign assistance overall. However, these studies may obscure important differences that might exist across different types of international assistance. Whereas evidence that strategic interests effect the provision of development assistance may not be particularly surprising or controversial, a similar finding with respect to the allocation of humanitarian assistance would directly contradict the core principals of the humanitarian enterprise. To date, however, there exists no systematic empirical evidence that foreign policy interests dominate humanitarian concerns in the allocation of humanitarian aid specifically.

In this paper, I replicate an analysis by Alesina and Dollar (2000) that studied the overall pattern of foreign aid giving to developing countries, however I estimate the

relative significance of recipient-need versus strategic interests specifically in the provision of humanitarian assistance to civil conflict and post-conflict states using cross-national panel data on humanitarian aid provisions from 1969-2009. To foreshadow the results, I find that humanitarian assistance to ongoing civil wars is substantially more humanitarian than strategic in its allocation. However, in the post-conflict context, I find little evidence that humanitarian assistance is a special case of foreign aid giving. Despite a principled commitment to assist people in need equally – wherever they are – more strategic, supply-side factors that effect donors' willingness and ability to provide humanitarian assistance are at least as important – and arguably more important – in explaining the allocation of international humanitarian assistance as more-humanitarian, demand-side factors that measure need in recipient states after civil wars. High levels of humanitarian aid appear to go to post-conflicts states where donors perceive important strategic and political interests even after controlling for the level of need. This suggests that needier conflicts are, in fact, gradually "forgotten" over time in favor of strategically important recipients. I also find some evidence that these results vary across different sources of humanitarian assistance: aid from DAC donors appears to be more strategic than un-earmarked humanitarian aid disbursed through IOs and NGOs.

It is important to note at the start that this paper does not propose a new theory to explain the allocation of humanitarian assistance across conflict-affected states, nor does it attempt to test competing theories of humanitarian giving. Rather, as a first step to understanding the global humanitarian response to civil conflict, I hope to provide some empirical findings on the allocation of humanitarian aid that will allow us to evaluate the

relative importance of donors' political and strategic interests versus recipients-need in the provision emergency relief. However, while not explicitly theoretical, this exercise should prove useful for future observational studies attempting to identify the impact of humanitarian assistance across conflict settings, since many of the variables tested here are plausibly correlated with other important dimensions of civil war (e.g. war initiation, duration, termination, recurrence, prevalence etc),

The remainder of this paper proceeds in six principal sections. In Section 2, I outline the puzzle this paper aims to explain by describing variation in the allocation of international humanitarian assistance across recipient states. I show that despite a principled commitment to assist people in need equally – wherever they are – the humanitarian response to complex emergencies appears to be remarkably varied and inequitable. In Section 3, I survey existing attempts to explain variation in the allocation of overall foreign aid while highlighting important gaps that this study aims to address. In Section 4, I describe the research design and data used to assess the relative importance of humanitarian need and political priorities in the allocation of international humanitarian aid. Section 5 discusses the main findings for two different sub-samples of conflict affected states: an in conflict sample of ongoing civil wars and a post-conflict sample. The final section concludes.

A Puzzle: Humanitarian Assistance in Principle and the Politics of 'Forgotten Conflicts' in Practice

The idea behind humanitarian assistance is straightforward in its simplicity: individuals struggling in the context of natural and man-made emergencies have the right

to life sustaining resources and protection of their basic human rights (Smillie & Minear, 2004). And while the notion of helping people in need is certainly not new, the key principles that drive the modern humanitarian enterprise only began to take shape in the late 19th century, when Swiss businessman Henry Dunant called for the formation of national voluntary organizations to professionalize the provision of humanitarian relief. Dunant's efforts eventually culminated in the formation of the International Red Cross in 1863 based on three primary governing principles: impartiality, neutrality, and independence. These principles – now codified in the Geneva Conventions as international law – have become the industry standard for the humanitarian aid community, as they constitute the most broadly accepted principles governing the provision of relief worldwide.

At their core, the distributional principles above are designed to ensure that the "humanitarian imperative" – the fundamental notion that humanitarian assistance be provided based on need alone (not other factors such as political or strategic interest, cultural affinity, or availability of resources) wherever it is most needed – remains the most significant determinant of humanitarian action worldwide. The principle of 'impartiality' requires that assistance be provided without regard to nationality, race, religion, or political point of view. This is meant to ensure that need is assessed equally across all parties in a crisis. The principle of 'independence' requires that humanitarian agencies formulate and implement a response independently of government interests. This is meant to limit donors from dictating the allocation of humanitarian assistance in order to further their own foreign policy goals. Finally, the principle of 'neutrality'

requires that humanitarian agencies not take sides in hostilities or engage in any ongoing political, racial, religious, or ideological controversies within a crisis area. This is designed to avoid agencies furthering the interests of one party over another in an armed conflict.

It turns out, however, that the humanitarian idea is much more complex in practice. As Gourevitch notes, "The scenes of suffering that we tend to call humanitarian crises are almost always symptoms of political circumstances, and there's no apolitical way of responding to them – no way to act without having a political effect." Impartiality and neutrality, it seems, are impossible when humanitarianism is bound to relieve warring parties of the burdens attached to waging war (the demands of governing while fighting, the costs of sustaining causalities, and the need to provide food, medical supplies, and logistical support to the frontlines), often times asymmetrically. And independence may be a luxury few organizations can afford in today's increasingly competitive humanitarian aid industry, where donors' ability to choose among several implementing agencies has given them greater bargaining power and more control over the allocation of assistance than ever before (Cooley & Ron, 2002).

As a result, the 'politicization' of aid has become an important topic of debate in the last decade (Vaux, 2006).[2] Policymakers and practitioners often criticize the modern humanitarian enterprise for disproportionately focusing resources on high-profile areas rather than those where need is greatest. This criticism is perhaps loudest with respect to the humanitarian response in complex emergencies like civil wars. For instance, Vaux

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[2] See Vaux 2006:3

claims that, "after 11 September 2001, Western security has come to dominate all other agendas, moving aid and humanitarianism even further towards the core of politics" (Vaux, 2006)[3]. And indeed, as **Figure 1** suggests, much of the variation in the allocation of humanitarian assistance across the 12 largest recipients of humanitarian assistance over the last decade appears to coincide with the strategic interests of Western donors. For instance, humanitarian aid disbursements to Afghanistan and Iraq disproportionally increased with the War in Afghanistan in 2001 and Operation Iraqi Freedom in 2003 respectively. Meanwhile, refugees and internally displaced persons in even deadlier conflicts in Sudan and the Democratic Republic of Congo – where the World Health Organization estimates as many as four million people died between 2000 and 2005 – went relatively neglected over the first part of the decade.[4]

To be sure, there are probably thousands more people suffering in 'forgotten conflicts' for whom donors' declarations of support and commitment to humanitarian principals means relatively little. But, increasingly, this has led critics to the more general presumption that many conflicts are neglected because they provide no compelling reason for action beyond need. In other words, the provision of humanitarian relief to conflict and post-conflict states appears to be governed just as much, if not more,

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[3] See Vaux 2006:4

[4] The absolute increase in the amount of humanitarian assistance to crisis areas (rather than the relative amount allocated) may be less interesting, as the amount of international humanitarian aid reported through the OECD Development Assistance Committee (DAC) from member and non-members states increased nearly 1400-percent in real terms, from $796 million USD in 1989 to well over $11 billion USD in 2008. Moreover, This increase was especially dramatic during the ten year period from 1999-2008 global spending on humanitarian assistance amounted to roughly $98 billion dollars ($97, 964, 440,000 USD in constant 2008 prices)

by the political priorities and strategic interests of international donors as it is by genuine humanitarian need.

And indeed, at first glance, the global humanitarian response to conflict affected states often appears to bear little relationship to some of the most common indicators of global needs, like the number of conflict related deaths, income per capita (GDP per-capita), infant mortality rates, or the number of refugees and internally displaced people as the result of conflict. **Figure 2** plots the bivariate relationship between each indicator of humanitarian need (increasing from left to right on the X-axes) and the total amount of humanitarian aid provided (on each Y-axis) for every post-conflict year following civil wars from 1945-2004. Notice that while the total amount of humanitarian assistance disbursed across post-conflict years appears to be weakly correlated with (i) the intensity of the previous conflict (measured by the number of conflict related deaths) and (ii) income per capita (GDP per capita), there appears to be virtually no relationship between the amount of humanitarian aid and (iii) the infant mortality rate or (iv) the total number of refugees and internally displaced civilians in need of assistance after a civil conflict. And, in all cases, there appears to be significant residual variation in the amount of humanitarian aid allocated across post-conflict states that remains unexplained with respect to each indicator.

However, while bivariate relationships are useful in visualizing what policy makers and aid-practitioners tend to observe in reality, they are certainly not controlled tests from which we can assess the independent effect of humanitarian need on the provision of global humanitarian assistance. In reality, the provision of humanitarian

assistance is likely to be dictated by a range of factors that effect both a conflict recipient's need for humanitarian assistance (demand-side factors) and donors willingness and/or ability to provide humanitarian relief to a conflict affected state (supply-side factors). And within this complex calculus, it may be the case that indicators of recipient need have a systematic effect on that allocation of humanitarian assistance globally after controlling for other factors that vary across conflict affected states. Perhaps more importantly, the same may be true for indicators of strategic interests: controlling for the level of need across conflict-affected states, the political priorities of donors may systematically effect the choices of humanitarian aid donors. In the empirical section of this paper, I attempt to identify the role of humanitarian and strategic motives using a multivariate regression analysis to isolate the independent effect of indicators that may that reflect these different motives.

Previous Literature

Despite the vast amount of literature on foreign aid giving, there is surprisingly little empirical research focusing specifically on the allocation humanitarian assistance across international crises. For the most part, existing research has generally focused on the allocation of net foreign aid by aggregating development assistance, humanitarian assistance, post-conflict aid, and others into a single measure of official development assistance (ODA). Meanwhile, the few studies exploring the determinants of humanitarian assistance specifically are generally qualitative case studies that provide little in the way of systematic empirical evidence.

With respect to the first category – research investigating the determinants of foreign aid generally – a large part of the literature finds that donor interests appear to better explain the nature and allocation of aid giving than recipient need (e.g., Alesina 2000; Dudley 1976; Maizels 1984; McKinlay 1977; McKinlay 1978; Burnside and Dollar 2000). For example, in a recent study Alesina and Dollar (2000) explore patterns in the allocation of foreign aid from various donors to receiving countries and conclude that, "the pattern of aid giving is dictated by political and strategic considerations. An inefficient, economically closed, mismanaged non-democratic former colony politically friendly to its former colonizer receives more foreign aid than another country with similar levels of poverty, a superior policy stance but without a past as a colony."

Prior to this, Maizels and Nissanke (1984) found a similar pattern with respect to bilateral aid. Using OECD estimates of foreign aid transfers from 80 developing countries in 1969-1970 and 1978-1980, they find that donors' political/security investment and trade interests provides a better explanation of bilateral aid transfers than a competing recipient-need model that assumes aid is given to compensate for shortfalls in domestic resources.

McKinlay and Little (1977; 1979) test the same two views – one explaining the allocation of aid in terms of the humanitarian needs of recipients and the other explaining it in terms of the foreign policy interests of donors – in a cross national, longitudinal study of US aid allocation specifically over the years 1960-1970. They find substantial support that, while the US does not appear to withhold aid from politically and strategically unimportant allies, it increases and decreases the *absolute* level of aid to

reward countries with which it has strategic ties and penalizes countries with which it does not. They conclude that US aid relationships are generally not driven by recipient need but rather donor interests. Particularly significant within their model were measures of security ties rather than economic interests.

Only one study supports a more idealist view of international aid provisions. Lumsdaine (1993) assess the relationship between net aid provisions and several potential factors such as colonial history, the democratic status of the recipients, income levels, etc. He concludes that "aid money went not to countries of economic and political importance to donors but to recipients with great needs (p.39)", a finding he argues demonstrates the "power of moral concerns in international politics " (p.67).

On balance, however, more idealistic assessments of foreign aid allocation tend to be dwarfed by a significant literature demonstrating that strategic foreign policy concerns explain the pattern of foreign aid giving much better, culminating more recently with findings by Burnside and Dollar (2000) and Alesina and Dollar (2000) that donors' strategic interests, proxied by arms imports and UN voting patterns respectively, are better predictors of bilateral aid flows than factors that approximate need such as poverty and infant mortality.

While instructive, it is important to note three important shortcomings of this literature with respect to the question posed in this paper. First, nearly all of the cross-national studies reviewed above focus on the *net* allocation official ODA as reported by

the World Bank or OECD (Berthélemy, 2006; Berthélemy & Tichit, 2004) [5]. By exploring the determinants of net foreign aid, however, these studies may obscure important differences between various types of international assistance. For example, the factors that drive the allocation of humanitarian assistance may be quite different than the factors that determine the allocation of international development assistance, post-conflict aid, or environmental aid. Indeed, it is reasonable to suppose that humanitarian aid only exists as a distinct category of aid precisely because its provision is expected to follow a separate, less-strategic logic than other categories of foreign assistance.

Second, the studies above tend to emphasize either aggregate patterns in global ODA by analyzing all donors simultaneously or focusing exclusively on bilateral aid giving from an individual donor to recipient states (see also (Gounder, 1994; Tsoutsoplides, 1991). This is problematic because it becomes difficult to assess whether the balance between humanitarian and strategic motivations varies between different donors and over time. Indeed, most empirical analysis of aid patterns conducted over the last three decades have concluded that donors not only distribute bilateral aid differently from each other (Alesina and Dollar 2000), but that bilateral aid itself is more strategic than multilateral aid (Maizels & Nissanke, 1984; Martens, 2002; Milner, 2006; Nancy & Yontcheva, 2006; Schneider & Frey, 1985).

A third and final shortcoming of the existing literature with respect to the question posed in this paper is that by exploring the allocation of ODA from developed countries

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[5] The OECD defines aid (official development assistance) as non-military grants and net disbursements of concessional loans that have at least a 25% grant element.

to all potential recipient states, the conflict and post-conflict situations are never explicitly considered. Yet we know that the economic circumstances of conflict societies are unique in several respects (Collier, 1999; Collier, 2003). Countries emerging from civil war are made especially fragile by weak institutional capacity, weak governance, political instability, frequently on-going violence, and/or the legacy of past conflict. These factors may combine to make the allocation and impact of aid distinct in the conflict and post-conflict environment. As Collier et al. (Collier & Hoeffler, 2004) note:

> 'Typically, opportunities for recovery enable a phase when growth is supra-normal. The need to restore infrastructure, juxtaposed against the collapse of revenue, tend to make aid unusually productive. However, offsetting this, during civil war the normal incentive to maintain a reputation for honesty is often disrupted, switching the society in to a persistent high-corruption equilibrium. This, together with the weakening of civil administration, can make aid less effective. Hence, apriori, aid might be more or less productive in post-conflict societies.' [6]

Expectations over the unique impact of aid in conflict situations may, in turn, affect the allocation choices of donors. Indeed, the World Bank's International Development Association (IDA) recently revised its allocation formula in 2000 to allow post-conflict societies additional temporary resources, while most other donors now have special units dedicated to post-conflict development as well. Still, we know very little about how aid is allocated across conflict and post-conflict societies, and even less about the factors that determine the allocation of humanitarian aid specifically within these contexts.

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[6] Indeed, in their analysis of aid allocation to post-conflict societies, they find that aid is considerably more effective in augmenting growth in post-conflict situations than in other situations, but that the effect is non-linear over time. The greatest gains from aid occur in the first four years after conflict, and then gradually taper back to normal levels by the end of the decade.

There are, of course, a few notable exceptions to the critiques above that explore the allocation of humanitarian assistance across conflict areas from various donors. However, these studies have their own limitations with respect to the question posed in this paper. First, several studies are qualitative assessments of humanitarian aid allocation across only a few select cases of conflict (Olsen, Carstensen & Høyen, 2003; Smillie & Minear, 2003; Smillie & Minear, 2004). This is problematic because it is difficult to assess whether particular factors that appear to influence the amount of aid in one conflict actually constitute a systematic explanation for the provision of humanitarian assistance globally. And second, many of the existing studies are institutional reports or briefings by organizations involved in the humanitarian enterprise itself (like those cited in the preceding section). While interesting, these studies generally do little in the way of hypothesis testing, focusing instead on presenting descriptive statistics to support the claim that donors appear to be neglecting their responsibilities by underfunding humanitarian crises in general, or that donors are allocating funds inefficiently across crises by targeting strategically important recipients (Christian Aid, 2004/2004; Cosgrave, 2004; Oxfam, 2000). As a result, they typically select on the dependant variable, presenting evidence from a few cases that support their critiques.

In contrast to the studies reviewed above, the primary purpose of this paper is to explore the puzzle of "forgotten conflicts" by estimating the relative significance of recipient-need versus strategic interests in the provision of humanitarian aid to conflict and post-conflict states specifically. In the next section, I outline a research design to assess popular claims about the increasing 'politicization' of international humanitarian

assistance. I discuss the measurement of humanitarian aid (the dependant variable), the econometric framework, and the main explanatory variables I use to test whether donors' strategic interests or recipient needs dominate merits motives in bilateral assistance. In section 5, I provide the estimation results for the behavior of all donors combined (or disbursements from the average donor) and I test for differences in parameters across important groups of aid donors. The statistical results provide a useful first-glance at the factors that systematically effect the provision of humanitarian relief across conflict areas.

Research Design and Method

What distinguishes conflicts areas that receive high levels of humanitarian assistance from conflicts that receive little to no assistance? In this paper I use the largest and most exhaustive data available on humanitarian aid disbursements from 1969-2009 assembled by the OECD to assess the relative importance of strategic and humanitarian factors in the allocation of humanitarian aid across conflict and post-conflict areas. The OECD data covers the humanitarian aid disbursements from the 24 donors of the

Development Assistance Committee[7] (typically the largest donors of in the OECD), 20 non-DAC aid donors[8], and several multilateral organizations[9].

Here I chose to explain humanitarian aid disbursements rather than aid commitments, despite an emphasis on the former in existing studies of development assistance. In the OECD data, c*ommitments* represent "a firm obligation, expressed in writing and backed by the necessary funds, undertaken by an official donor to provide specified assistance to a recipient country or a multilateral organization", while *disbursements* represent the actual "release of funds to or the purchase of goods or services for a recipient; by extension, the amount thus spent. Disbursements record the actual international transfer of financial resources, or of goods or services valued at the cost to the donor".

Earlier work has argued that aid commitments better reflect donors' decisions because they have more control over this than over disbursements, which also depend on recipients' willingness and administrative capacity to successfully get development aid

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[7] Currently, the 24 members of the DAC are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Italy, Ireland, Japan, Korea, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom, the United States and the European Commission.

[8] Currently, the 20 non-DAC members that report their aid flows to the DAC: are Chinese Taipei, Cyprus*, Czech Republic, Estonia, Hungary, Iceland, Israel, Korea, Kuwait, Latvia, Liechtenstein, Lithuania, Poland, Romania, Saudi Arabia, Slovak Republic, Slovenia, Thailand, Turkey, United Arab Emirates

[9] The OECD compiles statistics on bilaterally-funded activities implemented by several multilateral organizations. Projects executed by multilateral institutions or non-governmental organizations on behalf of DAC Members are classified as bilateral aid disbursements (since it is the donor country that effectively controls the use of the funds), whereas DAC Members' multilateral aid contributions to the regular budgets of the multilateral institutions are considered pure "multilateral outflows" as the multilateral institutions or non-governmental organization controls the disbursement. See www.oecd.org/dac/stats/crsguide for more details on the OECD coding and http://www.oecd.org/dataoecd/44/54/45896100.pdf for a list of multilateral institutions or non-governmental organizations reporting

money (Berthélemy, 2006; Tarp, Bach, Hansen & Baunsgaard, 1999; White & McGillivray, 1995). For this reason, using disbursements could introduce noise into the data because the measure does not purely reflect donors' decisions. However, for this study disbursements are the preferred choice of aid variable for two main reasons. First, I am specifically interested in explaining why certain conflict and post-conflict states appear to *receive* more assistance than others. Substantively, this means that disbursements are the relevant variable even if both the donor and the recipient influence it. Second, in addition to assessing donor motives, I am also interested in evaluating the impact of humanitarian assistance in conflict areas in related work. In most cases, the consequences of aid will depend on the amount disbursed rather than the amount committed, especially since it can take several years to disburse a single commitment. Using donor commitments would therefore overestimate the amount of assistance transferred in a given year by including undisbursed funds that were committed but not yet spent.

Finally, with respect to measuring the dependant variable, aid disbursed in each conflict and post-conflict year is converted into constant 2008 US dollars using the OECD GDP deflator index in order to neutralize the effect of (i) inflation in the donor's currency between the year in question and the reference year, and (ii) changes in the exchange rate between that currency and the United States dollar over the same period. The adjustments should provide a truer idea of the volume of flows over time.

With respect to the model specifications in the analyses below, it is necessary to take into account the censored nature of the dependant variable, as the range of

humanitarian assistance to recipients in a conflict-year or post-conflict year is always bounded on the lower end by zero. As a result, running a simple OLS-regression may generate sample selection bias because the process of aid allocation by donors and humanitarian agencies entails two sequential questions: first whether to allocate any positive amount of humanitarian aid to a particular conflict recipient in a given year and, if so, how much aid to give a conflict recipient within the cases chosen to receive some assistance (Berthélemy, 2006). Several estimation methods have been used to evaluate this process empirically in studies of development assistance, including using a two-part model (McGillivray & Oczkowski, 1991; McGillivray & Oczkowski, 1992), a Heckman procedure (Tarp et al., 1999), or a Tobit regression (Gang & Lehman, 1990).

In evaluating the allocation total official humanitarian aid to recipient conflicts, I use a standard tobit model to estimate the aggregate behavior of donors' with respect to the total amount of humanitarian assistance disbursed in each conflict setting. In the subsequent analyses, I also divide my sample of conflict-affected states into two subsamples: an in-war sample and a post-conflict sample. In the in-war sample, the unit of analysis is the civil war-year, and in the post-conflict sample, the unit of analysis is the post-conflict-year. For each recipient-state, I observe total humanitarian aid disbursements in each civil war-year and post-conflict-year respectively from all donors listed in the OECD database. Additionally, I perform separate analyses on aid disbursements from three different classes of donors, observing DAC, non-DAC and multilateral humanitarian aid disbursements in each conflict and post-conflict year.

Measuring Donor Interests During and After Civil War

In the conflict and post-conflict environments, our objective is to indentify measures of both humanitarian need and political-strategic interests in order to estimate (i) each model separately and (ii) a full model of donor behavior so that we can assess the relative importance of the two sets of interests for the allocation of humanitarian aid during and after civil wars. With respect to humanitarian indicators, we focus on explanatory variables traditionally used to approximate recipient need in the previous literature reviewed above. For the political-strategic interests of donors, we also follow previous literature by emphasizing the interests of the five permanent members of the UN Security Council (P-5) because the P-5 tend to be the largest donors of humanitarian assistance globally (e.g., Alesina 2000; Burnside and Dollar 2000; Berthélemy, 2006). Moreover, they hold disproportionate influence – by virtue of their veto power – over multilateral peacebuilding efforts implemented through UN humanitarian agencies like the UNHCR , UNDP, UNICEF, and WFP. Specifically, we relate humanitarian aid disbursements to the following 10 explanatory variables (five "humanitarian" factors and five "strategic" factors)[10]:

<u>Humanitarian Need Indicators:</u>

- *GDP per capita* – Income per capita is perhaps the most straightforward and most often utilized indicator of beneficiary needs in the existing literature. If humanitarian assistance is to be allocated on the basis of recipient needs, one

[10] The humanitarian and political-strategic interests of donors may be characterized by several different factors – too many to model here. In general, we focus on the most common variables used to proxy for the political priorities of donors in the previous literature

would expect the poorest conflict affected countries to receive more aid and the richest conflict affected countries to receive less. The GDP per capita data is from Doyle and Sambanis (2000) and filled in for new cases from the World Bank Development Indicators (WDI).

- *Infant mortality rates* - Infant mortality rate measured at the start of the war for the in-conflict sample and at the end of the war for the post-conflict sample. Both measures are originally from Doyle and Sambanis (2000) and filled in for new cases through 2004 using WDI.
- *Life expectancy* – Life expectancy measured at the end of the war for both samples. The measure is from Doyle and Sambanis (2000) and updated through 2004 in Fortna (2004).
- *Logged Number of Conflict Related Deaths* – The natural log of the total estimated battle and civilian deaths from the civil war for both samples. The measure is from Doyle and Sambanis (2000).
- *Logged Number of Refugees and Internally Displaced Persons* – The logged number of refugees and internationally displaced persons in each civil year for the in-conflict sample and the total number displaced at the time the war ended for the post-conflict sample. The in-conflict data is from Gelditsch and Salehyan (2006) and the post-conflict data is from Doyle and Sambanis (2000).

Political-Strategic Interests Indicators:

- *Oil Exports* – A dummy variable indicating whether the recipient country has significant oil exports. Coded 1 if more than one third of a state's export revenues comes from fuels. From Fearon and Laitin (2003).
- *Former P-5 Colony* – A dummy variable coded 1 if the recipient country is a former colony of one of the five permanent members of the UN Security Council (US, Britain, France, China or Russia). From Gilligan and Stedman (2003) (Gilligan & stedman, 2003)
- *P-5 Contiguity* – A dummy variable coded 1 if the recipient country is located within 400 miles of a permanent members of the UN Security Council. From Fortna (2004)
- *P-5 "Affinity"* – Max "affinity" score of the recipient state with the five permanent members of the UN Security Council in the year the war starts. Affinity is based on the "sun3cati" value from Gartzke and Jo 2002 "Affinity of Nations" (v. 3.0) data, which provides a metric that reflects the similarity of state preferences based on voting positions of pairs of countries (dyads) in the United Nations General Assembly.
- *Democracy* – Polity average over the five years before the war, originally from Doyle and Sambanis (2000) and updated through 2004 by Fortna (2004

Findings

The Determinants of Humanitarian Relief during Civil Conflict: The In-War Sample

Table 1 reports the regression results for the in-conflict analyses described above. We display the results for six different models. In each case, the dependant variable is the logged value of humanitarian aid disbursed in each civil war-year observation.

Model 1 estimates the relationship between the humanitarian factors and the total amount of humanitarian assistance disbursed in every year of an ongoing civil war. Notice that while most of the coefficient estimates are consistent with the expectation that the level of humanitarian assistance should increase with recipient need, some run counter to these expectations. For instance, the negative coefficient on *GDP per capita* and the positive coefficients on the logged number of *battle deaths* and *Refugees and Internally Displaced Persons* suggest that amount of humanitarian aid increases in line with the needs of the conflict affected population: greater levels of humanitarian aid disbursements are associated with lower standards of living and deadlier civil conflicts that produce more population displacements. On the other hand, the coefficient estimate on *Life Expectancy* suggests the opposite: greater levels of humanitarian relief tend to be provided in civil wars where victims live longer on average. And contrary to what one might expect, there is no significant relationship between the amount of humanitarian assistance and *infant mortality rates*

Model 2 estimates the relationship between the political-strategic factors described above and the total amount of humanitarian assistance disbursed in every civil

war-year. These results appear to be somewhat less mixed. For example, there is no evidence that humanitarian assistance targets civil war-states with significant *oil exports*, nor does there appear to be any association between the amount of humanitarian aid and the former colonial status of the country (*Former colony of P-5*). Moreover, the negative and significant coefficients on *Contiguity with the P-5* and the prior five-year *Democracy* average suggests, first, that humanitarian assistance is actually *less* likely to be provided to neighboring states where the primary donors may have greater geo-political interests, and second, that humanitarian aid is negatively associated with democratic institutions. These results appear to support the idea that humanitarian assistance is in fact independent from many of the strategic interests found to effect development assistance. The only apparent exception to is the strong positive relationship between humanitarian aid disbursements and the similarity of state preferences based on voting positions in the UN General Assembly (*UN Voting Affinity with P-5*), which may indicate that humanitarian assistance goes to civil wars where the political preferences of the recipient state are more in line with the P-5.

Model 3 displays the results of the full model in order to assess the independent effect of each motivational factor holding the other factors constant. In almost all cases, the magnitude, direction and significance of each coefficient estimate remains the same. The only exceptions are the two coefficient estimates on *UN Voting Affinity with P-5* and *Democracy,* which both go from positively associated with humanitarian aid disbursed to un-associated. These results are especially revealing because they suggest that holding

the level of humanitarian need constant across civil wars and over time, the same strategic factors that mattered in Model 2 no longer remain significant.

Overall, these results provide little support for the notion that the strategic interests of donors substantially dictate the allocation of humanitarian aid across ongoing civil wars. With few expectations, humanitarian assistance to ongoing civil wars appears to be positively associated with rising indicators of humanitarian need and generally uncorrelated with the strategic interests of the largest donors. To see this more quantitatively, I ran a Wald test to see if excluding the nested strategic factors from the full model (Model 3) significantly reduces the fit of the model overall. Comparing the full model including all ten parameters to a restricted model excluding the strategic factors does not significantly harm the fit of the model ($F(5, 1843) = 1.96$, $p = .0815$).

Models 4 and 5 estimate the full model in the Cold War and Post-Cold War period respectively. It has been suggested previously that the post-cold war period marked a substantial change in the political priorities of states - particularly the major powers. For example, as McKinlay and Little (1977) previously noted:

> It is frequently asserted that the aid program of the United states has only been maintained because of rivalry and competition between the superpowers: Soviet Russia has emerged as a rival source of assistance; the cold war has a global impact; and the United States has become progressively aware that many of its interests – such as security, trade, and investment – are closely tied to the economic well-being of the low income countries. These criticisms have encourages the general acceptance of a foreign policy view of aid allocation.

In general, our in-conflict results provide little support for the claim that the Cold War period was a time of more strategic humanitarian aid provision to ongoing civil wars. With few exceptions, the direction and significance of the coefficient estimates for

the humanitarian strategic factors remain largely the same between the two periods. The only exceptions among the humanitarian factors are the sign inversions on *GDP per capita* and *Infant mortality rate*. Greater humanitarian giving to civil wars was associated with lower income per capita countries during the Cold War (Model 4), but, surprisingly, aid was associated with higher income per capita civil wars after the Cold War (Model 5). On the other hand – and somewhat consistent with the claim the Cold War was a period of more strategic aid giving – the coefficient and significance for *infant mortality rate* changes from an insignificant predictor of humanitarian aid giving to civil wars during the Cold War, to positively correlated with rising infant mortality rates in civil wars after the Cold War. Likewise, while most of the strategic factors remain unchanged, one exception is the coefficient on *Former colony of P-5*, which changes from an insignificant predictor of humanitarian aid to civil wars during Cold War, to a negative predictor of aid giving in the post-Cold War era. Together, these results suggest that the strategic interests of the P-5 for the allocation of humanitarian assistance may have waned somewhat after the dissolution of the Soviet Union in 1989.

Table 2 displays the relationship between the same ten humanitarian and strategic indicators and the level of humanitarian aid disbursements in ongoing civil wars, this time disaggregating aid provisions by their source. Model 1 and Model 2 compare humanitarian aid provisions from DAC members (the 22 largest Western Donors) and Multilateral aid agencies and NGOs with respect to the more humanitarian indicators of recipient need. Models 3 and 4 compare the same two donor types with respect to the

more strategic factors. Finally, Models 5 and 6 compare the allocation of aid from DAC donors and Multilateral agencies using the full model of all ten indicators combined.

Interestingly, the results of Model 5 and 6 suggest that although there are generally very few differences between the factors that influence the bilateral provision of humanitarian assistance from DAC donors and the multilateral provision of humanitarian assistance from IOs and NGOs, there are some important differences. First, the level of humanitarian aid from multilateral donors is positively correlated with the number of conflict related deaths, while there appears to be no relationship between this variable and the humanitarian giving from DAC donors. This may suggest that aid from multilateral agencies is slightly more humanitarian. On the other hand, multilateral humanitarian aid giving appears to be positively correlated with whether a country is a former colony of the P-5, which is not a significant predictor of bilateral giving from DAC donors. Finally, DAC donors tend to provide more humanitarian assistance to formally democratic post-conflict recipients, whereas this variable has no relationship with multilateral aid provisions. In summary, the results suggest that while neither donor type appears to be significantly effected by strategic factors, multilateral aid disbursements from IOs and NGOs to ongoing civil wars tends to be more responsive to humanitarian factors indicating recipient need.

The Determinants of Humanitarian Relief *after* Civil Conflict: The Post-Conflict Sample

Are the neediest conflict-affected states "forgotten", or gradually neglected, over time? In this section we estimate the exact same models in the post-conflict environment

to investigate whether the significance of humanitarian indicators wane over time while the significance of strategic interests rise once civil wars end. **Table 3** reports these results. Like before, we display the regression results for six different models. In each case, the dependant variable is the logged value of humanitarian aid disbursements in each *post*-conflict year observation for all civil wars that ended before 2004.

Model 1 estimates the relationship between the humanitarian factors proxying for recipient need and the total amount of humanitarian assistance disbursed in every post-conflict year. Notice, again, that while some of the coefficient estimates are consistent with the expectation that the level of humanitarian assistance should increase with recipient need, others run counter to these expectations. Similar to the in-conflict findings, the positive coefficients on *infant mortality rates* and the logged number of *battle deaths* suggest that humanitarian aid provisions increase in line with the needs of the conflict affected population: more humanitarian aid tends to be disbursed in post-conflict states with a lower standard of living and following deadlier civil wars. However, the coefficient estimates on *GDP per capita*, *Life Expectancy*, and the logged number of *Refugees and Internally Displaced Persons* suggest the opposite. Surprisingly, higher levels of humanitarian aid provisions appear to be associated with more developed post-conflict environments (as indicated by the positive coefficient on GDP per capita) where the conflict-affected population has a higher average life expectancy and there are fewer Refugees and IDPs (as indicated by the negative coefficient on Refugee-IDP) in need of humanitarian relief. The change in coefficient estimates for *GDP per capita* and *Refugees and Internally Displaced Persons* are

particularly surprising because they suggest that some indicators of need that were significant determinants of humanitarian aid giving during a civil war become insignificant once the war has ended.

Model 2 estimates the relationship between the political-strategic factors described above and the total amount of humanitarian assistance disbursed in every post-conflict year. As was the case with the in-conflict findings, the results here are somewhat mixed. There appears to be no evidence that humanitarian assistance targets post-conflict states with significant *oil exports*, nor is there any association between the amount of humanitarian aid and the regime type (level of *Democracy*). And similar to in-conflict results, the negative and significant coefficient on *P-5 contiguity* suggests that humanitarian assistance is actually *less* likely to be provided to neighboring post-conflict states where the primary donors of aid have greater geo-political interests. However, there does appear to be some evidence that political-strategic interests matter with respect to the colonial status and affinity of post-conflict recipient states. The large, positive coefficient on *Former colony of P-5* indicates that post-conflict states that are a former colony of one of the five permanent members of the UN Security Council (US, Britain, France, China or Russia) are significantly more likely to receive higher humanitarian aid provisions. Finally, the strongest relationship in the data is between the amount of humanitarian aid provided and the similarity of state preferences based on voting positions in the UN General, suggesting that assistance goes to post-conflict states who political preferences are most in line with the P-5.

Model 3 displays the results of the full model in order to assess the independent effect of each motivational factor after civil war holding the other factors constant. In all cases except for GDP per capita, the direction and significance of each relationship remains the same. These results are revealing because they suggest that holding the level of need across post-conflict states constant, the same strategic factors that mattered in Model 2 remain strongly associated with greater levels of humanitarian aid disbursements. Specifically, at similar levels of need, the allocation of humanitarian aid appears to be influenced by the colonial status and affinity of post-conflict recipient states relative to the P-5.

We ran an Wald test comparing the full model (Model 3) with the strictly humanitarian model (Model 1) to test the hypothesis that the effects of the strategic variable are equal to 0 (i.e. a model with only humanitarian factors better fits the data), and found that the full model with the nested strategic factors provides a significantly better fit to the data ($F(5, 1843) = 14.56$, $p = .0000$).

Another way to look at the relative importance of different variables is to introduce them into the regression sequentially and compare the size of the R-squared for each set of factors using a simple OLS estimate with robust standard errors (Alesina 2000) (see Appendix Table A1). The five humanitarian factors alone explain roughly 4.1-percent of the variation in humanitarian aid flows across post-conflict states. On the other hand, the five strategic factors explain roughly 5.2-percent of the variation. This suggests that strategic factors, which effect donors' willingness and ability to provide humanitarian assistance, are at least as important, and arguably more important, in

explaining the allocation of international humanitarian assistance across post-conflict states as more-humanitarian factors that measure recipient-need. Moreover, adding the two models together returns an R-squared 8.8-percent, suggesting that the two sets of factors are mostly independent. In all cases, the direction and significance of the coefficients remain the same for all ten variables when estimating the coefficients using an OLS versus Tobit model.

Models 4 and 5 estimate the full model in the Cold War and Post-Cold War period respectively. Unlike the in-conflict sample, our results provide some support for the claim that the Cold War period was a time of more strategic humanitarian aid provision to post-conflict states. For example, whereas the positive relationship between aid levels and GDP-per capita during Cold War period (Model 4), suggests that higher levels of humanitarian aid went to more developed post-conflict states, the negative and significant coefficient for this variable in the post-Cold War period (Model 5) suggests that higher levels of humanitarian aid began to target less developed post-conflict states after the Cold War rivalry ended in 1989. The same sign inversion can be seen with respect to the infant mortality rate and number of refugees/IDPs in post-conflict recipients: whereas higher infant mortality rates and greater numbers of refugees/IDPs were negatively associated with the level of humanitarian assistance during the Cold War period, after the Cold war rivalry ended in 1989, higher levels of humanitarian aid began to be allocated to needier post-conflict environments with lower standards of living and more refugees and IDPs.

Also, with respect to the political-strategic factors – and consistent with the claim that the Cold War period was much more strategic – we find that a post-conflict recipient's status as a former P-5 colony goes from positively correlated with humanitarian aid disbursements before 1989 to uncorrelated with humanitarian assistance after 1989. Additionally, the political affinity between the P-5 and post-conflict recipient states goes from being positively correlated with humanitarian aid disbursements to negatively correlated. Interestingly, however, there is some evidence that a new strategic calculus for humanitarian giving may have emerged in the post-Cold War period, as a post-conflict state's status as an oil exporter and democracy – both insignificant at the 5 percent level during the Cold War period – are positively correlated with the level of humanitarian aid disbursements in the post-cold war period.

Table 4 displays the relationship between the same ten humanitarian and strategic indicators and the level of humanitarian aid disbursements disaggregated again by source. Model 1 and Model 2 compare humanitarian aid provisions from DAC members and multilateral aid agencies and NGOs with respect to humanitarian indicators of recipient need, Models 3 and 4 compare the same two donor types with respect to the more strategic factors, and Models 5 and 6 compare the allocation of aid from both donor-types using the full models.

As was the case in the in-conflict sample, the results of Model 5 and 6 suggest that there are some important differences between the factors that influence the bilateral provision of humanitarian assistance from DAC donors and the multilateral provision of humanitarian assistance from IOs and NGOs. First, multilateral donors and NGOs

appears to be much more responsive to the number of conflict related deaths than DAC donors. And second, DAC donors tend to target humanitarian assistance in more democratic post-conflict recipients, whereas this factor has no relationship with multilateral aid provisions. Together, these results may suggest that aid from multilateral agencies is slightly more humanitarian. An interesting exception is that multilateral humanitarian aid giving appears to be positively correlated with whether a country is a former colony of the P-5, which is not a significant predictor of bilateral giving from DAC donors.

Conclusion

This paper was motivated by two closely related research questions: Which crisis areas receive humanitarian assistance and why? And, second, how does recipient need effect the allocation of international humanitarian aid in relation to more strategic considerations. These questions are important from a policy perspective because the 'politicization' of humanitarian aid has become an important topic of debate in the last decade. Funding crises according to need lies at the heart of humanitarian intervention, yet policymakers and practitioners increasingly criticize the modern humanitarian enterprise for disproportionately focusing resources on high-profile areas rather than those where need is greatest. To the degree that such claims are true, they do not bode well for the overall humanitarian enterprise

In this paper we began by describing the allocation of humanitarian assistance across post-conflict states. We demonstrated empirically that the global humanitarian

response often appears to bear little relationship to some of the most common indicators of global needs, like the number of conflict related deaths, income per capita (GDP per-capita), infant mortality rates, or the number of refugees and internally displaced people.

We then assessed the relative effect of humanitarian versus strategic interests in the allocation of humanitarian assistance across conflict and post-conflict states. Consistent with principles of humanitarian action outlined in the Geneva Conventions, we found that humanitarian aid provided to ongoing civil wars is substantially more humanitarian than strategic in its allocation. However, in the post-conflict context, we find little evidence that humanitarian assistance is a special case of foreign aid giving. Despite a principled commitment to assist people in need equally – wherever they are – more strategic, supply-side factors that effect donors' willingness and ability to provide humanitarian assistance appear to be just as important – and arguably more important – in explaining the allocation of international humanitarian assistance as more-humanitarian, demand-side factors that measure need in recipient states.

Once civil wars terminate, high levels of humanitarian aid appear to go to post-conflicts recipients where donors perceive important strategic and political interests, even after controlling for the level of need. This finding supports the observation among aid practitioners that conflict-affected states are often gradually "forgotten" over time in favor of strategically more-important recipients despite persistently high levels of need. We also found some evidence that the determinants of humanitarian aid giving vary somewhat across different sources: aid from DAC donors to post-conflict states appears

to be more strategic than un-earmarked humanitarian aid disbursed through IOs and NGOs.

FIGURES AND TABLES

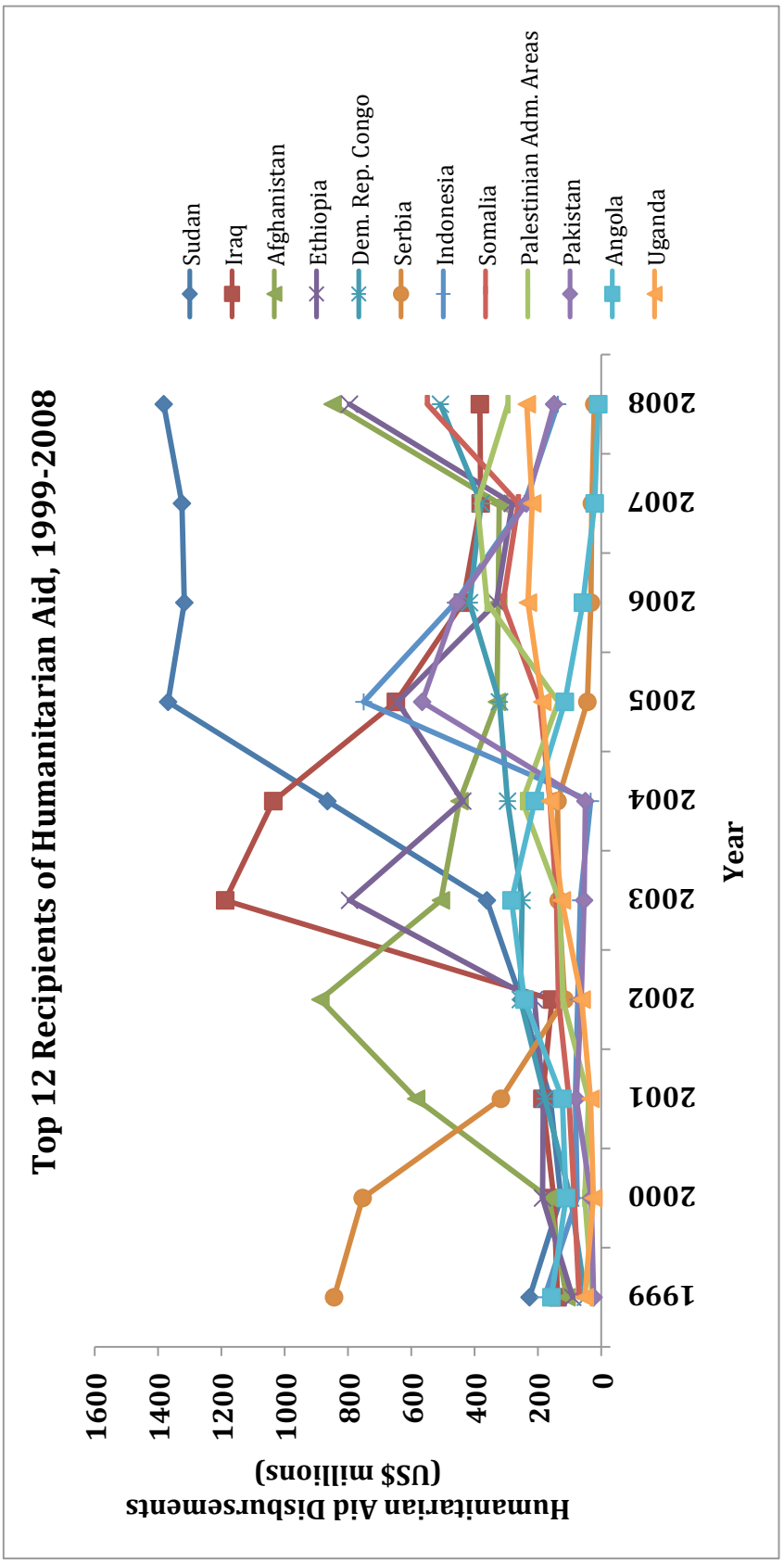

Figure 2.1: Annual Disbursements Humanitarian Aid Disbursements for Top 12 Recipients of Humanitarian Assistance, 1999-2008 (Source: OECD Dac2a)

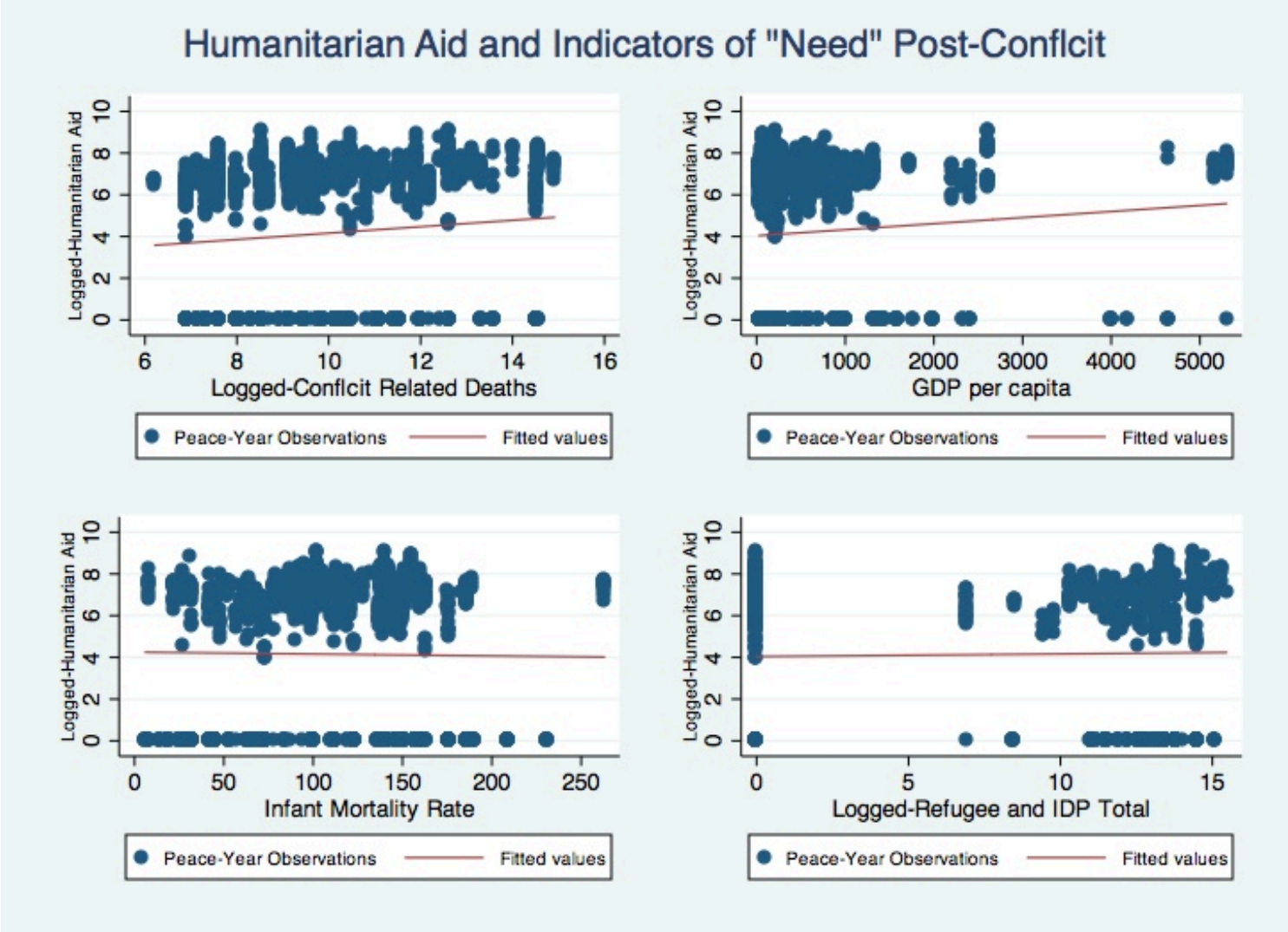

Figure 2.2: Bivariate Relationship between Humanitarian Aid and Common Indicators of "Need " Post-Conflict

Table 2.1: Humanitarian versus Political-Strategic Factors *during* Civil War (In-conflict sample). Dependant Variable: Log of Total Humanitarian Aid Disbursements.

VARIABLES	(1) Tobit *Full Sample*	(2) Tobit *Full Sample*	(3) Tobit *Full Sample*	(4) Tobit *Cold War*	(5) Tobit *Post-Cold War*
GDP per capita	-0.00139***		-0.00112***	-0.00133***	0.0189***
	(0.000162)		(0.000203)	(0.000384)	(0.00479)
Infant Mortality	0.00472		0.00254	-0.00819	0.0655***
	(0.00688)		(0.00738)	(0.0133)	(0.0201)
Life Expectancy	0.0902***		0.0927***	0.101*	0.283***
	(0.0283)		(0.0303)	(0.0545)	(0.0623)
Logged Dead	0.453***		0.480***	0.634***	0.113***
	(0.0840)		(0.0879)	(0.152)	(0.0243)
Log Refugee-IDP	0.579***		0.534***	0.691***	0.0923
	(0.0304)		(0.0323)	(0.0552)	(0.336)
Oil Exporter		-0.731	-0.0275	-0.265	0.414
		(0.547)	(0.492)	(0.900)	(0.266)
Former colony of P-5 member		0.459	0.526	0.625	-1.231***
		(0.443)	(0.379)	(0.650)	(0.292)
Contiguous with P-5 member		-1.206***	-0.839**	-1.643**	-1.579***
		(0.460)	(0.421)	(0.770)	(0.578)
UN Voting Affinity with P-5		2.822***	0.590	-0.723	-0.00104
		(0.868)	(0.809)	(1.426)	(0.0308)
Prior 5 year Democracy Avg.		-0.145***	-0.0456	-0.204***	-1.614
		(0.0401)	(0.0385)	(0.0660)	(1.617)
Constant	-9.290***	0.955	-9.446***	-11.23***	0.0189***
	(2.235)	(0.759)	(2.386)	(4.223)	(0.00479)
sigma	4.399***	5.900***	4.314***	5.098***	2.020***
	(0.152)	(0.203)	(0.150)	(0.276)	(0.0865)
Observations	977	1036	907	586	321

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.2: Humanitarian versus Political-Strategic Factors *during* Civil Wars *by Donor Type* (Post-conflict Sample). Dependant Variable: Log of Humanitarian Aid Disbursements.

VARIABLES	(1) Tobit DAC Donors Disbursements	(2) Tobit Multilateral Disbursements	(3) Tobit DAC Donors Disbursements	(4) Tobit Multilateral Disbursements	(5) Tobit DAC Donors Disbursements	(6) Tobit Multilateral Disbursements
GDP per capita	-0.00125**	-0.00144***			-0.00158**	-0.00112***
	(0.000519)	(0.000165)			(0.000664)	(0.000206)
Infant Mortality	0.0103	0.00966			0.00421	0.00764
	(0.0218)	(0.00686)			(0.0238)	(0.00741)
Life Expectancy	0.110	0.0975***			0.0642	0.104***
	(0.0906)	(0.0283)			(0.0987)	(0.0304)
Logged Dead	0.580**	0.403***			0.470	0.434***
	(0.276)	(0.0834)			(0.290)	(0.0879)
Log Refugee-IDP	0.720***	0.573***			0.724***	0.530***
	(0.104)	(0.0304)			(0.114)	(0.0324)
Oil Exporter			0.875	-0.544	1.591	0.0743
			(1.313)	(0.541)	(1.552)	(0.493)
Former colony of P-5			2.347**	0.681	1.740	0.748**
			(1.085)	(0.438)	(1.242)	(0.380)
Contiguous with P-5			1.014	-1.345***	2.211	-0.962**
			(1.088)	(0.456)	(1.361)	(0.424)
UN Voting Affinity with P-5			5.079**	2.473***	3.285	0.373
			(2.137)	(0.859)	(2.682)	(0.811)
Prior 5 year Democracy Avg.			-0.0831	-0.145***	0.130	-0.0419
			(0.0975)	(0.0397)	(0.131)	(0.0387)
Constant	-26.08***	-9.859***	-14.31***	0.886	-26.03***	-10.32***
	(7.325)	(2.225)	(2.146)	(0.751)	(7.919)	(2.391)
sigma	10.71***	4.352***	11.12***	5.819***	10.50***	4.294***
	(0.752)	(0.152)	(0.716)	(0.203)	(0.743)	(0.151)
Observations	977	977	1036	1036	907	907

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.3: Humanitarian versus Political-Strategic Factors *after* Civil Wars (Post-conflict Sample). Dependant Variable: Log of Total Humanitarian Aid Disbursements.

VARIABLES	(1) Tobit *Full Sample*	(2) Tobit *Full Sample*	(3) Tobit *Full Sample*	(4) Tobit *Cold War*	(5) Tobit *Post-Cold War*
GDP per capita	0.000364**		0.000297*	0.00119***	-0.000763***
	(0.000158)		(0.000176)	(0.000280)	(9.66e-05)
Infant Mortality	0.0108**		0.0123**	-0.00650	0.0110***
	(0.00526)		(0.00529)	(0.00777)	(0.00326)
Life Expectancy	0.0842***		0.0926***	-0.0250	0.0504***
	(0.0245)		(0.0248)	(0.0369)	(0.0164)
Logged Dead	0.499***		0.508***	0.638***	0.205***
	(0.0811)		(0.0835)	(0.119)	(0.0563)
Log Refugee-IDP	-0.0780***		-0.135***	-0.342***	0.0905***
	(0.0251)		(0.0266)	(0.0371)	(0.0223)
Oil Exporter		0.305	-0.0302	-0.690	1.674***
		(0.325)	(0.349)	(0.496)	(0.238)
Former colony of P-5 member		1.731***	1.618***	2.048***	0.228
		(0.284)	(0.285)	(0.394)	(0.203)
Contiguous with P-5 member		-0.607**	-0.943***	0.205	-2.102***
		(0.305)	(0.306)	(0.465)	(0.261)
UN Voting Affinity with P-5		4.835***	4.334***	2.715***	-4.386***
		(0.618)	(0.652)	(0.917)	(0.593)
Prior 5 year Democracy Avg.		0.0382	0.0269	0.0646*	0.0511**
		(0.0253)	(0.0255)	(0.0352)	(0.0203)
Constant	-7.474***	-1.062**	-11.02***	-4.340	3.566***
	(1.768)	(0.493)	(1.819)	(2.715)	(1.200)
sigma	5.361***	5.378***	5.274***	5.981***	1.785***
	(0.126)	(0.130)	(0.127)	(0.183)	(0.0666)
Observations	1945	1857	1853	1425	428

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.4: Humanitarian versus Political-Strategic Factors *after* Civil Wars *by Donor Type* (Post-conflict Sample). Dependant Variable: Log of Humanitarian Aid Disbursements.

VARIABLES	(1) Tobit DAC Donors	(2) Tobit Multilateral	(3) Tobit DAC Donors	(4) Tobit Multilateral	(5) Tobit DAC Donors	(6) Tobit Multilateral
GDP per capita	0.000877***	0.000287*			0.000523*	0.000267
	(0.000262)	(0.000154)			(0.000290)	(0.000170)
Infant Mortality	0.0380***	0.0110**			0.0409***	0.0120**
	(0.00913)	(0.00510)			(0.00921)	(0.00511)
Life Expectancy	0.251***	0.0840***			0.265***	0.0905***
	(0.0429)	(0.0237)			(0.0437)	(0.0239)
Logged Dead	0.168	0.488***			0.270*	0.497***
	(0.141)	(0.0785)			(0.145)	(0.0806)
Log Refugee-IDP	0.0758*	-0.0794***			-0.0325	-0.135***
	(0.0441)	(0.0243)			(0.0470)	(0.0257)
Oil Exporter			0.810	0.272	0.775	-0.0499
			(0.565)	(0.314)	(0.599)	(0.337)
Former colony of P-5			0.0418	1.747***	0.177	1.636***
			(0.505)	(0.274)	(0.502)	(0.275)
Contiguous with P-5			-1.323**	-0.476	-1.605***	-0.807***
			(0.537)	(0.295)	(0.533)	(0.296)
UN Voting Affinity with P-5			10.49***	4.392***	8.371***	3.946***
			(1.129)	(0.597)	(1.173)	(0.630)
Prior 5 year Democracy Avg.			0.138***	0.0321	0.108**	0.0205
			(0.0453)	(0.0244)	(0.0452)	(0.0246)
Constant	-23.53***	-7.473***	-10.85***	-0.903*	-30.46***	-10.62***
	(3.121)	(1.714)	(0.980)	(0.476)	(3.282)	(1.756)
sigma	8.218***	5.191***	8.264***	5.194***	8.034***	5.090***
	(0.284)	(0.122)	(0.294)	(0.126)	(0.285)	(0.123)
Observations	1945	1945	1857	1857	1853	1853

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

APPENDIX Table A1: OLS Estimations: Humanitarian versus Political-Strategic Factors after Civil Wars (Post-conflict Sample). Dependant Variable: Log of Total Humanitarian Aid Disbursements.

VARIABLES	(1) OLS Total Hum Aid. (Full Sample)	(2) OLS Total Hum Aid. (Full Sample)	(3) OLS Total Hum Aid. (Full Sample)
GDP per capita	0.000255***		0.000202*
	(8.99e-05)		(0.000105)
Infant Mortality	0.00820***		0.00906***
	(0.00300)		(0.00296)
Life Expectancy	0.0586***		0.0631***
	(0.0139)		(0.0143)
Logged Dead	0.320***		0.321***
	(0.0479)		(0.0486)
Log Refugee-IDP	-0.0535***		-0.0884***
	(0.0150)		(0.0158)
Oil Exporter		0.199	-0.0308
		(0.199)	(0.217)
Former colony of P-5 member		1.028***	0.967***
		(0.162)	(0.164)
Contiguous with P-5 member		-0.367**	-0.567***
		(0.181)	(0.181)
UN Voting Affinity with P-5		2.965***	2.603***
		(0.346)	(0.369)
Prior 5 year Democracy Avg.		0.0212	0.00984
		(0.0148)	(0.0153)
Constant	-2.774***	1.864***	-4.748***
	(0.983)	(0.276)	(0.980)
Observations	1945	1857	1853
R-squared	0.041	0.052	0.088

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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Chapter Three
Can Humanitarian Aid Inadvertently Prolong Conflict? A Theory and Evidence from Panel Data

The provision of humanitarian assistance has rapidly become a core component of modern peacebuilding and post-conflict reconstruction. In just the two decades since the end of the Cold War, the amount of international humanitarian aid reported through the OECD Development Assistance Committee (DAC) from member and non-members states increased nearly 1400-percent in real terms, from $796 million USD in 1989 to well over $11 billion USD in 2008 **(Figure 1)**[1]. The overwhelming majority of these resources have been allocated across conflict and post-conflict areas to provide basic resources like food, shelter, and medical supplies to victims of violent conflict. In the 1990's, the international community provided billions of dollars in humanitarian relief to assist refugees and internally displaced persons (IDPs) in places like Rwanda and Bosnia. Since 2001, this strategy has continued more intensely in Afghanistan, Iraq, and Sub-Saharan Africa. By one estimate, 80 percent of all social services like healthcare and education in Afghanistan today are provided through contracts with international aid organizations (Cohen et al. 2009).

Despite the normative appeal of increasing humanitarian assistance to conflict and post-conflict areas, there is growing controversy over the unintended consequences of aid in recipient states. Among the most popular of these critiques is that humanitarian assistance can inadvertently prolong conflict. During the Rwandan Civil War, for

[1] DAC figures are of the humanitarian aid component of ODA from all donors reported in constant 2007 USD.

example, western journalists and international watchdog organizations joined the Rwandan government in accusing the UNHCR and its aid contractors of indirectly fueling the conflict by assisting Hutu war criminals in competition with the state (Gourevitch, 1999). In Bosnia, aid workers and international observers argued that safe-zones created to provide relief services prolonged fighting and resulted in the death of nearly 20,000 people in and around the aid enclaves (Woodward & Institution, 1995). Similar claims have been made about humanitarian assistance in Tajikistan, Somalia, Chechnya, Afghanistan, and Cambodia among others (Anderson, 1999; Luttwak, 1999; Terry, 2002).

Although analysts of these conflicts have been quick to establish a direct link between humanitarian aid and the duration of war, many aid workers and policy experts continue to be skeptical. Humanitarian aid has been sent to many conflict and post-conflict areas since the end of the Cold War and not every conflict appears to have reignited like it did in Rwanda or continued for as long it did in Bosnia (Shearer, 2000). John Borton is one of several analysts to have criticized the overall emphasis on humanitarian aid as a significant factor in conflict:

> Whilst there have been many instances where humanitarian aid has been hijacked and diverted to the benefit of warring factions, the empirical evidence is simply not available to warrant a focus upon humanitarian aid "doing no harm" as against harm done by, say, other states, business interests, illegal and semi-legal trading activities and arms trade. The manipulation and occasional diversion of relief aid have been wrongly equated with an analysis of the war economy. In most, if not all, conflicts the role of humanitarian aid as a source of support for warring factions has probably been slight" (Borton, 1998)

Conclusions about the impact of humanitarian aid on conflict are mixed because the evidence to date is mostly anecdotal and because a satisfying theoretical link between humanitarian assistance and the duration of conflict has yet to be specified. Existing research has failed to determine how robust the empirical relationship is or what underlying factors help explain variation in the effect across cases. This is surprising given that the empirical relationship is the biggest issue at stake for policy makers and practitioners debating whether the humanitarian assistance is an effective peacebuilding strategy.

This paper aims to make two main contributions to this end. First, I propose a theory of humanitarian aid and conflict duration based on existing bargaining models of war termination. The theory implies that making was less costly—as international humanitarian aid is explicitly designed to do—can inadvertently prolong war by slowing down the accrual of information that allows opponents to converge on more congruent estimates of relative strength, and accordingly, coordinate expectations about what each is prepared to accept in a settlement. Second, I test the empirical expectations of the theory using panel data on the amount of humanitarian aid administered cross-nationally from 1969-2008. After controlling for factors that may be correlated with both civil war duration and the decision by aid organizations to treat crises more or less intensively, I find some evidence that humanitarian aid can inadvertently prolong civil wars. However, consistent with an informational mechanism, I show that this effect is largely mediated by the degree to which it aid adds uncertainty to the crisis bargaining process. The tendency

for humanitarian assistance to prolong civil conflict appears to be much more acute under conditions where the allocation of aid itself is likely to be uncertain.

It is important to emphasize at the start that I do not intend to offer a full evaluation of the impact of humanitarian assistance in conflict areas. Instead, I aim to specify one mechanism through which humanitarian assistance can prolong conflict that is consistent with the existing empirical research. I do not pretend that the mechanism presented here is the only way humanitarian assistance can affect the conflict bargaining process or even the only mechanism through which it might affect the duration of war. A broader evaluation of humanitarian assistance would certainly include many other factors that a richer theory might then seek to incorporate. However, evaluating whether humanitarian assistance is, on balance, a net-positive or negative peacebuilding strategy is a tremendously complicated welfare question that this paper purposely avoids.

The remainder of this paper proceeds in four principal sections. First, I review the existing literature on humanitarian aid and conflict in order to highlight popular mechanisms through which aid is thought to prolong war. Second, I outline the bargaining framework I use to explore the interaction between humanitarian assistance and conflict. In the third section, I derive an informational theory of humanitarian assistance and war termination from which I generate testable hypotheses about how humanitarian aid will effect the duration of war. In the fourth section, I outline my research design and test my hypotheses empirically. Finally, I conclude in the fourth and fifth sections with a discussion of the results and their implications for international peacebuilding.

Previous Literature: Linking Humanitarian Aid and the Duration of Conflict

How can humanitarian aid prolong conflict? Aid workers and analysts often identify this tendency as the core "paradox of humanitarian action": in aiming to alleviate suffering, humanitarian aid appears to sustain conflict and inadvertently prolongs suffering (Anderson 1999; Terry 2002). To date, the most common explanations have focused on how aid "fuels" conflict by supplying resources to competing parties. But while resource theories of conflict are intuitively appealing, they often fail to address the underlying incentive parties have to avoid the costs of war altogether. This section reviews existing explanations for this phenomenon before outlining the theoretical critique motivating the informational theory that follows.

Four mechanisms linking humanitarian assistance to the duration of conflict have been proposed in the existing literature. First, humanitarian assistance can prolong conflict by directly or indirectly providing the material resources needed to finance an insurgency. Although international law obliges combatants to distinguish themselves from civilian populations, non-uniformed insurgents routinely intermingle with civilians as a deliberate policy. As a result, humanitarian agencies find it difficult to distinguish between civilians and combatants, and often directly supply insurgents with the food, shelter and medical supplies needed to sustain a war effort. In the aftermath of the Rwandan Civil War, for example, humanitarian organizations discovered that relief supplies were directly used towards military operations by Hutu armed forces operating in and around relief camps (Cooley & Ron, 2002; Gourevitch, 1999).

More indirectly, theft has proven to be an effective strategy in financing the costs of war. By one account, more than half of all aid in the former Yugoslavia was diverted to feeding and supplying soldiers (Woodward & Institution, 1995). In 1994, aid organizations operating in Liberia had more than $5 million worth of material stolen, including 74 vehicles, 27 trucks, 18 motorcycles, communication equipment and thousands of tons of food (Terry, 2002). This was eclipsed in 1996, when warring factions in Monrovia stole $20 million worth of equipment from the UN and NGOs (Atkinson, 1997). Over time, factions have grown increasingly sophisticated in their relation to the distribution of relief, even creating local NGOs to gain control of the distribution process (Prendergast & Scott, 1996). In Somalia, where estimates of the quantity of food stolen range from 20 to 80 percent, bandits registered fake villages and coerced real villages to sign for food that never arrived (de Waal, 1994).

Second, humanitarian assistance can prolong conflict by creating internationally protected spaces from which combatants can launch attacks with relative immunity. For decades militant groups facing defeat and possible annihilation have used internationally protected refugee camps and aid enclaves as de facto safe-havens for rest, recuperation, and recruitment (Terry, 2002). During the Bosnian Civil War, for example, analysts argued that the creation of safe-zones prolonged the conflict by offering international protection to Bosnian forces (Boyd, 1995; Landgren, 1995; Woodward & Institution, 1995). Recent empirical work has offered even broader support for this mechanism by demonstrating a relationship between protected refugee populations in neighboring states and the occurrence and continuation of civil conflict in refugee-sending states (Salehyan,

2007). As a result, hosting states are significantly more likely to be attacked by refugee-sending states pursuing combatants across borders (Salehyan, 2008).

Third, humanitarian assistance can prolong conflict through a substitution effect, by relieving competing parties from the political burden of sustaining a war. High levels of humanitarian assistance can often fill so great a proportion of civilian needs for public services that significant local resources can be freed up and reallocated towards the war effort (Anderson & Duffield, 1998; Duffield, 1994). One interpretation of Israel's periodic blockade of UN aid convoys into Gaza, for example, is that international assistance has effectively insulated the Hamas leadership from the domestic political costs of a waging a protracted war. Were it not for high levels of humanitarian assistance provided by the international community, Hamas might be forced to redirect resources away from the ongoing conflict and towards the provision of public goods necessary to maintain political support (Luttwak, 1999). By blockading aid convoys into Gaza, Israel hopes to increase political costs of war for Hamas and secure a more favorable settlement in ending the contest.

Fourth, humanitarian assistance can prolong conflict when aid organizations participate in, or create, a local war economy with interests tied to the continuation of war. When humanitarian aid arrives from abroad, military leaders and local warlords can profit from visas, import duties, airport and port charges, along with other administrative fees (Human Rights Watch 1992). Charles Taylor, for example, demanded 15 percent of all aid entering his territory to be paid in cash or kind as a form of import tax (Terry, 2002). Once delivered, aid organizations attempting to protect relief supplies and staff

are often forced to hire guards from local militias operating security rackets (Sommer, 1994). Humanitarian agencies then hire local staffs that require hotel rooms and office spaces to organize relief operations. From delivery to distribution, the entire relief process creates local industries with economic interests tied to the continuation of war. The emergence of a war economy effectively reduces the opportunity costs of sustaining an otherwise costly competition.

In sum, these mechanisms suggest that humanitarian assistance can inadvertently prolong conflict by reducing the costs of war in four ways: (i) directly or indirectly (through theft) financing the material resources needed by an insurgency, (ii) creating protected spaces that shield combatants from costly military attacks, (iii) insulating combatants from the political burden of sustaining a costly war, and (iv) creating new economic interests tied to the continuation of conflict.

However, while these mechanisms are certainly plausible contributing factors, they do not explain why participants in these violent and risky events cannot do better by negotiating a deal with the government (Fearon, 1995; 2004). The fundamental challenge for any theory of war duration is to explain why, if fighting a war is extremely costly and risky, sides ever have the incentive to delay settlement in favor of continuing to fight. Structured this way, the argument that humanitarian aid prolongs conflict simply by supplying resources seems incomplete, since the total level of aid is always less than the total costs incurred by the parties involved in the war. Even in conflicts that receive relatively high levels of humanitarian assistance, war remains sufficiently costly such that sides should seek an immediate settlement. Analysts linking humanitarian aid to conflict

must therefore demonstrate how increasing levels of aid can independently shift the incentives of competing parties such that they elect to pay the costs of an additional period of warfare rather than accept a bargained solution.

In the next section, I review a class of bargaining models that provide a useful framework for analyzing the interaction between humanitarian aid and war termination. In these models, fighting, while costly, occurs in equilibrium because it resolves uncertainty in a less-manipulable forum than the bargaining table (Wagner, 2000). Fighting ends when opponents learn enough about their prospects in war to decide that continuation is unprofitable. The costs of war, therefore, serve an informational purpose because they resolve the uncertainty that marks protracted conflicts. It follows that making was less costly—as humanitarian aid is explicitly designed to do—may paradoxically prolong violent conflict by slowing down the accrual of information that allows opponents to converge on more congruent beliefs about relative strength, and accordingly, coordinate expectations about what each side is prepared to receive in a settlement of the stakes.

A Framework: Bargaining Theory and Models of War Termination

Scholars of international relations generally regard the occurrence, conduct and termination of war as a bargaining process. Building from Rubenstein's (1982) analysis of non-cooperate bargaining theory, recent formal work has applied the basic bargaining framework to reorient the study of war in order to address the question of why actors

(states, ethnic groups, etc.) are unable to settle their disagreements short of costly fighting. Fearon (1995) proposed what is now generally regarded as the standard or canonical model for the occurrence of war. In the model, two sides (S_1 and S_2) have divergent preferences over the division of some issue space represented by the interval X = [0,1]. S_1 prefers issues resolutions closer to 1, while S_2 prefers outcomes closer to 0. Supposing states fight a war, S_1 prevails with probability $p \in [0,1]$ and gets to choose its favorite outcome closer to 1. S_1's expected utility is $pu_{S1}(1) + (1-p)u_{S1}(0) - c_{S1}$, or $p - c_{S1}$. S_2's expected utility for war is $1+p-c_{S2}$. The parameters, c_{S1} and c_{S2} represent the costs for fighting a war to each side along with the value of winning and losing on the issues at stake. Importantly, the costs of fighting open up a range of bargained solutions between $p-c_1$ and $p+c_2$ that both sides should strictly prefer to paying the costs of war. Structured this way, the puzzle becomes why sides ever fail to identify a negotiated settlement within this range *ex ante*, knowing that war is always inefficient *ex post*.

Fearon suggests that coherent rationalist explanations for war will fall into one of two categories: sides can fail to reach a bargain because they have private information with incentives to misrepresent or because sides are unable to credibly commit themselves to follow through on the terms of the agreement. According to the first explanation, sides have asymmetric information about their own capabilities, *p,* and resolve*, c*, and they have an incentive to over-represent (or under-represent) their ability on these dimensions to their opponent in order to secure a better settlement. As a result, while the costs of fighting open up a range of negotiated settlements both sides prefer to war, sides have the incentive to bluff in order to shift the bargaining range in their favor.

The second explanation is more straightforward. Sides may prefer to fight now if certain elements of the strategic environment make it so that their opponent is unlikely to honor a negotiated settlement in the future (for example see Walter 1997).

An important critique of this model with respect to its application in the study of war is that in formalizing war as a game-ending move, or "outside option", it assumes away any strategic behavior within wars and makes it impossible to ask important questions about war termination (Wagner 2000; Powell 2004). The model effectively limits the analysis of conflict to the origins of war. Given that many wars end short of total military collapse, broadening the model from a single-shot bargaining game to a dynamic process is useful in exploring the factors that lead some wars to last longer than others. For example, if war results from private information with incentives to misrepresent or commitment problems, certain events within a conflict must somehow resolve these issues for sides to ultimately stop fighting. Moreover, a dynamic understanding of war is simply more consistent with the intuition that war is a costly instrument used to coerce policy concessions rather than an end in itself (Blainey, 1988; Clausewitz, Howard, Paret & Heuser, 2007; Schelling, 1966).

Several models relax the game-ending, costly-lottery assumption by modeling war as a costly *process* that occurs simultaneously with bargaining (Filson & Werner, 2002; Filson & Werner, 2007; Powell, 2004; Slantchev, 2003; Slantchev, 2004; Smith & Stam, 2004; Wagner, 2000). These models generally take informational asymmetries as their starting point, and treat war as an "inside option" that sides utilize in order to reduce uncertainty. The basic idea is that since sides have private information about their

willingness and ability to fight, and because they have the incentive to over-represent these values at the bargaining table (posture like strong-types) in order to secure a more favorable agreement within a range of negotiated settlements, the bargaining table becomes a relatively unreliable source of information on which to base settlements. The battlefield, by contrast, offers a less manipulable source of information (or, at the very least, an additional source of information). It is simply more difficult for opponents to bluff about particular battlefield outcomes or how these results affect their ability to fight in subsequent challenges. Assuming sides value the future sufficiently, they have the incentive to delay settlement in favor of fighting in order to accrue enough information to formulate reliable beliefs about their opponent's strength and avoid settling prematurely on worst terms (Slantchev, 2004). In this way, war can be thought of as a costly learning process whereby opponents signal their ability to both endure and impose costs in an attempt to converge on common beliefs about relative strength. War eventually terminates when sides have accrued enough information that fighting loses its informational content.

Although these models appear to be a promising starting point to explore the effect of humanitarian assistance on the duration of conflict, Fearon raises in important critique with respect to their ability to explain the duration of civil war specifically (Fearon, 2004). He argues that while asymmetric information may explain the early phases of some civil conflicts, it does not provide a compelling explanation of prolonged civil wars, since "after years of war, fighters on both sides of an insurgency typically develop accurate understandings of the other side's capabilities, tactics and resolve". In

this way, informational explanations may provide a poor account of prolonged civil conflict (Powell, 2006). However, this conclusion follows from a fairly simple model in which third party actors—including those involved in peace building missions—do not add exogenous sources of uncertainty throughout the duration of the conflict. Indeed, there is growing evidence that factors that fluctuate over the course of the war, like the number competing parties (Cunningham, 2006), level of transnational support (Gleditsch, 2007), type of international mediation (Beardsley, 2008), and level of peacekeeping (Fortna, 2004), can significantly effect the duration of civil war by making informational asymmetries more or less acute in the intra-war period.

An Informational Theory: Learning while Fighting, Humanitarian Aid, and the Duration of Conflict

The goal in this section is to move from popular anecdotes about how humanitarian aid can prolong conflict to theoretically grounded propositions. I do this by outlining a mechanism through which exogenous assistance in the form of humanitarian aid can effect the duration of civil war by increasing opponents' uncertainty in the intra-war period. The mechanism holds regardless of the level of aid, which addresses the critique articulated in the previous section that traditional resource theories are insufficient to explain prolonged conflict. Despite the fact that the amount of humanitarian assistance administered during a conflict is never so large as to make war completely costless for the parties involved, the effect of mitigating the costs of war is to

reduce the informational value of fighting and increase the number of battles needed to converge on reliable estimates of opponent's strength.

Consider the bargaining dynamics between two sides—a Government, *G*, and Rebel Group, *R*—over the division of some issue space represented by the interval X = [0,1] following Powell (2004). *G* begins by makes a take it or leave offer to *R* who can either accept the offer immediately or reject the offer in favor of fighting. Under complete information, the optimal strategy is for *G* to offer exactly $p+c_R$ (the probability of *R* winning a fight to the finish minus its expected costs of war) and for *R* to accept the offer immediately since it can do no better from fighting. However, given *G* is uncertain about *R*'s cost of fighting, *G* must formulate its initial offer based on imperfect beliefs about the expected costs of conflict for *R*. In this situation, *G* faces a "risk-return trade-off" between obtaining a more favorable settlement and a higher probability of fighting. If *G* underestimates how costly war will be for *R*, it begins with offers that are too high and guarantees worst terms for itself but a lower probability of fighting. If, on the other hand, *G* overestimates how costly war will be for *R* it begins with offers that are too low (values closer to 1) and effectively provokes fighting. As Powell notes, the equilibrium dynamics in the model turn out to be the same for both types of uncertainty—that is, uncertainty over *R's* costs of fighting or risk military collapse (power). In equilibrium *G* makes a series of increasing concessions that "screen" *R* by type. Higher-cost types and weaker types with a higher risk of military collapse accept earlier offers, while lower-cost and stronger types fight longer until they eventually accept a more favorable offer.

Now, consider the role of humanitarian assistance. Rebels groups must maintain their organizational integrity long enough to signal their strength and extract concessions from a stronger opponent. In doing so, they require substantial resources to finance a military campaign, recruit rebels and generate support from civilians who can supply food, shelter, and intelligence (Weinstein 2008:7). Without access to a sufficient supply of resources, an insurgency can expect to collapse relatively quickly. They will find it difficult to provide the private incentives to maintain a fighting force and the public goods needed to maintain political support among the local population.

As described above, humanitarian can assistance can provide a relatively straightforward solution to some of these key challenges faced by competitors to the state. First, by providing easy access to food, shelter and medical care, humanitarian aid can directly or indirectly finance the costs of an insurgency. Second, by providing protected spaces that shield combatants from costly military attacks, humanitarian assistance can limit the government's power to impose costs on the rebel group. Third, by providing public goods to the local community, humanitarian aid insulates a rebel group from the mounting political costs of fighting a rebellion.[2] And fourth, by creating new economic interests tied to the continuation of conflict, aid organization can reduce the opportunity costs of participating a rebellion.

[2] Fighting an insurgency creates costly externalities for the community in which it takes place. General economic conditions are likely to suffer (Collier and Hoeffler 2003; Murdoch and Sandler, 2002), public provisions by the government are sure to decrease (Lai and Thyne 2008), and the direct civilian toll in terms of casualties can be expected to be very high (Ghobarah et al. 2003, 2004). Moreover, counterinsurgency tactics employed by the government can often be invasive and brutal for civilians that cohabitate with rebels (Lyall 2009). In order for a rebel group's home population to withstand these costs and still provide support for a rebel group, they must receive some level of basic services and protection in return. Aid organizations provide these goods, and often from within protected spaces.

These factors, in turn, affect the bargaining dynamics between the rebel group and the state. The more aid a rebel group receives, the lower its costs of fighting appear to be and the less likely the rebellion is to collapse. In the bargaining process outlined above ("screening" logic), this added resilience forces the government to make a greater number of offers and fight longer as it gradually updates its beliefs and finally offers enough concessions to leave the rebel group indifferent between continuing to fight and accepting a settlement. When aid enters mid-conflict after the government has begun to converge on a more accurate estimate, the government will become less certain of its original beliefs formed in the prior period, and must fight additional periods in order to re-estimate its opponent's strength in light of the advantages gained from external assistance. This suggests the following hypothesis:

> ***Hypothesis 1:*** *The greater the level of humanitarian aid administered during a civil conflict, the longer that conflict will appear to last.*

Importantly, however, the government always has the incentive to avoid the costs of war under complete information. If government could directly observe the degree to which aid mitigated its opponent's costs of fighting, it would do better to factor this into the terms of its settlement offers over time and arrive at an agreement just as quickly as if there was no relief provided. This point is critical because, if the informational mechanism I outline above is correct, one unique implication that follows from the model is that the tendency for humanitarian aid to prolong conflict will be mediated by the

degree to which aid actually adds uncertainty to the crisis bargaining process over time. If the exact amount of humanitarian assistance disbursed is directly observable, or access to future resources can be easily anticipated, competing parties would have the incentive to simply factor any advantage gained by their opponent into an updated assessment of their relative strength and arrive at a settlement that avoids the costs of war just as quickly. Conversely, if the provision of humanitarian assistance is not directly observable, the government not only has incomplete information about its opponents capabilities and resolve, they now have to estimate (or re-estimate) these values in light of the added uncertainty introduced by the exogenous provision of humanitarian assistance by fighting additional periods of warfare to increase the reliability of these estimates.

Stated more formally, the provision of aid in the crisis bargaining framework is like adding a random variable, h_R (the amount of humanitarian aid received by the rebel group and its constituency), to random variables c_R (the costs from fighting a war for a rebel group) and p_R (the probability if winning or losing for a rebel group). Although the expected mean of h_R+c_R is straightforwardly equal to the sum of the independent means of h_R *and* c_R, the *variance (a* measure of the dispersion of that variable around its mean) of h_R plus c_R, is equal to the variance of h_R *plus* the variance of c_R plus two times the covariance of h_R and c_R. Supposing that h_R and c_R are independent, the covariance term is zero and the variance simply reduces to the variance of h_R plus the variance of c_R, *or* $(\sigma^2_{hR+cR}=\sigma^2_{hR}+\sigma^2_{cR})$.[3]

[3] For example, if the variance of h_R were 1000, the variance of c_R were 1100 and the covariance were 0, then the total variance of the unknown parameters would be 2100

Substantively, this means that the effect of providing humanitarian assistance is to increase the variance of the probability distributions for the unknown parameters of interest, c_R and p_R, from which the government is essentially drawing a random sample with each battle (with a per-period/per-draw cost) in attempt to converge on a more accurate estimate of a rebel group's capabilities and resolve over time. Increasing the cumulative variance of the theoretical probability distribution by adding random variable h_R to the unknown parameters of interest c_R and p_R effectively reduces the informational value of any one battlefield outcome (or any one random draw) as it will take a greater number of battles (or a larger sample size) from a distribution with greater dispersion to approach the same level of confidence in estimating the expected mean due to the higher sample variance.

Note that the uncertainty introduced by the exogenous provision of aid is further compounded by the same problem of asymmetric information which characterizes the government's estimate of the rebel group's true capabilities and resolve - rebel groups not only have private information about the amount of humanitarian assistance they and their primary constituency received (as well as the degree to which this relief actually increased their underlying capabilities and resolve), they also has an incentive to misrepresent the advantage gained from aid to the government in order to extract higher concessions in crisis bargaining.

This unique proposition suggests that the general tendency for humanitarian aid to prolong will be more acute under (i.e. mediated by) conditions in which the provision of humanitarian assistance itself is more uncertain. For example, humanitarian assistance

may be especially prone to prolonging war in conflicts with weak central governments that lack the military, police, and intelligence capabilities to directly observe the allocation of aid in parts of the state where insurgents operate. Another possibility is that humanitarian assistance may be more prone to prolonging conflict when insurgents operate primarily on the outskirts of a state where the organizational capacity to government does not easily reach. It is also reasonable to expect this effect will be more acute when the primary recipients of humanitarian assistance are outside the borders of a civil war state rather than within them. Each of these hypothetical conditions makes it more difficult to directly observe the advantage gained by humanitarian assistance and adjust settlement offers based on updated beliefs. This uncertainty is unlikely to be resolved at the bargaining table because rebel leaders have the incentive to over-represent just how much aid increases their ability to fight a protracted conflict.

> ***Hypothesis 2:*** *The tendency for humanitarian assistance to prolong conflict will be stronger under conditions where the level of aid is more difficult to observe and weaker under conditions in which it is more easy to observe.*

In the empirical tests that follow, I operationalize the Government's level of uncertainty over the exact level of aid, by interacting aid with a dummy variable indicating whether the civil war was fought between a rebel group operating in the outskirts of a state, or a *peripheral insurgency*. My expectation based on the informational theory above is that the tendency for aid to prolong civil war will be mediated by whether aid was provided to rebellion under these uncertain circumstances.

That is, the tendency for aid to prolong conflict will be more acute when rebel groups are operating a peripheral insurgency on the outskirts of a state, and less acute when rebel groups are operating in the core of a state. To this end, I limit the analysis of Hypothesis 2 to only treated cases, showing that within the sample of cases getting some level of humanitarian aid, the effect is contingent on the conditions uncertainty under which aid is provided.

In the next section, I test both of these hypotheses empirically using cross-national OECD estimates of the total amount of humanitarian aid disbursed in every civil war-year from 1969-2004 for all civil wars since 1945. Analysis of the first hypothesis will be straightforward, estimating the overall relationship between the level humanitarian aid and duration of conflict while controlling for confounding factors. However, if the results suggest that increasing levels of humanitarian aid in civil conflicts actually prolongs war, it would still remain unclear from this test as to what the exact mechanism is through which this effect occurs. Recall, existing theories posit that aid prolongs conflict simply by providing material resources, which presumably enter into rebels groups' production function for violence thereby enabling them to continue fighting beyond the point they would have otherwise in the absence of humanitarian relief. But, as argued above, this interpretation is not completely coherent when one acknowledges that war itself remains an extremely costly way to settle disputes. Even in crises treated with relatively high levels of humanitarian assistance, war is always inefficient *ex post*.

Therefore, I proposed an alternative theoretical mechanism based on the assumption that war, while costly, occurs in equilibrium because it provides information in a less manipulable forum than the bargaining table. Here, war is modeled as a dynamic costly-learning process, wherein competing parties observe their opponent's cost of fighting and risk of military collapse over time and gradually update their beliefs about their opponent's "type" (as either strong of weak) until they converge on more accurate or more congruent estimates of relative strength. Assuming that the costs of war serve an important informational purpose, it follows that even the most well-intentioned humanitarian aid may have the paradoxical effect of reducing the informational value of fighting and effectively prolonging conflict. However, this mechanism uniquely suggests that the relationship will be mediated by the amount of uncertainty the provision of aid may add to the dynamic conflict bargaining process, as per Hypothesis 2.

Research Design and Data

This section examines the impact of humanitarian assistance on the duration of civil wars since 1945. The data used to define the population of civil wars in the study is taken from Cunningham (2007), which is based on the Armed Conflict Dataset (ACD). The ACD defines a civil 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". Because the ACD counts all conflicts within a country over separate territories as distinct

wars and all conflicts in a single country over the government as one civil war, the dataset does not easily lend itself to duration analysis since two wars punctuated by a 20-year period of peace would be counted as the same conflict. To correct for this, Cunningham (2007) counts any conflict within a country that occurs after a 24-month break in fighting as a new war. This refinement produces a dataset of 288 separate civil wars since 1945.

To test the effect of humanitarian assistance on the duration of conflict, I employ a Cox proportional hazards model to estimate the effect of humanitarian assistance—a time varying covariate—on the instantaneous hazard rate, or underlying risk, of war failing in any particular year. The model estimates the probability that a war will fail at time t based on a set of covariates and given that the observation has survived until t, without making assumptions about the shape of the hazard function over time[4]. The unit of observation is the civil war "spell" defined annually starting from the year in which a conflict initiates to the year in which it terminates. Data on the start and end dates for each conflict is drawn from Gates and Strand (2004). Splitting each of the 288 civil wars into calendar years over which the level of humanitarian assistance can vary produces 1689 unique civil war years under observation. Setting the data for survival analysis and dropping all conflicts that started and ended quickly in the same calendar year leaves 193 conflicts and 1308 observations in the sample. This last step is useful in limiting the analysis to a comparison of similar conflicts that were at least eligible to receive intra-war

[4] The baseline hazard rate of conflict terminating may be constant, decreasing, increasing, decreasing and then increasing, increasing and then decreasing, or anything else as war continues over time.

humanitarian assistance, as aid organizations can react only after observing sufficient costs from the conflict.

Data used to estimate the amount of humanitarian assistance disbursed in each conflict-year is taken from the OECD DAC (Organizations for Economic Co-operation – Development Assistance Committee) data on Official Development Assistance (ODA). Within the overall definition of ODA, humanitarian aid is defined as "assistance designed to save lives, alleviate suffering and maintain and protect human dignity during and in the aftermath of emergencies. To be classified as humanitarian, aid must be consistent with the humanitarian principles of humanity, impartiality, neutrality and independence." This includes relief coordination, protection, support services and material assistance like food and medical supplies. Disbursements record the actual international transfer of financial resources, or of goods and services, valued at the cost to the donor over a given accounting period less repayments of any loan principal or interest over that period. The data includes, (i) bilateral humanitarian aid disbursements from DAC member countries, (ii) aggregated non-DAC member disbursements, and (iii) aid activities financed through multilateral institutions' regular budgets and international NGOs. The last category is counted in the multilateral outflows component of the total humanitarian aid disbursement only if a contribution to an agency is pooled with other contributions and disbursed at the discretion of the agency.[5] Projects executed by multilateral organizations on behalf of donor countries are counted as bilateral flows, since it is the donor country

[5] The data includes humanitarian aid outflows from World Bank, the regional development banks and several UN agencies, including the UNHCR, UNAID, UNDP, UNRWA UNICEF, World Food Program, UNTA, UNFPA, and several others. The list also includes disbursements from over 50 of the largest International NGO's. Full list at http://www.oecd.org/dataoecd/36/16/31724727.pdf

that effectively controls the use of the funds. The DAC list of potential recipients includes all developing countries and territories eligible for receiving ODA from 1969-2009. This includes virtually every state in the international system that was not a member of the DAC in that year. Currently there are 24-members of the DAC based on their status as highly developed states.[6]

In this study, disbursements of all bilateral and multilateral aid are aggregated for a total estimate of humanitarian assistance disbursed in recipient-state for each year in a conflict. Humanitarian aid disbursements are recorded in constant 2007 prices and exchange rates. This means adjustments have been made to cover both inflation in the donor's currency between each year and the reference year, and changes in the exchange rate between that currency and the United States dollar over the same period. In the analysis below, I also log-transform these values because the variances are not homogenous: most observations receive no humanitarian assistance, many observations receive small amounts, and a few observations receive very high levels of humanitarian assistance. The transformation yields a more normal distribution closer to the assumptions of parametric statistical tests.

[6] The DAC 24 members as 0f 2010: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Korea, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, United States and the Commission of the European Communities. In the period under observation, membership changed seven times with New Zealand (1973), Finland (1975), Ireland (1985), Luxmbourg (1992), Portugal (1991), Spain (1991) Greece in (1999).

Endogeneity Bias and Selection Effects

The provision of humanitarian assistance across civil war states is probably not random. Aid organizations and donor governments are likely to make allocation choices based on where humanitarian assistance is most needed or where it is likely to be most effective. This non-random selection raises two important concerns for estimating the impact of humanitarian assistance on the duration of civil war.

First, the level of humanitarian assistance may be endogenous to the duration of conflict. If the amount of humanitarian assistance in any period is determined based on donors' expectations for a short or long war, then any relationship between the level of humanitarian aid and conflict duration observed in the data may be flowing from the latter to the former. In other words, humanitarian aid may not be prolonging conflict itself, but responding more or less intensively depending on donors' expectations that a conflict is likely to last.

Second, the level of humanitarian assistance may be selected based on other observable indicators of conflict that are themselves correlated with the duration of civil war. For example, it may be the case that aid organizations are choosing to allocate humanitarian assistance more or less intensively across conflicts based on the number of refugees or the number of casualties produced as a result of the conflict. If these variables are then correlated with the duration of war, omitting them from the analysis would lead to underspecified models with biased estimates.

We might think of the endogeneity problem in this case as a doctor varying a treatment based on the prognosis of a particular patient and the selection problem as a doctor varying a treatment based on other observable symptoms of a patient that could be correlated with the prognosis. Both forms of strategic selection may bias our estimate if not properly controlled.

In the case of humanitarian assistance, it is unlikely that donors make allocation choices in a given year based directly on the duration of war (endogeneity bias). This is because the amount of humanitarian assistance distributed in a conflict each year must be determined prior to observing if and when a conflict actually terminates. In other words, the prognosis for a civil war is not directly observable during the intra-war period. Aid organizations can, however, treat conflicts more or less intensively based on beliefs about how likely a certain conflict will last or not, but these expectations must be formed indirectly based on observable indicators (or symptoms) that are correlated with the duration of civil war. If this is the case, the endogeneity problem essentially becomes a selection problem where the level of aid observed in any intra-war period is determined with respect to observable factors that are either known or unknown to be correlated with the duration of conflict.

To limit bias from strategic selection in the estimates, the ideal statistical analysis would include any variables that are correlated with both the amount of humanitarian aid allocated to a conflict in a given year and the likelihood of the conflict continuing past that year. If, after controlling for these factors, the relationship between humanitarian aid

and the duration of war is still greater than the baseline survival time, we can be more confident that the results support the expectations of the theory.

Several factors could potentially be associated with both the intensity of the humanitarian response and the risk of conflict failing (or continuing) past a certain moment. First, there is some empirical evidence that more costly wars, in terms of causalities, tend to last longer than others Sambanis 2000; Fortna 2004; Cunningham 2006}. This could be because more deadly wars indicate more intractable bargains underlie the contest or because compatriots find it harder to reconcile with their opposition as they lose more friends and family from in the war. In either case, the number of causalities produced by a conflict is likely to be associated with the level of humanitarian relief provided to the victims of that war and should therefore be included as a control. Because aid organizations tend to make budget appeals retrospectively after observing the level of humanitarian need, I use lagged values of yearly battle-deaths estimated by Gleditech and Lacina in the analysis below (Lacina & Gleditsch, 2005).

Second, the amount of human suffering targeted by humanitarian assistance is likely to increase with the population of the state in conflict. Since higher population states are also more likely to reach the 25-battle-deaths cut-off in a given year to be counted in the sample, the total population in each conflict year should also be included as a control. I include the natural log of a country's population in each conflict year (Gleditsch, 2002)

Third, recent research indicates that geographic conditions matter for the duration of civil war. Rough mountainous terrain and heavy forestation appear to make conditions

ripe for a protracted insurgency against the state (Fearon & Laitin, 2003). Because rough terrain also represents an operational constraint for aid organizations needing access to the victims of war, it should probably be included as a control. It is important to note, however, that the direction of this bias would cut in favor of the theory. If aid appears to prolong conflict despite the fact that it tends to treat inherently shorter conflicts that lack rough terrain, the relationship would appear to hold in the least likely cases, which adds extra support to the expectations. I use Huhaug and Lajala's measure of the percentage of a country's conflict zone that is mountainous or forested as a control in the analysis that follows (Buhaug & Lujala, 2005).

Fourth, recent research has found a strong relationship between countries' per-capita GDP and both the initiation and duration of civil war (Fearon & Laitin, 2003). One explanation that has been proposed is that low GDP per-capita indicates a weak government capacity to resist challenges against the state over time. A second is that high per-capita GDP proxies for more developed terrain that can be penetrated more easily by government forces pursuing rebels. A third reason is that low per-capita GDP may indicate weak economic conditions, which result in low opportunity costs for participating in a rebellion. Regardless of the mechanism, it is also reasonable to expect that humanitarian assistance will asymmetrically target economically weak states that lack the resources or ability to provide high levels of human needs. Therefore, a measure of annual real gross domestic product is incorporated as a control in the analysis below to limit selection bias from this indicator (Gleditsch, 2002).

Fifth, I include a measure for the level of democracy from the Polity project because there is some evidence that regime type effects civil war duration and international interventions – though in a non-linear way {(Hegre et al. 2001). Both full democracies and full autocracies have been shown to be less susceptible to civil war onset and longer civil duration, while transitioning states with middling values of democracy appear more likely to experience civil conflict (the well-known inverted-U relationship between the level of democracy and the likelihood of conflict). Moreover, there has been some evidence that foreign assistance is disproportionately allocated to more democratic countries in order to "reward" good policy (Collier 2004).

Sixth, previous research has demonstrated a strong empirical relationship between the duration of a civil war and whether there an international guarantee exists to enforce a previous settlement between competing parties (Walter, 2002). The logic is that third-party guarantees solve commitment problems endemic to civil conflicts and thus make wars less likely continue. Because humanitarian assistance is likely to be correlated with the broader international peace building response (increasing with other peace-building measures), I include a dichotomous variable measuring whether an international guarantee to enforce a previous peace agreement existed between the parties in each year in order to isolate the independent effect of humanitarian assistance on war duration (Walter, 2002).

Finally, several studies find that rebel or government access to "lootable" resources like diamonds, drugs or oil can fuel prolonged conflict (Collier & Hoeffler, 2000; Collier & Hoeffler, 2002; Elbadawi & Sambanis, 2000; Lujala, Gleditsch &

Gilmore, 2005; Ross, 2004; Sambanis, 2000). If the presence of these resources is uncorrelated with the level of humanitarian assistance in a conflict, it may not be necessary to include them in the following analyses. However, because a common argument against the claim that humanitarian assistance can prolong war is that the effect is probably slight compared to other easily lootable resources (Borton, 1998), it may be instructive to include these variables as controls. To this end, I include the same Buhaug, Gates, and Lujala (2002) indicators for whether any lootable resources were present in a conflict used by Cunningham (2007).

In sum, the previous research indicates that the costliness of war, the population of a civil-war state, the geographic conditions of the conflict zone, the level of government capacity, the use of other international peacebuilding strategies and the presence of lootable resources may affect both the baseline prospects for war and the level of humanitarian assistance to that country in a given year. In the following analysis I include measures for each variable to control for selection effects in the allocation of humanitarian assistance. While not perfect, the inclusion of these variables as controls should decrease bias in estimating the impact of humanitarian assistance on the duration of civil war.

Results

Table 1 shows the results for Hypothesis 1 outlined in the previous section. In the tables, hazard ratios are reported rather than the familiar coefficient estimates from standard linear or logistic regressions. Hazard ratios are interpreted relative to 1, where hazard ratios greater than 1 indicate variables that increase the risk of war failing over

time and hazard ratios less than 1 indicate variables that decrease the risk of war failing over time. For example, if the results indicate that a dummy-variable has a hazard ratio of 0.5, that variable decreases the risk of war failing by 50-percent, *meaning it tends to prolong civil conflict*. Conversely, if a variable has a hazard ratio of 2, it doubles the risk of war failing, *meaning it tends to shorten conflict*. As per the theory outlined above, I expect the hazard ratio—or risk of war terminating—to decrease as the level of humanitarian assistance increases in each civil conflict year.

Model 1 reports the results for the full analysis of 193 conflicts that last longer than one year. Of these, 159 civil wars eventually terminated, while the remainder are censored because they were ongoing when the data was collected. The sample includes 1308 observations—or years of civil war—that are under risk of potentially failing in the study period.

The results indicate that increasing levels of humanitarian assistance across the sample decreases the instantaneous risk of war terminating by roughly 8.7-percent when holding all covariates at their mean. In other words, controlling for the selection of humanitarian assistance into harder or easier cases where the underlying conditions are more or less prone for prolonged conflict, greater levels of humanitarian aid appear to independently increase the risk of a conflict surviving. The result holds even when controlling for access to other lootable resources in a civil war, a factor previously thought to mitigate the effect of humanitarian aid provisions when present (Borton 1998). While conflicts with access to drug production and diamond deposits are approximately 65-percent less likely to terminate at any moment in the analysis (i.e 65-percent more

likely to continue), access to humanitarian aid remains strongly associated with prolonged war below the 5-percent level. Not surprisingly, third party guarantees of pre-existing agreements between belligerents appears to double the risk of war terminating at any moment.

Figure 2 plots the Nelson-Aalen cumulative hazard rates, which is the rate of conflict termination over time (starting in 1945), for three different groups. Plotted in blue are all civil wars that received zero humanitarian assistance, essentially the control cases. Plotted in red, are all civil wars that received some positive amount of humanitarian assistance. And plotted in green, are the subset of conflicts that were major recipients of humanitarian assistance, where the amount of humanitarian assistance disbursed was in the 90^{th}-percentile. The analysis time is reported in days since civil war initiation. The earliest observations in the sample begin in 1945, while observations treated with humanitarian assistance start in 1969 when the OECD began collecting data on the humanitarian component of Official Development Assistance. Because the amount of humanitarian assistance allocated to conflict areas in earlier periods of the war is generally small, the baseline hazard rate appears unchanged.

Notice that that control cases in blue – the conflicts that received no humanitarian assistance – are uniformly more likely to terminate at any given moment than the treated cases in red, and notice that these cases which received and positive amount of humanitarian assistance are uniformly more likely to terminate at any given moment than the major recipients of humanitarian assistance plotted in green. Also clear is that the cumulative hazard rates for the untreated conflicts begin to diverge significantly at the

end of the Cold War, when there is an explosion in the amount of humanitarian assistance provided by the international community.

Model 2 reports results for the same model in only 119 post-Cold War cases of conflict initiated after 1988. Of these, 89 civil wars eventually terminated, while the remainder are censored. The sample includes 498 observations under risk of failing in the Post-Cold War period. The results indicate that humanitarian assistance was even more prone to prolonging war in this period. Increasing humanitarian assistance in these conflicts resulted in a statistically significant 11.4% drop from the baseline hazard rate of conflict termination.

Figure 3 plots the Nelson-Aalen cumulative hazard rates comparing the untreated and treated civil conflicts in the post-Cold War period after 1989, along with the 95-percent confidence intervals for both groups. In this plot it is clear that the negative relationship between the provision of humanitarian assistance and the likelihood of conflict termination is statistically significant at all point in the analysis.

When compared to the results of **Model 3**, which reports the hazard rate for the 104 pre-Cold War conflicts initiated before 1988, the effect of the Cold War is telling. Humanitarian assistance does not significantly affect the underlying risk of war terminating in the 810 observations before 1989. This suggests that the overall relationship between humanitarian assistance and war duration in the full population of wars is largely driven by conflicts treated in the post-Cold War period. This period is also when humanitarian assistance levels rose the most rapidly and civil wars began to simultaneously increase in duration (Fearon, 2004). This could suggest that there are

other important factors unique to this period that were omitted from the analysis, or that the mechanism outlined by the model works more acutely at relatively high levels of humanitarian assistance. In other words, the relationship between the amount of humanitarian aid and the risk of war termination may decrease the hazard rate non-linearly.

Table 2 shows the results for Hypothesis 2 outlined in the previous section. As in Table 1, hazard ratios are reported rather than the familiar coefficient estimates from standard linear or logistic regressions. Recall that in testing Hypothesis 2, we restrict the analysis to only treated conflicts primarily to demonstrate that the observed tendency for aid to prolong conflict (by reducing the instantaneous likelihood of conflict termination) is largely mediated by the degree to which humanitarian assistance adds uncertainty to the conflict bargaining process. Also, an additional advantage of sub-sampling on treated cases is that it should reduce some of the remaining risk of selection bias, as much of the un-modeled heterogeneity between the treated and un-treated conflicts should be accounted for by the fact each of these conflicts received humanitarian assistance. In this way, the analysis of Hypothesis 2 is essentially estimating a dose-response to humanitarian assistance among only the conflicts that expressed sufficiently high levels of need so as to receive assistance from the international humanitarian aid community.

Model 1 reports the results for the full analysis of 124 treated conflicts that last longer than one year. Of these, 91 civil wars eventually terminated, while the remainder are censored because they were ongoing when the data was collected. The sample includes 720 observations—or years of civil war—that are under risk of potentially

failing in the study period. The key variable of interest in this analysis is the interaction between the level of humanitarian aid and the dichotomous measure of whether the conflict was a peripheral insurgency in which the rebel group and its primary base of support was on the outskirts of a state. The variable is coded 1 if the conflict was a peripheral insurgency and 0 if the civil war was fought between the government and a rebel group primarily operating in the center of the state.

The results appear to support the theory. When the level of humanitarian aid is interacted with whether or not the primary recipients are a peripheral insurgency, humanitarian aid *decreases* instantaneous probability of war terminating by over 50-percent. The result only barely misses the 5% significant level at 6%. Perhaps most interesting is that independent effect of increasing humanitarian assistance when controlling for this interaction is insignificant. Overall, these results suggest that, among conflicts that receive some humanitarian assistance, the level of government uncertainty over the provision of assistance mediates the tendency for humanitarian aid to prolong conflict. Empirically, humanitarian aid is associated with longer conflicts *only* when it is administered in civil conflicts marked by a peripheral insurgency. The findings are consistent with the theoretical mechanism formulated above: aid appears to prolong conflict only to the degree that it adds uncertainty to the bargaining process.

Model 2 reports results for the same model in the 99 post-Cold War cases of conflict initiated after 1988 that were treated with some positive amount of humanitarian aid. Of these, 70 civil wars eventually terminated, while the remainder are censored. The sample includes 441 treated observations under risk of failing in the Post-Cold War

period. Here, the results of the interaction are much stronger. When the level of humanitarian aid is interacted with whether or not the primary recipients are a peripheral insurgency, humanitarian aid *decreases* instantaneous probability of war terminating by roughly 65-percent. Interestingly, the results of this model suggest that higher levels humanitarian relief are actually correlated with a 50% *greater* risk of conflict terminating at any given moment when controlling for conditions that might make the provision of aid less observable over time. These findings are both consistent with theory articulated here and the observation by practitioners that provision humanitarian aid can often reduce grievances and shorten conflict. As was the case in the analysis of Hypothesis 1, the results of Model 3 finds no evidence of humanitarian assistance prolonging conflict in the 48 treated pre-Cold War conflicts initiated before 1988. This could be because the amount of humanitarian aid provided by the international community was relatively small, or the non-result could be an artifact of the small sample size of treated cases.

Conclusion

Dynamic theories of conflict bargaining suggest that opponents fight in order to reduce uncertainty in a less-manipulable forum than the bargaining table by observing their opponent's costs of war and risk of military collapse over time. In these models, war is treated as a costly learning process in which sides signal their strength by enduring the costs of war. It follows that if war is not costly, sides learn nothing from engaging in it. The less costly war becomes, the longer crises will be marked by uncertainty. International humanitarian assistance—an increasingly utilized peace-building strategy—

is explicitly designed to mitigate the costs of war. This suggests that conflicts treated with greater levels of humanitarian assistance may actually last longer than those that receive less assistance. This effect, however, should only hold provided opponents are somewhat uncertain about the advantage gained from international assistance. If sides could directly observe the degree to which aid mitigated their opponent's costs of fighting, they would factor this into the terms of their settlement offers over time and converge on an agreement just as quickly as if there was no relief provided.

This article tests the observable implications of this logic and finds support for the argument that humanitarian assistance can inadvertently prolong conflict. Empirical analysis of 288 civil wars initiated since 1945 indicate that the level of humanitarian assistance is negatively correlated with the risk of civil war termination over time. Wars that receive greater levels of humanitarian assistance appear to survive longer than wars that receive little or no humanitarian assistance. Because conflicts that elicit a greater humanitarian response do so for a reason, the analysis attempts to control for factors that may be correlated with both civil war duration and the decision by aid organizations to treat crises more or less intensively. In so far as humanitarian relief responds to war-related causalities, the size of the population effected, low government capacity and economic opportunity, geographic conditions that effect access, the presence of third-party peace guarantees, and the presence of "lootable" resources, the results indicate that humanitarian assistance reduces the instantaneous risk of war terminating by nearly 10-percent. This relationship is mostly driven by civil conflicts occurring in the post-Cold War period after 1989. In this period, the instantaneous hazard rate of war termination is

nearly 12-percent lower than non-treated cases while the relationship in the period before the Cold War is insignificant.

This paper also tests the effect of uncertainty outlined in Hypothesis 2. If the informational theory outlined in this paper is accurate, it uniquely suggests that if parties could directly observe the degree to which aid mitigated opponent's costs of fighting, they should factor this advantage into the terms of its settlement offers over time and arrive at an agreement just as quickly as if there was no relief provided. Hypothesis 2 therefore tested the expectation that the tendency for humanitarian assistance to prolong conflict will be stronger under conditions where the level of aid is more difficult to observe and weaker under conditions in which it is more easy to observe. I found strong empirical support for this expectation. When the level of humanitarian aid is interacted with whether or not the primary recipients are a peripheral insurgency – conflicts in which rebels groups and primary recipients of aid primarily operate on the outskirts of a state, and are thus more difficult to observe – humanitarian aid significantly decreases instantaneous probability of war terminating while the independent effect of humanitarian aid alone when controlling for this interaction is consistently insignificant. The results suggest that aid can prolong conflict, but only to the degree that it adds uncertainty to the bargaining process

The policy implications of these findings are not immediately clear for several reasons. First, it remains to be seen just how robust the empirical relationship is. The analysis in this paper tests alternate specifications of each of measure in the model, but it has yet to systematically test the relationship between every known variable correlated

with civil war duration and the level of humanitarian assistance. A more specified model could change the estimates. Second, it is not obvious what the result says about the humanitarian assistance as a peace-building strategy. If humanitarian assistance can prolong conflict, it does so while attempting to save lives and rebuild failing states. Similar to how seatbelts have been shown to increase riskier driving while also reducing the number of driving-related deaths, humanitarian assistance may encourage more risk-seeking activity by rebels bargaining with the state while also limiting war-related deaths.

FIGURES AND TABLES

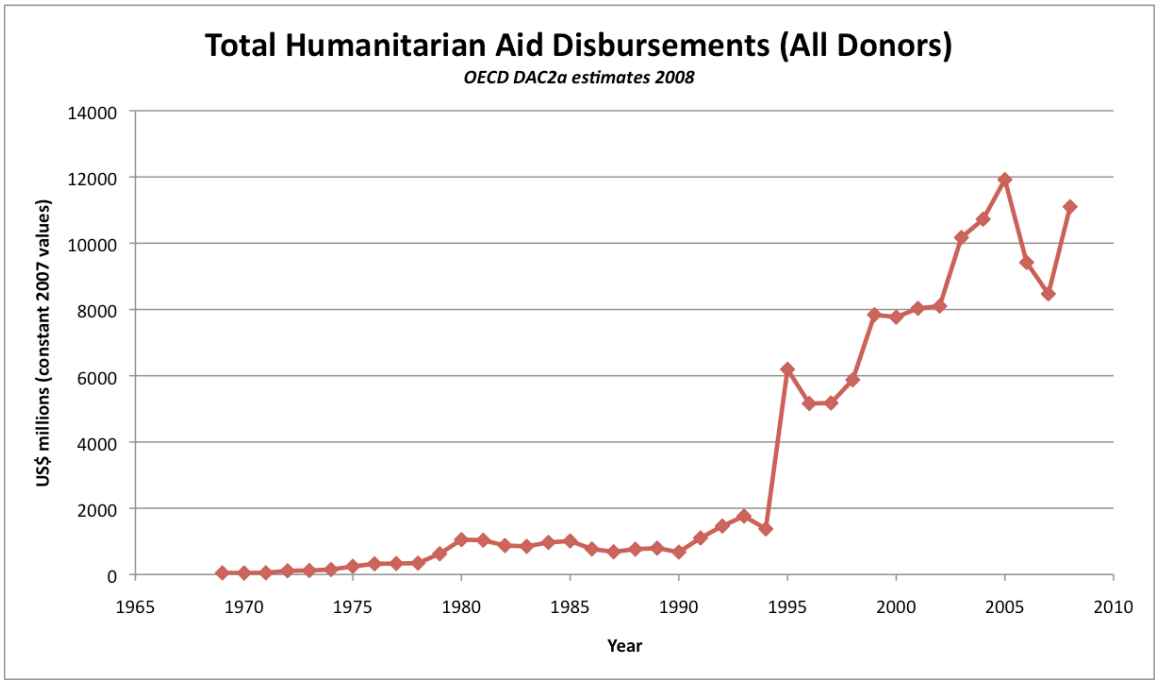

Figure 3.1: OECD DAC Estimates of Global Humanitarian Assistance Disbursed to Recipient States by Year

Table 3.1: Humanitarian Aid Level and the Risk of Civil War Termination

VARIABLES	(1) *ALL Conflicts 1949-* Haz. Ratio	(2) *Post* Cold-*War Conflicts 1989 – 2004* Haz. Ratio	(3) *Pre* Cold-War Conflicts *1945 – 1989* Haz. Ratio
Log Human. Aid	**0.9129****	**0.886****	**0.9657**
	(0.0344)	(0.04447)	(0.0526)
Lag Battle Deaths	0.9999	0.99996	0.9999
	(1.47e-05)	(2.65e-05)	(1.75e-05)
Log Population	0.9434	0.91537	0.9783
	(0.0580)	(0.07321)	(0.1024)
GDP Per Capita	1.00017	0.9999	1.000127*
	(3.63e-05)	(4.59e-05)	(7.45e-05)
Polity2 Score	0.9663**	0.96335	0.9598*
	(0.0154)	(0.0238)	(0.2148)
Diamonds	0.3819*	0.40359	0.4643
	(0.1907)	(0.2516)	(0.4123)
Drugs	0.3352**	0.4910	0.20886*
	(0.1683)	(0.3177)	(0.17571)
Resources	1.9635	2.485	1.0845
	(1.087)	(1.7548)	(1.03914)
Guarantee	2.3497**	2.0581*	24.2676***
	(0.8175)	(0.8294)	(19.234)
Mountains	1.00178	1.00505	0.9988
	(0.00307)	(0.00386)	(0.00524)
Forests	1.00409	1.00247	1.0116**
	(0.00296)	(0.00396)	(0.0082)
Observations	1308	498	810
Number of Subjects	193	119	104
Number of Failures	159	89	70

Standard errors in parentheses. ***p<0.01, ** p<0.05, * p<0.10

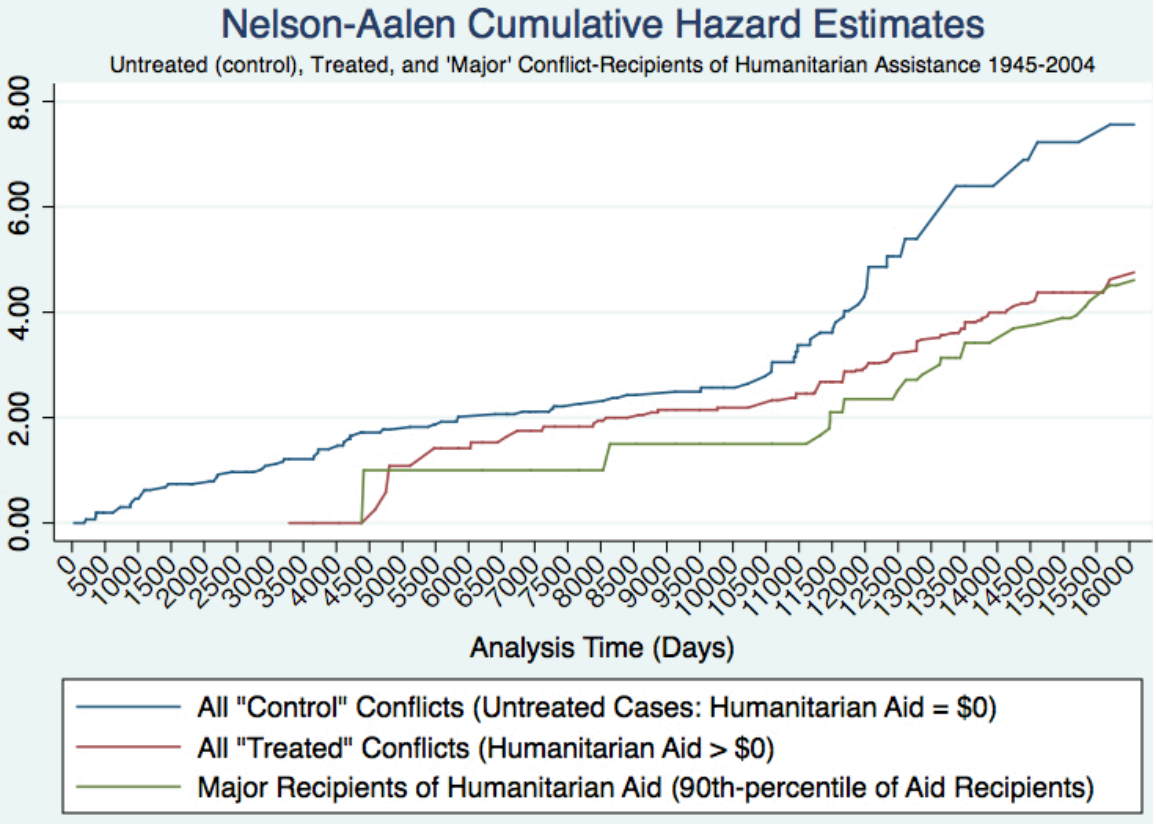

Figure 3.2: Estimated Risk of Civil War Termination for *Untreated*, *Treated* and *Major* Conflict-Recipients of Humanitarian Assistance
Note: Analysis time reported in days since civil war initiation. The earliest observations in the sample begin in 1945. Observations treated with humanitarian assistance begin in 1969, when the OECD began collecting data on the humanitarian component of Official Development Assistance. The amount of humanitarian assistance allocated to conflict areas in early observations is small, so the baseline hazard rate is unchanged.

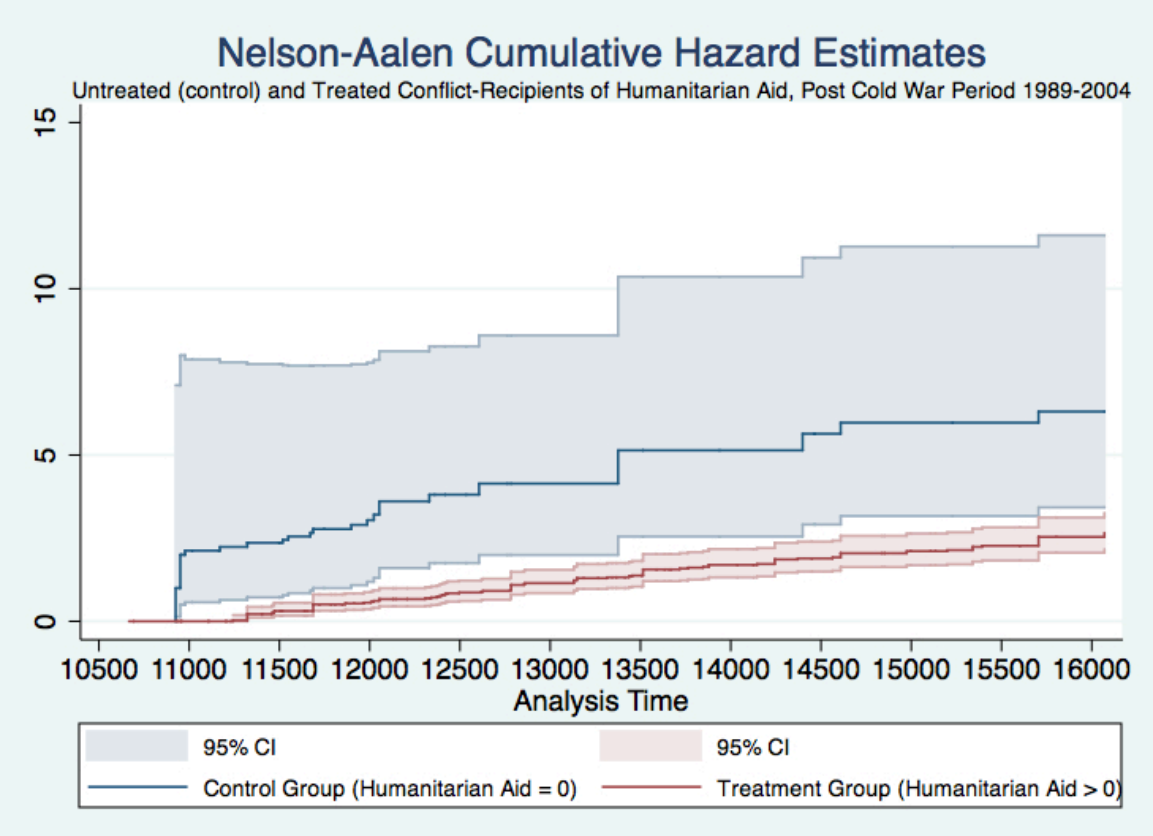

Figure 3.3: Estimated Risk of Civil War Termination for *Untreated* and *Treated* Conflict-Recipients of Humanitarian Assistance Occurring Post- Cold War (1989-)

Table 3.2: Humanitarian Aid Level and the Risk of Civil War Termination under conditions of uncertainty

VARIABLES	(1) *ALL Conflicts 1949-* Haz. Ratio	(2) *Post* Cold-*War Conflicts 1989 – 2004* Haz. Ratio	(3) *Pre* Cold-War Conflicts *1945 – 1989* Haz. Ratio
Log Human.Aid x Peripheral Insurgency	**0.4877***	**0.3640****	**1.6938**
	(0.1892)	(0.1604)	(2.1874)
Log Human. Aid	1.3054	1.4636*	0.9308
	(0.2265)	(0.3138)	(0.2754)
Lag Battle Deaths	0.9999*	0.9999	0.9994*
	(3.61e-05)	(3.18e-05)	(2.98e-04)
Log Population	0.8511*	0.8338*	0.8658
	(0.0703)	(0.0781)	(0.1849)
GDP Per Capita	1.0003	1.0000	1.0004*
	(6.36e-05)	(7.02e-05)	(2.66e-04)
Polity2 Score	0.9825	0.9699	1.0275
	(0.0240)	(0.0289)	(0.05839)
Diamonds	0.4682	0.4059	0.4856
	(0.1907)	(0.3316)	(0.5493)
Drugs	0.3352	0.3759	0.2334*
	(0.4099)	(0.3113)	(0.3086)
Resources	2.2659	4.3513	-
	(1.9669)	(3.9169)	-
Guarantee	1.4039	1.4463	42.3126
	(0.6215)	(0.7019)	(125.1699)
Mountains	1.0001	1.005	0.9803*
	(0.0039)	(0.0044)	(0.0105)
Forests	1.0033	1.0016	1.0166*
	(0.00296)	(0.0045)	(0.0092)
Peripheral Insurgency	119.9369*	1404.39**	0.0086
	(319.8238)	(4286.321)	(0.0748)
Observations	720	441	279
Number of Subjects	124	99	48
Number of Failures	91	70	21

Standard errors in parentheses. ***p<0.01, ** p<0.05, * p<0.10

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Chapter Four
Humanitarian Assistance, Bias, and the Duration of Peace After Civil War

In the aftermath of the Rwandan Civil War, nearly 1.5 million Hutu refugees crossed the borders of Rwanda into Zaire and Tanzania. Within weeks of the exodus, the international community began delivering billions of dollars in aid to provide every basic humanitarian and developmental service to the victims of the conflict.[1] Soon after relief arrived, however, it became apparent that Hutu rebels were using relief provisions – like food, water, and medical supplies – to reconstitute a war effort. Well-supplied refugee camps became de facto safe havens for Hutu fighters who used the camps as bases for rest, recuperation, and resupply as they launched cross-border attacks on Tutsi civilians. The Rwandan government eventually joined international watchdog and media organizations in accusing the United Nations High Commission for Refugees (UNHCR) and its humanitarian aid contractors of indirectly reigniting the conflict by assisting Hutu war criminals in competition with the state.[2]

The possibility that well-intentioned humanitarian relief provisions may have inadvertently undermined the prospects for peace in the aftermath of the Rwandan Genocide has been the subject of intense speculation in the decades since the Rwandan Civil War first began. At one extreme, a number of critics have claimed, more broadly,

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[1] International Donors spent $1.4 billion on relief contracts in Goma from April to December of 1994 alone. Millwood, *The International Response to Conflict and Genocide,* Vol. 3, pp.24-25. With private funds the actual amount was likely much larger over this period.

[2] Human Rights Watch/Arms Project, *Rearming with Impunity: International Support for the Perpetrators of the Rwandan Genocide* (New York: Human Rights Watch, 1995) and Gourevitch (1999)

that the modern humanitarian enterprise contributes to the very suffering it aims to redress by creating a classic moral hazard problem, wherein warring parties anticipate future aid provisions and then become emboldened to reassert their interests expecting to be relieved of their costs of war and risk of military collapse (Anderson, 1999; Gourevitch, 1999; Kuperman, 2008; Luttwak, 1999; Polman, 2010). And indeed, beyond Rwanda, such claims appear to describe the side effects of humanitarian assistance in other post-conflict situations quite well. For example, after the Cambodian–Vietnamese War in the mid-1980s, aid provisions provided to the fleeing Khmer Rouge enabled militants to fortify themselves in camps along the Thai-Cambodian border, re-constitute a fighting force, and re-visit another ten years of war and terror upon the Cambodian people.

But while humanitarian assistance appears to have been at least partially responsible for undermining peace in the aftermath of these conflicts, aid provisions have been disbursed after many civil conflicts and not all appear to have reignited like the ones in Rwanda or Cambodia. For instance, annual disbursements of humanitarian aid in Bangladesh after the 1992 ceasefire and negotiated settlement between the government and Shanti Bahini military group placed it among the top 10-percent of post-conflict recipients after the Cold War, yet there appears to have been little risk of renewed conflict in the two decades that followed. And indeed, at least one analyst in the humanitarian aid community has argued, "the empirical evidence is simply not available to warrant a focus upon humanitarian aid 'doing no harm'… In most, if not all, conflicts

the role of humanitarian aid as a source of support for warring factions has probably been slight" (Borton, 1998).

Can international humanitarian assistance inadvertently undermine peace when administered as a post-conflict reconstruction strategy in the aftermath of civil war? And, if it can, why do relief provisions appear to be associated with renewed conflict after some wars and not others? To date, conclusions about the role of humanitarian aid in undermining peace have been mixed because the evidence has been mostly anecdotal and because a satisfying theoretical link between humanitarian assistance and the duration of peace has yet to be specified (Shearer, 2000). In other words, analysts have failed to articulate a coherent mechanism through which humanitarian relief may interact with the conflict bargaining process such that sides might sometimes elect to re-initiate a violent conflict while other times they might choose to honor the previous settlement.

In this paper, I propose a theory and provide some empirical evidence for how humanitarian assistance may inadvertently undermine peace when disbursed in the aftermath of civil war. The theory follows from what I identify to be a fundamental contradiction in the global humanitarian aid model: although the principles of humanitarian assistance dictate that aid be distributed in accordance with need while remaining neutral with respect to the political stakes, these principles are prone to contradiction in the post-conflict context where need is often correlated with opponents' performance in the previous contest. In these cases, I argue, humanitarian assistance is likely to be biased towards the conflict-loser and – as a result – aid can create a revisionist party with the incentive to renegotiate the post-conflict settlement.

Importantly, however, I expect these effects to be highly conditional. I hypothesize that aid is most likely to create a revisionist party after decisive military victories where one side suffered a disproportionate share of the costs and thus exhibits a greater level of humanitarian need to be targeted by aid providers. Conversely, I expect that the effect of aid on the durability of peace after stalemates and relatively close victories will be far less significant, as competing parties are likely to exhibit similar levels of need, which in turn causes them to receive relatively similar levels of humanitarian assistance, thus leaving the post-conflict distribution of power relatively unaffected.

The remainder of this paper proceeds in five principal sections. First, I review the existing literature on the "conflict trap" and the duration of peace after civil war in order to introduce variation in the phenomenon humanitarian assistance is purported to effect perversely. Second, I utilize a crisis bargaining framework to outline a simple theoretical mechanism for how humanitarian aid can inadvertently undermine peace and I derive testable hypotheses about when I expect this conditional effect to uniquely occur. The third section describes the research design and data used to evaluate the impact of humanitarian assistance on the duration of peace. The fourth section discusses the main findings and the fifth section concludes.

Previous Literature: The Conflict Trap, the Duration of Peace, and International Interventions

Peace has proven to be extremely fragile in the aftermath of civil war. Between 1945 and 2000, there have been roughly 156 civil wars initiated between an organized rebel group and a sovereign state that ultimately ended in a period of peace.[3] Of these 156 spells of peace, 84 experienced another round of fighting between the same parties (roughly 54-percent).[4] And in the frequently-studied post-Cold War period, where the prevalence of civil war increased markedly, the record has been slightly worse, with 55 out of 95 spells of peace ending with the recurrence of conflict between the same two parties (roughly 58-percent of the cases). This latter, more contemporaneous subset of failed peace-attempts is especially discouraging because it coincides with the dramatic increase in international peacebuilding efforts by the United Nations and its member states.

The empirical tendency for civil wars to recur forms an important part of the widely studied phenomenon known as the "conflict trap" – so named after a recent Collier et al. study commissioned by the World Bank (Collier, 2003)[5]. In the study, the author's report that the typical country reaching the end of a civil war faces around a 44

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[3] The universe of cases is from Fortna (2008), which is adapted from the Doyle and Sambanis data (Doyle and Sambanis 2000; 2006) on civil wars. D&S define a civil war as an armed conflict that meets the following criteria: (a) the war has caused more than 1,000 battle deaths (b) the war represented a challenge to the sovereignty of an internationally recognized state (c) the war occurred within the recognized boundary of that state (d) the war involved the state as one of the principal combatants (e) the rebels were able to mount an organized military opposition to the state and to inflict significant casualties on the state.

[4] This estimate is contestable. The rate of recurring civil war is difficult to estimate because one must distinguish between new civil wars and civil wars that move in and out of violent phases. Following Fortna 2008, I utilize a more disaggregated coding that divides civil wars into separate cases when the parties in the conflict change or the war aims change.

[5] See (Collier 2003) page 43 for thorough discussion and literature review.

percent risk of returning to conflict within five years. The reason for this high risk, they suggest, is that the same factors that caused the initial war are usually still present after the conflict has ended. And indeed, a slew of studies have provided confirming evidence that countries emerging from civil war that had low average income (GDP per-capita) (Walter, 2004), rich natural resource endowments (Collier & Bank, 2000; Collier & Hoeffler, 2002; Doyle & Sambanis, 2000; Dubey, 2002; Elbadawi & Sambanis, 2002), hostile neighbors willing to provide rebels with external support, large population emigrations (Salehyan, 2007), and mountainous or forested terrain (Fearon & Laitin, 2003) at the beginning of a civil war are much more likely to experience further conflict if these same factors are present once it has reached peace. Even more worrisome is that conflict in the previous period can often times feed back and inflame the risks for a subsequent conflict – forming a vicious circle of civil war[6].

But there is also a second important dimension of the conflict trap. While over 50-percent of civil wars that ended ultimately recur, there is dramatic variation in the duration of peace among the spells of peace that ultimately fail. **Figure 1** illustrates this variation graphically, by plotting the distribution in the duration of peace after civil war for all conflicts that initiated and terminated between 1945 and 2004. The mean duration of peace is roughly 12.5 years, however there is considerable variation in the duration of peace among these failures (the standard deviation is roughly 15.26 years). For example, the longest peace in this period has lasted over 57 years in Paraguay, which has not

[6] For example, Walter (2004) argues that economic development affects the likelihood of a return to war because potential combatants who assess the opportunity costs of rebellion do so in generally poor economic conditions that follow a civil war.

experienced renewed conflict since 1947, while the shortest spells of peace lasted only 15 days in Yugoslavia-Croatia in November 1991 and 30 days in Sudan from April to May of 1999. And although the longest spells of peace in the top-quartile of this distribution lasted more than 15 years, the median is clearly skewed lower at 7.25 years, and the shortest spells of peace in the bottom-quartile lasted less than 1.4 years, suggesting that most instances of peace tend to be surprisingly brief.

Yet despite the fact that civil wars are prone to recurrence, the figures above also suggest some reasons for optimism. First, civil wars are by no means intractable, as many countries appear to have broken the conflict trap entirely. For instance, countries like Nicaragua, El Salvador and Guatemala were all devastated by civil conflict, yet each has managed to avoid a second civil war (Walter, 2009). And in general, nearly half of the civil wars that have terminated since the end of World War II have not experienced a recurrence of civil war: 29 of the 61 post-conflict states emerging from civil war between 1945 and 1989 have not recurred, while 40 out of the 95 civil wars that terminated after 1989 have not experienced renewed conflict. What's more, these figures also suggest an additional cause for optimism with respect to the second dimension of the conflict trap. As Fortna notes, it seems unreasonable to consider the resumption of war in Rwanda in 1990 after 26 years of peace to be just as much a failure as the renewed fighting in Rwanda in 1994 after less than a year of peace. Indeed, the fact that peace lasted for decades in some cases before failing suggests that some underlying factors or some international peacebuilding measures may effectively function to prolong peace and delay violent a conflict.

Why is it, then, that peace can last after some civil wars and not others? And among those that ultimately fail, what explains why some spells of peace were particularly durable while others failed relatively quickly? These questions have important implications for academics and policymakers. For academics, civil war recurrence provides another interesting and puzzling dimension along which scholars can investigate the causes and consequences of civil war. If war can be understood to result from bargaining failure due to problems of private information and credible commitment, it seems reasonable to believe that the previous conflict must have resolved enough of these uncertainties for war to terminate in the first instance. Yet, if war tends to recur after combatants have at least tacitly agreed to stop fighting, something exogenous to the initial crisis must shift in the post-conflict environment for combatants to prefer costly-fighting to peace. And for policymakers, understanding when and how incentives may shift in the post-conflict environment is the first step in designing more effective and targeted peacebuilding and post-conflict reconstruction strategies.

Existing scholarly research attempting to explain civil war recurrence and the duration of peace have fallen into roughly two categories. The first category investigates whether peace is more or less likely to endure depending on the strategic context at the time war terminated. For example, we know peace is harder to maintain when war ends in a stalemate or compromise settlement than if one side achieves a *military victory* (Dubey, 2002; Fortna, 2004; Licklider, 1995; Maoz, 1984; Stinnett & Diehl, 2001; Toft

& Security, 2006).[7] This may be because indecisive military outcomes leave all sides capable of resuming the fighting and no side fully satisfied with the terms of the peace, whereas decisive military victories generally leave at least one side incapable of challenging the outcome and both sides with relatively little uncertainty about who would win in another round of fighting.

Relatedly, studies by Doyle & Sambanis (2002), Hartzell et al. (2001), Walter (2004), Fortna (2004), and Dubey (2004) have all found that the *duration of a civil war* is significantly related to the post-war peace. Similar to the logic outlined before, this may be because the longer the first war, the more opportunity combatants had to gather information about their opponent and correctly calculate the risks and costs of future wars. There is also mixed evidence that the *costs of war* – in terms of lives lost – effects the durability of peace after civil war. For example, Doyle and Sambanis (2000) and Dubey (2002) have both found that civil wars with higher death tolls are more likely to resume than less deadly conflicts, perhaps because either deadlier conflicts are symptomatic of more intractable disputes or that higher death tolls make it more difficult for sides to reconcile with their adversaries. But other evidence appears to suggest the opposite relationship (Fortna, 2004; Hensel, 1994; Werner, 1999), which also seems reasonable given that higher costs of war should provide more information about the likely outcome of a second war while increasing the incentive to avoid bargaining failure.

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[7] For example, Licklider (1995) finds that 50% of negotiated settlements broke down into renewed war, in contrast to only 15% of decisive military victories.

Finally, there is conflicting evidence that *ethnic divisions* within a state affect its probability of experiencing civil war both in the first instance and the recurrence of civil war in the aftermath of conflict. It seems intuitive that peace might be harder to keep after conflicts that matched different ethnic or religious groups against each other, particularly if the previous contest served to inflame these differences. However, while Licklider (1995) and Doyle and Sambanis (2000) found identity wars to be more likely to resume than others, Hartzell et al. (2001) and Dubey (2002) found no significant difference. And along these same lines, Doyle and Sambanis (2000) have found that complicated wars involving *multiple factions* have proven harder to solve in a lasting way than wars with only two sides.

The second category of research has sought to determine whether the international community can effectively discourage the resumption of violence by investigating the impact of various third-party interventions on the durability of peace. For example, following Walter's (1997) finding that civil wars are more likely to end in a negotiated settlement when an outside third-party explicitly promises to guarantee the safety of the belligerents, Fortna (2004) found that international peacekeeping missions are associated with longer spells of peace after controlling for factors that might influence the degree of difficulty of a particular case, and that this positive relationship between the peacekeeping and the duration of peace is especially strong in post-Cold War period after 1989. Fortna's results largely confirm previous findings by Doyle & Sambanis (2002)

and Hartzell et al. (2001) that international peacebuilding missions can effectively keep peace.[8]

However, within this category of peace research, various studies have found surprising results for other international peacebuilding measures. For example, there is some evidence that while third-party mediation has a strong short-term impact on decreasing the likelihood a crisis will recur in the first few years, it may have the perverse effect of increasing the long-term probability of crisis recurrence, suggesting that recipients of mediation more often become dissatisfied with settlements when their capabilities and resolve change and the artificial incentives from the mediator diminish (Beardsley, 2008). Similarly, there is growing systematic evidence that although refugee camps established by third-parties like the UNHCR and humanitarian NGOs provide valuable assistance to vulnerable populations, larger refugee diasporas in neighboring states appear to be associated with longer civil conflicts in refugee-sending states, implying that rebels may utilize refugee camps as external bases to organize and fight transitionally in places where state strength is limited (Salehyan, 2007). Together, these findings may support the claim by Luttwak (1999) that if the international community ultimately seeks a lasting peace, an optimal, albeit unpleasant, strategy may be to simply "give war a chance".

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[8] Fortna's study also presents a significant methodological innovation relative to these previous studies by estimating the impact of peacekeeping on a continuous measure of peace duration using survival analysis, rather than estimating the likelihood that peace still exists at some discrete time arbitrarily chosen (i.e. two years after war termination, five-years after, etc.)

That some third-party interventions may actually have the perverse side effect of undermining a lasting peace is the issue this study seeks to investigate further. It is generally assumed that the provision of humanitarian relief is an effective means by which the international community can assist the victims of violent conflict. As a result, the provision of humanitarian assistance has rapidly become a core component of modern peacebuilding and post-conflict reconstruction. Figure 2 demonstrates that in just the two decades since the end of the Cold War, the amount of humanitarian aid reported by donor governments through the OECD Development Assistance Committee (DAC) has increased nearly 1400-percent in real terms from $796 million USD in 1989 to well over $11 billion USD in 2008. This policy is motivated by the belief that individuals struggling in conflict areas have the fundamental right to life sustaining resources and protection of their basic human rights as codified in the Geneva Conventions (Smillie & Minear, 2004).

Yet, as noted earlier, policymakers and practitioners have grown increasingly weary of the negative side effects generated by even well intentioned humanitarian assistance (Anderson, 1999; Gourevitch 1999; Luttwak, 1999; Terry, 2002; Kuperman 2008; Polman 2010). Basic resources like food, water and medical supplies may certainly help mitigate suffering by limiting causalities, preventing famine and disease, and caring for displaced populations, but if these short-term benefits can inadvertently undermine the long-term prospects for a lasting peace, then the very treatment that the international community has been employing to address the consequences of violent conflict may actually be undermining peace and increasing the amount of suffering over time.

However, this does not imply that policymakers should simply give up on humanitarian aid all together. If the negative effect of aid on peace duration is conditional – as the theory and evidence presented in this paper suggest – then it may be possible to allocate humanitarian relief across post-conflict areas in a way that limits this negative side effect. Of course, this requires a better understanding of the mechanism through which humanitarian assistance may interact with the conflict bargaining process such that sides would sometimes elect to re-initiate a violent conflict while other times they would choose to honor the previous settlement.

A Theory: Humanitarian Aid and Conflict Bargaining in the Aftermath of Civil Conflict

Existing claims about the effect of humanitarian assistance on the durability of peace after civil war emphasize the role of relief provisions in creating a revisionist party with the incentive to renegotiate the post-war settlement. The theory here aims to qualify this claim in an important way by demonstrating that this tendency is in fact quite conditional. Specifically, I argue that the tendency for aid to reignite conflict depends critically on the nature of the post-conflict settlement. Aid is *least* likely to undermine peace when the previous contest ended short of a decisive victory: with a temporary truce, negotiated settlement or military stalemate. By contrast, humanitarian assistance is *most* likely to reignite conflict and undermine peace in post-conflict environments where the previous contest ended with a decisive military victory for one side (either the government military or the rebel army).

The remainder of this section develops this argument in two parts. First, I describe a baseline bargaining model of war initiation in the aftermath of civil conflict in order to identify the proximate causes of civil war recurrence. Second, I describe a mechanism through which humanitarian assistance may interact with this conflict bargaining process to generate a greater risk of bargaining failure after civil war under certain conditions.

Reaching Peace and Returning to Civil War

Fearon (1995) outlined what is now generally regarded as the standard bargaining model for the occurrence of war. The model suggests that coherent rationalist explanations for war will fall into one of two categories: sides can fail to reach a peaceful negotiated settlement that avoids the costs of war because they have private information with incentives to misrepresent or because sides are unable to credibly commit themselves to follow through on the terms of the agreement. According to the first explanation, sides have private information about their own capabilities and resolve and they have an incentive to misrepresent their ability on these dimensions to their opponent in order to secure a better settlement. As a result, while the costs of fighting open up a range of negotiated settlements both sides should prefer to war, war can occur in equilibrium because parties seek to resolves uncertainty in a less-manipulable forum than the bargaining table before agreeing to terms prematurely (Filson & Werner, 2002; Filson & Werner, 2007; Powell, 2004; Slantchev, 2004; Smith & Stam, 2004; Wagner, 2000). The second explanation is more straightforward: sides may prefer to fight now if

certain elements of the strategic environment make it so that their opponent is unlikely to honor a negotiated settlement in the future (Fearon, 1998; Fortna, 2003; Leeds & Brett, 2000; Walter, 1997).

Importantly, in the post-conflict context, the very fact that belligerents successfully terminated the first war indicates that these issues must have been sufficiently resolved for peace to be obtained in the first place. In other words, if either party believed that they could have secured a better deal by continuing to fight and learn about their opponent's true capabilities and resolve rather than settle, the war would never have terminated. Similarly, if either party in the contest believed the other would not honor the distributional terms of the agreement sometime in the future, it is unlikely they would have chosen to end hostilities.

Therefore, following Werner (1999), my starting assumption is that the peace settlement ending a civil war contains a division of the disputed good which – either explicitly through a formal treaty or implicitly through the ending of hostilities – reflects what the belligerents agree the balance of power represents, and that this common understanding of the balance of power was reached through the information provided by fighting during the war. Thus, the peace agreement that represents the post-conflict status quo is fundamentally an implicit or explicit settlement that details how the war ended, and this agreement implies that parties solved the initial problems of private information and credible commitment.

What, then, would cause the resumption of conflict if the very presence of a post-conflict period of peace implies a mutually agreeable settlement relative to which the

continuation of fighting appeared inefficient to all parties? Or rather, what conditions would suddenly create a party that is dissatisfied with the status quo? One clear source of dissatisfaction would be a change in the distribution of power after the initial peace was struck. The post-conflict settlement could easily become untenable if expectations over the likely outcome of war change. That is, if at least one party suddenly believes that challenging the post-conflict status quo would result in a better outcome than the one they initially agreed to when they ended hostilities, this could create an incentive for a belligerent to challenge in an attempt to renegotiate the distribution of benefits, which, in turn, increases the risk of war in the event that sides are unable to strike a negotiated settlement.

Notice that the logic here requires the underlying *distribution* of power must change sufficiently relative to the underlying distribution of benefits represented by the post-conflict status quo. This is important for, as Powell (1999) notes, if the distribution of benefits continues to reflect the distribution of power, both parties should remain satisfied and there is no incentive for either to challenge the pre-existing settlement because neither would benefit form the use of force (the risk of war is zero). By contrast, if a once weak party grows stronger for exogenous reasons, that actor may become dissatisfied with the existing settlement and demand that the status quo be revised in its favor. Should these demands go unmet, the rising party may resort to force in order to impose a new, more favorable settlement if it is relatively willing to use force (fighting is less costly). This distinction is critical because it makes clear that exogenous shifts in power are not sufficient to undermine peace. Rather, power transitions are only

dangerous if they asymmetrically increase the power of one party relative to another sufficiently such that it becomes dissatisfied with the existing distribution of benefits and challenges.

But even this situation will not necessarily lead to conflict. Shifts in the distribution of power could also lead to a new negotiated settlement that allows both parties to avoid the high costs of war (Fearon 1995). Under complete information, the declining party should revise its expectations about the likely outcome of war and offer greater concessions to the rising party. However, it is important to note that while large changes in the distribution of power are indeed never sufficient to generate war with certainty, they can increase the risk of war by creating a dissatisfied state. Power transitions thus create the preconditions for bargaining failure by creating a dissatisfied party willing to spark a crisis.

This logic provides a framework for identifying a set of conditions under which a humanitarian assistance may interact with the bargaining environment to increase the risk of war. If the benefits of humanitarian aid generate a sufficient disparity between the post-conflict distribution of power and the distribution of benefits it may create a revisionist party with the incentive to challenge the post-conflict status quo.

The Role of Humanitarian Aid

The idea behind humanitarian assistance is straightforward in its simplicity: individuals struggling in the context of natural and "complex" emergencies like civil war

have the right to life sustaining resources and protection of their basic human rights (Smillie & Minear, 2004). In practice, the global allocation of these resources across crises is explicitly driven by three foundational principles codified in the Geneva Conventions: impartiality, neutrality, and independence. These principles have become the industry standard for the humanitarian enterprise, as they constitute the most broadly accepted principles governing the provision of relief worldwide.

At their core, the distributional principles above are designed to ensure that the "humanitarian imperative" – the fundamental notion that humanitarian assistance be provided in proportion to on need alone (not other factors such as political or strategic interest, cultural affinity, or availability of resources) wherever it is most needed – remains the most significant determinant of humanitarian action worldwide. The principle of 'impartiality' requires that assistance be provided without regard to nationality, race, religion, or political point of view. This is meant to ensure that need is assessed equally across all parties in a crisis. The principle of 'independence' requires that humanitarian agencies formulate and implement a response independently of government interests. This is meant to limit donors from dictating the allocation of humanitarian assistance in order to further their own foreign policy goals. Finally, the principle of 'neutrality' requires that humanitarian agencies not take sides in hostilities or engage in any ongoing political, racial, religious, or ideological controversies within a crisis area. This is designed to avoid agencies furthering the interests of one party over another in an armed conflict.

It turns out, however, that the humanitarian idea is much more complex in practice. As Gourevitch notes, "The scenes of suffering that we tend to call humanitarian crises are almost always symptoms of political circumstances, and there's no apolitical way of responding to them – no way to act without having a political effect". On this point, for instance, Duffield (1994) and Anderson et al. (1998) have both argued that humanitarian relief provisions in conflict-affected states have often filled so great a proportion of civilian needs for public services that significant local resources have been freed up and reallocated towards the war effort. And indeed, Luttwak (1998) has argued that the Palestinian peace process has been repeatedly undermined for exactly this reason: humanitarian assistance provided through the UN has insulated the Hamas leadership from the demands of governing while fighting. Separately, Terry (2002) has argued that protected aid enclaves have inadvertently prolonged conflict by shielding warring factions from the costs of sustaining causalities, a tendency clearly illustrated by the refugee camps on the Thai-Cambodian border and the safe-zones set up during the Bosnian Civil War (Boyd, 1995; Landgren, 1995; Woodward & Institution, 1995). And, finally, several analysts have argued that humanitarian relief provisions have facilitated conflict by directly providing food, medical supplies, and logistical support to the frontlines (Anderson & Duffield, 1998; Atkinson, 1997; Cooley & Ron, 2002; Gourevitch, 1999; de Waal, 1994).

It is interesting to note that each of these cases also illustrates a fundamental contradiction in the core principals of humanitarian action. In each conflict, relief provisions tended to target the weaker or defeated party for whom the war was relatively

more costly. While this is certainly consistent with the humanitarian imperative to provide assistance in proportion to need, it contradicts the core principle of neutrality, which requires that humanitarian agencies not take sides in hostilities by furthering the interests of one party over another. Achieving impartiality and neutrality, it seems, is impossible because humanitarianism is bound to asymmetrically relieve warring parties of the burdens attached to war.

This contradiction has particularly important implications in the post-conflict context, where need is generally correlated with opponents' performance in the previous contest. Following the bargaining logic outlined above, it is reasonable to expect that in post-conflict contexts where peace was established following a decisive military victory by either the government or a rebel group, the suffering that humanitarian relief is explicitly designed to mitigate will be disproportionately concentrated with the losing party (and its primary constituency) that experienced higher costs from the prior conflict. In these cases, aid may create a revisionist party by shifting the distribution of power sufficiently in the conflict-loser's favor relative to the distribution of benefits represented by the post-conflict status quo. However, when humanitarian assistance is provided in peace spells following civil wars that did not end decisively, aid should create less risk of peace failing because the costs of war to each party will be more equal. In these cases, the provision of relief will also be distributed more equally, and will not substantially alter the distribution of power relative to the post-conflict distribution of benefits. Both parties should remain relatively satisfied and there is less incentive for either to challenge. Hence, I arrive at two related hypotheses:

Hypothesis 1: *Increasing humanitarian aid disbursements following civil wars that ended in a decisive military victory for either the government or rebel group will be associated with a higher risk of peace failing*

Hypothesis 2: *Increasing humanitarian aid disbursements following civil wars that did not end with a decisive military victory will have little or no effect on the risk of peace failing.*

To be sure, both the government and rebel group should still prefer to reach a new settlement that avoids the destruction of resources from a second civil war. Why, then, might the provision of aid following a decisive victory not lead the declining party to update its beliefs and offer a revised agreement? A simple point clarifies these expectations. Recall that the logic here posits that increasing humanitarian disbursements after decisive military victories will increase the *risk* of peace failing by sparking a new crisis. That is, humanitarian relief can increase the opportunity for bargaining failure by generating a dissatisfied party even if the reasons a crisis *ultimately* ends in war are still problems of private information and credible commitment.[9]

Research Design: Data and Methodology

This section describes a research design for evaluating the impact of humanitarian assistance on the duration of peace after civil war. The dataset used to define the universe of cases (peace-spells) is drawn from Fortna (2008), which encompasses all cease-fires of at least one month in civil wars (as defined by Doyle and Sambanis 2000;

[9] In this way, the theory presented here is probabilistic rather than deterministic.

2006) between January 1, 1989 and December 31, 1999. The key dependant variable of interest is *the duration of peace*, defined from the date on which a civil war terminated to the date fighting resumed.[10] Note that the data codes peace as failing if a new war occurs in the same country involving the same or similar parties, not if another war occurs in the same country between substantially different actors. In each case, the duration of peace after a civil war is observed through the end of 2004, after which the duration of peace is considered censored.

Including only cases of peace that started between 1989 and 2000 has several advantages. First, with respect to the beginning of the observation period in 1989, recent research has shown that the end of the Cold War represents a major structural break in the data generating processes for both the dynamics of civil war (Fearon, 2004) and international interventions (Gilligan & Sergenti, 2008). Indeed, Figure 2 from above demonstrated that global disbursements of humanitarian assistance also increased dramatically in the period after 1989. Second, with respect to the end of the observation period, some of the key control variables correlated with both the duration of peace and the implementation of peacebuilding measures are unavailable for civil wars that started after 2004. Setting the cut-off for inclusion to require a break in fighting occur before 2000 allows us to observe whether peace lasts for at least five years after the point of a cease-fire for all cases (Fortna 2008).

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[10] In most cases, the precise date of a cease-fire agreement or the date of large-scale return to fighting is known, but in cases where the exact date of a cease-fire agreement or the date of resumed fighting is unknown, the dataset codes specific date that is approximated according to the best information available. For example if research indicates a cease-fire in May, the dataset codes May 15 as the start date of peace. Or if research indicates hostilities began again at the end of May, the dataset codes May 31.

To test the effect of humanitarian assistance on the duration of peace in different post-conflict settings, I employ duration models (also known as hazard or survival models) to estimate the effect of humanitarian assistance—a time varying covariate—on the instantaneous hazard rate, or underlying risk, of peace failing in a particular year. I employ both Cox proportional hazard models and Weibull models. The Cox model estimates the probability that a peace-spell will fail at time *t* based on a set of covariates and given that the peace has survived until *t*, without making assumptions about the shape of the hazard function over time[11]. The Weibull model makes the same estimation and can be preferable in relatively small datasets, however it makes the more restrictive assumption that the baseline hazard rate in monotonically rising or falling over time (i.e peace becomes either hard or easier to keep over time). In the analyses that follow, I estimate various specifications using both models to check if the results are robust to the different assumptions.

Data used to estimate the amount of humanitarian assistance disbursed in each post-conflict peace year is taken from the OECD DAC (Organizations for Economic Co-operation – Development Assistance Committee) data on Official Development Assistance (ODA). Within the overall definition of ODA, humanitarian aid is defined as "assistance designed to save lives, alleviate suffering and maintain and protect human dignity during and in the aftermath of emergencies. To be classified as humanitarian, aid must be consistent with the humanitarian principles of humanity, impartiality, neutrality and independence." This includes relief coordination, protection, support services and

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[11] The baseline hazard rate of conflict terminating may be constant, decreasing, increasing, decreasing and then increasing, increasing and then decreasing, or anything else as war continues over time.

material assistance like food and medical supplies. Disbursements record the actual international transfer of financial resources, or of goods and services, valued at the cost to the donor over a given accounting period less repayments of any loan principal or interest over that period. The data includes, (i) bilateral humanitarian aid disbursements from DAC member countries, (ii) aggregated non-DAC member disbursements, and (iii) aid activities financed through multilateral institutions' regular budgets and international NGOs. The last category is counted in the multilateral outflows component of the total humanitarian aid disbursement only if a contribution to an agency is pooled with other contributions and disbursed at the discretion of the agency.[12] Projects executed by multilateral organizations on behalf of donor countries are counted as bilateral flows, since it is the donor country that effectively controls the use of the funds. The DAC list of potential recipients includes all developing countries and territories eligible for receiving ODA from 1969-2009. This includes virtually every state in the international system that was not a member of the DAC in that year. Currently there are 24-members of the DAC based on their status as highly developed states.[13]

In this study, disbursements of all bilateral and multilateral aid are aggregated for a total estimate of humanitarian assistance disbursed in recipient-state for each year of

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[12] The data includes humanitarian aid outflows from World Bank, the regional development banks and several UN agencies, including the UNHCR, UNAID, UNDP, UNRWA UNICEF, World Food Program, UNTA, UNFPA, and several others. The list also includes disbursements from over 50 of the largest International NGO's. Full list at http://www.oecd.org/dataoecd/36/16/31724727.pdf

[13] The DAC 24 members as 0f 2010: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Korea, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, United States and the Commission of the European Communities. In the period under observation, membership changed seven times with New Zealand (1973), Finland (1975), Ireland (1985), Luxmbourg (1992), Portugal (1991), Spain (1991) Greece in (1999).

peace. Humanitarian aid disbursements are recorded in constant 2007 prices and exchange rates. This means adjustments have been made to cover both inflation in the donor's currency between each year and the reference year, and changes in the exchange rate between that currency and the United States dollar over the same period. In the analysis below, I also log-transform these values because the variances are not homogenous: most observations receive no humanitarian assistance, many observations receive small amounts, and a few observations receive very high levels of humanitarian assistance. The transformation yields a more normal distribution closer to the assumptions of parametric statistical tests.

In total, the sample for this study include 94 cease fires, or breaks in fighting, from 1989 through 1999 that lasted at least one month in almost 60 civil wars. Splitting each of the 94 spells of peace into calendar years over which the level of humanitarian assistance can vary produces 560 unique peace years under observation. Of these 94 cases, 54 failed with the recurrence of civil war between the same two parties.

Endogeneity Bias and Selection Effects

The provision of humanitarian assistance across post-conflict states is almost certainly not random. Aid organizations and donor governments are likely to make allocation choices based on where humanitarian assistance is most needed or where it is likely to be most effective. This non-random selection raises two important concerns for estimating the impact of humanitarian assistance on the duration of peace after civil war.

First, the level of humanitarian assistance may be endogenous to the duration of peace. If the amount of humanitarian assistance in any period is determined based on donors' expectations for a short or long peace, then any relationship between the level of humanitarian aid and the durability of peace observed in the data may be flowing from the latter to the former. In other words, humanitarian aid may not be undermining peace itself, but responding more or less intensively depending on donors' expectations of when a civil war is likely to recur. Second, the level of humanitarian assistance may be selected based on other observable indicators of conflict that are themselves correlated with the durability of peace after civil war. For example, it may be the case that aid organizations are choosing to allocate humanitarian assistance more or less intensively across post-conflict states based on the number of refugees or the number of casualties produced as a result of the previous conflict. If these variables are then correlated with the duration of peace (as the literature reviewed about suggests), omitting them from the analysis could lead to underspecified models with biased estimates.

Both forms of strategic selection may bias our estimate if not properly controlled. However, in the case of humanitarian assistance, I argue that it is unlikely that donors make allocation choices in a given year based directly on the duration of peace (endogeneity bias). This is because the amount of humanitarian assistance distributed each year following a civil war must be determined prior to observing if and when a peace actually fails. In other words, the prognosis for a peace-spell is not directly observed in the post-conflict period prior to the actual resumption of conflict. Aid organizations can, however, provide more or less assistance based on beliefs about how

likely a certain peace will last, but these expectations must be formed indirectly based on observable indicators (or symptoms) that are correlated with the duration of peace. If this is the case, the endogeneity problem essentially becomes a selection problem where the level of aid observed in any post-civil conflict year is determined with respect to observable factors that are either known or unknown to be correlated with the duration of peace.

To limit bias from strategic selection in the estimates, the ideal statistical analysis would include any variables that are correlated with both the amount of humanitarian aid allocated to a conflict in a given year and the likelihood of the peace continuing past that year. These variables must also be causally prior to the treatment to avoid post-treatment bias (King & Zeng, 2006; King & Zeng, 2007; Rosenbaum, 2002). If, after controlling for these factors, the relationship between humanitarian aid and the duration of peace is still greater than the baseline survival time, we can be more confident that the results support the expectations of the theory.

To account for potential confounding variables and to understand how the effect of humanitarian assistance compares to other common predictors of the duration of peace after civil war, I include a variety of control variables commonly used in the peacebuilding literature (Doyle & Sambanis, 2000; Fortna, 2004; Gilligan & Sergenti, 2008). In addition to including the two individual components for the interaction term of interest (total humanitarian aid disbursements-logged and whether the previous war ended in decisive victory), I control for the presence of lootable resources, a peace treaty, whether the prior conflict was an identity war, the number of war related deaths, the

number of factions in the prior war, the level of democracy at the end of the war, the infant mortality rate after the war, whether there was a third-party guarantee, the government army size, mountainous terrain, whether the state was contiguous with a member of the P-5 or a former P-5 Colony, and a measure for the duration of the civil war. [14]

Results and Discussion

This section discusses the main findings from the statistical tests outlined above. **Table 1** begins by estimating the relationship between the amount of humanitarian assistance disbursed in each peace-year following a civil war and the risk of peace failing in the full sample of 94 cease-fires beginning in 1989 – making no distinction yet between cases that ended in a decisive victory and cases that did not. Recall that the overall effect of humanitarian assistance on conflict recurrence is mixed based on the anecdotal evidence in the policy literature. In some cases of humanitarian aid appeared to create a revisionist party – which led to the recurrence of civil war – while in other cases it did not. My own theoretical expectations are agnostic with respect to an average effect, positing instead that the tendency for aid to undermine peace will be strongly mediated by the conditions under which the previous civil war ended.

Table 1 displays the results for seven different model specifications, each estimating the relationship between humanitarian aid disbursements and the risk of

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[14] For a complete discussion of these variables and why they should be included see Gilligan (2008)

conflict recurrence over time. In all cases, hazard ratios are reported rather than the familiar coefficient estimates from standard linear or logistic regressions. Hazard ratios are interpreted relative to 1, where hazard ratios greater than 1 indicate variables that increase the risk of peace failing over time and hazard ratios less than 1 indicate variables that decrease the risk of peace failing over time. For example, if the results indicate that a dummy-variable has a hazard ratio of 0.5, that variable decreases the risk of peace failing by 50-percent, meaning it tends to be associated with longer peace after civil war. Conversely, if a variable has a hazard ratio of 2, it doubles the risk of peace failing, meaning it tends to be associated with shorter peace after civil war.

Regardless of model specification, there appears to be no significant relationship between the amount of humanitarian assistance provided after a civil war, and the risk of peace failing with the recurrence of conflict. The results of Model 1, which includes the full list of covariates discussed above, suggest that post-conflict states that received higher amounts of humanitarian aid relief were at no greater risk of relapsing into a second civil war. This non-effect is stable across various model specifications that gradually remove control variables to check for co linearity. Model 2 excludes controls for the post-conflict recipient's relationship to the permanent-five members of the UN Security Council that hold disproportionate control over the allocation of international peacebuilding measures, and models 3-7 demonstrate that the effect of humanitarian aid is statistically indistinguishable from 0 (a baseline hazard ratio of 1) when removing controls for the recipients' level of democracy, infant mortality rate, mountainous terrain, government army size, and presence of past peace agreement. If humanitarian aid can

undermine peace after civil war, there appears to little evidence of such an effect from a general analysis estimating the impact of aid on the duration of peace across all post-Cold War cases.

The theory above, however, suggested that the tendency for aid to reignite conflict will depend critically on the nature of the post-conflict settlement, positing that aid is unlikely to undermine peace when the previous contest ended short of a decisive victory and highly likely to reignite conflict when the previous contest ended with a decisive military victory for one side. It is possible the analysis above disguises this effect by lumping these two types of post-conflict environments together, creating greater variance in the estimated effect of aid on average.

Table 2 provides a more direct test of the hypotheses derived above. Here the key variable of interest is the interaction between the level of humanitarian aid and the way the previous civil war ended – with either a decisive military victory for the government or rebel group (coded 1) or not (coded 0 for a military stalemate or truce). As before, I run seven different model specifications – each estimating the relationship between humanitarian aid disbursements and the risk of conflict recurrence over time using a Cox proportional hazard model – but this time interacting the level of aid with decisive victory.

The results provide support for the theory. Regardless of model specification, increasing humanitarian aid disbursements following civil wars that ended in a decisive military victory for either the government or rebel group is associated with a higher risk of peace failing. The hazard ratio on the interaction term in Model 1 is 2.241 and is

statistically significant at the 5-percent level (p=0.046). More substantively, this means for every 1-unit increase in the log-value of humanitarian aid disbursements, the risk of peace ending with a second civil war more than doubles. Gradually dropping covariates as a robustness check in Models 2-7 produces roughly the same size coefficient estimate for the hazard ratio of the interaction term, though the significant level drops slightly to the 10-percent level (Model 2 p=0.10, Model 3 p=0.098, Model 4 p=0.091, Model 5 p=0.092, Model 6 p=0.09, Model 7 p=0.085).

Also consistent with the theory, the coefficient estimate for humanitarian aid disbursements alone - controlling for whether or not relief provisions were disbursed following a decisive victory – appears to be associated with little or no additional risk of peace failing. In other words, aid provided in post-conflict areas following non-decisive victories has no statistically significant effect on the risk of peace failing after civil war, as the hazard ratio on total aid disbursements is statistically indistinguishable from the baseline rate of 1.

Table 3 estimates the effect of the interaction across the same seven specifications using a Weibull model for the small sample size. In all cases, the direction, magnitude and significance of the estimated hazard ratios remain relatively unchanged. The risk of peace ending with a second civil war increases from 67-percent in the lowest estimation (Model 2) to 84-percent in the highest estimation (Model 1) as function of a 1-unit increase in the log-value of humanitarian aid disbursements, while the significance level on the hazard ratios actually increases slightly (Model 1 p=0.032, Model 2 p=0.069,

Model 3 p=0.073, Model 4 p=0.071, Model 5 p=0.074, Model 6 p=0.064, Model 7 p=0.066).

The hazard ratios on many of the other covariates are also largely consistent with the previous literature. Decisive military victories, peace treaties and the duration of the previous war appear to be robustly correlated with a lower risk of peace failing, while the presence of lootable resources appears to be robustly correlated with a higher risk of peace failing.

Conclusion

Within the growing literature on humanitarian aid, the UN border camps set up after the Rwandan genocide figure as the ultimate example of humanitarianism corrupted. As Hutu genocidaires manipulated and leveraged humanitarianism towards extreme acts of inhumanity, they exposed many weaknesses in the current humanitarian system. Chief among these was the possibility that the modern humanitarian enterprise might inadvertently contribute to the very suffering it aims to redress by providing the resources and international protection needed to reconstitute a war effort. To the degree that such claims are true, it does not bode well for the overall humanitarian enterprise.

The purpose of this paper was two fold. First, I sought to discipline existing claims about the relationship between humanitarian assistance and the durability of peace by outlining a theoretically coherent mechanism through which humanitarian aid may interact with the conflict bargaining process such that sides might sometimes elect to re-

initiate a violent conflict while other times they might choose to honor the previous settlement. Drawing on the bargaining model of war, I argued that humanitarian aid should only create a revisionist party after civil wars that ended in a decisive military victory. Under these conditions, aid will asymmetrically increase the power of one party relative to the other because of a fundamental contradiction in the humanitarian model. Although the principles of humanitarian assistance dictate that aid be distributed in accordance with need while remaining neutral with respect to the political stakes, need in the aftermath of conflict is generally correlated with opponents' performance in the previous contest. As a result, aid is most likely to create a revisionist party after conflicts where one side suffered a disproportionate share of the costs and thus exhibits a greater level of humanitarian need to be treated by aid providers.

Second, I sought to determine if there was any systematic empirical evidence that international humanitarian assistance can undermine peace when administered as a post-conflict reconstruction strategy in the aftermath of civil war. Consistent with the theoretical expectations outlined in this paper, I found that humanitarian assistance is most likely to reignite conflict and undermine peace in after civil wars that ended with a decisive military victory for one side (either the government military or the rebel army), and least likely to undermine peace after civil wars that ended short of a decisive victory – with a temporary truce, negotiated settlement or military stalemate.

Figures and Tables

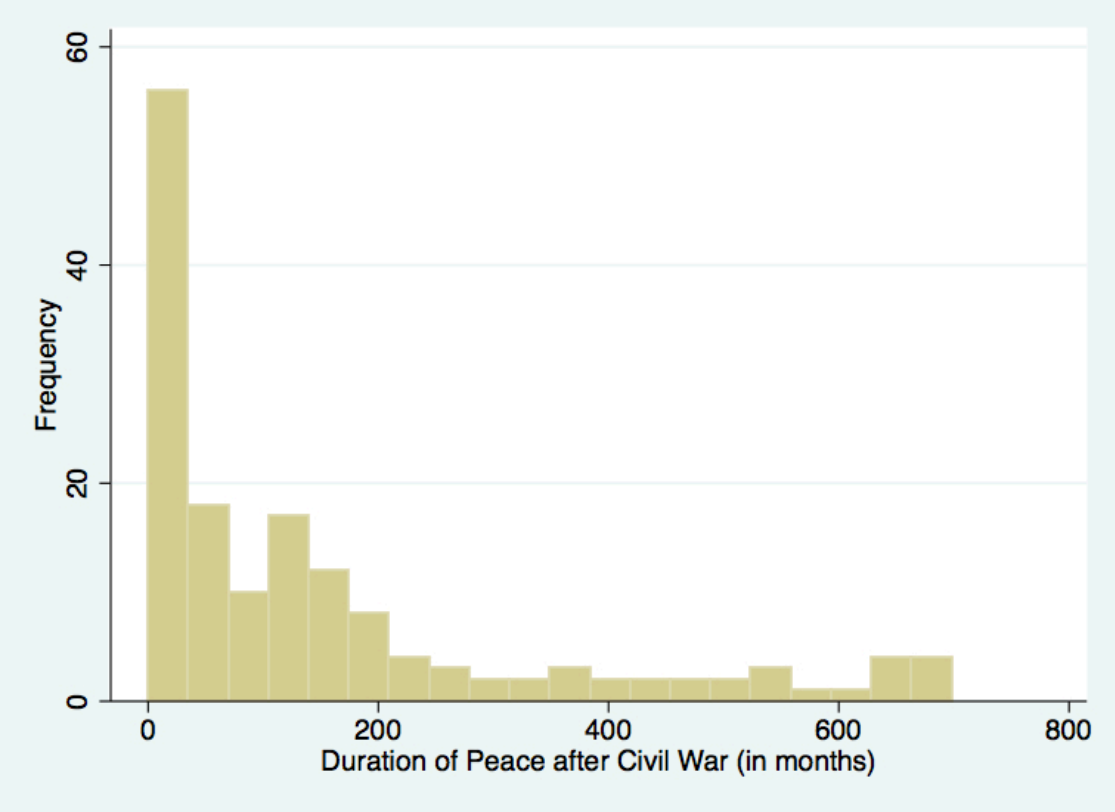

Figure 4.1: Distribution in the duration of peace after civil war for all conflicts that initiated and terminated between 1945 and 2004 (n=94)

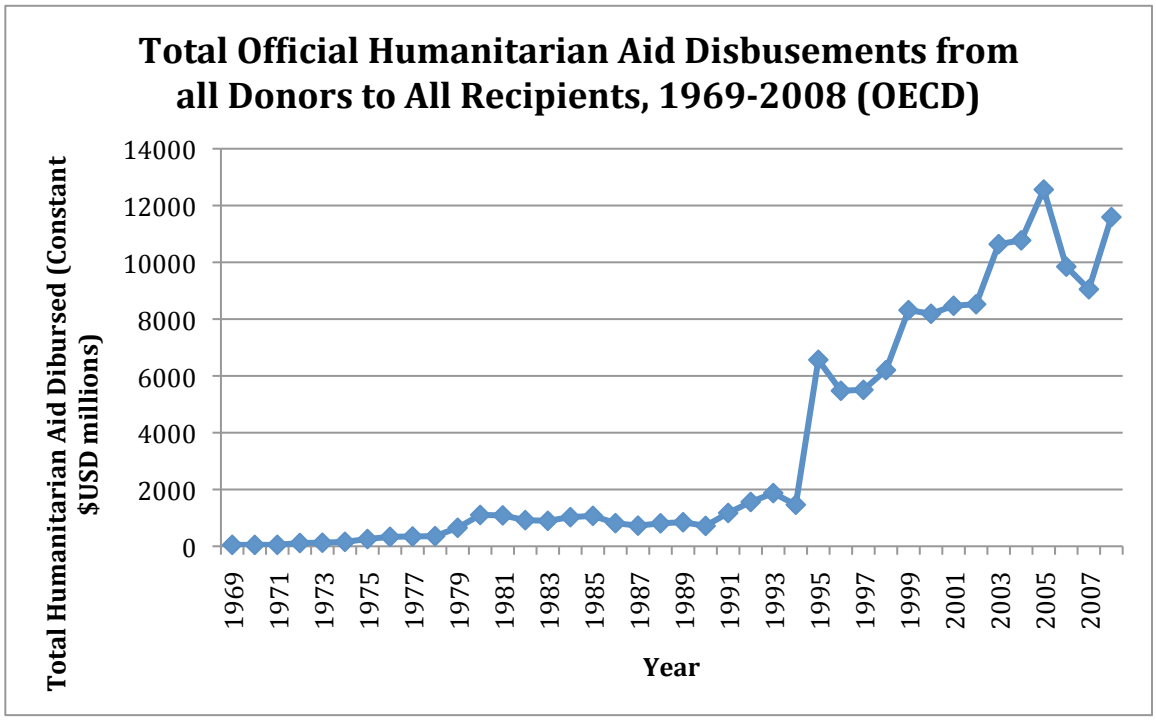

Figure 4.2: OECD DAC Estimates of Global Humanitarian Assistance Disbursed to Recipient States by Year

Table 4.1: Effect of Increasing Humanitarian Aid on the Risk of Peace Failing after all Civil Wars (1989-2004), Cox Estimates

VARIABLES	Model 1 Cox Haz. Ratio (RSE)	Model 2 Cox Haz. Ratio (RSE)	Model 3 Cox Haz. Ratio (RSE)	Model 4 Cox Haz. Ratio (RSE)	Model 5 Cox Haz. Ratio (RSE)	Model 6 Cox Haz. Ratio	Model 7 Cox Haz. Ratio
Total Humanitarian Assistance	0.967	0.985	0.982	1.021	1.021	0.998	0.999
	(0.0537)	(0.0602)	(0.0578)	(0.0472)	(0.0496)	(0.0452)	(0.0440)
Decisive Victory	0.176***	0.170***	0.175***	0.185***	0.178***	0.195***	0.201***
	(0.0803)	(0.0679)	(0.0656)	(0.0671)	(0.0660)	(0.0732)	(0.0744)
Lootable Resources	1.796**	1.773*	1.763*	2.062***	2.149***	2.246***	2.206***
	(0.514)	(0.557)	(0.553)	(0.576)	(0.590)	(0.593)	(0.540)
Treaty	0.248***	0.235***	0.232***	0.268***	0.271***	0.278***	0.282***
	(0.0937)	(0.0807)	(0.0793)	(0.0886)	(0.0922)	(0.0972)	(0.0938)
Identity War	1.253	1.330	1.317	1.150	1.055	1.110	1.048
	(0.386)	(0.430)	(0.423)	(0.374)	(0.349)	(0.394)	(0.365)
War Related Deaths	1.169*	1.149*	1.153*	1.191**	1.186**	1.214**	1.210**
	(0.101)	(0.0909)	(0.0909)	(0.0952)	(0.0955)	(0.101)	(0.0944)
Factions	0.603*	0.619*	0.627	0.608*	0.573*	0.600*	0.606
	(0.165)	(0.177)	(0.181)	(0.166)	(0.168)	(0.185)	(0.186)
Democracy	0.990	0.993					
	(0.0282)	(0.0258)					
Infant Mortality Rate	1.005	1.006	1.006				
	(0.00462)	(0.00454)	(0.00441)				
Past Agreement	1.083	1.003	1.007	0.909	0.899	0.857	
	(0.428)	(0.381)	(0.383)	(0.350)	(0.345)	(0.326)	
Government Army Size	1.000	1.000	1.000	1.000	1.000		
	(0.000437)	(0.000345)	(0.000320)	(0.000281)	(0.000272)		
Mountainous Terrain	0.971	0.927	0.931	0.899			
	(0.134)	(0.126)	(0.127)	(0.115)			
P-5 Contiguity	0.795						
	(0.442)						
Former P-5 Colony	1.487						
	(0.607)						
Duration of War	0.938***	0.946***	0.946***	0.950***	0.949***	0.950***	0.952***
	(0.0208)	(0.0197)	(0.0200)	(0.0164)	(0.0149)	(0.0161)	(0.0153)
Number of Subjects	92	92	92	92	92	92	92
Number of Observations	545	545	545	545	545	559	559
Log Pseudo-Likelihood	-205.37685	-206.25344	-206.28809	-207.33308	-207.74505	-210.39922	-210.53161

Robust Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.2: Effect Humanitarian Aid on the Risk of Peace Failing after *Decisive/Non-Decisive Victories* (1989-2004), Cox Estimates

VARIABLES	Model 1 Cox Haz. Ratio (RSE)	Model 2 Cox Haz. Ratio (RSE)	Model 3 Cox Haz. Ratio (RSE)	Model 4 Cox Haz. Ratio (RSE)	Model 5 Cox Haz. Ratio (RSE)	Model 6 Cox Haz. Ratio	Model 7 Cox Haz. Ratio
Humanitarian Assistance * Decisive Victory	**2.241****	**1.942***	**1.969***	**2.064***	**2.121***	**2.016***	**1.989***
	(0.907)	(0.784)	(0.807)	(0.886)	(0.945)	(0.835)	(0.795)
Total Humanitarian Assistance	0.919	0.946	0.944	0.973	0.972	0.958	0.958
	(0.0547)	(0.0674)	(0.0646)	(0.0555)	(0.0551)	(0.0482)	(0.0484)
Decisive Victory	0.000593**	0.00151**	0.00139**	0.00105**	0.000837**	0.00130**	0.00149**
	(0.00184)	(0.00470)	(0.00440)	(0.00350)	(0.00289)	(0.00418)	(0.00462)
Lootable Resources	2.148**	2.079**	2.073**	2.369***	2.458***	2.502***	2.478***
	(0.679)	(0.723)	(0.726)	(0.755)	(0.738)	(0.705)	(0.679)
Treaty	0.242***	0.222***	0.220***	0.247***	0.250***	0.272***	0.276***
	(0.0937)	(0.0772)	(0.0755)	(0.0825)	(0.0847)	(0.0926)	(0.0903)
Identity War	1.117	1.229	1.217	1.076	1.006	1.028	0.982
	(0.341)	(0.397)	(0.387)	(0.346)	(0.323)	(0.352)	(0.331)
War Related Deaths	1.170*	1.134	1.136	1.166*	1.159*	1.188*	1.183**
	(0.105)	(0.0942)	(0.0954)	(0.0994)	(0.0983)	(0.105)	(0.100)
Factions	0.501**	0.542**	0.546*	0.522**	0.497**	0.535**	0.540**
	(0.148)	(0.168)	(0.174)	(0.160)	(0.153)	(0.164)	(0.165)
Democracy	0.989	0.995					
	(0.0288)	(0.0261)					
Infant Mortality Rate	1.003	1.005	1.005				
	(0.00456)	(0.00440)	(0.00431)				
Past Agreement	1.088	0.994	0.997	0.923	0.910	0.871	
	(0.420)	(0.355)	(0.356)	(0.328)	(0.323)	(0.311)	
Government Army Size	1.000	1.000	1.000	1.000	1.000		
	(0.000483)	(0.000384)	(0.000344)	(0.000306)	(0.000293)		
Mountainous Terrain	0.986	0.941	0.943	0.920			
	(0.135)	(0.127)	(0.128)	(0.120)			
P-5 Contiguity	0.844						
	(0.491)						
Former P-5 Colony	1.780						
	(0.768)						
Duration of War	0.934***	0.945***	0.945***	0.949***	0.948***	0.949***	0.950***
	(0.0195)	(0.0186)	(0.0188)	(0.0159)	(0.0146)	(0.0156)	(0.0149)
Number of Subjects	92	92	92	92	92	92	92
Number of Observations	545	545	545	545	545	559	559
Log Pseudo-Likelihood	-202.9855	-204.39965	-204.41787	-205.16509	-205.41996	-208.11544	-208.22128

Robust Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.3: Effect Humanitarian Aid on the Risk of Peace Failing after *Decisive/Non-Decisive Victories* (1989-2004), Weibull Estimates

VARIABLES	Model 1 Weibull Haz. Ratio (RSE)	Model 2 Weibull Haz. Ratio (RSE)	Model 3 Weibull Haz. Ratio (RSE)	Model 4 Weibull Haz. Ratio (RSE)	Model 5 Weibull Haz. Ratio (RSE)	Model 6 Weibull Haz. Ratio (RSE)	Model 7 Weibull Haz. Ratio (RSE)
Humanitarian Assistance * Decisive Victory	1.842**	1.678*	1.710*	1.771*	1.810*	1.772*	1.771*
	(0.523)	(0.478)	(0.511)	(0.561)	(0.601)	(0.547)	(0.551)
Total Humanitarian Assistance	0.914	0.941	0.937	0.970	0.971	0.952	0.953
	(0.0529)	(0.0698)	(0.0676)	(0.0523)	(0.0536)	(0.0459)	(0.0464)
Decisive Victory	0.00226***	0.00431**	0.00389**	0.00310**	0.00251**	0.00321**	0.00318**
	(0.00504)	(0.00950)	(0.00906)	(0.00774)	(0.00654)	(0.00780)	(0.00775)
Lootable Resources	2.525***	2.392***	2.383**	2.698***	2.787***	2.854***	2.858***
	(0.744)	(0.810)	(0.813)	(0.847)	(0.867)	(0.849)	(0.835)
Treaty	0.222***	0.215***	0.210***	0.236***	0.239***	0.253***	0.253***
	(0.0941)	(0.0805)	(0.0780)	(0.0830)	(0.0868)	(0.0944)	(0.0926)
Identity War	1.169	1.262	1.243	1.091	1.003	1.040	1.055
	(0.410)	(0.467)	(0.451)	(0.379)	(0.358)	(0.397)	(0.400)
War Related Deaths	1.174*	1.142	1.146	1.179*	1.175*	1.202*	1.203*
	(0.111)	(0.106)	(0.106)	(0.112)	(0.114)	(0.121)	(0.119)
Factions	0.499**	0.521**	0.528*	0.507**	0.473**	0.506**	0.505**
	(0.149)	(0.169)	(0.176)	(0.158)	(0.152)	(0.167)	(0.167)
Democracy	0.987	0.991					
	(0.0310)	(0.0277)					
Infant Mortality Rate	1.004	1.005	1.005				
	(0.00466)	(0.00471)	(0.00464)				
Past Agreement	1.362	1.186	1.190	1.102	1.081	1.038	
	(0.544)	(0.445)	(0.447)	(0.410)	(0.404)	(0.392)	
Government Army Size	1.000	1.000	1.000	1.000	1.000		
	(0.000516)	(0.000413)	(0.000383)	(0.000335)	(0.000321)		
Mountainous Terrain	0.993	0.923	0.928	0.899			
	(0.153)	(0.132)	(0.133)	(0.122)			
P-5 Contiguity	0.683						
	(0.428)						
Former P-5 Colony	1.768						
	(0.810)						
Duration of War	0.927***	0.939***	0.938**	0.944***	0.942***	0.943***	0.943***
	(0.0239)	(0.0230)	(0.0234)	(0.0187)	(0.0175)	(0.0185)	(0.0189)
Number of Subjects	92	92	92	92	92	92	92
Number of Observations	545	545	545	545	545	559	559
Log Pseudo-Likelihood	-139.16894	-141.02642	-141.07671	-141.93983	-142.35638	-144.83147	-144.83905

Robust Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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Chapter Five
Biting the Hand that Feeds: An Organizational Theory Explaining Attacks Against Aid Workers in Civil Conflict

The humanitarian effort in Darfur has kept millions of displaced civilians from dying as the result of a civil war that has claimed roughly 400,000 lives since 2003. Yet in 2007, both UN and USAID officials considered withdrawing humanitarian relief as more than a dozen aid workers had been killed, dozens of vehicles stolen, compounds robbed, and aid workers beaten, harassed and sexually assaulted.[1] Similarly, in January 2010, the United Nations World Food Program suspended food deliveries to one million people in southern Somalia – affecting roughly one third of the total 2.8 million people the program anticipated feeding that year – after attacks killed four staff members in the months immediately prior.[2] Later that year, a Security Council report concluded that as much as half the food aid sent to Somalia is hijacked and diverted from needy people for military purposes and recommended that Secretary General Ban Ki-moon open an investigation into the World Food Program's roughly $485 million operation in Somalia.[3] And finally, in Afghanistan last year, at least 100 aid workers were killed – far more than any previous year of the war in Afghanistan – prompting debate within humanitarian organizations about operating in provinces that put them at unnecessary risk.[4]

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[1] Polgreen 2007. At the Gereida camp of 130,000 displaced people, aid workers were forced to retreat completely after armed men raided an aid compound, raped two women and stole several cars, satellite phones and computers
[2] MacFarquhar 2010.
[3] Gettleman and MacFarquhar 2010,
[4] Nordland 2010.

These attacks, and the subsequent withdrawal of humanitarian aid in conflict areas, suggest an important theoretical and empirical puzzle: why do warring parties in civil conflict ever direct violence against international aid workers? Despite the fact that most humanitarian aid missions operate under explicit guidelines to remain politically neutral in providing basic human needs like food, water, and medical supplies, aid workers are frequently the targets of violent attacks. In the period from 1997 to 2010, there were a total of 1026 acts of major violence reported against aid workers involving 2129 victims and resulting in 880 fatalities.[5] The vast majority of these attacks took place in the context of civil conflict (809 attacks, or 80%), where the absolute number of attacks has nearly doubled every three years from 15 incidents reported in 1997 to a high of 163 incidents in 2008 (**Figure 1)**.

On a theoretical level, attacks against aid workers are puzzling because they have the potential to be extremely costly for warring parties. In addition to the immediate logistical and material costs associated with planning and executing attacks, warring parties can expect to incur more substantial long-term costs if aid provisions are withdrawn altogether. Indeed, as the cases above illustrate, aid organizations and international donors have demonstrated a consistent willingness to suspend relief operations in response to aid diversions and perceptions of operational insecurity, thereby denying rebels access to future aid provisions altogether.[6] Perhaps even more costly is that the withdrawal of humanitarian assistance risks undermining combatants' base of

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[5] The Aid Worker Security Database. Available online at http://www.aidworkersecurity.org

[6] Other well-known examples of this include the MSF withdrawal from Rwandan refugee camps in Tanzania in response to aid diversions (Terry 2004) and Sudan's Darfur region following the kidnapping and subsequent release of MSF volunteers in 2009.

support among the local civilian population – the primary recipients and largest beneficiaries of relief provisions. To the degree that warring parties generally depend on civilian populations to supply the labor and resources needed to wage a successful civil war (including strategic advantages like mobility and invisibility), a strategy of attacking aid workers for resources in the short-term appears to be suboptimal given their long-term interests in maintaining popular support.

Even more puzzling is the empirical variation in attacks across different regions and conflicts. **Figure 2** plots the total number of aid worker attacks from 1997-2009 disaggregated by region. Overall, 54% of aid worker attacks in this period occurred in civil wars in Africa, while 28% of occurred in civil wars in Asia. Far fewer attacks occurred in civil conflicts in the Middle East (9%), Europe (5%) and North and South America (4%). Much of this variation might be explained by the fact that there are far more civil conflicts in Africa and Asia than in the other three regions of the world, but **Figure 3,** which plots the percentage and overall number of civil conflicts in which an aid worker was attacked each year, demonstrates that there is substantial variation even across countries undergoing civil war. For example, aid worker attacks occurred in only eight of the 40 ongoing civil wars (20%) in 1997. However, the percentage of civil wars in which there was an attack steadily increased over time, peaking at roughly 58% of all civil conflicts in 2006 before settling at 50% in 2009.

Beyond the theoretical puzzle of why parties would ever attack aid workers at all, then, this empirical variation is especially puzzling because we might expect parties across different civil conflicts to have identical incentives with respect to hijacking and

diverting humanitarian assistance. Yet, as the data above suggests, warring parties appear to exhibit very different patterns of behavior with respect to aid workers and the assistance they provide. One might suspect that these differences are simply attributable to the overall intensity of the war, but the most comprehensive description of aid worker attacks to date has found little empirical relationship between the overall level of violence and the total number of aid worker attacks.[7]

In this paper, we propose a theory of aid worker attacks to explain the variation in attacks across conflicts and over time. Unlike most theories of violence in civil war, which tend to conflate the collective incentives of rebel groups with the private incentives of the individual combatants that comprise them, we argue that attacks against aid workers are often the result of within-group collective action problems. Although warring factions have a long-term collective incentive to exercise restraint in their relationship with humanitarian aid provisions – lest aid organizations withdraw entirely – they are also composed of individual members with private incentives to hijack and divert aid for personal gain. We therefore posit that the likelihood of abuse will depend on at least two important factors related to groups' willingness and ability to solve this collective action problem: first, whether sufficient collective incentives exist for armed groups to exercise restrain in anticipation of future punishments (or rewards), and second, whether factions exhibit strong centralized control that enables them to set in place disciplinary structures to identify and sanction opportunistic defections by individual members.

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[7] Stoddard, Harmer & Haver 2006.

We test these expectations empirically using data on the organizational characteristics of rebel groups across conflicts and find strong support for our argument. We find that rebel groups that operate a political wing are much more likely to exercise restraint compared to groups that lack the same collective incentives to avoid the political costs associated with the withdrawal of aid. Moreover, we also find that members of more organized rebel groups which exercise a stronger degree of territorial control are much less likely to opportunistically attack aid workers when compared to combatants in less organized groups that lack the same ability to monitor and enforce restraint among individual members.

Before proceeding, it is important to emphasize at the start that we do not intend to offer a full causal theory explaining attacks against aid workers across civil conflicts. Instead, we aim to specify one important set of risk factors that follow from a logic collection action and an organizational theory of rebel groups – factors we believe to have been systematically neglected by aid organizations and academics alike. In doing so, we knowingly leave aside the very real possibility that attacks against aid workers are often times strategic.[8] Indeed, a richer story would almost certainly need to include these strategic factors along with many other characteristics of the actual aid provisions themselves (i.e. the type, amount, and organizational provider of aid, as well as the level of security provided to the aid caravans). However, so long as the factors that follow from an organizational theory of attacking aid workers are even somewhat orthogonal to the factors that would follow from a strategic logic of attacking aid workers, then the

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[8] Strategic incentives for parties to attack aid workers might include the desire to deny resources directly to one's enemy or to cut off assistance to unsupportive civilian populations

results of this paper should serve as an important first step towards ultimately forming a more complete model of the phenomenon when combined with future research.

The remainder of this paper proceeds in four principle sections. In the next section, we outline an organizational logic of rebel groups from which we derive testable implications about the conditions under which aid workers are more or less likely to experience an attack. Section 3 describes the research design and data used to test our hypotheses and Section 4 discusses the main findings. The final section concludes

A Theory: Collective Action, the Structure of Rebel Organizations, and Attacks Against Aid Workers

In this section we aim to articulate a view of rebel groups as organizations that face a variety of collective dilemmas that must be solved in order to operate effectively. In doing so, we follow in a long tradition of scholarship that effectively models a rebellion as a team of laborers specializing in the production of violence with the common goal of capturing a share of the rents that come from exercising political authority.[9] Because the remainder of this paper deals with propositions that follow from the general topics of collective action problems and organizational design, it is useful to quickly review these ideas before applying them to rebel groups and their relationship to humanitarian assistance.

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[9] Lichbach, 1998; Olson, 1971; Popkin, 1979

A collective action problem is a situation in which rational behavior on the part of individuals can lead to unanimously dispreffered outcomes.[10] More formally, collective action problems are situations that can be modeled as a game possessing Pareto-inefficient Nash equilibria, as in the famed prisoner's dilemma where players have a dominant strategy to defect on the cooperative outcome.[11] The result in these situations is in an inefficient equilibrium where all players pay the relative costs associated with defection – even though they could be made better off through cooperating – because they cannot trust the other players not to defect. Such dilemmas are thought to characterize several common social interactions and are thus used to explain 'suboptimal' levels of cooperation in many domains, particularly those that involve the under-production of collective goods.

One key reason collective action problems are important is that they form the basis for what is perhaps the most comprehensive theory of institutional origins and design. Most clearly articulated as the Alchian-Demsetz theory of the firm, and part of the broader perspective of new institutional economics, the theory seeks to explain institutional origins and features in terms of the choices made by rational individuals facing collective dilemmas.[12] In this view, institutions often emerge in order to solve collective action problems endemic to team production through the establishment of a central authority whose occupant(s) has a personal incentive to ensure that the collective dilemma is overcome. This "entrepreneur" is distinguished by three essential features:

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[10] Olson 1971.
[11] A simple explanation of this logic applied to actors in political competition can be found in (Bates, 1988; Cox & McCubbins, 1993; Taylor, 1976)
[12] Alchian & Demsetz 1972.

(1) he bears the costs of monitoring the organization facing the collective dilemma in order to prevent shirking by individuals, (2) he possess selective incentives with which to reward those he finds cooperating and punish those he finds defecting (e.g. hiring and firing individuals), and (3) he is compensated for the services he provides.[13] Centralized agents of this kind are appointed (emerge) when purely voluntary agreements cannot be relied on to solve the collective action problems in team production.

To illustrate this logic more concretely, consider the well-known example of the Yangtze rivermen used to depict an institutional solution to a quasi-prisoner's dilemma. As retold by Cox and McCubbins (1993),

> In prerevolutionary China, large gangs of men would tug fair-sized boats up the Yangtze. The problem was that each man was tempted to slack off a bit. After all, if enough others were pulling, the boat would still progress; if too few others were pulling, it did not matter how hard one pulled anyway. (p.92)

The rivermen's situation presented a collective dilemma: the Nash equilibrium was for no one to pull at all, for if one figured the other rivermen would pull (cooperate) then it was unnecessary to contribute his own effort in order to get the boat across and be paid. And, if one expected the other rivermen would all not pull (defect), it would again be unnecessary to pull because the effort of just one person in moving the boat would be futile. According to Cheung, the problem of loafing among the Yangtze rivermen became so severe that they actually agreed to the hiring of someone to whip them in order to ensure that everyone both pulled and got paid.[14]

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[13] Cox and McCubbins, 1993.

[14] Cheung 1983.

It is this general logic that we wish to apply to the origin and structure of rebel organizations. Rebels face a directly analogous situation to the one described above: they share a collective dissatisfaction with the existing government and the common goal of gaining policy concessions from the state. However, producing the level of violence needed to effectively coerce concessions from the state requires a team effort saddled with the same problem of shirking faced by the Yangtze rivermen. Individually, the effort of any single combatant is futile, but collectively each one has the incentive to defect from taking costly action to contribute his own effort to the group.[15] As in the case above, the Nash equilibrium is for no one to contribute their individual effort towards production of the common goal, which is inefficient because each would prefer the outcome in which they all contributed their effort to the rebellion and, together, coerced concessions from the state.

It is worthwhile to note that the incentives to defect in this case are particularly acute because the costs to individual combatants for collaborating in a joint rebellion go well beyond those of the Chinese riverboat pullers. Individual combatants must not only pull their own weight with respect to fighting the government, they must also conform to a host of other costly practices designed to maximize team production, including abiding by onerous membership requirements[16] and exercising restraint towards the civilian population once armed.[17] This latter point is especially important because rebel groups typically depend on civilian populations to supply the labor, resources, and strategic

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[15] Taylor 1988.
[16] Gates 2002.
[17] Humphreys and Weinstein, 2006

advantages like mobility and invisibility, needed to wage a civil war.[18] Indeed, it is generally well accepted that the success of an insurgency turns on the popularity of insurgents within their local population, for, as Mao famously noted, "the guerrilla must swim in the people as the fish swims in the sea".

Yet, for as critical as maintaining the hearts and minds of the civilian population may be to the overall success of a rebel group, individual armed combatants have the incentive to defect from exercising restraint and use coercive tactics to extract resources like food, labor, and property for private gain. Of course, if one combatant expects all others to not exhibit restraint (defect), it becomes unnecessary (and inefficient) to show restraint oneself since realizing the collective goal of challenging the state is impossible if everyone defects and alienates the civilian population. The fact that individual combatants must agree to not only collaborate on the production of violence but also on exercising restraint towards the civilian population thus presents a collective dilemma.

And indeed, the problem of defection among individual combatants is generally so severe that combatants agree to the formation of a centralized command to "whip" them in order to ensure everyone contributes. This is because, although rebels could, in theory, agree to collaborate on taking the costly actions necessary to effectively challenge the state through a series of voluntary agreements, the individual incentive for each combatant to later defect – by free riding on the efforts of others – leads them to appoint a leadership that bears the direct costs of monitoring and sanctioning defectors within the group. These tasks are typically accomplished with a set of selective incentives designed

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[18] Valentino, Huth & Balch-Lindsay, 2004

to reward cooperative members and punish defectors. Rebel leaders exercise these rights by "hiring" or "firing" individual rebels and by negotiating their compensation. In exchange for providing these services, rebel leaders either claim a substantial share of the group's collective output or they have a residual claim to all profits once all members have been compensated. Thus, the collective action problem facing members of a rebel group can be solved by institutionalizing a central authority motivated to ensure members do not defect.

It is now straightforward enough to leverage this general theory to explain the particular phenomenon of interest in this paper: attacks against aid workers. Recall from our introduction that it was not immediately obvious why the provision of humanitarian assistance would ever become the target of rebel attacks, especially since aid can be expected to enter into a rebels' production function for violence by directly supplying insurgents with access to resources over the long term, and by indirectly preserving ties to civilian sources of support (supplies, shelter, moral legitimacy, etc.). For our answer, we relax the assumption that rebel groups are unitary actors with perfect coherence of collective purpose and, instead, model them as organizations that emerge from the voluntary interaction of individual combatants.

We propose that attacks against aid workers are often the result of within-group collective action problems. Although warring factions have a long-term collective incentive to exercise restraint in their relationship with humanitarian aid provisions, they are also composed of self-interested individuals with private incentives to opportunistically hijack and divert aid for private gain. Here a collective dilemma arises

because, while it is the dominant strategy for each individual combatant to attack and hijack aid caravans, if too many attacks against aid workers occur, rebels risk the withdrawal of aid organizations altogether and fracturing ties with the local community, who are the primary recipient of humanitarian assistance. For individual combatants to exercise restrain with respect to easily-lootable aid provisions thus requires collaboration among rebels.

A solution, as in the case of the Yangtze rivermen, is to institutionalize a central authority that is both *willing* and *able* to ameliorate the collective dilemma by taking costly action to monitor and punish defections among individual combatants. With respect to the latter condition, it is reasonable to expect that members of more organized rebel groups that have empowered stronger central commands will be much less likely to opportunistically attack aid workers (defect) when compared to combatants in less organized groups that lack the same internal mechanisms to monitor and enforce restraint among individual members.

> ***Hypothesis 1****: The greater the degree to which a central command exercises control over participants in a rebellion, (a) the lower the likelihood that an aid worker will be attacked in that civil conflict, and (b) the lower the total number of attacks will be in that civil conflict.*

But note that while combatants may be *able* to collectively monitor and sanction individuals for opportunistically shirking on their obligations to the group, the degree to which they themselves, or an appointed agent, are actually *motivated* to take costly action in enforcing this punishment depends, in part, on having shared beliefs about the expected costs of not achieving the common goal (i.e gaining concessions from the state)

relative to benefits of defecting. For example, the Yangtze rivermen were *motivated* to hire someone to whip them because they each shared a preference for the cooperative outcome in which they all pulled and got paid rather than the outcome in which they all loafed (saving their effort) but did not get paid. Likewise, their agent was *motivated* to expend time and effort in whipping them by a personal interest in collecting the profits that came from promoting efficient collective action rather than slacking. Thus, the relative costs of ending up in the inefficient Nash equilibrium were sufficiently high and sufficiently clear to everyone involved in the production that they were willing to create and institutionalize a solution.

With respect the combatants' relationship to aid provisions, it follows that the greater the expected costs of the inefficient equilibrium (in which combatants defect by attacking aid workers for private gain) relative to the cooperative outcome (in which they exercise restraint for the sake of maintaining the popular support necessary to effectively coerce concessions from the state), the more motivated group members will be to implement a solution to the collective dilemma. While there are many different factors that could influence how costly it might be to a rebel group if individual combatants defect and attack aid workers, here we focus on one specific condition: whether a rebel group operates a political wing. Following the logic above, we expect that rebel groups that operate a political wing will be much more motivated to ensure restraint among their members compared to groups which lack the same collective incentive to avoid the political costs associated with the withdrawal of aid. We formulate this expectation based on the assumption that rebel groups operate a political wing for a reason:

presumably, their members have collectively agreed to challenge the state using a combination of non-violent political channels and coercive bargaining through battlefield outcomes.[19] For this reason, we expect rebel groups that operate a political wing to be much more sensitive to the possibility of alienating the civilian population and losing political support if aid provisions are withdrawn due to attacks.

> ***Hypothesis 2****: The likelihood of an aid worker attack will be significantly lower in civil conflicts where a rebel group maintains a political wing compared to conflicts in which a rebel group does not maintain a political wing.*

In the next section, we outline the research design we use to test these empirical expectations. We start by introducing the data for our main independent and dependant variables and then describe the estimation technique used to analyze our hypotheses. To foreshadow the results, we find that the dramatic variation in attacks against aid workers across different conflict areas can be partially explained by differences in the capabilities and incentives of rebel groups to solve the collective action problem outlined above. Specifically, we show that in civil wars where rebel groups are more organized, with a strong central command, and in civil wars where rebel groups operate a political wing, aid workers are much less vulnerable to attacks.

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[19] Heger 2010.

Data and Methodology

Because the theory outlined above links variations in the organizational structure of rebel groups with attacks against aid workers, an ideal empirical strategy might begin with rebel groups as the unit of analysis and attempt to explain cross-sectional variation in attacks committed by each group using data on their capabilities and political opportunities as independent variables. Unfortunately, it is often impossible to identify which rebel group perpetrated the attack in each event. This is because, much like terrorist attacks, violence against civilians, or even common crimes/banditry, observers on the ground do not always know who was responsible for the violence and why – they simply observe the aftermath.[20] Moreover, rebel groups often have the incentive to deny responsibly for attacks when they think it will be costly to claim, which means that any data attributing aid worker attacks to a particular group is sure to be controversial.

To avoid complications that would arise from attributing individual attacks, the unit of analysis for this study is the civil war year. We defined the population of cases using the UCDP Armed Conflict Data, which contains a list of all civil war years from 1945-2010. We dropped all conflict years from the ACD data before 1997, leaving us with a study population of 457 civil war-year observations that occurred in 83 different civil wars between 1997 and 2009.[21] We then merged the data on our dependant variable – attacks against aid workers – onto each conflict year, matching the country-location and

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[20] Stoddard et al., 2006.

[21] We limit our analysis to ony this time period because it is the range for which we have data on the reported incidence of aid worker attacks from the Aid Worker Security Database.

year of each aid worker attack with the Correlates of War identifier of the country in which fighting primarily occurred.

Data

For data on aid worker attacks, we used the Aid Worker Security Database, which collects incident reports from public sources augmented with internal information provided directly by aid organizations. The dataset is a compilation of reports on major security incidents – defined as killings, kidnappings, and armed attacks that result in serious injury – involving deliberate acts of violence affecting aid workers.[22] Aid workers are defined as "employees and associated personnel of not for profit aid agencies (both national and international staff) that provide material and technical assistance in humanitarian relief contexts".[23] This includes employees of both relief and multi-mandated (relief and development) organizations.[24] Not included are UN peacekeeping personnel, human rights workers, election monitors or purely political, religious, or advocacy organizations.

The full version of the Aid Worker Security dataset downloaded for this article contained a total of 1026 attacks on aid workers recorded during the period from 1997-

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[22] Ibid.
[23] Cited from coding notes online at: http://www.aidworkersecurity.org/about-the-data.php
[24] Included are NGOs, the International Movement of the Red Cross/Red Crescent, donor agencies and the UN agencies belonging to the Inter-Agency Standing Committee on Humanitarian Affairs (FAO, OCHA, UNDP, UNFPA, UNHCR, UNICEF, WFP and WHO) plus IOM and UNRWA.

2010.[25] Of these, 809 attacks (79%) occurred in the civil war years included in our study population. And of the reported 2129 total victims from these attacks in the full dataset, 1726 of the victims (81%) were observed in conflict-years included in our study. The other 217 attacks against aid workers and 403 aid worker victims were excluded because they occurred outside the context of a civil war either in an international conflict or a natural disaster.

For data on our main explanatory variables, we used the Expanded Uppsala Armed Conflict Data (EUACD).[26] These data expand on the original Uppsala Armed Conflict Data on civil conflicts in two important ways. First, the data codes specific information about the *non-state actors* involved in a civil conflict, such as the estimated size of the rebel group, whether a rebel group has a clear central command, whether a rebel group controls territory, or whether a rebel group has a political wing (in addition to many other attributes). Second, the data adds information about the *external dimensions* of civil conflicts, including access to external support and extraterritorial features. For example, the data codes for whether a particular rebel group was supported by another government or transitional non-state actor in either a military or non-military manner.

The variables within these data that are of primary interest for our study are the indicators we chose to proxy for the constructs 'degree of central command' and 'maintains a political wing'. To measure the first – the degree to which a centralized authority can monitor and sanction opportunistic defections among individual members

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[25] Downloaded on December 06, 2010 at http://www.aidworkersecurity.org/search.php
[26] Cunningham, Gleditsch, and Salehyan, 2009

of a rebel group, and thus potentially solve the collective action problem that contributes to attacks against aid workers – we use an ordinal coding of the variable *effterrcont* in the EUACD data, which measures the degree of effective control a rebel group exercises over the territory in which it operates. While this measurement is, at best, only a proximate indicator of the degree of centralized authority in a rebel group, it has been used in previous work to measure rebels' capacity to enforce discipline.[27] Moreover, this measure is much more refined than the alternative, dichotomous indicators for the strength of central command available in the dataset. Finally, to measure the second construct – whether a rebel group has a political wing, and thus the added incentive to police defections in order to avoid the political costs of alienating the local population – we use the dichotomous variable *rebpolwing,* which directly codes for whether the group is known to operate a political wing in that conflict-year.

Before moving to a discussion of our analysis and results, there are two important concerns with using these data that are worth noting. First**,** following the original Uppsala Armed Conflict data, the unit of analysis for the expanded non-state actor data is the conflict dyad, where a single conflict is distinguished by a non-state actor (Side B) fighting the government (Side A). Importantly, because a civil conflict may have many side B actors that each forms a separate conflict dyad with the government, we were forced to make a coding choice in terms of which group's attributes to merge onto the single observation for that conflict year in our study. For example, Conflict 1360 in the expanded armed conflict data between Guatemala and five insurgent groups—MR-13,

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[27] Humphreys & Weinstein, 2006

FAR, EGP, PGT, and ORPA – gives rise to five separate dyadic conflicts for which individual information was collected. All told, there are 52 civil conflicts in our study generating 254 civil war year observations in which more than one rebel group was present.

In these cases, our solution was to merge the minimum value on variables like '*clear central command'*, '*strength of central command'* and the maximum values for variables like '*size of rebel armed forces'* across all of the groups operating in each conflict-year. Our logic in doing this is that a particular conflict environment is only as secure as its least cohesive/centralized rebel organization. That is, it is reasonable to suppose that one "bad apple" (highly de-centralized rebel group prone to opportunistic defection) could ruin the environment for everyone because, even if the other rebel groups are highly cohesive – with highly centralized command and control – aid workers must still confront the risk of opportunistic defection posed by the least cohesive group in that environment. One could certainly argue that, empirically, this measurement choice may bias the independent variable values in favor of our theory, but in the analyses that follow, we run robustness checks using other measurement choices for the independent variables (i.e. the max, median, and mean values of each independent variable across the rebel groups battling the government in that conflict-year) to show that this is not the case, and we also subset the analysis on just the conflicts in which a single rebel group was present to ensure that the coding choice is not driving the results.

The second concern with using the expanded non-state actor data for our analysis is that the data only spans the period from 1946-2003. This means that the data for our

main explanatory variables (attributes of the rebel groups) are incomplete for the period from 2003-2008. This is especially problematic because 615 of the total 809 attacks and 1284 of the total 1726 victims from these attacks were observed in 105 conflict years in which an attack occurred after 2003 (out of the 205 total conflict-year observations after 2003).

One solution to this would be to manually code the attributes for the rebel groups involved in these civil war years ourselves. However, because the coding rules employed by the EUACD data project are not sufficiently detailed to accomplish this with a high degree of inter-coder reliability, it is not an ideal solution. Instead, the solution we employed was to use the last observed value coded for each of the explanatory variables if a conflict was ongoing after 2003. In other words, if a civil conflict began before 2003 and continued until 2008, we used the last observed value coded by Cunningham et al. for 2003 as the best approximation for the key organizational characteristics identified by our theory during years 2004-2009. This imputation filled in values for the key explanatory variables for 142 of the 205 conflict year observations after 2003, leaving only 63 conflict years for which we were unable to reasonably infer an approximation of the rebel group characteristics after 2003. The reason these 63 conflict years could not be imputed using this strategy is because they belong to civil conflicts that started after 2003, meaning there were no measurements taken by the original coders from which we could approximate values for period 2004-2009. Even after this imputation there are still 68 conflict years after 2003 (five before 2003), and 110 conflict years total, for which we

have no data on our key explanatory variables. Unfortunately, this will exclude roughly 300 total attacks and 250 victims from most of our analyses.

As a result of these data limitations, the analyses that follow are generally conducted over approximately 350 conflict year observations for which we have a reasonable approximation of the rebel group characteristics in that conflict environment. Across these 350 conflict years, roughly 500 attacks resulting in over 1000 victims were coded in just 112 observations, suggesting that attacks against aid workers are relatively concentrated within our study population.

Analysis

To test Hypotheses 1 and 2, we estimate the following models using the data described above:

Baseline Models Hypothesis 1:

$$Pr(AidWorker Attack_{it}) = \alpha_{it} + \beta_1 centralcommand_{it} + \beta_X X_{it} + \mu_{it}$$

$$TotalAidWorkerAttacks_{it} = \alpha_{it} + \beta_1 centralcommand_{it} + \beta_X X_{it} + \mu_{it}$$

Baseline Model Hypothesis 2:

$$Pr(AidWorker Attack_{it}) = \alpha_{it} + \beta_1 rebelpoliticalwing_{it} + \beta_X X_{it} + \mu_{it}$$

In all cases, the parameter $\boldsymbol{AidWorkerAttack_{it}}$ is a dichotomous variable that captures whether *any* attack against an aid worker occurred in a civil war, *i,* in year, t. $\boldsymbol{\beta_1}$ is the parameter of interest for the key independent variables for each hypothesis, $\boldsymbol{B_X}$ is the vector of covariate parameters for covariates $\boldsymbol{X_{it}}$, and μ_{it} is the error term. In testing Hypothesis 1, we also estimate the relationship between our explanatory variables and the variable $\boldsymbol{TotalAidWorkerAttacks_{it}}$, which is a continuous measure of the total number of aid worker attacks in civil war, *i,* in year, t, for reasons discussed in the results section below.

Control Variables

In each of the analyses below, we use a combination of control variables to account for the fact that our independent variables may not be randomly 'assigned' across different conflict environments. That is, rebel groups may establish stronger central command or maintain a political wing in civil conflicts where the underlying risk of an aid worker attack is greater for reasons not directly related to the collective action problems faced by rebel groups. To limit the risk of selection bias, we considered factors that may increase the overall likelihood of an 'incidental' aid worker attack. **Table 1** reports the correlation coefficients for the full list of candidate-variables we considered. We ultimately selected variables that were *significantly* correlated with the occurrence of an aid worker attack at the 0.10-level or greater as controls.

We included a measure for the total number of *Battle Deaths* reported in a civil war as an approximate measure for the intensity of violence in that civil war year. The variable is used to approximate the number of bullets flying through the air and thus the likelihood that an aid worker might be randomly "caught in the crossfire". Estimates for the total number of soldiers and civilians killed in each conflict year were taken from the PRIO Battle Deaths Dataset 3.0.[28]

Next, we included a measure for the *Total Humanitarian Assistance* disbursed in a civil war. We use the variable as a proxy-measure for the total number of aid workers in a particular conflict environment, with the logic that an incidental attack is almost certainly more likely when aid workers are present in greater numbers. Estimates of the amount of humanitarian assistance provided in a conflict-year are taken from the OECD DAC data on official humanitarian aid disbursements.

Additionally, we included a measure for the *Size of the Largest Rebel Group* in a civil war year. Our logic in including this variable was that the likelihood of an incidental attack should increase in the opportunity for an aid worker–rebel interaction. Data on the size of rebel groups are taken from the Expanded Uppsala Armed Conflict Data on non-state actors described above.[29]

We also included a measure for the *Total Number of Groups* operating in a civil war-year based on the same logic that the number of aid worker attacks should increase in the opportunity for an aid worker–rebel group interaction. However, we also include

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[28] Lacina & Gleditsch 2005.
[29] Cunningham, Skrede Gleditsch & Salehyan 2009.

the variable because we expect the likelihood of violence against non-combatants to increase in the level of contestation in that environment.[30] The total number of rebel groups operating in a civil war year was calculated by summing the total number of unique conflict dyads in each civil war based on the Expanded Uppsala Armed Conflict Data cited above.

Finally, in some of the analyses below, we include *regional* controls to account for the possibility that rebels might have different tendencies to establish strong central command and maintain political wings across different regions of the globe. If this is the case, any relationship between our main independent variables and the likelihood of an aid worker attack (which, as we demonstrated in the introduction, varies significantly by region), could be spurious due to reasons not observed in the data.

Results and Discussion

Table 2 reports the regression results for Hypothesis 1. We display the results for six different models, each of which estimates the relationship between aid worker attacks and increasing central control – as measured by the degree to which a rebel group in the civil war exercises effective control over the territory in which it operates – in the population of all 457 civil war years in the period from 1997-2009.

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[30] See Humphreys and Weinstein 2006; Keen 2005.

For Models 1-3, we estimate the likelihood of observing *any* aid worker attack at all using a dichotomous dependant variable coded 1 when the number of aid worker attacks is greater than or equal to 1 and 0 otherwise. Model 1 reports the result of a logistic regression with the most basic model specification, including only our main independent variable and the total number of *battle deaths*, total *level of humanitarian aid*, and *size of the largest rebel group* as controls. Model 2 reports the results of logistic regression using the same basic model but including the total *number of rebel groups* in that civil war year as an additional control. And finally, Model 3 reports results of the same logistic regression as Model 2 but includes regional dummies.

Models 4-6 estimate the relationship between the logged-number of total aid worker attacks and our explanatory variables using ordinary least squares regression. As before, we report the results for the most basic model specification on the left in Model 4, and then include additional controls for the number of rebel groups in Model 5, and regional controls in Model 6. Again, the only difference between Models 1-3 and Models 4-6 is that the former estimate the likelihood of observing any aid worker attack while the latter estimate the relationship between the logged-number of total aid worker attacks and our main independent variables.

We estimated the relationship between our explanatory variables and two separate measurements of the dependant variable because the same factors that predict whether or not rebels will opportunistically attack an aid caravan may not necessarily predict the absolute number of attacks that occur. This is because whether rebels attack more than once will depend on many other factors that could be unrelated to the decision to hijack

aid in the first instance. For example, a weak central command may present the opportunity for individual combatants to attack aid convoys, but whether or not rebels orchestrate subsequent attacks depends on how large or small the amount of diverted aid was. In cases where the amount of aid hijacked was large, it is reasonable to think that the same factors that successfully predicted the first attack (i.e low levels of central command) might incorrectly predict a third and fourth attack as rebels no longer have the incentive (demand) to attack aid workers again for more resources.

Nevertheless, the results in Table 2 demonstrate that both the likelihood and total number of aid worker attacks across conflicts is negatively correlated with the degree of effective territorial control exercised by a rebel group in that civil war. This result is consistent with our theoretical expectation that the greater the degree to which a central command exercises control over participants in a rebellion, the better able participants are to monitor and sanction opportunistic defections among individual members and solve the collective action problem that contributes to attacks against aid workers. Across almost all specifications, the relationship between increasing strength of central command and the occurrence of aid worker attacks is negative and significant at the 5-percent level[31]. The lone exception is Model 5, which estimates the relationship between the degree of control and the logged-number of aid worker attacks including the total number of rebel groups as a control.[32]

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[31] These results should be interpreted with caution due to the small sample size from missing data.
[32] We do not have a good explanation for why the degree of control is not significant in Model 5

While not included in the tables here, we also tested to see if the results above were robust to the inclusion of the other control variables listed in Table 1. In all cases, increasing the effective territorial control of a rebel group is negatively associated with the occurrence of an aid worker attack. We also found the results held when using conflict fixed-effects. Finally, in cases where more than one rebel group operated in a particular conflict environment, we confirmed that the results were robust to using the minimum, maximum, mean, and median values of the independent variables.

Figure 4 plots the substantive effect of moving from the lowest degree of effective territorial control exercised by a rebel group (on the left) to the highest degree of territorial control (on the right) holding all other covariates at their mean. Over the full range, the estimated likelihood of an aid worker attack decreases roughly 66-percent when moving from the lowest level of control to the highest level of control. This effect is consistently significant, as shown by the gray lines indicating the upper and lower bounds of the 95-percent confidence intervals.

Before proceeding to Hypothesis 2, it is worthwhile to discuss the results on the control variables. Similar to previous work, we found that the likelihood and total number of aid worker attacks is uncorrelated with the overall intensity of violence. This result may be surprising to some, but it is consistent with the idea that attacks are not random. Occurrences of aid worker attacks also appear to be generally uncorrelated with the size of rebel groups and the total number of rebel groups involved in a contest. Interestingly, the one variable that is most significantly and robustly correlated with attacks against aid workers is the total amount of humanitarian aid disbursed in a

particular conflict. This empirical relationship may not be entirely surprising, but the exact reason for the result is not immediately obvious: larger amounts of aid could be associated with more attacks because it means more aid workers are randomly caught in the cross-fire or because larger amounts of aid represent a larger prize and thus a greater incentive for combatants to defect from exhibiting restraint.

Table 3 reports the regression results for Hypothesis 2. We display the results of five different models, each of which estimates the relationship between the likelihood of an aid worker attack and the presence of a rebel group that operates a political wing in each civil war year.

Models 1-3 estimate the likelihood of observing any aid worker attack at all in the full population of 457 civil years from 1997-2009, this time substituting whether a rebel group operates a political wing as our main independent variable. As in the previous table, we report the results for the most basic model specification on the left with Model 1, and then include additional controls for the number of rebel groups in Model 2, and regional controls in Model 3. Models 4 and 5 estimate the relationship in two separate subsample of the full population. Model 4 estimates the relationship between our explanatory variables and the probability of an attack in non-democracies and Model 5 estimates the relationship in democracies.

The results of Models 1-3 show that the likelihood of an aid worker attack is negatively correlated with whether a rebel group operates a political wing in that civil war. This result is consistent with our theoretical expectation that rebel groups with a political wing will have the added incentive to police defections in order to avoid the

political costs of alienating the local population. Across all three specifications, the relationship is negative and statistically significant at the 5-percent level. As one might expect, the results of Model 4 estimated in the subsample of non-democracies undergoing civil war shows no relationship between a rebel political wing and the likelihood of an attack against an aid worker, while the results of Model 5, estimated in the subsample of democracies undergoing civil war, shows a negative and strongly insignificant relationship between a rebel political wing and the likelihood of an attack. This result makes sense if one assumes that failing to exercise restraint and alienating the local population is much more costly in democracies where citizens can more easily punish rebels by withdrawing support. As before the results for hypothesis 2 should be interpreted with caution due to the small sample size from missing data.

Conclusion

Across the different conflict environments, rebel organizations face a similar challenge with respect to their relationship to humanitarian assistance: while foreign aid provisions represent an attractive source of easily lootable resources that could be used to sustain a war effort, the diversion of these resources away from the local civilian population can undermine a rebel organization's base of support when aid workers suspend their relief effort completely. And yet, despite the collective incentive to exhibit restraint, different rebel groups display marked variation in their behavior towards aid workers providing assistance. The occurrence and variation in attacks against aid workers thus present an interesting theoretical and empirical puzzle.

In this paper we outlined a view of rebel organizations as solutions to the collective action problems that individual combatants face. We focused specifically on collective dilemmas that arise with respect to rebels' relationship to humanitarian aid provisions, and we suggested that rebels face competing individual- and group-level incentives. While attacking aid workers to divert humanitarian resources may be a rational way to maximize personal gains in the short-term, if enough combatants defect from exercising restraint with respect to aid, rebels are likely undermine their collective goal of challenging the state. Based on this logic, we argued that that the likelihood of abuse will depend on at least two important factors related to groups' willingness and ability to solve this collective dilemma: first, whether sufficient collective incentives exist for armed groups to exercise restrain in anticipation of future punishments (or rewards), and second, whether factions exhibit strong centralized control that enables them to set in place disciplinary structures to identify and sanction opportunistic defections among individual members.

We tested two observable implications of this logic empirically using data on the organizational characteristics of rebel groups and found strong support for the theory. First, we found that members of more organized rebel groups which exercise a stronger degree of territorial control are much less likely to opportunistically attack aid workers when compared to combatants in less organized groups that lack the same ability to monitor and enforce restraint among individual members. Second, we found that rebel groups that operate a political wing are much more motivated to exercise restraint when

compared to groups that lack the same collective incentives to avoid the political costs associated with the withdrawal of aid.

We believe that this paper makes an important contribution to our understanding of political violence because it suggests another way in which the organizational structure of groups could affect their willingness and ability to use violence (see also Berman 2009). Most models of rebellion assume too much internal unity of rebel groups. Indeed, most of the recent political science literature on civil conflict in the last decade formally models rebels as unitary actors who collectively bargain for maximal concessions from the government. While this assumption has certainly proved useful in generating implications about the onset and duration of war, it helps very little in explaining the conduct of war. Rather, the behavior of rebels within a conflict can best be explained by examining individual incentives to perform acts of violence.

Figures and Tables

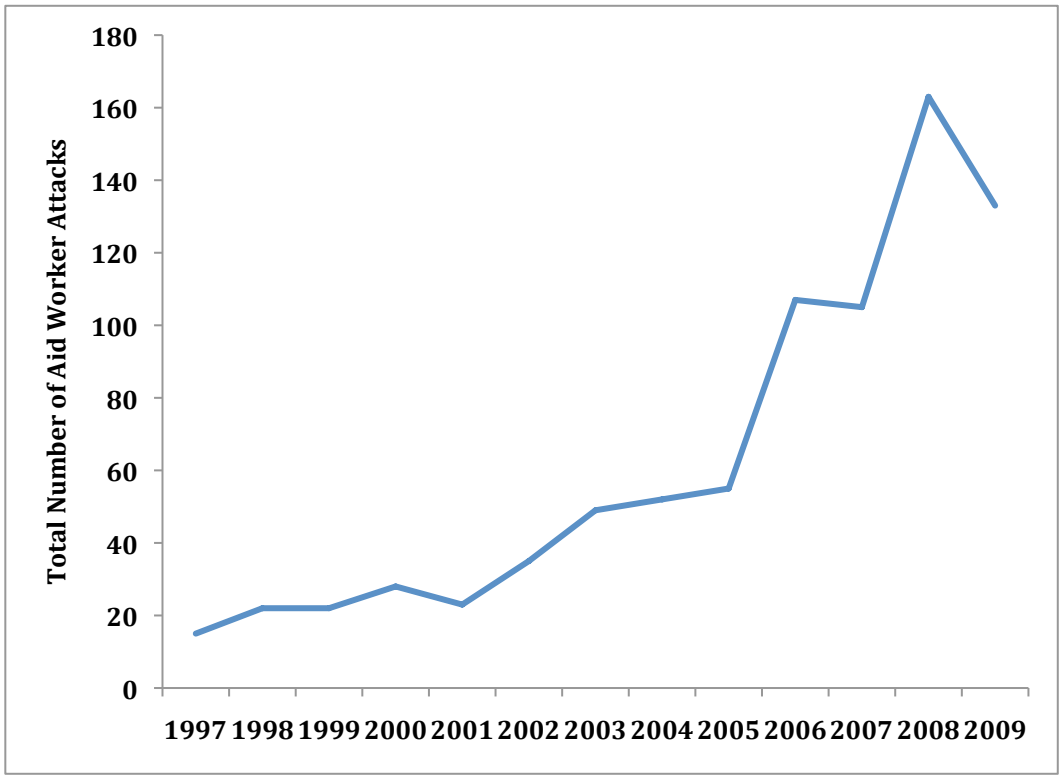

Figure 5.1: Total Number of Aid Worker Attacks over Time, 1997-2009

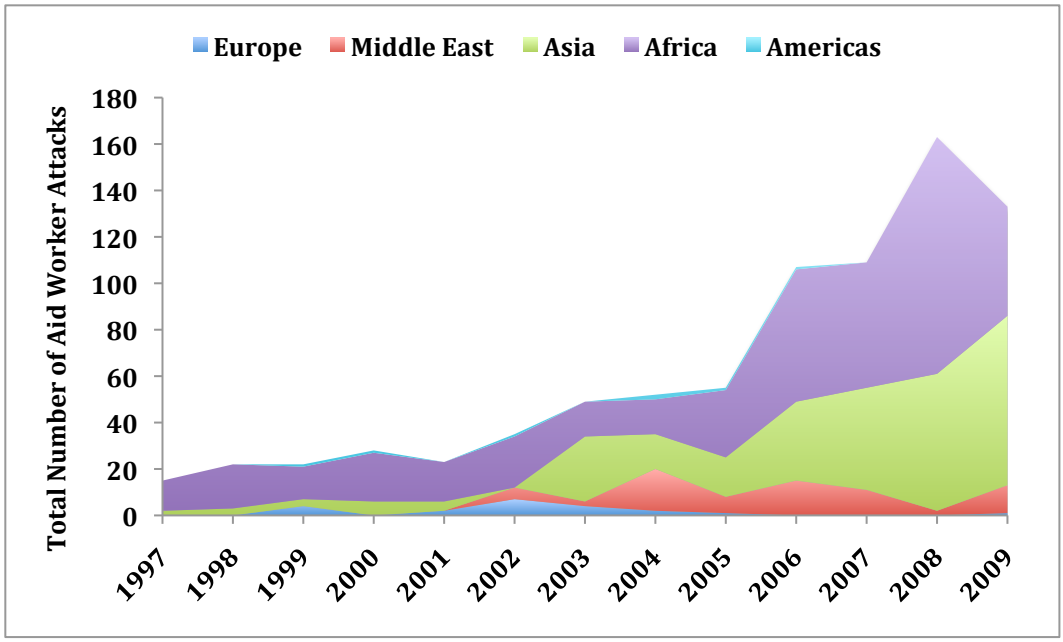

Figure 5.2: Total Attacks Against Aid Workers over Time stacked by Region, 1997-2009

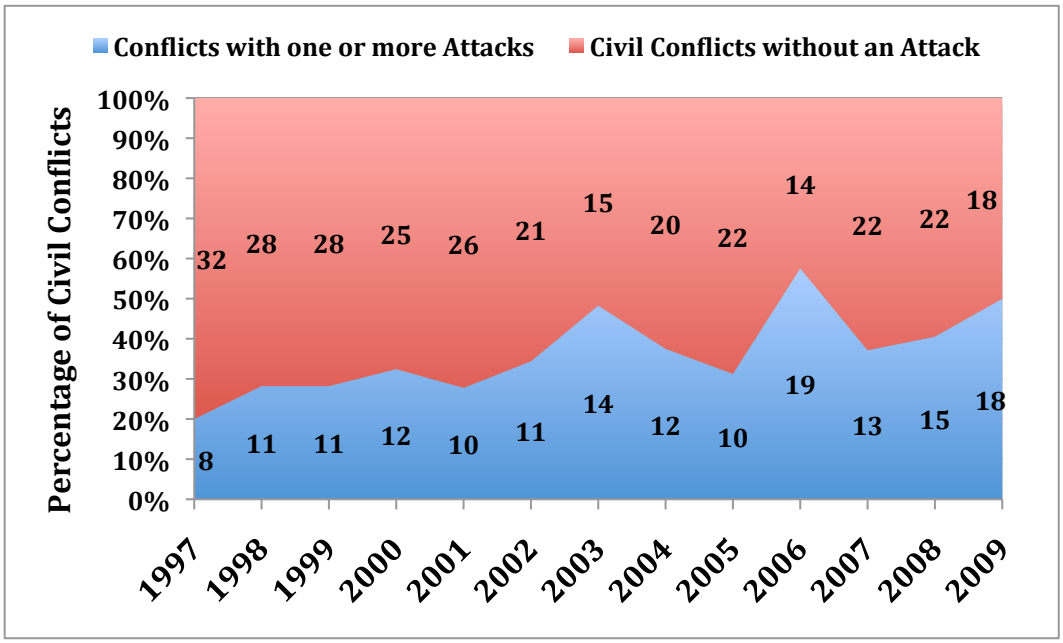

Figure 5.3: Percentage and Number of Civil Wars in which an Aid Worker Attack Occurred over Time, 1997-2009

Table 5.1: Correlation Coefficients

	Aid Worker Attack	Control Territory	Rebel Political Wing	Number of Groups	Rebel Group Size	Battle Deaths	Level of Aid Provisions	Lootable Resources	Mountainous Terrain
Aid Worker Attack	1								
Control Territory	-0.3150*	1							
Rebel Political Wing	-0.1738*	0.3213*	1						
Number of Groups	0.2452*	-0.6693*	-0.1721*	1					
Rebel Group Size	0.2779*	-0.2263*	-0.0739	0.2963*	1				
Battle Deaths	0.2041*	-0.1738*	-0.1555*	0.3751*	0.3864*	1			
Level of Aid Provisions	0.3280*	0.0125	0.0066	-0.0237	0.1464*	0.1354*	1		
Lootable Resources	0.1075*	-0.1716*	0.0331	0.1226*	-0.0057	0.1859*	0.049	1	
Mountainous Terrain	-0.0933	0.5843*	0.3535*	-0.2216*	0.2812*	-0.079	-0.0046	-0.2221*	1

Note: The table presents correlation coefficients among country average values of the main variables used in the article.
*Correlation significant at 5%.

Table 5.2: Degree of Rebel Territorial Control and the Probability of an Aid Worker Attack (Models 1-3) and the Number of Aid Worker Attacks (Models 4-6), 1997-2009)

VARIABLES	(1) Logit Pr(Attack)	(2) Logit Pr(Attack)	(3) Logit Pr(Attack)	(4) OLS Total Attacks	(5) OLS Total Attacks	(6) OLS Total Attacks
Degree of Rebel Territorial Control	-1.202***	-1.154**	-1.502*	-0.129**	-0.0680	-0.189**
	(0.462)	(0.587)	(0.869)	(0.0508)	(0.0670)	(0.0747)
Battle Deaths	7.95e-05*	7.94e-05	6.45e-05	7.04e-06**	5.09e-06	8.07e-06*
	(4.79e-05)	(4.84e-05)	(4.63e-05)	(3.44e-06)	(3.71e-06)	(4.29e-06)
Total Humanitarian Aid	0.00317***	0.00315***	0.00441***	0.000817***	0.000814***	0.00110***
	(0.00122)	(0.00122)	(0.00145)	(0.000150)	(0.000149)	(0.000152)
Size of Largest Rebel Group	3.26e-05*	3.11e-05	2.40e-05	4.49e-07	1.01e-07	-1.33e-06
	(1.86e-05)	(2.14e-05)	(1.62e-05)	(1.27e-06)	(1.29e-06)	(1.25e-06)
Number of Groups		0.0230	-0.0284		0.0254	0.0288
		(0.177)	(0.241)		(0.0183)	(0.0233)
Middle East			-0.930			-0.202
			(1.772)			(0.194)
Asia			-1.763**			-0.219**
			(0.813)			(0.0989)
Africa			-2.078**			-0.527***
			(1.026)			(0.119)
Americas			-0.630			-0.265**
			(0.988)			(0.125)
Constant	0.881	0.771	2.991	0.355***	0.200	0.673***
	(0.945)	(1.273)	(1.867)	(0.105)	(0.153)	(0.172)
Observations	143	143	143	143	143	143
R-squared				0.246	0.257	0.379

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

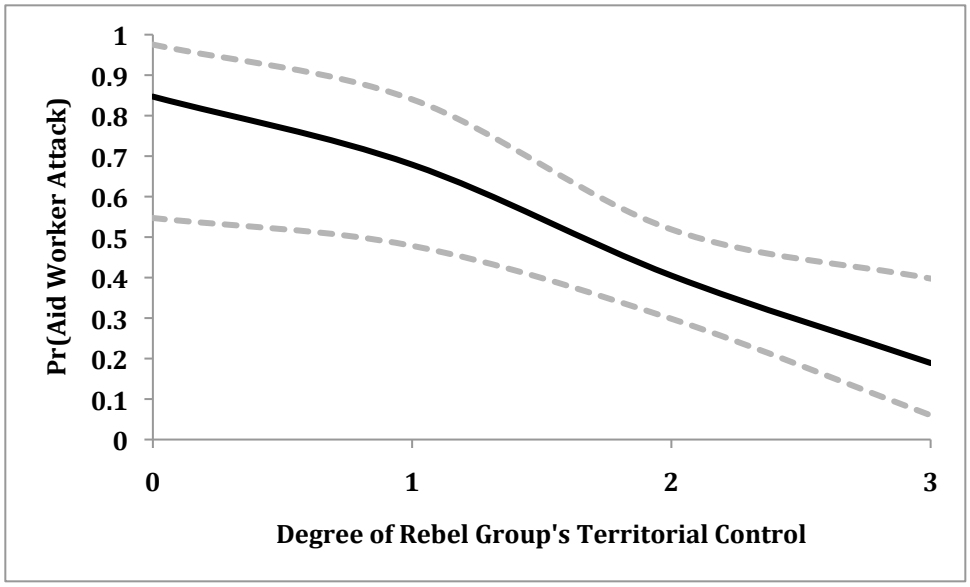

Figure 5.4: Substantive effect of territorial control on the probability of an aid worker attack moving from the lowest degree of effective territorial control exercised by a rebel group on the left to the highest degree of territorial control on the right holding all other covariates at their mean. Over this range, the estimated likelihood of an aid worker attack decreases roughly 66-percent when moving from the lowest level of control to the highest level of control. Gray lines indicate the upper and lower bounds of the 95-percent confidence interval

Table 5.3: Rebel Political Wing and Probability of an Attack Against Aid Workers (1997-2009)

	(1) Logistic	(2) Logistic	(3) Logistic	(4) Logistic	(5) Logistic
VARIABLES	pr(attack)	pr(attack)	pr(attack)	pr(attack) Non Democracy	pr(attack) Democracy
Political Wing	-0.782***	-0.634**	-0.644**	0.683	-0.721**
	(0.281)	(0.293)	(0.319)	(0.730)	(0.361)
Battle Deaths	5.76e-05*	5.93e-05	6.86e-06	0.000408	5.01e-05
	(3.33e-05)	(3.76e-05)	(3.70e-05)	(0.000281)	(3.40e-05)
Total Humanitarian Aid	0.00519***	0.00544***	0.00540***	0.00714	0.00471***
	(0.00132)	(0.00136)	(0.00141)	(0.00534)	(0.00139)
Size of Largest Rebel Group	4.66e-05***	3.98e-05***	4.65e-05***	2.92e-05	2.15e-05
	(1.39e-05)	(1.52e-05)	(1.61e-05)	(6.11e-05)	(1.42e-05)
Number of Groups		0.226**	0.303**	0.706*	0.0733
		(0.109)	(0.126)	(0.384)	(0.137)
Middle East			-2.850***		
			(1.038)		
Asia			-2.218***		
			(0.723)		
Africa			-1.145		
			(0.734)		
Americas			-1.015		
			(0.869)		
Constant	-1.426***	-1.935***	-0.350	-4.785***	-1.080***
	(0.231)	(0.317)	(0.693)	(1.122)	(0.352)
Observations	320	306	306	121	185

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

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