Humanitarian Assistance and the Duration of Peace after Civil War

Neil Narang University of California, Santa Barbara and the Center for International Security and Cooperation (CISAC), Stanford University

The principles of humanitarian assistance dictate that aid be distributed in accordance with need while remaining neutral with respect to the political stakes. However, these principles have unique implications in the postconflict 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 article describes a simple logic for how humanitarian aid can inadvertently undermine peace by creating a revisionist party with the incentive to renegotiate the postwar settlement. The empirical expectations of the theory are tested using a panel dataset of cross-national humanitarian aid expenditures in civil conflicts since the end of the Cold War. As the theory predicts, postconflict states treated with higher levels of humanitarian assistance exhibit shorter spells of peace; however, this effect only occurs after conflicts that ended with a decisive victory.

In the aftermath of the Rwandan Civil War, nearly 1.5 million Hutu refugees crossed into Zaire and Tanzania.[1] Within weeks, the international community began delivering billions of dollars in aid to provide every basic humanitarian and developmental service to the victims of the conflict. 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 to rest, recuperate, 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.

The possibility that well-intentioned humanitarian relief might 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. At one extreme, several critics have claimed that the humanitarian enterprise contributes to the very suffering it aims to redress by creating a moral hazard problem, wherein warring parties anticipate future aid provisions and become emboldened to reassert their interests expecting to be relieved of their costs of war (Anderson 1999; Gourevitch 1999; Kuperman 2008; Luttwak 1999; Polman 2010). And indeed, beyond Rwanda, such claims appear to describe the side effects of humanitarian aid in other postconflict situations quite well. For example, after the Cambodian-Vietnamese War in the mid-1980s, aid provided to the fleeing Khmer Rouge enabled militants to fortify themselves in camps, reconstitute a fighting force, and revisit another 10 years of war and terror upon the Cambodian people.

But while humanitarian aid 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 wars 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 placed it among the top 10% of postconflict recipients after the Cold War, yet there appears to have

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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, 3).

Can international humanitarian assistance inadvertently undermine peace when administered as a postconflict 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 reinitiate a violent conflict while other times they might choose to honor the previous settlement.

In this article, I propose a theory and provide some empirical evidence for how humanitarian aid may inadvertently undermine peace in the aftermath of civil war. The theory follows from what I identify to be a fundamental contradiction in the global humanitarian model: although the principles of humanitarian assistance dictate that aid be distributed in accordance with need while remaining neutral to the political stakes, these principles are prone to contradiction in the postconflict 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 postwar 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 need to be targeted by aid providers. Conversely, I expect 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 assistance, thus leaving the postconflict distribution of power relatively unaffected.

The remainder of this article 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

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 study commissioned by the World Bank (Collier 2003).[2] In the study, the authors report that a typical country reaching the end of a civil war faces a roughly 44% risk of returning to war within five years. The reason, they suggest, is that the same factors that caused the initial war are usually still present after the war has ended. And indeed, several studies show that countries emerging from war that had low average income (Walter 2004), rich resource endowments (Collier and Bank 2000; Collier and Hoeffler 2002; Doyle and Sambanis 2000; Dubey 2002; Elbadawi and Sambanis 2002), hostile neighbors providing external support, large population emigrations (Salehyan 2007), and mountainous or forested terrain (Fearon and 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. More worrisome is that conflict in the previous period can often feedback and inflame these risks—forming a vicious circle of civil war.[3]

But there is also a second important dimension of the conflict trap. While roughly 50% of civil wars that end ultimately recur, there is dramatic variation in the duration of peace among the peace spells that fail.[4] The mean duration of peace is roughly 12.5

[2]See Collier (2003, 43) for a thorough review.

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

[4]Figure A1 in the online appendix plots the distribution in the duration of peace after all civil war between 1945 and 2004.

years; however, there is considerable variation among these failures (the standard deviation is 15.26 years). For example, the longest peace has lasted over 57 years in Paraguay, while the shortest spells of peace lasted only 15 days in Yugoslavia-Croatia in 1991 and 30 days in Sudan from 1999. And although the longest spells in the top quartile lasted more than 15 years, the shortest spells in the bottom quartile lasted less than 1.4 years, suggesting that most instances of peace tend to be surprisingly brief.

Why is it 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? Existing scholarly research attempting to explain civil war recurrence and the duration of peace have fallen into roughly two categories. The first 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 a 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 and Diehl 2001; Toft and Security 2006). This may be because indecisive outcomes leave all sides capable of resuming the fighting and no side fully satisfied, whereas decisive victories generally leave at least one side incapable of challenging and both sides with little uncertainty about who would win in another round.

Relatedly, studies by Doyle and Sambanis (2002), Hartzell, Hoddie, and Rothchild (2001), Walter (2004), Fortna (2004), and Dubey (2004) have found that the *duration of civil war* is significantly related to the postwar peace. Similar to the logic above, 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* effects the durability of peace. For example, Doyle and Sambanis (2000) and Dubey (2002) have both found that civil wars with higher death tolls are more likely to resume, perhaps because deadlier conflicts are symptomatic of more intractable disputes or because higher death tolls make it more difficult to reconcile. But other evidence appears to suggest the opposite (Fortna 2004; Hensel 1994; Werner 1999), which also seems reasonable given that higher costs should provide more information about the likely outcome of a second war.

Finally, there is conflicting evidence that *ethnic divisions* can affect both the probability of civil war and the recurrence of war. It seems intuitive that peace may be harder to keep after conflicts that matched different ethnic or religious groups, 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, Hartzell et al. (2001) and Dubey (2002) found no difference.

A second category of research has sought to determine if the international community can discourage the resumption of violence by investigating the impact of various third-party interventions. For example, following Walter's (1997) finding that civil wars are more likely to end in negotiated settlement when third parties guarantee the safety of the belligerents, Fortna (2004) found that peacekeeping missions are associated with longer spells of peace after controlling for the degree of difficulty across cases. Fortna's results largely confirm previous findings by Doyle and Sambanis (2002) and Hartzell et al. (2001) that international peace-building missions can keep peace.

However, various studies have also found surprisingly negative results for other peace-building measures. For example, there is some evidence that while third-party mediation has a short-term impact on decreasing the likelihood a crisis will recur, it may have the perverse effect of increasing the long-term probability of crisis recurrence (Beardsley 2008). Similarly, there is growing evidence that although refugee camps provide valuable assistance to vulnerable populations, larger refugee diasporas in neighboring states appear to be associated with longer civil conflicts in refugee-sending states (Salehyan 2007). Together, these findings may support the claim that if the international community seeks a lasting peace, an optimal, albeit unpleasant, strategy may be to "give war a chance" (Luttwak 1999).

That some third-party interventions may actually have the perverse 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 conflict. As a result, humanitarian assistance has rapidly become a core component of modern peace building. For example, since the end of the Cold War, the amount of humanitarian aid reported through the OECD Development Assistance Committee (DAC) has increased nearly 1,400% in real terms from US$796 million in 1989 to well over US$11 billion 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 and Minear 2004).

Yet, as noted earlier, policy makers and practitioners have grown increasingly weary of the negative side effects generated by even well-intentioned humanitarian assistance (Anderson 1999; Gourevitch 1999; Kuperman 2008; Luttwak 1999; Polman 2010; Terry 2002). Basic resources like food, water, and medical supplies may certainly help mitigate suffering, but if the 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 conflict may actually be undermining peace and increasing the amount of suffering over time.

However, this does not imply that policy makers should simply give up on humanitarian aid all together. If the negative effects of aid on the duration of peace after civil war are conditional—as the theory and evidence in this article suggest—then it may be possible to allocate relief in a way that limits the negative effects.

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 postwar settlement. The theory here aims to qualify this claim 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 postconflict 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 aid is *most* likely to reignite conflict and undermine peace in postconflict environments where the previous contest ended with a decisive military victory for one side (either the government or rebel army).

The remainder of this section develops this argument in two parts. First, I describe a baseline bargaining model of war initiation 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 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 resolve uncertainty in a less manipulable forum than the bargaining table before agreeing to terms prematurely (Filson and Werner 2002, 2007; Powell 2004; Slantchev 2004; Smith and 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 and Brett 2000; Walter 1997).

Importantly, in the postconflict context, the very fact that belligerents 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 they could have secured a better deal by continuing to fight and learn about their opponent's capabilities and resolve, the war would never have terminated. Similarly, if either party believed that 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 was reached through the information provided by fighting during the war. Thus, the peace agreement that represents the postconflict 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 postconflict 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 enough with the status quo that it is willing to fight? One clear source of dissatisfaction would be a change in the distribution of power after the initial peace was struck. The postconflict 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 postconflict status quo would result in a better outcome than the one they initially agreed to, this could create an incentive 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 settlement.

Notice that the logic here requires that the underlying distribution of power must change sufficiently between two parties in a civil conflict *relative* to the underlying distribution of benefits represented by the postconflict 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 as neither would benefit from 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 sufficiently 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. This distinction is critical because it makes clear that exogenous shifts in power are not sufficient to undermine peace in the aftermath of civil war. Rather, power transitions (between a government and a rebel group) 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 makes a credible challenge.

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 postconflict distribution of power and the distribution of benefits, it may create a revisionist party with the incentive to challenge the postconflict status quo.

The Role of Humanitarian Aid

The idea behind humanitarian assistance is straightforward: 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 and Minear 2004). In practice, the 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 industry standard 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 need alone—remains the most significant determinant of humanitarian action. 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 assistance to further their own policy. 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. 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'' (1999, 5). On this point, Duffield (1994) and Anderson and Duffield (1998) have argued that humanitarian provisions have often filled so great a proportion of civilian needs in conflict that significant local resources have been freed up and reallocated towards the war. Indeed, Luttwak (1999) has argued that the Palestinian peace process has been repeatedly undermined for exactly this reason: humanitarian aid provided by 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 illustrated by the camps on the Thai-Cambodian border and the safe zones during the Bosnian Civil War (Boyd 1995; Landgren 1995; Woodward1995).[5] And, finally, several analysts have argued that relief provisions have facilitated conflict by directly providing food, medical supplies, and logistical support to the frontlines (Anderson and Duffield 1998; Atkinson 1997; Cooley and Ron 2002; de Waal 1994; Gourevitch 1999).

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. For example, humanitarian assistance provided during the peace spells punctuating the Arab-Israeli conflict have overwhelmingly gone to feed and shelter Arab refugees fleeing Israeli zones in the former territory of Palestine—a population for whom the UN has created a dedicated agency, the UN Relief and Works Agency (UNRWA). Similarly in the Bosnian War, humanitarian assistance was almost exclusively allocated to the neediest Bosnian Muslim populations who were surrounded in safe havens and on the verge of decisive military defeat (Woodward 1995: 319–25). However, while this allocation principal is certainly consistent with the humanitarian imperative to provide assistance in proportion to need, it also contradicts the core principle of neutrality, which requires that agencies not take sides 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. Such was the case in Israel following the Oslo accords, where a doubling of humanitarian disbursements by the UNRWA appears to have directly facilitated the enlistment of Palestinian refugees by armed organizations and effectively retarded efforts towards a sustainable peace (Luttwack 1999).[6] Similarly, in Bosnia, according to Woodward, the ''humanitarian objective'' made possible a blatant contradiction: the moral task of protecting the most vulnerable victims led to a refusal to confront the strategic implications of humanitarian relief and allowed Bosnian forces on the verge of collapse to turn safe areas into de facto military bases (Woodward 1995).

These cases, as well as others, suggest that this contradiction may have particularly important implications in the postconflict 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 contexts where peace was established following a decisive military victory by either the government or 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 previous 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 postconflict status quo.[7] However, when aid is provided following civil wars that did not end decisively, it should create less risk of peace failing because the costs of war to each party will be more equal. In these cases, humanitarian relief will also be distributed more equally, and it will not substantially alter the distribution of power relative to the postconflict 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:

> *H1*: 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
>
> *H2*: 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 costs from a second war. Why, then, might the provision of aid following a decisive victory not

[5]In Bosnia, analysts argued that safe zones created to provide relief services prolonged fighting and resulted in nearly 20,000 deaths in and around the enclaves (Woodward 1995).

[6]According to the OECD, UNRWA disbursements of humanitarian aid to the West Bank and Gaza Strip increased roughly 120% the year after the Oslo Accords, from US$82.25 million to US$179.45 million.

[7]One might suppose that aid provisions simultaneously increase the cost of war, as renewed fighting may cause aid organizations to withdraw. However, existing studies have overwhelmingly shown that aid continues through conflicts and may increase with the intensity of violence (Berman, Shapiro, and Felter2011; Narang 2011; Nielsen et al. 2011). Moreover, studies of Rwanda, El Salvador, Sri Lanka, and Angola suggest organizations are unlikely to decrease aid in expectation of violence (Chauvet 2003; Muscat 2002, 53; Uvin 1998).

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 relief after decisive victories will increase the *risk* of peace failing by sparking a new crisis. That is, 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.[8]

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, 2006) between January 1, 1989 and December 31, 1999. The key dependent variable is *the duration of peace*, defined from the date on which a civil war terminated to the date fighting resumed.[9] Note that the data codes peace as failing if a new war occurs in the same country involving the same 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 it is considered censored.

Including only cases of peace that started between 1989 and 2000 has several advantages. First, with respect to starting 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 and Sergenti 2008). As a result, including cases before 1989 would be inappropriate because the treatment was qualitatively and quantitatively different during the Cold War. Second, with respect to the end of the observation period, requiring a break in fighting before 2000 allows us to observe whether peace lasts for at least five years after a cease-fire (Fortna 2008). Beyond this, many key controls are unavailable.[10]

To estimate the relationship between humanitarian aid and the duration of peace in different postwar settings, I employ duration models to estimate the effect of aid—a time-varying covariate—on the risk of peace failing in a particular year. I employ both Cox and Weibull models to check if the results are robust to the different assumptions.[11]

Data used to estimate the amount of humanitarian aid disbursed in each postconflict year is taken from the OECD DAC data on Official Development Assistance (ODA). Specifically, I use only the humanitarian-aid component of the DAC2a ODA disbursements. Within the 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. The data includes (1) bilateral disbursements from DAC members,[12] (2) aggregated non-DAC member disbursements, and (3) aid activities financed through multilateral institutions and international NGOs.[13]

Disbursements of all bilateral and multilateral humanitarian aid are aggregated for a total estimate

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

[9]Fortna (2008) codes peace as failing if Doyle and Sambanis (2000, 2006) code a new war in the same country and research indicates it involved the same or similar parties, or UCDP code a restart to a war previously terminated.

[10]Note that I also conducted additional analyses on the extended period following 2004 using multiple imputation techniques and confirmed the results are robust to the extended time period.

[11]The Cox model estimates the probability that a peace-spell will fail at time t based on a set of covariates and given that peace has survived until t, without assumptions about the shape of the hazard function over time. The Weibull model makes the same estimation and can be preferable in small datasets; however it makes the restrictive assumption that the baseline hazard rate is monotonically rising or falling over time.

[12]For the 24 members of the DAC during the study period, see www.oecd.org/dac/dacmembers.htm.

[13]The data includes outflows from the World Bank, regional development banks, and several UN agencies, including UNHCR, UNAID, UNDP, UNRWA UNICEF, World Food Program, and several others. The list also includes 50 of the largest international NGO's. See http://www.oecd.org/dataoecd/36/16/31724727.pdf.

of disbursements in each recipient for each year.[14] Disbursements are recorded in constant 2007 prices and exchange rates to adjust for inflation and changes between that currency and the US dollar. In the analyses below, I also log-transform these values because the variances are not homogenous. The transformation yields a more normal distribution closer to the assumptions of parametric statistical tests.

In total, the sample for includes 94 cease-fires from 1989 to 1999 that lasted at least one month. Splitting the 94 spells of peace into calendar years over which the level of aid can vary produces 560 observations. Of these, 54 failed with the recurrence of civil war.

Endogeneity Bias and Selection Effects

The provision of humanitarian assistance across postconflict states is not random. Aid organizations and donors are likely to make allocation choices based on where humanitarian relief is most needed or likely to be most effective. This raises two important concerns for estimating the impact. First, aid disbursements may be endogenous to the duration of peace. If the amount of aid is determined based on donors' expectations for a short or long peace, then any relationship between the level of aid and the durability of peace may be flowing from the latter to the former. Second, aid may be disbursed based on other observable indicators that are themselves correlated with the durability of peace. For example, organizations may allocate aid more intensively based on the number of refugees or casualties from the prior conflict. If these variables are then correlated with the duration of peace, omitting them could lead to underspecified models.

Both forms of strategic selection may lead to biased estimates. However, in the case of humanitarian assistance, I argue that it is unlikely donors make allocation choices in a given year based directly on the duration of peace (endogeneity bias). This is because the amount of aid distributed each year following a civil war must be determined prior to observing if and when a peace eventually fails. Organizations can, however, provide assistance based on beliefs about how likely a certain peace will last, but these expectations must be formed indirectly based on observable indicators that are correlated with the duration of peace. To limit bias from strategic selection, the ideal test would include any variable correlated with the amount of aid allocated in a given year and the likelihood of the peace continuing past that year, and it is also causally prior to the treatment (King and Zeng 2006, 2007; Rosenbaum 2002). If, after controlling for these factors, the relationship between humanitarian aid and peace is greater than the baseline survival time, we can be more confident that the results support the expectations.

To account for potential confounds and to understand how the effect of humanitarian assistance compares to other predictors of the duration of peace after civil war, I include a variety of controls commonly used in the peace-building literature (Doyle and Sambanis 2000; Fortna 2004; Gilligan and Sergenti 2008). In addition to the two individual components of the interaction term (total humanitarian disbursements 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 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.[15] Summary statistics for all variables can be found in the online Appendix Table A1.[16]

Results and Discussion

This section discusses the main findings. Table 1 begins by estimating the relationship between

[14]Objections may be raised to aggregating these components since bilateral aid can be allocated through governments more often than multilateral. However, previous work has shown this is not necessarily the case postconflict, where bilateral provisions often bypass the state (Seybolt 2009). Nevertheless, I ran the estimations on each component and found similar effects across type. This is because both components are highly correlated, perhaps due to conditionality by donors demanding allocation to needy populations.

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

[16]Modeling selection in this way may not address unobserved heterogeneity. However, even if the true effect of humanitarian aid is not perfectly identified, the goal here is to present evidence consistent with the direction of causation outlined by the theory while also providing evidence to show the effect is mediated in a way that is consistent with the causal claim. Nevertheless, the online Appendix Table A8 implements an instrumental variable approach using out-of-region natural disasters (Ramsay 2011). Although the results are consistent, out-of-area natural disasters is a poor instrument for humanitarian aid in wars that ended nondecisively.

TABLE 1 Effect of Increasing Humanitarian Aid on the Risk of Peace Failing after All Civil Wars (1989–2004), Cox Estimates

Variables	Model 1 Cox Hazard Ratio (RSE)	Model 2 Cox Hazard Ratio (RSE)	Model 3 Cox Hazard Ratio (RSE)	Model 4 Cox Hazard Ratio (RSE)	Model 5 Cox Hazard Ratio (RSE)	Model 6 Cox Weibull Hazard Ratio (RSE)	Model 7 Cox Weibull Hazard Ratio (RSE)
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

Note: Robust Standard errors in parentheses. *$p < 0.05$; **$p < 0.01$.

humanitarian aid 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 aid on conflict recurrence is mixed based on the anecdotal evidence in the policy literature. In some cases, aid appeared to lead to the recurrence of war, while in others it did not. My own theoretical expectations are agnostic with respect to an average effect, positing instead that the effect will be strongly mediated by how the prior war ended.

Table 1 displays the results for seven different model specifications, each estimating the relationship between humanitarian disbursements and the risk of conflict recurrence over time. In all cases, hazard ratios are reported rather than coefficient estimates. Hazard ratios are interpreted relative to 1, where ratios greater than 1 indicate variables that increase the risk of peace failing and ratios less than 1 indicate variables that decrease the risk of peace failing. For example, if a dummy variable has a hazard ratio of 0.5, that variable decreases the risk of peace failing by 50%, meaning it tends to be associated with longer peace. 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.

Regardless of model specification, there appears to be no significant relationship between the amount of humanitarian aid provided after a civil war and the risk of peace failing. The results of Model 1, which includes the full list of covariates discussed above, suggests that postconflict states that received higher amounts of humanitarian aid were at no greater risk of relapsing into a second civil war *on average*. This noneffect is stable across model specifications that gradually remove control variables to check for collinearity. If humanitarian aid can undermine peace after civil war, there appears to be little systematic evidence of such an effect from a general analysis across 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 postwar 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. It is possible the analysis above disguises this effect by lumping these two types of environments together, creating greater variance in the estimated effect of aid. Indeed, a simple bivariate correlation between humanitarian aid and the duration of peace—without controlling for selection—appears negative following decisive military victories (-0.162) and positive following negotiated settlements and stalemates (0.098).

Table 2 provides a more direct test of the hypothesis 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).[17] I follow the distinction for civil war outcomes made by Doyle and Sambanis (2000) and coded by Fortna (2008). As before, I run seven different model specifications—each estimating the relationship between humanitarian aid and the risk of conflict recurrence—but this time interacting the level of aid with decisive victory.

The results support the theory. Regardless of model specification, increasing humanitarian 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 statistically significant at the 5% level ($p = 0.046$). More substantively, this means for every one-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 significance levels drop slightly (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 $p = 0.085$).

Figure 1 illustrates the impact of humanitarian assistance on conflict recurrence following decisive victories versus nondecisive stalemates or settlements by plotting the predicted survival curves of conflicts treated with the median amount of aid. Notice the median survival time for peace spells in which aid was sent to decisive military victories (dashed) is approximately three years from the end of the previous conflict, while the median survival time following stalemates and settlements is censored, as 90% of cases in this condition do not fail within 16 years.

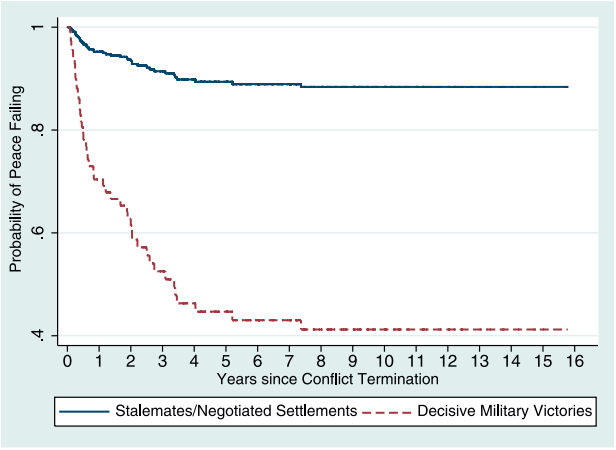
[17]Note that I purposely do not distinguish which group won decisively because the general theory here is agnostic to the exact winner. If the costs are concentrated among supporters of the government (like after the Rwandan Civil War), a disproportionate share of aid can compel it to seek a revision just as it would following a decisive victory in which the rebels lost. In the online appendix Table A6, I reran the analysis in the two separate subsamples to show that, as per the theory, it does not matter which side lost decisively.

TABLE 2 Effect of Humanitarian Aid on the Risk of Peace Failing after *Decisive/Nondecisive Victories* (1989–2004), Cox Estimates

Variables	Model 1 Cox Hazard Ratio (RSE)	Model 2 Cox Hazard Ratio (RSE)	Model 3 Cox Hazard Ratio (RSE)	Model 4 Cox Hazard Ratio (RSE)	Model 5 Cox Hazard Ratio (RSE)	Model 6 Cox Weibull Hazard Ratio (RSE)	Model 7 Cox Weibull Hazard Ratio (RSE)
Humanitarian assistance ×	2.241*	1.942	1.969	2.064	2.121	2.016	1.989
Decisive victory	(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	1.17010	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

Note: Robust Standard errors in parentheses. *$p < 0.05$; **$p < 0.01$.

FIGURE 1 Impact of Humanitarian Aid on War Recurrence Following Decisive Victories versus Stalemates/ Settlements



Note: Plotted are the predicted survival curves of conflicts treated with the median amount of aid. Source for data on humanitarian-aid disbursements is Organization for Economic Cooperation and Development's (OECD) Development Assistance Committee (DAC2a).

Humanitarian aid is uniformly associated with a greater risk of peace failing following decisive victories at any moment. Interestingly, the plots also indicate that this risk is greatest in the years immediately following conflict for both subsamples before asymptoting around five years.

Also consistent with the theory, the coefficient on humanitarian disbursements alone—controlling for whether provisions were disbursed following a decisive victory—is associated with little or no additional risk of peace failing. That is, aid provided following non-decisive wars has no significant effect on the risk of peace failing, as the hazard ratio on aid is statistically indistinguishable from the baseline rate of 1.

It is worthwhile to note that the hazard ratios on many other covariates are largely consistent with the previous literature. Decisive victories, peace treaties, and the duration of the previous war are robustly correlated with a lower risk of peace failing, while the presence of lootable resources is robustly correlated with a higher risk of peace failing.

These results are remarkably robust. Table A2 in the appendix estimates the same specifications using a Weibull model for small-sample sizes. In all cases the hazard ratios remain relatively unchanged. However, the results suggest an additional cause for concern. The coefficient on the shape parameter is negative and significant, suggesting the baseline hazard rate is decreasing over time (around −0.45 with a p-value of 0.00). Since the greatest risk of failure is immediately after termination, and because the level of aid is generally highest immediately after a civil war, it is possible that the relationship between the interaction term and risk of peace failing is spurious: confounded by time.[18] To investigate this, I reran all models controlling for time and found the direction and significance of the interaction term and its components do not change.

In Table A3 of the online appendix, I also show that the results are consistent across subsamples of peace lasting at least two months, three months, four months, five months, six months, and one year, a concern worth investigating since very short ceasefires may not signal to the international community that the conflict is "over." Table A4 demonstrates the results are robust to the inclusion of additional covariates, specifically the number of refugees and the internally displaced (Salehyan 2007, 2008). Also consistent with the theory, Table A5 shows the direction and significance of the key interaction term does not change depending on whether the government or rebel group lost decisively. As argued before, as long as any one side suffered a disproportionate share of the costs and can expect to receive a disproportionate share of the aid, increasing humanitarian provisions should increase the risk of peace failing with another conflict. Interestingly, however, the magnitude of the effect is greater after victories by the government. This matches the recent historical record, as the most fragile postconflict recipients of humanitarian aid are those in which rebels overwhelmingly lost. Finally, I performed a robustness check using an alternative measure for the postconflict distribution of power constructed by Cunningham et al. (2009) in Table A7, which codes the fighting capabilities of the rebel group relative to government at the end of the war. Importantly, using this measure does not change the direction or significance of the results.

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

[18] I thank an anonymous reviewer for bringing this possibility to my attention.

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 article was twofold. First, I sought to discipline existing claims by outlining a theoretically coherent mechanism through which humanitarian aid may interact with the conflict-bargaining process such that sides might sometimes elect to reinitiate conflict while other times they might choose to honor the previous settlement. Drawing on bargaining models of war, I argued that aid should only create a revisionist party after wars that ended decisively. Under these conditions, aid will asymmetrically increase the power of one party relative to the other due to 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 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 evidence that humanitarian aid can undermine peace when administered after civil war. Consistent with expectations, I found that humanitarian aid is *most* likely to undermine peace after civil wars that ended with a decisive military victory for either the government 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.

These results have important implications as humanitarian assistance has become a core component of postconflict reconstruction. Despite claims that humanitarian relief is antithetical to peace building, I show that—far from being an inherent feature of humanitarian giving—the negative consequences are neither unavoidable nor random. Rather, there may be preexisting conditions that systematically lead to this negative side effect. Investigating the conditional nature of other negative effects associated with aid may be a fruitful path for future research. Similar to evaluating the side effects of a medical treatment, identifying segments of the international population that are poor candidates for treatment may be preferable to abandoning an otherwise effective strategy. A potential empirical challenge for this research, and one confronted here, is identifying causality using observational data. However, this article hopes to demonstrate that analysts can use coherent theories of an underlying process—like war—to add confidence in a causal claim by deriving a testable set of mediating conditions ex ante.

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Neil Narang is an Assistant Professor in the Department of Political Science, University of California – Santa Barbara, Santa Barbara, CA 93106-9420 and Stanton Junior Faculty Fellow, at the Center for International Security and Cooperation (CISAC), Stanford University, Stanford, CA, 94305-6055.