Rape and the Failure of Peace: The Destabilizing Force of Sexual Violence in Civil War

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Abstract

Existing scholarship on post-conflict peace duration fails to account for the level of sexual violence within conflicts as a predictor of the risk of returning to war. This paper presents a new theory, linking the prevalence of rape to post-conflict instability, with the resumption of fighting as a hard test of social and political weakness. Through the merging of two data sets on peace duration and sexual violence within civil conflicts, I offer a unique quantitative, cross-national analysis of the relationship between sexual violence and peace stability. My findings support the theory, and indicate that high rates of sexual abuse in war are indeed correlated with shaky post-conflict peace. The results suggest a need for further analysis and the incorporation of violence against women into security studies.

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1 Introduction

Academic research on civil war has offered explanations of why civil war begins, why it continues, and how it can end. Many scholars focus on understanding why some conflicts end while others are unable to achieve a lasting peace as ceasefires repeatedly fail. Notably, Virginia Page Fortna (2004, 2008) has provided convincing evidence that peacekeeping allows opposing sides in civil conflicts to overcome commitment problems and restore stability to the polity. However, her groundbreaking study takes no account of sexual violence in civil war. Fortna is far from alone in omitting this variable, as most conflict scholarship makes no mention of this specific type of civilian-targeted violence. Sexual violence is a massive topic and needs to be incorporated into all aspects of civil war theory to gain insight into why it is sometimes rampant, while in other conflicts there are few reports. This paper offers a first look at the question of how sexual violence in civil war affects the duration of post-conflict peace.

2 Why Study Sexual Violence?

Civil war scholars generally highlight the costs of conflict as important variables, by measuring the number of battle and civilian deaths, and number of internally displaced persons. Any study failing to include control variables for the level of violence would be open to charges of omitted variable bias driving their findings. However, I am aware of no quantitative studies that either attempt to control for the amount of sexual violence committed in a conflict when testing for the war duration or the success of post-conflict reconstruction, or provide evidence that measures of sexual violence are already captured by death or displacement variables. This omission likely stems from several related sources: the general dearth of feminist scholars in mainstream security studies, the absence of sexual violence from quantitative political science literature more broadly, and the failure to recognize sexual violence as a qualitatively different type of violence than battle, or even civilian, deaths.

Sexual violence is present in every conflict, as no country (including those at peace) has eradicated the rape and sexual assault perpetrated daily against women

(MacKinnon 2006). Furthermore, in a single year cross-national analysis, Butler, Gluch, and Mitchell (2007) find that civil conflict significantly increases the rate of sexual violence among security forces. Over fifty percent of civil wars involve "routine" or "widespread" reports of sexual violence as determined by the U.S. State Department's annual reports on human rights (Cohen 2010. See Appendix for coding information). This is clearly not a trivial level of violence, and yet little is known about the societal costs of sexual violence during conflict. Current findings on the effect of death toll (both battle deaths and civilian deaths) suggest that bloodier wars lead to less stable peace. Specifically, Fortna notes, "While longer wars yield longer peace, the same cannot be said for more deadly wars. Rather, peace is more likely to falter after more costly wars. This finding is relatively robust, but is sometimes only marginally significant" (2008, 177).

While death toll is obviously important, there is not theoretical justification for why existing measures should account for the rate of sexual violence. Lacina and Gleditsch (2005) assert that death toll fails to capture the myriad dimensions of cost borne by civilians and society during conflict. Indeed, given the specific targeting of women and the social degradation and shame that accompany rape and sexual assault, sexual violence seems a different monster than civilian killings. Seifert (1996) argues there is a unique political meaning of sexual violence, asserting that rape "destroys the physical and psychological existence of the women concerned and, moreover, inflicts harm on the culture and collective identity of the whole group, ethnicity, or nation under attack" (35). This view of rape as an attack against society conducted through the violation and destruction of women's bodies is clearly different from other forms of violence. Moreover, sexual violence elicits very different responses, often blaming and shaming victims. This is a unique societal and legal response to survivors, in that victims of torture, amputation, or murder are never accused of "asking for it" (MacKinnon 2006, Estrich 1997). Beyond the acts of sexual violence themselves, the context in which rape and assault occurs is fundamentally different from other forms of violence.

Additionally, current measures of violence are already gendered, as death toll during conflict (even accounting for civilian deaths) disproportionately measures violence

as experienced by men in war (Ormhaug et al. 2009).[1] The Women's Commission for Refugee Women and Children notes:

> Although more men die in battle than women, women and girls are deliberately targeted for rape, torture, sexual slavery, trafficking and forced marriages in conflict zones. In addition to being abducted or forced to become sex slaves, women and girls are also forced to become servants and combatants for the armed militia groups (from Farr 2009, 11).

Given the gendered bias of death toll as a control variable, it seems likely that the occurrence and effect of sexual violence is not successfully captured by the number of casualties of war. At the very least, this variable should be tested to rule out the possibility that the exclusion of mostly female experiences of violence in quantitative scholarship systematically biases results.

Finally, sexual violence should be studied because it presents a serious security issue for the international community.[2] International relations scholars have shifted their focus to civil war because of the recognition that wars within states destabilize the international system. As civil wars are now more prevalent than interstate wars, they impact more lives than interstate wars and constitute a major threat to international peace and security (Fearon and Laitin 2003). Sexual violence exists in every society, presenting a threat to over half of the population, and yet gender-based violence is not treated as a true security issue or human rights violation (MacKinnon 1994). Civil war does not create an environment in which sexual violence spontaneously occurs, but rather one in which new opportunities are created and often encouraged for the bodies of women and girls— and to a lesser extent men and boys— to be violated under the guise of war. Just as civilian deaths are now considered a challenge to international security, sexual violence must be reframed as such and incorporated into the study of civil war (Hudson and Den Boer 2002).

The rest of this paper proceeds as follows. In the third section I review the existing literature on post-conflict peace duration and sexual violence in conflict. In

[1] Although men are more likely to die during conflict, women are more likely to die once the war has officially ended (Ormhaug et al., Plümper and Neumayer 2006).

[2] A recent article by Carter (2010) further articulates the argument that rape should be studied in international relations because of its use as a war weapon.

the fourth section I explicate my theory relating sexual violence to post-conflict peace, and state the hypotheses I derive from the theory. In the fifth section I describe the data and methods used to investigate the questions driving my research, and in the sixth section I present the results of my analyses. Lastly, I offer a discussion of my findings and the implications for future research.

3 Related Literature

This study connects several strands of literature from civil war scholarship, as well as some work in the fields of political psychology and feminist legal scholarship on sexual violence. Existing quantitative work on civil war duration is led by Fearon (2004), who discovered several factors tied to the length of civil war across countries. Notably, he found "sons of soil" dynamics, rebel reliance on contraband for funding, and insurgencies based around the borders all significantly increase war duration, while ethnic diversity, per capita income, level of democracy, and the type of war (either identity or ethnic conflict) have no predictive power.

On the other side of the conflict, Fortna's (2004, 2008) work on peace duration following conflict identifies factors related to successful post-conflict peace. Her findings demonstrate the large and robust positive effect of peacekeeping missions on peace duration, meaning peacekeeping decreases the risk of returning to conflict. While this is her primary finding, she further demonstrates the positive effect of outright victory, treaties, and war duration, as well as the negative impact of contraband financing of rebels. As mentioned earlier, her models yield suggestive although not robust findings of more deadly conflicts negatively affecting post-conflict peace.

Relatedly, Walter (2004) examines those factors that predict whether civil war recurs, and posits that in those societies where individuals find little hope in peacetime, they are more likely to fall back to conflict. Walter finds that political openness, as measured by Polity III democracy/autocracy data, and human misery, as measured by infant mortality, are both highly predictive of renewed conflict. Notably, this work is concerned with whether conflict breaks out at all, but not with the length of peace spells, and the independent variables that exert an effect are often different from those that are predictive in Fortna's work.

Existing work on violence documents the efficacy of indiscriminate versus selective violence in civil wars. Kalyvas (2004) defines indiscriminate violence as the selection of victims "on the basis of their membership in some group and irrespective of their individual actions" (97). Kalyvas (2000) further argues, "civil war is not just a function of body count" (2) and thus attempts to distinguish between types of violence and the logic motivating their implementation. He posits that the goal behind all violence is to gain compliance and discourage defection to the opposition through fear and intimidation. While selective violence is typically more effective, it is also more costly and therefore at times, indiscriminate violence is a strategic and rational alternative. Kalyvas's conclusions are not undisputed however, and Lyall (2009) presents some limited data on Russian shelling of Chechen villages to suggest that indiscriminate violence can effectively suppress rebel activity.

While this work takes us closer to understanding how violence can vary in its purpose and implementation, it is still quite limited in scope. Most indiscriminate violence is measured as shelling, as this is by its very nature not targeted toward specific individuals. However, by Kalyvas's definition, sexual violence is typically indiscriminate, targeting primarily women for their gender identity, either alone or in conjunction with their ethnic, racial, religious, or political identities. Although qualitative evidence suggests that certain wars involve rape as punishment, meaning women are selectively targeted, even in these wars rape is simultaneously conducted indiscriminately (Farr, 2009). Yet unlike mass shelling, sexual abuses are conducted interpersonally in close range. This study will provide new insight into the effect of a quite different type of indiscriminate violence on peace outcomes.

Current scholarship addressing the presence of sexual violence in civil war attempts to identify when sexual violence occurs rather than its post-conflict impact. Several researchers have used case studies to categorize conflicts with high levels of rape. Unfortunately, many of these studies suffer from selection on the dependent variable. Wood (2006) includes "negative" cases, or those conflicts that lacked significant reports of sexual violence, in order to more rigorously analyze when rape occurs in war. She documents significant variation in the types of war with sexual violence, suggesting that its prevalence cannot be predetermined by conflict or country attributes. This casts doubt on a theory of certain cultures being specifically

inclined to higher rates of sexual violence in war.

Wood discovers that in certain cases sexual violence is promoted as a weapon, while in other civil wars it is discouraged by military and political leaders. In Bosnia-Herzegovina, Serbian soldiers utilized ethnically targeted rape, forced prostitution, and forced impregnation of non-Serbian women as a tool of genocide to create a new generation of Serbian men; in the Rwandan genocide, Tutsi women were raped and assaulted at the urging of Hutu leaders. In contrast, in Sierra Leone acts of sexual violence were committed indiscriminately across ethnic lines and against women and girls of all ages. Wood notes:

> Sexual violence in these cases appears to vary substantially in prevalence; in form; in who is targeted (all women, girls and men as well as women, or particular persons, perhaps members of an ethnic out-group); in whether it is exercised by combatants from a single party or more generally; whether it is pursued as a strategy of war; where it occurs (in detention, at home, or in public); in duration; whether it is carried out by a single perpetrator or by a group; whether victims are killed afterward; and whether its incidence varies with other forms of violence against civilians or occurs in a distinct pattern. (Wood, 317).

From her case studies, it appears that sexual violence occurs in many different contexts in civil wars and it seems that its presence or absence in any conflict cannot be easily predicted by conflict attributes.

Farr specifically studies wars with high levels of rape, or extreme war rape, in which perpetrators intentionally injure and psychologically torture their victims, to attempt to identify and document patterns. She finds cross-country similarities in how war rape is perpetrated and argues that specific contextual factors contribute to high levels. Farr asserts that patriarchal institutions in civil and military society, the devaluation of women and gender inequality contribute to a culture of impunity for gender-based violence in wartime. As noted earlier, this work suffers from selection on the dependent variable, making this claim somewhat dubious. Further, she argues that the occurrence of civil war in developing or transitional countries facilitates war rape, as the conflict environment differs from wars of earlier times. However, this is

not a testable claim given the lack of systematic information on sexual violence in previous wars. Moreover, given the existence of rape in conflict for centuries, and the notable example of high levels of sexual abuse and mass rape committed by soldiers of all sides in World War II, it seems unlikely that wars in developing countries are unique in their abuse of women (Burds, 2009).

Cohen (2010) and Butler, Gluch, and Mitchell (2008) have conducted two of the few quantitative studies of the causes of sexual violence among security forces and/or rebels. In their single-year analysis, Butler et al. find that not only does civil war increase the prevalence of sexual abuse, but that agent control significantly minimizes the rate of sexual abuses perpetrated by government security forces. Furthermore, their work suggests a negative relationship between democratic governance and sexual abuses by security agents. Cohen uses cross-country multi-year data to determine why some civil wars exhibit such high rates of sexual violence while others do not. Her models suggest a structural problem as well, namely the soldier recruitment method. Her findings mirror social psychology work in the U.S. context suggesting that sexual violence is a tool to bond men (and sometimes women) to one another. In wars where soldiers and/or rebels have been forcibly recruited, rape becomes a means of creating unit cohesion.

Research on the social and psychological effects of sexual violence also bears on this project. Jefferson (2004) argues that experiences of sexual violence are poorly addressed, if at all, meaning women are not effectively reintegrated into post-conflict society, but left alone to deal with the psychological trauma from the war, compounded by the assault on their own bodies. Although sexual violence can be understood as a personal experience, it still exists within a social and political context. Koopman (1997) asserts the importance of recognizing the potential for traumatic experiences to have political meaning. Her work centers on the intersection of political and psychological approaches, specifically how traumatic political events may have psychological ramifications, but notes the equal importance of examining the political consequences of psychological trauma.

Kessler's (1999) work on the societal costs of Post-Traumatic Stress Disorder (PTSD) in the field of psychology dovetails with this line of reasoning. He argues persuasively that while the current literature on this topic is primarily in the U.S.

context, in countries emerging from war this is likely to be an urgent issue, as severe trauma to civilians destroys social capital leading to an unstable civil society. As so many survivors of sexual violence experience PTSD, higher rates of sexual violence in war could very well be linked to instability in post-conflict society. Finally, Skjelsbaek (2001) and Ward and Marsh (2006) argue that post-conflict peace will not be successful unless recovery from sexual violence becomes an integral part of all peace operations. While each of these works speaks to the centrality of sexual violence to post-conflict reconstruction, there is little work to systematically interrogate the connection between sexual violence and peace. This study advances a theory linking mass experiences of psychological trauma during war to post-conflict outcomes.

4 Theory and Hypotheses

This paper is a first attempt to present quantitative evidence of the effect of sexual violence on peace duration. While much of the work described above addresses the question of when rape occurs widely in war, nothing specifically discusses the logic or rationality of sexual violence as it relates to the outcome of war. My research will speak to the efficacy of this specific type of indiscriminate violence, as measured by peace duration which is presumably desirable for any victor. Kalyvas's logic for the use of indiscriminate violence does not seem to fully hold in the case of sexual abuses, as rapes are also committed during times of peace when there is no opposition to which citizens could defect. However, it is possible that rape is employed specifically in war as a tactic to terrorize populations, while rape committed by non-combatants occurs simultaneously. This is perhaps the most difficult challenge to the study of sexual violence: it is always present, but the context or frequency with which it occurs may shift or vary as societies move between peace and conflict. Furthermore there is a question of whether the reason why rape occurs would matter. Should the outcome of women being brutalized and raped en masse be different if the state urges this to happen versus simply turning a blind eye? This paper will not be able to answer all of these questions, but it presents an opportunity to interrogate the larger relationship between sexual violence in civil war and post-conflict stability.

Existing work identifies several mechanisms by which ceasefires may fail in both

interstate and civil wars. Fortna specifically highlights aggression, fear and security dilemma spirals, accidental escalation, and political exclusion as means through which war can recur. Aggression and political exclusion do not present specific challenges to a theory of sexual violence limiting peace duration, and controls of civilian death during conflict as well as war outcome should help control for any bias. Mistrust plays an important role in fomenting war via entering a security dilemma spiral even though both sides may prefer to be at peace, or by decreasing the ability of either side to overcome an accident and hold the peace.

Accidents are more likely to occur with spoiler effects, whereby principles have poor control over agents who do not commit to the peace. Particularly relevant to this work is Fortna's observation that "Abuse by soldiers against civilians may continue after a cease-fire, especially in conflicts in which targeting the civilian population has been either a military strategy or a means of financing an army" (85). This poses a potential problem for this study. If sexual abuses of civilians are continuing, or even increasing, during peace spells this may affect the durability of the peace, but it is difficult to incorporate into this study. Fortna further notes the heightened risk of continued civilian abuses occurring in identity conflicts. If spoiler dynamics are at work post-conflict, this may be a strong challenge to my theory as it could be correlated with the presence of sexual violence during conflict. This mechanism presents the greatest challenge of endogeneity to my work, and I will attempt to respond to some of its implications.

None of these mechanisms are likely to act in isolation, and several may occur simultaneously. However, each of these explanations focuses on only the belligerents: behavior of former fighters, differences in political power between former combatant groups, and mistrust of each side by the other. It seems that even accounting for these different mechanisms to resume fighting, the social situation in which they take place could matter. This paper attempts to fill this theoretical void by arguing that the strength of civil society matters in the creation of a stable peace.

While the behavior of the fighting parties is clearly important, we can consider civil society to be an institutional constraint on the resumption of conflict. When the civil society in which belligerents are located is strong, peace will be more durable, whereas belligerents within a weak civil society will have lower barriers to the re-

sumption of conflict. The theory informing this study is that mass sexual abuses in civil war will keep victims from being full participants in civil society, in turn weakening social capital and stunting the success of post-conflict reconstruction. This may seem like a stretch to some, but it is difficult to imagine that in a context of extreme sexual violence and woefully inadequate resources for survivors of rape, frequently combined with community or familial (sometimes violent) punishment for victims, civilians would emerge unscarred from civil war.

An environment of mass sexual violence may negatively impact even those who are not themselves victims, as research on secondary trauma would suggest (Nelson and Wampler, 2000, Motta et al., 1997), thereby further spreading the impact of sexual abuses across the population. Without access to the specialized treatment necessary in the wake of such trauma, civil society will likely suffer. Moreover, this theory is integral to the U.N. Security Council Resolution 1325 on Women, Peace, and Security which, "recognizes the important role women can and should be playing in pre and post-conflict societies, as a means to prevent conflict in the first place and as a means to achieve sustainable peace once conflict has ended" (Jefferson, 14). Sexual violence is a destabilizing factor within civil society, as it destroys social bonds between groups and individuals. Therefore we should observe that higher rates of sexual violence are related to shorter peace duration while controlling for all other factors.

A competing theory could argue that sexual violence is costly, and it should deter the return to war.[3] This competing hypothesis will be tested by the models. Furthermore, my models will allow me to test the claim that sexual violence does not have a separate effect from other forms of cost to civilians, such as death and displacement. By including these variables in the models I will be able to demonstrate the independent effect of civilian rape on peace duration.

Testing the relationship between sexual violence and the resumption of fighting is a hard test of this theory. Presumably there are intermediate signs of instability in civil society, however the breakout of war is a high threshold of a weak society. This brings two benefits: first, peace duration is relatively easily determined and not

[3]Indeed, Cohen argues that committing acts of sexual violence is costly to fighters and this cost is the reason perpetrating rape is a bonding mechanism.

prone to accusations of inaccurate measures or invalid proxy measures, and second it biases my research against finding a relationship as sexual violence could weaken society without leading back to war.

I therefore present two hypotheses to test my theory.

H_1: Higher rates of sexual violence will be negatively related to peace duration.

As my theory is grounded in the concept of civilian trauma, and trauma does not depend on the identity of perpetrators, the actors committing abuses should not change the outcomes. Rather, effects on peace duration should be a result solely of the prevalence of sexual violence. I test this claim with my second hypothesis:

H_2: Controlling for perpetrator identity should not affect the outcomes.

A corollary of the second hypothesis implied by the theory is that regardless of which side is victorious after the conflict, the damage of sexual abuses against civilians will negatively affect post-war peace. Therefore, even if the victors engage in rape, this should not be more strongly linked to the breakdown of ceasefire. Luckily, my data can bear on this implication of the theory, through the interaction of perpetrator and victor identity.

5 Data and Methods

The data for this project combines Fortna's 2008 data set on peace duration with Cohen's data on sexual violence in civil wars. Cohen's data spans civil wars from 1980-1999 and Fortna's covers cease fires from 1989 to 1999; each uses slightly different battle death thresholds to define conflicts, therefore some cases were removed in the merging process. After cleaning the data, 97 conflicts with 132 observations remained for study. As is the case in Fortna's work, a single war may have several observations if peace was negotiated and broke down again multiple times.

This paper utilizes Cox Proportional Hazards models to model the survival rate of peace with a host of covariates. Covariates are included in an attempt to control for,

and isolate, the role of sexual violence prevalence in civil conflicts. A full description of variables are included in the Appendix, and the most important are described briefly here.

Sexual violence is defined in this study using Cohen's definition, borrowed from Wood (2003). Cohen notes: "Sexual violence encompasses rape, but also includes a larger set of sexually oriented actions. I use Wood's definition of sexual violence as a 'broader' category that includes rape, non-penetrating sexual assault and coerced undressing." Cohen borrows the Political Terror Scale from Butler, Gluch, and Mitchell and rescales it from 0 to 3, determining levels of sexual violence from State Department Human Rights Reports. Each conflict is assigned a score for the highest level of sexual abuse experienced at any point during the fighting. Scores were determined by use of the terms "rape," "sexual assault," and "sexual abuse," with prevalence levels ranging from there being no reports to "widespread" or "systematic," or sexual violence "used as a weapon of war." Clearly this provides only a rough sorting of cases, and importantly a score of 0 does not indicate a lack of sexual violence in a given conflict. However, this ranking provides a means of differentiating between those wars where sexual violence is not vastly different from peacetime abuses and those where war creates an environment of impunity for the violation of sexual autonomy.

Table 1 indicates the variation in sexual violence across conflicts. Slightly over half of the cases have high rates of sexual abuse (scoring a 2 or 3 on the amended Political Terror Scale). Furthermore, the distribution of countries across violence levels is unsurprising and fits with popular understandings of sexual violence in civil wars. The Democratic Republic of Congo, Rwanda, Liberia, Sierra Leone, and Bosnia all fall under the highest ranking, while Lebanon, Iran, and the conflict between Northern Ireland and Great Britain had no reports. These rankings have face validity given media discussion of the fighting in each of these conflicts. Furthermore, the presences of several countries in multiple columns suggests that the rate of sexual violence is not culturally pre-determined, alleviating some concern over the biased distribution of sexual violence in war.

[TABLE 1 HERE]

The data set includes binary indicators for rebel and government perpetrators of sexual violence. Each war with a violence score of 1 or higher has at least one of these two perpetrator types associated with it, indicating that all wars in which rape is reported is on some level "institutional rape," as these are institutional actors rather than individuals committing atrocities. This suggests that sexual violence as measured is not simply a result of a state of lawlessness in which civilians increase their number of attacks on other civilians (although this is quite possibly true), but one in which combatants are targeting civilian women and girls, and to a lesser extent men and boys.

The perpetrator variables are useful, but they present a potential problem: it is not uncommon for victims of sexual abuse to not know to which group, if any, their attackers belong (Farr). Secondly, in wars with multiple rebel groups, we cannot determine which rebel groups were responsible for the abuses. These issues mean the perpetrator variables are not perfect measures, but nonetheless are useful in at least indicating which groups were primarily responsible for the abuses.

As sexual violence is a specific cost of war, I include other more traditional measures of human cost: death toll and displacement. The death toll is calculated as both battle and civilian deaths and is then logged and included in all models. I include displacement in several of the models, simply as the log of the number of refugees and Internally Displaced Persons (IDPs), as a separate dimension of the cost of war. The other control variables used are: peacekeeping, contraband funding of rebels, victory, treaty, war type (ideological versus identity), war duration, factions (whether there were more than two groups fighting), Polity scores at the start of war, infant mortality rate, whether there had been a past agreement, government army size, mountainousness of terrain, geographic contiguity with a permanent member of the UN Security Council, former colony of permanent member of the UNSC, aiding of rebel group by a neighboring country, and whether a permanent member of the UNSC (major power) was involved in the war.

6 Results

As a preliminary investigation of the relationship between sexual violence prevalence and peace duration, I first analyzed the average peace duration (in days) for each level of sexual violence. Using the mean peace durations for each level of violence, I calculated the probability of peace failure on any given day using the binomial model:

$$P(X=x \mid p_i) = p_i^{(x-1)}(1\text{-}p_i)$$

As can be seen in Table 2, the average duration of peace decreases across conflicts as the level of sexual violence increases. Using Likelihood Ratio Tests between the unrestricted model and restricted models (in which I separate by level of violence), these differences are found to be statistically significant (p=0.007). This preliminary finding suggests that there is a relationship between sexual violence and peace duration in support of my hypothesis. However, this finding may be biased, as there are right-censored cases used in the calculations of the mean peace duration. This simple comparison, while suggestive of a relationship, cannot conclusively demonstrate that any such pattern exists.

[TABLE 2 HERE]

Table 3 shows a second set of descriptive statistics on the relationship between sexual violence and the breakdown of peace. Through analyzing not only the proportion of wars within each violence level that return to conflict, but the speed with which wars relapse, this data provides further support for my first hypothesis. Clearly wars with the lowest levels of sexual violence are less likely to relapse, while peace is much more likely to fail after conflicts with the highest rates of abuse. It is not clear, however, that this is a linear increase as there is little difference between the highest two levels. Finally, over time it appears that wars with higher levels of violence are more prone to rapid peace failure than those less prevalent sexual violence. Again, these are very basic comparisons with no accounting for censoring of data or potential omitted variables, but the first cut analysis of the data supports my hypothesis.

[TABLE 3 HERE]

[FIGURE 1 HERE]

As one of the primary concerns with this investigation is that sexual violence may simply be captured by other measures of cost of war, I plot civilian and battle deaths and total displacement versus the level of sexual violence in conflict. Figure 1 indicates that there is not a strong relationship between the level of sexual violence and these two separate measures of cost. This suggests that the prevalence of sexual violence is likely not captured by models that exclude rape as an explanatory variable in favor of other measures of cost to civilians.

Turning specifically to survival analysis, I begin by plotting the Kaplan-Meier curves for each level of sexual violence. As Figure 2 demonstrates, as sexual violence increases there appears to be a general pattern of increasingly rapid peace failure. Unlike the previous descriptive statistics, these calculations do account for censored cases, and a significance test yields a high chi-square value, suggesting there is a significant difference in the peace duration of conflicts by the level of sexual violence (p=0.03). However, as this preliminary test is without any controls, it is possible that controlling for other factors will alter the preliminary relationship between sexual violence and peace duration.

[FIGURE 2 HERE]

To determine whether these preliminary findings are robust to other variables, I incorporate variables on sexual violence into Fortna's Cox models of peace duration. I begin with her best fitting model (Model 1) and then incorporate my sexual violence measures.[2] After running my analyses, I find that the differences between sexual violence scores of zero and one are not substantive or significant, therefore I recode the four-point scale into three levels (1, 2, and 3). The results in Model 2 confirm the findings of preliminary analyses. The hazard ratio on the both the second and third level of sexual violence is significant (p=0.03 and p=0.01); in a Cox Proportional

[2]While Fortna uses an averaged Polity scores over the five years prior to the start of war, her data has many missing cases. Therefore I opted to use the Polity score at the start of war in my models, in order to limit the number of deleted cases.

Hazards model, the hazard ratio indicates the relative risk of failure, meaning that the ratio of 2.56 translates to a 256 percent increase in the risk of returning to war for conflicts with sexual violence scores of 2. Shifting from a level 1 to 3 in this model leads to a 434 percent increase in risk of peace failure. With the incorporation of the sexual violence measure, the death toll variable is no longer significant at even the 10 percent level, suggesting that once sexual violence is controlled for, bloodier wars are no longer correlated with more rapid peace failure. Furthermore, this model yields a significantly better fit than Fortna's model, as determined through Likelihood Ratio Tests (p=0.01).

The baseline model (Model 1) includes several variables that fail to ever achieve significance in Fortna's models, which raises concerns that the models are over-specified. Therefore I test her baseline model against more parsimonious models by removing single covariates and then removing covariates two, three, four, five, and six at a time. Due to the changing size of the dataset, I test the various models against one another using the five year lagged democracy variable. Through further Likelihood Ratio Tests of the models against Fortna's baseline, I parse down the model into an equally well-fitting new baseline model (Model 3), by removing the following variables: infant mortality, government army size, mountainousness of terrain, contiguity with a permanent member of the UNSC, former colony of a permanent member of the UNSC, and whether a major power was involved in the war. Then I reintroduce the single-year Polity scores. In the more parsimonious model, sexual violence is again an important addition, as is clear in the results for Model 3. The hazard ratios remain quite large, and while they are no longer significant at conventional levels, the p-values are still quite low (p=0.09 and p=0.05).

[TABLE 4 HERE]

Unfortunately, each of these models remains problematic due to the assumptions made in their calculation. Cox models require proportionality of the covariates in order to be properly estimated, and this requirement fails on multiple variables in each of the first three models. Although Fortna suggests that because her peacekeeping variable is proportional this is of no concern, I find this violation of the models' assumption to be quite problematic. Therefore I stratify the data on two of

the covariates: whether there had been a past agreement and whether the conflict terminated in victory for one side. Using an analysis of variance test, the stratified model (Model 4) is significantly better in fit than Model 3 (p=0.00) and, each of the covariates now passes the proportional hazards test in Model 4 (although several do yield low p-values).

[TABLE 5 HERE]

Tables 5 and 6 contain a series of models to check the robustness of the findings in my first models. In Model 5, I explicitly test my second hypothesis that perpetrator identity should not matter, by including the binary indicators for whether government forces and/or rebel soldiers committed abuses. These variables are not mutually exclusive, therefore it is possible that in a given conflict both sides engaged in sexual violence against civilians. As predicted, neither of these produce significant results. Although insignificant, the opposite signs on the coefficients present an interesting puzzle. Perhaps there is a difference by perpetrator and this model was simply unable to unmask it. As the hazard ratio on the government perpetrators covariate is greater than 1, while the rebel perpetrators hazard ratio is less than 1, it is possible that the lack of a relationship in the model stems from the higher likelihood of governments winning civil wars, and the possibility that when the victorious side engages in sexual violence there is a more rapid peace failure. This would cut against my civilian trauma theory in favor of a theory of rational response to illegitimate victors who abuse civilians during conflict.

Model 6 addresses this concern by incorporating a new binary indicator on whether the winning side committed sexual abuses. I combine cases of government victors and abusers with cases of rebel victors and abusers. I did not separate out the two phenomena as there simply are not enough observations to merit this: there are 27 outright victories and only 14 cases in which the victorious side engaged in sexual violence. The variable is coded one if this was true, and zero in any other circumstance (for example if the war ended in a truce or if the victorious side did not engage in sexual violence). The null finding on this covariate and the continued null findings on the perpetrator variables lead me to accept my second hypothesis.

[FIGURE 3 HERE]

A competing hypothesis could contend that sexual violence is related to shorter peace duration because it is a symptom of underlying gender inequality rather than a cause itself, as Farr suggests. If highly unequal societies are simply less stable (work by Hudson suggests this may be so), perhaps sexual violence levels in conflict are not leading to shorter peace duration, but the two variables are just correlated. This would be a particularly damning claim to my theory, as it would suggest that sexual violence is itself determined by societal subjugation of women. I attempt to answer this potential challenge in Model 7.

Unfortunately the Gender Empowerment Measure (GEM)— which measures inequality in political participation and decision making, economic participation and decision making, and power over economic resources— does not exist prior to 1995 and lacks data in many developing countries. As such, it cannot be leveraged in this data set. As a proxy measure for general women's equality, I include the difference between men and women's labor force participation.[4] Obviously this is only a rough proxy, but it can allay concerns over whether sexual violence is merely doing the work of broad gender inequality in the model. As can be seen in Figure 3, it does not appear that gendered labor force participation is highly predictive of the level of sexual violence in conflict. For the lowest three levels of violence, the distribution of labor participation is quite similar, while for those countries with the highest levels of war rape, there is a smaller gap in labor participation by gender. This indicates that there is no clear relationship between high levels of rape and gender inequality that would nullify the results. Incorporating the gendered labor force participation variable into the Cox models (Model 7) demonstrates the continued predictive power of sexual violence on post-conflict peace duration, and the null relationship between gender inequality and peace duration.

Model 8 includes the displacement variable to test whether sexual violence has an effect separate from that of displacement. While previous models determine that rape is indeed different from death toll, it is possible that rape and displacement are not distinguishable in their effects as both are traumatic experiences affecting survivors

[4]This data is taken from the World Bank (available at http://data.worldbank.org). There are a handful of country cases where this number is negative, meaning women participate in the labor market at a higher rate than men. In these few cases, the difference is quite small.

of the war. Controlling for displacement increases the hazard ratio on the second level of sexual violence to 3.56 (p=0.03) and decreases the hazard ratio on the highest level of violence to 2.53 (p=0.17). Interestingly, the directional impact of death toll flips, although it fails to achieve statistical significance. This is consistent with my theory of civilian trauma, and suggests that sexual violence and displacement are two separate forms of trauma, but both place states returning from war in greater peril. In Model 9 I include GDP per capita, as there is some literature suggesting a relationship between GDP and war, which yields similar results and finds no relationship between GDP and peace duration. Although Models 8 and 9 provide some evidence that perhaps the highest risk of returning to conflict occurs within those mid-scoring conflicts of sexual violence, as the other models do not have this finding I am reluctant to conclude that this is in fact the case.

[TABLE 6 HERE]

The final set of models in Table 7 takes a closer look at the effect of peacekeeping on peace duration, by interacting peacekeeping and sexual violence. It is possible that peacekeeping forces may be particularly important when sexual violence is widespread, although conversely, the presence of peacekeeping missions could increase rates of sexual violence when it is already high. There is no cross-national data on peacekeepers engaging in sexual abuses, but MacKinnon (2006) notes the alarming increase in rape and trafficking in women in Bosnia after the arrival of peacekeeping forces.

Model 10 reduces both the sexual violence and peacekeeping variables to binary indicators in order to make the results more interpretable. In this model, the hazard ratio of the sexual violence indicator remains large and significant, while the peacekeeping variable loses significance, and the interaction fails to achieve significance as well. However, the combining of multiple categories of peacekeeping may be problematic. In Models 11 and 12, I once again use Fortna's six level peacekeeping variable, but treat it as ordinal. For simplification in the earlier models, the peacekeeping data is treated as an interval scale, but this assumption may not be strictly justified, and there is no reason to assume the difference between a political and monitoring mission to be the same as the difference between an interpositional and

multidimensional mission. Model 11 demonstrates that this is in fact the case. None of the levels achieves statistical significance except the highest level of peacekeeping. This radically alters Fortna's conclusion, as it appears that controlling for sexual violence, peacekeeping missions have no statistical effect except at the highest level.

Finally, Model 12 interacts the sexual violence binary indicator with the factored peacekeeping scale. We should be cautious about interpreting this model, as it is clear that the interaction of these variables results in extreme coefficients due to the small number of cases in each cell. However, the direction of the coefficients does suggest how these factors interact with one another. In this model the sexual violence indicator loses significance although it is still quite suggestive (p=.11). Once again, the presence of peacekeepers is only significant at the highest level of the scale, confirming the previous model's finding that the scale should indeed be treated as ordinal.

The interaction between the sexual violence indicator and the peacekeeping scale provides an interesting result. When sexual violence is prevalent and there is the lowest level of peacekeeping present (political missions with only a handful of observers) the hazard ratio is extremely high at 10.79 although it does not quite achieve significance at conventional levels (p=0.06). The interaction of the two variables is not significant at the second or third levels of peacekeeping, but is significant at the highest two levels (multidimensional missions and enforcement missions). Multidimensional missions seem highly successful at reducing the risk of recurring conflict, while the interaction between sexual violence and enforcement missions runs in the opposite direction. However, as the highest level of peacekeeping is by itself associated with an incredibly low risk of war outbreak, this suggests that sexual violence and peacekeeping work in opposite directions such that enforcement missions are quite successful when there is little or no war rape, but are less effective at discouraging the renewal of violence when rape is widespread. The significance of the interaction between multidimensional missions and sexual violence is intriguing and provides support for my theory, as it suggests that when sexual violence is high, peacekeeping missions that specifically aim to strengthen civil society are able to mitigate the negative effect of violence on peace duration.

[TABLE 7 HERE]

Clearly the negative effect of widespread sexual violence on post-conflict peace remains robust across model specifications, in support of my theory. Several other variables are equally robust, and merit discussion. Notably, peacekeeping is related to longer-lasting peace, reducing the hazard of war by about 25 percent. Although the results are consistent with Fortna's findings, the final set of models does cast doubt as to whether we can model peacekeeping in this fashion. By treating the variable as a multi-level factor it seems that the type of peacekeeping mission is quite important and only the strongest missions truly keep peace.

Across models, war duration is positively related to peace duration, indicating that longer wars yield longer peace. Furthermore, wars that end in treaty do not resume as quickly. Democratic governance is at times associated with more rapid peace failure, although never by more than about seven percent. By stratifying on victory and past agreement we lose the ability to interpret these effects, but the unstratified models demonstrate the positive effects of both on lasting peace, although the latter variable does not achieve significance. Once the models are properly stratified to meet the assumption of proportionality, the effect of contraband funding found in Fortna's work disappears. The coefficient is still in the expected direction, but it is no longer significant across models.

The null finding on the contraband variable is particularly important, as it provides evidence against an alternative causal mechanism to the one presented in this paper. Existing work finds that when rebel organizations rely on contraband funding, rebels are more likely to commit acts of indiscriminate violence (Weinstein 2007). The low levels of commitment exhibited by resource-funded rebels could be related to peace time spoiler effects. If sexual violence were merely an indicator of conflicts in which rebels are likely to exhibit spoiler dynamics, by controlling for contraband funding the significance of the sexual violence level should disappear. As the sexual violence level remains robust while the contraband variable fails to achieve significance, it appears that a greater prevalence of sexual violence is related to more rapid peace failure, *ceteris paribus*. The results in each model specification lead me to accept both of my hypotheses that sexual violence leads to less stable peace regardless of perpetrator identity. Furthermore, the observable results suggest that this is due to the connection between sexual violence and unstable civil society, as demonstrated

by the mitigating effect of multidimensional missions during wars with high levels of wartime rape.

7 Discussion and Conclusions

The models in the previous section demonstrate the predictive power of sexual violence on peace duration. Specifically, controlling for other factors, as sexual violence against civilians by soldiers and rebels increases, peace is less durable. However, as with any study of this type, my findings are open to attacks on causality. Perhaps higher levels of sexual violence do not cause peace to fail more rapidly, but the finding is simply driven by some omitted variable that correlates with both. Given the robustness of my findings to the inclusion of all variables understood to correlate with peace duration, as well as the distinct difference between sexual violence and other measures of cost (death toll and displacement), this charge is not easily supported. More importantly, these findings suggest at the least, that sexual violence is a marker of wars that are difficult to end and that the occurrence of sexual atrocities should concern the international community. Few concrete steps have been taken to eliminate sexual violence or to seriously address its existence, but the relationship discovered here indicates a further reason why violence against women is an issue that affects everyone's security.

My findings suggest that it is in fact in leaders' interests to actively discourage sexual violence in conflict—a step no armies or rebels (to my knowledge) have taken. While terrorizing a population or allowing soldiers to act as they please may seem like a good strategy, if the goal of a fighting group is to take control of the state, sexual violence will undermine their control and increase the likelihood of their government returning to conflict. It is possible that this is not a concern for some fighting groups, if their goal is not to gain control of a stable country. Future work could attempt to control for this factor by identifying goals of the main fighting groups.

Although my findings present support for my theory, it is difficult to positively prove a causal mechanism from this type of analysis. Rather, the results of this study cut against several other possible theories. The insignificance of interacting victor with sexual violence severely undercuts any claim that civilians rationally punish

abusive victors by supporting the opposition in a return to war. The null finding on the relationship between contraband funding for rebels and peace duration also weakens an alternative theory of peace failure in which spoiler dynamics feed the breakout of war. And finally, the models presented here dispel any argument that sexual violence is simply a proxy measure for more violent or costly conflicts overall, as the negative effect of sexual violence not only remains strong as other forms of violence and trauma are added as controls, but is consistently associated with the highest risk of returning to conflict of all the cost variables.

Several data concerns remain in this analysis. Although this study is unique in its attempt to document cross-national trends, there is a resulting lack of cultural context. It is possible that high rates of rape in one state do not have the same meaning as in another, due to the varying conceptions of what constitutes rape, differences between pre-conflict and wartime sexual abuses, or the role of rape during war—for instance, whether rape is a strategy and tool of war implemented by rebel or government leaders, or is the rampant assault of women by soldiers without leadership intervening to stop it. Doubtless, the victims experience trauma in either situation, however given Seifert's assertion that the cultural meaning of rape matters, it is possible to imagine these cases having different magnitudes of influence on peace duration.

An important first step to answer these questions is to identify pre-war levels of sexual violence so that wartime levels could be modeled as a shift from the baseline. Unfortunately, I attempted to allay this concern by scoring countries pre-war rape rates using the same coding scheme as during war years. However, I have grave misgivings about this data and hence did not incorporate it into the analyses. The majority of cases are scored as zeros, and notably those countries that are not zero tend to be those where a previous civil war had high levels of sexual abuse. It is possible that pre-conflict abuse rates by state actors are comparable across cases, and as such all wartime rape rates represent deviations from the baseline. This conclusion fits with Butler et al.'s conclusion that civil war predicts higher rape rates. Alternatively, this may simply suggest that war is predictive of higher *reporting* of sexual assault and rape rather than higher rates of abuse. If conflict illuminates the problem of sexual abuse and causes organizations such as the State Department

to focus on the issue, it may be impossible to address the claim that war rape is predicted by peacetime abuse.

While my attempt to control for gender inequality does provide some relief for concerns that my findings are driven by cultures of gender inequality and patriarchy, labor force participation data is merely a proxy measure. The gender gap in labor force participation does not necessarily indicate the level of gender inequality, as it cannot capture whether men and women are segregated into different labor markets, the equality of wages, or the parity of the genders in political and social spheres. Female access to the labor market does suggest a certain level of gender empowerment, but future work should develop better cross-national measures for gender equality.

On the opposite side of conflict, post-war violence is missing from this analysis. This study essentially presumes there is no variation in sexual violence once ceasefire is declared. However, this assumption is laughably naive. Many cases have demonstrated the continuance, or even increase, in rape after peace is established. MacKinnon (2006) argues that this is not only a result of opportunity in a destroyed state, but that when soldiers return home they bring with them the "skills" learned at war, increasing the levels of domestic violence, sexual assault, and rape within fighters' communities. If post-conflict levels of abuse could be determined, this would help to strengthen a causal argument for the effect of sexual violence on peace duration, as the rate of post-conflict abuse should also be negatively related to the duration of ceasefire. A different coding scheme would need to be developed to address this question, as many peace spells are not long enough for variation to be identified in the Human Rights Reports compared to the level of sexual violence during the conflict.

A final implication of this work is the importance of understanding sexual violence beyond the context of war. The central finding of this paper is essentially the fact that violently targeting half of the population makes society less stable. There is no theoretical reason why this should only be true in post-conflict society. As MacKinnon argues, "Because so much violence against women takes place in what is called peacetime, its atrocities do not count as war crimes unless a war among men is going on at the same time" (2006, 261). The question of whether peacetime sexual violence undermines civil society would be much more difficult to study. No

country is currently undergoing civil war because women have risen up to protest the war against their sexual autonomy, yet this does not indicate that such rampant gendered violence is not negatively impacting states.

Although this is a preliminary study, the findings presented here indicate a strong negative relationship between the prevalence of sexual violence in civil war and the stability of post-conflict peace. The robustness of this result suggests the need for further research to more thoroughly interrogate the causal mechanism through which sexual violence leads to the breakdown of ceasefire. Future research will attempt to account for those factors that could not be tested in this paper, with the goal of providing a more complete account of why sexual violence differs from other forms of civilian-targeted violence, and how high rates of rape in civil war destabilize post-conflict society before the fighting resumes.

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8 Appendix A: Coding

Codings from Cohen (2010), all else from Fortna (2008).

Levels of Sexual Violence:
0 = No mention of rape, sexual assault, and sexual abuse
1 = There are isolated reports, some reports, reports, or there continued to be reports of rape, sexual assault, and sexual abuse
2 = There were numerous, scores of or many reports of rape, sexual assault, and sexual abuse, which were routine, common, or reported repeatedly
3 = Rape, sexual assault, and sexual abuse was widespread, systematic, used as a tool of war or a systematic weapon of war

Definition of Rape:
Sexual penetration, however slight, of the vagina or anus of the victim by the penis of the perpetrator or any other ob ject used by him or of the mouth of the victim by the penis of the perpetrator, where such sexual penetration occurs without the consent of the victim (Cohen, 3 from Klip and Sluiter 2005).

Definition of Sexual Violence:
Sexual violence encompasses rape, but also includes a larger set of sexually oriented actions. I use Wood's definition of sexual violence as a broader category that includes rape, non-penetrating sexual assault and coerced undressing (Cohen, 3, from Wood 2009: 308).

Peacekeeping
0= No peacekeeping
1 = Political mission- consisting of a special representative or a handful of observers. Also includes U.S. "political and peacebuilding missions" run through the Department of Political Affairs, as opposed to the Department of Peacekeeping Operations.
2 = Monitoring mission- unarmed missions, mandated to watch and report what they see. These are generally relatively small missions (personnel numbering in the

hundreds).
3 = Interpositional missions (a.k.a. traditional peacekeeping)- lightly armed missions, mandated to monitor but also to separate forces or to disarm and demobilize factions. These missions are generally somewhat larger than monitoring missions.
4 = Multidimensional mission- in addition to monitoring and interpositional roles, these missions include substantial civilian components to organize elections, monitor human rights, reform police, etc. Also includes transitional administration mission.
5= Enforcement mission- substantial military force mandated to use force for purposes other than self-defense. Not necessarily deployed with the consent of both sides.
In some cases there is more than one peacekeeping mission present at one time. The statistical analyses use the highest mission type present.

9 Appendix B: Tables and Figures

Table 1: Distribution of Civil Wars by Highest Level of Sexual Violence

No Reports	Some	Widespread	Systemic/Weapon of War
Azerbaijan	Chad	Afghanistan	Dem. Republic of Congo
Cambodia	Colombia	Algeria	Georgia-Abkhazia
Ethiopia	Croatia	Angola	Indonesia-East Timor
Iran	Djibouti	Bangladesh	Liberia
Lebanon	El Salvador	Myanmar/Burma	Rwanda
Mali	Guatemala	Burundi	Sierra Leone
Moldova	Guinea-Bissau	Central African Rep.	Tajikistan
Morocco-W. Sahara	Nicaragua	Congo-Brazzaville	Uganda-LRA
U.K.-N. Ireland	Pakistan	Ethiopia-Eritrea	Bosnia
South Africa	Papua New Guinea	Haiti	
Sri Lanka (JVPII)	Russia-Chechnya	India	
Yemen	Turkey-Kurds	Indonesia	
Yugoslavia-Croatia	Zimbabwe/Rhodesia	Mozambique	
		Peru	
		Philippines	
		Senegal	
		Somalia	
		Sri Lanka (Tamil)	
		Sudan	
		Uganda	

For the sake of space and simplicity, countries are only listed once in each category of sexual violence prevalence. In cases where countries had multiple conflicts falling under different levels of abuse, I attempt to differentiate conflicts from one another.

Table 2: Comparative Statistics: Peace Duration by Level of Sexual Violence

Rate of Sexual Violence	Average Duration of Peace (Days)	Probability of Failure	Log-Likelihood	# of Cases Censored
0 (n=19)	3198.84	0.0003	-172.34	12
1 (n=25)	2290.72	0.0004	-218.41	11
2 (n=38)	1504.05	0.0007	-315.99	9
3 (n=15)	1144.20	0.0009	-120.63	4
All levels (n=97)	1983.12	0.0005	-833.44	36

-2(LR-LU)= 12.14 χ^2 on 3 degrees of freedom
p=0.007

Table 3: Speed of Return to War by Level of Sexual Violence in Conflict

Rate of Sexual Violence	Returns to War	Within 1 Year	Within 2 Years	Within 5 Years
0 (n=19)	0.3684	0.1579	0.2632	0.3684
1 (n=25)	0.5600	0.4000	0.4400	0.5200
2 (n=38)	0.7632	0.3684	0.4737	0.6842
3 (n=15)	0.7333	0.4000	0.5333	0.7333
All levels (n=97)	0.6289	0.3402	0.4330	0.5876

Proportions are listed in each category. Categories are cumulative, meaning cases that return to war within one year are also counted in those that return within two and five years.

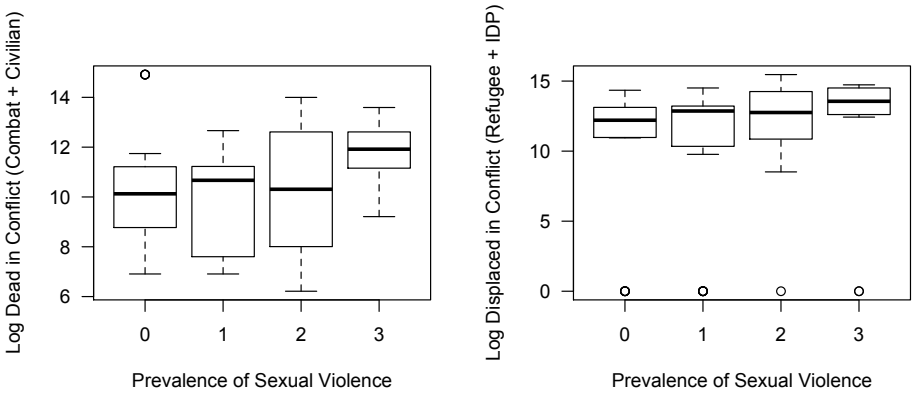

Figure 1: Sexual Violence and Other Dimensions of Cost

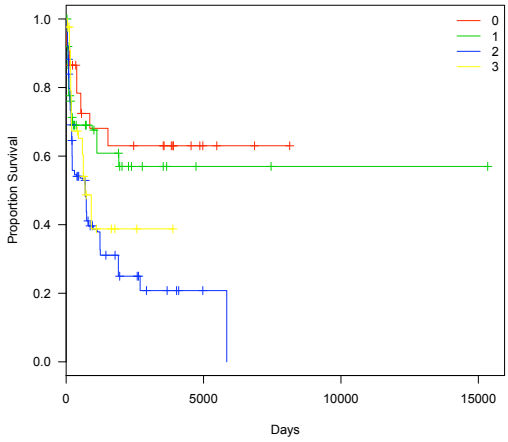

Figure 2: Kaplan-Meier Curves by Sexual Violence Prevalence

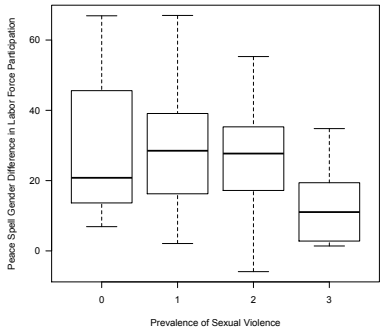

Figure 3: Gender Difference in Labor Force Participation by Sexual Violence Prevalence During Conflict

Table 4: Baseline Models

	Model 1	Model 2	Model 3	Model 4
Sexual Violence (Level 2)		2.56*	2.01†	2.28*
		(0.44)	(0.42)	(0.40)
Sexual Violence (Level 3)		4.34*	2.86†	2.41†
		(0.58)	(0.54)	(0.52)
Peacekeeping	0.85	0.74*	0.72**	0.74*
	(0.12)	(0.14)	(0.12)	(0.12)
Contraband	3.05*	2.76*	1.51	1.34
	(0.47)	(0.51)	(0.37)	(0.34)
Victory	0.14***	0.09***	0.14***	
	(0.49)	(0.52)	(0.47)	
Treaty	0.44	0.43	0.44†	0.48†
	(0.55)	(0.52)	(0.47)	(0.44)
Identity War	1.91	1.45	1.00	0.84
	(0.50)	(0.46)	(0.40)	(0.39)
Death Toll	1.21†	1.12	1.13	1.14
	(0.10)	(0.10)	(0.09)	(0.09)
Factions	0.86	0.79	0.71	0.69
	(0.54)	(0.63)	(0.42)	(0.39)
Democracy	1.02	1.05	1.05	1.06
	(0.04)	(0.04)	(0.04)	(0.03)
Infant Mortality	1.00	1.00		
	(0.01)	(0.01)		
Past Agreement	0.78	0.90	0.90	
	(0.39)	(0.35)	(0.34)	
Army Size	1.00	1.00		
	(0.00)	(0.00)		
Mountains	1.21	1.35		
	(0.18)	(0.21)		
P5 Contiguity	0.60	0.42†		
	(0.50)	(0.50)		
Former P5 Colony	1.93	1.63		
	(0.50)	(0.43)		
War Duration	0.91***	0.91***	0.92***	0.92***
	(0.03)	(0.03)	(0.02)	(0.02)
Neighbor Aids Rebels	1.09	1.38	1.12	1.16
	(0.53)	(0.51)	(0.42)	(0.39)
Major Power	0.60	0.53		
	(0.38)	(0.40)		
N	107	107	112	112
R^2	0.306	0.366		0.29
Wald (p-values)	0.00	0.00	0.00	0.00
Degrees of Freedom	17	19	13	11

For ease of interpretation, Hazard Ratios are reported rather than coefficients.

Robust standard errors in parentheses

† significant at $p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$

Table 5: Robustness Checks

	Model 4	Model 5	Model 6
Sexual Violence (Level 2)	2.28*	2.57†	2.57†
	(0.40)	(0.50)	(0.50)
Sexual Violence (Level 3)	2.41†	2.84†	2.84†
	(0.52)	(0.59)	(0.58)
Rebel Perpetrators		0.76	0.74
		(0.44)	(0.45)
Government Perpetrators		1.22	1.20
		(0.47)	(0.48)
Victor Engages in Rape			1.26
			(0.81)
Peacekeeping	0.74*	0.74*	0.74*
	(0.12)	(0.12)	(0.12)
Contraband	1.43	1.29	1.28
	(0.34)	(0.35)	(0.35)
Treaty	0.48†	0.47†	0.47†
	(0.44)	(0.44)	(0.44)
Identity War	0.84	0.81	0.81
	(0.39)	(0.41)	(0.41)
Death Toll	1.14	1.12	1.11
	(0.09)	(0.10)	(0.10)
Factions	0.69	0.79	0.80
	(0.39)	(0.46)	(0.46)
Democracy	1.06	1.06	1.06
	(0.03)	(0.04)	(0.04)
War Duration	0.92***	0.92***	0.92***
	(0.02)	(0.02)	(0.02)
Neighbor Aids Rebels	1.16	1.21	1.21
	(0.39)	(0.42)	(0.42)
N	112	112	112
Degrees of Freedom	11	13	14
Wald (p-value)	35(0.00)	37(0.00)	39(0.00)
R^2	0.25	0.26	0.26

For ease of interpretation, Hazard Ratios are reported rather than coefficients.

Robust standard errors in parentheses

† significant at $p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$

Table 6: More Robustness Checks

	Model 7	Model 8	Model 9
Sexual Violence (Level 2)	2.59†	3.56*	3.42†
	(0.49)	(0.59)	(0.67)
Sexual Violence (Level 3)	3.04†	2.53	2.66
	(0.63)	(0.68)	(0.76)
Rebel Perpetrators	0.77	0.84	0.78
	(0.45)	(0.51)	(0.54)
Government Perpetrators	1.27	1.11	1.07
	(0.50)	(0.50)	(0.50)
Victor Engages in Rape	1.24	0.75	
	(0.77)	(0.72)	
Gendered Labor Force Participation	1.01	1.00	
	(0.01)	(0.01)	
Peacekeeping	0.75*	0.73*	0.73*
	(0.12)	(0.12)	(0.13)
Contraband	1.27	1.67	1.57
	(0.35)	(0.37)	(0.37)
Treaty	0.46†	0.42*	0.41*
	(0.43)	(0.42)	(0.43)
Identity War	0.71		
	(0.44)		
Death Toll	1.11	0.97	0.99
	(0.10)	(0.11)	(0.12)
Displacement		1.11	1.10
		(0.08)	(0.08)
Factions	0.84	1.05	0.98
	(0.46)	(0.45)	(0.45)
Democracy	1.07†	1.09*	1.07*
	(0.04)	(0.03)	(0.03)
War Duration	0.92***	0.91***	0.91***
	(0.02)	(0.02)	(0.02)
Neighbor Aids Rebels	1.19	1.63	1.61
	(0.40)	(0.43)	(0.45)
GDP per capita			1.00
			(0.00)
N	111	105	106
Degrees of Freedom	15	15	14
Wald (p-value)	37(0.00)	39(0.00)	39(0.00)
R^2	0.26	0.30	0.29

For ease of interpretation, Hazard Ratios are reported rather than coefficients.

Robust standard errors in parentheses

† significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 7: Re-examining Peacekeeping

	Model 10	Model 11	Model 12
Sexual Violence (Dummy)	2.90*	2.58*	1.94
	(0.54)	(0.46)	(0.41)
Peacekeeping Dummy	0.53		
	(0.61)		
Treaty	0.43†	0.47†	0.47†
	(0.43)	(0.45)	(0.45)
War Duration	0.92***	0.92***	0.92***
	(0.02)	(0.02)	(0.02)
Sexual Violence*Peacekeeping	0.71		
(dummies)	(0.81)		
Peacekeeping (Level 1)		0.64	0.20
		(0.75)	(1.21)
Peacekeeping (Level 2)		0.48	0.21
		(0.64)	(1.27)
Peacekeeping (Level 3)		0.50	0.83
		(0.54)	(0.68)
Peacekeeping (Level 4)		0.23	1.06
		(1.38)	(1.13)
Peacekeeping (Level 5)		0.26*	0.00***
		(0.62)	(0.80)
Sexual Violence*Peacekeeping (1)			10.79†
			(1.24)
Sexual Violence*Peacekeeping (2)			4.22
			(1.36)
Sexual Violence*Peacekeeping (3)			0.45
			(1.03)
Sexual Violence*Peacekeeping (4)			0.00***
			(1.55)
Sexual Violence*Peacekeeping (5)			2.13e+07***
			(1.03)
N	111	111	112
Degrees of Freedom	15	18	19
Wald (p-value)	34(0.00)	37(0.01)	1206(0.00)
R^2	0.25	0.26	0.32

Covariates that fail to achieve significance have been dropped from this table.

For ease of interpretation, Hazard Ratios are reported rather than coefficients.

Robust standard errors in parentheses

† significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$