Sexual Violence by Government Security Forces: Can Peacetime Levels of Sexual Violence Predict Levels of Sexual Violence in Civil Conflict?*

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Abstract

Are levels of sexual violence committed by government security forces in a country prior to conflict predictive of levels of sexual violence in that country during conflict? Most of the scholarship on sexual violence focuses on the phenomenon during armed conflict, and in general the assumption made by these scholars is that conflict exacerbates the sexual violence problem. Cross-sectional analysis appears to support this assertion; however, we argue that the comparison group used by cross-sectional analyses is inappropriate for answering the question of whether conflict impacts the amount of sexual violence in a country. Instead, we propose that the appropriate comparison is between peacetime levels of sexual violence and conflict levels of sexual violence for the same country. To test this relationship, we employ data on sexual violence committed by government security forces in a sample of 170 countries for the time period 1999-2011, using a measure similar to that from Butler, Gluch, and Mitchell (2007). Then, we use a variety of descriptive and inferential statistical tests to examine the relationship between conflict from the UCDP/PRIO Armed Conflict Dataset and the level of sexual violence in a country. We find that for cases with variation in conflict across our time period, pre-conflict levels of sexual violence are predictive of conflict levels, but contrary to the common assumption, the prediction is no change in the level of sexual violence for most cases.

Keywords: sexual violence, rape, human rights, conflict, security forces, data, methodology

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Are levels of sexual violence committed by government security forces in a country prior to conflict predictive of levels of sexual violence in that country during conflict? In order to isolate the impact of conflict dynamics on this form of violence, studies of conflict-related sexual violence need to account for prior levels of such violence perpetrated on the population (Wood, 2006; Baaz and Stern, 2008). Therefore, the goal of this project is to examine how sexual violence committed by government security forces changes—or does not change— as a country moves from a period of non-conflict to a period of conflict.[1] We discuss how the current literature generally presumes this potential relationship while relying on theoretical approaches with implications for both non-conflict and conflict years.

Butler, Gluch, and Mitchell (2007) found, among other things, that civil conflict and civil war are related to higher levels of sexual violence perpetrated by security forces, but their cross-sectional study cannot answer how the level of sexual violence changed for a country as it entered its conflict period. Thus, we examine whether levels of sexual violence in peace-years predict the levels of sexual violence in conflict-years. We find that civil conflict is a significant predictor of higher levels of sexual violence when comparisons are made between peace and conflict years, across all country-years, which supports the general assumption of this literature. However, when we make inter-temporal comparisons of sexual violence levels in the few years prior to conflict and the succeeding of conflict, the modal case is that of "no change." Therefore, this initial examination leads us to question the assumption in much of the sexual violence literature that conflict increases the prevalence of sexual violence.

To examine this relationship requires data collection on sexual violence both in and out of conflict-years and the selection of appropriate statistical methods. We believe our research contributes to the literature on human rights violations on both fronts. Current

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[1]We do not develop a theory of how sexual violence committed by government security forces in non-conflict years is related that of conflict years, nor do we address potential shifts in the kind of sexual violence (Cohen, 2013; Farr, 2009).

cross-national data sources are limited in scope. The Sexual Violence and Armed Conflict (SVAC) data records levels of sexual violence during and in the aftermath of armed conflict in a state (Cohen and Nordås, 2014), for both governments security forces and other armed groups. Such data allow researchers to explore variation in levels of sexual violence across conflicts, and actors within a conflict, but do not capture the level of sexual violence prior to the conflict, which we believe contributes to our understanding of this phenomenon. The WomanStats Project provides data on the prevalence of rape and sexual assault in a country's population, but these measures are not specific to government security forces, nor do they have the time series data structure necessary to examine changes in the level of sexual violence (See Caprioli et al. (2009) for a detailed discussion of the data and project).[2] Another source of data on sexual violence comes from Butler, Gluch, and Mitchell (2007), who create an ordinal measure of the level of sexual violence committed by government security forces in a state following the template of the Political Terror Scale (Gibney and Dalton, 1996). These data include all available countries for the year 2003, so their analysis is limited to a single cross-national sample. Using a modified ordinal measure—which we describe in detail below—we expand the universe of cases to include data on sexual violence committed by government security forces for the years 1999–2011. We include all available country years, whether countries are experiencing conflict or not, thus allowing us to test the relationship between conflict and levels of sexual violence across both time and space.

The paper is organized in the following manner. First, the literature on sexual violence in armed conflict is reviewed to emphasize the importance of including pre-conflict years in our models of the presence of and variation in sexual violence in armed conflict. Next, we discuss our sources of data for our dependent variable, level of sexual violence, and our independent variable, conflict. Then, we analyze the relationship between peacetime

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[2]WomanStats Project Database (n.d.).

levels of sexual violence and conflict levels of sexual violence in two sets of models. The first set examines whether conflict is a significant predictor of our measure of the level of sexual violence in a country. The second set makes comparisons of the level of sexual violence in non-conflict and conflict years for a country. We conclude with a discussion of the importance of our findings for the study of sexual violence and ideas for future research.

1 Literature Review

Does the level of sexual violence (SV) committed by security forces for non-conflict years predict the levels of the same phenomenon during a conflict? The need to establish a relationship between pre-conflict and conflict levels of SV is apparent in the empirical literature. "Some simple hypotheses do not explain the puzzling variation in the extent and form of sexual violence in war" (Wood, 2006, 308). Some actors in conflicts exhibit very little SV, like the LTTE in Sri Lanka (Wood, 2009, 2012), while other actors exhibit SV on a massive scale, like Serbian forces during the Bosnian War (MacKinnon, 1994).

Yet, the majority of researchers interested in SV examine the phenomenon in the context of conflict, treating conflict—through various causal processes—as the primary driver of SV, or at least an exacerbating factor. There is little doubt that conflict adversely impacts civilians, women especially (Plümper and Neumayer, 2006; Leiby, 2009). Also, evidence of the character of SV in conflict settings suggests a causal link. Cohen (2013) notes the increasing prevalence of gang rape in conflict settings, and Farr (2009) describes the "extreme" character of SV in recent civil conflicts. However, without accounting for levels of SV prior to conflict, theories of SV in conflict remain muddled. We do not develop our own theory on the link between SV in peace and conflict years. Instead, we summarize existing theoretical perspectives on SV in conflict to demonstrate that these approaches make implicit assumptions about the relationship between SV in peace and conflict years

that have not been empirically examined, and may not support the assumption that conflict increases the prevalence of SV.

It is important to note that other conflict dynamics may produce an increase in SV. Conflict is often associated with a proliferation of armed actors that may not have existed in peacetime, including rebel groups and militias. These actors also vary in their use of SV, as recent work on pro-government militias demonstrates (Cohen and Nordås, 2015), but their behaviors undoubtedly contribute to the general observation that SV levels increase during conflict. In contrast, government security forces are consistently present in both conflict and non-conflict years, and it is an open question whether the SV committed by government security forces increases from pre-conflict to conflict years. Therefore, we examine these actors in isolation from pro-government militias and non-state actor groups, as described in Table 1, and we discuss the theoretical approaches to SV below with respect only to government security forces.

Opportunistic explanations suggest that SV is a product of the breakdown of social norms or the failures of law enforcement that accompany conflict. Rape and other forms of SV that occur in this context are the product of individuals' (for our purposes, members of government security forces) motivations to sate sexual desires or to extract material resources from the population through intimidation, as well as a lack of normative or institutional prohibitions against such acts. This opportunistic behavior may present itself in a variety of conflict contexts, for example in both ethnic and non-ethnic wars. Although the empirical evidence suggests that opportunism is not driving the propensity of state actors to rape during civil wars (Cohen, 2013, 470-471), the assumption that—if not monitored or sanctioned—members of security forces will perpetrate more SV underlies much of the literature. Theoretical explanations for cross-national variation generally fall into "institutional" and "instrumental" groups.

4

Institutional approaches emphasize the importance of institutional characteristics as drivers for violent behavior. Variation in the *repertoires of violence* employed by armed groups based on the differential incentives these groups face to control troops is proposed as an explanation for inter-armed group variation in the use and extent of SV (Hoover Green, 2011). Similarly, scholars taking a principal-agent approach to SV emphasize the impact that conflict has on the command-and-control structure of the military and other government security forces; if leadership is unable to monitor the behavior of ground forces, troops recognize this and are more likely to act out of self-interest. General opportunistic theoretical approaches predict that levels of SV will increase during conflict; however, principal-agent approaches highlight the fact that opportunities for SV are contingent upon two factors: the presence of conflict and a lack of institutional mechanisms for sanctioning inappropriate behavior. It may be the case that the "chaos" of conflict provides more opportunities for some members of government security forces to commit acts of SV because the military leadership is at an information disadvantage relative to its ground troops. At the same time, conflict may create incentives for military leadership to reinforce the hierarchical structure of the bureaucracy and establish strong controls on members' behavior, which *ceteris paribus* is expected to decrease the amount of SV in a conflict. Butler, Gluch, and Mitchell (2007) find that when states have lower levels of financial corruption (a proxy measure for principal-agent dynamics), levels of SV are also lower.

Institutional approaches emphasize that SV serves an instrumental purpose for government security forces. Mitchell (2004); Leiby (2009) argue that states do not always "lose" control over security forces, as traditional principal-agent approaches suggest, but that states may allow SV to occur because it provides a strategic benefit, while the bureaucratic structure of government security forces provides a "plausible deniability" cushion against accusations of human rights violations. Cohen (2013) argues that rape, particularly gang rape, is used to generate social cohesion in military units that rely on

5

forced recruitment. Also, the state may employ SV as a repression strategy against political opposition, or in the machinery of politicide or genocide (Sharlach, 2000; Sharratt, 2011). These theoretical approaches suggest that even in the absence of information disadvantages, states may not have an incentive to stop opportunistic behavior within security forces, or that it may be rational for states to encourage (or not discourage) SV when faced with certain resource constraints.

Normative prohibitions, a potential moderating variable, also point to the importance of examining pre-conflict levels of SV. It may be the case that groups exhibiting normative prohibitions against SV are less likely to commit such acts in conflict, while those without prohibitions are likely to employ more SV as conflict sets in. For example, the Liberation Tigers of Tamil Elam (LTTE) exhibited a strong moral prohibition against SV during the civil war in Sri Lanka (Wood, 2009). It is not a stretch to suggest that such organizations, whether state security forces or insurgent groups, likely have this normative prohibition in place prior to a conflict. For government security forces with such a prohibition in place, *ceteris paribus*, levels of SV before and during conflict should not fluctuate. If they do, theorists relying upon normative predispositions must either show a shift in the normative preferences of the organization once engaged in conflict, or refine their theories to incorporate the factor that drives this preference shift. The same is true if SV is simply one *practice* of many that condition security forces' behavior (Cohen, Hoover Green, and Wood, 2013). Scholars must identify why conflict would change the practiced behaviors of a group

Elements of each of these approaches often overlap in explanations of SV in conflict. For example, normative prohibitions against the use of SV may exist, but principal-agent dynamics may allow opportunistic behavior on the part of individual security force members. Without getting drawn into arguments about the best explanations for variation in SV in conflict, we instead propose that pre-conflict levels of SV ought to relate to levels of SV during conflict. Thus, this paper makes a first attempt at establishing an empirical

"baseline" of pre-conflict levels against which levels of SV during conflict can be compared. We argue that the comparison group used by researchers is critical in answering the question of how much SV we expect in a given conflict. Specifically, any large-N empirical analysis of this phenomenon needs to perform within-country comparisons of non-conflict country-years to conflict country-years. Cross-sectional analyses, studies including only conflict-years, and even time-series cross-sectional analyses including the standard binary conflict variable can provide misleading results about the relationship between conflict and SV.

2   Methods

There are a number of ways to compare the level of SV (or human rights violations more generally) across country-year observations. The usual comparison has been across countries having different values of an independent variable, as in the consistent finding that democracies have better human rights records when compared to non-democracies (Poe and Tate, 1994; Zanger, 2000; Davenport, 2007). While regime type is a time-varying variable, most of the variance is cross-sectional. Other variables may have a different mix of cross-temporal versus cross-sectional variance. For a variable with more cross-temporal variance, like civil conflict, the usual comparison across countries may not be the correct comparison to make. We elaborate on these methodological concerns below, and then introduce our sources of data for SV and conflict.

Analysts of cross sections "...typically observe covariation presumed to be produced by unobserved causal processes operating at some time before the data were gathered" (Stimson, 1985, 917). Variation observed on the dependent variable in many cases cannot be accurately modeled because of this "unobserved causal processes" occurring at times prior to measurement. Since cross-sectional analyses cannot account for these processes,

unexplained variation from these models is relegated to the error term. For example, Butler, Gluch, and Mitchell (2007) find that the probability of a country experiencing more intense SV committed by government security forces is higher in countries that have civil conflicts, relative to countries that do not for the year 2003. The authors include civil conflict variables as predictors of the level of SV in a country, but also include controls for factors like a country's wealth, regime type, number of military personnel, level of corruption, and population. This cross-sectional analysis provides some evidence for civil conflict increasing the prevalence of SV. However, it is unclear whether the levels of SV in countries experiencing civil conflict are already higher than those without because only one year of data is analyzed. The civil conflict indicator may substitute for some other factor that varies systematically between countries, making the relationship identified by Butler, Gluch, and Mitchell (2007) spurious.

Dealing with variation in time and space is something that scholars of human rights and conflict must confront. Time-series cross-sectional analyses of the impact of conflict on human rights violations are common in the literature (Poe and Tate, 1994; Davenport, 1995; Poe, Tate, and Keith, 1999; Zanger, 2000; Wright, 2014), all finding a significant positive relationship between conflict and human rights violations. While these multivariate analyses identify important covariates that contribute to states' propensities to abuse their citizens, we argue that they are not able to test the relationship of most interest. That is, whether the shift from a state of peace to conflict in a single country generates a degradation in human rights abuses. Standard practice is to cluster standard errors to account for the fact that observations within panel units are likely correlated across time or to employ fixed-effects to capture unaccounted for cross-sectional heterogeneity. [3]

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[3]For examples of each, respectively, see Davenport and Armstrong (2004); Neumayer (2005). More advanced matching techniques feature in the recent work on treaty compliance and human rights violations (Hill, 2010), and these matching techniques may allow for more appropriate comparisons of conflict and non-conflict countries.

However, the comparison made by the binary conflict variable in such models remains between all peace and conflict years, across all country-years. Clustering models inflate standard errors to account for non-independence of observations within panel units but do not isolate the impact of the transition from non-conflict to conflict on the level of SV within a panel-unit. Fixed-effects models provide a method for avoiding confounds from unaccounted for cross-sectional variation, but the comparison being made is between conflict country-years and non-conflict country years *across all cases*. That is not to say that the search for the average impact of conflict on countries' respect for human rights is not worthy of study; these studies identify factors that likely influence the propensity of state security forces, and other armed groups, to abuse civilians. We argue that when looking to develop better theoretical explanations for variation in human rights abuses, and SV in particular, analyses need to search for evidence of these explanations operating within cases as well as between cases, and that large-N empirical studies are able to contribute to our knowledge on this front.

Including more cross-sections from time periods before, during, and after a conflict helps capture those processes that may go unobserved in a cross-section. For the study of SV and conflict, this approach becomes particularly important for two reasons. First, conflict is not a static process. Some conflicts are brief; they may last a single year, and only show up once in the dataset. However, other conflicts are long-lasting, and for those that are, the impact of conflict on SV levels lasts beyond a single cross-section. Second, if conflict does generate or exacerbate SV in a country, we should observe this effect as a country transitions from non-conflict to conflict years. If levels of SV in a country remain the same or even decrease during conflict, this challenges the widely held assumption that conflict increases the amount of SV. Not only this, but it points to the need for more theorizing about the reasons countries vary in the presence and level of SV.

To examine the extent to which conflict increases SV, we conduct a series of bivariate

9

analyses. We conduct ordered probit analyses looking at the full sample and conditional sub-samples of the data; these analyses generally support the usual contention that SV is at higher levels during conflict years. To examine change, we conduct cross-tabular analyses which focus on much smaller subsets of cases wherein we can isolate pre-conflict and conflict SV levels; these analyses suggest that the SV level in the succeeding conflict year is likely to be the *same* as the immediate pre-conflict SV level (i.e., for the first or second year prior to conflict).[4]

2.1   Dependent Variable

The dependent variable is an ordinal scale of the intensity of SV committed by government security forces. It is a five-point scale ranging from 0 when reports indicate an absence of SV committed by security forces to 4 when reports indicate that SV is a "tool" or "systematic weapon" of war. This scale is adapted from Butler, Gluch, and Mitchell (2007), and we therefore adopt their definition of SV as "any act of a sexual nature which is committed on a person under circumstances which are coercive," which is from the International Tribunal for Rwanda (Askin, 1999, 109). The measure is coded using information obtained from the Department of State Human Rights Country Reports. The unit of analysis is the country-year, and we score every country for which a report is available across the time period 1999–2011. Measuring across this thirteen-year period for 170 countries gives us a total 2,207 observations. The coding rubric is found in Table 1.

The coding rubric used by Butler, Gluch, and Mitchell is provided in Table A1 for comparison with the rubric used for this project in Table 1. There are two primary differences between our scale and the original. First, the original scale codes as a 1 only

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[4]In Appendix C, we compare our SV measure against the SVAC measure of Cohen and Nordås (2014). This analysis shows a fair degree of agreement between the two measures ($\gamma = 0.76$) but that our SV measure generally codes a higher level of SV committed by security forces than the SVAC measure.

Table 1: Ordinal Measure of Sexual Violence Committed by Government Security Forces

Level	Description
0	No mention of 'rape, 'sexual assault' or 'sexual abuse' by security forces.
1	Isolated or alleged reports of 'rape', 'sexual assault' or 'sexual abuse' by security forces.
2	There were 'reports', 'there continued to be reports' or 'some' reports of 'rape', 'sexual assault' or 'sexual abuse' by security forces, which occurred 'sometimes', or 'remained a problem.'
3	There were 'numerous reports' of 'rape', 'sexual assault' or 'sexual abuse' by security forces, which were 'routine', 'common', 'widespread', 'systematic', 'reported repeatedly', or 'rape', 'sexual assault', and 'the threat of rape' was used as a tool of torture 'to extract information, to intimidate and to punish' a targeted segment of the population (such as detainees, opposition members, or an ethnic minority).
4	'Rape', 'sexual assault' or 'sexual abuse' by security forces was used as 'a tool of war.' or 'a systematic weapon of war.'

Source: United States Department of State Human Rights Country Reports.
The term 'security forces' refers to militaries and government-supported militia, police, and prison guards. Warlords and their forces, pro-government militias, or other non-state actors are excluded. SV of interest is committed against civilians, not combatants within the same forces or the opposing force.

"Isolated reports of 'rape', 'sexual assault', or 'sexual abuse' by security forces" (2007: 682). We explicitly include "allegations" or "unconfirmed reports" of SV committed by security forces as a 1. Given that SV is a phenomenon that likely goes under reported, including these potential incidents in the coding of our cases more accurately measures the true levels of SV in a country.[5] Allegations are qualitatively distinct from "No mention" of SV (coded 0), but do not reach the level of multiple confirmed or continuing confirmed reports required for a country-year to be coded 2 on the scale. So, allegations are treated as on par with isolated reports.

The second change we made to the ordinal measure of SV created by Butler, Gluch,

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[5]For a discussion of data quality issues specific to SV see Cohen, Hoover Green, and Wood (2013, 8-9), and for an exploration of reporting bias in human rights documents, including the State Department Human Rights reports, see Fariss (2014).

and Mitchell (2007, 682) is to the category for "numerous" or "widespread" reports of SV (a 3 on the scale). The second half of this category includes SV or threats to commit such violence "against detainees and their family members" and specifies that SV "was used as a tool of torture 'to extract information, to intimidate and to punish'." Such incidents involved the targeting of a particular group (such as opposition party members or their families *outside* of a detention setting) in an instrumental fashion. We thus incorporate these forms of violence into the coding rubric by changing the language for the Level 3 Category to "a targeted segment of the population (such as detainees, opposition members, or an ethnic minority)".

2.2   Independent Variable

The independent variable of interest for this study is the incidence of armed civil conflict in a country. Data on armed conflict come from the UCDP/PRIO Armed Conflict Dataset (ACD), specifically the *intensity* variable (Themnér and Wallensteen, 2012). This variable contains values for the absence of conflict, 0, the presence of minor conflict, 1, and the presence of major conflict or civil war, 2. Minor conflicts occur in the dataset if anywhere between 25-999 battle-related deaths are recorded in a given year; civil wars occur in the dataset when at least 1,000 battle-related deaths are recorded in a given year. Of the 2,207 observations in our dataset, there are 1,888 country-years without conflict, 239 country-years with minor conflict, and 80 country-years with civil wars. In the models below, the effects of minor conflict and civil war are found statistically indistinguishable from one another; therefore, we create a dummy variable, called incidence, for which either level of conflict is given a value of 1, with zero-values indicating conflict below 25 battle-related deaths or the absence of conflict altogether.[6]

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[6]The two levels of conflict discussed above however, allow us to highlight a potential oversight in the data collection process on SV. Table 2 reveals that of the 16 cases of SV reaching a 4 on our scale—being used as a

3   Results

Our results are divided into three subsections. In the first subsection, we examine SV by level of conflict intensity to see whether we need to maintain the distinction between minor conflict and civil war. We find that the level of SV is higher in country-years with either minor conflicts or civil war, relative to those with no conflict; but levels of SV are statistically indistinguishable between the two categories of armed conflict. In the second subsection, we examine SV by different amounts of conflict variance. Many countries in our sample did not experience armed conflict for our years of data collection, which we refer to as *non-conflict countries*. Other countries experienced armed conflict for all of our years of data collection, which we refer to as *constant conflict countries*. A third group of countries experience both peace-years and conflict-years in our data, and we call these *conflict variance countries*. Comparing the two groups with no variation in conflict to the *conflict variance* group reveals a large disparity of SV between them but raises methodological questions about the appropriateness of that comparison. This leads us to our third subsection, in which we finally examine levels of SV before and during conflict for only those countries that transition from peace to conflict in our data, thus we restrict our sample for these analyses to the *conflict variance countries*. Here we find that the strongest relationship is one of no change. That is, the SV level in the subsequent year of conflict is most likely to be the same as the SV level one or two years prior to the conflict.

"systematic weapon of war"—only two occur during civil war. Five of these cases occur in non-conflict years, and the 14 remaining cases occur in the context of minor conflicts. The two cases that occur in the context of civil war are those of Yugoslavia in 1999 and Libya in 2011. Of the 14 cases that occur in the context of minor conflict, half are country-years for Myanmar, two are for Sudan, two are for Burundi, and three are for the Democratic Republic of Congo (DRC). The remaining five cases, which occur with no conflict present, are also from the DRC. This is further discussed in footnote 10.

## 3.1 Sexual violence by levels of conflict intensity

Table 2 shows the distribution of country-year cases across SV levels by conflict intensity
level (as a three category measure). There is a marked increase in the percentage of cases
at higher SV levels ("some reports. . . " and worse) for minor conflict *and* civil war
compared to the no-conflict distribution. However, with the exception of SV level 3
("widespread reports" of SV), the percentage of cases at a given SV level is the same or
less during civil war as compared to minor conflict. This suggests that the presence of
armed conflict (rather than its intensity) explains this difference in distribution of cases.

Table 2: Sexual violence by conflict intensity

Sexual Violence Code	Conflict intensity level							
	No conflict		Minor		Civil War		Total	
	No.	%	No.	%	No.	%	No.	%
No mention. . .	1363	72.2	78	32.6	17	21.2	1458	66.1
Isolated reports. . .	191	10.1	18	7.5	6	7.5	215	9.7
Some reports. . .	166	8.8	32	13.4	9	11.2	207	9.4
Widespread reports. . .	163	8.6	97	40.6	46	57.5	306	13.9
Systematic weapon of war. . .	5	0.3	14	5.9	2	2.5	21	1.0
Total	1888	100.0	239	100.0	80	100.0	2207	100.0

As an additional test, we ran annual ordered-probit analyses of minor-conflict and
civil-war dummies on SV level, treating each year as an independent cross-section. Figure 1
summarizes these results, showing the coefficients and 95% confidence intervals (CIs) for
the minor-conflict and civil-war dummy variables for each annual cross-section. The results
show that the coefficient for minor conflict is always inside the CI of the civil-war estimated
coefficient, suggesting that using the incidence of armed conflict (whether minor or civil
war) is sufficient for analyzing the effect of armed conflict on SV. Therefore, in the following
section we combine minor conflicts and civil wars into a single indicator of armed conflict.

Figure 1: Coefficients and CIs from Annual Ordered Probit Results
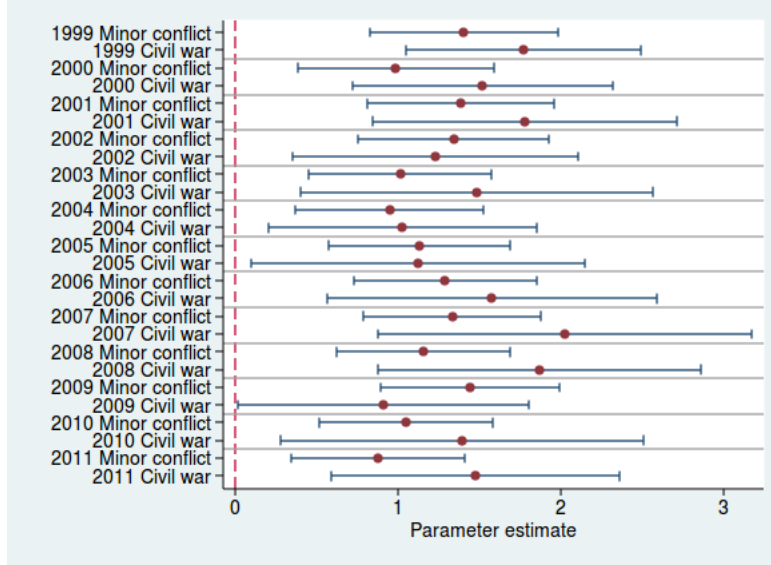
3.2 Comparing SV according to variance in armed conflict

Table 3 shows the results of three ordered-probit estimations using different slices of the data. Each model examines the effect of armed conflict on the level of SV committed by security forces, clustering by country. Model 1 includes all country-years (*conflict variance, non-conflict*, and *constant conflict*) and makes the unconditional comparison of armed-conflict country-years versus non-armed-conflict country-years. Model 2 uses the *conflict variance* sub-sample from our data, and thus makes the conditional comparison of armed-conflict years versus non-armed-conflict years for countries that transition from peace to conflict in our sample. Finally, Model 3 only includes country-year observations for countries with no variation on armed conflict across the study period. Of these 130 countries, ten experienced armed conflict throughout the study period, the *constant conflict* group.[7] The remaining countries constitute our *non-conflict* group. Model 3 thus makes the conditional comparison of armed-conflict countries versus non-armed-conflict

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[7]The ten countries are Algeria, Colombia, Ethiopia, India, Israel, Philippines, Russia, Sudan, Turkey, and Uganda.

countries across the years of the study period.

Overall, the results in Tables 3 demonstrate that SV levels are higher during armed-conflict than otherwise. However, these results also show how examining different samples attenuates the magnitude of the effect. The effect is largest when comparing (in Model 3) *constant conflict* countries with *non-conflict* countries; the effect is smallest when comparing (in Model 2) *conflict variance* countries' SV levels for their conflict years against their peace years. When combining these two samples (in Model 1), the effect is notably closer to that of Model 3 as there are many more country-year observations without armed conflict.

Table 3: Ordered-probit results of civil conflict on sexual violence

	Model 1	Model 2	Model 3
	All Country-Years	Conflict Variance	No Temporal Variance
Armed conflict	1.2272***	0.7017***	1.3526***
cut point 1	0.5991***	0.0701	0.7332***
cut point 2	0.9151***	0.2907*	1.1125***
cut point 3	1.311***	0.6896***	1.5395***
cut point 4	2.8546***	2.1661***	3.5910***
country-years	2207	520	1687
country-clusters	170	40	130
pseudo r^2	0.0671	0.0335	0.0560
log-likelihood	-2143.292	-669.675	-1407.904
χ^2	58.999	16.7813	9.0965
p	0.0000	0.0000	0.0000

legend: * p<0.050; ** p<0.010; *** p<0.001

To get a better sense of the different magnitude of effects depending on the sample, Figure 2 shows the predicted probabilities across SV levels for Models 2 and 3. The "Non-Armed-Conflict Years" constitute peace years for the *conflict variance* sample in Model 2 and the *non-conflict* country-years for Model 3. The "Armed-Conflict Years" constitute conflict years for the *conflict variance* sample in Model 2 and the *constant conflict* country-years for Model 3. So, when examining the Model 3 values, the largest

16

difference in predicted probabilities is for SV level "0" in which *non-conflict* country-years have a 0.768 probability of having no mention of SV, while the *constant conflict* country-years have a 0.268 probability. When examining the Model 2 values, the largest difference is also for SV level "0" but is much smaller; in *conflict variance* countries, the probability of having no mention of SV is 0.528 during peace years and 0.264 during conflict years. A similar feature exists for the probability of being classified SV level "3" (widespread reports of SV). Comparing *non-conflict* countries with *constant conflict* countries yields a 0.352 increase in predicted probability, while comparing peace years of *conflict variance* countries with conflict years of the same countries yields a 0.203 increase in predicted probability.

The most interesting feature of Figure 2 is that the distribution of SV for *non-conflict* countries is substantively different than the distributions of the other three histograms; it shows a progressive decline of the probability of being classified in a higher category. The other three distributions show increases in the probability of higher levels of SV across levels 1-3. The distributions that are most similar are those during armed-conflict years, though the probability of SV level 4 is higher for *conflict variance* countries (0.072) than for *constant conflict* countries over the study period (0.013).

The analysis and discussion above has implications for the general quantitative human-rights literature, as we mention above. If the level of human rights violations has a similar distribution to the level of SV, as found in our data, then inappropriate comparisons may be driving the general finding that conflict increases human rights violations. To our knowledge, scholars of human rights have yet to differentiate, in large-N empirical studies, between those countries that never experience conflict and those countries that move from peace to conflict in their samples. Thus, they potentially conflate *non-conflict* country-years with peace years for those countries experiencing *conflict variance*.

17

Figure 2: Ordered Probit Predicted Probabilities
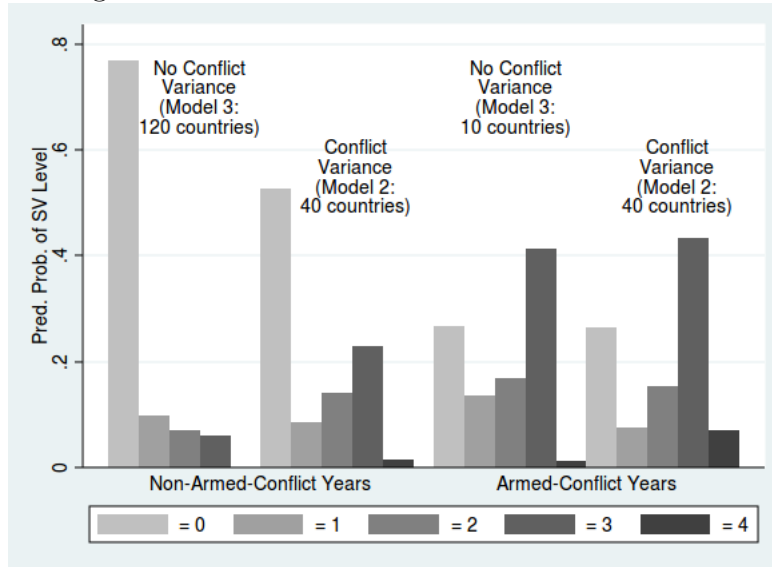
3.3   Comparing SV before and during conflict

In order to get closer to the idea of a baseline, we now focus on whether *changes* in conflict intensity from one year to the next lead to changes in the SV level. This effectively alters the unit of analysis to "biennial" country cases as we are looking at potential changes over two years in the same country. As we are interested in how a particular change in the IV affects the change in SV level, we are also examining subsets of the data in which that particular change was present. Thus, we employ simple cross-tabular analysis to examine these data.[8]

Table 4 shows the distribution of biennial cases for which there was no conflict in one year and some level of conflict (minor or civil war) in the next, a subset of our *conflict variance* group. The relationship between the SV levels from one year to the next in

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[8]Readers may be concerned that the highest point on the scale includes both "prevalence" and "strategic" elements, which are qualitatively different from the focus on prevalence in the lower points of the scale. A benefit of having this fifth point of the scale is that it is less likely that SV has reached the measurement ceiling during peacetime merely due to the definition of the measure. However, as as robustness check of our measure, we re-run our main analyses in Appendix D using a collapsed measure in which the top two levels are combined as one. The results concerning SV by government security forces generally showing no change from peacetime to conflict still hold.

Table 4 is significant ($\chi^2_{(12)} = 32.5391$, $\gamma = 0.7592$ with $ASE = 0.098$).[9] It is important to note that this is not just a positive relationship between the pre-conflict SV level and the first-year-of-conflict SV level. The relationship that is strongest is that of no change (30 of the 46 biennial cases—65%—have the same SV level in the year before and in the subsequent year of conflict).[10]

Table 4: Sexual Violence Levels for No-conflict to Conflict Biennial Cases

| First-year-of-conflict SV Level | Pre-conflict SV Level | | | | | |
	SV0	SV1	SV2	SV3	SV4	Total
SV0 = No mention...	**15**	2	1	2	0	20
SV1 = Isolated reports...	1	**1**	0	0	0	2
SV2 = Some reports...	0	0	**1**	1	0	2
SV3 = Widespread reports...	3	0	4	**13**	0	20
SV4 = Systematic weapon of war...	0	0	1	1	**0**	2
Total	19	3	7	17	0	46

Only three cases experience jumps in conflict intensity from no conflict to civil war in the ACD data during this time period. As such, no meaningful relationship would be found by tabulating those cases. The remaining 43 cases which move from no conflict to minor conflict are shown in Table 5. The relationship here is still significant ($\chi^2_{(12)} = 32.2384$, $\gamma = 0.8386$ with $ASE = 0.082$). While there is a positive relationship, the relationship that is strongest is again that of no change (29 of the 43 biennial cases—67%—have the same SV level in the year before and during the succeeding year of minor conflict).

Even though we have argued that minor conflict and civil war have the same statistical effect on SV level, Table 6 shows the distribution of biennial cases for which

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[9]The degrees of freedom for this table is only 12 because there are no cases of SV4 for the independent variable of pre-conflict SV. As a result, γ is a more appropriate associational statistic.

[10]As a robustness check, we separated out the cases in Table 4 into biennial cases involving recurring conflict with intermittent peace and those that are the first instance of conflict (at least within our sample). Neither tabulation contradicts our overall results. For the recurrent conflict cases ($N=25$), half have high levels of SV in both the intervening peace year and the next year of conflict. However, there are also eight cases that have no reported incidence of SV by government security forces for either the intervening peace year or the next year of conflict. For the first instance of conflict cases ($N=21$), about half of the cases are along the main diagonal, still supporting our argument that SV before conflict is generally predictive of SV in the succeeding year of conflict.

Table 5: Sexual Violence Levels for No-conflict to Minor Conflict Biennial Cases

	Pre-conflict SV Level					
First-year-of-conflict SV Level	SV0	SV1	SV2	SV3	SV4	Total
SV0 = No mention...	**14**	2	1	1	0	18
SV1 = Isolated reports...	1	**1**	0	0	0	2
SV2 = Some reports...	0	0	**1**	1	0	2
SV3 = Widespread reports...	3	0	4	**13**	0	20
SV4 = Systematic weapon of war...	0	0	0	1	**0**	1
Total	18	3	6	16	0	43

there was minor conflict in one year and civil war in the next. The relationship between the SV levels from one year to the next in Table 6 is significant ($\chi^2_{(9)} = 17.3905$, $\tau_B = 0.6050$ with $ASE = 0.156$).[11] Again, while there is a positive relationship between the SV level during minor conflict and SV level during the succeeding year of a civil war, the relationship that is strongest is that of no change (10 of the 18 biennial cases—56%—have the same SV level in the minor conflict and subsequent civil war years).

Table 6: Sexual Violence Levels for Minor Conflict to Civil War Biennial Cases

	Minor conflict SV Level					
First-year-of-war SV Level	SV0	SV1	SV2	SV3	SV4	Total
SV0 = No mention...	**4**	1	0	0	0	5
SV1 = Isolated reports...	0	**0**	0	1	0	1
SV2 = Some reports...	1	0	**2**	0	0	3
SV3 = Widespread reports...	1	0	2	**6**	0	9
SV4 = Systematic weapon of war...	0	0	0	0	**0**	0
Total	6	1	4	7	0	18

Returning to our comparison of non-conflict and conflict years, the relationship between the SV level two-years prior to conflict and the SV level in the succeeding year of conflict are also most likely to mirror one another. Table 7 shows a significant relationship ($\chi^2_{(16)} = 33.3936$, $\tau_B = 0.4907$ with $ASE = 0.134$) in which 59% of the cases have the same SV level two-years prior to conflict as the subsequent year of conflict.

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[11]The degrees of freedom for this table are only 9 because there are no cases of SV4 for either variable. Consequently, τ_B is a more appropriate associational statistic.

Table 7: Sexual Violence Levels over a Two-year Lag

| First-year-of-conflict SV Level | SV Level Two-years Earlier | | | | | |
	SV0	SV1	SV2	SV3	SV4	Total
SV0 = No mention...	**12**	3	1	1	0	17
SV1 = Isolated reports...	1	**0**	0	1	0	2
SV2 = Some reports...	0	0	**0**	2	0	2
SV3 = Widespread reports...	2	1	1	**7**	0	11
SV4 = Systematic weapon of war...	1	0	0	0	**1**	2
Total	16	4	2	11	1	34

Moving to three- and four-year lags no longer shows a significant relationship between lagged SV level and the SV level in the succeeding year of conflict. Tables B1 and B2 in Appendix B show these results. Note, however, that these insignificant results also do *not* show a pattern of lower SV levels in pre-conflict years leading to higher SV levels during conflict.

3.4   Summary of results

Our results overall suggest that conflict per se may not be the primary driver of higher SV in armed conflict compared to no-conflict years. When all country-years are analyzed together, the low levels of SV in countries not experiencing armed conflict increases the magnitude of the effect armed conflict seems to have on SV. Restricting the sample to countries that have variance on armed conflict over the study period still shows a positive effect of armed conflict on SV but at a lower magnitude. This portion of the results clearly allow us to say that country-years afflicted with conflict have higher levels of SV on average than country-years not afflicted with conflict. However, these results do not let us say that that a given country moving from no conflict to conflict is likely to experience higher levels of SV.

When shifting the analysis to before and during conflict, we find that countries with high levels of SV before conflict (i.e., one or two years) are likely to have high levels of SV

during the succeeding year of conflict, and countries with low levels of SV before conflict are likely to have low levels of SV during succeeding year of conflict. We find no significant relationships when comparing SV levels three and four years before conflict with the succeeding year of conflict. This suggests that the immediate pre-conflict SV levels are the best predictor of SV levels early in the conflict rather than conflict itself being the cause of higher SV levels.

4   Conclusion

Our findings suggest that the levels of sexual violence present in a country prior to the outbreak of conflict are predictive of the level of sexual violence the country will experience during conflict. Specifically, when we examine the same country before and during conflict, levels of sexual violence do not differ significantly from one another. This runs contrary to the widely held assumption—supported by cross-sectional empirical evidence—that conflict increases levels of sexual violence in a country. These results are preliminary. Also, we find that in some cases the level of sexual violence during conflict does not remain the same as it was prior to the conflict. Either the level of sexual violence increases a great deal from prior levels or actually decreases relative to prior levels. These cases warrant explanation and may provide valuable insight for current theories of sexual violence in armed conflict.

This finding of consistency in levels of sexual violence before and during conflict should not lead us to dismiss current theoretical explanations for the presence and severity of sexual violence in armed conflict. This is for two reasons. First, this study does not test a causal theory of sexual violence in conflict. Instead, we question the assumption that conflict increases levels of sexual violence by government security forces without taking a specific theoretical approach. We find reasons for questioning this assumption, but this does not necessarily undermine current explanations nor the likelihood that sexual violence

by all actors is greater in conflict than in peace. Instead, it points to the need for scholars to address the implications of a given theoretical approach in both peace and conflict settings to generate more robust theory-testing. Purely opportunistic explanations, which the evidence militates against, are able to identify the general breakdown of societal rules and norms associated with conflict as the cause for increases in the level of sexual violence. Normative predispositions provide us with a reason as to why sexual violence is *absent* in certain conflicts, but have yet to develop testable expectations for when these normative predispositions will impact the violent behavior of groups. Finally, institutional and instrumental explanations have yet to identify the mechanisms associated with conflict that would cause a shift in repertoires of violence, levels of corruption, and strategic advantage (in its various forms) as a country transitions from peacetime to wartime.

Second, some approaches to the study of sexual violence simultaneously address the *form* and *level* of sexual violence in armed conflict. Our results do not contradict the findings noted above that gang rape and more "extreme" forms of rape are more likely to occur during conflict (Cohen, 2013; Farr, 2009). This is because our measure focuses on the incidence of sexual violence; in other words, we focus on how often sexual violence occurs, only identifying *in passing* the form of sexual violence when it appears to coincide with torture or in detention. So, this study points to the need for systematic empirical study of the form of sexual violence committed by government security forces before and during conflict. It also points to the need for critical evaluation of our aggregate measures of sexual violence and how well they are able to capture potential shifts in its qualitative character (or whether they should). To this end, we are currently engaged in the collection of data on multidimensional, country-year measures of sexual violence in peacetime and wartime, which allows us to examine the *severity* of sexual violence, the *frequency* with which sexual violence is perpetrated, and the extent to which government security forces are *targeting* individuals from particular groups within the population. To capture more

23

qualitative information on sexual violence committed by government security forces, we are developing a series of indicators for the various forms sexual violence may take, which will allow researchers to examine trends in single forms of sexual violence, sub-types of sexual violence, or the sum total of sexual violence across both states and time. This measure will allow us to examine, in greater depth, the relationship between conflict and the level of sexual violence in a country, and in doing so, will improve our understanding of the causal process behind sexual violence committed by government security forces more generally.

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A Appendix: Butler, Gluch, and Mitchell (2007) Original Scale of Sexual Violence

Table A1: Coding Rubric from Butler, Gluch, and Mitchell (2007)

Level	Description
0	No mention of 'rape, 'sexual assault' or 'sexual abuse' by security forces.
1	Isolated reports of 'rape,' 'sexual assault' or 'sexual abuse' by security forces.
2	There were reports,' 'there continued to be reports' or 'some' reports of 'rape,' 'sexual assault' or 'sexual abuse' by security forces, which occurred 'sometimes', or 'remained a problem.'
3	There were 'numerous reports' of 'rape,' 'sexual assault' or 'sexual abuse' by security forces, which were 'routine,' 'common,' 'widespread,' 'systematic,' 'reported repeatedly,' or 'rape,' 'sexual assault,' and 'the threat of rape' against detainees and their family members was used as a tool of torture 'to extract information, to intimidate and to punish.'
4	Rape,' 'sexual assault' or 'sexual abuse' by security forces was used as 'a tool of war' or 'a systematic weapon of war.'

Source: US Department of State Country Reports

The term 'security forces' refers to both militaries and government-supported militia, police, and prison guards.

Inter-coder reliability: Two coders ranked each country. There were seven cases of disagreement resulting from their different interpretation of the language used in the reports that were ultimately resolved. Where the coders disagreed, it was by one scale point, and mostly between values of 2 and 3.

B   Appendix: Relationship Between Sexual Violence Levels Three and Four Years Prior to Conflict Onset

Table B1: Sexual Violence Levels over a Three-year Lag

First-year-of-conflict SV Level	SV Level Three-years Earlier					
	SV0	SV1	SV2	SV3	SV4	Total
SV0 = No mention...	**5**	1	4	4	0	14
SV1 = Isolated reports...	2	**0**	0	0	0	2
SV2 = Some reports...	0	0	**0**	1	0	1
SV3 = Widespread reports...	2	1	2	**3**	0	8
SV4 = Systematic weapon of war...	0	0	1	0	**1**	2
Total	9	2	7	8	1	27

$\chi^2_{(16)} = 21.3033$, p-value $= 0.167$
$\tau_B = 0.1701$ with $ASE = 0.160$

Table B2: Sexual Violence Levels over a Four-year Lag

First-year-of-conflict SV Level	SV Level Four-years Earlier					
	SV0	SV1	SV2	SV3	SV4	Total
SV0 = No mention...	**5**	0	2	3	0	10
SV1 = Isolated reports...	1	**1**	0	0	0	2
SV2 = Some reports...	0	0	**0**	1	0	1
SV3 = Widespread reports...	3	1	0	**1**	0	5
SV4 = Systematic weapon of war...	0	0	1	1	**0**	2
Total	9	2	3	6	0	20

$\chi^2_{(12)} = 12.3333$, p-value $= 0.419$
$\gamma = 0.1158$ with $ASE = 0.273$

C   Appendix: Comparing Our SV Measure to the Sexual Violence in Armed Conflict (SVAC) Measure

The Sexual Violence in Armed Conflict (SVAC) dataset contains information on sexual violence committed by a variety of armed conflict actors for all UCDP/PRIO active conflicts during the period from 1989–2009. While the dataset contains a wealth of information, the measure of interest to us is an ordinal measure of the "prevalence" of sexual violence in a country, which is based on the same human rights reports we use for our measure (For more information on SVAC, see Cohen and Nordås, 2014). Though similarly constructed, there are several differences to note, and these speak to the difficulty of measuring this multidimensional phenomenon. First, the SVAC measure only codes sexual violence in conflict years and immediate post-conflict years whereas our measure codes sexual violence in both conflict and all non-conflict years in our time series. Second, the SVAC measure only captures sexual violence that is "likely related to the conflict." Our measure records any sexual violence committed by government security forces, whether such violence is related to a conflict or not. Finally, the SVAC measure has only four points on the scale (versus our five-point scale), ranging from zero to three. While our scale distinguishes between "isolated" and "reports" or "continued reports" by scoring the former as a one and the latter phrases as a two, the SVAC measure collapses these incidents into a single category. Also, the SVAC measure divides what we consider Level 3 sexual violence into two levels: "Widespread" sexual violence is coded as a two by SVAC, while the use of sexual violence as a "means of intimidation" or "instrument of control and punishment" receives the highest score of three on the scale. Also in this category is sexual violence that is "systematic" or a "tool of war." It is difficult to distinguish qualitatively between the

use of sexual violence to intimidate and punish versus its use as a tool of war, but our scale distinguishes between the two by assigning a 3 to the former and a 4 to the latter.

The unit of analysis for the SVAC dataset is the conflict-actor-year including state and non-state actors. In order to draw comparisons, we compare only those conflict-years in which the actor is the state, as our measure includes only sexual violence committed by government security forces. So, we compare conflict-government-years (once isolated, effectively country-conflict-years) from the SVAC dataset to those years for which we share data (i.e., conflict years). The maximum prevalence score for sexual violence is used for those countries that experience more than one conflict in a given year (e.g., India). Also, the SVAC data contains information on sexual violence for several types of conflict: interstate conflict, internationalized internal armed conflict, and intrastate conflict. We use intrastate conflicts in which the government was the actor for our comparison. This is because our measure includes only incidents of sexual violence committed by government security forces against the civilians of that state, not another. The SVAC data are more inclusive, identifying any victims of sexual violence and attributing these acts to a perpetrator; so, examining only intrastate conflicts makes the comparison more direct. Table C1 displays the joint frequency distribution for the SVAC measure of sexual violence and our own.

Table C1: Joint Frequency Distribution for SVAC and Our SV Indicators

| SVAC Measure | Our SV Measure | | | | | |
	No SV (0)	Isolated (1)	Some (2)	Widespread (3)	Systematic (4)	Total
No SV (0)	158	27	46	68	1	300
Some (1)	4	14	28	77	8	131
Several/Many (2)	1	0	1	10	0	12
Massive (3)	0	0	0	7	7	14
Total	163	41	75	162	16	457

There are 457 observations shared between the datasets with $\gamma = .76$. However,

several differences can be identified in Table C1 above. Overall, our measure suggests that, when we examine the same cases, levels of sexual violence are higher than the SVAC measure records. The frequency distributions of the two variables reveal that our measure contains far fewer zero values than the SVAC variable, which is roughly 66% composed of zero values relative to 36% of our measure for the same cases. Also, the second largest category for the SVAC measure is "some" sexual violence, while for our measure it is "widespread" sexual violence. The category for widespread sexual violence on our measure is also one case shy of making our distribution truly bimodal (163 versus 162 cases), while the category for widespread sexual violence for the SVAC variable (2) contains only 12 cases.

Given that the above discussion includes conflict and immediate post-conflict country-years, one may wonder if the inclusion of the post-conflict years is affecting the comparison of measures. Thus, we next looked at only conflict-year cases. Such an examination allows a more direct comparison because of the conflict-specific focus of the SVAC dataset. However, the larger pattern still holds ($\gamma=.72$), which we see in Table C2. Roughly 58% of the SVAC cases are zero values, while roughly 28% of our cases have a value of zero. Few cases of "several" or "massive" prevalence of sexual violence are found on the SVAC measure, but for our measure 45% of the cases contain "widespread" sexual violence.

Table C2: Sexual Violence Indicators for Conflict-years Only

| | Our SV Measure | | | | | |
| | No SV | Isolated | Some | Widespread | Systematic | |
SVAC Measure	(0)	(1)	(2)	(3)	(4)	Total
No SV (0)	72	14	19	50	0	155
Some (1)	3	8	16	56	7	90
Several/Many (2)	1	0	1	9	0	11
Massive (3)	0	0	0	6	6	12
Total	76	22	36	121	13	268

This is a striking contrast, and it appears to point to a systematic difference in the coding processes behind the two measures. A potential explanation comes from the coding rules used for the SVAC measure. The SVAC dataset "employ[s] a conservative coding protocol, such that a source must identify the armed group by name and at least the year of the reported violation, in order to be coded" (Cohen and Nordås, 2014, 422). Cohen and Nordås (2014) state that this may be a reason for Cohen (2013) providing a higher sexual violence (specifically rape) participation rate among conflict actors; Cohen also pays attention only to the type of actor, not the name of the armed group. For our data, the coding team was instructed to attribute sexual violence to government security forces if the actor in a given anecdote met the definition for "security forces" provided at the bottom of Table 1. However, they were not required to identify any more specific characteristics of the armed group. Also, our coders assumed that any incident in a case occurred during the year of the human rights report unless otherwise specified (which the reports often do). The SVAC dataset requires that a source identify the year of an incident along with the name of an armed group.

The requirement of attribution to a specific actor as well as the identification of the timing of an incident could generate the observed difference between our measure of the intensity of sexual violence and that of the SVAC dataset. This is potentially worrisome, as under-reporting of incidents of sexual violence is already a chronic problem. However, the SVAC measure may be performing better than ours on a different metric. The category "3" on our scale, denoting "widespread" sexual violence for which we have "numerous reports" includes phrasing that may be driving the higher incidence of three codes in our data. A country-year may receive a "3" in our data because sexual violence was widespread *or because sexual violence was used to extract information from a targeted segment of the population.* Incidents that fit this description occur frequently in the human rights reports, and so we admit that our measure may be inflated. However, we believe this inflation is

not fatal to our analysis, as it is applied consistently in the coding of both peace- and conflict-years.

D Appendix: Analysis using a Four-point SV Scale

Table D1: Sexual Violence Levels for No-conflict to Conflict Biennial Cases

	Pre-conflict SV Level				
First-year-of-conflict SV Level	SV0	SV1	SV2	SV3	Total
SV0 = No mention...	**15**	2	1	2	20
SV1 = Isolated reports...	1	**1**	0	0	2
SV2 = Some reports...	0	0	**1**	1	2
SV3 = Widespread reports...	3	0	5	**14**	22
Total	19	3	7	17	46

$\chi^2_{(9)} = 31.2540$, p-value = 0.000
$\tau_B = 0.6003$ with $ASE = 0.101$
Cases at the same SV level = 67.4%

Table D2: Sexual Violence Levels for No-conflict to Minor Conflict Biennial Cases

	Pre-conflict SV Level				
First-year-of-conflict SV Level	SV0	SV1	SV2	SV3	Total
SV0 = No mention...	**14**	2	1	1	18
SV1 = Isolated reports...	1	**1**	0	0	2
SV2 = Some reports...	0	0	**1**	1	2
SV3 = Widespread reports...	3	0	4	**14**	20
Total	18	3	6	16	43

$\chi^2_{(9)} = 31.6006$, p-value = 0.000
$\tau_B = 0.6396$ with $ASE = 0.097$
Cases at the same SV level = 69.8%

Table D3: Sexual Violence Levels for Minor Conflict to Civil War Biennial Cases

	Minor conflict SV Level				
First-year-of-war SV Level	SV0	SV1	SV2	SV3	Total
SV0 = No mention...	**4**	1	0	0	5
SV1 = Isolated reports...	0	**0**	0	1	1
SV2 = Some reports...	1	0	**2**	0	3
SV3 = Widespread reports...	1	0	2	**6**	9
Total	6	1	4	7	18

$\chi^2_{(9)} = 17.3905$, p-value = 0.043
$\tau_B = 0.6050$ with $ASE = 0.156$
Cases at the same SV level = 66.7%

Table D4: Sexual Violence Levels over a Two-year Lag

| | SV Level Two-years Earlier | | | | |
First-year-of-conflict SV Level	SV0	SV1	SV2	SV3	Total
SV0 = No mention...	**12**	3	1	1	17
SV1 = Isolated reports...	1	**0**	0	1	2
SV2 = Some reports...	0	0	**0**	2	2
SV3 = Widespread reports...	3	1	1	**8**	13
Total	16	4	2	12	34

$\chi^2_{(9)} = 15.1939$, p-value $= 0.086$
$\tau_B = 0.4968$ with $ASE = 0.121$
Cases at the same SV level $= 58.8\%$

Table D5: Sexual Violence Levels over a Three-year Lag

| | SV Level Three-years Earlier | | | | |
First-year-of-conflict SV Level	SV0	SV1	SV2	SV3	Total
SV0 = No mention...	**5**	1	4	4	14
SV1 = Isolated reports...	2	**0**	0	0	2
SV2 = Some reports...	0	0	**0**	1	1
SV3 = Widespread reports...	2	1	3	**4**	10
Total	9	2	7	9	27

$\chi^2_{(9)} = 6.9796$, p-value $= 0.639$
$\tau_B = 0.1237$ with $ASE = 0.159$

Table D6: Sexual Violence Levels over a Four-year Lag

| | SV Level Four-years Earlier | | | | | |
First-year-of-conflict SV Level	SV0	SV1	SV2	SV3	SV4	Total
SV0 = No mention...	**5**	0	2	3	10	
SV1 = Isolated reports...	1	**1**	0	0	2	
SV2 = Some reports...	0	0	**0**	1	1	
SV3 = Widespread reports...	3	1	1	**2**	7	
Total	9	2	3	6	20	

$\chi^2_{(9)} = 7.8095$, p-value $= 0.553$
$\tau_B = 0.0310$ with $ASE = 0.202$