

Do States Delegate Shameful Violence to Militias? Patterns of Sexual Violence in Recent Armed Conflicts

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Abstract

Existing research maintains that governments delegate extreme, gratuitous, or excessively brutal violence to militias. However, analyzing all militias in armed conflicts from 1989 to 2009, we find that this argument does not account for the observed patterns of sexual violence, a form of violence that should be especially likely to be delegated by governments. Instead, we find that states commit sexual violence as a complement to—rather than a substitute for—violence perpetrated by militias. Rather than the logic of delegation, we argue that two characteristics of militia groups increase the probability of perpetrating sexual violence. First, we find that militias that have recruited children are associated with higher levels of sexual violence. This lends support to a socialization hypothesis, in which sexual violence may be used as a tool for building group cohesion. Second, we find that militias that were trained by states are associated with higher levels of sexual violence, which provides evidence for sexual violence as a “practice” of armed groups. These two complementary results suggest that militia-perpetrated sexual violence follows a different logic and is neither the result of delegation nor, perhaps, indiscipline.

Keywords

conflict, militias, sexual violence, child soldiers

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Why do some pro-government militias (PGMs) perpetrate terrible sexual abuse against civilians, while others mostly refrain from such violence? Militias are an important but understudied actor of the repressive apparatus of states. Conventional wisdom dictates that militias are often tasked with perpetrating the worst abuses against civilian populations, and recent research has found a suggestive relationship between the existence of militias and high levels of some forms of state repression (Mitchell, Carey, and Butler 2014).¹ Scholars have argued that states delegate the perpetration of atrocities to militias to avoid being held accountable for acts that violate the laws of war, such as the intentional targeting of civilians, and to escape being associated with especially brutal or shameful violence (e.g., Ron 2002; Alvarez 2006). In this article, we examine patterns in the perpetration of sexual violence by states and PGMs.² Building on the previous arguments, sexual violence—as one of the most widely condemned forms of wartime violence—should be especially likely to be delegated by states to militia groups in order to allow states to maintain plausible deniability for ordering, or at the very least, for permitting, such atrocities.

Using a new data set that contains detailed information on the reported sexual violence by all states and PGMs engaged in armed conflicts between 1989 and 2009 (Cohen and Nordås 2014), there is scant evidence that states are delegating the task of perpetrating sexual violence to militia groups. Instead, the data show that states commit sexual violence as a complement to—rather than a substitute for—violence perpetrated by militias.³ In addition, a temporal analysis reveals that in the years following the first reports of militias perpetrating sexual violence, states are reported to commit *higher* levels of sexual violence. The findings strongly contradict the delegation logic, which would instead predict that states should be *less* likely to perpetrate sexual violence once a militia has begun to perpetrate violations.

We argue that the principal-agent framework employed by much of the existing literature is too narrow and does not account for the most important factors that predict militia violence. Instead of a delegation logic, we analyze the organizational characteristics of armed groups to explain the wide variation in the level of militia-perpetrated sexual violence.⁴ Specifically, we find that militias that recruit children are associated with a higher reported prevalence of sexual violence. We also find that militias that have received training from states are reported to commit higher levels of sexual violence than groups that have not received such training. We interpret these results as supportive of two distinct but complementary arguments about why militias use sexual violence: first, that armed groups that have low internal social cohesion may be more likely to perpetrate sexual violence (Cohen 2013), and second, that sexual violence in armed conflict can spread among and between armed actors as a “practice,” or violence that is tolerated rather than ordered, in contrast to opportunistic or strategic behavior (Wood 2012).

Through exploring patterns in the perpetration of violence, we shed light on questions of relevance to both scholars and policymakers especially the issue of whether states appear to be outsourcing violence. The lack of evidence for the delegation argument lends credence to recent arguments that sexual violence is not typically

used as a “weapon of war,” as it is often described in the policy discourse, but may be the result of other nonstrategic factors (Cohen 2013; Eriksson Baaz and Stern 2013). More broadly, the study of militia-perpetrated sexual violence can offer important insight into how and why armed groups perpetrate violence considered gratuitous, excessively brutal, or especially shameful.

The article is organized as follows. After defining PGMs, the central actors in this analysis, we discuss the dominant theory regarding militia violence in the literature—namely, the delegation argument. Through several tests, we show how the observable implications of the delegation argument are not supported by the data on sexual violence by state forces and militias. In the third section, we develop a theoretical argument for why some militia groups are more likely to perpetrate sexual violence than others, focusing on group-level characteristics related to recruitment and training, and propose a set of testable hypotheses. In the fourth section, we review the research design and describe the data used in the analysis. In the fifth section, we present the statistical analyses and discuss the theoretical and policy implications of the study. In the final section, we conclude.

Defining Militias

In the broader conflict literature, the understanding of what a militia group is varies considerably. Jentsch, Kalyvas, and Schubiger (2015) define militias as “armed groups that operate alongside state security forces or work independently of the state to shield the local population from rebel demands or depredations.” In this article, we focus on PGMs, utilizing a data set by Carey, Mitchell, and Lowe (2013, 5) who define such militias more narrowly using four criteria: the group is (1) pro-government or sponsored by the government (national or subnational), (2) not a part of the regular security forces, (3) armed, and (4) organized to some degree. For the sake of clarity, in the proceeding analysis, militias and rebel groups are treated as distinct categories of armed actors, and our focus is exclusively on the former. Included in the universe of PGMs are also groups fighting (or claiming to fight) to protect their local communities. Offering civilian protection may be particularly common when the state lacks the capacity or political will to protect its population from attacks by rebel groups. For instance, the Kamajors in Sierra Leone resulted from the militarization of an existing social network, based on a hunter community, as a grassroots response to attacks on civilians by the Revolutionary United Front rebel group (Hoffman 2007). Although the group was not the product of a deliberate outsourcing strategy by the Sierra Leonean government, it is defined as a PGM because the government armed the Kamajors in order to fight the rebel group.

Challenging the Delegation Argument

Most existing studies argue that governments strategically employ militias because they allow for effective deniability (e.g., Ahram 2011; Mitchell, Carey,

and Butler 2014; Carey, Colaresi, and Mitchell 2015) and offer strategic advantages during periods of crisis and conflict (Eck 2015). States can be sensitive to accountability for human rights violations due to the risk of withdrawal of international aid, trade, and investment, as well as the loss of public support (Bueno de Mesquita et al. 2005). By sponsoring a militia group, a government can indirectly commit violence through delegation, while denying any involvement (e.g., Byman and Kreps 2010). Carey, Mitchell, and Lowe (2013, 250) describe militias as a tool “to evade accountability for strategically useful violence” so government leaders can claim a lack of control.⁵ When violence is brutal and violates the laws of war, delegation is widely viewed as an effective means for states to repress civilians.

What are the observable implications of the delegation argument in terms of sexual violence? The most straightforward implication of the delegation argument is substitution. That is, if a government seeks to avoid being associated with sexual violence, we should observe that government troops refrain from such behavior when a militia is present that can instead commit such violence. Due to principal–agent problems associated with delegation, it cannot be assumed that *all* sexual violence by militias is directly ordered by governments—after all, governments might lack oversight or be unable to control sexual violence by agents. Nevertheless, delegation implies that both actor types should *not* generally be committing such violence simultaneously. Two hypotheses follow: (1) sexual violence by state forces and militias in the same country should not occur simultaneously and (2) sexual violence by government forces should cease, or be significantly reduced, once a militia group operating in the country engages in sexual violence.

An empirical investigation shows that the data do not support these delegation hypotheses. In fact, data on all militia groups in the fifty countries engaged in armed conflicts from 1989 to 2009 show that in *all but one* case of militia-perpetrated sexual violence, state forces were also reported to be perpetrators (Cohen and Nordås 2014).⁶ Additionally, in all but four countries, state forces named as perpetrators committed *the same or higher* levels of sexual violence than did the militias.⁷ Finally, there were reports of sexual violence by thirty-four state forces involved in conflict while there were no reports of similar violence by militias in the same conflicts.⁸ The data clearly demonstrate that states were reported as perpetrators even in cases where the militia committed the very worst levels of sexual violence. These patterns present a strong challenge to arguments that states delegate the most reprehensible forms of violence; simply put, sexual violence does not follow a pattern of outsourcing from states to militias.

As a final test of the delegation argument, we analyzed temporal patterns of sexual violence by state forces and militia groups to determine the effect of militia participation on state violence. Using data on the level of the country-year, we noted the year in which the militia was first reported to have committed sexual violence and constructed a dummy variable for *post-militia perpetration*, which

is equal to zero for all years prior to the first report of sexual violence and one otherwise. *Sexual violence prevalence*, the dependent variable, is an ordinal scale estimate of reported sexual violence, ranging from 0 (none) to 3 (massive) (Cohen and Nordås 2014).⁹

We estimated an ordered logit regression with the prevalence of sexual violence perpetrated by state forces as the dependent variable, and country and year fixed effects with standard errors clustered by country.¹⁰ If there was evidence for the delegation logic, the coefficient for the post-militia perpetration dummy should be negative, indicating that state forces are associated with decreased levels of sexual violence after militia groups start to commit sexual violence.¹¹

The regression results are shown in Table 1. Once again, the results strongly contradict the delegation logic. The dummy for post-militia perpetration is positive and highly statistically significant at the 1 percent level. Rather than exhibiting reductions in the level of sexual violence by states, the years following the first perpetration of sexual violence by militias are instead associated with *higher* levels of sexual violence committed by the state. The control for severity of the conflict (battle deaths) means that the increase cannot be attributed to more intense fighting overall.¹² In sum, both the country-level patterns and the statistical results show that the delegation argument cannot account for militia-perpetrated sexual violence.

Table 1. Delegation Test: Sexual Violence by State Forces after Militias Begin Perpetrating Sexual Violence.

DV: Sexual violence prevalence by state forces

Post-PGM perpetration	1.363 (0.522)***
Battle deaths	.000 (0.000)
Population (ln)	1.388 (2.435)
GDPpc (ln)	-0.221 (1.328)
Regime type	-0.015 (0.038)
Regime type (squared)	-0.007 (0.012)
Middle East	12.670 (6.883)*
Europe	14.014 (2.282)***
Asia	13.833 (6.499)**
Africa	15.317 (2.921)***
N (Country years)	344
R ²	0.287

Note: Robust standard errors in parentheses, clustered on country (twenty-two). Sample includes only cases of non-zero sexual violence prevalence by militias. Reference category for region is the Americas. GDPpc = gross domestic product per capita; PGM = pro-government militia.

***p < .01.

**p < .05.

*p < .1.

Explaining Sexual Violence by Militias: Arguments and Hypotheses

If a delegation logic does not explain the patterns of perpetration, what does explain why some militias perpetrate sexual violence against civilians, while others mostly refrain from committing such violence? In recent years, increasing attention has been given to the relative absence of sexual violence by some actors, and what can account for variation in levels of violence (e.g., Butler, Gluch, and Mitchell 2007; Wood 2009; Cohen 2013). These studies concur that sexual violence is not a ubiquitous feature of the repertoire of violence of rebel groups or state armies. However, no study has yet analyzed variation in sexual violence by militia groups. Of the 224 militia groups active in conflicts during the study period, only 38 groups, or about 17 percent, were reported as perpetrators of sexual violence. Militia groups in Africa are overrepresented on this statistic, but militias perpetrating sexual violence still constitute a minority (about 30 percent) in the African context.

Our analysis departs from previous research on militia violence in that we do not employ a principal–agent framework. Instead, we argue that group-level factors are essential to understanding variation in the perpetration of sexual violence, including the types of training, socialization, and recruitment practices.¹³ The focus on armed groups is also relatively recent in the study of violence overall, as most scholars have previously sought explanations for variation at the level of the conflict or the country.

We test several arguments about how group-level factors can account for why some actors commit sexual violence on a large scale whereas others restrain such behavior. First, military sociologists have shown that performing acts of violence is central to creating social cohesion in an armed group. Starting from the earliest work in this realm (e.g., Shils and Janowitz 1948), scholars have found that violence builds new loyalties and severs previous ties. Building on these prior studies, Cohen (2013) argues that the level of internal cohesion in an armed group can be critical for understanding variation in wartime rape. Because it is difficult to measure levels of intragroup cohesion directly, Cohen uses the abduction of fighters as a proxy measure.¹⁴ Cohen argues that combatant groups that randomly recruit new members through abduction face the challenge of creating a coherent fighting force from a group of frightened and mistrustful strangers. In such groups, rape, and particularly gang rape, enables armed groups to create bonds of loyalty. For armed groups that suffer from low cohesion, the immediate benefits of rape are likely to be perceived to outweigh the short-term and long-term costs,¹⁵ because a minimum level of intragroup cohesion is necessary for the group to sustain itself. Cohen finds empirical support for this argument in a global statistical analysis of wartime rape and recruitment practices by both state militaries and rebel actors in large-scale civil wars.¹⁶ Insofar as militia groups that abduct their fighters also face the same dilemmas of organization building and experience problems of low cohesion, this mechanism may also hold explanatory power for these groups.¹⁷ Based on this, we propose the following hypothesis:

Hypothesis 1: Sexual violence is more likely by militias that suffer from low cohesion than by militias that do not have low cohesion.

A second set of armed group-level factors discussed in the literature are the ideological orientation and norms of the armed group, as well as the intensive training and political education associated with particularly ideological organizations (Wood 2008; Hoover Green 2011). Wood finds that normative concerns in ideological groups may effectively prohibit sexual violence. She writes (2008, 341), “Members of a revolutionary group seeking to carry out a social revolution may see themselves as the disciplined bearers of a new, more just social order for all citizens; sexual violence may conflict with their self-image.”¹⁸ Building on this logic, we formulate the following hypothesis:

Hypothesis 2: Sexual violence is less likely by militias that espouse an explicit ideology than by militias that are nonideological.

On the flip side, loot-seeking groups with weak or no explicit ideological motivation for fighting and with little or no traditional military training may be less likely to regulate sexual violence by the fighters, due either to a lack of command and control or to low strategic interest in enforcing such regulations (Weinstein 2007). These groups may use sexual violence for extortion and terrorizing populations in areas with valuable lootable resources.¹⁹ This leads to the last hypothesis:

Hypothesis 3: Sexual violence is less likely by militias that have received formal military training than by militias that lack such training.

Alternative Explanations

In contrast to armed group-based arguments, most existing studies of conflict-related violence—including sexual violence—present arguments at the level of the country or the conflict. We therefore examine explanations at the state- and conflict levels as alternatives to the hypotheses proposed above.

At the state level, researchers have used the mere existence of militias as a proxy for weak states (Bates 2008), and as a sign that states with limited capacity are outsourcing territorial control (Kalyvas 2006). Delegating atrocities to militias may be especially useful if the capabilities of the state are limited (Butler, Gluch, and Mitchell 2007; Carey, Mitchell, and Lowe 2013), because militias can operate in geographically remote areas that are otherwise difficult for the state to monitor.²⁰ An alternative hypothesis is therefore that sexual violence is perpetrated more frequently by militias operating in contexts with low state capacity, particularly in failed states.²¹

Delegating atrocities such as sexual violence to militias may also be an attractive strategy for democracies, in which governments are dependent on popular support to stay in office.²² Autocrats, in contrast, have fewer constraints on their

behavior and less incentive to hide or deny abuses. States that have some democratic features but are not fully democratic (e.g., semi-democracies, anocracies, or illiberal democracies) may therefore be the most likely regime type to delegate sexual violence to militias. In these cases, constraints on leaders are weaker than in consolidated democracies, but popular support through elections is still required for leaders to remain in power.²³

At the conflict level, scholars have sought to explain variation in civilian victimization, broadly defined, and sexual violence in particular, by whether a conflict is ethnic. For example, violence can be used as a strategy to clear enemy populations from contested territory in wars of territorial expansion (Sharlach 2000; Downes 2006). There is evidence from some cases that militias are formed around ethnic ties (Guichaoua 2006) and may be more likely to use sexual violence as a tactic to achieve political ends motivated by ethnic grievances. However, although several well-studied ethnic conflicts have exhibited high levels of sexual violence, the relationship between ethnic conflict and rape did not find empirical support in a cross-national statistical analysis of civil wars (Cohen 2013).²⁴

It has also been common to assume that when male victims are killed, female victims are raped (and then sometimes also killed), and that the number of deaths or the lethality of a conflict can be used as a proxy for civilian victimization (Kalyvas 2006; Weinstein 2007; Downes 2008; Human Security Report 2012). Militias are sometimes viewed as “violence machines” that kill men and rape women, and as central to understanding “new wars” (e.g., Kaldor 2006) in which “gratuitous and senseless violence” is “meted out by undisciplined militias, private armies and independent warlords” (Kalyvas 2001, 102).²⁵ In these cases, commanders of armed groups may turn a blind eye to atrocities of all types by their fighters, whether strategic or not. Following from this, a final alternative explanation for why some militias are more frequently reported as sexual violence perpetrators might be that the overall level of violence in the conflict more generally increases the probability of sexual violence.

Empirical Strategy and Data

To test the hypotheses, we examine the reported use of sexual violence by all active militias in conflict-affected countries between 1989 and 2009. We use the Uppsala Conflict Data Program Dyadic Conflict Dataset v. 1-2010 (Harbom, Melander, and Wallensteen 2008) to define the universe of conflicts and the Pro-Government Militias Database (PGMD) (Carey, Mitchell, and Lowe 2013) to identify the active PGM groups within this sample. We use data from the Sexual Violence in Armed Conflict (SVAC) data set (Cohen and Nordås 2014) for information about militias’ perpetration of sexual violence.²⁶

The unit of observation is the militia group. The sexual violence data (described subsequently) are collected on the level of the conflict-actor-year, but the lack of temporal variation on key explanatory variables makes temporal

analyses difficult. Due to the relatively small number of cases, we estimate a linear regression²⁷ with Huber–White robust standard errors clustered on the country in order to account for heteroscedasticity and serial correlation.²⁸ In the next section, we outline the data sources and the variables used to test our hypotheses. A table of summary statistics and a correlation matrix of the main variables are displayed in the Online Appendix (Tables A3 and A4, respectively).²⁹

Dependent Variable: Sexual Violence Prevalence

The dependent variable is the highest reported prevalence of sexual violence by the militia group in any year the militia group was active, as reported in the SVAC data set (Cohen and Nordås 2014).³⁰ As described previously, sexual violence prevalence is measured on an ordinal scale, coded from 0 to 3 (from no reports of sexual violence to reports of massive sexual violence).

To test Hypothesis 1, that *sexual violence is more likely by militias that suffer from low cohesion*, we use a dummy variable from the PGMD for whether or not a militia had children in its ranks.³¹ Abduction is associated with poor morale, a lack of loyalty to the armed group, and low cohesion (Cohen 2015). However, as no direct measures of such recruitment practices by militia groups exist, we use the child soldier dummy as a proxy measure. Previous research suggests that the recruitment of children is a reliable proxy of forced recruitment. For example, Beber and Blattman (2013) find that child soldiering is highly correlated with coercive recruitment across a sample of African³² armed groups, perhaps because children may more easily be forced into joining (Tynes and Early 2013, 7).³³ The cross-tabulation presented in Table 2 offers initial support to Hypothesis 1—34 percent of militia groups that recruited children were reported as perpetrators of sexual violence, while only 12 percent of militia groups that did not recruit children were reported as sexual violence perpetrators.

To test Hypothesis 2, that *sexual violence is less likely by militias that espouse an explicit ideology*, we use a dummy variable from the PGMD indicating whether a group's membership was based on ideology.³⁴

Table 2. Cross-tabulation: Sexual Violence and Child Recruitment by Militias.

		Child recruitment	
		No	Yes
Sexual violence	No	88% (155)	66% (31)
	Yes	12% (22)	34% (16)
		100% (177)	100% (47)

Note: Numbers in parentheses is the N.

To test Hypothesis 3, that *sexual violence is less likely by militias that have received formal military training*, we use a dummy variable from the PGMD indicating whether the militia received government training.³⁵ This measure is the best indicator of training available, but there is one caveat: a value of one for this measure captures militias that are trained by governments, but there could be cases where PGMs are trained by other groups. Such (likely very rare) cases would be coded 0 on this measure.

To test the alternative explanations from the previous literature, we include measures of country- and conflict-level factors. As measures of state capacity, we use the log of annual gross domestic product per capita of the state in which the militia group operates (GDPpc, ln), derived from the World Bank's World Development Indicators. We also use the maximum reported level of state failure from the Political Instability Task Force (PITF) (Marshall, Gurr, and Harff 2009), ranging from one (adverse regime change with no significant weakening of state institutions) to four (complete collapse of state authority). We control for regime type by including the Polity IV scale, ranging from full autocracy (−10) to full democracy (+10), as well as a squared measure of this scale (Marshall and Jaggers 2002).³⁶ As controls for conflict-level factors, we include a dummy for ethnic conflict, defined as whether there was ethnic mobilization (Forsberg 2008). To account for conflict severity, we use the log of the mean of the best estimate of annual casualties over the course of the armed conflict, generated from the UCDP Battle-related Deaths data set (Sundberg 2013).

Finally, in one model, we include three control variables widely used in studies of human rights violations during armed conflicts (e.g., Poe and Tate 1994).³⁷ First, we include the log of population size of the country, using a variable from the World Development Indicators, with the expectation that more atrocities are committed in more populous countries. Second, we include the duration of the conflict (the number of years) to account for the likelihood that longer conflicts have a higher probability of exhibiting at least one year of sexual violence perpetrated by a militia group. Third, we include whether the militia is a “semi-official group” that is closely linked to the government, as opposed to an informal group only “loosely affiliated” with the state, using a dummy variable from the PGMD (Mitchell, Carey, and Butler 2014, 13).³⁸ Based on Mitchell, Carey, and Butler (2014), informal militias should be associated with an increased prevalence of “agent-centric” violations, of which sexual violence is one form.³⁹

Analyses

We test the proposed hypotheses in a data set covering all 224 militia groups active in the sixty-four armed conflicts that took place in fifty countries between the years 1989 and 2009. Table 3 displays the results from two multivariate regression models. Model 1 includes the three main explanatory variables, as well as the controls that do not reduce the sample, and model 2 is the full model with all controls.

Table 3. Sexual Violence by Militias in Armed Conflict.

DV: sexual violence prevalence	(1)	(2)
Child recruitment	0.398 (0.143)***	0.472 (0.170)***
Ideological militia	0.024 (0.072)	-0.028 (0.095)
Government-trained militia	0.211 (0.088)**	0.221 (0.095)**
GDPpc (ln)	-0.172 (0.075)**	-0.178 (0.073)**
State failure	0.081 (0.075)	0.068 (0.096)
Regime type	-0.006 (0.009)	0.001 (0.010)
Regime type (squared)	0.001 (0.002)	0.001 (0.002)
Ethnic mobilization		0.002 (0.002)
Battle deaths (annual mean) (ln)		0.054 (0.109)
Population (ln)	-0.060 (0.025)**	-0.076 (0.026)***
Conflict years (ln)	0.017 (0.006)***	0.016 (0.007)**
Semi-official militia	-0.185 (0.093)*	-0.186 (0.104)*
Constant	2.206 (0.765)***	2.361 (0.811)***
N (militia groups)	224	200
R ²	0.229	0.245

Note: Robust standard errors in parentheses, clustered on country. Sexual violence prevalence is the maximum reported prevalence across the three sources in the SVAC dataset.

*** $p < .01$.

** $p < .05$.

* $p < .1$.

In model 1, child recruitment is strongly associated with sexual violence by militia groups. In other words, militia groups that recruit child fighters are more likely to perpetrate higher levels of sexual violence than groups that do not recruit children. Based on the assumption that child recruitment serves as a proxy for abduction and low cohesion, this result provides support for Hypothesis 1 that *sexual violence is more likely by militias that suffer from low cohesion*. The finding is also supportive of the combatant socialization hypothesis that armed groups may use sexual violence to build cohesion when recruitment results from abduction.

We do not find support for Hypothesis 2, that *sexual violence is less likely by militias that espouse an explicit ideology*, in model 1. The coefficient for ideological militias is statistically insignificant and positive, which is not in the expected direction. Hence, ideological militias are not less likely to perpetrate sexual violence than militias that are not ideological.

Hypothesis 3, that *sexual violence is less likely by militias that have received formal military training*, is also tested in model 1. Here, we find that groups that have received government training are associated with *higher* reported prevalence of sexual violence than groups that have not received such training. The relationship is statistically significant at the 5 percent level, but is in the opposite direction of the expectation. On the face of it, this is a curious finding, as training is often assumed to increase discipline and restraint in the use of sexual violence.

Why might training be associated with increased sexual violence? Several explanations were considered but ultimately rejected. First, an association between government training and sexual violence could suggest that government troops are training militias to commit atrocities. However, we are not aware of any evidence from the sexual violence literature that suggests such training has occurred. Alternatively, but equally unlikely, it is possible that the causation is reversed and that militias seek training by state militaries to improve discipline and to rein in abhorrent behavior, including sexual violence. However, the temporal evidence presented earlier shows that states often commit sexual violence simultaneous to militias rather than serving as paragons of restraint. Finally, semiofficial militias may be driving the relationship between government training and sexual violence; however, government training and semiofficial status are only weakly positively correlated (.31), and the control for semiofficial status in model 2 is negative and only marginally significant (at the 10 percent level).

We argue instead that the fact that government-trained militias are associated with more sexual violence suggests that sexual violence is, at a minimum, implicitly sanctioned by these states, and that sexual violence spreads between armed actors as a “practice.” Elisabeth Wood (2012, 393; 2015) introduces the notion of rape as a practice, an intermediary category between rape as strategic or “purposefully adopted by commanders in pursuit of group objectives,” and rape as opportunistic or not instrumental for group reasons. A “practice” describes violence that is not ordered, but is tolerated by commanders. Wood explains the practice of rape originates in innovation or imitation across armed groups through a process of *diffusion*. The relationship between government training and sexual violence by militia groups may be evidence of imitation and diffusion, whereby the practice of sexual violence spreads from government troops to the militias they train. Indeed, it is well established that in some cases, such as Sierra Leone, the membership of state militaries and PGMs is fluid with combatants serving in both groups over the course of a conflict. Such fluidity further serves to diffuse practices across groups. Hence, while states do not explicitly train militias to commit such atrocities, the practice of sexual violence by the state leads to replication of this behavior by militias.⁴¹

Finally, the alternative explanations find little support in model 1. Only GDPpc is statistically significant and in the expected negative direction: militias operating in countries with low GDPpc are reported to commit more sexual violence than are militias in more economically developed countries. This may support the alternative hypothesis of a relationship between militia violence and state weakness; however, the finding should be interpreted with some caution for two main reasons. First, neither state failure nor military quality, the other measures of state weakness tested in the main model and the robustness checks, are statistically significant. Second, GDPpc may not be adequately capturing the mechanism through which states find it necessary to delegate violence to militias—namely, their inability to carry out such violence themselves, particularly in remote areas. Finally, regime type, another alternative argument, also has no statistically significant effect.

In terms of controls, conflict duration is significant and positive as expected. This may be because more conflict years means more opportunity for committing sexual violence or that a longer conflict allows more time for sexual violence to diffuse between armed actors as a “practice.” Contrary to expectations, however, there is a negative effect of the size of the population of the country where the militia operates. Turning to the status of militias, model 1 shows that the dummy variable for semiofficial militias (as opposed to informal militias) is negative as hypothesized, and is significant at the 10 percent level. The direction of this finding is in accordance with the hypothesis that semiofficial militias are less likely than informal militias to commit human rights violations because they are more closely linked with the government and offer less opportunity to shift responsibility (Mitchell, Carey, and Butler 2014, 13). Although we do not find support for the delegation argument, this finding merits further analysis in future studies.

Model 2 shows the relationship between militia group factors and sexual violence prevalence, including ethnic conflict and battle deaths as additional controls, which reduces the sample size from 224 to 200 militia groups. The additional controls included in this model do not have a significant impact on sexual violence prevalence by militia groups. The insignificant results for ethnic conflict echo similar findings from Cohen (2013) for rape by rebel groups and state forces in civil wars. Finally, sexual violence is not significantly correlated with battle deaths, contrary to assumptions in the literature about covariation between killings and other forms of civilian victimization. This finding strongly suggests that sexual violence ought to be studied in its own right and cannot simply be proxied by lethal killings.

The results in model 2 are largely the same as in model 1. Militia groups that recruit children are significantly more likely to have a high prevalence of sexual violence than groups that do not recruit children, and the substantive impact of child recruitment is by far the largest of any of the variables. Government training is associated with a higher prevalence of sexual violence by militias and remains statistically significant at the 5 percent level. The coefficient for ideological militias, however, changes to a negative sign—in the expected direction for Hypothesis 2, but remains statistically insignificant.

Robustness Checks

To test the robustness of the main findings, we estimated alternative specifications of model 2 in Table 3. In all cases, the results remain robust and are displayed in Table A6 in the Online Appendix. First, we estimated a model with clustering on conflict rather than country. Second, we tested an alternative dummy variable of ideological versus nonideological groups to determine if there are differences between militias that are religious (one) and those that are not (zero).⁴² The religion dummy variable is insignificant, as was the original measure of ideological militias. Third, neither of the two alternative control variables, military quality and log of militia size, were significant predictors of sexual violence, but they dramatically reduced the sample. Fourth, model 2 could potentially be critiqued for including too many variables for the

relatively low N in the model. We have therefore also estimated a scaled down version of the model with all insignificant variables removed (Online Appendix, Table A6, model 4), and this does not alter the findings, providing additional confidence in the robustness of the results. Finally, the results are also consistent when we limit the sample to the subset of African militias ($N = 75$; not shown).⁴⁰

Limitations

Our central question is why some militias terribly abuse civilians, while others mostly refrain from such violence. While the findings reported in the empirical analysis hold across a variety of specifications and robustness tests, there exist three main limitations of the current study. First, although we examine a particularly pernicious form of civilian victimization—one sexual abuse is only a subset of the repressive violations that states may choose to delegate.⁴³ Second, the analysis is cross sectional, due to a lack of temporally variant data on most militia group characteristics. However, case studies could potentially trace with greater temporal specificity the recruitment behavior of particular militia groups as well as their relationships with state forces. Third, though we have interpreted the child recruitment result as supportive of an argument about combatant socialization, other potential explanations might also account for the association between child soldiers and sexual violence. Some have argued that children can be readily manipulated to fight (Wessells 2006) and that they do so in an unrestrained, fearless fashion, which might imply that sexual violence is more easily commanded in organizations where children are recruited for combat. In addition, the literature on child soldiering proposes that the insecurity of refugees and internally displaced persons are associated with child soldiering (Achvarina and Reich 2006). Hence, a possible complementary explanation for the relationship between child soldiers and sexual violence could be an opportunity-based argument, related to the access of militia groups to vulnerable populations.

Finally, in this nascent field of research, some characteristics of militias are not yet documented systematically, such as to which constituency militias are most loyal, whether they are operating in their home areas and recruitment bases, and whether they control territory, all of which could affect an armed group's behavior (Johnston 2008; Kalyvas 2006). More detail on the relationship between state forces and militias would also be useful, such as what is communicated during government training of militias, and the degree to which membership between them is fluid or fixed, as well as other characteristics of militias, such as measures of fighting effectiveness and duration of service in a unit. Future studies could fruitfully explore these aspects of militias further.

Conclusion

We examine one important type of civilian abuse that current theories suggest should be very likely to be delegated to militias by states: sexual violence. We began

this analysis by pointing to two empirical patterns that are not well explained by existing arguments about militia victimization of civilians. First, militia-perpetrated sexual violence and state-perpetrated sexual violence appear to be complements rather than substitutes. This pattern presents a strong challenge to explanations about the delegation of violence to militias because it does not support what should be the most obvious observable implication of the delegation argument: if states are delegating violence to militias, it should follow that they do not also commit the violence themselves. Indeed, militia violence follows state violence in a majority of the cases, rather than being part of a system of delegation or division of labor. This important finding presents opportunities for future research into the conditions under which states co-perpetrate violence with militias.

Second, there is substantial variation in the perpetration of violence by militia groups. About 8 percent of the militias in the study period perpetrated the two worst levels of sexual violations against civilians, but the majority of militias—some 83 percent—were not reported to commit any sexual violence.

The findings indicate that two main factors help to explain such variation. First, militia groups that rely on the abduction of fighters, measured by child recruitment, are significantly more likely to be associated with a high prevalence of sexual violence than groups that refrain from such recruitment. Second, government-trained militias are more likely to perpetrate sexual violence than groups that have not received such training. These findings support two related arguments: first, that sexual violence serves to generate intragroup cohesion among fighters recruited through abduction; and second, that sexual violence by militias may be in part the result of diffusion, whereby states that use sexual violence disseminate this practice through interactions with the militias they train.

Research on militias and their repertoires of violence is still developing. The current study can contribute to the growing field of knowledge in at least two ways. First, we present a cross-national analysis at the level of the militia group, an advance beyond analyzing violence at the level of the conflict or country. The findings are broadly generalizable because the analysis includes all militia groups involved in armed conflict in the study period, and the findings are consistent across world regions. Second, we study a form of violence that, although very important, has typically been excluded from cross-national studies of armed conflict, state repression, and human rights violations, which more commonly focus on deaths and imprisonment. In addition, this study contributes to the expanding literature on sexual violence by focusing on militias, an understudied perpetrator group type incorrectly assumed to be among the most common violators.

Finally, researchers and policymakers, most notably within the United Nations, have called for more systematic analyses of wartime sexual violence so that policies can be based on empirical evidence.⁴⁴ Based on the patterns identified in this study, reports of sexual violence by state forces could serve as an early warning sign that militias may be likely to follow suit. One key lesson for policymakers is therefore to monitor and to respond quickly to sexual violations by state forces, in order to

prevent the escalation and spread of violence. More broadly, the data and results presented here can shed light on the dynamics of conflict-related violence, and perhaps eventually, to a mitigation of its grievous harms.

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Notes

1. Mitchell, Carey, and Butler (2014) perform a matching analysis on the level of the country and argue that the observed variation in repression is caused by the presence of militias.
2. For recent studies of rape and others forms of sexual violence during wartime, see Wood (2009), Leiby (2011), and Cohen (2013).
3. While the present analysis is focused on sexual violence, Stanton (2015) examines civilian targeting—defined broadly to include massacres, bombing campaigns, and other deliberate attacks on civilians—and finds similar patterns of covariation, with militia violence *complementing* rather than replacing state violence.
4. As is presented later in the article, only 17 percent of the 224 militia groups included in the analysis were ever reported as perpetrators of sexual violence.
5. In contrast to previous research that has typically described a relationship of simple delegation between states and militias, Staniland (2015) presents a more complex set of relationships that includes suppression, containment, collusion, and incorporation.
6. The Kurdish conflict in Turkey is the exceptional case, where Village Guard militias were reported as perpetrators of sexual violence.
7. In four countries—Afghanistan, Haiti, Liberia, and Sierra Leone—the highest reported prevalence of sexual violence by a militia was greater than the sexual violence by the state forces.

8. Micro-level evidence from others' studies confirms these broad patterns. For example, in a detailed analysis of 8,000 testimonies of witnesses and survivors in El Salvador, Leiby (2011) finds that the regular armed forces reportedly committed far more sexual violence than the paramilitary.
9. Sexual violence data are from the Sexual Violence in Armed Conflict (SVAC) data set (Cohen and Nordås 2014). A code of 3 (massive) indicates cases where sexual violence was described as "systematic," "massive," or "innumerable," that the actor used sexual violence as a "tool of war" on a "massive scale," or there were 1,000 or more victims of sexual violence by the given actor in a given year. A code of 2 (numerous) is assigned when sexual violence was described as "common," "frequent," "recurring," or there were descriptions of 25 to 999 victims of sexual violence in a given year. A code of 1 (isolated) indicates that there were "reports," "isolated reports," or "there continued to be reports" of occurrences of sexual violence, or less than 25 victims of sexual violence were reported in a given year.
10. We include as controls the log of the population, the log of gross domestic product per capita, severity of the conflict in the country (as measured by the total number of battle deaths in the country in a given year), the Polity score, the square of the Polity score, and regional dummies.
11. An analysis of whether state forces or militias were the "first movers" shows that state forces committed sexual violence prior to militias in the majority (64 percent) of conflicts. This pattern is consistent across all regions of the world (see Online Appendix, Table A1).
12. We also check for the proportional odds assumption. The ordered logit model assumes that for any given level of the dependent variable, the effects of the independent variables are the same (i.e., the effects are the same for each cumulative logit). A Brant test shows that this assumption is not violated (see Online Appendix, Table A2).
13. Recent research that examines these features of armed groups includes Wood (2009), Hoover Green (2011), and Cohen (2013).
14. Other plausible proxies for intragroup cohesion might include fighting effectiveness or the average length of service in a unit, but cross-national data for these measures do not currently exist.
15. Costs, or potential costs, include the short-term risk of contracting sexually transmitted infections that can weaken the fighting capacity of the troops (Cohen 2013; Kelly 2010), indiscipline, and the loss of constituent support and access to high-quality intelligence from the civilian population (Wood 2009). Longer-term costs include sanctions or punishment; however, in the vast majority of cases, wartime sexual violence is perpetrated with almost total impunity (e.g., Lindgren 2011).
16. Cohen (2013) does not disaggregate militia groups in the empirical tests nor in the analyses of variation across armed groups.
17. See Forney (2015) on how the process of careful screening of recruits by militias—even in the presence of a wealth of material resources—can help to prevent civilian abuses.
18. Wood (2008, 340) also writes, "if an organization aspires to govern the civilian population, leaders will probably attempt to restrain combatants' engagement in sexual violence against those civilians" so as to not lose civilian support and high-quality intelligence.

19. In an innovative study of right-wing paramilitary fighters in Colombia, Oppenheim, Vargas, and Weintraub (2012) find that military training had no effect on civilian casualties, but that political training reduced such casualties. There are also well-known historical examples of state armies with extensive military training committing mass rape, such as the rape of Chinese women in Nanking by Japanese soldiers in 1937 and rape during the fall of Berlin in 1945.
20. The state can also incur costs from delegation, as they may be unable to control unwanted behavior by militias due to problems of control over semiautonomous actors; in addition, members of militias may commit atrocities for private reasons, such as securing loot. Such problems are often discussed in a principal–agent framework (e.g., Butler, Gluch, and Mitchell 2007; Mitchell, Carey, and Butler 2014). States with a strong capacity for enforcing decisions and overseeing territory are less in need of militias in the first place (Carey, Colaresi, and Mitchell 2015), and states endowed with such capacities are also likely to have stronger and more professional militaries that are better equipped to prevent sexual violence.
21. States with low capacity might include new regimes, countries where the regime faces internal threats, such as a civil war, or countries with weak regular militaries. Some of these elements of weak states are less relevant—or difficult to test—for the sample of countries in the current study. Carey et al. (2015) find that the use of militias in general is more likely in weaker democracies, and in those countries facing internal threats.
22. It is a consistent finding in the repression literature that full democracies engage in less severe human rights violations (e.g., Davenport and Armstrong 2004), mainly because of checks on political leaders.
23. Semi-democracies are also most likely to experience intrastate armed conflicts (Hegre et al. 2001).
24. An alternative, but less frequently mentioned, hypothesis is that there may exist ideological reasons for restraint in ethnic conflicts, such as the belief that engaging in sexual violence against enemy women is “polluting” to the perpetrator (e.g., Wood 2009; Hoover Green 2011).
25. See Kalyvas (2001) for a critique of the distinction between “new” and “old” wars.
26. The SVAC data include years when the conflict was active, the five years post-conflict, and “intermediary years,” when lethal violence dropped below twenty-five battle-related deaths for one to four consecutive years but then crossed the threshold again for one or more years (Cohen and Nordås 2014).
27. Ordered probit analyses would be an alternative estimation strategy, but estimating models using this method makes it more difficult for models to converge and more difficult to evaluate the substantive impact of the independent variables. We estimate ordered probit models as a robustness tests (not shown), revealing similar results as those reported in Table 1.
28. We also estimate the same models with clustering on conflict as a robustness check, which does not alter the findings.
29. The correlation matrix does not show any correlations that give rise to concerns about the models in the analyses. Estimating variance inflation factor scores indicates there are no issues of multicollinearity in the reported models, as all scores are < 2 .

30. The SVAC data include information on sexual violence prevalence based on reports from the US State Department, Amnesty International, and Human Rights Watch. We use the highest reported level of sexual violence during a conflict coded from any of these three sources.
31. We also considered a dummy from the Pro-Government Militias Database indicating whether any volunteers were ever reported as members of each militia group. This variable, however, does not preclude the possibility that abduction is also taking place and is therefore a less reliable proxy for low cohesion.
32. We include regional dummies as controls in robustness checks to verify that the results are not being driven by a regional effect (see Online Appendix, Table A5).
33. The leadership of the Lord's Resistance Army, the central case in Beber and Blattman (2013), used the spiritual practices of a cult religion to create cohesion among abducted child recruits. The LRA, however, is an unusual case in a number of ways, and does not fit well with the general argument of sexual violence against civilians creating cohesion.
34. As an alternative, we also test whether religiously oriented groups have a similar effect. While a dichotomous variable is a rough measure of the presence or absence of an ideology, there do not currently exist more detailed data on types or content of ideologies.
35. There are a number of missing cases for this variable. We recode the variable so that both of the missing and unclear cases are coded zero to avoid sample reduction. Estimating models with the missing observations left as missing does not alter the findings. The findings also remain robust if unclear cases are coded one.
36. We use the mean of the score for the relevant years.
37. These variables were also selected because they avoid significant reduction of the sample size, as well as problems of multicollinearity. However, we include these in only one model, because the relatively low N makes the inclusion of all control variables potentially problematic.
38. One potential concern is that a strong affiliation between the militia group and the state may also imply training by the state. However, the two measures are not highly correlated ($-.17$), allowing them to be used in the same model.
39. We estimate models using alternative controls for robustness checks. This includes log of military quality in terms of size of the state's regular armed forces (Lacina 2006) and the size of the militia group (Carey, Mitchell, and Lowe 2013). These variables have no significant impact on the results and are cut from the models in Table 3 due to missing values that reduce the sample size to eighty-eight armed groups; results are reported in Table A6 in the Online Appendix. When these controls are introduced, ethnic mobilization as well as conflict severity turn significant (and positive). These results seem primarily due to the reduced sample in the case of conflict severity. In addition, there is little variation on the ethnic mobilization variable in the reduced sample, rendering this result less certain.
40. When estimating models with regional dummies, none of the regional dummies are significant, while the results for the variables of interest remain robust (Online Appendix, Table A5).
41. The temporal analysis of "first movers" (Online Appendix, Table A1) confirms that state forces commit sexual violence *before* militia groups start to commit such violence in a clear majority of conflicts.
42. The correlation between ideological and religious militias is both negative and low ($-.06$).

43. When we estimate the same main model with the dependent variable as battle deaths, used in the previous literature as a proxy for civilian abuse, none of the explanatory variables are significant. This again emphasizes the importance of analyzing lethal violations separately from sexual violations.
44. For instance, United Nations Security Council *Resolution 1960* (2010) reiterates that the United Nations wishes to “enhance data collection and analysis of incidents, trends, and patterns of rape and other forms of sexual violence” (p. 4).

Supplemental Material

The online appendix is available at <http://jcr.sagepub.com/supplemental>.

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