

# Civilian Victimization and Ethnic Civil War\*

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## Abstract

While many studies provide insights into the causes of wartime civilian victimization, we know little about how the targeting of particular segments of the civilian population affects the onset and escalation of armed conflict. Previous research on conflict onset has been largely limited to structural variables, both theoretically and empirically. Moving beyond these static approaches, this paper assesses how state-led civilian victimization targeting members of specific ethnic groups affects the likelihood of ethnic conflict onset, and the evolutions of conflicts once they break out. Relying on a new dataset with global coverage that captures the ethnic identity of civilian victims of targeted violence, we find evidence that the state-led civilian victimization of particular ethnic groups increases the likelihood that the latter become involved in ethnic civil war. We also find tentative, yet more nuanced, evidence that ethnic targeting by state forces affects the escalation of ongoing conflicts.

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# 1 Introduction

In recent years, conflict researchers have made a major effort to understand the relationship between civilian victimization and civil war. We now have a much better understanding of why armed actors use violence against civilians and the consequences of this type of strategy. Focusing on the micro-level, [Kalyvas \(2006\)](#) spearheaded this research program by treating one-sided violence as a predictable wartime phenomenon, whose spatial and temporal variation is accounted for by territorial combat dynamics. Other scholars have identified structural variables such as the type of pre-existing resources as important determinants of wartime violence ([Weinstein 2007a](#)). Following this pioneering work, scholarly attention to the dynamics of civil wars intensified, including the wartime consequences of victimization (e.g. [Downes 2007](#); [Kalyvas and Kocher 2007](#); [Lyall 2009](#); [Kocher, Pepinsky and Kalyvas 2011](#); [Condra and Shapiro 2012](#); [Schubiger 2013a](#)).

However, one-sided violence is not only a wartime phenomenon. Victimization also takes place in peacetime, for example when governments resort to violent repression of non-violent challengers. In fact, repression could escalate violence and play a role in the onset of civil war itself. In Syria, for example, the deadly crackdown of pro-democracy protests by the Syrian regime in 2011 has been widely identified as one of the key determinants of the country's rapid descent into civil war ([BBC 2017](#)). Thus, to assess the impact of violent repression on conflict onset and escalation systematically and on a large scale, it is necessary to study civilian victimization during peaceful episodes. Indeed, previous attempts to go beyond structural explanations of civil wars have typically studied such dynamics ([Davenport, Armstrong and Lichbach 2006](#); [Young 2013](#)).

Moreover, the focus on micro-level dynamics can be limiting in that it tends to overlook more aggregate phenomena. If we want to understand the evolution of conflicts in their entirety, it may not be sufficient to analyze the spatiotemporal distribution of individual conflict events, or the effects of counterinsurgency tactics. Going beyond the level of individuals, groups, and communities, escalation exhibits patterns that are produced by

complex interactions among these actors. When explaining conflict onset, this limitation becomes even more obvious. In many countries, state-led repression targets particular groups because of their alleged links to violent or non-violent challengers. Therefore, it is important to account for processes of mobilization or radicalization that take place at the level of such groups.

As illustrated by the conflict in the Niger Delta, victimized groups are frequently identified based on their ethnicity. This conflict started in the early 1990s, when indigenous ethnic groups in the Niger Delta first protested against the actions of foreign oil corporations. Although the struggle was initially mainly non-violent, the Nigerian government increased its repressive measures, resorting in some cases to mass killings. An infamous example of indiscriminate violence took place in November 1999, when the Nigerian military killed hundreds of unarmed civilians in the Ijaw village of Odi, in Bayelsa State ([Human Rights Watch 1999](#)). This event, among others, is said to have played a crucial role in triggering the armed conflict that broke out around 2004 between the Nigerian government and several local rebel groups, such as the Ijaw-based Niger Delta People's Volunteer Force (NDPVF).

In this paper, we study whether state-led ethnic targeting of civilians affects the outbreak and escalation of conflicts around the globe. We argue that state-led victimization of ethnic groups increases the risk of civil war by increasing rebel organizations' pool of potential recruits. State violence amplifies the injustice felt by these groups by creating new violent-related grievances, that are qualitatively different from the previous structural grievances. State repression thus reinforces the idea that violent action is the only viable path to political change ([Goodwin 2001](#)) and increases individual motivation to participate based on outrage and self-defense ([Wood 2003](#); [Mason and Krane 1989](#); [Kalyvas and Kocher 2007](#); [Schubiger 2013a](#)). Therefore, we propose that state-led one-sided violence against an ethnic group will increase the likelihood that organizations linked to this group will take up arms against the government. We also posit that state violence against members of particular groups will help rebels increase their fighting effort in on-

going conflict, hence contributing to the escalation of civil wars. Further exploration indicates that this effect should be most pronounced for groups without a prior war history, or in first-time conflicts. As violence triggers several one-off mechanisms related to mobilization and organizational patterns, a history of prior conflict lessens the impact of further ethnic violence by the state.

To test these hypotheses, we draw on the newly collected Ethnic One-Sided Violence (EOSV) dataset (Fjelde et al. 2017), which records the ethnic identity of victims in campaigns of one-sided violence around the globe. Using the ethnic group as our unit of analysis, we address the question of whether targeting civilians along ethnic lines increases the risk of civil war onset between the government and targeted groups. We also probe the effect of one-sided violence along ethnic lines on the escalation of ongoing conflicts. Our results show that targeting civilians from a particular ethnic group implies a higher risk of civil war onset by armed groups linked to this group in subsequent years. Once the conflict is under way, such violence makes a deescalation much less likely. However, these effects are largely driven by first-time ethnic civil wars. In the case of recurring conflicts, we find no such effect. We conclude that ethnic targeting can be a trigger of conflict onset and an obstacle to pacification.

This study contributes to the literature on the endogenous origins of civil war which, by focusing mainly on the strategic incentives of elites, has paid little attention to the relationship between combat violence and other types of violence. We also contribute to the literature on the consequences of civilian victimization by analyzing new data at an intermediate level of analysis, namely that of the ethnic group. This approach overcomes the limitations of many studies that tend to treat all opposition actors as if they were unitary.

We proceed as follows. The next section reviews the literature on the relationship between different types of political violence. We then introduce our argument and outline our theoretical expectations, followed by a description of the data and our research design.

The following section presents the results. We conclude by discussing the implications of the results and highlighting the avenues for further research.

## 2 Previous Research

Because there is little literature on civilian victimization and conflict escalation, we review three related bodies of research that focus on social movements and the repression-dissent nexus, the effects of victimization on the origin, evolution, and persistence of revolutions and civil conflicts, and micro-dynamics of civil wars.

First, the literature on social movements has traditionally analyzed repression as an important factor that affects the level of contention within countries ([Gurr 1970](#); [Tilly 1978](#); [Tarrow 1994](#)). The focus of these studies is mainly on explaining protest activity rather than civil war. However, this literature has not reached any agreement on the effect of repression. Theory and empirics suggest two different effects of repression, as it could both increase dissent by intensifying motivations and decrease it by increasing its costs.

In a pioneering attempt to resolve the problem of the effect of repression, [Lichbach \(1987\)](#) argues that dissidents tend to choose between alternative violent and non-violent methods depending on the costs of each strategy. Since repression raises the cost of the method currently in use, it will increase the likelihood that dissidents substitute their method for its alternative. Other accounts of this double-edged effect of repression explain that it depends on regime type ([Gupta, Singh and Sprague 1993](#)), or that short-term effects are different from long-term ones ([Rasler 1996](#)). Putting these theories to an empirical test, [Moore \(1998\)](#) finds evidence in favor of [Lichbach's \(1987\)](#) argument. However, his study only uses data from Sri Lanka and Peru, which limits the external validity of his findings.

Yet, despite being highly informative about the dynamics of protests, riots and violent attacks, these studies say less about the effect of repression on civil war. Clearly, civil

war onset differs from less dramatic protest events. [Davenport, Armstrong and Lichbach \(2006\)](#) try to overcome this limitation by applying this approach to the study of civil war onset. Arguing that structural explanations of war onset (see e.g. [Hegre and Sambanis 2006](#)) fail to account for previous state-opposition interactions as the breeding ground from which wars emerge, they try to identify escalation patterns of low-scale conflicts in dissident-government interactions. Based on arguably overly aggregated analysis, they fail to find definite support for specific explanations. In a similar test of whether state-dissident interactions are able to predict civil war, [Young \(2013\)](#) finds some evidence for the argument that state repression triggers civil war. Yet, similarly to [Davenport, Armstrong and Lichbach \(2006\)](#), he aggregates his repression and contention variables at the country level, which renders the study of dynamics within particular dyadic conflicts more difficult.

A second body of research that also explores the effects of victimization are the studies on the origins and evolution of revolutions and civil conflicts. In a seminal contribution, [Goodwin \(2001\)](#) explains how revolutions are triggered by state policies rather than merely being the product of material and economic conditions. In particular, he claims that violent political oppression by state authorities is a crucial catalyst of revolutionary movements. This helps to explain why some of these conflicts are much more persistent than others, as repression increases the motivations to fight and blocks alternative, non-violent methods of political change. This argument resonates with studies that have argued that indiscriminate violence by the incumbent backfires by increasing civilian collaboration and the supply of recruits for the rebels, as only selective violence has the capacity to deter civilians from joining or supporting rebel groups ([Kalyvas and Kocher 2007](#); [Kalyvas 2006](#); [Mason and Krane 1989](#)). Highlighting emotional instead of security-based mechanisms, and showing how grievances build up in reaction to the repressive response of state authorities, [Wood \(2003\)](#) argues that the moral outrage felt by the targeted groups increases support for the rebellion. Similarly, [Petersen \(2002\)](#) contends that emotions such as anger and resentment are more important than fear in explanations

of ethnic violence. While highly insightful and important, these studies fail to directly address the question of whether state violence against civilians increases the risk and intensity of civil wars.

Finally, the most recent literature that deals with the effect of one-sided violence focuses on the wartime dynamics of violence. Revisiting an older debate about the effectiveness of certain counter-insurgency methods (Merom 2003), most of these studies explore how incumbent violence against civilians may affect armed competition and subsequent insurgent attacks in the short run. Several studies find that state violence against civilians does in fact increase subsequent levels of insurgent violence (e.g., Condra and Shapiro 2012), while others have shown that state violence also increases downstream insurgent territorial control (Kocher, Pepinsky and Kalyvas 2011). However, such effects have also shown to be contingent on which segment of the population is being targeted (Condra and Shapiro 2012), and on how much insurgents rely on local civilian support (Toft and Zhukov 2015). Moreover, indiscriminate violence against civilians has also been found to have a negative effect on insurgent violence in some contexts (Lyal 2009). Thus, the debate is far from settled. More recent studies have tried to assess the effects of wartime state violence by proposing more refined explanations and empirical strategies. One example is Schubiger (2013a), who uncovers more complex results consistent with a positive effect of state violence on pro- and counterinsurgent mobilization during war, as well as a positive effect on the fragmentation of rebel groups. This literature has limitations, however. As argued above, the exclusive focus on wartime dynamics loses sight of the *onset* of civil wars, and it is only useful in explaining dynamics of violence once conflict is already underway.

All in all, much of the existing literature still suffers from a lack of attention to different types of violence, grouping together events that might not have much in common (Stanton 2016; Gutiérrez-Sanín and Wood 2017; Kalyvas 2006). Moreover, ethnicity is largely ignored in many of these accounts, which obscures its important role in defining

social groups, shaping patterns of targeting, and intensifying the moral outrage brought about by government violence.

Pitching our analysis at an intermediate level of aggregation, we explicitly highlight the role of ethnicity by linking together the identity of perpetrators, victims, and insurgents. Moreover, we explore the role of identity-based targeting in campaigns of one-sided state violence. Finally, our analysis offers a more general perspective than case-based studies by focusing on ethnic groups around the globe.

### 3 Theory

State repression is usually the response to a challenge posed by a non-state actor. The “law of coercive responsiveness” (Davenport 2007) suggests that the use of repressive tactics by governments to control dissent constitutes one of the most stable patterns of violent behavior. In the context of ethnic politics, this challenge may come from an excluded ethnic group that is trying to redress a situation of political inequality.

Existing research shows that political and economic inequalities along group lines motivate members of disadvantaged groups to pursue political change. Cederman, Gleditsch and Buhaug (2013) postulate a causal chain reaching from such horizontal inequalities to civil war via group-based grievances. The first step of the process, from inequality to grievances, entails the politicization of existing inequalities, which requires members of the opposing group to develop a collective identity, compare their status with that of the ethnic group in power, and blame this unfair situation on state authorities. The formation of widespread grievances facilitates the triggering of a mobilization process that enables the group to back up its claims with collective action.

Faced with such oppositional pressure, the government needs to decide how to respond. Either it can accommodate the demands of the ethnic group through negotiation and compromise, or it can block further action by rejecting the group’s demands. Under such circumstances, depending on the level of threat and its ideological commitment,



the government may resort to violence. We argue that the government's use of violence at this crucial point of the contention process will greatly increase the odds that a civil war eventually breaks out. Although political discrimination can also lead to civil war, as the ethnic group will still try to achieve political change, the government's resort to one-sided violence increases the likelihood of mutual combat. In fact, the use of violence by state authorities creates a new situation in which new violence-related grievances and incentives add to what is already perceived to be an unfair situation that motivated the challenge in the first place. These violence-related grievances are analytically different from the original structural grievances that affected previous interactions.

Our reasoning about how violence-related grievances augment and intensify pre-existing resentment with injustice and inequality builds on previous research by students of social movements and revolutions. [Wood \(2003\)](#) shows how government violence right before the outbreak of the Salvadorian Civil War motivated many *campesinos* (i.e. farm workers) to join the armed insurgency. Although long-felt injustices obviously played a role in determining participation, a major factor triggering rebellious mobilization was the government's repressive response to early, and mostly peaceful, challenges posed by social movements that called for political change. In this case, government violence represents a "moral shock" ([Jasper and Poulsen 1995](#)) that deviated from basic rules of legitimate government behavior. This shock motivates even those who did not have direct connections to the victims to engage in violent political action, thus reinforcing the process of collective identification that takes place in the context of ethnic movements and conflicts. This could set in motion micro-mobilization processes that facilitate future mobilization ([Opp and Roehl 1990](#)). Most importantly, governmental violence demonstrates that any alternative, non-violent path to political change is being blocked and that fighting is the only way forward: "Like political exclusion, indiscriminate state violence against mobilized groups and opposition figures is likely to reinforce the plausibility, justifiability, and (hence) diffusion of the idea that the state needs to be violently 'smashed' and radically reorganized" ([Goodwin 1997](#), 19).

Beyond outrage and resentment, state-perpetrated violence also provokes fear among the members of the targeted ethnic group because it is likely to affect the strategic incentives of both active and potential challengers (Sambanis and Zinn 2005). Particularly if collectively targeting members of entire identity groups, violent repression will alleviate the free-riding problem of insurgent collective action (Lichbach 1995), as joining the rebels could actually reduce the risk of being killed by government forces (Mason and Krane 1989; Kalyvas and Kocher 2007).

Thus, the use of violent repression by governments should increase the risk of civil war onset. Although repression is usually a direct response to a challenge by the opposition, the argument does not require that this challenge was initially supported by widely held grievances. Indeed, even if the challenge resulted from opportunistic elites or grievances felt only by a small minority, a repressive response by the government will create new violence-related grievances in an endogenous manner, thus widening and amplifying the claim advanced by the early challengers, even if they are not genuine or widespread.

In light of this discussion, we state the first hypothesis as follows:

**Hypothesis 1:** The use of state-led ethnic targeting increases the risk of civil war onset during the following years.

There are good reasons to believe that the effect of state-led ethnic targeting is not consistent across time. Previous relations between the ethnic group and the state should condition the effect of state repression on the risk of civil war onset, which is especially relevant for the outrage and resentment mechanisms. First, the distinction between structural and violence-related grievances will be particularly important if the previous situation did not include mass violations of physical rights. In the absence of past instances of violent conflict, the novel inclusion of violence in the state forces' repertoire constitutes a radical change in the rules of game, and thus we should see a corresponding impact on the group's decision to fight. If a previous conflict has already taken place, pre-repression grievances will likely also include past instances of violence, and thus the immediate impact should be less pronounced.

Second, as outlined above, violence often has a deeply polarizing effect on collective identities that helps reinforcing the process of collective identifications. Past studies suggest that the effect of wartime violence on collective identities tends to last long after armed conflicts end (Balcells 2012; Dyrstad 2012), especially in the context of intractable conflicts (Bar-Tal 2013; Bar-Tal, Halpern and Pliskin 2015; Kahn et al. 2016). If such lasting polarization occurs, it is less likely that new waves of repression will dramatically shift allegiances and the dynamics of mobilization in later conflict episodes. This should apply particularly to the case of ethnically salient social divisions, in which historic relations between groups often severely constrain the ability of leaders to mobilize followers and organize groups (Petersen 2002). Therefore, we would expect conflict onset dynamics to be less dramatically affected by state-led ethnic violence in contexts where identities are already polarized from prior war.

Following this, we state a second hypothesis:

**Hypothesis 2:** The positive effect of state-led ethnic targeting on civil war onset is more pronounced for groups without a prior war history than for those with such a history.

With regard to the effect of violence on the escalation of already ongoing conflicts, we build on the same logic and argue that once a civil conflict starts, governmental repression will exert a similar effect on the course of the conflict. As we have seen, more has been written on this in recent years than in connection with onset, especially within the counterinsurgency literature (e.g. Kalyvas 2006; Lyall 2009, 2010; Condra and Shapiro 2012). However, much of this scholarship has concerned the extent to which governmental repression is able to reduce or suppress short-term insurgent violence rather than the likelihood of conflict escalation in the long run.

We postulate that state violence against civilians will intensify the conflict in question. Both mechanisms outlined above, outrage and defense, should be applicable once armed conflict erupts. It is reasonable to expect the grievance mechanism to be less pronounced

during ongoing war, as moral shocks are likely to be less dramatic given that violence is already being used by both sides. In the context of a civil war, constant political violence comes to be seen as a normal aspect of civilians' everyday life. However, ongoing conflicts may also carry violence to segments of the population previously unharmed, and state violence typically reaches new, hitherto unmet levels. Moreover, and even if deliberate civilian victimization may sometimes be difficult to separate from collateral damage, there is strong evidence that government-led violence against civilians will exacerbate grievances during war and increase civilian support for insurgent groups (Goodwin 2001; Wood 2003; Condra and Shapiro 2012; Lyall, Blair and Imai 2013). Hence, and especially in cases where violence is clearly one-sided and characterized by identity-based collective targeting against members of particular groups, there is a strong potential for grievance-based and revenge-driven support for the insurgents that will help them increase their fighting effort against the state. In short, wartime state violence against civilians can be expected to exacerbate grievances and create new ones, thereby intensifying ongoing civil wars.

Previous research suggests that the defense mechanism continues to operate during wartime, and that it does so even more strongly than before conflict erupts (Goodwin 2001; Kalyvas and Kocher 2007). Indeed, under conditions of collective and indiscriminate state violence, and once the rebels are fully operating, organized, and armed, being a member of an insurgent group might be much safer than remaining in civilian life, given that rebels are typically also more mobile and better informed (Kalyvas and Kocher 2007). There is further evidence that wartime victimization drives processes of local polarization that make it even more difficult and dangerous for civilians to stay neutral in times of ongoing war (Schubiger 2013a; Weidmann and Zürcher 2013; Wood 2008).

In addition to grievances and defense, and partially driven by these dynamics, state violence against civilians has also been shown to drive insurgent fragmentation. Schubiger (2013a, 2014) shows that while state-orchestrated violence against civilians will help insurgents to enlarge their ranks, civilian victimization also tends to hinder insurgent co-

ordination as well as mechanisms for screening and indoctrination, thus increasing the probability of insurgent splits. Such fragmentation, in turn, is likely to further escalate violence against the government fueled by competition between rebel groups (Cunningham, Bakke and Seymour 2012; Wucherpfennig 2011). Fragmentation and competition might also have more indirect effects on escalation, as inter-group rivalry can help insurgent groups innovate and become more robust (Phillips 2015).

Finally, once combat starts, the government may find itself to be less constrained by norms or laws, especially if it has to resort to a “national emergency” or it is not a democratic regime (Valentino, Huth and Balch-Lindsay 2004; Valentino 2014). Fighting back, the rebels will have to intensify their struggle to protect civilians and to survive.

We capture this reasoning with a second hypothesis:

**Hypothesis 3:** The use of state-led ethnic targeting during armed conflict increases the probability that the conflict escalates in intensity during the following years.

As in the context of onset dynamics, we further argue that the impact of state-led ethnic violence should be mitigated in contexts with a history of prior conflict, particularly since our theory is focused on ethnic civil wars. We argue above that the effect of violence on collective grievances and identities weakens in the case of recurring conflicts. Processes of collective mobilization benefit greatly from violence when it constitutes a breaking point with the previous context. However, in a context of recurring conflict, the use of violence is no longer a ‘moral shock.’ In addition to this logic, we also emphasize a dynamics of network consolidations that should be especially relevant for the effect of state-led violence on ongoing conflict dynamics.

A history of ethnic conflict will likely have formed and consolidated ties between ordinary civilians and rebel groups, as well as members of armed networks themselves. Whereas in new conflicts such networks may still have to be forged, the integration into strong pre-existing social ties should greatly facilitate the ability of formed rebels to re-group, organize, and persist (Staniland 2014). Groups with prior exposure to war should

hence be less affected by external pressures, including state-sponsored violence against them and their alleged civilian allies. By contrast, newly formed armed groups can be expected to have the strongest incentives to respond to recruitment surges generated by state violence, and gain strength and momentum as a result.<sup>1</sup> At the same time, and as outlined above, while enabling armed groups to grow quickly in size, such recruitment surges also increase the armed groups' propensity to fragment, especially in younger armed groups where pre-existing institutions for screening and indoctrination are less strong, likewise fostering the intensification of violence (Schubiger 2013a, 2014). Moreover, newly formed armed groups will have greater incentives to signal their ability to protect their constituencies, to demonstrate strength, and to swiftly fight back, as their reputation and support base still have to be built. In the absence of prior ethnic war, we should hence see the strongest impact of state violence on the escalation of ethnic civil war.

In short, we argue that collective grievances, identity polarization, and network consolidation shape and constrain armed organizations, and thus condition the relationship between ethnic targeting and conflict escalation as a result. The expected effect is that subsequent rounds of violence will partly exhaust the effect of state-led violence on these dimensions, thus weakening its overall effect on the intensity of ethnic civil wars.

We summarize this reasoning in our last hypothesis:

**Hypothesis 4:** The impact of state-led ethnic targeting on ongoing conflict dynamics will be more pronounced during the first conflict.

## 4 Data

To test our hypotheses, we draw on newly collected data, the *Ethnic One-Sided Violence* dataset (EOSV) (Fjelde et al. 2017), which is the first dataset that identifies the ethnic identity of civilians killed by armed actors at a global level. The EOSV dataset links

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<sup>1</sup>On the importance of moving fast during early stages of mobilization, see for example (Weinstein 2007b).

the UCDP One-Sided Violence dataset ([Eck and Hultman 2007](#)) with the information on ethnic groups from the Ethnic Power Relations (EPR) dataset ([Vogt et al. 2015](#)) and identifies the ethnic identity of the victims of campaigns of one-sided violence around the globe. In addition, EOSV also identifies whether the armed actor engaged in ethnic targeting, i.e., cases in which the killings took place in a context of ethnic profiling of the prospective victims.

We extract our main explanatory variable from this dataset based on a binary variable, *Ethnic Targeting*, which indicates whether members of a certain ethnic group were identified among the victims of government-led one-sided violence in a given year *and* there is evidence of deliberate ethnic targeting. More specifically, given identified members of an ethnic group among the victims, we code whether there is evidence that at least 50% of the victims per ethnic group, perpetrator, and year, were subject to collective, identity-based targeting ([Gutiérrez-Sanín and Wood 2017](#)) along ethnic lines. Put differently, for ethnic targeting to be coded as 1 for a given ethnic group and perpetrator and year, we need direct or indirect evidence that at least half of the victims belonging to that ethnic group were killed in a context that exhibited such targeting patterns. Direct evidence of ethnic profiling of civilian victims includes for example the explicit announcement of ethnic targeting by leaders of perpetrating groups, or well-documented evidence of ethnic targeting gathered by independent human rights organizations on the ground. Indirect evidence includes, for example, information that only members of particular ethnic groups were subject to one-sided violence, while other groups engaging in similar behavior (e.g., participation in protests) were not (for more information see [Fjelde et al. 2017](#)). The variable is coded zero otherwise, regardless of the overall number of victims belonging to that particular ethnic group. We track whether there was ethnic targeting during the last year, and include results in the appendix using a coding that track ethnic targeting during the previous two or five years.

As this new dataset offers information on the victims of OSV events, and the type of targeting in which the perpetrator engaged, we can link these victims to the civilian

constituency of rebel groups. To do so, our dependent variable links ethnic groups with armed rebel organizations. Specifically, we use the ACD2EPR dataset (Wucherpfennig et al. 2012) to link conflict actors in the UCDP *Armed Conflict Dataset* (Gleditsch et al. 2002) with EPR ethnic groups (Vogt et al. 2015), and thus code an ethnic group-based measure of yearly conflict intensity. As we explain below in the analysis section, we perform two types of empirical tests. First, we examine the effect of ethnic targeting on conflict onset, using as dependent variable a binary measure of the outbreak of conflict between an ethnic group and the incumbent government. Following a similar approach as Cederman, Gleditsch and Buhaug (2013), we drop ongoing conflict observations, and exclude ethnic groups who enjoy a position of monopoly or dominance. Second, we estimate a transition model to account for conflict escalation, using an ordinal measure of conflict intensity as the dependent variable. This variable takes three different values: 0, when there is no conflict ongoing; 1, if there is a low-intensity conflict ongoing, and 2, if there is a high-intensity conflict ongoing. The difference between low- and high-intensity conflicts is measured based on the UCDP dataset, and defined in terms of yearly battle deaths: at least 25 for low-intensity conflicts, and 1000 for high-intensity ones. This allows us to also use observations of ongoing conflicts and thus to assess in more detail the conflict dynamics.

To test whether prior ethnic civil war alters the effect of ethnic targeting, we code additional variables referring to the the previous conflict history. In particular we code a binary variable, which indicates whether a group has previously experienced armed conflict, meaning that it is linked to an armed rebel organization that fought against the government in the past. If it is not, this indicator is set to zero, also for all yearly observations of the first conflict.<sup>2</sup>

In addition, we include a number of control variables. We follow Cederman, Gleditsch and Buhaug (2013) to provide a baseline model for our analyses. In particular, at

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<sup>2</sup>We also explore another version of this variable that also includes those groups that were involved in a conflict as incumbents, i.e., had a political status of senior partner, monopoly, or dominant during an ethnic civil war. As the results for our onset analyses suggested no substantive differences, we report the former in tables in the appendix.



the ethnic-group level, we include variables that indicate whether the ethnic group was excluded from central executive power, whether it was downgraded during the previous two years, its relative size to the national population, and the number of previous conflicts for the non-interaction models. At the country level, we include lagged measures of population and GDP per capita, as well as a dummy variable indicating whether there was another conflict ongoing in the same country the previous year.

## 5 Analysis

In this section we test our four hypotheses. We first check whether ethnic targeting by state forces increases the risk of civil war onset, and then turn to its effects once conflict is ongoing. For the onset part, we rely on probit regression models. In the second part on escalation we use Markov transition models (see [Amemiya 1985](#), 412ff) based on an ordered probit regression, as we use an ordinal measure of conflict intensity as defined above. In both cases, we probe whether the effect of ethnic targeting differs in first-time conflicts as opposed to repeated instances of war.

### 5.1 Civil War Onset

Table 1 shows the results of a probit analysis of the effect of ethnic targeting by the government on civil war. In the first column we report the results of a model offering a direct test of our first hypothesis, while the second model (column 2) comprises the interaction term with prior war history to allow a test of our second hypothesis.

The first column shows that the effect of ethnic targeting, although positive, fails to reach significance in the whole sample. Thus, we do not have sufficient evidence to support hypothesis 1, suggesting that this type of state-led violence does not affect all cases in the same way.

The second column provides results allowing to test hypothesis 2, which states that the effect of state targeting should be larger for groups without prior civil war history.

Table 1: Ethnic targeting (previous year) and conflict onset, probit model

	(1)	(2)
Ethnic targeting $t_{-1}$	0.263 (0.218)	0.833* (0.325)
Prior conflict		0.417*** (0.109)
Target * Prior conflict		-0.736+ (0.407)
Status excluded	0.279** (0.093)	0.287** (0.093)
Downgraded	0.790*** (0.120)	0.781*** (0.126)
Previous conflicts	0.170** (0.057)	
Log. Population, lag	0.059+ (0.030)	0.053+ (0.028)
Log. GDPpc, lag	-0.160*** (0.033)	-0.136*** (0.032)
Group size	0.534* (0.228)	0.508* (0.209)
Ongoing conflict, lag	0.330** (0.106)	0.347*** (0.104)
(Intercept)	-1.189*** (0.346)	-1.395*** (0.322)
Observations	13,508	13,508
Akaike Inf. Crit.	1,124.785	1,123.903

**Note:** + $p < 0.1$ ; \* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$   
Peace-year correction (linear terms, plus 2nd and 3rd order polynomials) omitted, clustered SE (country).

In this case, we find support for our hypothesis. Ethnic targeting has a positive effect for those cases without prior war, an effect that decreases once the first civil war has broken out. The interaction term, however, is only significant at the  $p = 0.10$  level. This result suggests that ethnic targeting increases the risk of civil war, but only among those groups that did not fight against the government before.<sup>3</sup>

The fact that the effect is stronger and significant for groups without prior war history guards against finding a spurious correlation, as previous conflicts may predict both previous OSV and current conflict. Although it is difficult to assess the actual causal impact of ethnic targeting, this result is consistent with our argument.

In the appendix we show results from the same models but measuring the occurrence of ethnic targeting during a period of two and five years, respectively. Again, the findings are very similar and in some cases stronger than for the one-year coding. Although we do not find a significant effect of ethnic targeting in the overall sample, ethnic targeting seems to have a positive effect on the risk of conflict onset for groups without prior war history. When conflict erupts, this effect becomes smaller.

As regards the control variables, the table shows results that are consistent with our expectations and previous research. Political exclusion increases the risk of civil war, particularly when the group was downgraded in the previous two years. Group size also shows a positive effect on conflict outbreaks, and so do country-level variables, such as population, GDP per capita, and ongoing conflicts in the same country.

## 5.2 Conflict Escalation

We now turn to the test of hypotheses 3 and 4, which state that state-orchestrated ethnic targeting during conflict makes the intensification of conflict more likely, particularly if it is the first conflict. As explained above, we use a Markov transition model (see [Amemiya](#)

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<sup>3</sup>In the appendix we provide additional analyses for which the prior war indicator does not only account for war participation as rebel force (as in table 1, but also for prior war participation as incumbent (see table 7). As the results are largely identical we refrain from using this more broadly defined variable in the subsequent analyses.

1985, 412ff) to test these hypotheses. As in this model our dependent variable can take three values, namely 0 for no conflict, 1 for low intensity conflict and 2 for high intensity conflict, we estimate an ordered probit model.<sup>4</sup>

Table 2 reports the results of this model without any interaction with prior conflict, testing hypothesis 3. We find that ethnic targeting by the government has a significantly positive effect during peacetime. In other words, this increases the likelihood of conflict onset.<sup>5</sup> When a low-intensity conflict is already ongoing this effect is reduced, so that, in contrast to hypothesis 3, ethnic targeting does not lead to conflict escalation. For ongoing high-intensity conflicts, we again find a positive effect, yet one that is very far from reaching statistical significance.

For our control variables we find the expected effects for conflict onset as in the previous model on onset. For ongoing conflicts, however, some of these effects are reversed. For example, a downgraded group is much more likely to engage in an ethnic conflict, but once it is involved in such a conflict, this effect is lessened to be completely reversed during high-intensity conflicts.

As the results from non-linear models are generally difficult to interpret, and even more so if they underlie a Markov transition model, we rely on average predictive differences in probabilities. For this we draw 1000 sets of coefficients from the estimated distribution and generate predicted probabilities for each of these 1000 sets under different scenarios (see Gelman and Hill 2007b). More specifically, we generate predicted probabilities for six scenarios, namely situations of peace, low and high intensity conflict, and each of these situations either with ethnic targeting or not. All other variables are kept for each observation at their sample values.<sup>6</sup> We then subtracted the average predicted

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<sup>4</sup>As we encounter some issues of quasi-complete separation mostly linked to the temporal controls, we employed a Bayesian version of the ordered probit model (see Gelman and Hill 2007a).

<sup>5</sup>We note that this coefficient is in support of our hypothesis 1, which found little support in our individual tests reported in table 1. This is due to the fact that in models we report in this latter table, we exclude observations of ongoing conflicts, which induces a loss of information that the transition model overcomes.

<sup>6</sup>The exception to this rule are the peace- and war-years, which were set to values reflective of the presence or absence of a (low/high intensity) conflict.

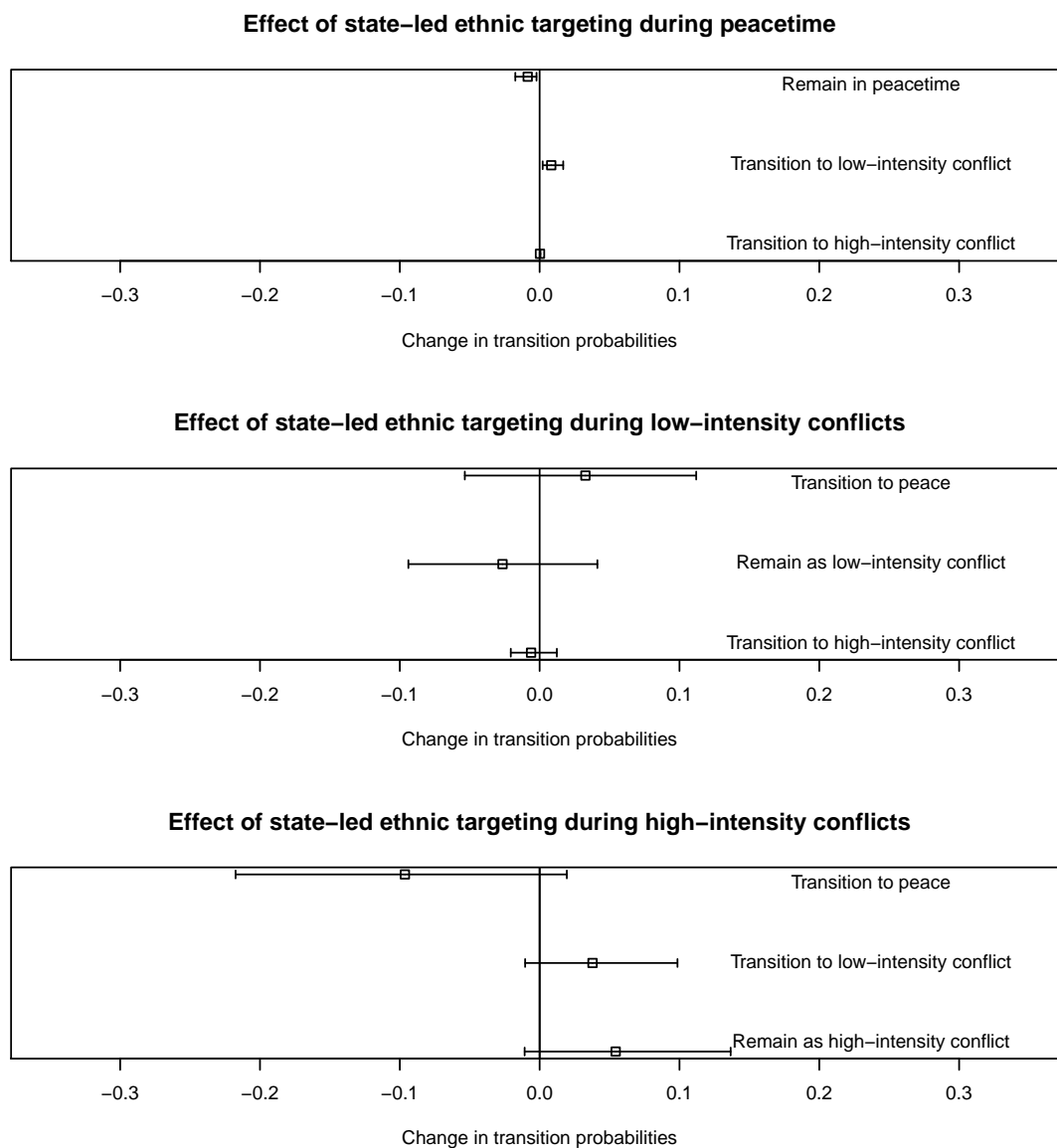
Table 2: Ethnic targeting and conflict evolution, Bayesian ordered probit

	Peace <sub>t-1</sub>	$\Delta$ low <sub>t-1</sub>	$\Delta$ high <sub>t-1</sub>
Ethnic targeting <sub>t-1</sub>	0.26** (0.10)	-0.38** (0.13)	0.08 (0.14)
Low intensity conflict <sub>t-1</sub>	0.31** (0.12)		
High intensity conflict <sub>t-1</sub>	1.20*** (0.01)		
Status excluded	0.20* (0.09)	0.15 (0.16)	0.13 (0.25)
Downgraded	0.87*** (0.13)	-0.34 (0.25)	-0.87*** (0.08)
Previous conflicts	0.18*** (0.04)	-0.30*** (0.06)	0.25 (0.24)
Log. Population, lag	0.05* (0.02)	0.06* (0.03)	0.01 (0.07)
Log. GDPpc, lag	-0.16*** (0.03)	0.15** (0.05)	0.16 (0.10)
Group size	0.49** (0.18)	0.52** (0.17)	-0.48*** (0.06)
Ongoing conflict, lag	0.31*** (0.08)	0.62*** (0.14)	1.20*** (0.01)
AIC		2944.21	
BIC		3710.14	
Log Likelihood		-1371.11	
Deviance		2742.21	
Num. obs.		14522	

**Note:** <sup>+</sup> $p < 0.1$ ; \* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$   
 Model controls for peaceyears (dummies) and  
 waryears (3 polynomials).

probabilities from respective scenarios to assess the effect of OSV on nine transition probabilities depicted in figure 1.

Figure 1: Average predicted differences in probabilities due to governmental ethnic targeting and 95% confidence intervals



The figure nicely illustrates our substantive findings. First, in line with our first hypothesis, we find that when governments engage in targeted OSV against an ethnic group, the likelihood that peace will last decreases (see the top horizontal bar in the top panel). However, the effect is arguably small. We also find marginal support for

hypothesis 3. The third panel of figure 1 shows that the average predicted probability for a high-intensity conflict to remain in that state or in low-intensity form increases as a consequence of ethnic targeting, although the corresponding confidence intervals fails, just barely, to exclude the value of zero. Correspondingly, if a government engages in ethnic targeting, the probability that a high-intensity conflict transitions to peace decreases.

Turning to the conditioning effect of prior war, we estimate the same models including an interaction between the ethnic targeting variable and the prior-conflict binary variable defined in the previous section. With regards to the effect of ethnic targeting during peacetime, we find results coherent with hypothesis 2. The effect is larger and statistically significant when there is no prior history of conflict, but decreases in subsequent conflict cycles. During low-intensity conflicts, the effect of state targeting is relatively close to zero, regardless of whether it is the first conflict or not. During high-intensity conflicts, however, the effect of state targeting is again positive, particularly during the first conflict, in line with hypothesis 2.

Adding an interaction term makes the interpretation of these models even more difficult, so we use the same simulation method to show the average predicted probabilities. Figure 2 shows the same nine transition scenarios for groups that are during the first conflict cycle, while figure 3 does so for groups that are in second or subsequent conflict cycles. In line with hypothesis 2, we can see that the effect of ethnic targeting before a war breaks out is positive and significant for first-conflict groups, but it disappears once the first conflict has broken out. Similarly, supporting hypothesis 4, we find that the the conflict-perpetuating effect of ethnic targeting is larger and reaches statistical significance for groups that have not yet experienced repeated conflicts, but fails to do so for the remaining observations. Conversely, the probability that a high-intensity conflict transitions to peace in these cases is smaller if there was recent ethnic targeting.

Table 3: Ethnic targeting and prior civil wars, Bayesian ordered probit

	Peace <sub>t-1</sub>	$\Delta$ low <sub>t-1</sub>	$\Delta$ high <sub>t-1</sub>
Ethnic targeting <sub>t-1</sub>	0.86*** (0.13)	-1.05*** (0.16)	-0.40* (0.19)
Prior conflict	0.47*** (0.09)	-0.73*** (0.14)	0.10 (0.23)
Targeting * Prior conflict	-0.93*** (0.17)	1.04*** (0.14)	0.31*** (0.07)
Low intensity conflict <sub>t-1</sub>	0.31** (0.12)		
High intensity conflict <sub>t-1</sub>	1.29*** (0.02)		
Status excluded	0.21* (0.09)	0.11 (0.16)	0.15 (0.25)
Downgraded	0.86*** (0.13)	-0.32 (0.25)	-0.83*** (0.08)
Log. Population, lag	0.04* (0.02)	0.08** (0.03)	0.04 (0.07)
Log. GDPpc, lag	-0.14*** (0.03)	0.13** (0.05)	0.15 (0.10)
Group size	0.45* (0.18)	0.64*** (0.18)	-0.37*** (0.06)
Ongoing conflict, lag	0.33*** (0.08)	0.59*** (0.14)	1.29*** (0.02)
AIC		2951.62	
BIC		3740.29	
Log Likelihood		-1371.81	
Deviance		2743.62	
Num. obs.		14522	

**Note:** + $p < 0.1$ ; \* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$   
 Model controls for peaceyears (dummies) and  
 waryears (3 polynomials).



Figure 2: Average predicted differences in probabilities due to governmental ethnic targeting and 95% confidence intervals, during the first conflict cycle

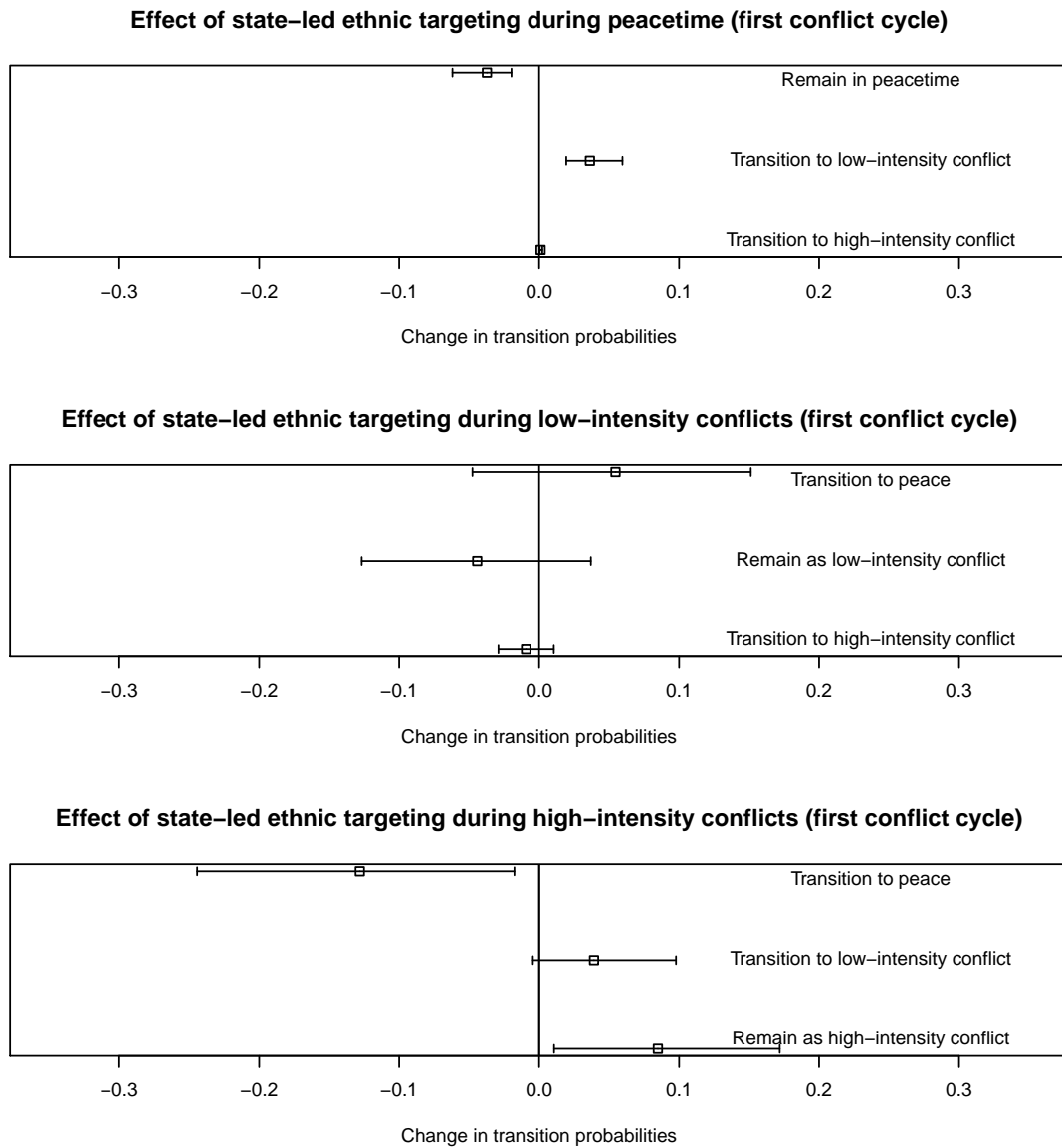
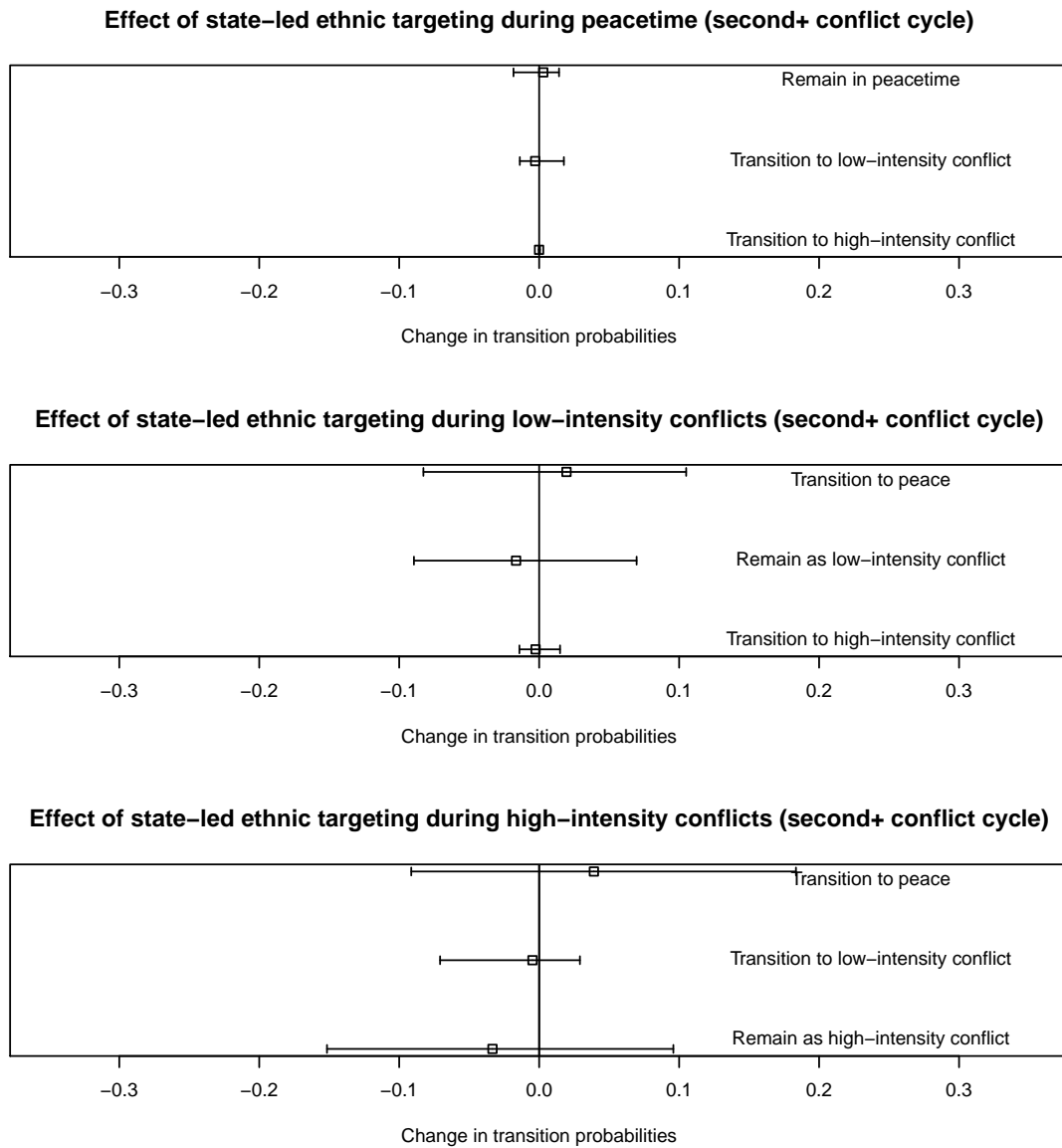


Figure 3: Average predicted differences in probabilities due to governmental ethnic targeting and 95% confidence intervals, during the second or subsequent conflict cycles



## 6 Conclusion and Outlook

What effect does state violence against civilians have on the onset and escalation of ethnic civil war? Despite a large body of literature dedicated to the causes and effects of civilian victimization, this question has so far remained almost completely unaddressed. In this paper we have argued that state-led targeting of particular ethnic groups increases the risk of armed conflict onset, and that it will contribute to conflict escalation once armed hostilities wear on. We also argued that these effects should be particularly strong for groups that are fighting their first conflict against the government.

We have tested our hypotheses based on a novel dataset that captures the ethnic identity of victims of one-sided state violence, as well as targeting patterns, around the globe. Our preliminary findings suggest that state violence against the members of particular ethnic groups indeed increases the risk of ethnic civil war between the perpetrating governments and the targeted groups, particularly when armed conflict has not yet taken place between that group and the government. In cases of repeated civil wars, the relationship between ethnic targeting and conflict outbreak becomes much weaker.

When it comes to the effects of deliberate ethnic targeting on conflict escalation, we find that ethnic targeting during high-intensity conflicts is related to a higher probability that the conflict remains in such a state, thus making any transition to peace less likely. Again, this effect is particularly strong during first-time conflicts, and is reduced in the case of repeated conflicts. During low-intensity conflicts, we do not find clear effects of state targeting, regardless of prior war history.

There are several ways in which we aim to go forward and improve our study. To start with, we aim to improve our analysis methodologically by more convincingly addressing inferential threats. Currently, our analysis does not allow us to conclusively identify causal effects, as several endogeneity concerns still remain unaddressed. Indeed, while the comprehensiveness of our data coverage allows us to study patterns of violence and conflict escalation on a global scale, this broad empirical focus also makes the identification of

causal relationship particularly challenging. We aim to incorporate more direct strategies to deal with these issues in future versions.

So far, our aggregate results also do not reveal much about the validity of our theorized causal mechanisms or the potential heterogeneity of the effects. We aim to explore these issues to a greater extent in future versions as well. For example, previous research has shown that the state-orchestrated collective targeting of particular groups can lead to the very counterintuitive effect of civilian mobilization *against* insurgent groups (Schubiger 2013a,b), an effect not necessarily at odds with those that we theorize above. Importantly, such micro-level dynamics might be limited to very particular conditions, and interact with the mechanisms outlined in this paper in complex ways, which makes it difficult to predict aggregate effects. Similarly, state violence is likely to be effective in preventing conflict and in suppressing and defeating insurgencies under particular circumstances as well (Merom 2003; Downes 2007). Indeed, cases such as Sri Lanka suggest that extreme levels of state violence against civilians can yield conflict-dampening, or even conflict-terminating, effects. Moreover, our analysis so far excludes insurgent violence, which likely interacts with state violence in consequential ways. We aim to explore the heterogeneous effects of state violence and to incorporate the role of insurgent violence in future versions of the paper to explore such possibilities in greater depth.

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## Appendix

Table 6 shows the descriptives statistics for the main variables used in the analyses above. Table 6 shows how many observations and ethnic groups are included in the sample, grouped by country.

Table 6 replicates the analyses of table 1, but relying on a coding of prior war including those fought as the incumbent. Table 7 comprises both of these prior war variables, but relies tracking ethnic targeting during the previous two years, whereas table 8 does so during a period of 5 years. As mentioned in the main text, the results hold when looking a larger temporal windows. While the effect of state targeting is positive but not significant in the whole sample, the interaction models show how it increases and reaches statistical significance for those groups prior civil war history. In this case, we find again a weakening effect of prior conflict.

Tables 9 and 10 repeat the transition model analyses measuring ethnic targeting over two years. The results are similar to the one-year specification. The positive effect of ethnic targeting during peacetime has the same size, although it is not significant in this case. Once conflicts starts, this positive effect disappears, and increases again for high-intensity conflicts.

Table 4: Descriptive statistics of main variables

	Min	Max	Mean	SD	Median	Missing obs
Conflict onset	0.00	1.00	0.01	0.10	0.00	1170
Targeting $t-1$	0.00	1.00	0.01	0.12	0.00	628
Targeting $t-1, t-2$	0.00	1.00	0.02	0.15	0.00	1265
Targeting $t-1, \dots, t-5$	0.00	1.00	0.04	0.19	0.00	3120
Status excluded	0.00	1.00	0.63	0.48	1.00	85
Downgraded	0.00	1.00	0.02	0.14	0.00	85
Previous conflicts	0.00	6.00	0.29	0.71	0.00	0
Log. Population, lag	6.14	14.10	10.06	1.90	9.94	103
Log. GDPpc, lag	4.89	11.08	8.26	1.19	8.22	103
Group size	0.00	0.95	0.12	0.18	0.04	0
Ongoing conflict, lag	0.00	1.00	0.30	0.46	0.00	628
Prior war	0.00	1.00	0.19	0.39	0.00	0

Table 5: Observations and ethnic groups by country

Country	<i>n</i>	Groups	Country	<i>n</i>	Gr.	Country	<i>n</i>	Gr.
Afghanistan	134	8	Georgia	92	4	Niger	118	5
Albania	48	2	Ghana	125	5	Nigeria	140	6
Algeria	25	1	Greece	75	3	Pakistan	200	8
Angola	108	5	Guatemala	61	3	Panama	100	4
Argentina	25	1	Guinea	55	3	Paraguay	25	1
Armenia	46	2	Guinea-Bissau	69	3	Peru	75	3
Australia	25	1	Guyana	50	3	Philippines	75	3
Austria	25	1	Honduras	50	2	Poland	101	5
Azerbaijan	46	2	Hungary	25	1	Romania	91	4
Bahrain	25	1	India	500	20	Russia	1007	58
Bangladesh	75	3	Indonesia	202	12	Rwanda	25	2
Belarus	69	3	Iran	275	11	Saudi Arabia	100	4
Belgium	75	3	Iraq	60	3	Senegal	125	5
Benin	99	4	Israel	118	5	Serbia	42	6
Bhutan	50	2	Italy	125	5	Serbia and Mont.	121	9
Bolivia	88	4	Japan	58	3	Sierra Leone	97	6
Bosnia-Herz.	88	4	Jordan	50	2	Singapore	100	4
Botswana	250	10	Kazakhstan	123	8	Slovakia	32	2
Brazil	58	3	Kenya	191	8	Slovenia	161	7
Bulgaria	74	5	Kosovo	30	5	South Africa	300	15
Burundi	42	2	Kuwait	75	3	South Sudan	30	10
Cambodia	125	5	Kyrgyzstan	83	4	Spain	125	5
Cameroon	150	6	Laos	150	6	Sri Lanka	92	4
Canada	75	3	Latvia	69	3	Sudan	320	16
CAR	97	6	Lebanon	275	11	Switzerland	75	3
Chad	125	5	Liberia	137	7	Syria	100	4
Chile	50	2	Libya	75	3	Taiwan	68	3
China	898	36	Lithuania	46	2	Tajikistan	75	4
Colombia	50	2	Macedonia	115	5	Tanzania	80	5
Congo	147	6	Madagascar	26	2	Thailand	65	3
Congo, DRC	291	12	Malawi	69	3	The Gambia	25	5
Costa Rica	50	2	Malaysia	125	5	Togo	34	2
Cote d'Ivoire	120	5	Mali	71	3	Trin. and Tob.	46	2
Croatia	100	5	Mauritania	75	3	Turkey	50	2
Cyprus	25	1	Mauritius	155	8	Turkmenistan	69	3
Czechoslovakia	16	4	Mexico	66	3	Uganda	149	7
Djibouti	44	2	Moldova	78	4	Ukraine	115	5
Ecuador	75	3	Mongolia	25	1	United Kingdom	175	7
Egypt	37	2	Montenegro	48	6	United States	130	6
El Salvador	25	1	Morocco	50	2	Uruguay	25	1
Eritrea	82	6	Mozambique	75	3	Uzbekistan	69	3
Estonia	69	3	Myanmar	275	11	Venezuela	39	2
Ethiopia	196	11	Namibia	288	12	Vietnam	225	9
Fiji	33	2	Nepal	146	6	Yemen	94	5
Finland	50	2	New Zealand	92	4	Zambia	175	7
France	75	3	Nicaragua	75	3	Zimbabwe	72	6
Gabon	105	6						

Table 6: Ethnic targeting (previous year) and conflict onset with prior conflict both as rebels and incumbent, probit model

	(1)
Ethnic targeting $t_{-1}$	0.826* (0.360)
Prior conflict (rebel/incumbent)	0.450*** (0.111)
Target * Prior conflict (rebel/incumbent)	-0.637 (0.414)
Status excluded	0.348*** (0.096)
Downgraded	0.726*** (0.130)
Log. Population, lag	0.054 <sup>+</sup> (0.028)
Log. GDPpc, lag	-0.129*** (0.033)
Group size	0.390 <sup>+</sup> (0.231)
Ongoing conflict, lag	0.318** (0.105)
(Intercept)	-1.516*** (0.322)
Observations	13,508
Akaike Inf. Crit.	1,118.339

**Note:** <sup>+</sup> $p < 0.1$ ; \* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$   
Peace-year correction (linear terms, plus 2nd and 3rd order polynomials) omitted, clustered SE (country).

Table 7: Ethnic targeting (last 2 years) and conflict onset, probit model

	(1)	(2)	(3)
Ethnic targeting $t-1, t-2$	0.317 (0.203)	0.882*** (0.267)	0.974** (0.315)
Prior conflict (rebel)		0.474*** (0.123)	
Prior conflict (rebel/incumb)			0.521*** (0.120)
Target * Prior conflict (r)		-0.711+ (0.380)	
Target * Prior conflict (r/i)			-0.773+ (0.407)
Status excluded	0.301** (0.099)	0.311** (0.098)	0.385*** (0.098)
Downgraded	0.796*** (0.127)	0.791*** (0.134)	0.742*** (0.141)
Previous conflicts	0.181** (0.060)		
Log. Population, lag	0.046+ (0.028)	0.038 (0.026)	0.039 (0.026)
Log. GDPpc, lag	-0.180*** (0.036)	-0.150*** (0.038)	-0.140*** (0.037)
Group size	0.648** (0.233)	0.613** (0.213)	0.474* (0.238)
Ongoing conflict, lag	0.356*** (0.101)	0.382*** (0.095)	0.348*** (0.098)
(Intercept)	-0.921* (0.358)	-1.177** (0.363)	-1.328*** (0.361)
Observations	13,026	13,026	13,026
Akaike Inf. Crit.	1,000.721	998.053	990.795

**Note:** + $p < 0.1$ ; \* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$

Peace-year correction (linear terms, plus 2nd and 3rd order polynomials) omitted, clustered SE (country).

Table 8: Ethnic targeting (last 5 years) and conflict onset, probit model

	(1)	(2)	(3)
Ethnic targeting $t-1, \dots, t-5$	0.204 (0.143)	0.781*** (0.230)	0.896*** (0.268)
Prior conflict (rebel)		0.390** (0.150)	
Prior conflict (rebel/incumb)			0.407** (0.135)
Target * Prior conflict (r)		-0.800+ (0.426)	
Target * Prior conflict (r/i)			-0.885* (0.433)
Status excluded	0.338** (0.110)	0.337** (0.107)	0.378*** (0.113)
Downgraded	0.891*** (0.136)	0.877*** (0.144)	0.860*** (0.146)
Previous conflicts	0.129* (0.062)		
Log. Population, lag	0.046+ (0.027)	0.038 (0.026)	0.040 (0.025)
Log. GDPpc, lag	-0.174*** (0.032)	-0.148*** (0.034)	-0.138*** (0.033)
Group size	0.574* (0.284)	0.509+ (0.269)	0.416 (0.288)
Ongoing conflict, lag	0.299** (0.104)	0.320*** (0.096)	0.299** (0.095)
(Intercept)	-0.689 (0.420)	-0.891* (0.427)	-1.024* (0.404)
Observations	11,348	11,348	11,348
Akaike Inf. Crit.	808.544	804.186	801.513

**Note:** + $p < 0.1$ ; \* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$

Peace-year correction (linear terms, plus 2nd and 3rd order polynomials) omitted, clustered SE (country).



Table 9: Ethnic targeting (last 2 years) and prior civil wars (Bayesian ordered probit)

	Peace <sub>t-1</sub>	$\Delta$ low <sub>t-1</sub>	$\Delta$ high <sub>t-1</sub>
Ethnic targeting <sub>t-1,t-2</sub>	0.28 <sup>+</sup> (0.16)	-0.41 <sup>+</sup> (0.21)	-0.02 (0.24)
Low intensity conflict <sub>t-1</sub>	0.09 (0.12)		
High intensity conflict <sub>t-1</sub>	0.97*** (0.02)		
Status excluded	0.24** (0.09)	0.17 (0.17)	0.02 (0.25)
Downgraded	0.82*** (0.13)	-0.18 (0.25)	-0.83*** (0.10)
Previous conflicts	0.18*** (0.04)	-0.28*** (0.06)	0.26 (0.24)
Log. Population, lag	0.04* (0.02)	0.08** (0.03)	0.03 (0.07)
Log. GDPpc, lag	-0.18*** (0.03)	0.15** (0.05)	0.22* (0.10)
Group size	0.61** (0.19)	0.47* (0.20)	-0.65*** (0.06)
Ongoing conflict, lag	0.29** (0.09)	0.58*** (0.15)	0.97*** (0.02)
AIC		2739.87	
BIC		3501.32	
Log Likelihood		-1268.93	
Deviance		2537.87	
Num. obs.		13893	

**Note:** <sup>+</sup> $p < 0.1$ ; \* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$   
Model controls for peaceyears (dummies) and  
waryears (3 polynomials).

Table 10: Ethnic targeting (last 2 years) and prior civil wars (Bayesian ordered probit)

	Peace <sub>t-1</sub>	$\Delta$ low <sub>t-1</sub>	$\Delta$ high <sub>t-1</sub>
Ethnic targeting <sub>t-1,t-2</sub>	0.81*** (0.14)	-0.97*** (0.16)	-0.43 <sup>+</sup> (0.22)
Prior conflict	0.50*** (0.09)	-0.70*** (0.14)	0.14 (0.22)
Targeting * Prior conflict	-0.84*** (0.16)	0.86*** (0.16)	0.23* (0.10)
Status excluded	0.25** (0.09)	0.13 (0.17)	0.03 (0.25)
Downgraded	0.80*** (0.13)	-0.15 (0.25)	-0.78*** (0.10)
Log. Population, lag	0.03 (0.02)	0.10*** (0.03)	0.06 (0.07)
Log. GDPpc, lag	-0.16*** (0.03)	0.13** (0.05)	0.21* (0.10)
Group size	0.57** (0.19)	0.59** (0.20)	-0.60*** (0.06)
Ongoing conflict, lag	0.32*** (0.09)	0.53*** (0.15)	1.08*** (0.02)
Low intensity conflict <sub>t-1</sub>	0.13 (0.13)		
High intensity conflict <sub>t-1</sub>	1.08*** (0.02)		
AIC		2744.99	
BIC		3529.06	
Log Likelihood		-1268.49	
Deviance		2536.99	
Num. obs.		13893	

**Note:** <sup>+</sup> $p < 0.1$ ; \* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$   
Model controls for peaceyears (dummies) and  
waryears (3 polynomials).