Failure to Protect:
Civilian Victimization and State Legitimacy in Civil War

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Abstract

How does civilian victimization during civil war affect interactions with the state following conflict? We highlight on an oft-overlooked fact: participating in transitional justice and reconciliation policies can require contact with state institutions that failed to protect civilians or perpetrated violence themselves. Recent research suggests that conflict-affected individuals can become more pro-social and increase political engagement. These findings, however, originate from contexts where violence was not perpetrated by state institutions. We argue that though victims may be more pro-social than non-victims, state-led victimization decreases trust in the state, causing individuals to withdraw from behaviors that presuppose trust in government, such as registering as a victim. In this paper, we leverage variation in civilian victimization by perpetrator over time and across space. We find that indiscriminate violence carried out by the state translates into lower levels of victim registration in the registry held by Colombia's state-run victim's unit. We examine what this implies for state legitimacy and peace consolidation after civil war. Our results also shed light on dynamics influencing victims' engagement with transitional justice after violence in contexts where the state carried out violence.

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What are the political legacies of violence? How do victims participate in politics after violence has ceased? Does the identity of the perpetrator affect their patterns of engagement? Questions concerning the repercussions of political violence have widespread implications. Almost half the world experienced internal or external armed struggle in the past half century. State repression likewise can leave scores of victims in its wake. Understanding how these incidents affect political life is therefore paramount.

Existing literature remains divided concerning the legacies of conflict. While a host of studies documents an activation effect, linking exposure to violence and victimization to heightened political participation, other research posits a withdrawal effect, where individuals who experience conflict remove themselves from the political realm. The conflicting expectations and empirical findings highlight the need for additional research that takes into account varying measures of victimization, the perpetrators of violence, and different dimensions of political activity. Doing so allows us to create more nuanced hypotheses that reflect the complexities of wartime and its aftermath.

In this endeavor, we advance existing literature both theoretically and empirically. Theoretically, we argue that considering the perpetrator of violence is critical for understanding its repercussions. Though it may be the case that victims are more prosocial after violence, when the state holds responsibility for victimization, individuals will be more inclined to withdraw from reconciled political activities, such as transitional justice policies. This is because doing so requires them to trust or confer legitimacy in the government. When violence is perpetrated by non-state groups, we expect that our findings will concur with existing accounts documenting an activation effect, measured by higher levels of political activity among those affected by violence. Though we test our expectations in the Colombian case, our findings might describe other situations of civil war and state repression where state institutions carried out violence.

Empirically, we leverage micro-level panel data to test our claims. This approach allows us to capture intertemporal dynamics within and across units and helps to mitigate selection issues. Moreover, as our main dependent variable, we consider whether or not victims register with the Victim's Unit (*Unidad para la atención y reparación integral a las víctimas*). Registering as a victim is a bureaucratic procedure that makes individuals eligible for material reparations, a transitional justice policy first implemented by the government in 2011. We suggest that this variable can be considered an indicator of political participation, as it requires engagement with state entities. Moreover, we argue that it is suggestive of a certain type of political engagement - reconciled political engagement - that necessitates

trust in the state. This dependent variable - registering as a victim - is consequential for a number of broader phenomena of interest to political scientists and policy-makers alike, including reconciliation, democratic consolidation, and peace.

We report three key findings. First, we find that political participation in the form of victim registration increases after conflict, but that increases are concentrated among areas with high levels of guerrilla and paramilitary victims. Second, among those victimized by the state, we find no significant increase in the propensity to register with the Victim's Unit following political violence, suggesting that the identity of the perpetrator should be considered when analyzing the legacy of conflict. Third, we explore the mechanisms that might be behind our main results. To do so, we use survey data to test the notion that violence perpetrated by the state lowers trust in the government and its institutions, in turn reducing the desire to engage in forms of reconciled political engagement. We find support for this proposed pathway.

This study advances existing literature on the political legacies of conflict in several key ways. We consider heterogeneity along the lines of the entity carrying out offenses during the period of political violence. Doing so allows us to uncover patterns in participation that reflect former transgressions carried out by the state. We suggest that the government's implication in atrocities has reverberating impacts that can influence victims' political engagement once violence has ceased. Though we expect our findings to apply to contexts where the government was implicated in violence, the Colombian case provides a useful entrypoint to consider these dynamics. Specifically, it allows us to consider how violence carried out by different entities shapes subsequent behaviors, since the decades-long civil war was characterized by the involvement of numerous armed groups, including those affiliated with the state, guerrilla groups, and paramilitaries. Second, we draw on an under-studied yet important indicator of political participation: registering as a conflict victim. We argue that this dependent variable carries inherent importance in normatively desirable processes such as attending to victims and aiding peace-building and reconciliation. Additionally, registering as a victim requires some level of confidence in state entities, providing a useful way to consider how violence exposure maps onto trust in the political system. Finally, we analyze micro-level patterns across time and space to account for evolving dynamics and complexities that typify societies engulfed in conflict.

We also contribute to study of transitional justice. Transitional justice policies have greatly increased in recent years (Sikkink, 2011; Olsen et al., 2010). Governments commonly implement a host of policies - including material reparations, truth commissions,

and trials - to address the crimes committed during the past period of political violence. A burgeoning body of research analyzes citizens' and victims' preferences toward transitional justice policies (Samii, 2013; Hall et al., 2018; Nussio et al., 2015; Aguilar et al., 2011). Increasingly, researchers are also evaluating how these policies shape individuals' political behavior and attitudes (see Cilliers et al. (2016) for example). We argue that in addition to considering these important dimensions of transitional justice, we should also emphasize the process of participating in transitional justice policies from a victims' perspective. Analyses of the uptake of transitional justice policies is a critical antecedent to understanding its practical use and consequences.

Theoretical framework

How do citizens transition to political life after periods of violence? The theoretical expectations and findings on legacies of political violence are varied. By and large, existing scholarship can be divided into two main theoretical camps that hypothesize contrasting consequences of violence exposure and victimization; we refer to them as the activation and withdrawal effects.

The withdrawal effect suggests that after exposure to violence, individuals retreat from political life. This is seen as an element of a broader response to violence and the trauma associated with conflict, wherein individuals display symptoms consistent with depression and post-traumatic stress. In response to the violence witnessed, as well as the physical and social destruction that often accompanies periods of political violence, individuals in conflict and post-conflict settings commonly experience undesired rehashings of the traumatic event, lack of interest in relationships, trouble maintaining interpersonal relationships, and a lack of positive emotions and optimism about the future (Ehlers and Clark, 2000; Galovski and Lyons, 2004; Miller and Rasmussen, 2010). The durability of these symptoms varies, but some have shown significant persistence (Sutker and Allain Jr, 1996; Miller and Rasmussen, 2010; Kuwert et al., 2007). Thus, in addition to withdrawing from political spheres, war-exposed communities experience broader negative mental health consequences. In line with the direct and indirect psychological costs of conflict exposure, then, we would expect to observe lower levels of political engagement among those affected by conflict. We will argue that this response is particularly likely when the state and government entities are behind the violence.

The activation school of thought suggests that individuals exposed to war violence ex-

perience increases in prosociality and political activity. Several hypothesized mechanisms might drive this relationship. First, rather than the withdrawal effect just detailed, psychologists have also documented post-traumatic growth after experiences of deep trauma. In these scenarios, individuals exposed to severe trauma –be it in the form of political violence, rape, or other distressing episodes– respond by placing renewed value on their relationships and life. Doing so results in an increased emphasis on interpersonal and community relationships and might compel individuals to seek out opportunities to participate politically (Tedeschi et al., 1998; Tedeschi and Calhoun, 2004). Thus, the activation effect would suggest that individuals in post-conflict settings should engage in higher levels of political participation:

- *Hypothesis 1:* Victimization increases engagement and political participation.

Indeed, a host of studies examine the impact of exposure to violence find this pattern in contexts including Uganda (Blattman, 2009), Sierra Leone (Bellows and Miguel, 2009), and Latin America (Bateson, 2012). Rarely do these studies theoretically differentiate between the sources of this violence, a task we take on in this research. Bauer et al. (2016) overview the recent literature on the effects of violence on cooperation. They find that while there is broad support for the notion that exposure to violence leads to changes in pro-sociality (as measured in experimental games), the results of their meta-analysis find that the effects on observed political participation (voting and knowledge/interest in politics) are much smaller and insignificant in random-effects specifications. This finding suggests that either the true effect is near zero *or*, as we argue here, results are heterogeneous and highly dependent on context. While Bauer et al. (2016) distinguish between criminal and wartime violence,[1] scholars should also consider who is responsible for violence and the forms of political participation which that violence might induce.

Though the activation and withdrawal schools of thought provide valuable starting points for considering the ramifications of exposure to and victimization by political violence, we suggest that additional attention should be paid to both the type of political engagement displayed after conflict as well as the identity of the actor responsible for said violence. Additional nuance in these dimensions allows us to more accurately reflect the complexity of post-conflict dynamics. In doing so, we argue for an approach that pays attention to the position of the perpetrating party, because we expect political engagement to increase among those victimized by non-state entities but for withdrawal effects to accurately cap-

[1]They find that the effects for criminal violence are somewhat larger but not statistically significant.

ture political behavioral responses to violence among state victims. This withdrawal effect manifests when increases in prosociality documented by scholars adhering to an activation logic is subsumed by decreased trust in state entities, which reduces willingness to engage in reconciled political activity.

Therefore, we draw a distinction between types of political participation. Reconciled political participation, which takes the form of political engagement via government institutions and policies, presupposes some level of trust, efficacy, and belief in the legitimacy of these state entities. In this article, we focus on one manifestation of reconciled political engagement: registering as a victim with the state, an action that entails subsequent material compensation. Reconciled political participation is not costless, as it requires that individuals engage with state authorities and in many cases, make themselves known as political citizens. Thus, individuals may calculate that reconciled political participation is only worthwhile if they can count on the state to act in a neutral fashion. Doing so requires that they place trust in the state, believing it will carry out its end of the proposed interaction. Defiant political participation, rather than taking the form of engaging in status quo politics, consists of activity meant to fundamentally alter the status quo. Frequently, this behavior is extrainstitutional, in that it takes place beyond the boundaries of state-established policies and institutions. Examples include protest behavior and writing and signing petitions. In post-conflict settings, it can take the form of activities meant to establish punishment mechanisms or to exact retribution particularly if the state itself has not established such means. Examples include *funas* in Chile wherein individuals publicly shame those accused of committing human rights violations[2] or *escraches* in Argentina and Uruguay, a similar type of protest behavior wherein affected citizens take justice into their own hands.

Reconciled political participation becomes increasingly costly when the state was engaged in violence in the past. Here we concur with the basic premise of those adhering to the withdrawal school of thought: exposure to violence and victimization can result in detriments to an individuals' well-being. Though we do not deny that victimization might lead to greater prosociality, we argue that the identity of the perpetrator has countervailing effects. If the state was responsible for victimization, reconciled political participation carries a hefty psychological cost, as it requires victims to trust a political system and authority that not only failed to protect them during wartime but one that perhaps directly carried out violence. Reasonably, victims might respond with distrust in political systems and may view them as fundamentally illegitimate. Existing literature on violence and trust bears

[2] NY Times (2017).

this out.

For instance, Gates and Justesen (2016) compare levels of trust in democratic institutions among survey respondents in Mali immediately before and after the incidence of unanticipated attacks by Taureg militias. They find that individuals surveyed after the attacks had lower approval of political actors deemed responsible –namely the president and parliament– though retained trust in democratic institutions writ large. Similarly, Ishiyama et al. (2018) find that while levels of violence carried out by Drug Trafficking Organizations (DTOs) within Mexican municipalities do not appear to have meaningful effects on trust in institutions, individual beliefs about personal security are positively associated with trust in state institutions. That is, the failure of state institutions to provide perceived security can reduce trust in those institutions. Closer in spirit to our civil war case, De Juan and Pierskalla (2016) find that exposure to violence carried out during the Nepalese civil war decreases trust in the national government. These studies suggest two things. First, victimization has the potential to decrease trust in institutions and second, the actors involved are important for how this effect is realized. Moreover, these findings lead us to our first proposition which we will later examine using individual-level survey data in Colombia:

- *Proposition 1:* Victimization will decrease trust in the state and this effect will be strongest among those who are victimized by the state.

As De Juan and Pierskalla (2016) point out, it is especially in post-conflict contexts where trust in state institutions can be of particular importance. Belief in the ability and willingness of the government to stand by the bargains made during peace negotiation can influence overall support for the peace process and thereby recurrence of conflict (Sacks and Larizza, 2012; Hutchison and Johnson, 2011). Moreover, inducing widespread compliance with and participation in transitional justice policies and additional regulations similarly require trust in the state as demonstrated by the literature on trust and participation generally (Levi and Stoker, 2000) and on compliance with regulation (e.g. taxation) (Fjeldstad, 2004). Therefore, we expect that trust will be an important determinant in participation:

- *Proposition 2:* Trust in the State will be positively associated with reconciled forms of political participation.

Combining propositions 1 and 2, we should expect victims of state-perpetrated violence to withdraw from forms of reconciled political engagement. When, however, violence was

perpetrated by non-state entities, such as paramilitary or guerrilla groups, we do not expect that victims will react to state institutions and policies in the same way. Though trust might be damaged because state entities failed to protect innocent civilians, views of the government will be less negative when the state did not directly carry out violence or when violence was predominantly carried out by non-state groups. In this setting, individuals may turn to political systems as a way to obtain personal benefits, such as material ones that might accrue as a result of reparations policies or social welfare systems. Benefits may take a more symbolic or psychological form, as individuals might feel pleasure from expressing themselves (Brennan and Lomasky, 1997; Schuessler, 2000; Green et al., 2004; Huddy et al., 2015; Brennan and Buchanan, 1984). Thus, we expect to evince activation effects when we consider levels of reconciled political participation among those victimized by non-state actors. In sum, with respect to reconciled political participation, state-led victimization should be linked to withdrawal from reconciled political participation while non-state-led victimization should activate reconciled political engagement.

- *Hypothesis 2:* From Propositions 1 and 2, the effect of victimization by state entities on reconciled participation should be less than the effect of victimization by non-state actors.

[FIGURE 1 HERE]

Figure 1 lays out our general expectations concerning patterns of political behavior, distinguishing between the different entities responsible for victimization. Though the focus of our analysis is on reconciled political engagement, it is worth briefly touching on our expectations concerning defiant political participation in post-conflict settings. We expect defiant political participation to occur against the state when the state is responsible for violence, though if individuals experience post-traumatic stress disorder symptoms and a general increase in apathy and malaise, we would still expect these individuals to withdraw from political life. In the case of non-state perpetration of violence, we expect that this withdrawal will also occur alongside broader negative psychological ramifications; that said, we might expect that if the state fails to recognize victims or to sanction those responsible for crimes, defiant political engagement could be increasingly likely.

Conflict and Victimization in Colombia

We expect the logic just outlined to extend to a wide-range of post-violence settings, particularly those where the state carried out violence. In these cases, we believe that state-led victimization will decrease trust in the state, resulting in behavioral responses such as lower willingness to engage in reconciled political activity. Presently, Colombia provides an ideal environment to test the initial expectations generated from our theory. During the Colombian conflict, both state and non-state entities carried out violence, allowing us to compare the repercussions of each type while holding other variables constant. Doing so enhances our confidence in our results, as it permits us to rule out many omitted variables that might drive our findings. Additionally, Colombia's conflict was lengthy and devastating, and many fear the possibility of reversion. Understanding the impacts of policies meant to prevent such reversions is therefore critical. In this section, we further describe the Colombian case and justify our dependent variable: registering as a victim in the state-run Victim's Unit.

Prior to the 2016 peace deal, Colombia had been engaged in armed conflict for decades. The conflict has involved multiple groups: paramilitary forces, guerrilla groups, crime syndicates, and the Colombian government. That said, the dynamics of violence morphed throughout the conflict period and warrant discussion (Ch et al., 2018). In the later 1980s to mid-1990s, violence was largely perpetrated by the FARC. During this time, no victim registry existed (see below for a discussion of its history). By the time our data on victim registration begins in 2001, paramilitaries were increasingly powerful and united under the umbrella of United Self-Defense Forces of Colombia (AUC). As Figure 2 shows, paramilitaries, the state, and guerrilla groups were all engaged in violence during this period. The relative share of victimization by paramilitary groups soon decreased, as paramilitaries agreed to a ceasefire and gradually demobilized from 2003-2005. From late 2006 to 2010, the Colombian military and police increasingly established a presence throughout the country, while the FARC weakened. In 2010, the FARC and government entered into peace talks. Though violence continued and paramilitary groups persisted, overall levels of violence decreased relative to the early 2000s. As Figure 2 shows, throughout the period of study, violence was perpetrated by different actors, allowing us to analyze heterogeneity according to perpetrator. Note that all analyses control for year-specific effects in violence and victimization, allowing us to more precisely estimate registration rates.

[FIGURE 2 HERE]

In 1997, the Colombian government established the Single Registry of Displaced Populations (Rivas, 2016). To register, victims of forced displacement needed to present themselves to an office of the Public Ministry within a year of the alleged crimes. In 2011, Victim's Law mandated an expanded database, the Single Registry of Victims in Colombia, which includes victims of forced disappearance and sequestration, torture or inhumane treatment, sexual violence, forced recruitment, forced displacement, assassination, and other violence that produced injuries. In cases of assassination and forced displacement, family members (spouses/partners, children, and parents) of direct victims are also eligible to register.

To date, nearly 8.8 million individuals have registered as a victim with Colombia's Victim's Unit. To register, victims can call the victim hotline or go to the closest *Punto de Atención* (Center of Attention) located throughout the Colombian territory. Victims must present the required documents (identification and two witness declarations attesting to their victimhood). The state then issues a response in 120 days. This response is not monetary in nature, but rather takes the form of an administrative notice. Once the reparation process is initiated by the Victim's Unit, victims receive a personalized note from the government expressing the state's commitment to reparation. For many victims, this is the sole response they have so far received, as only 10.9% of victims had been paid as of January 2019 (Unidad para las victimas, 2019). For our purposes, we are concerned not with the response of the Victim's Unit, but with the decision of individuals to take the first step in the process: registering with the Victim's Unit.

We note that registering with the Victim's Unit is an example of reconciled political engagement. That said, unlike many other forms of political activity, registering as a victim is thought to yield an economic benefit in the form of a reparation payment. As such, we consider victim registration to be a "hard test" for our theory, since individuals might be particularly motivated to participate because of the promised economic gain even if they have lost trust in the government. As our analyses suggest, however, the distrust generated from state-led victimization appears to outweigh the economic benefit, which does not hold true for victimization by other entities.

Empirical Strategy

From our theory, we enumerate two main hypotheses. Again, we argue that individuals who are victimized by non-state actors will be more likely to engage in what we call reconciled political activity. In contrast, those who are victimized by state forces – the army, the police,

etc. – will lose trust in the government and be less likely to engage in particular forms of political participation relative to those victimized by non-state actors. We test these hypotheses across multiple data sources and find support in each. We begin by examining the association with victimization and our key measure of engagement (registration as a victim) at the municipality-year level from 2001 to 2017. We probe the causal relationship between observed levels of victimization by three categories of armed groups and registration using municipality and year fixed-effects. Finally, we validate our main findings (re-examining hypotheses 1 and 2) and explore the validity of propositions 1 and 2 using individual survey responses from two rounds of the Latin American Public Opinion Project (LAPOP) Americas Barometer (LAPOP, 2018).

Time-Series Cross-Sectional Analysis

As mentioned above, our two main data sources for the municipality-year analysis are 1) *El Centro de Investigación y Educación Popular's* (CINEP) violent events database[3] and 2) administrative data on victim registration from the government of Colombia. Based on regional and national press coverage, the CINEP data catalogs approximate location, number of victims, type of human rights violation, and, most importantly, the groups involved in the event. Our main independent variables are the total number of victims in a given municipality in a given year that are associated with each of our three categories of armed actors – guerrillas, paramilitaries, and the state. Our dependent variable is constructed by aggregating the total number of individuals who register as a victim in each municipality year. Because individuals who have been displaced may not reside in the location where victimization occurred, we test our hypotheses using both the total number of registration and excluding individuals whose main grievance is displacement.

[FIGURE 3 HERE]

At the municipality-year level, the total number of registered victims can often outnumber *observed* victims from the CINEP data. This is because the victims register also includes individuals indirectly affected by the conflict. Imagine a young man killed in conflict. This event would be viewed as having a single victim in the CINEP data, whereas the total number of registered victims could be much higher (his spouse, children, parents, etc. may all have reason to register). Nevertheless, we see in Figure 3 that the total number of observed

[3]In particular, we use CINEP's databse on Human Rights and Political Violence, accessible from *Noche y Niebla*'s website.

victims per 10,000pop is highly correlated with the total number of registered victims normalized in the same manner. This relationship also holds when victims of displacement are excluded. Our main goal is to examine whether this relationship varies depending on who carried out violence.

To do so, we estimate a simple fixed-effects specification, exploiting spatial and temporal variation in both our dependent and independent variables. Following Equation 1, we test whether victimization V by armed group $\in \{G, P, S\}$ or Guerrilla, Paramilitary, or State respectively, is associated with levels of victim registration in a municipality (m) year (t) controlling for time-invariant municipality-specific characteristics (δ_m), and year specific Fixed Effects (γ_t):

$$\text{Registration}_{mt} = \alpha + \beta_G V^G_{mt} + \beta_P V^P_{mt} + \beta_S V^S_{mt} + \delta_m + \gamma_t + \varepsilon_{mt} \tag{1}$$

Of course, victim registration and victim counts are not normally distributed. Accordingly, we present findings using a number of different data transformations and the results are similar throughout. In particular, we estimate the above equation using the raw counts, per-capitized levels, as well as per-capitized levels that are then either logged or transformed using the inverse hyperbolic sine transformation. The advantage of this transformation is that unlike the natural log, $\text{arcsinh}(x)$ is defined at 0, which allows us to account for municipalities which experienced no instances of victimization or no registrations in a particular year. Standard errors for each specification are calculated using heteroskedasticity consistent robust standard errors clustered at the municipality level.

Table 1: Two-Way Fixed Effects Estimates of Victimization on Registration

	Total Registration				Excluding Displacement			
	Raw (1)	Per Cap. (2)	log(y) (3)	arcsinh(y) (4)	Raw (5)	Per Cap. (6)	log(y) (7)	arcsinh(y) (8)
State Victims	−6.566	−3.772	0.045	0.017	2.714	−0.210	0.056	0.047
	(6.652)	(7.311)	(0.088)	(0.071)	(1.146)	(0.633)	(0.082)	(0.077)
Guerrilla Victims	29.355	34.339	0.727	0.573	6.570	3.580	0.738	0.660
	(8.117)	(9.844)	(0.099)	(0.080)	(1.141)	(1.111)	(0.079)	(0.070)
Paramilitary Victims	27.603	43.851	1.086	0.878	2.575	3.113	0.756	0.679
	(6.585)	(10.072)	(0.118)	(0.099)	(0.824)	(0.850)	(0.085)	(0.080)
$\chi^2 : (\beta_S = \beta_G)$	148.28	245.86	63.79	53.26	82.12	143.53	102.52	85.16
$Pr(> \chi^2)$	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Municipio FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	1,172	1,172	1,172	1,172	1,172	1,172	1,172	1,172
Observations	16,890	16,030	16,030	16,030	16,890	16,030	16,030	16,030
R-squared	0.071	0.163	0.051	0.040	0.191	0.076	0.054	0.044

Note: Heteroskedasticity Consistent Robust Standard Errors Clustered at the Municipality level in parentheses. Columns (1) through (4) present results where the dependent variable is the total number of registered victims per municipality-year. Columns (5)-(6) exclude victims registering as displaced. Columns (1) and (5) present estimates from raw count figures; in Columns (2) and (6) all key variables are per-capitized; in (3) and (7) variables are per-capitized and then logged; finally (4) and (8) present results using the inverse hyperbolic sine transformation of our key variables. The row denoted χ^2 $(\beta_S = \beta_G)$ presents the χ^2 of a linear hypothesis test of whether the coefficient on the state victims (β_S) is the same as the coefficient on the variable measuring victims of guerrilla forces (β_G). Estimation includes both municipality and year fixed effects.

*** Significant at the .1 percent level.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

To examine hypothesis 2, we test the null hypothesis of whether the coefficient on the number of government victims (β_S) is the same as the coefficient on the number of guerrilla victims (β_G). In particular, we expect that β_S will be substantively and significantly less than β_G.

Figure 4 and Table 1 present our main findings. Each panel of Figure 4 presents the coefficients from Column (8) of the table and data points adjusted for each other variable. From the table, we observe that across all specifications the numbers of victims attributable to guerrillas and those attributable to paramilitaries are positively associated with the total number of victims registered by the state. In contrast, the estimated coefficient for state victims is often insignificant at conventional alpha levels and can be positive or negative depending on the specification. We use Chi-squared tests to compare β_S and β_G. Across all specifications, the relationship between state victims and registrations is significantly less than the relationship between guerrilla victims and registrations at the .1% level. This finding provides strong initial support for hypothesis 2 and rejects our null.

These findings suggest that, within municipalities, greater victimization by non-state actors in a given year also increased victim registration rates. Again, these findings are consistent with the activation effect we discuss above. In contrast, once we control for victimization by these entities, victimization by the state appears to have no effect whatsoever on registration rates. If the activation effect held regardless of the position of the perpetrator, we should expect this effect to be positive and of similar magnitude to non-state actors. We propose that this null-result is consistent with an explanation which incorporates the mediating effect of state victimization on trust in the state. While individuals who have been victimized by the state may be more prosocial compared to non-victims, they also are more likely to withdraw from activities which require trust in the state. In particular, they are less likely (relative to victims of non-state actors) to participate in transitional justice programs.

[FIGURE 4 HERE]

In Appendix A, we present a number of additional robustness checks to this main result. In particular, we examine whether the removal of year-specific effects, different lag structures, and time varying municipality characteristics. Full details are in the appendix, but we provide a summary below.

We might be concerned, for instance, that there are characteristics of particular years

which determine either levels of registration or victimization such as a policy change which made it easier to register people. To account for this, Table A1 in the appendix replicates our main specification with the exclusion of year fixed effects. Results are substantively identical.

Another feature of the data which may challenge our findings is that registration does not depend on victimization in a given year. Thus, many individuals who are victims may wait a year or more to begin the process. To examine whether this might meaningfully impact our inference we re-run our main analysis with the inclusion of one and two year lags in Table A2. The interpretation of these results is less straightforward, but still in-line with our hypotheses. The coefficients for contemporaneous levels of state victimization are uniformly positive and significant in several specifications. Coefficients on the one and two period lags vary in effect direction and statistical significance. However, the estimated coefficient for each lag of state victims is significantly less than its analogous lag for guerrilla victims (which are consistently positive and significant). When we exclude displaced individuals, the net effect of state victims on registration over a three year period is near zero.

Finally, while year fixed-effects eliminate geographically invariant (national-level) variation and municipality fixed-effects eliminate time-invariant characteristics of a locality, there remains the possibility that municipality specific characteristics change over time and that these characteristics are driving both changes in victimization and in registration. To account for this concern, we estimate additional specifications (comparable to Table 1 with municipality and year fixed-effects) which include a set of time-varying controls which might influence either or both our dependent and independent variables: unemployment rate, number of homicides, levels of government spending, and GDP per-capita. Each of these variables are drawn from data compiled by *El Centro de Estudios Sobre Desarrollo Económico* (CEDE) at the *Universidad de los Andes*. However, the time period covered by these data is significantly shorter than the the time period covered by the rest of our data. For this reason, Table A3 is separated into two panels. Panel A replicates the estimation performed in Table 1 on the subset of data for which we have the controls. While point estimates change across samples, we continue to reject the null for hypotheses 1 and 2; victimization by guerrillas and paramilitaries increases registration and the effect for state victims (with the exception of one specification which per-capitized variables and includes displacement) is significantly and substantively less than for victims of other actors. Panel B estimates the same specifications but this time adds in our additional controls. Point estimates for our main variables are broadly similar to those in Panel A and we consistently

reject the null hypothesis that victimization does not change registration. Again, in nine out of ten specifications we similarly reject that the effect for state victims is equal to the effect of non-state victims.

Altogether these findings provide strong evidence in support of the idea that victimization shapes engagement and that the identity of the perpetrator in some way determines the size of that effect. However, it would be challenging to use the same administrative data to test the individual-level mechanisms which we believe drive this result. For this reason, we turn to LAPOP survey data to re-examine our primary hypotheses and to probe the veracity of our propositions.

Individual-Level Analysis

The survey data used here come from the 2014 and 2016 rounds of LAPOP's Americas Barometer in Colombia. For each round we have basic demographic characteristics of each respondent, municipality of residence, trust in state institutions, and several questions related to the conflict –in particular experiences of victimization. For the 2016 round, we also have information related to registration as a victim and whether or not the respondent has received reparations. We begin with an analysis of the 2016 data, re-examining the relationships we observe in the panel data at the individual level before exploring mechanisms.

We first estimate a regression specification analogous to Equation 1 where individual-level victim registration status is regressed on dummy variables indicating whether or not the respondent self-reports as being victimized by a particular group[4] as well as basic demographic information (sex, age, and a quadratic term of age). Column (1) of Table 2 reports this analysis. We observe that individuals who consider themselves victims of the guerrillas or victims of the paramilitaries are much more likely than non-victims to register as a victim conditional on demographic characteristics. In contrast, state victims are statistically indistinguishable from non-victims in terms of registration rates. A similar pattern holds for whether or not individuals received reparations, suggesting that this lack of registration on behalf of state victims implies foregoing potential income in addition to participation in transitional justice. The findings are consistent with the results from the panel analysis.

While it is reassuring that the same relationship holds at the unit of analysis around

[4]The question follows two parts. First, respondents are asked whether or not they were victimized during the conflict and how and if they were who was responsible for that victimization. We merge other actors (not state, paramilitary, or guerrilla) into a single alternative category.

Table 2: Individual Victimization, Registration, and Reparations

	Trust Index (1)	Register as Victim (2)	Receive Reparations (3)
Victim of:			
State	−0.531	0.015	−0.053
	(0.240)	(0.062)	(0.034)
Guerrilla	−0.091	0.181	0.043
	(0.089)	(0.027)	(0.018)
Paramilitary	−0.008	0.163	0.085
	(0.117)	(0.041)	(0.033)
Other	0.033	0.017	−0.011
	(0.140)	(0.040)	(0.030)
Demographics:			
Female	0.014	0.027	0.028
	(0.070)	(0.013)	(0.011)
Age	−0.029	−0.001	0.003
	(0.013)	(0.003)	(0.002)
Age^2	0.000	0.000	−0.000
	(0.000)	(0.000)	(0.000)
PSU FE	Yes	Yes	Yes
R^2	0.273	0.358	0.231
Num. obs.	1561	1562	1562

Note: Heteroskedasticity consistent robust standard errors clustered at the PSU-Survey Round level in parentheses. Estimates based on LAPOP's 2016 survey round (registration and reparation questions were not asked in earlier round) and include PSU fixed effects.

which our theory was constructed, the primary benefit of the survey data is that it allows us to examine our proposed mechanisms. We argue that when individuals are victimized by the state during civil war trust is eroded (proposition 1), and that reconciled engagement with state institutions requires some level of trust in those institutions (proposition 2). Together, victimization by the state should decrease participation in forms of engagement which are predicated on that trust, such as registration as a victim, voting, *etc.* (hypothesis 2).

[FIGURE 5 HERE]

We first test whether victimization by different actors is associated with levels of trust in state institutions (compared to non-victims) using simple Ordinary Least Squares (OLS) regressions controlling for demographic factors and including primary sampling unit (PSU)-Wave fixed-effects and standard errors clustered at the PSU-Wave level.[5] Figure 5 plots estimated coefficients of guerrilla, paramilitary, and state victimization on trust in the judiciary, the armed forces, congress, police, *etc.* as well as an average index of each of these 7-point likert-type items. The data used pooled observations from both the 2014 and 2016 survey rounds. Overall, victimization by guerrillas is not associated in any meaningful way with trust in state institutions. Similarly, while paramilitary victims on average trust the police less than non-victims there is no systematic relationship. In contrast, individuals who have been victimized by the state report lower levels of trust in the judiciary, the armed forces, congress, police, the president, and elections. These coefficients are significant at the 95% level for the judiciary, the police, and elections. Moreover, state victims score lower on an index averaging across trust in all institutions. A full tabular presentation of these results may be found in appendix Table A4.

These results provide support for our first proposition. In line with theoretical expectations, victimization by the state is associated with reduced trust in relevant state institutions. Unfortunately, due to the low number of individuals reporting victimization by the state (fewer than 100 across both survey rounds) we are unable to conduct a formal mediation analysis. Nevertheless, from these findings and from the consistent results at the municipality-year level we are able to draw several meaningful conclusions. Additionally, Table A5 provides additional support for the link between trust and political participation

[5] As each of these outcome variables are measured via Likert-type items, there is a concern that the outcome scale is not continuous and therefore violates OLS assumptions. For ease of interpretation and comparison we have included OLS estimates here though Table A6 in the appendix presents analogous ordinal logistic regression specifications. Results are comparable throughout – holding other variables constant, state victimization decreases the ordered log-odds of trusting the judiciary, the armed forces, congress, the police, the president, and elections.

more broadly (proposition 2). Regressing our trust index and demographic covariates on voting in the previous election, intention to vote in the next election, and on participation in community groups suggest that higher levels of trust in state institutions are positively associated with reconciled forms of political participation in the sample we consider.

While these results are purely associational, our argument relies on a causal relationship between victimization and trust. We control for a limited set of pre-treatment, observable characteristics as well as PSU-Wave Fixed Effects to account for selection based on these characteristics, but there are likely many unobserved qualities that make individuals more or less likely to be victimized by particular groups. Though we cannot directly examine the effects of these characteristics, we can use coefficient stability approaches to assess the degree to which selection on unobserved characteristics might bias our results and limit our ability to make causal claims. The intuition is that we can analyze how selection on unobserved characteristics might influence our estimates based on how selection on observed variables changes the coefficients of interest. To do so, we use the approach presented in Oster (2019) and first estimate the OLS coefficient without controls ($\hat{\beta}$) and then with controls ($\hat{\beta}^*$). We can use the difference between these estimates, their respective R^2 values, and assumptions regarding the upper bound of variance explained (R^2_{max}) and the strength of selection on unobservables relative to observables (d).[6] Per Oster (2019), we identify a bound for this coefficient ($\hat{\tilde{\beta}}$) by subtracting from the controlled coefficient the movement from scaled by the assumed strength of this relationship and the amount of variance yet to be explained following equation 2:

$$\hat{\tilde{\beta}} = \hat{\beta}^* - d(\hat{\beta} - \hat{\beta}^*) \times \frac{R_{max} - R^*}{R^* - R} \tag{2}$$

The estimates used for this analysis are presented in Table A7 and the sets of identified coefficients under many values of d (including negative values) and R^2_{max} are presented in Figure A2. We find that adding controls induces a small increases in the magnitude of our effect size (effect of victimization is more negative in the controlled regression) and R^2 increases dramatically. To reduce our observed effect to zero, we would need to be able to account for more than twice the variation in our trust index compared to our controlled regression and selection on unobservable characteristics would need to be twice as strong as selection based on observed variables and operate in the *opposite* direction. This analysis suggests that our findings are not particularly sensitive to selection on unobservables.

[6] For example, a value of $d = 1$ would assume that unobservables equally are as important as observable characteristics in selection into victimization by the state.

Conclusion

Across administrative and survey data we find that the consequences of victimization during civil war vary by perpetrator. We observe that in contrast to victimization by non-state actors, state violence does not lead to a significant rise in engagement with transitional justice institutions. Moreover, we offer suggestive evidence that the relationship between victimization and participation is mediated by the effect of victimization on trust in state institutions. Altogether, these results suggest that understanding the legacies of conflict requires a more nuanced look at the processes of civilian victimization. In particular, victimization by the state during conflict has implications for state legitimacy and consolidation of peace after conflict. Transitional justice seeks to repair instances of past violence and construct a durable peace; our results suggest that its uneven uptake warrants further attention. Given that state victims access governmental benefits at lower rates than other victims, they may continue to distrust and feel aggrieved by the state, posing challenges in post-conflict settings prone to conflict reversion.

In terms of external validity, our study draws on evidence from the Colombian context, though we expect its results to lend insights to other instances of political violence where the state commits crimes, including those of both state repression (such as Chile) and other civil conflicts (including Guatemala). In these contexts, we believe the mechanisms we examine –decreased state trust as a result of violence carried out by governmental entities– will hold true and might yield subsequent decreases in reconciled political activity and transitional justice participation. At the very least, scholars and practitioners should consider how the position of the perpetrator might influence participation and success of post-conflict policies and interventions.

Moreover, the ideal transitional justice regime may differ according to *who* perpetrated violence. We cannot treat victims monolithically. When the state is the perpetrator, it may be advantageous to have international or non-state entities involved. This will allow victims to benefit from transitional justice measures without having to engage with the entity responsible for their victimization. Alternatively, states and other organizations should explicitly cater to victims of state violence, recognizing that distrust is likely to exist. Involving victims in the design of transitional justice policies may help to send additional signals that the state is trustworthy and intent on helping victims.

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Failure to Protect:

Civilian Victimization and State Legitimacy in Civil War

Figures and Graphs

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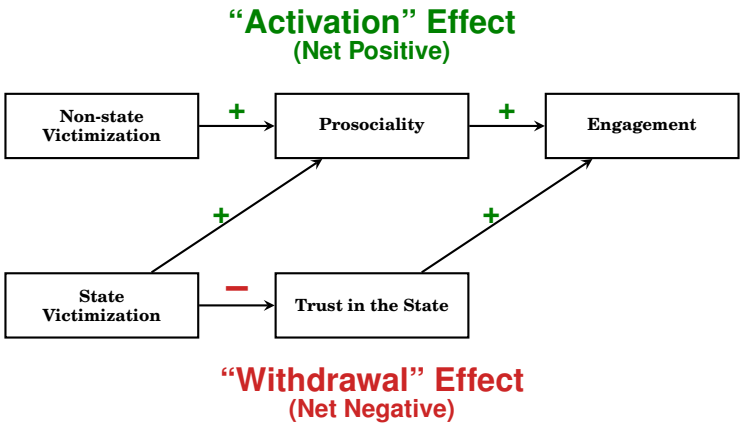
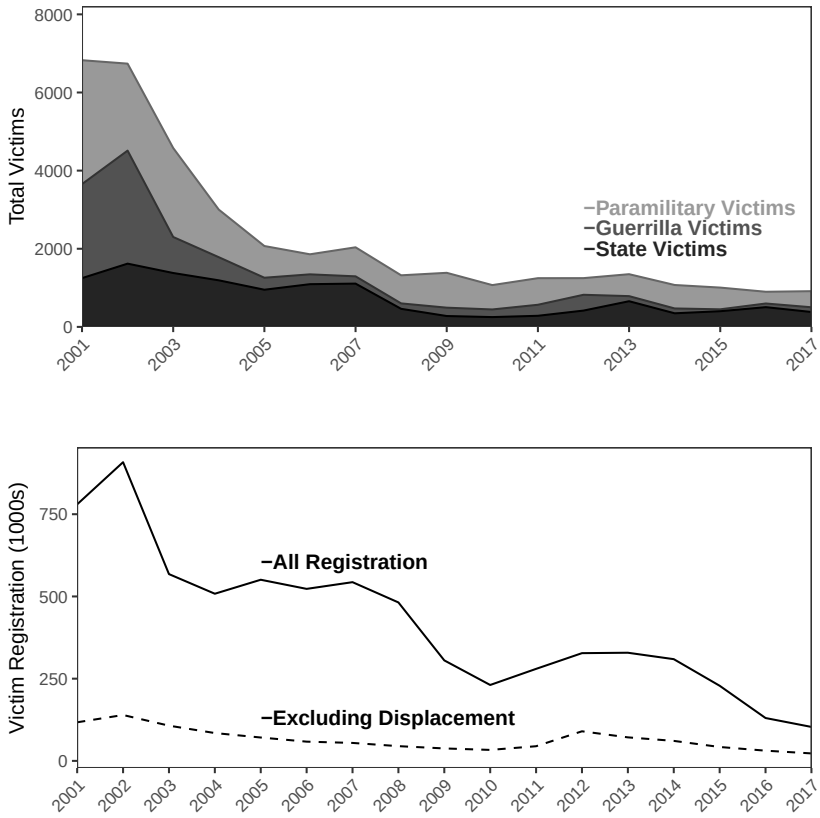
June 20, 2019

Figure 1: Expected Processes



Note: (+) and (–) signs indicate the expected direction of effects between variables. Thus, net effects are multiplicative (by decreasing trust in the state, the expected effect of state victimization mediated through changes in state trust is negative, despite the positive effect of state trust on engagement).

Figure 2: Trends in Victimization and Registration over Time



Note: Each panel represents country-level aggregated trends in key variables over time. Data on victimization are from CINEP. Victim Registration data are from Colombia's Victim Unit.

Figure 3: Victimization and Registration

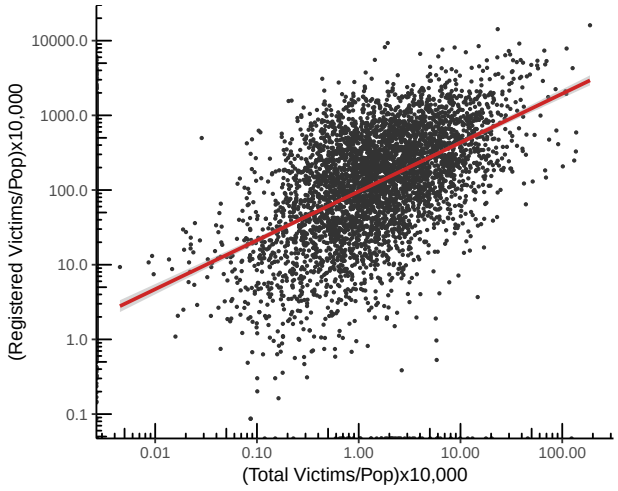
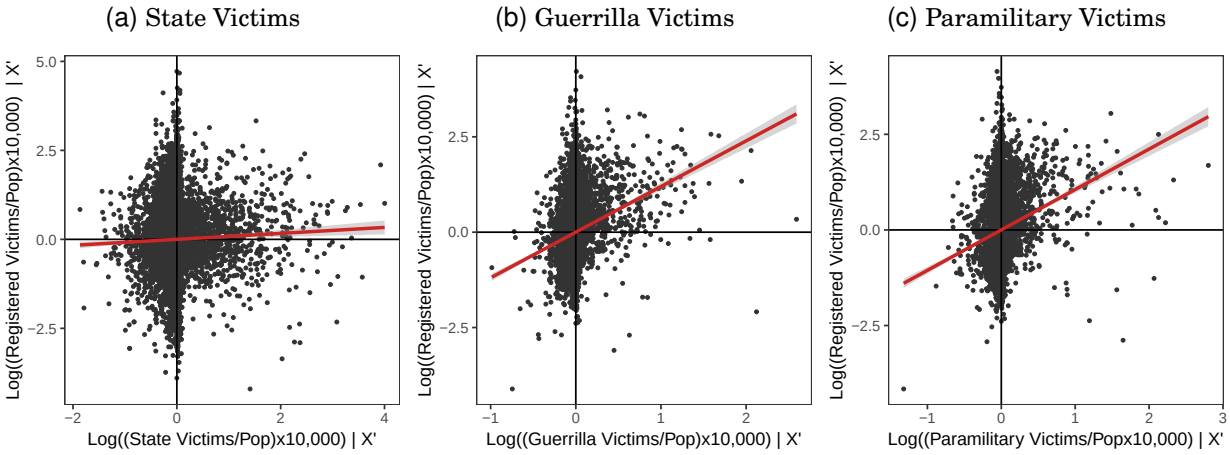
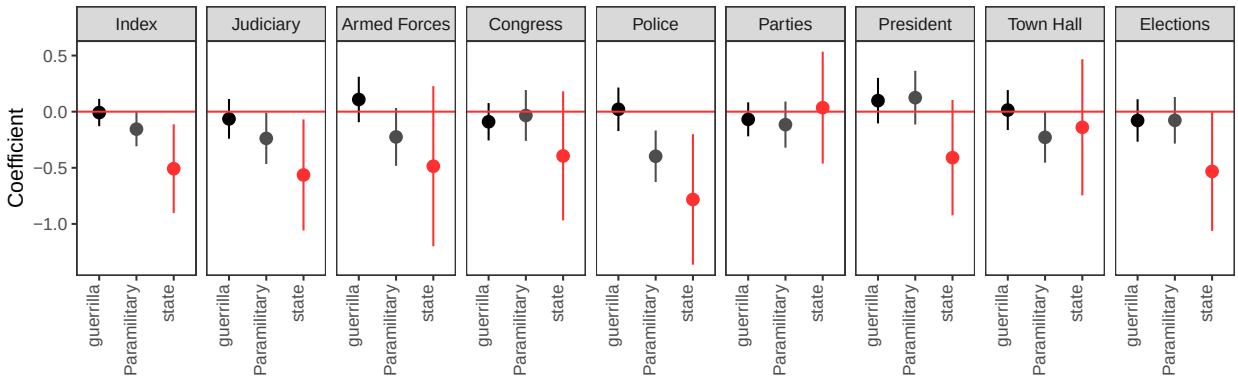


Figure 4: Added Variable Plots



Note: Each panel presents results partial regression plots for each of our main explanatory variables using the specification presented in Column (8) of Table **??**. The slope of the line is equivalent to the coefficients presented in said column, representing the variation in registered victims -excluding displacement– explained by the variable of interest that is unexplained by all other variables in the regression. Appendix Figure **??** presents similar plots for the raw specifications (column (1) of Table **??**).

Figure 5: Individual Victimization and Trust in State Institutions



Note: Each facet displays coefficients and 95% confidence intervals estimated in Table A4. Only coefficients for victimization by the state, guerrillas, and paramilitaries are presented, but specifications additionally control for gender, age and age-squared. All outcomes range from 0 to 7 and 'index' is the average across each of the other outcomes. Estimated on a pooled sample of LAPOP survey rounds conducted in 2014 and 2016.

A. Supplementary Information

Table A1: One- Way Fixed Effects Estimates of Victimization on Registration

	Total Registration				Excluding Displacement			
	Raw	Per Cap.	log(y)	arcsinh(y)	Raw	Per Cap.	log(y)	arcsinh(y)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State Victims	−8.094	−4.766	0.216	0.171	2.404	−0.480	0.053	0.049
	(7.088)	(7.489)	(0.122)	(0.101)	(1.065)	(0.679)	(0.106)	(0.099)
Guerrilla Victims	33.419	38.974	1.333	1.110	7.223	4.339	1.285	1.197
	(8.571)	(10.167)	(0.126)	(0.102)	(1.209)	(1.255)	(0.102)	(0.094)
Paramilitary Victims	30.935	46.449	1.538	1.281	3.050	3.550	1.135	1.057
	(7.071)	(10.275)	(0.149)	(0.128)	(0.909)	(0.909)	(0.110)	(0.105)
χ^2 : $(\beta_S = \beta_G)$	191.788	312.27	118.131	101.732	124.742	220.377	264.063	231.081
$Pr(> \chi^2)$	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Municipio FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	1,172	1,172	1,172	1,172	1,172	1,172	1,172	1,172
Observations	16,890	16,030	16,030	16,030	16,890	16,030	16,030	16,030
R-squared	0.087	0.181	0.091	0.077	0.204	0.094	0.108	0.094

Note: Heteroskedasticity Consistent Robust Standard Errors Clustered at the Municipality level in parentheses. Columns (1) through (4) present results where the dependent variable is the total number of registered victims per municipality-year. Columns (5)-(6) exclude victims registering as displaced. Columns (1) and (5) present estimates from raw count figures; in Columns (2) and (6) all key variables are per-capitized; in (3) and (7) variables are per-capitized and then logged; finally (4) and (8) present results using the inverse hyperbolic sine transformation of our key variables. The row denoted χ^2 $(\beta_S = \beta_G)$ presents the χ^2 of a linear hypothesis test of whether the coefficient on the state victims (β_S) is the same as the coefficient on the variable measuring victims of guerrilla forces (β_G).

*** Significant at the .1 percent level.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

Table A2: FE Estimates of Victimization on Registration with Temporal Lags

	Total Registration			Excluding Displacement		
	(1)	(2)	(3)	(4)	(5)	(6)
State Victims$_t$	0.171	0.326	0.419	0.049	0.142	0.203
	(0.101)	(0.087)	(0.080)	(0.099)	(0.089)	(0.082)
State Victims$_{t-1}$		0.061	0.148		−0.127	−0.099
		(0.076)	(0.069)		(0.082)	(0.076)
State Victims$_{t-2}$			0.058			−0.145
			(0.083)			(0.067)
Guerrilla Victims$_t$	1.110	0.975	0.785	1.197	1.089	0.929
	(0.102)	(0.089)	(0.105)	(0.094)	(0.091)	(0.092)
Guerrilla Victims$_{t-1}$		0.741	0.532		0.748	0.564
		(0.074)	(0.085)		(0.068)	(0.081)
Guerrilla Victims$_{t-2}$			0.595			0.559
			(0.076)			(0.075)
Paramilitary Victims$_t$	1.281	0.777	0.767	1.057	0.690	0.747
	(0.128)	(0.179)	(0.219)	(0.105)	(0.136)	(0.138)
Paramilitary Victims$_{t-1}$		0.828	0.462		0.595	0.262
		(0.075)	(0.106)		(0.072)	(0.094)
Paramilitary Victims$_{t-2}$			0.593			0.475
			(0.066)			(0.059)
χ^2 : $(\beta_S = \beta_G)$	101.73	39.34	9.45	231.08	128.32	59.71
$Pr(> \chi^2)$	0.000	0.000	0.002	0.000	0.000	0.000
Municipio FE	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	1,172	1,172	1,172	1,172	1,172	1,172
Observations	16,030	14,327	12,984	16,030	14,327	12,984
R-squared	0.077	0.090	0.086	0.094	0.103	0.093

Note: Heteroskedasticity Consistent Robust Standard Errors Clustered at the Municipality level in parentheses. Each column presents results using the inverse hyperbolic sine transformation of per-capitized values of key variables. The row denoted χ^2 $(\beta_S = \beta_G)$ presents the χ^2 of a linear hypothesis test of whether the coefficient on the state victims (β_S) is the same as the coefficient on the variable measuring victims of guerrilla forces (β_G).

*** Significant at the .1 percent level.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

Table A3: Two-Way Fixed Effects Estimates of Victimization on Registration with Controls

Panel A: Subset with CEDE Data (no covariates)

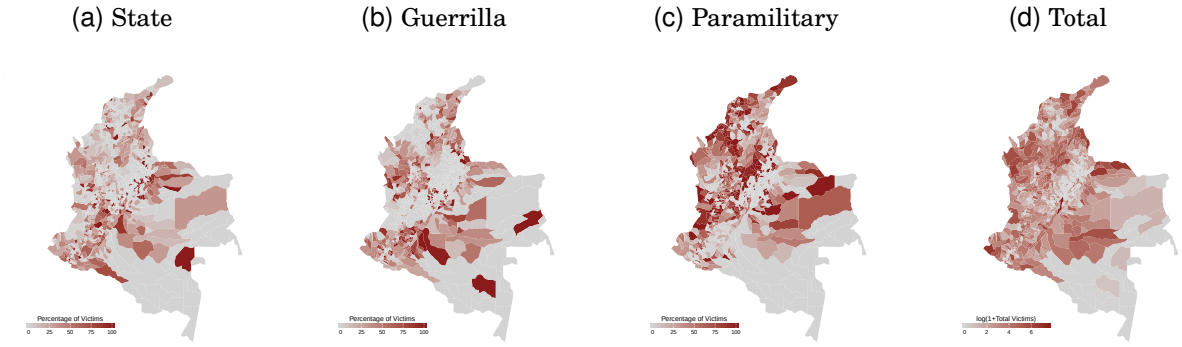
	Total Registration				Excluding Displacement			
	Raw	Per Cap.	log(y)	arcsinh(y)	Raw	Per Cap.	log(y)	arcsinh(y)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State Victims	7.063	12.208	0.256	0.193	1.386	0.951	0.205	0.191
	(6.361)	(5.584)	(0.094)	(0.077)	(0.676)	(0.510)	(0.076)	(0.076)
Guerrilla Victims	38.474	21.998	0.640	0.524	2.997	2.765	0.812	0.794
	(16.246)	(8.921)	(0.144)	(0.122)	(0.836)	(0.736)	(0.124)	(0.118)
Paramilitary Victims	12.864	7.751	0.376	0.314	0.038	1.122	0.399	0.375
	(16.328)	(7.908)	(0.151)	(0.131)	(0.738)	(0.632)	(0.130)	(0.135)
χ^2 : $(\beta_S = \beta_G)$	13.346	3.205	5.75	5.206	4.39	13.287	19.824	18.539
$Pr(>\chi^2)$	0.000	0.073	0.016	0.023	0.036	0.000	0.000	0.000
Municipio FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	1,026	1,026	1,026	1,026	1,026	1,026	1,026	1,026
Observations	4,841	4,841	4,841	4,841	4,841	4,841	4,841	4,841
R-squared	0.016	0.014	0.012	0.009	0.014	0.017	0.019	0.017

Panel B: With Additional Covariates

	Total Registration							
	Raw	Per Cap.	log(y)	arcsinh(y)	Raw	Per Cap.	log(y)	arcsinh(y)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State Victims	6.335	12.112	0.252	0.190	0.684	0.931	0.195	0.181
	(6.445)	(5.533)	(0.094)	(0.076)	(0.558)	(0.503)	(0.075)	(0.075)
Guerrilla Victims	36.996	21.077	0.586	0.476	3.266	2.668	0.778	0.759
	(16.123)	(8.824)	(0.144)	(0.123)	(0.860)	(0.728)	(0.120)	(0.115)
Paramilitary Victims	4.388	6.308	0.305	0.252	−1.010	0.951	0.343	0.319
	(17.920)	(7.838)	(0.148)	(0.127)	(0.737)	(0.622)	(0.129)	(0.133)
Controls								
Unemployment	2.708	0.063	0.003	0.003	0.219	0.005	0.001	0.001
	(1.078)	(0.043)	(0.001)	(0.001)	(0.078)	(0.003)	(0.001)	(0.001)
Homicides	9.122	0.110	0.003	0.004	1.163	0.015	0.003	0.004
	(2.135)	(0.056)	(0.001)	(0.001)	(0.360)	(0.008)	(0.002)	(0.002)
Gov. Spending	0.001	0.00001	0.00000	0.00000	−0.0002	0.00000	0.00000	0.00000
	(0.001)	(0.00001)	(0.00000)	(0.00000)	(0.0001)	(0.00000)	(0.00000)	(0.00000)
Constant GDP p.c.	−37.454	−1.780	−0.087	−0.096	0.621	−0.064	−0.024	−0.031
	(18.756)	(0.908)	(0.020)	(0.022)	(1.438)	(0.096)	(0.016)	(0.020)
χ^2 : $(\beta_S = \beta_G)$	13.193	2.698	4.451	3.975	12.489	12.279	18.648	17.366
$Pr(>\chi^2)$	0.000	0.100	0.035	0.046	0.000	0.000	0.000	0.000
Municipio FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	1,026	1,026	1,026	1,026	1,026	1,026	1,026	1,026
Observations	4,841	4,841	4,841	4,841	4,841	4,841	4,841	4,841
R-squared	0.064	0.021	0.039	0.035	0.121	0.028	0.037	0.034

Note: Heteroskedasticity Consistent Robust Standard Errors Clustered at the Municipality level in parentheses. Columns (1) through (4) present results where the dependent variable is the total number of registered victims per municipality-year. Columns (5)-(6) exclude victims registering as displaced. Columns (1) and (5) present estimates from raw count figures; in Columns (2) and (6) all key variables are per-capitized; in (3) and (7) variables are per-capitized and then logged; finally (4) and (8) present results using the inverse hyperbolic sine transformation of our key variables. The row denoted χ^2 $(\beta_S = \beta_G)$ presents the χ^2 of a linear hypothesis test of whether the coefficient on the state victims (β_S) is the same as the coefficient on the variable measuring victims of guerrilla forces (β_G).

Figure A1: Spatial Variation in Victimization



Source: The data on victimization are from CINEP. Disaggregation into responsible actors was coded by the authors. Boundaries are from WFPGeoNode.

Table A4: Victimization and Individual Trust in the State

		Trust in:							
	Index	Judicial	Armed Forces	Congress	Police	Parties	President	Town Hall	Elections
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Victim of:									
State	−0.508	−0.564	−0.486	−0.394	−0.782	0.036	−0.409	−0.139	−0.532
	(0.196)	(0.245)	(0.354)	(0.285)	(0.289)	(0.247)	(0.255)	(0.301)	(0.263)
Guerrilla	−0.008	−0.064	0.109	−0.090	0.021	−0.068	0.099	0.015	−0.078
	(0.062)	(0.090)	(0.103)	(0.085)	(0.098)	(0.077)	(0.103)	(0.090)	(0.096)
Paramilitary	−0.155	−0.238	−0.225	−0.034	−0.397	−0.115	0.125	−0.228	−0.077
	(0.078)	(0.116)	(0.131)	(0.115)	(0.116)	(0.104)	(0.121)	(0.115)	(0.105)
Other	−0.013	−0.097	−0.015	0.021	0.056	−0.157	0.053	−0.015	−0.104
	(0.104)	(0.145)	(0.166)	(0.143)	(0.150)	(0.128)	(0.151)	(0.144)	(0.143)
Demographics:									
Female	−0.032	0.033	−0.409	0.105	0.160	−0.042	−0.045	0.104	−0.161
	(0.048)	(0.064)	(0.074)	(0.065)	(0.067)	(0.057)	(0.071)	(0.069)	(0.066)
Age	−0.015	−0.019	0.003	−0.025	−0.011	−0.029	−0.023	−0.006	−0.016
	(0.009)	(0.013)	(0.013)	(0.012)	(0.013)	(0.011)	(0.013)	(0.012)	(0.012)
Age^2	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
PSU-Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.262	0.237	0.215	0.238	0.213	0.219	0.280	0.279	0.225
Num. obs.	3056	3019	3028	2968	3042	3024	3038	3014	3023

Note: Heteroskedasticity consistent robust standard errors clustered at the PSU-Survey Round level in parentheses. Estimates based on pooled samples of LAPOP 2014 and 2016 survey rounds and include PSU-Wave fixed effects. Index variable is simply the row-wise mean of all trust in state institutions survey items. Survey wording: "Hasta qué punto tiene confianza usted en…"

Table A5: Trust in the State and Political Participation

	Vote Previous (1)	Vote Next (2)	Community Group (3)
Trust	0.017	0.029	0.062
Index	(0.008)	(0.006)	(0.013)
Demographics:			
Female	0.000	−0.019	−0.056
	(0.016)	(0.014)	(0.028)
Age	0.048	0.009	0.008
	(0.003)	(0.002)	(0.005)
Age^2	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)
PSU-Wave FE	Yes	Yes	Yes
R^2	0.304	0.513	0.253
Num. obs.	3021	2863	3047

Note: Heteroskedasticity consistent robust standard errors clustered at the PSU-Survey Round level in parentheses. Estimates based on Combined LAPOP 2016 and 2014 survey rounds and include PSU-Wave fixed effects.

Table A6: Ordinal Logit Specification: Victimization and Individual Trust in the State

		Trust in:						
	Judicial (1)	Armed Forces (2)	Congress (3)	Police (4)	Parties (5)	President (6)	Town Hall (7)	Elections (8)
Victim of:								
State	−0.649	−0.499	−0.633	−0.829	−0.035	−0.562	−0.144	−0.715
	(0.009)	(0.008)	(0.007)	(0.009)	(0.008)	(0.007)	(0.008)	(0.008)
Guerrilla	−0.048	0.111	−0.120	0.039	−0.119	0.058	0.010	−0.133
	(0.079)	(0.085)	(0.079)	(0.082)	(0.082)	(0.087)	(0.079)	(0.089)
Paramilitary	−0.296	−0.214	−0.044	−0.472	−0.164	0.109	−0.277	−0.112
	(0.106)	(0.106)	(0.108)	(0.099)	(0.110)	(0.102)	(0.099)	(0.098)
Other	−0.090	−0.011	0.050	0.063	−0.172	0.106	−0.027	−0.157
	(0.133)	(0.135)	(0.138)	(0.124)	(0.138)	(0.134)	(0.129)	(0.136)
Demographics:								
Female	0.034	−0.450	0.116	0.173	−0.080	−0.050	0.096	−0.223
	(0.067)	(0.071)	(0.072)	(0.066)	(0.072)	(0.071)	(0.072)	(0.072)
Age	−0.022	−0.003	−0.036	−0.011	−0.038	−0.023	−0.006	−0.027
	(0.010)	(0.009)	(0.010)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)
Age^2	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
PSU-Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
AIC	11542.801	11745.330	11076.887	11932.409	10081.491	11171.818	11521.007	11188.457
Log Likelihood	-5257.400	-5358.665	-5024.444	-5452.205	-4526.745	-5071.909	-5246.504	-5080.228
Deviance	10514.801	10717.330	10048.887	10904.409	9053.491	10143.818	10493.007	10160.457
Num. obs.	3019	3028	2968	3042	3024	3038	3014	3023

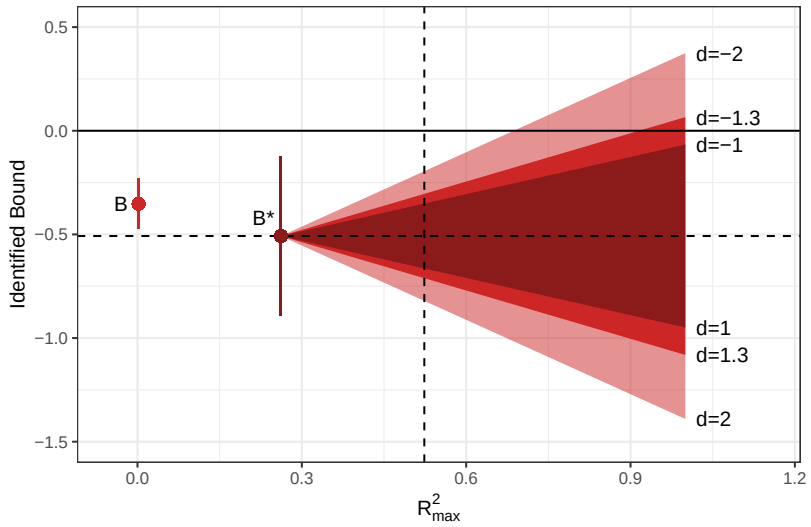
Note: Robust standard errors clustered at the PSU-Survey Round level in parentheses. Estimates are from ordinal logistic regressions estimated on pooled samples of LAPOP 2014 and 2016 survey rounds and include PSU-Wave fixed effects. Survey wording: "Hasta qué punto tiene confianza usted en…"

Table A7: Estimates Used in Coefficient Stability Exercise

	Raw (1)	Controlled (2)
Victim of:		
State	−0.353	−0.508
	(0.201)	(0.196)
Guerrilla	0.030	−0.008
	(0.060)	(0.062)
Paramilitary	−0.063	−0.155
	(0.077)	(0.078)
Other	−0.085	−0.013
	(0.094)	(0.104)
Demographics:		
Female		−0.032
		(0.048)
Age		−0.015
		(0.009)
Age^2		0.000
		(0.000)
PSU-Wave FE	No	Yes
R^2	0.002	0.262
Num. obs.	3058	3056

Note: Heteroskedasticity consistent robust standard errors clustered at the PSU-Wave level in parentheses. Estimates based on Combined LAPOP 2016 and 2014 survey rounds and include PSU-Wave fixed effects.

Figure A2: Coefficient Stability Bounds



Note: This figure plots an identified set of coefficients conditional on movement in our coefficient of interest in response to observable characteristics (moving from B to B* along the Y-axis), plausible levels of R^2_{max} and the strength of selection on unobservables, d. $d = 1$ assumes selection on unobservables is as strong as selection on observables and in the same direction. A value of $d = -2$ would assume selection on unobservables is twice as strong and in the *opposite* direction. The dashed vertical line highlights an theoretical upper bound on R^2 that assumes accounting for all unobservable characteristics would lead to a doubling of R^2 compared to the observed regression.