State Violence and Participation in Transitional Justice: Evidence from Colombia

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Abstract

Can the legacy of state violence undermine participation in transitional justice policies designed to consolidate peace after conflict? This article argues that there are differential levels of transitional justice uptake, and that state-led violence can generate lower participation in government reconciliation policies. We leverage spatial and temporal variation in civilian victimization by perpetrator and find that in contrast to violence by non-state groups, violence carried out by the state against civilians translates into lower levels of enlistment in Colombia's state-run victim's registry. Combined with survey evidence linking victimization to levels of trust in the government, our analyses demonstrate that the identity of the perpetrator and the context of participation determine the effects of wartime violence on political engagement. Consequently, our findings have implications both for the design of transitional justice policies and the study of the legacies of conflict on political and social outcomes.
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Over the course of five decades of civil conflict, Colombia has amassed one of the world's largest populations of war victims. This level of victimization has necessitated the development of transitional justice services to advocate for the rights of victims and to deliver services to victims in the hope of reconciliation. Registration with Colombia's Victim's Unit (*Unidad para la atención y reparación integral a las víctimas*) is a bureaucratic procedure that makes individuals eligible for material reparations, a transitional justice policy first implemented by the government in 2011. With 8.9 million victims registered to date, this policy constitutes the most broadly utilized transitional justice policy in existence.

In this letter, we examine whether victimization by the state leads to differential uptake of victim registration relative to victimization by non-state actors. While many studies have shown that exposure to violence can increase prosociality and participation in voting or civic groups (Bauer *et al.* 2016), participating in transitional justice policies often requires contact with state institutions that failed to protect civilians or perpetrated violence themselves. We hypothesize that this dynamic leads to a lower propensity to register as a victim –despite material incentives– among those victimized by the state compared to those victimized by non-state groups.

We first test this hypothesis using panel analysis of administrative data. We find that registration as a victim is strongly correlated with observed levels of victimization. However, when we account for victimization by guerrillas and paramilitaries, victimization by the state is not associated with any contemporaneous increase in registration. We then replicate this finding at the micro-level using survey data from the Latin American Public Opinion Project Americas Barometer (LAPOP 2018). Individuals who self-report as a victim of non-state actors exhibit higher levels of registration and greater propensity to receive reparations compared to state victims. We also use these data to provide support for our hypothesized mechanism and find that violence perpetrated by the state lowers trust in state-run institutions.

Theoretical framework

In brief, we expect that compared to violence conducted by other actors, state-led victimization will reduce trust in state institutions, leading to lower levels of transitional justice participation. Existing literature on the relationship between wartime violence and trust suggests that conflict exposure is linked to lower levels of generalized trust (De Luca & Verpoorten 2015; Kijewski & Freitag 2018; Rohner *et al.* 2013). Citizens can also lose trust in actors and institutions perpetrating or permitting violence. This has occurred, for instance, in response to militia violence in Mali (Gates & Justesen 2016), drug related violence in Mexico (Ishiyama *et al.* 2018), and civil war violence in Nepal (De Juan & Pierskalla 2016). Kreutz & Nussio (2019) suggest in the Colombian case that the government's policy of extradition to the United States eroded trust among those affected –former combatants– and Kaplan & Nussio (2018) highlight how low levels of trust in government can fuel recidivism.

Together, these findings and expectations lead us to our first hypothesis which we later

examine using individual-level survey data in Colombia:

- *Hypothesis 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 & Pierskalla (2016) point out, it is especially in post-conflict contexts where trust in state institutions can be of 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 reduce recurrence of conflict (Sacks & Larizza 2012; Hutchison & Johnson 2011). Moreover, widespread compliance with and participation in transitional justice policies requires trust in the state as demonstrated by the literature on trust and participation generally (Levi & Stoker 2000) and on compliance with regulation (e.g. taxation) (Fjeldstad 2004). Therefore, we expect that trust will be an important determinant of participation in transitional justice.

Few studies directly measure this outcome. Instead, research often focuses on attitudes toward transitional justice policies (as a proxy for participation) and find that that victims generally support policies of reparation (Nussio *et al.* 2015; Miklos *et al.* 2004; Gibson 2002; Nalepa 2010; Laplante & Theidon 2007; David & Choi 2009). However, research suggests that heterogeneous conflict experiences predicts distinct transitional justice preferences (Pham *et al.* 2004). For example, Hall *et al.* (2018) show that while exposure to violence heightens support for retributive transitional justice, preferences are also shaped by contemporary contact with perpetrators, which increases restorative justice support. In the Spanish case, Aguilar *et al.* (2011) suggest that ideology, familial victimization, and regional context predict attitudes toward transitional justice while Samii (2013) similarly finds that partisanship shapes transitional justice preferences in postwar Burundi.

We extend this literature in two ways. First, we argue that the identity of the perpetrator is an important dimension of conflict heterogeneity. Second, we go beyond reported attitudes to measure behavioral engagement with transitional justice.

Overall, we expect victims of state-perpetrated violence will be reluctant to participate in government-led transitional justice policies given their distrust in the state. When violence is perpetrated by non-state entities, such as paramilitary or guerrilla groups, we do not expect victims to 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 to obtain personal benefits, such as material ones that accrue from reparations policies or social welfare systems.

- *Hypothesis 2:* State-led victimization will generate lower levels of transitional justice engagement than non-state victimization.

Conflict and Victimization in Colombia

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 government. Dynamics of violence changed considerably throughout the conflict and can generally be categorized into four distinct time periods (Ch *et al.* 2018). In the late 1980s to mid-1990s, violence was largely perpetrated by the FARC. During this time, no victim registry existed. By the time our data on victim registration begin in 2001, paramilitaries were increasingly powerful and had consolidated under the umbrella of United Self-Defense Forces of Colombia (AUC). The share of victimization by paramilitary groups soon decreased, as paramilitaries agreed to a ceasefire and 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 2012, the FARC and government began peace talks.

In 1997, the 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, the 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. Victims must present the required documentation (identification and two witness declarations). The state then issues a mailed administrative notice within 120 days.

Once victims initiate the reparation process, they receive a personalized note from the government expressing its commitment to reparation. For many victims, this is the sole response they have received, as only 10.9% of victims had been paid as of January 2019 (Unidad para las victimas 2019). We are concerned with the decision of individuals to take the first step in the process: registering with the Victim's Unit. Given that registration may yield an economic benefit in the form of a reparation payment, we consider victim registration to be a "hard test" for our theory. Individuals might be particularly motivated to participate because of the promised economic gain even if they have lost trust in the government.

Empirical Strategy

We argue that individuals who are victimized by non-state actors will be more likely to participate in transitional justice policies while those victimized by state forces will lose trust in the government and be less likely to engage relative to those victimized by non-state actors. We test these hypotheses across multiple sources of data and find support in each. We begin by examining the association with victimization and registration as a victim at

the municipality-year level from 2001 to 2017. We then replicate these findings and provide initial evidence for the trust mechanism using individual survey responses from two rounds of the LAPOP Americas Barometer (LAPOP 2018).

Municipality Panel Analysis

The two main data sources for the municipality-year analysis are 1) *El Centro de Investigación y Educación Popular's* (CINEP) violent events database[1] and 2) administrative data on victim registration obtained by the authors from the government of Colombia. Based on regional and national press coverage, the CINEP data catalogs location, number of victims, type of human rights violation, and the groups involved in the event. Our main independent variables are the total number of victims in a given municipality-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 register in the location where victimization occurred, we also test our hypotheses using both the total number of registration and excluding individuals whose main grievance is displacement.

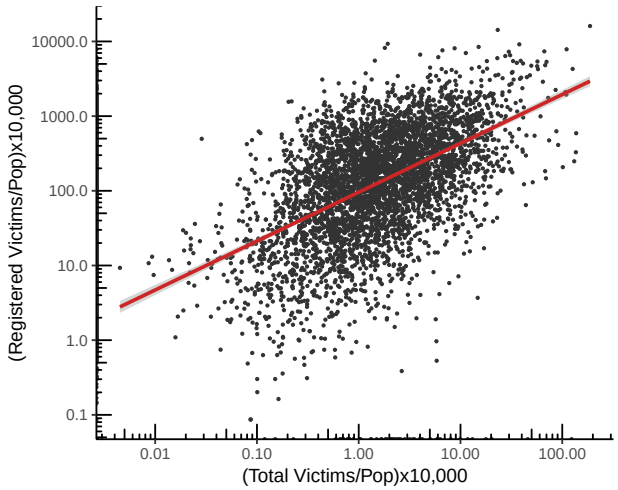


Figure 1: Victimization and Registration

The total number of *registered* victims often outnumbers *observed* victims from the CINEP data because the victim registry also includes individuals indirectly affected by the conflict such as a direct victims' sibling. Nevertheless, we see in Figure 1 that the total number of observed victims per 10,000pop is highly correlated with the level of registered victims. Our goal is to examine whether this relationship varies depending on who carried out violence.

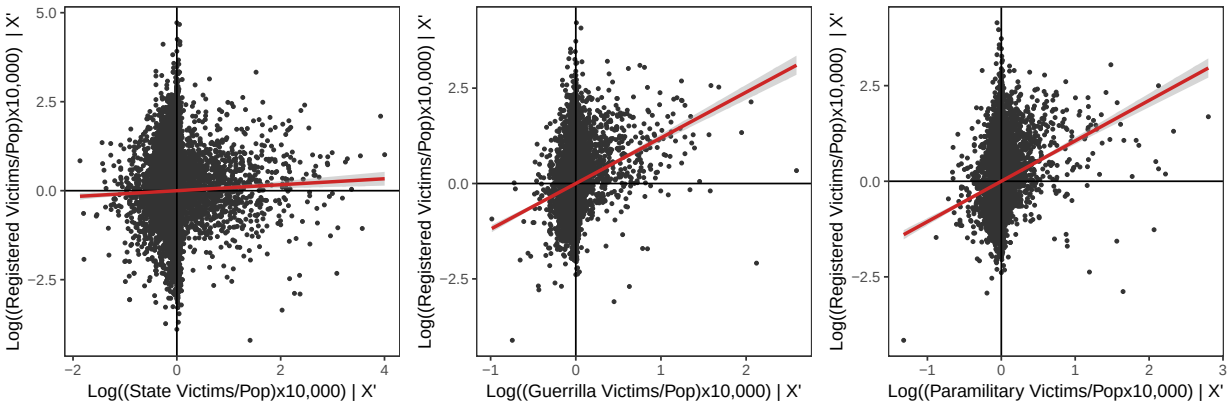
We first estimate a fixed-effects specification following Equation 1. We test whether victimization V by armed group $\in \{G,P,S\}$ or Guerrilla, Paramilitary, or State respectively,

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

is associated with levels of victim registration in a municipality (m) in year (t) controlling for time-invariant municipality-specific characteristics (δ_m), and year-specific fixedeEffects (γ_t). The central challenge in identifying the causal effect of violence, at the individual or administrative level, is that violence is a *strategic* outcome and therefore inherently selective. We include municipality fixed effects to remove time-invariant geographical factors which may influence levels of violence and registration. We also include year fixed effects to remove year-on-year variation in overall levels of violence. We implicitly assume that the remaining systematic variation in victim registration can be captured by observed levels of victimization by armed groups –including the state. However, our findings are robust to the inclusion of additional time-varying controls.

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

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). Our theoretical framework predicts that β_S will be substantively and significantly less than β_G. Figure 2 and Table 1 present our main findings. Each panel of Figure 2 presents the coefficients from Column (8) of the table and data points adjusted for each other variable. Because victim registration and victim counts are not normally distributed we present findings using several data transformations and the results are similar throughout. Standard errors for each specification are calculated using heteroskedasticity consistent robust standard errors clustered at the municipality level.



(a) State Victims (b) Guerrilla Victims (c) Paramilitary Victims

Figure 2: Added Variable Plots. Each panel presents results partial regression plots for each of our main explanatory variables using the specification presented in Column (8) of Table 1. 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.

Table 1: Two-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	−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.

* $p < .05$

** $p < .01$

*** $p < .001$

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. 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.

In Appendix A, we present robustness checks to this main result, examining whether the removal of year-specific effects, different lag structures, matching, and time varying municipality characteristics change our inference.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.

Individual-Level Analysis

We argue that greater distrust in the state and its institutions is one mechanism underlying this finding. To test this hypothesis, we use survey data 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 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 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 a victim of a particular group[2] as well as basic demographic information (sex, age, and a quadratic age term) using the 2016 data. Column (1) of Table 2 reports this analysis. Individuals who self-report as victims of the guerrillas or victims of the paramilitaries are 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.

These data also allow us to examine our proposed mechanism, that state victimization erodes trust. Therefore victimization by the state should decrease participation in forms of engagement which are predicated on that trust, such as registration as a victim.

We test whether victimization by different actors is associated with levels of trust in state institutions (compared to non-victims) using Ordinary Least Squares (OLS) regres-

[2]The question follows two parts. First, respondents are asked whether and how they were victimized during the conflict and, if they were, who was responsible. We merge other actors 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.
* $p < .05$
** $p < .01$
*** $p < .001$

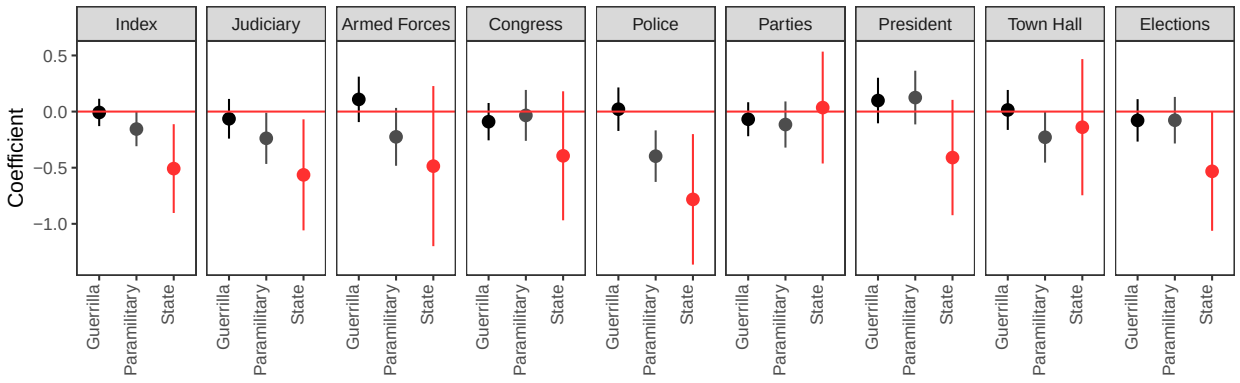


Figure 3: Individual Victimization and Trust in State Institutions. Each facet displays coefficients and 95% confidence intervals estimated in Table A4. Only coefficients for victimization by the state, guerrillas, and paramilitaries are presented, but specifications additionally control for gender, 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.

sions controlling for demographic factors and including primary sampling unit (PSU)-Wave fixed-effects and standard errors clustered at the PSU-Wave level.[3] Figure 3 plots estimated coefficients of guerrilla, paramilitary, and state victimization on trust in the judiciary, 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 the 2014 and 2016 survey rounds. Overall, victimization by guerrillas is not systematically associated 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, armed forces, congress, police, the president, and elections. These coefficients are significant at the 95% level for the judiciary, the police, and elections. State victims also 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.

While these results are associational, our argument relies on a causal relationship between victimization and trust. We control for a limited set of pre-treatment, observable 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 using the approach presented in Oster (2019). More details on this test can be found in the appendix, but we find that 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

[3] 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.

selection on unobservables.

Conclusion

Across administrative and survey data we find that the consequences of victimization during civil war vary by perpetrator. In contrast to victimization by non-state actors, state violence does not lead to a significant rise in transitional justice engagement. We offer suggestive evidence that the relationship between victimization and participation is mediated by the effect of victimization on trust in state institutions. 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. 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. Depending on how indicative our findings are of patterns of other types of political participation predicated on trust, the heterogeneity we detect might more broadly shape post-violence political engagement.

This analysis has implications for the study and design of transitional justice policies which are becoming more common (Sikkink 2011; Olsen *et al.* 2010). Governments commonly implement a host of policies to address the crimes committed during the past period of political violence. Increasingly, researchers are evaluating how these policies shape individuals' political behavior and attitudes (Samii 2013; Hall *et al.* 2018; Nussio *et al.* 2015; Aguilar *et al.* 2011; Cilliers *et al.* 2016). In addition to considering these important dimensions of transitional justice, we should emphasize the process of participating in transitional justice policies from a victim's perspective. Our results suggest that those for whom reconciliation is often most needed (e.g. between victims of the state and the state itself) are the least likely to actually engage in transitional justice. The ideal transitional justice regime may differ according to who perpetrated violence. When the state is a perpetrator, it may be advantageous to have international or non-state entities involved so victims can 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. More generally, analyses of the uptake of transitional justice policies is a critical antecedent to understanding the practical use and consequences of transitional justice programs.

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A. Supplementary Information

Descriptive Information

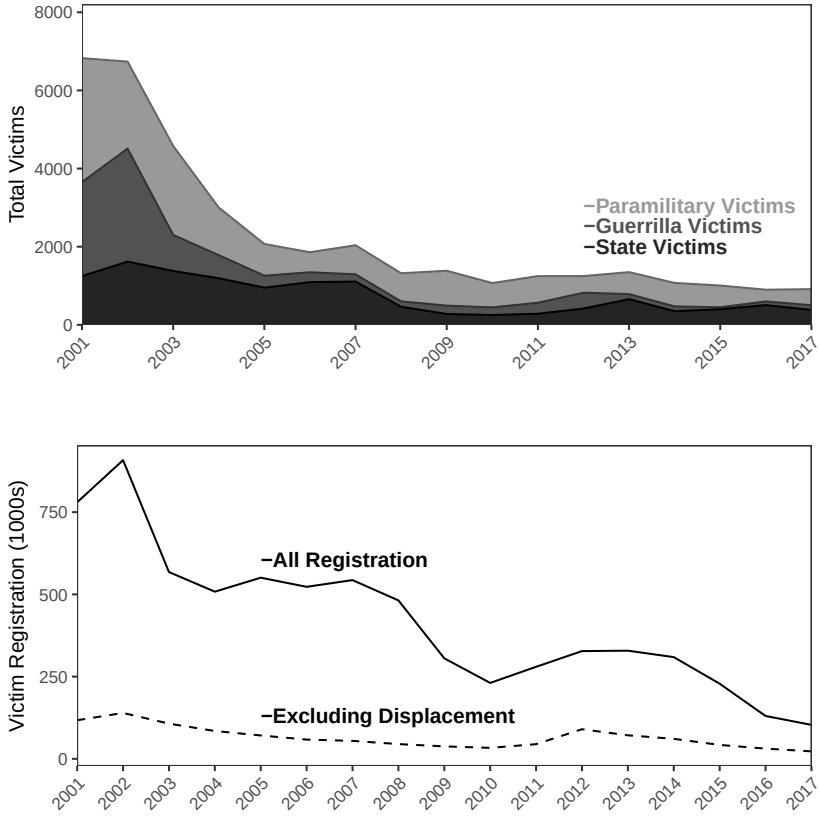
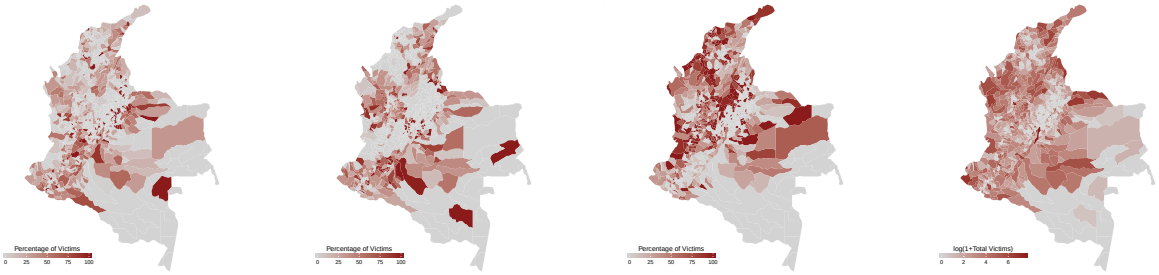


Figure A1: Trends in Victimization and Registration over Time. Each panel represents country-level aggregated trends in key variables over time. Data on victimization are from CINEP. Victim Registration data are from Colombia's Victim Unit.



(a) State (b) Guerrilla (c) Paramilitary (d) Total

Figure A2: Spatial Variation in Victimization. The data on victimization are from CINEP. Disaggregation into responsible actors was coded by the authors. Boundaries are from WFPGeoNode.

Panel-Data Robustness Checks

In this section, we present several robustness checks to our municipality panel dataset. 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.

We might also be concerned that registration does not depend on victimization in a given year. This would be the case if many individuals who are victims 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 of victim levels by each armed group 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.

Our second approach to account for temporal dependencies is to follow the panel-matching procedure outlined in Imai *et al.* (2018) for each type of victimization as 'treatment.' This tactic creates matched sets of observations by comparing 'treated' and 'untreated' units with identical treatment histories, helping to assuage concerns related to time-varying confounding. For each estimate, the treatment variable of interest is dichotomized according to the presence or lack of victims from each type of armed group (e.g. $T = \mathbb{1}[V^G_{mt} > 0]$). Municipality-year observations are matched on prior values of that treatment (previous four years); prior levels of victim registration; current and prior levels of victimization by the remaining armed groups, and population. Findings from these estimations are presented in Appendix Figure A3. The results suggest that once we account for prior and contemporaneous levels of violence and despite there being, by construction, more victims in places where the state harmed civilians, there is no distinguishable difference in victim registration compared to places without state victims. When we repeat the same exercise for municipalities with guerrilla victims, we find that the presence of guerrilla violence against civilians causes an increase in registration of around 16.36 victims registered. This value is very close to the mean value of observed victims conditional on there being at least one victim (18.77).

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.

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)
$\chi^2 : (\beta_S = \beta_G)$	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).

* $p < .05$

** $p < .01$

*** $p < .001$

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).

* $p < .05$

** $p < .01$

*** $p < .001$

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				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	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).

* $p < .05$

** $p < .01$

*** $p < .001$

Individual Analysis Robustness Checks

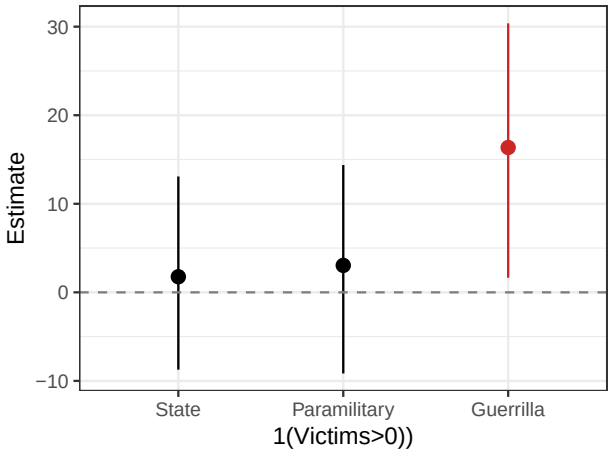


Figure A3: ATT of Victimization on Registration Excluding Displacement. This figure plots estimated Average Treatment Effects on the Treated (ATT) for each type of victimization following the procedure outlined in Imai *et al.* (2018). Vertical bars are 95% confidence intervals calculated using 1000 weighted bootstrapped samples.

In this section we present additional robustness checks to the analyses of the LAPOP survey data. In line with theoretical expectations, victimization by the state is associated with reduced trust in relevant state institutions and reduced participation.

Table A4 presents a full tabular presentation of the results presented in Figure 3. Table A5 also provides support for the link between trust and political participation more broadly. 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 institutionalized forms of political participation in the sample we consider. Table A6 presents an alternative specifiction, using ordered logit rather than OLS to account for the fact that many of our outcome variable are categorical rather than continuous. Finally, Table A7 presents the estimates used in the coefficient stability analysis.

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…"

* $p < .05$

** $p < .01$

*** $p < .001$

Table A5: Trust in the State and Political Participation

	Vote Previous (1)	Vote Next (2)	Community Group (3)
Trust Index	0.017^{*}	0.029^{***}	0.062^{***}
	(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.

* $p < .05$

** $p < .01$

*** $p < .001$

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…"

* $p < .05$

** $p < .01$

*** $p < .001$

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.
* $p < .05$
** $p < .01$
*** $p < .001$

Following Oster (2019), we 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).[4] Per Oster (2019), we identify a bound for this coefficient ($\hat{\hat{\beta}}$) 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:

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

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 A4. We find that adding controls induces a small increase in the magnitude of our effect size (effect of victimization is more negative in the controll ed regression) and R^2 increases dramatically.

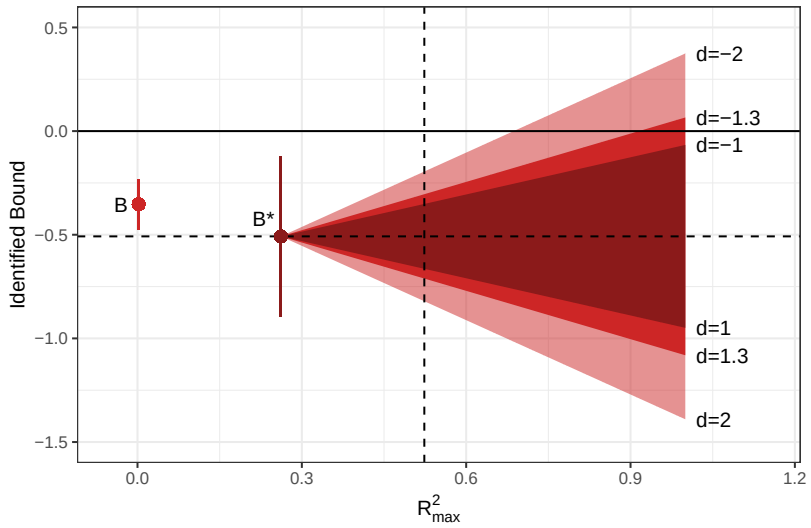


Figure A4: Coefficient Stability Bounds. This figure plots an identified set of coefficients conditional on movement in our coefficient of interest in response to observable characteristics (moving from B to B* along the Y-axis), plausible levels of R^2_{max} and the strength of selection on unobservables, 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.

[4]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.