# 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 services designed to consolidate peace after conflict? We argue that, in the aftermath of armed fighting, state-perpetrated violence leads to reduced uptake of government reconciliation policies. We leverage spatial and temporal variation in civilian victimization by perpetrator and find that in contrast to violence committed by non-state groups, violence carried out by state forces against civilians is associated with lower levels of enlistment in Colombia's state-run victim's registry. We also present survey evidence linking victimization to lower levels of trust in the government overall. Together, our analyses demonstrate that disaggregating the identity of armed actors can provide significant theoretical and empirical advances in our understanding of peacemaking and post-conflict reconstruction. In the Colombian case, the legacy of state violence leads to the systematic exclusion of certain types of victims from transitional justice and undermines trust in the institutions responsible for building durable pathways to peace. Consequently, our findings have implications both for the design of transitional justice policies, the study of the legacies of conflict on political and social outcomes, and processes of post-conflict peacebuilding.

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Participation in transitional justice and reconciliation policies can require civilians to interact with state institutions that often failed to protect them or, in many cases, perpetrated violence themselves. This simple fact creates tensions for individual civilians and for the society as a whole. Victims may be uninterested in engaging with the state, even when doing so could yield material and symbolic benefits, and the state may engage in deliberate exclusion of those it victimized in the past. For either or both of these reasons, reconciliation and reparation may remain incomplete, potentially increasing the likelihood of continued or repeat violence.

In this note, we examine this tension empirically and test whether victimization by the state leads to differential uptake of victim registration relative to victimization by non-state armed groups in the context of Colombia's five decade long civil war. We then examine what these findings, in light of the disaggregation of perpetrator, imply for our understanding of peacemaking, transitional justice, and post-conflict reconstruction. While many studies have shown that exposure to violence can increase prosociality and participation in community groups (Bauer *et al.* 2016), we examine a context where political engagement requires interacting directly with state institutions and can be subject to state consent. We hypothesize that this dynamic leads to systematically lower victim registration rates among those who experienced violence committed by state forces compared to those victimized by non-state groups.

In this endeavor, we advance the existing literature both theoretically and empirically. By disaggregating victims and their experiences by perpetrator, we highlight a pathway to create more nuanced theory that reflects the complexities of wartime and its aftermath. In considering differences by perpetrator, we also develop a better understanding of the scope of transitional justice's impacts and ability to address inequities after violence. In particular, we suggest that when the state holds responsibility for victimization, certain individuals are more likely (whether voluntarily or involuntarily) to be excluded from transitional justice.

This suggests that the legacy of violence is heterogeneous and that the potential benefits of transitional justice may accrue to only a subset of those affected by conflict.

Empirically, we leverage administrative panel data in conjunction with individual-level survey data to test our claims. This multifaceted approach allows us to capture intertemporal dynamics within and across geographic regions as well as make interpersonal comparisons between victims of different armed groups and between victims and non-victims. As our main dependent variable, we consider whether or not victims are part of the Single Registry of Victims (Registro Único de Víctimas, hereafter RUV) held by the Victim's Unit (Unidad para la atención y reparación integral a las víctimas). Inclusion in the RUV is determined via a bureaucratic procedure and those on the registry are eligible for material reparations –a transitional justice policy first implemented by the government in 2011. This outcome –inclusion in the RUV– is consequential for a number of phenomena of broader interest to political scientists and policy-makers alike, including reconciliation, democratic consolidation, and peacebuilding.

We report several key findings. From the analysis of the administrative data, we find that the observed number of *total victims* is strongly and positively correlated with recorded victim *registrations* at the municipality level. However, when we separate out and control for victimization by guerrillas and paramilitaries, victimization by the state is not associated with any contemporaneous increase in registration or any net change over a multi-year period. We then replicate this finding at the micro-level using survey data from the Latin American Public Opinion Project's Americas Barometer (LAPOP 2018). Individuals who self-report as a victim of non-state actors exhibit higher levels of registration and greater likelihood of receiving reparations compared to state victims whose registration and reparation rates are statistically indistinguishable from non-victims.

Next, we use these data to explore whether a similar pattern can be observed between victimization by the state and trust in state-run institutions. Indeed, we find that not only

are state victims less likely to participate in state-run transitional justice, they have lower levels of trust in the government and its institutions more broadly. This finding is intuitive but normatively important and consequential, as a lack of trust may undermine processes of peacebuilding and restoring the rule-of-law and could ultimately lead to conflict recurrence. And while this relationship could be a cause or consequence of reduced inclusion in transitional justice, the fact that state victims are distrusting of state institutions and excluded from transitional justice suggests that a segment of conflict victims have persistent grievances with the state. Moreover, it suggests that the current deployment of transitional justice does not assuage these grievances but instead may deepen them.

These finding have implications for how we conceive of conflict's aftermath and how we design policies meant to address the legacies of human rights violations. 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 provide services in the hope of reconciliation. With over 9 million victims registered to date,[1] this policy constitutes the most broadly utilized transitional justice policy in existence internationally. In light of this scale, our results are troubling. Our findings suggest that the construction of transitional justice programs as a state-led enterprise can lead to systematic inequalities in the provision of justice. Exclusion from transitional justice can have long-run consequences, particularly since receipt of compensation has been linked to increased levels of political engagement (Voytas 2021). More broadly, this research highlights an ongoing need to disaggregate and deconstruct 'victimhood' when considering the potential legacies of violence and pathways to enduring peace. Though we test our expectations in the Colombian case, our findings may also describe other cases of civil war, repression, and violations of international human rights and humanitarian law where state institutions hold significant responsibility.

[1] See `https://www.unidadvictimas.gov.co/`

## Perpetrator identity, participation, and trust in the state

In brief, we expect that the perpetrator of violence will shape how victims engage with peacebuilding and post-conflict institutions. In particular, we anticipate that state-led victimization will reduce trust in state institutions and lead to lower levels of participation in transitional justice programs.

Few studies directly measure engagement with such services. Instead, research often focuses on attitudes toward particular transitional justice policies and find 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 also suggests that heterogeneous conflict experiences predict 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 support for restorative justice policies. In the Spanish case, Aguilar *et al.* (2011) suggest that ideology, familial victimization, and regional context predict attitudes toward transitional justice. Samii (2013) similarly finds that partisanship shapes transitional justice preferences in postwar Burundi and Capoccia & Pop-Eleches (2020) suggest that citizens of West Germany respond to lenient sentences among their coethnics whose guilt is contested with higher levels of support for democracy. And in Colombia, Tellez (2019) finds that exposure to conflict significantly influences whether Colombians support the peace process and granting concessions to armed actors.

We extend this literature in two ways. First, we extend the notion that differences in victimization matter for subsequent political attitudes and behavior and argue that the identity of the perpetrator is an important dimension of conflict experiences. Second, we complement data on reported attitudes with a measure of behavioral engagement with transitional

justice.

Among conflict victims, the identity of the perpetrator is a particularly important variable that shapes subsequent political engagement and attitudes. When violence was perpetrated by non-state groups, we expect transitional justice participation to be relatively straightforward, particularly when such participation is incentivized by the offer of a material benefit (e.g. in the case of reparations). But when a significant portion of violence was perpetrated by the state, the dynamics of transitional justice become more complicated. Participation requires identifying oneself and engaging with representatives of the same entity responsible for one's abuses. It may also require victims to denounce their perpetrator, a task that may be perceived as risky or uncomfortable when the denunciation occurs before a party affiliated with the entity being denounced. Finally, when participation is contingent on state approval, which is often the case with transitional justice policies established by the government, there may be incentives for the state to reject victims' attempt to participate. This is likely to apply when participation requires victims to identify their perpetrator and have their testimony corroborated by state entities involved in providing transitional justice. For victims of the state, state entities may be reluctant to validate victim accounts because verification implicitly acknowledges and recognizes state guilt. As a result, these victims will be unable to participate fully in transitional justice. For these reasons, we advance our first hypothesis:

- *Hypothesis 1:* State-led victimization will be associated with lower levels of transitional justice engagement compared to non-state victimization.

This pattern may in part be caused by, and also contribute to, lower levels of trust in the state. As De Juan & Pierskalla (2016) point out, it is especially in fragile and conflict affected 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 the risk of conflict recurrence (Sacks & Larizza 2012; Hutchison & Johnson 2011). 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 the particular actors and institutions perpetrating or failing to prevent violence. This effect has been observed in response to militia violence in Mali (Gates & Justesen 2013), drug related violence in Mexico (Ishiyama *et al.* 2018), and civil war violence in Nepal (De Juan & Pierskalla 2016). Kreutz & Nussio (2019) suggest that in the Colombian case, the government's policy of extradition to the United States eroded trust among former combatants and Kaplan & Nussio (2018) highlight how low levels of trust in government can fuel recidivism.

Our analysis examines this question in the context of victims of conflict. Overall, we expect that victims of state-perpetrated violence will have lower trust in the state. When violence is perpetrated by non-state entities, such as the paramilitaries 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.

Together, these expectations lead us to our second hypothesis which we examine using individual-level survey data in Colombia:

- *Hypothesis 2:* Victimization will decrease trust in the state, and this effect will be strongest among those who are victimized by the state.

Trust in state institutions is important in its own right, but also illuminates some of the

potential mechanisms and impacts of unequal access to transitional justice and potentially, other arenas of citizen engagement. In recent work, scholars have posited the idea that certain subsets of the population are more or less likely to claim their rights from the state, in contexts ranging from rural India to immigrants in the US (Kruks-Wisner 2018; Nielsen 2000; Abrego 2008). While some individuals are aware of their rights and utilize the state to help realize them, others are suspicious and distrusting of government solutions (Ewick & Silbey 1998). The same logic can be extended to victim populations in post-conflict contexts who differentially claim benefits established as a reparative measure for their harms. As suggested by the literature on trust and civic participation generally (Levi & Stoker 2000) and on compliance with regulation (e.g. taxation) (Fjeldstad 2004), citizens' decisions to comply and participate often rests on the assumption that the government is trustworthy. In post-conflict settings, as mentioned above, mistrust can be rampant. When victims fear and distrust the state, even the financial incentives associated with some transitionatice may prove incapable of generating participationl justice policies may not be enough to overcome tomay prcipation and inclusionfor some.

Regardless of the pathway, the observed systematic under-representation of a particular subset of victims from justice constitutes an issue that is at once normatively important, theoretically informative, and indicative of an under-recognized failure of public policy in potentially fragile contexts. Our current note focuses on highlighting the high-level patterns between the identity of perpetrators, levels of trust, and inclusion in transitional justice. Further research will be required to provide causal weight to either proposed pathway and to inform potential policy responses.

## Conflict and Victimization in Colombia

We expect the logics outlined above to extend to a wide-range of conflict and post-conflict settings internationally, particularly those where the state carried out violence. In these cases, we believe that similar dynamics may create barriers and preclude certain victims from accessing transitional justice. In fact, international bodies recognize these barriers. Though international guidelines established by the UN call for reducing inconveniences placed on victims,[2] they still exist in practice both in Colombia and beyond. These obstacles, we argue, can be particularly prohibitive for those abused by the state, who must encounter their perpetrator (when transitional justice is state-run) as a prerequisite to participate.

As Figure 1 shows, reparations policies have been administered at least 41 times between 1978 and 2007 in countries transitioning to peace or democracy (Olsen *et al.* 2010). Data on reparations during conflict count 154 reparations processes between 1946 and 2011 (Loyle & Binningsbø 2018). They havaaalso becominge an increasingly common provision in peace agreements (see lower panel in Figure 1). Beyond compensation, tresults we find and mechanisms we suggest are relevant for other commonly-implemented transitional justice policies such as truth commissions and trials. Reparations, trials, and truth commissions are all typically premised on victim participation orand testimony and require state cooperation, all of which can be complicated for any victim and for victims of state forcesa state-victim in particular. In this sense, tfind may have implications both beyond Colombia and beyond the specific coext of compensation. id, it is worth noting is worth benoting cognizant of several important dimensions of ia's policy is unique in its size and scope, which could perhaps contribute to the idea that registering is futile (few victims have been paid) but could alternatively increase the policy's visibility, making registration more common. Though its size is atypical, Colombia's reparations program consequently has a large number of stakeholders, making understanding its workings all the more important. Second, the RUV was established prior to Colombia's peace deal, meaning that victims could register while conflict ws ongoing, though it has remained open after the signing of the agreementpeace deal

[2]See `https://www.ohchr.org/en/professionalinterest/pages/remedyandreparation.aspx`.

signing. While Loyle & Binningsbø (2018) have documented that reparations during conflict (and other processes including trials and truth commissions) are in fact quite common globally,Future work will examine how levels of violence affect uptake of during- and post-conflict justice.

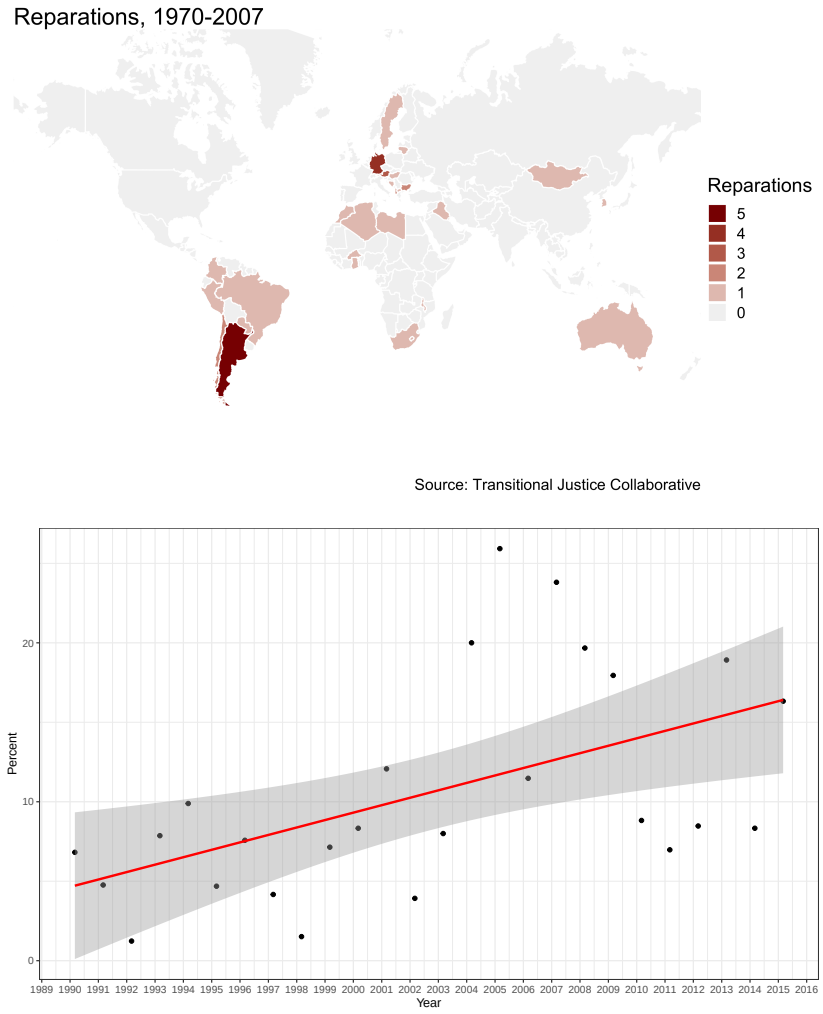



Figure 1: Top panel presents prevalence of reparations globally from 1970-2007 with data from the Transitional Justice Collaborative. Bottom panel shows the percentage of peace agreements from 990 to 2000 that contain provisions for material reparations. Data on peace agreements are from the P-X Peace agreement database.

Despite these caveats, contemporary Colombia provides an ideal environment to test these initial expectations. 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 provides additional confidence in our results, as it permits us to rule out many relevant omitted variables that might drive the observed relationships. Additionally, the duration and intensity of Colombia's conflict make the fear of conflict recurrence particularly salient. Peace in Colombia is not only desirable domestically, but internationally, as evinced by large sums of international aid supporting the peace process. Thus, understanding the impacts of policies designed to prevent such reversions is critical. In this section, we further describe the Colombian case and justify our dependent variable: inclusion as a victim in the RUV held by 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 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 can contact their closest Victim's Unit office (there are over 200) and request to join the RUV virtually, via mail, in-person, or over the phone. To join, victims must present the required documentation (identification and two witness declarations). At that point, the Victims' Unit verifies a victim's application by cross-referencing her account with databases comprised within the National Information Network for Care and Reparation for Victims.[3] Victims are eligible irregardless of the identity of their perpetrator, and according to the 2011 Victim's Law:

> "In the event that the victim mentions the name or names of the potential perpetrator of the damage he claims to have suffered in order to access the measures of attention, assistance and reparation provided for in this law, this name or names shall in no case be included in the administrative act by which the registration is granted or denied. (Ley de Víctimas y Restitución de Tierras, Law 1448)"

Once reviewed, the state then issues a mailed administrative notice of approval or rejection within 120 days.

After victims initiate the reparation process, they receive a personalized note from the government confirming inclusion in the RUV and expressing its commitment to reparation. For many victims, this is the sole response they have received, as only 11.1% of victims had been paid as of April 2020 (Unidad para las Victimas 2020).[4] We are concerned primarily with an antecedent to compensation: inclusion in the RUV.

[3]According to the Victims' Unit website, this network culls information from national and territorial government, non-governmental, and private organizations' victim databases. See `https://www.unidadvictimas.gov.co/es/direccion-de-registro-y-gestion-de-la-informacion/red-nacional-de-informacion-rni/37825`.

[4]Vulnerable victims (because of age, illness, socioeconomic status, and disability) are prioritized. The amount of compensation varies according to the crime and relationship to victim, but ranges between 10 and 40 times the minimum monthly wage. See `https://www.unidadvictimas.gov.co/es/indemnizacion/`.

# 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 have reduced 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 recorded incidents of 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[5] 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 identified as being 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.[6] An event was coded as involving the state if the associated actors

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

[6]Scholars of the Colombian conflict will be familiar with the documented relationships between the paramilitaries and several arms of the Colombian government. The connection between these two groups likely shapes how victims assess the state and its services –particularly those related to the conflict. However, states often sanction paramilitaries for the express purpose of giving themselves plausible deniability in instances of violence. Moreover, the explicit connection between high level political actors and the paramilitaries was relatively less well known until the onset of the 'Parapolitics' scandal. After this scandal, the state underwent a highly publicized campaign of arrests and prosecutions of those involved. Thus, while this relationship is certainly important, we do not have strong expectations regarding whether victims will treat paramilitaries as state or non-state actors in our framework. Empirically, we code them as a separate category and let the data speak for themselves.

included any of the following: "Estado Colombiano," "Fiscalia," "Fuerza Publica," "Fuerza Aera," "Fuerzas Militares," "Policía," "Armada," "CTI," "DAS", "DIJIN," "GAULA," "INPEC", "SIJIN," and "Ejercito." We conduct a similar exercise for the many guerrilla and paramilitary groups and aggregate the total number of victims in events attributed to each category of armed actor to the municipality-year level. Consequently, some events are characterized as having multiple armed groups involved. For instances involving state actors, 74% are *only* attributed to state actors. Such instances often involve instances of human rights violations such as arbitrary detention, extrajudicial killing, sexual assault, or torture.[7] 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, and because the coverage of non-displacement registration varies over time, we also test our hypotheses using both the total number of registration and excluding individuals whose main grievance is displacement. A break down of injury recorded in the registry can be found in Figure 2.

The total number of *registered* victims often outnumbers *observed* victims from the CINEP data because the victim registry can also include individuals indirectly affected by the conflict such as a direct victims' spouse, siblings, and other family members. Nevertheless, we see in Figure 3 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, is associated with levels of victim registration in a municipality ($m$) in year ($t$) controlling

[7]For example, one state associated event is described: "Troops attached to the Cisneros Battalion under the command of Lieutenant Colonel Carlos Eduardo Mora Gómez, Brigade 8 under the command of Brigadier General Jairo Antonio Herazo Marzola, National Army, executed the two farmers in the Llanitos de Guaralá neighborhood." Original: "Tropas adscritas al Batalón Cisneros al mando del Teniente Coronel Carlos Eduardo Mora Gómez, Brigada 8 al mando del Brigadier General Jairo Antonio Herazo Marzola, Ejército Nacional, ejecutaron a los dos campesinos, en el barrio Llanitos de Guaralá."

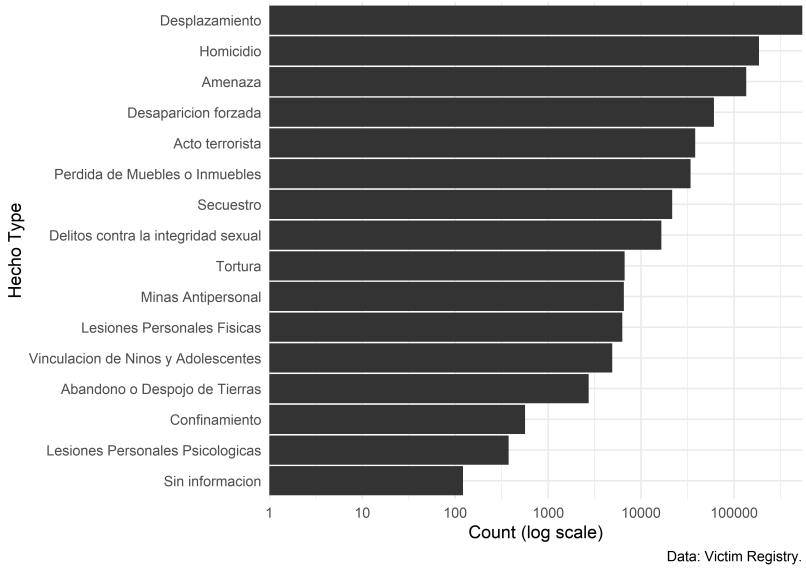



Figure 2: Victimization and Registration

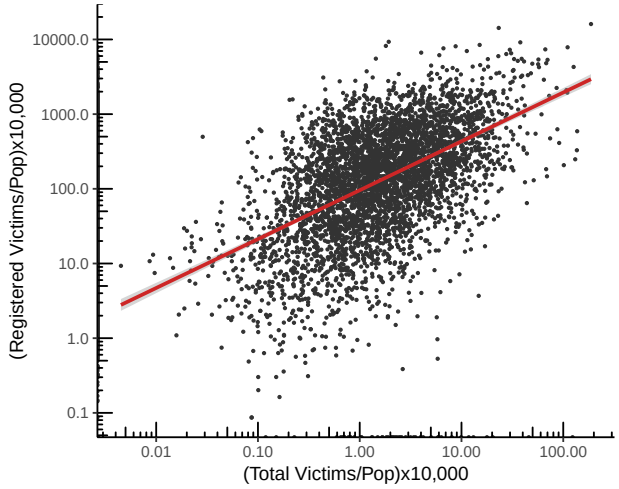


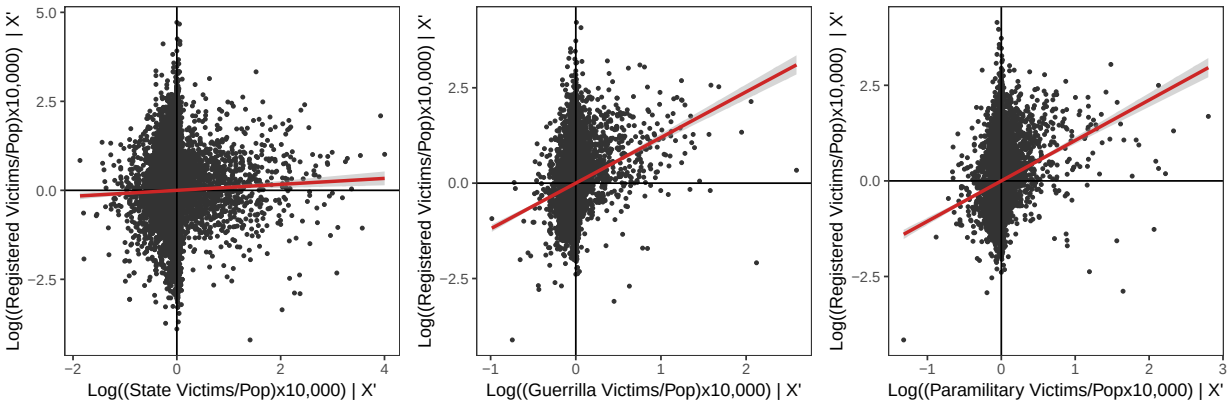

Figure 3: Victimization and Registration

for time-invariant municipality-specific characteristics ($\delta_m$), and year-specific fixed-effects ($\gamma_t$). We also ral 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.

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

To examine Hypothesis 2, we test the null hypothesis of whether the coefficient on the number of government victims ($\beta_S$) is the same as the coefficient on the number of guerrilla victims ($\beta_G$). Our theoretical framework predicts that $\beta_S$ will be substantively and significantly less than $\beta_G$. Figure 4 and Table **??** 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. 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.

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 levels and can be positive or negative depending on the specification. We use Chi-squared tests to compare $\beta_S$ and $\beta_G$. The relationship between state victims




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

Figure 4: 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 **??**. 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.

and registrations is significantly less than the relationship between guerrilla victims and registrations at the .1% level. This finding provides strong 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 in transitional justice policies 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[8] as well as basic demographic information (sex, age, and a quadratic age term) using the 2016 data. Column (1) of Table 1 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.

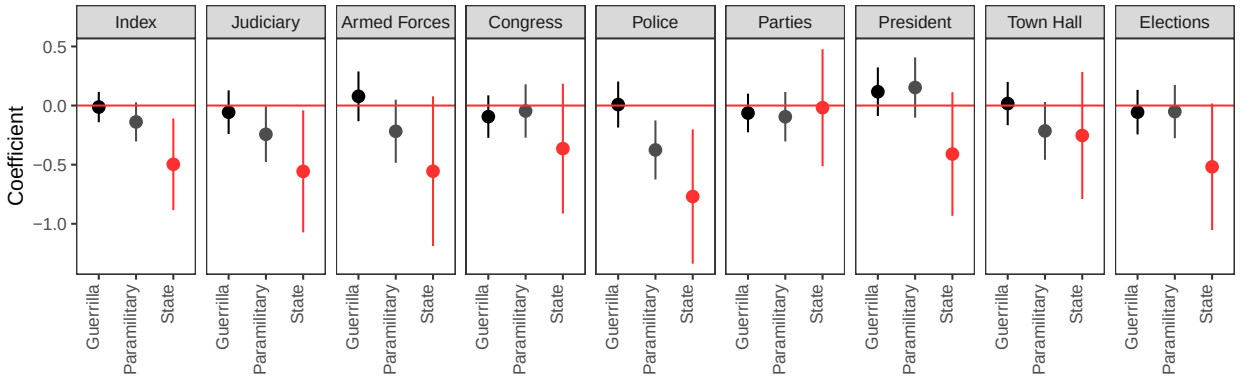



Figure 5: Individual Victimization and Trust in State Institutions. Each facet displays coefficients and 95% confidence intervals estimated in Table A4. Only coefficients for victimization by the state, guerrillas, and paramilitaries are presented, but specifications additionally control for gender, 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.

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) regressions controlling for demographic factors and including primary sampling unit (PSU)-Wave

[8]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 1: Individual Victimization, Registration, and Reparations

| | Trust Index (1) | Register as Victim (2) | Receive Reparations (3) |
|---|---|---|---|
| **Victim of:** | | | |
| State | −0.501* | 0.044 | −0.039 |
| | (0.243) | (0.069) | (0.036) |
| Guerrilla | −0.105 | 0.175*** | 0.033 |
| | (0.090) | (0.028) | (0.017) |
| Paramilitary | 0.009 | 0.174*** | 0.093** |
| | (0.116) | (0.041) | (0.034) |
| Other | 0.032 | 0.015 | −0.011 |
| | (0.146) | (0.041) | (0.031) |
| **Demographics:** | | | |
| Female | −0.010 | 0.020 | 0.027* |
| | (0.072) | (0.014) | (0.011) |
| Age | −0.024 | −0.000 | 0.002 |
| | (0.015) | (0.003) | (0.002) |
| $Age^2$ | 0.000* | 0.000 | −0.000 |
| | (0.000) | (0.000) | (0.000) |
| Eth: Indigenous | 0.046 | −0.002 | 0.024 |
| | (0.281) | (0.064) | (0.055) |
| Eth: Mestizo | −0.042 | −0.015 | −0.016 |
| | (0.162) | (0.043) | (0.035) |
| Eth: Mulatto | −0.119 | −0.011 | 0.019 |
| | (0.206) | (0.053) | (0.048) |
| Eth: Other | −0.272 | −0.033 | −0.009 |
| | (0.235) | (0.059) | (0.041) |
| Eth: White | −0.053 | −0.026 | 0.006 |
| | (0.172) | (0.043) | (0.036) |
| Years Educ. | −0.026* | −0.007** | −0.002 |
| | (0.011) | (0.002) | (0.002) |
| Unemployed | −0.035 | 0.010 | 0.005 |
| | (0.102) | (0.023) | (0.017) |
| Married | −0.028 | 0.008 | 0.018 |
| | (0.078) | (0.017) | (0.012) |
| $R^2$ | 0.292 | 0.379 | 0.234 |
| Num. obs. | 1470 | 1471 | 1471 |
| N Clusters | 251 | 251 | 251 |

*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.

fixed-effects and standard errors clustered at the PSU-Wave level.[9] Figure 5 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 includes 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.

We want to highlight that these results are associational. While we control for a limited set of pre-treatment, observable characteristics, 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 this relationship is not particularly sensitive to selection on unobservables.

[9] 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.

## Potential pathways

As noted, the purpose of this research note is to shine light on high-level disparities in transitional justice uptake. We described two possible pathways behind this phenomenon. The first is a demand side mechanism wherein victim experiences of violence committed by the state lead to lower trust in state institutions and, therefore, lower participation in state-run transitional justice. The second is from the supply side: the state may –deliberately or otherwise– de-prioritize the registration of individuals who declare to be victims of the state. While this note cannot adjudicate between these two mechanisms empirically, this section expands on these ideas for the purposes of guiding future research efforts.

### *Selection into reporting of state-led victimization*

Victims may choose not to register as a victim because of a lack of state trust and an unwillingness to engage with it. This decision could be a rebuke of the policy (Cronin-Furman & Krystalli 2020; Sandoval Rojas 2015). It may also be generated by fear, which is a conclusion reached by the Red Cross in Colombia. They write that underreporting arises because those affected "have fear of speaking, a lack of resources, or are unaware of the options for recourse" (Comite Internacional de la Cruz Roja 2017). It is easy to imagine how speaking out would be particularly tolling for those who were victimized by the state, the very entity to which they must report their abuses. Remaining silent in this context can be a way for victims to protect themselves, even if silence means exclusion from potential material benefits (Riano-Alcalá & Baines 2011). Additionally, if experiences of violence lead to lower levels of trust in the state more generally, it may be the case that victims do not believe the state will uphold its end of the bargain. Thus, from their perspective, registration may constitute a 'no-win' scenario in which they must make themselves more visible to the state with little expectation of actual compensation.

### *Selective approval of RUV applications*

As we discussed earlier, the 2011 Victims' Law prohibits exclusion on the basis of a perpetrator's identity. Still, an alternative explanation is that state victims do report, but are excluded from the RUV through the Victim's Unit approval process. This could generate both the lower inclusion rates we document and lower levels of state trust. Though it is difficult to document deliberate exclusion of state victims, it is worth noting that the Victim's Unit has received hundreds of thousands of *tutelas*, or legal measures taken by citizens when their rights are violated or threatened (El Tiempo 2016). Many of these are initiated from cases rejected by the Victim's Unit. Disappointment in the experience is a common response, even when individuals *are* included in the RUV (Pham *et al.* 2016; Cronin-Furman & Krystalli 2020). The Victim's Unit has been criticized by public officials suggesting that it is not on the side of victims, that it is the source of civic distrust among victims, and that it fails to comply to its mission of assisting all victims (El Tiempo 2019). Though we cannot and do not wish to assert that the state is deliberately excluding victims, we note that this is an additional possible cause for lower levels of enlistment among state victims. Either or both of the two possible explanations reviewed are consistent with the empirical analysis.

### *Registration capacity*

An additional supply-side mechanism could be due to limits on the capacity to register victims that are correlated with incidences of state violence. For instance, if lower state capacity is correlated with both higher incidences of civilian victimization *and* lower registration capacity, then we would expect to see lower victim registration when controlling for instances of state-led violence. However, in our robustness checks, we control for municipality-level government spending, a weak proxy for state capacity, and find no systematic correlation with registration rates.

# 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. 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 engagement in peacebuilding and post-conflict institutions.

This analysis has implications for the study and design of transitional justice policies which are becoming more and 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; Balcells *et al.* 2020). 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. Consequently, 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 in the process so that all victims can benefit from transi-

tional justice measures without having to confront and engage with the entity responsible for their victimization. Alternatively, states and other organizations should explicitly cater to victims of state violence, recognizing that distrust likely exists. Involving victims like these 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.

We examine one dimension of heterogeneity: identity of the perpetrator. But victims in Colombia and elsewhere differ along many additional dimensions, including gender, socioeconomic status, level of organization with others, and severity of violence (Rettberg 2015). These victim-level characteristics may also generate differential rates of inclusion in transitional justice policies. By considering perpetrator identities, we hope to encourage future research to disaggregate victim populations and examine inequalities in the reach of policies meant to prevent repeat violence.

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# A. Supplementary Information

## Descriptive Information

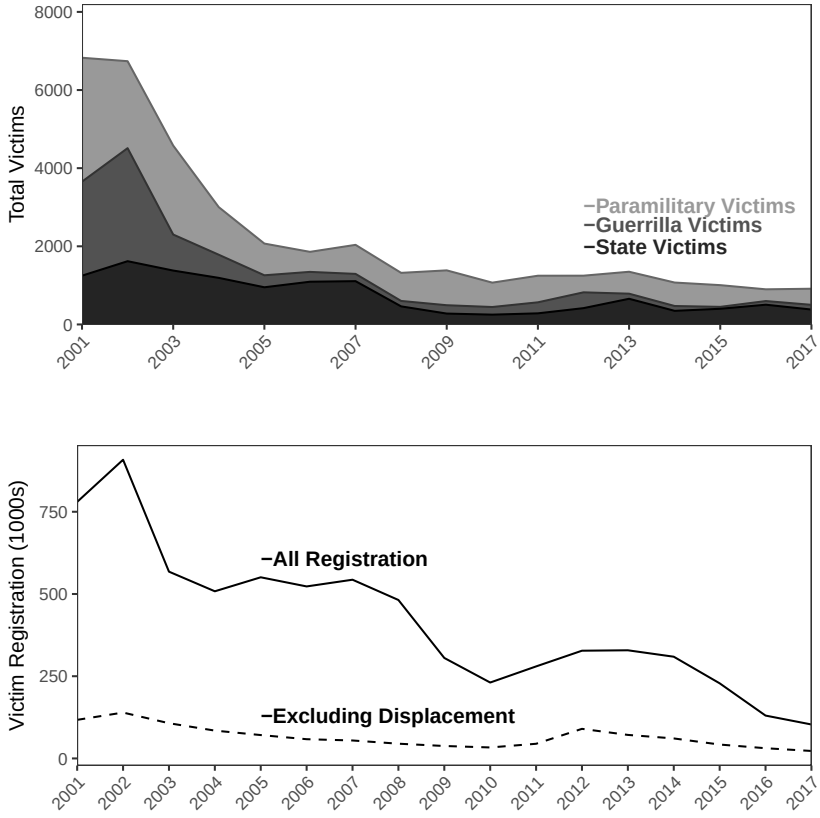
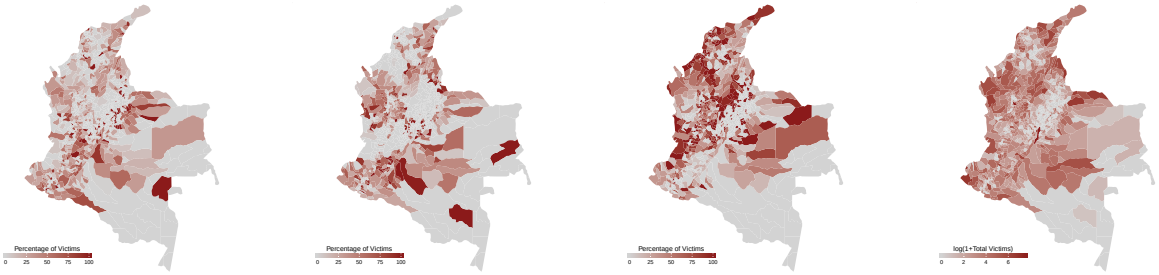



Figure A1: Trends in Victimization and Registration over Time. Each panel represents country-level aggregated trends in key variables over time. Data on victimization are from CINEP. Victim Registration data are from Colombia's Victim Unit.




(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 **??** 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 **??** 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 $\chi^2$ ($\beta_S = \beta_G$) presents the $\chi^2$ of a linear hypothesis test of whether the coefficient on the state victims ($\beta_S$) is the same as the coefficient on the variable measuring victims of guerrilla forces ($\beta_G$).

* $p < .05$

** $p < .01$

*** $p < .001$

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) |
| $\chi^2$ : $(\beta_S = \beta_G)$ | 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 $\chi^2$ $(\beta_S = \beta_G)$ presents the $\chi^2$ of a linear hypothesis test of whether the coefficient on the state victims $(\beta_S)$ is the same as the coefficient on the variable measuring victims of guerrilla forces $(\beta_G)$.

* $p < .05$

** $p < .01$

*** $p < .001$

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 (1) | Per Cap. (2) | log($y$) (3) | arcsinh($y$) (4) | Raw (5) | Per Cap. (6) | log($y$) (7) | arcsinh($y$) (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) |
| $\chi^2$ : $(\beta_S = \beta_G)$ | 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 (1) | Per Cap. (2) | log($y$) (3) | arcsinh($y$) (4) | Raw (5) | Per Cap. (6) | log($y$) (7) | arcsinh($y$) (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) |
| $\chi^2$ : $(\beta_S = \beta_G)$ | 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 $\chi^2$ $(\beta_S = \beta_G)$ presents the $\chi^2$ of a linear hypothesis test of whether the coefficient on the state victims $(\beta_S)$ is the same as the coefficient on the variable measuring victims of guerrilla forces $(\beta_G)$.

* $p < .05$

** $p < .01$

*** $p < .001$

# Individual Analysis Robustness Checks

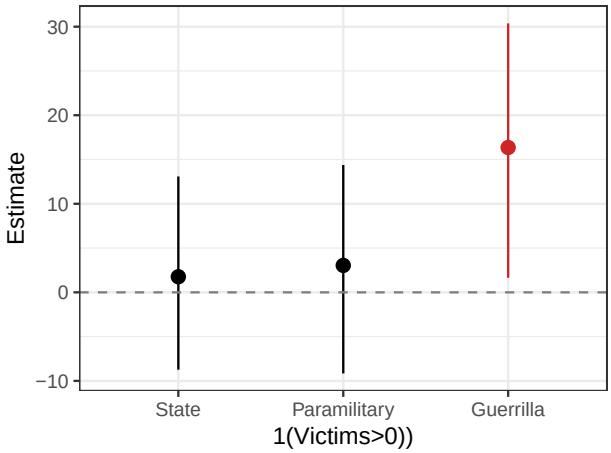



Figure A3: ATT of Victimization on Registration Excluding Displacement. This figure plots estimated Average Treatment Effects on the Treated (ATT) for each type of victimization following the procedure outlined in Imai *et al.* (2018). Vertical bars are 95% confidence intervals calculated using 1000 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 (1) | Judicial (2) | Armed Forces (3) | Congress (4) | Police (5) | Parties (6) | President (7) | Town Hall (8) | Elections (9) |
| **Victim of:** | | | | | | | | | |
| State | −0.497* | −0.557* | −0.555 | −0.364 | −0.769* | −0.018 | −0.410 | −0.253 | −0.518 |
| | (0.195) | (0.263) | (0.315) | (0.286) | (0.294) | (0.254) | (0.261) | (0.296) | (0.266) |
| Guerrilla | −0.013 | −0.056 | 0.078 | −0.093 | 0.009 | −0.063 | 0.117 | 0.017 | −0.056 |
| | (0.062) | (0.089) | (0.104) | (0.086) | (0.100) | (0.079) | (0.105) | (0.090) | (0.099) |
| Paramilitary | −0.138 | −0.243* | −0.217 | −0.046 | −0.375** | −0.094 | 0.153 | −0.215 | −0.051 |
| | (0.078) | (0.118) | (0.134) | (0.115) | (0.119) | (0.105) | (0.123) | (0.116) | (0.107) |
| Other | 0.003 | −0.100 | −0.017 | 0.025 | 0.034 | −0.162 | 0.083 | 0.040 | −0.079 |
| | (0.105) | (0.148) | (0.165) | (0.144) | (0.151) | (0.132) | (0.155) | (0.147) | (0.146) |
| **Demographics:** | | | | | | | | | |
| Female | −0.047 | 0.006 | −0.405*** | 0.115 | 0.147* | −0.039 | −0.069 | 0.085 | −0.174* |
| | (0.049) | (0.066) | (0.075) | (0.066) | (0.069) | (0.058) | (0.073) | (0.072) | (0.068) |
| Age | −0.011 | −0.009 | 0.004 | −0.016 | −0.007 | −0.030** | −0.017 | −0.004 | −0.019 |
| | (0.010) | (0.014) | (0.014) | (0.014) | (0.014) | (0.012) | (0.015) | (0.013) | (0.014) |
| $Age^2$ | 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) |
| Eth: Indigenous | −0.201 | −0.217 | −0.254 | −0.298 | −0.271 | −0.467* | −0.263 | −0.198 | 0.075 |
| | (0.184) | (0.245) | (0.299) | (0.252) | (0.286) | (0.223) | (0.291) | (0.257) | (0.234) |
| Eth: Mestizo | −0.057 | −0.254 | 0.147 | −0.072 | −0.028 | −0.261 | −0.187 | −0.093 | 0.110 |
| | (0.120) | (0.167) | (0.188) | (0.152) | (0.179) | (0.164) | (0.189) | (0.159) | (0.155) |
| Eth: Mulatto | −0.033 | −0.415 | 0.216 | −0.142 | −0.071 | 0.073 | −0.149 | 0.175 | 0.222 |
| | (0.161) | (0.222) | (0.247) | (0.198) | (0.229) | (0.226) | (0.241) | (0.227) | (0.208) |
| Eth: Other | −0.318 | −0.482 | −0.099 | −0.197 | −0.672* | −0.379 | −0.148 | −0.258 | −0.228 |
| | (0.190) | (0.263) | (0.286) | (0.262) | (0.284) | (0.241) | (0.251) | (0.237) | (0.262) |
| Eth: White | −0.064 | −0.205 | 0.104 | −0.182 | −0.042 | −0.337 | −0.122 | −0.046 | 0.032 |
| | (0.126) | (0.178) | (0.198) | (0.161) | (0.189) | (0.171) | (0.194) | (0.166) | (0.163) |
| Years Educ. | −0.033*** | −0.033** | −0.037** | −0.048*** | −0.024* | −0.013 | −0.047*** | −0.032** | −0.014 |
| | (0.007) | (0.011) | (0.012) | (0.010) | (0.011) | (0.010) | (0.012) | (0.011) | (0.010) |
| Unemployed | −0.144 | −0.120 | −0.042 | −0.174 | −0.047 | −0.260* | −0.203 | −0.287* | −0.193 |
| | (0.079) | (0.114) | (0.135) | (0.112) | (0.127) | (0.101) | (0.123) | (0.120) | (0.111) |
| Married | −0.028 | −0.057 | 0.061 | −0.081 | −0.087 | −0.009 | −0.047 | 0.049 | −0.036 |
| | (0.053) | (0.071) | (0.085) | (0.074) | (0.078) | (0.067) | (0.082) | (0.075) | (0.080) |
| $R^2$ | 0.280 | 0.251 | 0.230 | 0.258 | 0.232 | 0.235 | 0.294 | 0.291 | 0.240 |
| Num. obs. | 2927 | 2897 | 2902 | 2846 | 2915 | 2898 | 2911 | 2890 | 2895 |
| N Clusters | 502 | 502 | 502 | 502 | 502 | 502 | 502 | 502 | 502 |

*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 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** | 0.021** | 0.032*** | 0.064*** |
| | (0.008) | (0.006) | (0.013) |
| **Demographics:** | | | |
| Female | 0.011 | −0.009 | −0.059* |
| | (0.016) | (0.014) | (0.028) |
| Age | 0.044*** | 0.009*** | 0.008 |
| | (0.003) | (0.003) | (0.006) |
| $Age^2$ | −0.000*** | −0.000** | −0.000 |
| | (0.000) | (0.000) | (0.000) |
| Eth: Indigenous | 0.099 | 0.086 | 0.013 |
| | (0.068) | (0.046) | (0.101) |
| Eth: Mestizo | 0.091* | 0.007 | 0.086 |
| | (0.045) | (0.026) | (0.063) |
| Eth: Mulatto | 0.157** | 0.021 | 0.040 |
| | (0.059) | (0.035) | (0.102) |
| Eth: Other | 0.146* | 0.014 | 0.084 |
| | (0.064) | (0.040) | (0.101) |
| Eth: White | 0.082 | 0.028 | 0.112 |
| | (0.046) | (0.028) | (0.068) |
| Years Educ. | 0.016*** | 0.009*** | 0.015*** |
| | (0.003) | (0.002) | (0.005) |
| Unemployed | 0.043 | 0.010 | 0.010 |
| | (0.033) | (0.020) | (0.056) |
| Married | 0.067** | −0.019 | 0.011 |
| | (0.021) | (0.015) | (0.031) |
| $R^2$ | 0.328 | 0.522 | 0.263 |
| Num. obs. | 2895 | 2754 | 2918 |
| N Clusters | 502 | 502 | 502 |

*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 | −0.695*** | −0.637*** | −0.604*** | −0.864*** | −0.097*** | −0.608*** | −0.244*** | −0.688*** |
| | (0.008) | (0.007) | (0.006) | (0.009) | (0.007) | (0.008) | (0.009) | (0.008) |
| Guerrilla | −0.039 | 0.071 | −0.131 | 0.032 | −0.112 | 0.079 | 0.012 | −0.117 |
| | (0.086) | (0.088) | (0.088) | (0.086) | (0.091) | (0.090) | (0.087) | (0.089) |
| Paramilitary | −0.302** | −0.215 | −0.054 | −0.455*** | −0.122 | 0.138 | −0.278* | −0.080 |
| | (0.109) | (0.110) | (0.111) | (0.107) | (0.115) | (0.111) | (0.109) | (0.110) |
| Other | −0.097 | −0.018 | 0.034 | 0.044 | −0.204 | 0.129 | 0.045 | −0.155 |
| | (0.135) | (0.134) | (0.136) | (0.132) | (0.139) | (0.137) | (0.135) | (0.136) |
| **Demographics:** | | | | | | | | |
| Female | 0.009 | −0.466*** | 0.142* | 0.168** | −0.070 | −0.079 | 0.071 | −0.229*** |
| | (0.065) | (0.065) | (0.067) | (0.065) | (0.068) | (0.066) | (0.066) | (0.066) |
| Age | −0.012 | −0.001 | −0.024 | −0.005 | −0.042 | −0.017 | −0.002 | −0.033 |
| $Age^2$ | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Eth: Indigenous | −0.276** | −0.308** | −0.388*** | −0.316** | −0.511*** | −0.252* | −0.171 | 0.093 |
| | (0.102) | (0.109) | (0.100) | (0.106) | (0.107) | (0.107) | (0.100) | (0.099) |
| Eth: Mestizo | −0.292*** | 0.103 | −0.094 | −0.045 | −0.276*** | −0.199** | −0.080 | 0.097 |
| | (0.065) | (0.067) | (0.067) | (0.066) | (0.068) | (0.067) | (0.067) | (0.066) |
| Eth: Mulatto | −0.535*** | 0.181*** | −0.160*** | −0.070* | 0.113*** | −0.126*** | 0.206*** | 0.284*** |
| | (0.027) | (0.033) | (0.028) | (0.034) | (0.033) | (0.033) | (0.027) | (0.031) |
| Eth: Other | −0.571*** | −0.101*** | −0.268*** | −0.849*** | −0.645*** | −0.212*** | −0.295*** | −0.475*** |
| | (0.020) | (0.019) | (0.022) | (0.020) | (0.018) | (0.020) | (0.023) | (0.019) |
| Eth: White | −0.257*** | 0.044 | −0.253*** | −0.037 | −0.406*** | −0.130 | −0.040 | 0.017 |
| | (0.075) | (0.076) | (0.077) | (0.075) | (0.078) | (0.076) | (0.075) | (0.076) |
| Years Educ. | −0.037*** | −0.048*** | −0.059*** | −0.029*** | −0.016* | −0.051*** | −0.039*** | −0.014 |
| | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) |
| Unemployed | −0.091 | −0.062 | −0.168 | −0.035 | −0.297* | −0.166 | −0.346** | −0.192 |
| | (0.111) | (0.111) | (0.112) | (0.109) | (0.118) | (0.113) | (0.110) | (0.113) |
| Married | −0.039 | 0.070 | −0.092 | −0.106 | −0.018 | −0.024 | 0.049 | −0.038 |
| | (0.067) | (0.068) | (0.068) | (0.066) | (0.070) | (0.069) | (0.068) | (0.068) |
| AIC | 11083.031 | 11244.332 | 10599.193 | 11420.322 | 9681.817 | 10715.830 | 11052.164 | 10706.664 |
| Log Likelihood | -5019.516 | -5100.166 | -4777.596 | -5188.161 | -4318.909 | -4835.915 | -5004.082 | -4831.332 |
| Deviance | 10039.031 | 10200.332 | 9555.193 | 10376.322 | 8637.817 | 9671.830 | 10008.164 | 9662.664 |
| Num. obs. | 2897 | 2902 | 2846 | 2915 | 2898 | 2911 | 2890 | 2895 |

*Note:* Robust standard errors 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 | Controlled |
|---|---|---|
| | (1) | (2) |
| **Victim of:** | | |
| State | −0.353 | −0.497* |
| | (0.201) | (0.195) |
| Guerrilla | 0.030 | −0.013 |
| | (0.060) | (0.062) |
| Paramilitary | −0.063 | −0.138 |
| | (0.077) | (0.078) |
| Other | −0.085 | 0.003 |
| | (0.094) | (0.105) |
| **Demographics:** | | |
| Female | | −0.047 |
| | | (0.049) |
| Age | | −0.011 |
| | | (0.010) |
| $Age^2$ | | 0.000 |
| | | (0.000) |
| Eth: Indigenous | | −0.201 |
| | | (0.184) |
| Eth: Mestizo | | −0.057 |
| | | (0.120) |
| Eth: Mulatto | | −0.033 |
| | | (0.161) |
| Eth: Other | | −0.318 |
| | | (0.190) |
| Eth: White | | −0.064 |
| | | (0.126) |
| Years Educ. | | −0.033*** |
| | | (0.007) |
| Unemployed | | −0.144 |
| | | (0.079) |
| Married | | −0.028 |
| | | (0.053) |
| $R^2$ | 0.002 | 0.280 |
| Num. obs. | 3058 | 2927 |
| N Clusters | | 502 |

*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.

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 ($d$).[10] 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.

[10]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.

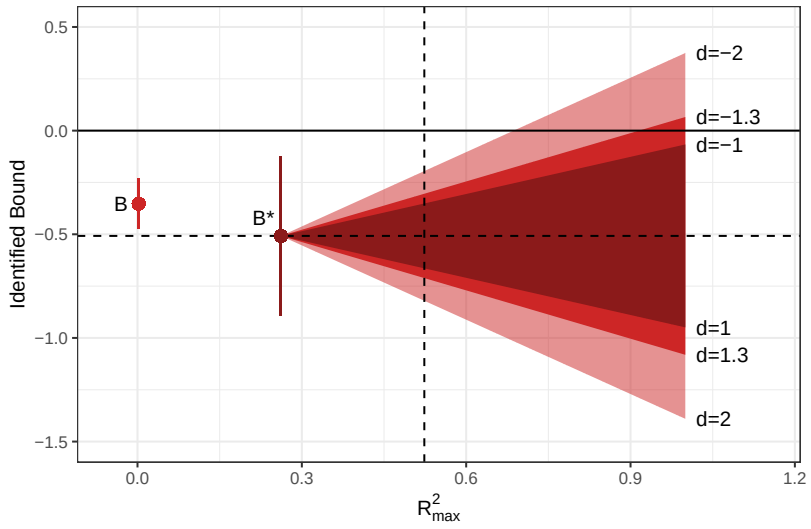



Figure A4: Coefficient Stability Bounds. This figure plots an identified set of coefficients conditional on movement in our coefficient of interest in response to observable characteristics (moving from B to B* along the Y-axis), plausible levels of $R^2_{\text{max}}$ and the strength of selection on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