The Demobilizing Effect of Violence: Evidence of Heterogeneous Exposure and Response

Supporting Information

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December 12, 2025

Contents

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Appendix A Principal Strata Framework: Proofs

This section provides formal derivations for the results in the main paper's section "A Principal Strata Framework."

A1.1 Preliminaries

Let units belong to one of four principal strata: Always Participate (A), Participate if Treated (T), Participate if Untreated (U), and Never Participate (N).
Let π_θ denote the population share of stratum θ, with $\sum_\theta \pi_\theta = 1$. Let ρ_θ denote the probability of treatment for units in stratum θ. The marginal probability of treatment is

$$Pr(Z=1) = \sum_\theta \pi_\theta \rho_\theta.$$

For binary participation outcomes, individual treatment effects are 0 in strata A and N, 1 in T, and -1 in U.

A1.2 Average Treatment Effect (ATE)

Proof that $ATE = \pi_T - \pi_U$. By definition,

$$ATE = E[Y(1)] - E[Y(0)].$$

Using the potential outcomes in Table 1,

$$E[Y(1)] = \pi_A \cdot 1 + \pi_T \cdot 1 + \pi_U \cdot 0 + \pi_N \cdot 0 = \pi_A + \pi_T,$$

$$E[Y(0)] = \pi_A \cdot 1 + \pi_T \cdot 0 + \pi_U \cdot 1 + \pi_N \cdot 0 = \pi_A + \pi_U.$$

Thus,

$$ATE = (\pi_A + \pi_T) - (\pi_A + \pi_U) = \pi_T - \pi_U.$$

□

A1.3 Average Treatment Effect on the Treated (ATT)

Proof that $ATT = \frac{\pi_T \rho_T - \pi_U \rho_U}{Pr(Z=1)}$. By definition,

$$ATT = E[Y(1) - Y(0) \mid Z = 1].$$

Only strata T and U contribute nonzero treatment effects. Conditioning on strata and using the law of total probability,

$$ATT = \frac{\pi_T \rho_T \cdot 1 + \pi_U \rho_U \cdot (-1)}{Pr(Z=1)} = \frac{\pi_T \rho_T - \pi_U \rho_U}{Pr(Z=1)}.$$

□

It is useful to re-express the numerator as

$$\pi_T \rho_T - \pi_U \rho_U = \pi_T(Pr(Z=1) - (Pr(Z=1) - \rho_T)) - \pi_U(Pr(Z=1) - (Pr(Z=1) - \rho_U)),$$

which will be used below.

A1.4 Relationship Between ATE and ATT

Let $p = Pr(Z = 1)$ for notational convenience. From the expressions above,

$$ATE - ATT = (\pi_T - \pi_U) - \frac{\pi_T\rho_T - \pi_U\rho_U}{p} = \frac{(\pi_T - \pi_U)\,p - (\pi_T\rho_T - \pi_U\rho_U)}{p}.$$

Define

$$N \equiv (\pi_T - \pi_U)\,p - (\pi_T\rho_T - \pi_U\rho_U)\,.$$

Then

$$ATE > ATT \quad \Longleftrightarrow \quad N > 0,$$

and

$$N = \pi_T(p - \rho_T) + \pi_U(\rho_U - p).$$

Condition for $ATE > ATT$

Proof. Using the expressions above,

$$ATE > ATT \quad \Longleftrightarrow \quad \pi_T - \pi_U > \frac{\pi_T\rho_T - \pi_U\rho_U}{p},$$

which is equivalent to

$$(\pi_T - \pi_U)p > \pi_T\rho_T - \pi_U\rho_U,$$

or

$$N = \pi_T(p - \rho_T) + \pi_U(\rho_U - p) > 0.$$

□

The sign of N depends on the relative magnitudes of (π_T, π_U) and (ρ_T, ρ_U).

Case: $\rho_T < p < \rho_U$. In this case $(p - \rho_T) > 0$ and $(\rho_U - p) > 0$. Since $\pi_T, \pi_U \geq 0$ and at least one of them is positive whenever there is any treatment effect, $N > 0$ and therefore $ATE > ATT$.

Case: $\rho_T > p > \rho_U$. In this case $(p - \rho_T) < 0$ and $(\rho_U - p) < 0$, so $N < 0$ (again, provided π_T and π_U are not both zero), implying $ATE < ATT$.

Additional cases. The inequality $ATE > ATT$ can also hold when both strata are either less likely than average or more likely than average to be treated. Using $N = \pi_T(p - \rho_T) + \pi_U(\rho_U - p)$:

- If both strata have probabilities of receiving treatment less than average, $\max\{\rho_T, \rho_U\} < p$, then $(p - \rho_T) > 0$ and $(p - \rho_U) > 0$, so $(\rho_U - p) < 0$. In this case $N > 0$ whenever

$$\frac{\pi_T}{\pi_U} > \frac{p - \rho_U}{p - \rho_T}.$$

- If both strata have probabilities of receiving treatment greater than average, $\min\{\rho_T, \rho_U\} > p$, then $(p - \rho_T) < 0$ and $(p - \rho_U) < 0$, so $(\rho_U - p) > 0$. In this case $N > 0$ whenever

$$\frac{\pi_T}{\pi_U} < \frac{p - \rho_U}{p - \rho_T}.$$

A1.5 Equality of ATE and ATT

Equality $ATE = ATT$ requires $N = 0$, that is,

$$\pi_T(p - \rho_T) + \pi_U(\rho_U - p) = 0.$$

Proof. From the expressions above,

$$ATE = ATT \quad \Longleftrightarrow \quad (\pi_T - \pi_U)p = \pi_T\rho_T - \pi_U\rho_U,$$

which is equivalent to

$$\pi_T(p - \rho_T) + \pi_U(\rho_U - p) = 0.$$

$\square$

A simple and substantively important sufficient condition for this equality is homogeneous treatment probabilities for the treatment-responsive strata,

$$\rho_T = \rho_U = p,$$

which is the case under standard random assignment.

A1.6 Scaling Identity

Proof that $ATT \times Pr(Z = 1) = \pi_T\rho_T - \pi_U\rho_U$. Starting from

$$ATT = \frac{\pi_T\rho_T - \pi_U\rho_U}{Pr(Z = 1)},$$

multiplying both sides by $Pr(Z = 1)$ yields

$$ATT \times Pr(Z = 1) = \pi_T\rho_T - \pi_U\rho_U.$$

This expression coincides with the average change in the outcome, ΔY, induced by treatment in the population. $\square$

Appendix B Survey Data

A2.1 Samples

This section describes the temporal and geographic coverage of the different LAPOP survey waves and the ENSU survey waves used in the analyses.

A2.1.1 LAPOP

Table A1 shows all the included LAPOP country rounds, the year each round was conducted, and the number of respondents per year. The data was culled so that only respondents who were 19 years or older when responding to the survey were included. This ensures that all survey respondents are entitled to engage in all political participation activities in the prior year by law.

Year	Countries surveyed	N
2010	Argentina, Belize, Bolivia, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Honduras, Jamaica, Mexico, Nicaragua, Panama, Paraguay, Peru, Uruguay, Venezuela	33,022
2011	Colombia	1,444
2012	Argentina, Belize, Bolivia, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Honduras, Jamaica, Mexico, Nicaragua, Panama, Paraguay, Peru, Uruguay, Venezuela	31,061
2014	Argentina, Belize, Bolivia, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Honduras, Jamaica, Mexico, Nicaragua, Panama, Paraguay, Peru, Uruguay, Venezuela	31,186
2016	Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Honduras, Mexico, Nicaragua, Paraguay	13,233
2017	Argentina, Bolivia, Brazil, Chile, Guatemala, Jamaica, Panama, Peru, Uruguay	14,386
2018	Colombia, Costa Rica, El Salvador, Honduras, Panama	7,406
2019	Argentina, Bolivia, Brazil, Chile, Dominican Republic, Ecuador, Guatemala, Jamaica, Mexico, Nicaragua, Paraguay, Peru, Uruguay	19,073

Table A1: Table lists all the country-year LAPOP surveys included in the pooled data. All country surveys between 2010 and 2019 were included.

A2.1.2 ENSU

Table A2 shows the number of respondents per ENSU survey wave, as well as whether criminal victimization was asked during that specific period. During the COVID pandemic in 2020, INEGI canceled the third wave of the ENSU but collected data on criminal victimization for that period during the next survey round.

Year	Quarter	N	Victimization
2017	1	14,497	No
2017	2	15,272	Yes
2017	3	15,303	No
2017	4	15,072	Yes
2018	1	15,172	No
2018	2	17,548	Yes
2018	3	20,163	No
2018	4	18,017	No
2019	1	18,113	No
2019	2	19,010	Yes
2019	3	22,392	No
2019	4	22,158	Yes
2020	1	22,416	No
2020	3	22,122	Yes
2020	4	22,283	Yes
2021	1	22,307	No
2021	2	22,411	Yes
2021	3	23,356	No
2021	4	23,428	Yes
2022	1	23,577	No
2022	2	22,411	Yes
2022	3	24,435	No
2022	4	24,402	Yes
2023	1	23,778	No
2023	2	24,435	Yes
2023	3	24,493	No
2023	4	24,064	Yes

Table A2: Table shows the number of survey respondents per survey wave included in the ENSU data used in the analyses and whether criminal victimization was asked during each survey round.

A2.2 Measurement

A2.2.1 LAPOP

Table A3 reports the survey questions and measures employed in the paper for the LAPOP analyses.

Construct	LAPOP Survey Question	Scale
Community problem-solving	In the past 12 months, have you contributed to solving a community or neighborhood problem? How frequently?	1–4
Attended political meetings	In the past 12 months, have you attended a political party or political movement meeting? How frequently?	1–4
Attended community improvement meetings	In the past 12 months, have you attended community improvement meetings? How frequently?	1–4
Attended protest	In the past 12 months, have you attended a public protest?	Yes/No
Crime victimization	Have you been a victim of any type of crime in the past 12 months? That is, have you been a victim of robbery, burglary, assault, fraud, blackmail, extortion, violent threats, or any other type of crime in the past 12 months?	Yes/No
Would vote next week	If the next presidential elections were being held this week, what would you do?	4 response options

Table A3: LAPOP survey questions used to measure political participation and criminal victimization. The table shows English translations and response scales for each question. For analysis, all continuous participation measures were dichotomized, with 1 indicating any activity during the previous year and 0 indicating no engagement in the activity.

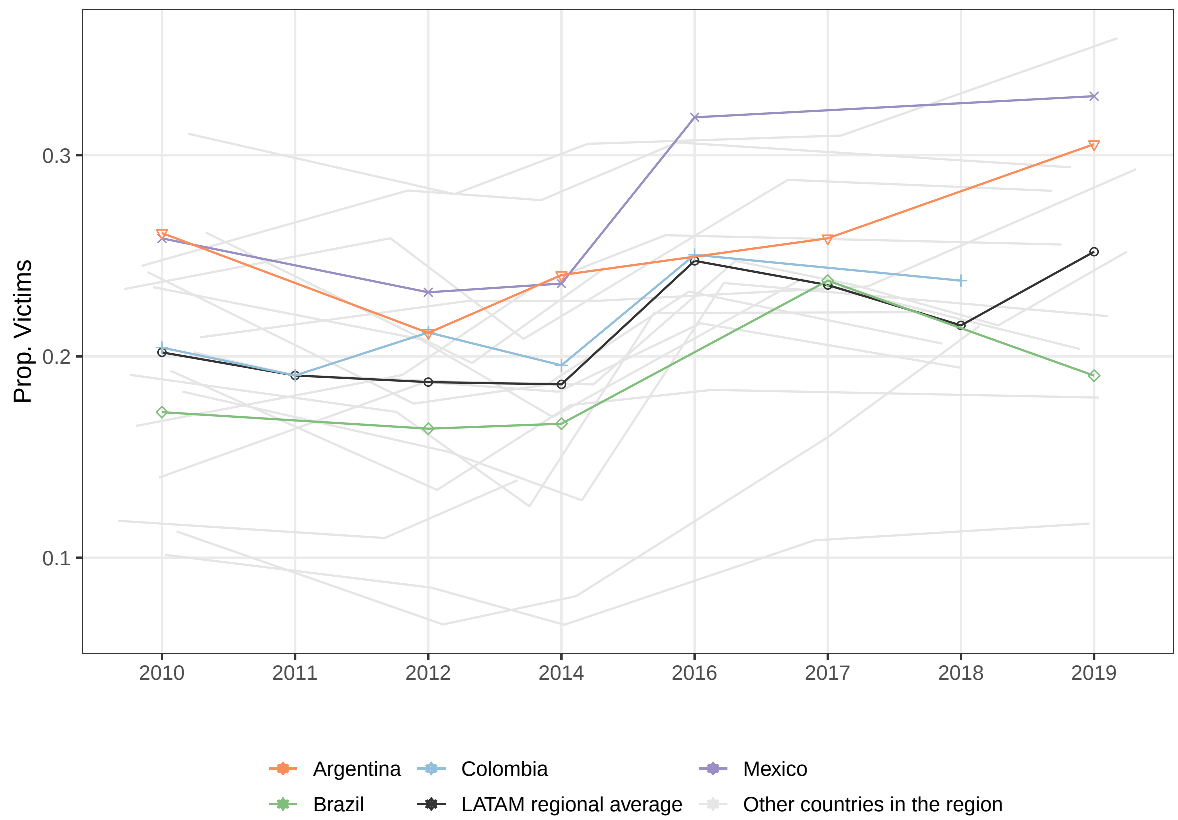

Figure A1: Figure shows the proportion of respondents who report being a crime victim in the past 12 months by survey round and country.

Last, Figure A1 shows the country-specific and pooled proportion of respondents who reported being victims of crime in each survey wave. As can be seen, the regional average fluctuates slightly between .19 and .26 for the entire period, with significant within-country variation.

A2.2.2 Variable Construction

All simulation results are run with binarized versions of the treatment variables. To recode, binarized variables for political meeting attendance and community meeting attendance take the value of 1 if respondent said they attended the meeting at least once in the previous year, 0 otherwise. For "would vote next week" the variable takes the value of 1 when respondents reported thew would cast a ballot for any candidate, and 0 when they report they would not vote.

A2.2.3 ENSU

Construct	ENSU question	Scale
Crime victimization	In the last six months, have you or a member of your household experienced any of the following: vehicle theft, vehicle accessory theft, any other kind of theft, burglary, robbery, threats, or extortion?	Yes/No
Unsafe: City	When thinking about crime, do you believe living in (CITY) is?	Safe/Unsafe
Unsafe: Frequented streets	When thinking about crime, do you you feel safe or unsafe in the streets you frequent?	Safe/Unsafe
Unsafe: House	When thinking about crime, do you feel safe or unsafe in your house?	Safe/Unsafe
Unsafe: Public transport	When thinking about crime, do you feel safe or unsafe in public transport?	Safe/Unsafe
Unsafe: Work	When thinking about crime, do you feel safe or unsafe at work?	Safe/Unsafe
Witnessed: Gangs	In the last three months, have you seen or heard violent gangs in your home's surrounding area?	Yes/No
Witnessed: Robberies	In the last three months, have you seen or heard robberies or thefts in your home's surrounding area?	Yes/No
Witnessed: Shots fired	In the last three months, have you seen or heard frequent gunshots in your home's surrounding area?	Yes/No
Witnessed: Vandalism	In the last three months, have you seen or heard vandalism, graffiti, broken glass, etc., in your home's surrounding area?	Yes/No
Witnessed: Drugs used or sold	In the last three months, have you seen or heard in your home's surrounding area of drugs being used or sold?	Yes/No
For fear of crime: Stopped going out at night	In the last three months, for fear of being victimized, have you changed your habits regarding walking around your neighborhood after 8 pm	Yes/No
For fear of crime: Stopped visiting friends or family	In the last three months, for fear of being victimized, have you changed your habits regarding visiting friends or family	Yes/No
For fear of crime: Stopped wearing valuables	In the last three months, for fear of being victimized, have you changed your habits regarding wearing jewelry, valuables, carrying money, or carrying credit cards	Yes/No
Trust: Army	How much trust do you trust the Army	1-4
Trust: Police	How much trust do you trust the State Police	1-4
Conflicts: Had any?	Have you had any conflicts with family members, neighbors, work or school colleagues, commercial establishments, or government authorities due to situations you consider detrimental to you or annoying?	Yes/No
Conflicts: Resulted in shoving, punching, or kicking	Did the conflict result in punching, shoving, or kicking?	Yes/No
Conflicts: With alcohol users, drug users, or gang	Did you have a conflict due to harassment by alcohol abusers, drug abusers, or gang members?	Yes/No
Conflicts: With Police	Did you have a conflict due to harassment by police officers?	Yes/No
Conflicts: With authorities	Did you have a direct conflict with a government authority?	Yes/No
Conflicts: With neighbors	Did you have a direct conflict with a neighbor?	Yes/No
Conflicts: With family members	Did you have a direct conflict with a family member?	Yes/No
Conflicts: With strangers on the street	Did you have a direct conflict with a stranger on the street	Yes/No
Mobility: Frequency of leaving home	Over the last six months, be it to go to work, school, the doctor, shopping, or any reason, how often did you leave your home?	1-7

Table A4: Table shows the English translation and the scale of all the ENSU survey questions used in the analyses.

All continuous measures, including trust in the Army, trust in the Police, and frequency of leaving home, are standardized within quarter and locality. To do so, I use the following formula:
For a variable X_i:

$$X_i^Z = \frac{X_i - \overline{X}_i}{\sqrt{\mathrm{Var}[X_i]}} \tag{1}$$

Figure A2 shows the proportion of survey respondents who report having experienced any of the crimes included in the measure of 'crime victimization.' The first quarter of 2020 shows a significant reduction, also seen in administrative data, due to COVID restrictions. The crime rate increases and flattens after that period, although it remains lower than in the prior years.

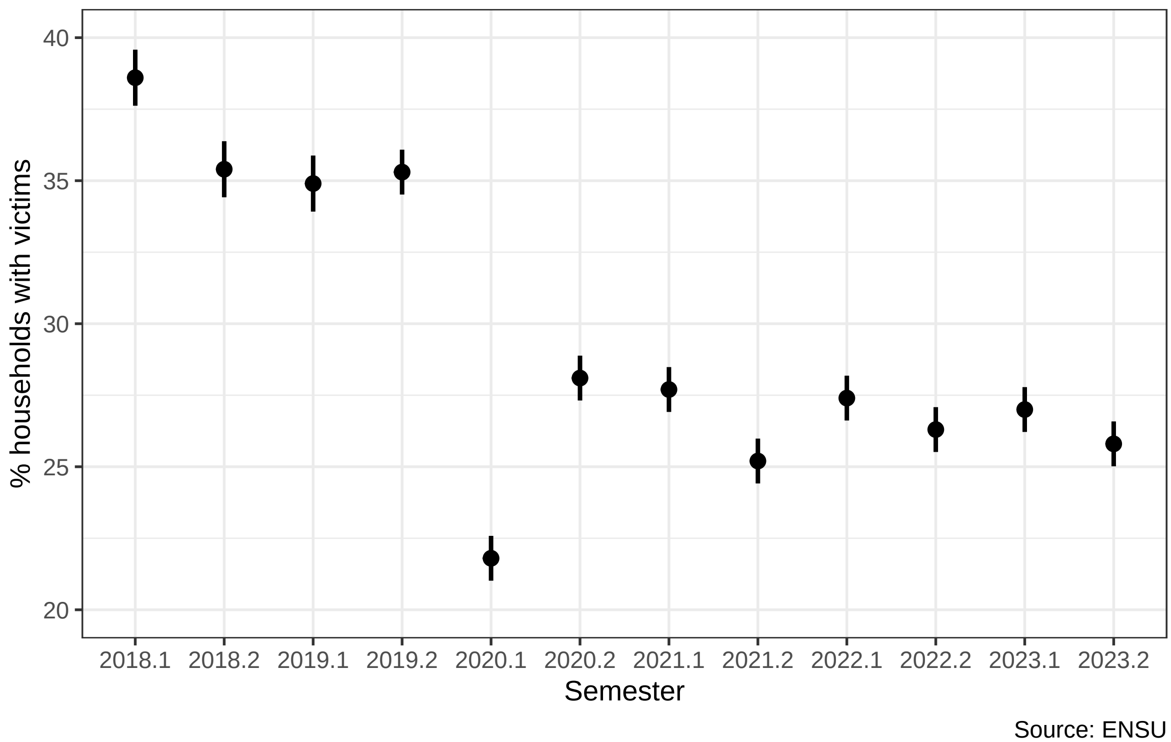

Figure A2: Figure shows the proportion of respondents who report having been a victim of vehicle theft, vehicle accessory theft, any other kind of theft, burglary, robbery, threats, or extortion during the last six months.

A2.3 Bateson (2012) Replication

The Latin American results in the canonical paper by Bateson (2012) show that self-reported victimization is positively associated with all seven civic and political engagement measures. Specifically, the author shows that victims report more interest in politics, attending protests more often, attempting to convince people to support a political candidate (proselytization), and attending community problem-solving, community improvement, municipal council, and political meetings. I use repeated cross-sections from the 20 countries included in my sample for the analysis. This extended dataset allows me to replicate the pooled analyses in Bateson (2012) and examine country-specific differences in political behavior between victims and non-victims.

Figure A3 shows the results. As reported in Bateson (2012), all political participation and civic engagement measures positively correlate to victimization in the pooled analyses. Additionally, I find that the same holds true within countries. Although results are noisier for measures like political interest or proselytization, all estimated effects are either positive or statistically indistinguishable from zero at conventional levels.

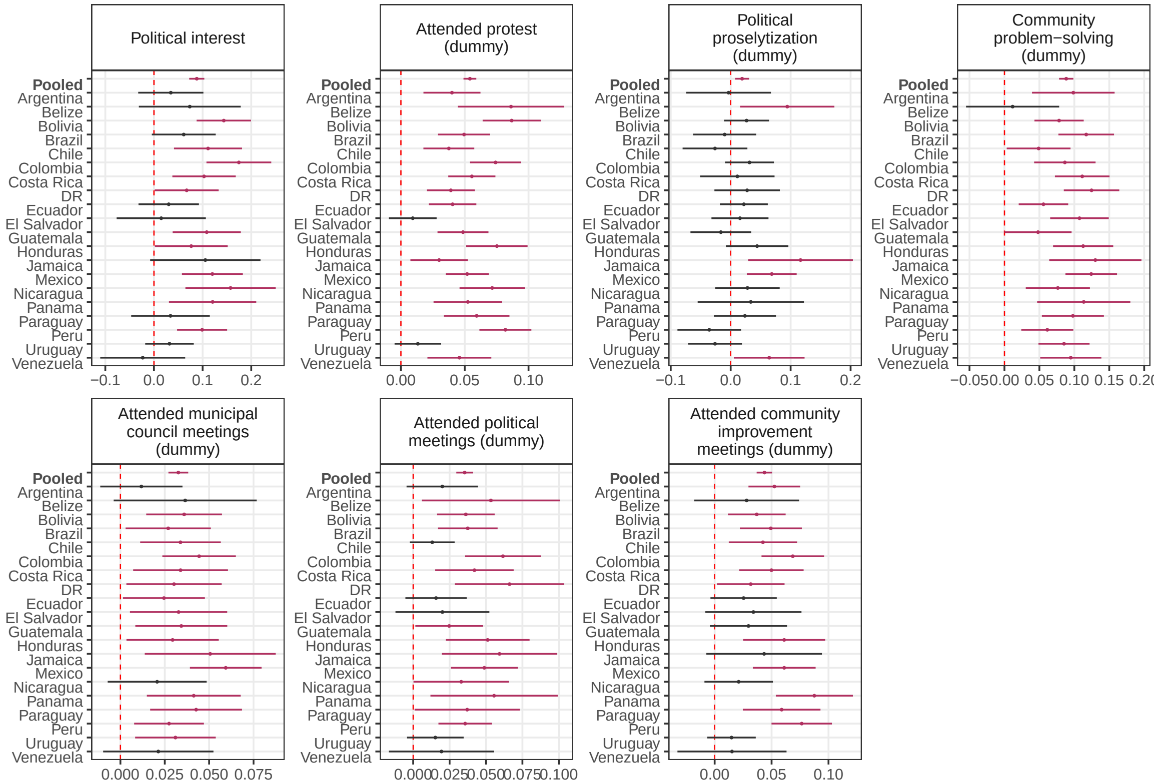

Figure A3: Figure shows the difference in participation and civic engagement outcomes between self-reported victims and non-victims, estimated with LAPOP (2022). The specification comes from Bateson (2012) but includes $Year \times Country$ fixed effects. Robust errors clustered at the primary sampling unit.

Appendix C Technical Details for the Empirical Strategy

This appendix provides the algebraic relationships, feasibility constraints, and computational steps used to recover the identified set of feasible parameter vectors $(\pi_\theta, \rho_\theta)$ that rationalize the participation and victimization moments observed in the LAPOP data. These parameter vectors are then mapped into the ATE and ATT expressions derived in Section 1.

A3.1 Moment Conditions Linking Observables to Principal Strata

Let $\theta \in \{A, T, U, N\}$ denote the four principal strata as in Table 1. The population shares π_θ satisfy $\sum_\theta \pi_\theta = 1$, and the stratum-specific treatment probabilities are $\rho_\theta \in [0, 1]$. The marginal probability of treatment is

$$Pr(Z = 1) = \pi_A\rho_A + \pi_T\rho_T + \pi_U\rho_U + \pi_N\rho_N.$$

For binary outcomes, the potential outcomes in Table 1 imply:

$$Y(1) = \begin{cases} 1 & \text{if } \theta \in \{A, T\} \\ 0 & \text{if } \theta \in \{U, N\}, \end{cases} \qquad Y(0) = \begin{cases} 1 & \text{if } \theta \in \{A, U\} \\ 0 & \text{if } \theta \in \{T, N\}. \end{cases}$$

Thus the unconditional participation rate satisfies:

$$Pr(Y = 1) = \pi_A + \pi_T \rho_T + \pi_U (1 - \rho_U).$$

The conditional participation rates satisfy:

$$Pr(Y = 1 \mid Z = 1) = \frac{\pi_A \rho_A + \pi_T \rho_T}{Pr(Z = 1)},$$

$$Pr(Y = 1 \mid Z = 0) = \frac{\pi_A (1 - \rho_A) + \pi_U (1 - \rho_U)}{1 - Pr(Z = 1)}.$$

These four expressions define a system of equations that any feasible vector $(\pi_\theta, \rho_\theta)$ must satisfy in order to reproduce the observed moments:

$$Pr(Z = 1), \quad Pr(Y = 1), \quad Pr(Y = 1 \mid Z = 1), \quad Pr(Y = 1 \mid Z = 0).$$

A3.2 Feasibility Constraints

A parameter vector $(\pi_\theta, \rho_\theta)$ is admissible only if:

1. $\pi_\theta \in [0, 1]$ for all θ and $\sum_\theta \pi_\theta = 1$.
2. $\rho_\theta \in [0, 1]$ for all θ.
3. The moment equations above are satisfied exactly when observed moments are substituted.
4. Participation rates generated by the model are compatible with potential outcomes (e.g., strata A and N produce no variation in participation across treatment states).
5. The implied ρ_θ solve the moment conditions uniquely (e.g., $Pr(Z = 1)$ must equal its observed value).

Parameter vectors violating any of these constraints are discarded.

A3.3 Recovering Treatment Probabilities from Observed Moments

For each candidate vector of population shares $(\pi_A, \pi_T, \pi_U, \pi_N)$ on the grid, the moment conditions allow identification (up to feasibility) of the implied ρ_T and ρ_U.
From the conditional participation rate among the treated:

$$Pr(Y = 1 \mid Z = 1) \cdot Pr(Z = 1) = \pi_A \rho_A + \pi_T \rho_T,$$

and from the conditional participation rate among the untreated:

$$Pr(Y = 1 \mid Z = 0) \cdot (1 - Pr(Z = 1)) = \pi_A (1 - \rho_A) + \pi_U (1 - \rho_U).$$

Substituting:

$$Pr(Z = 1) = \pi_A \rho_A + \pi_T \rho_T + \pi_U \rho_U + \pi_N \rho_N,$$

and using the unconditional participation moment,

$$Pr(Y = 1) = \pi_A + \pi_T \rho_T + \pi_U (1 - \rho_U),$$

yields a solvable system for ρ_T and ρ_U. Then ρ_A and ρ_N are solved as the remaining unknowns satisfying the $Pr(Z = 1)$ equation.
Any solution outside $[0, 1]$ is infeasible.

A3.4 Enumeration Algorithm

The computational procedure used in the paper is:

1. Construct a grid over $(\pi_A, \pi_T, \pi_U, \pi_N)$ with resolution 0.005 subject to $\sum_\theta \pi_\theta = 1$.
2. For each candidate vector, solve for ρ_T and ρ_U from the participation moment equations.
3. Solve for ρ_A and ρ_N from the marginal victimization moment.
4. Discard the parameter vector if any $\rho_\theta \notin [0, 1]$ or if any moment equation fails to match the observed values.
5. For each remaining vector, compute:

$$ATE = \pi_T - \pi_U, \qquad ATT = \frac{\pi_T \rho_T - \pi_U \rho_U}{Pr(Z = 1)}.$$

This generates the identified sets of ATEs and ATTs used in the main text.

A3.5 Mapping Feasible Parameter Sets into ATE and ATT

Because the ATE depends only on π_T and π_U (Proposition 1.1), while the ATT depends on both population shares and heterogeneous treatment probabilities (Proposition 1.2), comparing the two estimands across feasible parameter values reveals how heterogeneous exposure patterns shape the empirical consequences of victimization.

The divergence conditions shown in Section 1 provide theoretical guidance: whenever $\rho_U > \rho_T$, the ATT will generally be smaller than the ATE, and can differ in sign. The simulations quantify this divergence by enumerating all allowable parameter vectors consistent with the observed data.

Appendix D Simulation Results

A4.1 Assuming monotonicity

This section presents the simulation results from the simulation using the LAPOP data to recover the parameter sets of $\{\pi_\theta, \rho_\theta\}$ $\forall \theta \in \{A, T, N\}$ consistent with the data when the effect of victimization on the four participatory outcomes is assumed to be increasing. That is we assume $\pi_U = 0$

Figue A4 shows the results. By construction, both the ATE and the ATT are greater than zero in all parameter sets. However, we can see that it is still the case in the majority of the sets, $ATE > ATT$, since most sets are below the red line that passes through the origin. Further, we can see that the more frequent an outcome is, the wider the range of positive ATTs compatible with the data. Thus, while protest and political meeting attendance have a median ATT of only 7.2 pp and 9.2 pp, respectively, community problem-solving and community meeting attendance have an ATT of 21.3 pp and 16, respectively.

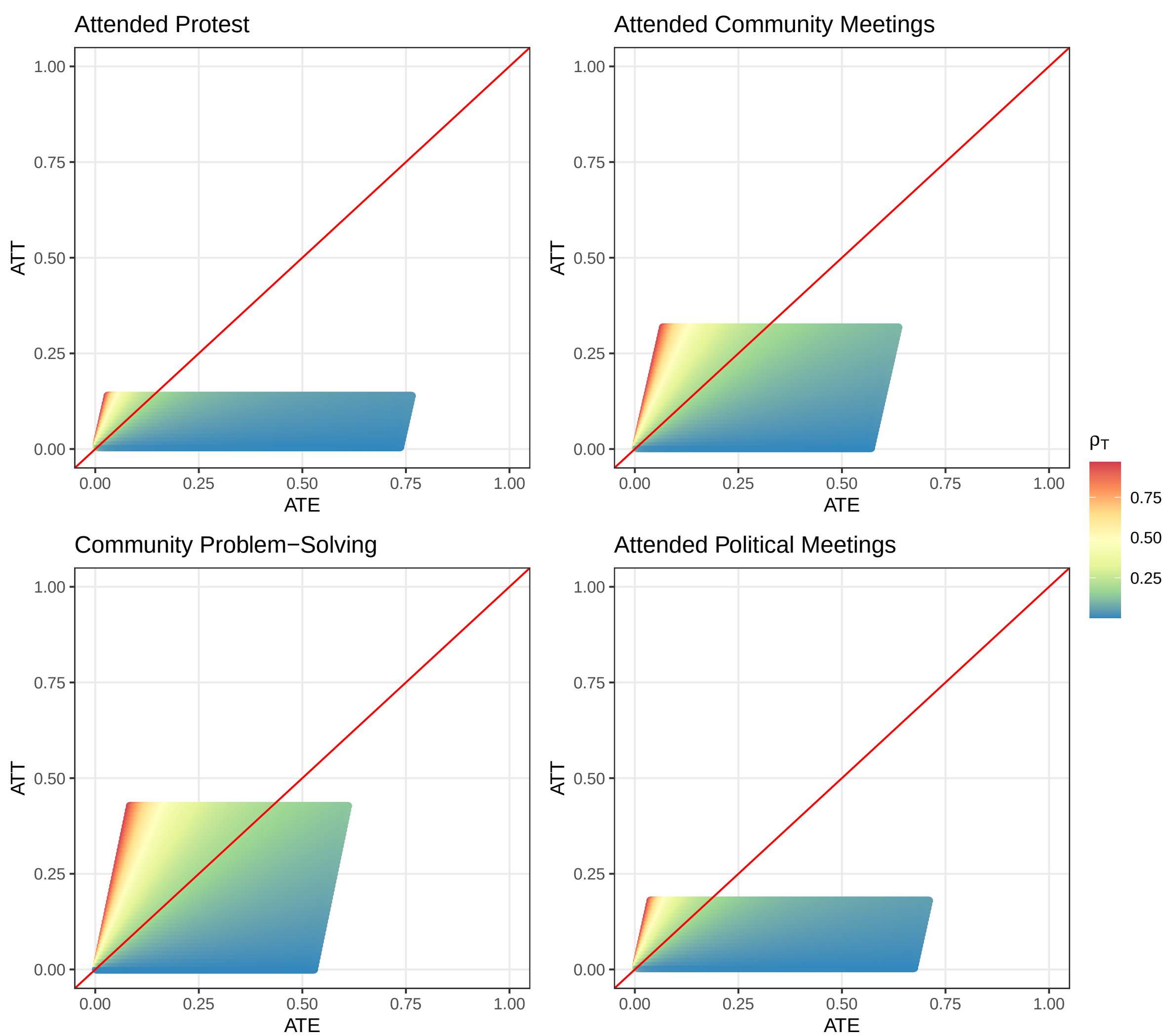

Figure A4: Figure shows the combination of potential ATTs and ATEs of victimization on four measures of political participation, conditioning on observed participation, and criminal victimization (LAPOP, 2022). The simulation assumes that the effect of victimization on participation is monotonic (i.e., $\Pi_{PINV} = 0$).

A4.2 Constraining the Parameter Set

The Mexico findings leveraging panel-data presented in the main paper suggest that people who become demobilized after crime are at greater risk of experiencing it in the first place. This helps us further narrow down the plausible sets of parameters. Since we know this relationship exists, we can be more certain about which parameters are actually producing our data. In this subsection, I present results comparable to Table 3 but constrain the parameter sets to only those where $\rho_U > \rho_T$. Table A5 reports these findings. The constrained analysis yields consistent but more pronounced patterns, which is expected given the ATT construction. A larger proportion of the plausible parameter sets show negative ATTs and positive ATEs, resulting in a more defined posterior distribution. However, when it comes to non-electoral participation, the results strongly emphasize that the true ATT is likely negative while the ATE is most likely positive. This pattern is particularly evident for protest attendance and political meetings, where over 90% of parameter

		ATE			ATT			ΔY	
Participation Type	$Pr(Z=1)$	**Min**	**Max**	$Pr(ATE<0)$	**Min**	**Max**	$Pr(ATT<0)$	**Min**	**Max**
Attended Protest	0.19	-0.23	0.76	0.13	-0.84	0.12	0.93	-0.18	0.02
Community Problem-Solving	0.19	-0.38	0.61	0.21	-0.57	0.42	0.76	-0.11	0.08
Attended Political Meetings	0.20	-0.28	0.71	0.18	-0.82	0.16	0.91	-0.17	0.03
Attended Community Meetings	0.21	-0.36	0.63	0.32	-0.68	0.30	0.85	-0.14	0.06
Would vote next week	0.22	-0.64	0.33	0.49	-0.13	0.81	0.62	-0.03	0.18

Table A5: Results from parameter sets compatible with the LAPOP data where $\rho_U > \rho_T$, in line with results from the panel analysis suggesting people who are more exposed to crime are also likely to become demobilized as a consequence of experiencing it. $\Delta Y = ATT \times Pr(Z = 1)$ represents the average treatment effect in the population. By definition, $Pr(\Delta Y) < 0$ is the same as $Pr(ATT < 0)$.

sets yield negative ATTs. However, there is one notable exception: the “would vote next week” measure. While the unconstrained parameter sets suggest predominantly positive ATTs (95%), constraining the sets reverses this pattern, with only 38% positive — a substantial shift. Additionally, the ATE for voting becomes equally likely to be positive or negative. This finding may h elp explain contradictory results regarding turnout in the literature, suggesting that crime’s effect on political participation varies significantly across contexts and populations.

A4.3 Results using Dorff (2017)

One potential concern is that the simulation results might reflect sampling issues, whereby issues with the LAPOP sampling are in turn reflected in the results. For example we might be worried LAPOP over-represent urban areas, under-represents dangeorous areas, or generally results in survey responses that do not generalize. To address such concerns, I analyze data from Dorff (2017), who conducted a nationally representative survey of 1,000 Mexican respondents that included questions about victimization and both electoral and non-electoral participation. I apply the same partial identification procedure from the main paper to this alternative dataset. For ease of comparison, I also present results from simulations conducted using only Mexican LAPOP data. All results are reported in Table A6.

As shown in Table A6, the results for comparable participation types show remarkable consistency. For political participation, 88% of ATTs in the plausible parameter sets using Dorff data are negative, compared to 85% for LAPOP data. Similarly, neighborhood meetings in Dorff data (53%) closely align with community problem-solving in LAPOP data (59%). Furthermore, most ATEs across all measures fall within the positive range of 70-80%. Overall, the main findings remain consistent regardless of data source: while most ATEs consistent with the data are positive (aligning with extant research), most ATTs are negative.

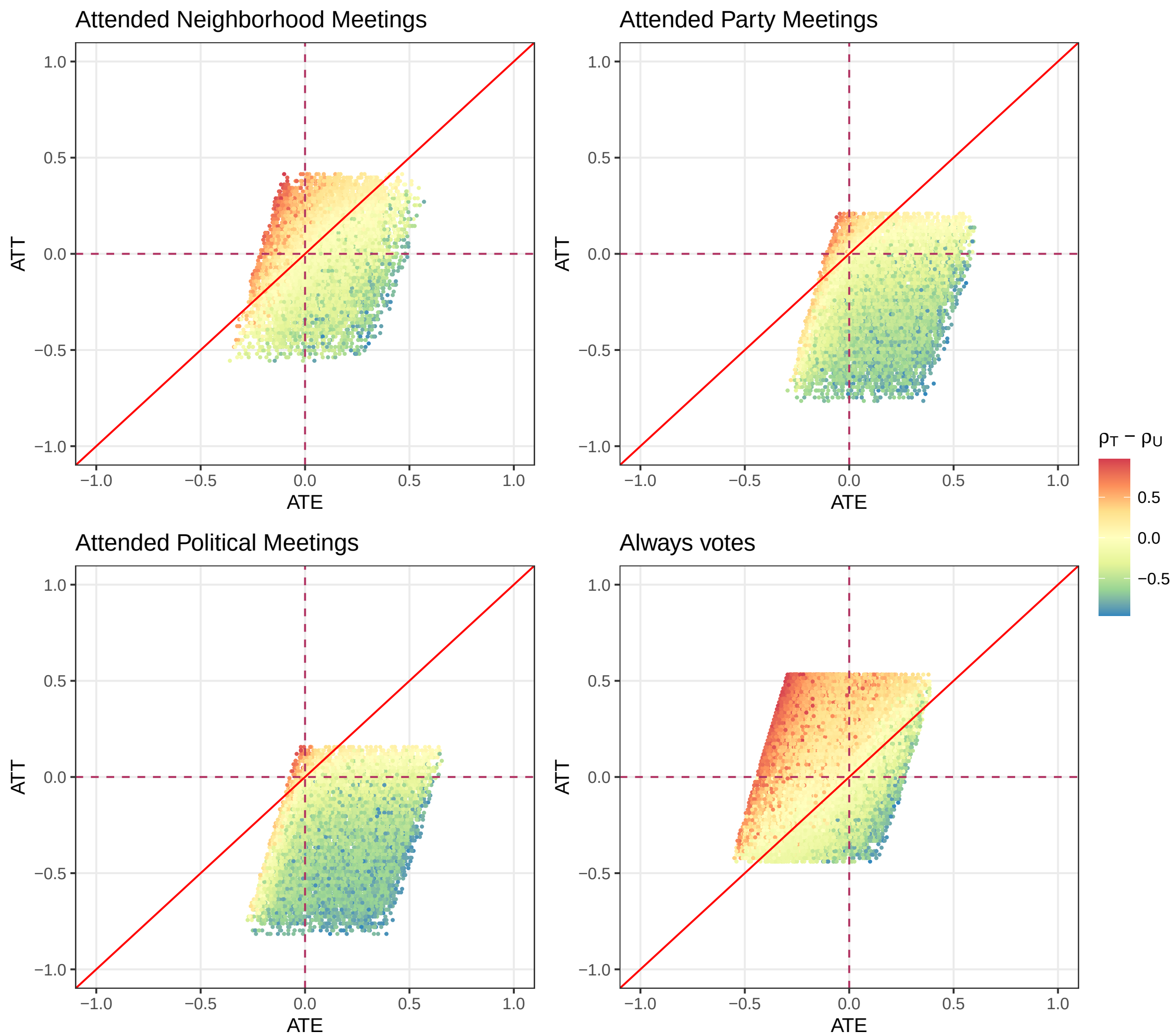

Figure A5: Figure shows the combination of potential ATTs and ATEs of victimization on four measures of political participation from Dorff (2017)), conditioning on observed participation, and criminal victimization.

Panel A: Dorff (2017)									
		ATE			**ATT**			ΔY	
Participation Type	$Pr(Z=1)$	**Min**	**Max**	$Pr(ATE<0)$	**Min**	**Max**	$Pr(ATT<0)$	**Min**	**Max**
Attended Neighborhood Meetings	0.28	-0.38	0.61	0.31	-0.57	0.41	0.53	-0.16	0.12
Attended Party Meetings	0.28	-0.32	0.66	0.24	-0.76	0.21	0.82	-0.21	0.06
Attended Political Meetings	0.28	-0.30	0.69	0.21	-0.83	0.16	0.88	-0.23	0.04
Panel B: LAPOP - Mexico									
		ATE			**ATT**			ΔY	
Participation Type	$Pr(Z=1)$	**Min**	**Max**	$Pr(ATE<0)$	**Min**	**Max**	$Pr(ATT<0)$	**Min**	**Max**
Would vote next week	0.28	-0.65	0.34	0.87	-0.12	0.87	0.04	-0.03	0.24
Attended Community Meetings	0.28	-0.37	0.62	0.31	-0.68	0.30	0.72	-0.19	0.08
Community Problem-Solving	0.24	-0.36	0.63	0.30	-0.58	0.39	0.59	-0.16	0.11
Attended Political Meetings	0.28	-0.32	0.67	0.24	-0.82	0.17	0.85	-0.22	0.05
Attended Protest	0.28	-0.28	0.71	0.18	-0.88	0.10	0.92	-0.22	0.03

Table A6: Results from all parameter sets compatible with data from Dorff (2017) and LAPOP Mexico data. $\Delta Y = ATT \times Pr(Z=1)$ represents the average treatment effect in the population. By definition, $Pr(\Delta Y < 0)$ is equivalent to $Pr(ATT < 0)$.

Appendix E Reporting Bias

The empirical results presented in the paper are based on surveys. Given the known issues with criminal underreporting (Jaitman et al., 2017), researchers favor surveys over administrative data to explore the political consequences of criminal victimization. However, survey responses are subject to misreporting. The issue might be especially important when it comes to criminal victimization. "Crime" and "crime victim" are legal and social categories. Individuals might interpret events as criminal or non-criminal differently (Skogan, 1982; Elias, 1986). Importantly, such sensitivity could correlate with demographic characteristics, political attitudes, and behavior, leading to biased conclusions regarding the difference between the ATE and the ATT (Skogan, 1982; Boulding, Mullenax and Schauer, 2022).

Heterogeneous reporting is difficult to test because it requires the researcher to observe the "ground truth" or the behavior before respondents classify it as criminal or not criminal. However, I conduct two descriptive analyses to explore whether reporting is independent of criminal exposure. First, using the ENSU survey of Mexican city dwellers, I look at whether respondents who report having been asked for a bribe by a government official also report being victims of extortion.

In a legal sense, all individuals who were asked for a bribe were also extorted by the Police. However, bribe solicitation is plausibly a more straightforward behavior to identify than extortion. Thus, it operates as "ground truth." Suppose differences in self-reported criminal victimization are driven by differences in reporting that covary in political behavior instead of differential *exposure* to crime. In that case, individuals asked for a bribe should be no more likely to report having been extorted than the rest of the respondents. I report the results of this analysis in table A7. Reassuringly, those asked for a bribe are 7.7 pp more likely to report being extorted, as we would expect if exposure to crime mapped onto the report of being a victim. However, results show that the more educated are also more likely to report being victimized, conditional on being asked for a bribe.

As a further test, I compare the proportion of LAPOP respondents from Mexico, Brazil, and Colombia who reported being victims of a crime the previous year with the homicide rate from the municipality where they reside. Homicide data is thought to be the best-measured crime in administrative data. To the degree that homicides covary with another type of crime, we should expect more dangerous municipalities to lead to more crime victims if exposure to crime maps onto self-reported victimization. Results are shown in Figure

A6 in the Appendix. The proportion of victims is generally increasing with the municipal homicide rate for all three countries.

Together, these analyses suggest that self-reported victimization is indeed increasing with criminal victimization. However, results also suggest the politically engaged could report victimization more often, holding exposure to crime fixed. This non-rivalous mechanism should be explored in future research.

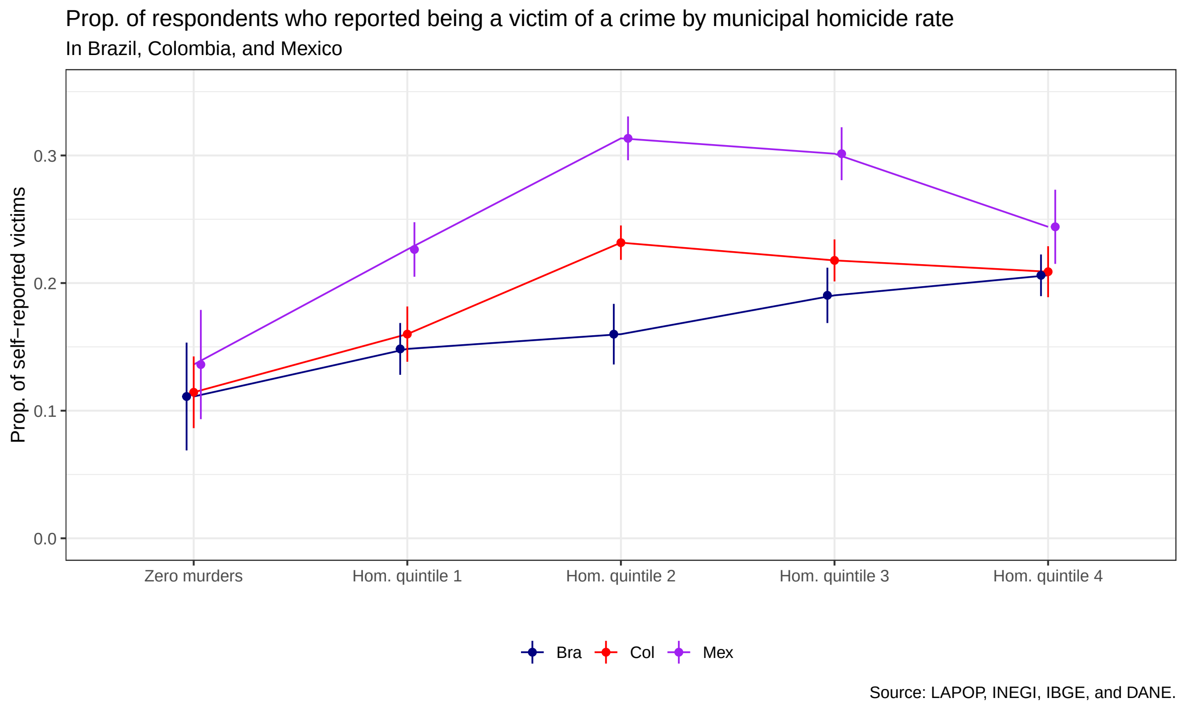

Figure A6: Figure shows the proportion of self-reported victims in LAPOP in Brazilian, Mexican, and Colombian municipalities, according to the municipal homicide rate.

	Extortion	**No Extortion**
Bribe	5972	1528
No Bribe	5585	2122

Table A7: Number of survey respondents in ENSU according to self-reported extortion victimization status and whether they were asked for a bribe during the same period.

Table A8: Extortion report and bribe solicitation

	Victim of Extortion?
Bribe solicited	0.076***
	(0.007)
Age	0.006***
	(0.001)
Age^2	0.000**
	(0.000)
Education (std)	0.027***
	(0.004)
Is employed	−0.006
	(0.009)
Is a man	−0.024**
	(0.007)
Num.Obs.	15 089
R2 Adj.	0.012

Cluster-robust standard errors shown in parentheses.
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix F Participatory Consequences of Civil War and Police Contact

In this section, I reevaluate the results presented in two canonical papers on the political consequences of other types of violence. I first reassess results from Blattman (2009) linking exposure to civil war violence during childhood in Uganda to increased electoral participation in adulthood. The second is a paper linking exposure to unwanted contact with police in the US to more non-electoral and electoral participation (Walker, 2020). Both papers target the ATE as their main estimand.

When analyzing Blattman (2009), I find that, unlike the crime victimization results, both the ATT and ATE of electoral participation are positive in approximately 75-77% of parameter sets, consistent with the paper's central finding of increased electoral participation. Conversely, between 93-96% of the ATTs from non-electoral participation measures are, in fact, *negative.* For Walker (2020), we observe patterns similar to the LAPOP results: for non-electoral participation, police contact yields positive ATEs while the ATTs are negative; for electoral participation, this pattern reverses —93% of parameter sets show a positive ATT while 89% show a negative ATE, highlighting how plausible heterogeneity in treatment effects can substantially alter our understanding of violence's political consequences.

A6.1 Civil War Violence as Treatment

The political and participatory consequences of exposure to civil war violence have been studied extensively. Canonical research indicates that, contrary to theoretical predictions whereby civil war violence would make participation harder, more costly, and more dangerous, civil war experiences of violence encourage voting and non-voting participatory behavior (Bellows and Miguel, 2009; Blattman, 2009; Voors et al., 2012). One such paper is Blattman (2009). In it, the author examines how forced recruitment of youths into the LRA insurgency in Northern Uganda shapes this group's downstream participatory decisions. To do so, the author leverages quasi-random variation in abduction and survey data to estimate the ATE of abduction on political outcomes.

The author argues using qualitative evidence that abduction was as-if random, or put differently, that the probability of abduction was homogeneous regadless of individuals' strata. In such a scenario, the ATE would not only be theoretically relevant but could shape how actual participatory outcomes changed in practice. Table A9 summarizes the results. 77% of the parameters in the identified set of ATEs is positive. Interestingly and unlike results for the LAPOP analysis, we have a very similar proportion of positive ATTs in the identified set. If abduction was as-if random, then the probability of abduction was homogeneous regadless of individuals' strata. These results seem to support such assesment when it comes to strata of voting behavior. Overall, the results suggest it is very likely that abduction increases future voting, even if exposure to treatment were not random. However, the results seem less clear when it comes to non-electoral participation. Only in 35% of the sets does such experience lead to higher odds of becoming a community organizer if abduction were in fact random, despite the paper's point estimate of the ATE being positive and the only significant effect on non-electoral participation. Conversely, the identified ATTs in the plausible sets of all non-electoral participation suggest that abduction is linked to less civic engagement if certain individuals were heterogeneously exposed to it. Between 93%-96% of the identified sets include a negative ATT.

Blattman (2009)									
		ATE			**ATT**			ΔY	
Participation Type	$Pr(Z=1)$	**Min**	**Max**	$Pr(ATE<0)$	**Min**	**Max**	$Pr(ATT<0)$	**Min**	**Max**
Voted	0.65	-0.44	0.55	0.23	-0.46	0.54	0.25	-0.30	0.35
Peace Group Member	0.62	-0.58	0.41	0.64	-0.91	0.08	0.93	-0.57	0.05
Community Mobilizer	0.62	-0.59	0.40	0.65	-0.93	0.06	0.95	-0.58	0.04
Volunteer	0.62	-0.61	0.38	0.32	-0.61	0.38	0.96	-0.38	0.24

Table A9: Results from all parameter sets compatible with data from Blattman (2009). $\Delta Y = ATT \times Pr(Z = 1)$ represents the average treatment effect in the population. By definition, $Pr(\Delta Y) < 0$ is equivalent to $Pr(ATT < 0)$.

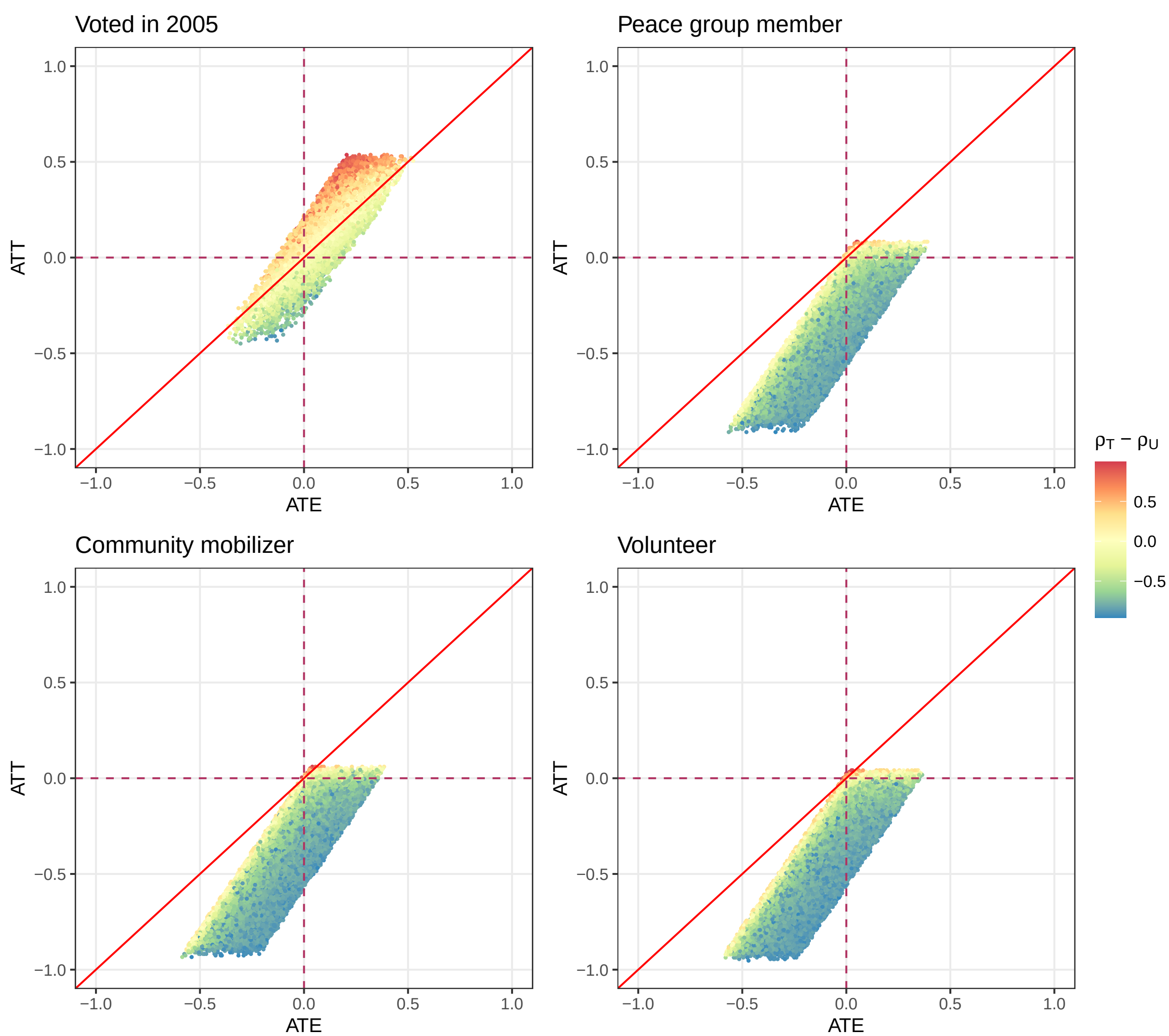

Figure A7: Figure shows the combination of potential ATTs and ATEs of child abduction on all measures of political participation from Blattman (2009)).

Appendix G Contact with the Police as Treatment

Exploring the political consequences of interactions with the police for minority communities and individuals is a long tradition of US research. Scholars have argued that these interactions teach communities about the state and their role as second-class citizens and lead to less political participation (Soss and Weaver, 2017; Lerman and Weaver, 2014; Weaver and Lerman, 2010). However, other researchers challenge this view and argue that the experience of injustice can mobilize targetted individuals (Ang and Tebes, 2023; Walker, 2020). In this section, I reassess the part of the evidence presented in Walker (2020). Walker challenges the assumption that criminal justice contact always demobilizes political participation. She argues that people's responses to interactions with the state depend critically on how they interpret these experiences and what resources they have. When individuals —especially those with proximal contact or from minority groups— view their negative experiences as examples of systemic injustice rather than personal failure, and when they recognize their shared fate with others, they may actually increase their political participation outside of voting as a direct response to prevent further discrimination. In line with this theoretical argument, the author finds that direct and indirect experiences with the police result in more non-electoral political participation. To do so, the author relies on survey data, a research design that, conditional on observable covariates, targets the ATE of interactions with the police on participation.

I examine the parameter sets compatible with data from the National Crime and Politics Survey (NCPS), a nationally representative U.S. survey conducted in the fall of 2013. While Walker (2020) looks at the political consequences of direct and indirect contact with the police, I focus only on direct police contact, maintaining consistency with my other analyses. For dependent variables, again, I use all measures of electoral and non-electoral political participation in the preceding 12 months. I report the results in Table A10. The original study found that personal police contact increased political participation by 0.38 points on a 0-8 scale index, targeting the ATE through conditioning strategies for causal identification. Similar to the findings from the simulations fitted on the LAPOP data, the NCPS data reveal that while most parameter sets show positive ATEs for non-electoral participation, the ATTs for these same activities are predominantly negative. For voting, however, this pattern *again* reverses: 89% of parameter sets yield negative ATEs, while only 7% show negative ATTs. These results suggest that police contact is more likely to occur among individuals who respond by increasing their electoral participation while decreasing their non-electoral political engagement.

NCPS (Walker, 2017)										
		ATE			**ATT**			ΔY	
Participation Type	$Pr(Z=1)$	**Min**	**Max**	$Pr(ATE<0)$	**Min**	**Max**	$Pr(ATT<0)$	**Min**	**Max**
Voted	0.19	-0.65	0.34	0.89	-0.16	0.82	0.07	-0.03	0.16
Helped in campaign	0.19	-0.27	0.72	0.20	-0.75	0.24	0.79	-0.14	0.05
Attended Political Meeting	0.20	-0.32	0.67	0.25	-0.66	0.32	0.67	-0.13	0.06
Attended protest	0.20	-0.23	0.76	0.14	-0.75	0.22	0.82	-0.15	0.04
Written letter to politician	0.19	-0.40	0.59	0.41	-0.60	0.35	0.64	-0.14	0.04
Donated or raised funds	0.19	-0.36	0.63	0.33	-0.61	0.37	0.64	-0.15	0.04

Table A10: Results from all parameter sets compatible with data from NCPS (Walker, 2017). $\Delta Y = ATT \times Pr(Z=1)$ represents the average treatment effect in the population. By definition, $Pr(\Delta Y) < 0$ is equivalent to $Pr(ATT < 0)$.

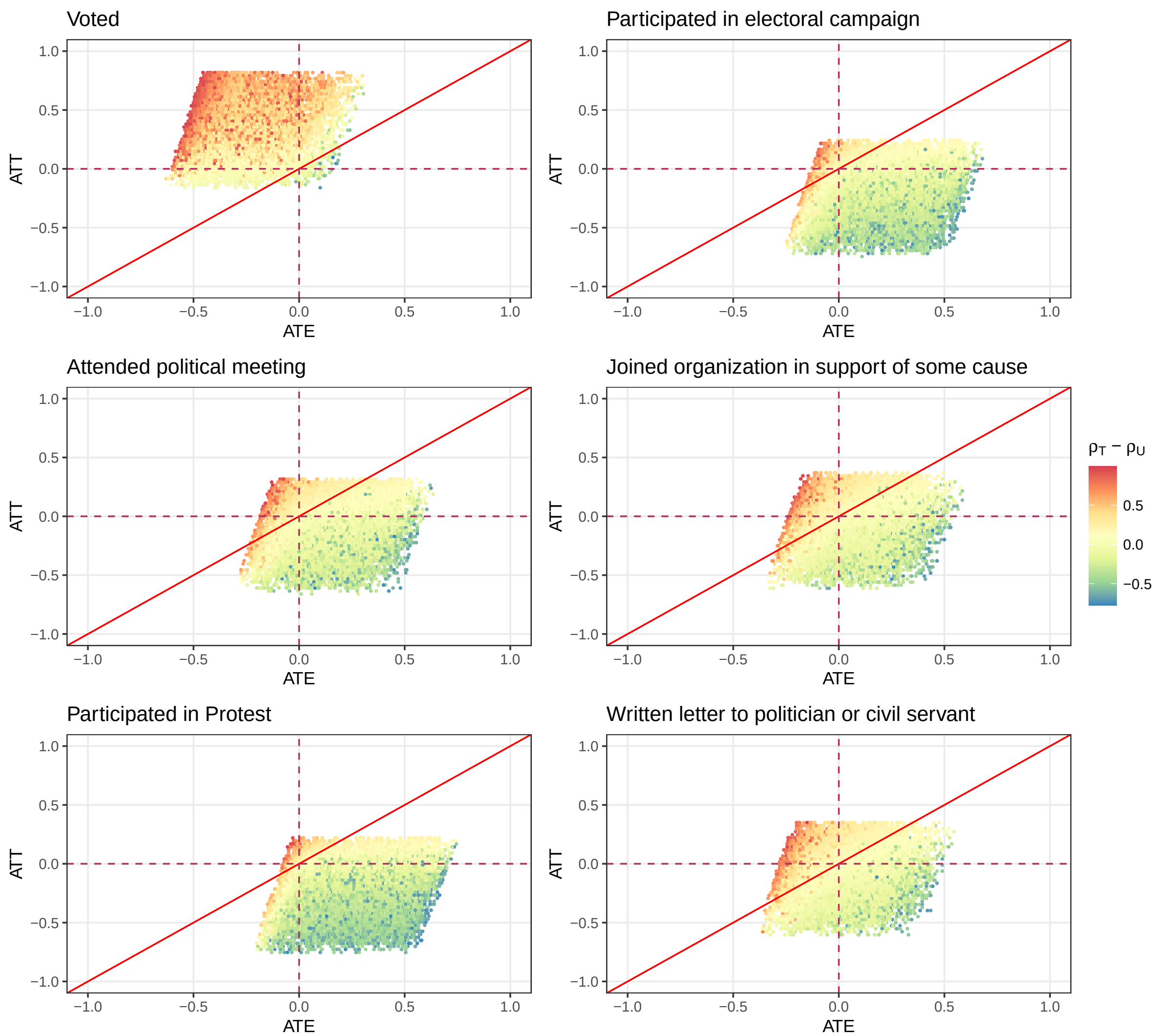

Figure A8: Figure shows the combination of potential ATTs and ATEs of direct contact with the police on all measures of political participation from Walker (2020)).

Appendix H Other ENSU results

This section provides complementary analyses to those presented in the main paper. Figure A9 shows the cross-sectional difference in means between soon-to-be victims and non-victims living in the same locality (dark blue), the same neighborhood/census tract (light blue), and the within-individual ATT of criminal victimization on all outcomes used in the paper. As can be seen, the ATT follows the cross-sectional differences in all but the mobility outcomes. Results support the finding that victimization depresses participation while primarily targeting the more extroverted and mobile individuals in a locality or neighborhood.

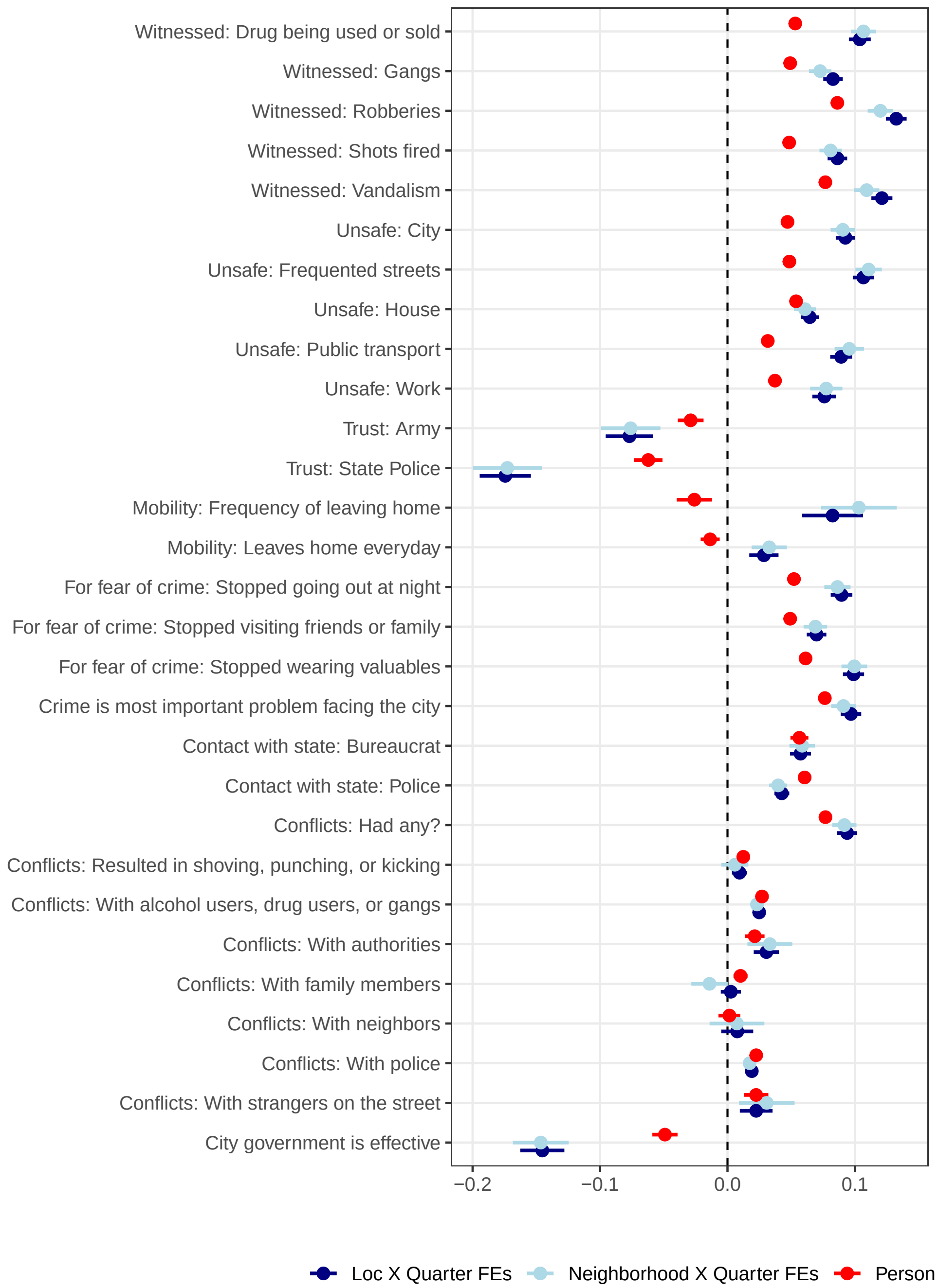

Figure A9: Figure shows the cross-sectional difference between soon-to-be victims and non-victims living in the same locality (dark blue), the same neighborhood/census tract (light blue), and the ATT (red) of criminal victimization on self-reported outcomes relating to exposure to crime. Robust errors clustered at the primary sampling unit.

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