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

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Abstract

Theoretical accounts of the participatory effects of violence suggest it demobilizes some individuals but mobilizes others, making exposure patterns critical for understanding consequences. I develop a principal stratification framework incorporating heterogeneous treatment probabilities to assess the implications of accounting for systematic over/underexposure to violence. Empirically, I focus on criminal violence. Work targeting average treatment effects shows crime spurs participation. However, criminological research suggests that people are heterogeneously vulnerable to crime. Using survey data from 150,000 Latin Americans, panel data from Mexico, and millions of simulations, I demonstrate that individuals who participate less post-victimization are more likely to become victims, resulting in an overall negative change of non-electoral participation due to crime. Similar patterns emerge when reassessing results from civil war exposure and police contact. These results challenge our understanding of violence's political consequences and highlight the distinction between how violence could shape politics and how, in fact, it does so.

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Violence is political. Not only does the monopoly of its legitimate use characterize modern states, but the distinction between legal and illegal violence itself emanates from politics. The participatory consequences of experiencing violence have consequently become a prominent strand of political science research. Scholars have examined how exposure to violence related to civil war (Blattman, 2009; Gilligan, Pasquale and Samii, 2014), crime (Bateson, 2012; Sønderskov et al., 2022), policing (Ang and Tebes, 2023; Soss and Weaver, 2017), and repression (Rozenas and Zhukov, 2019) change the ways in which individuals engage in politics.

The theory and evidence on the participatory consequences of distinct types of violence reveal two overarching themes. First, that violence can produce disjointed consequences for participation. Thus, despite taxonomical differences, theoretical mechanisms link distinct types of violence to sometimes more participation (Blattman, 2009; Bateson, 2012; Gilligan, Pasquale and Samii, 2014) and sometimes less participation (Ley, 2018; Koos and Traunmüller, 2024; Weaver and Lerman, 2010). Second, the perpetration of and exposure to different classes of violence are heterogeneous: criminals target the vulnerable, police are strategic in their stops, and civil war violence devastates certain areas while leaving others untouched (Kalyvas et al., 2006; Skogan, 2005).

Yet causal identification-oriented research on violence rarely incorporates these theoretical insights into research designs in a crucial way: the estimands they target. Identification-oriented research is often constrained by the need to deploy credible designs within specific empirical settings (Imbens and Angrist, 1994; Austin, 2011; Angrist, Imbens and Rubin, 1996; Hahn, Todd and Van der Klaauw, 2001). The primary question becomes “What causal research design can be credibly deployed in this context?" rather than “Which estimand best captures the theoretical quantity of interest?" Different research designs recover different estimands that incorporate heterogeneity in exposure and treatment effects differently, fundamentally shaping what we can learn about violence’s political consequences.

I develop a framework that extends classical principal stratification (Frangakis and Rubin, 2002) to incorporate stratum-specific treatment probabilities. This approach conceptualizes dif-

ferent strata as subpopulations that respond distinctly to violence (positively, negatively, or not at all) while allowing for heterogeneous exposure likelihood across these strata.

Within this framework, I show that the average treatment effect (ATE) depends solely on the relative proportions of the population in treatment-responsive strata. In contrast, the average treatment effect on the treated (ATT) additionally accounts for stratum-specific treatment probabilities. This distinction is crucial: the ATT incorporates the fundamental heterogeneity in exposure to violence that the ATE overlooks. These estimands will differ unless treatment probability is constant across all strata and may even take opposite signs depending on which strata are most likely to experience violence.

This paper demonstrates that when we disregard heterogeneity in exposure to violence and target estimands that recover population-wide average effects, we are likely to conclude that violence spurs political participation. But it also shows that violence is disproportionately experienced by those who respond by participating less. When we account for who actually gets exposed, we find that violence likely depresses participation among those most likely to experience it. The implication is stark: given who experiences violence, it has probably reduced rather than increased political participation.

I leverage this framework to reconsider criminal victimization's political consequences. While crime's harmful effects on democracy are widely acknowledged (Czabański, 2008; Jaitman et al., 2017), recent research suggests crime breeds active citizenry, increasing civic and political engagement (Bateson, 2012; Sønderskov et al., 2022).[1] This interpretation of crime as a participation catalyst suggests a silver lining: violence might foster engaged societies and more accountable governments. However, criminological research demonstrates that crime exposure is unevenly distributed. Wealthier, risk-averse individuals take more preventive measures (Skogan, 1995; Hale, 1996), while more mobile, risk-prone community members face higher victimization risks and receive less protection (Rader, 2004; Browning, Pinchak and Calder, 2021). Since risk preferences,

[1]Though some studies find negative effects on turnout (Ley, 2018; Trelles and Carreras, 2012)

mobility patterns, and sociodemographics relate to both political participation *and* victimization risk, the population-wide average effect of victimization likely differs substantially from its effect on those actually victimized.

I use repeated cross-sectional surveys that measure political participation and personal victimization (LAPOP, 2022), the same (extended) data from which the canonical results on the mobilizing effects of victimization arose (Bateson, 2012). The data allows us to observe the proportion of respondents who report engaging in non-electoral civic or political participation: those who attended protests, political meetings, community meetings, community problem-solving activities and willingness to engage in future electoral participation. Additionally, we can observe the proportion of respondents who reported having been victimized in the last 12 months and the intersection of these groups. Using these quantities and the principal stratification framework, I conduct data-driven analyses to test how targeting estimands that incorporate heterogeneous treatment probabilities or not affect our conclusions of the effects of violence on participation.

I characterize the sets of ATEs and ATTs consistent with relevant moments in the data. Specifically, I derive the identified set of plausible values that the true ATE and ATT can take, given the data and the framework, thus binding these estimates (Kline and Tamer, 2023). The identified sets consistent with the LAPOP data reveal that 85%-63% of parameter combinations yield positive ATEs for the four measures of non-electoral political participation,[2] while 93%-53% yield negative ATTs for these same measures. For the willingness to vote in future elections, the pattern reverses: 89% of parameter combinations yield negative ATEs, while 95% yield positive ATTs.

Under a uniform distribution over the identified sets—treating all parameter combinations as equally likely—these results suggest the average effect of victimization likely spurs non-electoral political participation in Latin America, consistent with extant research (Bateson, 2012). However, individuals who participate less as a consequence of being victimized are at greater risk of becoming victims. Consequently, because crime victimization is not equally likely for all citizens, the

[2]The percentage varies as a function of the outcome of interest.

overall change in participation due to crime is likely negative in Latin America.

Conversely, I show that in 89% of the parameter sets compatible with the data, the ATE of victimization on willingness to vote in future elections is *negative*, while in 95% of the sets, the ATT is positive. This discrepancy between the current understanding of criminal violence as engendering political participation and the results from these analyses emphasizes the importance of selecting the correct estimand: Research designs linking crime to more political participation target the ATT. For example, Sønderskov et al. (2022) finds a positive ATT of participation in voting but a negative ATE of participation in voting when looking at data from Denmark. Conversely, Ley (2018) uses survey data and, in an analysis targeting the ATE, estimates a *negative* effect of crime on voting. Consequently, these results can help reconcile the seemingly disjoint empirical findings from recent research on the electoral consequences of crime.

Imposing a uniform prior allows for an intuitive interpretation of the simulations' results by treating all parameter combinations as equally plausible. However, this agnostic approach cannot reveal which combinations actually occur. To move from the theoretically plausible to the empirically grounded, I test whether individuals who respond to crime by participating less are indeed more exposed to it. Using a quarterly rotating panel survey of Mexican city residents (INEGI, 2024), I find that soon-to-be victims were more exposed to criminal behavior: they were more mobile, witnessed more criminal and gang activity, and had more violent conflicts. Leveraging the panel structure, I show that after victimization, individuals become less trusting, more crime-aware, and less mobile, suggesting demobilization (Lee et al., 2021; Gerell, 2021). These results confirm that exposure risk and participatory responses to victimization systematically correlate.

To showcase the relevance of these methods for studying violence beyond crime, I reevaluate results from two papers: Blattman (2009), linking civil war violence exposure in Uganda to increased electoral participation, and Walker (2020), connecting police contact in the US to political participation. Simulation results again show that if exposure to both police contact and recruitment of youths into the LRA insurgency in Northern Uganda were random, then the change in partic-

ipation due to these treatments was positive. Conversely, if exposure was heterogeneous, likely, non-electoral participation in the Uganda case and participation broadly in the case of police contact in the US were reduced—highlighting how plausible heterogeneity in treatment effects can substantially alter our understanding of violence's political consequences.

This paper makes three important contributions. Empirically, the paper challenges conventional wisdom regarding criminal victimization's political effects. While the prevailing literature suggests victimization catalyzes civic engagement (Bateson, 2012; Sønderskov et al., 2022), the analyses reveal that conditional on who is likely to become a victim, the opposite is true in Latin America. Though randomly assigned victimization might indeed increase participation, were it to happen, the empirical distribution of victimization probability among Latin Americans systematically covaries with their potential responses to it in ways that fundamentally change aggregate effects. The critical insight emerges: precisely because individuals predisposed to participate less after victimization likely disproportionately become victims, victimization likely suppresses—rather than stimulates—non-electoral participation across the region. Such results have grave implications for accountability and democratic quality in the world's most violent region. Furthermore, there is suggestive evidence that results from studies on policing and civil conflict are also worth rethinking in light of this estimand distinction.

Second, it provides a framework for thinking about the disjoint consequences of violence that incorporates one of its key features: heterogeneity in exposure. This principal stratification approach is grounded in the robust empirical finding that violence engenders divergent participatory responses across populations. By explicitly modeling both response heterogeneity and exposure heterogeneity, the framework enables a more theoretically coherent mapping between the strategic logic of violence perpetration and the causal estimands we target. In so doing, it contributes to our understanding and study of the political economy of violence.

Finally, the paper contributes to the methodological literature on the link between theory and empirical evidence in causal inference. A growing body of work emphasizes that estimand selec-

tion determines what we can learn from statistical evidence and how reduced-form causal estimates should be interpreted for policy and theory (Lundberg, Johnson and Stewart, 2021; Slough, 2023; Deaton, 2009; Hanson, Kronick and Slough, 2024). Building on Lundberg, Johnson and Stewart (2021)'s framework for connecting statistical evidence to theory through precise estimands, this paper demonstrates when average treatment effects fail to capture real-world causal processes, provides data-driven tools to diagnose these failures, and shows how ATTs can quantify empirical outcome changes conditional on equilibrium treatment probability distributions.

1 A Principal Strata Framework

Within a potential outcomes framework, causal estimands of interest can be intuitively defined as the difference in outcomes for one unit if the unit had and had not been exposed to some treatment, averaged over the entire population (ATE) or a specific subpopulation —for instance, only those who were treated (ATT) or only those who were affected by the treatment (LATE) (Rubin, 1978; Aronow and Samii, 2016). I detail the implications of heterogeneous treatment probabilities within such a framework.

Consider a population of units indexed by i and a binary treatment $Z \in \{0, 1\}$, for example experiencing violence or not. Denote the potential outcome for unit i under treatment assignment z as $Y_i(z)$. We can classify experimental units into strata (θ) defined by their potential outcomes, $Y_i(1)$ and $Y_i(0)$ (Holland, 1986; Frangakis and Rubin, 2002). By definition, the individual treatment effect for each i, $ITE_i = Y_i(1) - Y_i(0)$, will be identical for all units in the same stratum. Further denote the proportion of units in each stratum π_θ, with $\sum_\theta \pi_\theta = 1$. Unlike the canonical principal-strata setup, assume units have stratum-specific probabilities of receiving treatment, ρ_θ.

Table 1 synthesizes the set-up for the case of a binary outcome $Y_i(z) \in \{0, 1\}$. One such outcome is political participation. An individual may vote or not, participate in a protest, or petition their representative. The number of people who vote for a certain candidate decides the election, signatures in a petition might mean the difference between policy change or not, protest attendance

numbers can make a topic nationally important versus neglected. Thus, as political scientists we are interested in whether a treatment can forster or depresses participation and by how much. Further, note that we observe substantial cross-sectional and intertemporal variation in participation outcomes, making it especially well-suited to such a framework.

Stratum (θ)	**Share**	**Pr(Z=1)**	**Y(1)**	**Y(0)**	**ITE**
Always Participate	π_A	ρ_A	1	1	0
Participate if **T**reated	π_T	ρ_T	1	0	1
Participate if **U**ntreated	π_U	ρ_U	0	1	-1
Never Participate	π_N	ρ_N	0	0	0

Table 1: Stratum-specific individual treatment effects and treatment probabilities.

For a binary treatment such as exposure to violence, we can classify individuals into four strata: those who always participate (A), those who never participate (N), those who participate if and only if treated (T), and those who participate if and only if untreated (U).[3] Formal proofs for all expressions in this section are provided in Appendix A.

Proposition 1.1. *The average treatment effect is* $ATE = \pi_T - \pi_U$.

The ATE depends exclusively on the shares of strata T and U. Understanding why is straightforward when we consider all units in strata A and N contribute an ITE of zero to the ATE. Consequently, the magnitude and direction of the ATE will only depend on the frequency with which T appears in the population, relative to U.

Proposition 1.2. *The average treatment effect on the treated is*

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

[3] I make the stable unit treatment value assumption (SUTVA) throughout.

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

Conversely, the ATT, or the average expected change in outcomes for those who are treated, will be a function of population shares and also of the stratum-specific probabilities of receiving treatment for all strata. These stratum-specific probabilities scale the relative contributions of the T and U strata within the treated population. Intuitively, strata with higher treatment probabilities constitute a larger share of those observed in the treated group, and therefore should contribute more to the ATT.

Corollary 1.1. *The ATE and ATT are equal when* $\rho_T = \rho_U = Pr(Z = 1)$.

Three important implications follow directly from Propositions 1.1 and 1.2.

Inequality of estimands. The ATT will generally differ from the ATE unless treatment probabilities are homogeneous across the T and U strata. Corollary 1.1 characterizes the special case in which the two coincide. The intuition is simple: the two estimands average over different populations. The ATE averages over the entire population, whereas the ATT averages only over those who are treated. If some strata are more likely to be treated than others, the treated population has a different composition than the full population, and the average effect will differ. When $\rho_T = \rho_U = Pr(Z = 1)$, treatment is equally likely across strata, so conditioning on treatment does not change the relative representation of the T and U strata in the comparison. In this case, the ATT and ATE effectively average over the same population weights.

Ordering of the estimands. The ATE may be larger or smaller than the ATT. Consider the case in which $\rho_T < Pr(Z = 1) < \rho_U$, so the U stratum is more likely to be treated than the T stratum. In this situation, the ATT places more weight on units whose treatment effect is negative, while the ATE depends only on population shares; thus, $ATE > ATT$. The reverse holds when $\rho_T > Pr(Z = 1) > \rho_U$.[4] More generally, the ATE aggregates treatment effects using

[4]Additional parameter configurations, including the full algebraic conditions for $ATE > ATT$, appear in Appendix A.

population shares, whereas the ATT aggregates them using the composition of the treated sample. Importantly, treatment rarity magnifies these differences because strata with higher treatment probabilities constitute a larger share of the treated group when $Pr(Z = 1)$ is small.

Change in the population outcome. The overall impact of treatment on the outcome in the population can be recovered by scaling the ATT by the marginal probability of treatment. Rearranging the expression in Proposition 1.2 yields:

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

This identity captures the average change in Y across the entire population under the realized treatment assignment. To understand the intuition, recall that only outcome-sensitive treated units—those in the T and U strata who are actually treated—can change their outcome. Consequently, the population-level effect depends on how many such units there are and which strata they belong to. Specifically, a share $\pi_T \rho_T$ of the population who would not have participated unless treated contribute positive changes in the outcome. However, a share $\pi_U \rho_U$ who would have participated only if untreated contribute negative changes. The average change in Y is therefore the difference between these two quantities.

2 The Political Consequences of Exposure to Crime

The framework from the previous section provides a basis for reassessing the political consequences of violence generally. However, I empirically focus on criminal violence for the main analyses. Crime victimization is the most common form of violence in many developing world contexts, and, much like other types of violence, the theoretical understanding of how crime affects political participation is disjunctive. Competing arguments exist for both participation-spurring and participation-depressing mechanisms. Berens and Dallendörfer (2019) succinctly capture this dichotomy of potential consequences as one between "apathy or anger," wherein anger mobilizes political action while apathy depresses participation.

For the participation-depressing mechanisms or the expectation of apathy, the literature draws from established theoretical frameworks in economics and criminology. Crime is costly. It makes individuals poorer, imposes significant psychological costs, frightens its victims, and teaches victims about the government's ability or lack thereof to prevent crime, engendering mistrust in the state. These mechanisms are thus posited to depress participation (Guedes, Domingos and Cardoso, 2018; Becker, 1968; Hale, 1996). Empirical evidence has shown support for these mechanisms; crime victims can become disenchanted and more distrustful of the political system (Carreras, 2013; Blanco, 2013) and supportive of tough-on-crime policing (Visconti, 2020; García-Ponce, Young and Zeitzoff, 2023). Further, when crime is a major concern, people are less willing to assume the added costs of participating in elections (Ley, 2018; Trelles and Carreras, 2012). Such results indicate that the demobilizing effect of criminal victimization may sometimes activate.

For the participation-spurring mechanisms or the expectation of anger, researchers borrow heavily from psychology and civil war studies. Theories suggest criminal victimization may activate psychological processes that increase prosociality, empathy, and community engagement in individuals (Bateson, 2012; Sønderskov et al., 2022). Additionally, victims potentially have instrumental reasons to participate after exposure to crime, seeking to punish politicians responsible or address policy failures that led to rising crime (Ley, 2017; Kronick, 2014). Theoretical mechanisms also suggest prosocial responses may be amplified when violence is experienced collectively, as shared victimization can strengthen community bonds and facilitate collective action (Dorff, 2017; Ley, 2022).

Empirical evidence in support of these participation-spurring effects has also been uncovered, and the current consensus, by and large, is that personal victimization fosters civic and political engagement, on average. In a now canonical article, Bateson (2012) analyzes a cross-section of survey data from 70 countries in four continents and finds that crime victims everywhere, including Latin America, report attending community meetings, protests, town meetings, and political

meetings more often. Further, the author finds that victims are more interested in politics, engage in political persuasion more often, and are likelier to take action to improve the community. Bateson (2012) attributes this increased political and civic engagement to criminal victimization. The magnitude of the effect is comparable to those of an additional 5-10 years of schooling. The author also finds that while participating more, crime victims are more likely to reject democracy, favor authoritarian government, and support tough-on-crime policies. Further supporting these findings, Ley (2022) demonstrates that community-embedded individuals are more likely to protest against criminal violence than isolated community members, while Dorff (2017) finds strong kinship networks can foster participation after crime. Additionally, Berens and Dallendörfer (2019) find that victims of non-violent crime participate more, while Sønderskov et al. (2022) find that they vote more after non-violent crime in a within-person analysis using Danish administrative data.[5]

Within this framework, we can think of mechanisms linking victimization to increased rates of political participation, such as posttraumatic growth, a desire for retribution, and instrumental incentives to effect policy change, as being strongest among the *Participate if Treated* stratum. Conversely, we can think of the *Participate if Untreated* stratum as made up of individuals who become afraid, disengaged, and disappointed in the political system after experiencing crime. The understanding of crime as engendering participation, on average, that is currently pervasive in the discipline maps directly onto the principal-strata framework, with such findings consistent with a larger share of the *Participate if Treated* stratum relative to the *Participate if Untreated*.

2.1 Why Reassess the Evidence?

The research design in Bateson (2012) targets a conditional Average Treatment Effect (CATE). Though Bateson never explicitly defines her estimand, her regression specifications condition on age, gender, education, economic status, urban residence, and country fixed effects while invoking

[5]Bateson (2012)'s analysis is limited to non-electoral participation. While other researchers have found that voting increases for victims (Berens and Dallendörfer, 2019; Sønderskov et al., 2022), the results regarding electoral participation after crime are particularly mixed. Some studies have found that high homicide rates are linked to lower electoral participation at both aggregate and individual levels (Ley, 2018; Trelles and Carreras, 2012).

conditional ignorability, the assumption that victimization is independent of potential outcomes given these covariates. This strategy thus identifies the average causal effect of victimization for individuals sharing the same covariate profile, averaged across all such profiles in the population. Formally, this is $E[Y(1) - Y(0)|X]$, where the expectation is taken over the distribution of covariates X. The CATE asks: within groups defined by observable characteristics, what is the average effect of victimization on political participation? Thus, the CATE is fundamentally an average treatment effect: it estimates what would happen if victimization were randomly assigned within covariate strata, not what actually happens given the empirical patterns of who becomes victimized.[6] Bateson's interpretation itself confirms that average effects are the theoretical quantity under investigation, as she argues that victimization is "plausibly random conditional on individual and neighborhood characteristics" and makes broad causal claims that "men and women who report that they were recently the victims of crimes are more active in civic and political life" (Bateson, 2012: p. 578).

However, criminological research demonstrates that exposure to crime is heterogeneous, with victims differing systematically from non-victims in both their exposure patterns and reactions to victimization. Risk of victimization covaries in socioeconomic status, risk preferences, and demographic characteristics (Hale, 1996; Ferraro and LaGrange, 2000; Guedes, Domingos and Cardoso, 2018; Skogan, 1995). Crime occurs when "a motivated offender, a suitable target, and the absence of capable guardianship" converge (Browning, Pinchak and Calder, 2021). Wealthier, risk-averse individuals better avoid victimization (Skogan, 1995; Hale, 1996) and often receive preferential protection from law enforcement (González and Mayka, 2022; Magaloni, Franco-Vivanco and Melo, 2020). Meanwhile, more mobile, risk-prone individuals embedded in communities face greater victimization risk with less protection (Rader, 2004; Browning, Pinchak and Calder, 2021;

[6]One important clarification is that this paper's main critique differs from standard methodological concerns about our ability to infer causality from observational designs. While criticisms of confoundedness focus on biased estimates of the ATE —analogous to consistently missing the bullseye on a target— this critique addresses a more fundamental issue. The concern here is not about missing the bullseye of a given target but rather about aiming at entirely different targets altogether (the ATT versus the ATE).

Boggs, 1965; Guedes, Domingos and Cardoso, 2018).

If high-risk individuals respond differently to victimization, the real-world effects might diverge substantially from the estimated ATEs in existing research. The ATT captures how outcomes actually change in the population by incorporating who receives treatment, similar to how retrospective intervention effects account for baseline treatment distributions to assess policy-relevant counterfactuals (Samii, Paler and Daly, 2016). Given crime's centrality for millions, especially in developing regions, reassessing these findings is crucial. The empirical analyses that follow evaluate this possibility.

2.2 Participation and Crime in Latin America

I focus on Latin America. The democracies of Latin America are some of the most violent in the world (Vilalta, 2020), making security a central demand from voters. Crime and insecurity consistently top the list of Latin Americans' most pressing concerns in surveys (LAPOP, 2022), with direct crime costs estimated at approximately 3.4% of the region's GDP (Inter-American Development Bank, 2024). Consequently, Latin America has been a frequent setting for extant research on the political consequences of victimization (Bateson, 2012; Visconti, 2020; Ley, 2018; Blanco, 2013; Marshall, 2022). In such contexts, the participatory implications of victimization are crucial to understand.

As the primary data source, I employ an extended version of the LAPOP survey, data used in the canonical paper by Bateson (2012) for the Latin American results. I incorporate data from all the surveys conducted in 20 Latin American countries between 2010 and 2019. Altogether, these include 150K unique respondents from 94 survey waves. Each survey round was designed to represent the country's voting-age population that year.[7] LAPOP data includes respondents' self-reported victimization in the past 12 months and several measures of individuals' civic and political engagement. For the analyses, I focus on five participation measures of theoretical interest, all binary or binarized: protest participation, political meeting attendance, community meeting

[7] See A2.1.1 in the Appendix for details about the sample.

attendance, participation in community problem-solving activities, and willingness to vote in upcoming national elections if they were held next week.

For these measures, both policymakers and researchers seek to infer crime-induced changes in overall participation levels. When studying protests in high-violence contexts, Ley (2022) emphasizes how protest size can pressure governments to implement stronger security policies, threaten criminals, and reduce violence against participants from both government and criminal actors. Anecdotal evidence shows that larger protests against crime achieve national attention and prompt swifter, more forceful political responses (Ferri, 2024). Furthermore, Alvarado and Muggah (2018) conclude that the most effective urban initiatives against crime in Latin America were those that enhanced community participation, particularly during policy planning stages. More broadly, higher levels of community engagement have been linked to better policy outcomes and stronger democratic norms across Latin America, leading to innovations like participatory budgeting and community policing (Souza, 2001; Peyton, Sierra-Arévalo and Rand, 2019).

2.3 Empirical Strategy

I use the LAPOP data to demonstrate that victimization risk likely covaries with how individuals respond to it and to benchmark the difference between the ATE and ATT of victimization on participation. I focus on the ATT as a quantity of particular importance because the real-world empirical consequences of crime depend specifically on who actually experiences victimization, not on hypothetical average effects across the entire population.

With the pooled LAPOP data, I can retrieve the proportion of respondents who report being victims, $Pr[Z = 1]$ and its converse probability, the proportion of respondents who report engaging in each of the participatory activities, $Pr[Y = 1]$, and its converse, and the proportion of victimized/unvictimized respondents who report participating $Pr[Y = 1|Z = 1]$, $Pr[Y = 1|Z = 0]$, respectively. Table 2 reports the observed probabilities in the data for each of the outcomes.[8]

[8]These proportions are estimated from survey data and therefore contain uncertainty. However, the large sample size yields very precise estimates, rendering the incorporation of this uncertainty into subsequent simulations negligi-

Leveraging the structure of the principal-strata framework, these observed quantities restrict the possible values of the population shares of each stratum (π_θ) and their corresponding treatment probabilities (ρ_θ). The expressions derived in Section 1 imply that any feasible parameter vector must reproduce the observed participation moments through the identities linking $(\pi_\theta, \rho_\theta)$ to $(Pr[Y=1], Pr[Y=1 \mid Z=1], Pr[Y=1 \mid Z=0], Pr[Z=1])$. For each feasible vector, I then compute the ATE and ATT directly using the closed-form expressions established earlier:

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

To characterize the full set of parameter values consistent with the data, I explore the feasible parameter space for $\{\pi_\theta, \rho_\theta\}$ for all $\theta \in \{A, T, U, N\}$. The approach follows the logic of partial identification: instead of selecting assumptions that point-identify either estimand, I recover the entire identified set of parameter combinations implied jointly by the observed moments and the model's structure (Kline and Tamer, 2023). This proceeds as follows.

1. I construct a fine grid over all possible population shares $(\pi_A, \pi_T, \pi_U, \pi_N)$ satisfying $\sum_\theta \pi_\theta = 1$.

2. For each candidate vector of population shares, I use the observed moments in Table 2 to solve for the implied treatment probabilities ρ_T and ρ_U that rationalize the observed conditional participation rates.

3. I then solve for ρ_A and ρ_N using the marginal probability of victimization, $Pr(Z=1)$.

4. Any parameter vector that violates logical or probabilistic constraints (e.g., treatment probabilities outside $[0, 1]$ or participation rates inconsistent with potential outcomes) is discarded.

5. For every remaining feasible parameter vector, I compute the corresponding ATE and ATT using the expressions above. Because the ATT depends on heterogeneous treatment prob-

ble and immaterial to the results. For simplicity and clarity, I omit this uncertainty from the explanation.

Participation Type	**N**	$\boldsymbol{Pr(Z=1)}$	$\boldsymbol{Pr(Y=1)}$	$\boldsymbol{Pr(Y=1\|Z=1)}$	$\boldsymbol{Pr(Y=1\|Z=0)}$
Attended Protest	149,728	0.21 (0.001)	0.08 (0.0007)	0.14 (0.002)	0.07 (0.0007)
Community Problem-Solving	94,022	0.19 (0.001)	0.36 (0.001)	0.43 (0.004)	0.34 (0.002)
Attended Community Meeting	149,405	0.21 (0.001)	0.29 (0.001)	0.32 (0.003)	0.28 (0.001)
Attended Political Meeting	137,308	0.20 (0.001)	0.16 (0.0009)	0.18 (0.002)	0.15 (0.001)
Would vote next week	127,256	0.22 (0.001)	0.81 (0.001)	0.85 (0.002)	0.80 (0.001)

Table 2: Table shows the number of respondents who provided a valid answer for each measure of political participation, N, the unconditional proportion of LAPOP respondents who report being the victims of crime (for the subset of respondents whose respective participatory response is non-missing), $Pr(Z = 1)$, the proportion of respondents who report engaging in any of the four participatory outcomes, $Pr(Y = 1)$, the proportion of victimized respondents who report engaging in any of the four participatory outcomes, $Pr(Y = 1|Z = 1)$, and the proportion of unvictimized respondents who report engaging in any of the four participatory outcomes, $Pr(Y = 1|Z = 0)$. Standard errors in parentheses.

abilities, while the ATE depends only on population shares (Proposition 1.1), differences between the two estimands across feasible vectors reveal the extent to which heterogeneous exposure patterns can reverse or attenuate conclusions about victimization's political effects.

This procedure yields a distribution of ATEs and ATTs, the identified set, that fully characterizes the causal effects consistent with the LAPOP data under heterogeneous treatment probabilities. The contrast between these identified sets illustrates the empirical relevance of the theoretical results in Section 1: whenever individuals who reduce participation after victimization are more likely to be victimized (i.e., $\rho_U > \rho_T$), the ATE and ATT diverge, often in magnitude and sometimes in sign. All algebraic derivations, feasibility constraints, and computational details are reported in Appendix C.

With this set of parameters, I can benchmark the expected change in outcome, a function of the ATT, and the frequency with which victimization happens relative to the shift suggested by the ATE if victimization were randomly assigned. I allow for the effect of victimization to mobilize or demobilize participation. That is, I assume $\pi_U \geq 0$.[9]

[9]However, Figure A4 in the Appendix reports the results when the effect of victimization on participation is assumed to be only increasing.

3 Results

After repeating the algorithm detailed in the previous section for each of the participatory outcomes, I obtained 12.1 million, 16 million, 20.6 million, 18.34 million, and 3.63 million feasible sets of $\{\pi_\theta, \rho_\theta\}\ \forall\theta \in \{A, T, U, N\}$ for the protest attendance, political meeting attendance, community meeting attendance, community problem-solving outcomes, and willingness to vote respectively. That is, internally consistent set of values given my framework that are also consistent with the LAPOP data that *bound* the actual value of the parameter. The difference in the sizes of the parameter sets depends on how informative the data is; for infrequent outcomes, fewer parameter sets could be consistent with the data.

For each set, I can then calculate the corresponding ATE and ATT of victimization on each of the four measures of political participation. Figure 1 plots the results. Each point in the graph corresponds to a plausible combination of π_θ, ρ_θ for a given participatory outcome. The red diagonal line that passes through the origin indicates the points where the ATE and the ATT are equal. For all the points below, the ATE is larger than the ATT. Conversely, the ATT is larger than the ATE for all points above that line. The point's color indicates the difference between the probability of becoming a victim for individuals who become mobilized due to victimization and the probability of becoming a victim for individuals who respond by decreasing political participation in the set.

As can be seen, the implied ATE is almost always larger than the corresponding ATT across all measures of non-electoral political participation. When participating in a protest is the outcome, the ATE is larger than the ATT in 99% of the parameter sets. When attending political meetings is the outcome; the same holds for 96% of the parameter sets. When it is attending community meetings, the ATE is larger in 83% of the parameter sets. When participating in community problem-solving activities is the outcome, the percentage equals 64%. Further, we can see that the points above the red diagonal line correspond to the largest differences between $\rho_T - \rho_U$. For the ATT to be larger than the ATE, selection into victimization has to be strongly correlated with the

treatment, making individuals more participative.

While the majority of the ATEs compatible with the data are positive for all four measures of non-electoral participation (85% for protest attendance, 79% for political meeting attendance, 67% for community meeting attendance, and 63% for community problem solving), the converse is true for the ATTs across measures. Specifically, only 9% of the ATTs are positive for protest attendance, only 13% for political meeting attendance, 29% for community meeting attendance, and 46% of the cases for community problem-solving. Even for the most frequently reported outcome, community meeting attendance, in most cases, the ATTs compatible with the data are negative. As reported in Table 2, the larger share of positive and ATTs reflect the larger conditional probabilities of participating in these activities given victimization implied in the respondents' answers.

The results from willingness to vote if elections were held the following week, show that electoral participation, to the degree that this is a construct-valid question, shows a markedly different pattern than non-electoral forms of engagement. While the ATE exceeds the ATT across most non-electoral participation measures, this relationship reverses for voting intention. Here, the ATE is greater than the ATT in just 1% of parameter sets. More strikingly, in 89% of parameter sets compatible with the data, the ATE of victimization on willingness to vote is *negative*, while in 95% of the sets, the ATT is positive. The selection parameters show somewhat attenuated differences - for the ATE to exceed the ATT, ρ_U must be larger than ρ_U, yet the visualization reveals predominantly yellow dots, indicating less extreme differences between these parameters.

These findings align with extant empirical evidence. While work targeting the ATE of crime on voting often uncovers negative effects (Ley, 2018), Sønderskov et al. (2022) finds a positive ATT of participation in voting but a negative ATE of participation in voting when looking at data from Denmark. Consequently, these results can help discipline the seemingly disjoint empirical results from recent research on the electoral consequences of crime.

Recall that the ATE of victimization will depend only on the relative size of each stratum affected by the treatment, π_U and π_T. Conversely, the ATT is a function of the stratum-specific

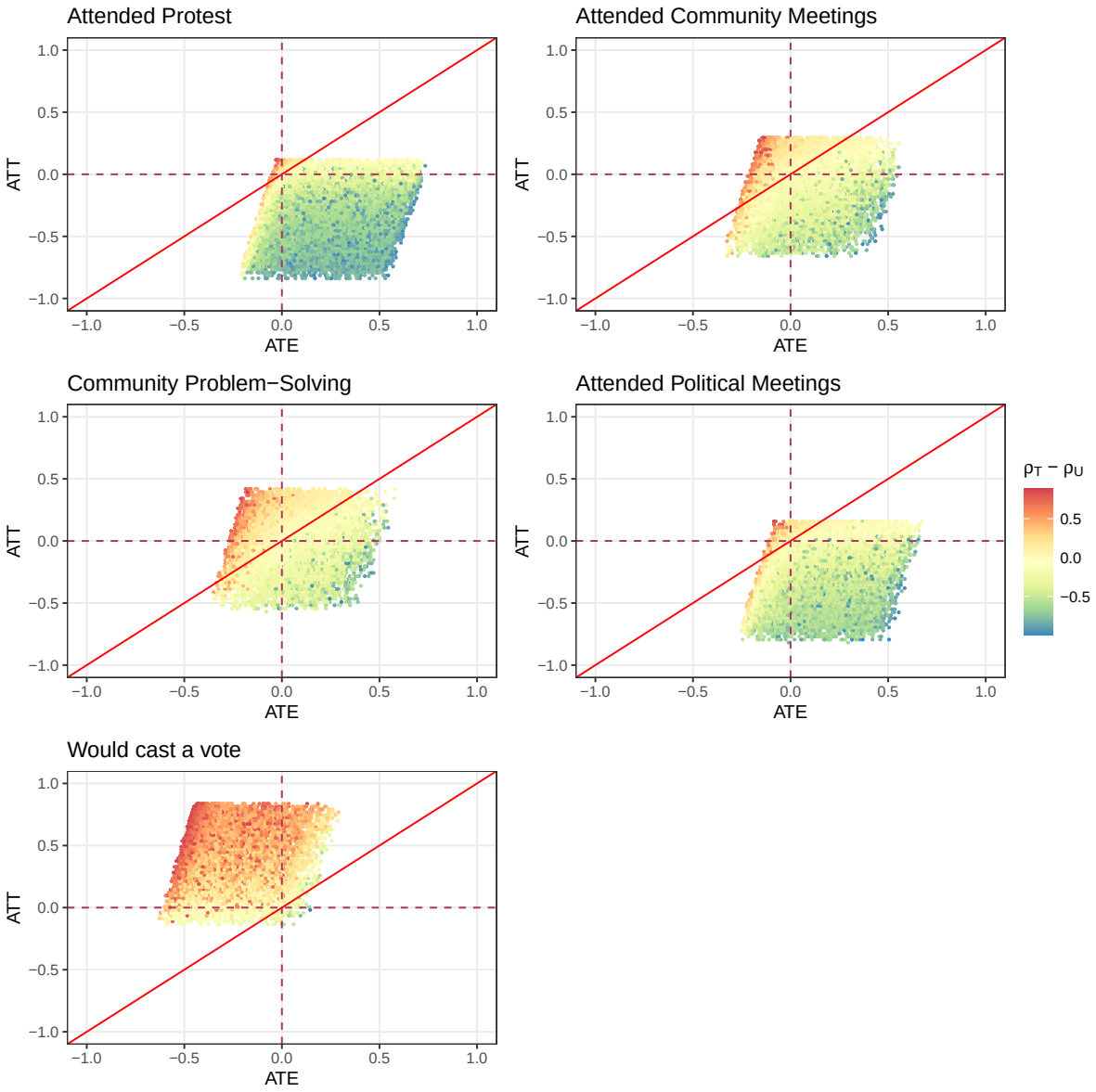

Figure 1: Figure shows the combination of potential ATTs and ATEs of victimization on four measures of political participation, conditioning on observed participation, and criminal victimization (LAPOP, 2022).

probabilities of victimization. The divergence/similarity between ρ_U and ρ_T will result in a smaller or larger discrepancy between the ATT and the ATE. The left panel of Figure 2 exemplifies this. It plots the cumulative proportion with which each difference in magnitudes between $\pi_T - \pi_U$ appears in the candidate sets, while the right panel plots the equivalent quantity for the difference between $\rho_T - \rho_U$ for each of the parameters sets compatible with the data.

As can be seen, the proportion of sets for which $\pi_T - \pi_U \leq 0$ is equivalent to the proportion of sets for which the $ATE \leq 0$ exactly because the ATE depends exclusively on these two shares. Further, observe that the difference between these shares is almost always positive for non-electoral participation, as expected, given the more frequent positive ATEs compatible with the data. For protest attendance, the median difference between the two proportions is 16.5 pp; for community problem solving, it is 9.7 pp; for political meeting attendance, it is 12 pp; and for community meeting attendance, it is 10.5 pp. In contrast, for willingness to vote the median difference is *negative*; 27 pp.

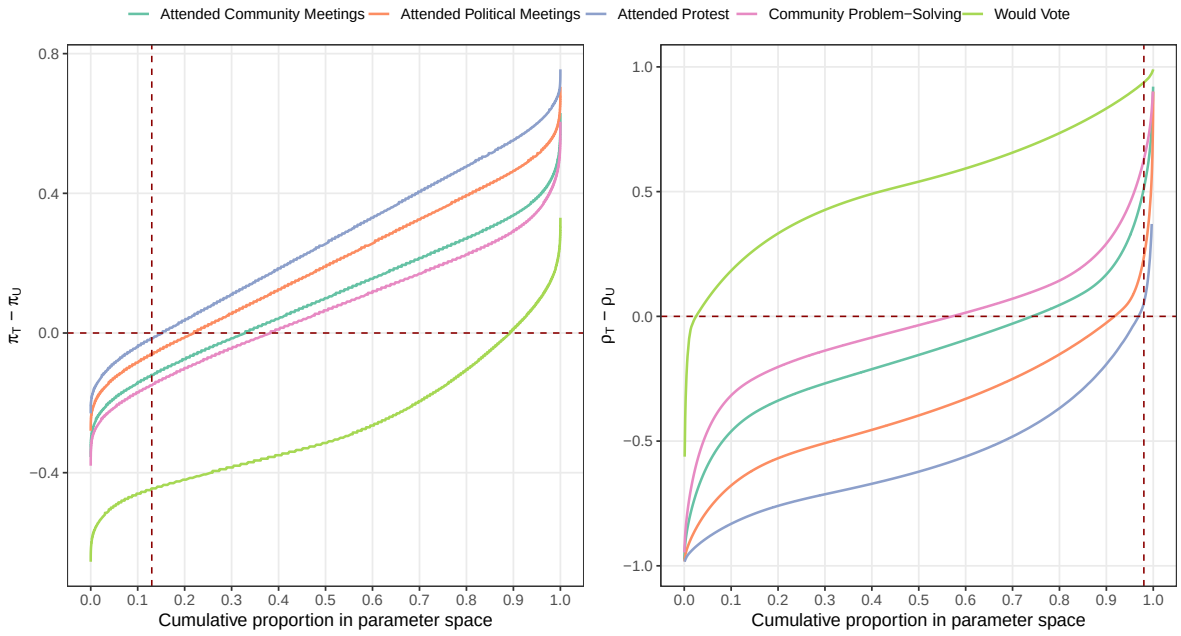

Figure 2: Figure shows the cumulative proportion of sets for which the difference between $\pi_T - \pi_U$ (left panel) and $\rho_T - \rho_U$ (right panel) is equal to the y-axis, given the observed rates of victimization and participation in the LAPOP survey.

Conversely, the right panel shows that despite being fewer in the population, the risk of victimization for individuals who become demobilized in non-electoral participation from crime is likely larger. The proportion of sets for which $\rho_T - \rho_U \leq 0$ is close, but not the same, as the proportion of sets for which $ATT \leq 0$ since the corresponding π scales the ATT. However, notice that the difference in these two probabilities is almost always *negative* for non-electoral participation. For protest attendance, the median difference is -63.7 pp; for community problem solving, it is -4 pp; for political meeting attendance, it is -40.7 pp; and for community meeting attendance, it is -16.1 pp. In contrast, the difference is positive for willingness to vote in a hypothetical upcoming election; 52 pp.

The results suggest that if victimization were random, it would likely increase non-electoral participation. These findings align with Bateson (2012) and the field's prevailing understanding based on research designs that seek to equalize treatment probabilities between treatment and control.[10] The magnitude of this participation boost, or how participation would change empirically, would depend on the overall risk of becoming a victim. However, the results also reveal that individuals who would participate less after experiencing crime are disproportionately likely to become victims. The identified sets suggest that crime reduces all four types of non-electoral political participation in the region. This presents a striking contrast with (intended) electoral behavior: while crime, were it random, would reduce (intended) voting, given the actual patterns of victimization, it likely increases willingness to engage in voting.

What are the magnitudes of the changes in participation? Recall from expression (**??**) that the change in the outcome from treatment with stratum-specific probabilities of occurring can be backed out simply by scaling the ATT by the unconditional probability of receiving treatment. Table 3 synthesizes all the results. Participation in protests could decrease by as much as 18 pp, engagement in community problem-solving could decrease by 11 pp, political meeting attendance

[10]Results in Bateson (2012) are based on one wave of the LAPOP data. Section A2.3 in the Appendix replicates the results from Bateson (2012) with repeated cross-sections of the LAPOP data, as well as within-country analyses.

		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	0.15	-0.84	0.12	0.91	-0.18	0.02
Community Problem-Solving	0.19	-0.38	0.61	0.37	-0.57	0.42	0.53	-0.11	0.08
Attended Political Meetings	0.20	-0.28	0.70	0.21	-0.82	0.16	0.87	-0.17	0.03
Attended Community Meetings	0.21	-0.36	0.63	0.32	-0.68	0.30	0.71	-0.14	0.06
Would vote next week	0.22	-0.65	0.33	0.89	-0.13	0.84	0.05	-0.03	0.18

Table 3: Results from all plausible parameter sets compatible with the LAPOP data, without constraining the values of ρ_U or ρ_T. $\Delta Y = ATT \times Pr(Z=1)$ represents the average treatment effect in the population. By definition, $Pr(\Delta Y) < 0$ is the same as $Pr(ATT < 0)$.

could decrease by 17 pp, while community meeting attendance could decrease by as much as 14 pp. Willingness to vote next week, however, at worst could decrease by 3 pp. Conversely, conditional on the data, protest attendance could *at best* increase only by 2 pp; community problem solving by 8 pp; political meeting attendance by 3 pp; and community meeting attendance by 6 pp. Hypothetical williness to vote could increase by at most 18 pp. The *upper bound effects* within the identified set of non-electoral participation estimands are of a comparable magnitude to, and often smaller than, the ATEs identified in Bateson (2012).

One potential concern is that the identified sets from these simulations reflect peculiarities that are only generalizable to the LAPOP data instead of crime and participation in the region more broadly. For example, we might be worried that LAPOP over-represents urban areas, under-represents dangerous areas, or generally results in survey responses that do not generalize. To address such concerns, I analyze data from Dorff (2017), which 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. I present the results in Table A6 in the Appendix. I find strikingly similar results that point in the same direction: the identified sets suggest that while the ATE is likely positive, the ATT is almost certainly smaller and likely negative.

Before proceeding, an important clarification is needed. How can this framework accommodate crime-spurring voting while depressing non-electoral participation? Should all results not point

in the same direction? The short answer is “no.” First, there is nothing in the framework that requires people to be in the same strata when it comes to different outcomes. A person might vote less but participate more in community outreach after victimization, or vice versa. Further, an individual might participate less in protests while their voting behavior remains unaffected. Second, notice the relative differences in base rates: many more people vote than participate in protests, and criminal victimization is relatively rare. Most people who vote are logically not doing so *because* they were victimized since victimization is not as common as voting. Thus, within this framework, we can think of most people voting as being either in the “always participate" or the “participate if not victimized" strata. This would be consistent with what we know about the “stickiness" of voting (Coppock and Green, 2016) and the behavioral implications of electoral institutions such as mandatory voting that constrain fluctuations in voting. However, these same individuals can simultaneously belong to different strata when considering the effect of crime on various forms of non-electoral participation. So, a person might vote regardless, but potentially, they could participate only if treated. In summary, we need not be looking at the same people in the same strata when comparing across outcomes.

4 Panel Evidence from Mexico

The previous section’s results give us a distribution of the frequency, magnitude, and direction of plausible causal effects compatible with the LAPOP data. It shows that individuals who would be demobilized from crime are more likely to be exposed to it to begin with. Consequently, the compatible ATTs of victimization on non-electoral participation are smaller than the ATEs and overall negative. The results also support findings from canonical work showing a positive ATE of victimization on crime since the “participate only if treated” stratum generally makes up a larger share of the population in the simulations than the “participate only if untreated” stratum while being *less* at risk of crime. Suggestive evidence however points to different effects of crime on electoral participation, often likely spurring it conditional on who is victimized. In this section, I

analyze a panel survey to provide evidence of who becomes a victim in a real-world setting and how victims react to victimization, moving from the plausible to the empirical.

I use a quarterly rotating panel survey of Mexican city residents that asks questions related to crime and insecurity, the National Survey of Urban Public Safety, ENSU, for its name in Spanish (INEGI, 2024). This survey has been conducted four times per year since 2013[11] and, starting in 2017, has included direct questions regarding personal victimization. This rolling survey panel allows me to observe individuals at five different points over 15 months. In each quarter, respondents are asked about experiences with the state, crime, and conflict, and in at least two waves per year, they are asked about direct victimization. During each survey wave, around 20% of respondents are first-time respondents. I keep all of the survey waves from 2017, when crime victimization was first incorporated, until the last quarter of 2023, resulting in 560k responses from 174K unique respondents collected over 27 quarters.

Unlike the LAPOP data, the panel nature of the data allows for comparisons of attitudes and behavior related to crime exposure and reactions to crime, both between and within individuals. I compare the attitudes and behavior of non-victims with victims *before* their first reported instance of victimization, as well as conduct a within-individual analysis to estimate how people's attitudes and behavior change due to victimization after parsing out all time-invariant individual effects.

Like the LAPOP data, ENSU is a survey, and 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 (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, reported in Appendix E. Reassuringly, these show

[11] Except the second quarter of 2020 due to the COVID emergency.

self-reported victimization is indeed increasing with criminal victimization, as we would expect if survey responses of victimization mapped onto its actual occurrence.

Using the ENSU data, I first focus on heterogeneous exposure to victimization. To assess, I identify questions related to mobility, exposure to crime, fear of crime, and conflicts. I use self-reported measures of exposure to conflicts, instances of observing criminal or violent behavior, feelings of unsafety, and mobility patterns. Exposure to conflict and witnessing criminal behavior directly map onto the risk of victimization. Further, since the spatial distribution of crime is not constant in a city, researchers have shown that mobility patterns are important in increasing and decreasing individuals' exposure to crime, and individuals who move a lot within a city tend to be more exposed (Rader, 2004; Browning, Pinchak and Calder, 2021; Boggs, 1965). Fear of crime has also been a central correlate of the risk of victimization explored in criminological research. Findings show that vulnerability to criminal victimization increases people's fear of crime (Rader, 2004). Similarly, fear of crime has been posited to increase, the lesser capacity to avoid crime individuals possess (Guedes, Domingos and Cardoso, 2018).

Second, I focus on how higher exposure to crime covaries in attitudinal and behavioral profiles suggestive of differential rates of civic and political engagement before victimization. While ENSU does not directly inquire about political participation, it does report behavioral and attitudinal measures that recent research has linked to different participation rates. Political participation and civic engagement require people to interact with neighbors and authorities, familiarize themselves with community problems, and engage with strangers more often. Specifically, I again focus on mobility, fear, and exposure to crime and conflict. Research shows that extroverted (Gerber et al., 2011), well-networked, community-oriented (Ley, 2022; Skogan, 1986), and risk-prone (Kam, 2012) individuals more often participate in electoral and non-electoral politics. I leverage the mobility measures and self-reported instances of witnessing diverse criminal phenomena in their daily lives as measures of extraversion, community engagement, and risk acceptance.

Last, I concentrate on how victimization changes attitudes and behaviors. To do so, I exploit

the within-individual intertemporal variation in the data to target the ATT. I use two estimators to do so: a simple two-way fixed effect estimator and the fixed-effect counterfactual estimator with equal unit weights proposed by Liu, Wang and Xu (2022). I focus on measures individuals take to prevent crime, self-reported fear of crime, trust in law enforcement institutions, and mobility. I reemphasize that extraversion, mobility, and risk-proneness are systematically related to more political participation and community engagement (Gerber et al., 2011; Kam, 2012; Ley, 2022). At the same time, victimization has been shown to increase people's fear of crime (Rader, 2004) and risk aversion (Moya, 2018), in turn resulting in less political and civic engagement (Skogan, 1986; Ley, 2018).

4.1 Exposure to Crime

First, I subset the data and retain only respondents who never reported criminal victimization and respondents who reported criminal victimization in some, but not all, survey waves. For the latter, I retain responses only before their first reported instance of victimization. Consequently, I compare the responses of this group of future victims *before they reported victimization* with those of their never-victimized neighbors — either from the same sampling unit or the same locality — in the same quarter.

I estimate

$$PM_{i[q,g]} = \beta_1 * FutureVictim_{i[q,g]} + \theta_{q \times g} + \epsilon_{i[q,g]} \tag{1}$$

where $PM_{i[q,g]}$ is respondent $i's$ self-reported behavior, experiences, or attitude in year-quarter q, and neighborhood/census tract or locality g, or a binary measure that takes the value of 1 if respondent i reports that type of behavior or experience and 0 otherwise. $FutureVictim_{i[q,g]}$ is a binary variable that takes the value of 1 when respondent i will report having been a victim of crime in a future survey and 0 otherwise. $\theta_{q \times g}$ represent either $Neighborhood \times Quarter$ fixed effects, or $Locality \times Quarter$ fixed effects. $\epsilon_{i[q,g]}$ are robust errors clustered at the primary sampling unit.

Figure 3 shows results. When we compare future victims with never-victims living in the

same locality in that same quarter (dark blue) or the same small neighborhood (light blue), we see that future victims report having more conflicts, including conflicts that resulted in physical violence, conflicts with authorities, conflicts with the police, and conflicts with strangers on the street, *before* being victimized. Future victims also witnessed gangs, robberies, and vandalism and heard gunshots more frequently. They also report feeling unsafe in the city, streets, public transport, and even their house. Last, they report moving more around their city; they leave home more frequently.

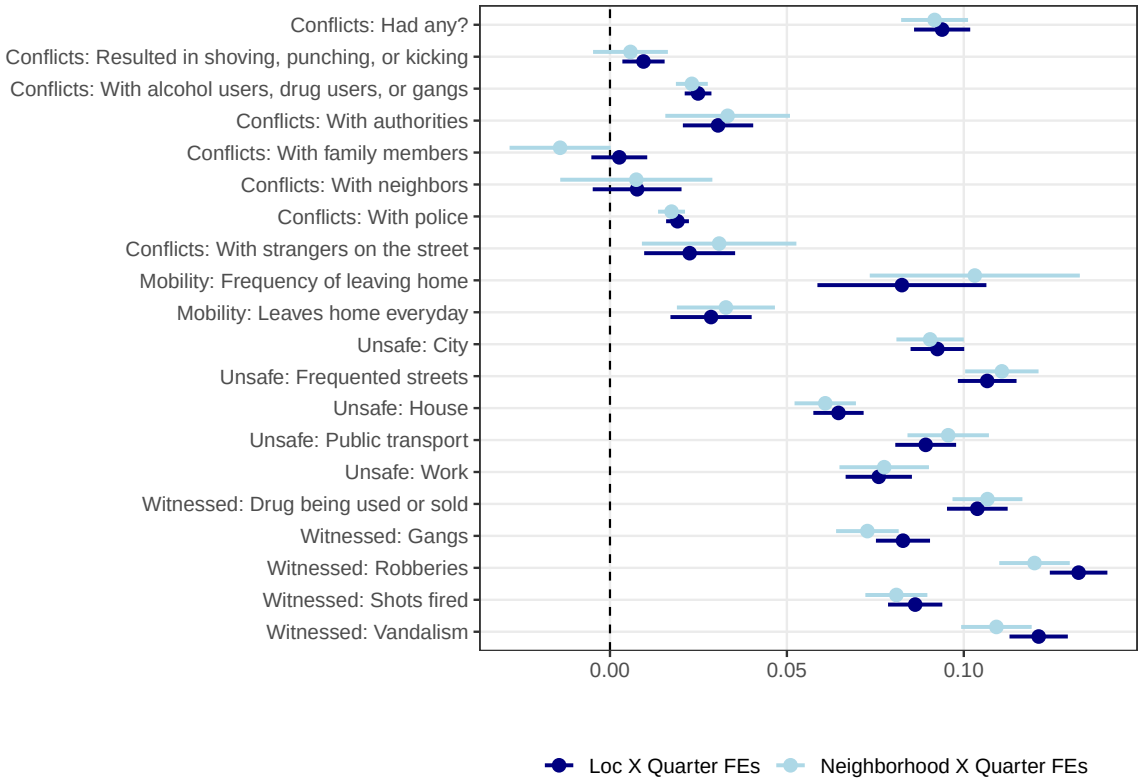

Figure 3: Figure shows the cross-sectional difference between soon-to-be victims and non-victims living in the same locality (dark blue) and the same neighborhood/census tract (light blue) on self-reported outcomes relating to exposure to crime. Robust errors clustered at the primary sampling unit.

Increased mobility exposes individuals more frequently to crime (Rader, 2004; Browning, Pin-

chak and Calder, 2021; Boggs, 1965), while increased fear of crime correlates with more (subjective) vulnerability to criminal victimization and less perceived capacity to avoid becoming a victim (Rader, 2004; Guedes, Domingos and Cardoso, 2018). Similarly, fear of crime has been posited to increase. Last, explicit differences in exposure to criminal and violent behavior should correspond directly to an increased probability of becoming a victim. Overall, the results link future victimization with increased exposure to crime and show victims and non-victims were differentially exposed to crime even before becoming victims.

Additionally, these findings point to a majority of victims who can be thought of as coming from the "participate only if untreated" stratum. Because before victimization, future victims were more mobile, witnessed more disruptive behavior in their community, and had more conflicts with strangers, neighbors, and law enforcement agents, this subset of respondents was likely more extroverted, risk-accepting, and embedded in their community. Such attitudinal and behavioral profiles are consistent with those research has linked to higher rates of political participation, relative to their never victimized neighbors (Gerber et al., 2011; Ley, 2022; Kam, 2012).

4.2 Reactions to Crime

Additionally, I exploit the within-individual intertemporal variation in the data to target the ATT and provide evidence of how victimized individuals react to victimization.

$$PM_{i[q]} = \beta_1 * Victim_{i[q]} + \theta_i + \gamma_q + \epsilon_{i[q]} \tag{2}$$

where $PM_{i[q]}$ is either a continuous measure of respondent $i's$ self-reported behavior, experiences, or attitude in year-quarter q, and neighborhood or locality g, or a binary measure that takes the value of 1 if respondent i reports that type of behavior, experience or attitude and 0 otherwise. $Victim_{i[q]}$ is an indicator variable that takes the value of 1 when respondent i reports having been victimized in the past six months in quarter q and 0 otherwise. Because victimization is asked every six months, while response variables are asked every three months, each respondent's vic-

timization report is extended to cover the prior period. θ_i are individual fixed effects while γ_q are period fixed effects. $\epsilon_{i[q]}$ are robust errors clustered at the primary sampling unit. I use a TWFE estimator of the ATT as well as the counterfactual estimator with equal weights proposed by Liu, Wang and Xu (2022).

Figure 4 shows the results. It plots the ATT of victimization on several behavioral and attitudinal outcomes, per specification (2). It shows that victimization changed attitudes and behaviors in a way that is consistent with individuals becoming demobilized after becoming victims. The results are consistent regardless of the preferred estimator.

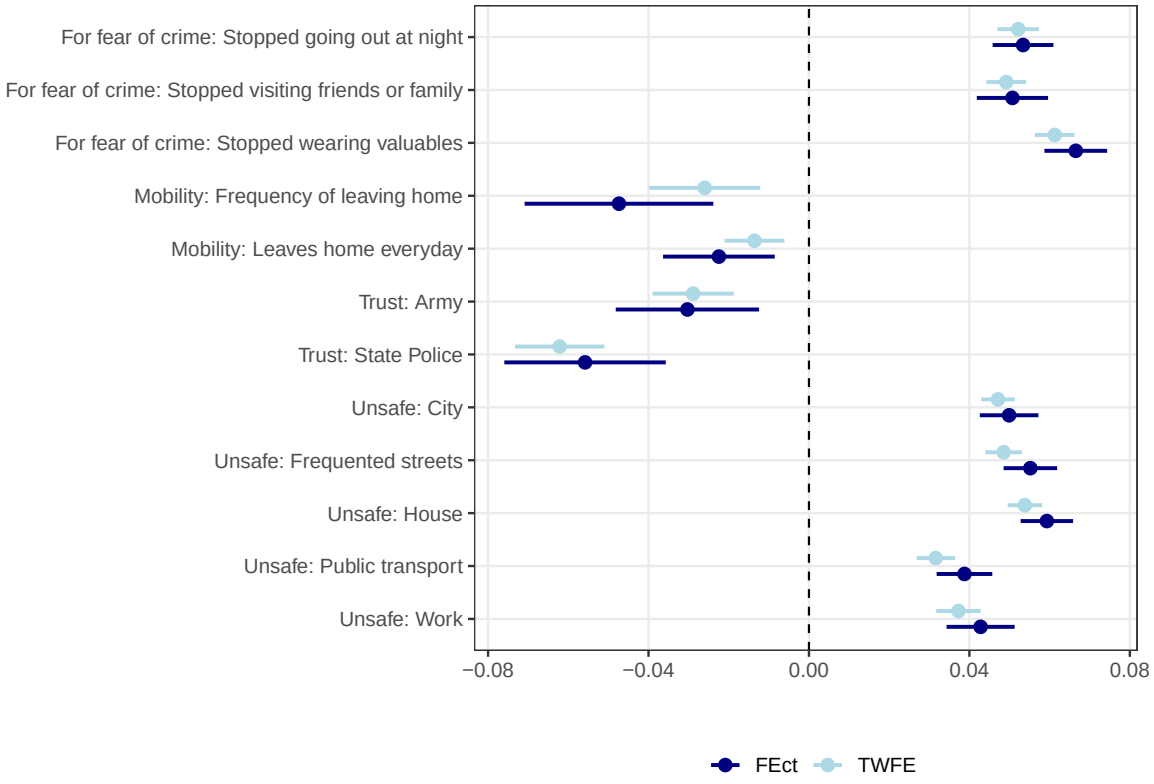

Figure 4: Figure shows the ATT of victimization on self-reported behavioral and attitudinal measures when estimated with the fixed-effect counterfactual estimator from Liu, Wang and Xu (2022) (dark blue) and a two-way fixed effect estimator (in light blue) Robust errors clustered at the primary sampling unit.

Compared to their behavior before becoming victims of crime, respondents report taking more steps to prevent crime, including leaving home less frequently and visiting friends and family less often. Along the same line, they report being less mobile after victimization and are less likely to say they leave home every day. These two findings suggest victims become —generally— less extroverted. Further, victims report trusting law enforcement authorities less, being more afraid of crime, and taking more steps to prevent victimization. These latter results suggest a reduction in people's risk acceptance and the adoption of more constrained behavior, consistent with less political and civic engagement. The participatory consequences of such behavior change should unambiguously lead to less participation generally, including participating in political protests and community organizing specifically (Gerber et al., 2011; Kam, 2012; Ley, 2022; Skogan, 1986; Ley, 2018). These findings further indicated that victims could be conceptually thought to come mostly from the "participate only if untreated" stratum.

Overall, the findings from the Mexico survey show that the risk of victimization is heterogeneous and higher for individuals who become demobilized after becoming victims. Consequently, we should empirically expect to recover ATEs larger than the theoretical ATT of victimization on political participation measures, especially participation measures that are less inelastic due to the institutional environment like non-electoral participation. This insight is consistent with the results from the prior LAPOP analysis.

Together, the results from the LAPOP and the ENSU analyses question the conventional wisdom about criminal victimization functioning as a catalyst for participation in the developing world (Bateson, 2012). Because such conclusions are based on research designs that implicitly or explicitly target the ATE, they speak only to the counterfactual question, "How would participation change due to criminal victimization if victimization were random?" While "no one chooses to become a victim of crime," as Bateson (2012) points out, results from this paper show that certain individuals will be systematically more exposed to crime than others in ways that covary with how they respond politically to becoming victims. Because Latin American citizens who participate

less after victimization more frequently become victims, victimization is a force that dampens, not fosters, active citizenry in the region.[12]

5 The Participatory Consequences of Other Forms of Violence

The results from the previous section show that our understanding of the political consequences of crime critically depends on our estimand of choice. When we target the ATT, results upend our understanding of crime as a participation-spurring treatment, given the heterogeneity in exposure and reactions to it. In the following sections, I leverage this framework and empirical strategy to exemplify how this insight is not limited to crime victimization but extends to other forms of violence that have been studied in political science.

5.1 Civil War Violence

Studies of civil war violence have generally found that exposure to civil war violence, whether direct or indirect, makes victims more likely to participate in politics later on by engendering in them psychological processes, such as post-traumatic growth and increased prosociality (Bellows and Miguel, 2009; Blattman, 2009; Voors et al., 2012).

One such paper is Blattman (2009). In it, the author examines how the forced recruitment of youths into the LRA insurgency in Northern Uganda shapes this group's downstream participatory decisions. To do so, the author leverages theorized quasi-random variation in abduction and survey data to estimate the ATE of abduction on political outcomes. Results indicate that abduction led to significantly increased political participation, specifically a 27% increase in the likelihood of voting, as well as a doubled likelihood of later becoming a community leader. The author attributes the findings to post-traumatic growth experienced by former abductees. I reassess the results through my principal strata framework. I repeat the process detailed in the previous sections and derive the

[12]This results suggest that $\rho_U \geq \rho_T$ in Mexico. In section A4.2 in the Appendix, I further constrain the identified set to only those where that inequality holds — generally in Latin America and specifically in Mexico. I find that the main findings for non-electoral participation become more accentuated, while the identified set for electoral participation now resembles that of non-electoral participation more closely.

identified set of ATTs, ATEs, and ρs consistent with data from Blattman (2009). In Section A6.1 in the Appendix, I report results.

For future electoral participation, 77% of the parameters in the identified set of ATEs consistent with the data from Uganda are positive. Interestingly, unlike the results for the LAPOP analysis, we have a very similar proportion of positive ATTs in the identified set—75%. However, the results regarding non-electoral participation are mixed. Only in 35% of the sets does such experience lead to higher odds of becoming a community organizer if abduction were random (ATE) despite the paper's point estimate of estimand being positive and significant. 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. If this sort of civil war violence were random, voting and certain non-electoral participation actions would likely increase, but if it were heterogeneously experienced, then abduction likely depressed civic engagement in Uganda by a substantial margin.

Whether abduction was, in fact, random is an assumption that requires theoretical justification. These analyses help to clarify the epistemological implications of departures from this assumption. If abduction were indeed random, as the author argues, then the ATE would equal the ATT, and the two estimands would be the same. In that case, the ATE would be both theoretically relevant and empirically important precisely because it would capture the same quantity as the ATT. Research designs targeting either estimand would then be equally suitable for measuring actual changes in participatory outcomes. The fact that these causal parameters converge under random assignment highlights how the mechanism of treatment assignment fundamentally determines which estimand best captures the substantive question we are trying to answer.

5.2 Contact with the Police

That police engage in statistical discrimination while performing their duties is a well-established empirical regularity (Ba et al., 2021; Mummolo, 2017; Owens and Ba, 2021). The literature on

the political consequences of police contact presents mixed empirical results and competing theoretical frameworks. Some researchers find that contact with police teaches individuals that they are worthless and meaningless—what Weaver and Lerman (2010); Soss and Weaver (2017) term the "interpretative effects" of police contact. Conversely, others find that police contact, whether direct or indirect, can stimulate electoral participation (Walker, 2014, 2020).

Walker (2020) represents the latter perspective. Using two different datasets, Walker (2020) finds that direct and indirect contact with the police is associated with higher rates of both electoral and non-electoral participation. Throughout her analysis, Walker (2020) targets the ATE as the primary causal estimand. To reassess these findings through the heterogeneous treatment probability framework, I employ one of the same data sources—-the National Crime and Politics Survey (NCPS), a nationally representative U.S. survey conducted in the fall of 2013.

I examine the parameter sets compatible with the NCPS data, focusing exclusively on direct police contact to maintain consistency with my other analyses. For dependent variables, I use all available measures of electoral and non-electoral political participation from the preceding 12 months. The results, reported in Table A10, reveal a pattern that complicates Walker's original findings. Similar to my simulations using LAPOP data, the NCPS analysis reveals 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.

Our theoretical understanding of police contact inherently acknowledges statistical discrimination: certain communities and individuals are disproportionately targeted. Given this understanding, the ATT likely offers a more relevant parameter for interpreting these results than the ATE. The simulation results are consistent with a scenario where interactions with police ultimately depress political participation among those most likely to experience such contact despite the positive

ATEs reported in previous research.

6 Discussion

This paper upends our understanding of criminal violence as a force engendering civic and political engagement. In so doing, it emphasizes the fundamental importance of the choice of estimand when doing causal quantitative research. While theory has long recognized that violence mobilizes some individuals while demobilizing others, this duality rarely influences which causal estimand researchers target in empirical work. To address this gap and formalize the knowledge we have of violence's nature, I proposed a principal strata framework with heterogeneous treatment probabilities that provides a more nuanced foundation for estimand selection. Using this framework, I revisited seminal research on the political consequences of criminal victimization to demonstrate how heterogeneous treatment probabilities reshape our understanding of the participatory effects of crime in Latin America.

While canonical research suggests victimization produces a more active citizenry through increased non-electoral participation, my analyses reveal a more complex reality. I demonstrate that this conclusion holds only if the crime is randomly assigned —that is, if the ATT and ATE are equivalent. However, ancillary results from Mexico strongly suggest this is not the case. This finding changes our understanding of crime's political implications, which are a central concern for citizens in the world's most violent region. Similar patterns emerge when reconsidering results from seminal research on civil war violence and unwanted police contact. When shifting our focus from the ATE to the ATT, our substantive interpretation not only changes but even occasionally reverses; the silver lining of violence as spurring political participation disappears, and violence becomes a force that instead produces less civic and political engagement empirically.

7 Conclusion

How does violence or its threat shape our politics? Unevenly. This is the nature of violence —it is experienced and perpetrated differentially. People react distinctly to it; some become enraged, others alienated. Where one finds in it a reason to march, another retreats. In this paper, I have argued that to understand how participation has changed on the ground as a consequence of violence, we need to think of who experiences violence in the first place and how these people are likely to react.

What determines an individual's risk of experiencing violence and their reaction to it? The answer is the entire surrounding political structure. This structure shapes both the stratum-specific probabilities of treatment and constrains how people are able to respond to victimization. Because populations exposed to violence vary across contexts, findings from criminal violence in Latin America should not be presumed applicable elsewhere. This underscores the necessity for strong, explicit theoretical arguments justifying claims about the generalizability of violence's effects across different populations and political environments.

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