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Research Note

Crime and Participation

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Does exposure to crime mobilize or demobilize political participation? We report findings from a preregistered design, leveraging data from an original survey of 21 major U.S. cities and five waves of the Cooperative Election Study. We assess the relationships between direct victimization, local (ZCTA-level) crime rates, and perceived neighborhood safety and two outcomes: turnout (including validated turnout) and non-electoral participation. We find that direct victimization is significantly associated with more non-electoral engagement but lower turnout. The negative estimated effects of victimization on turnout exceed the magnitude of positive effects associated with common get-out-the-vote efforts. We find only limited evidence that crime rates are associated with participation. Perceived neighborhood safety is associated with higher rates of non-electoral, but not electoral, participation. Our findings offer new, comprehensive evidence on the relationship between crime exposure and political participation and illustrate the importance of distinguishing between types of exposure and forms of participation.

Why do some people participate in politics, while others abstain? A rich body of research has demonstrated how factors such as individual-level characteristics and resources (Burden et al. 2017; Verba, Schlozman, and Brady 1995), socialization dynamics (Carlos 2021; Holbein 2017; Jennings, Stoker, and Bowers 2009), and direct political communications (Green and Gerber 2019) shape patterns of political engagement. Institutional and environmental conditions—including sudden demographic shifts (Enos 2016), direct and indirect contact with the criminal justice system (Lerman and Weaver 2014; Walker 2014), and broader patterns of state capacity and policy feedback (Mettler 2005)—also influence whether and how people participate in politics.

In this vein, some researchers have considered how exposure to crime influences political participation. The motivation is straightforward: crime remains one of the most visible and politically charged features of everyday life. It is an issue that Americans consistently rank among their top concerns and expect the government to address (Gallup N.d.) and there is evidence that exposure to crime is associated with changes in emotions linked to participation (Albertson and Gadarian 2015; Orth et al. 2008; Skogan 1987; Valentino et al. 2011), support for incumbent officials (Cummins 2009; Markarian and Kousser 2025), and shifts in public opinion across a range of policy domains (Enns 2016; Krause 2014).

What is less clear is whether exposure to crime mobilizes or demobilizes individuals. Some argue that victimization and community violence heighten personal stakes, elicit galvanizing emotional responses, and create traumatic experiences that induce community-seeking behavior, mechanisms that may mobilize participation. For example, exposure to crime may increase feelings of threat and vulnerability (Bateson 2025; Berens and Dallendörfer 2019; Laterzo 2021; Sønderskov et al. 2022), and lead people to engage in political action as a way to "mitigate the emotional consequences of victimization" (Bateson 2012, 571). However, exposure may also demobilize by eroding trust in institutions, lowering political efficacy, reinforcing perceptions that the state is unresponsive to disorder, or making people reluctant to spend time in public (Berens and Dallendörfer 2019; Ley 2018; McCarthy, Hagan, and Herda 2025; Trelles and Carreras 2012).

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Empirically, the literature offers results as mixed as these theoretical expectations. Some studies find that crime mobilizes engagement (e.g., Bateson 2012; Sønderskov et al. 2022); others find demobilizing effects, particularly in contexts of persistent or concentrated violence (e.g., Jones Forthcoming; McCarthy, Hagan, and Herda 2025; Trelles and Carreras 2012). Against this backdrop of empirical inconsistency, we take a different tack: rather than offering a new theoretical account, we seek to clarify existing debates through a comprehensive empirical approach. We examine the relationship between multiple forms of crime exposure and both turnout (self-reported and validated) and non-electoral participation (like contacting an elected official or attending a protest), all within a preregistered research design.

We draw on an original survey of residents in 21 of the 30 most populous U.S. cities and respondents from those same cities in the Cooperative Election Study (CES; 2016–24). Approximately 33.5 million Americans (10% of the total U.S. population) reside in these 21 cities. We pair responses with hyper-local administrative crime rates constructed from public-facing databases. We find that direct victimization is associated with higher levels of non-electoral participation but lower levels of turnout. By contrast, associations between contextual crime rates and participation are limited and inconsistent, emerging only in select specifications and exploratory tests, where we find some evidence that higher violent crime rates—but not property crime rates—are associated with reduced turnout. Additionally, we find that *perceived* neighborhood safety is positively associated with higher rates of non-electoral participation, but is unrelated to turnout. Our results are robust across modeling specifications and datasets. Further, in preregistered exploratory analysis, we show that they are insensitive to reasonable alternative measurement strategies, persist when we control for the amount of time respondents say they spend outside of their homes, and replicate in an additional survey of residents of Cook County, Illinois. In unregistered robustness tests, we find that our victimization and perception findings are robust to the inclusion of ZCTA fixed effects and that effect sizes remain similar in magnitude when using two-wave panel data, though they do not reach statistical significance in these latter tests (likely due to limited statistical power).

Additionally, in unregistered exploratory analysis, we find suggestive evidence that the apparent demobilizing effects of victimization on voter turnout are primarily driven by individuals who were attacked rather than those who suffered vehicle theft, stolen items, or break-ins. Similarly, when we separately test the association between violent and property crime rates and participation, we find that violent crime rates are significantly associated with decreased turnout, while property crime rates are not. Finally, we report exploratory analyses that may inspire future research. We assess whether the relationship between exposure to crime and participation systematically varies across racial groups and find little evidence that it does. We also explore the role of institutional trust. We find little evidence that trust mediates the relationship between crime exposure and participation, but suggestive evidence that pre-victimization levels of trust moderate this relationship.

This study contributes to ongoing debates in three ways. First, we simultaneously consider three measures of crime exposure—contextual crime rates, direct victimization, and perceived neighborhood safety. Second, we examine both turnout and non-electoral participation in two independently-fielded surveys. Third, we use high-resolution administrative crime data alongside validated measures of individual turnout. Embedding these contributions in a preregistered framework limits researcher degrees of freedom, overfitting, and post-hoc theorizing, thereby enhancing the credibility of our findings. Although our approach has many benefits and we take numerous steps to confirm the robustness of our findings, our observational data do not position us to make strong causal claims. Our contribution instead lies in providing a comprehensive descriptive account of the relationship between crime exposure and political participation.

CONCEPTUALIZING EXPOSURE TO CRIME AND PARTICIPATION

One plausible explanation for the competing findings in existing work pertains to how exposure to crime is conceptualized. Scholars who focus on direct victimization often find a mobilizing relationship (Bateson 2012; Berens and Dallendörfer 2019; Laterzo 2021; Sønderskov et al. 2022), generally positing that victimization affects participation through ego-relevant mechanisms: victims may seek protection, advocate for justice, find solidarity, or make sense of their experiences through collective engagement. Victimization can also prompt emotional responses like anger (Orth et al. 2008), fear (Skogan 1987), and anxiety (Albertson and Gadarian 2015), which are associated with increased participation (Valentino et al. 2011)—particularly for those seeking justice or policy change (Bateson 2012). Some argue participation can serve as a form of "meaning-making" where victims reframe private loss as a public issue and find purpose in activism aimed at prevention, accountability, and honoring loved ones (Bateson 2025). Others point to post-traumatic growth, arguing that victimization can produce psychological change that fosters empowerment and new goals after adversity (Tedeschi and Calhoun 1996; 2004). These frameworks highlight both expressive and instrumental incentives linking direct exposure and participation.

In contrast, studies of contextual exposure—typically using homicide rates or proximity to violent events as treatments—tend to find that exposure to crime demobilizes (Jones Forthcoming; Ley 2018; McCarthy, Hagan, and Herda 2025; Trelles and Carreras 2012). These scholars point to sociotropic mechanisms emphasizing contextual conditions to explain how crime shapes participation indirectly through concerns

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about social order, community well-being, and government performance (e.g., Jones Forthcoming; Ley 2018; McCarthy, Hagan, and Herda 2025; Trelles and Carreras 2012). They contend that persistent or salient crime demobilizes because people fear public participation (Ley 2018; Trelles and Carreras 2012), become alienated from politics (Jones Forthcoming), or interpret disorder as institutional failure, pushing them to retreat from the state (Blanco and Ruiz 2013; Trelles and Carreras 2012).[1] Additionally, living in environments of sustained violence and crime may expose victims to less episodic and more sustained forms of victimization, where criminal perpetrators subjugate and dominate victims constantly (Moncada 2020). In these cases, victims of sustained criminal subjugation may substitute formal participation with what Moncada (2020) labels "everyday resistance."

Related studies often emphasize institutional trust, contending it mediates or moderates the link between crime exposure and participation. Constant disorder may depress faith in institutions, from elected officials to local police (Blanco and Ruiz 2013; Fernandez and Kuenzi 2010; Singer et al. 2019), leading people to disengage from institutionally sanctioned forms of participation like elections. A related, but conceptually distinct, mechanism is that pre-exposure trust conditions how people respond: victims who trust institutions may seek redress through participation, while those already skeptical may become further alienated and withdraw. Research on the two faces of policing in high-crime communities reinforces this possibility: as Prowse, Weaver, and Meares (2020) show, residents often experience the state as simultaneously hyper-present through surveillance and under-responsive in providing protection—a pattern of "distorted responsiveness" that erodes institutional trust and may lead victims to interpret subsequent crime exposure as further evidence of state failure.

Still others examine subjective perceptions of crime —such as safety concerns or the belief that crime is worsening—which tend to be associated with lower participation, diminished trust, and reduced democratic satisfaction (Blanco and Ruiz 2013; Fernandez and Kuenzi 2010; Rahn and Transue 1996; Trelles and Carreras 2012). Some perception measures tap sociotropic concerns (e.g., beliefs about national crime trends), while others capture ego-relevant feelings (e.g., perceived safety near one's home). Criminological research underscores that these reflect different constructs: many more respondents worry about crime than report feeling unsafe walking near home (Ferraro 1995). The latter—used in this study—is generally viewed as a closer indicator of personal fear. Here, we consider these three forms of exposure—direct victimization, contextual crime, and subjective safety perceptions—side by side.

We illustrate the value of our approach with a concrete example. Figure 1 reports the bivariate relationship between per capita crime at the Census block group level and presidential election turnout (Morris and Shoub 2024).[2] A clear pattern emerges whereby higher crime areas in these cities have substantially lower turnout rates (a two standard deviation increase in crime is associated with an approximately 6 percentage point lower rate of turnout). However, this aggregate pattern offers only superficial insights: it could be a product of residents' responses to contextual crime, lower rates of turnout among those who have been directly victimized, or an array of other individual- or contextual-level forces that are correlated with local crime rates. By analyzing individual level behavior merged with neighborhood crime and demographic data, our study allows us to explore these different mechanisms with greater nuance.

Another possibility is that crime shapes different forms of engagement in different ways. Elections are administered by the state, offer individual voters limited agenda-setting power, and constrain choices to those set by elites. This contrasts with non-electoral activities, such as protesting or attending community meetings. If crime and insecurity reduce trust in government without dampening political efficacy, as some scholars argue (Blanco and Ruiz 2013; Fernandez and Kuenzi 2010; Singer et al. 2019), crime may prompt individuals to "turn away from electoral politics to express their grievances in the more active forms of political participation" (Trelles and Carreras 2012, 98). Additionally, if those exposed to crime participate as a form of post-traumatic growth, to better understand their lived experiences, or to find solidarity in collective activism (Bateson 2025; Tedeschi and Calhoun 1996; 2004), then post-exposure mobilization is more likely to flow into sustained collective activities, like joining an organization, than individual, one-time acts like voting.

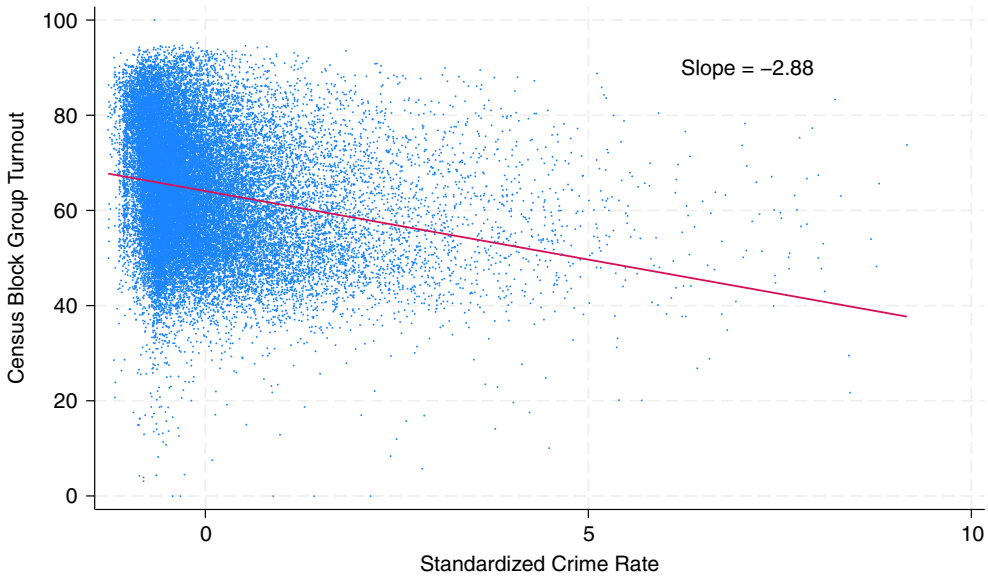
Existing empirical work offers suggestive but mixed support for this possibility. Studies focusing on contextual exposure tend to find decreases in turnout (Jones Forthcoming; Ley 2018; McCarthy, Hagan, and Herda 2025; Moffett-Bateau 2024; Trelles and Carreras 2012), though some find that direct victimization increases turnout (Berens and Dallendörfer 2019; Sønderskov et al. 2022). Studies of non-electoral participation—including protest, party meetings, or expressive behavior—tend to find mobilizing effects, though they generally examine direct victimization (e.g., Bateson 2012; Laterzo 2021, but see Córdova 2019). Here, we consider relationships between multiple forms of crime exposure and both forms of participation.

[1] Research on state violence often finds that proximity to police killings mobilizes protest and turnout (Ang and Tebes 2024; Morris and Shoub 2024; Williamson, Trump, and Einstein 2018, but see Markarian 2023). More broadly, direct contact with police is associated with *demobilization* (Lerman and Weaver 2014), while vicarious or network-based contact is associated with *mobilization* (Walker 2014).

[2] We leverage data from 13 (out of the 21 cities we consider below) where consistent crime data were available for both 2016 and 2020 and where information from official city data portals allowed us to use the latitude and longitude of each reported crime to match them to census block groups (N = 32,400).

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FIGURE 1. Census Block Groups and Turnout



Note: Figure illustrates bivariate relationship between Census block group crime rate (standardized to have a mean of 0 and standard deviation of 1 within cities) and administratively reported turnout rates calculated using data from Morris and Shoub (2024).

Prior studies also vary widely in measurement strategies—especially in how contextual crime is defined. Some use coarse administrative data like state homicide rates (Trelles and Carreras 2012), subjective perceptions of gang control or insecurity (Córdova 2019; Trelles and Carreras 2012), or instances of strategic cartel violence (Ley 2018). Measures of participation also vary, with most relying on self-reports that may be susceptible to social desirability (e.g., Berens and Dallendörfer 2019; Trelles and Carreras 2012), and others using aggregate data that risk ecological fallacy (e.g., McCarthy, Hagan, and Herda 2025). These choices reflect practical constraints, but come with tradeoffs. Our administrative, geographically granular data linked to validated individual level responses help mitigate these measurement concerns.

Finally, many studies emphasize particular subpopulations or report conditionality tied to specific forms of crime. For example, Sønderskov et al. (2022) find positive effects of violent—but not property—victimization on turnout, while Berens and Dallendörfer (2019) find positive effects of nonviolent but not violent victimization. Laterzo (2021) finds heterogeneous effects by race, gender, and neighborhood safety, but only for party meeting attendance. Some studies, like the Moffett-Bateau (2024) ethnography of poor Black women in Chicago, focus on theoretically important but demographically narrow populations. These designs offer valuable insights but may risk overfitting to specific contexts, and some findings may stem from exploratory analyses that do not always replicate cleanly. We mitigate these risks through preregistration and by clearly flagging exploratory analyses.

The U.S. Context

Our study is conducted exclusively in major cities in the United States. While the cities we study are home to over 30 million people, large urban centers in the United States have distinctive features. Further, the United States has notably higher levels of violent crime than other developed democracies (Kalish 1988). For example, homicide rates in Canada, South Korea, and Denmark were 1.98, 0.48, and 0.84 per 100,000 residents, respectively, in 2023. The rate was much higher —5.76 per 100,000—in the United States, but higher still in countries like Colombia (24.91), Ecuador (45.72), and Nigeria (15.75) (United Nations Office on Drugs and Crime 2025). Violence in the United States is particularly concentrated in poor communities of color (Lodge et al. 2021). This said, the United States does not stand out from its peers in terms of property crime (van Kesteren, van Dijk, and Mayhew 2014), nor is it more violent than many nations on the American continents (United Nations Office on Drugs and Crime 2025). Further, in similarly racially diverse countries like Brazil, crime is also highly segregated, affecting people of color more than white residents (Schwartzman 2020).

Perhaps as importantly, the United States has lower levels of institutional trust than many of its counterparts, which may moderate the impact of crime on participation. For example, in 2024, only 15% of the U.S. public reported having confidence in the national government, compared to 55% of the public in Denmark (OECD 2025). Taken together, differences in baseline institutional trust and violence offer plausible reasons why the participatory consequences of victimization may diverge across settings. Ultimately, our

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findings are less likely to generalize to high-trust and low-crime nations like Denmark than to low-trust and high-crime nations. Similarly, they may not generalize to post-conflict societies like those studied in Blattman (2009) where democratic institutions are weaker, participatory cultures are still forming, and exposure to violence typically involves organized civil conflict rather than sustained, unorganized crime. We return to the question of the generalizability of our findings in the discussion when considering our findings within the broader literature.

DATA AND MEASURES

Our analysis focuses on individuals living in ZIP codes that are entirely within 21 of the 30 largest cities in the United States. We selected these 21 cities based on preliminary assessments of the availability of public-facing data on the locations of reported crimes.[3] We leverage data from two survey sources. First, we contracted with Dynata to recruit approximately 90 respondents from each targeted city to complete an original survey (Major Cities Survey [MCS]). Participants were directed to our survey and provided their ZIP code to validate their eligibility (N = 1,819).[4] We used custom quotas within each city to ensure that respondents' demographics approximated Census estimates of the racial, gender, and age composition of each city. Second, we use CES respondents residing in those targeted ZIP codes from federal election years 2016–24 (Ansolabehere et al. 2016–2024). Pooling across years, our analysis leverages 29,025 CES respondents, including 6,302 responses to the 2024 survey which were not fully published at the time of preregistration. See Tables SM.A2 and SM.A3 in the Supplementary Material for the descriptive characteristics of our samples and the distributions of respondents across cities, respectively. In each case, we use a Census crosswalk file to match respondents to their ZCTA based on their ZIP code.[5]

Measures of Exposure to Crime

We attempted to gather data on reported crimes for the years 2016 through 2024 from public-facing city data portals and were mostly successful (see Section SM A.1 of the Supplementary Material for details about this process). For most cities, we used the latitude and longitude of each reported crime to spatially map the location to a ZCTA's polygon area using R (R Core Team 2024) with the *sf* (Pebesma 2018; Pebesma and Bivand 2023) and *tidycensus* (Walker and Herman 2025) packages.[6] Some cities do not report latitude and longitude, but do report the city block (including ZIP code) where reported crimes occurred. Once crimes were matched to a ZCTA, a tally of the total number of crimes of any type (violent or not) reported within each ZCTA in each year was calculated.[7] Per our preregistered analysis plan, within each city we divided the total number of reported crimes by the sum of the total number of residents residing in the ZCTA (five-year ACS estimate for that year; U.S. Census Bureau 2024) and the total number of people employed by businesses in the ZCTA (ZIP code-level estimates from Census County Business Pattern Data; U.S. Census Bureau 2022).[8]

Because cities vary in their reporting systems and norms (Rice and Ellison 2025), we standardize our calculated ZCTA-level rates to have a mean of zero and standard deviation of one across ZCTAs and years *within* each city.[9] Thus, our measure captures annual crime in respondents' ZCTAs *relative to* the distribution of annual crime rates across ZCTAs in the respondent's city across years.

In the MCS, we measure victimization using items adapted from the National Crime Victimization Survey (NCVS; Bureau of Justice Statistics)—a high-quality, government-sponsored survey that is billed as "the nation's primary source of information on criminal victimization" (Bureau of Justice Statistics 2025). We ask respondents if they had been the victim of each of four attempted or successful crimes in the last 12 months: (1) a home break-in, (2) vehicle or vehicle part theft, (3) theft of a personal belonging, and (4) physical threat or attack with a weapon (full question wording for all items is provided in Section SM A.2 of the Supplementary Material).

Per our pre-analysis plan (PAP), we create a count of victimization instances that ranges from 0 to 4.[10] The CES surveys each asked respondents a single yes/no question: "Over the past year have you [b]een a victim of a crime?" We report averages for each crime exposure variable broken down by respondent demographics in Table SM.A4 in the Supplementary Material, as well as comparable estimates drawn from the 2024 NCVS. In

[3] We originally targeted 22 cities, but crime data from Indianapolis proved to be unusable. We were unable to find fine-grained crime data for the remaining 8 (of the 30 largest) cities.

[4] Before beginning the MCS, as well as the Cook County Community Survey (CCCS; discussed below), respondents consented to participate in the study. No deception was involved and the data gathering process for both the CCCS and MCS was reviewed and received approval from the Loyola University Chicago Institutional Review Board (project 3115). Respondents who failed an attention check or were flagged as speeders were disqualified early in the survey—prior to providing responses to any items used in our analysis.

[5] ZIP codes define mail routes, rather than geographic areas, though in many cases, ZCTAs and ZIP codes are functionally identical.

[6] Each ZCTA's area was found yearly based on the ACS five-year estimate and matched to the proper year when the reported crime was committed to account for small changes in ZCTA polygon areas—particularly in the wake of the 2020 Census.

[7] Below we report analysis disaggregating our measure by type of crime.

[8] The purpose of this strategy is to account for business-rich downtown areas in many large cities with few residents, but many potential crime victims. In these areas, dividing solely by number of residents can wildly inflate the rate at which people are victimized within the area. We use 2023 ACS population estimates for our 2024 crime data because 2024 ZCTA estimates have not been released.

[9] Because survey respondents cannot reside in entirely nonresidential ZCTAs, we exclude these ZCTAs from our standardization process.

[10] Our measures record whether respondents experienced each type of crime and do not capture repeated victimization of the same type.

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Figure SM.A1 in the Supplementary Material, we report the relationship between victimization rates among key demographic groups captured in the MCS and CES surveys and rates reflected in 2024 NCVS.[11] The patterns that emerge are reassuring: groups that reported high rates of victimization in the "gold standard" NCVS survey tended to report high rates of victimization in both the CES and MCS surveys.[12] This said, virtually any measure of victimization may be distorted by biased self-reports: official crime rates are calculated based on crimes that individuals *report* to the police and, although the NCVS bills itself as having the advantage of capturing victimization that is not reported to the police, it does so by relying on self-reports. Replication data are available at Doherty et al. (2026).

ANALYSIS AND FINDINGS

We registered our PAP prior to fielding the MCS and before having access to data from the 2024 CES.[13] Here, we pool CES data from federal election years 2016–24 and report preregistered analysis restricting the analysis to the 2024 data in Tables SM.A12–SM.A15 in the Supplementary Material.[14] Per our analysis plan, all estimates are from mixed effects models with ZCTA-level random intercepts and city fixed effects, clustering standard errors at the city level.[15] Pooled CES analyses also include year fixed effects.

For each dataset and outcome, we focus on estimates from two model specifications. Model 1 includes the victimization and crime rate measures, along with respondent demographics (race, gender, age, educational attainment, income, and an indicator for income refusals). We control for three ZCTA characteristics: area in square miles, residential population, and number of people employed in the ZCTA. Model 2 adds ZCTA-level controls for an array of ZCTA-level demographics that are likely to be related to crime rates, but difficult to situate in the causal chain between crime exposure and participation including percent in ZCTA: (1) identifying as white (only, non-Hispanic), (2) Black (only, non-Hispanic), (3) Asian (only, non-Hispanic), (4) Hispanic (any race), (5) with an HS diploma or more, (6) with a BA or more, (7) living in poverty, and (8) 65+ years old.[16] Estimates from these models are reported in columns 2 and 3 of Tables SM.A5–SM.A10 in the Supplementary Material. We report a preregistered "demographics only" specification for interested readers in column 1 of those tables.

In Panel A of Figure 2, we consider the relationship between victimization and turnout. We consider self-reported turnout in the most recent federal election in the MCS and CES surveys, as well as two measures of validated turnout from the CES. In each case, we report coefficients from model 1 (solid markers) and model 2 (hollow markers).

The estimated effect of victimization is negative and statistically significant across turnout measures, surveys, and model specifications. In the MCS survey, a one-unit increase in victimization on the 0–4 measure is associated with a 3 percentage point decrease in the likelihood of turning out.[17] The coefficient on the victimization indicator in the CES is approximately –0.03 in each model. Estimates from the validated turnout models are similar. When we treat respondents who were matched to the voter file and validated as having turned out as voters and all other respondents as not voting, victimization is associated with a 2 percentage point decrease in the probability of turning out. When we only count those who were matched to the voter file, but did not vote as nonvoters (dropping unmatched respondents from the analysis), those who report having been victims are almost 5 percentage points less likely to turn out.

Panel B of Figure 2 reports coefficients on the crime rate measure from the same set of models. The relationships between crime rate and self-reported turnout are small and statistically insignificant. Estimates from the validated turnout models suggest that a two standard deviation increase in the ZCTA-level crime rate is associated with an approximately 2–3 percentage point decrease in turnout. However, the coefficients are attenuated and not statistically significant when more robust ZCTA-level controls are included in the models.

In Panels C and D, we repeat the same exercise, specifying tallies of the number of nonvoting political acts respondents reported participating in over the past year as our outcome. On the MCS survey, activities included: (1) participating in a protest, (2) discussing a political issue with someone, (3) contacting a public official, and (4) attending a meeting about a political issue ($M = 1.34$; $SD = 1.01$). The CES items included: (1) attending a local political meeting, (2) putting up a political sign, (3) working

[11] To maximize comparability with our other surveys, we only use NCVS respondents living in urban areas at the center of their MSA. We use items from the legacy survey instrument that inspired the MCS battery, rather than the redesigned instrument used for some 2024 respondents. See note to Figure SM.A1 in the Supplementary Material for details.

[12] Additionally, we find similar rates of victimization among CES respondents who reported voting but were not validated as having done so ($M = 0.0492$) and among those whose reported voting was validated ($M = 0.0547$), providing suggestive evidence that our victimization measures are not biased by some respondents simply over-reporting experiences and behaviors.

[13] The PAP can be accessed here: https://osf.io/9dzc6/?view_only=4c1b3968bd124ddeaddf421e7a9e6871.

[14] The point estimates of interest are similar to those reported below when we restrict our analysis to 2024, but, in some cases, fall short of conventional thresholds of statistical significance in this smaller sample.

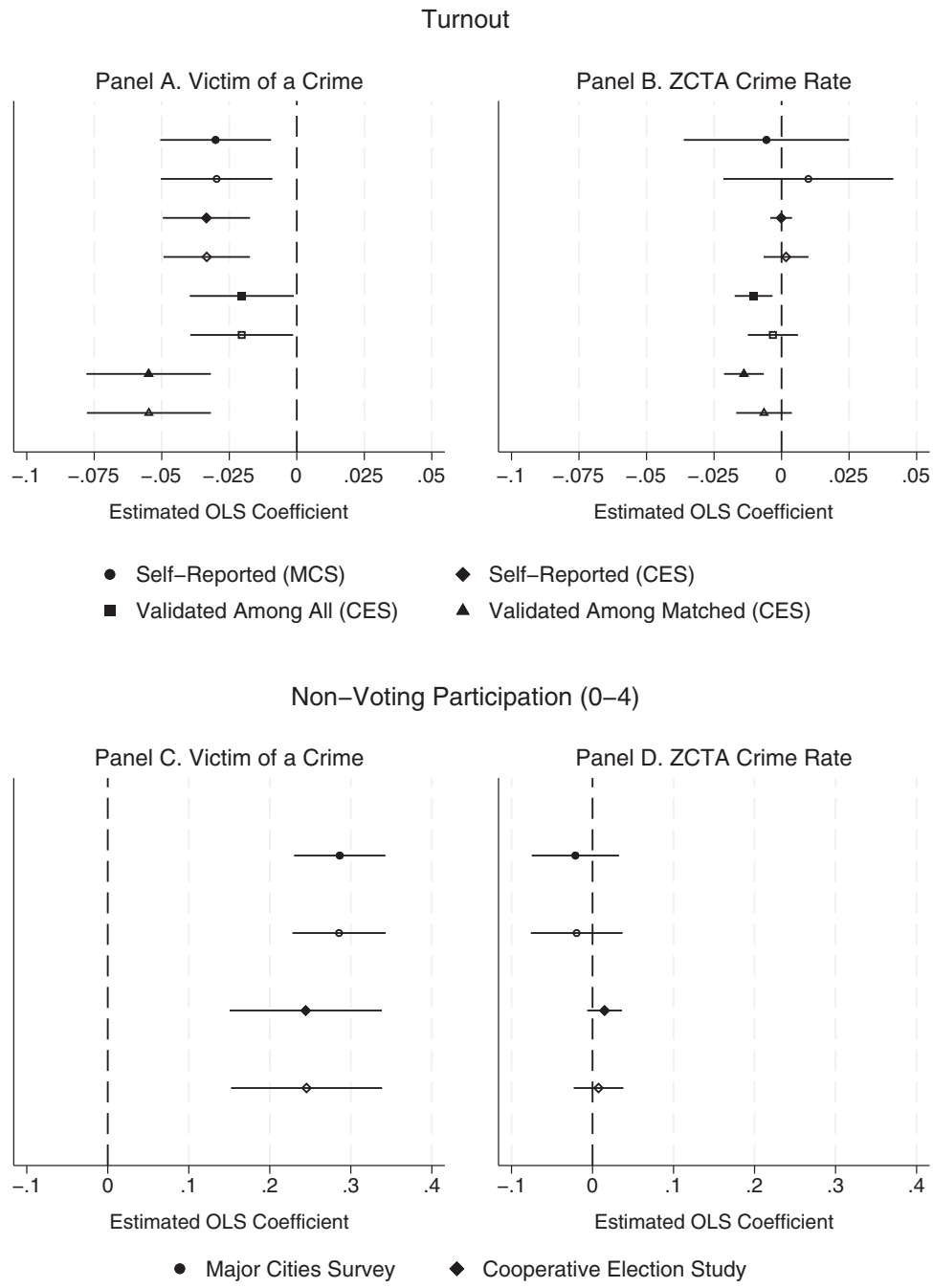
[15] An alternative, less conservative approach, would be to cluster standard errors at the ZCTA level. We re-estimate our core analyses using this alternative approach in Tables SM.A16–SM.A18 in the Supplementary Material and reach similar conclusions to those reported below.

[16] On average, the four ethnoracial composition measures account for 95% of residents in ZCTAs. Models excluding controls for percent Black, Hispanic, and Asian—that is, using only percent white to control for the racial composition of the ZCTA—yield substantively similar conclusions.

[17] Most respondents reported either 0 (70%) or 1 (17%) instances of victimization. Estimates from models treating victimization as a dichotomous indicator yield coefficients of approximately –0.054 in each specification ($p < 0.05$).

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FIGURE 2. Estimated Effects of Victimization and Crime Rates on Participation



Note: Solid markers are estimates from models controlling for respondent demographics and three basic ZCTA-level variables (area, number of residents, and number employed). Hollow markers are from models also controlling for percentage of residents in respondents' ZCTAs: (1) identifying as white (only, non-Hispanic), (2) Black (only, non-Hispanic), (3) Asian (only, non-Hispanic), (4) Hispanic (any race), (5) with an HS diploma or more, (6) with a BA or more, (7) living in poverty, and (8) 65+ years old. Whiskers are 95% confidence intervals. Full models reported in Tables SM.A5–SM.A10 in the Supplementary Material. Observations in voting models: self-reported (MCS; N = 1,819); self-reported (CES; N = 21,461); validated among all (CES; N = 29,025); validated among matched (CES; N = 18,644). Number of observations in nonvoting participation models: Major Cities Survey (N = 1,819); Cooperative Election Study (N = 22,778).

for a candidate or campaign, and (4) donating to a candidate or political organization ($M = 0.61$; $SD = 0.93$).[18]

[18] Our PAP indicated that we would use a 6-item battery for the CES analysis. However upon closer inspection we realized two items—attending a protest and contacting a public official—were not included in the 2016 survey. Results using the 6-item measure for the 2018–24 surveys are reported in Table SM.A11 in the Supplementary Material and are substantively similar, though the coefficient on crime rate is positive and statistically significant in the model that only includes basic ZCTA-level controls ($b = 0.036, p < 0.05$).

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In contrast to the estimates from the voting models, victimization is associated with *increased* nonvoting political participation. The coefficients in the MCS and CES data are approximately 0.29 and 0.23, respectively.[19] For context, in each survey, the estimated change in non-electoral participation associated with a one unit increase in victimization is comparable to that associated with a respondent having a four year college degree as their highest level of educational attainment, rather than high school diploma. The estimates in Panel D show little evidence of a relationship between ZCTA-level crime rates and nonvoting participation.

Next, per our PAP, using the MCS data, we assess whether *subjective* safety perceptions are associated with political participation (net of any relationship with our other exposure measures), as well as the plausibility of the argument that subjective safety perceptions are a mechanism for the relationships we find between victimization and participation. Our perceived neighborhood safety measure averages two five-point items that asked respondents to rate how safe they feel walking in their neighborhood during the day and at night. When we add this measure to the model 1 and 2 turnout specifications discussed above, the coefficient is positive, but not statistically significant in the voting models ($p = 0.378$ and 0.418, respectively; see columns 4 and 5 in Table SM.A5 in the Supplementary Material) and the coefficient on the victimization measure is essentially unchanged.[20]

When we repeat this exercise in the nonvoting participation models we find that perceived neighborhood safety is positively associated with participation. A two standard deviation (approximately 2 units on the 1–5 measure) increase in perceived safety is associated with reported participation in approximately 0.15 more of the 4 acts (see columns 4 and 5 of Table SM.A9 in the Supplementary Material). This pattern suggests that subjective *concerns about* crime can be demobilizing—even after controlling for more objective measures of exposure that might be expected to impact safety concerns. Our data do not position us to identify the mechanisms driving this pattern. However, safety concerns that operate independently of objective exposure to crime may reflect more general social anxieties. These anxieties may make people reluctant to engage in the collective action or political dialogue the non-electoral acts we consider entail, but be unrelated to the relatively solitary act of voting. Before proceeding, we note that the coefficient on victimization is essentially unchanged in these models, casting doubt on the notion that perceived safety is a key mediator of that relationship.

Robustness Checks and Exploratory Analysis

In this section, we report findings from several robustness checks and exploratory analyses.[21] We begin with three preregistered robustness tests. First, in Tables SM.A19 and SM.A20 in the Supplementary Material, we show that our main results hold when we calculate ZCTA-level crime rates using only the residential population, rather than the preregistered denominator that includes both residents and workers. Under this alternative specification, our findings are broadly similar, but the negative coefficient on the crime rate variable reaches conventional levels of statistical significance in three (rather than two) of the four CES validated turnout models.

Second, we address the possibility that our victimization findings are confounded by variation in respondents' routine exposure to public spaces, adding measures of reported average weekday and weekend time spent away from home to our MCS models. Including these covariates does not meaningfully affect the coefficients on victimization (see column 6 in Tables SM.A5 and SM.A9 in the Supplementary Material). The patterns hold when we allow these controls to exert nonlinear effects in Table SM.A23 in the Supplementary Material by "dummying out" our ordinal measures of time out of the house, as well as the two variables used to construct our perceived neighborhood safety measure.[22]

Third, we replicate our analysis using Chicago residents who completed the 2024 or 2025 wave of the Cook County Community Survey (CCCS), fielded in early 2024 and early 2025 (see Section SM A.2.3 of the Supplementary Material for details). Our non-electoral participation findings (pooling 2024 and 2025 CCCS surveys; N = 1,932) are virtually identical in this dataset.[23] The estimated relationship between victimization and turnout in the 2024 presidential election (2025 CCCS survey only; N = 662) is similar in direction and magnitude, but not statistically significant, possibly due to the smaller sample size (see Tables SM.A21 and SM.A22 in section SM A.9 of the Supplementary Material.)

Next, although our observational data do not position us to make strong causal claims, we report findings from two (unregistered) analyses to partially address concerns that omitted variable bias is shaping our estimated relationships. We first consider the possibility that our estimated victimization and perceived safety relationships are contaminated by omitted *ZCTA-level* factors by re-estimating our models using ZCTA fixed effects, and excluding all ZCTA-level covariates (see Tables SM.A24 and SM.A25 in the

[19] Estimates from MCS models dichotomizing the victimization measure are approximately 0.48.

[20] We note that our three exposure measures—victimization (V), crime rate (R), and perceived safety (S)—are correlated as expected, but only modestly ($r_{VR} = 0.061$; $r_{VS} = -0.101$; $r_{RS} = -0.147$).

[21] A summary of the analysis is located in SM A.3.

[22] This approach was not preregistered, but also partially addresses concerns about measurement error in our victimization measure. For example, to the extent that subjective concerns about crime lead people to over-report victimization, more fulsome controls for perceived neighborhood safety will help to purge the victimization predictor of that error.

[23] The non-electoral participation items on these surveys asked specifically about participation in *local* politics.

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Supplementary Material). The coefficients on our predictors of interest—victimization and perceived safety—are similar in these models.[24]

We then leverage the 2020–22 CES Recontact Survey to consider the relationship between *changes* in exposure to crime and participation (Ansolabehere, Schaffner, and Luks 2024). We restrict our analysis to respondents residing in ZCTAs for which we have crime data who did not report having been victimized in the 2020 wave of the survey and whose ZIP code in 2022 matched their 2020 ZIP code. We estimate models predicting each of three measures of 2022 participation—reported turnout, validated turnout, and non-electoral participation (0–6). In each, we include a lagged dependent variable, an indicator for reported victimization in the 2022 wave, a measure of *change* in ZCTA-level crime rate, and respondent demographics (see Table SM.A26 in the Supplementary Material). Estimates from these models are underpowered (N = 640 to 788 respondents across models, only 31 of whom were "new victims" in 2022). Additionally, the correlation between ZCTA-level crime rates in 2020 and 2022 is 0.942 in our data. This said, the point estimates are similar to those reported in our main analysis. The estimated "effects" of victimization on reported and validated turnout are –0.055 and –0.015, respectively—quite similar to the comparable estimates of –0.033 and –0.020 reported in Figure 2. The estimated relationship tied to non-electoral participation is almost identical to that reported in Table SM.A15 in the Supplementary Material (where we use the same 6-item battery and the 2024 CES data), but somewhat weaker than the relationship we find in our pooled CES data.

Avenues for Future Research

We next turn to (unregistered) exploratory analyses that suggest potential pathways for future research. In Figure 3, we examine whether the effect of victimization varies by type of incident, using indicators for each of the four victimization items included on the MCS (see Table SM.A27 in the Supplementary Material for regression models). All four types of victimization are positively associated with nonvoting forms of political participation, but only physical assault is significantly and negatively associated with turnout. One possible explanation for this pattern is that property crimes—particularly those that occur when the victim is not present, such as break-ins or vehicle theft—generate anger or a desire for redress without inducing the same sense of personal threat. These emotions may motivate individuals to engage in non-electoral forms of participation, such as contacting officials or attending public meetings. Violent crimes like physical assault may also provoke fear and alienation, which may lead individuals to specifically disengage from formal political processes like voting.

We observe similar patterns when we replicate our model 2 specification using estimates of ZCTA-level violent crime rates and then, separately, nonviolent crime—at least in the CES data (see Tables SM.A28 and SM.A29 in the Supplementary Material).[25] Even after controlling for an array of ZCTA-level demographics, a two standard deviation increase in the *violent* crime rate is associated with an approximately 3 percentage point, statistically significant, decrease in turnout.[26] In contrast, the coefficients in the *nonviolent* crime rate models are tiny and not statistically significant.[27]

Next, we disaggregate our non-electoral participation outcomes into their constituent acts and report estimates from both model 1 and model 2 specifications in Tables SM.A32–SM.A35 in the Supplementary Material. We find that victimization predicts increased engagement across all nonvoting behaviors except for discussing politics. These patterns suggest that victimization broadly motivates active, outward-facing non-electoral engagement and is not specifically limited to less-conventional forms of engagement like protests.

Another possibility is that the relationships between exposure to crime and patterns of participation vary across groups, particularly race which is closely linked with crime and surveillance in the United States (Lodge et al. 2021). In Tables SM.A36 (MCS) and SM.A37 (CES) in the Supplementary Material, we replicate our model 1 specification, interacting our measures of exposure to crime with indicators for respondents' reported race. The interaction coefficients of interest are almost all statistically insignificant and do not follow any discernible pattern across models and surveys.[28]

Finally, although our data do not position us to identify the mechanisms driving our findings, trust in government may play a role—particularly given that crime exposure is associated with less participation in government-run elections but more participation in other forms. We assess whether our data are consistent

[24] The point estimate of the relationship between victimization and validated turnout is unchanged in the CES model, but is more noisily estimated ($p = 0.108$).

[25] We categorized assault, homicide, robbery, harassment, sex crimes, and weapons crimes as "violent," and burglary, arson, theft, and motor vehicle theft as "non-violent."

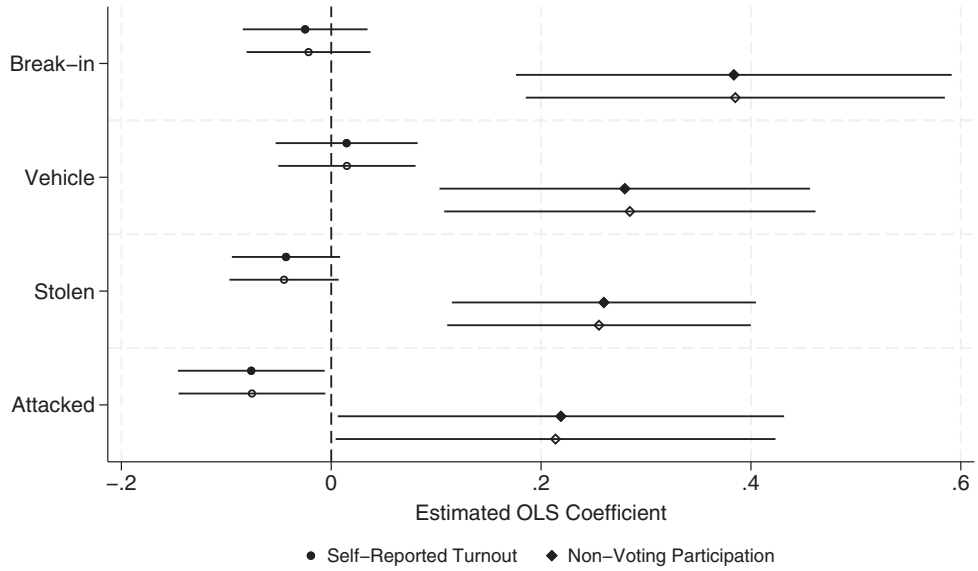
[26] We replicate this exercise using homicide rates in Tables SM.A30 and SM.A31 in the Supplementary Material. The relationships between this measure and participation fall short of conventional thresholds of statistical significance in all of the models we estimate, with the exception of the CES model predicting non-electoral participation (column 3 of Table SM.A31 in the Supplementary Material).

[27] We note that one plausible explanation for the weaker relationship between nonviolent crime and participation is that these crimes are less likely to be reported than violent crimes. If so, this would introduce measurement error, potentially biasing estimates downward (Justus and Scorzafave 2014).

[28] Statistical power constraints limit our ability to detect heterogeneous effects by race (see Table SM.A37), particularly given the correlation between race and neighborhood crime rates. However, we note that in our data and in the 2024 NCVS, Black respondents are not substantially more likely to report direct victimization than other racial groups (see Table SM.A4 in the Supplementary Material). Black residents are, however, more likely to reside in higher-crime neighborhoods, meaning that to the extent contextual exposure carries participatory consequences, these burdens likely fall disproportionately on Black and other non-white populations.

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FIGURE 3. Estimated Effects of Type of Victimization on Participation



Note: Solid markers are estimates from models controlling for respondent demographics and basic ZCTA-level variables (area, number of residents, and number employed). Hollow markers are from models also controlling for percentage of ZCTA residents: (1) identifying as white (only, non-Hispanic), (2) Black (only, non-Hispanic), (3) Asian (only, non-Hispanic), (4) Hispanic (any race), (5) with an HS diploma or more, (6) with a BA or more, (7) living in poverty, and (8) 65+ years old. Whiskers are 95% confidence intervals. See Table SM.A27 in the Supplementary Material for regression models. N = 1,819.

with a dynamic where exposure to crime affects trust, which, in turn, affects participation. We also consider whether trust in government authorities is better thought of as a more stable disposition that, like other markers of social connection, insulates individuals from any demobilizing effects of exposure to crime (Moffett-Bateau 2024). If so, trust may *condition* the relationship between exposure and participation even if it is not *affected* by exposure to crime.

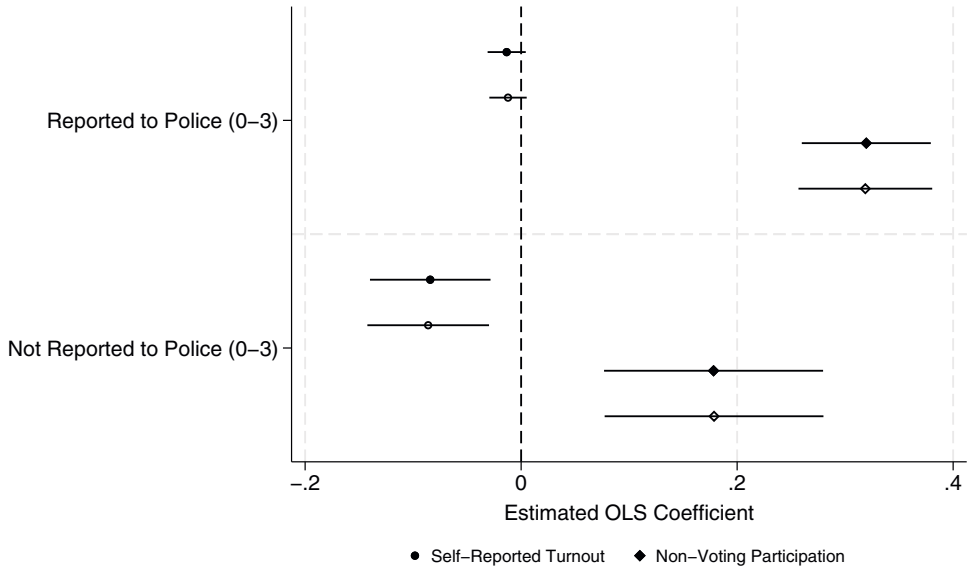
We explore these possibilities in several ways, emphasizing that these analyses are not preregistered, nor are the findings definitive. First, in Table SM.A38 in the Supplementary Material, we use data from the 2024 CES to assess whether exposure to crime is associated with reported trust in state and federal governments (items only available on 2024 postelection wave). We find that victimization is associated with lower reported trust in both levels of government, but no relationship between contextual exposure and trust in either form of government. We then add these measures of trust in federal and state governments to our participation models in column 4 of Tables SM.A12–SM.A15 in the Supplementary Material. The coefficient on our victimization measure is only slightly attenuated in each turnout model (self-reported, validated, and validated among matched) and slightly amplified in our non-electoral participation model, casting doubt on the notion that trust substantially mediates the relationship between crime exposure and participation. In columns 5 and 6, we re-estimate our model 1 and 2 specifications, including interactions between each trust measure and our victimization and crime rate measures and find that the estimated demobilizing effects of victimization on turnout are particularly strong among those low on trust in the *state* government and attenuated among those higher on trust. The coefficient on this interaction only reaches statistical significance in our *self-reported* turnout models. The point estimates in the validated turnout models are similar, but the standard errors are notably larger. The other interactions in the turnout models, as well as interactions in the non-electoral participation models, fall well short of conventional thresholds of statistical significance.

In the MCS data, we further explore the role of trust by disaggregating our victimization measure based on follow-up questions that asked victims whether they reported the crime to the police. We posit that low-trust individuals will be less inclined to report victimization to the police. We examine whether the relationship between *reported* victimization and participation differs from the relationship between *unreported* victimization and participation in Table SM.A39 in the Supplementary Material (using measures of the number of reported and unreported instances of victimization).[29] Figure 4 reports estimates of interest. We find a negative association between unreported victimization and turnout, but not between reported victimization and turnout. Both victimization measures are associated with higher rates of non-electoral participation,

[29] Although these measures could each theoretically range from 0 to 4, in our data, they each range from 0 to 3.

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FIGURE 4. Victimization, by Whether Reported to Police



Note: Solid markers are estimates from models controlling for respondent demographics and basic ZCTA-level variables (area, number of residents, and number employed). Hollow markers are from models also controlling for percentage of ZCTA residents: (1) identifying as white (only, non-Hispanic), (2) Black (only, non-Hispanic), (3) Asian (only, non-Hispanic), (4) Hispanic (any race), (5) with an HS diploma or more, (6) with a BA or more, (7) living in poverty, and (8) 65+ years old. Whiskers are 95% confidence intervals. See Table SM.A39 in the Supplementary Material for regression models.

though the relationship is somewhat stronger for reported victimization.

Taken together, these patterns are consistent with the notion that the effects of victimization on participation depend on how much victims trust government. Trust appears to insulate individuals from the demobilizing effects of victimization in the domain of turnout and, perhaps, enhance the mobilizing effects of victimization when it comes to non-electoral participation.

DISCUSSION

This study offers new insight into a longstanding question in political behavior: how does exposure to negative events government is expected to prevent affect political participation? Our preregistered findings draw on multiple surveys to build on existing research. We consider the relationships between three measures of exposure to crime—direct victimization, local crime rates, and perceptions of neighborhood safety—and both turnout (including validated turnout) and non-electoral forms of participation. To the best of our knowledge, our findings offer the most comprehensive analysis of the relationship between exposure to crime and political participation in the U.S. context to date.

We find that direct victimization is consistently associated with higher levels of non-electoral participation but lower levels of turnout. The relationships between contextual crime rates and participation, by contrast, are far less consistent and potentially limited to a negative association between violent crime rates and turnout uncovered in exploratory analysis. This may be due to the fact that victimization tends to be a fairly rare and distinctive experience, whereas people may get used to persistent crime in their neighborhoods. Additionally, perceptions of neighborhood safety are positively associated with non-electoral engagement but unrelated to turnout, suggesting that subjective safety concerns can affect participation in ways that are independent of the effects of more objective exposure to crime. The findings suggest that personal, direct exposure to crime plays a particularly central role in shaping participation. They also suggest that victimization "redirects" the inclination to participate from state-run avenues to other forms of political engagement.

In addition to our preregistered analysis, we conduct an array of exploratory tests to deepen our inquiry. We show that our results are robust to alternative approaches to operationalizing crime rates and the inclusion of controls for a key potential confound: time spent outside one's home. We also find similar patterns when analyzing additional surveys of Cook County residents. Beyond these preregistered exploratory tests, we report additional analyses seeking to account for omitted variable bias and probing heterogeneous effects across racial groups (which we find little evidence of).

Our exploratory analysis also suggests that trust plays an important role in shaping responses to crime exposure. Including measures of institutional trust as covariates does not meaningfully change the coefficients on our predictors of interest. This suggests that, at least in this context, institutional trust is not a substantial pathway whereby exposure to crime affects participation. Instead, we find that victims who trust institutions less appear to be more electorally

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demobilized and less non-electorally mobilized than victims who trust institutions more. In short, rather than mediating the relationship between exposure and participation, trust appears to moderate this relationship.

At first glance, these patterns may appear to offer an explanation for our countervailing findings showing that victimization is associated with a decrease in turnout but increase in non-electoral participation: when low-trust individuals are victimized, they may withdraw from state-run elections and instead pursue less institutionally constrained forms of participation like protesting. However, we find limited support for a pure substitution mechanism: as we show in Tables SM.A32-SM.A35 in the Supplementary Material, victimization is not only associated with increased participation in activities like protesting, but also in "institution-facing" activities like donating to campaigns and contacting elected officials. Thus, the divergent relationships between victimization and the two types of participation we consider may be better explained by theories of post-traumatic growth, which posit that some victims experience positive psychological change and a renewed sense of agency (Tedeschi and Calhoun 1996; 2004) or that political activism can help victims make sense of their experience by reframing their private loss as a public problem, pursuing prevention and accountability, and honoring loved ones (Bateson 2025). From this perspective, non-electoral activities—including more "conventional" acts like contacting elected officials or attending a community meeting—may be attractive because they are experienced as social, sustained, and instrumentally purposeful, rather than as one-off, low-feedback actions like voting.

The notion that trust conditions the relationship between exposure to crime and participation may also help reconcile discrepancies between our findings and Sønderskov et al. (2022), who—using validated individual-level panel data in Denmark—find that violent victimization mobilizes voters. On average, Denmark displays higher institutional trust than the United States, and OECD indicators place the United States far below the OECD average for confidence in the national government (OECD 2024a; 2024b). If crime victimization has differential effects on participation depending on individuals' pre-existing trust, then aggregate effects are likely to vary across countries as a function of baseline trust. That said, our findings are broadly consistent with studies conducted in low-trust settings linking victimization to non-electoral engagement and violent contexts to lower turnout (e.g., Bateson 2012; Trelles and Carreras 2012), as well as with recent working papers on policing and surveillance in the United States, which find that localized policing mobilizes turnout (Naftel 2024; Romero 2024). While scholars advance different mechanisms to explain these patterns, our findings suggest that, to the extent that policing reduces crime—and thus, the number of people who are victimized—we should expect turnout to increase absent other indirect effects (Romero 2024).

This study is not without limitations. Our measures of nonvoting political behavior may be contaminated by self-reporting biases, though this concern is partly mitigated by the strong correspondence between results using self-reported and validated turnout, as well as by the expectation that social desirability is more acute for voting than for contacting officials or attending protests. While we take several steps to address omitted variable bias concerns by preregistering our analysis to limit researcher degrees of freedom (Simmons, Nelson, and Simonsohn 2011), including a rich set of controls, providing analysis leveraging ZCTA fixed effects, and analyzing (albeit limited) panel data, reliance on observational data limits our ability to make strong causal inferences. Our crime-rate measures, while carefully constructed and standardized, rely on public reporting systems that vary in coverage and classification (Rice and Ellison 2025). And although ZCTAs are far more granular than cities or counties, some remain sizable (median ZCTA area for CES respondents = 3.6 square miles), meaning crime rates may obscure hyper-local variation. Finally, as noted above, our analysis focuses on individuals living in major U.S. cities and may not generalize to other contexts.

Future research can build on these findings in several directions. First, although we report exploratory analyses that suggest political trust may moderate the relationship between crime exposure and participation, this evidence is limited and preliminary. Scholars should pursue more suitable data sources and research designs to more convincingly examine potential psychological and attitudinal pathways linking victimization to distinct participatory outcomes and conditioning those relationships. Although it appears that trust plays a role, personal efficacy, personality traits, and other individual and contextual factors may also be at play. Second, future studies should further explore heterogeneity in responses to crime across racial, ethnic, and socioeconomic groups, especially given persistent inequalities in both crime exposure and political voice. Finally, attention to institutional context—such as policing regimes, access to participatory networks, or electoral competitiveness—may help explain when victimization triggers engagement rather than withdrawal.

SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit https://doi.org/10.1017/S0003055426102007.

DATA AVAILABILITY STATEMENT

Research documentation and data that support the findings of this study are openly available at the American Political Science Review Dataverse: https://doi.org/10.7910/DVN/53YI22.

CONFLICT OF INTEREST

The authors declare no ethical issues or conflicts of interest in this research.

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ETHICAL STANDARDS

The authors declare that the human subjects research in this article was reviewed and approved by Loyola University Chicago's Institutional Review Board (Project No. 3115). The authors affirm that this research note adheres to the principles concerning research with human participants laid out in APSA's Principles and Guidance on Human Subject Research (2020).

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