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Crime Victimization Increases Turnout:
Evidence from Individual-Level Administrative Panel Data

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Abstract What are the consequences of being the victim of crime for political participation? Previous studies report mixed results with respect to voter turnout, in contrast to the positive effects found for other indicators of political engagement. However, previous turnout studies have failed to differentiate between violent and non-violent crime, and have relied on cross-sectional survey data prone to measurement biases and selection effects. We address these shortcomings via a panel analysis of official registry data from Denmark recording individual-level turnout in two municipal elections (in 2009 and 2013) and victimization from violent and non-violent crime. We identify the effect of victimization by comparing changes in turnout between the two elections for victims and two different counterfactual groups: non-victims in the general population, and individuals who were victimized *after* the 2013 election. The results show that victimization from *violent* crime increases turnout by 2 to 3 percentage points. We further demonstrate a large negative *between-individual* effect of victimization, suggesting that previous studies have been marred by severe selection bias.

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INTRODUCTION

What are the consequences of being the victim of crime for political participation? The answer to this question is of great substantive and theoretical importance. Understanding how citizens respond to experienced trauma and injustice is an essential first step in grasping their ability—or lack thereof—to channel such grievances into political action. On a theoretical level, citizens' responses to crime— ostensibly a salient experience—can be viewed as a key test of the general proposition that personal experiences shape political behavior (Egan & Mullins 2013).

Bateson (2012), in a now seminal study, finds that self-reported crime victimization increases levels of a broad range of manifestations of political engagement in virtually every region of the world. Victims were up to 8 percentage points more likely than non-victims to participate in community action, protest, political and town meetings, and victims were more likely to report interest in politics, engage in political conversations, and attempt to persuade others as well. Viewed through a broader lens, Bateson's findings resonate with work in the civil war and conflict literatures, where it has been found that "[in] case after case, people exposed to…violence go on to behave more cooperatively and altruistically", and "tend to increase their social participation by joining more local social and civic groups or taking on more leadership roles in their communities" (Bauer *et al.* 2014, 250). Although the precise causal mechanism underlying the effects of exposure to crime are not yet completely understood, with possibilities ranging from instrumental (crime prevention or retaliation) concerns (Bateson 2012), victims' "post-traumatic growth" (Blattman 2009), anger, or the cathartic effects of participation in alleviating psychic distress (Morrison and Blackmore 2015), and the social affirmation of in-group identity, empathy and solidarity through participation (Dorff 2017; Grosjean 2014; Hartman and Morse 2018), the empirical regularity seems clear: "victimization is always associated … with increases in the probability that an individual will engage in high levels of political activity" (Bateson 2012, p.575).

Curiously, this "regularity" is far from settled in the field of *voter turnout*, the most prevalent form of political participation in virtually every established and emerging democracy in the world. Some work does find that victimization increases voting, voter registration or vote intentions (Bellows and Miguel 2009; Berens and Dallendörfer 2019; Blattman 2009), but a large swath of other research suggests a *negative* effect of victimization on turnout in both national and local elections, presumably due to a withdrawal from public life and loss of faith in institutions (Trelles and Carreras 2012; Ley 2018). Malone (2013) and Ley (2018), for example, show that self-reported victimization leads to lower levels

of turnout in Mexico. Similarly, Coupe and Obrizan (2016) report negative effects of experiencing physical violence on individual-level turnout in Ukraine. Relatedly, there are also several studies showing negative effects of self-reported exposure to electoral violence or intimidation on subsequent turnout (e.g. Bratton 2008; Collier and Vicente 2014). In short, the existing evidence suggests that, in contrast to the consistent positive patterns seen for other indicators of political engagement, the effect of victimization on voter turnout is much less clear.

Apart from inconsistent results and limited geographic scope, it is also the case that previous turnout studies have been based on suboptimal data and research designs. More specifically, they have relied exclusively on self-reported survey measures of both victimization and turnout, most likely resulting in biased estimates given that both constructs are measured with considerable random and systematic error due to, e.g., recall bias and social desirability bias (Bernstein, Chadha and Montjoy 2001; Skogan 1981), and given the existence of systematic non-response, in that victims may be less likely to respond to surveys in the first place (Elliott and Ellingworth 1997). Another shortcoming is the failure of many previous studies to differentiate between different types of crime, most pertinently whether the crime is violent or not. The "severity" of the crime is of fundamental importance for the health-related and psychological consequences of crime (Lurigio 1987), and therefore there are good reasons to expect differential effects of violent and non-violent crimes in terms of political impacts as well (Berens and Dallendörfer 2019; Coupe and Obrizan 2016).

Aside from being vulnerable to these problems of conceptualization, measurement, and non-response, previous turnout studies have all been based on cross-sectional data, with clear deficiencies in terms of causal identification. Even with the inclusion of a range of control variables, it is impossible, using this design, to rule out the possibility that victimized and non-victimized individuals differ on unobserved factors—for example in the composition of their social network, in their lifestyle, or in their personality—which also influence their likelihood of turning out to vote. This possibility is buttressed by previous studies reporting significant negative relationships between *prior* political engagement and subsequent crime victimization (Bateson 2012; Morrison and Rockmore 2015).

Given these shortcomings in previous studies, we argue that the true impact of crime victimization on voter turnout remains essentially unknown. What is needed to provide a credible causal estimate is a study that simultaneously a) measures *actual* voter turnout and crime victimization rather than relies on individual self-reports; b) is free from biases due to non-response; c) distinguishes between victims of

violent and *non-violent* crime; and d) accounts for systematic pre-existing (i.e., pre-victimization) differences in the likelihood of turning out to vote between crime victims and non-victims (i.e. handles "selection" into victimization).

In this paper, we satisfy each of these requirements via a panel analysis of official government registry data for the entire population of eligible voters residing in a subset of Danish municipalities between 2009 to 2013. The registry data contain information on both individual-level turnout in the 2009 and 2013 municipal elections as well as victimization from various types of crime before, between and after these elections. We identify the causal effect of victimization by comparing *change*s in turnout from the municipal election in 2009 to the one in 2013 for those victimized between the two elections and two different counterfactual groups: non-victims in the general population (i), and individuals who were victimized within a year *after* the 2013 election (ii). The analysis shows that victimization from *violent* crime between the two elections increases the propensity to vote by approximately 2 to 3 percentage points, while victimization from non-violent crime does not affect turnout. We further show that the positive *within*-individual effect of violent crime on turnout contrasts markedly with a large negative *between*-individual effect, indicating that previous cross-sectional studies have likely been marred by substantial selection bias. The direction and consistency of the effects fits squarely with the view that exposure to violence has "pro-social", positive consequences on political participation of all kinds. At the same time, the Danish context – a high trust society with high-quality institutions and little of the organized crime, gang-related violence and ethnic conflicts found in other parts of the world – places possible limits on the external generalizability of the findings; we discuss these issues and implications for further work on crime victimization in the concluding section.

RESEARCH DESIGN, DATA AND MEASUREMENT

Our analysis relies on fine-grained individual-level administrative panel data from the official Danish population registers, which contain longitudinal information for everyone residing in Denmark, and which can be linked using (anonymized) official identification numbers.[1] We utilize highly reliable data

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[1] Access to the data is legally restricted under Danish law, and they can only be accessed through secure servers under Statistics Denmark, and only if permission is granted by Statistics Denmark to an authorized Danish research institution. The code used to analyze the data is available from the authors upon request.

on verified turnout in Danish municipal elections[2] in 2009 and 2013 as well as data on crime victimization and other covariates. Using registry-based population data eliminates or minimizes methodological concerns marring studies based on survey-data. Importantly, non-response (including, in our case, panel attrition), which is inherent to survey research—and which likely biases the estimated relationship between victimization to crime and turnout—is not an issue. Further, various measurement problems are significantly reduced. Official data on voter turnout is patently a more valid indicator than survey-based measures, which come with systematic and random measurement error due to various biases including recall bias and social desirability bias (Bernstein, Chadha and Montjoy 2001). Some of the same advantages apply to official government data on crime victimization. However, because crime victimization is not observed directly, but rather is reported to the police, government crime data does not pick up unreported crime. This is unlikely to be a major problem in the Danish context (at least comparatively speaking), where trust in the police and the quality of state institutions, which presumably covaries with the likelihood of reporting crime, is very high, and among the highest in Europe (Kääriäinen 2007).

Data on individual turnout is registered at each polling station in each municipality. We obtained access to turnout data from 44 out of the 98 Danish municipalities in 2009 and from all municipalities in 2013. This implies that our panel sample comprises the entire population of residents who were eligible to vote in 2009 and 2013, and who lived in one of the 44 municipalities included in 2009. There are no significant differences between the municipalities included and not included in the data in 2009, although the former are slightly more populous, and inhabited by residents with somewhat higher socio-economic status (Bhatti, Danckert and Hansen 2017). The overall turnout rate was 65.8% in 2009 and 71.9% in 2013, with virtually identical trends in turnout rate between the included and the excluded municipalities (5.8 versus 5.9 percentage point increase). This means that our results are likely to be generalizable to the entire Danish population of eligible voters. Appendix A in the supplementary material describes the sample in further detail.

We measure crime victimization by means of a population-wide register recording all crime incidents (including the identity of the victim) that were reported to the police from 2005 until 2014. The

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[2] While local politics and municipal elections are inconsequential in some contexts, this is not the case in Denmark. Around 50% of public expenditures are spent in the municipalities, with local councils deciding on resource allocation for a range of important welfare services including childcare, schools and elderly care, as well as setting the local tax rate.

crime registers hold two essential qualities in relation to our research design. First, they index the type of crime in question in granular detail (see Appendix B for further information). We therefore know if someone has been the victim of a non-violent crime, e.g. fraud or pickpocketing, or a violent crime such as assault, robbery and sexual offences. This enables us to examine the effect of violent and non-violent crime separately. The second key feature of the crime registers for our purposes is that they contain the exact date of a given crime. Consequently, we know whether an individual was victimized between the municipality elections in 2009 and 2013, but also if someone was victimized before the 2009 election and after the election in 2013. To eliminate the potential confounding from prior victimization and to control for other selection effects, we focus exclusively on those who were not victimized (from 2005 onwards) prior to the first election in 2009 (see Appendix A for details, and see Visconti (2019) for a similar measurement approach).

We analyze the two-wave panel data by means of an individual and time ("two-way") fixed effect estimator, which is identical to a difference-in-differences estimator.[3] In the first set of analyses, we compare *changes* in electoral turnout over time for those victimized between 2009 and 2013 and those not victimized during 2009-2013 or in subsequent years. The comparison of changes, rather than differences in levels between the two groups at a given point in time, strengthens causal inference considerably, as it controls for pre-existing differences between those victimized and not victimized due to unobserved time-invariant confounders. The model identifies the causal effect of victimization under the assumption of parallel time trends for the two groups, such that crime victims, had they not been victimized, would have followed the same trend in turnout from 2009 to 2013 as the non-victims. We buttress the plausibility of the parallel trends assumption in these models by including standard time-varying variables as controls (all measured through the administrative registers): education, employment, income, citizenship, and aggregate municipality victimization (see Appendix C for measurement details).

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[3] The causal effect γ of the "treatment" D (i.e., being a crime victim between 2009 and 2013) is estimated using the fixed effects model: $(Y_{it} - \bar{Y}) = \beta_0 + \gamma(D_{it} - \bar{D}_i) + \beta_j(X_{ijt} - \bar{X}_j) + (\varepsilon_{it} - \bar{\varepsilon}_i)$, where β_0 represents the predicted change (adjusted for the time-varying controls in X) in mean-deviated Y for the control group (where D_{it} is 0 in both waves), and γ represents the additional effect on mean-deviated Y from the mean-deviated value of D for the treatment group. In the two-wave case, this is equivalent to a pooled OLS difference-in-differences model predicting Y_{it} with the treatment group indicator D_i, a time indicator T equaling '0' in wave 1 and '1' in wave 2, and the interaction D_iT, with γ being the regression coefficient for the interaction term. It also yields an equivalent estimate of the causal effect γ in a two-wave panel model in first differences, $\Delta Y_i = \alpha + \gamma D_i + \Delta X_{ij} + \varepsilon_i$, where D_i is an indicator for whether individual i was victimized between waves 1 and 2, and the X_j are time-varying controls.

The parallel trends assumption would be violated if some unobserved factor that is correlated with victimization also led to differential changes in turnout for the victims over time. To guard against this possibility, we estimate a second set of analyses using a different (though smaller) comparison group, namely those individuals victimized just *after* the 2013 election (until the end of 2014). This "soon-to-be victimized" group provides a close to ideal counterfactual group for those who were victimized before the last election, as they are presumably identical save for the plausibly random timing of their victimization. This means they are likely to have nearly identical values on *all* unobservables, even those which conceivably could produce violations of the parallel trends assumption in a comparison of victims to non-victims in the general population.

We estimate the effects using a linear probability model, similar to many prominent studies of turnout (e.g., Gerber, Green and Larimer 2008). In a panel setup like ours, the LPM is preferable over a binary model because the latter excludes individuals without temporal variation on the turnout variable.

RESULTS

Table 1 presents the fixed effects estimates of the effect of crime victimization on the change in turnout in municipal elections in Denmark between 2009 and 2013 (coefficients for the control variables are reported in Appendix D). The *within* estimates reported in Table 1 are the predicted changes in turnout from 2009 to 2013 for those who were victimized between 2009 and 2013, over and above the changes among non-victims in the general population. Hence, a positive coefficient signifies that the trend for the victims is more positive (or less negative) than the trend for the control group. Model 1 displays the estimated effect of crime victimization in general, whereas Model 2 and 3 shows the effect on violent and non-violent crime, respectively.

Model 1 shows a significant positive effect of crime victimization in general, with turnout rising by about half a percentage point. However, from Model 2 and 3 it is clear that this effect is entirely driven by victimization from violent crime. The effect for violent crime reported in Model 3 is highly significant and amounts to an increase in turnout of almost 3 percentage points. In contrast, the effect of non-violent crime is virtually zero, not significantly different from zero, and significantly smaller than the estimate for violent crime. The estimated 3-percentage point increase in turnout for violent crime is substantial, and roughly similar in magnitude to the effects of victimization on other indicators of political engagement reported by Bateson (2012: 576).

Table 1: The effect of crime victimization on municipal election turnout

Model	1	2	3	4	5
Victimization	Both	Non-violent	Violent	Violent	Violent
Victim *-within*	0.005[*]	-0.002	0.029[***]	0.029[***]	0.019[*]
	(2.56)	(-1.14)	(7.44)	(7.44)	(2.10)
Victim *-between*				-0.150[***]	-0.005
				(-26.61)	(-0.40)
Constant	0.611[***]	0.610[***]	0.606[***]	-0.058[***]	-0.303[***]
	(66.48)	(65.83)	(64.02)	(-18.96)	(-12.59)
Time trend	Yes	Yes	Yes	Yes	Yes
Counterfactual trend	Non-victims	Non-victims	Non-victims	Non-victims	Future victims
Time-variant covariates	Yes	Yes	Yes	Yes	Yes
Time-invariant covariates	No	No	No	Yes	Yes
$N_{individuals}$	1,993,359	1,972,752	1,920,847	1,920,847	23,366

Note: *t* statistics in parentheses; two-sided tests. [*] $p < 0.05$, [**] $p < 0.01$, [***] $p < 0.001$
See Appendix A for details on sample sizes for each model, and Appendix D for the full results.

Model 4 replicates the within estimate for violent crime victimization using the "random effects hybrid" model (Bell and Jones 2015), which allows for the simultaneous estimation of both within- and between-individual effects.[4] Based on these results, Figure 1 shows the effect of violent crime by means of the predicted turnout levels in 2009 and 2013 for those victimized and those not victimized between the two elections.[5] From Figure 1 it is evident that while turnout increases in general from 2009 to 2013, it

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[4] The random effects hybrid model estimates a random intercept model that includes the effects of both the individual's average level of victimization across the two time periods (the "between" effect), as well as the time-specific deviations from the overall average level (the "within" effect"): $Y_{it} = \beta_0 + \gamma(D_{it} - \bar{D}_i) + \rho\bar{D}_i + \beta_j X_{ijt} + \theta_i + \varepsilon_{it}$, where γ represents the "within" effect (identical to model 3), ρ represents the "between effect", or the average across-wave differences between victims and non-victims, and θ_i represents random individual-level intercept.

[5] To minimize confounding of the between-effect, and to make the analysis as comparable as possible to existing cross-sectional studies, we include a number of additional time-invariant control variables (see Appendix C and D).

increases significantly more for those who were victims of violent crime between the two elections than for the group that was not victimized.

Model 4 and Figure 1 also indicate that the within-individual approach may account for why our results differ from some previous cross-sectional studies of the relationship between violent crime victimization and turnout. In contrast to the positive within-individual effect, the between-individual relationship is strongly *negative*, with victims of violent crime being, across the two elections, approximately 7 percentage points less likely to vote than non-victims, controlling for all other covariates in the model (including time-invariant controls typically used in cross-sectional studies). This strongly suggests that much previous cross-sectional research has not sufficiently accounted for the differential turnout likelihood among individuals who experience violent crime; once these (unobserved) between-individual differences are taken into account, victimization from violent crime has a stimulating effect on subsequent voter turnout.

Figure 1: Estimated trends in Turnout
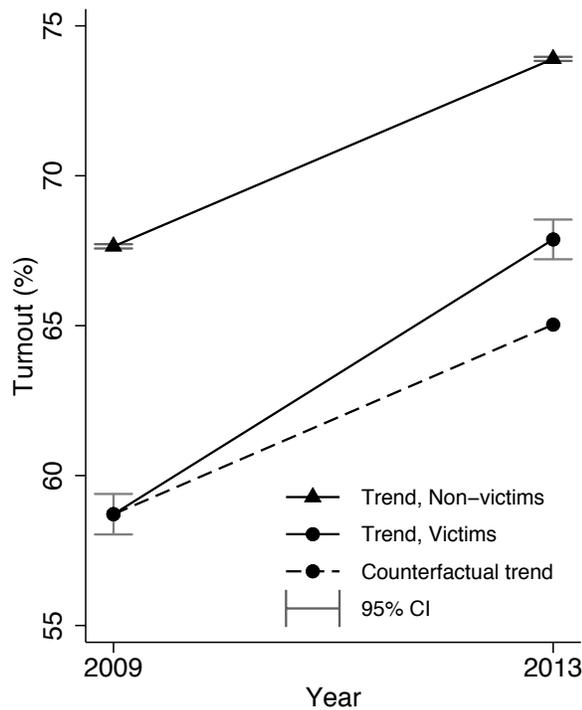
Note: Based on the estimates from Model 4

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As described above, we also employ an alternative, and plausibly more similar counterfactual group to those victimized in the 2009-2013 period—those who were victimized *after* the 2013 election (until the end of 2014). Model 5 in Table 1 shows the difference-in-differences estimate of the change in turnout for those who were victimized from a violent crime between the two elections compared to the change among the "soon-to-be" victimized group, with the same control variables included as in the other models. We again find a positive and significant within-individual effect (1.9 percentage points) of victimization from violent crime on turnout. Further, the negligible between-effect of victimization estimated using this counterfactual suggests that the two groups are indeed highly comparable. The robustness of the result using this plausibly identical counterfactual group gives strong confidence in the turnout-enhancing effect of violent crime victimization.

We probed the robustness of the effects of violent crime victimization in a number of ways (all reported in Appendix E). First, some citizens change location between elections, and features of the new environment may confound exposure to crime. To assess this possibility, we restricted our sample to non-movers. Second, we allowed for the effect of all covariates in Models 3 and 5 to vary across the two waves of the panel (i.e. interacting the 2013 time-dummy with all variables in the model). These models guard further against possible biases due to non-parallel trends in turnout between the victimized and non-victimized group. Third, it may be that victims are geographically concentrated in certain municipalities which, for unrelated reasons, show differential trends in turnout relative to areas with fewer victims. We therefore estimate a model that includes municipality fixed effects interacted with time, which allows for municipality-specific changes in turnout (Appendix Table E1.3 and E2.3). Lastly, for reference, Appendix Table E1.4 and E2.4 additionally report the results of Model 3 and 5 estimated without covariates. In all of these models, the effect of violent victimization remains positive, significant and highly similar to the results reported in Table 1. Our analyses thus provide consistent support for our main finding—that victimization from violent crime increases electoral turnout—as the effect is robust to alternative model specifications, samples and comparison groups.

CONCLUSION

Employing administrative panel data from the Danish population registries, we have shown that victimization from *violent* crime has a substantial positive effect on voter turnout. Our findings are consistent with the results from much previous work on political engagement aside from voting, but also

add the important qualification that differentiating between types of crime is critical, as victimization from non-violent crime has virtually no effect. More broadly, our findings regarding victimization from violent crime provide further support for the emerging view that exposure to violence can have pro-social, positive participatory consequences for ordinary individuals (Bauer et al. 2016). Our results also highlight the benefits of using panel data as opposed to cross-sectional data for studying the effect of crime victimization on political participation since this, as we have shown, leads to drastically different results. The large negative "between individual" effect of victimization found here strongly suggests that much previous research in the field has failed to adequately address selection effects in the victimization-turnout relationship.

While the analysis has succeeded in identifying the direction and magnitude of the causal effect of crime victimization on voter turnout, we cannot yet pinpoint the specific mechanism underlying this effect. However, consistent with Bateson (2012), our results speak against instrumental (self-interest) concerns as a key mechanism. From an instrumental perspective, the positive effect on turnout in municipal elections squares poorly with the fact that immediate crime-sanctioning mechanisms—law enforcement and sentencing policies—are not subject to decision at the local level in Denmark. Furthermore, given the difference between the consequences of violent and non-violent crime victimization that we identify here, mechanisms related to post-traumatic growth and anger appear to be more plausible candidates. Exploring how these processes, along with those related to identity affirmation and social solidarity, mediate the relationship between violent crime victimization and voter turnout should be important tasks for future research.

While we have argued that our design constitutes a significant improvement over previous studies, it does have its own limitations. We have only examined victimizations from crime incidents that were reported to the police, which thereby overlooks unreported crimes. We argued above, though, that underreported victimization is not likely to be a major problem in Denmark; further, unless unobserved victimization both varies between our control and treatment group *and* relates to changes in turnout, this should not result in biased estimates. A more significant concern is whether our results generalize to other types of elections and geographical settings. Given the relatively high salience of local elections in Denmark, we speculate that the effects found here are likely to obtain in high salience national elections as well; it may even be the case that the effects would be enhanced in those contexts, given victims' possible mobilization due to instrumental concerns or changes in their crime-oriented

policy preferences that could be enacted politically at the national level (Visconti 2019). More generally, it seems reasonable to expect our findings from Denmark—from what we believe to be the first study of the effects of crime victimization on turnout in advanced democratic contexts—to be generalizable to similar types of crime in other relatively low-crime Western societies with relative well-functioning state institutions.

However, the results may travel less comfortably to settings where crime is of a different nature (e.g., organized crime, crime originating in civil conflict, or crime related to electoral violence), or where state-citizens relations are more strained—both factors that might plausibly condition the effect of victimization on electoral turnout. We note that positive effects of victimization on registration and turnout have been found in several previous studies from conflict-laden contexts (e.g. Bellow and Miguel 2009; Blattman 2009), but more rigorous research on this question from a variety of contexts is clearly needed in order to assess the generalizability of the turnout-enhancing effects of (violent) crime victimization reported here. In this regard, departing from our study's two-wave panel set-up, future studies could be strengthened further via multi-wave analyses that could accommodate individually-varying pre-victimization turnout trajectories, and attempt to account more fully for possible biases due to unobserved time-varying confounders.

Finally, our results inescapably raise the provocative and seemingly paradoxical question of whether violent crime is good for democracy. Such a conclusion would be premature and too simplistic. For one, increased participation may not be accompanied by more democratic attitudes—in fact, sometimes the opposite (Bateson 2012, p. 583). Yet, in our view, this is not the most fruitful way of framing the question. Instead, a more reasonable—and sanguine—conclusion is that people are apparently willing and able to act politically, based on the hardships they experience.

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CRIME VICTIMIZATION INCREASES TURNOUT:
EVIDENCE FROM INDIVIDUAL-LEVEL ADMINISTRATIVE PANEL DATA

ONLINE SUPPLEMENTARY MATERIAL

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Appendix A
Data, sample details and trends in turnout

Data
The data on turnout, victimization and the control variables for the analysis is based on administrative population registers maintained by Statistics Denmark, and are merged via (encrypted) social security numbers. The data is stored at Statistics Denmark and can only be accessed through Statistics Denmark by researchers affiliated with a Danish Research Institution. Further information about the population registers, including access, can be found at http://www.dst.dk/en/TilSalg/-Forskningsservice.aspx and in Pedersen (2011).

Sample details
The *Danish Turnout Project* has collected individual-level turnout data form each of the about 1,300 polling stations in Denmark and verified them for analysis since 2009. For the 2009 election, we have turnout data for all eligible voters living in 44 out of a total of 98 municipalities, while we have data for almost the entire population of eligible voters in 2013. See Section 6 in the supporting information for Dahlgaard (2018) for more information. Eligible voters at Danish municipality elections are adults (18+) residing in Denmark who are either Danish, EU, Icelandic or Norwegian citizens. Other nationals have to live three years in Denmark in order to become eligible.[1]

Given our focus on the within-individual variation in turnout, we limit our sample to those individuals whose turnout we can observe in both 2009 and 2013; this effectively limits our sample to individuals who were eligible to vote in both 2009 and 2013 and who lived in one of 44 municipalities at the time of the 2009 election.[2]

Furthermore, we exclude individuals that were victimized before the 2009 election period (from January 1st 2005 and onwards) to eliminate the confounding effects of prior victimization and to control for other selection effects. This also enable us to avoid conflating the effect of prior victimization with victimization occurring in our observation period.

The individual-level turnout data contains information on turnout and when the vote was cast for some of the voters; i.e. on election day or via early voting within one month prior to the election. In the case of early voting, the data does not consistently reveal the exact date. This implies that we consider the entire month leading up to an election as the election period and excludes all individuals who were victimized in this period.

To avoid conflating the effect of non-violent and violent victimization, we exclude victims of non-violent crimes when estimating the effect of victimization from violent crimes and victims of violent crimes when estimating the effect of victimization from non-violent crimes.

In our last model (Model 5), we compare individuals that were victims of violent crimes between the two elections to those individuals that only were victimized from violent crimes between the 2013 election and the 31st of December 2014, which excludes all individuals who were not victimized between 2005 and 2014 (or victimized from non-violent crimes in the same period). Table A1 provides an overview of the sample sizes for the estimated models.

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[1] In 2009, 5,4% of the eligible voters were non-Danish citizens, see
https://valg.oim.dk/media/16130/saernummer-af-statistiske-efterretninger-om-kommunal-og-regionalvalget-2009.pdf.
[2] In analyses, reported in Model E1.1 and E2.1 in Appendix E below, we exclude those voters who moved between elections, which implies that we only analyze voters who lived in one of the 44 included municipalities in both 2009 and 2013. Reassuringly, the results are substantially similar to the results obtained with the full sample.

Table A1: sample sizes for the estimated models

Description	N
Eligible voters in the 2009 election	4,191,725
- voters not living in one of the 44 municipalities at the start of the 2009 election period	1,848,622
- 2009 eligible voters not eligible to vote in 2013 election (deceased, migrated)	182,124
= Voters with observed turnout in both 2009 and 2013	2,160,979
- Voters victimized at least once before 2009 election period (2005 to 2009)	107,155
- Voters victimized in the early voting period in 2009 and/or 2013	4,480
- Voters with missing information on covariates in 2009 and/or 2013*	55,985
= Sample Size, Model 1	**1,993,359**
- Voters who were victims of violent crimes 2009-2013	20,607
= Sample size, Model 2	**1,972,752**
Sample size, Model 1	1,993,359
- Voters who were victims of non-violent crimes 2009-2013	72,512
= Sample size, Model 3-4	**1,920,847**
- not victimized at all between January 2005 and December 2014	1,897,481
= Sample size, Model 5	**23,366**

*Approximately 90% of missing values is due to missing information about level of education. Level of education is missing for a fraction of older people and for a fraction of immigrants. Excluding education as a covariate in the model reported in the paper increases the sample with approximately 50.000 individuals but leaves the within-coefficients virtually unaffected.

Trends in turnout 2009-2013

Table A2 shows the turnout rate in 2009 and 2013 across individuals included in Models 1 through 5, respectively. Table A3 shows the individual-level transitions with respect to turnout in 2009 and 2013 for individuals included in Model 1.

Table A2: Turnout 2009 and 2013

	2009	2013	N
Individuals included in Model 1	66.9%	74.2%	1,993,359
Individuals included in Model 2	67,1%	74,4%	1,972,752
Individuals included in Model 3/4	67,1%	74,3%	1,920,847
Individuals included in Model 5	47,9%	58,0%	23,366

Table A3: Individual-level changes in turnout

	2013	
	Voted	Did not vote
2009 Voted	59,0%	7,9%
2009 Did not vote	15.2%	17.9%

The table is based on individuals included in Model 1
N=1,993,359

Appendix B
Victimization data

We have access to individual-level victimization data for the entire population for the period 2005 to 2014. The data is provided by Statistics Denmark, which obtains the data from the Danish Police. The data contains information about the identity, the type of offense, and the date of the incident for all alleged victims from certain types of crimes (see below). The data records incidents that were reported to the police (by the victim, by others or by the police). The data records all reports about alleged victims to crimes of the following types: assault, robbery, sexual offences, violations of restraining orders and certain types of larceny (fraud and theft, but not burglary) as well other crimes such libel, defamation, illegal discrimination, illegal surveillance, and identity theft.

To distinguish violent crimes from non-violent, we code assault, robbery, sexual offences, violations of restraining orders as violent crimes, larceny and the remaining offenses as non-violent crimes. Table B1 shows the number of victims based on the individuals included in Models 1 through 5, respectively.

Table B1

Description	N	Share of sample
Individuals from Model 1 victimized from any crime (at least once) between the 2009 and 2013 election	95,541	4.6%
Individuals from Model 2 victimized only from non-violent crime (at least once) between the 2009 and 2013 election	70.934	3.6%
Individuals from Model 3-5 victimized only from violent crime (at least once) between the 2009 and 2013 election	19.029	1.0%
Individuals from Model 5 not victimized from violent crime between the 2009 and 2013 election, but victimized between the 2013 election and the end of 2014	4.337	18.6%

Appendix C
Control variables

Table C1 provides details about coding and descriptives for the control variables. All control variables are measured using official records maintained by Statistics Denmark.

Table C1: Control variables

Variable	Description	Mean/Share	Standard deviation	
			Within individuals	Between individuals
Time-variant controls				
Danish Citizen (yes)	Citizenship status at time of the election	0.96	0.03	0.20
Employment status	Main activity in year of election			
Unemployed		0.09	0.16	0.24
Employed		0.63	0.20	0.44
Retired		0.25	0.13	0.41
Student		0.03	0.11	0.14
Income	Yearly income (including social transfers) in election year measured in million Danish kroner	$2.99 \cdot 10^{-7}$	$5.99 \cdot 10^{-8}$	$1.68 \cdot 10^{-7}$
Education	Years of full-time education completed at the end of the election year measured as the time required to obtain the highest level of education completed	13.49	0.49	2.92
Violent Victimization %	Share of municipality population victimized from violent crimes in election year	3.82	0.41	0.87
Time-invariant controls				
Male (yes)	-	0.50	-	0.50
Age	Age in years halfway between elections (November 2011)	46.61	-	17.33
Danish Ancestry				
2nd generation or more	Individuals with at least one parent, who was born in Denmark and who holds Danish citizenship	0.92	-	0.28
1st generation	Individuals with parents born outside of Denmark	0.01	-	0.10
Immigrant	Individuals born outside of Denmark	0.07	-	0.26

The table is based on individuals included in Model 1
N=1,993,359

Table D1: Full Results from Table 1

Model	1	2	3	4	5
Victimization	Both	Non-violent	Violent	Violent	Violent
Counterfactual trend	Non-victims	Non-victims	Non-victims	Non-victims	Future victims
Victim	0.005*	-0.002	0.029***	0.029***	0.019*
-*within*	(2.56)	(-1.14)	(7.44)	(7.44)	(2.10)
				-0.150***	-0.005
-*between*				(-26.61)	(-0.40)
Danish Citizen	-0.026***	-0.026***	-0.026***	-0.026***	-0.035
-*within*	(-3.54)	(-3.48)	(-4.44)	(-3.44)	(-0.70)
				0.072***	0.039**
-*between*				(34.69)	(2.88)
Employment Status			Reference = unemployed		
Employed	-0.007***	-0.007***	-0.008***	-0.008***	-0.013
-*within*	(-5.81)	(-5.73)	(-6.59)	(-6.59)	(-1.45)
				0.172***	0.200***
-*between*				(132.50)	(23.42)
Retired	-0.011***	-0.010***	-0.011***	-0.011***	-0.032†
-*within*	(-6.89)	(-6.68)	(-6.79)	(-6.79)	(-1.82)
				0.073***	0.031**
-*between*				(49.32)	(2.62)
Student	-0.002	-0.002	-0.000	-0.000	0.004
-*within*	(-1.05)	(-1.03)	(-0.05)	(-0.05)	(0.28)
				0.224***	0.285***
-*between*				(94.93)	(19.16)
Income (mil. DKK)	0.029***	0.029***	0.030***	0.030***	-0.002*
-*within*	(8.86)	(8.99)	(9.05)	(9.05)	(-0.05)
				0.002***	0.024
-*between*				(11.69)	(1.10)
Education	0.010***	0.010***	0.010***	0.010***	0.001
-*within*	(23.76)	(24.00)	(24.08)	(24.08)	(0.30)
				0.022***	0.027***
-*between*				(216.40)	(23.46)
Violent Victimization % (Municipality)					
-within	-0.012***	-0.012***	-0.012***	-0.012***	-0.008
	(-20.91)	(-20.86)	(-20.55)	(-20.55)	(-1.44)
				-0.014***	0.002
-between				(-46.73)	(0.51)
Male				-0.031***	-0.012*
-between				(-59.11)	(-2.25)
Age				0.006***	0.007***
-between				(259.22)	(31.11)

cont.

Table D1 cont.

Model	1	2	3	4	5
Victimization	Both	Non-violent	Violent	Violent	Violent
Counterfactual trend	Non-victims	Non-victims	Non-victims	Non-victims	Future victims
Danish Ancestry	Reference = 2nd generation or more				
First generation -between				-0.177*** (-109.79)	-0.139*** (-12.58)
Immigrant -between				-0.177*** (-60.86)	-0.137*** (-9.05)
2013	0.063*** (130.12)	0.063*** (129.59)	0.063*** (128.62)	0.063*** (128.62)	0.082*** (9.39)
Constant	0.611*** (76.80)	0.610*** (65.83)	0.606*** (64.02)	-0.058*** (-18.96)	0.543*** (8.47)
R-squared -within	0.024	0.024	0.024	0.024	0.037
R-squared -between				0.132	0.028
ICC (rho)	0.566	0.565	0.567	0.396	0.550
N$_{individuals}$	1,993,359	1,972,752	1,920,847	1,920,847	23,366

t statistics in parentheses, two-sided test. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E1: Robustness Checks, Model 3

Model	E1.1	E1.2		E1.3		E1.4
	Movers Excluded	Interactive Time Effects		Interactive Time Effects With municipality time trends		Without covariates
		Baseline	Interactions	Baseline	Interactions	
Victim	0.034***	0.018***		0.016***	0.018***	0.032***
-within	(7.97)	(4.47)		(4.04)	(5.08)	(8.20)
Danish Citizen	-0.015†	-0.043***	0.035***	-0.043***	0.034***	
-within	(-1.74)	(-5.69)	(18.57)	(-5.73)	(18.13)	
Employment Status			Reference = unemployed			
Employed	-0.012***	-0.001	-0.005***	-0.003†	-0.002	
-within	(-9.92)	(-0.82)	(-3.21)	(-1.86)	(-1.15)	
Retired	-0.010***	0.065***	-0.108***	0.064***	-0.105***	
-within	(-5.97)	36.96	(-65.43)	(36.18)	(-63.30)	
Student	0.003	0.014***	-0.029***	0.015***	-0.027***	
-within	(1.03)	(5.71)	-8.12	(5.99)	(-7.54)	
Income (mil. DKK)	0.025***	0.103***	-0.019***	0.013***	-0.023***	
-within	(7.16)	(3.55)	(-7.04)	(3.61)	(-8.51)	
Education	0.013***	0.003***	0.003**	0.003***	-0.000***	
-within	(24.94)	(7.25)	(2.37)	(6.46)	(-0.06)	
Violent Victimization % (Municipality) *-within*	-0.002***	-0.012***	-0.001***			
	(-2.54)	(-19.22)	(-3.04)			
2013	0.060***	0.069***		0.061***		0.072***
	(110.17)	(22.74)		(22.78)		(211.14)
Constant	0.546***	0.695***		0.629***		0.671***
	(57.76)	(70.49)		(63.61)		(3923.14)
Municipality fixed effects	No	No		Yes		No
R-squared (within)	0.020	0.031		0.034		0.023
ICC (rho)	0.575	0.572		0.572		0.572
N$_{individuals}$	1,691,780	1,920,847		1,920,847		1,920,847

t statistics in parentheses, two-sided test. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E2: Robustness Checks, Model 5

Model	E2.1	E2.2		E2.3		E2.4
	Movers Excluded	Interactive Time Effects		Interactive Time Effects With municipality time trends		Without covariates
		Baseline	Interactions	Baseline	Interactions	
Victim	0.026**	0.019*		0.017†		0.019*
-*within*	(2.61)	(2.11)		(1.88)		(2.11)
Danish Citizen	-0.025	-0.038	0.056***	-0.028	0.052***	
-*within*	(-1.46)	(-0.76)	(4.11)	(-0.56)	(3.76)	
Employment Status		Reference = unemployed				
Employed	-0.024*	-0.020†	-0.016	-0.022*	0.021†	
-*within*	(-2.28)	(-1.81)	(-1.33)	(-2.01)	(1.72)	
Retired	-0.033†	0.015	-0.053***	0.010	-0.047**	
-*within*	(-1.74)	(0.75)	(-3.64)	(0.49)	(-3.27)	
Student	0.005	0.017	-0.035	0.014	-0.027	
-*within*	(1.31)	(1.00)	-1.47	(0.79)	(-1.12)	
Income (mil. DKK) -*within*	0.005	0.024	0.017	-0.024	0.017	
	(0.04)	(-0.67)	(0.57)	(-0.69)	(0.59)	
Education	0.002	0.001	-0.001	0.001	-0.001	
-*within*	(0.06)	(0.17)	(-0.97)	(0.27)	(-0.88)	
Violent Victimization % (Municipality) -*within*	0.019*	-0.005	-0.007†			
	(2.03)	(-0.91)	(-1.75)			
2013	0.087***	0.071*		0.024		0.086***
	(8.61)	(2.48)		(1.05)		(10.54)
Constant	0.461***	0.544***		0.500***		0.479***
	(5.84)	(8.03)		(7.51)		(272.71)
Municipality fixed effects	No	No		Yes		No
R-squared (within)	0.034	0.037		0.046		0.024
ICC (rho)	0.568	0.550		0.554		0.547
N$_{individuals}$	18,392	23,366		23,366		23,366

t statistics in parentheses, two-sided test. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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