

Resisting Broken Windows

The Effect of Neighborhood Disorder on Political Behavior

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Abstract Concurrent housing and opioid crises have increased exposure to street-crime, homelessness and addiction in American cities. What are the political consequences of this increased neighborhood disorder? We examine a change in social context following the relocation of homelessness and drug treatment services in Boston. In 2014, an unexpected bridge closing forced nearly 1,000 people receiving emergency shelter or addiction treatment to relocate from an island in the Boston Harbor to mainland Boston, causing sustained increases in drug-use, loitering, and other features of neighborhood disorder. Residents near the relocation facilities mobilized to maintain order in their community. In the subsequent Mayoral election, their turnout grew 9 percentage points while participation in state and national elections was unchanged. However, increased turnout favored the incumbent Mayor, consistent with voter learning about candidate quality following local shocks. Voters responded to neighborhood changes at the relevant electoral scale and rewarded responsive politicians.

Keywords Neighborhood disorder · local politics · political behavior · voter attribution

1 Introduction

Concurrent homelessness and drug crises rage across many North American cities, sparking debates about the appropriate political response (Wing, 2019). These spatially concentrated crises portend particular worry because their visible elements – tent encampments, drug sales, and syringes – encroach on the spaces lived and traveled on by citizens (Kelling and Coles, 1997). Consequently, responding to homelessness and addiction is a central question of dozens of recent local elections (Appendix Table A1), defining the tenures of incumbent mayors across the country (Malas and Lazo, 2018). This anxiety around exposure to homelessness and addiction reflects the longstanding concern “that the street is disorderly, a source of distasteful, worrisome encounters” (Wilson, 1975, 65). Neighborhood disorder – often metaphorically described as “broken-windows” – has for decades motivated aggressive “quality-of-life” policing and dominated mayoral elections (Vitale, 2008).

Despite the importance of order maintenance to local politics, political science has been largely silent about its effect at the ballot box.¹ Yet a strong theoretical basis suggests that exposure to neighborhood disorder will increase political participation and influence vote choice, particularly in local elections (Michener, 2013). While political opinion in the United States is increasingly nationalized – and hence

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¹ But see van Noord et al. (2018).

decoupled from spatial variation in issue impacts – proximity to crime remains an important determinant of attitudes towards crime and policing (Hopkins, 2018). Street-level encounters with homeless and drug-using populations heighten fear of crime (Wilson, 1968; Vitale, 2008), and provoke discomfort and disgust among many individuals (Clifford and Piston, 2017). Fear and discomfort stemming from repeated exposure to disorder likely impels residents to lobby police and officials to address disorder and to organize new forms of social capital to amplify their collective voice. Thus, we hypothesize that residents will respond to neighborhood disorder by mobilizing to protect their neighborhood, resulting in higher levels of political participation, and that the intensity of the response will increase with proximity to disorder.

To identify how disorder affects voting behavior, we examine an abrupt relocation of homeless and drug-using populations in Boston. We combine rich contextual analysis of the case with large-scale quantitative analyses of (1) daily requests for government services, (2) individual-level participation in local, state, and national elections, and (3) electoral choices in Mayoral and Presidential elections. We find that requests for government services increased, turnout increased in local elections (but not in state or national ones), and, contrary to our expectations, affected precincts swung towards the incumbent Mayor, but showed no changes in the next Presidential election.

These results provide the first systematic evidence that increased disorder alters voter behavior, increasing turnout and changing vote choice. We further show that changes in neighborhood disorder cause individual-level political responses at local, but not state or national, levels. In this case, voters responded solely at the relevant electoral scale – the local government, responsible for policing, public works, and siting shelters and drug treatment facilities. This mobilizing effect is potentially worrisome to the extent that it hinders the construction of needed, but locally unwanted, social-services like homeless shelters. Moreover, these results suggest that the electoral effect of negative neighborhood shocks is contingent on the incumbent's response. The abrupt relocation we study is neither a natural disaster nor an economic downturn but shares a complex causal origin, high salience, and an opportunity to learn about the incumbent's quality through her response (Ashworth et al., 2018). Consistent with studies of natural disasters (Healy and Malhotra, 2009, 2010), we find the incumbent benefited from adeptly managing the fallout.

2 The Politics of Disorder

Disorder comprises the physical (e.g. litter, graffiti, vacant buildings) and behavioral (e.g. public drug use, loitering, panhandling) features of neighborhoods that suggest the deterioration of social control and quality of life (Gracia, 2014, 4325). More informally, disorder tracks “readily observable” features of “bad” neighborhoods (Michener, 2013; Sampson and Raudenbush, 1999). Neighborhood residents report highly similar levels of disorder regardless of race, class, homeownership, and age (Skogan, 1992, 54–77), though the racial and economic composition of neighborhoods also shapes perception of disorder (Sampson and Raudenbush, 2004). A rich literature in sociology and public health suggests that disorder forms an important part of neighborhood context, associated with fear of crime (Hinkle and Weisburd, 2008), distrust of neighbors (Ross and Jang, 2000), stress (Latkin and Curry, 2003), and reduced outdoor exercise (Molnar et al., 2004).

Disorder is linked to the political process through the government’s role in providing safety and sanitation on the streets and sidewalks and in providing shelter and social services to disadvantaged people. Political participation can pressure governments to provide additional services to manage disorder, such as policing. A community leader in Ridgewood, Queens explains, “When you hear of a drug location, you have to scream bloody murder to the police captain until you get results” (Kelling and Coles, 1997, 56). Indeed, maintaining order is a central demand that citizens place on their police, and police foot patrols are a common response to these demands (Wilson, 1968, 1975). Moreover, political action can pressure governments to devote additional resources towards managing public streets and sidewalks to reduce physical features of disorder like discarded needles (Melendez, 2018) and human feces (Tyler, 2018). Lastly, political action can disperse sources of disorder like tent encampments (Holland, 2018) and halt or delay the construction of new facilities perceived to herald disorder (Bittle, 2017) – to defend neighborhoods from unwanted changes (Einstein et al., 2019).

Because people are more likely to participate when benefits exceed costs (Riker and Ordeshook, 1968), we expect that disorder will increase political participation. Our argument proceeds as follows: Disorder diminishes quality-of-life and is linked to the political process through the provision of government services like assistance to the poor, policing, and sanitation. Consequently, citizens will become

involved in politics to impel government officials to manage disorder. In other words, the instrumental benefits of participation increase as disorder increases. Such reactive political behavior parallels NIMBY opposition to local development (Hankinson, 2018) and other “locally unwanted land uses” (Popper, 1983). Dahl (1961) captures our argument in his account of neighborhood resistance to metal-roof apartments in mid-century New Haven. Faced with what they perceived to be the existential threat of “slum-housing” development, residents of the Hill Neighborhood saw their fate linked with the political approval of the development’s zoning permit. Organizing to pressure the city to deny a permit for the development, they formed a neighborhood association, raised money to mount a legal defense, and protested at city meetings (Dahl, 1961, 193-197).

Living in a neighborhood marked by features of disorder is a source of aggravation, fear, and disgust for many residents (Skogan, 1992; Michener, 2013). As one Seattle resident explains, “There’s a level of anxiety having a homeless camp on the other side of my hedge. It gets higher as the garbage grows” (Wing, 2019). Threat of neighborhood decay may increase the benefits of political participation to the extent that residents believe that relief comes from government intervention. Mobilized residents may create new forms of social capital that amplify the collective voice (Orbell and Uno, 1972). For example, people who perceive more disorder in their community are more likely to attend community meetings (Michener, 2013). The development or renewal of civic organizations, in turn, alters the calculus of political behavior in favor of more participation: group membership subsidizes information, increases mobilization, and provides opportunities for honing civic skills (Leighley, 1996). Moreover, disorder may induce expressive political participation, providing an outlet for residents to “move beyond fear and submission” and reinvent themselves as organizers and activists (Bateson, 2012, 572).

Thus, increased disorder should provoke a sharp political response. Disorder portends both fear of crime and a more general neighborhood deterioration (Skogan, 1992), and resolution of these fears are linked to the political process through government provision of public safety, sanitation, and social services to manage disorder. Political action provides both the means for residents to impel government to better manage disorder and a sense of control amid the chaos. These theoretical considerations lead to the following hypothesis:

H1: Increased neighborhood disorder will increase political participation.

Changes in neighborhood disorder may also affect vote choice. Local voters may blame elected officials and vote against incumbents in subsequent elections (Hopkins and Pettingill, 2018). Anecdotal evidence suggests that voters punished incumbent Mayors who failed to manage growing disorder in the 1990s (Vitale, 2008). More generally, a robust literature documents retrospective voting in American elections, as voters consider changes in their own lives during an incumbent’s tenure to assess candidate performance and inform their vote choices (Fiorina, 1981; Healy and Malhotra, 2013; Malhotra and Margalit, 2014; Burnett and Kogan, 2017). Recent work assesses whether natural disasters and other “shocks” influence incumbent electoral fortunes (Healy and Malhotra, 2009, 2010). Events outside the direct control of a politician still offer an opportunity for rational voters to learn about the competence of their elected official through her response, potentially shaping re-election prospects (Ashworth et al., 2018). When neighborhood quality-of-life declines during the tenure of the incumbent, voters may learn that the incumbent is not equipped to deal with the problem and instead support a potentially higher-quality challenger. These works undergird our second hypothesis:

H2: Increased neighborhood disorder will increase anti-incumbent voting.

The mechanisms through which neighborhood disorder provokes political responses are functions of individual geographic proximity to neighborhood changes. Those living closest to sources of neighborhood disorder will be those most affected by it (Faraji et al., 2018). Thus, threat perception, and ensuing political response, should intensify with proximity to the sources of neighborhood disorder (Enos, 2017). Testing this hypothesis speaks to whether residents are reacting to real changes in their everyday experience (“objective” disorder) or their perceptions of changes from local media and politicians (“subjective” disorder) (Michener, 2013). We formalize this intuition as hypothesis three:

H3: Treatment effects will increase with proximity to the affected sites.

We test these hypotheses by measuring changes in political behavior in national, statewide, and local elections before-and-after a change in neighborhood disorder. Estimating effects at different electoral scales provides insight into whether the political response to disorder is targeted at the level of government closest to management of the problem (Larsen, 2018) or spread across all levels of government (Sances, 2017).

Fig. 1: Key Events in the Long Island Shock

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- A vertical timeline chart with a blue vertical line on the left. To the right of the line are circular markers indicating specific dates and events. The events are listed from top to bottom:
- 1951: City builds a bridge connecting Long Island in the Boston Harbor to the mainland.
 - 1983: City opens its largest homeless shelter and addiction treatment services on island.
 - Oct. 8, 2014 (3PM): Long Island Bridge deemed imminently unsafe.
 - Oct. 8, 2014 (3:50 PM): City declares immediate evacuation of island. Police block island access.
 - Oct. 8, 2014 (7PM): Hundreds of residents relocated to emergency sites. Island services are terminated.
 - Jan. 2015: Newly built Southampton Street Shelter opens near emergency sites.
 - April 2016: Supportive Place for Observation and Treatment (SPOT) opens in South End.
 - Oct. 2016: Opioid Urgent Care Center opens in South End.
 - Nov. 7. 2017: Walsh defeats challenger Tito Jackson in Mayoral race.

3 Research Design

To test the effect of neighborhood disorder on political participation, we analyze an abrupt change in the geographic distribution of homeless and drug-using populations in the city of Boston, detailed in Figure 1. For decades, Long Island, an island located in the Boston Harbor, was the hub of the city's social-services, including the city's largest homeless shelter and most of its addiction treatment services (BHCHP, 2014; Ramirez, 2015). That changed when an inspector unexpectedly condemned the bridge connecting the island with the mainland in October 2014.² Following the condemnation of the bridge, the city of Boston abruptly terminated the social services provided on the island, including 57% of the city's total substance use treatment beds (BHCHP, 2014). About 1,000 people receiving daily services were displaced, including 450 homeless people previously staying in the Long Island Homeless Shelter and another 300 people in recovery living in residential detoxification facilities and transitional housing programs (Ramirez, 2015). Displaced populations were primarily relocated to the Boston Medical Center area at the boundaries of the South End, Roxbury and Dorchester neighborhoods, as shown in Figures 2 and 3. This area was the site of the immediate re-location because it contained several facilities operated by the Boston Public Health Commission, which had just three hours to coordinate the relocation (BHCHP, 2014). Temporary relocation facilities were eventually replaced by a newly built shelter and renovated sites in the area (Pressley, 2015).

Closure of Long Island facilities resulted in rapid changes in neighborhoods bordering the relocation sites. This part of Boston – along a stretch of Massachusetts Avenue containing the Boston Medical Center, neighborhoods of the South End and Lower Roxbury, and the mostly industrial Newmarket Square district – has long been home to a concentration of social service providers and methadone clinics, giving it the reputation of “Methadone Mile” (Daniel, 2016). But existing residents and local employees reported that the quality-of-life in the neighborhood declined sharply after the Long Island Bridge closure. Between January and October of 2015, violent crimes increased 30% and drug violations by 55% near the new shelter, while these crimes declined citywide (Abel, 2015). Quantitative trends match qualitative accounts of the neighborhood change. One resident posted that “I've been periodically walking from my home in the South End to my business at Newmarket since 1988. the situation in 2015 is far worse than I've ever seen. I now walk up Albany St. rather than Mass Ave. because of the crowds... to avoid any drug trafficking. Also, the level of theft and break-ins in the Newmarket neighborhood is sky-rocketing” (Reilly, 2016). Years later, residents, politicians, and the media have continued to blame the closure of the Long Island Bridge for exacerbating public safety and quality-of-life issues in the neighborhood (Jonas, 2019).

Ground zero of the crisis is the busy intersection of Melnea Cass Boulevard and Massachusetts Avenue, situated between Boston Health Care for the Homeless and the Wood Mullens Shelter to the north of the intersection and the Southampton Street Shelter and two methadone clinics south of the intersection.

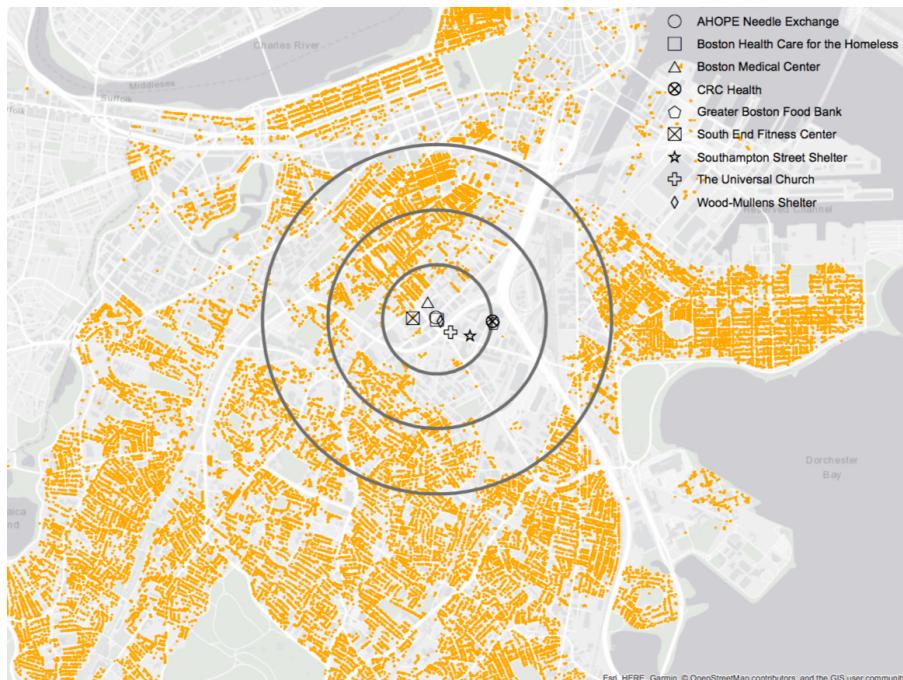
² The city had previously announced plans to begin repairing the 63-year old bridge in September. An engineer involved with the reconstruction project told local media that reconstruction would only require partial lane closures, and “under no circumstance will the whole bridge be closed” (Burrell, 2014).

Fig. 2: Map of Long Island and Methadone Mile



The approximate stretch of Massachusetts Avenue called Methadone Mile, the site of Long Island services relocation, is colored in red. Long Island is outlined in black.

Fig. 3: Map of Voters Near Sites



Dots represent individual voters. Circles mark areas within 500, 1000, and 1600 meters of Wood-Mullens Shelter.

To illustrate this, we downloaded images from Google Street View of the same plots of land at the intersection before and after the Long Island Shock. Figure 4 shows dozens of people loitering, and pervasive litter after the closure of the Long Island Bridge. While differences in the photos may also be attributable to seasonal differences³, these images reflect the neighborhood changes induced by the Long Island shock.

The closing of the Long Island Bridge and termination of the island's facilities increased the local density of homeless individuals and people who use drugs. The re-location of the displaced populations was not random, as they relocated to an area with existing facilities. But that the re-location occurred at all, and the timing of the increased neighborhood disorder, is plausibly unrelated to the attitudes and behaviors of voters living near the re-location sites. As such, we argue that this event constitutes an as-if random increase in neighborhood disorder.

Our primary measure of political behavior is geolocated, individual-level administrative voting records for every registered voter in Massachusetts. These data, which include gender, race and ethnicity, place of residence, and voting history, were organized and provided to the researchers by the political analytics firm L2. We supplement these data with citywide calls for services records, precinct-level electoral results that we digitized and geocoded, and home-ownership parcel data downloaded from the city assessor. We collected 311 non-emergency call data, a common measure of local political behavior ([Lerman and Weaver, 2014](#)), from the city of Boston's website and 911 call data from the Boston Police Department via public records request.

For each registered voter in Boston, we calculate the distances in meters from their residence to the sites that saw an influx in homeless persons and drug users following the Long Island Shock. A respondent's distance to the nearest site – the minimum of the distance to any of the sites – is our measure of exposure. With these data, we measure differences in political participation before and after the bridge closing as a function of proximity to the sources of neighborhood changes. Defining treatment (living close to the re-location sites) and control (living far from the re-location sites) by increasing distances, we measure differences in participation across various distances to address problems of aggregation and scale common to geographic analysis ([Openshaw, 1983; White, 1983](#)). To test the effect of increased neighborhood disorder on vote choice, we examine changes in incumbent vote share, calculating the distance of each voting precinct centroid to the relocation sites.

Following a Boston City Council report ([Pressley, 2015](#)), we define the scope of relocated sites as those shown in Table 1.⁴ While we measure distance to these specific sites, they are all within a few blocks of one another, and collectively serve as a measure of proximity to the increase in neighborhood disorder.

Table 1: New Locations for Long Island Facilities

Program	Beds	Bed Type	New Address
SOAR	50	Transitional Housing	112 Southampton Street
Safe Harbor	20	Housing for HIV patients	112 Southampton Street
Long Island Shelter	368	Emergency Shelter	112 Southampton Street
Woods Mullen Shelter	200	Emergency Shelter	794 Massachusetts Avenue
South End Fitness Center	200	Emergency Shelter	35 Northampton Street

We use a difference-in-differences design, estimating the change in turnout and vote share between like-elections before and after the shock to neighborhood disorder. Our unit of analysis for turnout is individual registered voters,⁵ and the unit of analysis for vote share is electoral precincts. We estimate the following average treatment effects:

$$\text{ATE} = [P(\text{Vote}_{t=2}|d < d^*) - P(\text{Vote}_{t=1}|d < d^*)] - [P(\text{Vote}_{t=2}|d > d^*) - P(\text{Vote}_{t=1}|d > d^*)] \quad (1)$$

³ The 2017 images are from July, while the 2013 images are from September. Google did not offer images in 2017 and 2013 from the same month.

⁴ The South End Fitness Center was used as a shelter until the Southampton Street Shelter opened in June 2015. We include it as a treatment indicator for the 2014 election only.

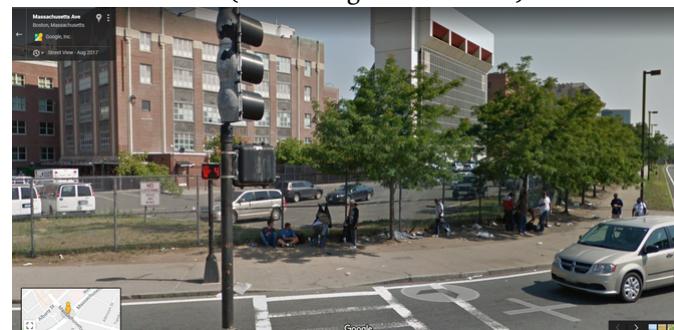
⁵ Our turnout analyses are restricted to voters who lived in the same residence and were registered to vote across both election cycles.

Fig. 4: Neighborhood Disorder: Evidence from Google Street View

Mass Ave. & Melnea Cass, 2013 (Before Long Island Shock)



2017 (After Long Island Shock)



Mass Ave. & Melnea Cass, 2014 (Before Long Island Shock)



2017 (After Long Island Shock)



In a linear regression, we estimate this ATE for turnout with the following equation,⁶ for voter i in election t :

$$\text{Turnout}_{it} = \alpha_i + \gamma_t + \beta[I(d_i < d^*) \cdot \gamma_t] + \epsilon_{it} \quad (2)$$

Here, α_i represents individual-level fixed effects, γ_t is a dummy for the treated period (or the change in turnout, on average, between election $t = 1$ and $t = 2$ for the untreated individuals). A voter's distance to the closest relocation facility is denoted d_i . $I(d_i < d^*)$ is an indicator variable equal to one for individuals who live within the designated distance of any facility, thus delineating treated units. We estimate this equation at increasing distances of d^* (100, 200, 300, ..., 1600 meters). Finally, ϵ_{it} is model error. The estimate for β – the change in turnout for treated units, relative to control – is our difference-in-differences estimator.

We also measure treatment as a continuous function of logged distance from the sites. We log distance because we expect that the effect of proximity decays exponentially; i.e., marginal increases in distance from the sites have less influence on the treatment the further away from the sites. That is, we estimate the following:

$$\text{Turnout}_{it} = \alpha_i + \gamma_t + \beta[\log(d_i) \cdot \gamma_t] + \epsilon_{it} \quad (3)$$

We use a similar difference-in-differences approach to estimate the effects on vote share. In precinct p , election t , the vote share received by the incumbent is modeled as follows:

$$\text{Incumbent Vote Share}_{pt} = \alpha_p + \gamma_p + \beta[I(d_p < 1 \text{ mile}) \cdot \gamma_t] + \Omega[\Lambda_i \cdot \gamma_t] + \epsilon \quad (4)$$

where α_p represents a precinct fixed effect, γ_p represents an election fixed effect (i.e., the swing to Mayor Walsh from 2013 to 2017 when d is zero), and β captures the extent to which the change in incumbent vote share varies between the 24 precincts that lie within one-mile of the vector of facilities and the remaining precincts in Boston (d_i is the minimum distance of every p th precinct centroid to any of the facilities). Λ_p is a vector of precinct-level controls for representing average quantities from the voterfile or relevant indicators for particular districts/neighborhoods (more details below) where Ω is a vector of coefficients corresponding to each control.

We estimate equations for 3 election pairs: the 2010 and 2014 midterm elections, the 2012 and 2016 general elections, and the 2013 and 2017 Boston mayoral elections.⁷ For all models, we first estimate the specification without co-variate controls. Second, we estimate specifications that use exact matching to compare treated and control individuals on the following characteristics: race, age, gender, income, home-ownership, and party identification. Age, gender and party identification are recorded directly in the Massachusetts voterfile. Race is imputed by L2. Income matching uses the average census block group income category of the voter's residence. We combined the voterfile data with property data from the city of Boston to identify if a voter is homeowner. The exact-matching algorithm matches each treated unit to all possible control units with exactly equal co-variate values (age is coarsened into 10-year bins), creating subclasses in which all treated and control units have the same covariate values. We then use the size of each subclass as weights in a Weighted Least Squares regression analysis while including the matching variables as covariates. Standard errors are clustered at the individual level in all turnout specifications.⁸

We estimate a third specification to account for potential confounding from different municipal candidates in 2013 compared to 2017. In 2013, Marty Walsh's opponent, John Connolly, was an at-large City Councillor from West Roxbury, and turnout in that neighborhood was much higher in 2013 than 2017. In 2017, Mayor Walsh ran against Tito Jackson, the city councillor from District 7, which covers parts of the Roxbury, Dorchester, South End, and Fenway neighborhoods of Boston. Changes in voter turnout or vote share between 2013 and 2017 may be a function of different candidates running, and since the candidates receive disproportionate shares of their home district's votes, a Jackson or Connolly effect may appear as an effect of proximity to the Long Island re-location sites. Therefore, we estimate a specification that matches on the same covariates as in the previous specification and includes "fixed-effects"

⁶ Because of the equivalence between "fixed-effects" and first differences with two time periods, an equivalent model can be expressed in terms of changes in turnout for each voter: $\text{Turnout}_{i1} - \text{Turnout}_{i2} = \beta[I(d_i < d^*)] + \epsilon_i$

⁷ We do not analyze changes in voting between city council local elections in non-mayoral election years due to variation across city council districts in challenges to incumbent city councillors. We also do not estimate changes in vote share between the 2010 and 2014 midterm elections because Boston's electoral precincts were changed in 2011 following the decennial census.

⁸ Our pre-analysis plan specified bootstrap standard errors. Results are not substantially or substantively different using either method, but the clustered standard errors tend to be larger than the bootstrapped ones, so we chose to report the more conservative clustered standard errors. We include bootstrapped standard errors in the Appendix Table A8.

for Boston's city council districts and the West Roxbury neighborhood (which is not coterminous with any city council district). These fixed effects are interacted with the time trend and represent the average trend in turnout for these places and account for changes in voting behavior driven by the presence of neighborhood locals on the mayoral ballot in one election but not the other, as well as competitiveness of ward city council races concurrent with the mayoral election. This makes the comparison for these places "within-neighborhood," a conservative approach that rules out the possibility that any of the changes in turnout between council districts and between West Roxbury and the rest of the city of Boston are due to distance from the relocation sites.⁹ We estimate such specifications for both the turnout and vote share outcomes.

Causal inference in the difference-in-differences setup hinges on the parallel trends assumption that changes in political behavior between residents located near the relocation sites, and residents of matched gender, income, age, party identification, homeownership, and race who live further away, would have been similar in expectation had the Long Island Shock not occurred. In Appendix Section 2, we examine pre-trends in turnout, finding that turnout rates moved in parallel between treatment and control areas prior to the shock (Figure A1).

4 Results

To evidence increased disorder, and that this increase altered interactions with government, we first show changes in requests for government services related to disorder. In the areas surrounding the relocation facilities, there was an uptick in 311 non-emergency requests for government services related to homelessness.¹⁰ As Figure 5 illustrates, the frequency of weekly service requests within one-mile of the relocation sites increased both relative to the rest of Boston and in absolute terms in the months following the October 2014 bridge closure. As these requests are "by definition a measure of the service demands that neighborhoods place on city governments" (White and Trump, 2018), their increase is consistent with burdensome neighborhood changes. But 311 calls have at least two kinds of measurement error: conceptual (311 calls in these categories may not exactly map onto to disorder) and false-positive (311 calls can be made without basis). Therefore, we perform a parallel analysis of 911 calls, focusing on 911 calls for overdoses and "emotionally disturbed persons" (police terminology for calls responsive to erratic behavior and/or mental health emergencies), obtained by public records request. These are less susceptible to measurement error because they are more specific and because fraudulent calls are illegal. Weekly 911 calls for drug poisonings and emotionally disturbed persons in the vicinity of relocation sites increased in absolute terms and relative to the rest of Boston (Figure 5). A negative binomial regression finds significant evidence of a larger discontinuous jump within a mile of relocation sites for both 311 calls and 911 calls ($p < 0.01$) after the Long Island Shock (Appendix Table A3).

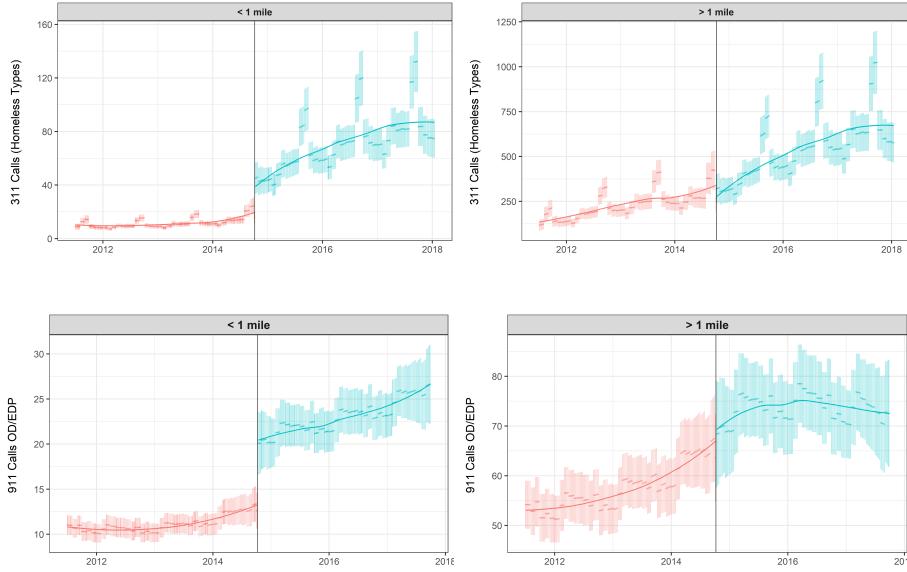
We next present results for the effect of the Long Island Bridge closing on turnout. Figure 6 presents the effect of the closing on the likelihood of voting in the next mayoral election with the treatment group defined as living within increasing distances from the re-locations sites. The number above each point estimate indicates the number of registered voters living within that distance – the treated sample. The estimates demonstrate that the neighborhood change following the Long Island Bridge closing caused increased turnout among proximate residents in the 2017 mayoral election relative to residents living farther away from the sites. The first plot displays the point estimates and 95% confidence intervals for the models without covariates. The second plot shows the point estimates for the specifications with both matching and separate trends by city council district and in West Roxbury. Except for the results with treatment defined as living within 300 meters of the sites, where the treated sample size is lowest, we find consistently significant ($p < .05$) positive effects on turnout across treatment definitions. Effect sizes range from approximately 5 to 9 percentage points, initially decreasing as treatment is defined by greater distances, stabilizing by about 800 meters.

Decreasing effects on turnout further away from the relocation sites suggest that the effect of neighborhood disorder increases with proximity. To formally test this hypothesis, we estimate models with

⁹ Including fixed effects for all city council districts, instead of a single dummy for Tito Jackson's district, deviates from our pre-registration. After receiving feedback about differential levels of competitiveness in ward city council races concurrent with the mayoral race, we were persuaded to use council district fixed effects. Results are substantively similar in sign, magnitude, and significance.

¹⁰ Homelessness-related 311 calls were identified by string searching the open-ended CLOSURE_REASON field in the data for the string "homeless" and using only calls in the set of call types ("Requests for Street Cleaning", "Ground Maintenance", "Poor Conditions of Property", "CE Collection", "Illegal Dumping", "General Comments For a Program or Policy", "Housing Discrimination Intake Form", and "Highway Maintenance") corresponding to these explicitly designated homelessness calls.

Fig. 5: Spike in weekly 311 Calls and 911 Calls following Long Island Closure



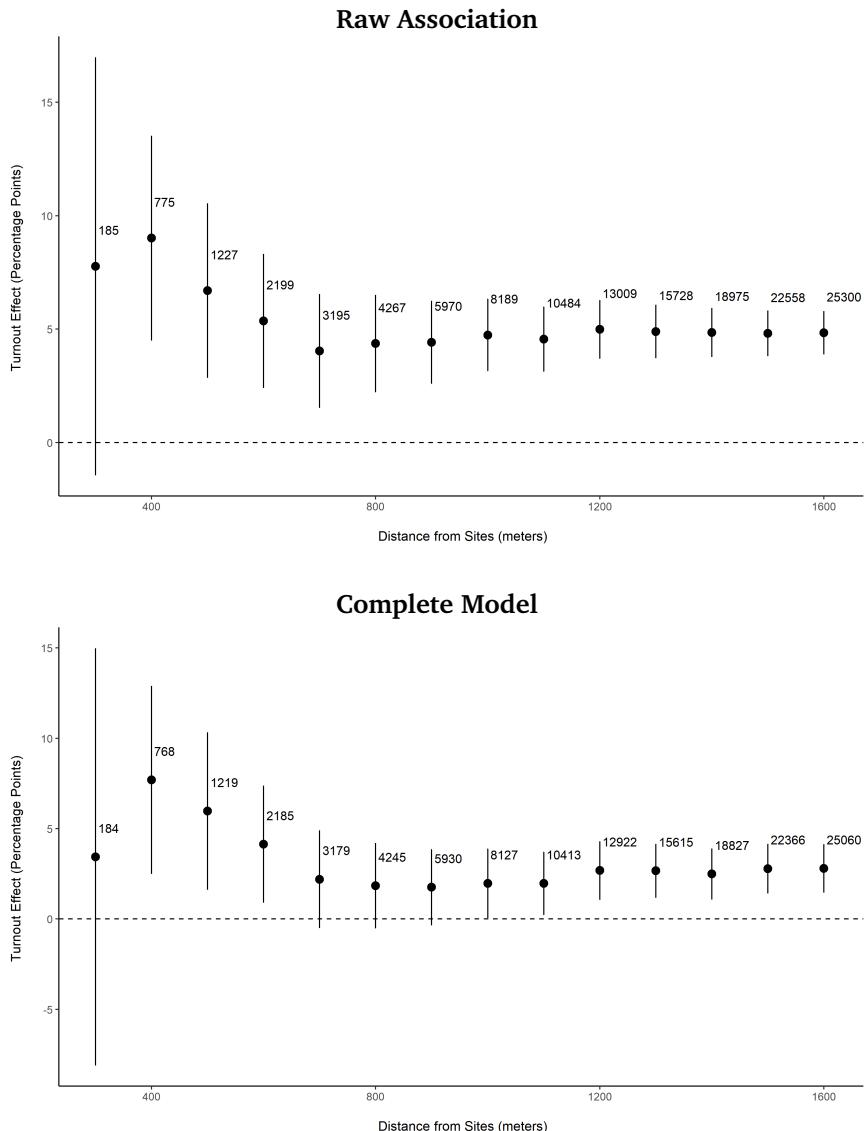
Plots show predicted weekly counts of 311 calls (top) and 911 calls (bottom), with 95% confidence intervals, from negative binomial regressions with month fixed effects, an indicator for after the date of the bridge closure, and linear and quadratic trends interacted with the date of the bridge closure. Points are connected by a Loess fit.

treatment as a continuous function of logged distance from the sites. Table 2 reports the results from 3 specifications: without covariates, with covariates, and with covariates and the time trend interacted with council district fixed effects and the West Roxbury neighborhood. Across models, voters are less likely to vote in 2017 compared to 2013 the greater the distance they live from the sites.¹¹ The effect sizes are substantially similar across models. These results provide evidence in support of Hypothesis 3, that the effect on turnout is a function of proximity.

Unlike mayoral elections, there is not evidence of substantial impact on turnout in statewide and national elections. Figure 7 shows the effect of proximity on changes in presidential turnout between 2016 and 2012 (top) and midterm voting between 2014 and 2010 (bottom) without any controls. The presidential election results are substantively small and generally indistinguishable from zero, with small, negative estimated effects at lower distances but small and positive at greater distances. Adjusting for demographic covariates shrinks estimates at higher distances towards zero, as shown in Appendix Table A10. The midterm election estimates hover around zero and are not statistically significant (Appendix Table A8 shows results adjusted for demographic covariates). The midterm and presidential specifications measuring the effect of treatment as a continuous function of logged distance from the sites (Appendix Tables A13 and A14) also show small (no demographic controls) or null (with demographic controls) effects on turnout.

¹¹ The 2017 coefficient is positive, but this is conditional on the distance being at zero (and the covariates being at base categories, where applicable). Generally, mayoral turnout was lower in 2017 than 2013.

Fig. 6: Mayoral Turnout, 2017 vs 2013



Point estimates represent change in turnout (for treatment relative to control) under the definition of treatment as living within the distance to the relocation sites indicated on the x-axis. Error bars are 95% confidence intervals. Numbers above each estimate indicate the number of treated voters. The top plot shows raw association changes in turnout; the bottom plot includes matching/controls for demographic variables, council district fixed effects, and an indicator for the West Roxbury neighborhood.

Taken together, these results show that increased disorder mobilized voters, but only in local elections.¹² The 5 to 9 percentage point effect on mayoral turnout is striking against baseline levels in local elections. In 2013, 38% of registered voters in the city of Boston voted in the mayoral election, and just 27.8% voted in the 2017 election.

The political effects were not limited to participation. Contrary to Hypothesis 2, proximate voters were *more likely* to support the incumbent Mayor, other things equal. Across various council-district fixed effect specifications¹³ (Table 3), the swing towards incumbent Marty Walsh was more than 5 percentage points larger in precincts within one-mile of the relocation sites, including when adjusting for

¹² The 2014 midterms occurred shortly after the Long Island Bridge closing, and voters may have not had time to process and react to the neighborhood changes. The national election turnout results do show positive turnout coefficients at the largest definitions of treatment, but these coefficients shrink towards zero and all but two lose significance in the specifications with matched covariates (Table A11 in the Appendix).

¹³ The main specifications for vote choice differ from those in our pre-registration. A detailed explanation is provided in Appendix Section 7.

Table 2: Turnout Change as function of log distance

	<i>Dependent variable:</i>		
	Mayoral Turnout, 2017 v 2013		
	(1)	(2)	(3)
2017	-0.126*** (0.003)	0.318** (0.139)	0.338** (0.133)
2017 · log(Distance)	-0.036*** (0.002)	-0.026*** (0.002)	-0.032*** (0.004)
2017 · West Roxbury			-0.052*** (0.009)
Covariates	N	Y	Y
Council District Trends	N	N	Y
Observations	430,444	430,444	430,444
R ²	0.747	0.752	0.753
Adjusted R ²	0.494	0.503	0.505
Residual Std. Error	0.355 (df = 215220)	0.352 (df = 215180)	0.351 (df = 215171)

Note:

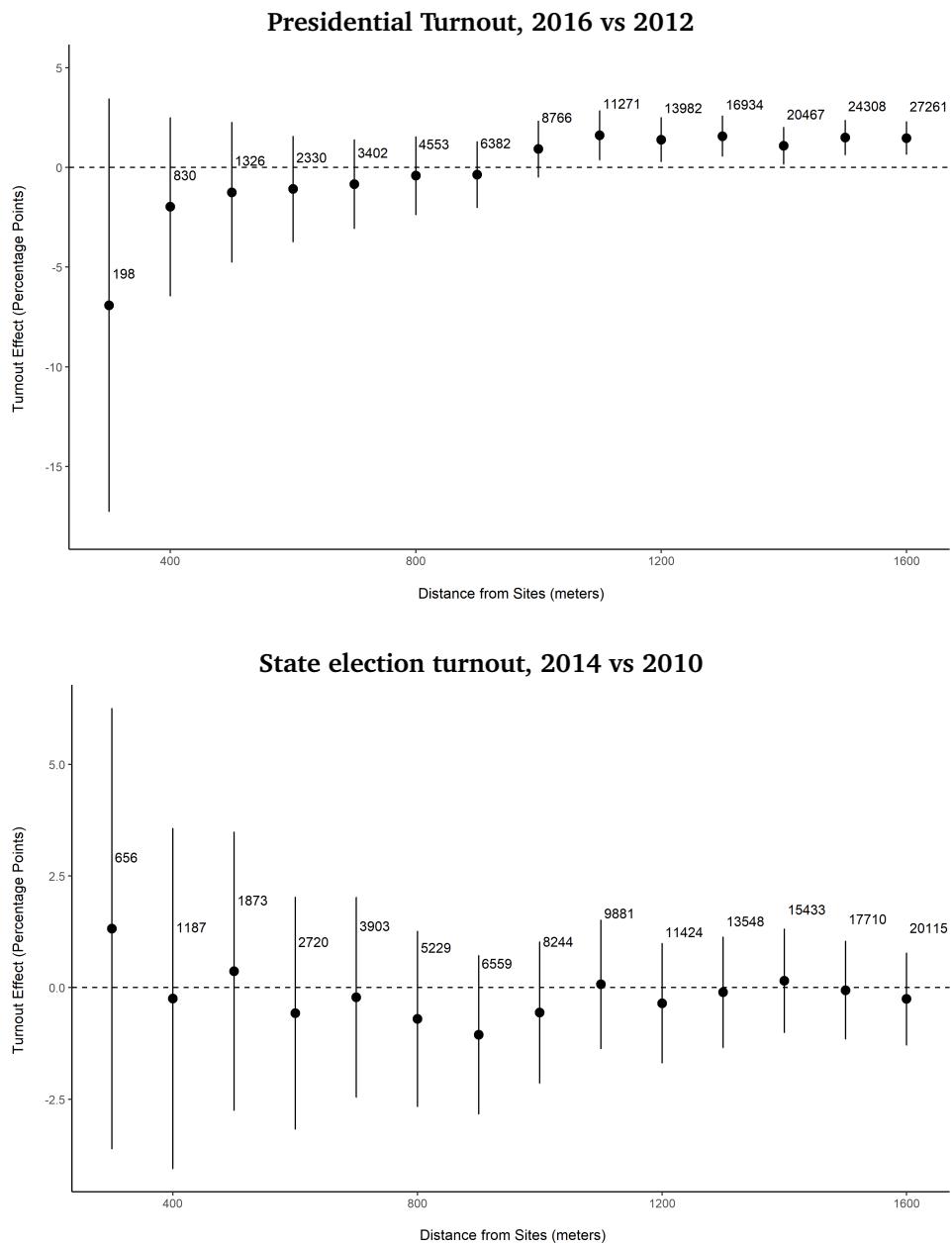
*p<0.1; **p<0.05; ***p<0.01

Table shows the coefficient estimates for the change in turnout as a function of the logged distance voters live from the relocation sites. Standard errors clustered at the individual level are shown in parentheses. Matching covariates in models 2 and 3 include voter race, age, gender, party identification, homeownership, and the median income of their census block.

demographic covariates (Model 2) and adding an indicator for the West Roxbury neighborhood, home of Walsh's previous (2013) opponent (Model 3). To give a sense of the effect-size, Figure 8 illustrates the change in Walsh Vote share at precincts near and far away from the relocation sites and the uncertainty of these estimates using the parameters from Model 3; precincts whose geographic centers fell within one-mile of the relocation sites experienced about a 15 percentage point swing to Walsh, while those further away experienced about a 10 percentage point swing (his citywide swing was 13.8%). No such incumbent swing was observed between the 2016 and 2012 Presidential races (Appendix Table A15).¹⁴

¹⁴ Precinct boundaries changed before the 2012 election, precluding a similar analysis of 2014 and 2010 vote share.

Fig. 7: Effect of Proximity on Turnout at other scales (raw associations)



Point estimates represent change in turnout (for treatment relative to control) under the definition of treatment as living within the distance indicated on the x-axis to the relocation sites. Error bars are 95% confidence intervals. Numbers above each estimate indicate the number of treated voters. There are more state election voters because at the time of the 2014 election, the South End Fitness Shelter was used as a relocation site prior to the opening of the Southampton Street Shelter.

Table 3: Mayoral Vote Share, 2017 vs 2013

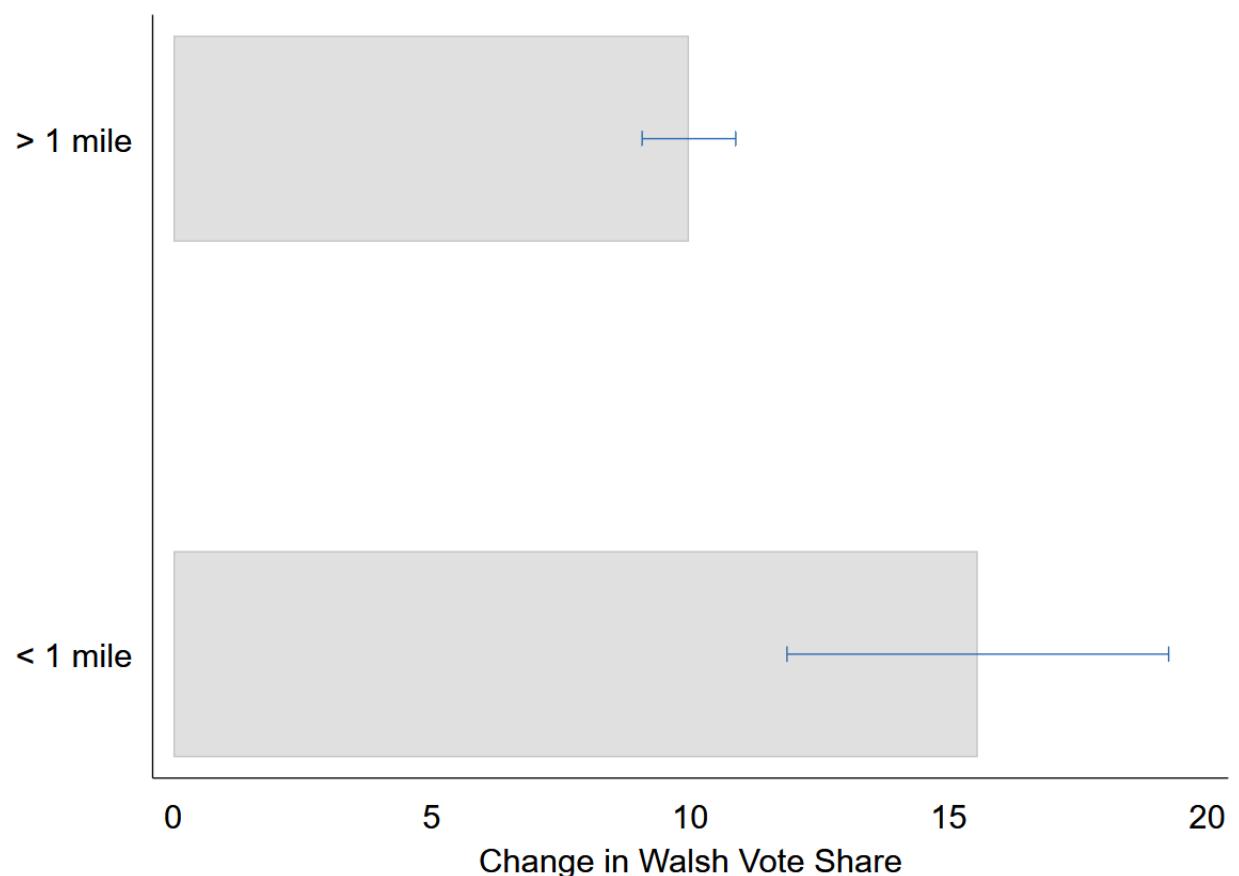
	<i>Dependent variable:</i>		
	Change in Walsh Vote Share		
	(1)	(2)	(3)
< 1 mile	8.698** (3.558)	6.714*** (2.337)	5.504*** (1.846)
White %		-0.043 (0.103)	-0.141* (0.082)
Hispanic %		-0.442*** (0.114)	-0.347*** (0.090)
Mean Age		1.377*** (0.179)	0.744*** (0.150)
Low Income %		0.115 (0.156)	0.060 (0.123)
High Income %		1.727*** (0.313)	2.470*** (0.254)
Black %		-0.352*** (0.091)	-0.393*** (0.072)
West Roxbury Dummy			32.028*** (2.658)
Council FE	Yes	Yes	Yes
Precincts	253	253	253
Adjusted R ²	0.519	0.800	0.876

Note:

*p<0.1; **p<0.05; ***p<0.01

Table shows coefficient estimates for the change in support for Mayor Walsh (for treatment relative to control) from 2013 to 2017 for Boston electoral precincts. Treatment is defined as precincts within 1 mile of the relocation sites. Covariates are calculated at the precinct level from the voterfile data. Standard errors clustered at the precinct level are shown in parentheses.

Fig. 8: Model 3: Swing to Walsh in Nearby Precincts



Bar plots represent the change in Walsh vote share from 2013 to 2017 for Boston electoral precincts within 1 mile of the relocation sites (bottom) and beyond 1 mile (top). Error bars are 95% confidence intervals.

5 Discussion

5.1 Neighborhood Disorder & Local Political Participation

These results indicate that a shock to neighborhood disorder leads to a substantively large increase in political participation. Residents living in neighborhoods proximate to the relocation sites responded to increased neighborhood disorder by voting at a higher rate in the subsequent Mayoral election than voters living further from the sites (Hypothesis 1).¹⁵ Moreover, the effect sizes were largest for voters closest to the sources of neighborhood disorder (Hypothesis 3), consistent with geographic proximity capturing a change in everyday life that modulates the political response.

While our quantitative analyses illustrate the impact on electoral politics, the political response to the Long Island Bridge closure was not limited to voting or requesting government services. Exposure to the affected areas has led residents and others to protest (Irons, 2017; Maniscalco, 2017), attend neighborhood meetings (Daniel, 2016), oppose the construction of new social services (CSNA, 2016; Seth, 2018b), and submit letters in support of rebuilding the Long Island Bridge (Seth, 2018a), as detailed in Appendix Section 11.¹⁶ This multifaceted response shows how neighborhood disorder galvanizes voters to participate in politics on both intensive margins (more intense participation) and extensive margins (more people participating).

Another important finding is that increased neighborhood disorder influences participation and vote choice only in local elections. Proximity to the relocation sites did not structure turnout or vote choice in national and statewide elections. Thus, voters responded to local events by participating in local politics and holding local politicians accountable. This contrasts with voter “attribution errors” posited in other contexts (Sances, 2017). Instead of mechanically punishing incumbents across levels of government, it appears voters identified the proper scale at which to direct action (Larsen, 2018). The power to curb neighborhood disorder in these Boston neighborhoods lies with the municipal government, which is responsible for local policing, and operating public homeless shelters and drug treatment services.

5.2 When do Negative Shocks Benefit the Incumbent?

Why did voters closer to the re-location sites choose to reward Mayor Walsh? And how do these pro-incumbent results cohere with the increases in political participation? The Long Island Bridge closure and the Mayor’s response to the opioid crisis loomed over the 2017 Mayoral election (Freyer, 2017). We argue that the Long Island Shock raised the salience of addiction and homelessness for voters near the relocation sites, advantaging candidates like Walsh who were perceived as qualified on these issues. Negative shocks provide insight into incumbent quality (Ashworth et al., 2018). In the natural disaster literature, an effective incumbent response has been shown to produce electoral benefits (Healy and Malhotra, 2009) in part by mobilizing supporters to turnout to vote (Chen, 2013). Walsh appears to have managed the Long Island Shock adequately, replacing treatment beds and responding to local complaints with additional police patrols, needle pickups (City of Boston, 2015), street outreach to drug users, beautification efforts (Kaeslin, 2017), and providing an air-conditioned day-center to keep “some of the chaos off the street” (Rasmussen and Alulema, 2018). The neighborhoods’ problems made the Mayor a frequent presence at local events, where he repeatedly promised to clean-up the area (Marcelo, 2017). Walsh also added treatment referral services to the city’s 311 hotline, boosted appropriations for addiction treatment, expanded housing services to address chronic homelessness, and formed the only municipal Office of Recovery Services in the US (Becker, 2016; Marcelo, 2017). Anecdotal evidence reinforces the view that voters rewarded Walsh for effectively managing the crisis: The head of a drug rehabilitation organization said the Mayor “has my full support and endorsement... the fact that we

¹⁵ In Section 8 of the Appendix, we estimate heterogeneous treatment effects by race (Figures A6 and A7) and placebo tests (Figures A8-A10) which explore racial threat as a channel through which these effects took place. We find some evidence consistent with a larger treatment effect among whites, though treatment effects were present among all racial groups. Placebo tests of racially similar neighborhoods as those around the relocation sites do not exhibit the turnout increase we have shown. In Appendix Section 9, we present results from a robustness check (Figures A17 and A18) using randomization inference (comparing the observed result with thousands of hypothetical alternative relocation sites).

¹⁶ In Appendix Section 8, we interact treatment with homeownership status and do not find larger impacts on homeownership (Figure A18).

[replaced the beds lost on Long Island] in 2 and a half years was nothing short of a miracle” ([Freyer, 2017](#)).¹⁷

Moreover, Walsh benefited from prior credibility on addiction and homelessness ([Becker, 2017](#)). Walsh is a recovered alcoholic and actually received treatment on Long Island. He has spoken about recovery from addiction in his political campaigns, and has been described as having “more credibility and legitimacy with the recovery community than any elected leader” ([Solow, 2015](#)). This credibility buffered Walsh against criticism. In contrast, the City Councillor who challenged Walsh previously worked as a pharmaceutical sales representative marketing an opioid drug for a pharmaceutical company ([O’Sullivan, 2017](#)). Voters likely saw Walsh as better equipped to address drug problems. This logic comports with partisan issue ownership and issue salience ([Bélanger and Meguid, 2008](#)), though at play here is candidate issue ownership.¹⁸

5.3 Limitations

There are several limitations to this study. First, it is difficult to quantify the increase in disorder to make comparisons to other cases. Second, the effect of increased disorder likely varies across neighborhoods and our study is limited in its ability to speak to the political impacts in other neighborhoods. This case involves an increase in neighborhood disorder in an area with already high concentrations of homeless and drug-treatment services. Consequently, residents may have been desensitized somewhat by previous exposure to disorder. On the other hand, the apparent distributive injustice – residents noted that they were forced to shoulder an even greater burden of the region’s social services – may have been particularly mobilizing. A different political response may occur in cases where disorder increases in neighborhoods with low levels of disorder. Lastly, it is difficult to disentangle the political effects of a change in disorder from the government (and incumbent’s) response to disorder.¹⁹ It seems implausible, for example, that incumbent vote share would have increased near the relocation sites absent substantial efforts by the Mayor’s office to mitigate disorder. We hope future research advances our understanding of disorder by addressing these limitations.

6 Conclusion

This study shows that increased neighborhood disorder causes increased participation across a range of political behaviors. Voters living near the re-location sites were more likely to request government services, vote in the next mayoral election, and more likely to vote for the incumbent Mayor. But the level of government mattered: We did not find similar effects in national and state elections. The response was limited to the relevant, local scale. This contrasts with “attribution errors” posited in other contexts, motivating inquiry into the mechanisms by which neighborhood change provokes political action.

Closure of the Long Island Bridge – and consequent relocation of homeless individuals, people with addiction, and social services – provides insight into the political consequences of increased neighborhood disorder. Boston is one of many cities dealing with highly concentrated – and politicized – homelessness and drug use ([Kim, 2017](#)), and thus these results speak to a broader phenomenon of growing disorder in the shadow of addiction. Growing problems with homelessness have prompted declarations of “homelessness states of emergency” in major cities like Los Angeles, Portland, Sacramento, San Diego, and Seattle, along with the state of Hawaii and dozens of other municipal and county governments. Our results suggest that exposure to these crises will increase participation in local politics, but the electoral effect will depend on the quality of incumbent response.

Our discussion has focused on the political response of stably-housed residents near the re-location sites. But it seems appropriate to give the final words to the Long Island Refugees, the already-vulnerable people disrupted by the termination of the island’s facilities. We hope our research invites scholarship into the geographic factors that structure the services available for vulnerable people, such as whether treatment services concentrate or diffuse vulnerable people. This has clear policy relevance. Federal law requires patients treated with methadone—the gold standard therapy for opioid addiction—make

¹⁷ It is also possible that the pro-incumbent effect was caused by increased Walsh campaign activity in 2017. We have found no clear evidence that the Walsh campaign devoted greater resources to voter contact or campaign events in the treatment areas, but it is difficult to rule out that this could have contributed to the effects.

¹⁸ It is possible that other newly-salient issues in this area (e.g. crime) also favored Walsh.

¹⁹ The political effects of other kinds of shocks (such as natural disasters) similarly depend on the government’s response ([Healy and Malhotra, 2009, 2010; Chen, 2013](#)).

frequent (typically daily) visits to a specialized clinic, a unique burden that both clusters vulnerable people (potentially provoking NIMBY opposition) and limits access (Samet et al., 2018; de Benedictis-Kessner and Hankinson, 2019). Despite these barriers, efforts to site new services can be successful (Lupick, 2018), and future research could assess the conditions under which they succeed. The continued suffering on “Methadone Mile” and beyond adds urgency to such work.

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Conflict of interest

The authors declare that they have no conflict of interest.