Resisting Broken Windows: The Effect of Neighborhood Disorder on Political Behavior – Online Appendix

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1 Context

TIMELINE 1: *Key events in the Long Island Shock*

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. Police block entry to island.
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.
January 1, 2018	Mayor Walsh promises to re-build Long Island Bridge in Inauguration Speech.
September 27, 2018	State environmental board approves permit for bridge reconstruction

TIMELINE 2: *Political Behaviors Beyond Voting*

2014-	Local neighborhood associations form crime watch and safety groups.
2014-	NIMBY-opposition to medical marijuana dispensary and liquor store permits.
2016	South End Forum requests moratorium on new social services within one mile of BMC.
July 2017	Members of South End organizations ambush Walsh at playground renovation.
Nov. 2017	Probation officer who worked on Long Island wins local city council race.
2018	Over 900 South End residents write in support of rebuilding bridge.
Sept. 2018	Local doctor running on opioid crisis in neighborhood unseats Assistant Majority Leader.

Table A1: Recent Local Elections where Homeless or Opioids were Defining Issues

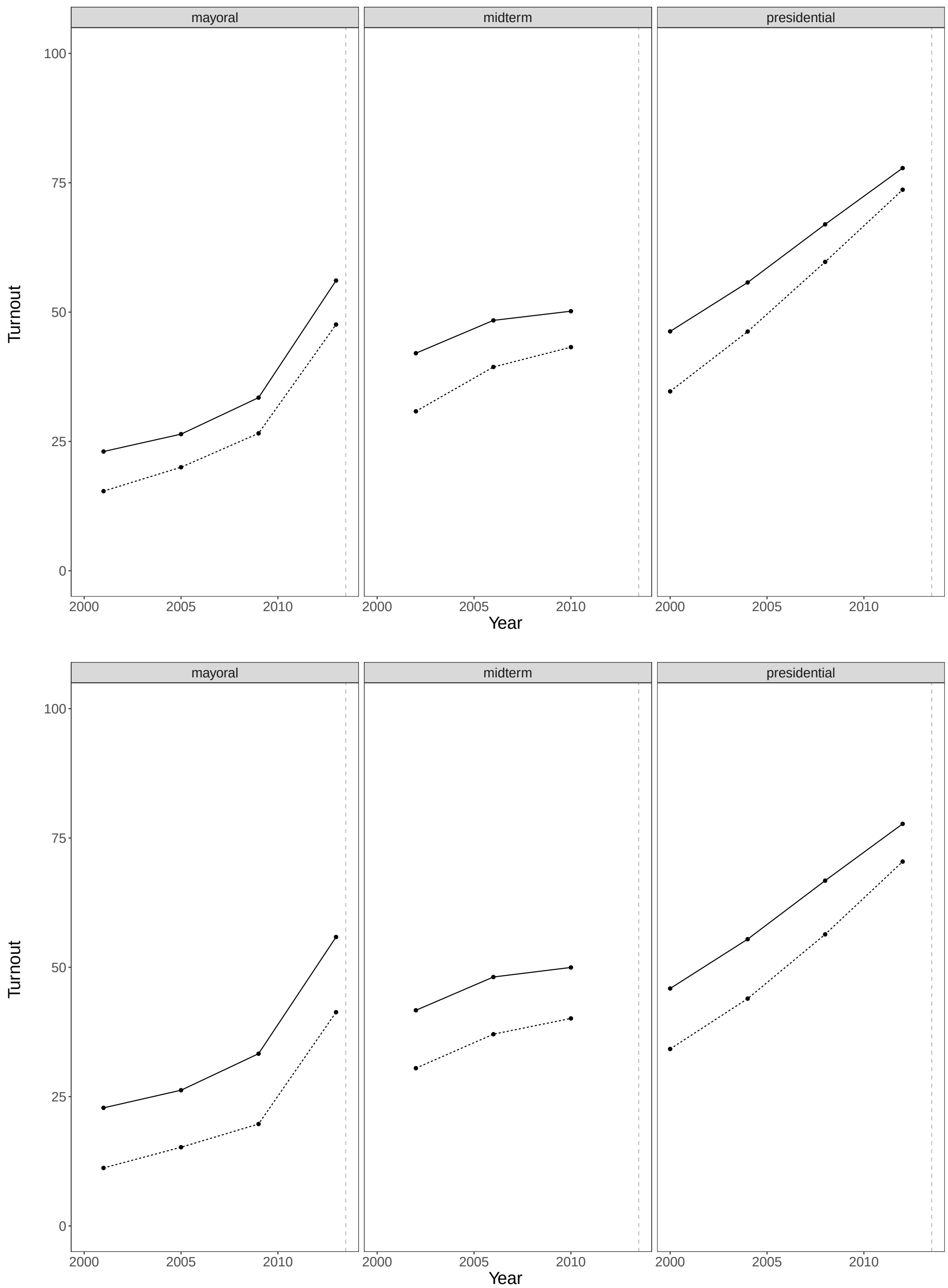
	City	Election	Source
1	Alameda, CA	2019 Referendum	CBS Local: San Francisco
2	Astoria, OR	2018 Mayoral	Daily Astorian
3	Boston, MA	2017 Mayoral	WBUR
4	Braidwood IL	2019 Mayoral	Herald News
5	Chicago, IL	2019 Mayoral	Chicago Sun Times
6	Chicago, IL	2019 Referendum	Chicago Sun Times
7	Citrus Heights, CA	2016 City Council	Citrus Heights Sentinel
8	Colorado Springs, CO	2019 Mayoral	KOAA News
9	Colorado Springs, CO	2019 City Council	KOAA News
10	Corvallis, OR	2018 Mayoral	Corvallis Gazette Times
11	Costa Mesa, CA	2018 Mayoral	Los Angeles Times
12	Dallas, TX	2019 Mayoral	KERA News
13	Denver, CO	2019 Mayoral	KDVR, Colorado Politics, Cudenvert Today
14	Denver, CO	2019 Referendum	Ballotpedia
15	Fairfield, CA	2016 City Council	Daily Republic
16	Fresno, CA	2020 Mayoral	Fresno Bee
17	Fresno County, CA	2019 Board of Supervisors	ABC News
18	La Plata County, CO	2018 Sheriff Election	Durango Herald
19	Las Vegas, NV	2019 City Council	Review Journal
20	Los Angeles, CA	2016 Referendum	Los Angeles HCID
21	Los Angeles, CA	2019 City Council	Los Angeles Times
22	Los Angeles County, CA	2017 Referendum	Ballotpedia
23	Manchester, NH	2017 Mayoral	WMUR
24	Marion County, OR	2018 Board of Commissioners	Statesman Journal
25	Merced, CA	2018 City Council	Merced Sun Star
26	Mountainview, CA	2018 Referendum	Ballotpedia
27	Nashville, TN	2019 Mayoral	Nashville Scene
28	Newark, DE	2019 Mayoral	Newark Advocate
29	Oakland, CA	2018 Mayoral	Post News Group
30	Oakland, CA	2018 Referendum	Ballotpedia
31	Phoenix, AZ	2019 Mayoral	Tucson Sentinel
32	Pomona, CA	2017 City Council	Los Angeles Times
33	Pomona, CA	2018 Referendum	Ballotpedia
34	Portland, OR	2017 Neighborhood Association	OPB
35	Portland, ME	2015 Mayoral	Bangor Daily News
36	Portland, OR	2016 Mayoral	Oregon Live
37	Richmond, CA	2018 Referendum	East Bay Times
38	Sacramento, CA	2016 Mayoral	KCRA
39	Sacramento, CA	2020 Mayoral	Sacramento Bee
40	San Diego, CA	2018 City Council	San Deigo Union Tribune
41	San Diego, CA	2020 Referendum	Fox 5 San Diego
42	San Diego, CA	2018 Referendum	Voice of San Diego
43	San Diego, CA	2018 City Council	KPBS
44	San Francisco, CA	2018 Mayoral	New York Times
45	San Francisco, CA	2018 Board of Supervisors	KPBS
46	San Francisco, CA	2018 Referenda	Ballotpedia, Ballotpedia
47	San Francisco, CA	2016 Referendum	Ballotpedia
48	Santa Monica, CA	2018 City Council	Surf Santa Monica
49	Santa Rosa, CA	2018 City Council	Press Democrat
50	Seattle, WA	2017 Mayoral	King 5 News
51	South Bend, IN	2019 Mayoral	South Bend Tribune
52	Spokane, OR	2019 Mayoral	Inlander
53	Springfield, IL	2019 Mayoral	State Journal-Register
54	West Palm Beach, FL	2019 Mayoral	WFLX

2 Parallel Trends

Causal inference in the difference-in-differences setup hinges on the parallel trends assumption that changes in political behavior between residents located near the relocations, and residents of matched gender, income, age, homeownership, and race who live further away, would have been similar had the Long Island shock not occurred. Figure A1 shows the parallel trends plots comparing turnout between treatment[1] and control across mayoral, midterm, and national elections from 2000 through 2016. The plots show that changes in voter behavior were generally parallel across election types prior to the Long Island Bridge closing. Note that we are limited in our analysis of parallel trends because the voterfile data we use in our main analysis does not have voting history in local elections before 2013. Thus, we supplement our data with a voterfile for the city of Boston dated April 2014 with voting history going back to 2001, and construct the treated and control samples for the pre-2013 dates in the local election plot off of that data. Turnout rates among treatment and control are higher than general turnout rates because we limit our analysis to voters registered to vote at the same address in both comparison elections, so we are conditioning on past registration. In practice, this means that the sample for mayoral turnout consists of voters registered to vote in the same location in 2013 and 2017, the sample for midterm turnout is voters registered to vote in the same location in 2010 and 2014, and the sample for presidential turnout is voters registered to vote at the same location in 2012 and 2016. These different sample definitions account for differences in turnout across election type, and when compared to turnout in the general Boston electorate.

[1] Figure A1 shows the pre-trends in turnout for treatment defined as 500 meters and 1000 meters. The plots at other definitions of treatment are substantively similar.

Figure A1: Pre-Trend Plots by Election Type



Note: Points show the average turnout within treatment and control of voters old enough to vote in the election. Upper panel is treatment within 1000 meters and lower panel is within 500 meters.

3 Long Island Shock Supplementary Figures/Table

Figure A2: Neighborhood Change: Bus stop benches & Fences (Google Street View)

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

After Long Island Shock (2017)

Mass Ave & Melnea Cass, 2009 (Before Long Island Shock)

After Long Island Shock, 2017 (Fence constructed)

Figure A3: Raw Data: Weekly 311 Homeless Type Calls and 911 Calls following Long Island Closure

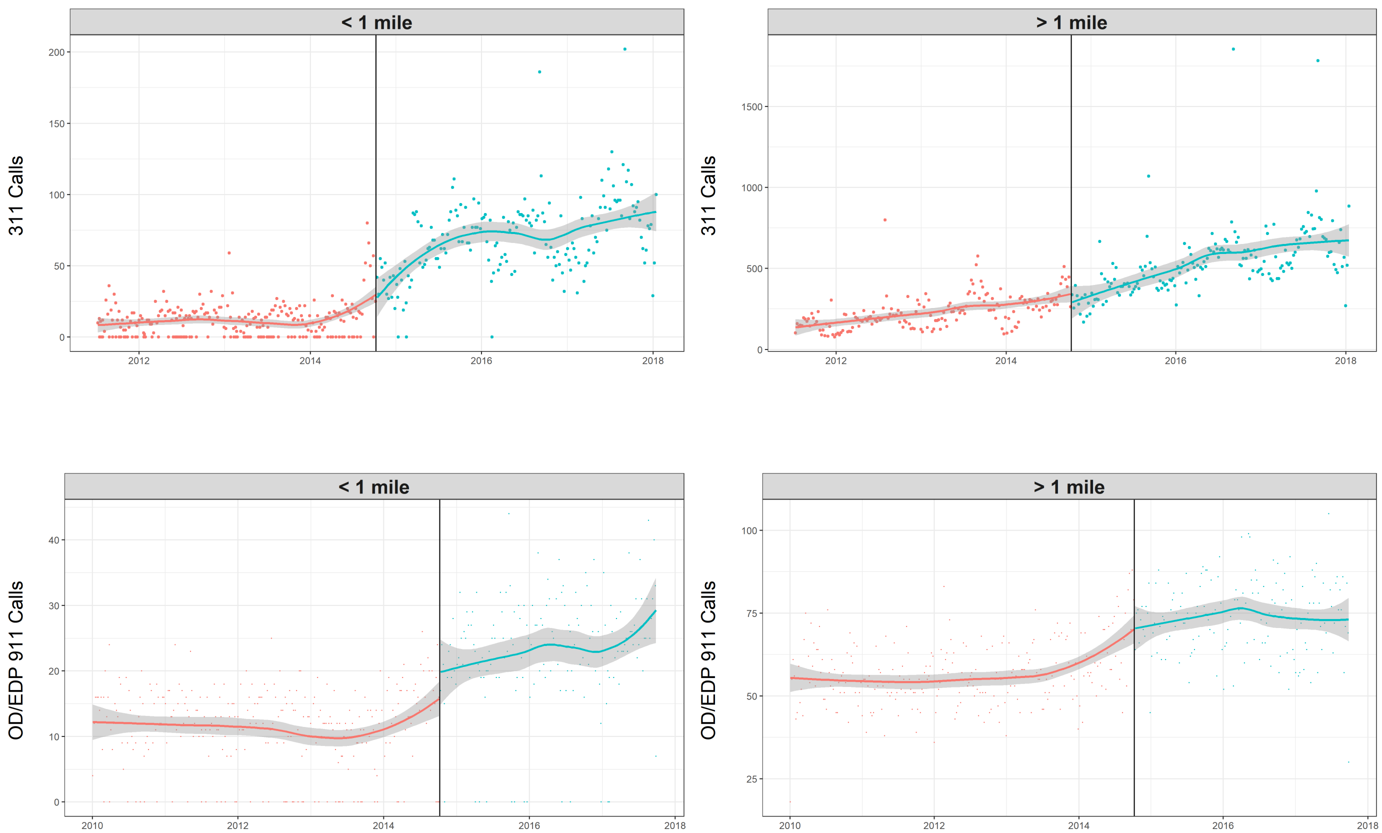


Table A2: Negative Binomial Regression of 311 & 911 call frequency

	Dependent variable:	
	Weekly Call Count	
	OD/EDP 911	All 311
$<$ 1 Mile, pre-shock	-1.603^{***}	-2.980^{***}
	(0.030)	(0.055)
Post-Shock	0.147^{***}	0.256^{***}
	(0.054)	(0.087)
Post-Shock $\cdot <$ 1 mile	0.440^{***}	0.955^{***}
	(0.047)	(0.077)
Linear	1.780^{***}	10.543^{***}
	(0.690)	(1.102)
Quadratic	0.623	-0.481
	(0.381)	(0.549)
Observations	869	800
Note:	*p$<$0.1; **p$<$0.05; ***p$<$0.01	

Month fixed effects omitted.

4 Turnout Result Tables

Mayoral, 2017 vs 2013

Table A3: Turnout Effects 2017 v 2013: Raw Association

	m300	m400	m500	m600	m700	m800	m900	m1000	m1100	m1200	m1300	m1400	m1500	m1600
$\widehat{ATE}$	0.0777	0.0901	0.0669	0.0536	0.0403	0.0436	0.0441	0.0474	0.0456	0.0499	0.0489	0.0485	0.0481	0.0483
SE	0.0461	0.0226	0.0192	0.0148	0.0125	0.0107	0.0091	0.0079	0.0071	0.0065	0.0059	0.0054	0.0050	0.0048
p	0.0916	0.0001	0.0005	0.0003	0.0013	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
$3 \cdot p$	0.2749	0.0002	0.0015	0.0009	0.0040	0.0001	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
N_t	185	775	1227	2199	3195	4267	5970	8189	10484	13009	15728	18975	22558	25300

Table A4: Turnout Effects 2017 v 2013: Matching/Conditioning on Voter File covariates

	m300	m400	m500	m600	m700	m800	m900	m1000	m1100	m1200	m1300	m1400	m1500	m1600
$\widehat{ATE}$	0.0518	0.0718	0.0482	0.0393	0.0263	0.0277	0.0291	0.0329	0.0337	0.0383	0.0376	0.0361	0.0365	0.0356
SE	0.0481	0.0233	0.0197	0.0152	0.0128	0.0110	0.0095	0.0083	0.0075	0.0068	0.0062	0.0057	0.0053	0.0051
p	0.2821	0.0020	0.0142	0.0096	0.0402	0.0120	0.0021	0.0001	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
$3 \cdot p$	0.8464	0.0061	0.0425	0.0287	0.1206	0.0361	0.0064	0.0002	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
N_t	184	768	1219	2185	3179	4245	5930	8127	10413	12922	15615	18827	22366	25060

Table A5: Turnout Effects 2017 v 2013: Voter File covariates & Council District Fixed Effects, and West Roxbury dummy

	m300	m400	m500	m600	m700	m800	m900	m1000	m1100	m1200	m1300	m1400	m1500	m1600
$\widehat{ATE}$	0.0344	0.0770	0.0598	0.0414	0.0220	0.0184	0.0176	0.0197	0.0197	0.0269	0.0266	0.0249	0.0279	0.0280
SE	0.0577	0.0260	0.0218	0.0162	0.0135	0.0118	0.0105	0.0096	0.0087	0.0081	0.0075	0.0071	0.0069	0.0067
p	0.5509	0.0031	0.0060	0.0108	0.1042	0.1182	0.0947	0.0395	0.0240	0.0009	0.0004	0.0004	0.0000	0.0000
$3 \cdot p$	1.6528	0.0094	0.0181	0.0323	0.3126	0.3545	0.2841	0.1185	0.0719	0.0028	0.0011	0.0013	0.0001	0.0001
N_t	184	768	1219	2185	3179	4245	5930	8127	10413	12922	15615	18827	22366	25060

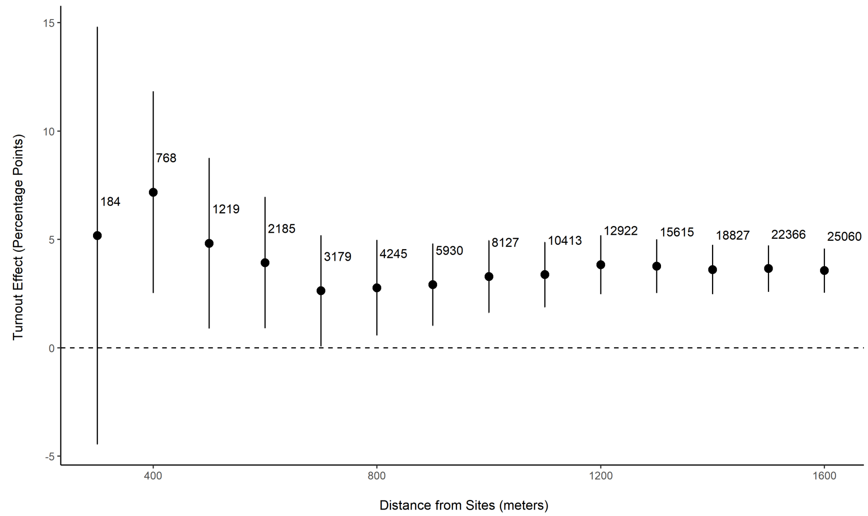


Figure A4: 2017 vs 2013 change in turnout, matching on voter file covariates

Table A6: Turnout Effects 2017 v 2013: Raw Association with bootstrapped standard errors

	m300	m400	m500	m600	m700	m800	m900	m1000	m1100	m1200	m1300	m1400	m1500	m1600
$\widehat{ATE}$	0.0777	0.0901	0.0669	0.0536	0.0403	0.0436	0.0441	0.0474	0.0456	0.0499	0.0489	0.0489	0.0481	0.04834
SE	0.03277	0.01668	0.01399	0.01020	0.00868	0.00763	0.00671	0.00565	0.00493	0.00465	0.00416	0.00377	0.00348	0.00346
z	2.37030	5.39989	4.78090	5.25332	4.64232	5.71550	6.57994	8.39170	9.25445	10.72609	11.75367	12.86617	13.82411	13.97515
p	0.01777	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000

State/Midterm, 2014 vs 2010

Table A7: Turnout Effects 2014 v 2010: Raw Association

	m300	m400	m500	m600	m700	m800	m900	m1000	m1100	m1200	m1300	m1400	m1500	m1600
$\widehat{ATE}$	0.0132	-0.0024	0.0036	-0.0057	-0.0022	-0.0070	-0.0106	-0.0056	0.0007	-0.0035	-0.0011	0.0015	-0.0006	-0.0026
SE	0.0247	0.0191	0.0156	0.0130	0.0112	0.0099	0.0089	0.0079	0.0072	0.0067	0.0062	0.0058	0.0055	0.0052
p	0.5937	0.8978	0.8167	0.6593	0.8444	0.4764	0.2345	0.4776	0.9228	0.6016	0.8612	0.7942	0.9141	0.6174
$3 \cdot p$	1.7810	2.6934	2.4500	1.9778	2.5333	1.4292	0.7036	1.4329	2.7683	1.8047	2.5835	2.3825	2.7423	1.8522
N_t	656	1187	1873	2720	3903	5229	6559	8244	9881	11424	13548	15433	17710	20115

Table A8: Turnout Effects 2014 v 2010: Matching/Conditioning on Voter File covariates

	m300	m400	m500	m600	m700	m800	m900	m1000	m1100	m1200	m1300	m1400	m1500	m1600
$\widehat{ATE}$	0.0117	-0.0029	0.0070	-0.0046	-0.0015	-0.0068	-0.0102	-0.0058	0.0011	-0.0028	0.0006	0.0027	0.0007	-0.0001
SE	0.0259	0.0198	0.0161	0.0135	0.0117	0.0104	0.0094	0.0085	0.0077	0.0071	0.0066	0.0063	0.0059	0.0056
p	0.6523	0.8834	0.6659	0.7349	0.8991	0.5146	0.2789	0.4929	0.8846	0.6903	0.9219	0.6683	0.9060	0.9892
$3 \cdot p$	1.9569	2.6501	1.9977	2.2048	2.6974	1.5439	0.8366	1.4787	2.6538	2.0710	2.7658	2.0048	2.7181	2.9676
N_t	654	1179	1859	2700	3871	5180	6500	8175	9791	11314	13411	15274	17511	19884

Presidential, 2016 vs 2012

Table A9: Turnout Effects 2016 v 2012: Raw Association

	m300	m400	m500	m600	m700	m800	m900	m1000	m1100	m1200	m1300	m1400	m1500	m1600
$\widehat{ATE}$	-0.0692	-0.0198	-0.0125	-0.0109	-0.0085	-0.0042	-0.0037	0.0092	0.0161	0.0139	0.0156	0.0108	0.0149	0.0147
SE	0.0518	0.0224	0.0176	0.0133	0.0112	0.0098	0.0084	0.0071	0.0062	0.0056	0.0051	0.0047	0.0044	0.0042
p	0.1820	0.3775	0.4777	0.4156	0.4509	0.6701	0.6586	0.1964	0.0098	0.0130	0.0022	0.0215	0.0006	0.0004
$3 \cdot p$	0.5459	1.1325	1.4332	1.2468	1.3526	2.0102	1.9757	0.5892	0.0293	0.0390	0.0066	0.0646	0.0019	0.0012
N_t	198	830	1326	2330	3402	4553	6382	8766	11271	13982	16934	20467	24308	27261

Table A10: Turnout Effects 2016 v 2012: Matching/Conditioning on Voter File covariates

	m300	m400	m500	m600	m700	m800	m900	m1000	m1100	m1200	m1300	m1400	m1500	m1600
$\widehat{ATE}$	-0.0402	-0.0232	-0.0177	-0.0147	-0.0134	-0.0095	-0.0098	0.0020	0.0084	0.0075	0.0090	0.0063	0.0118	0.0122
SE	0.0554	0.0232	0.0181	0.0137	0.0115	0.0101	0.0087	0.0075	0.0065	0.0059	0.0054	0.0050	0.0047	0.0045
p	0.4673	0.3166	0.3263	0.2840	0.2450	0.3471	0.2567	0.7921	0.1985	0.2049	0.0951	0.2066	0.0111	0.0067
$3 \cdot p$	1.4018	0.9499	0.9789	0.8521	0.7351	1.0413	0.7701	2.3764	0.5956	0.6146	0.2853	0.6198	0.0332	0.0201
N_t	197	823	1317	2315	3385	4528	6337	8700	11197	13891	16809	20310	24106	27018

5 Continuous Turnout

Table A11: State Election Turnout Change as function of log distance

	Dependent variable:	
	Midterm/State Election Turnout, 2014 v 2010	
	(1)	(2)
2014	-0.071^{***}	0.031
	(0.019)	(0.144)
2014 · Distance	0.005^{**}	0.002
	(0.002)	(0.002)
Covariates	N	Y
Observations	368,870	368,870
R^2	0.755	0.757
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A12: Presidential Turnout Change as function of log distance

	Dependent variable:	
	Presidential Turnout, 2016 v 2012	
	(1)	(2)
2016	-0.124^{***}	0.672^{***}
	(0.016)	(0.233)
2016 · log(Distance)	0.007^{***}	0.001
	(0.002)	(0.002)
Covariates	N	Y
Observations	466,642	466,642
R^2	0.740	0.745
Note:		*p<0.1; **p<0.05; ***p<0.01

6 Vote Share

Table A13: Change in Democratic Presidential Voteshare with distance (1 mile)

	Dependent variable:		
	Democratic Presidential Swing, 2016 vs 2012		
	(1)	(2)	(3)
$<$ 1 Mile	0.176	-0.327	-0.376
	(0.834)	(0.604)	(0.603)
Black %		-0.055^{***}	-0.053^{***}
		(0.011)	(0.011)
Hispanic %		-0.094^{***}	-0.086^{***}
		(0.019)	(0.019)
Mean Age		-0.071	-0.101^{**}
		(0.046)	(0.049)
Low Income %		0.011	0.010
		(0.041)	(0.041)
High Income %		0.769^{***}	0.803^{***}
		(0.087)	(0.089)
West Roxbury Dummy			1.405
			(0.879)
Constant	2.107^{***}	5.476^{***}	6.611^{***}
	(0.673)	(2.082)	(2.194)
Council FE	Yes	Yes	Yes
Observations	253	253	253
R^2	0.459	0.741	0.744
Adjusted R^2	0.439	0.726	0.728
Residual Std. Error	3.363 (df = 243)	2.351 (df = 238)	2.344 (df = 237)
F Statistic	22.923^{***} (df = 9; 243)	48.671^{***} (df = 14; 238)	45.893^{***} (df = 15; 237)
Note:			*p<0.1; **p<0.05; ***p<0.01

7 Vote Share Specifications: Departure from Pre-Registration

In our pre-registration, we said that we would estimate effects on vote share by modeling changes in incumbent vote share as a continuous function of distance from the source of neighborhood disorder. Our original pre-registered specification, as shown in Figure A15, shows that incumbent vote share decreased close to the sites relative to further from the sites between 2013 and 2017. These results would have validated our hypothesis that increased participation as a result of neighborhood disorder would coincide with anti-incumbent voting. However, upon further investigation, the specification that we pre-registered is obscuring the true effect. First, we conducted robustness checks where we dropped the most distant precincts, to see if a small number of precincts very far from the sites were driving the results. When we drop precincts further than 5 kilometers away (Figure A16) we see that the effects reverse, now in agreement with the pro-incumbent swing we saw in our main specification in the paper. Next, we further include council district fixed effects to this analysis, which we should do to best account for parallel trends between 2013 and 2017, as different city council districts had people running against opponents in 2013 compared to 2017. Here (Figure A17) we similarly see a pro-incumbent effect. We next estimated the models shown in the main body of the paper, operationalizing treatment as within certain distances from the sites, in keeping with our main turnout specifications – finding again strong evidence of pro-incumbent effect. Given this wealth of evidence, we felt compelled to go off of our pre-registration despite the fact that the original results were more in line with our expectations. The model we originally intended to estimate was not the best methodological approach, and superior specifications show a robust pro-incumbent effect.

Table A14: One-Mile Treatment with Council Fixed Effects

	Dependent variable:		
	Walsh Swing		
	(1)	(2)	(3)
< 1 Mile	8.698**	6.771***	5.587***
	(3.558)	(2.322)	(1.864)
White %		−0.057	−0.150*
		(0.106)	(0.086)
Hispanic %		−0.456***	−0.361***
		(0.116)	(0.094)
Mean Age		1.414***	0.724***
		(0.176)	(0.153)
Low Income %		0.112	0.049
		(0.164)	(0.131)
High Income %		1.888***	2.683***
		(0.334)	(0.277)
Black %		−0.366***	−0.400***
		(0.093)	(0.075)
Wext Roxbury Dummy			31.428***
			(2.728)
Council FE	Yes	Yes	Yes
Observations	253	253	253
R^2	0.519	0.814	0.881
Adjusted R^2	0.501	0.803	0.873
Residual Std. Error	14.349 (df = 243)	9.027 (df = 237)	7.237 (df = 236)
F Statistic	29.120*** (df = 9; 243)	69.275*** (df = 15; 237)	109.349*** (df = 16; 236)

Note: *p<0.1; **p<0.05; ***p<0.01

Table A15: Original Pre-Registered Specification

	Dependent variable:		
	Walsh Swing		
	(1)	(2)	(3)
Distance (log)	3.976**	4.359***	2.386*
	(1.885)	(1.244)	(1.355)
White %		0.002	−0.0005
		(0.118)	(0.115)
Hispanic %		−0.213*	−0.150
		(0.121)	(0.118)
Mean Age		0.494***	0.156
		(0.188)	(0.204)
Low Income %		0.233	0.284
		(0.199)	(0.195)
High Income %		3.550***	3.754***
		(0.388)	(0.382)
Black %		−0.386***	−0.332***
		(0.096)	(0.095)
Tito Jackson's District			−6.371**
			(2.778)
Wext Roxbury Dummy			13.747***
			(3.460)
Constant	5.196*	−12.893	2.159
	(2.845)	(11.049)	(11.511)
Observations	253	253	253
R^2	0.017	0.704	0.725
Adjusted R^2	0.014	0.695	0.715
Residual Std. Error	20.176 (df = 251)	11.214 (df = 245)	10.847 (df = 243)
F Statistic	4.449** (df = 1; 251)	83.128*** (df = 7; 245)	71.197*** (df = 9; 243)
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A16: Vote Share Analyses omitting >5KM Precincts

	Dependent variable:		
	Walsh_Swing		
	(1)	(2)	(3)
Distance (log)	−3.664	−4.219**	−7.105***
	(3.587)	(1.876)	(2.016)
White %		−0.140	−0.085
		(0.142)	(0.138)
Hispanic %		−0.516***	−0.537***
		(0.170)	(0.164)
Mean Age		−0.225	−0.229
		(0.284)	(0.274)
Low Income %		0.228	0.412*
		(0.224)	(0.223)
High Income %		4.336***	4.353***
		(0.458)	(0.442)
Black %		−0.401***	−0.264**
		(0.117)	(0.120)
Tito Jackson's District			−10.104***
			(3.100)
Constant	10.598***	34.217**	32.731**
	(3.699)	(16.623)	(16.055)
Observations	140	140	140
R^2	0.008	0.774	0.791
Adjusted R^2	0.0003	0.762	0.778
Residual Std. Error	22.711 (df = 138)	11.081 (df = 132)	10.698 (df = 131)
F Statistic	1.044 (df = 1; 138)	64.582*** (df = 7; 132)	61.958*** (df = 8; 131)
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A17: Walsh Swing omitting precincts >5KM & including council fixed effects

	Dependent variable:	
	Walsh Swing	
	(1)	(2)
Distance (log)	-6.853^{**}	-5.695^{***}
	(2.892)	(1.755)
White %		-0.239^{**}
		(0.111)
Hispanic %		-0.414^{***}
		(0.133)
Mean Age %		0.439^{*}
		(0.224)
Low Income %		0.054
		(0.167)
High Income %		3.201^{***}
		(0.355)
Black %		-0.492^{***}
		(0.101)
Council FE	Yes	Yes
Observations	140	140
R^2	0.644	0.899
Adjusted R^2	0.622	0.887
Residual Std. Error	13.961 (df = 131)	7.629 (df = 125)
F Statistic	29.622^{***} (df = 8; 131)	79.101^{***} (df = 14; 125)
Note:		*p<0.1; **p<0.05; ***p<0.01

8 Racial Threat as an Alternative Explanation

Given the potency of racial geography in shaping political behavior (Key 1949, Enos 2017), we ran a series of models to assess how racial threat might impact these analyses. This is a particularly important explanation to consider because the Boston mayoral election in 2017 was notable for having a Black candidate (Tito Jackson) running against the incumbent White Mayor. This is a difficult explanation to rule out definitively, we do not think racial threat is the predominant driver of the observed effects. First, while we do not know with certainty the racial makeup of the displaced population from Long Island, the homeless and drug-using populations in Boston are majority White (although Blacks and Hispanic are over-represented compared to their share in the general population) (of Boston 2008). The 2010 Census population listed the Harbor Islands census tract–which almost exclusively comprised the residents of shelters and treatment centers on Long Island–as 31% White, 38% Black, and 25% Hispanic. Thus, the displaced population was racially mixed (but majority non-white), so the change in exposure to neighborhood disorder is not solely attributable to an influx of people of color. Second, our main specifications exactly match voters on race, comparing changes in turnout among voters near the relocation sites with voters of the same race who lived further away. To further explore how racial context might modulate the impact of the Long Island Shock, we examine results subset by race for White, Black, and Hispanic voters, as the area around the relocation sites is racially heterogeneous (Figure A5. Treatment effects solely among Whites might suggest that racial threat is the primary driver. Importantly, treatment effects across racial groups are consistently positive, indicating that exposure to increased disorder increased turnout across racial groups (Figure A6). Across definitions of treatment by distance, the results for Whites are usually higher than those for Blacks or Hispanics. From 1000-1300 meters, White-Black differences are statistically distinguishable from zero at conventional levels ($\alpha = 0.05$), but the differences are not significant at other distances. Treatment effects for Hispanics are typically statistically significantly smaller than those for Whites, but not statistically different from the effect for Blacks (Table A18).

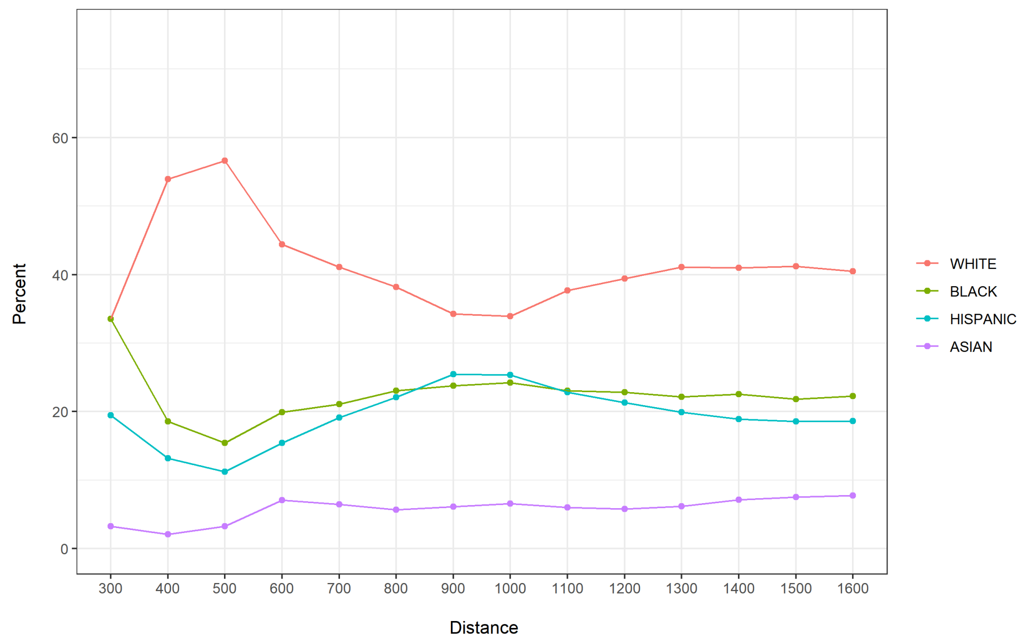


Figure A5: Racial demography of voters near relocation sites

Racial Boundary Placebo Tests

We were also concerned that our measure of proximity to the source of increased neighborhood disorder was correlated with proximity to the South End and Roxbury neighborhood boundary, which represents a racial boundary between the mostly white South End and the predominantly Black and Hispanic Roxbury. There is some evidence the proximity to contested neighborhood boundaries promotes group conflict (Legewie & Schaeffer 2016), and, given the racial context of the 2017 mayoral election, this aspect of social geography might drive greater turnout. To test this hypothesis, we chose coordinate points along similar racial boundaries in the city of Boston: along the Roslindale-Hyde Park boundary, within Roslindale along Washington Street and the Ashmont-Adams Village sub-neighborhood boundaries in southern Dorchester. Each of these boundaries separate abutting neighborhoods with starkly different racial compositions. Figure A7 maps these points along with the block-group level racial composition. Proximity to these boundaries may make racial competition salient in much the same way as proximity to the Roxbury-South End border. We re-ran our main specifications testing for effects on turnout between the 2013 and 2017. For all placebo points, the effects are null across definitions of treatment. Thus, it seems unlikely that proximity to the borders of racially-different neighborhood is confounding the effect of increased neighborhood disorder. Overall, we conclude that racial threat is not likely the sole explanation for the observed changes in political behavior near the relocation sites.

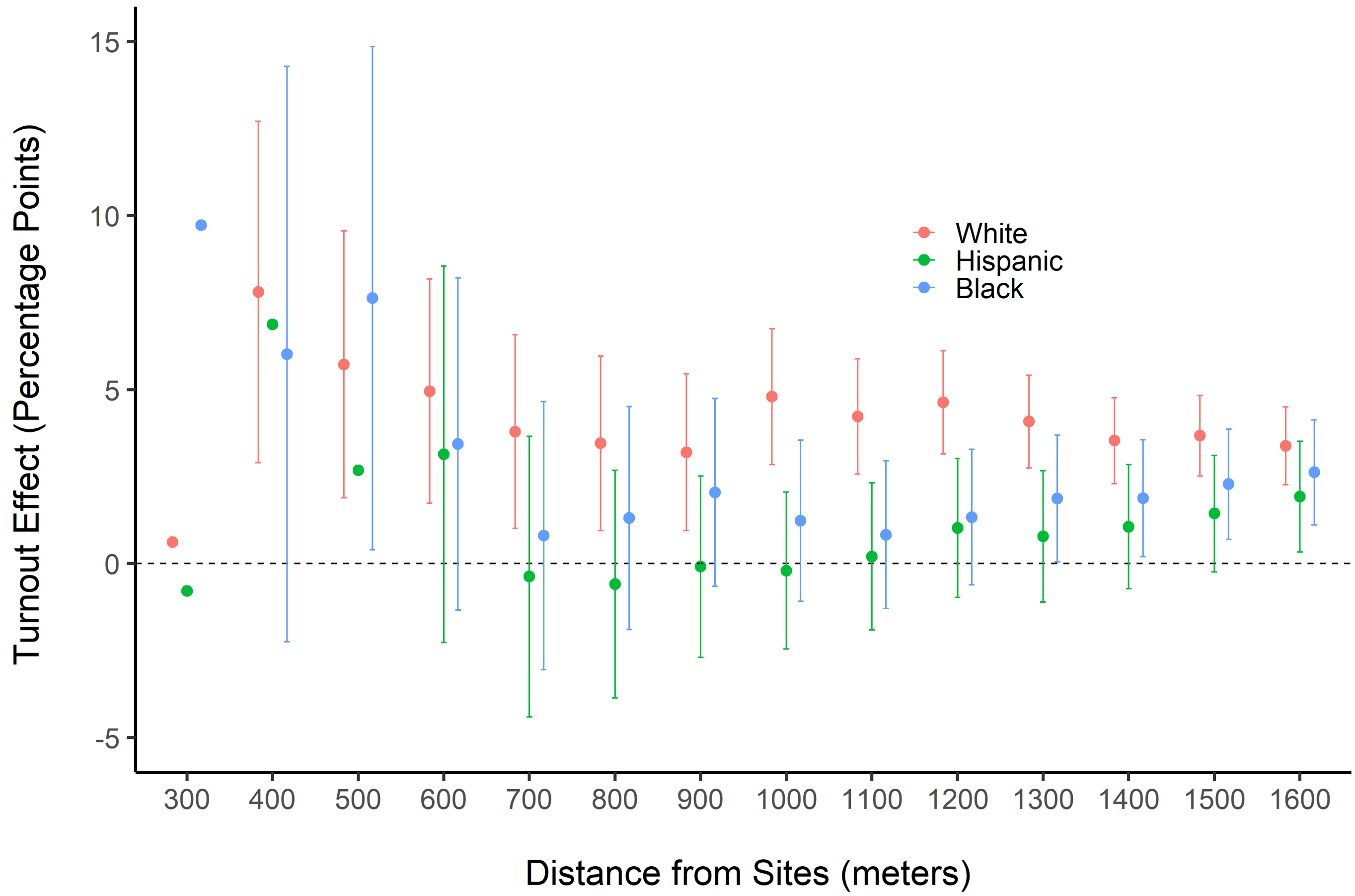


Figure A6: Treatment Effect of Proximity by Race of Voter. Estimates from complete model specification (matching, controls, and council district fixed effects). Where error bars not shown, confidence intervals exceed y-axis limits.

Distance	White-Black Difference	White-Hispanic Difference	Black-Hispanic Difference
300	0.301	0.892	0.310
400	0.715	0.867	0.897
500	0.646	0.520	0.383
600	0.604	0.571	0.935
700	0.215	0.094	0.677
800	0.298	0.052	0.413
900	0.516	0.058	0.258
1000	0.019	0.001	0.375
1100	0.011	0.003	0.675
1200	0.007	0.003	0.821
1300	0.048	0.004	0.400
1400	0.106	0.020	0.493
1500	0.146	0.024	0.450
1600	0.402	0.122	0.511

Table A18: Pairwise Differences in Treatment Effects (two-sided p-values calculated using the *linearHypothesis* function in R's *car* package)

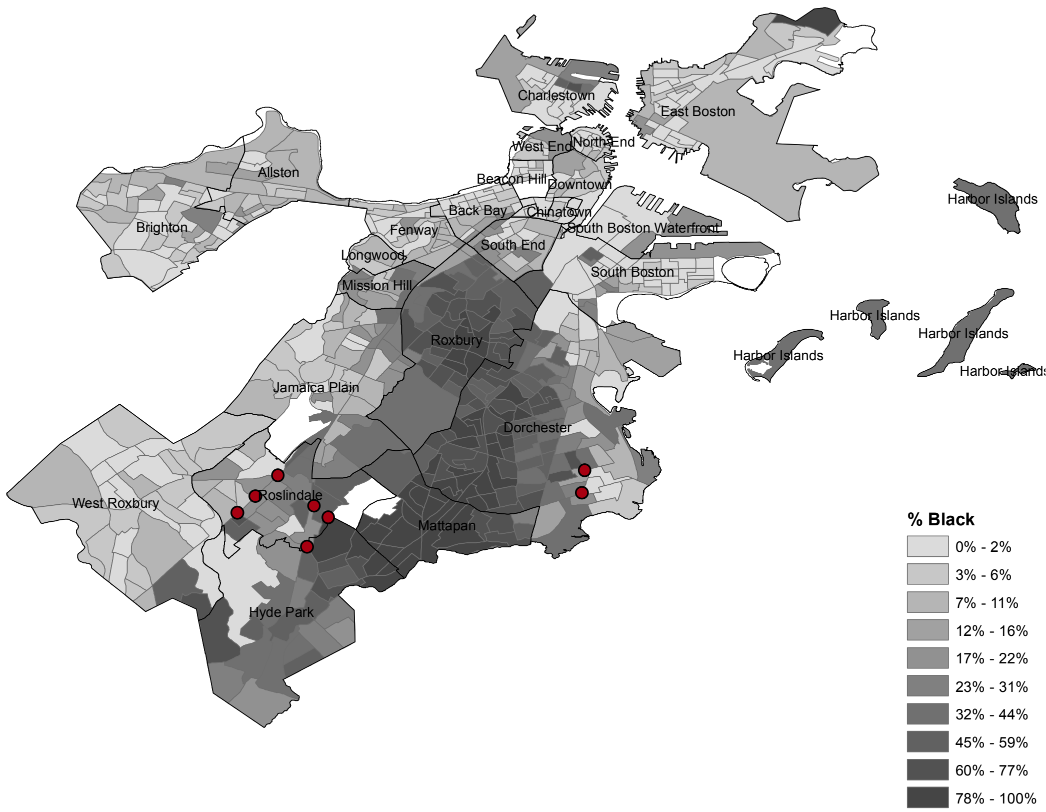


Figure A7: Map of Race Placebo Points

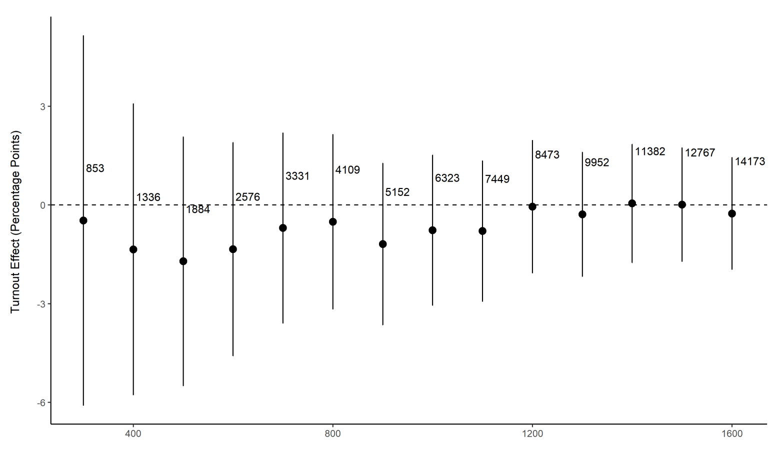


Figure A8: Racial Threat Placebo Regression 1: (42.278358, -71.137021). Estimates from complete model specification (matching, controls, and council district fixed effects).

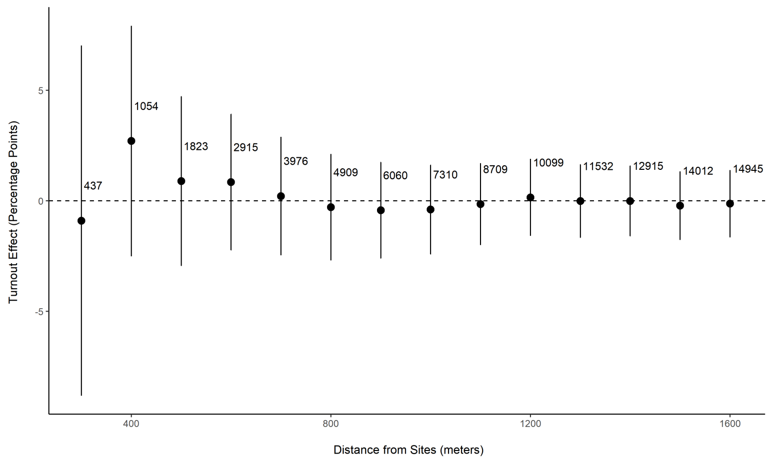


Figure A9: Racial Threat Placebo Regression 2: (42.287065 -71.127461)

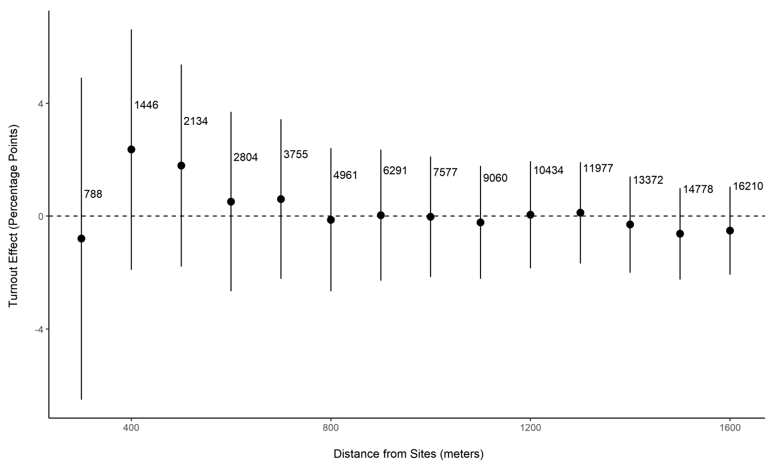


Figure A10: Racial Threat Placebo Regression 3: (42.2822, -71.13274). Estimates from complete model specification (matching, controls, and council district fixed effects).

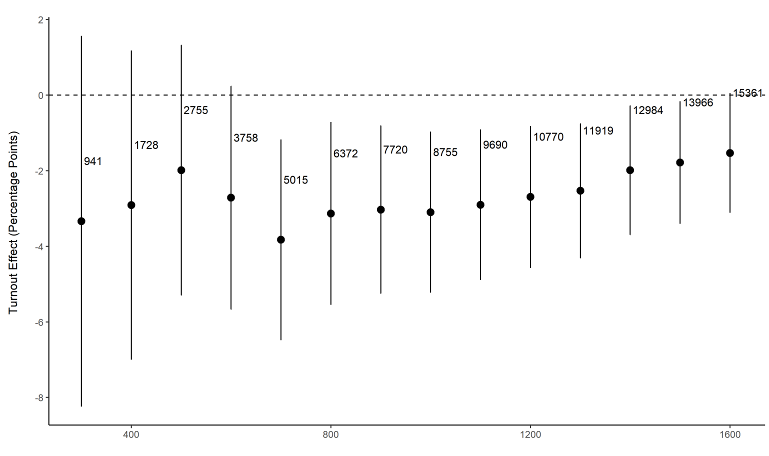


Figure A11: Racial Threat Placebo Regression 4: (42.288257, -71.05531). Estimates from complete model specification (matching, controls, and council district fixed effects).

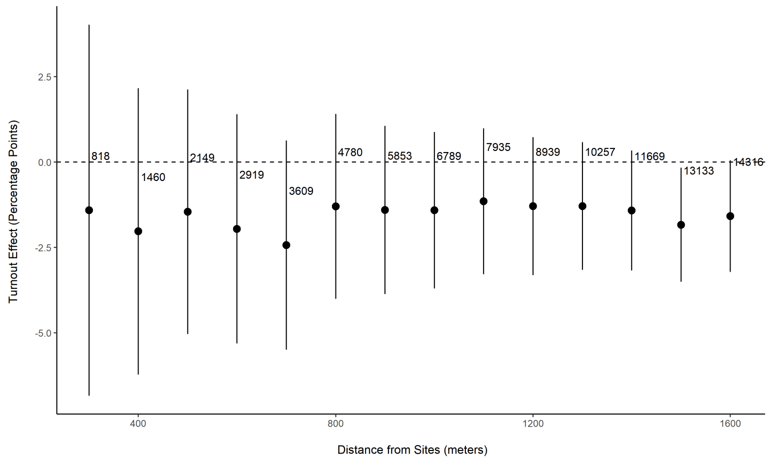


Figure A12: Racial Threat Placebo Regression 5: (42.282955, -71.055964). Estimates from complete model specification (matching, controls, and council district fixed effects).

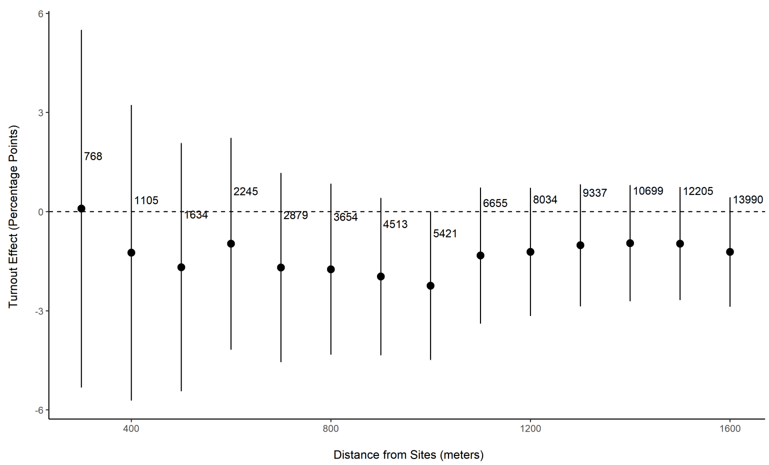


Figure A13: Racial Threat Placebo Regression 6: (42.279906, -71.118913). Estimates from complete model specification (matching, controls, and council district fixed effects)

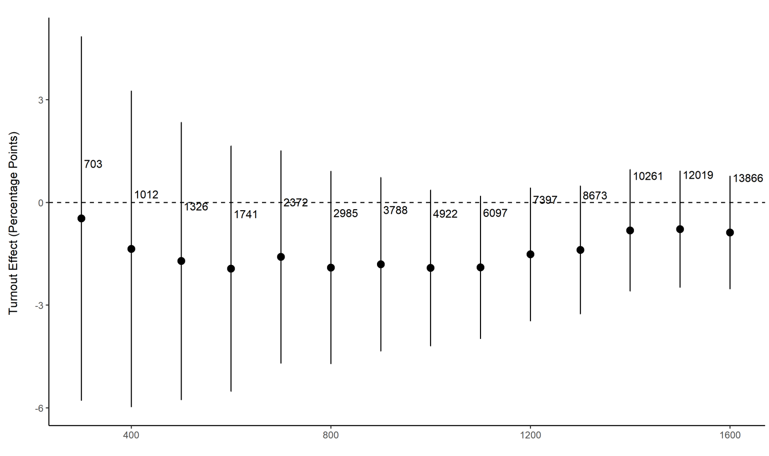


Figure A14: Racial Threat Placebo Regression 7: (42.278961, -71.117008). Estimates from complete model specification (matching, controls, and council district fixed effects)

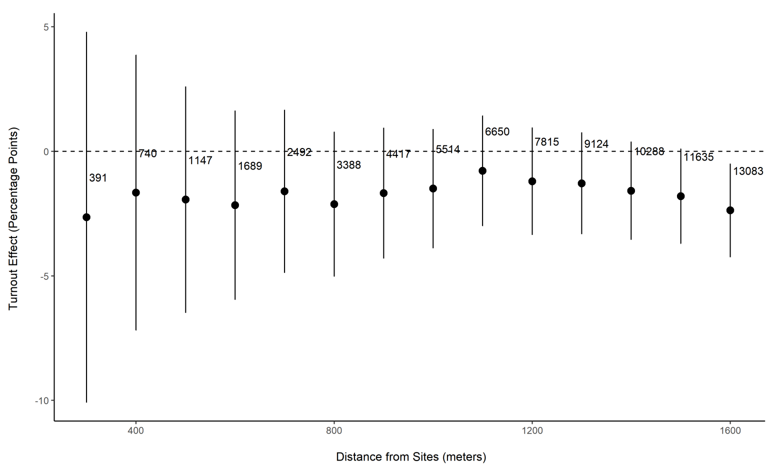


Figure A15: Racial Threat Placebo Regression 8: (42.270353, -71.120604). Estimates from complete model specification (matching, controls, and council district fixed effects)

9 Monte Carlo Permutation Test

As an additional robustness check, we estimated treatment effects for changes in Mayoral turnout between 2013 and 2017 using a non-parametric Monte Carlo permutation test (or random permutation test). To estimate this statistic, we randomly sampled 12,000 points contained within the city of Boston shapefile. For each randomly sampled point, a "treated" group was defined as all registered voters living within 400 meters of the point, while the "control group" was defined as all registered voters living elsewhere. A treatment effect was estimated for each randomly sampled point using the raw association specification. This produced a reference distribution of treatment effects corresponding to the relative change in turnout of voters living within 400 meter radii of random points in Boston compared to the rest of Boston. This reference distribution can be compared against our observed treatment effect to assess how uncommon it is to observe an effect at least as large as the increased turnout effect reported earlier for voters living near the relocation sites. An advantage of this analytic approach is that it does not make distributional assumptions.

One complication of applying the random permutation test is that the sample size of the treated group varied substantially depending on the location of the random point. Indeed, in 1,266 of the 12,000 randomly sampled points, there were no registered voters within 400 meters, as shown in Figure A16. These points were on Boston's unpopulated harbor islands, the airport, the Shattuck Hospital complex, the zoo, a golf course, and edges of the city. Points with zero registered voters were dropped from the reference distribution.

For the 775 voters living within 400 meters of the relocation sites, mayoral turnout increased 9.0% relative to the rest of the city's voters. We can compare this observed treatment effect with the reference distribution of points across the city of Boston. Using as a reference distribution all samples with at least 1 registered voter within 400 meters of the point (n = 10,734), the observed treatment effect fell in the 94.3% percentile (one-sided p-value = 0.057). Dropping samples with fewer than 20 observations (n = 10,456), the observed treatment effect fell in the 95.4% percentile (one sided p-value = 0.046). This is shown in Figure A17. Limiting the reference distribution only to those with sample sizes at least as large as the observed sample size of the treatment group of 775 limits the reference distribution to 5,928 points. Under this reference distribution, the observed treatment effect falls in the 97.7% percentile (one-sided p-value = 0.023). These analyses suggest that it is rare to find a change in turnout as large as the one observed in the vicinity of the relocation sites. This provides additional support for the claim that residents experiencing an exogenous increase in neighborhood disorder increased their participation in the subsequent local election compared to voters living further away.

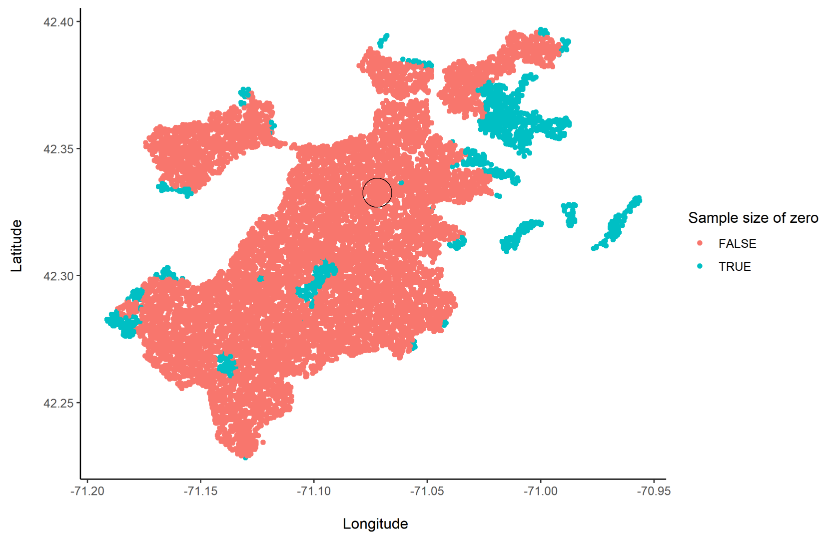


Figure A16: Each point represents a randomly sampled point used in the Monte Carlo Test. Samples with zero registered voters within 400 meters are shown in blue. The circled area is approximately the actual "treated" area.

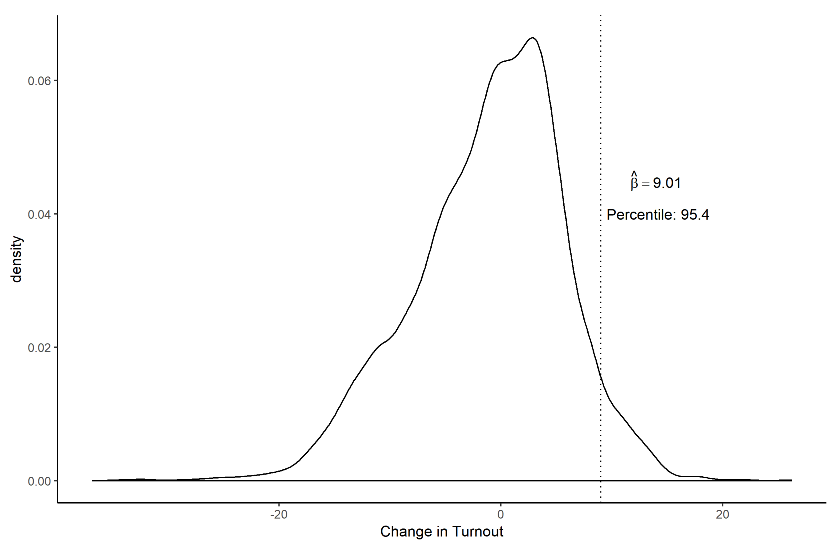


Figure A17: Observed treatment effect (dashed line) in distribution of simulated treatment effects for random points across Boston. Simulations with fewer than 20 voters falling in the "treated" group are excluded, leaving a reference distribution of 10,456 observations.

10 Interacting Treatment Effect with Homeownership

One might expect that homeowners are especially likely to be mobilized by an increase in neighborhood disorder, since homes are often people's most valuable assets and the value of homes may be threatened by higher levels of disorder (Fischel 2001). To test this, we collected home ownership data from parcel data provided by the city of Boston and merged these data to our voterfile data by matching the name of the property owner in the parcel data to the name of the voter in the voterfile and the address of the property in the parcel data to the voter's residential address. However, estimating the complete model specifications with an interaction between treatment and home ownership does not provide evidence for a differential turnout impact among homeowners, as Figure A18 illustrates. Treatment effects are consistently positive for both homeowners and renters. At close distances, where the sample is small, the estimated effect among homeowners is large and extremely imprecisely estimated, as the top panel shows. At larger distances, with more precisely estimated treatment effects, the point estimate for homeowners converges with (and is slightly smaller than) the effect among renters. At no distance is the interaction with homeownership statistically significant (Table A19). These analyses fail to produce evidence of a differential treatment effect among homeowners, though the substantial uncertainty in these estimates precludes any confident inferences.

Distance	p value
300	0.603
400	0.393
500	0.186
600	0.122
700	0.229
800	0.128
900	0.191
1000	0.918
1100	0.438
1200	0.663
1300	0.567
1400	0.848
1500	0.552
1600	0.444

Table A19: Two-sided p-values for the interaction between distance and homeownership in the complete model specification.

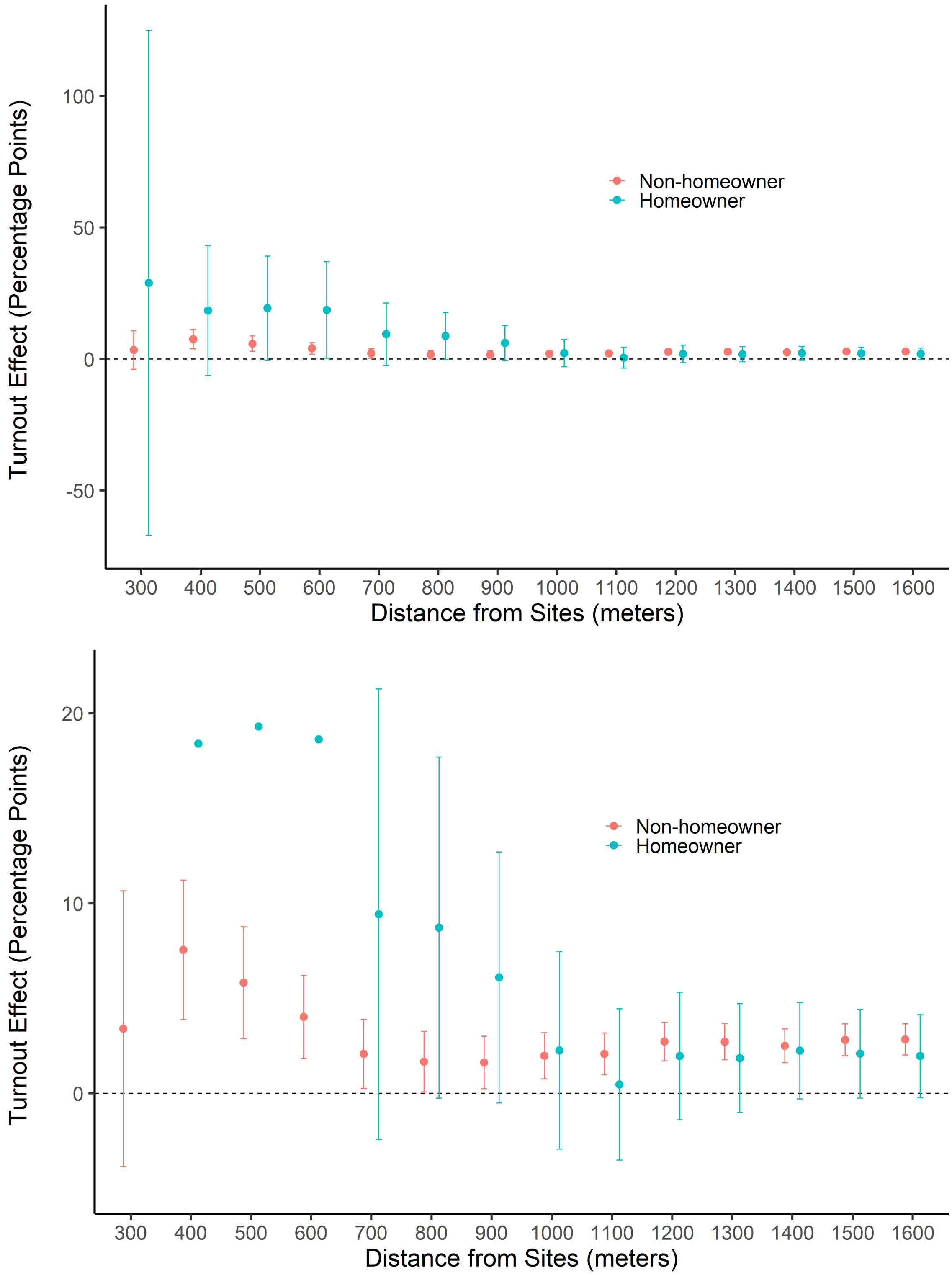


Figure A18: Treatment effects separated by homeownership. The second plots shows the same estimates with the y-axis shrunk to highlight the more precisely estimated effects. Where point estimates or error bars not shown in the second plot, the estimate or confidence intervals exceed y-axis limits.

11 Political Behaviors Beyond Voting

The political response following the Long Island Bridge closure was not limited to voting. Exposure to the affected areas has led residents and others to introduce legislation, hold protests, attend city council hearings, and contact the mayor and other local officials to demand that the city government curb disorder. Local organizations and elected officials responded to this political pressure. Managing Methadone Mile has mobilized residents to organize various sophisticated political activities through their local neighborhood associations (Rich 1980, Pollock III 1982, Tissot 2015, Levine 2016). Dozens of members from local neighborhood associations ambushed Mayor Walsh at a 2017 event commemorating a playground renovation urging him to address homelessness and drug problems in the neighborhood (Maniscalco 2017). The President of the Blackstone-Franklin Squares Neighbor Association told a local forum of "ongoing use of Franklin Square as a homeless encampment in the aftermath of last year's abrupt closure of the Long Island Bridge" and stated his intention to organize his neighbors "to take Franklin Square back" by holding events in the park (Maniscalco 2015). The group formed a Public Safety Committee in 2015 (BFSNA 2015) which has gone on "walking-tours" with local politicians to identify suitable locations to install security cameras (BFSNA 2016*a*) and encouraged the Boston Police Department to make additional arrests for public drinking in parks (BFSNA 2016*b*). The Worcester Square Area Neighborhood Association (WSANA) formed a Safety Committee to identify policies to recommend to their city councillor to manage Methadone Mile, including additional regulations on methadone clinics (Daniel 2016*c*). The same group has proposed a uniform "Good Neighbor's Policy" regulating the conduct of methadone clinics and homeless shelters (Daniel 2017*c*). George Stergios, the president of WSANA, is sometimes called "Mayor of Methadone Mile" for his efforts to deal with the consequences of opiate use in the neighborhood (Daniel 2016*a*). The Chester Square Neighborhood Association (CSNA) meeting minutes for July 9th 2015 summarize the group's report on homelessness and methadone clients presented in a meeting attended by local city councillor Frank Baker, city outreach staff, the Boston Police Department, and others, though the minutes also note that "district politicians", "City Hall Department leaders", and "state Representatives" were absent. In their August meeting, the CSNA formed a "Crime Watch" group and encouraged members to write letters to "neighborhood leaders, city counselors, and state representatives" about the issue and planned "walkthroughs" of the affected areas with city councillors and city of Boston staff members (CSNA 2015). In 2018, Steve Fox, head of the South End Forum, a conglomeration of neighborhood associations, coordinated a campaign to send public letters in support of re-opening the Long Island Bridge to the Massachusetts State Environmental Board. Fox provided email instructions and advocacy talking points to community members on Facebook in a post shared by other neighborhood groups (Fox 2018). Reportedly 900 people wrote letters, prompting Mayor Walsh to thank members of the

South End Forum at a meeting with the group for supporting the city's attempts to rebuild the bridge (Seth 2018*a*).

Local residents have also changed their political behavior by engaging in vociferous NIMBYism towards siting additional social services, medical marijuana dispensaries, and liquor stores perceived to exacerbate neighborhood problems. Attendees of a 2015 South End Forum meeting with Mayor Walsh informed him that "additional services... may be unwise in maintaining a reasonable balance" in the neighborhood (South End News 2015). The group proposed a moratorium on new social services within a one-mile radius of the Boston Medical Center in 2016 (CSNA 2016, Seth 2018*b*). Safe injection sites (SIFs) have provoked a particularly fierce response (Daniel 2018*b*). Many residents and representatives of local associations attended a July 2017 Boston City Council hearing on safe injection sites (SIFs) to state their opposition to opening a SIF in the area. Steve Fox, moderator of the South End Forum, said at the hearing: "We are dying by a thousand cuts and no one has addressed the influx of new clients coming in the SIF" (Daniel 2017*b*). Sue Sullivan, head of a local business group, chastised (Bebinger 2017) the Massachusetts medical community at the city hearing on SIFS, saying "the Massachusetts Medical society should be ashamed of themselves... this is the best they can come up with?" Local residents have also mobilized to rein-in the construction of marijuana dispensaries (Maniscalco 2016*a*, Daniel 2018*b*) through political advocacy and issuing advisory opinions to the city zoning board (Daniel 2018*a*) and blocked the opening of a proposed liquor store over fears it would exacerbate neighborhood problems (Maniscalco 2016*b*, Daniel 2016*b*, Gaffin 2016). An attendee of a local neighborhood association meeting explained his opposition to marijuana dispensaries:"If you put cheese in a rat infested area, you'll draw more rats" (BFSNA 2016*a*).

MA State Senator John Keenan walked around the Methadone Mile area for several hours in April 2017, talking to local security guards, social service employees, and homeless people. He wrote an essay about the experience on his website entitled "Miles to Go", describing the peculiar juxtaposition of profound human suffering and compassion that he witnessed (Keenan 2017). Keenan later (Lannan & Young 2017) wrote an amendment to a criminal justice reform bill to require all jails to provide medication assisted treatment for opioids, citing his experiences walking around the area, and urging his colleagues to "Look at those people, talk to those people, understand those people." Reactions among ordinary citizens were similarly stark. Fifty students from nearby Orchard Gardens K-8 Pilot School, along with teachers and school administrators, held a rally demanding that the city address constant discarded needles around the school, chanting "Hey Ho! All the needles have to go," and holding signs that read "no more needles", "safe schools", and "protect our community" (Irons 2017). One parent posted a large banner at the school saying "#over414000" referring to the number of needles collected by the Boston Public Health Commission over the previous year (Becker & Amer 2017). She told a journalist that "For me, this year the problem has gotten worse because we see a lot of people as we're walking from school, and it's not the needle. It's the people actively using drugs" Even periodic commutes through the neighborhood have led to political behaviors like contacting elected officials. Ted from Boston's Dorchester neighborhood contacted Boston Public Radio's "Ask the Mayor" Show in August 2018 to tell Mayor Walsh that driving down Massachusetts Avenue was "like somebody hit me with a shovel in my Irish face. To see all those, you know, the Methadonians and all those people crawling around on the ground. I had tears running down my [face]..." before asking the Mayor "Is there a miracle that we can help these poor son of a guns?" Walsh responded that "I have this conversation almost every single day."

Local candidates have changed their behavior by campaigning on their expertise in managing drug problems and homelessness. Ed Flynn, a probation officer who had worked with the homeless population on Long Island, was elected city councillor in the South End in 2017. He told a local neighborhood association that he has "a lot of experience dealing with the homeless and addicted communities in the district, especially at Methadone Mile" (Daniel 2017*a*, Deehan 2017). Jon Santiago's successful primary challenge in a local state house race against the Assistant Majority Leader Byron Rushing similarly emphasized his expertise in dealing with drug crises as a physician at the Boston Medical Center and advocate on opioid policy (Capelouto 2018). These events collectively evidence a multifaceted local political response to neighborhood disorder beyond voting. In addition to higher turnout and anti-incumbent voting, residents near the relocation sites changed their political behavior in other ways including attending protests, contacting local elected officials, writing letters, shutting down the construction of new social services, and devising and lobbying for codes of conduct for existing social service facilities. To maintain control over their neighborhoods, residents acting both individually and via their neighborhood associations coordinated a sophisticated political response that achieved tangible policy changes.

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