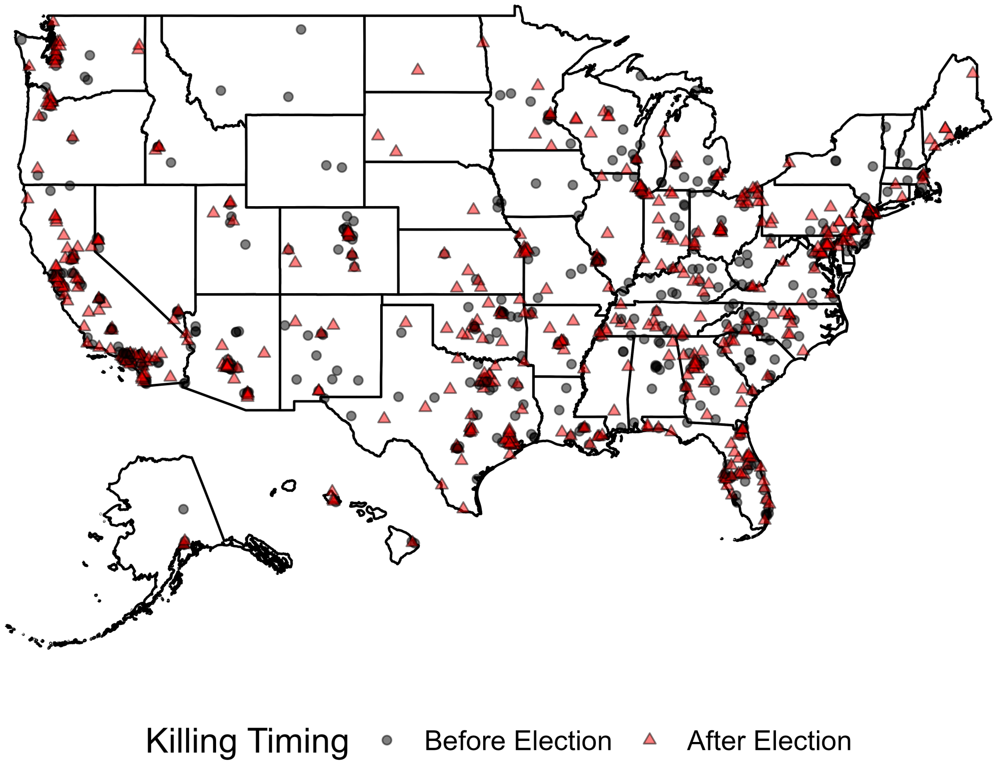
# Supplementary Information-A

## Contents

# 1 Map of Killings Before and After 2016 Election

Figure A1: Map of Killings Within 6 Months of 2016 Election




# 2 Dates of Voter File Snapshots from L2

Following the best-practices for voter file research outlined in Kim and Fraga (2022), we list the date of each voter file snapshot used in Table A1. The dates always post-date the election and reflect the earliest snapshot made available by L2 for each state in which the general-election turnout is recorded.

Table A1: Snapshot Dates

| state | 2014 | 2016 | 2018 | 2020 |
|---|---|---|---|---|
| AK | 2015-03-13 | 2017-01-27 | 2019-02-11 | 2021-02-03 |
| AL | 2015-04-10 | 2017-03-07 | 2019-01-27 | 2021-02-04 |

Table A1: Snapshot Dates *(continued)*

| state | 2014 | 2016 | 2018 | 2020 |
|---|---|---|---|---|
| AR | 2015-03-24 | 2017-03-29 | 2018-09-21 | 2021-01-19 |
| AZ | 2015-04-22 | 2017-04-12 | 2018-09-07 | 2021-04-27 |
| CA | 2015-05-21 | 2017-03-25 | 2019-01-31 | 2021-02-19 |
| CO | 2015-05-05 | 2017-02-08 | 2019-08-31 | 2020-12-23 |
| CT | 2015-03-25 | 2017-01-20 | 2019-06-03 | 2021-03-30 |
| DC | 2015-03-07 | 2017-02-15 | 2019-01-17 | 2021-01-30 |
| DE | 2015-02-23 | 2017-01-17 | 2019-04-02 | 2021-03-24 |
| FL | 2015-01-28 | 2017-01-27 | 2019-02-08 | 2021-02-04 |
| GA | 2015-05-16 | 2017-01-27 | 2018-12-22 | 2021-02-04 |
| HI | 2015-03-05 | 2017-03-22 | 2019-04-05 | 2021-04-01 |
| IA | 2015-03-25 | 2017-01-31 | 2019-03-06 | 2021-03-04 |
| ID | 2015-02-23 | 2017-03-20 | 2019-03-04 | 2021-03-16 |
| IL | 2015-03-02 | 2017-03-18 | 2019-02-21 | 2021-03-05 |
| IN | 2015-05-06 | 2017-04-07 | 2019-02-13 | 2021-01-15 |
| KS | 2015-02-26 | 2017-02-16 | 2019-01-31 | 2021-03-16 |
| KY | 2015-03-05 | 2017-03-03 | 2018-09-29 | 2021-05-11 |
| LA | 2015-02-23 | 2017-02-14 | 2019-01-15 | 2021-01-22 |
| MA | 2015-04-02 | 2017-04-11 | 2019-02-14 | 2021-01-19 |
| MD | 2015-02-25 | 2017-01-20 | 2018-12-14 | 2021-02-15 |
| ME | 2015-04-29 | 2017-04-07 | 2018-09-26 | 2021-05-28 |
| MI | 2015-02-28 | 2017-02-21 | 2019-03-22 | 2021-01-30 |
| MN | 2015-03-03 | 2017-03-10 | 2019-04-02 | 2021-02-14 |
| MO | 2015-03-02 | 2017-02-08 | 2019-06-03 | 2021-02-11 |
| MS | 2015-03-17 | 2017-03-07 | 2019-03-11 | 2021-03-23 |
| MT | 2015-03-27 | 2017-01-25 | 2019-02-07 | 2020-12-14 |
| NC | 2015-07-29 | 2017-01-12 | 2019-02-01 | 2021-01-28 |
| ND | 2015-04-15 | 2017-02-09 | 2019-03-22 | 2021-03-18 |
| NE | 2015-03-25 | 2017-01-13 | 2019-01-10 | 2021-01-20 |
| NH | 2015-03-20 | 2018-08-15 | 2019-04-10 | 2021-03-25 |
| NJ | 2015-02-25 | 2017-03-31 | 2019-04-03 | 2021-03-11 |

Table A1: Snapshot Dates *(continued)*

| state | 2014 | 2016 | 2018 | 2020 |
|---|---|---|---|---|
| NM | 2015-03-19 | 2017-02-08 | 2019-02-22 | 2021-02-25 |
| NV | 2015-01-30 | 2017-01-13 | 2019-01-23 | 2020-12-17 |
| NY | 2015-03-25 | 2017-03-14 | 2019-02-27 | 2021-03-15 |
| OH | 2015-01-08 | 2017-01-09 | 2019-01-22 | 2021-01-07 |
| OK | 2015-03-26 | 2017-01-12 | 2019-03-01 | 2021-02-08 |
| OR | 2015-04-16 | 2017-01-13 | 2019-02-24 | 2021-02-05 |
| PA | 2015-05-01 | 2017-02-14 | 2019-09-23 | 2021-02-17 |
| RI | 2015-03-06 | 2017-01-18 | 2019-03-15 | 2021-02-10 |
| SC | 2015-04-09 | 2017-02-24 | 2019-03-12 | 2021-04-16 |
| SD | 2015-03-13 | 2017-02-20 | 2019-01-14 | 2021-01-22 |
| TN | 2015-02-23 | 2017-02-17 | 2019-01-30 | 2021-03-29 |
| TX | 2015-04-15 | 2017-03-12 | 2019-02-24 | 2021-03-25 |
| UT | 2015-03-06 | 2017-01-25 | 2019-03-07 | 2021-03-26 |
| VA | 2015-04-18 | 2017-03-29 | 2019-03-12 | 2021-02-18 |
| VT | 2015-03-20 | 2017-02-14 | 2019-03-08 | 2021-03-04 |
| WA | 2015-05-05 | 2017-05-24 | 2019-01-08 | 2020-12-09 |
| WI | 2015-03-03 | 2017-03-30 | 2019-02-01 | 2021-02-24 |
| WV | 2015-03-16 | 2017-04-03 | 2019-03-22 | 2021-03-11 |
| WY | 2015-03-30 | 2017-02-02 | 2019-04-02 | 2021-01-13 |

# 3 Robustness Checks for Primary RD

The point estimates, confidence intervals, sample sizes, and other information about each robustness regression can be found in the SI located in the Dataverse, along with similar tables for models presented in the body of the manuscript.

## 3.1 Alternative Specifications for Regression Discontinuity

In the body of this manuscript we present estimated local average treatment effects (LATEs) using a regression discontinuity approach that incorporates both entropy balancing and OLS covariates. Here, we show that our primary results hold when we include only entropy balancing, only OLS covariates, or no adjustment at all. The regression estimates for these figures can be found in Tables B13–B15, respectively.

Figure A2: Alternative Approaches for Ensuring Balance Across Cut-Point

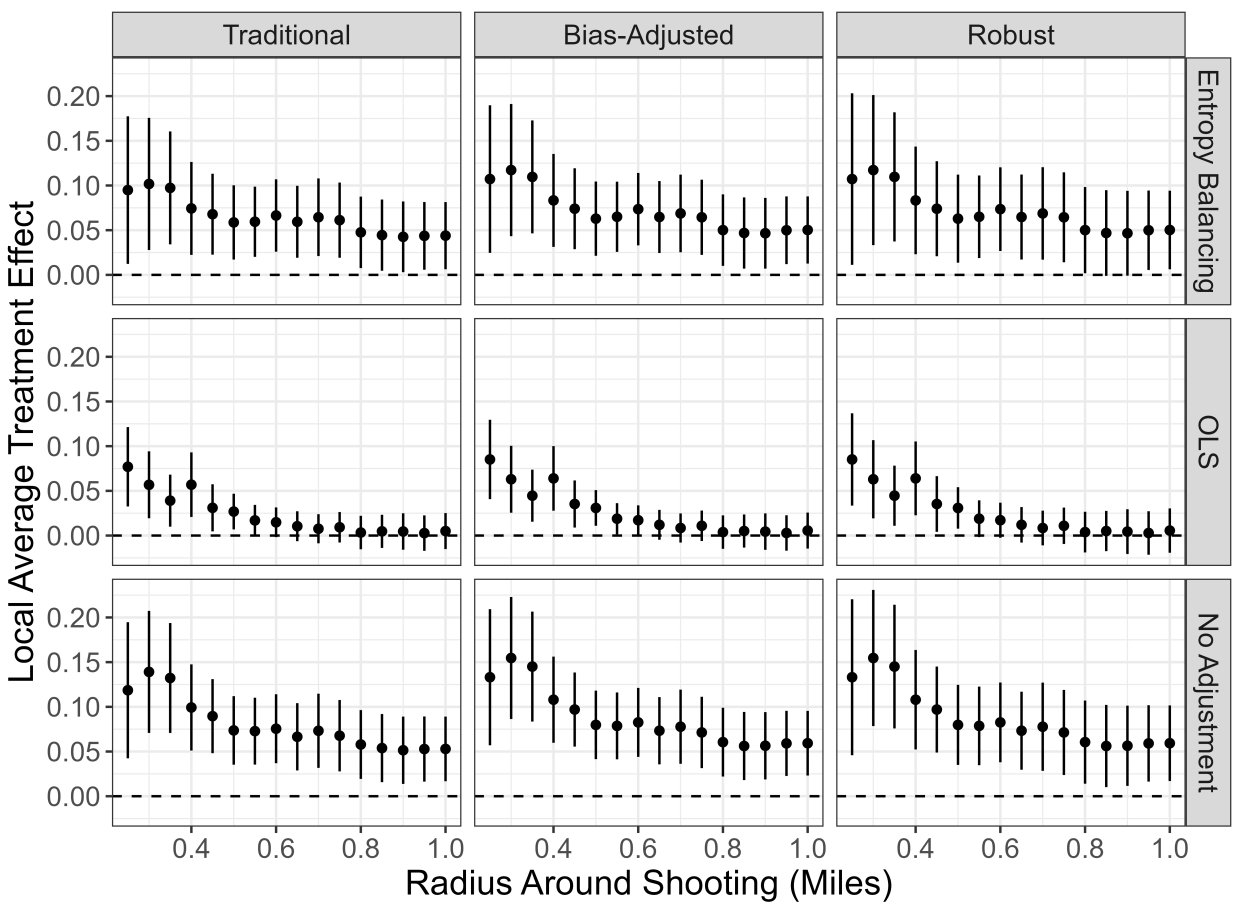
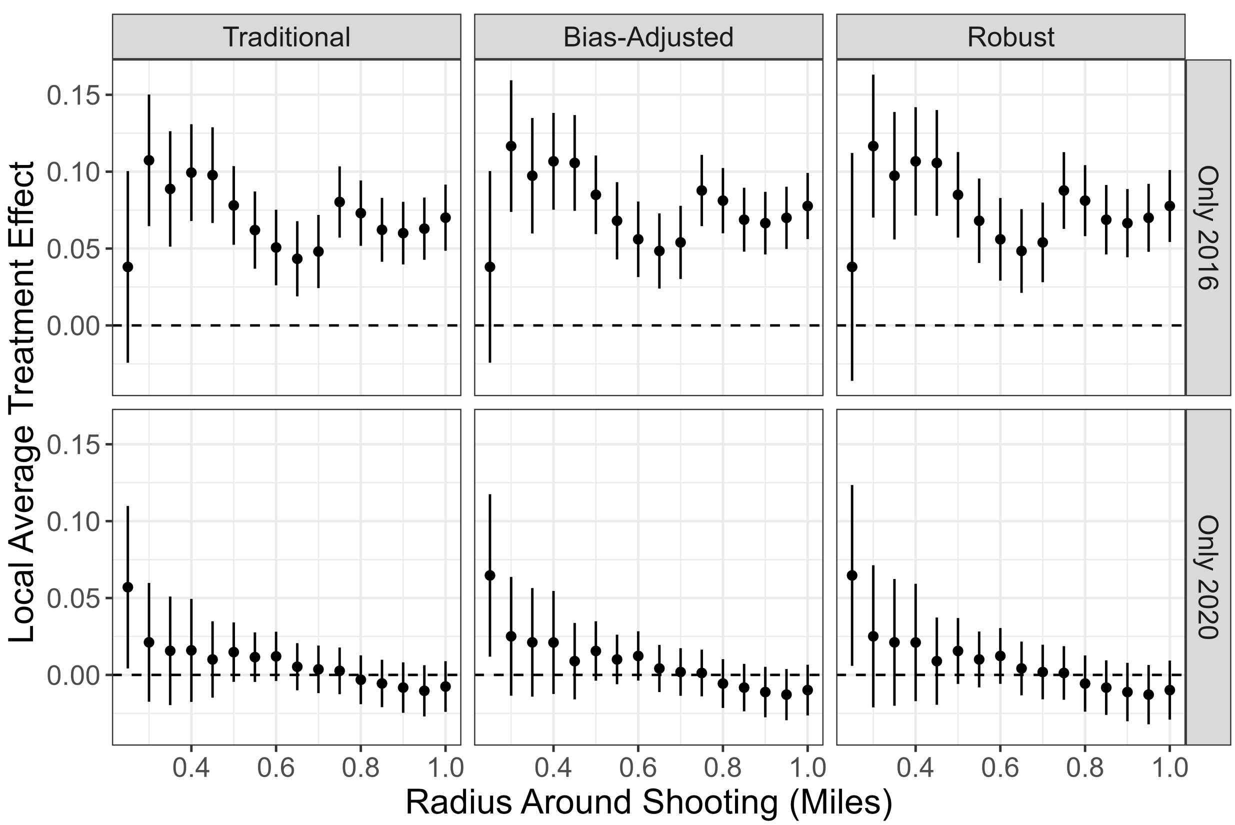



Figure A2 makes clear that the overall LATEs we identify in the body of the manuscript are robust to alternative ways of ensuring that observations on either side of the cut-point closely mirror one another. In fact, the estimated LATEs in the body of the manuscript are in many cases *smaller* than those presented here, indicating that our primary approach is, if anything, conservative.

## 3.2 Individual Plots for 2016 and 2020

Throughout the body of this manuscript we present results from 2016 and 2020 in combined models. Here we present the plots from 2016 and 2020 individually. Although the estimated effects are higher for 2016 and they do not drop to zero, the estimated effects follow the same pattern for both years: neighborhoods close to a police killing see higher turnout, and these effects decay with spatial distance. Future research should investigate why these effects were different in 2016 and 2020, but such an investigation is beyond the scope of this study. The regression estimates for 2016 and 2020 can be found in Tables B16–B17, respectively.

Figure A3: LATEs for 2016 and 2020



### 3.3 Sample Sizes for LATEs

In Figure A4 below, we present the effective sample size for each of the overall models using the robust methodology. These range from 308 to 6,799. It is worth noting that the sample size does not monotonically increase. This is possible because each model uses a different non-parametric bandwidth, and the bandwidth could be narrower despite using a wider radius around the police killing.

Figure A5 plots the effective sample sizes for the robust models presented in Figure 4 in the body of the manuscript; Figure A6 plots the effective sample sizes for each model presented in Figure 5 in the body of this manuscript.

Figure A4: Sample Sizes at Different Radii

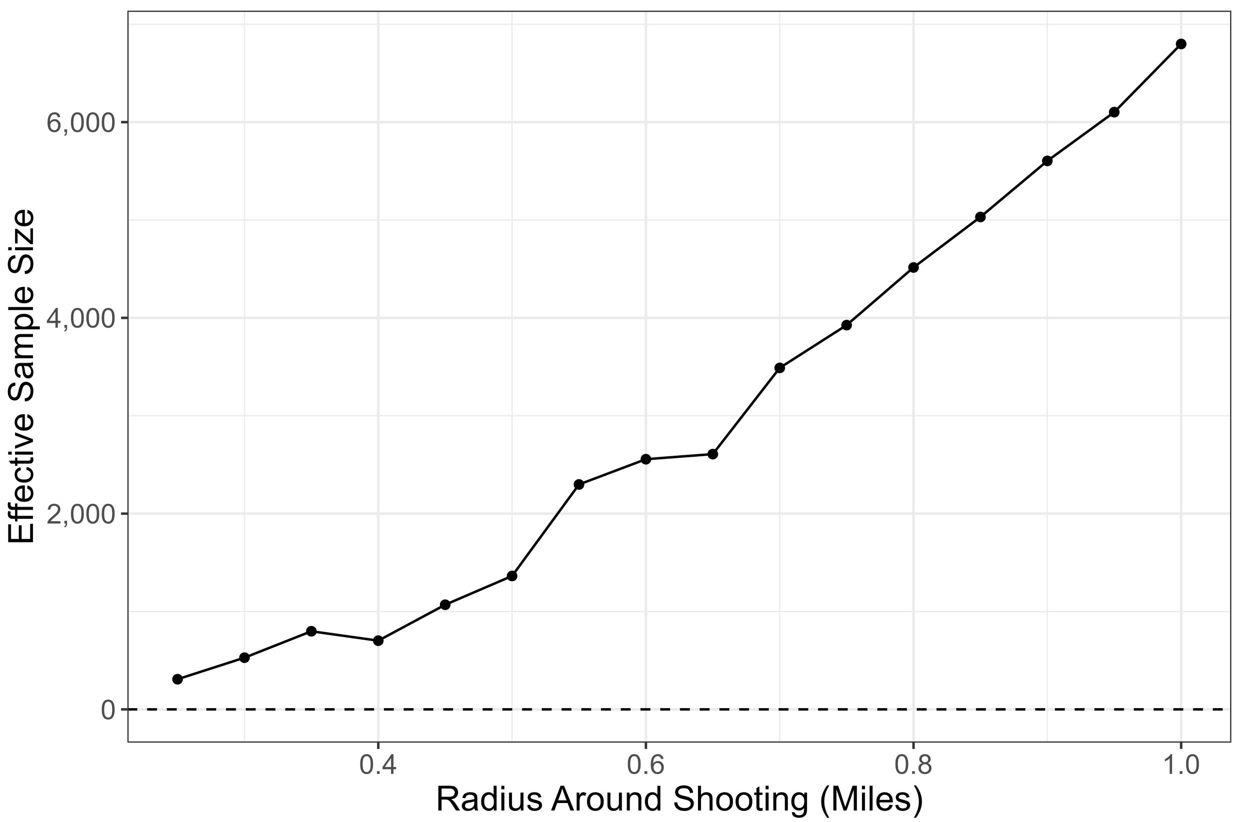


Figure A5: Sample Sizes at Different Radii

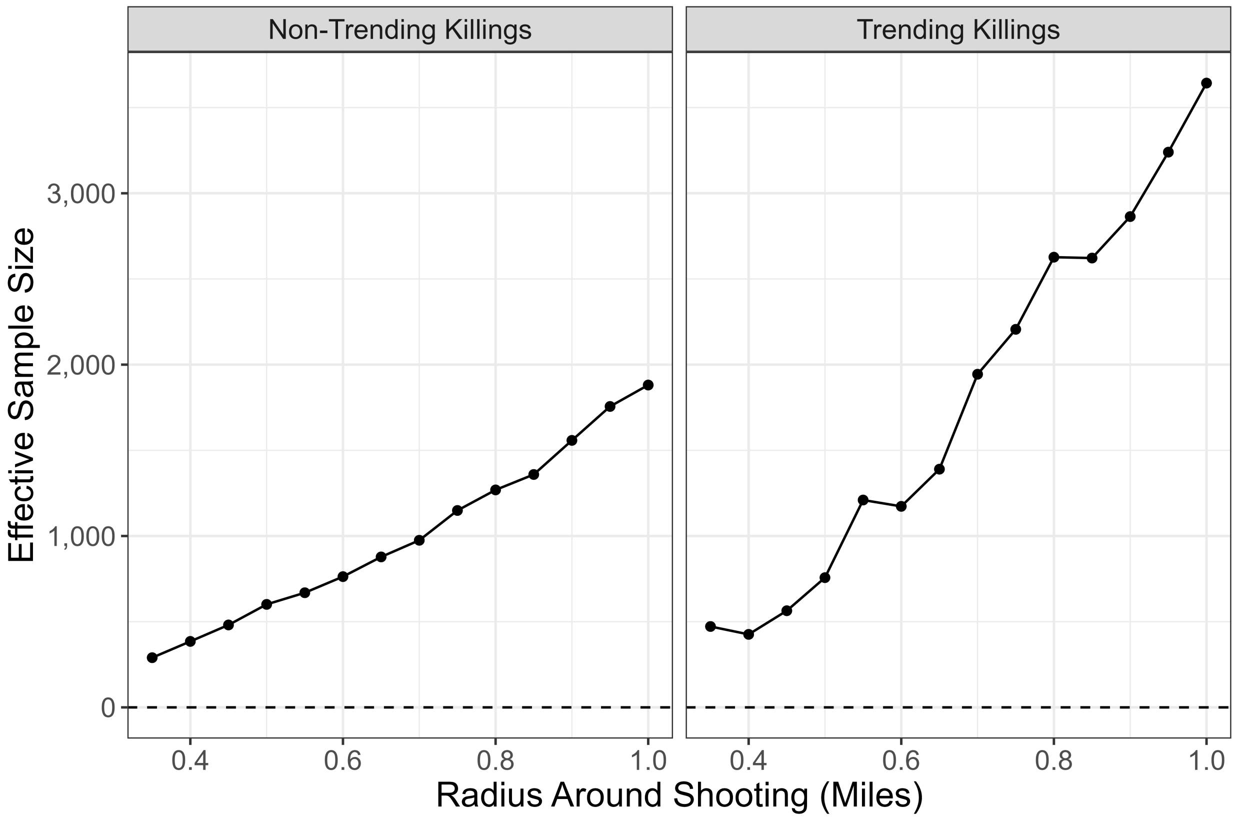
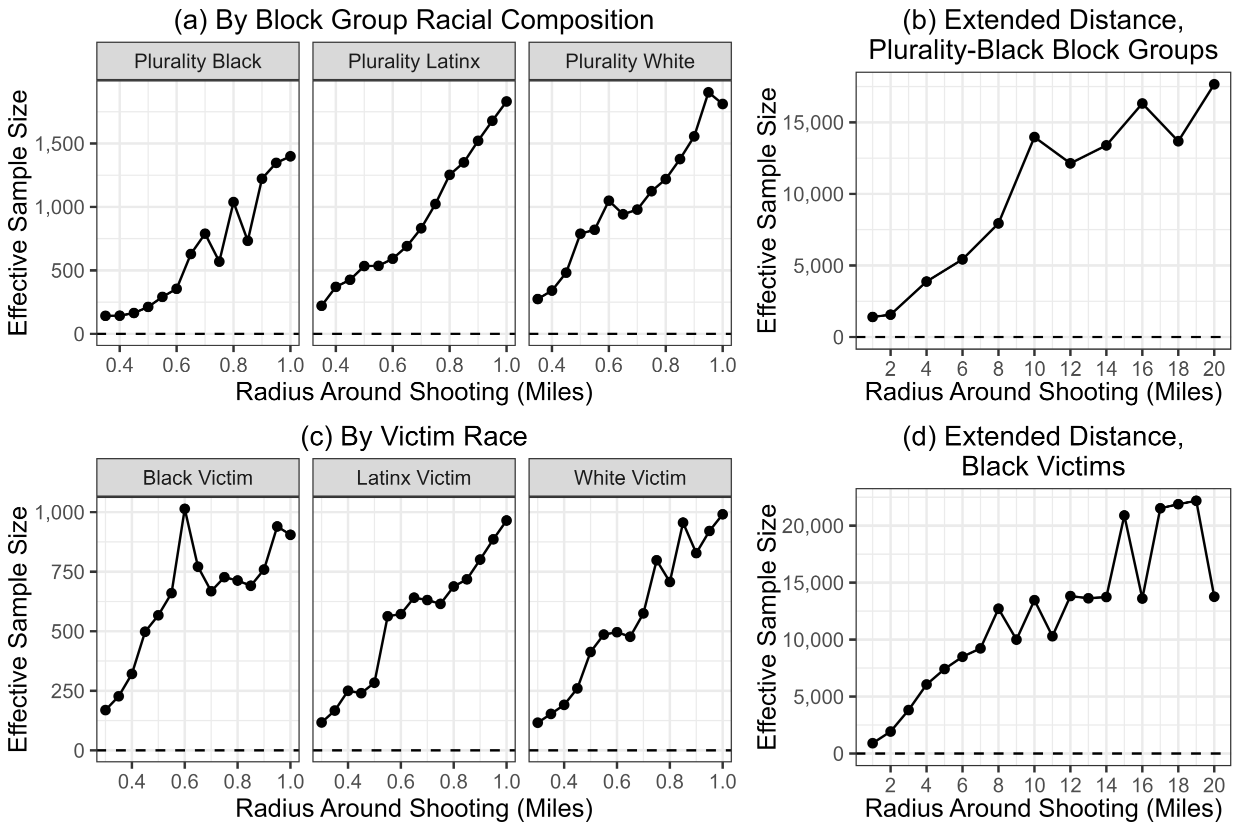


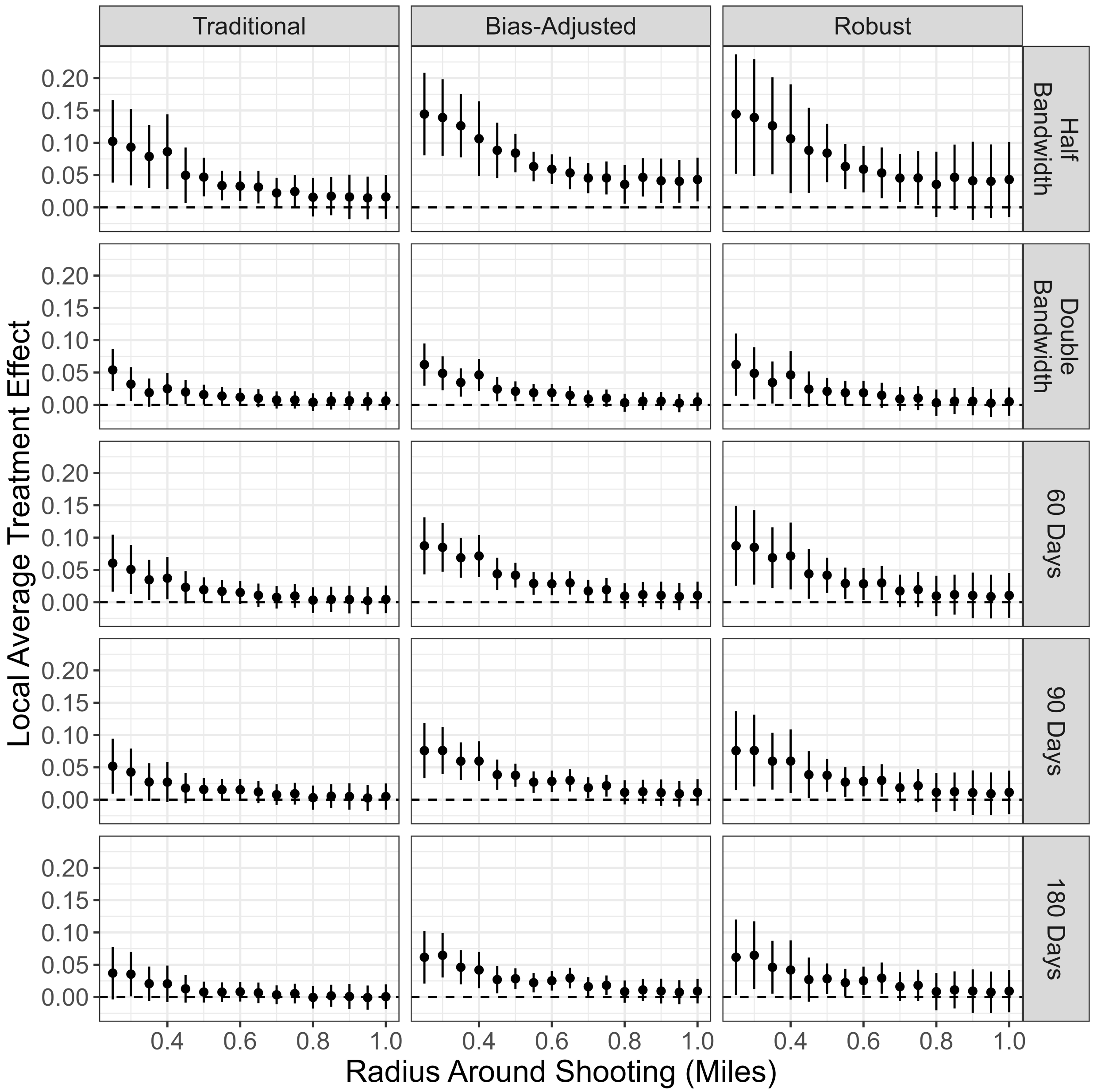
Figure A6: Sample Sizes at Different Radii




## 3.4 Alternative Bandwidths for Regression Discontinuity

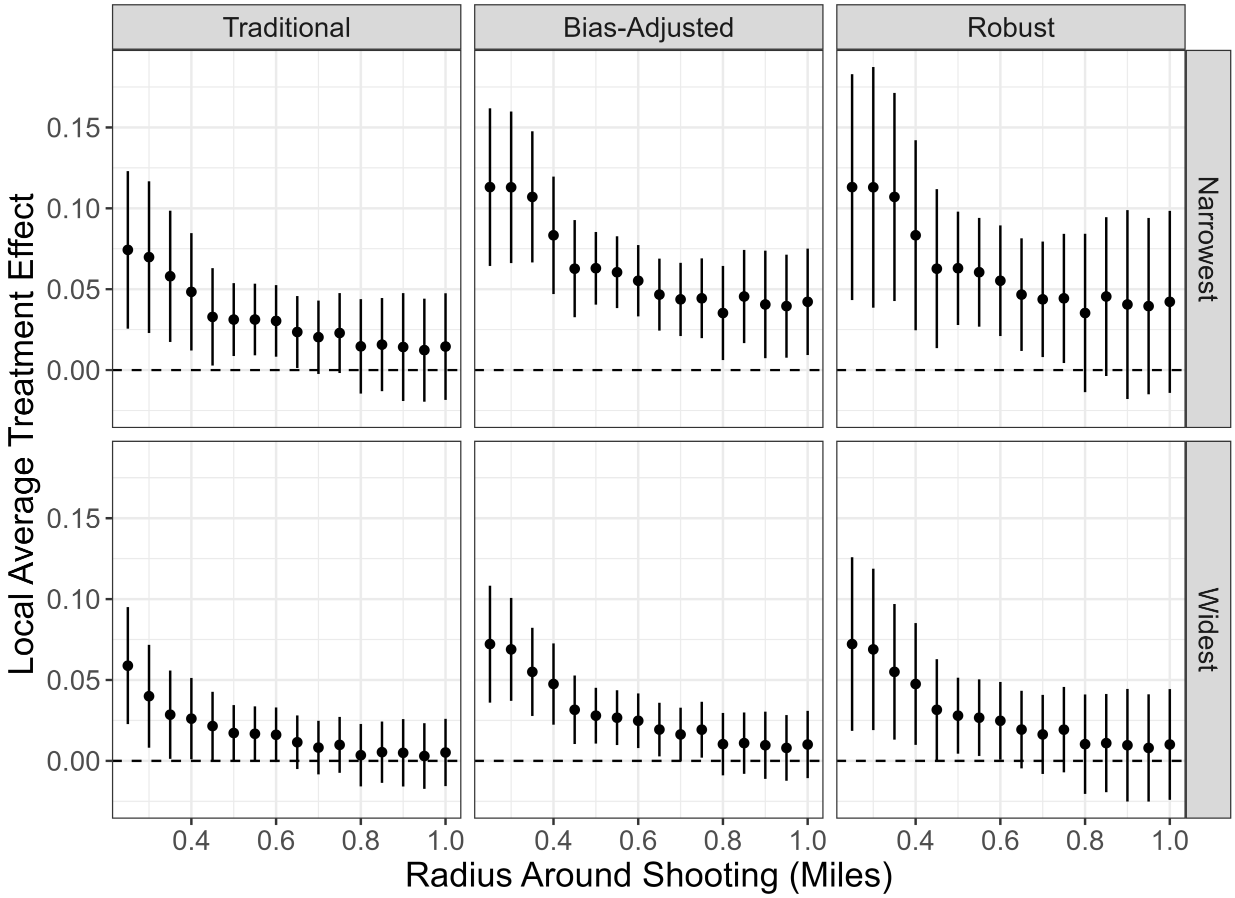
In Figure A7 we show the estimated treatment effects using different bandwidths. In the first two rows, we show the LATEs using a bandwidth half and twice as wide as the nonparametric one from the body of the manuscript. In the third row, we use the primary nonparametric bandwidth for treated units, while allowing the RDiT to include all block groups near a police killing between election day and 60 days following the election. In the fourth row, we allow this to extend to 90 days, and in the fifth to 180. They tell the same story as that presented in the body of the manuscript, demonstrating a large LATE that decays with spatial distance. The regression estimates these different bandwidths can be found in Tables B18–B22, respectively.

Figure A7: LATE Using Different Bandwidths




Each model in our primary approach re-estimates a different nonparametric bandwidth. In Figure A8 we re-estimate our primary models forcing each model to take the narrowest (in the top row) and widest (in the bottom row) automatically selected bandwidth. Our results are not driven by different bandwidths being selected for different thresholds. The regression estimates for the narrowest and widest bandwidths can be found in Tables B23–B24, respectively.

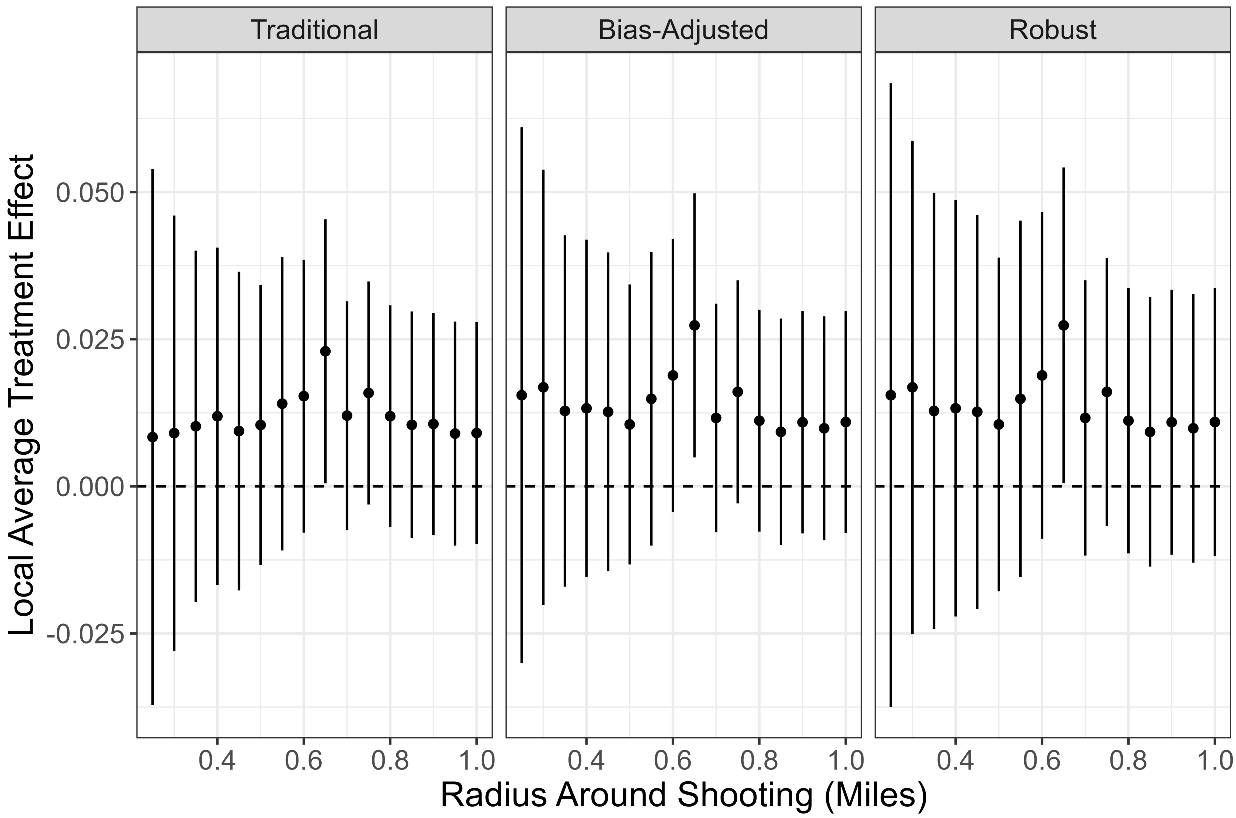
Figure A8: LATE Using Constant Bandwidth




## 3.5 Placebo Regressions: Cut-point Moved by 1 Year

We argue that the cut-point of election day is meaningful and not randomly associated with changes in turnout. In Figure A9 we run placebo regressions in which we look for differences in 2016 turnout for block groups treated immediately before or after November 8, 2015, and in 2020 turnout for block groups treated before or after November 3, 2019. As expected, these placebo regressions return unpatterned, null results. The regression estimates for this placebo test can be found in Table B25.

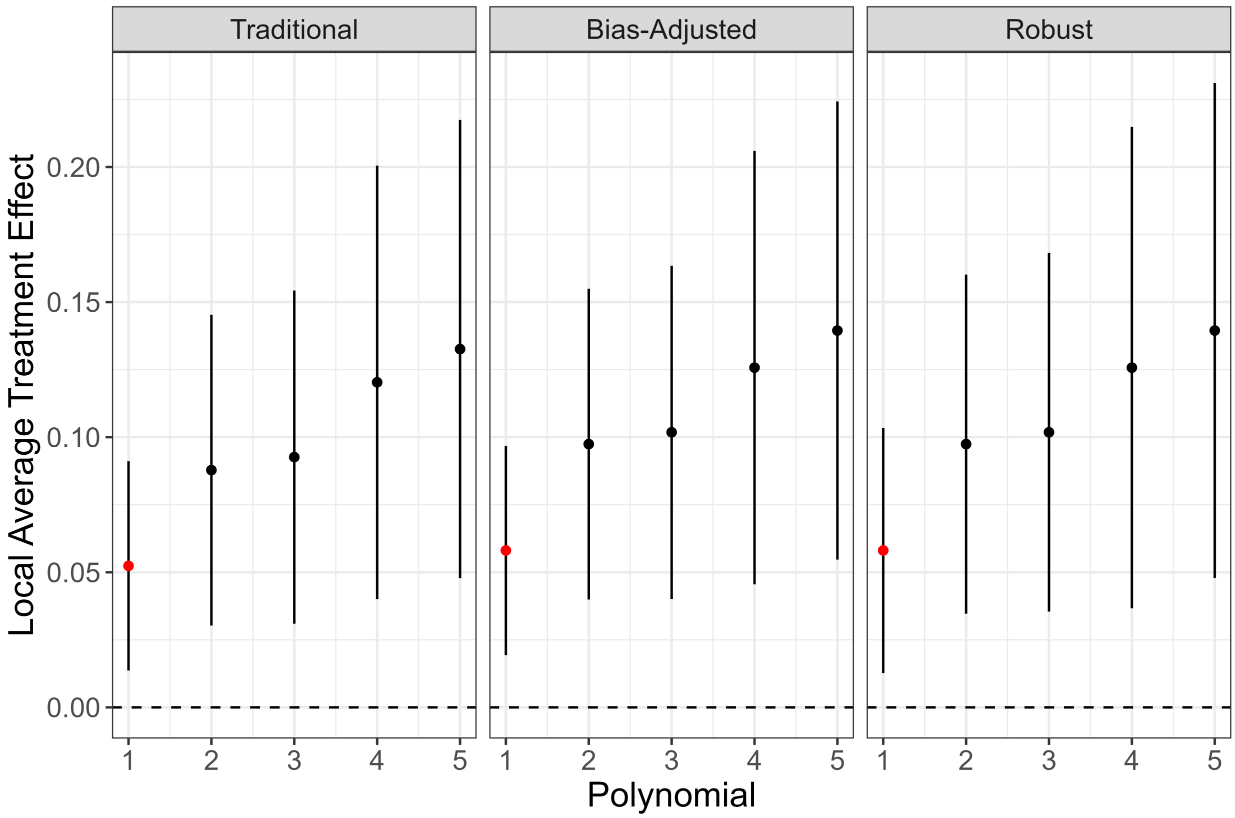
Figure A9: Placebo: Cut-point Set Year Before Election Day




### 3.6 Alternative Polynomials for Regression Discontinuity

Throughout the body of the manuscript, we use a local polynomial of 1, following best practice. In Figure A10 we show that our primary results for the 0.3-mile threshold are consistent when we use a local polynomial anywhere between 1 and 5. Again, our choice of a local polynomial of 1 appears to be, if anything, a conservative approach. The regression estimates using different polynomials can be found in Table B26.

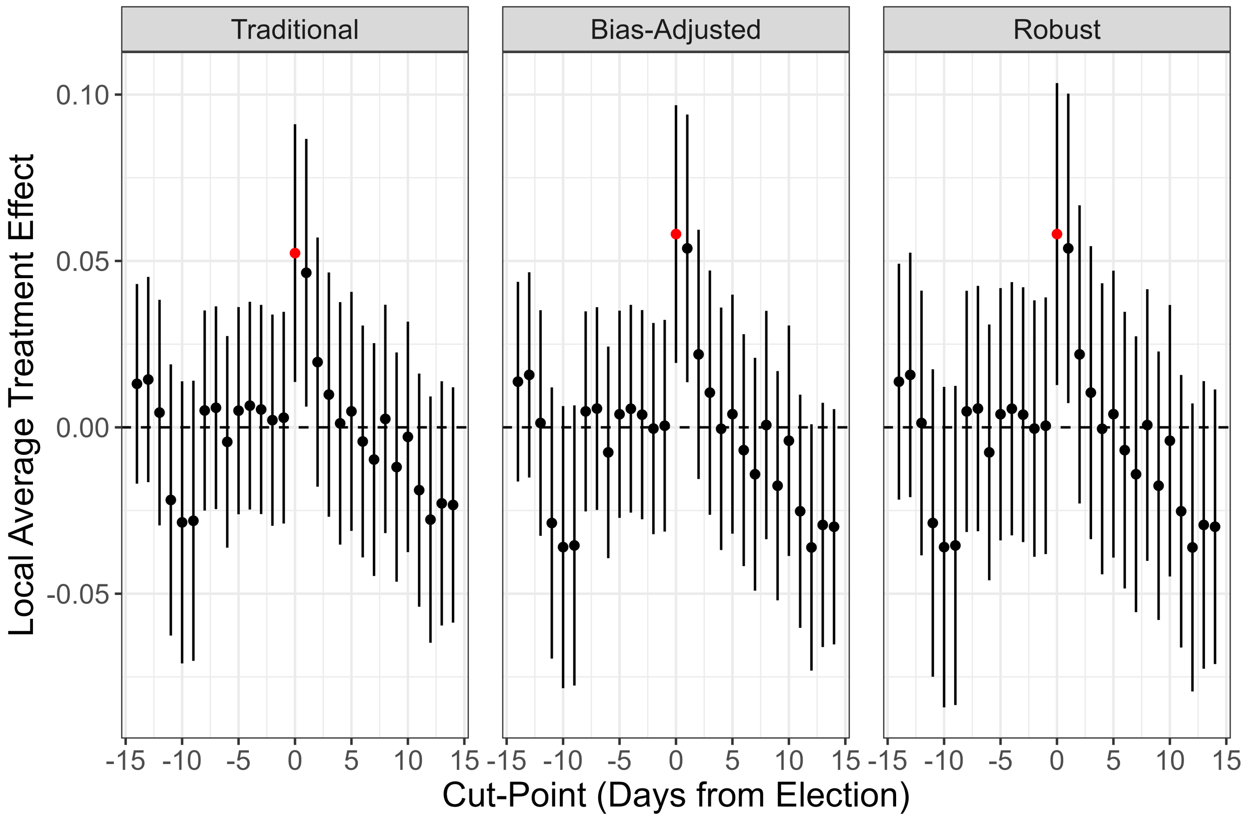
Figure A10: LATE Using Different Polynomials




## 3.7 Alternative Cut-Points for Regression Discontinuity (Placebo)

Figure A11 shows that election day is a meaningful cut-point and that, as expected, other cut-points before and after election day do not map on to differences in turnout. This plot uses the 0.3-mile threshold. The regression estimates using these placebo cut-points can be found in Table B27.

Figure A11: Placebo Alternate Cut-Points

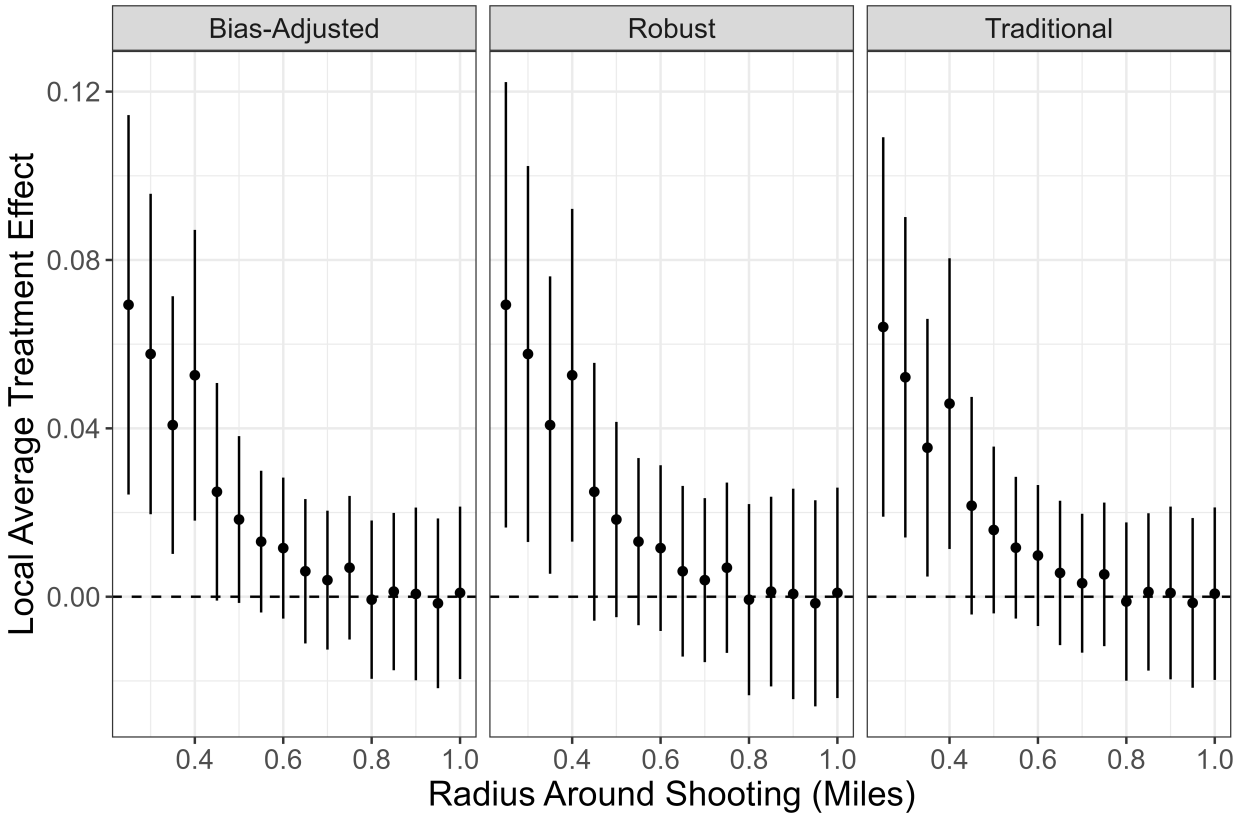



## 3.8 First-Difference in Turnout

We here present another set of robustness RDiTs in which we change our dependent variable from *turnout* in 2016 and 2020 to *change* in turnout in from the previous midterm election. This means that the dependent variable for treated and control block groups in 2016 is the first difference between 2014 and 2016 turnout; for those in 2020, it is the first difference between the 2018 midterms and 2020 presidential election.

In Figure A12 we present the estimated coefficients from the regression discontinuity models using this first-differencing approach. These models are identical to those presented in the body of the manuscript, except turnout from the preceding midterm is excluded from the entropy balancing process and the OLS adjustments. The estimated local average treatment effects are virtually identical to those presented in the main body of this manuscript, lending credence to our claims regarding the causal effect of police killings on turnout at the local level. The regression estimates for the models using first-difference of turnout can be found in Table B28.

Figure A12: LATEs Using First Difference of Turnout




# 4 Discussion of Trending Killings

In the body of the manuscript we show that killings that "trended" in Google searches were mobilizing, while those that did not trend did not meaningfully impact turnout.

Here we discuss in greater detail the methodology used to determine whether a killing got attention, as proxied by data from Google Trends. In early 2022, Google Trends did not allow users to test the popularity of a given search term relative to other terms, an approach used by Burch (2021). Instead, Google Trends determines how frequently a given term is searched across a period of time. The day on which a term is searched the most is given a value of 100; other days are given value relative to that. To use an example: if we look at the term "political science" over the period from April 1–5, 2022, the values are 68, 67, 74, 80, and 100. In other words, "political science" was searched most frequently in the US on April 5 (though we do not know *how many times* it was searched) and the least on April 2. On April 2, "political science" was searched only 67% as frequently as on April 5. If a term receives very few searches, it receives a value of 0 on all days. "Flamingos love pizza" is one such example. Unfortunately, Google Trends does not allow us to track the relative popularity of a search term over time at a lower geographical level than the entire United States.

To test whether a given killing "trended" on Google, we measure the change in the frequency of searches for the victim's name. Specifically, we consider a name to be trending if the mean relative frequency of searches on the day of the killing and 2 days following are more than double the frequency in the two days prior to the killing. A name that thus had a frequency of 20 and 25 in the two days prior to the killing, and 80, 100, and 90 on the day of the killing and two subsequent days would be considered trending. If, on the other hand, these values were 45, 50, 100, 70, and 70, the name would not be considered trending.

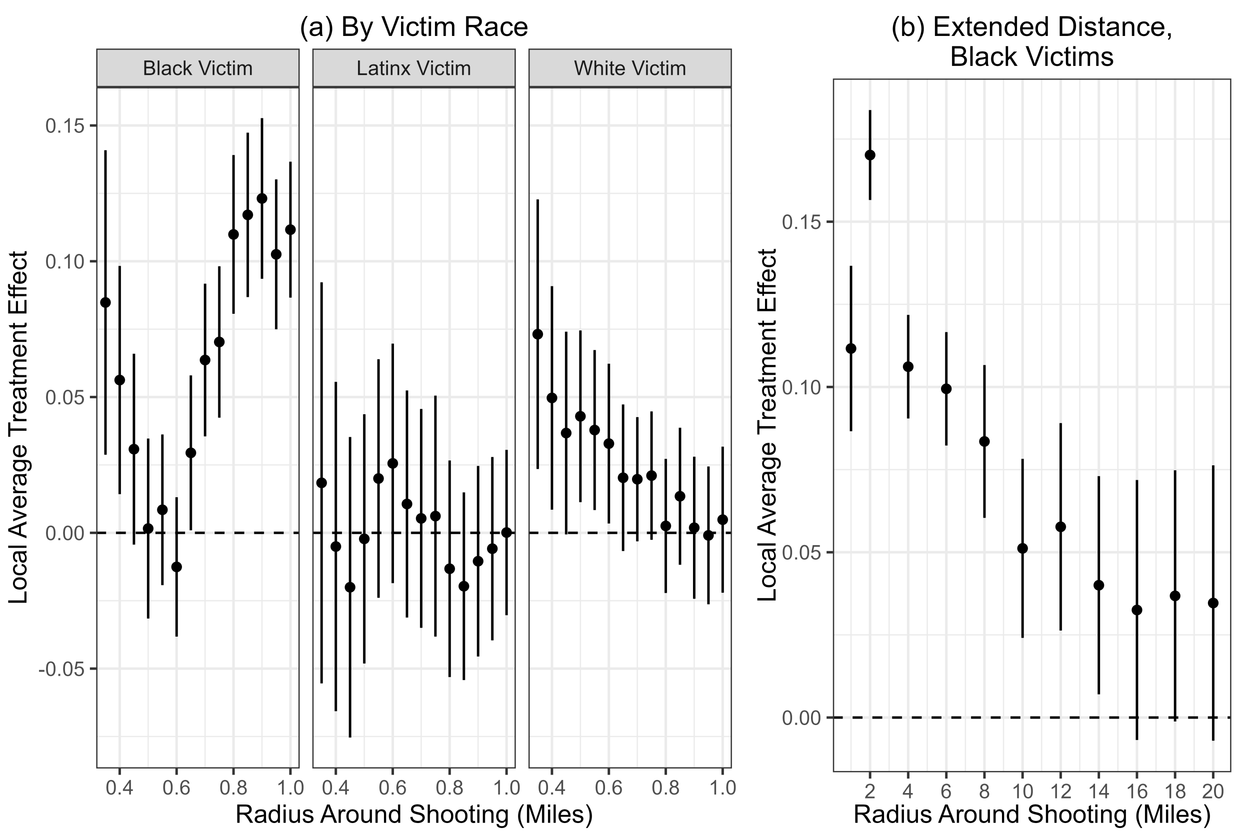
Approximately 4% of records are missing names and thus cannot be included in this analysis. Of the remaining, 48% are considered trending. It is worth noting that the threshold for determining trending is not particularly important here. If instead we consider killings trending only if the average relative frequency increases by four-fold, the share trending drops only to 45%. The 40% of victim names who received a 0 before and after the killing are considered not-trending. Some 70% of the victim names that were searched frequently enough to have a non-zero value following the killing were not searched in the days prior to the killing.

# 5 Modelled Race for Victims with Unreported Race

There is some reason to believe that the victims whose race is unknown are non-random: it seems probable that the victim's race would be reported at a higher rate for high-profile killings—the sorts of killings most likely to spur turnout.

Here, we replicate Panels c and d of Figure 5 from the main body of the manuscript, where the race of victims with unreported races are estimated using the `rethnicty` package (Xie, 2022). For each victim, we assign the most probable race, using first and last names alike. Our results do not change. The regression estimates for this approach can be found in Table B10.

Figure A13: LATEs Including Modelled Race Victims




# 6 Minneapolis Ballot Initiative Regression Table

Table A2: Support for Abolishing Minneapolis Police Department

| | Share of Precinct Supporting Abolition | | |
|---|---|---|---|
| | Model 1 | Model 2 | Model 3 |
| Distance to Closest Police Killing | -0.068* | -0.059** | -0.026* |
| | (0.022) | (0.016) | (0.010) |
| Logged Number of Police Stops in 2021 | | 0.032 | -0.028 |
| | | (0.048) | (0.027) |
| Logged Number of Crimes in 2021 | | -0.017 | 0.035 |
| | | (0.046) | (0.028) |
| Pct. Non-Hispanic Black | | | -0.173 |
| | | | (0.250) |
| Pct. Non-Hispanic White | | | 0.421 |
| | | | (0.289) |
| Pct. Latinx | | | 0.535 |
| | | | (0.304) |
| Pct. Asian | | | 0.228 |
| | | | (0.233) |
| Median Income ($10,000s) | | | -0.031*** |
| | | | (0.003) |
| Pct. with Some College | | | 0.097 |
| | | | (0.045) |
| Median Age | | | -0.010*** |
| | | | (0.001) |
| Logged Population Density | | | -0.040** |
| | | | (0.012) |
| Biden Voteshare in 2020 | | | 0.832*** |
| | | | (0.155) |
| Intercept | 0.544*** | 0.492** | 0.255 |
| | (0.032) | (0.131) | (0.229) |
| Num.Obs. | 134 | 134 | 134 |
| R2 | 0.161 | 0.175 | 0.774 |
| R2 Adj. | 0.154 | 0.156 | 0.752 |
| RMSE | 0.13 | 0.12 | 0.06 |

Standard errors clustered by nearest killing.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

# References

Burch, Traci. 2021. "Not All Black Lives Matter: Officer-Involved Deaths and the Role of Victim Characteristics in Shaping Political Interest and Voter Turnout." *Perspectives on Politics* .

Kim, Seo-young Silvia and Bernard Fraga. 2022. "When Do Voter Files Accurately Measure Turnout? How Transitory Voter File Snapshots Impact Research and Representation." *Working Paper* .

Xie, Fangzhou. 2022. "Rethnicity: An R Package for Predicting Ethnicity from Names." *SoftwareX* 17:100965.