Local Prosecutors and Public Health*

Panka Bencsik
Vanderbilt University

Tyler Giles†
Wellesley College

June 3, 2025

Abstract

This paper investigates the causal connections between approaches taken by local criminal prosecutors and community-level mortality rates. We exploit plausibly exogenous variation in prosecutorial approaches generated by closely contested partisan elections. Using a vote share regression discontinuity design, we find that narrow election of a Republican prosecutor reduces all-cause mortality rates among young adults aged 20 to 29 by 5.6 percent, driven by reductions in external causes like homicide, suicide, and motor vehicle accidents. These effects are concentrated almost entirely among young Black adults. In exploring mechanisms, we show that narrow election of a Republican prosecutor is associated with a substantial increase in new prison admissions among Black Americans but no other demographic group, suggesting incarceration-induced incapacitation of potential victims and perpetrators to be a key channel. These findings highlight the elevated dangers faced by this demographic group and the opportunities for cost-effective policies to reduce mortality risk in these communities.

*We thank Clara Chambers and Jansen Hammock for their excellent research assistance. We thank Victoria Barone and Kyung Park for their exceedingly helpful feedback. We thank Ashna Arora for many fruitful conversations at various stages of this project, and for sharing election data, and thank Allison Stashko for sharing election data as well. Further, we thank seminar participants at Wellesley College and conference participants at the American Society of Health Economists (ASHEcon) for their invaluable feedback.

†Bencsik: Vanderbilt University, 2201 West End Ave, Nashville, TN 37235. (email: panka.bencsik@vanderbilt.edu); Giles: Wellesley College .

The U.S. criminal justice system involves a substantial degree of discretion; many agents (e.g., police offers, judges, parole boards) possess leeway to take significantly different approaches to their roles, even as codified policies are kept fixed. An extensive body of research shows that discretion-induced variation in individual experiences with the criminal justice system (e.g., whether one happens to face a lenient or strict judge) can lead to markedly different outcomes in areas such as employment, recidivism, and physical health.[1] Exposure to the criminal justice system, both direct and indirect, is also extensive - the average American is just as likely to possess a criminal record as a college degree (Friedman, 2015), and 39% of children grow up in households where someone has faced criminal charges (Finlay et al., 2023). This then raises a natural question: if discretionary approaches at just one level of the system were varied consistently within an entire community, could the effects be observed in aggregate, community-level data?

This paper seeks to quantify both the potence of discretion in the criminal justice system and the extent to which changes at just one of its levels can impact important community-wide outcomes. We explore this question with a focus on health outcomes, and mortality in particular, as many of the most pressing public health issues in the U.S. are deeply intertwined with the criminal justice system. Homicide has consistently ranked among the top three causes of death for young Americans for decades (David-Ferdon et al., 2013), and the U.S. Surgeon General recently declared gun violence to be an official public health crisis (Office of the Surgeon General, 2024). In addition, the opioid crisis, now driven by illicit narcotics like heroin and fentanyl, remains a major public health emergency (McGreal, 2018).[2] Several other common causes of death also tie in with criminal justice; for example, drunk-driving accidents claim a life every 39 minutes (NHSA, 2022), and suicide mortality risk is disproportionately high among the incarcerated both during (Carson, 2021) and after

[1]For examples, see Dobbie et al. (2018); Mueller-Smith (2015); Kling (2006); Agan et al. (2023); Leslie and Pope (2017).

[2]The Covid-19 pandemic only exacerbated these crises - both homicide and overdose mortality spiked in 2020 and neither has yet to fully return to its pre-pandemic level (Centers for Disease Control and Prevention, 2024, 2025).

(Fitch et al., 2024) incarceration.

We focus on variation in community-wide criminal justice practice driven by variation in the approaches taken by local prosecutors. Also called district attorneys, local prosecutors are public officials tasked with representing the state in criminal proceedings. With hundreds of thousands of cases being handled by prosecutors across the country in any given year, prosecutors and their approaches impact a substantial share of Americans directly, and even more indirectly.[3] Prosecutors also have substantial discretion in their decision-making, leading some legal scholars to conclude that local prosecutors are "the criminal justice system's real lawmakers" (Stuntz, 2001).

Our identification strategy leverages plausibly exogenous variation in prosecutorial approaches generated by closely contested partisan local prosecutor elections. A majority of states elect prosecutors in partisan local elections; in these contexts, Republican-affiliated prosecutors are typically more "aggressive" than their Democratic counterparts, pursuing more convictions (Fischer and Ludwig, 2023; Yntiso, 2022) and increasing incarceration rates in their communities (Arora, 2019; Krumholz, 2020). We use a vote share regression discontinuity design (RDD) with the Republican prosecutorial candidate's vote share as the running variable to identify the causal effects of a change in prosecutorial approaches. Intuitively, this approach assumes that the observed post-election mortality rates in counties which narrowly fail to elect a Republican candidate are a reasonable counterfactual for districts which narrowly do elect one.

Our key result, robust to a number of analysis choices and placebo tests, suggests that narrow election of a Republican local prosecutor is associated with a reduction in all-cause mortality among the most criminal justice system-involved adult age demographic: young adults ages 20 to 29 years old. This reduction is meaningful (5.8 fewer deaths per 100,000, or a 5.6% reduction) and is driven by a decline in deaths from external causes like homicide,

[3]For example, U.S. District Courts alone handle 72,000 cases annually, while large counties like Cook County (Chicago) or Suffolk County (Boston) handle 25,000-33,000 cases each (Gramlich, 2023; Cook County State's Attorney, 2018; Becker, 2022).

suicide, and motor vehicle accidents. A finding among only this age group conforms with the well-documented fact that criminal activity is highest among people in the 20s and declines quickly afterwards (Rocque et al., 2015).

Heterogeneity analysis reveals that the observed reductions in mortality among young adults is concentrated almost entirely among Black Americans. In exploring potential mechanisms, we leverage county-level data on incarceration and examine how prosecutorial affiliation affects annual admissions to prisons and local jails among different demographic groups. We find that narrow election of a Republican prosecutor leads to a substantial increase in new admissions to prison among Black Americans but among no other demographic group. These incarceration patterns align with our mortality estimates, suggesting that incapacitation through incarceration explains a substantial portion of our findings. We also find no effects on admissions to local jails, suggesting little change in policing behavior or general criminal deterrence effect.

In addition to the patterns observed for young adults, we find that for age groups 30 years and older—who are at substantially higher risk of dying of an opioid overdose than young adults are—the narrow election of a Republican prosecutor leads to a reduction in heroin overdose mortality and an increase in prescription opioid overdose mortality, the two drug types effectively offsetting each other.

We also take several steps to valid our results. A natural concern with this research design is that narrow election of a Republican prosecutor might be correlated with the party affiliation of other local office holders; in particular, sheriffs. We show that, in districts with contested partisan prosecutor and sheriff elections occurring in the same year, the vote shares for the Republican prosecutor and sheriff candidates are positively but only weakly correlated (correlation coefficient = 0.284). More directly, we find no statistically significant association between narrow election of a Republican prosecutor and the party affiliation of the sheriff in office.

The results of a number of other empirical exercises validate our research strategy as

well. We show that geographic and baseline county-level 2010 demographic characteristics are balanced across the vote share threshold. We also conduct placebo exercises using pre-election mortality rates and find no effects of the vote share discontinuity on mortality before it should conceivably matter. We also find no effects on causes of mortality that should not reasonably be affected by local prosecutors, such as that of internal causes (e.g., cancer, etc). Lastly, our key results are robust to a number of slight changes to the estimation: dropping the counties with the largest regression weights from the analysis, and varying the bandwidth used in estimation, among others.

This paper contributes to several strands of literature, the first of which examines how discretion shapes outcomes for individuals involved in the criminal justice system. Many studies exploit quasi-random variation in the baseline leniency of assigned agents (typically judges) to estimate downstream consequences for those individuals and, in some cases, their household members. However, these studies face inherent limitations when making broader claims about (1) what would happen if everyone were simultaneously assigned to a stricter agent, and (2) how such discretion might generate spillover effects at the community level.

The latter is particularly important in the context of health outcomes and mortality. To illustrate, the arrest and prosecution of a narcotics distributor may have wide-ranging effects on community health beyond the consequences for the distributor themselves (Ray et al., 2023). Similarly, assaults have direct and serious health consequences for victims, and previous research shows that individuals on the margin of gun violence involvement face high risks of both perpetration and victimization.[4] Hence, studies that focus solely on the health consequences for perpetrators likely miss out on important effects on potential victims.

We most directly complement recent papers that estimate the direct health effects of incarceration on the incarcerated and find that it reduces mortality risk, largely through incapacitation-based reductions in external causes like homicide, overdose, and suicide (Norris et al., 2022; Hjalmarsson and Lindquist, 2022). Our findings are generally compatible—

[4]Bhatt et al. (2024) find that among Chicago men identified by machine learning or referrals as high-risk, approximately 11% became victims of gun violence, while 5% became perpetrators of serious violent crime.

mortality primarily falls among the demographic group (Black Americans) that experiences increased incarceration when a more aggressive local prosecutor is narrowly elected, and we see declines in these same causes of death. However, a simple back-of-the-envelope calculation suggests that our mortality estimates are unlikely to be driven solely by the direct effect of lowered mortality risk among the incarcerated themselves. This suggests that either incarceration reduces community-level mortality beyond its direct effects or that mechanisms other than incarceration associated with more aggressive prosecutors reduce mortality; the lack of mortality effects among groups without an increase in incarceration suggests it is mainly the former.

This paper also contributes to the literature on the opioid crisis and its intersection with the criminal justice system. We find no evidence that local prosecutorial affiliation is associated with either increases or decreases in overall drug overdose mortality, suggesting that more aggressive prosecution is not an effective lever to combat the crisis.[5] If anything, our findings likely point to substitution between misuse of prescription and illicit opioids. This phenomenon has been documented in previous work on the supply-side aspects of the crisis; in particular, the abuse-resistant reformulation of OxyContin led to substitutions to heroin, and each OxyContin overdose death prevented by reformulation was replaced with one from heroin.[6] Our opioid mortality results suggest a similar story, although in the opposite direction—efforts to reduce the supply of illicit opioids or the potential laxer justice system environment for licit ones through prosecution may lead people to substitute to misuse of licit ones.

Lastly, we contribute to a growing collection of papers on local prosecutors. Several previous papers document a first stage relationship between local prosecutor party affiliation and prosecutorial approaches: Republican prosecutors dismiss fewer cases (Fischer and Ludwig,

[5]Norris et al. (2022) find that incarceration reduces drug overdose mortality risk, which may at first seem to conflict with our null findings. However, the findings are reasonably compatible—we only find an increase in incarceration among Black Americans, and Black Americans had significantly lower baseline overdose risk throughout much of the 2010s than White Americans.

[6]See Alpert et al. (2018); Evans et al. (2019).

2023) and increase incarceration rates (Arora, 2019; Krumholz, 2020), especially for Black defendants (Yntiso, 2022). Several of these papers (Krumholz, 2020; Fischer and Ludwig, 2023) along with Agan et al. (2021) also investigate the relationship between prosecutorial approaches and crime rates and find no clear relationship.

We go beyond the current literature on prosecutors by characterizing the consequences of prosecutorial approaches for community outcomes. Stashko and Garro (2021) show that prosecutor turnover (regardless of party) decreases deaths caused by police officers; beyond this, we know of no causal evidence linking local prosecutorial approaches to non-criminal justice outcomes at the community level. As with other work on criminal justice and health, our estimates should not be considered as a full characterization of the community welfare effects of prosecutorial approaches.[7] More work on other outcomes is needed to assess the full consequences on communities of different prosecutorial approaches, but our results demonstrate that the office of local prosecutor affects high-level outcomes like health and mortality. Our findings also highlight the elevated dangers faced by young Black Americans and the need for other more cost-effective policies to reduce mortality risk in their communities.

The rest of this paper is organized as follows. In Section I, we describe the institutional details surrounding local prosecutors and prosecutor elections in the United States, as well as the data used in our analysis. Section II lays out our empirical strategy and regression specifications. Section III presents our main results, findings on heterogeneity, and robustness and placebo checks. Section IV then investigates potential mechanisms, and Section V offers conclusions from this research.

[7] A simple example to illustrate this very important point: a policy that incarcerates *all* young men would virtually eliminate community gun violence through incapacitation, but such a policy would be socially devastating.

I Institutional Background and Data

I.1 The Role of Local Prosecutors

In the United States, local prosecutors, sometimes also called district attorneys (DAs) or county attorneys, are public officials tasked with representing the state in criminal judicial proceedings. Local prosecutors are - as the name suggests - *local* officials who serve districts comprised of either a single county or a collection of counties.[8] Local prosecutors lead offices consisting of tens to hundreds of employees, and the attorneys hired to work under a local prosecutor are often called "assistant district attorneys," or ADAs. Most prosecution decisions are delegated to ADAs, especially in larger districts, with at least high-level direction from the local prosecutor. Local prosecutors make ADA hiring decisions, and qualitative evidence suggests there is significant ADA turnover when a new local prosecutor takes office (Tziperman and Toralva, 2018; Palmer, 2019).

Prosecution of criminal cases proceeds in several stages, and the local prosecutor's office has substantial discretion at each one. First, the prosecutor's office must decide whether and which criminal charges to file in response to police reports and criminal complaints. The extensive margin decision of whether to prosecute at all gives prosecutors massive discretion over the experiences and outcomes for those entering the justice system. While this choice is often delegated to ADAs among whom the propensity to prosecute varies (Agan et al., 2023), local prosecutors do exert a top down influence on this margin above and beyond their choice of ADAs.[9] Discretion over the extensive margin decision to prosecute lower-level offenses in particular has recently generated national discussions, questionings of the constitutional role of local prosecutors, and efforts to remove them from office or circumvent their decisions (Hessick and Su, 2023). The intensive margin choice of the number, type, and severity of

[8]The official semantics used to describe local prosecutors vary across states and occasionally within states; for example, Kansas has 105 total prosecutorial districts, ninety-nine of which are served by "county attorneys" and six of which are served by "district attorneys." However, there are no material differences between these two positions in Kansas.

[9]For example, the former district attorney of Suffolk County, MA, provided a list of offenses she intended to decline to prosecute when running for office (Cramer, 2018).

initial charges levied is also left to the prosecutor.

Conditional on the decision to prosecute, prosecutors next have input on bail and pre-trial detention decisions. These decisions are ultimately made by judges, but prosecutors typically make recommendations, and evidence suggests these recommendations are often followed (Coalition to End Money Bond, 2017) or are at minimum influential (Miller, 2013). The bulk of the work and influence of the prosecutor then occurs during plea bargaining. Finally, conditional on guilt, prosecutors usually make sentencing recommendations to the judge. These recommendations are typically agreed upon by the defense and prosecution as part of the plea bargaining process.

I.2 Local Prosecutor Elections and Data

Local prosecutors are overwhelmingly chosen in local elections. In forty-five states, local prosecutors are elected; out of these, thirty-eight states elect prosecutors in partisan elections and seven states elect them in non-partisan ones.[10] This paper focuses on states with partisan prosecutor elections. The overall differences in approaches across the political isle are often captured by both parties as characterizing Republican-affiliated prosecutors as more aggressive and "tougher on crime" (Gambino and Greve, 2022; Sears, 2023).

Table A1 conveys the number of districts, term lengths, and election timings for all thirty-eight states with partisan local prosecutor elections. Term lengths for local prosecutors vary across states, but the vast majority are four years. Table A1 also shows the timing of when elections occur relative to national presidential and midterm elections: "presidential" and "midterm" states are ones where local prosecutor elections consistently coincide with each national election, respectively, "staggered" states are ones with term lengths different than four years and thus swap between presidential and midterm years, and "mixed" states are ones where the timing of local prosecutor elections varies across districts within the state.

[10]Local prosecutors are appointed by the state's Attorney General, a state commission, or the Governor in Alaska, Connecticut, Delaware, and New Jersey. All criminal cases are handled by the Attorney General in Rhode Island.

Overall, there is a mix of election timings.

Our data on local prosecutor elections cover thirty-four of the thirty-eight states with partisan prosecutor elections for ten years, from 2010 to 2019. Our analysis is done using county-by-year observations, and our key variable is the Republican candidate vote share in the most recent local prosecutor election in the given county and year. To illustrate this variable's construction, consider a district which holds an election in the fall of 2018 where a Democratic and a Republican candidate face one another in the general election. If the Republican receives 600 votes and the Democrat receives 400 votes, then starting in 2019, our vote share variable for counties in this district will be 600/(400+600) = 0.6.[11] When a candidate runs unopposed in the general election, the vote share variable will be coded as 0 (unopposed Democrat) or 1 (unopposed Republican).[12]

Practically, the only county-by-year observations that will be used in our analysis are ones where the vote share in the most recent election is reasonably close to the 50 percent vote share cutoff, and our data-driven bandwidth selection methods will generally choose bandwidths on either side of about 10 percentage points (i.e., 40 percent to 60 percent). Figure 1 shows a map of the United States and highlights counties that enter our sample at some point in our ten year period. In-sample counties are dispersed both across the geographic United States and within states, with in-sample counties being drawn from all major geographical regions and containing a range of county sizes.

Table 1 presents summary statistics from 2010 for counties that possess a Republican

[11]Elections overwhelmingly take place at the end of the year in November, so we allow changes to the vote share variable within a district to take effect in the following year.

[12]Some districts do not require unopposed general election candidates to appear on the election ballot. If not careful, this phenomenon could lead to misleading values for our vote share variable. To illustrate, suppose a Republican candidate wins a closely contested election with 55% of the vote in 2012 and runs for reelection uncontested and without appearing on the ballot in 2016. In this case, we code the 2017 Republican vote share variable as "1" because the Republican candidate essentially won the election unopposed, even though the election didn't technically end up taking place. This subtlety can make it difficult to be certain of the party affiliation of the winning candidate when they run unopposed; however, this is not a problem for our research design because we only need to identify party affiliation in closely contested elections. In rare cases, the general election will contain candidates from more than two parties (e.g., a Republican, Democratic, and Independent candidate); in these cases, Republican vote share is the vote share relative to either the second place party (if the Republican won) or winning party (if the Republican lost).

prosecutor vote share value between 0.4 and 0.6 in at least one year from 2010 to 2019 (Column 1) and for the rest of the United States (Column 2). We summarize a number of county-level characteristics obtained from the 2010 US Census: racial demographics, urban/rural population shares, unemployment and poverty rates, the fraction of residents without health insurance, and education levels. We also use data from the U.S. Drug Enforcement Administration (DEA)'s Automation of Reports and Consolidated Orders System (ARCOS) pill shipment database to summarize county-level opioid pill volume per capita.

Table 1 conveys that there are 584 counties that contribute one or more observations in our analysis. These counties reside within a total of 427 electoral districts and contain about 25.4% of the total U.S. population.[13] The first several rows of Table 1 show that in-sample counties had a higher fraction of White Americans than other counties (80.86 vs 77.9), but the difference is small in magnitude, and there are no differences in urban/rural population shares. Counties with closely contested DA elections also had a slightly lower baseline unemployment rate (9.1 vs 9.9), but similar poverty rates and fractions of individuals without health insurance. Education levels are similar in both sets of counties and any differences are statistically indistinguishable from zero at the 5% level. Lastly, in-sample counties received more shipments of opioid pills than the rest of the U.S., implying that the counties we study may be slightly more predisposed to the opioid crisis than the rest of the US. In summary, Table 1 conveys that the characteristics of in-sample counties are largely similar to the rest of the U.S. and that there should be reasonable external validity of any findings.

I.3 Mortality Data

Our mortality data come from the Centers for Disease Control and Prevention's (CDC's) Multiple Cause of Death files, which provide a census of all deaths in the United States from 2010 to 2019. We aggregate mortality counts to the county-by-year level and calculate

[13]Table A2 shows the twenty biggest counties within the sample by 2010 middle-aged population.

mortality rates per 100,000 individuals using population counts from the Surveillance, Epidemiology, and End Results (SEER) U.S. Population Data. We focus on mortality among adults ages 20 to 59 and, given that mortality risk and its sources vary across the age distribution, analyze mortality rates in ten-year age groupings (20–29, 30–39, etc.).[14]

The CDC data identify causes of death using the 10th revision of the International Classification of Diseases (ICD-10). Causes of death can first be partitioned into two broad categories: external causes due to outside forces or substances acting upon the body, and internal causes due to diseases or medical conditions. We focus primarily on mortality due to external causes. We further divide external causes into five categories: homicides, drug poisonings, non-poisoning suicides, motor vehicle accidents, and other external causes.

In addition, we examine opioid overdose mortality in detail. The term "opioid" conventionally refers to any drug or medication, licit or otherwise, that is derived from or mimics substances found in the opium poppy plant. Following conventions in the literature, we separately analyze deaths involving three categories of opioids. First is the "natural and semi-synthetic opioids" category, which essentially refers to prescription pain medications like oxycodone (e.g., OxyContin) and hydrocodone (e.g., Vicodin); hence, we refer to this category as "prescription opioids" in our results. The remaining two opioid categories are heroin and fentanyl.[15] Finally, because it is the main driver of homicides in the United States, we also specifically examine gun homicide mortality. A full list of the ICD-10 codes used to identify causes of death appears in the Appendix.

Several figures in the Appendix provide context for our later mortality results. Figures A1 and A2 depict mortality trends across our sample period and convey that the levels and trends in mortality in the counties with closely contested local prosecutor elections are quite

[14]These ten-year age group rates are age-adjusted; i.e., they are weighted averages of the two five-year age group rates they comprise and are calculated using a consistent set of weights across all counties and years. The weights are based on the U.S. age distribution in 2000. This adjustment prevents misleading comparisons of mortality rates across time and/or location that could arise purely from differing age distributions within the ten-year groups.

[15]Fentanyl is a synthetic opioid, meaning that while it acts on the same brain receptors, it is not naturally occurring.

similar to the U.S. as a whole - much more so than the counties not used in our analysis. Figure A3 visualizes opioid mortality trends by age and conveys several well-known facts about the crisis: it is predominately concentrated among the middle-aged and proceeded in stages—heroin mortality rises in the beginning of the 2010s and is then overshadowed by the explosion of fentanyl halfway through the decade.[16] Figure A3 also summarizes trends in homicide mortality—rates are highest for people in their twenties and ticked up in the middle of the decade but remained constant or fell slightly towards the end.

I.4 Prison and Jail Admission Data

Our data on prison and jail admissions comes from the Vera Institute for Justice's Incarceration Trends dataset, which uses several sources to calculate county-by-year correctional facility admission rates. The prison admission data covers up to 2016 and is derived by Vera from the National Corrections Reporting Program (NCRP) and—when NCRP data is unavailable or unreliable—data collected directly from state departments of correction. The Vera jail admission data covers up to 2018 and is constructed from three sources: the Annual Survey of Jails, the Census of Jails, and the Death in Custody Reporting Program. Our main mortality estimates use data from 2010 to 2019, so the prison and jail data cover three and one fewer years, respectively.

Our variables of interest are county-level annual jail admission and prison admission rates per 100,000 residents ages 15 to 64 years old. Prison admission rates are available both by race and by gender, while jail admission rates are available by gender.[17] Overall prison admission rates are missing for about 10% of the county-by-year observations from 2010 to 2016 with a closely contested vote share value, and rates by race are missing for a more substantial portion (42%); jail admission data is missing for about 8% of these observations

[16] More specifically, the opioid crisis is known to be concentrated among white middle-aged Americans without a college degree (Case and Deaton, 2015, 2017). However, the most recent trends in opioid mortality are different—the crisis now spreading to younger and non-white Americans (Gibbons et al., 2023).

[17] Neither are available by age group, so we opt to estimate all-ages patterns with regards to changes in incarceration.

from 2010 to 2018. These data limitations should appropriately temper interpretations of these findings, but we note that recent ongoing work has documented a relationship between prosecutorial affiliation and incarceration rates (Arora, 2019; Yntiso, 2022; Krumholz, 2020; Fischer and Ludwig, 2023), and thus our goal with this data is more to verify this relationship in our data than to produce novel estimates along this dimension.

II Empirical Strategy

Straightforward comparisons of mortality rates between counties that do and do not possess a Republican local prosecutor are unlikely to capture the causal effect of criminal justice practices due to omitted variable bias. For example, counties with Republican prosecutors may be, on average, more rural or less likely to have expansive public health insurance, and these factors themselves affect health outcomes. To isolate the plausibly causal effect of a change in prosecutorial approaches, we use a vote share regression discontinuity design, an increasingly popular approach to uncover causal estimates.[18] We use Republican vote share as the running variable, but the results are essentially the same (with the sign reversed) if we use Democratic candidate vote share instead. This design assumes that the average potential mortality outcomes as a function of the running variable are continuous across the 50 percent vote share cutoff. Under this assumption, the observed post-election mortality rates in counties which narrowly fail to elect a Republican candidate provide a credible estimate of the counterfactual outcomes for districts which narrowly do elect one.

Our primary regression specification is given by Equation 1. The outcome variable, Y_{cdst}, is a mortality rate in county c in electoral district d in state s and year t. The running variable in this RDD is R_{dt}, which is the Republican vote share in the most recent election in electoral district d and year t. Here, $\mathbb{1}$ is an indicator function; hence, τ is the coefficient of interest which captures any discontinuous change in mortality rates across the 50 percent vote share threshold.

[18] See Lee (2008); Gamalerio (2020); Broockman (2014).

$$Y_{cdst} = \alpha + \tau \mathbb{1}[R_{dt} \geq 50] + \beta_1 R_{dt} + \beta_2 \mathbb{1}[R_{dt} \geq 50] R_{dt} + X_c' \beta_3 + \delta_s + \gamma_t + \epsilon_{cdst} \qquad (1)$$

Our preferred specification includes a number of covariates. X_c is a vector containing county-level characteristics, beginning with the 2010 characteristics summarized in Table 1: racial composition (the fractions of adults that identify Black, White, and Hispanic), urban/rural population shares, the unemployment and poverty rates, the fraction of adults without health insurance, education levels (the fractions of adults with less than a high school degree, only a high school degree, and at least some college education), and opioid pill shipment volume per capita.

We also include a collection of baseline county-level mortality rates in X_c. We use 2006 as the baseline year and control for six mortality rates: all-cause mortality and mortality from the five external causes listed in Section I.3.[19] δ_s and γ_t are sets of state and year fixed effects, respectively. We include these county characteristics and fixed effects in hope of explaining variation in mortality rates across counties and thus improving the precision of our estimates.[20]

Our preferred specification uses county-level population weights, both so that our estimate reflects an average effect equally weighted across individuals (not across locations of differing populations) and to improve precision (Solon et al., 2015). We cluster our standard errors at the district level. Bandwidths are chosen according to a data-driven process (Cattaneo et al., 2019) and we use a triangular kernel weighting function. For statistical inference, we report both conventional standard errors calculated using a second order robust plug-in residuals variance estimator and robust bias-corrected p-values (Calonico et al.,

[19] We choose 2006 as the baseline year because it is four years earlier than the first year in our analysis and thus can't pick up the effects of a Republican narrowly election in 2007 and who serves until at least 2010.

[20] Nearly all of the variation in Republican vote share comes from across district comparisons, so we are unable to include county or electoral district fixed effects without throwing out essentially all of the identifying variation.

2014). Though they are often quite similar, these robust bias-corrected p-values vary slightly from conventional ones and thus may imply a different level of statistical significance. Our key findings are precise under either approach.[21]

We perform a number tests to probe the validity of this empirical strategy throughout the paper. To begin, Figure A4 plots the empirical distribution of our vote share variable and reveals no clear discontinuity in the number of observations across the threshold, suggesting that the vote share variable is not being manipulated. Second, Table 2 estimates whether the 50% vote share cutoff is associated with discontinuous changes in any geographic or 2010 census demographic characteristics; we find none, which offers reassurance that the potential mortality outcomes of counties close to either side of the cutoff are similar.

We also conduct two simple empirical exercises to test whether our estimates are contaminated by the effect of electing a Republican sheriff. First, using data from the American Local Government Election (ALGE) database, we estimate the simple correlation between Republican prosecutorial vote share and Republican sheriff vote share in elections in which both were contested. This result is shown in Panel A of Figure A5. Unsurprisingly, vote share for the Republican prosecutorial candidate is positively correlated with vote share for the Republican sheriff; however, vote share for the prosecutor explains very little of the variation in vote share for the sheriff. In other words, even though Republican sheriff vote share tends to increase when Republican prosecutor vote share does, there is still very substantial dispersion in sheriff vote share at each level of prosecutor vote share close to cutoff. Second, we estimate an RDD using an indicator for whether Republican sheriff is in office in a given county and year as an outcome. This estimate is visualized in Panel B of Figure A5.[22] The estimate is negative and statistically insignificant, conveying that the party affiliation of sheriffs is not confounding our estimates.

[21]The estimation of the robust bias-corrected p-value involves using a higher order polynomial than is used to obtain the point estimate (in this case, quadratic fits instead of linear ones); hence, reporting both the conventional and robust bias-corrected p-values is also valuable as a check on the sensitivity to the polynomial order used.

[22]Here we use our prosecutor election data and merge in sheriff data from ALGE.

III Results

III.1 Main Results

Table 3 presents RDD estimates of the effect of narrow election of a Republican prosecutor on mortality by age group. Panel A examines effects on mortality from all causes, and Column (1) shows a precisely estimated reduction among adults ages 20 to 29 (here forward referred to as "young adults") of 5.8 fewer deaths per 100,000 people per year. This constitutes a 5.6% reduction from the baseline control mortality rate of 103.1 deaths per 100,000.[23] The Columns (2)-(4) estimates for the other age groups in Panel A are neither precisely estimated nor meaningful in magnitude relative to their baseline rates, suggesting no effect on mortality for anyone 30 years old or older.

Panels B and C of Table 3 partition causes of death into two broad categories—external and internal causes—and present separate estimates for each category. Column (1) of Panel B shows that the effect on all-cause mortality among young adults is driven entirely by reductions in external causes (7.05 fewer deaths per 100,000 people per year). Figure 2 visualizes the effects on all-cause and external cause mortality among young adults. The effect on internal cause mortality among young adults from Panel C is positive and relatively precise but much smaller in magnitude (1.14 additional deaths per 100,000); this is consistent with weak but non-zero competing risks between external and internal causes (Chiang et al., 1991). Columns (2)-(4) of Panels B and C show that the null effect on all-cause mortality among older age groups is not obscuring offsetting effects on these two mortality categories.[24]

Figure 3 explores the specific external causes of death that drive the reduction in mortality among young adults. There is no single external cause driving the results; instead, we see reductions of similar magnitudes for homicides, suicide, and motor vehicle accidents ($\approx$ 2 fewer deaths per 100,000 in each category). The largest estimated effect is on homicide

[23]This estimate and other ones among the 20-29 year old age group remain virtually unchanged when we broaden this age group to include 18-19 year olds as well.

[24]Figure A6 breaks internal causes of death into three categories (heart disease, cancer, and other diseases/conditions) and still finds no meaningful effects within these more specific categories.

mortality (2.2 fewer deaths per 100,000), and this effect is visualized in Panel B of Figure 3.[25]

Next, Figure 4 explores the connection between local prosecutorial affiliation and opioid overdose mortality. We find no effects of narrow election of a Republican prosecutor on overall opioid overdose mortality among any age group. However, among Americans ages 30 and up, these null effects likely obscure offsetting effects on prescription opioid overdose mortality (2 fewer deaths per 100,000) and heroin overdose mortality (2 additional deaths per 100,00). Figure A8 examines effects on fentanyl overdose mortality separately and also finds no effects among any age groups. Overall, Figure 4 suggests that more aggressive prosecution is not an effective lever to combat the opioid crisis.

Figure 5 investigates whether any subgroups in particular are driving our results among young adults. Panel A of Figure 5 visualizes separate estimates for Black and White Americans, and Panel B gives estimates by sex. These figures reveal that the reduction in mortality is driven by effects on Black Americans and men; all-cause mortality estimates for white and female Americans are statistically indistinguishable from zero. The heterogeneity is more pronounced along the race dimension; for example, the reduction in all-cause mortality for Black young adults (17.5 fewer deaths per 100,000) is about twice as big as the reduction for male young adults (9.4 fewer deaths per 100,000).[26] The effects on mortality among young Black adults is still driven by external causes, but homicide in particular is the biggest one (9.6 fewer deaths per 100,000).[27]

[25]Figure A7 examines effects on gun-involved mortality in particular and finds statistically significant reductions in all gun deaths, gun homicides, and gun suicides.

[26]See Table A5 for these estimates in more detail. Figure A10 explores effects by race-sex combinations and, as suggested by Figure 5, shows largest effects among Black men. Estimates among White men are marginally statistically insignificant, furthering the conclusion of racial heterogeneity. However, given the number regressions already estimated in this paper and concerns about multiple hypothesis testing, we are hesitant to interpret these estimates race-by-sex estimates other than as a confirmation that the effects are concentrated among Black Americans.

[27]One might also wonder whether there is informative heterogeneity in effects across time (e.g., before/after 2016) or geography (e.g., in historically Republican vs Democratic states). The nature of this research design and the limited frequency of closely contested elections makes investigations of heterogeneity along these dimensions difficult to interpret. Counties fall in/out of the sample when their most recent vote share value is/isn't close to the 50% threshold, so the sample of counties contributing observations in later years is not identical to the sample of counties contributing observations in earlier ones. Hence, it is challenging to infer whether any heterogeneity in effects is due to differing effects across time or the differing sample of counties. To illustrate, Figure A9 visualizes the samples of counties that contribute observations after 2016 only, in

III.2 Placebo/Robustness Exercises

In this section, we probe the credibility of our findings with several robustness and placebo tests. To start, Tables A3 and A4 present our preferred specification all-cause and external cause mortality estimates along with several specifications. Our preferred specification—which includes state and year fixed effects, baseline county-level demographics and mortality rates, and uses population weights - is shown in Column (6). Column (1) is estimated without any covariates, and Columns (2) and (3) add in state and year fixed effects, respectively. The incremental addition of covariates improves the precision of the estimates for 20-29 year olds but does not meaningfully affect their magnitude; in contrast, the estimates for other age groups remain statistically insignificant or attenuate further.

Column (4) of Tables A3 and A4 includes covariates but does not use population weights. The population weights substantially improve precision (in line with theory, see Solon et al. (2015)), and it is reassuring that magnitude of the estimate for 20-29 years olds remains relatively similar without the weights. Lastly, Column (5) uses our preferred specification but drops the four counties given the most weight in our analysis due to the population weights and triangular kernel weighting function. As shown in Figure A12, these four counties receive substantial weight in the estimation (both because of their population size and possession of vote share values very close to the 50% cutoff), and thus it is natural to wonder whether the findings are robust to their exclusion. Reassuringly, the estimates for 20-29 year old remain precisely estimated and of a similar magnitude with these counties excluded.

Next, Figure A11 investigates whether the findings are robust to different bandwidth choices. This figure conveys that our all-cause and external cause mortality estimates among young adults and young Black adults are not especially sensitive to deviations from the MSE optimal bandwidth choice.

We now turn to placebo checks. Our research design relies on the key assumption that the

or before 2016 only, and in both periods; only 36% of counties ever in the sample contribute at least one observation in both periods. Attempts to estimate heterogeneity along geographic dimensions face this same challenge but in reverse.

observed post-election mortality rates in counties which narrowly fail to elect a Republican candidate are a reasonable counterfactual for post-election rates in districts which narrowly do elect one. One natural idea for a placebo test is, for counties that have a closely contested election during our time frame, to assign their closely contested vote share value to county-by-year observations *prior* to the closely contested election (and thus before it should conceivably matter), then estimate the vote share RDD using these pre-election years.

Table A7 shows the results of this estimation. Column (1) restates our main estimates and Column (2) shows the corresponding pre-election placebo estimates. The placebo estimates are small in magnitude and statistically indistinguishable from zero, furthering the point that our empirical strategy is not picking up time invariant characteristics of counties but is instead capturing the time variant aspect of the local prosecutor election.

We also conduct a simple permutation exercise to verify that our empirical strategy is unlikely to produce an effect size similar to ours when vote share values are randomly permuted across districts. We begin with the collection of all districts with a vote share value within the bandwidths used in the main estimation at some point in our time frame. We then fix the yearly outcome and control variable values for these districts but randomly permute the observed vote share values across districts 1,000 times and plot the distribution of estimates produced by our preferred specification. The results are shown in Figure A13. Our estimates fall in the left tails of their distributions, especially the results for young Black adults, suggesting that our main findings would be unlikely to be obtained were the vote share values in reality orthogonal to our outcome.

IV Mechanisms

Next, we explore the mechanisms that might explain our mortality results. Local prosecutors predominantly influence the number and severity of criminal convictions in their communities, so one might first suppose that Republican prosecutors secure more convic-

tions resulting in prison sentences than the counterfactual non-Republican prosecutor. It is then reasonable to hypothesize that incarceration affects both the incarcerated person's own mortality risk (what we'll call the "direct channel") as well as the mortality risk of any would-be victims had the person not been incarcerated (an "indirect channel"). Since we find mortality effects essentially only among Black Americans, for this hypothesis to match our findings, Republican local prosecutors must disproportionately convict and incarcerate Black Americans as compared to the counterfactual.[28]

Table 4 presents RDD estimates of the effect of narrow Republican local prosecution election on annual county-level admissions to prisons. Column (1) reveals no effect on annual admissions to prison among all demographic groups; similarly, Columns (3) and (5) reveal no effect for either women or White Americans. Column (2) shows a precisely estimated increase in prison admissions among men of an additional 25.89 prison admissions per 100,000 men. Most strikingly, however, Column (4) suggests that narrow election of a Republican local prosecutor increases annual prison admissions among Black Americans aged 15 to 64 by 307.64 admissions per 100,000. Figure 6 visualizes these estimates.[29]

Could our mortality findings among young Black adults be explained solely by the direct channel effects of incarceration on mortality risk among the incarcerated themselves? We perform a simple back-of-the-envelope calculation to see whether this is reasonable. To begin, Column (4) from Table 4 suggests increased annual prison admissions among Black Americans aged 15 to 64 by 307.6 admissions per 100,000. This prison admission estimate should not be used as-is to scale our young adult mortality estimates given the differing age groups, but it might reasonably be adjusted as follows: about 40% of individuals issued felony sentences are aged 20 to 29 (Durose et al., 2009), and 20 to 29 year olds constitute about

[28]This must obviously hold under a strong direct channel, but also under an indirect one. Homicide mortality, the cause of death driving the effects among Black Americans, is concentrated in heavily racially segregated neighborhoods (Aufrichtig et al., 2017).

[29]Table A6 varies the specification choices to check for robustness in the same manner as Table 3. Figure A14 shows no effect on jail admissions; as admission to jail to largely decide by police, this null finding bolsters the idea that our empirical strategy is not picking up sheriff affiliation and that police behavior is not measurably responding to prosecutor behavior.

20% of the 15 to 64 year old age group; thus, the better estimate is likely $(307.6)(\frac{0.4}{0.2}) = 615.2$ additional annual prison admissions per 100,000 among young Black adults.

Next, this back-of-the-envelope calculation requires an assumption about the length of prison stays and thus the extent to which reductions in mortality risk generated by each new prison admission accumulate over time. The simplest approach assumes these additional prison admissions lead to short prison stays and that each prison admission only affects mortality in the year it occurs.[30] Then the assumption of effects solely on the incarcerated would suggest that prison admission reduces the likelihood of death in a given year by $\frac{17.5}{615.2}(100) = 2.84$ percentage points. The most relevant estimate from Norris et al. (2022) suggests that incarceration reduces the likelihood of death in a given year by 0.55 percentage points, which is much smaller than our estimate.[31]

Another approach would be to assume that each prison admission lasts several years and that mortality reductions accumulate over time. In this vein, consider a local prosecutor elected for a four year term, and that each prison admission included by a Republican prosecutor lasts at least four years. Note that those incarcerated in the first year of the term would have four years of exposure to incarceration across those four years, those incarcerated in the second would have three years of exposure, etc. Using the Norris et al. (2022) estimate within this exercise would suggest that the direct incapacitation effect should reduce cumulative mortality across all four years by $615.2(4+3+2+1)(0.0055) = 33.84$ deaths per 100,000. Our mortality point estimate suggests a cumulative $(4)(17.5) = 70$ fewer deaths across this four year term.[32]

Thus, regardless of the assumption about the length of these prison sentences, this exer-

[30]This might be a reasonable assumption given that these additional prison admissions are seemingly marginal ones that only occur if induced by election of a Republican local prosecutor.

[31]More precisely, see Column 7) of Table 4 in Norris et al. (2022)

[32]In line with this thinking, Figure A15 investigates whether the mortality effects of narrow election of a Republican prosecutor grow over time after the local prosecutor takes office. These estimates suggest this may be the case, especially for Black young adults. Note that we only include coefficients for three years after the election - the sample of counties contributing to each yearly estimate changes significantly moving from three to four years after the election because counties that contribute observations starting in 2017 (from a fall 2016 election) don't have a fourth year of post election data.

cise suggests that our findings are unlikely to be driven solely by the direct effect channel. Several aspects of this back-of-the-envelope calculation make the conclusions suggestive at best, but the result conveys that a significant fraction, but perhaps not all, of the reduction in mortality is due to the incapacitation effects of increased incarceration.

V Conclusion

This paper causally investigates how differing local prosecutorial approaches impact community-level mortality rates. We exploit plausibly exogenous variation in prosecutorial approaches generated by closely contested partisan elections and find that narrow election of a Republican prosecutor reduces all-cause mortality among young adults aged 20 to 29. This effect is concentrated among Black Americans, and our analysis of mechanisms using prison and jail data reveals that narrow election of a Republican prosecutor differentially increases prison admission rates for Black Americans, suggesting an incapacitation channel. We find no overall effects on opioid overdose mortality but some evidence of offsetting effects on prescription and illicit opioid overdoses. Our current findings motivate further investigation of the channels through which local prosecutors affect the well-being of their communities.

References

Amanda Agan, Jennifer L Doleac, and Anna Harvey. Prosecutorial reform and local crime rates. *Law & Economics Center at George Mason University Scalia Law School Research Paper Series*, (22-011), 2021.

Amanda Agan, Jennifer L Doleac, and Anna Harvey. Misdemeanor prosecution. *The Quarterly Journal of Economics*, 138(3):1453–1505, 2023.

Abby Alpert, David Powell, and Rosalie Liccardo Pacula. Supply-side drug policy in the presence of substitutes: Evidence from the introduction of abuse-deterrent opioids. *American Economic Journal: Economic Policy*, 10(4):1–35, 2018.

Ashna Arora. Too tough on crime? the impact of prosecutor politics on incarceration, 2019.

Aliza Aufrichtig, Lois Beckett, Jan Diehm, and Jamiles Lartey. Want to fix gun violence in america? go local. *The Guardian*, January 2017. URL `https://www.theguardian.com/us-news/ng-interactive/2017/jan/09/special-report-fixing-gun-violence-in-america`. Interactive feature.

Borah Becker. New Suffolk County DA says he'll focus on equity and fairness, no decision yet on seeking election. *WBUR*, January 2022. URL `https://www.wbur.org/news/2022/01/16/suffolk-county-da-kevin-hayden-interim`.

Monica P Bhatt, Sara B Heller, Max Kapustin, Marianne Bertrand, and Christopher Blattman. Predicting and Preventing Gun Violence: An Experimental Evaluation of READI Chicago*. *The Quarterly Journal of Economics*, 139(1):1–56, February 2024. ISSN 0033-5533. doi: 10.1093/qje/qjad031. URL `https://doi.org/10.1093/qje/qjad031`.

David E. Broockman. Do female politicians empower women to vote or run for office? A regression discontinuity approach. *Electoral Studies*, 34:190–204, June 2014. ISSN

0261-3794. doi: 10.1016/j.electstud.2013.10.002. URL https://www.sciencedirect.com/science/article/pii/S0261379413001583.

Sebastian Calonico, Matias D Cattaneo, and Rocio Titiunik. Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326, 2014.

E Ann Carson. Mortality in state and federal prisons, 2001–2018–statistical tables. *Bureau of Justice Statistics*, 2021.

Anne Case and Angus Deaton. Rising morbidity and mortality in midlife among white non-Hispanic Americans in the 21st century. *PNAS*, 112(49):15078–15083, 2015.

Anne Case and Angus Deaton. Mortality and morbidity in the 21st century. *Brookings Papers on Economic Activity*, art. 397-476, 2017.

Matias D Cattaneo, Nicolás Idrobo, and Rocío Titiunik. *A practical introduction to regression discontinuity designs: Foundations*. Cambridge University Press, 2019.

Centers for Disease Control and Prevention. Firearm homicide trends, 2024. URL https://www.cdc.gov/firearm-violence/php/data-trends/firearm-homicide-trends.html. Accessed: 2025-04-30.

Centers for Disease Control and Prevention. Vital statistics rapid release - drug overdose deaths. https://www.cdc.gov/nchs/nvss/vsrr/drug-overdose-data.htm, 2025. Accessed: 2025-04-30.

Chin Long Chiang et al. Competing risks in mortality analysis. *Annual review of public health*, 12(1):281–307, 1991.

Coalition to End Money Bond. Monitoring cook county's central bond court: A community courtwatching initiative, 2017. [Accessed 11-07-2024].

Cook County State's Attorney. 2017 Annual Data Report. Technical report, February 2018. URL https://www.cookcountystatesattorney.org/sites/default/files/files/documents/ccsao_2017_data_report_180220.pdf.

Maria Cramer. DA candidate Rachael Rollins hailed nationally, but locally her plan not to prosecute petty crimes alarms some - The Boston Globe. https://www.bostonglobe.com/metro/2018/09/12/candidate-hailed-nationally-but-locally-her-plan-not-prosecute-petty-crimes-alarms-so pyQX8MT4iWpyJ8XMi6SsFO/story.html, 2018. [Accessed 11-07-2024].

Corinne David-Ferdon, Linda L Dahlberg, and Scott R Kegler. Homicide rates among persons aged 10–24 years—united states, 1981–2010. *Morbidity and Mortality Weekly Report*, 62 (27):545, 2013.

Will Dobbie, Jacob Goldin, and Crystal S. Yang. The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges. *American Economic Review*, 108(2):201–240, February 2018. ISSN 0002-8282. doi: 10.1257/aer.20161503. URL https://www.aeaweb.org/articles?id=10.1257/aer.20161503.

Matthew R Durose, Donald Farole, and Sean P Rosenmerkel Jr. Felony sentences in state courts, 2006–statistical tables. *Bureau of Justice Statistics*, pages 1–45, 2009.

William N Evans, Ethan MJ Lieber, and Patrick Power. How the reformulation of oxycontin ignited the heroin epidemic. *Review of Economics and Statistics*, 101(1):1–15, 2019.

Keith Finlay, Michael Mueller-Smith, and Brittany Street. Children's indirect exposure to the u.s. justice system: Evidence from longitudinal links between survey and administrative data. *Quarterly Journal of Economics*, Forthcoming, 2023.

Brett Fischer and Tyler Ludwig. Politically charged: District attorney partisanship, dismissal rates, and recidivism, 2023.

Kate Vinita Fitch, Brian W Pence, David L Rosen, Vanessa E Miller, Bradley N Gaynes, Monica E Swilley-Martinez, Andrew L Kavee, Timothy S Carey, Scott K Proescholdbell, and Shabbar I Ranapurwala. Suicide mortality among formerly incarcerated people compared with the general population in north carolina, 2000–2020. *American journal of epidemiology*, 193(3):489–499, 2024.

Matthew Friedman. Just Facts: As Many Americans Have Criminal Records as College Diplomas. Technical report, Brennan Center for Justice, November 2015. URL https://www.brennancenter.org/our-work/analysis-opinion/just-facts-many-americans-have-criminal-records-college-diplomas.

Matteo Gamalerio. Do national political parties matter? Evidence from Italian municipalities. *European Journal of Political Economy*, 63:101862, June 2020. ISSN 0176-2680. doi: 10.1016/j.ejpoleco.2020.101862. URL https://www.sciencedirect.com/science/article/pii/S0176268020300100.

Lauren Gambino and Joan E. Greve. Democrats try to flip narrative amid barrage of 'soft on crime' attack ads. *The Guardian*, October 2022. ISSN 0261-3077. URL https://www.theguardian.com/us-news/2022/oct/27/republicans-crime-midterm-election.

Jason B Gibbons, Samantha J Harris, Keisha T Solomon, Olivia Sugarman, Carlos Hardy, and Brendan Saloner. Increasing overdose deaths among black americans: a review of the literature. *The Lancet Psychiatry*, 10(9):719–726, 2023.

John Gramlich. Fewer than 1% of federal criminal defendants were acquitted in 2022, June 2023. URL https://www.pewresearch.org/short-reads/2023/06/14/fewer-than-1-of-defendants-in-federal-criminal-cases-were-acquitted-in-2022/.

Carissa Byrne Hessick and Rick Su. The (local) prosecutor. *Wis. L. REv.*, page 1669, 2023.

Randi Hjalmarsson and Matthew J Lindquist. The health effects of prison. *American Economic Journal: Applied Economics*, 14(4):234–270, 2022.

Jeffrey R Kling. Incarceration length, employment, and earnings. *American Economic Review*, 96(3):863–876, 2006.

Sam Krumholz. The effect of district attorneys on local criminal justice outcomes, 2020.

David S. Lee. Randomized experiments from non-random selection in U.S. House elections. *Journal of Econometrics*, 142(2):675–697, February 2008. ISSN 0304-4076. doi: 10.1016/j.jeconom.2007.05.004. URL https://www.sciencedirect.com/science/article/pii/S0304407607001121.

Emily Leslie and Nolan G Pope. The unintended impact of pretrial detention on case outcomes: Evidence from new york city arraignments. *The Journal of Law and Economics*, 60(3):529–557, 2017.

Chris McGreal. *American overdose: The opioid tragedy in three acts*. PublicAffairs, 2018.

Colin Miller. Anchors away: why the anchoring effect suggests that judges should be able to participate in plea discussions. *BCL Rev.*, 54:1667, 2013.

Michael Mueller-Smith. The criminal and labor market impacts of incarceration. *Unpublished working paper*, 18, 2015.

Samuel Norris, Matthew Pecenco, and Jeffrey Weaver. The effect of incarceration on mortality. *Review of Economics and Statistics*, pages 1–45, 2022.

Office of the Surgeon General. Firearm violence: A public health crisis in america. https://www.hhs.gov/sites/default/files/firearm-violence-advisory.pdf, 2024. [Accessed 11-07-2024].

Chris Palmer. Larry Krasner's first year as Philly DA: Staff turnover, fewer cases, plenty of controversy. https://www.inquirer.com/news/larry-krasner-philadelphia-district-attorney-staff-reform-cases-first-year-20190106.html, 2019. [Accessed 11-07-2024].

Bradley Ray, Steven J Korzeniewski, George Mohler, Jennifer J Carroll, Brandon Del Pozo, Grant Victor, Philip Huynh, and Bethany J Hedden. Spatiotemporal analysis exploring the effect of law enforcement drug market disruptions on overdose, indianapolis, indiana, 2020–2021. *American journal of public health*, 113(7):750–758, 2023.

Michael Rocque, Chad Posick, and Justin Hoyle. Age and crime. *The encyclopedia of crime and punishment*, pages 1–8, 2015.

Bryan P. Sears. GOP calls on Democrats to join them in getting tough on crime legislation, November 2023. URL `https://marylandmatters.org/2023/11/15/gop-calls-on-democrats-to-join-them-in-getting-tough-on-crime-legislation/`.

Gary Solon, Steven J Haider, and Jeffrey M Wooldridge. What are we weighting for? *Journal of Human resources*, 50(2):301–316, 2015.

Allison Stashko and Haritz Garro. Prosecutor elections and police accountability, 2021.

William J Stuntz. The pathological politics of criminal law. *Mich. L. Rev.*, 100:505, 2001.

Gal Tziperman and Krista Toralva. Amid high turnover in first year, State Attorney Aramis Ayala says office is improving. `https://www.orlandosentinel.com/2018/02/11/amid-high-turnover-in-first-year-state-attorney-aramis-ayala-says-office-is-improving` 2018. [Accessed 11-07-2024].

Sidak Yntiso. Does prosecutor partisanship exacerbate the racial charging gap? evidence from district attorneys in three states, 2022.

Table 1: Comparison of Counties with Closely Contested Partisan Prosecutor Elections to the Rest of the United States, 2010 Characteristics

	(1) In Sample Counties	(2) Rest of U.S.	(3) Difference
% White	80.86	77.90	2.96**
% Black	12.34	12.89	-0.55
% Hispanic	16.13	16.92	-0.79
% Urban Population	75.89	71.67	4.22
% Rural Population	16.87	18.83	-1.96
Unemployment Rate	9.11	9.93	-0.82***
Poverty Rate	14.50	15.44	-0.94*
% Uninsured	17.79	17.65	0.14
Less than High School	13.32	14.71	-1.39*
High School	27.67	27.90	-0.23
Some College	29.37	28.77	0.60
Opioid Pill Volume/Capita	40.60	35.91	4.69**
% of Total U.S. Population	0.254	0.746	
N (Counties)	584	2548	
N (Districts)	427	1896	

Notes: This table presents summary statistics from 2010 separately for the counties that contribute to our regression samples and those that do not. Column (1) presents 2010 averages among the sample of counties that possess a Republican prosecutor vote share value between 0.4 and 0.6 in at least one year from 2010 to 2019. Column (2) presents 2010 averages for all remaining counties, and Column (3) presents the difference between the two averages. Averages are weighted by county population. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Balance in Baseline Characteristics Across the 50% Republican Prosecutor Vote Share Threshold

	(1) Narrow GOP Loss	(2) Narrow GOP Win	(3) Diff	(4) Robust BC p-Value	(5) BWs	(6) N
Geography						
Census Division 3	18.35	12.52	-5.83	0.613	(10.9,11.7)	2670
Census Division 5	18.37	7.01	-11.37	0.479	(10.5,10.8)	2609
Census Division 7	43.61	48.60	4.99	0.807	(12.9,12.9)	2874
Census Division 8	10.36	5.34	-5.02	0.220	(6.9,6.8)	1898
Other Census Division	9.06	10.19	1.13	0.710	(8.6,8.6)	2235
McCain State	55.99	63.43	7.44	0.732	(11.5,13.4)	2861
2010 County-Level Demographics						
% White	80.05	76.17	-3.89	0.171	(9.5,9.5)	2328
% Black	14.83	17.08	2.25	0.475	(10.2,9.0)	2337
% Hispanic	24.60	27.98	3.38	0.696	(11.8,12.8)	2780
% Urban Population	82.13	87.96	5.83	0.585	(11.0,10.6)	2619
% Rural Population	12.58	8.35	-4.23	0.586	(10.7,10.3)	2540
Unemployment Rate	9.08	9.31	0.23	0.931	(12.2,13.3)	2900
Poverty Rate	17.36	16.77	-0.59	0.604	(9.4,10.2)	2380
% Uninsured	22.56	23.57	1.01	0.813	(10.3,12.3)	2628
Less than High School	16.43	17.88	1.45	0.640	(11.6,9.7)	2527
High School	27.01	24.98	-2.03	0.273	(9.1,9.1)	2338
Some College	29.16	27.02	-2.14^{*}	0.087	(8.3,7.1)	1995
Opioid Pill Volumne/Capita	43.23	36.57	-6.65	0.263	(8.7,9.3)	2337

Notes: This table presents estimated effects of the 50% Republican prosecutor vote share cutoff on fixed or pre-determined state and county-level characteristics. Columns (1) and (2) provide the estimated intercepts to the left and right of the cutoff, respectively, from the same RDD regression, and Column (3) provides the RDD estimate itself. These regressions are estimated without covariates but using all other specification choices outlined in Section II. Column (4) provides the robust, bias-corrected p-value on the RDD estimate; Column (5) provides the bandwidths used to the left and right of the cutoff, and Column (6) provides the effective number of observations used in estimation. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Effects of Narrow Republican Prosecutor Election on Mortality

Panel A: All Causes

	(1)	(2)	(3)	(4)
	20-29 Year Olds	30-39 Year Olds	40-49 Year Olds	50-59 Year Olds
	-5.80**	0.58	-3.10	-8.15
	(2.51)	(4.27)	(6.08)	(11.19)
p-val	0.027	0.966	0.602	0.499
Control Mean	103.1	149.5	280.2	649.9
Bandwidths	7.7,8.2	7.5,7.4	10.9,9.0	9.8,9.9
N	2056	1961	2368	2384

Panel B: External Causes

	(1)	(2)	(3)	(4)
	20-29 Year Olds	30-39 Year Olds	40-49 Year Olds	50-59 Year Olds
	-7.05***	-0.84	-2.59	0.37
	(2.16)	(3.33)	(2.38)	(2.51)
p-val	0.002	0.663	0.270	0.979
Control Mean	75.6	74.6	78.6	85.3
Bandwidths	7.8,7.7	8.2,8.5	7.8,7.9	8.3,7.8
N	1999	2144	2011	2027

Panel C: Internal Causes

	(1)	(2)	(3)	(4)
	20-29 Year Olds	30-39 Year Olds	40-49 Year Olds	50-59 Year Olds
	1.14*	1.47	-1.38	-9.29
	(0.70)	(1.68)	(5.15)	(9.94)
p-val	0.069	0.537	0.909	0.405
Control Mean	27.3	73.0	200.7	563.4
Bandwidths	7.3,7.0	6.8,6.8	8.5,8.6	9.8,9.9
N	1894	1850	2173	2381

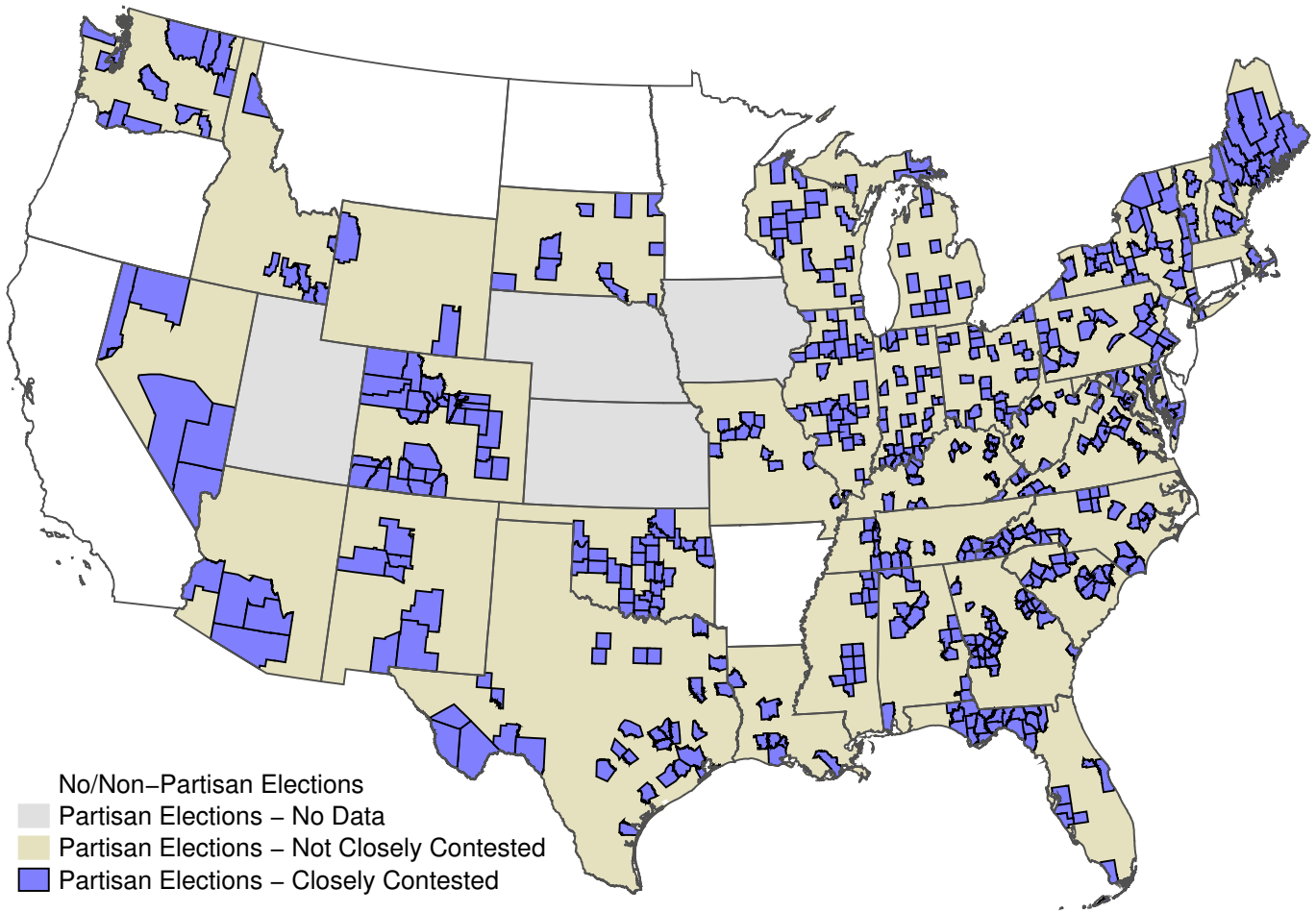
Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates. Panel A examines effects on mortality from all causes; Panels B and C focus on external and internal causes of death, respectively. See Section I.2 and the Appendix for more information on cause of death classification. Each column examines effects on mortality rates among a specific ten year age group. Mortality rates are calculated per 100,000 residents and are age-adjusted. All estimates are obtained using the baseline specification choices outlined in Section II. The control mean row provides the intercept to the left of the cutoff from a regression without covariates. The bandwidths used in estimation and the effective number of observations are also presented. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Effects of Narrow Republican Prosecutor Election on Prison Admission Rates

	(1) All	(2) Male	(3) Female	(4) Black	(5) White
	5.137	25.887**	-6.167	307.644***	-16.488
	(9.890)	(16.630)	(3.721)	(59.035)	(10.732)
p-val	0.206	0.030	0.624	0.001	0.500
Control Mean	393.2	688.3	104.7	978.7	279.9
BWs	6.9,6.9	6.7,6.7	8.0,8.0	8.1,8.7	9.1,10.5
N	1140	929	1039	853	1225

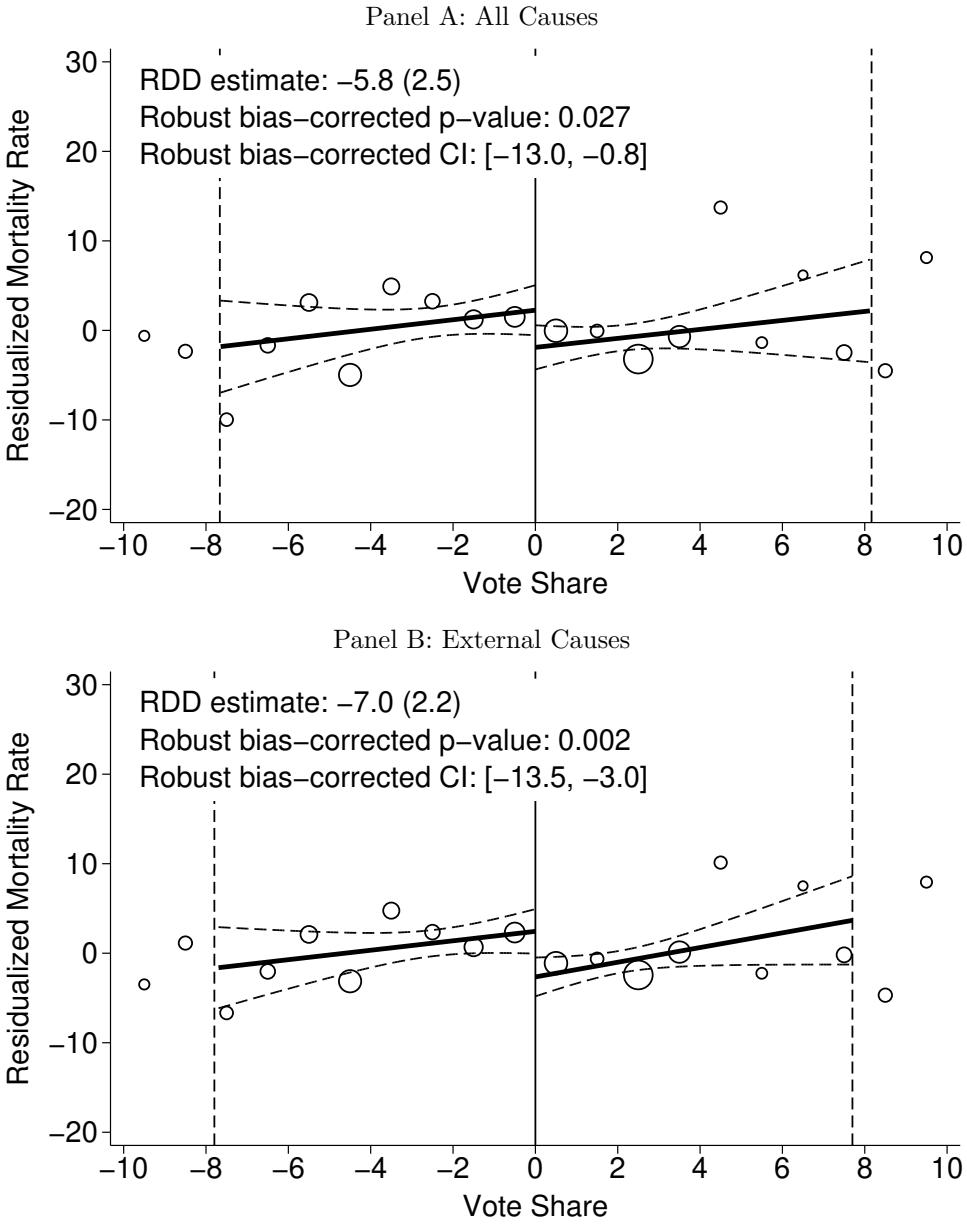
Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level prison admission rates per 100,000 people. Column 1) examines effects on prison admission rates among all demographic groups; the remaining columns examine effects among the demographic group indicated by the column header. See Section I.2 for more information on the prison admission data. All estimates are obtained using the baseline specification choices outlined in Section II. The control mean row provides the intercept to the left of the cutoff from a regression without covariates. The bandwidths used in estimation and the effective number of observations are also presented. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 1: Counties with Closely Contested Partisan Local Prosecutor Elections, 2010-2019



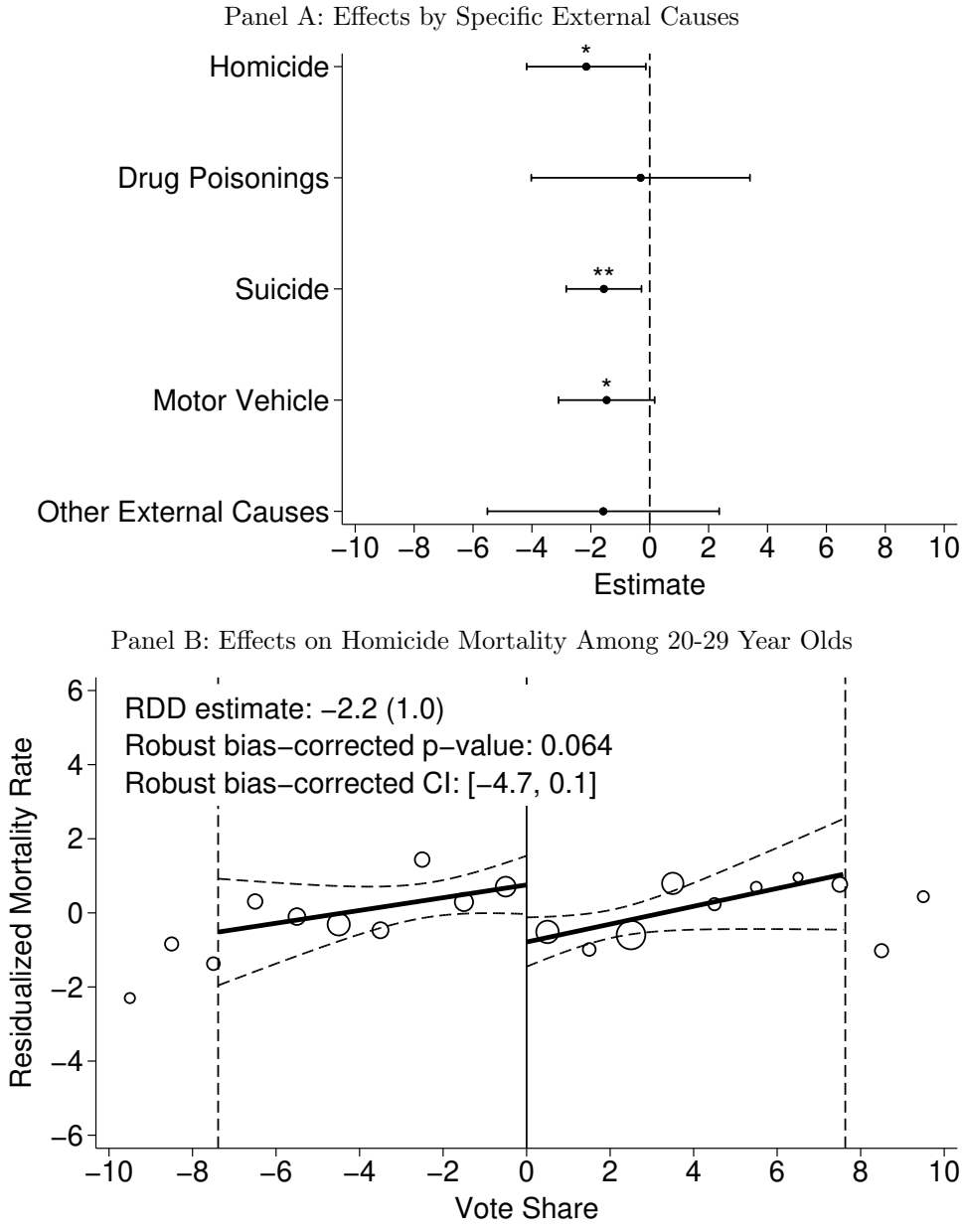
Notes: This map visualizes the counties which contribute to our regression samples. States shaded white are states where local prosecutors are either appointed or elected in non-partisan elections. States shaded light gray are states where local prosecutors are elected in partisan elections, but we do not possess election data over this time frame. Counties shaded khaki are counties in districts that elect local prosecutors in partisan elections but which did not have a closely contested election from the years 2010 to 2019. Counties shaded blue are counties in districts that elect local prosecutors in partisan elections and which possessed a Republican vote share value between 0.4 and 0.6 in at least one year from 2010 to 2019.

Figure 2: Effects of Narrow Republican Prosecutor Election on Mortality Among 20-29 Year Olds



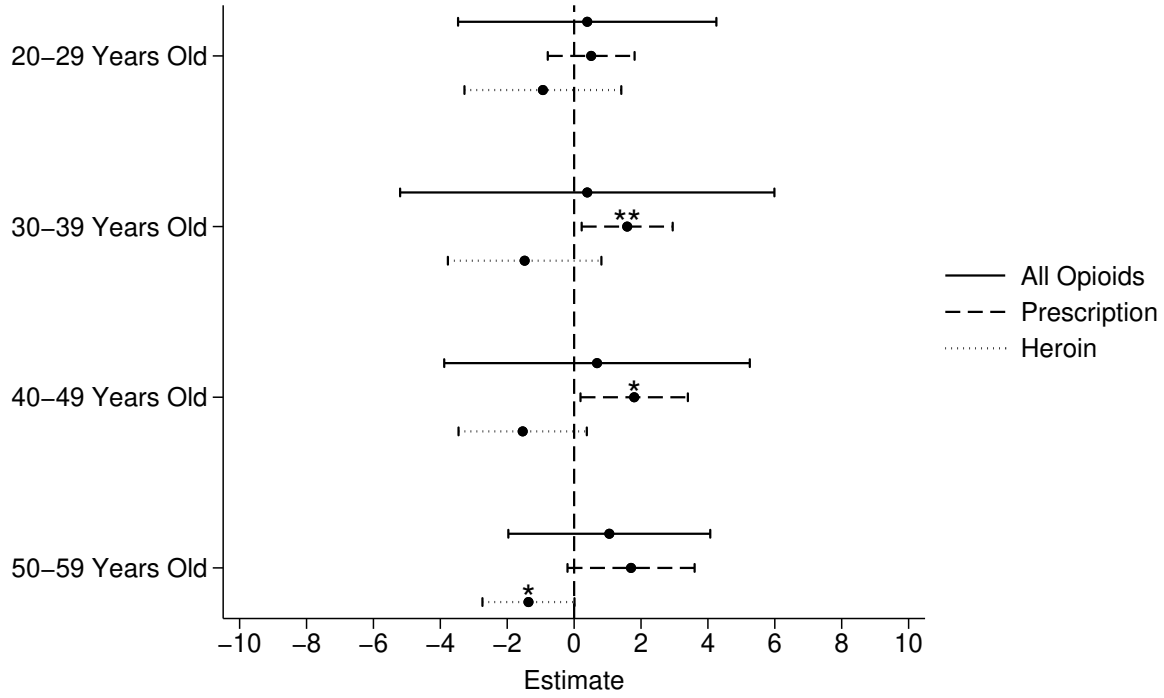
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality rates among Americans ages 20-29 years old. The circles visualize average residualized mortality rates within one percentage point wide bins on each side of the cutoff. Mortality rates have been residualized of the controls used in our preferred specification: state fixed effects, year fixed effects, 2010 county-level characteristics, and 2006 baseline mortality rates. The size of each circle corresponds to the total population across all observations within the bin. The dashed vertical lines denote the bandwidths. The thick solid lines within the bandwidths are the local linear fits, and the gap between them at the cutoff date equals the RDD point estimate. The dashed lines around the local linear fits are 95 percent confidence intervals. All other RDD figures in this paper use the same methodology.

Figure 3: Effects of Narrow Republican Prosecutor Election on Mortality from External Causes Among 20-29 Year Olds



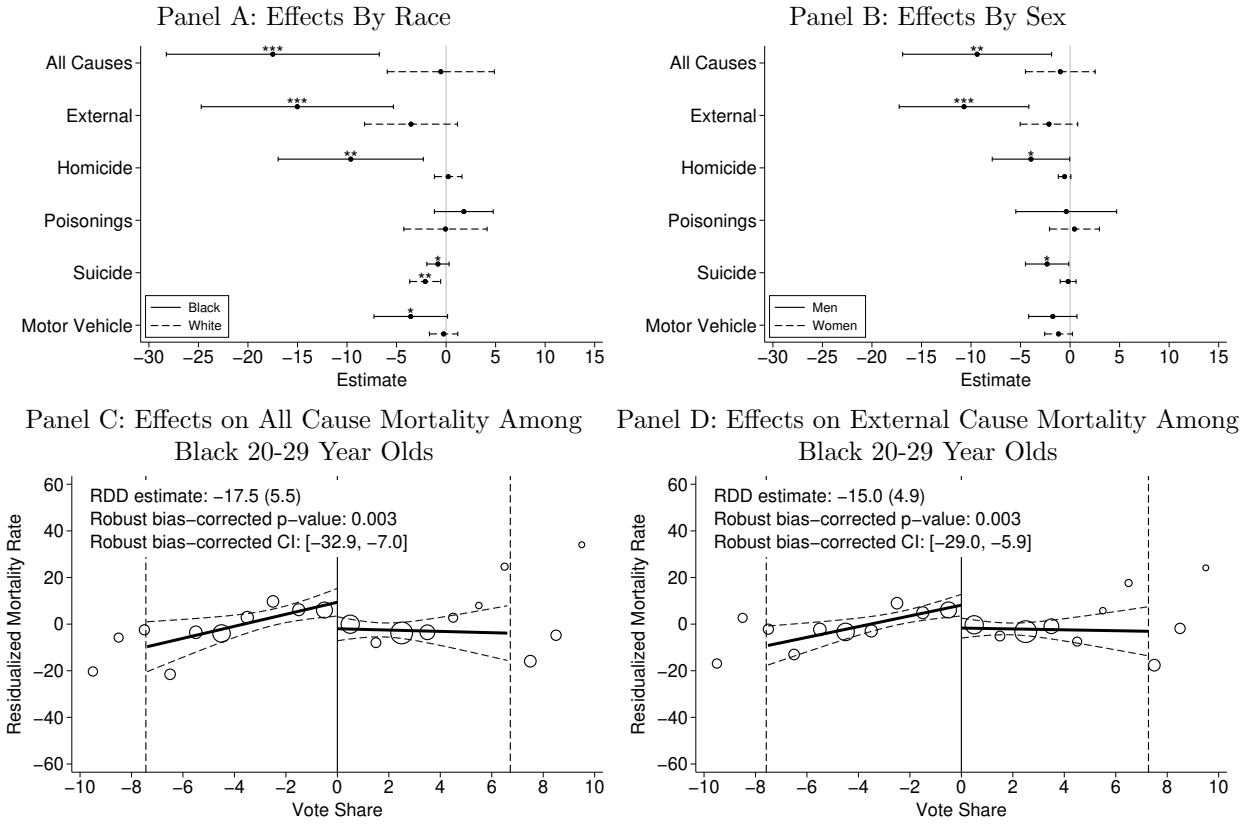
Notes: Panel A plots RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality rates among Americans ages 20-29 years old. Each coefficient and corresponding conventional 95% confidence comes from a separate regression estimated using our baseline specification described in Section II. Stars denote statistical significance using the robust bias-corrected p-value rather than the conventional one; hence, the stars and confidence intervals may not imply the same level of statistical significance. The outcome in each regression is the mortality rate due to the cause listed on the vertical axis. See Section I.2 and the Appendix for more information on cause of death classification. Panel B visualizes the covariate-adjusted RDD estimate of the effect of narrow election of a Republican local prosecutor on the homicide mortality rate among Americans ages 20-29 years old. See Figure 2 for notes on the construction of this paper's RDD plots. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 4: Effects of Narrow Republican Prosecutor Election on Opioid Mortality



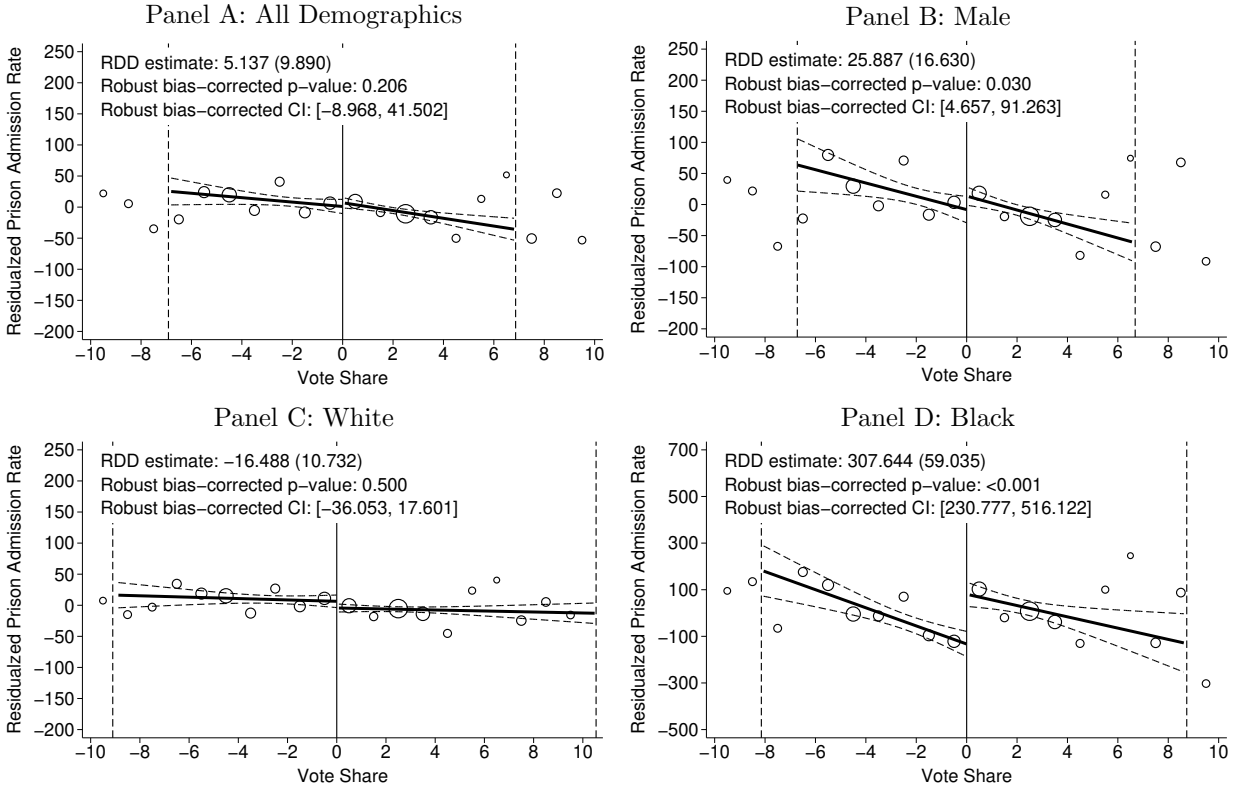
Notes: This coefficient plot conveys RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality due to opioid overdoses across across ten year age groups. Each coefficient and corresponding conventional 95% confidence comes from a separate regression estimated using our preferred specification described in Section II. Stars denote statistical significance using the robust bias-corrected p-value rather than the conventional one; hence, the stars and confidence intervals may not imply the same level of statistical significance. The outcome in each regression is the mortality rate due to the cause denoted in the figure legend among the age group denoted on the vertical axis. See Section I.2 and the Appendix for more information on cause of death classification. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ (p = robust bias-corrected p-value).

Figure 5: Effects of Narrow Republican Prosecutor Election on Mortality Among 20-29 Year Olds, Heterogeneity By Race and Sex



Notes: Panels A and B plot RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality rates among Americans ages 20-29 years old. Panel A presents estimates separately for Black and White Americans; Panel B presents estimates separately for men and women. Each coefficient and corresponding conventional 95% confidence comes from a separate regression estimated using our baseline specification described in Section II. Stars denote statistical significance using the robust bias-corrected p-value rather than the conventional one; hence, the stars and confidence intervals may not imply the same level of statistical significance. The outcome in each regression is the mortality rate due to the cause listed on the vertical axis. See Section I.2 and the Appendix for more information on cause of death classification. Panels C and D visualize the covariate-adjusted RDD estimates given in their titles. See Figure 2 for notes on the construction of this paper's RDD plots. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ (p = robust bias-corrected p-value).

Figure 6: Effects of Narrow Republican Prosecutor Election on Prison Admission Rates



Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on annual county-level admissions to prison. See Figure 2 for notes on the construction of this paper's RDD plots.

A Appendix

This section lists the ICD10 codes used to identify each cause of death examined in this paper. Many of our classifications of broader causes of death are taken directly from the Vital Statistic's 39 Causes of Death classification. These causes are:

- Major cardiovascular diseases: I00-I78
- Malignant neoplasms (cancer): C00-C97
- Other Diseases/Conditions:
  - Tuberculosis: A16-A19
  - Syphilis: A50-A53
  - Human immunodeficiency virus (HIV) disease: B20-B24
  - Diabetes mellitus: E10-E14
  - Alzheimer's disease: G30
  - Influenza and pneumonia: J10-J18
  - Chronic lower respiratory diseases: J40-J47
  - Peptic ulcer: K25-K28
  - Chronic liver disease and cirrhosis: K70,K73-K74
  - Nephritis, nephrotic syndrome and nephrosis: N00-N07, N17-N19, N25-N27
  - Pregnancy, childbirth and the puerperium: O00-O99
  - Perinatal conditions: P00-P96
  - Congenital malformations, deformations and chromosomal abnormalities: Q00-Q99
  - Sudden infant death syndrome: R95

  - Symptoms, signs and abnormal clinical and laboratory findings, not elsewhere classified (excluding Sudden infant death syndrome): R00-R94, R96-R99
  - All other diseases (residual): A00-A09, A20-A49, A54-B19, B25-B99, D00-E07, E15-G25, G31-H93, I80-J06, J20-J39, J60-K22, K29-K66, K71-K72, K75-M99, N10-N15, N20-N23, N28-N98

- Homicide: X85-Y09, Y87.1
- Non-Drug Poisoning Suicides: X65-X84 and Y87.0
- Drug Poisonings (Accidental/Purposeful/Unknown Intent): X40-X44, X60-X64, Y10-Y14, Y40-Y59
- Motor vehicle accidents: V02-V04, V09.0, V09.2, V12-V14, V19.0-V19.2, V19.4-V19.6, V20-V79, V80.3-V80.5, V81.0-V81.1, V82.0-V82.1, V83-V86, V87.0-V87.8, V88.0-V88.8, V89.0, V89.2
- All Other External Causes:
  - Accidents (non motor vehicle): V01, V05-V06, V09.1, V09.3-V09.9, V10-V11, V15-V18, V19.3, V19.8-V19.9, V80.0-V80.2, V80.6-V80.9, V81.2-V81.9, V82.2-V82.9, V87.9, V88.9, V89.1, V89.3, V89.9, V90-X59, Y60-Y86, Y88
  - Other: Y15-Y36, Y87.2, Y89

The remaining, more specific causes of death we examine are:

- Gun deaths: W23-W34, X72-74, X93-95, Y22-24, Y35.0
- Opioid overdose: T40.1-T40.4,T40.6
- Heroin overdose: T40.1
- Natural and semi-synthetic opioid overdose: T40.2
- Fentanyl overdose: T40.4

Table A1: Local Prosecutor District and Election Details by State

State	Number of Districts	Term Length	Timing	State	Number of Districts	Term Length	Timing
Alabama	42	6	Staggered	Nevada	17	4	Midterm
Arizona	15	4	Presidential	New Hampshire	10	2	Staggered
Colorado	22	4	Presidential	New Mexico	14	4	Presidential
Florida	20	4	Mixed	New York	62	4	Mixed
Georgia	49	4	Mixed	North Carolina	44	4	Mixed
Idaho	44	4	Presidential	Ohio	88	4	Presidential
Illinois	102	4	Presidential	Oklahoma	27	4	Midterm
Indiana	91	4	Midterm	Pennsylvania	67	4	Mixed
Iowa	99	4	Midterm	South Carolina	16	4	Mixed
Kansas	105	4	Presidential	South Dakota	64	4	Presidential
Kentucky	57	6	Staggered	Tennessee	31	8	Staggered
Louisiana	42	6	Staggered	Texas	163	4	Mixed
Maine	8	4	Midterm	Utah	29	4	Midterm
Maryland	24	4	Midterm	Vermont	14	4	Midterm
Massachusetts	11	4	Midterm	Virginia	121	4	Mixed
Michigan	83	4	Presidential	Washington	39	4	Midterm
Mississippi	22	4	2015	West Virginia	55	4	Presidential
Missouri	115	4	Mixed	Wisconsin	71	4	Presidential
Nebraska	93	4	Midterm	Wyoming	23	4	Mixed

Notes: This table provides summary information about the number of prosecutorial districts, lengths of terms, and timing of local prosecutor election in the states which elect them in partisan elections. "Presidential" and "Midterm" states are ones where local prosecutor elections consistently coincide with each national election, respectively. "Staggered" states are ones with term lengths different than four years and thus swap between presidential and midterm years. "Mixed" states are ones where the timing of local prosecutor elections varies across districts within the state.

Table A2: Top Twenty In-Sample Counties as Ranked by 2010 Middle-Aged Population

In Sample Rank	County Name	2010 Middle-Aged Population
1	Harris, TX	1192213
2	Maricopa, AZ	1051565
3	Dallas, TX	695896
4	Clark, NV	572316
5	Tarrant, TX	529860
6	Bexar, TX	471329
7	Nassau, NY	357847
8	Hillsborough, FL	352019
9	Oakland, MI	338710
10	Fairfax, VA	333636
11	Franklin, OH	327845
12	Mecklenburg, NC	289570
13	Wake, NC	287169
14	Salt Lake, UT	285272
15	Westchester, NY	263851
16	Marion, IN	250358
17	Pima, AZ	239841
18	Erie, NY	230496
19	Montgomery, PA	220661
20	Baltimore, MD	211981

Notes: This table lists the twenty largest counties (in terms of population) that possess a Republican prosecutor vote share value between 0.4 and 0.6 in at least one year from 2010 to 2019, along with the 2010 middle-aged population.

Table A3: Effects of Narrow Republican Prosecutor Election on All-Cause Mortality

	(1)	(2)	(3)	(4)	(5)	(6)
			Ages 20-29 (Control Mean = 103.08)			
	-5.82	-5.00	-5.97*	-7.61	-9.07**	-5.80**
	(6.48)	(4.06)	(3.67)	(6.34)	(3.77)	(2.51)
p-val	0.197	0.204	0.060	0.574	0.022	0.027
BWs	10.1,10.1	7.8,7.9	7.8,8.3	6.3,6.6	9.1,8.1	7.7,8.2
N	2483	2108	2127	1770	2083	2056
			Ages 30-39 (Control Mean = 149.52)			
	-15.78	-9.25	-9.73	-2.71	1.60	0.58
	(14.26)	(8.15)	(6.85)	(7.28)	(5.87)	(4.27)
p-val	0.187	0.300	0.159	0.569	0.923	0.966
BWs	10.1,10.9	9.2,8.6	7.6,7.7	9.3,9.4	8.4,8.3	7.5,7.4
N	2598	2252	2039	2318	2070	1961
			Ages 40-49 (Control Mean = 280.17)			
	-30.41*	-20.80*	-21.81*	6.87	9.84	-3.10
	(17.09)	(10.35)	(10.23)	(11.52)	(7.07)	(6.08)
p-val	0.051	0.076	0.062	0.400	0.140	0.602
BWs	8.9,9.3	9.0,9.0	9.0,9.0	6.7,7.7	7.9,7.8	10.9,9.0
N	2354	2324	2327	1923	1984	2368
			Ages 50-59 (Control Mean = 649.89)			
	-62.08*	-49.59**	-49.74**	7.95	6.05	-8.15
	(35.41)	(22.07)	(21.66)	(15.90)	(13.02)	(11.19)
p-val	0.058	0.047	0.045	0.542	0.560	0.499
BWs	8.7,9.2	8.7,8.9	8.7,9.0	7.5,7.8	8.9,8.8	9.8,9.9
N	2342	2262	2314	1995	2160	2384
Fixed Effects	No	State	State, Year	State, Year	State, Year	State, Year
County-Level Controls	No	No	No	Yes	Yes	Yes
Population Weights	Yes	Yes	Yes	No	Yes	Yes
4 Highest Weighted Counties	Include	Include	Include	Include	Exclude	Include

Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates from all causes. Each subpanel focuses on a different ten year age group, and each column varies the estimation choices. Column (1) does not includes covariates, Columns (2) and (3) add in state and year fixed effects, respectively, and Column (4) adds a set of 2010 county-level characteristics and 2006 baseline mortality rates as controls. Column (5) uses the same covariates as Column (4) but does not weight the regression by population, and Column (6) drops the four counties with the largest regression weights from the sample before estimating. Column (4) is our preferred specification. All estimates use a triangular kernel weighting function. Standard errors are clustered at the district level. Bandwidths are chosen as the median of three MSE optimal choices. The control mean estimate at the top of each subpanel gives the intercept to the left of the cutoff from the Column 1) regression. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$ (p = robust bias-corrected p-value).

Table A4: Effects of Narrow Republican Prosecutor Election on External Cause Mortality

	(1)	(2)	(3)	(4)	(5)	(6)
			Ages 20-29 (Control Mean = 75.64)			
	-4.94	-5.97*	-6.88**	-7.35	-10.39***	-7.05***
	(6.14)	(3.47)	(3.08)	(4.85)	(3.41)	(2.16)
p-val	0.240	0.073	0.012	0.332	0.004	0.002
BWs	10.6,11.6	8.6,8.4	7.7,8.6	6.1,7.2	9.8,6.5	7.8,7.7
N	2651	2202	2193	1806	1968	1999
			Ages 30-39 (Control Mean = 74.64)			
	-8.23	-6.20	-6.96	5.62	0.50	-0.84
	(10.03)	(5.16)	(4.49)	(6.08)	(4.36)	(3.33)
p-val	0.315	0.251	0.115	0.479	0.838	0.663
BWs	13.5,12.2	9.7,9.3	7.7,8.0	7.0,7.6	10.2,8.4	8.2,8.5
N	2841	2412	2111	1946	2208	2144
			Ages 40-49 (Control Mean = 78.59)			
	-12.16*	-6.51	-7.33*	-2.78	-1.17	-2.59
	(8.18)	(3.78)	(3.83)	(4.55)	(2.87)	(2.38)
p-val	0.067	0.108	0.064	0.764	0.623	0.270
BWs	10.0,10.3	8.2,8.4	7.7,8.0	9.0,9.5	8.6,8.1	7.8,7.9
N	2509	2190	2114	2299	2061	2011
			Ages 50-59 (Control Mean = 85.28)			
	-9.58	-3.60	-3.64	-3.28	-2.93	0.37
	(7.37)	(4.42)	(4.17)	(4.28)	(3.59)	(2.51)
p-val	0.105	0.483	0.385	0.492	0.353	0.979
BWs	9.4,9.0	9.0,9.5	7.8,9.0	7.1,7.1	8.1,7.0	8.3,7.8
N	2353	2362	2267	1884	1908	2027
Fixed Effects	No	State	State, Year	State, Year	State, Year	State, Year
County-Level Controls	No	No	No	Yes	Yes	Yes
Population Weights	Yes	Yes	Yes	No	Yes	Yes
4 Highest Weighted Counties	Include	Include	Include	Include	Exclude	Include

Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates from external causes. See Section I.2 and the Appendix for more information on cause of death classification. Each subpanel focuses on a different ten year age group, and each column varies the estimation choices. Column (1) does not includes covariates, Columns (2) and (3) add in state and year fixed effects, respectively, and Column (4) adds a set of 2010 county-level characteristics and 2006 baseline mortality rates as controls. Column (5) uses the same covariates as Column (4) but does not weight the regression by population, and Column (6) drops the four counties with the largest regression weights from the sample before estimating. Column (4) is our preferred specification. All estimates use a triangular kernel weighting function. Standard errors are clustered at the district level. Bandwidths are chosen as the median of three MSE optimal choices. The control mean estimate at the top of each subpanel gives the intercept to the left of the cutoff from the Column 1) regression. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ (p = robust bias-corrected p-value).

Table A5: Effects of Narrow Republican Prosecutor Election on Mortality Among Black and Male 20-29 Year Olds

Panel A: Black

	(1) All Causes	(2) External	(3) Homicide	(4) Drug Poisonings	(5) Suicide	(6) Motor Vehicle
	-17.5***	-15.0***	-9.6**	1.8	-0.8*	-3.6*
	(5.5)	(4.9)	(3.7)	(1.5)	(0.6)	(1.9)
p-val	0.003	0.003	0.024	0.185	0.059	0.092
Control Mean	144.4	98.8	49.5	9.1	11.1	21.0
BWs	7.4,6.7	7.6,7.3	6.7,7.5	6.8,7.7	7.7,9.2	7.5,8.0
N	1883	1933	1900	1943	2226	2039

Panel B: Male

	(1) All Causes	(2) External	(3) Homicide	(4) Drug Poisonings	(5) Suicide	(6) Motor Vehicle
	-9.4**	-10.7***	-3.9*	-0.4	-2.3*	-1.7
	(3.8)	(3.3)	(2.0)	(2.6)	(1.1)	(1.2)
p-val	0.015	0.001	0.083	0.776	0.058	0.107
Control Mean	147.9	116.9	24.5	27.8	26.1	27.3
BWs	8.2,9.1	9.0,8.4	6.9,7.4	8.3,8.8	10.5,10.0	8.0,7.9
N	2239	2126	1913	2180	2428	2018

Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates among 20-29 year olds. Panel A examines effects on Black Americans and Panel B examines effects on men. Each column focuses on a different specific cause of death; see Section I.2 and the Appendix for more information on cause of death classification. All estimates use a triangular kernel weighting function. Standard errors are clustered at the district level. Bandwidths are chosen as the median of three MSE optimal choices. The control mean estimate at the top of each subpanel gives the intercept to the left of the cutoff from the Column 1) regression. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ (p = robust bias-corrected p-value).

Table A6: Effects of Narrow Republican Prosecutor Election on Prison Admission Rates

	(1)	(2)	(3)	(4)	(5)	(6)
			All Demographics (Control Mean = 393.192)			
	-11.016	-17.356	-17.994	6.864	0.505	5.137
	(88.062)	(23.398)	(15.425)	(18.395)	(17.147)	(9.890)
p-val	0.905	0.778	0.456	0.567	0.477	0.206
BWs	12.2,12.2	8.1,7.9	7.8,7.7	10.5,10.4	7.4,7.8	6.9,6.9
N	1713	1310	1263	1516	1216	1140
			Black (Control Mean = 978.661)			
	149.071	206.540**	217.327***	391.980***	431.535***	307.644***
	(182.603)	(113.769)	(73.180)	(140.266)	(88.091)	(59.035)
p-val	0.287	0.025	0.001	0.008	0.001	0.001
BWs	10.3,11.9	9.9,10.4	8.9,9.3	10.2,11.0	6.6,7.2	8.1,8.7
N	1030	988	918	1003	699	853
			White (Control Mean = 279.911)			
	-32.900	-27.309	-27.713	-11.115	3.295	-16.488
	(54.859)	(19.922)	(20.371)	(18.078)	(14.917)	(10.732)
p-val	0.499	0.425	0.418	0.585	0.360	0.500
BWs	11.1,12.6	9.7,8.3	9.3,8.0	10.3,9.3	8.3,7.9	9.1,10.5
N	1350	1097	1078	1191	1002	1225
			Male (Control Mean = 688.275)			
	-16.658	-22.463	-25.259	12.592	21.846	25.887**
	(149.121)	(41.581)	(26.858)	(32.770)	(29.771)	(16.630)
p-val	0.961	0.979	0.659	0.492	0.164	0.030
BWs	12.3,12.4	8.2,7.8	7.9,7.9	8.9,9.6	7.1,7.8	6.7,6.7
N	1416	1045	1054	1164	975	929
			Female (Control Mean = 104.742)			
	-4.520	-7.138	-6.755	-2.686	-4.376	-6.167
	(24.992)	(6.278)	(4.891)	(10.252)	(6.530)	(3.721)
p-val	0.753	0.638	0.564	0.813	0.889	0.624
BWs	13.6,13.7	7.6,7.8	7.8,7.8	9.8,9.5	8.0,8.0	8.0,8.0
N	1474	1028	1032	1191	1019	1039
Fixed Effects	No	State	State, Year	State, Year	State, Year	State, Year
County-Level Controls	No	No	No	Yes	Yes	Yes
Population Weights	Yes	Yes	Yes	No	Yes	Yes
4 Highest Weighted Counties	Include	Include	Include	Include	Exclude	Include

Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level prison admission rates per 100,000 people. Each subtable focuses on a different demographic group. Column (1) does not includes covariates, Columns (2) and (3) add in state and year fixed effects, respectively, and Column (4) adds a set of 2010 county-level characteristics as controls. Column (5) uses the same covariates as Column (4) but does not weight the regression by population, and Column (6) drops the four counties with the largest regression weights from the sample before estimating. Column (4) is our preferred specification. All estimates use a triangular kernel weighting function. Standard errors are clustered at the district level. Bandwidths are chosen as the median of three MSE optimal choices. The control mean estimate at the top of each subpanel gives the intercept to the left of the cutoff from the Column 1) regression. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ (p = robust bias-corrected p-value).

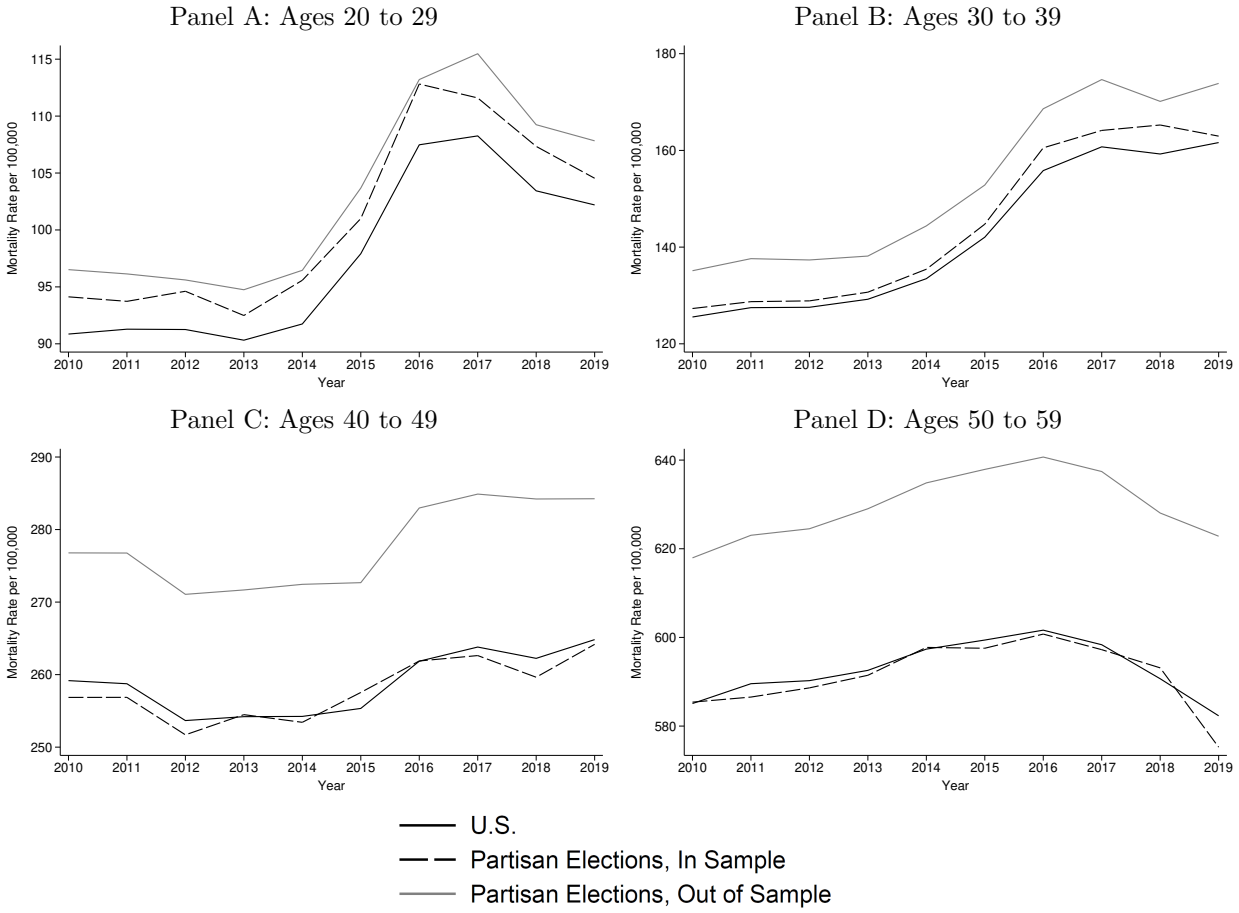
Table A7: Placebo Exercise Using Pre-Election Mortality Rates

Panel A: Ages 20 to 29 Years Old		
	(1)	(2)
	Real Estimate	Pre-Treatment Placebo Estimate
All Causes	-5.800	3.520
	(2.520)	(2.510)
p-value	0.020	0.160
N	2110	1497
Bandwidths	[7.70, 8.20]	
External	-7.050	3.490
	(2.320)	(2.540)
p-value	0.000	0.170
N	2053	1407
Bandwidths	[7.80, 7.70]	

Panel B: Black, Ages 20 to 29 Years Old		
	(1)	(2)
	Real Estimate	Pre-Treatment Placebo Estimate
All Causes	-17.490	9.550
	(8.120)	(7.990)
p-value	0.030	0.230
N	1937	1346
Bandwidths	[7.40, 6.70]	
External Causes	-15.020	0.710
	(6.560)	(5.970)
p-value	0.020	0.910
N	1987	1399
Bandwidths	[7.60, 7.30]	

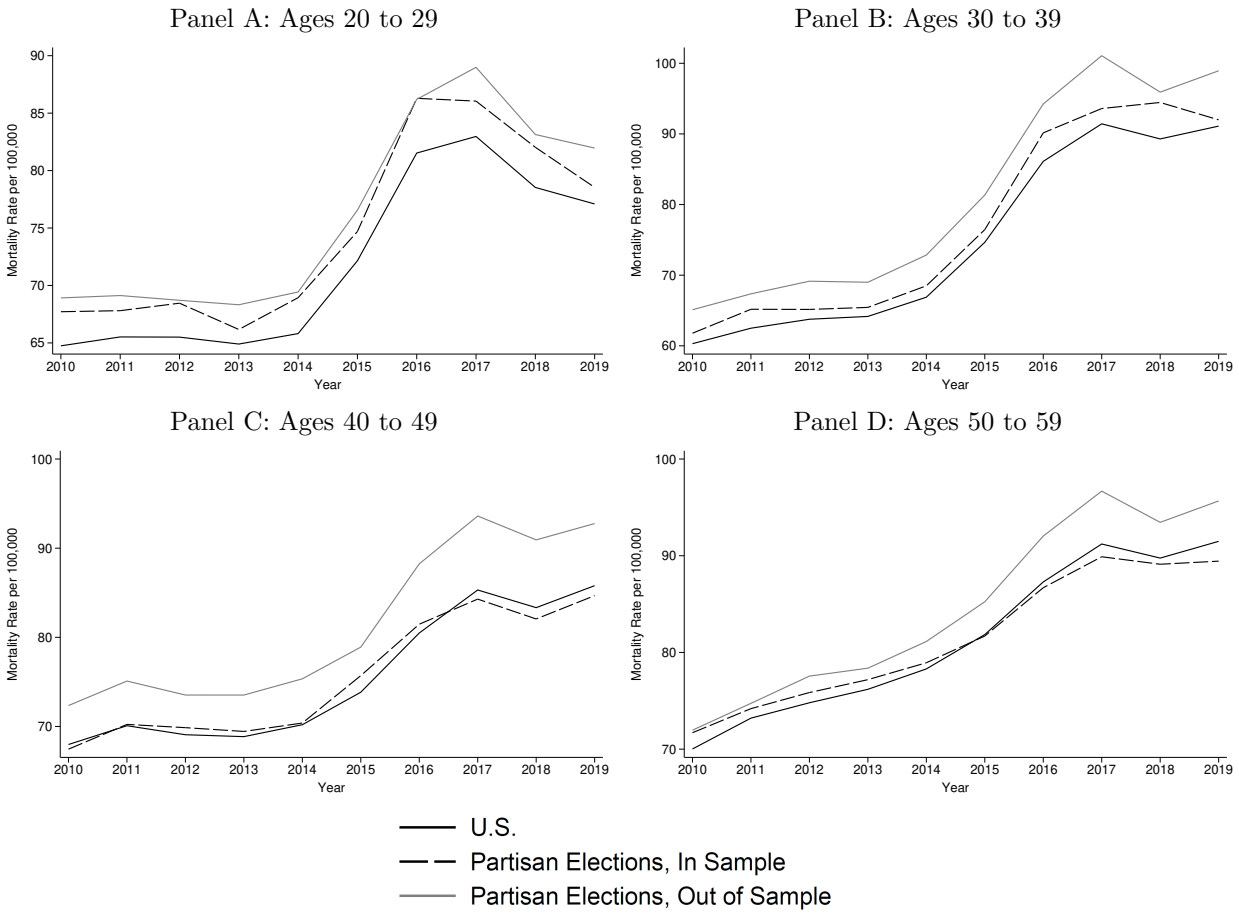
Notes: This table presents estimated obtained by performing the placebo exercise described in Section III.2. For counties that have a closely contested election during our time frame, we assign their closely contested vote share value to county-by-year observations *prior* to the closely contested election, then use these observations to obtain a pre-election placebo estimate. Column (1) restates our main estimates and Column (2) shows the corresponding pre-election placebo estimates. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ (p = robust bias-corrected p-value).

Figure A1: Trends in Mortality from All Causes



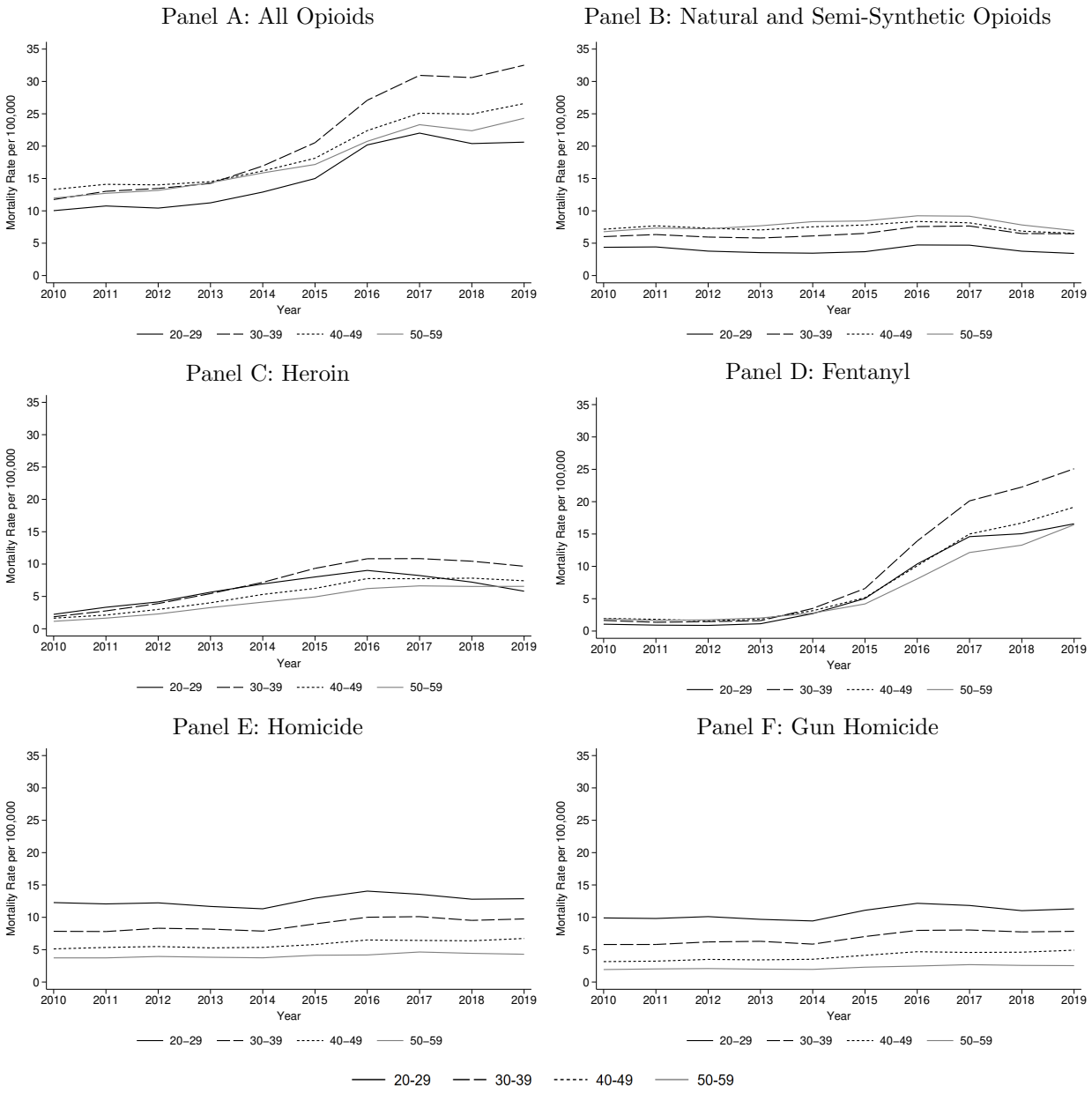
Notes: These figures show trends in all-cause mortality from 2010 to 2019. Each panel focuses on a different age group. Separate trend lines are shown for the U.S. as a whole (black solid line), for counties had at least one closely contested election from the years 2010 to 2019 (black dashed line), and for counties in states that have partisan prosecutor elections but do not have a closely contested election during our sample period (gray solid line). Closely contested is defined has having a Republican vote share in a general election between 0.4 and 0.6.

Figure A2: Trends in Mortality from External Causes



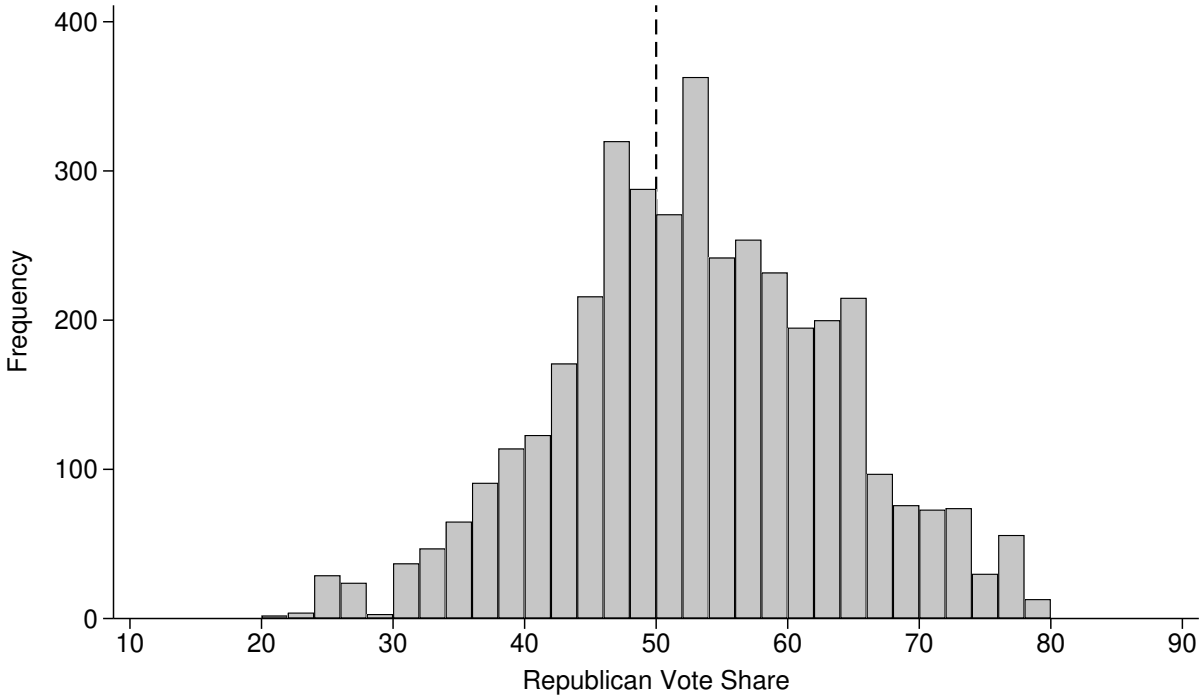
Notes: These figures show trends in mortality from external causes from 2010 to 2019. See Section I.2 and the Appendix for more information on cause of death classification. Each panel focuses on a different age group. Separate trend lines are shown for the U.S. as a whole (black solid line), for counties had at least one closely contested election from the years 2010 to 2019 (black dashed line), and for counties in states that have partisan prosecutor elections but do not have a closely contested election during our sample period (gray solid line). Closely contested is defined has having a Republican vote share in a general election between 0.4 and 0.6.

Figure A3: Trends in Mortality, By Cause



Notes: This figure show U.S. trends in mortality by specific causes. Each panel focuses on a different cause, and the lines within each panel show trends separately by ten year age groups. See Section I.2 and the Appendix for more information on cause of death classification.

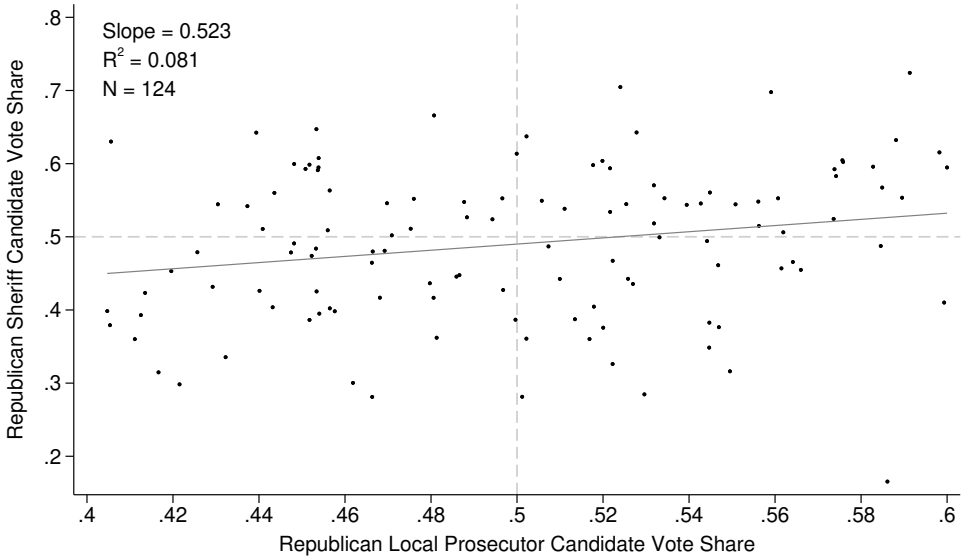
Figure A4: Distribution of Republican Vote Share, 2010 to 2019



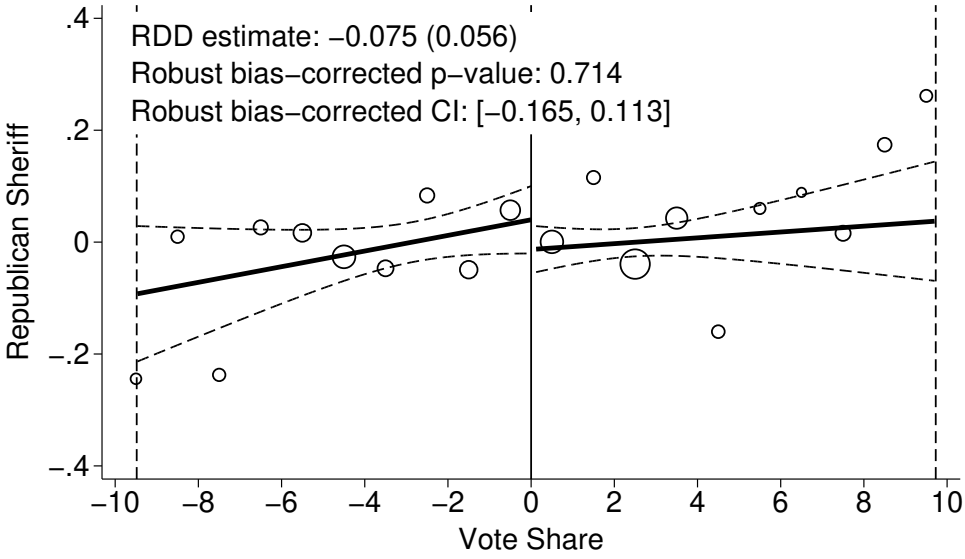
Notes: This figure visualizes the distribution of the Republican vote share variable in our sample. Bins are two percentage points wide.

Figure A5: Association Between Local Prosecutor and Sheriff Political Affiliation

Panel A: Correlation

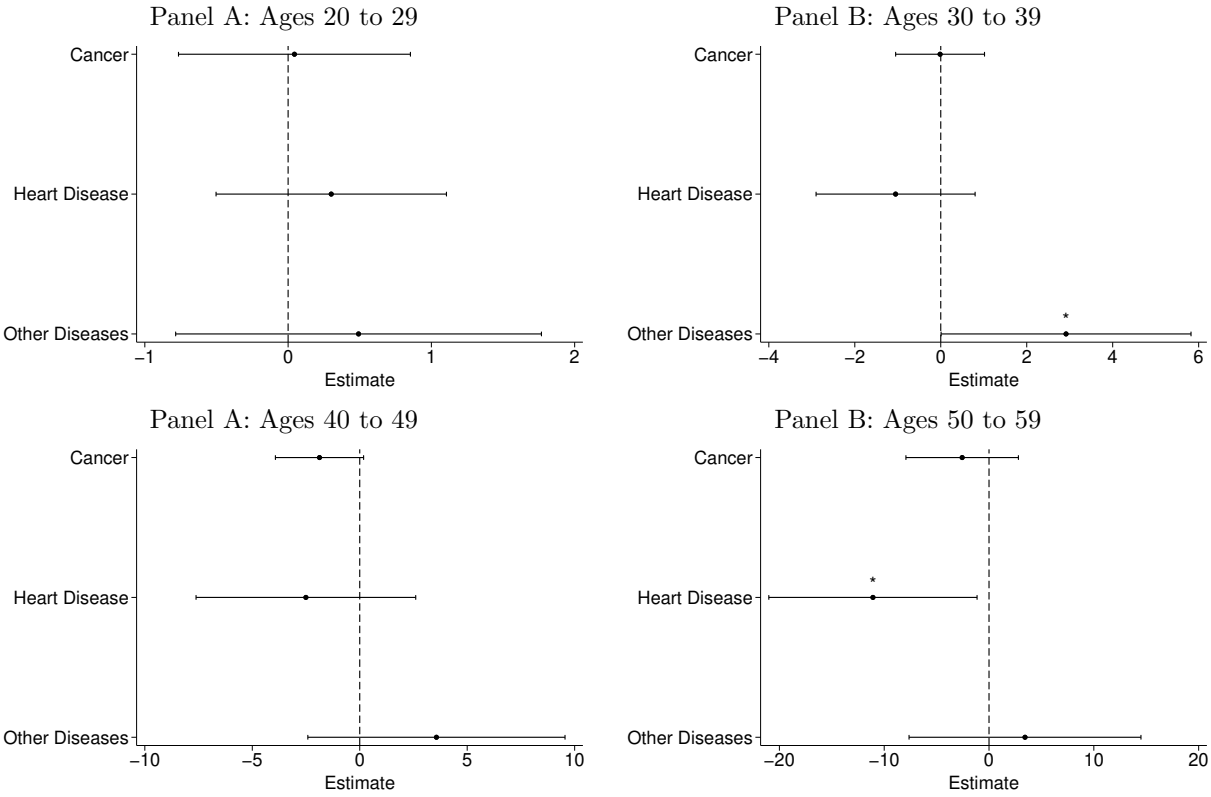


Panel B: Republican Sheriff Balance



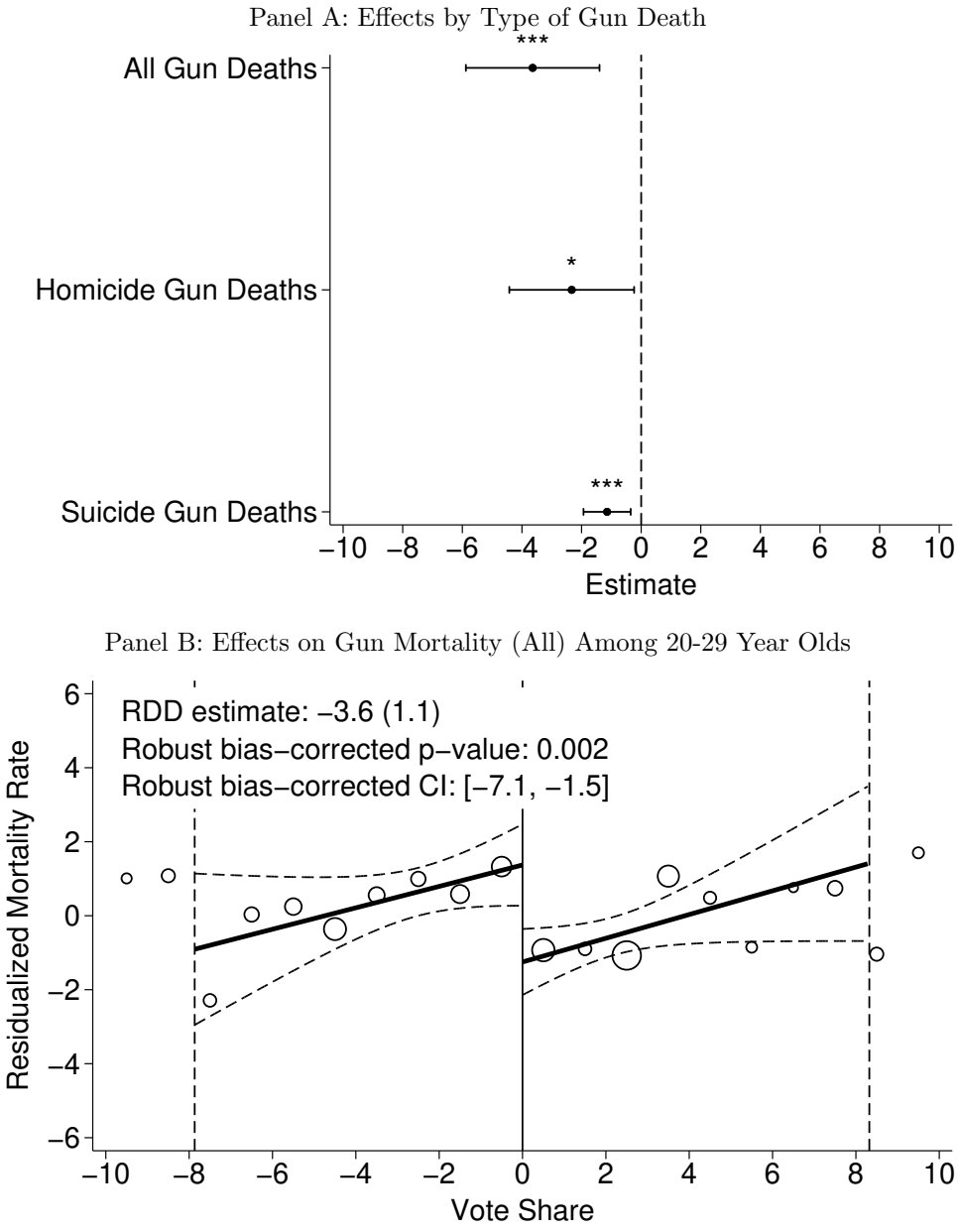
Notes: These figures use data from the American Local Government Election (ALGE) database to visualize the relationship between vote shares for Republican local prosecutors and sheriffs. Panel A visualizes the simple correlation between Republican prosecutorial vote share and Republican sheriff vote share in elections in which both were contested. Panel B visualizes the RDD estimate of the association between narrow election of a Republican local prosecutor and an indicator for whether a Republican sheriff held office in the given county and year.

Figure A6: Effects of Narrow Republican Prosecutor Election on Mortality from Internal Causes



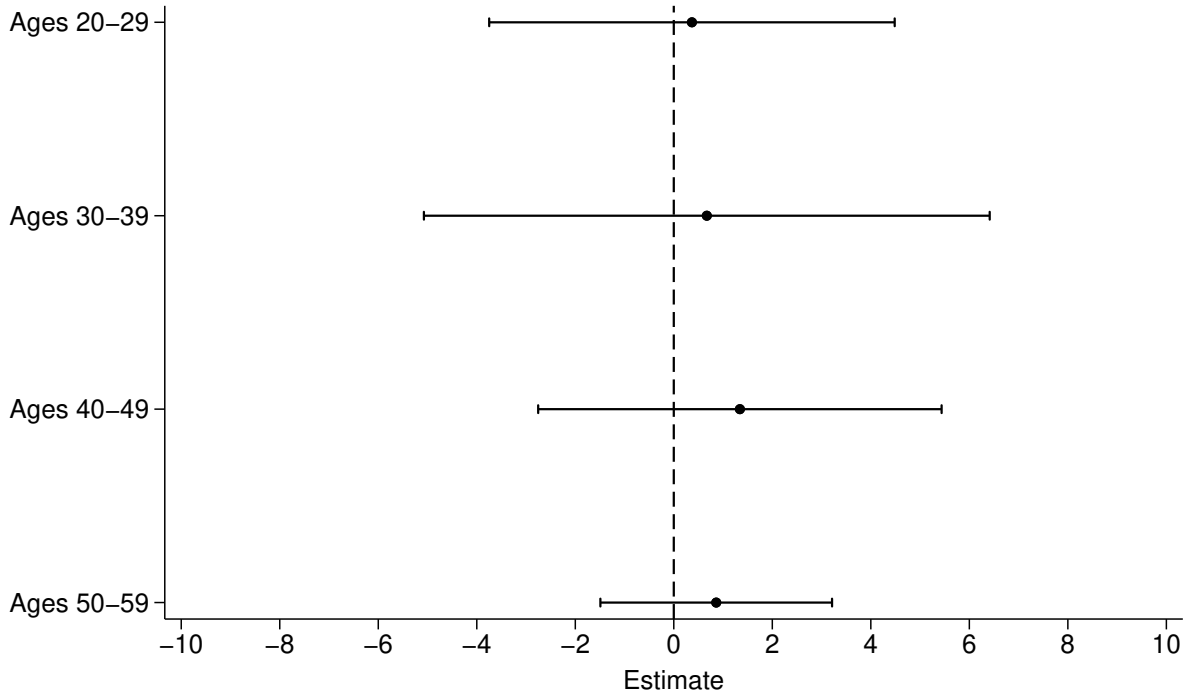
Notes: Each panel plots RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality rates among Americans ages 20-29 years old. Each coefficient and corresponding conventional 95% confidence comes from a separate regression estimated using our preferred specification described in Section II. Stars denote statistical significance using the robust bias-corrected p-value rather than the conventional one; hence, the stars and confidence intervals may not imply the same level of statistical significance. The outcome in each regression is the mortality rate due to the cause listed on the vertical axis. Each panel corresponds to a different ten year age group. See Section I.2 and the Appendix for more information on cause of death classification. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ (p = robust bias-corrected p-value).

Figure A7: Effects of Narrow Republican Prosecutor Election on Gun Mortality Among 20-29 Year Olds



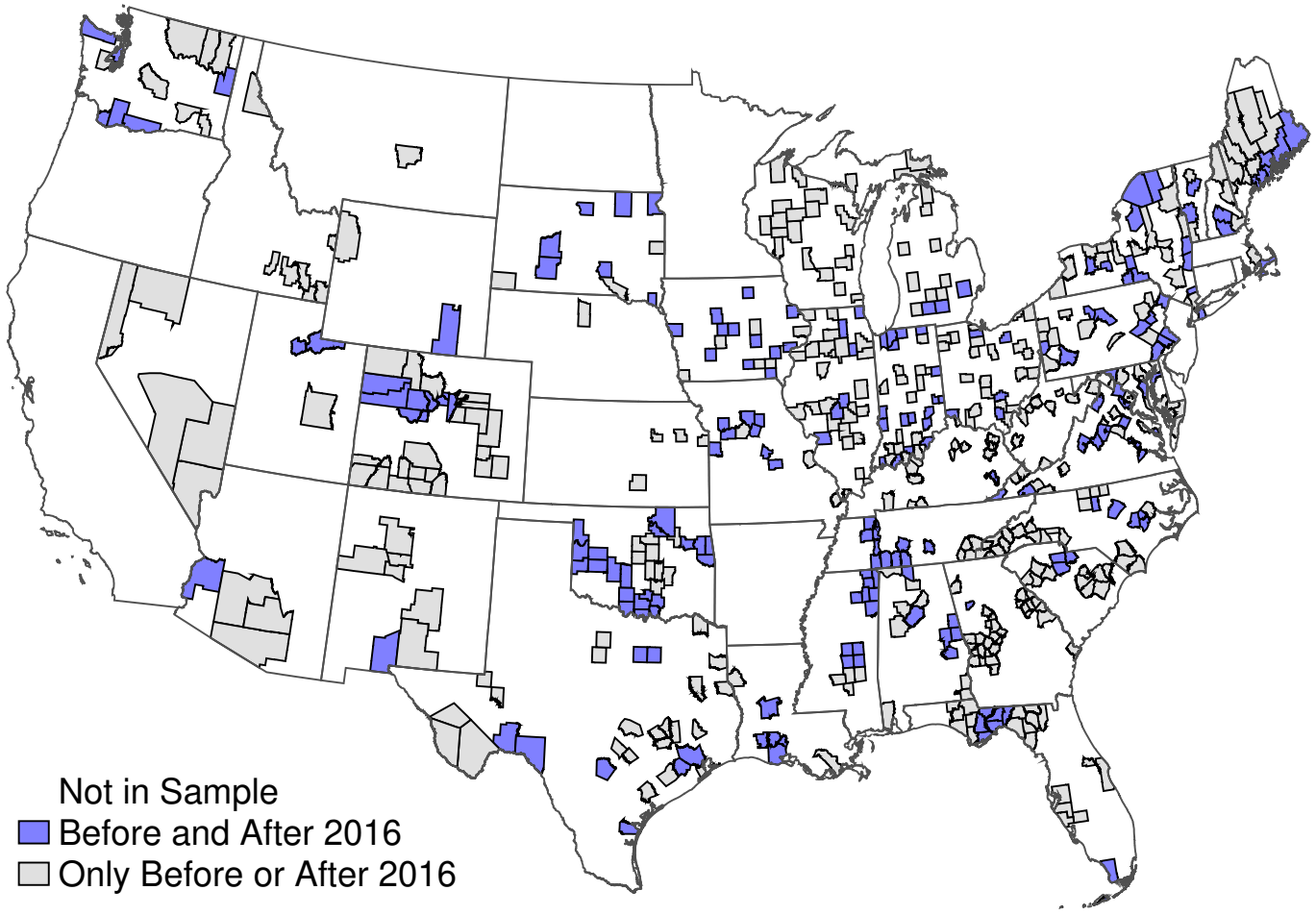
Notes: Panel A plots RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality rates among Americans ages 20-29 years old. Each coefficient and corresponding conventional 95% confidence comes from a separate regression estimated using our preferred specification described in Section II. Stars denote statistical significance using the robust bias-corrected p-value rather than the conventional one; hence, the stars and confidence intervals may not imply the same level of statistical significance. The outcome in each regression is the mortality rate due to the cause listed on the vertical axis. See Section I.2 and the Appendix for more information on cause of death classification. Panel B visualizes the covariate-adjusted RDD estimate of the effect of narrow election of a Republican local prosecutor on the gun death mortality rate from among Americans ages 20-29 years old. See Figure 2 for notes on the construction of this paper's RDD plots. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ (p = robust bias-corrected p-value).

Figure A8: Effects of Narrow Republican Prosecutor Election on Fentanyl Overdose Mortality



Notes: This coefficient plot conveys RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality due to fentanyl overdoses across ten year age groups. Each coefficient and corresponding conventional 95% confidence comes from a separate regression estimated using our preferred specification described in Section II. Stars denote statistical significance using the robust bias-corrected p-value rather than the conventional one; hence, the stars and confidence intervals may not imply the same level of statistical significance. The outcome in each regression is the fentanyl mortality rate among the age group denoted on the vertical axis. See Section I.2 and the Appendix for more information on cause of death classification. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ (p = robust bias-corrected p-value).

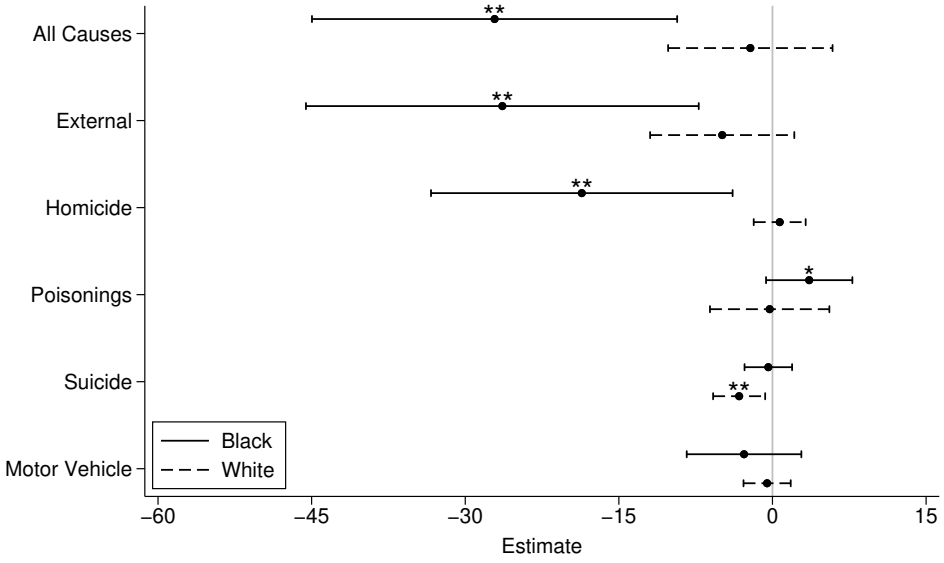
Figure A9: Counties That Contribute Observations to the Analysis Before and After 2016



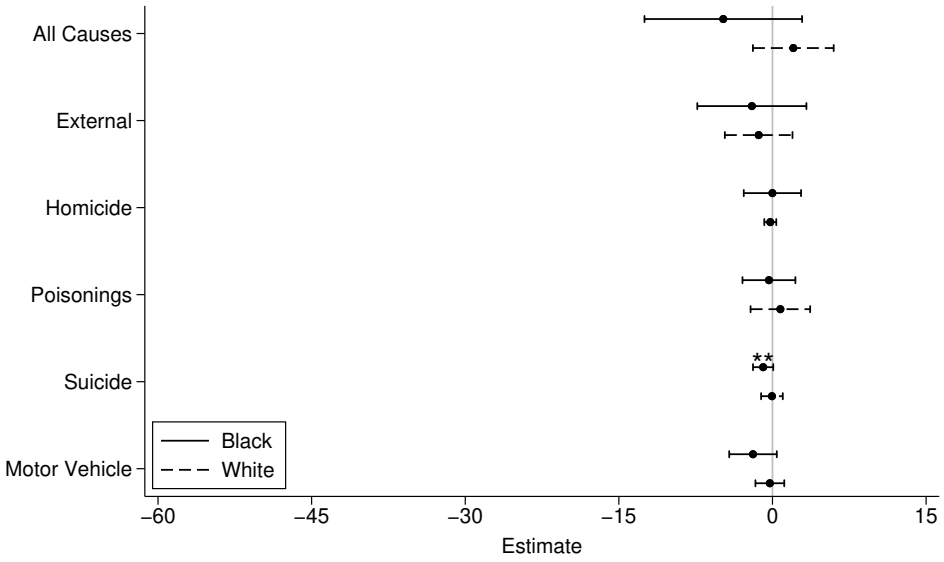
Notes: This map visualizes the counties which contribute to our regression samples and the years in which they contribute data. Counties shaded white are ones that are not used at all in the estimation. Counties shaded gray are ones that contribute observations to our estimation either before 2016 or after 2016, but not both. Counties shaded blues are ones that contribute observations both before and after 2016.

Figure A10: Effects of Narrow Republican Prosecutor Election on Mortality Among 20-29 Year Olds, By Race-Sex Combination

Panel A: Effects by Race, Men

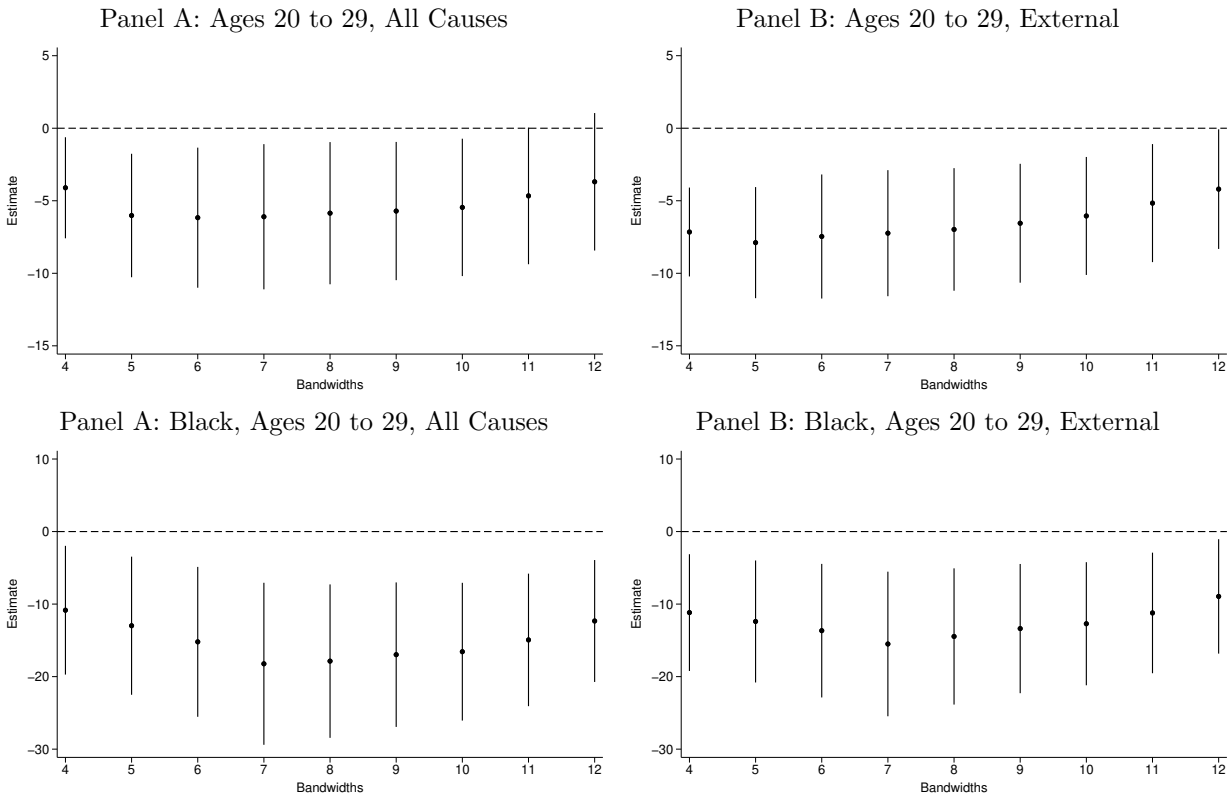


Panel B: Effects by Race, Women



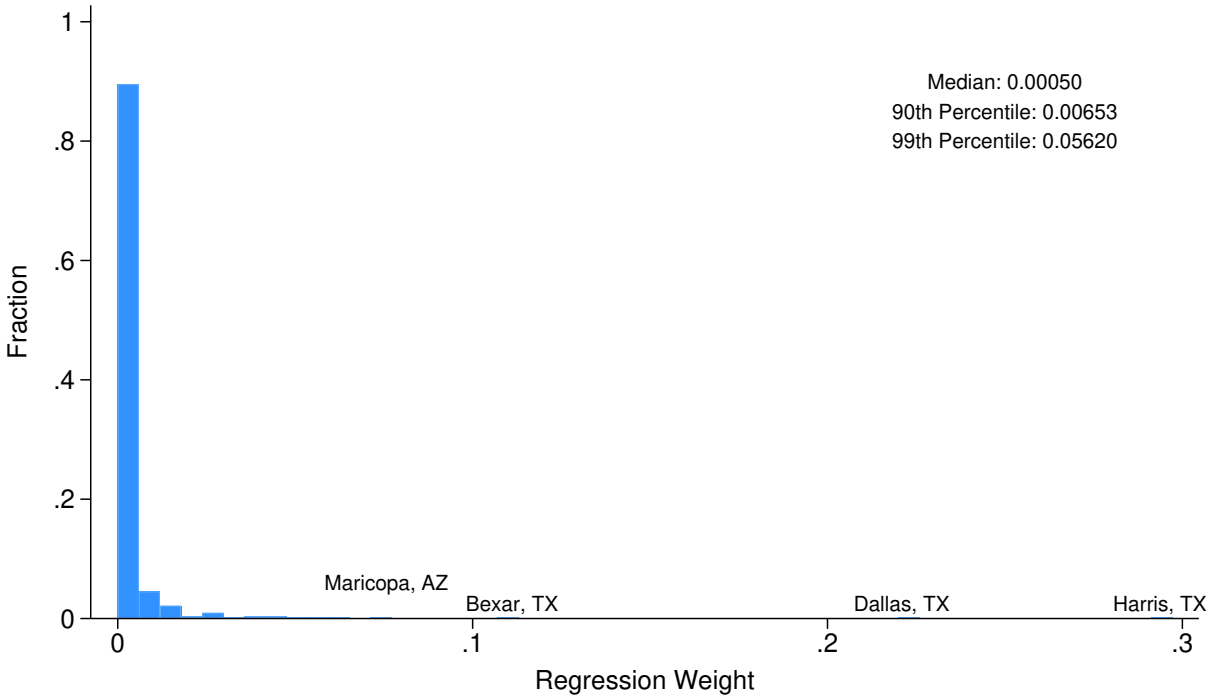
Notes: These panels plot RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality rates among Americans ages 20-29 years old. Panel A focuses on men and presents estimates separately for Black and White men, while Panel B focuses on women and presents estimates separately for Black and White women. Each coefficient and corresponding conventional 95% confidence comes from a separate regression estimated using our preferred specification described in Section II. Stars denote statistical significance using the robust bias-corrected p-value rather than the conventional one; hence, the stars and confidence intervals may not imply the same level of statistical significance. The outcome in each regression is the mortality rate due to the cause listed on the vertical axis. See Section I.2 and the Appendix for more information on cause of death classification. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ (p = robust bias-corrected p-value).

Figure A11: Varying Bandwidths Used in Estimation



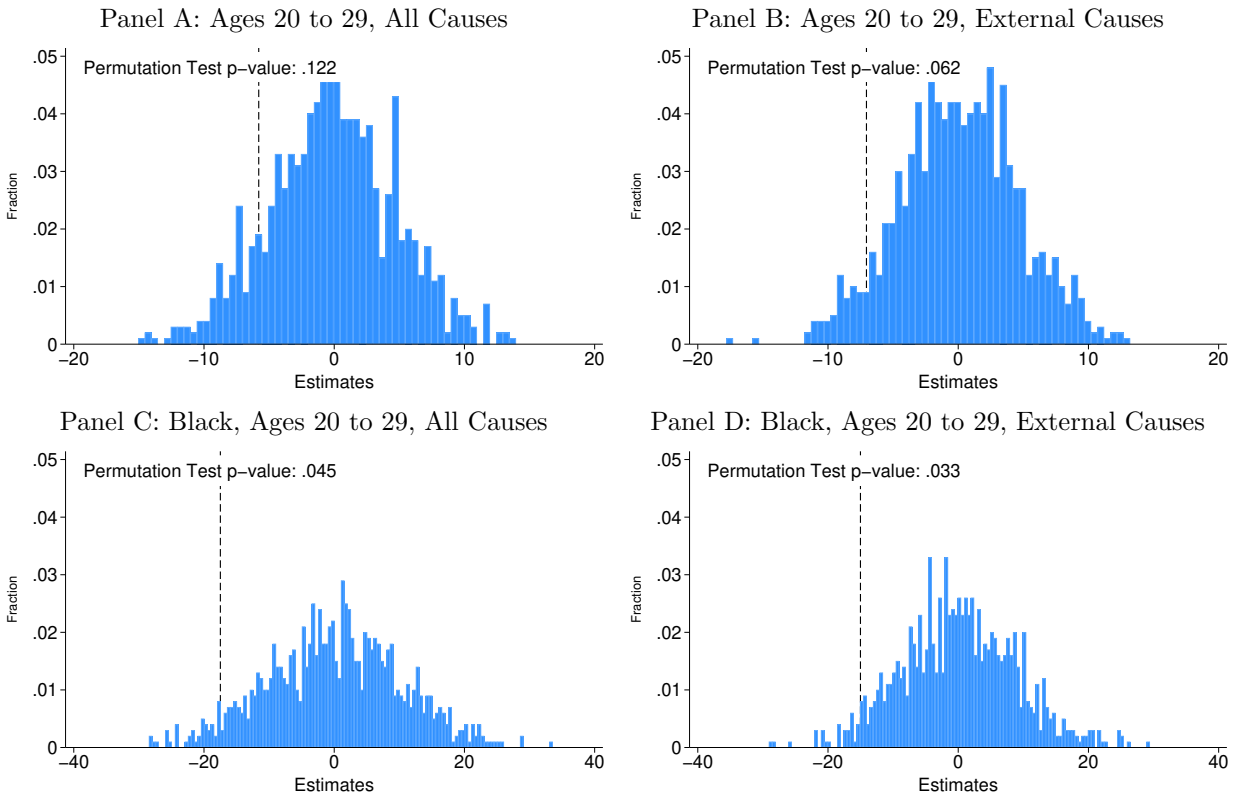
Notes: These figures plot the point estimates and conventional 95 percent confidence intervals from separate RDD regressions on the same outcome that vary the bandwidth by 1 percentage point at a time.

Figure A12: Distribution of Regression Weights Across Counties



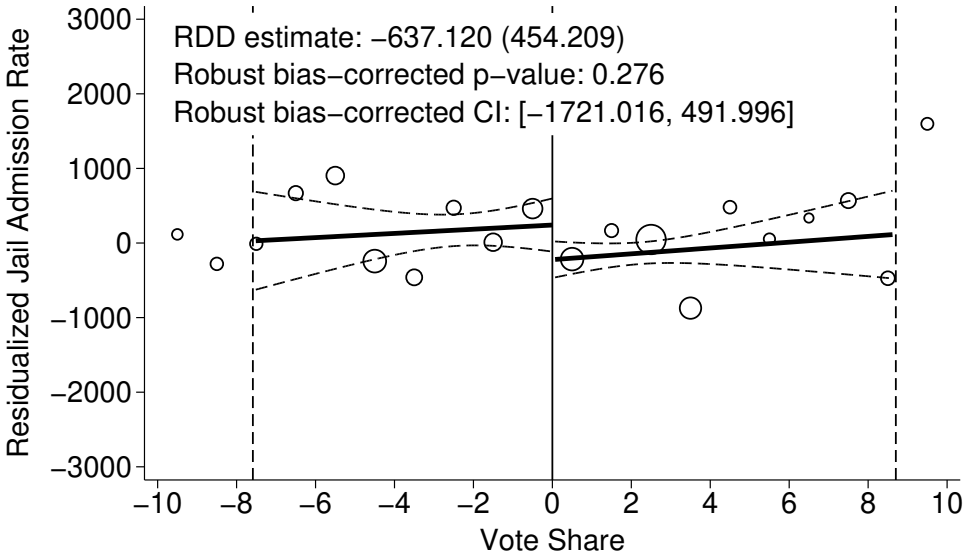
Notes: This figure plots the distributions of regression weights assigned to counties in our regression analysis. These weights correspond to the ones used estimate effects on all-cause mortality among 20-29 years olds. This figure aggregate weights across observations from the same county to the county-level.

Figure A13: Distributions of Estimates Obtained by Randomly Permuting Vote Shares Across Districts



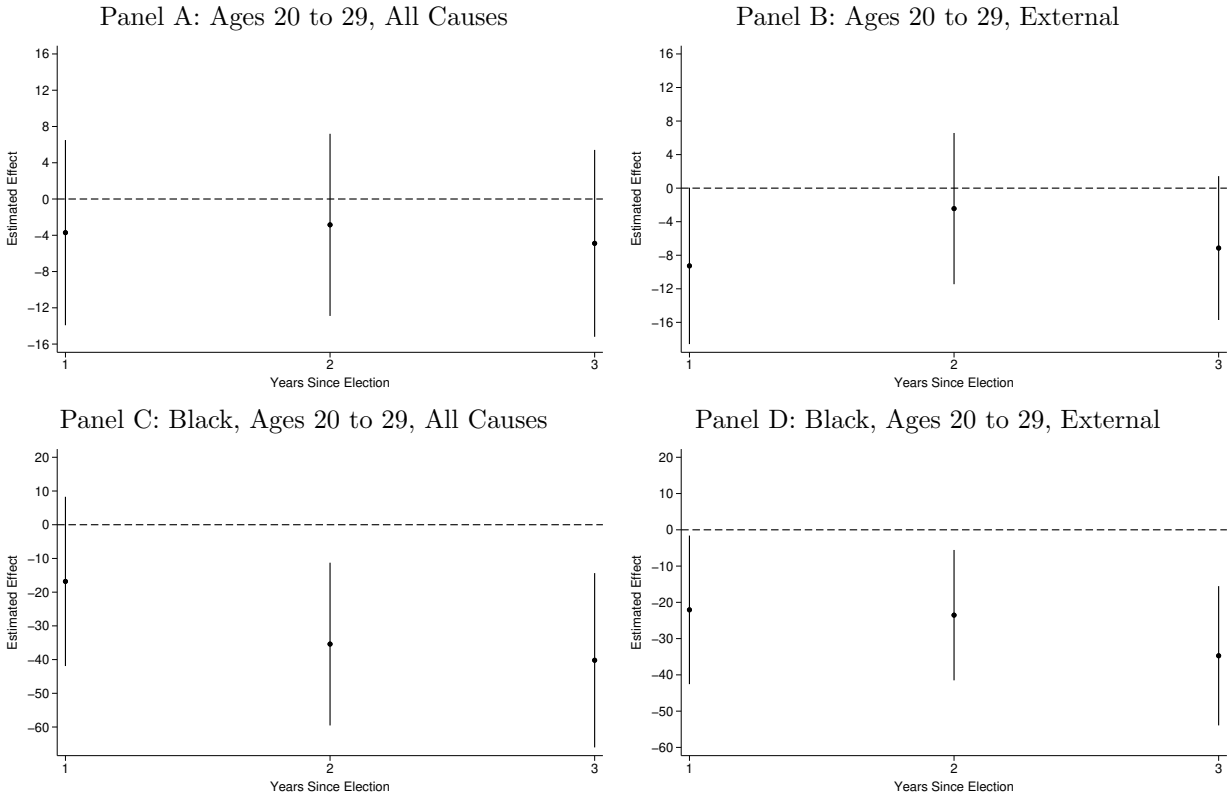
Notes: Each figure visualizes the distribution of estimates obtained through a permutation test. This test begins with the collection of all districts that possess a Republican prosecutor vote share value between 0.4 and 0.6 in at least one year from 2010 to 2019. We then fix the yearly outcome and control variable values for counties in these districts but randomly permute the observed vote share values across districts 1,000 times and plot the distribution of estimates produced by our preferred specification.

Figure A14: Effect of Narrow Republican Prosecutor Election on Jail Admission Rate



Notes: This figure visualizes the covariate-adjusted RDD estimate of the effect of narrow election of a Republican local prosecutor on annual county-level jail admission rates. See Figure 2 for notes on figure construction.

Figure A15: Effects of Narrow Republican Prosecutor Election on Mortality, By Years Since Election



Notes: These figures visualize RDD estimates of the effect of narrow election of a Republican local prosecutor that allow the effect to vary by the number of years since the election.