Local Prosecutors and Public Health*

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Abstract

This paper investigates the causal relationship 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 men ages 20 to 29 by 6.6%; this decline is driven exclusively by external causes of death and primarily by gun-involved homicides and suicides. Effects are heavily concentrated among young Black men. In exploring mechanisms, we show that narrow election of a Republican prosecutor leads to a substantial increase (18.6%) in new prison admissions among Black men but no other demographic group, suggesting incarceration-induced incapacitation of potential victims and perpetrators to be a key channel.

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In the United States, public safety and public health are inextricably linked. Homicide has consistently ranked among the top three causes of death for young Americans for decades (David-Ferdon et al., 2013), and the former U.S. Surgeon General recently declared gun violence to be an official public health crisis (Office of the Surgeon General, 2024). The opioid crisis, now driven by illicit narcotics like heroin and fentanyl, also remains a major public health emergency (McGreal, 2018).[1] Several other common causes of death also relate to public safety; 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 (Fitch et al., 2024) incarceration.

The strong link between these two areas raises important questions about the impacts of criminal justice practices on public health. In this paper, we study the causal link between community-level mortality and a notable but understudied source of variation in local criminal justice practice: criminal 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.[2] Prosecutors also have substantial discretion in their decision-making, leading many 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

[1]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).

[2]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).

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 leads to a reduction in all-cause mortality among the most criminal justice-involved demographic: young men ages 20 to 29 years old. This reduction is meaningful (9.7 fewer deaths per 100,000, or a 6.6% reduction) and is driven by a decline in deaths from two causes that predominantly involve firearms: homicide and suicide. A finding among only this 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).

Perhaps surprisingly, we find no effects on overall drug or opioid overdose mortality among any demographic groups. However, among the middle-aged, this null result obscures offsetting reductions in heroin overdose mortality and increases in prescription opioid overdose mortality. We further discuss these results and their connections to existing literature below.

Heterogeneity analysis reveals that the reduction in mortality among young men comes mainly from large reductions in homicide mortality among Black Americans. In exploring mechanisms, we leverage extensive, 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 men 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 clear effects on admissions to local jails, suggesting little change in policing behavior or general criminal deterrence effect.

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 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 mortality from internal causes (i.e., medical conditions and diseases), which should not reasonably be affected by local prosecutors. 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, varying the bandwidth used in estimation, and conducting statistical inference using permutation tests.

This paper contributes to multiple literatures, the first of which studies the connections between public safety and health. Several related papers 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—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 several of 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 and/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, which is consistent with two points. First, evidence shows that the criminal deterrence effects of incarceration spill over to others in close networks, families, and neighborhoods (Bhuller et al., 2018; Billings and Schnepel, 2022; Norris et al., 2021). Hence, incarceration of one individual reduces risky behavior, and thus mortality risk, among others as well. Second, individuals on the margin of gun violence involvement—the main cause of death driving our results—face high risks of both victimization and perpetration. 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. Hence, incarceration might reduce not only one's own mortality risk but in a subset of cases that of potential victims as well.

Our findings have important implications for our understanding of the social welfare effects of criminal prosecution and incarceration. On one hand, mortality is an important outcome, and the reductions we estimate are large enough to imply substantial welfare gains. However, a large volume of both popular and academic work spanning the social sciences and humanities notes how high levels of incarceration can be socially destructive.[3] From an economic perspective, incarceration is also financially costly (Mai and Subramanian, 2017), mechanically reduces cumulative earnings (Garin et al., 2025) and wealth (Humphries et al., 2025) during incapacitation, and may have even longer term costs as well (Mueller-Smith, 2015; Mueller-Smith and T. Schnepel, 2021).

This then leads to the question of how our findings compare to the effects of other public

[3]For example, see Alexander (2010) and Travis et al. (2014)

safety efforts on public health that are potentially less costly. Since our effects are driven by gun-involved deaths, a natural point of comparison is gun control policy. Evidence suggests that gun control measures can achieve similar reductions in community mortality without the social and economic costs of incarceration (Rudolph et al., 2015; Luca et al., 2017; Williams Jr, 2017; Donohue et al., 2019), although such policies, even when only anticipated and never implemented, can also backfire by spurring firearm sales in anticipation of looming restrictions (Bollman et al., 2025; Levine and McKnight, 2017).[4] Thus, our findings support two compatible conclusions: 1) existing estimates likely overstate the social costs of criminal prosecution by failing to fully account for community mortality reductions, and 2) there are other public safety avenues for reducing gun violence that, while not without complexities of their own, can achieve similar outcomes at substantially less social cost.

This paper also contributes to the literature on the opioid crisis and its intersection with the criminal justice system, wherein we find a likely substitution between misuse of illicit and prescription 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 (Alpert et al., 2018; Evans et al., 2019). Our opioid mortality results suggest a similar phenomenon, although in the opposite direction—efforts to reduce the supply of illicit opioids or the potential laxer justice system environment for licit ones may lead people to substitute to misuse of licit ones.

Our paper also contributes 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, 2023) and increase incarceration rates (Arora, 2019; Krumholz, 2020), especially for Black defendants (Yntiso, 2022). Several of these papers (Krumholz, 2020; Fischer and Ludwig,

[4]Luca et al. (2017) show that waiting period laws that delay firearm purchases by a handful of days reduce gun homicides by $\approx 17\%$; coincidentally, our effects on gun mortality among young men are of the same magnitude.

2023) along with Agan et al. (2021) also investigate the relationship between prosecutorial approaches and crime rates and find no clear relationship. Our mechanisms analysis leverages data from more districts than are analyzed in this existing work and largely fits with their findings.

More importantly, 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. Given their latitude in decision-making, legal scholars have long argued that prosecutors fundamentally shape outcomes in their districts (Bellin, 2019), and we provide evidence on the existence and extent of this influence. 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.

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.

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.[5] 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.[6] 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 initial charges levied is also left to the prosecutor.

Conditional on the decision to prosecute, prosecutors next have input on bail and pretrial detention decisions. These decisions are ultimately made by judges, but prosecutors typically make recommendations, and evidence suggests these recommendations are often

[5]The official names given to local prosecutors vary across states and occasionally within states; for example, Kansas has 105 total prosecutorial districts, 99 of which are served by "county attorneys" and 6 of which are served by "district attorneys." However, there are no material differences between these two positions in Kansas. In Texas, some counties divide the task of criminal prosecution between two offices: the county attorney and the district attorney. In these cases, the county attorney handles misdemeanor and some civil cases while the district attorney prosecutes felony cases. Our analysis focuses on the district attorneys in these situations, but our results are robust to differing treatments and to the omission of Texas entirely.

[6]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).

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.[7] 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. While term lengths vary across states, the vast majority are four years. Table A1 also shows the timing of these elections 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. Overall, the timings of these elections are quite varied across states.

Our data on local prosecutor elections cover thirty-four of the thirty-eight states with partisan prosecutor elections from 2010 to 2019. We conduct the analysis using county-by-year observations, and our key variable is the Republican candidate vote share in the most

[7]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.

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.[8] When a candidate runs unopposed in the general election, the vote share variable will be coded as 0 (unopposed Democrat) or 1 (unopposed Republican).[9]

As in any vote share cutoff design, our analysis only includes county-by-year observations where the most recent election vote share value is reasonably close to the 50 percent cutoff. Our baseline analysis includes observations with a value between 42 and 58 percent; we explain this choice in Section II and later show that the findings are robust to both wider and more narrow choices. 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 prosecutor vote share value between 42 and 58% 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: racial demographics, urban/rural population shares, unemployment and poverty rates, the fraction of residents without health insurance, and education

[8]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.

[9]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).

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 520 counties that contribute one or more observations in our analysis. These counties reside within a total of 368 electoral districts and contain about 23% of the total U.S. population.[10] The first several rows of Table 1 show no discernible differences between in- and out-of-sample counties in terms of race, age, and urban/rural populations. Counties with closely contested prosecutor elections had a slightly lower baseline unemployment rate (9.16 vs 9.89), 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 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.).[11]

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

[11] 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 US 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 CDC data identify causes of death using the 10th revision of the International Classification of Diseases (ICD-10). We first partition causes of death 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 partition external causes into five categories and analyze each separately: homicides, drug poisonings, non-poisoning suicides, motor vehicle accidents, and other external causes. We also explore effects on gun mortality - this category includes all homicide, suicide, and accidental deaths involving a firearm.

Finally, we examine opioid overdose mortality in detail. 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. 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 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.[12] Figure A3 also summarizes trends in homicide and gun 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.

the ten-year groups.

[12]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).

I.4 Local Jail and Prison Data

Our data on local jails and prisons comes from the Vera Institute for Justice's Incarceration Trends (VIJIT) dataset, which uses several sources to calculate county-by-year correctional facility admission and population rates. We use the May 2025 release, which is the third iteration of the dataset and the latest version at the time of writing.

The VIJIT jail data is constructed from three sources, all of which stem from the Bureau of Justice Statistics: the Annual Survey of Jails, the Census of Jails, and the Death in Custody Reporting Program. The prison data is derived from the National Corrections Reporting Program (NCRP) and—when NCRP data is unavailable or unreliable—data collected directly from state departments of correction. Importantly, this prison data links sentences to the court which issued them, allowing us to track the number of people sentenced to prison by the courts operating in a given county and year. This structure is well-suited for our analysis since we seek to connect prison admissions to prosecutors, and data that instead tracked prison admissions in terms of the county of the prison facility itself would make this difficult.

Table A3 conveys both the variables available in the VIJIT data and the data coverage across our sample. Information is available on both facility admissions and populations (i.e., on both inflows and stocks) as rates per 100,000 residents ages 15 to 64 years old.[13] We focus mainly on admissions as they better capture the influence of the current prosecutor, but as we will show, the results convey the same narrative when facility populations are used.

Data coverage for the counties in our estimating sample is incomplete but quite high—jail and prison admission rates are available for 96% and 87%, respectively, of all county-by-year observations from 2010 to 2019 with a closely contested vote share value. Facility admission rates are also available by race and sex in some cases. Jail admissions can be broken down by sex (but not race), and prison admissions are available by race, sex, and race-sex combinations. Data coverage is sparser for these demographic specific rates; for example,

[13]The VIJIT data only includes prison admissions corresponding to sentences issued for at least one year.

prison admission rates among Black individuals and Black men are only available for about 48% of our estimating sample observations. However, the missing data is concentrated among smaller counties, and measuring data coverage in terms of the fraction of the total population within our estimating sample covered reveals much higher rates. For example, the county-by-year observations available for Black prison admissions covers 90.1% of the total Black population within our estimating sample, and since our analysis weights by population, the loss of information for smaller counties should not meaningfully affect the results.[14]

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 a change in prosecutorial approaches 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.

We use a vote share regression discontinuity design, an approach commonly used to uncover causal estimates in settings with elected officials, to identify the causal effect of a change in prosecutorial approaches. 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

[14]Table A4 shows data coverage by state. Most of the missing data on prison admissions is driven by a lack of any data from Idaho, Vermont, and Virginia; prison data is also sparse in West Virginia and Wisconsin as well.

running variable, R_{dt}, equals the Republican vote share in the most recent election in electoral district d as of 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.

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

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. 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.[15] δ_s and γ_t are sets of state and year fixed effects, respectively. We include these baseline county characteristics and fixed effects to explain variation in mortality rates across counties and thus improve the precision of our estimates.[16] Our preferred specification uses county-level population weights, both so that our estimate reflects an average effect weighted equally across individuals (instead of weighted equally across counties with vastly different populations) and to improve precision (Solon et al., 2015). We also weight using a triangular kernel weighting function and cluster our standard errors at the district level.

We estimate our regressions using 8 percentage point wide bandwidths on each side of the cutoff; i.e., using observations where $42 \leq R_{dt} \leq 58$. Popular data-driven processes (Cattaneo et al., 2019) consistently suggest bandwidths between 6 and 12 percentage points in our estimates; we opt to use a single value within this range across all regressions to ensure

[15] 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.

[16] 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.

consistency in the estimating sample. In Section III.2, we show that our key findings remain of similar magnitude and precisely estimated when these bandwidths are shrunk or enlarged.

For statistical inference, we report both conventional standard errors calculated using a second order robust plug-in residuals variance estimator and robust bias-corrected (RBC) p-values (Calonico et al., 2014). Though they are often quite similar, RBC p-values vary slightly from conventionally calculated ones and thus may imply a different level of statistical significance. Our key findings are precise under either approach.[17]

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 county-level 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 the cutoff. Sec-

[17]Estimation of the RBC p-value uses 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 RBC p-values is also a valuable check on the sensitivity to the polynomial order used. Estimation of RBC p-values also often leverages observations outside of the bandwidths used to obtain the point estimates. In this context, the data-driven selection methods generally suggest inference bandwidths about twice as large as the point estimates ones, a choice we then consistently use for all estimates.

ond, 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.[18] The estimate is small and statistically insignificant, conveying that the party affiliation of sheriffs is not confounding our estimates.

III Results

III.1 Main Results

Table 3 presents RDD estimates of the effect of narrow election of a Republican prosecutor on mortality from all causes by age group and sex. Panel A examines effects on men, and Column (1) shows a precisely estimated reduction in mortality among men ages 20 to 29 (here forward referred to as "young men") of 9.7 fewer deaths per 100,000 people per year. This constitutes a 6.6% reduction from the baseline control mortality rate of 147.6 deaths per 100,000.[19] 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 men 30 years old or older. Panel B also reveals no significant effects on women in any age group, furthering the conclusion that effects are concentrated among young men.[20]

Table 4 explores the specific causes of death that drive the reduction in mortality among young men. Column (1) shows that the effect on all-cause mortality is driven entirely by reductions in death from external causes (10.6 fewer deaths per 100,000 people per year). Columns (2)-(6) further partition external causes into five mutually exclusive categories: homicide, drug poisonings, non-drug suicides, motor vehicles accidents, and other external

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

[19] This estimate and others among young men remain virtually unchanged when we broaden the group to include 18-19 year olds as well.

[20] Appendix Table A5 presents estimates that do not split the sample by sex and reveals that the reduction in mortality among young men is large enough to generate a precisely estimated reduction in mortality among all 20-29 year olds of 5.94 fewer deaths per 100,000.

causes. Homicides see the biggest and most precisely estimated decline (3.9 fewer deaths per 100,000), and suicides also fall by 2.3 deaths per 100,000. Effects on motor vehicle and other external cause mortality are both negative but imprecise, and the effect on drug poisonings is essentially zero. Column (7) pools together all causes involving guns (i.e., firearm homicides, accidents, and suicides) and shows a reduction of 6.9 deaths per 100,000, or about 18% of the baseline mean.[21]

Figure 2 visualizes the effects on mortality among young men by cause. Each figure plots an RDD estimate obtained after first residualizing the mortality rate of our regression controls; these visualized estimates are not mechanically identical to our regression results (which are restated in the corner of each panel) but are exceedingly close given covariate balance across the vote share threshold. Each figure reveals a clear discontinuous drop across the vote share threshold, with the most distinguishable effects among homicide (Panel C) and gun (Panel D) mortality.

The null result on drug poisonings among young men is perhaps surprising given the scale of the opioid crisis in the US during the 2010s. Figure 3 investigates whether there are effects on opioid overdose mortality among other age groups. We again find no effects on overall opioid overdose mortality among Americans ages 30 and older; however, estimates that focus on specific types of opioids reveal a more nuanced story. Among those 30 and older, we consistently see prescription opioid overdose deaths increase by about 2 per 100,000 and heroin deaths decrease by the same amount. These effects offset, and in combination with a null effect on fentanyl overdose mortality (see Figure A6), lead to an overall null effect on opioid overdose mortality.

Returning to effects on young men, Table 5 investigates whether any subgroups are driving the results. Panel A examines effects on mortality among young Black men, and Column (1) shows that narrow election of a Republican local prosecutor reduces mortality from external causes among this group by 27.6 deaths per 100,000 (a 16.4% reduction). This comes

[21]Table A6 examines effects on mortality from internal causes by age group and sex. Seven out of eight coefficients (including all four for men) are statistically insignifcant.

mainly from a substantial reduction in homicide mortality (20.3 fewer deaths per 100,000, see Column (2)), and there is also a smaller reduction in the residual external cause category (Column (6)). Figure 4 visualizes several of these estimates. Panel B of Table 5 focuses on white men, and Column (1) reveals a marginally statistically insignificant reduction in external cause mortality among this group, driven by a precisely estimated reduction in suicides (3.4 fewer per 100,000). As with Table 4, Column (7) from Table 5 again suggests gun mortality as a main driver of the results.[22]

III.2 Placebo and Robustness Exercises

In this section, we probe the credibility of our findings with several robustness and placebo tests. To start, Table A7 presents our preferred specification external cause mortality estimates for men along with several other specifications. Our preferred specification—which includes state and year fixed effects, baseline county-level demographics and mortality rates, and uses population weights, as appeared in Column (1) of Table 4—is shown again in Column (6). Column (1) is estimated without any covariates, and Columns (2) and (3) add in state and year fixed effects, respectively. Comparing these estimates to Column (6) shows that 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 Table A7 includes covariates but does not use population weights. The population weights improve precision (in line with theory, see Solon et al. (2015)), and it

[22]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 A7 visualizes the samples of counties that contribute observations after 2016 only, in 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.

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 A8, 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 A9 investigates whether our key findings among young men are robust to different bandwidth choices. Here we again focus on external cause mortality. Our baseline estimates use 8 percentage point (p.p.) bandwidths on each side of the cutoff, and Figure A9 shows how the estimates change when these bandwidths are changed one p.p. at a time from to 4 to 12 p.p. Reassuringly, the main estimates are not especially sensitive to deviations from our baseline bandwidth choice.

We now turn to placebo checks. Our research design relies on the key assumption that the 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 A8 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 check whether our estimation procedure

overstates the statistical precision of our estimates. 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 (at the district level) across districts 1,000 times and plot the distribution of test statistics produced by our preferred specification. The results are shown in Figure A10. The true test statistics fall in the left tails of the respective distributions, and the corresponding equivalents of two-tailed test p-values suggest that our main findings would be unlikely to be obtained were the vote share values in reality orthogonal to our outcome.

IV Mechanisms

IV.1 Results

We now explore the mechanisms that might explain our mortality results. We first investigate whether Republican prosecutors secure more convictions resulting in prison sentences than the counterfactual non-Republican prosecutor. Figure 5 presents RDD estimates of the effect of narrow Republican local prosecutor election on annual county-level admissions to prisons. Panel A reveals no effect on the prison admission rate among all Americans ages 15 to 64; similarly, Panels B through D show no effects on admission rates for men, white Americans, or white men. However, Panel E suggests that narrow election of a Republican local prosecutor increases annual prison admissions among Black Americans by 196.4 admissions per 100,000. Panel F focuses on Black men in particular and reveals an increase of 329.4 admission per 100,000 among this group, which constitutes a 18.6% increase over the baseline average.[23]

We also explore effects on admissions and populations of local jails. Nationwide, two

[23]Figure A11 examines effects on prison populations instead of prison admissions and reveals similar patterns. Table A9 varies the specification choices to check for robustness of the effects on Black Americans in the same manner as Table A7.

thirds of individuals in jails are awaiting their prosecution or potential sentencing and have not been convicted of a crime, while one third are completing short sentences (Dholakia, 2023). The channels through which prosecutors affect jail admissions and populations are less direct than prison (police most directly influence jail admissions), and ex-ante predictions surrounding how prosecutorial affiliation might affect these variables are also less clear. To illustrate, police might increase arrests under a more aggressive prosecutor with the belief that those arrested will be more likely to be prosecuted, which would increase jail admissions. However, jail admissions could also go down under a more aggressive prosecutor if prosecution or increased pretrial detention leads to longer jail stays on average, thus mechanically reducing the likelihood of a given person repeatedly returning to jail within a given time span (i.e., reducing what some term a "revolving door," where arrested but non-prosecuted individuals cycle in and out of jail). More aggressive prosecutors might also produce a general criminal deterrence effect whereby potential offenders are dissuaded from engaging in criminal activity in the first place. We note that these could lead to outcomes where jail admissions and jail populations move in opposite directions; e.g., admissions would fall and populations would rise should jail become less of a revolving door under a more aggressive prosecutor.

Our findings on local jails are presented in the Appendix in Figure A12. We find no clear effects on jail admissions among the overall population, nor among men specifically. In terms of jail population, we again find no effects measured broadly. We do estimate a statistically significant increase in jail populations for white Americans and not for Black Americans; however, the point estimates for each group are very similar and only differ precision-wise, making any conclusions about racial heterogeneity difficult. Overall, these findings—in combination with our mortality results—suggest that prison sentences are likely the strongest channel through which prosecutorial approaches affect community-level mortality.

IV.2 Incarceration & Mortality

Incarceration likely affects community-level mortality through two broad channels: it changes both the incarcerated person's own mortality risk (what we'll call the "direct channel") as well as the mortality risk of others in the community (the "indirect channel"). A natural question is whether our mortality findings among young Black men can 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, Panel F from Figure 5 suggests increased annual prison admissions among Black men ages 15 to 64 by 329.45 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 20% of the 15 to 64 year old age group; thus, the better estimate is likely $(329.45)(\frac{0.4}{0.2}) = 658.9$ additional annual prison admissions per 100,000 among young Black men.

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.[24] Then the assumption of effects solely on the incarcerated would suggest that prison admission reduces the likelihood of death in a given year by $\frac{31}{615.2}(100) = 4.7$ 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.[25]

Another approach would be to assume that each prison admission lasts several years and

[24]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.

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

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 $658.9(4+3+2+1)(0.0055) = 36.2$ deaths per 100,000. Our mortality point estimate suggests a cumulative $(4)(31) = 124$ fewer deaths across this four year term.[26]

Thus, regardless of the assumption about the length of these prison sentences, this exercise 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, where local proseuctors are consdiered an under-studied but highly powerful facet of the criminal justice system. We exploit plausibly exogenous variation in prosecutorial approaches generated by closely contested partisan elections to estimate the impact of Republican versus Democrat local prosecutors on health outcomes. We find that narrow election of a Republican prosecutor reduces all-cause mortality among young men aged 20 to 29, the age group most often interacting with the criminal justice sys-

[26]In line with this thinking, Figure A13 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.

tem. Examining causes of mortality, we find that reductions are fully driven by reductions in external causes, in particular concentrated in homicide and gun mortality.

We observe that effects are 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. However, we find that the effects we observe likely cannot be solely explained by mortality reductions among the incarcerated, suggesting that increased incapacitation might have an impact on outcomes in the community as well. We find no overall effects on opioid overdose mortality but some evidence of offsetting effects on prescription and illicit opioid overdoses, with increases in the former and reductions in the latter. We conclude that the narrow election of a Republican prosecutor leads to significant declines in young men's external cause mortality, coupled with increases in the incarceration rate of young Black men, suggesting that local prosecutors have a substantial impact on the wellbeing of their communities.

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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
% Non-Hispanic White Male	30.07	29.19	0.88
% Non-Hispanic White Female	29.16	28.42	0.74
% Non-Hispanic Black Male	6.68	6.85	-0.17
% Non-Hispanic Black Female	6.78	6.93	-0.15
% Hispanic Male	10.97	10.71	0.26
% Hispanic Female	9.61	9.34	0.27
% Adult Population 20-29	24.90	25.22	-0.32
% Adult Population 30-39	24.15	23.87	0.28
% Adult Population 40-49	26.10	25.98	0.12
% Adult Population 50-59	24.85	24.93	-0.08
ln(Adult Population)	12.47	12.23	0.24
% Urban Population	76.49	71.63	4.86
% Rural Population	16.32	18.93	-2.61
Unemployment Rate	9.16	9.89	-0.73***
Poverty Rate	14.55	15.40	-0.85
% No Health Insurance	21.73	21.49	0.24
Less than High School	13.45	14.63	-1.18
High School	27.53	27.94	-0.41
Some College	29.46	28.76	0.70
Opioid Pill Volume/Capita	40.79	36.01	4.78**
% of Total U.S. Population	0.229	0.771	
N (Counties)	520	2612	
N (Districts)	368	1960	

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 where the Republican vote share from the most recent prosecutor election fell between 42% and 58% in at least one year between 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% Vote Share Threshold

	(1) Narrow GOP Loss	(2) Narrow GOP Win	(3) Difference	(4) Robust BC p-Value	(5) N
Geography					
Census Division 3	16.46	12.60	-3.86	0.764	2149
Census Division 5	17.48	7.97	-9.51	0.549	2149
Census Division 7	47.12	59.31	12.19	0.703	2149
Census Division 8	9.75	9.94	0.19	0.872	2149
Other Census Division	9.18	10.17	0.99	0.739	2149
McCain State	58.46	68.61	10.15	0.726	2149
2010 County-Level Characteristics					
% Non-Hispanic White Male	0.25	0.21	-0.05	0.455	2149
% Non-Hispanic White Female	0.25	0.20	-0.05	0.351	2149
% Non-Hispanic Black Male	0.07	0.09	0.01	0.301	2149
% Non-Hispanic Black Female	0.08	0.09	0.02	0.332	2149
% Hispanic Male	0.16	0.19	0.03	0.596	2149
% Hispanic Female	0.14	0.16	0.02	0.719	2149
% Adult Population 20-29	0.27	0.26	-0.01	0.239	2149
% Adult Population 30-39	0.25	0.26	0.01	0.272	2149
% Adult Population 40-49	0.25	0.25	0.00	0.511	2149
% Adult Population 50-59	0.23	0.22	-0.01	0.775	2149
ln(Adult Population)	12.85	13.56	0.70	0.308	2149
% Urban Population	82.35	87.45	5.10	0.610	2149
% Rural Population	12.47	8.75	-3.72	0.592	2149
Unemployment Rate	9.11	9.08	-0.03	0.849	2149
Poverty Rate	17.51	16.95	-0.56	0.631	2149
% No Health Insurance	27.56	29.29	1.74	0.788	2149
Less than High School	16.98	18.39	1.41	0.649	2149
High School	27.09	25.14	-1.96	0.307	2149
Some College	29.16	27.37	-1.79	0.165	2149
Opioid Pill Volume/Capita	43.40	36.40	-7.00	0.294	2149

Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on fixed or pre-determined state and county-level characteristics. "McCain state" denotes a state won by John McCain in the 2008 US presidential election. 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 and Column (5) provides the effective number of observations used in estimation. All estimates use 8 percent point bandwidths on each side of the cutoff. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Effects of Narrow Republican Prosecutor Election on Mortality from All Causes

Panel A: Men

	(1)	(2)	(3)	(4)
	20-29 Year Olds	30-39 Year Olds	40-49 Year Olds	50-59 Year Olds
	-9.7***	-1.5	-3.3	-3.1
	(3.3)	(5.6)	(7.4)	(13.2)
p-value	0.003	0.693	0.622	0.837
Control Mean	147.6	193.5	345.3	815.7
N	2149	2149	2149	2149

Panel B: Women

	(1)	(2)	(3)	(4)
	20-29 Year Olds	30-39 Year Olds	40-49 Year Olds	50-59 Year Olds
	-1.6	4.0	-5.3	-10.1
	(1.6)	(2.8)	(6.6)	(8.5)
p-value	0.309	0.216	0.518	0.223
Control Mean	56.8	107.0	217.7	495.0
N	2149	2149	2149	2149

Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates from all causes. Panel A examines effects on men and Panel B focuses on women. Each column examines effects 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 preferred specification choices outlined in Section II. The control mean row provides the intercept to the left of the cutoff from our preferred RDD specification but without covariates. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Effects of Narrow Republican Prosecutor Election on Mortality from External Causes Among Men Ages 20-29 Years Old

	(1) All External Causes	(2) Homicide	(3) Drug Poisonings	(4) Suicide	(5) Motor Vehicle	(6) Other External	(7) Guns
	-10.6***	-3.9***	-0.5	-2.3**	-1.2	-2.2	-6.9***
	(3.0)	(1.5)	(2.6)	(1.1)	(1.2)	(2.6)	(1.8)
p-value	0.001	0.008	0.775	0.046	0.345	0.347	0.001
Control Mean	116.9	24.6	27.9	25.8	27.3	34.6	38.3
N	2149	2149	2149	2149	2149	2149	2149

Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates among men ages 20 to 29 ("young men"). Column (1) examines effects on mortality from all external causes. Columns (2)-(6) partition external causes into five categories - homicide, drug poisonings, non-drug suicides, motor vehicles accidents, and other - and examine effects on each separately. Column (7) focuses on any death involving a gun. See Section I.2 and the Appendix for more information on cause of death classification. All estimates are obtained using the preferred specification choices outlined in Section II. The control mean row provides the intercept to the left of the cutoff from a regression without covariates. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Effects of Narrow Republican Prosecutor Election on Mortality from External Causes Among Black and White Men Ages 20-29 Years Old

Panel A: Black Men

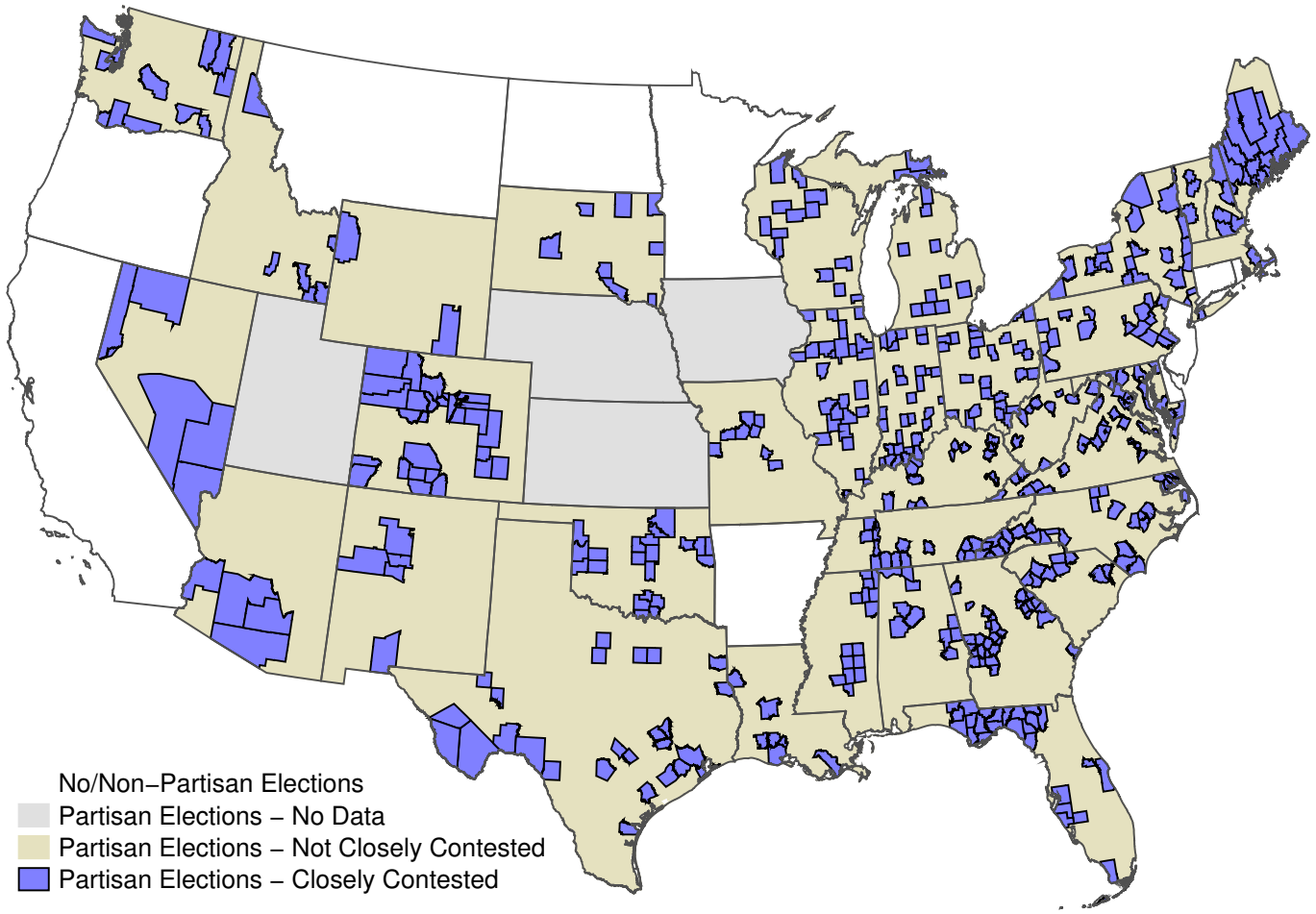
	(1) All External Causes	(2) Homicide	(3) Drug Poisonings	(4) Suicide	(5) Motor Vehicle	(6) Other External	(7) Guns
	-27.6***	-20.3***	3.3	-0.3	-2.6	-5.8**	-23.0***
	(7.3)	(5.3)	(2.1)	(1.2)	(2.7)	(2.4)	(5.9)
p-value	0.001	0.001	0.138	0.519	0.333	0.020	0.001
Control Mean	167.9	91.8	12.7	19.2	29.1	26.8	101.5

Panel B: White Men

	(1) All External Causes	(2) Homicide	(3) Drug Poisonings	(4) Suicide	(5) Motor Vehicle	(6) Other External	(7) Guns
	-5.5*	0.6	-1.2	-3.4**	0.0	-0.9	-3.4**
	(3.4)	(1.1)	(3.0)	(1.3)	(1.2)	(3.1)	(1.5)
p-value	0.099	0.582	0.596	0.017	0.954	0.672	0.017
Control Mean	112.1	11.8	32.6	28.4	28.4	37.9	27.2

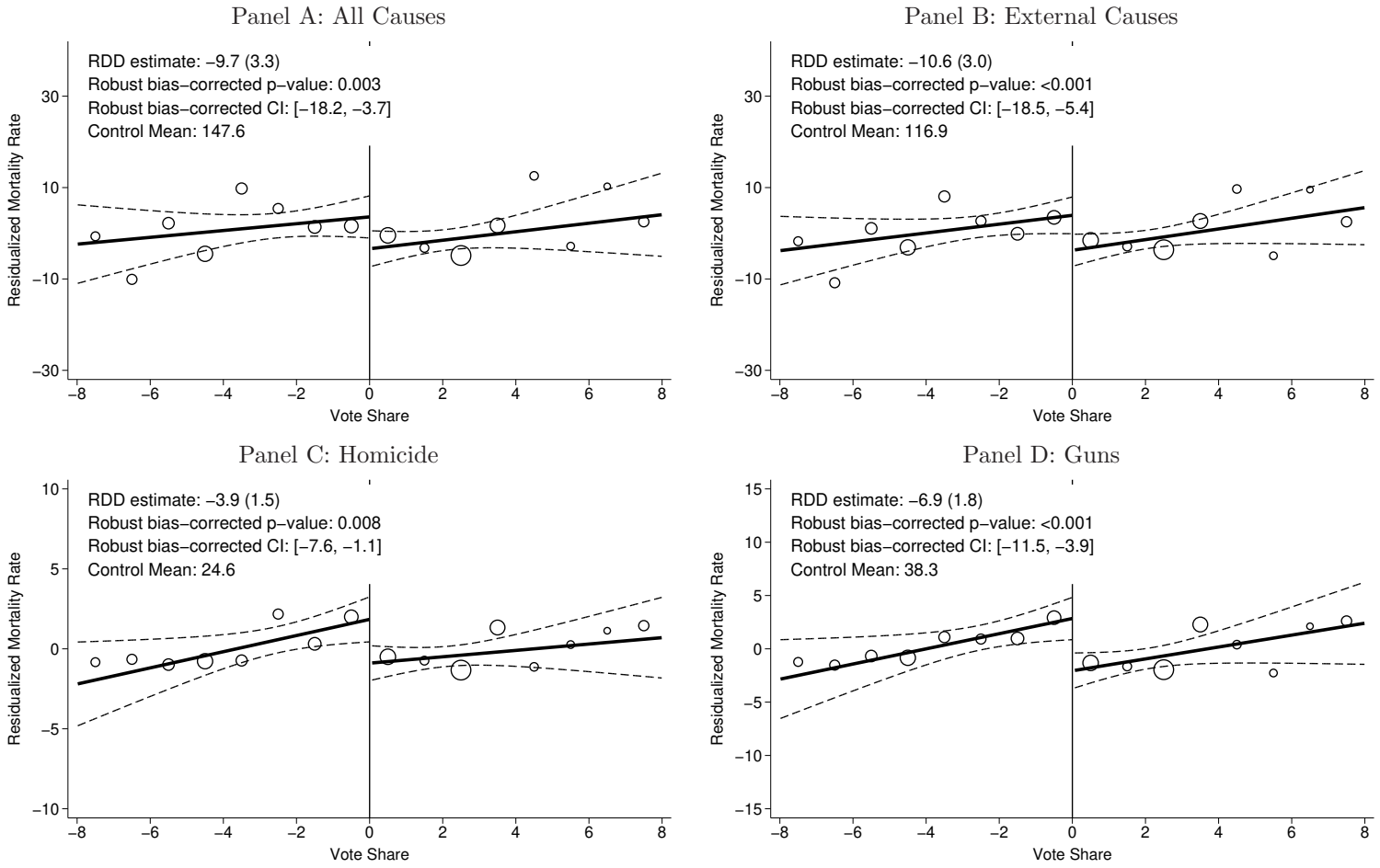
Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates among men ages 20 to 29 ("young men"). Panel A focuses on Black men and Panel B focuses on White men. Column (1) examines effects on mortality from all external causes. Columns (2)-(6) partition external causes into five categories - homicide, drug poisonings, non-drug suicides, motor vehicles accidents, and other - and examine effects on each separately. Column (7) focuses on deaths involving guns. See Section I.2 and the Appendix for more information on cause of death classification. All estimates are obtained using the preferred specification choices outlined in Section II. The control mean row provides the intercept to the left of the cutoff from a regression without covariates. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

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



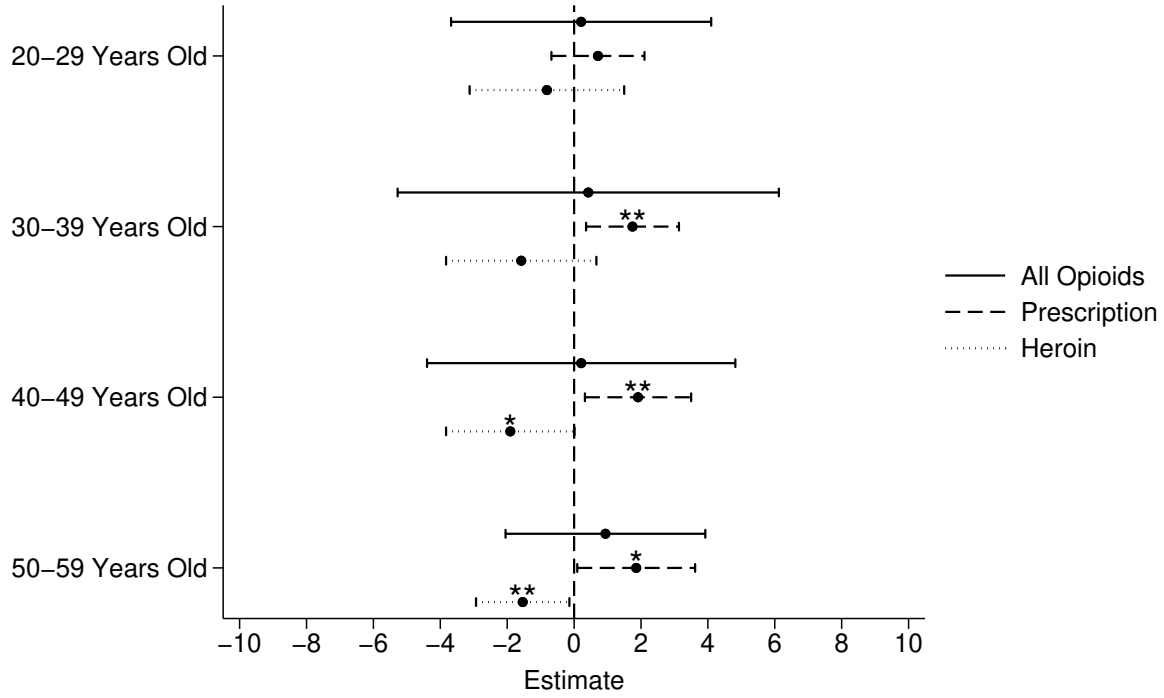
Notes: This map visualizes the counties which contribute to our regression samples. States shaded white are states where local prosecutors are either appointed or elected in non-partisan elections. States shaded light gray are states where local prosecutors are elected in partisan elections, but we do 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 where the Republican vote share from the most recent prosecutor election fell between 42% and 58% in at least one year between 2010 to 2019.

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



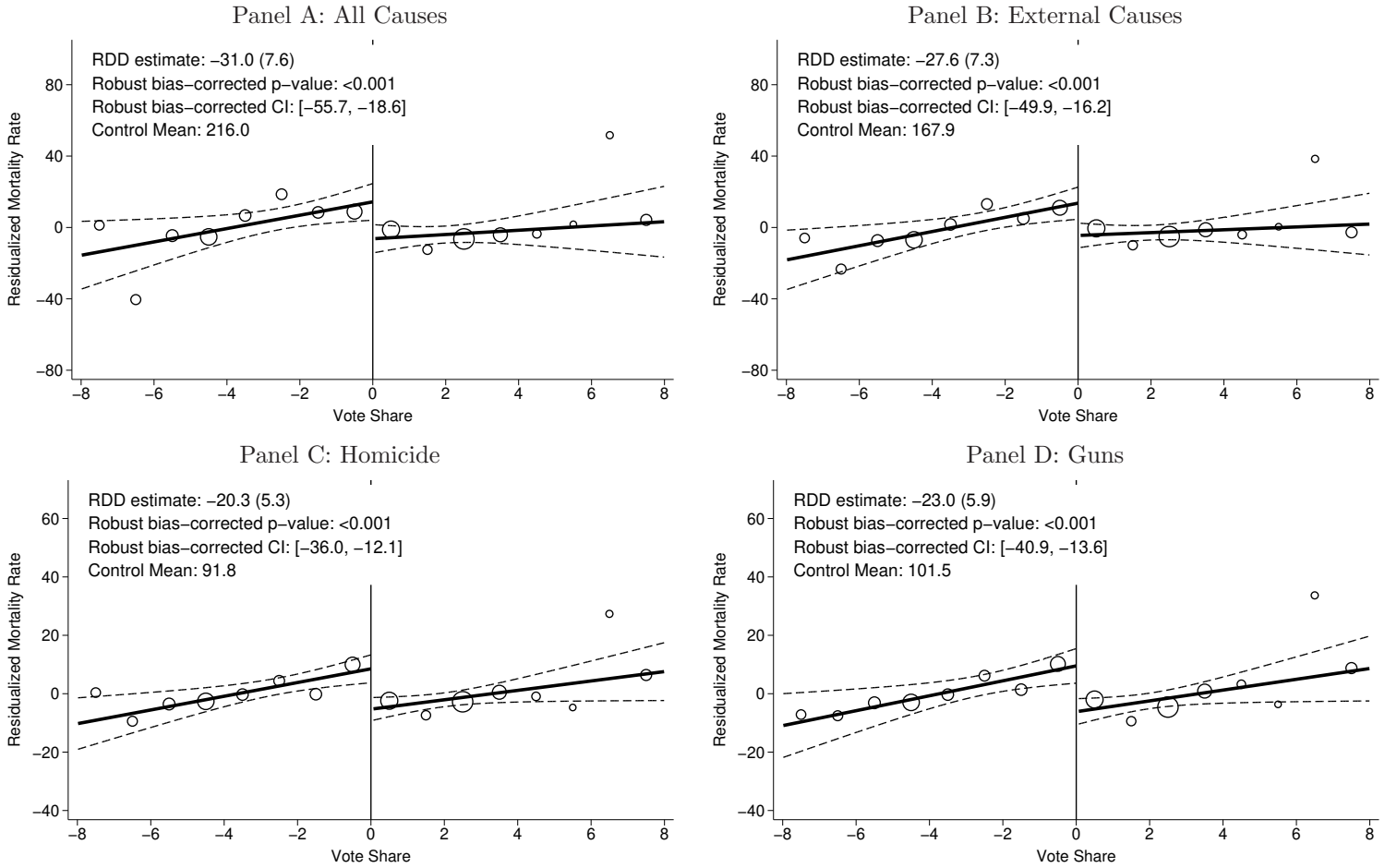
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality rates among men ages 20-29 years old. The information in the corner of each panel comes from estimation of Equation 1, and each plot visualizes RDD estimates obtained by first residualizing the mortality rates of the covariates from Equation 1. The circles visualize average residualized mortality rates within one percentage point wide bins on each side of the cutoff. The size of each circle corresponds to the total population across all observations within the bin. The thick solid lines 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 Opioid Mortality



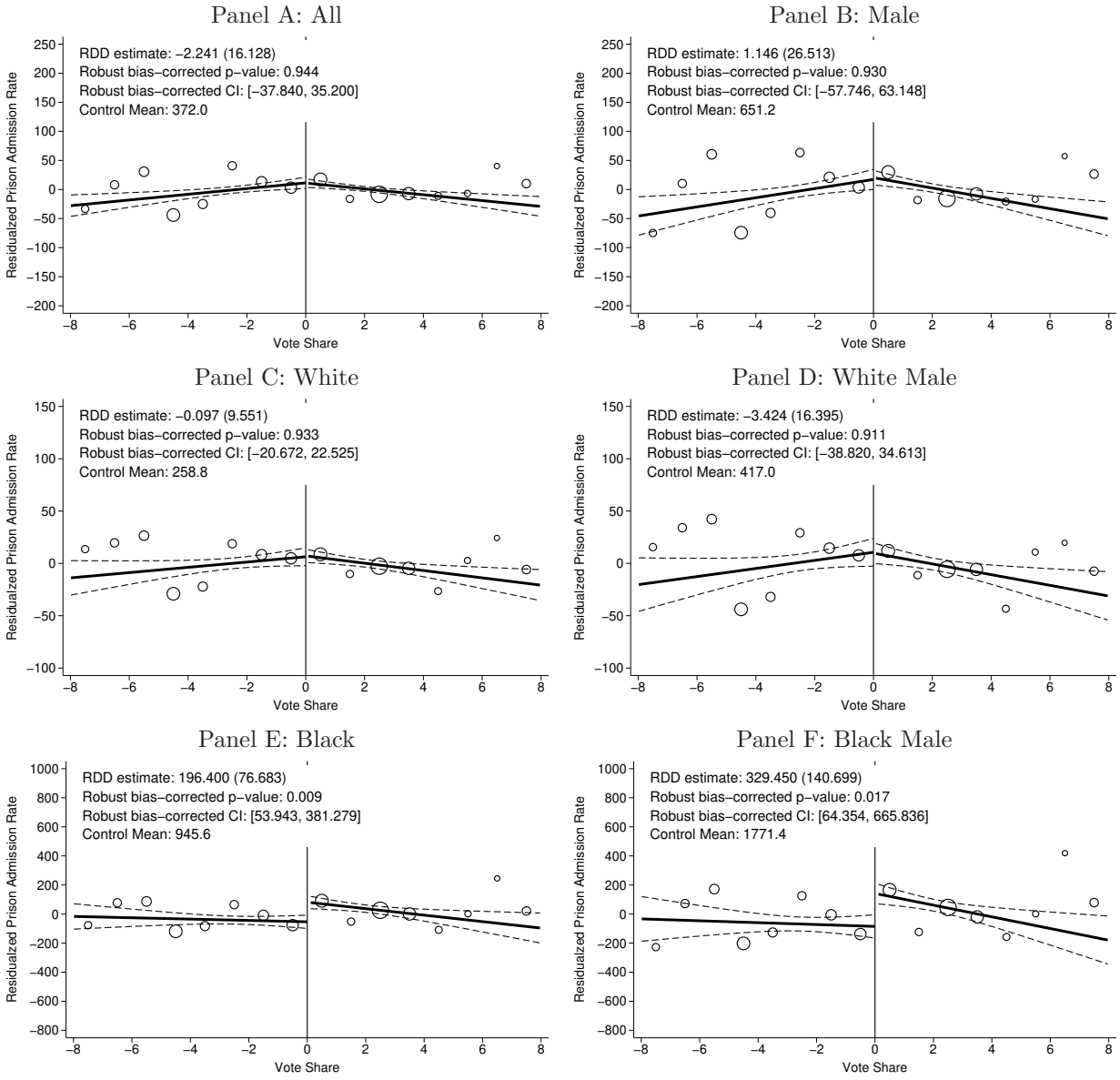
Notes: This coefficient plot conveys RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality due to opioid overdoses across ten year age groups. Each coefficient and corresponding conventional 95% confidence comes from a separate regression estimated using our preferred 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 4: Effects of Narrow Republican Prosecutor Election on Mortality Among Black Men 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 Black men ages 20-29 years old. The information in the corner of each panel comes from estimation of Equation 1 and each plot visualizes RDD estimates obtained by first residualizing the mortality rates of the covariates from Equation 1.

Figure 5: 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 prison admission rates among different demographic groups. The information in the corner of each panel comes from estimation of Equation 1 and each plot visualizes RDD estimates obtained by first residualizing the outcome of the covariates from Equation 1.

A Mortality Data

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:

- Internal Causes (Diseases/Conditions):
  - Major cardiovascular diseases: I00-I78
  - Malignant neoplasms (cancer): C00-C97
  - 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	Marion, IN	250358
16	Pima, AZ	239841
17	Erie, NY	230496
18	Montgomery, PA	220661
19	Baltimore, MD	211981
20	Will, IL	207033

Notes: This table lists the twenty largest counties (in terms of population) that possess a Republican prosecutor vote share value between 42% and 58% in at least one year from 2010 to 2019, along with the 2010 middle-aged population.

Table A3: Prison Admission Rate Data Availability

	Jail Admissions		Jail Population		Prison Admissions		Prison Population	
	% Observations Covered	% Population Covered	Observations Covered	% Population Covered	% Observations Covered	% Population Covered	% Observations Covered	% Population Covered
Total	96.2	98.7	96.1	98.7	87.1	92.5	91.9	94.3
Black	-		86.4	98.0	48.4	90.1	57.3	92.5
White	-		92.1	96.5	63.9	87.4	73.2	91.4
Men	93.5	97.0	94.7	98.1	72.3	90.5	79.1	93.0
Women	93.5	96.9	92.4	98.0	70.8	90.5	77.7	92.9
Black Men	-		-		47.4	88.5	57.0	91.3
White Men	-		-		63.3	86.5	72.9	90.5

Notes: This table presents sample information on the incarceration data used in this paper.

Table A4: Prison Admission Rate Data Availability, By State

	Jail Admissions	Prison Admission	Prison Admissions, Black	Prison Admissions, Black Men
AL	97.8	100.0	69.6	69.6
AZ	100.0	87.5	62.5	62.5
CO	93.9	93.3	20.7	20.7
FL	100.0	100.0	67.8	67.8
GA	86.2	98.5	48.5	48.5
ID	61.9	-	-	-
IL	99.0	97.1	64.4	63.5
IN	99.3	100.0	35.6	35.6
IA	96.5	94.7	35.1	35.1
KS	100.0	77.8	77.8	77.8
KY	100.0	100.0	38.9	38.9
LA	100.0	16.1	16.1	-
ME	97.8	100.0	28.9	20.0
MD	100.0	79.3	72.4	58.6
MA	100.0	92.3	84.6	84.6
MI	89.3	89.3	62.5	62.5
MS	97.5	98.8	61.3	61.3
MO	100.0	100.0	62.5	62.5
NV	100.0	91.3	52.2	52.2
NH	100.0	81.0	38.1	28.6
NM	81.8	68.2	27.3	27.3
NY	94.1	100.0	74.5	74.5
NC	97.6	100.0	71.4	71.4
OH	93.6	100.0	73.6	73.6
OK	96.1	93.5	33.8	33.8
PA	89.4	100.0	68.2	68.2
SC	100.0	100.0	70.8	70.8
SD	100.0	91.7	29.2	29.2
TN	100.0	100.0	47.1	47.1
TX	97.1	90.5	62.9	62.9
UT	100.0	100.0	66.7	66.7
VT	100.0	-	-	-
VA	100.0	-	-	-
WA	100.0	100.0	40.7	40.7
WV	100.0	37.8	37.8	24.3
WI	100.0	35.7	21.4	21.4
WY	100.0	100.0	33.3	33.3

Notes: This table presents sample information on the incarceration data used in this paper.

Table A5: Effects of Narrow Republican Prosecutor Election on Mortality from All Causes

	(1)	(2)	(3)	(4)
	20-29 Year Olds	30-39 Year Olds	40-49 Year Olds	50-59 Year Olds
	-5.94***	1.76	-4.41	-6.42
	(2.14)	(3.91)	(6.26)	(9.03)
p-val	0.005	0.776	0.514	0.456
Control Mean	102.9	150.3	281.2	651.7
N	2149	2149	2149	2149

Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates from all causes. Each column examines effects 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 preferred specification choices outlined in Section II. The control mean row provides the intercept to the left of the cutoff from our preferred RDD specification but without covariates. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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

Panel A: Men

	(1)	(2)	(3)	(4)
	20-29 Year Olds	30-39 Year Olds	40-49 Year Olds	50-59 Year Olds
	0.9	0.4	-3.9	-8.4
	(1.2)	(2.0)	(6.1)	(11.7)
p-value	0.436	0.945	0.500	0.498
Control Mean	30.7	80.2	234.2	693.5
N	2149	2149	2149	2149

Panel B: Women

	(1)	(2)	(3)	(4)
	20-29 Year Olds	30-39 Year Olds	40-49 Year Olds	50-59 Year Olds
	0.9	3.2**	-1.1	-8.3
	(0.7)	(1.4)	(5.5)	(8.2)
p-value	0.257	0.032	0.974	0.311
Control Mean	23.7	65.7	170.0	444.0
N	2149	2149	2149	2149

Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates from internal causes. Panel A examines effects on men and Panel B focuses on women. Each column examines effects 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 preferred specification choices outlined in Section II. The control mean row provides the intercept to the left of the cutoff from our preferred RDD specification but without covariates. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A7: Effects of Narrow Republican Prosecutor Election on External Cause Mortality Among Men

	(1)	(2)	(3)	(4)	(5)	(6)
			Ages 20-29 (Control Mean = 116.9)			
	-9.1	-8.9	-10.5**	-12.8	-13.9***	-10.6***
	(9.4)	(5.8)	(5.2)	(8.1)	(4.5)	(3.0)
p-val	0.258	0.117	0.030	0.150	0.004	0.001
N	2149	2149	2149	2149	2119	2149
			Ages 30-39 (Control Mean = 113.2)			
	-18.4	-9.1	-10.3	-4.5	0.0	-1.8
	(16.8)	(8.0)	(6.6)	(8.8)	(5.8)	(5.0)
p-val	0.246	0.288	0.113	0.602	0.893	0.639
N	2149	2149	2149	2149	2119	2149
			Ages 40-49 (Control Mean = 111.1)			
	-17.4	-5.4	-6.2	2.8	5.2	0.7
	(11.7)	(5.3)	(4.7)	(7.4)	(3.6)	(3.1)
p-val	0.111	0.395	0.222	0.770	0.143	0.905
N	2149	2149	2149	2149	2119	2149
			Ages 50-59 (Control Mean = 122.2)			
	-13.6	-2.9	-2.6	2.8	4.3	5.3
	(11.4)	(7.3)	(6.1)	(7.1)	(5.1)	(3.5)
p-val	0.184	0.798	0.751	0.494	0.398	0.137
N	2149	2149	2149	2149	2119	2149
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 (6) is our preferred specification. Column (1) does not includes covariates, Columns (2) and (3) add in state and year fixed effects, respectively, but do not include county-level controls. Column (4) uses the same covariates as Column (4) but does not weight the regression by population, and Column (5) drops the four counties with the largest regression weights from the sample before estimating. All estimates use a triangular kernel weighting function. Standard errors are clustered at the district level. 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 A8: Placebo Exercise Using Pre-Election Mortality Rates

Panel A: Men Ages 20 to 29 Years Old		
	(1)	(2)
	Real Estimate	Pre-Treatment Placebo Estimate
All Causes	-9.730	2.260
	(3.900)	(3.340)
p-value	0.010	0.500
N	2149	1556
External	-10.630	2.820
	(3.680)	(3.290)
p-value	0.000	0.390
N	2149	1556
Panel B: Black Men, Ages 20 to 29 Years Old		
	(1)	(2)
	Real Estimate	Pre-Treatment Placebo Estimate
All Causes	-31.000	14.600
	(12.850)	(10.730)
p-value	0.020	0.170
N	2149	1556
External	-27.630	4.330
	(12.080)	(9.180)
p-value	0.020	0.640
N	2149	1556

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).

Table A9: Effects of Narrow Republican Prosecutor Election on Prison Admissions

	(1)	(2)	(3)	(4)	(5)	(6)
	Black (Control Mean = 945.571)					
	155.752	85.449	86.745	470.897***	224.212***	196.400***
	(207.095)	(174.773)	(98.526)	(126.143)	(72.584)	(76.683)
p-val	0.466	0.632	0.365	0.001	0.001	0.009
N	1040	1040	1040	1040	1011	1040
	Black Male (Control Mean = 1771.381)					
	301.494	181.106	184.290	782.751***	347.941***	329.450**
	(393.574)	(326.974)	(178.667)	(204.444)	(124.150)	(140.699)
p-val	0.449	0.578	0.280	0.001	0.004	0.017
N	1019	1019	1019	1019	990	1019
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. Each subpanel focuses on a different demographic group, and each column varies the estimation choices. Column (6) is our preferred specification. Column (1) does not includes covariates, Columns (2) and (3) add in state and year fixed effects, respectively, but do not include county-level controls. Column (4) uses the same covariates as Column (4) but does not weight the regression by population, and Column (5) drops the four counties with the largest regression weights from the sample before estimating. All estimates use a triangular kernel weighting function. Standard errors are clustered at the district level. 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).