Local Prosecutors and Public Health*

Panka Bencsik
Vanderbilt University

Tyler Giles†
Wellesley College

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Abstract

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. Using a vote share regression discontinuity design, we find that narrow election of a Republican prosecutor reduces all-cause mortality among young children aged 1 to 4 (driven by accidental deaths) and young adults aged 20 to 29 (driven by gun deaths); and that both effects are concentrated among Black and male Americans. 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 incapacitation and changing home environment channels. We find no overall effects on opioid overdose mortality but some evidence of offsetting effects on prescription and illicit opioid overdoses.

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†Bencsik: Vanderbilt University, 2201 West End Ave, Nashville, TN 37235. (email: panka.bencsik@vanderbilt.edu); Giles: Wellesley College, 106 Central St, Wellesley, MA 02481 (email: tg105@wellesley.edu).

I Introduction

Criminal justice policy and public health in the United States are deeply intertwined for Americans of all ages. Homicide has consistently been a top three leading cause of death among young people for several 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 one of the biggest U.S. public health crises (McGreal, 2018). As for the youngest Americans, infant health is affected by maternal stress and thus by criminal justice polices that either reduce stress (e..g, by increasing feelings of physical safety) or increase it (e.g., through excess policing).[1] Finally, accidental injuries are the leading cause of death among toddlers and are disproportionately common in vulnerable households exposed to or on the margin of criminal justice involvement (Crandall et al., 2006; Sridharan and Crandall, 2011).

This paper investigates the plausibly causal effects of a shift in local criminal justice practice on health as measured by mortality. Specifically, we focus on variation in 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.[2] 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 ap-

[1] For work on the effects of maternal stress on infant health, see Behrman and Butler (2007); Aizer et al. (2016). See Currie et al. (2024) for evidence that gun violence-based stress affects infant health and Legewie (2019) for evidence on police violence and infant health.

[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).

proaches 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) 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 results, robust to a number of analysis choices and placebo tests, suggest that narrow election of a Republican-affiliated local prosecutor is associated with reductions in all-cause mortality among two age groups: young children aged one to four years old (2.7 fewer deaths per 100,000, or a 10% reduction), and young adults aged twenty to twenty-nine (4.9 fewer deaths per 100,000, or a 4.7% reduction). The reduction in mortality among young children is driven by a decline in accidental deaths, and the reduction for young adults by a decline in gun deaths. These effects on gun deaths are not found among other age groups. We find no effect of narrow election of a Republican prosecutor on overall opioid overdose mortality. However, for the middle-aged specifically, this null result obscures an offsetting reduction in heroin mortality and increase in prescription opioid mortality.

Heterogeneity analysis reveals that the reductions in mortality for both age groups are biggest for Black and male Americans. In exploring potential mechanisms, we find that narrow election of a Republican prosecutor has no detectable effects on overall yearly county-level admissions to jails or prison; however, we do find a significant increase in prison admissions for Black Americans specifically. These findings potentially explain why we find mortality effect among this subgroup in particular; we discuss their implications and connections to existing work below.

We 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. Using data from the American Local Government Elections (ALGE) database with information on both sheriff and prosecutor elections, 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. Lastly, our key results are robust to a number of slight changes to the estimation: dropping one state from the analysis at a time, changing the bandwidth selection process or kernel weighting function, etc.

This paper adds to several strands of literature, the first of which studies the effects of criminal justice involvement on health. Two related papers estimate the health effects of incarceration on the incarcerated and find that it reduces mortality risk, largely through incapacitation-based reductions in homicide and overdose mortality (Norris et al., 2022; Hjalmarsson and Lindquist, 2022). Our findings are generally compatible - mortality falls among the same group (Black Americans) that experiences increased incarceration when a more aggressive local prosecutor is narrowly elected. We complement this work by providing a more general equilibrium estimate relating criminal justice policy and mortality at the community-level. A natural question with partial equilibrium estimates of the effect of incarceration on the incarcerated is whether they miss out on indirect community mortality

effects or endogenous adjustments should incarceration be increased/decreased community-wide. A back of the envelope calculation suggests that our mortality estimates are unlikely to be driven solely by the direct effect of incarceration on the incarcerated. This suggests that either incarceration reduces mortality beyond its direct effects, or prosecutorial approaches other than incarceration taken by more aggressive prosecutors reduce mortality.

This paper also contributes to the literature on the opioid crisis and its intersection with the criminal justice system. Papers seeking to understand the supply-side dynamics of the crisis have documented substitution patterns between misuse of prescription and illicit opioids; for example, efforts to reformulate OxyContin to make it more misuse resistant 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 story, although in the opposite direction - aggressive efforts to reduce the supply of illicit opioids through prosecution may lead people to substitute to misuse of licit ones. Our null mortality and substitution findings complement other work examining the persistence of the opioid epidemic (Cutler and Donahoe, 2024) and further the point that the momentum of opioid demand spillovers likely explains much more of observed mortality rates than contemporaneous shocks to illicit opioids.

Next, we contribute to the literature on child well-being and the criminal justice system. Recent work documents pervasive childhood exposure to the criminal justice system, and thus local prosecutors - 39% of children reside in a household with at least one caretaker with a criminal charge, and this rate exceeds 60% for racial minority and low-income children (Finlay et al., 2023). The direction of our mortality estimates is consistent with prior work showing that parental/sibling incarceration can be associated with improvements in child outcomes (Norris et al., 2021).[3]

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

[3]As in that work, we do not suggest that parental incarceration and/or child separation are socially beneficial because it is associated with reductions along this mortality margin.

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, 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.[4] More work on other outcomes is needed to 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 II, 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 III lays out our empirical strategy and regression specifications. Section IV presents our main results and findings on heterogeneity, Section V investigates potential mechanisms, and Section VI offers conclusions from this research.

II Institutional Background and Data

II.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

[4] 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.

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. For example, the former district attorney of Suffolk County, MA, publicly announced her intent to decline to prosecute a subset of lower-level offenses when running for office (Cramer, 2018); examples such as this have led some legal scholars to conclude that local prosecutors are "the criminal justice system's real lawmakers" (Stuntz, 2001).[6] 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 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.

[6]This has also led to a questioning of exact constitutional role of local prosecutors and various efforts to remove local prosecutors from office or circumvent their decisions (Hessick and Su, 2023).

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.

We later investigate connections between prosecutorial approaches and child mortality; hence, it is worth discussing the role of the prosecutor's office in dealing with reported instances of child abuse or neglect. Local governments in the U.S. take a multi-pronged approach to the handling of child maltreatment, and the prosecutor's office is just one of a number of departments that may be involved. The complexity and variety of child welfare systems across the U.S. precludes a full description here;[7] we instead highlight the two most relevant aspects for our study: when the prosecutor's office will be involved at all, and how these offices typically handle the prosecution of child maltreatment.

Prosecutors can become involved in child abuse cases in one of two ways: through referral from the police (as in a typical criminal offense) or child protective services (CPS). CPS offices field reports of child maltreatment from community members (e.g., neighbors, educators, or healthcare providers), and each claim is typically handled by a single individual. CPS case workers investigate claims and then decide whether there is enough evidence to substantiate the claim; conditional on substantiation, they then evaluate the risk to the child, and outcomes are based on this risk assessment (Gross and Baron, 2022; Baron et al., 2024).

Whether a substantiated claim will be referred to the local prosecutor's office will depend the case worker's assessment and any existing policies mandating or encouraging cooperation between CPS, law enforcement, and prosecutors.[8] Scholars and practitioners in the area of child welfare often highlight the interplay between this trio of offices and acknowledge

[7] See Children's Bureau (2020) for detailed descriptions.

[8] Evidence suggests widely varying rates of referral for prosecution by CPS; Cross et al. (2003) find an average referral rate of 54% across the jurisdictions in their sample, but this average masks wide variation.

the importance of informal and formal agreements between them in resulting outcomes for children, including home removal and placements into foster care (Newman and Dannenfelser, 2005; Cross et al., 2015). Once referred to a prosecutor's office, child abuse cases are often handled by specialized units devoted to these cases and by prosecutors whose tasks spill over into family and civil legal procedure.

II.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.[9] 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 "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. 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 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

[9]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. There are subtle but important details inherent in this variable's construction. Beginning with a simple illustration, 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 our vote share variable for counties in this district in 2019 will be 600/(400+600) = 0.6.[10] If a Republican candidate were instead to run unopposed in the fall of 2018, the 2019 Republican vote share variable will be coded as 1; if a Democrat were to run unopposed, it would be 0.[11] 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).

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 chose 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 A2 presents summary statistics for counties both within and outside our sample. Columns (1) and (2) present average 2010 demographics across two samples of counties in

[10]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.

[11]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.

states with partisan local prosecutor elections. The first column focuses on counties that at some point have a closely contested election in the 2010s. In total, there are 583 counties that contribute one or more observations in our analysis, and these counties reside within a total of 426 electoral districts. The second column uses the sample of counties in states with partisan elections but which do not have a closely contested election in the 2010s. Column (3) shows few raw differences in these demographics between these two samples of counties. For nearly every variable, differences between in and out of sample counties attenuates when forcing within-state comparisons (Column 4), suggesting that the characteristics of in-sample counties are reasonably similar and that there should be reasonable external validity of any findings.[12]

II.3 Mortality Data

Our mortality data come from the Centers for Disease Control and Prevention's (CDC's) Multiple Cause of Death files. These files provide a census of all deaths in the United States from 2010 to 2019. The data identify causes of death using the 10th revision of the International Classification of Diseases (ICD10), and we list the ICD10 codes we use to identify causes of death in the Appendix. We aggregate mortality counts to the county-by-year level and create mortality rates per 100,000 individuals using population counts from the Surveillance, Epidemiology, and End Results (SEER) U.S. Population Data. Mortality risk and its sources vary quickly with age among young children; hence, we analyze their mortality rates in small age groupings: less than one year (infants), one to four years old (toddlers), and five to nine years old. Beyond that, we typically analyze mortality rates in ten year age groupings (10 to 19 years old, 20 to 29 years old, etc).[13]

Figure 2 depicts trends in all-cause mortality by age group across our sample period.

[12]Table A3 shows the twenty biggest counties within the sample by 2010 middle-aged population.

[13]These ten year age group rates and any other larger age group rates are age-adjusted; i.e., they are convex combinations of the separate five year age group rates within the broader age range and are calculated using the same set of weights across all counties and years. Our weights are based off the age distribution in the United States in 2000. This age adjustment prevents misleading comparisons of mortality rates across time and/or location that are driven purely by differing age distributions.

Separate trend lines are shown for the U.S. as a whole (black solid line), for counties that contribute to our regression samples (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). The U.S. lines visualize several well-known facts about mortality risk in the U.S. around this time. First, mortality rates are high among infants (Panel A), then fall precipitously conditional on surviving past infancy in the first decade of life (Panels B and C), then begin to increase in the teenage years (Panel D) and onward (Panels E and F). The time trends in all-cause mortality across the 2010s vary by age: infant and toddler mortality are falling on average, mortality among children five to nine years old is essentially constant, and mortality among the older groups typically begins to increase around 2014. In addition to these baseline facts, Figure 2 conveys that the levels and trends in mortality in the counties in our analysis are quite similar to the U.S. as a whole - much more so than the counties not used in our analysis. This provides some reassurance that the communities that identify our estimates are not outliers in terms of mortality risk.

Figure 3 visualizes mortality trends by age group for several specific causes of death. The top four panels show trends in opioid overdose mortality rates: all opioids (Panel A), natural and semi-synthetic opioids (Panel B), heroin (Panel C), and fentanyl (Panel D).[14] These panels convey several well-known facts about the opioid crisis; first, the crisis is predominately concentrated among the middle-aged.[15] Next, the opioid crisis 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. The last two panels show mortality trends for homicide and gun deaths; here, a "gun death" is any death involving a gun, whether

[14]The term "opioid" is conventionally used to refer to any drug or medication, licit or otherwise, that is either derived from or mimics substances found in the opium poppy plant. The "natural and semi-synthetic opioid" category predominately refers to prescription opioid-based pain medication like oxycodone (i.e., OxyContin) and hydrocodone (i.e., Vicodin). Heroin is also an opioid separate from the natural and semi-synthetic category, and fentanyl is a synthetic opioid, meaning that it acts on the same brain receptors as other opioids but is not naturally occurring.

[15]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).

it be accidental or purposeful. Homicide and gun mortality are highest for people in their twenties, and rates for both ticked up in the middle of the decade but remained constant or fell slightly towards the end.[16]

II.4 Jail and Prison Admission Data

Our data on jail and prison 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 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. The prison admission data covers up to 2016 and 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. Our main mortality estimates use data from 2010 to 2019, so the jail and prison data cover one and three fewer years, respectively. Our variables of interest are yearly jail admission and prison admission rates per 100,000 residents ages 15 to 64 years old. Prison admission rates are available by race and by gender, but jail admission rates are only available by gender, and neither are available by age group. Prison admission data is missing for about 10% of the county-by-year observations from 2010 to 2016 whose most recent Republican vote share variable is within ten percentage points of the 50% cutoff; jail admission data is missing for about 8% of these observations from 2010 to 2018.

III 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

[16]Figure A1 breaks out rates for these causes of death by sample like Figure 2; the levels and trends in mortality in the counties in our analysis are again similar to the U.S. as a whole.

may be, on average, more rural, less likely to have public health insurance, and each of 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. 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 approach assumes that the average potential outcomes are continuous across the 50 percent vote share cutoff; in other words, there should be no discontinuous jump between the average observed mortality rate for counties which just narrowly fail to elect a Republican DA and the counterfactual we'd observed for counties that do just narrowly elect a Republican DA if they hadn't.

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 RD design 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.

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

Our preferred specification includes a number of covariates. X_c is a vector of county-level demographic characteristics from the 2010 census. We do not allow these covariates to vary by year as they may be endogenously affected by prosecutor political affiliation. δ_s and γ_t are sets of state and year fixed effects, respectively. We include these sets of fixed effects in hope of explaining a significant fraction of the variation in mortality rates across counties and thus improving the precision of our estimates.[17] Lastly, ϵ_{cdst} is the population residual.

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

Our preferred specification uses county population weights, both so that our estimand is the population estimand 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.

In several contexts, we allow the effect of narrow election of a Republican DA to vary along some dimension. In these cases, we estimate some variant of Equation 2. Here, g indexes groups within the given context and $G_{g,cdst}$ is an indicator for whether the observation for county c in electoral district d in state s and year t belongs to group g. This regression specification omits a general constant term and interacts the group membership dummies which each term in a typical RDD regression specification; hence, the coefficients of interest (τ_g) are computationally identical to the ones that would be obtained if we instead split the sample by group and estimated separate regressions.[18]

$$Y_{cdst} = \sum_g \{G_{g,cdst} + G_{g,cdst}[\tau_g \mathbb{1}(R_{dt} \geq 50) + \beta_{g,l} R_{dt} + \beta_{g,r} \mathbb{1}(R_{dt} \geq 50) R_{dt} + X_c' \beta_{g,3} + \delta_{g,s} + \gamma_{g,t}]\} + \epsilon_{cdst} \tag{2}$$

To test for the validity of this identification strategy, we conduct two simple empirical tests. First, Figure 4 plots the empirical distribution of our vote share variable and reveals no clear discontinuity in the number of observations across the threshold. Second, Table 1 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 also picking up the effect of electing a Republican sheriff. First, using data from the recently compiled American Local Government Election (ALGE) database, we estimate the simple correlation between Republican prosecutorial vote share and Republican sheriff vote share

[18]This method allows for hypothesis tests about effects across groups. Following Feigenberg et al. (2023), we are careful to interact the group membership dummies with the covariates.

in elections in which both were contested. This result is shown in Panel A of Figure A2. 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 significant 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 A2.[19] The estimate is negative and statistically insignificant, conveying that the party affiliation of sheriffs is not confounding our estimates.

IV Main Results

IV.1 Mortality Results

Table 2 presents estimates of the effect of narrow election of a Republican prosecutor on all-cause mortality by age group. Each of the six columns varies the estimation procedure slightly to check for robustness. Column (1) uses no covariates, Columns (2) and (3) add in state and year fixed effects, respectively, and Column (4) adds a set of 2010 baseline county-level demographic controls. Column (4) is our preferred specification as we anticipate that the covariates will explain variation in mortality rates and improve the precision of our estimates. Column (5) uses the same covariates as Column (4) but does not weight the regression by population, and Column (6) drops the three biggest counties from the sample before estimating.

The top sub-table reports effects on infant mortality (ages<1). While some of the estimates are marginally significant and negative, the results are not robust to the omission of the three biggest counties in the sample. The sub-tables showing effects for children ages 5

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

to 9 and for teenagers (ages 10 to 19) are similarly imprecise and/or not robust, and thus do not convey a clear story of increases or reductions in mortality among these age groups.

However, the second sub-table suggests robust reductions in mortality for children ages 1 to 4. Column (4) suggests that narrow election of a Republican prosecutor reduces overall mortality among this age group by about 2.7 deaths per 100,000. This coefficient is precisely estimated and consistent across the columns. The magnitude of this estimate remains stable but loses precision without the population weights; however, this is in line econometric theory on regression weighting and how it can improve precision given varying group sizes (Solon et al., 2015). The bottom sub-table estimate for adults aged 20 to 29 is also negative and suggests that narrow election of a Republican prosecutor reduces mortality among this group by about 4.9 deaths per 100,000. Like the previous set of estimates, it becomes less precise when the weights are removed, but the magnitude of the coefficient remains stable.[20]

Figure 5 visualizes these estimated effects of narrow Republican prosecutor election on all-cause mortality. Panels A and B visualize the effect for young children ages 1 to 4; Panels C and D show the effect for adults aged 20 to 29. The effects visualized in Panels A and C are estimated without covariates and are identical to the Column (1) estimates from Table 2; the effects shown in Panels B and D are obtained by first residualizing the mortality rates of our covariates.[21] These figures convey two phenomena: first, there is a small but visible discontinuity in mortality rates across the vote share threshold; second, mortality is generally increasing with Republican prosecutor vote share, suggesting that a more naive approach to our research question would understate the mortality reductions associated with narrow election of a more aggressive prosecutor.

Figure 6 explores the specific causes of death that drive the effects found in Table 2. Panel A shows that reduction mortality among young children is driven by a reduction in

[20]Table A4 shows no effects on all-cause mortality among age groups from thirty years old and upward.

[21]These visualized estimates would be identical to our Column (4) estimates if the covariates were completely orthogonal to the running variable. Our balance tests show covariate balance across the vote share threshold, so while not mechanically identical to our Column (4) estimates, these figures are exceedingly close.

accidental injuries/poisonings. There is also a precisely estimated reduction in mortality tied to congenital disorders, although this effect is smaller in magnitude. Panel B conveys that the effects on young adults are primarily due to reductions in gun deaths. Effects on homicide, motor vehicle accidents, and suicide mortality are all negative but smaller in magnitude and on the margin of statistical significance at the 5 percent level; the effect on drug overdoses is essentially zero.[22] Reassuringly, neither panel in Figure 6 shows effects on mortality due to cancer or other diseases/conditions, which are outcomes that should not theoretically be affected by local prosecutors (other than through competing risks).

Next, we examine the connection between local prosecutors and the opioid crisis. Figure 7 shows effects on mortality tied to opioid overdoses by age group. Panel A visualizes estimates on mortality from overdoses on opioids of any kind and reveals no significant effects across any of the age groups. Panel B shows effects on mortality tied to natural and semi-synthetic opioids, which essentially captures overdoses from abuse of prescription opioids like OxyContin. Here we see increases in mortality among the middle-aged of around 1.5 to 2 additional deaths per 100,000. Panel C shows effect on heroin mortality and reveals that the null overall effect from Panel A obscures offsetting increases in Panel B and reductions in heroin mortality in Panel C among the middle-aged. Lastly, Panel D shows effects on fentanyl mortality and finds no effects.

We next investigate whether any demographic groups in particular are driving our results. Figure 8 shows results for several causes of death for young children (Panels A and C) and young adults (Panels B and D) by race (Panels A and B) and sex (Panels C and D).[23] These figures reveal that the reductions in all-cause mortality among both age groups are driven by effects on Black Americans and boys/men; all-cause mortality estimates for non-Black 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

[22]Figure A3 examines effects on deaths from homicide and guns across all age groups; these results suggest that the effect on gun deaths is unique to adults in their twenties.

[23]Tables A5,A6,A7, and A8 show estimates by race and sex in more detail.

Black young adults (17.6 fewer deaths per 100,000) is about twice as big as the reduction for male young adults (8.2 fewer deaths per 100,000). The specific causes of death driving these results are the same as in the main estimates - gun deaths.[24]

IV.2 Placebo/Robustness Exercises

Our null effects on mortality due to cancer and other diseases are a reassuring placebo finding; in this section, we describe another placebo test and robustness 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. Practically, we conduct this exercise by assigning the vote share variable to pre-election years and then estimating Equation 2 and splitting the sample between these pre-election "placebo" and post-election "real" observations.

Table A9 shows the results of this estimation. Column (1) restates our main estimates (this time estimated within the context of Equation 2) and Column (2) shows the corresponding pre-election placebo estimates. For both age groups and for all-cause mortality and specific drivers, the placebo test shows no significant results. This furthers 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.

[24]The nature of this research design and the limited frequency of closely contested elections makes investigations of heterogeneity along time and/or geographic 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 A4 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.

A common worry in empirical work is that results may hinge on seemingly minor estimation choices; in the context of RDDs, these include the process used to select the bandwidths and the functional form of the kernel weighting function used to weight the regression. Table A10 re-estimates our all-cause mortality estimates for young children and young adults several times, each time varying one of these choices. These results reveal that our findings are not dependent upon a particular estimation choice.

It is also natural to speculate whether our results are driven by any one state in particular. Figure A5 re-estimates our all-cause mortality estimates, each time dropping a different state from the sample. Our results are almost unanimously unaffected by dropping of states - the only exception is Texas, which should be expected given that we weight our regressions by population and that the state is home to four of the ten biggest counties in the sample (see Table A3). However, the estimates become bigger when Texas is dropped, showing that the high-level finding the Republican prosecutors reduce mortality is not dependent upon Texas's inclusion.

V Mechanisms

V.1 Incarceration

In this section, we explore mechanisms which might explain our mortality findings, beginning with incarceration. Table 3 displays estimates of the effect of narrow Republican local prosecution election on annual county-level admissions to jails and prisons. Each column varies the specification choices to check for robustness in the same manner as Table 2. The top five sub-tables examine prison admission, and the last sub-table analyzes jail admissions. The top sub-table reveals no effect on overall annual admissions to prison, but the second suggests increases in prison admissions for Black Americans specifically. Our preferred specification in Column (4) suggests that narrow election of a Republican local prosecutor increases annual prison admissions among Black Americans aged 15 to 64 by 256

admissions per 100,000. The other prison estimates show no meaningful effect among other demographic groups. Figure 9 visualizes the null effects on overall prison admissions and the increase among Black Americans. The last sub-table of Table 3 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.

Could our mortality findings among young Black adults be explained solely by the direct incapacitation effects of incarceration on the incarcerated? To begin, Column (4) from Table 3 suggests increased annual prison admissions among Black Americans aged 15 to 64 by 256 admissions per 100,000. This prison admission estimate should not be used to scale our mortality estimates as-is given the differing age groups across the two estimates,[25] but it might reasonably be adjusted as follows: about 40% of all felony sentences are issued to individuals 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 $(256)(\frac{0.4}{0.2}) = 512$ additional annual prison admissions per 100,000.

For simplicity, consider a local prosecutor elected for a four year term. Our prison estimates suggest that, in each year of this term, a Republican prosecutor would put 512 additional people in prison, and would thus incarcerate an additional $(4)(512) = 2,048$ people per 100,000 across this four year term. Note that those incarcerated in the first year of the term would have four years of exposure to incarceration, those incarcerated in the second would have three years of exposure, etc. The most relevant estimate from Norris et al. (2022) suggest that incarceration reduces the likelihood of death in a given year by 0.55 percentage points, and using this estimate within this exercise would suggest that the direct incapacitation effect should reduce cumulative mortality across all four years by $512(4 + 3 + 2 + 1)(0.0055) = 28.2$ deaths per 100,000.[26] We estimate 17.6 fewer deaths annually per 100,000 among Black Americans aged 20 to 29 (see Table A5 for this estimate

[25] Our prison admission data cannot be broken down more finely by age.

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

in more detail), so our mortality point estimate suggests a cumulative $(4)(17.6) = 70.4$ fewer deaths across this four year term.

Thus, our estimate of 70.4 fewer deaths is greater than the estimate of 28.2 fewer deaths implied by existing work on the mortality effects of incarceration on the incarcerated; however, 28.2 is within the 95% confidence interval implied by the variance of our mortality estimate. 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.

VI 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 children aged 1 to 4 and young adults aged 20 to 29. Both mortality effects are concentrated among Black and male 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 incapacitation and changing home environment channels. 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 young children; for example, whether prosecutorial approaches affect the number of children placed into foster care.

VII Tables and Figures

Table 1: Balance, County and State Characteristics

	(1) Narrow GOP Loss	(2) Narrow GOP Win	(3) Diff	(4) Robust BC p-Value	(5) BWs	(6) N
Geography						
Census Division 3	0.183	0.125	-0.058	0.613	(10.9,11.7)	2670
Census Division 5	0.184	0.070	-0.114	0.479	(10.5,10.8)	2609
Census Division 7	0.436	0.486	0.050	0.807	(12.9,12.9)	2874
Census Division 8	0.104	0.053	-0.050	0.220	(6.9,6.8)	1898
Other Census Division	0.091	0.102	0.011	0.710	(8.6,8.6)	2235
McCain State	0.560	0.634	0.074	0.732	(11.5,13.4)	2861
2010 County-Level Demographics						
Unemployment	0.091	0.093	0.002	0.931	(12.2,13.3)	2900
% White Alone	0.804	0.767	-0.037	0.224	(9.4,9.4)	2387
% White	0.817	0.780	-0.037	0.227	(9.3,9.3)	2386
% Black Alone	0.145	0.164	0.020	0.468	(10.0,8.9)	2361
% Black	0.151	0.171	0.020	0.469	(10.0,8.9)	2361
<High School	0.164	0.179	0.015	0.640	(11.6,9.7)	2527
High School	0.270	0.250	-0.020	0.273	(9.1,9.1)	2338
Some/More College	0.561	0.561	0.000	0.770	(9.1,7.4)	2078

Notes: This table shows estimated effects of the 50% Republican vote share cutoff on fixed or predetermined 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. Column (4) provides the robust, bias-corrected p-value on the RDD estimate, Column (5) provides the bandwidths used in estimation, and Column (6) provides the effective number of observations used in estimation.

Table 2: Effects of Republican Prosecutor Election on All-Cause Mortality

	(1)	(2)	(3)	(4)	(5)	(6)
			Ages <1 (Control Mean = 678.915)			
	-63.751	-40.946	-39.285	-39.456	33.123	-9.486
	(32.515)	(22.688)	(22.417)	(17.070)	(48.105)	(23.421)
p-val	0.056	0.139	0.170	0.061	0.319	0.955
BWs	8.8,9.5	9.2,8.1	8.7,8.3	9.5,7.6	8.8,8.5	9.5,8.4
N	2358	2185	2168	2128	2230	2231
			Ages 1-4 (Control Mean = 27.225)			
	-3.316	-2.181	-2.485	-2.698	-2.971	-5.371
	(1.671)	(1.194)	(1.128)	(0.927)	(4.207)	(1.509)
p-val	0.027	0.058	0.021	0.003	0.437	<0.001
BWs	7.6,9.2	9.1,8.9	9.1,9.0	9.6,8.0	9.2,8.7	7.6,7.1
N	2281	2274	2334	2221	2253	1945
			Ages 5-9 (Control Mean = 11.898)			
	-0.690	-1.148	-1.139	-1.120	-4.172	-0.663
	(0.886)	(0.704)	(0.690)	(0.678)	(2.123)	(1.058)
p-val	0.373	0.086	0.086	0.086	0.071	0.447
BWs	11.8,11.9	8.8,7.8	9.6,8.1	7.5,7.4	9.4,9.0	7.3,7.2
N	2756	2113	2221	2007	2359	1952
			Ages 10-19 (Control Mean = 33.707)			
	-1.473	-1.306	-1.478	-0.803	-1.827	-3.587
	(2.144)	(1.769)	(1.522)	(1.128)	(2.988)	(1.427)
p-val	0.359	0.418	0.268	0.358	0.648	0.007
BWs	9.5,10.1	8.7,7.9	8.6,7.8	10.8,10.6	7.3,7.6	7.6,7.8
N	2437	2113	2096	2616	2023	2029
			Ages 20-29 (Control Mean = 103.076)			
	-5.819	-5.004	-5.971	-4.876	-5.032	-6.735
	(6.475)	(4.057)	(3.667)	(2.358)	(8.629)	(3.513)
p-val	0.197	0.204	0.060	0.024	0.901	0.036
BWs	10.1,10.1	7.8,7.9	7.8,8.3	10.1,8.7	6.8,7.2	8.7,8.3
N	2483	2108	2127	2333	1924	2140
Weights	Yes	Yes	Yes	Yes	No	Yes
FEs	No	State	State, Year	State, Year	State, Year	State, Year
Census Controls	No	No	No	Yes	Yes	Yes
Drop 3 Biggest Counties	No	No	No	No	No	Yes

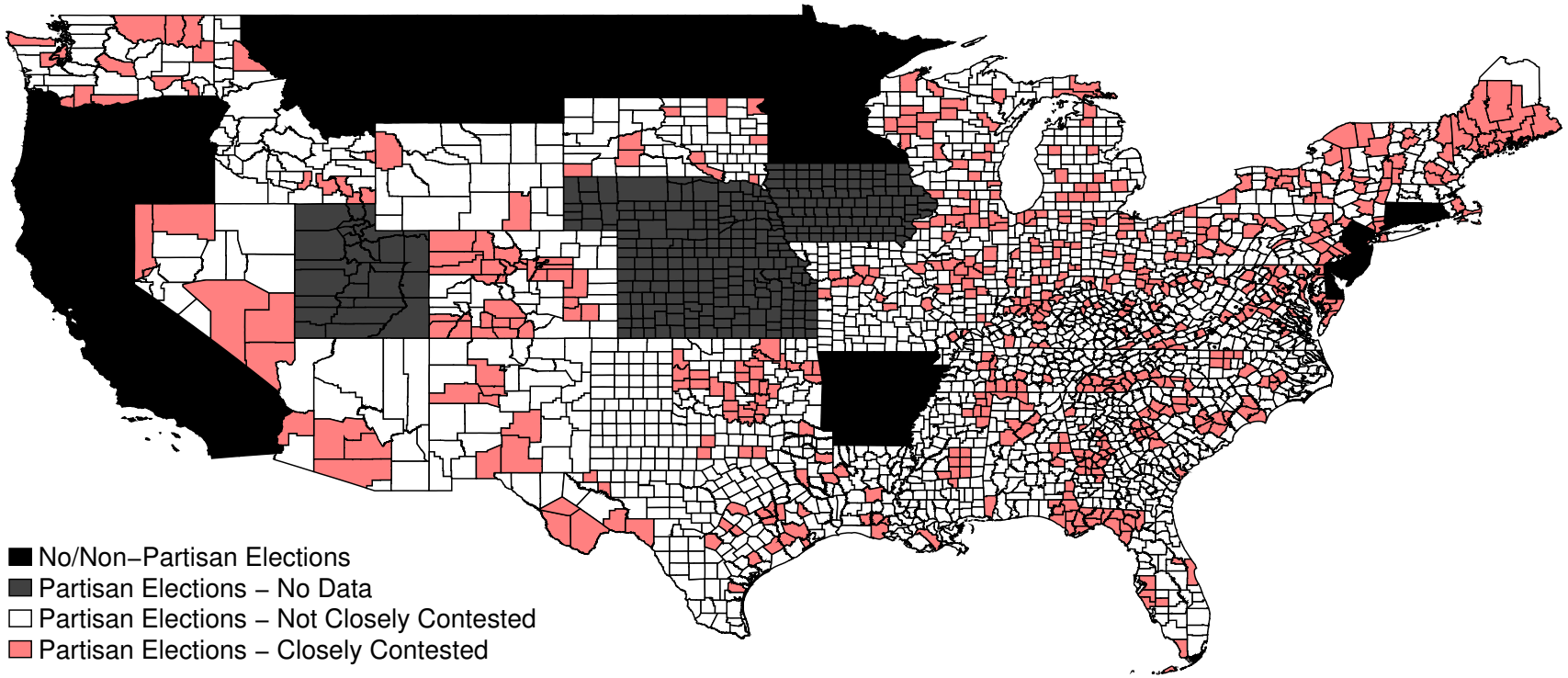
Notes: This table provides estimates from Equation 1 of the effect of narrow election of a Republican local prosecutor on mortality from all causes. Column (1) uses no covariates, Columns (2) and (3) add in state and year fixed effects, respectively, and Column (4) adds a set of 2010 baseline county-level demographic controls. Column (5) uses the same covariates as Column (4) but does not weight the regression by population, and Column (6) drops the three biggest counties from the sample before estimating. 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. Outcome is mortality rate per 100,000 residents. The control mean estimate at the top of each subtable gives the intercept to the left of the cutoff from the Column 1) regression.

Table 3: Effects of Republican Prosecutor Election on Jail & Prison Admissions

	(1)	(2)	(3)	(4)	(5)	(6)
	All, Prison (Control Mean = 393.192)					
	-11.016	-17.356	-17.994	9.579	-2.898	-16.778
	(88.062)	(23.398)	(15.425)	(14.004)	(19.789)	(18.345)
p-val	0.905	0.778	0.456	0.227	0.898	0.679
BWs	12.2,12.2	8.1,7.9	7.8,7.7	6.9,7.2	9.2,9.8	7.1,7.2
N	1713	1310	1263	1172	1458	1155
	Black, Prison (Control Mean = 978.661)					
	149.071	206.540	217.327	255.990	325.805	316.646
	(182.603)	(113.769)	(73.180)	(72.409)	(143.016)	(93.394)
p-val	0.287	0.025	0.001	<0.001	0.030	0.001
BWs	10.3,11.9	9.9,10.4	8.9,9.3	8.0,9.3	9.9,10.7	7.4,7.4
N	1030	988	918	902	1010	747
	White, Prison (Control Mean = 279.911)					
	-32.900	-27.309	-27.713	-22.011	-22.383	-6.249
	(54.859)	(19.922)	(20.371)	(12.924)	(18.390)	(14.500)
p-val	0.499	0.425	0.418	0.311	0.298	0.626
BWs	11.1,12.6	9.7,8.3	9.3,8.0	9.8,10.0	10.0,9.4	7.8,7.7
N	1350	1097	1078	1204	1198	994
	Male, Prison (Control Mean = 688.275)					
	-16.658	-22.463	-25.259	22.722	-8.533	-16.538
	(149.121)	(41.581)	(26.858)	(24.100)	(35.691)	(31.627)
p-val	0.961	0.979	0.659	0.114	0.843	0.976
BWs	12.3,12.4	8.2,7.8	7.9,7.9	7.0,8.0	8.3,9.1	7.2,7.2
N	1416	1045	1054	1024	1149	942
	Female, Prison (Control Mean = 104.742)					
	-4.520	-7.138	-6.755	-3.375	-4.109	-6.696
	(24.992)	(6.278)	(4.891)	(4.359)	(10.121)	(6.426)
p-val	0.753	0.638	0.564	0.899	0.870	0.813
BWs	13.6,13.7	7.6,7.8	7.8,7.8	7.7,8.1	10.6,9.8	7.8,7.3
N	1474	1028	1032	1052	1242	975
	All, Jail (Control Mean = 6182.296)					
	-780.118	-1028.615	-1132.937	-761.348	608.856	172.631
	(928.791)	(605.843)	(623.431)	(447.455)	(788.495)	(518.035)
p-val	0.293	0.119	0.108	0.218	0.256	0.382
BWs	10.7,12.1	7.9,7.9	7.8,8.1	8.1,9.2	7.7,8.1	8.3,9.0
N	2241	1725	1724	1890	1724	1850
Weights	Yes	Yes	Yes	Yes	No	Yes
FEs	No	State	State, Year	State, Year	State, Year	State, Year
Census Controls	No	No	No	Yes	Yes	Yes
Drop 3 Biggest Counties	No	No	No	No	No	Yes

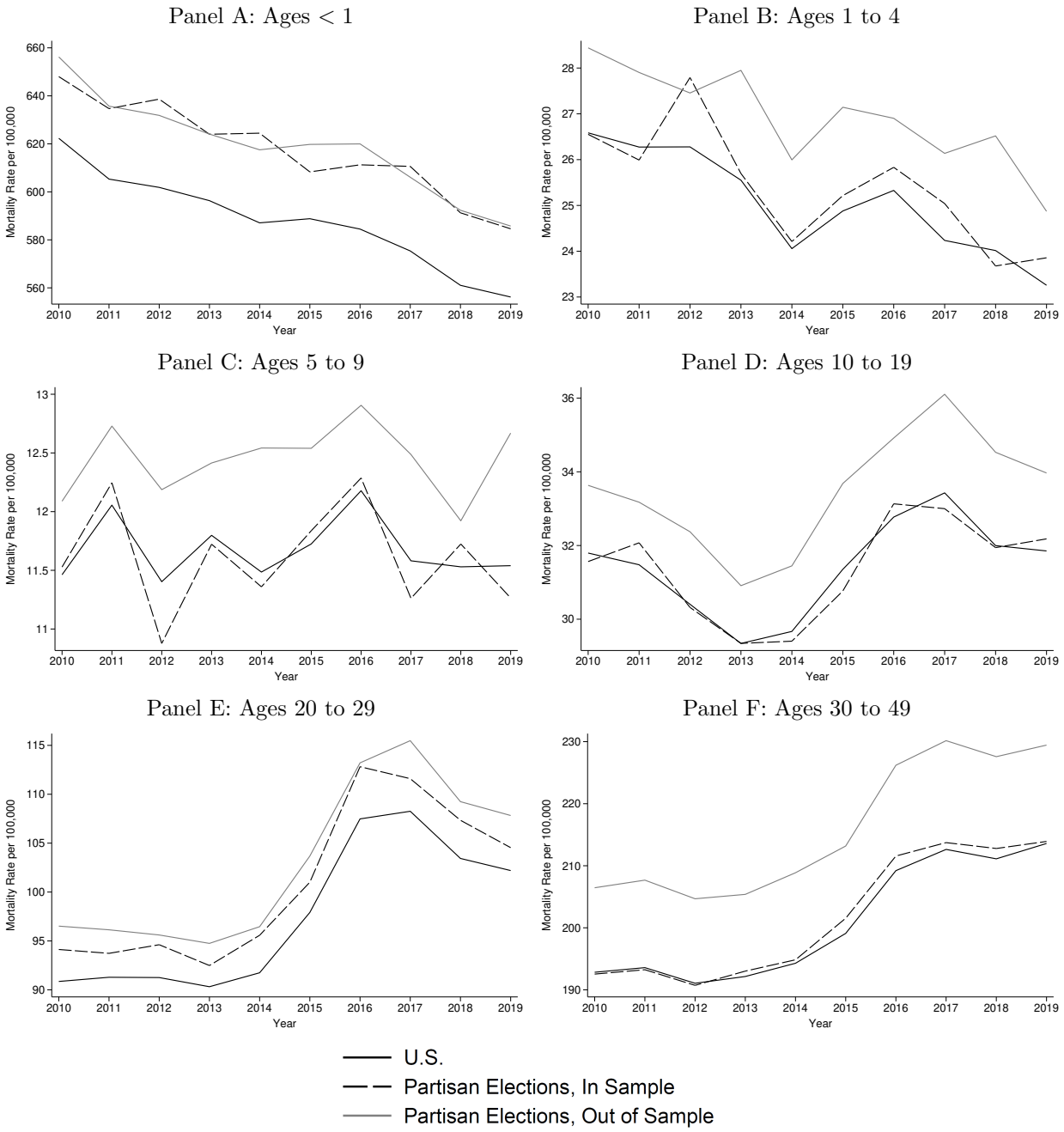
Notes: This table provides estimates from Equation 1 of the effect of narrow election of a Republican local prosecutor on county-level jail and prison admissions. Column (1) uses no covariates, Columns (2) and (3) add in state and year fixed effects, respectively, and Column (4) adds a set of 2010 baseline county-level demographic controls. Column (5) uses the same covariates as Column (4) but does not weight the regression by population, and Column (6) drops the three biggest counties from the sample before estimating. 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. Outcome is admission rate per 100,000 residents aged 15 to 64. The control mean estimate at the top of each subtable gives the intercept to the left of the cutoff from the Column 1) regression.

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



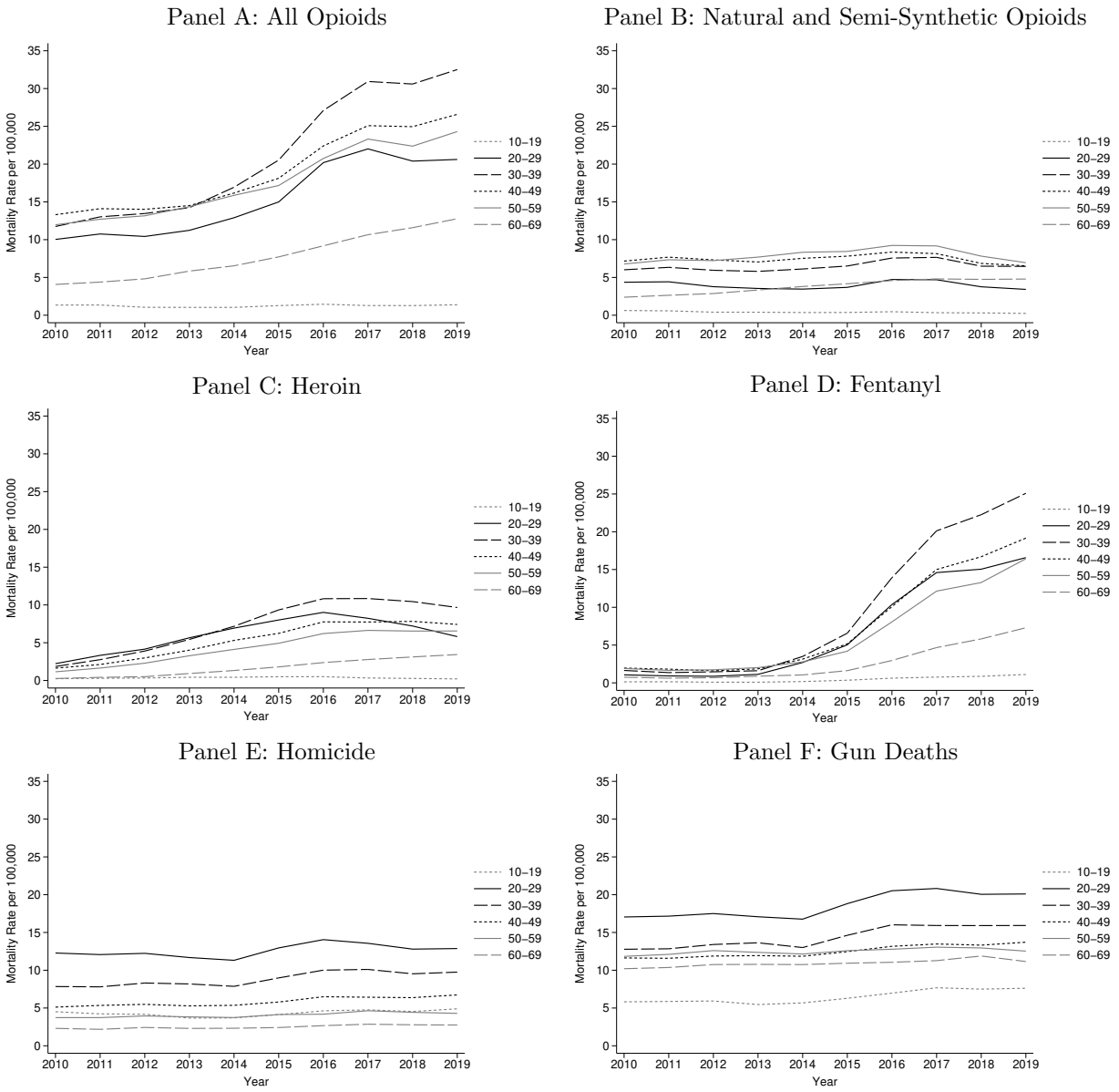
Notes: This map visualizes the counties which contribute to our regression samples. States shaded black are states where local prosecutors are either appointed instead of elected or elected nut in non-partisan elections. States shaded dark gray are states where local prosecutors are elected in partisan elections, but we do not possess election data over this time frame. Counties shaded white 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 red are counties in districts that elect local prosecutors in partisan elections and which had at least one closely contested election from the years 2010 to 2019. Closely contested is defined has having a Republican vote share in a general election between 0.4 and 0.6.

Figure 2: Trends in All-Cause Mortality



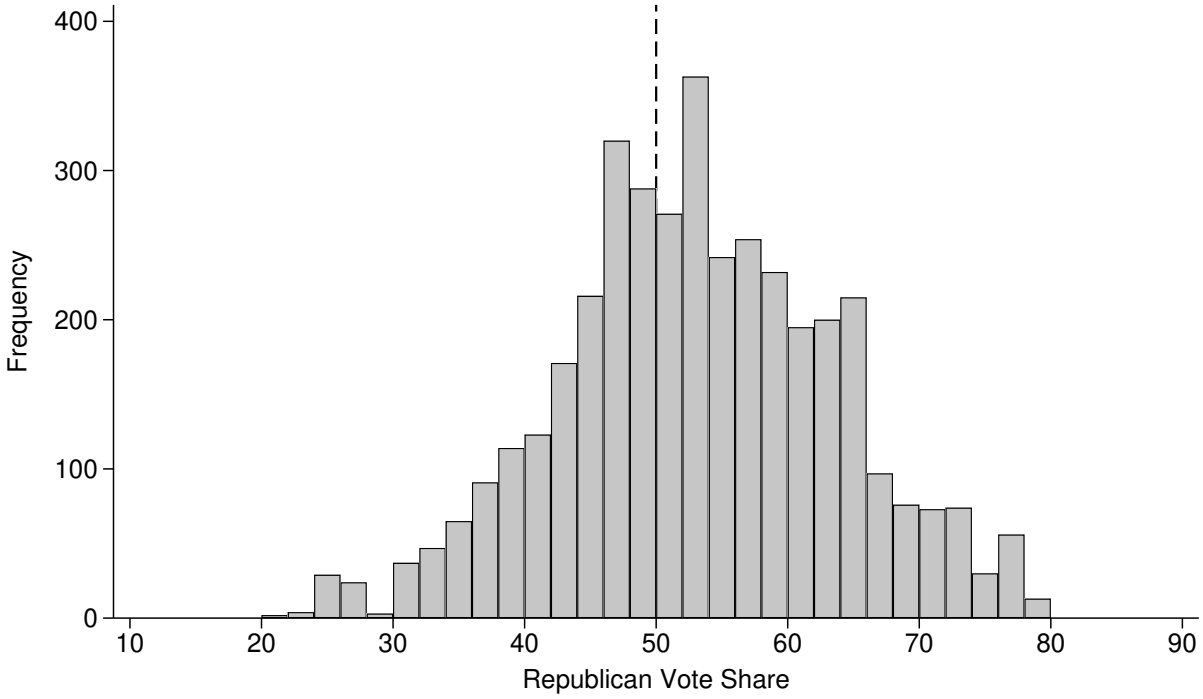
Notes: These figures show trends in all-cause mortality from 2010 to 2019. Each panel focuses on a different age group. Separate trend lines are shown for the U.S. as a whole (black solid line), for counties had at least one closely contested election from the years 2010 to 2019 (black dashed line), 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 3: Trends in Mortality, By Cause



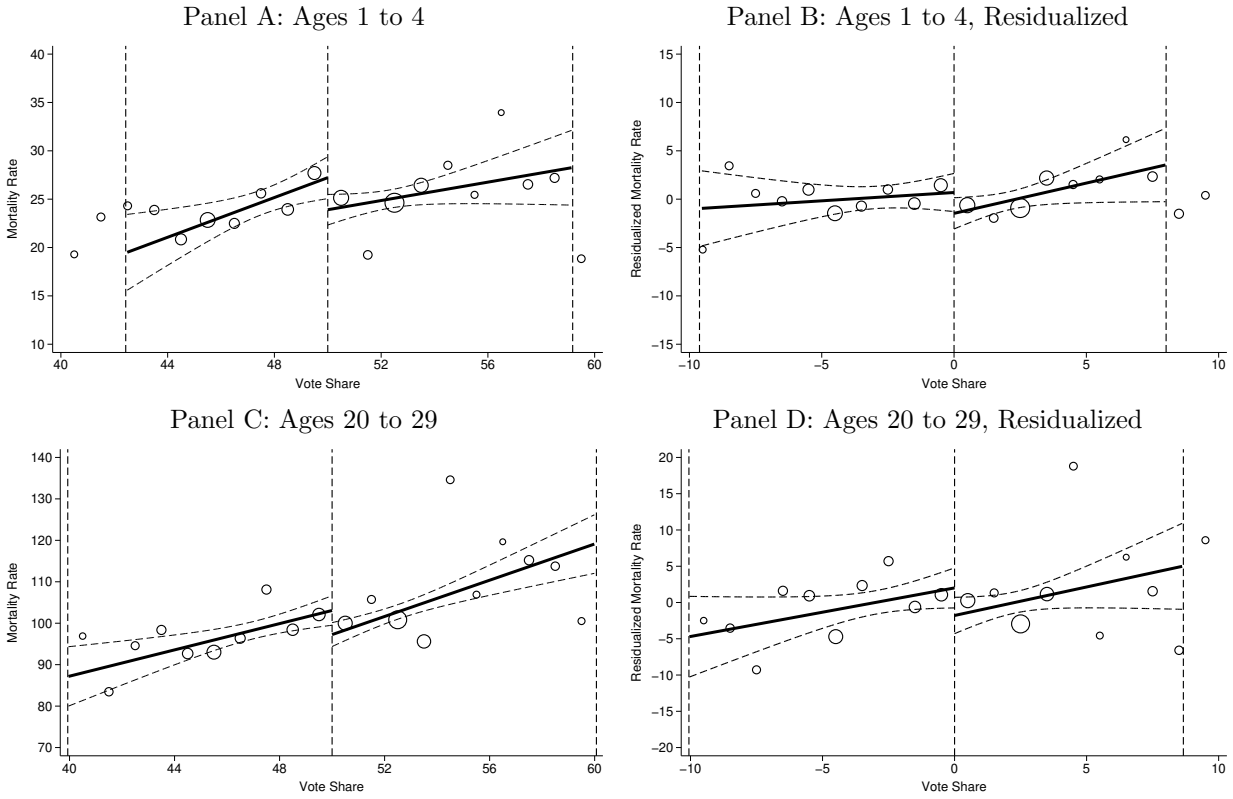
Notes: This figures show U.S. trends in mortality by specific causes. Each panel focuses on a different cause, and the lines within each panel show trends separately by ten year age groups. See the Appendix for the ICD10 codes used to identify each cause of death.

Figure 4: Distribution of Republican Vote Share, 2010 to 2019



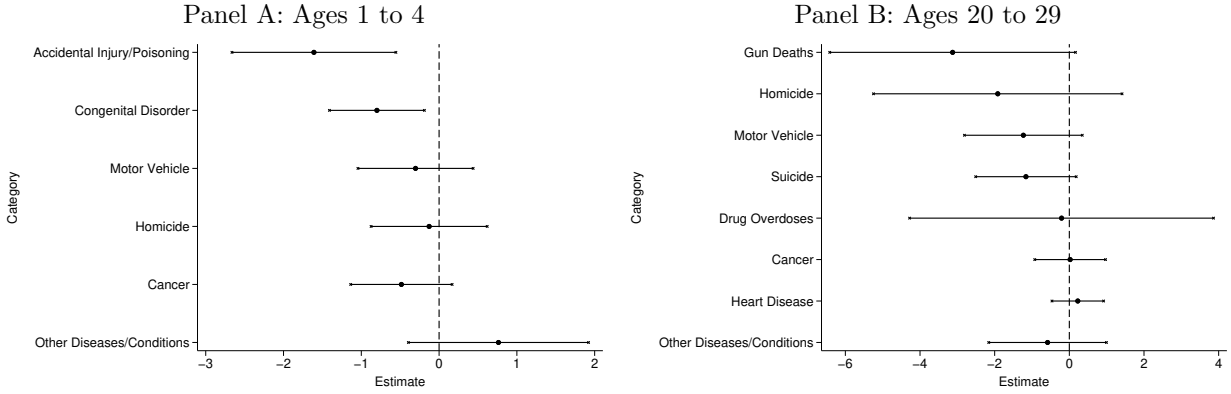
Notes: This figure visualizes the distribution of the Republican vote share variable in our sample. Bins are two percentage points wide.

Figure 5: Effects of Republican Prosecutor Election on All-Cause Mortality



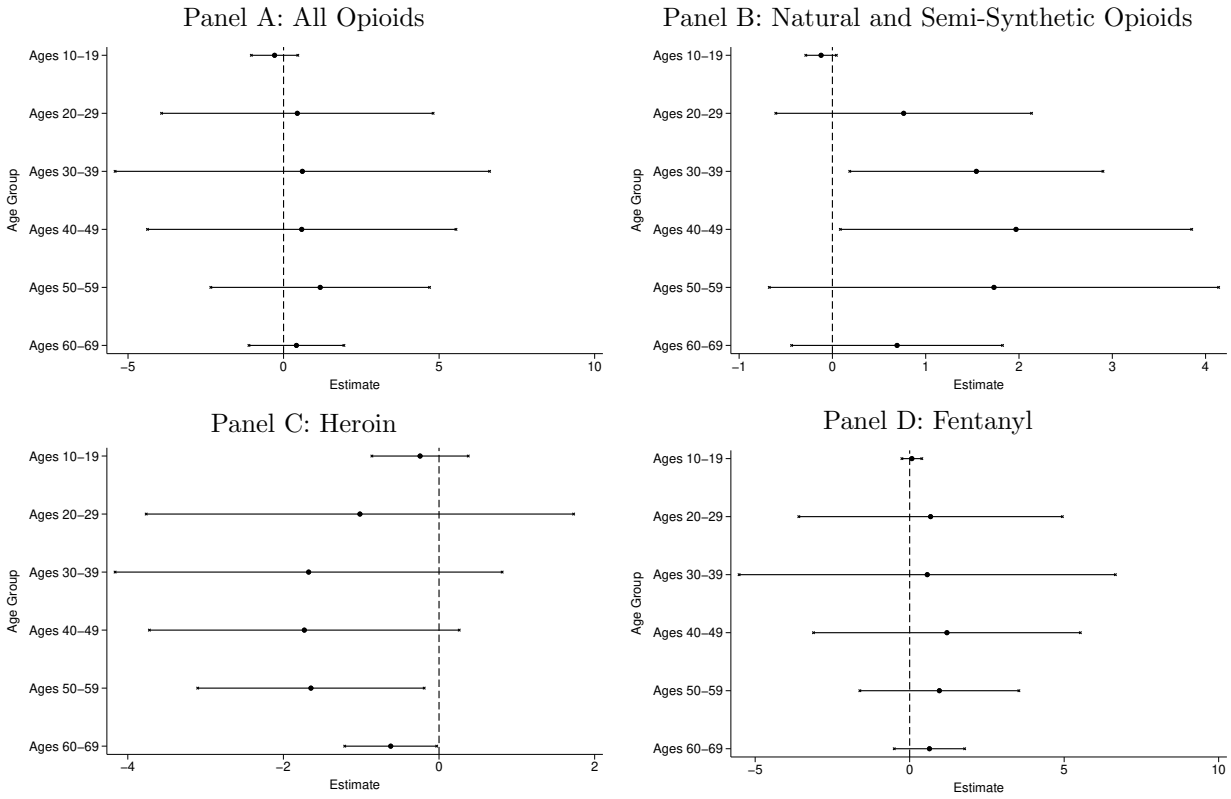
Notes: These figures visualize RDD estimates of the effect of election of a Republican local prosecutor on all-cause mortality. Each figure plots average mortality rates within one percentage point wide bins for observations with Republican vote share values within 10 percentage points of the cutoff. The size of each bin corresponds to the total population across all observations within the bin. The "residualized" figures plot mortality rates that have been residualized of state fixed effects, year fixed effects, and 2010 baseline county-level demographics. 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 6: Effects of Republican Prosecutor Election on Mortality, By Cause



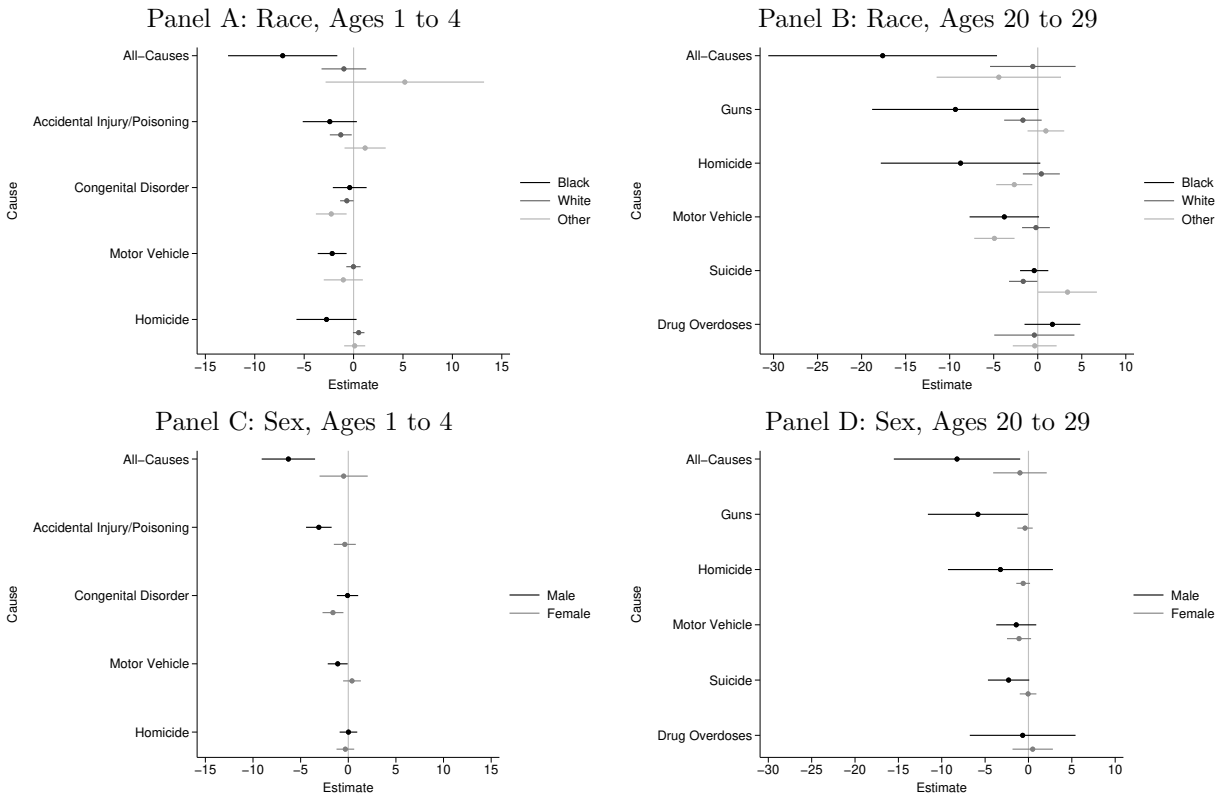
Notes: These coefficient plots convey RDD estimates of the effect of election of a Republican local prosecutor on mortality by cause. Each coefficient and 95% confidence comes from a separate regression estimated using our preferred specification with covariates and population weights.

Figure 7: Effects of Republican Prosecutor Election on Drug Overdose Mortality, By Age Group



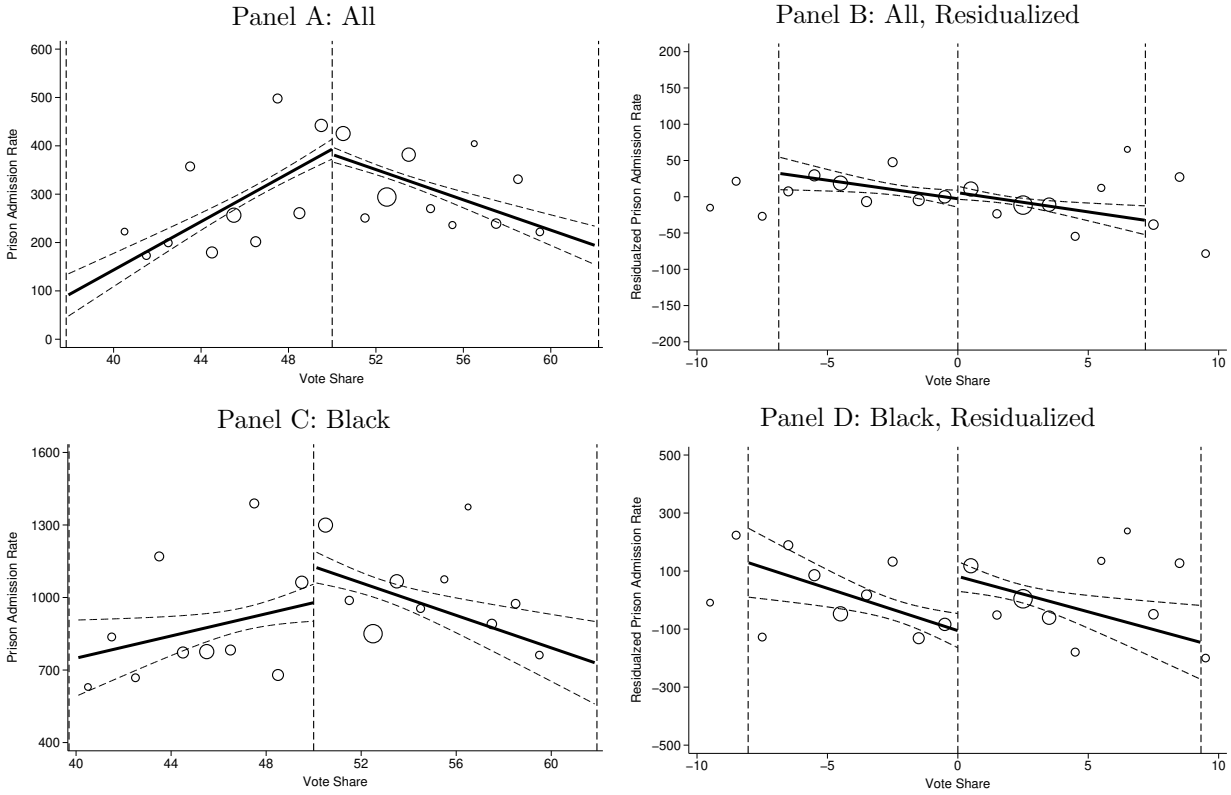
Notes: These coefficient plots convey RDD estimates of the effect of election of a Republican local prosecutor on mortality due drug overdoses across across ten year age groups. Each coefficient and 95% confidence comes from a separate regression estimated using our preferred specification with covariates and population weights. Each panel focuses on drug mortality stemming from a different type of opioid.

Figure 8: Effects of Republican Prosecutor Election on Mortality, By Race and Sex



Notes: These coefficient plots convey RDD estimates of the effect of election of a Republican local prosecutor on all-cause mortality separately by race and sex. Each coefficient and 95% confidence comes from a separate regression estimated using our preferred specification with covariates and population weights.

Figure 9: Effects of Republican Prosecutor Election on Prison Admissions



Notes: These figures visualize RDD estimates of the effect of election of a Republican local prosecutor on annual county-level admissions to prison. Each figure plots average prison admission rates within one percentage point wide bins for observations with Republican vote share values within 10 percentage points of the cutoff. The size of each bin corresponds to the total population across all observations within the bin. The "residualized" figures plot prison admission rates that have been residualized of state fixed effects, year fixed effects, and 2010 baseline county-level demographics. 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.

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A Appendix

A.1 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:

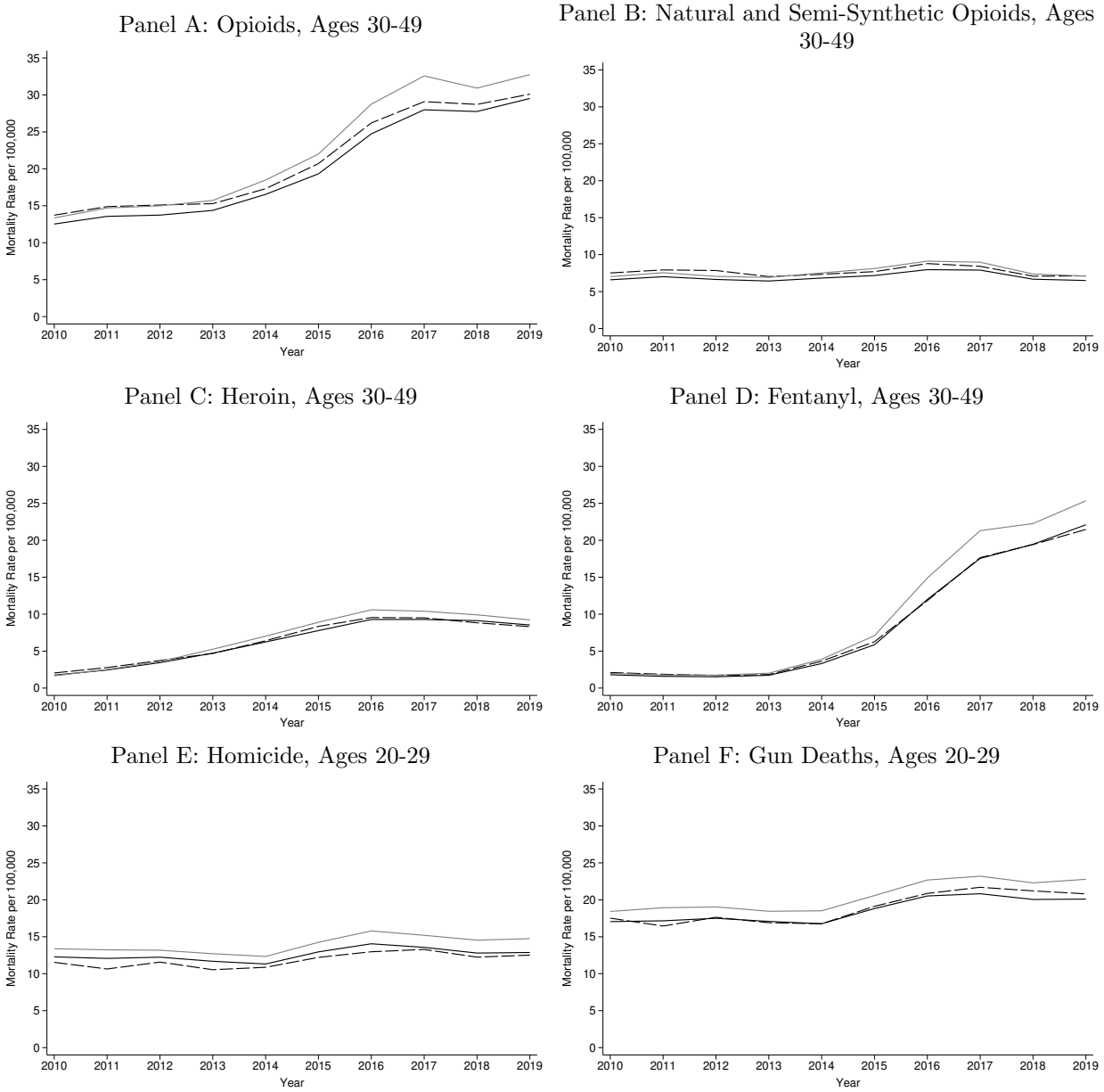
- Malignant neoplasms (cancer): C00-C97
- Major cardiovascular diseases: I00-I78
- Influenza and pneumonia: J10-J18
- Chronic lower respiratory diseases: J40-J47
- Chronic liver disease and cirrhosis: K70,K73-K74
- Pregnancy, childbirth and the puerperium: O00-O99
- Perinatal conditions: P00-P96
- Congenital malformations, deformations and chromosomal abnormalities: Q00-Q99
- 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
- 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
- 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, Y40-Y86, Y88

- Suicide: X60-X84, Y87.0
- Assault: X85-Y09, Y87.1

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

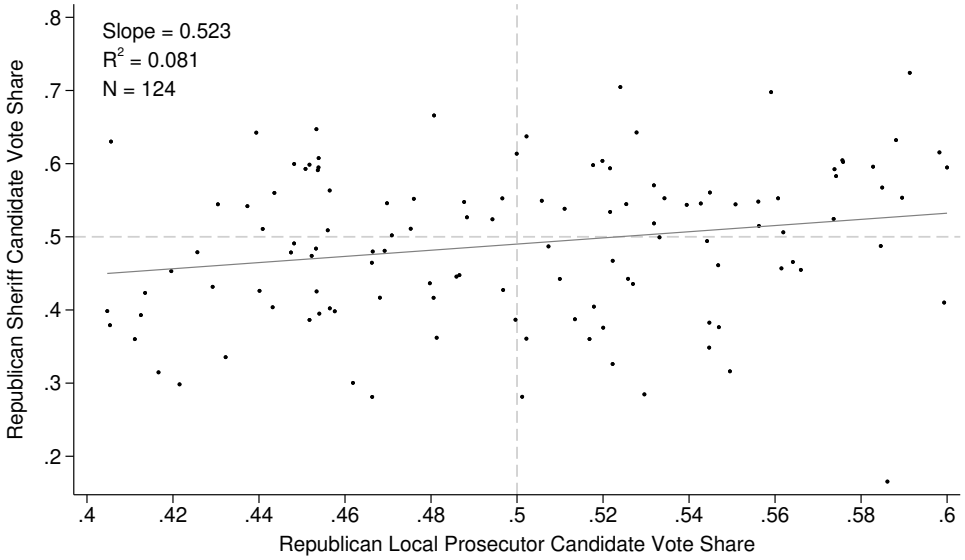
Figure A1: Trends in Mortality, By Cause, In and Out of Sample



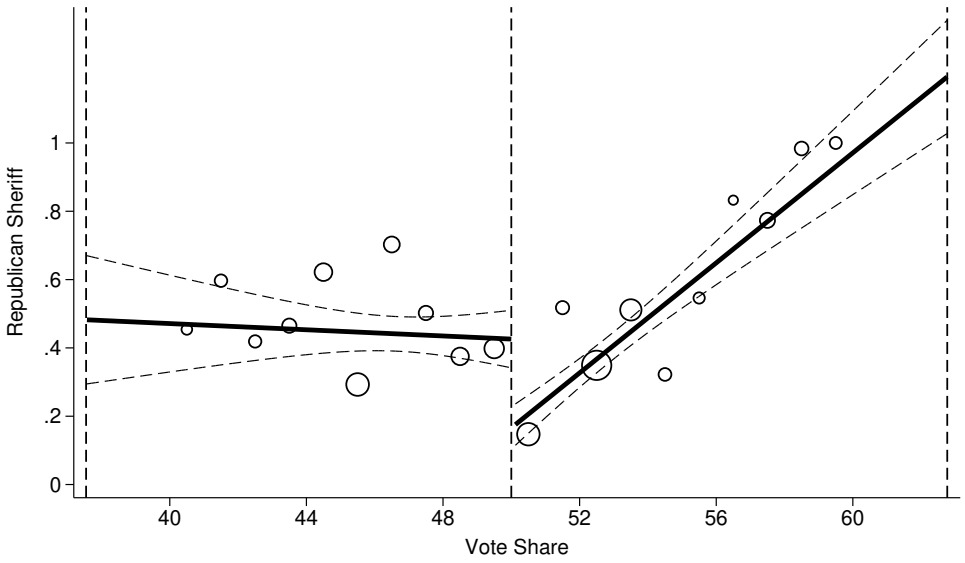
Notes: These figures show trends in mortality by cause from 2010 to 2019. Each panel focuses on a different cause of death and age group. Separate trend lines are shown for the U.S. as a whole (black solid line), for counties had at least one closely contested election from the years 2010 to 2019 (black 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: Sheriffs

Panel A: Correlation

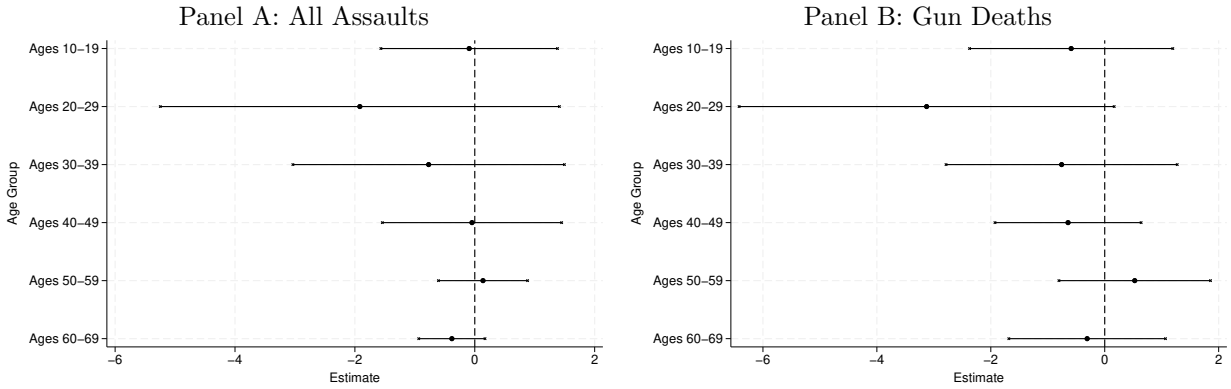


Panel B: Republican Sheriff Balance



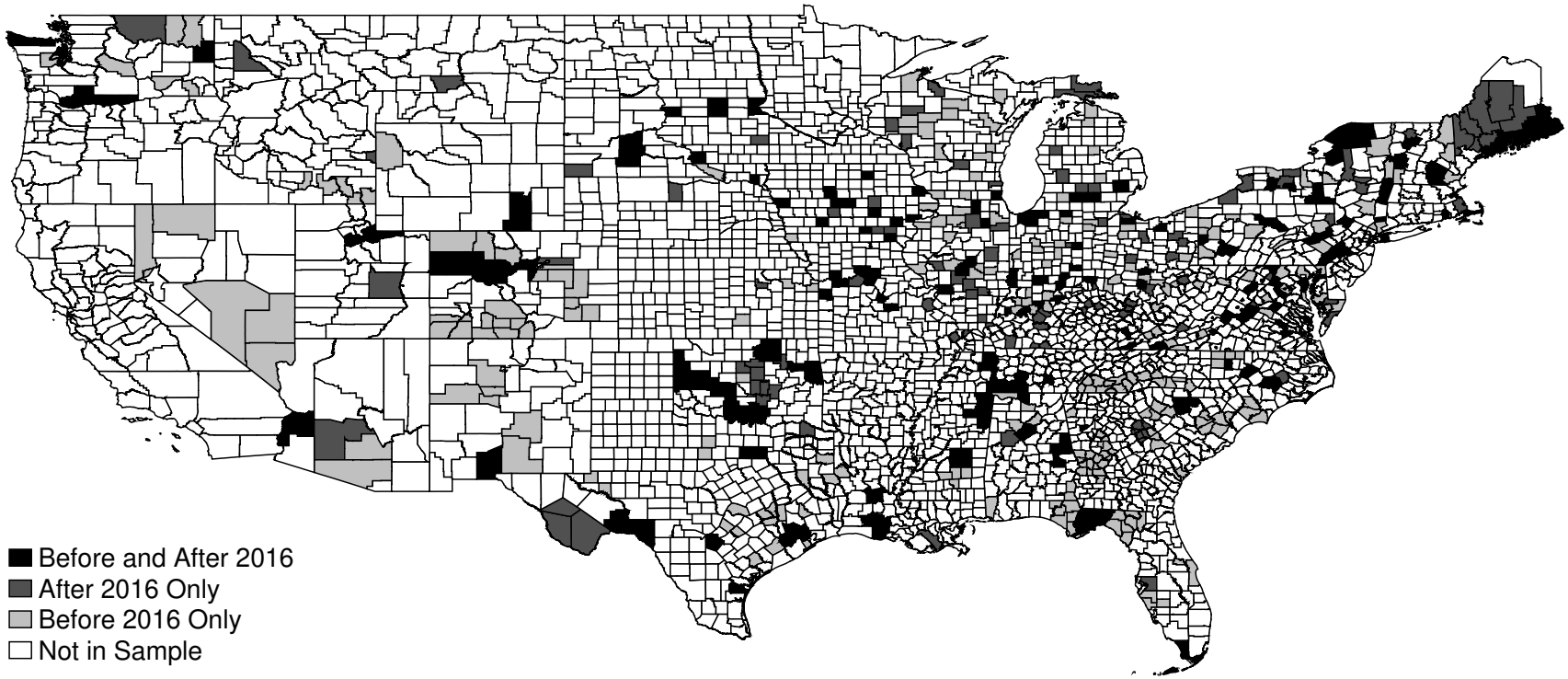
Notes: These figures use data from the American Local Government Election (ALGE) database to visualize the relationship between vote shares for Republican local prosecutors and sheriffs. Panel A visualizes the simple correlation between Republican prosecutorial vote share and Republican sheriff vote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

Figure A3: Effects on Mortality Due to Violence, By Age Group



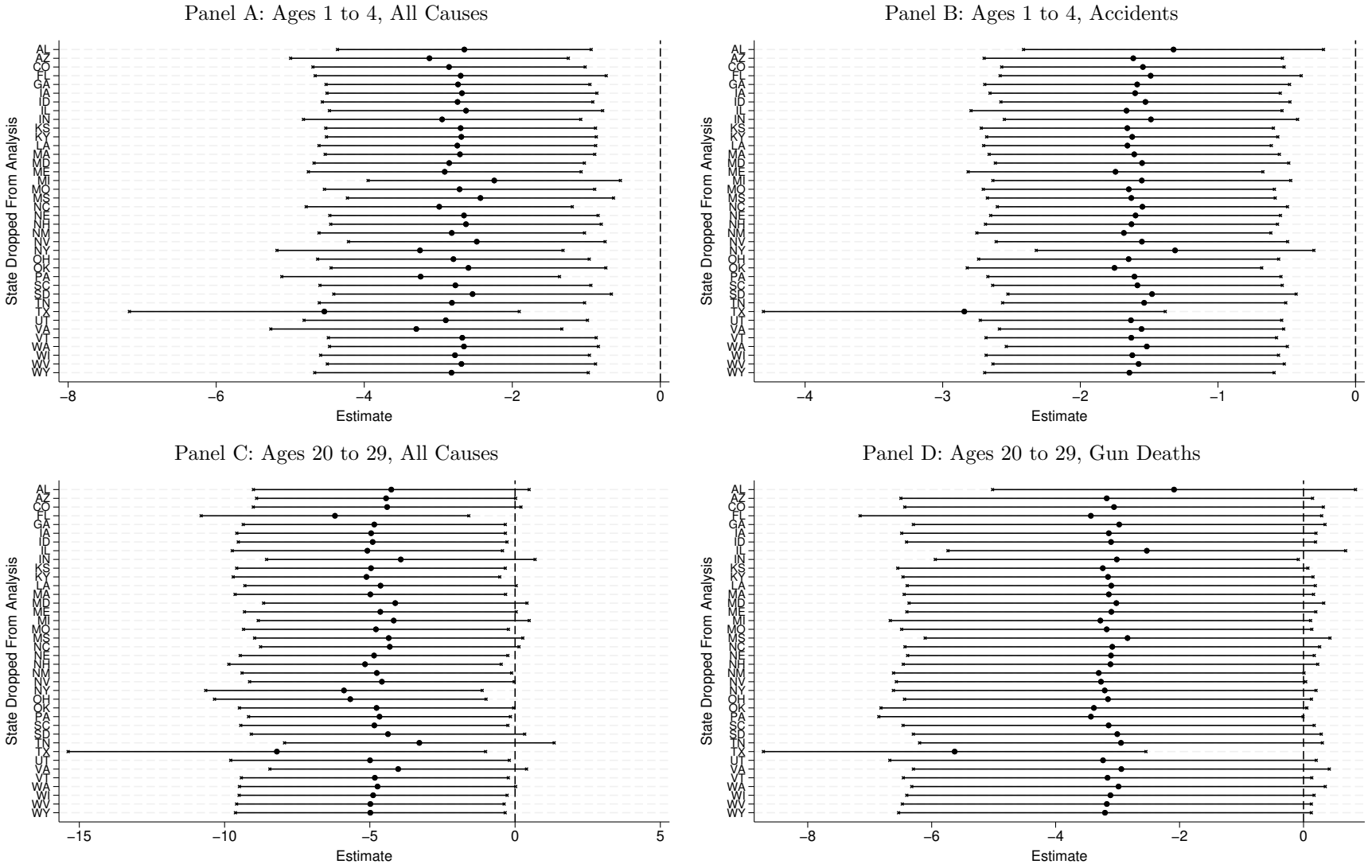
Notes: These coefficient plots convey RDD estimates of the effect of election of a Republican local prosecutor on mortality due to either homicide (Panel A) or gun death (Panel B) across ten year age groups. Each coefficient and 95% confidence comes from a separate regression estimated using our preferred specification with covariates and population weights.

Figure A4: Timing



Notes: This map visualizes the timing of when counties that ever have a closely contested local prosecutor election from 2010 to 2019 contribute observations to our regression samples.

Figure A5: Mortality Estimates, Dropping One State at a Time



Notes: These coefficient plots convey RDD estimates of the effect of election of a Republican local prosecutor on mortality. Each panel focuses on a different cause of death and age group. Each coefficient and 95% confidence interval comes from a separate regression and is estimated by first dropping the corresponding state from the analysis.

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 election. "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: Summary Statistics, 2010 Demographics, In vs Out of Sample Counties

	(1) In Sample Counties	(2) Out of Sample Counties	(3) Difference	(4) Within-State Difference
<HS	16.49	15.36	-1.14	-0.35
HS	35.56	34.75	-0.81	-0.60
Some College	29.27	29.11	-0.16	-0.02
College	18.67	20.78	2.11	0.96
Unemp.	9.34	9.78	0.44	0.08
% Black	10.81	10.87	0.06	0.13
Mortality	242.45	238.42	-4.04	3.36
Drug Overdoses	25.05	26.56	1.51	1.60
Gun Deaths	17.00	14.99	-2.01	-0.65
N (Counties)	583	2113		
N (Districts)	426	1584		

Notes: This table provides summary 2010 demographic information for counties in states with partisan local prosecutor elections. Column 1) includes counties which had at least one closely contested local prosecutor election from the years 2010 to 2019. Column 2) includes counties with partisan local prosecutor elections but which did not have a closely contested election from the years 2010 to 2019. Column 3) gives the raw difference between Columns 1) and 2) and Column 4) gives the regression adjusted within state difference between the Column 1) and 2) samples. Closely contested is defined has having a Republican vote share in a general election between 0.4 and 0.6.

Table A3: Big Counties in the Sample, By 2010 Middle-Aged Population

In Sample Rank	County Name	2010 Middle-Aged Population
1	Harris, TX	1,192,213
2	Maricopa, AZ	1,051,565
3	Dallas, TX	695,896
4	Clark, NV	572,316
5	Tarrant, TX	529,860
6	Bexar, TX	471,329
7	Nassau, NY	357,847
8	Hillsborough, FL	352,019
9	Oakland, MI	338,710
10	Fairfax, VA	333,636

Notes: This table lists the twenty biggest counties that had at least one closely contested election from the years 2010 to 2019, along with the 2010 middle-aged population. Closely contested is defined has having a Republican vote share in a general election between 0.4 and 0.6.

Table A4: Effects of Republican Prosecutor Election on All-Cause Mortality, Older Age Groups

	(1)	(2)	(3)	(4)	(5)	(6)
			Ages 30-39 (Control Mean = 149.525)			
	-15.784	-9.252	-9.728	-2.009	-18.402	-3.896
	(14.264)	(8.154)	(6.846)	(5.167)	(10.885)	(6.544)
p-val	0.187	0.300	0.159	0.652	0.111	0.431
BWs	10.1,10.9	9.2,8.6	7.6,7.7	9.1,9.1	9.0,8.8	10.1,8.7
N	2598	2252	2039	2334	2274	2318
			Ages 40-49 (Control Mean = 280.167)			
	-30.408	-20.799	-21.812	-5.998	-8.184	-0.237
	(17.095)	(10.352)	(10.233)	(7.352)	(15.053)	(11.165)
p-val	0.051	0.076	0.062	0.646	0.909	0.800
BWs	8.9,9.3	9.0,9.0	9.0,9.0	7.7,8.0	8.2,9.6	7.3,7.6
N	2354	2324	2327	2111	2330	2001
			Ages 50-59 (Control Mean = 649.886)			
	-62.085	-49.595	-49.743	-18.638	-10.842	-12.024
	(35.412)	(22.068)	(21.656)	(14.054)	(23.590)	(19.865)
p-val	0.058	0.047	0.045	0.326	0.996	0.710
BWs	8.7,9.2	8.7,8.9	8.7,9.0	7.3,7.5	7.3,8.6	7.1,7.9
N	2342	2262	2314	1996	2173	1994
			Ages 60-69 (Control Mean = 1344.299)			
	-59.312	-61.625	-61.596	-13.919	19.910	4.705
	(59.912)	(35.839)	(34.551)	(19.520)	(36.330)	(23.081)
p-val	0.286	0.112	0.097	0.770	0.344	0.525
BWs	9.0,8.1	8.2,7.2	8.2,7.2	7.4,7.5	8.9,8.9	6.8,7.6
N	2170	2015	2016	2009	2276	1948
Weights	Yes	Yes	Yes	Yes	No	Yes
FEs	No	State	State, Year	State, Year	State, Year	State, Year
Census Controls	No	No	No	Yes	Yes	Yes
Drop 3 Biggest Counties	No	No	No	No	No	Yes

Notes: This table provides estimates from Equation 1 of the effect of narrow election of a Republican local prosecutor on mortality from all causes. Column (1) uses no covariates, Columns (2) and (3) add in state and year fixed effects, respectively, and Column (4) adds a set of 2010 baseline county-level demographic controls. Column (5) uses the same covariates as Column (4) but does not weight the regression by population, and Column (6) drops the three biggest counties from the sample before estimating. 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. Outcome is mortality rate per 100,000 residents. The control mean estimate at the top of each subtable gives the intercept to the left of the cutoff from the Column 1) regression.

Table A5: Effects of Republican Prosecutor Election on All-Cause Mortality, Black Americans

	(1)	(2)	(3)	(4)	(5)	(6)
			Ages <1 (Control Mean = 1132.2)			
	-37.9	-7.7	-4.3	-1.1	-42.4	90.8
	(69.9)	(66.6)	(62.3)	(54.3)	(298.8)	(84.3)
p-val	0.564	0.800	0.737	0.830	0.845	0.318
BWs	9.5,10.6	9.8,9.4	9.5,9.6	8.9,8.3	8.2,8.5	7.2,6.4
N	2416	2282	2280	2050	2072	1768
			Ages 1-4 (Control Mean = 42.8)			
	-7.7	-7.7	-9.1	-7.2	-8.1	-10.8
	(3.7)	(3.3)	(3.1)	(2.8)	(11.8)	(3.6)
p-val	0.031	0.037	0.007	0.025	0.273	0.003
BWs	10.0,10.4	9.2,9.2	8.6,8.4	9.3,8.8	7.0,7.0	9.9,8.0
N	2502	2333	2183	2282	1892	2205
			Ages 5-9 (Control Mean = 19.4)			
	-0.7	-0.9	-1.3	-2.1	5.4	1.6
	(2.7)	(2.1)	(2.1)	(2.1)	(9.9)	(1.8)
p-val	0.693	0.466	0.341	0.206	0.809	0.667
BWs	10.1,9.7	12.4,11.0	12.4,10.9	9.6,10.0	9.5,9.7	10.3,9.8
N	2453	2712	2708	2447	2406	2432
			Ages 10-19 (Control Mean = 44.1)			
	-1.5	1.4	0.6	2.9	2.7	-11.2
	(6.3)	(4.9)	(3.6)	(3.6)	(17.7)	(4.3)
p-val	0.614	0.781	0.976	0.439	0.875	0.006
BWs	8.6,8.9	8.8,8.6	7.9,7.9	7.6,7.6	11.1,11.4	6.5,6.5
N	2263	2245	2062	2028	2670	1789
			Ages 20-29 (Control Mean = 144.4)			
	-12.8	-15.8	-18.8	-17.6	8.5	-46.5
	(13.4)	(9.0)	(7.2)	(6.6)	(128.7)	(10.3)
p-val	0.158	0.085	0.006	0.018	0.879	<0.001
BWs	8.7,9.0	7.6,8.1	7.2,8.0	7.4,8.1	10.3,10.8	6.6,5.8
N	2307	2099	2084	2095	2597	1690
Weights	Yes	Yes	Yes	Yes	No	Yes
FEs	No	State	State, Year	State, Year	State, Year	State, Year
Census Controls	No	No	No	Yes	Yes	Yes
Drop Biggest 3 Counties	No	No	No	No	No	Yes

Notes: This table provides estimates from Equation 1 of the effect of narrow election of a Republican local prosecutor on mortality from all causes. Column (1) uses no covariates, Columns (2) and (3) add in state and year fixed effects, respectively, and Column (4) adds a set of 2010 baseline county-level demographic controls. Column (5) uses the same covariates as Column (4) but does not weight the regression by population, and Column (6) drops the three biggest counties from the sample before estimating. 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. Outcome is mortality rate per 100,000 residents. The control mean estimate at the top of each subtable gives the intercept to the left of the cutoff from the Column 1) regression.

Table A6: Effects of Republican Prosecutor Election on All-Cause Mortality, White Americans

	(1)	(2)	(3)	(4)	(5)	(6)
			Ages <1 (Control Mean = 588.7)			
	-74.4	-75.9	-70.8	-48.6	-12.9	-31.6
	(24.7)	(17.8)	(19.3)	(17.3)	(57.1)	(20.3)
p-val	0.004	<0.001	0.001	0.018	0.928	0.305
BWs	9.2,10.0	8.3,8.3	9.2,9.4	10.4,8.0	7.1,7.9	10.6,9.3
N	2403	2143	2371	2253	2059	2432
			Ages 1-4 (Control Mean = 24.6)			
	-2.9	-1.7	-1.5	-1.0	-3.6	-4.0
	(1.7)	(1.3)	(1.2)	(1.2)	(4.7)	(1.7)
p-val	0.043	0.118	0.151	0.268	0.388	0.013
BWs	7.3,8.4	8.9,9.0	10.6,9.6	10.4,8.7	8.3,8.3	7.9,7.4
N	2138	2330	2460	2345	2147	2006
			Ages 5-9 (Control Mean = 10.5)			
	-0.7	-1.2	-1.2	-0.9	-6.0	-1.7
	(1.1)	(1.1)	(1.1)	(1.0)	(2.2)	(1.2)
p-val	0.393	0.302	0.303	0.390	0.014	0.162
BWs	10.8,11.2	8.8,8.1	8.8,8.0	8.5,7.6	8.5,8.1	7.9,7.2
N	2639	2166	2166	2080	2149	1981
			Ages 10-19 (Control Mean = 32.6)			
	-2.1	-2.6	-2.8	-1.9	-3.0	-2.0
	(2.0)	(1.5)	(1.4)	(1.0)	(3.5)	(1.5)
p-val	0.178	0.120	0.066	0.088	0.519	0.250
BWs	8.5,9.7	7.7,7.7	7.8,7.8	7.9,8.0	9.1,9.1	6.3,7.4
N	2349	2049	2061	2121	2338	1848
			Ages 20-29 (Control Mean = 98.7)			
	-3.8	-3.2	-3.9	-0.6	-5.5	0.9
	(6.1)	(3.7)	(3.5)	(2.5)	(8.5)	(3.4)
p-val	0.360	0.437	0.246	0.605	0.970	0.998
BWs	9.4,11.0	8.1,8.8	7.9,8.5	9.9,9.6	6.2,6.6	9.3,8.9
N	2552	2222	2185	2441	1802	2267
Weights	Yes	Yes	Yes	Yes	No	Yes
FEs	No	State	State, Year	State, Year	State, Year	State, Year
Census Controls	No	No	No	Yes	Yes	Yes
Drop Biggest 3 Counties	No	No	No	No	No	Yes

Notes: This table provides estimates from Equation 1 of the effect of narrow election of a Republican local prosecutor on mortality from all causes. Column (1) uses no covariates, Columns (2) and (3) add in state and year fixed effects, respectively, and Column (4) adds a set of 2010 baseline county-level demographic controls. Column (5) uses the same covariates as Column (4) but does not weight the regression by population, and Column (6) drops the three biggest counties from the sample before estimating. 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. Outcome is mortality rate per 100,000 residents. The control mean estimate at the top of each subtable gives the intercept to the left of the cutoff from the Column 1) regression.

Table A7: Effects of Republican Prosecutor Election on All-Cause Mortality, Men

	(1)	(2)	(3)	(4)	(5)	(6)
			Ages <1 (Control Mean = 753.6)			
	-92.9	-70.7	-69.1	-76.5	-34.6	-21.7
	(40.2)	(32.2)	(31.8)	(24.4)	(53.2)	(29.7)
p-val	0.018	0.043	0.056	0.004	0.575	0.653
BWs	9.2,9.2	9.7,8.7	9.3,8.9	8.3,7.8	10.4,9.5	8.3,7.7
N	2348	2313	2301	2083	2448	2047
			Ages 1-4 (Control Mean = 29.3)			
	-6.1	-5.0	-6.3	-6.3	-11.9	-7.6
	(2.2)	(1.6)	(1.6)	(1.4)	(5.0)	(2.3)
p-val	0.003	0.002	<0.001	<0.001	0.022	0.002
BWs	8.6,8.0	9.9,9.3	8.6,7.9	7.1,6.8	9.4,9.4	7.0,6.2
N	2146	2420	2104	1898	2388	1808
			Ages 5-9 (Control Mean = 13.9)			
	-3.1	-3.7	-3.7	-3.7	-2.3	-0.6
	(1.6)	(1.4)	(1.4)	(1.3)	(3.7)	(1.8)
p-val	0.041	0.008	0.007	0.006	0.468	0.710
BWs	9.6,9.0	7.5,7.3	7.6,7.4	7.6,7.1	8.5,8.4	7.1,6.9
N	2374	1983	2011	1967	2198	1890
			Ages 10-19 (Control Mean = 46.2)			
	-3.5	-3.4	-3.6	-2.6	-7.0	-7.4
	(3.3)	(2.5)	(2.3)	(1.6)	(5.5)	(2.2)
p-val	0.183	0.144	0.088	0.062	0.177	<0.001
BWs	8.8,9.2	8.6,8.2	8.5,8.4	10.2,10.1	10.0,9.6	7.2,7.9
N	2343	2158	2197	2485	2441	1994
			Ages 20-29 (Control Mean = 147.9)			
	-7.0	-8.1	-9.7	-8.2	-15.7	-12.4
	(10.0)	(6.9)	(5.9)	(3.7)	(13.7)	(5.6)
p-val	0.276	0.204	0.057	0.014	0.653	0.022
BWs	9.2,10.5	8.0,8.0	8.2,8.2	8.2,9.2	7.7,8.5	8.3,8.3
N	2499	2129	2136	2313	2172	2125
Weights	Yes	Yes	Yes	Yes	No	Yes
FEs	No	State	State, Year	State, Year	State, Year	State, Year
Census Controls	No	No	No	Yes	Yes	Yes
Drop Biggest 3 Counties	No	No	No	No	No	Yes

Notes: This table provides estimates from Equation 1 of the effect of narrow election of a Republican local prosecutor on mortality from all causes. Column (1) uses no covariates, Columns (2) and (3) add in state and year fixed effects, respectively, and Column (4) adds a set of 2010 baseline county-level demographic controls. Column (5) uses the same covariates as Column (4) but does not weight the regression by population, and Column (6) drops the three biggest counties from the sample before estimating. 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. Outcome is mortality rate per 100,000 residents. The control mean estimate at the top of each subtable gives the intercept to the left of the cutoff from the Column 1) regression.

Table A8: Effects of Republican Prosecutor Election on All-Cause Mortality, Women

	(1)	(2)	(3)	(4)	(5)	(6)
			Ages <1 (Control Mean = 598.6)			
	-28.2	-7.0	-4.3	-6.0	119.6	-3.4
	(29.1)	(17.4)	(17.4)	(17.7)	(106.9)	(24.9)
p-val	0.431	0.910	0.888	0.780	0.208	0.856
BWs	9.0,9.2	8.9,7.6	9.3,7.7	10.2,8.9	9.5,8.6	10.2,10.1
N	2347	2101	2136	2365	2274	2462
			Ages 1-4 (Control Mean = 24.5)			
	-0.3	1.1	0.7	-0.5	7.8	-3.8
	(1.9)	(1.4)	(1.5)	(1.3)	(7.8)	(2.3)
p-val	0.705	0.620	0.849	0.561	0.431	0.071
BWs	8.6,8.7	8.7,6.7	8.3,6.9	7.6,7.8	8.3,8.3	6.6,6.8
N	2235	1993	1985	2051	2139	1809
			Ages 5-9 (Control Mean = 10.2)			
	1.0	1.7	1.7	1.6	-6.5	-0.1
	(1.0)	(0.8)	(0.7)	(0.7)	(3.1)	(1.0)
p-val	0.481	0.065	0.045	0.037	0.107	0.753
BWs	9.4,8.7	8.4,7.7	8.3,7.6	8.9,8.7	9.3,9.3	8.0,8.2
N	2282	2087	2069	2248	2387	2101
			Ages 10-19 (Control Mean = 20.6)			
	0.5	0.7	0.5	0.6	5.0	-0.5
	(1.4)	(1.3)	(1.1)	(0.9)	(3.8)	(1.3)
p-val	0.880	0.592	0.633	0.604	0.176	0.696
BWs	11.4,10.6	10.8,8.7	10.3,8.4	12.7,12.3	6.1,6.2	8.7,8.9
N	2652	2361	2301	2814	1732	2248
			Ages 20-29 (Control Mean = 56.8)			
	-4.6	-2.0	-2.1	-1.0	-0.2	-2.7
	(3.8)	(2.0)	(2.1)	(1.6)	(12.5)	(2.4)
p-val	0.109	0.250	0.287	0.388	0.658	0.155
BWs	8.7,10.2	7.9,8.1	7.9,8.0	8.5,8.5	7.5,7.7	7.5,7.0
N	2395	2121	2121	2209	2039	1933
Weights	Yes	Yes	Yes	Yes	No	Yes
FEs	No	State	State, Year	State, Year	State, Year	State, Year
Census Controls	No	No	No	Yes	Yes	Yes
Drop Biggest 3 Counties	No	No	No	No	No	Yes

Notes: This table provides estimates from Equation 1 of the effect of narrow election of a Republican local prosecutor on mortality from all causes. Column (1) uses no covariates, Columns (2) and (3) add in state and year fixed effects, respectively, and Column (4) adds a set of 2010 baseline county-level demographic controls. Column (5) uses the same covariates as Column (4) but does not weight the regression by population, and Column (6) drops the three biggest counties from the sample before estimating. 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. Outcome is mortality rate per 100,000 residents. The control mean estimate at the top of each subtable gives the intercept to the left of the cutoff from the Column 1) regression.

Table A9: Placebo Exercise: Effects on Pre-Election Mortality Rates

	(1)	(2)
	Real Estimate	Pre-Treatment Placebo Estimate
Ages 1-4, All Causes	-2.700	2.530
	(1.080)	(1.840)
p-value	0.010	0.170
N	2221	1575
Bandwidths	[9.60, 8.00]	
Ages 1-4, Accidents	-1.610	0.590
	(0.670)	(0.920)
p-value	0.020	0.520
N	1967	1374
Bandwidths	[7.50, 7.10]	
Ages 20-29, All Causes	-4.880	2.010
	(2.910)	(3.520)
p-value	0.090	0.570
N	2333	1616
Bandwidths	[10.10, 8.70]	
Ages 20-29, Guns	-3.130	-0.510
	(1.530)	(1.080)
p-value	0.040	0.640
N	2035	1404
Bandwidths	[7.60, 7.60]	

Notes: This table presents results from the placebo exercise described in Section IV.2. The Column (1) and (2) estimates within the same subtable come from the same regression (estimated using Equation 2). The Column (1) estimate reproduces our main estimates within the context of Equation 2 and the Column (2) estimate conveys the effect of the Republican vote share variable in the years before the closely contested election took place.

Table A10: Robustness: Varying Bandwidth Selection and Kernel Weighting Function

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 1-4 (Control Mean = 44.235)					
	-2.698	-3.245	-2.070	-3.368	-2.995	-4.307
	(0.927)	(0.954)	(0.881)	(1.188)	(1.168)	(1.265)
p-val	0.003	0.001	0.017	0.005	0.009	<0.001
BWs	9.6,8.0	8.0,8.0	9.8,9.8	6.1,5.6	6.1,6.1	5.2,5.2
N	2221	2125	2441	1654	1729	1560
	Ages 20-29 (Control Mean = 56.802)					
	-4.876	-4.215	-4.792	-5.276	-6.864	-5.228
	(2.358)	(2.259)	(2.350)	(3.083)	(3.410)	(3.072)
p-val	0.024	0.052	0.023	0.026	0.011	0.045
BWs	10.1,8.7	10.1,10.1	8.7,8.7	5.8,6.2	5.6,5.6	6.2,6.2
N	2333	2483	2238	1708	1621	1737
BW Selection	msecomb2	mserd	msesum	msecomb2	mserd	msesum
Kernel Function	Triangular	Triangular	Triangular	Uniform	Uniform	Uniform

Notes: This table provides estimates from Equation 1 of the effect of narrow election of a Republican local prosecutor on mortality from all causes. Column (1) uses our preferred specification and estimation choices, Columns (2) and (3) vary the optimal bandwidth selection process, and Columns (4) through (6) use a uniform kernel weighting function and vary the optimal bandwidth selection process.