Local Prosecutors and Public Health*

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February 12, 2026

Abstract

Legal scholars argue that local prosecutors are the most powerful actors in the criminal justice system, as they make decisions from whether to charge or dismiss a case through directly negotiating almost all defendants' conviction. Yet, how prosecutorial approaches shape community level outcomes has a limited understanding. This paper investigates the causal relationship between approaches taken by local criminal prosecutors and community-level mortality rates. We leverage plausibly exogenous variation in prosecutorial approaches generated by closely contested partisan prosecutor elections, a context in which Republican prosecutorial candidates are commonly characterized as "tougher on crime." Using data from hundreds of closely contested partisan elections from 2010 to 2019 and 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 predominantly by reductions in firearm-related deaths, including a reduction in firearm homicide among Black men and a reduction in firearm suicides and accidents primarily among White men. Mechanism analyses indicate that increased prison-based incapacitation explains about one third of the effect among Black men and none of the effects among White men. Instead, the primary channel appears to be substantial increases in criminal conviction rates across racial groups and crime types, which then reduce firearm access through legal restrictions on gun ownership for the convicted.

*We thank Victoria Barone, Jesse Bruhn, Ellora Derenoncourt, Bill Evans, Sara Heller, Carl McPherson, Samuel Norris, and Kyung Park for their exceedingly helpful feedback. We also thank Ashna Arora for many fruitful conversations at various stages of this project, and for sharing election data, and thank Allison Stashko for feedback and for sharing election data as well. Clara Chambers and Jansen Hammock provided excellent research assistance. Further, we thank seminar and conference participants at Wellesley College, at the American Society of Health Economists (ASHEcon), and the Midwest Health Economics Conference for their invaluable feedback.

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In the United States, public safety and public health are inextricably linked. Gun violence has consistently ranked among the top causes of death for young Americans for decades (David-Ferdon et al., 2013), and the former U.S. Surgeon General recently declared it 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] Some other common and preventable causes of death also overlap with public safety; for example, 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 empirical strategy leverages plausibly exogenous variation in prosecutorial approaches generated by closely contested partisan local prosecutor elections. In line with the party's platform, Republican-affiliated prosecutors are often characterized as "tough on crime" candidates who will enforce criminal statutes more stringently than their Democratic counterparts.[3] To see whether this more aggressive approach to criminal prosecution translates into

[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).

[3]While data characterizing local prosecutorial candidate platforms is unavailable, the Republican Party's presidential platforms typically put forth this idea. Their 2008 platform stated: "Criminals behind bars

meaningfully different community health outcomes, we use data from 2010 to 2019 and a regression discontinuity design to compare mortality rates in counties which narrowly elect a Republican prosecutor to those in counties which narrowly fail to elect one. We then investigate the specific mechanisms by which these effects manifest.

Our key result shows 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 (9.8 fewer deaths per 100,000, or a 6.6% reduction) and conforms with the well-documented fact that criminal activity tends to peak in one's twenties and decline quickly thereafter (Rocque et al., 2015). We then show that the mortality reduction is concentrated in deaths involving firearms, and heterogeneity analysis indicates a large reduction in firearm homicides among young Black men and a smaller reduction in firearm suicides/accidents among young White men.

In relation to the opioid crisis, we find no evidence that narrow election of a Republican prosecutor affects overall drug or opioid overdose mortality among any demographic groups. However, among the middle-aged, this null result may obscure 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.

Our mechanisms analysis clarifies the channels through which these effects manifest. We observe increases in new prison admissions among Black men; however, we estimate that prison-based incapacitation only explains about one third of the mortality reduction among this group and none of the reduction among White men. We also find no clear effects on admissions to local jails, suggesting little change in policing behavior or general criminal deterrence.

cannot harm the general public...we support mandatory prison sentencing for gang crimes, violent or sexual offenses against children, repeat drug dealers, rape, robbery and murder." The 2012 platform contained nearly identical language. In contract, the Democratic Party's platforms tend to emphasize deterrence and social programs over aggressive prosecution. Their 2016 platform stated: "[We] are committed to reforming our criminal justice system and ending mass incarceration. Something is profoundly wrong when almost a quarter of the world's prison population is in the United States...We will reform mandatory minimum sentences and close private prisons and detention centers."

However, we find evidence of large increases in the number of criminal convictions issued per year. These increases occur for all severity levels (misdemeanors and felonies) and offense types (violent, drug, and property offenses). The demographic patterns also mirror our mortality results: large increases in convictions among Black individuals and smaller but nonzero effects among White ones. Taken together, these findings suggests that higher conviction rates - many of which impose legal restrictions on gun ownership - meaningfully reduce the likelihood of gun access for some on the margin of gun violence or self-harm, thus explaining the reductions in firearm mortality. The increase in drug convictions is also consistent with a shock to illict drug supply and substitution to licit ones, explaining our opioid mortality results.

We take several steps to validate our results. First, we show that geographic characteristics and baseline county-level demographics are balanced across the vote share threshold. Second, we demonstrate that narrow election of a Republican local prosecutor is unrelated to the political affiliation of the community's sheriff, which suggests that our results are not confounded by contemporaneous changes in other local offices. 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. In addition, we find no clear effects on mortality from internal causes (i.e., medical conditions and diseases), which should not reasonably be affected by local prosecutors. Finally, our key results are robust to a number of analysis choices: dropping the counties with the largest regression weights from the analysis, varying the bandwidths used in estimation, and conducting statistical inference using permutation tests.

This paper contributes to multiple literatures, beginning with a nascent empirical literature on local prosecutors. Several unpublished working papers also suggest a first stage relationship between local prosecutor party affiliation and prosecutorial approaches, suggesting that Republican prosecutors either increase conviction (Fischer and Ludwig, 2023) or incarceration rates (Arora, 2019; Krumholz, 2020) broadly, or among Black defendants

specifically (Yntiso, 2022). Our mechanisms analysis leverages data from more states districts than are analyzed in this existing work and largely fits with their findings. 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.

More importantly, we expand the literature on prosecutors by examining how prosecutorial approaches affect key non-criminal justice community outcomes. Stashko and Garro (2021) show that prosecutor turnover (regardless of party) decreases deaths caused by police; beyond this, we know of no causal evidence linking prosecutorial approaches to broad community outcomes, let alone one as core to social well-being as health. Legal scholars have long noted that prosecutors possess significant power and discretion (Misner, 1995; Stuntz, 2001; Bibas, 2009; Bellin, 2019), and recent attempts to constrain and remove them from office reflect a widespread belief that they can fundamentally influence conditions in their districts (Hessick and Su, 2023). We provide direct 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.

This paper also contribute to the literature on the mechanics and social costs of gun access in the United States. Specifically, our findings point to a much stronger link between criminal prosecution and gun access than is currently recognized. In theory, criminal convictions restrict gun access: The Gun Control Act of 1968 made firearm possession illegal for individuals with felony convictions, and the Brady Act of 1993 both extended this restriction to those convicted of misdemeanor domestic violence and established a national criminal background check system. Several states also impose further restrictions for those convicted of certain violent misdemeanor offenses (Giffords Law Center, 2025).

Some summary evidence suggests these restrictions may have some bite. For example, between 2015 and 2019, about 325,000 individuals failed a background check when attempt-

ing to purchase a gun due to a felony conviction, and about 109,000 failed due to a prior domestic violence conviction (Everytown Research & Policy, 2020). However, counts of failed background checks do not capture individuals deterred by their criminal histories from attempting a purchase in the first place, so such statistics may substantially understate the true effect. At the same time, the literature on gun markets shows that those who cannot legally possess a gun can often still obtain one through secondary markets, and that many firearms used in criminal activity are acquired from this source.[4]

Our findings suggest that criminal conviction-based restrictions, while not fool-proof, do meaningfully reduce gun access. Our findings on reduced gun suicides/accidents among young White men emphasis this point. Gun suicide mortality rates have been shown to be one of the best proxy measures of gun ownership rates (Azrael et al., 2004; Kleck, 2004; Cook and Ludwig, 2006), and since White men do not experience an increase in detention under a Republican prosecutor, the observed decline in gun mortality must operate through changes in gun access rather than incarceration.[5]

Our findings also suggest that conviction-based restrictions on gun access may be key to effectively reducing gun mortality. Our back-of-the-envelope calculations imply that, for roughly every 90 newly imposed restrictions per year among those on the margin of receiving one, about one firearm death is prevented. Broader forms of gun control are also effective when successfully implemented (Rudolph et al., 2015; Luca et al., 2017; Williams Jr, 2017; Donohue et al., 2019); however, they can also backfire by spurring firearm sales in anticipation of looming restrictions (Bollman et al., 2025; Levine and McKnight, 2017). Local gun control policy in the US also suffers from a well-documented externality problem: Firearms flow from states and cities with weaker restrictions to those with tighter ones (Knight, 2013), making it difficult to reduce the supply of guns without broad federal regulations. Our results suggest that restrictions targeted at those on the margin of harm to themselves or others can generate

[4]See Cook (2018) for a summary of the large literate on gun markets.

[5]More specifically, these studies argue that the share of suicides committed with a firearm is an especially informative proxy for gun ownership; we observe effects on this measure as well.

significant social welfare gains.

Last, this paper also contributes to the literature on the opioid crisis and its intersection with the criminal justice system, wherein we find suggestive evidence of 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 may lead people to substitute to misuse of licit ones.

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 mortality results and findings on heterogeneity. 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.[6] Local prosecutors lead offices

[6]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

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.[7] 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 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, a process by which the prosecution negotiates with the defense for at least one guilty/no

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.

[7]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).

plea in exchange for certain benefits.[8] 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.[9] This paper focuses on states with partisan prosecutor elections. Appendix Table A1 provides information on the number of districts, term lengths, and election timings for all thirty-eight states with partisan local prosecutor elections. Most terms are four years, and election timing (e.g., coinciding with presidential or midterm elections) varies across states and sometimes across districts within a state.

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 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.[10] When a candidate runs unopposed in the general election, the vote share variable will be coded as 0 (unopposed Democrat) or 1 (unopposed Republican).[11]

[8]Criminal cases in the US rarely go to trial. Estimates suggest that well over 90% of cases resolve with either a plea bargain or a dismissal (Johnson, 2023).

[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.

[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,

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 ages 18-64 without health insurance, and education levels. We also use data from the U.S. Drug Enforcement Administration (DEA)'s Automation of Reports and Consolidated Orders System (ARCOS) pill shipment database to summarize county-level opioid pill volume per capita.

Table 1 conveys that there are 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.[12] 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 base-

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).

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

line 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.).[13]

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 three broad categories: external causes due to outside forces or substances acting upon the body, internal causes due to diseases and other medical conditions, and unknown causes.[14] We focus primarily on mortality due to external causes. We further partition all external causes into four mutually exclusive categories: (1) homicides, (2) drug overdoses, (3) motor vehicle accidents, and 4) non-drug/non-vehicle suicides and accidents.[15] We also explore effects on three types

[13]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 ten-year groups.

[14]Deaths from an unknown cause make up about 1% of all deaths.

[15]We combine suicides and accidents for two reasons. First, it can be difficult for medical examin-

of firearm mortality: any death involving a firearm (i.e., firearm homicides, suicides, and accidents), firearm homicides, and firearm suicides/accidents.

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.

Appendix Section B contains a full list of the ICD-10 codes used to categorize causes of death along with several summary figures depicting trends in mortality across our sample period. These figures provide additional context for our mortality results and show that mortality levels and trends in counties with closely contested local prosecutor elections are quite similar to the U.S. as a whole.

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 sen-

ers/coroners to distinguish suicides from accidents, and a non-trivial number of deaths are classified as "undetermined intent," making it hard to reliably separate the two categories (Snowdon, 2023). Second, breaking deaths into more narrowly defined categories make them relatively more rare, which in our county-by-year-by-age group analysis can lead to excess zeros and issues with statistical inference.

tenced 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.

The VIJIT data report both facility admissions and populations (i.e., both inflows and stocks) as rates per 100,000 residents ages 15 to 64 years old. We focus 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.[16]

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. Coverage for these demographic specific rates is sparser; for example, 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 when coverage is measured by the share of the total estimating sample population covered, rates are much higher. For example, the county-by-year observations with available Black prison admission data account for 90.1% of the Black population within our estimating sample. Since our analysis weights by population, the loss of information for smaller counties should not meaningfully affect the results.[17]

I.5 Criminal Conviction Data

Our data on criminal conviction rates come from the Criminal Justice Administrative Records System (CJARS) Justice Outcome Explorers (JOE) dataset. We use the 2023Q3

[16]See Appendix Table A3 for more information on the data coverage across our sample.

[17]Appendix 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.

version. The JOE provides county-by-year information on two key variables: the per-capita rate of criminal charges issued by courts in the given county, and the fraction of charges issued that result in conviction. We multiply these variables to calculate criminal convictions per 100,000 residents. Conviction rates can be broken down by severity (misdemeanor or felony), offense type (drug, violent, or property offense), and the race of the defendant. Conviction rates are also available by the following age groups: 15-24, 25-40, and 40+ years old. We analyze the 25-40 age group, but our results are qualitatively similar among 15-24 year olds.

The JOE data possess some limitations. First, data coverage is limited to eleven of the thirty-four states studied in our mortality analysis. Second, county-level data are only available for counties with populations exceeding 80,000 people. These constraints produce modest coverage rates: Covered counties account for about 40% of the total population and 11% of all county-by-year observations within our estimating sample.[18] Despite these limitations, our sample includes over 200 county-by-year observations and can detect conviction rate effects of approximately 30%.[19] Logically, if changes in conviction rates are an important mechanism linking prosecutors to community-wide mortality, those changes would need to be sizable, and we are powered to detect large but realistic effect sizes.

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

[18] see Appendix Tables A5 and A6 for more information on data coverage.

[19] See Appendix Table A7 for more information on these power calculations.

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 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.

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

Our preferred specification includes a number of covariates, beginning with δ_s and γ_t, which are sets of state and year fixed effects. We also include a vector of county-level controls, X_c. In our mortality analysis, X_c contains all of the 2010 characteristics summarized in Table 1, along with a collection of baseline county-level mortality rates. We use 2006 as the baseline year and control for five mortality rates: all-cause mortality and mortality from the four external causes listed in Section I.3.[20] 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.[21] In our mechanisms analysis of prison and jail admissions, X_c

[20]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.

[21]Nearly all of the variation in Republican vote share comes from across district comparisons, so we

contains the same variables as our mortality analysis but swaps the baseline mortality rates for baseline outcomes averaged over 2004-2006.[22] In our analysis of conviction rates, due to the smaller sample size, we limit X_c to the set of racial demographic variables and the log of population from Table 1.

Our preferred regression 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., 2019a) 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 consistency in the estimating sample. In Appendix Section D, 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.[23]

We perform a number of tests to probe the validity of this empirical strategy throughout

are unable to include county or electoral district fixed effects without throwing out essentially all of the identifying variation.

[22]Since this data is sparser than the mortality data, we average across this period to increase the likelihood of at least one non-missing observation.

[23]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.

the paper. To begin, 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 note other exercises provided in the Appendix. Appendix Figure A1 reveals no clear discontinuity in the density of our running variable across the cutoff, and Appendix Section C verifies that our strategy does not confound the effects of the local prosecutor's political affiliation with that of the county's sheriff. Further tests are contained in Appendix Section D and referenced in the next section.

III Mortality 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 9.8 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.[24] 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.[25]

Table 4 explores the specific causes of death that drive the reduction in mortality among young men. The effect on all-cause mortality is driven entirely by reductions in death from external causes (10.1 fewer deaths per 100,000, Column 1) and predominantly by firearm deaths, which fall by 6.7 deaths (Column 2). The firearm reduction reflects significant de-

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

[25]Appendix Table A8 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 5.91 fewer deaths per 100,000.

clines in both firearm homicides (4.2 fewer deaths, Column 4) and firearm suicides/accidents (2.5 fewer deaths, Column 6). Overall homicides decline by 3.8 deaths (Column 3), suggesting little change in non-firearm homicides. Non-drug/non-vehicle suicides and accidents fall by 4.7 deaths (Column 5), driven mainly by the firearm component (Column 6). There are no significant effects on drug overdoses or vehicle accidents (Columns 7-8). Overall, these results point to gun access as an important mechanism.[26]

Figure 2 visualizes several of 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 effect on firearm mortality (Panel C). Appendix Section D shows that these mortality findings are robust to a number of alternate analysis choices and placebo tests.

The null result on drug overdoses among young men may be surprising given the scale of the opioid crisis in the US during the 2010s. Figure 3 focuses on opioid overdose mortality in particular and investigates whether there are effects among all 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 A3), lead to an overall null effect on opioid overdose mortality.

Table 5 returns to our analysis of young men and investigates whether any subgroups drive the effects. Panel A focuses on young Black men, and Column 1 shows that narrow election

[26]Appendix Figure A2 also shows clear effects on the percentage of suicides committed with a gun, which has been shown to be arguably the best proxy for gun ownership rates (Azrael et al., 2004; Kleck, 2004; Cook and Ludwig, 2006).

of a Republican local prosecutor reduces external cause mortality among this group by 30 deaths per 100,000 (a 18% reduction).[27] This reduction is driven primarily by a substantial decline in firearm homicides (22.5 fewer deaths). There is also a smaller reduction in deaths from suicides/accidents (6.8 fewer deaths, Column 5), and this is only explained only partial by firearm suicides/accidents (1.9 fewer deaths, Column 6).[28]

Among White men (Panel B), the pattern differs noticeably. Overall external cause mortality declines by 4.9 deaths, but this estimate is less precise. Firearm deaths fall by 3.3 deaths, but unlike Black men, this reduction stems entirely from firearm suicides/accidents, with no effect on firearm homicides. These racial patterns mirror baseline mortality - firearm homicides dominate among Black men (85% of firearm deaths), while firearm suicides/accidents dominate among White men (63% of firearm deaths). We find no significant effects on drug overdoses or vehicle accidents for either racial group.

Figure 4 visualizes several of these race-specific effects on firearm mortality. The figure shows clear discontinuities at the cutoff for firearm deaths and firearm homicides among Black men, as well as smaller but discernible discontinuities for firearm deaths and firearm suicides/accidents among White men. This heterogeneity motivates our subsequent mechanism analysis, which explores how prosecutorial approaches differentially affect firearm access and violence across these groups.[29]

[27] Among the 2,149 county-by-year observations in our main sample, 11 (or 0.5%) have zero Black men ages 20–29 and are therefore excluded from this analysis. We note that the usage of population weights in the regressions ensures that counties with very small populations have very little influence on the results and that the weight given to observations asymptotically approaches zero as their population shrinks.

[28] This pattern is consistent with other work showing that the criminally involved possess higher baseline rates of accidents and unintentional injuries (Siponen et al., 2023).

[29] 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 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.

IV Mechanisms

IV.1 Prison Admissions

We now explore mechanisms underlying our mortality results. Specifically, we look for patterns within outcomes plausibly connected to prosecutorial behavior and firearm mortality that match our heterogeneity findings (large effects among Black men and smaller but nonzero effects among White men). We first investigate whether Republican prosecutors secure more prison sentences. Prison-based incapacitation reduces mortality risk among marginal inmates (Norris et al., 2022b; Hjalmarsson and Lindquist, 2022), and prosecutors substantially influence charging and sentencing, suggesting incapacitation as a potential mechanism.

Figure 5 presents RDD estimates of the effect of narrow Republican local prosecutor election on annual county-level admissions to prisons. There are no effects for all Americans ages 15 to 64 (Panel A), nor among White Americans specifically (Panel B). In contrast, prison admissions rise among Black individuals by 201.2 per 100,00 (Panel C) and among Black men by 339.3 per 100,000 (Panel D). This second estimate constitutes a 19% increase over the baseline average. Appendix results show no effects by gender (see Figure A6). Figure A5 examines effects on prison populations instead of admissions and reveals similar patterns.

The absence of effects among White individuals–who experience firearm mortality reductions–indicates incapacitation cannot fully explain our findings. However, it is worth considering whether incarceration-based mortality reductions explain effects among Black men. Appendix Section E shows that this channel explains about one-third of the mortality reduction among Black men. While incarceration may generate indirect effects through deterrence or network spillovers, these are unlikely to explain the remaining two-thirds, and hence incarceration alone cannot fully account for our results.

IV.2 Jail Admissions

We next examine jail effects. Jails provide another incapacitation venue, and changes in jail admissions or populations may also reflect shifts in criminal activity from general deterrence. However, prosecutors influence jail admissions and populations less directly than prisons (police most directly influence jail admissions), making predictions less clear.[30]

Our findings on local jails are presented in the Appendix in Figure A7. 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 results suggest that our mortality findings are not explained by changes in policing behavior, general criminal deterrence, or jail-based detention.

IV.3 Criminal Convictions

Finally, we investigate whether increased criminal convictions reduce firearm mortality by restricting gun access, even without prison sentences. These restrictions arise from federal bans on gun ownership among convicted felons and those convicted of domestic violence offenses.

Figure 6 presents RDD estimates of the effect of narrow Republican local prosecutor election on the annual per-capita rate of criminal convictions issued among Americans ages 25 to 40. Panel A shows misdemeanor convictions increase by 1,615 per 100,000. Panel B

[30]To illustrate, police may increase arrests under a more aggressive prosecutor with the belief that those arrested are more likely to be prosecuted, which would increase jail admissions. Conversely, jail admissions could also fall if more aggressive prosecution or increased pretrial detention lengths average jail stays, thus mechanically reducing the likelihood of individuals repeatedly returning to jail within a given period. More aggressive prosecutors could also produce a general criminal deterrence effect that dissuades 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.

focuses on felony convictions and shows an increase of 650 convictions per 100,000. Panels C and D show increases in both drug and violent convictions, suggesting that these additional convictions are not concentrated in just one offense category.[31]

Table 6 shows that these conviction effects align with our mortality findings. Effects on convictions are seen for both Black and White Americans, but the effects are noticeably larger for Black individuals. Felony conviction rates - which most directly restrict gun ownership - increase by 62% for Black Americans, but only 28% for White ones. These mirror the noticeably larger mortality reductions for Black young men and smaller but nonzero effects for White men.

Last, we estimate the number of firearm possession restrictions implied by our conviction results and their effect on firearm mortality. The full calculations are found in Appendix Section E.2. We estimate that narrow election of a Republican prosecutor generates approximately 1,331 additional gun restrictions per 100,000 Black individuals and 299 per 100,000 White individuals. Combined with our mortality estimates and after accounting for incarceration effects among Black men, this implies one firearm death prevented per 82 restrictions among Black individuals and per 91 restrictions among White individuals. These estimates are plausible given that restrictions apply to individuals with elevated risks of firearm violence.

V Conclusion

This paper investigates how local prosecutors affect community-level mortality. Leveraging closely contested partisan prosecutor elections and a regression discontinuity design, we find that narrow election of a Republican prosecutor reduces all-cause mortality among young men ages 20 to 29 by 6.6%, stemming mainly from reductions in firearm deaths. Our mechanism analysis points to criminal convictions—-rather than incarceration alone—-as

[31] In the Appendix, we show two other sets of results: convictions for property crimes increase as well, and the increases in drug and violent crime convictions occur for both misdemeanors and felonies. See Appendix Figure A8.

the primary channel. Many of these convictions restrict firearm access under federal law, and we estimate that one firearm death is prevented for every 82-91 additional restrictions imposed.

These findings make several contributions. We provide causal evidence that prosecutorial approaches substantially affect mortality, an important outcome intimately tied to community wellbeing. We also identify conviction-based firearm restrictions as an important and previously understudied mechanism linking criminal justice policy to public health outcomes.

Our results raise important questions for future research and policy design. While conviction-based firearm restrictions generate substantial public health benefits, they are imposed through criminal justice contact that often comes with non-trivial social costs (Agan et al., 2023; Travis et al., 2014). This raises the question of whether alternative policies could achieve similar reductions in firearm access among high-risk individuals without requiring criminal justice contact. The long-term and intergenerational consequences of increased conviction rates for individuals and communities also warrant investigation. Understanding any trade-offs inherent in these questions is critical for developing policies that effectively reduce firearm mortality while minimizing other social costs.

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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
% Adults 18+64 w/o 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
	-9.8***	-1.9	-3.4	-2.8
	(3.3)	(5.5)	(7.6)	(13.2)
p-value	0.003	0.640	0.616	0.853
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
	-1.4	3.7	-5.1	-10.7
	(1.6)	(2.9)	(6.6)	(8.7)
p-value	0.341	0.277	0.540	0.211
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, Men Ages 20-29 Years Old

	(1) All External Causes	(2) Firearm Deaths	(3) All Homicides	(4) Firearm Homicides	(5) Non-Drug/Non-Vehicle Suicides/Accidents	(6) Firearm Suicides/Accidents	(7) Drug Overdoses	(8) Vehicle Accidents
	-10.1***	-6.7***	-3.8***	-4.2***	-4.7***	-2.5***	-0.4	-1.3
	(3.0)	(1.8)	(1.4)	(1.5)	(1.2)	(0.7)	(2.6)	(1.2)
p-value	0.001	0.001	0.008	0.005	0.001	0.001	0.765	0.350
Control Mean	115.4	38.3	25.3	22.2	35.6	16.1	26.9	27.6
N	2149	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. Each column examines effects on mortality due to the cause category noted at the top of the column. The causes examined in Columns (3), (5), (7), and (8) form a partition of all external causes of death. 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) Firearm Deaths	(3) All Homicides	(4) Firearm Homicides	(5) Non-Drug/Non-Vehicle Suicides/Accidents	(6) Firearm Suicides/Accidents	(7) Drug Overdoses	(8) Vehicle Accidents
	-30.0***	-24.4***	-22.4***	-22.5***	-6.8***	-1.9	2.5	-3.4
	(7.4)	(5.9)	(5.2)	(5.4)	(1.9)	(1.4)	(2.0)	(2.8)
p-value	0.001	0.001	0.001	0.001	0.001	0.163	0.233	0.228
Control Mean	166.9	101.5	94.0	86.6	31.2	14.9	12.6	29.1
N	2138	2138	2138	2138	2138	2138	2138	2138

Panel B: White Men

	(1) All External Causes	(2) Firearm Deaths	(3) All Homicides	(4) Firearm Homicides	(5) Non-Drug/Non-Vehicle Suicides/Accidents	(6) Firearm Suicides/Accidents	(7) Drug Overdoses	(8) Vehicle Accidents
	-4.9	-3.3**	0.7	0.0	-4.7***	-3.3***	-0.8	-0.1
	(3.4)	(1.4)	(1.0)	(0.9)	(1.5)	(0.9)	(3.1)	(1.2)
p-value	0.137	0.020	0.460	0.974	0.003	0.001	0.669	0.920
Control Mean	110.4	27.2	12.2	10.0	38.0	17.2	31.3	28.8
N	2149	2149	2149	2149	2149	2149	2149	2149

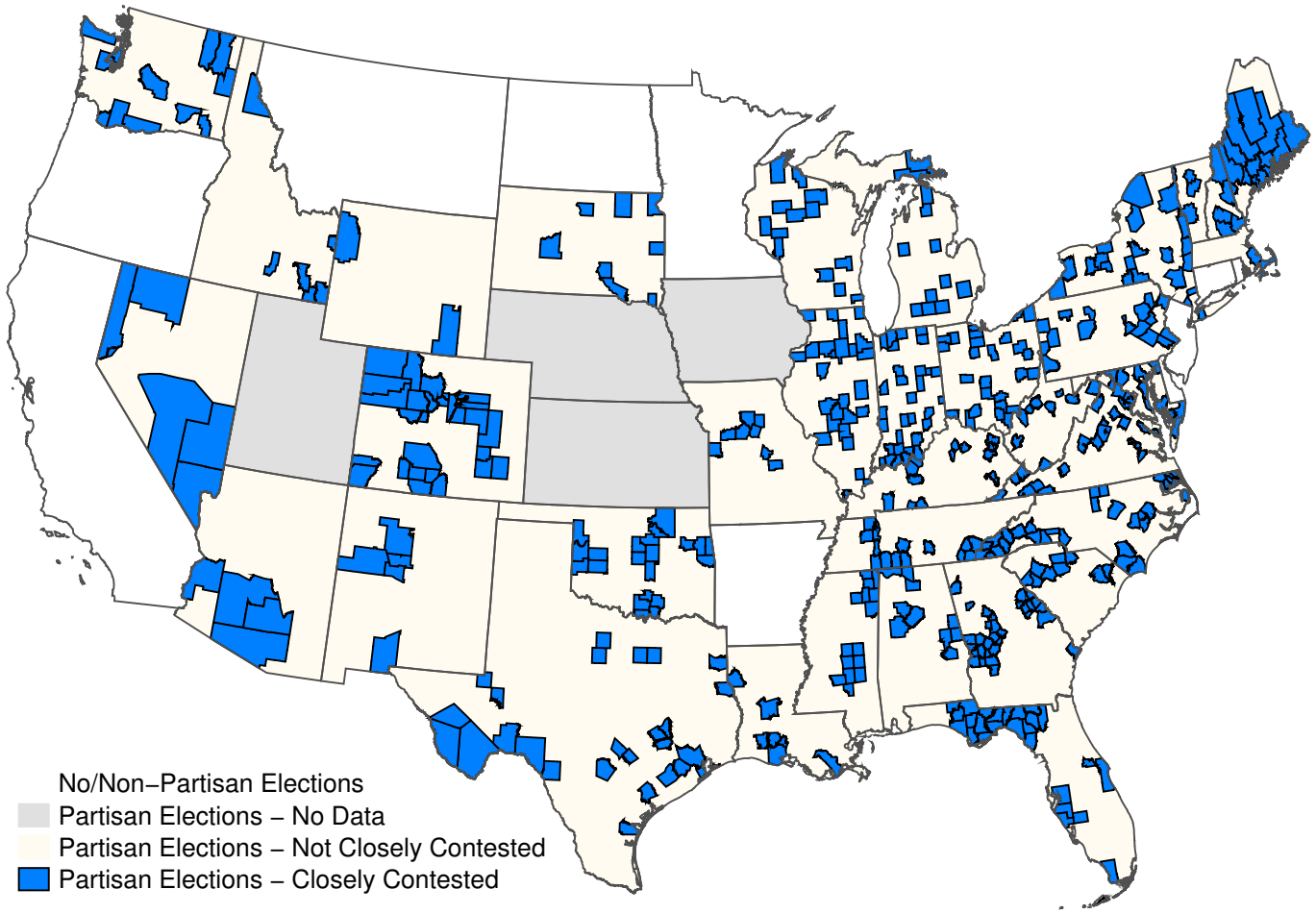
Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates among men ages 20 to 29. Panel A focuses on Black men and Panel B focuses on white men. Each column examines effects on mortality due to the cause category noted at the top of the column. The causes examined in Columns (3), (5), (7), and (8) form a partition of all external causes of death. See Section I.2 and the Appendix for more information on cause of death classification. There are 11 county-by-year observations with populations of Black men ages 20-29 equal to zero, which explains the differing number of observations between Panels A and B. 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 6: Effects of Narrow Republican Prosecutor Election on Criminal Conviction Rates, By Race and Severity

	(1) Black, Misdemeanor	(2) Black, Felony	(3) White, Misdemeanor	(4) White, Felony
	2990.7***	1866.5***	1072.9***	368.3***
	(782.2)	(400.7)	(220.7)	(103.0)
p-val	0.001	0.001	0.001	0.001
Control Mean	5279.3	3007.6	2431.7	1310.4
N	213	199	217	209

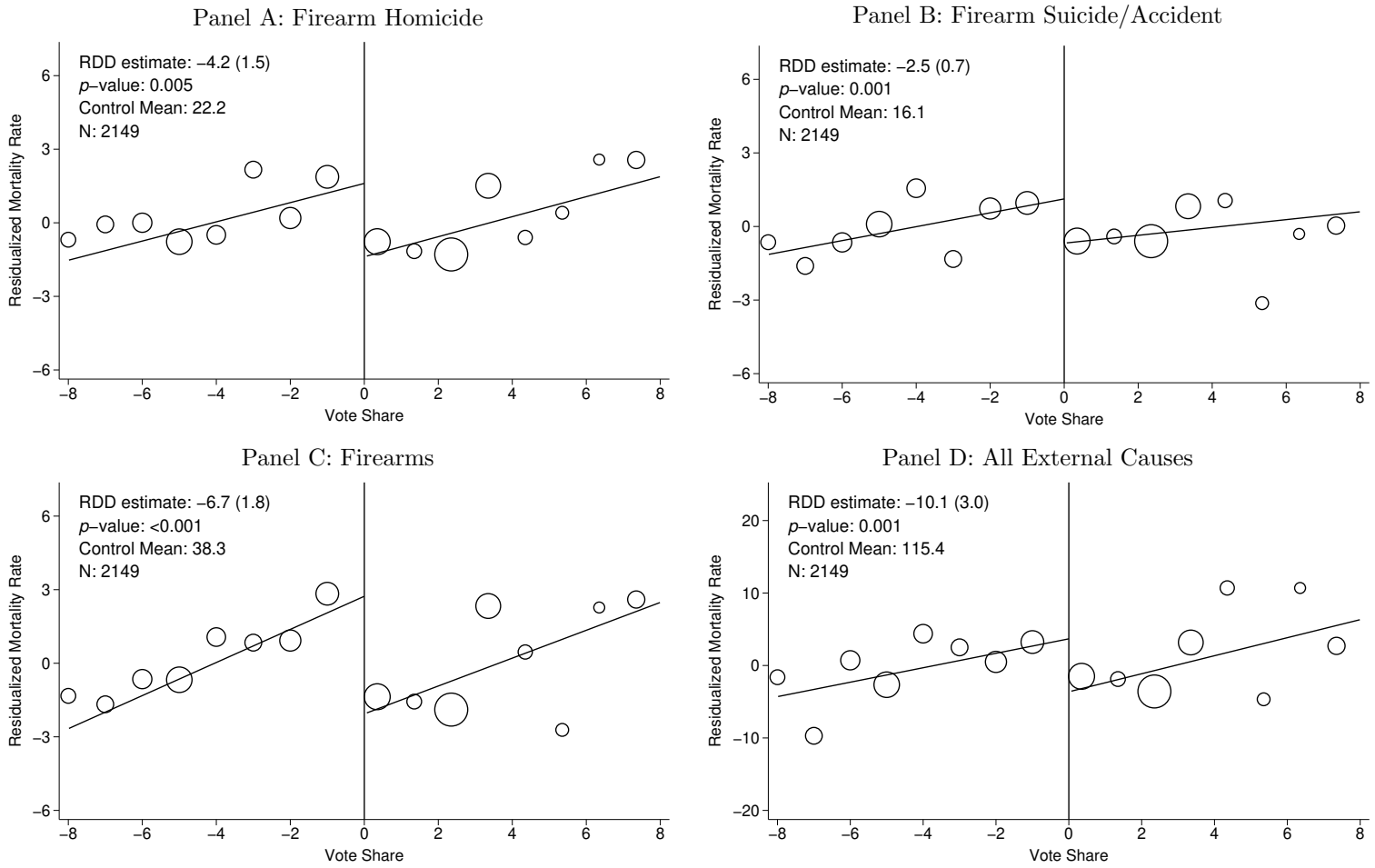
Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level rates of criminal convictions issued per 100,000 residents. Columns (1) and (2) examine effects on the misdemeanor and felony conviction rates among Black Americans ages 25-40. Columns (3) and (4) examines effects on the misdemeanor and felony conviction rates among white Americans ages 25-40. See Section I.5 for more information on the criminal conviction data. 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



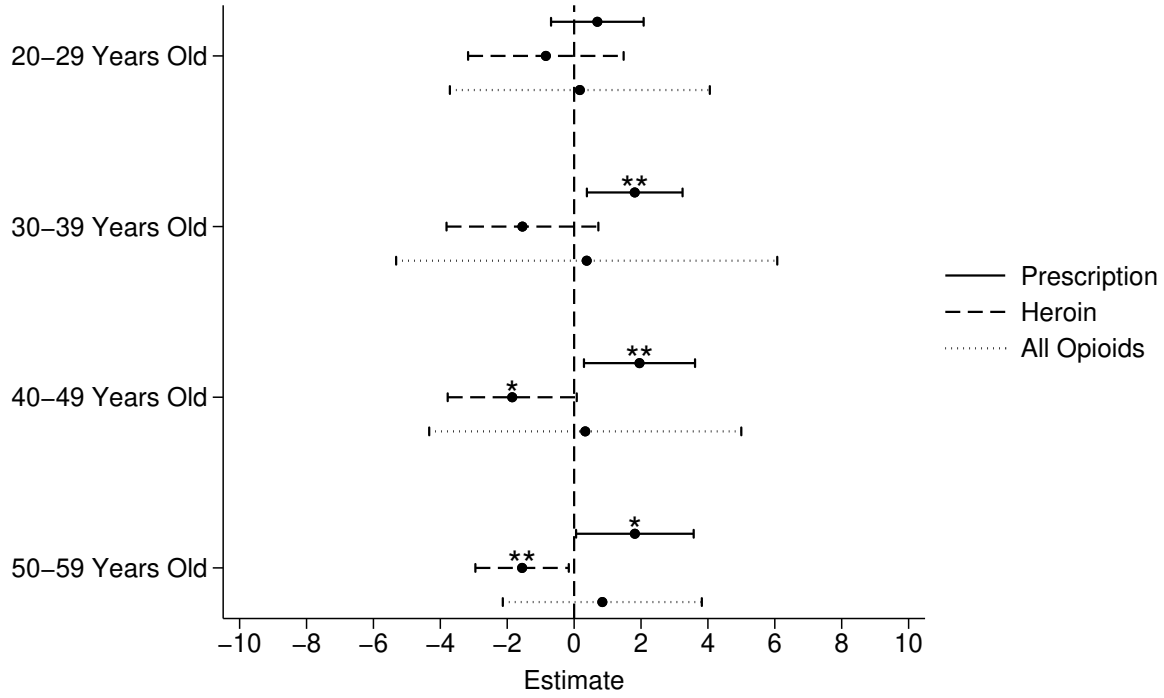
Notes: This map visualizes the counties which contribute to our regression samples. States shaded white are states where local prosecutors are either appointed or elected in non-partisan elections. States shaded light gray are states where local prosecutors are elected in partisan elections, but we do 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



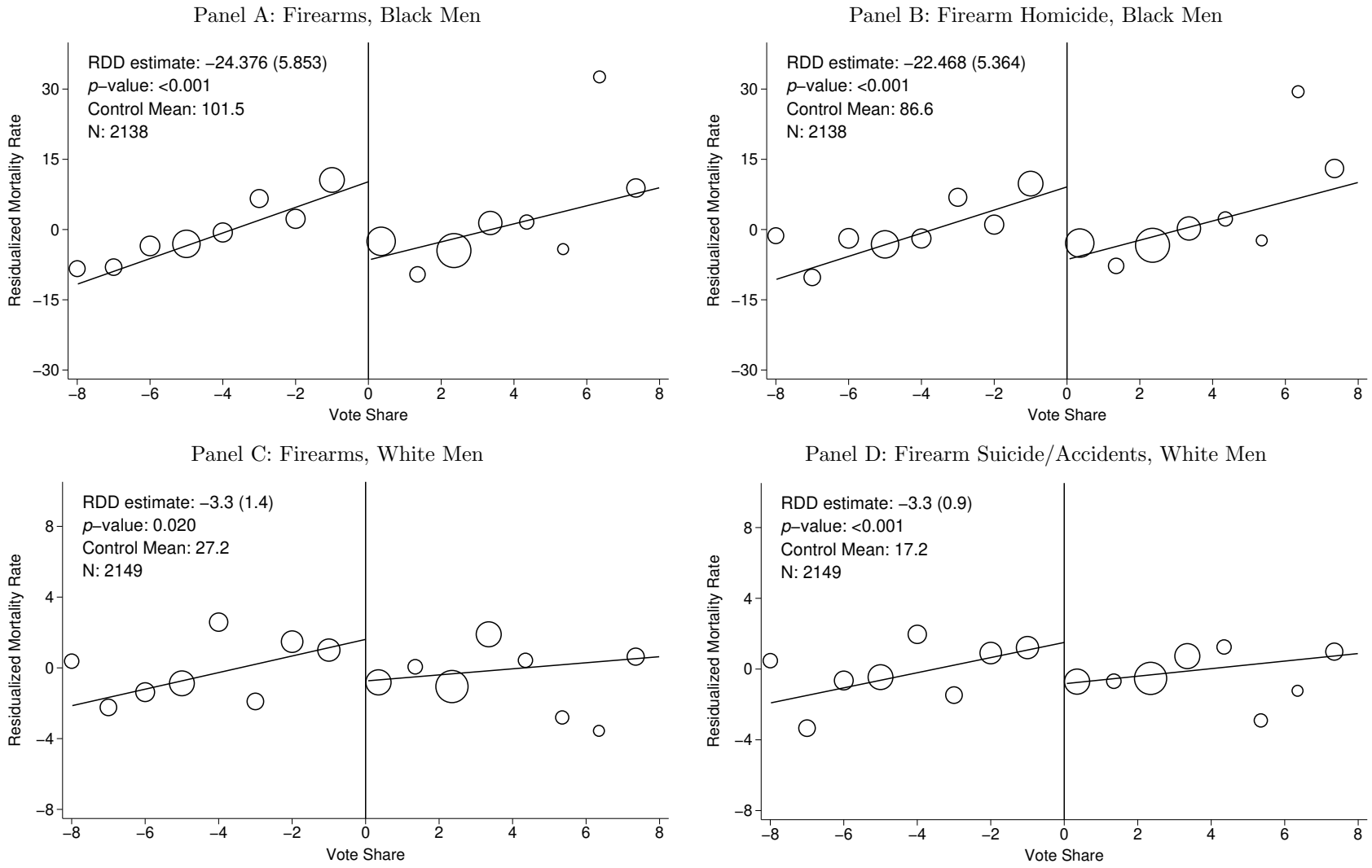
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality rates among men ages 20-29 years old. The information in the corner of each panel comes from estimation of Equation 1, and each plot visualizes RDD estimates obtained by first residualizing the mortality rates of the covariates from Equation 1. Circles represent average residualized mortality rates within one percentage point wide bins on each side of the cutoff, and circle size is proportional to the total population across all observations in each bin. The thick solid lines show local linear fits, and the gap between them at the cutoff equals the RDD point estimate. All other RDD figures in this paper use the same methodology.

Figure 3: Effects of Narrow Republican Prosecutor Election on Opioid Mortality



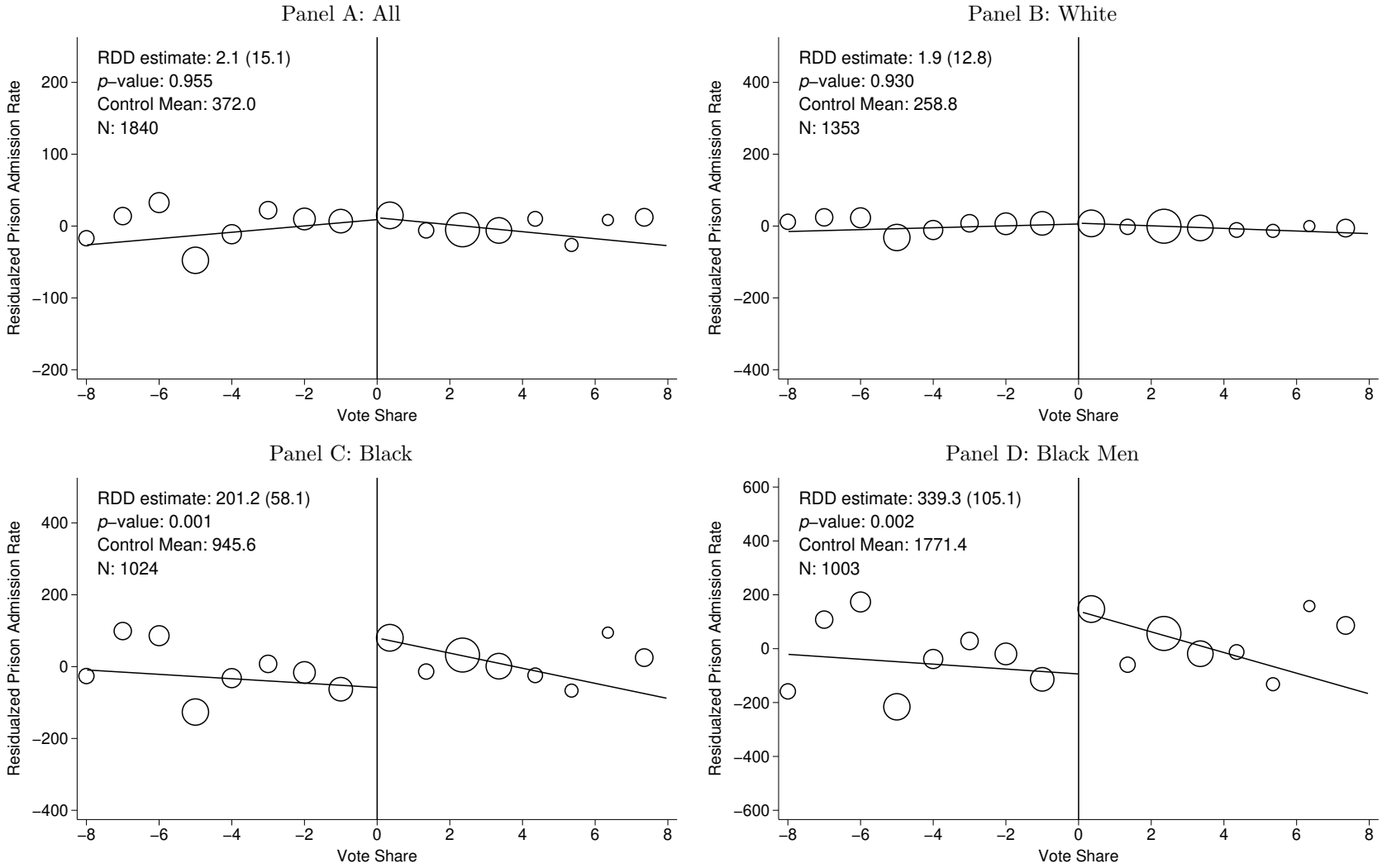
Notes: This coefficient plot conveys RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality due to opioid overdoses across ten year age groups. Each coefficient and corresponding conventional 95% confidence comes from a separate regression estimated using our preferred 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 Men 20-29 Year Olds, By Race



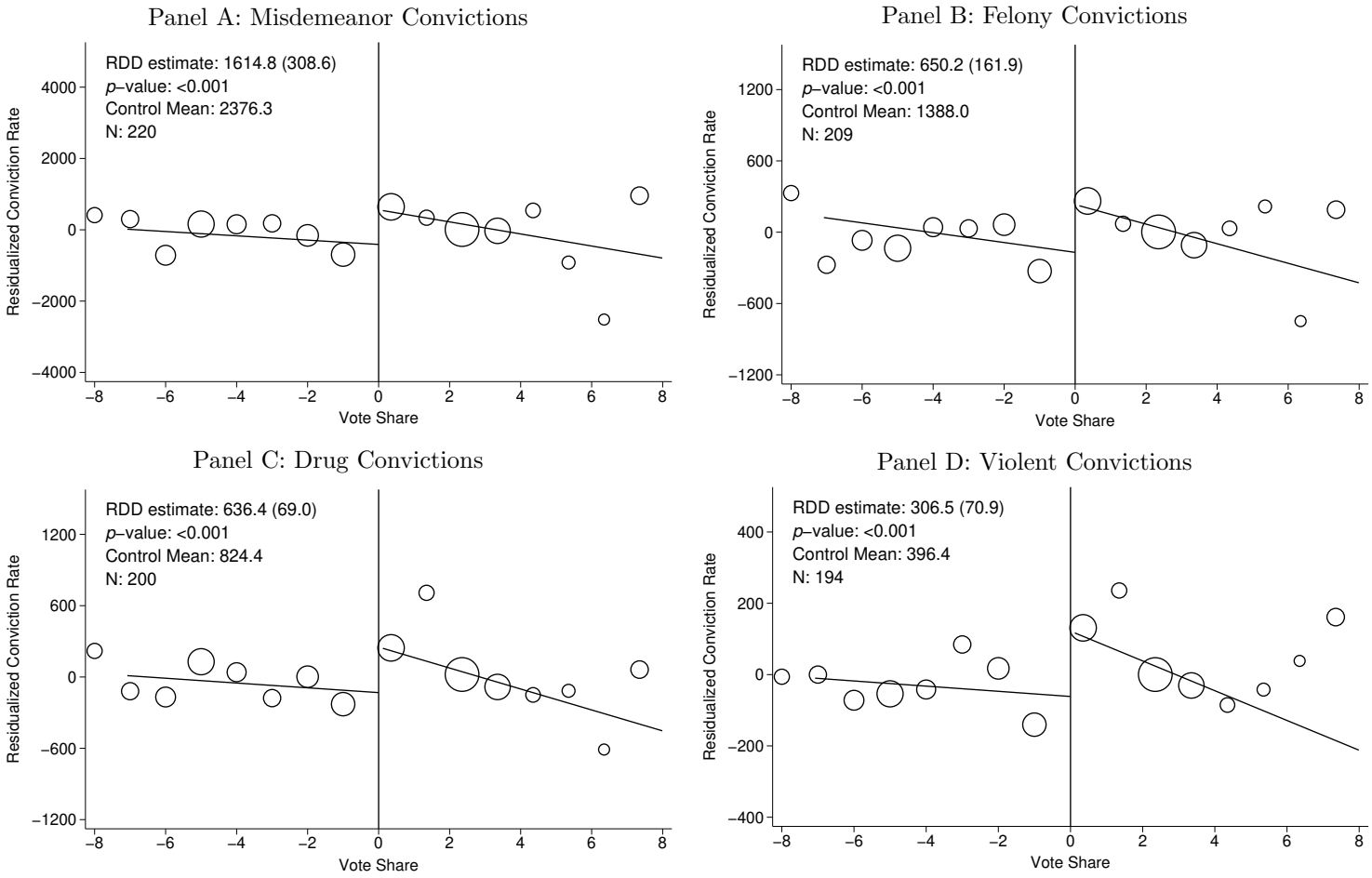
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality rates among Black men ages 20-29 years old. The information in the corner of each panel comes from estimation of Equation 1 and each plot visualizes RDD estimates obtained by first residualizing the mortality rates of the covariates from Equation 1. See Figure 2 for more information on figure construction.

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 annual county-level rates of prison admissions per 100,000 residents ages 15 to 64. 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. See Figure 2 for more information on figure construction.

Figure 6: Effects of Narrow Republican Prosecutor Election on Criminal Conviction Rates



Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on on annual county-level rates of criminal convictions issued per 100,000 residents ages 25 to 40. 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. See Figure 2 for more information on figure construction.

Appendix A Main Appendix Tables and Figures

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) contribute at least one county-by-year observation to our estimating sample, 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 information on the data coverage of the Vera Institute's Justice Incarceration Trends (VIJIT) database across our estimating sample. Our estimating sample consists of all county-by-year observations where the Republican vote share from the most recent prosecutor election fell between 42% and 58%. Each cell provides information on data coverage for the variable listed at the column header for the demographic group given by the row label. Numbers from columns titled "% Observations Covered" give the fraction of county-by-year observations in our estimating sample for which the given variable is available in the VIJIT data. Numbers from columns titled "% Population Covered" give the the share of the total estimating sample population contained in counties available in the VIJIT data. Blank cells denote that the corresponding variable is unavailable for the given demographic group. All variables are measured among individuals ages 15 to 64.

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
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
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 information on the data coverage of the Vera Institute's Justice Incarceration Trends (VIJIT) database across our estimating sample. Our estimating sample consists of all county-by-year observations where the Republican vote share from the most recent prosecutor election fell between 42% and 58%. Each cell provides information on the fraction of county-by-year observations in our estimating sample for which the given variable is available in the VIJIT data for the state given by the row label. Blank cells denote that the corresponding variable is unavailable for the given state. All variables are measured among individuals ages 15 to 64.

Table A5: Criminal Conviction Rate Data Availability

	Misdemeanor Convictions		Felony Convictions	
	% Observations Covered	% Population Covered	% Observations Covered	% Population Covered
Total	10.9	42.1	10.7	40.7
Black	10.9	42.3	10.7	41.7
White	10.9	37.2	10.7	35.9

Notes: This table presents information on the data coverage of the CJARS JOE database across our estimating sample. Our estimating sample consists of all county-by-year observations where the Republican vote share from the most recent prosecutor election fell between 42% and 58%. Each cell provides information on data coverage for the variable listed at the column header for the demographic group given by the row label. Numbers from columns titled "% Observations Covered" give the fraction of county-by-year observations in our estimating sample for which the given variable is available in the JOE data. Numbers from columns titled "% Population Covered" give the the share of the total estimating sample population contained in counties available in the JOE data. Blank cells denote that the corresponding variable is unavailable for the given demographic group. All variables are measured among individuals ages 25 to 40.

Table A6: Criminal Conviction Rate Data Availability, By State

	Misdemeanor Convictions	Felony Convictions
AL	-	-
AZ	62.5	43.8
CO	-	-
FL	24.0	24.0
GA	-	-
ID	-	-
IL	-	-
IN	-	-
KY	-	-
LA	-	-
ME	-	-
MD	51.7	51.7
MA	-	-
MI	60.7	60.7
MS	-	-
MO	-	-
NV	-	-
NH	-	-
NM	-	-
NY	-	-
NC	38.1	38.1
OH	-	-
OK	1.3	1.3
PA	66.7	66.7
SC	6.3	6.3
SD	-	-
TN	-	-
TX	28.6	28.6
VT	-	-
VA	27.6	25.5
WA	-	-
WV	-	-
WI	21.4	21.4
WY	-	-

Notes: This table presents information on the data coverage of the CJARS JOE database across our estimating sample. Our estimating sample consists of all county-by-year observations where the Republican vote share from the most recent prosecutor election fell between 42% and 58%. Each cell provides information on the fraction of county-by-year observations in our estimating sample for which the given variable is available in the JOE data for the state given by the row label. Blank cells denote that the corresponding variable is unavailable for the given state. All variables are measured among individuals ages 25 to 40.

Table A7: Power Calculations For Detecting Effects of Narrow Republican Prosecutor Election on Criminal Conviction Rates

	(1) Misdemeanor Convictions	(2) Felony Convictions	(3) Drug Convictions	(4) Violent Convictions
Minimum Detectable Effect	755.7	464.7	162.1	194.4
% of Control Mean	.318	.335	.197	.49

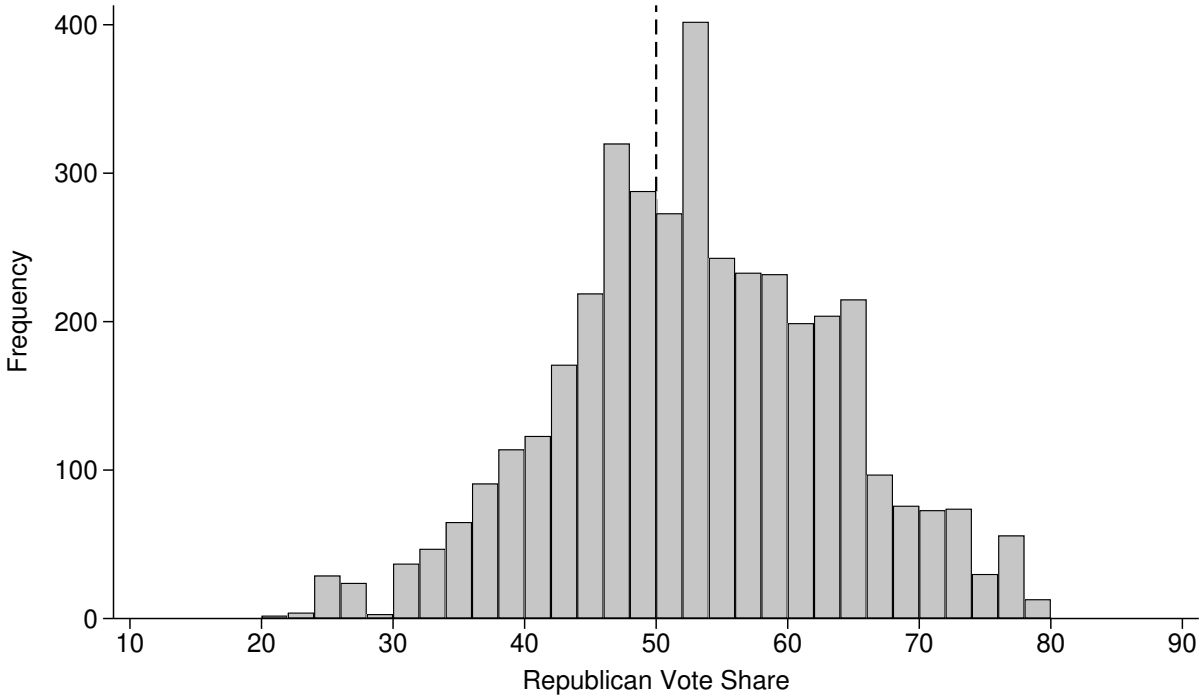
Notes: This table presents power calculations related to RDD estimation of the effect of narrow election of a Republican local prosecutor on county-by-year criminal conviction rates. The criminal conviction data is described in Section I.5. The table shows minimum detectable effects and the size of these effects relative to outcome variable means. We calculate the outcome means using the intercept to the left of the cutoff from an RDD regression without covariates. We use the power calculation method laid out in Cattaneo et al. (2019b) and obtain these estimates using the baseline specification choices laid out in Section II. We use a power of 0.8 in these calculations.

Table A8: 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
	-5.91***	1.39	-4.43	-4.97
	(2.17)	(3.84)	(6.30)	(9.08)
p-val	0.005	0.845	0.513	0.567
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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

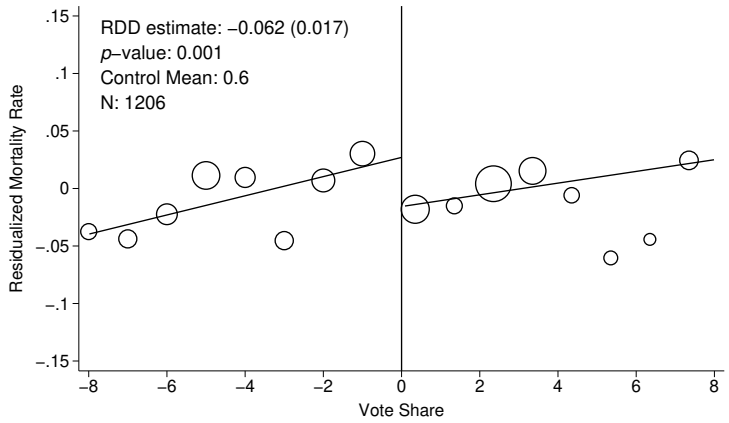
Figure A1: Distribution of Republican Vote Share, 2010 to 2019



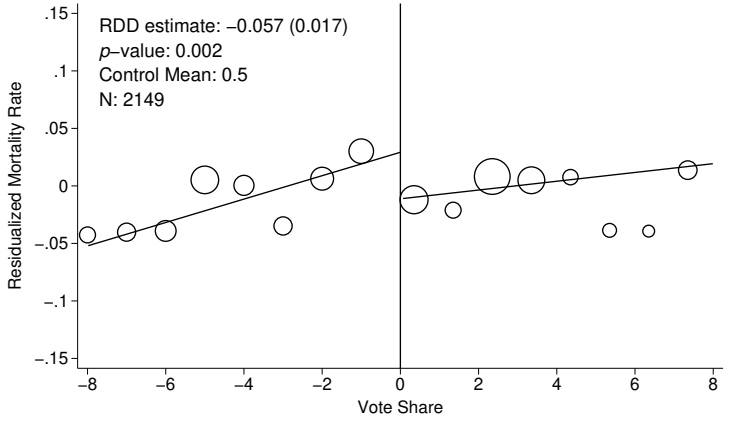
Notes: This figure visualizes the distribution of the Republican vote share variable in our sample. Bins are two percentage points wide.

Figure A2: Effects of Narrow Republican Prosecutor Election on The Fraction of All Suicides Committed Using A Firearm

Panel A: Drop Observations with Zero Suicides

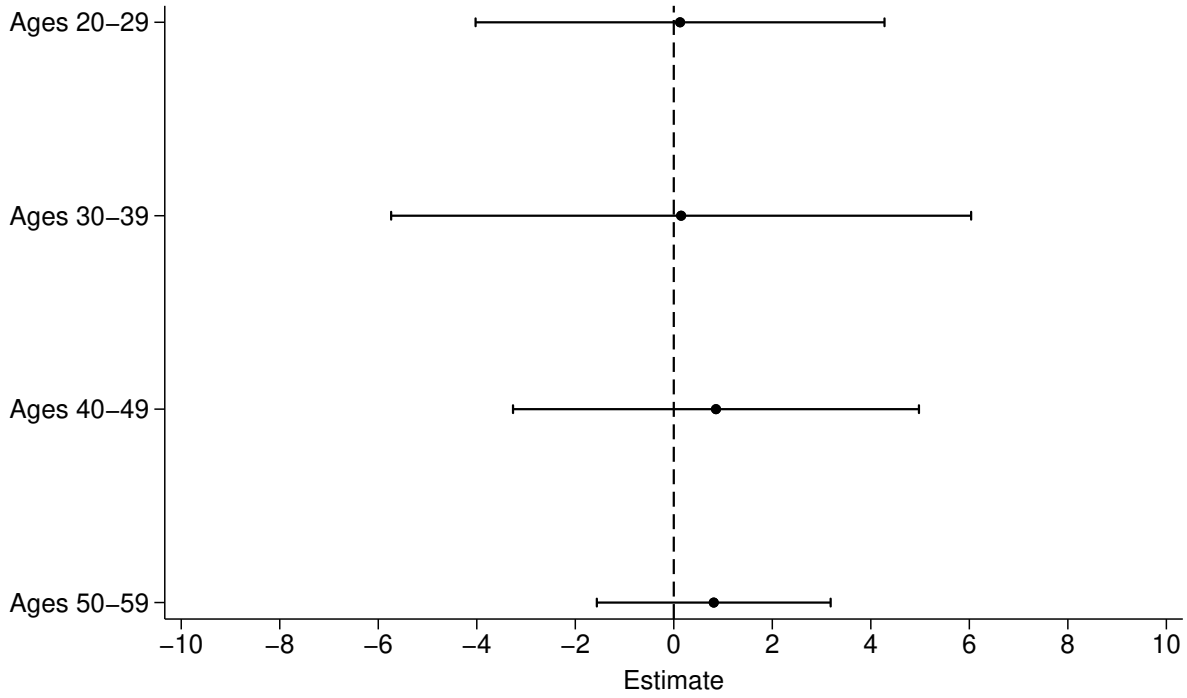


Panel B: Code Observations with Zero Suicides as Zero



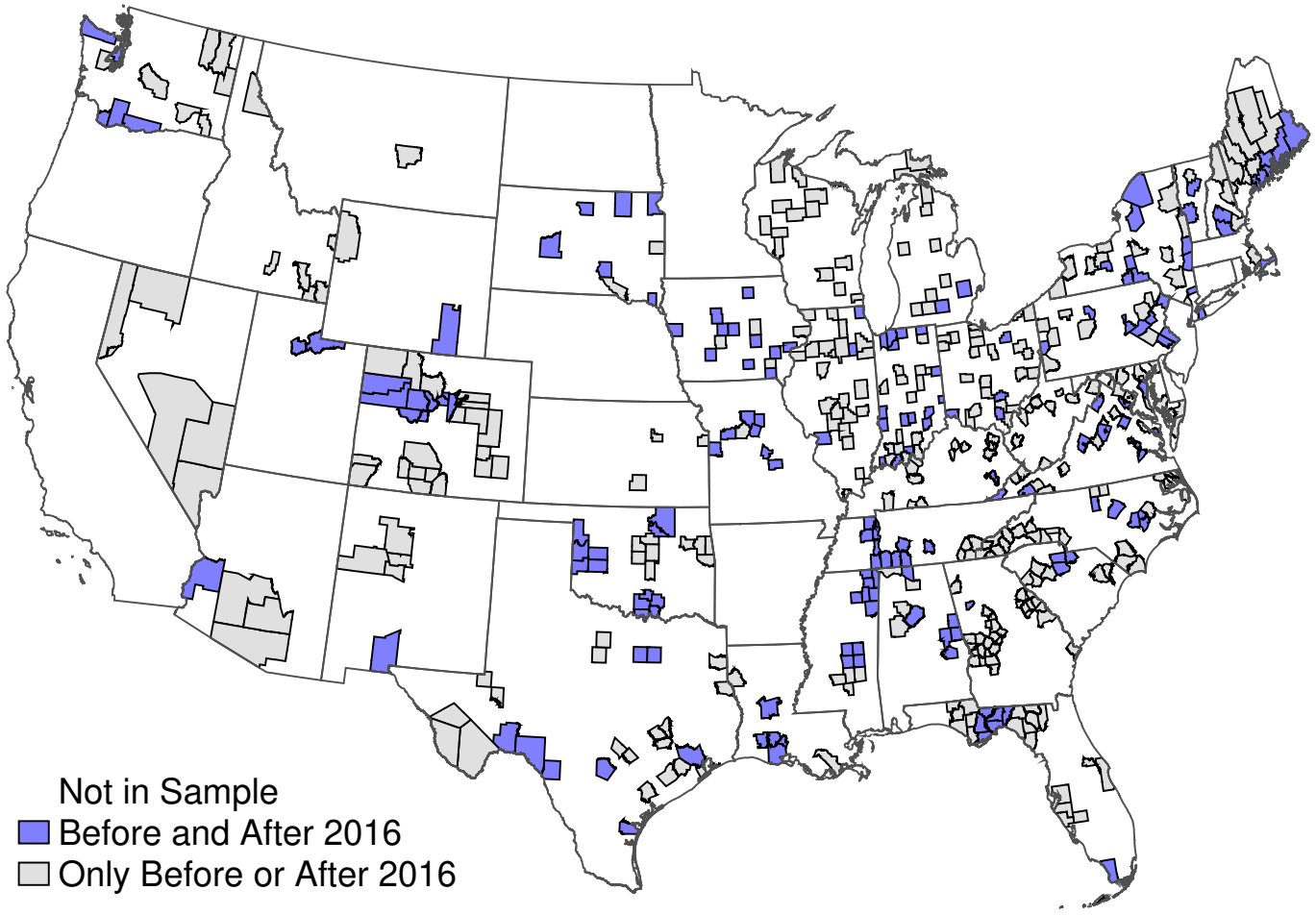
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on the fraction of suicides committed that involve a firearm. Panel A drops county-by-year observations with zero suicides, and Panel B includes these observations as zero values. 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. See Figure 2 for more information on figure construction.

Figure A3: Effects of Narrow Republican Prosecutor Election on Fentanyl Overdose Mortality



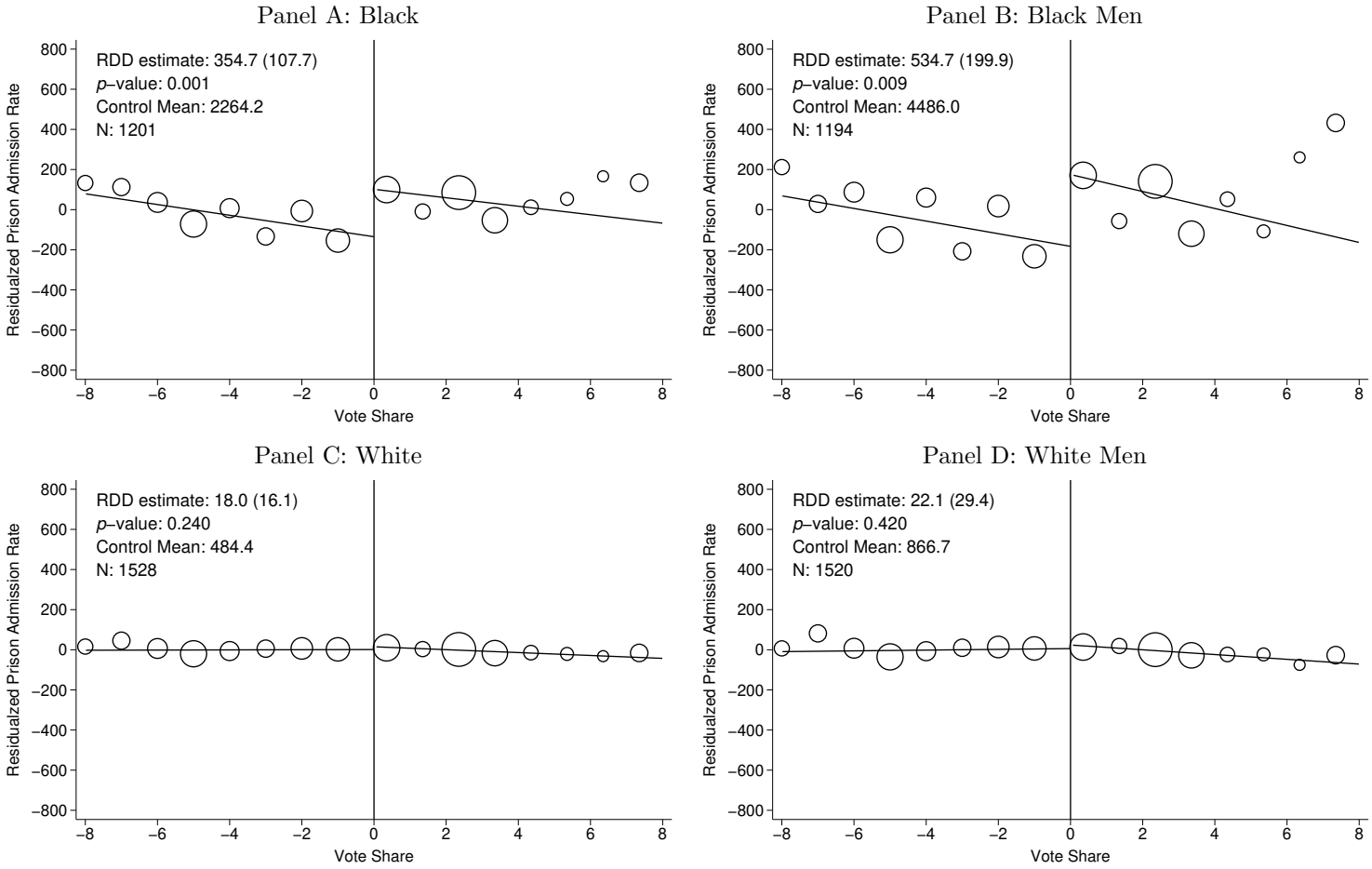
Notes: This coefficient plot conveys RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality due to fentanyl overdoses across ten year age groups. Each coefficient and corresponding conventional 95% confidence comes from a separate regression estimated using our preferred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

Figure A4: Counties That Contribute Observations to the Analysis Before and After 2016



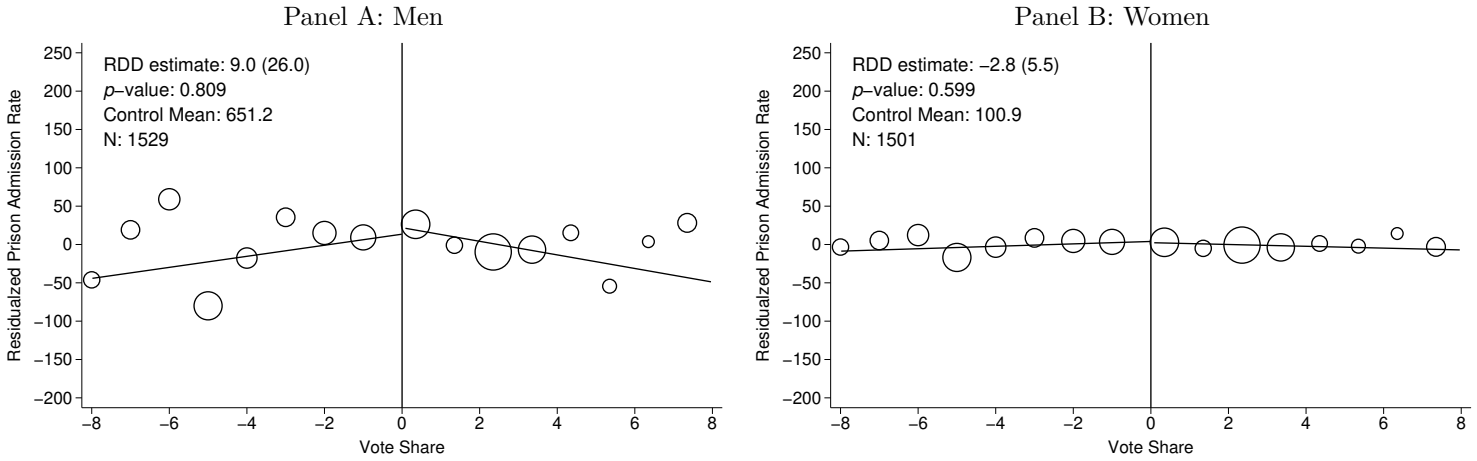
Notes: This map visualizes the counties which contribute to our regression samples and the years in which they contribute data. Counties shaded white are ones that are not used at all in the estimation. Counties shaded gray are ones that contribute observations to our estimation either before 2016 or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

Figure A5: Effects of Narrow Republican Prosecutor Election on Prison Population Rates



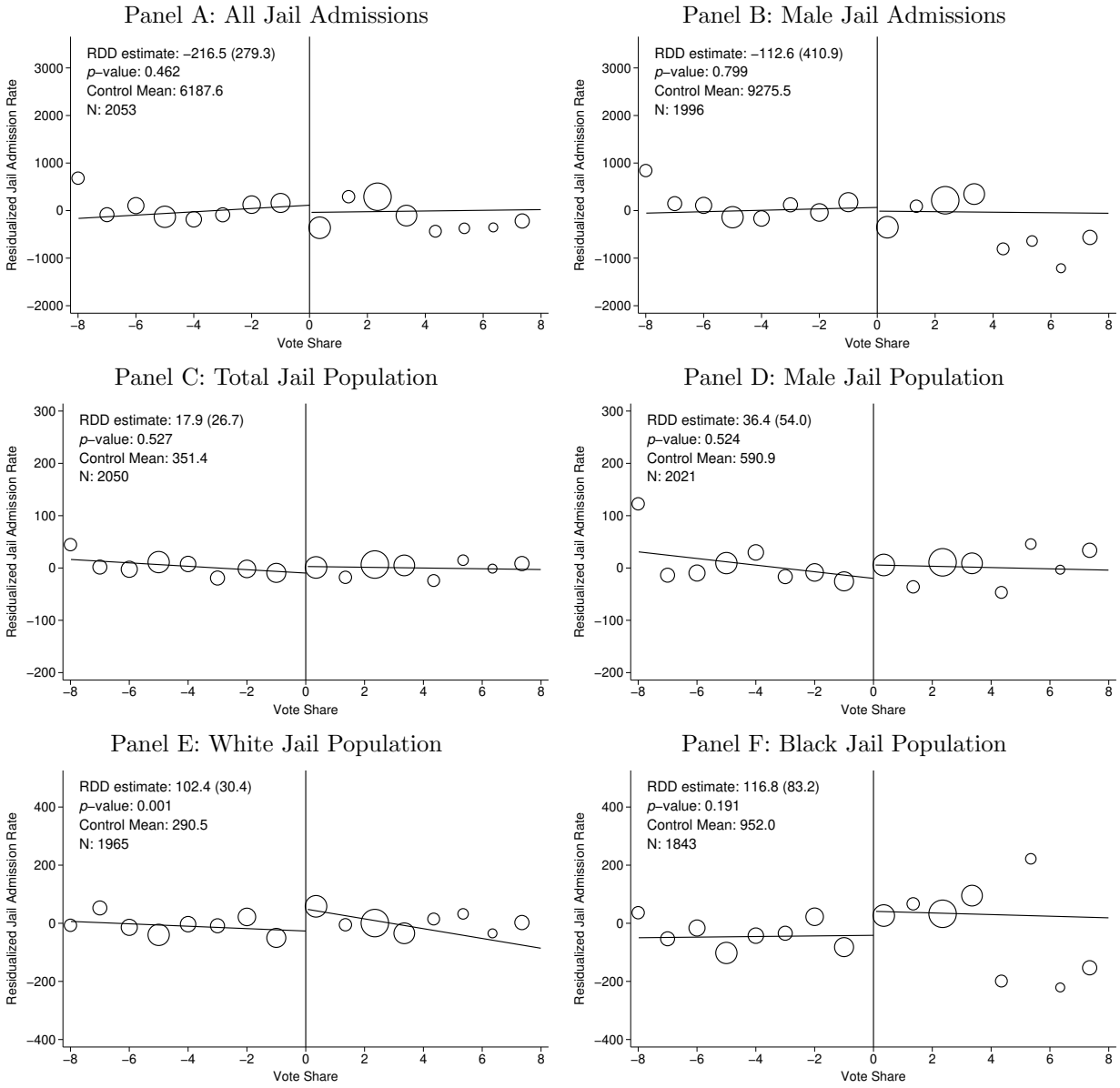
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on prison population rates among different demographic groups. The information in the corner of each panel comes from estimation of Equation 1 and each plot visualizes RDD estimates obtained by first residualizing the mortality rates of the covariates from Equation 1 as well as a county's average prison population rate among everyone ages 15 to 64 from 2004 to 2006.

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



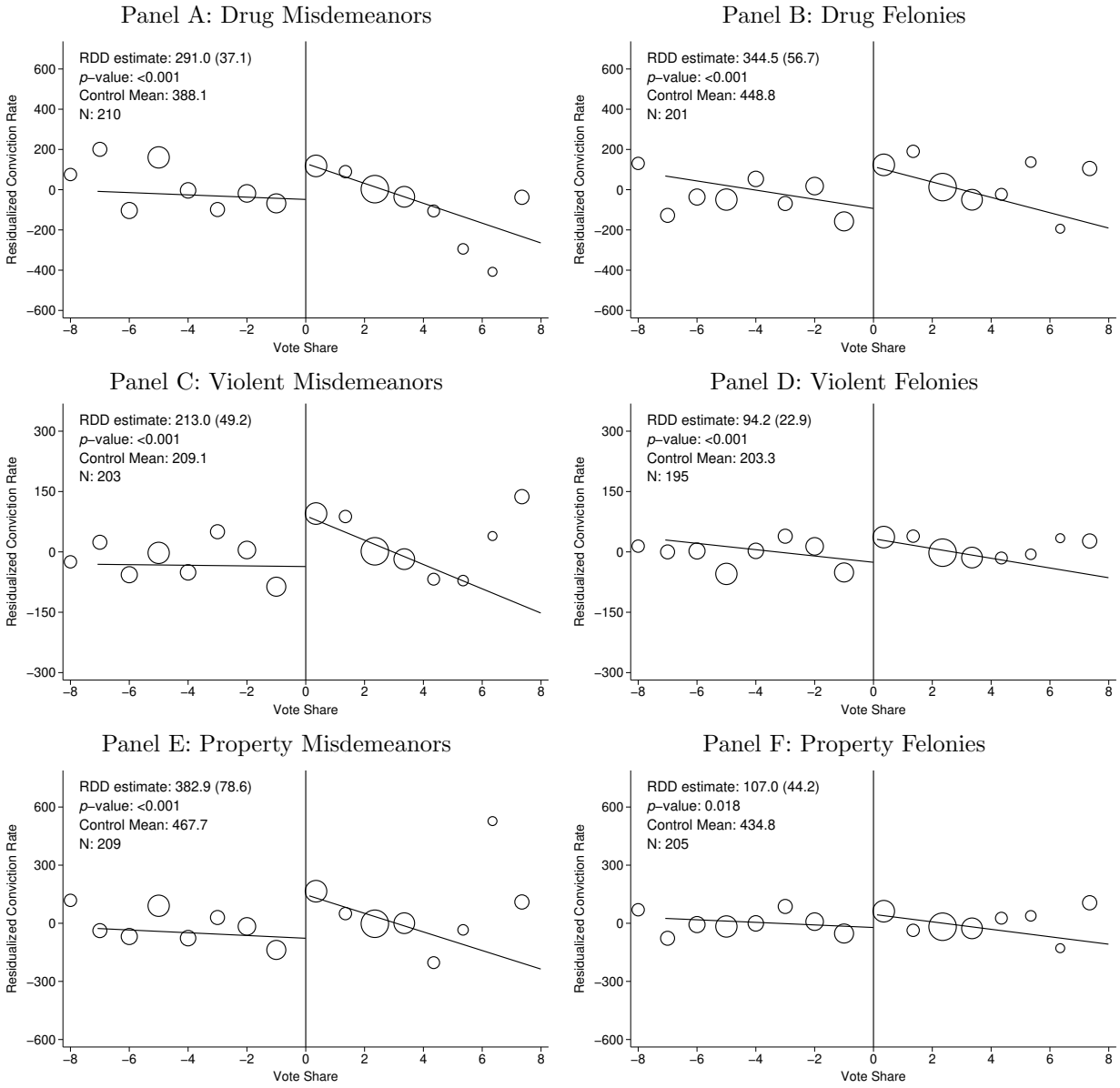
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on prison admission rates among different demographic groups. The information in the corner of each panel comes from estimation of Equation 1 and each plot visualizes RDD estimates obtained by first residualizing the outcome of the covariates from Equation 1.

Figure A7: Effect of Narrow Republican Prosecutor Election on Jail Admission Rate



Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on jail admission and population 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 mortality rates of the covariates from Equation 1.

Figure A8: Effects of Narrow Republican Prosecutor Election on Criminal Conviction Rates



Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on on annual county-level rates of criminal convictions issued per 100,000 residents ages 25 to 40. 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. See Figure 2 for more information on figure construction.

Appendix B Mortality Data and Trends

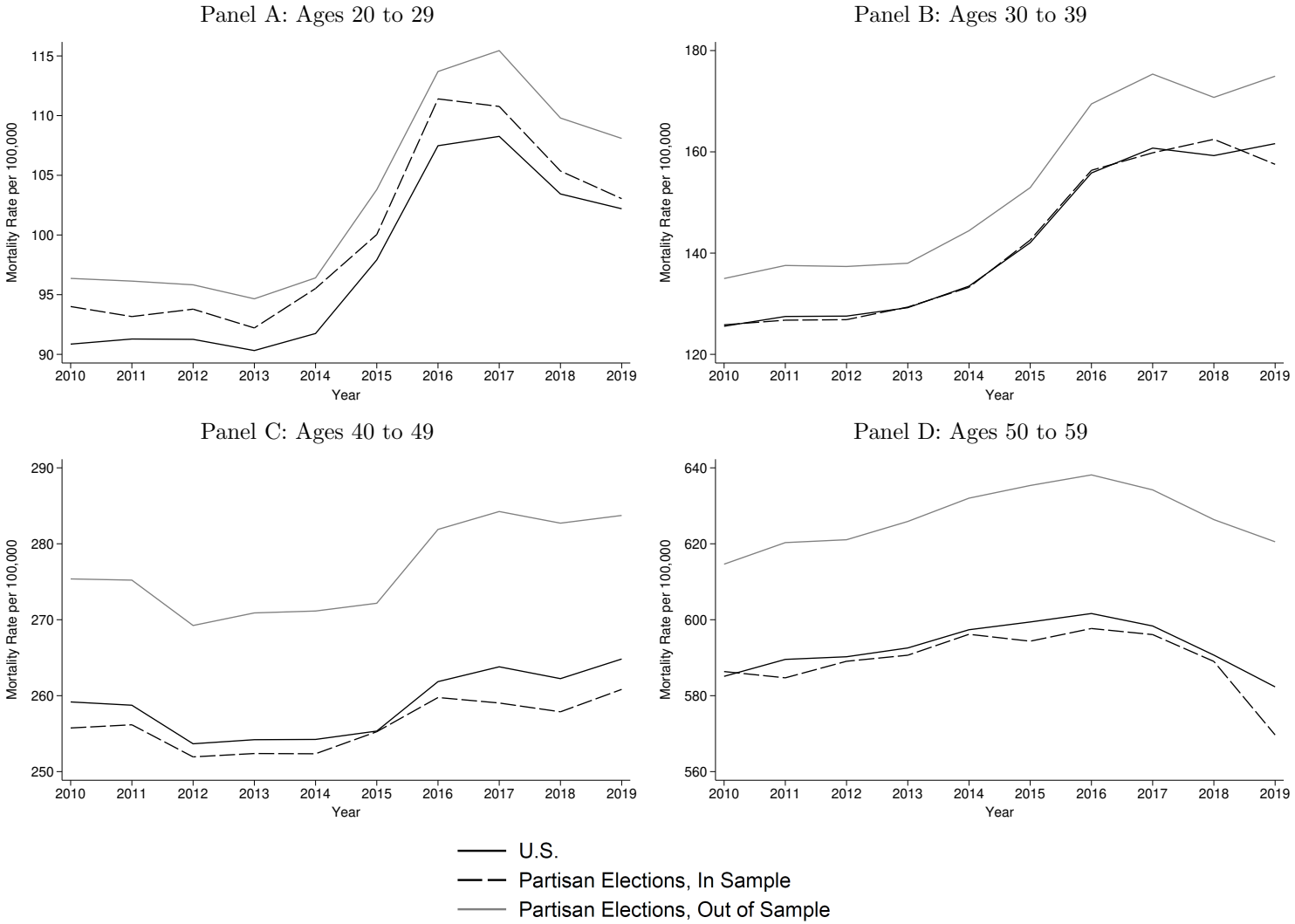
The list below gives the ICD-10 codes used to identify each cause of death in our analysis.

- External Causes: U00-Y89, F10-F19
- Unknown Causes: X59, R96-R99
- Internal Causes (Diseases/Conditions): all codes not listed for External or Unknown Causes
- Homicides: X85-Y09, Y35, Y87.1
- Drug Overdoses: F10-F19, 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, X82, Y85
- Non-Drug/Non-Vehicle Suicides/Accidents: all codes listed for External Causes but not for Homicides, Drug Overdoses, or Motor Vehicle Accidents
- Firearms: W23-W34, X72-74, X93-95, Y22-24, Y35.0
- Firearm Homicide: X93-95, Y35.0
- Firearm Suicides/Accidents: W23-W34, X72-74, Y22-24
- Opioid overdose: T40.1-T40.4,T40.6
- Heroin overdose: T40.1
- Natural and semi-synthetic opioid overdose: T40.2
- Fentanyl overdose: T40.4

Figures B1 and B2 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 B3 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.[32] Figure B3 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.

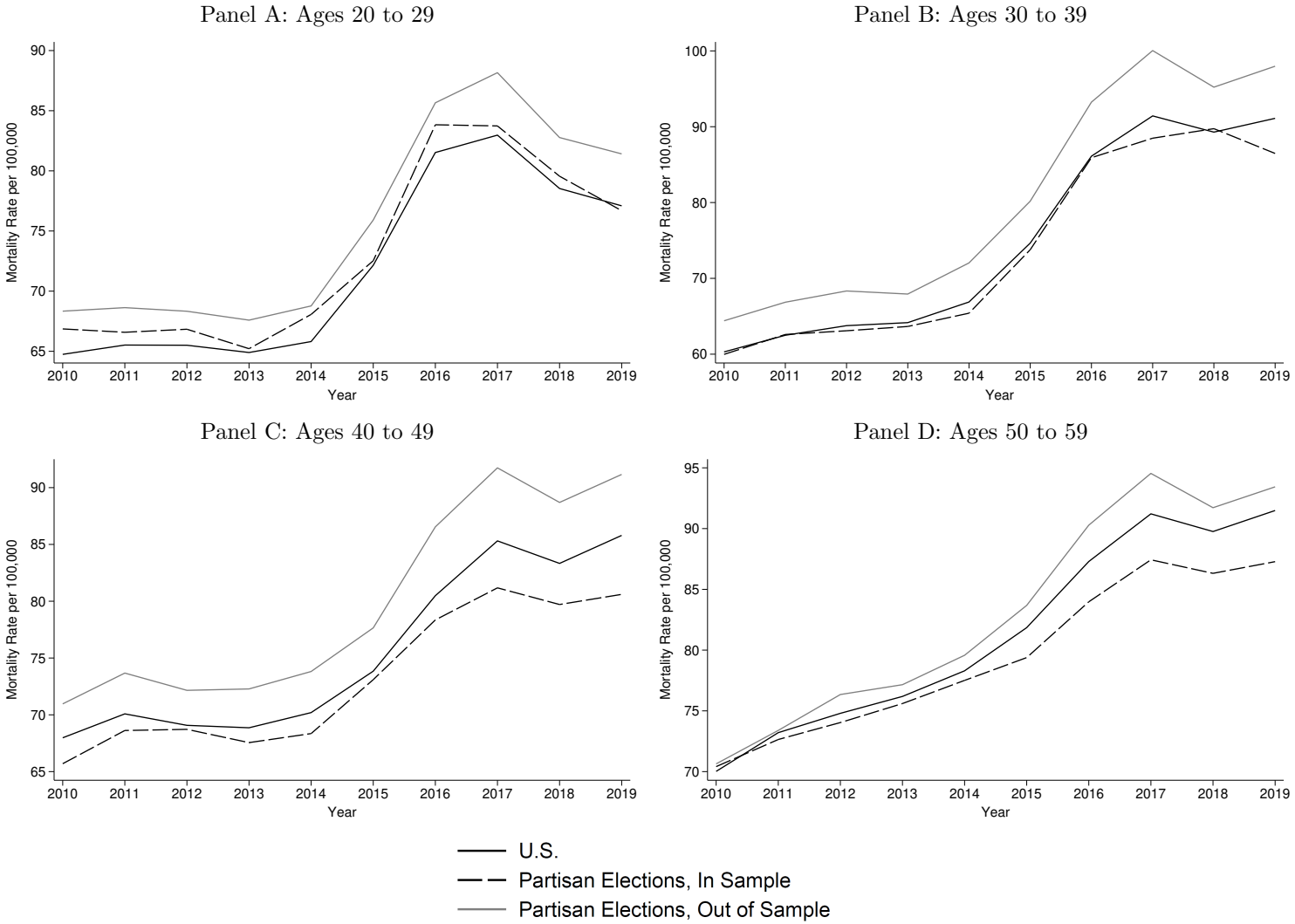
[32]More specifically, the opioid crisis is known to be concentrated among white middle-aged Americans without a college degree (Case and Deaton, 2015, 2017). However, the most recent trends in opioid mortality are different—the crisis now spreading to younger and non-white Americans (Gibbons et al.,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

Figure B1: Trends in Mortality from All Causes



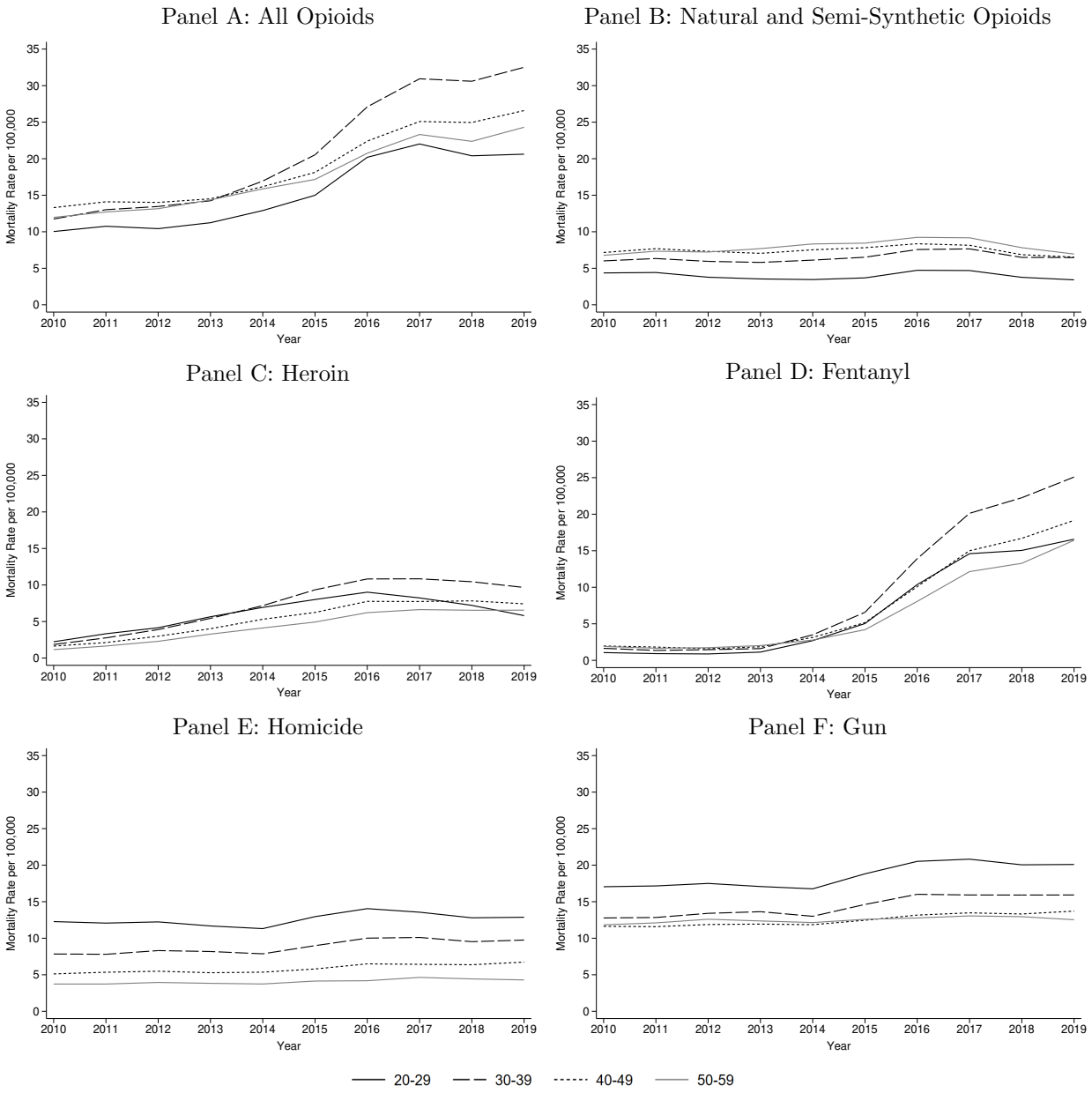
Notes: These figures show trends in all-cause mortality from 2010 to 2019. Each panel focuses on a different age group. Separate trend lines are shown for the U.S. as a whole (black solid line), for counties where the Republican vote share from the most recent prosecutor election fell between 42% and 58% in at least one year between 2010 to 2019 (black dashed line), and for counties in states that have partisan prosecutor elections but did not have a closely contested vote share value during our sample period (gray solid line).

Figure B2: Trends in Mortality from External Causes



Notes: These figures show trends in mortality from external causes from 2010 to 2019. See Section I.2 and the Appendix for more information on cause of death classification. Each panel focuses on a different age group. Separate trend lines are shown for the U.S. as a whole (black solid line), for counties 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 (black dashed line), and for counties in states that have partisan prosecutor elections but did not have a closely contested vote share value during our sample period (gray solid line).

Figure B3: Trends in Mortality, By Cause



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

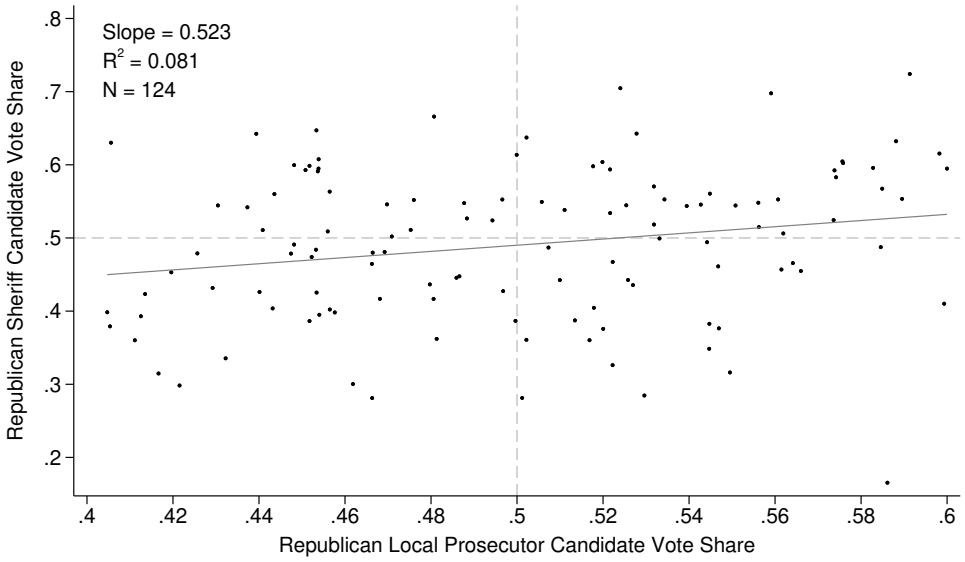
Appendix C Relationship Between Local Prosecutor and Sheriff Political Affiliation

We conduct two simple empirical exercises to test whether our estimates are contaminated by the effect of also 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 C1. 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. 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 C1.[33] The estimate is small and statistically insignificant, conveying that the party affiliation of sheriffs is not confounding our estimates.

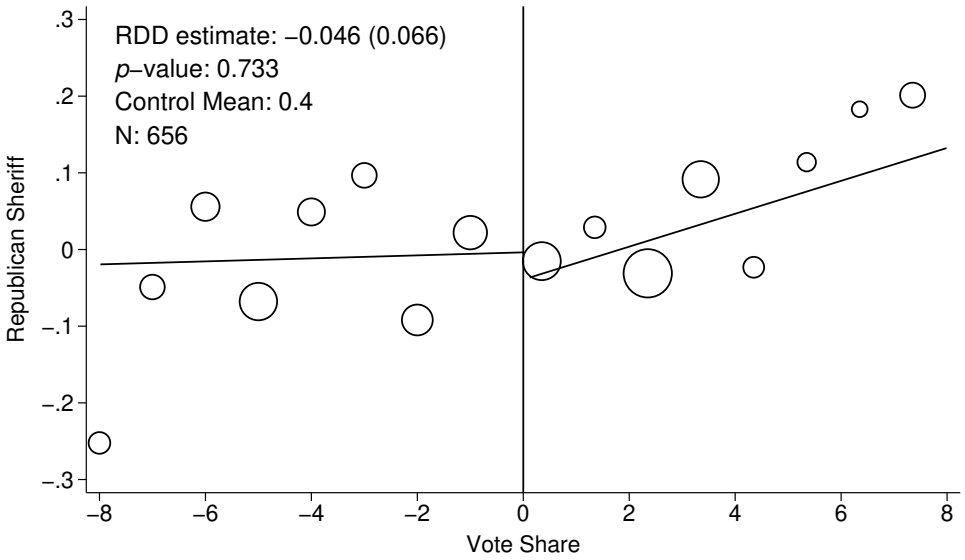
[33]Here we use our prosecutor election data and merge in sheriff data from ALGE.

Figure C1: Association Between Local Prosecutor and Sheriff Political Affiliation

Panel A: Correlation



Panel B: Republican Sheriff Balance



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

Appendix D Robustness and Placebo Exercises

In this section, we probe the credibility of our mortality results with several robustness and placebo tests. We focus on two causes death central to our analysis: all external causes and firearm deaths. Tables D1 and D2 present estimated effects on male mortality by age group under our preferred specification and several alternate specifications. Our preferred specification—which includes state and year fixed effects, baseline county-level demographics and mortality rates, and uses population weights —is reported in Column 6. Column 1 reports estimates without covariates, and Columns 2 and 3 sequentially add state and year fixed effects. Relative to Column 6, the incremental addition of covariates improves the precision of the estimates for 20-29 year olds but does not substantially affect their magnitude; in contrast, the estimates for other age groups remain statistically insignificant or attenuate further.

Column 4 of Tables D1 and D2 includes the full set of covariates but omits population weights. Consistent with theory (Solon et al., 2015), population weights improve precision , and it is reassuring that the estimated effect for young men remains relatively similar in magnitude without the weights. Finally, Column 5 uses our preferred specification but excludes the four counties given the most weight in our analysis due to the population weights and triangular kernel weighting function. As shown in Figure D1, these counties receive substantial weight both because of their population size and because their vote shares 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 D2 examines the sensitivity of our key findings among young men to alternate bandwidth choices. Our baseline estimates use 8 p.p. bandwidths on each side of the cutoff, and Figure D2 shows how the estimates change when these bandwidths are changed one-half of a p.p. at a time from to 4 to 12 p.p. Reassuringly, our findings on external cause and firearm mortality are not especially sensitive to deviations from our baseline bandwidth

choice.

We now turn to placebo checks. Our research design relies on the assumption that post-election mortality rates in counties which narrowly fail to elect a Republican prosecutor are a reasonable counterfactual for post-election rates in counties that narrowly do elect one. One natural placebo test exploits the timing of these closely contested elections. 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 focal election (and thus before it should conceivably matter), then estimate the vote share RDD using these pre-election years.

Table D3 reports the results. 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.

Another natural placebo check examines health outcomes that local prosecutors should not plausibly affect. Table D4 reports estimates for mortality from internal causes (i.e., diseases and medical conditions) by age group and sex. Seven of the eight coefficients—including all four for men—are statistically indistinguishable from zero. These results provide further reassurance that our empirical strategy is not capturing broad changes in health or mortality, but instead isolates effects specific to causes plausibly linked to prosecutorial behavior.

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 D3. 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.

Table D1: Effects of Narrow Republican Prosecutor Election on External Cause Mortality Among Men

	(1)	(2)	(3)	(4)	(5)	(6)
			Ages 20-29 (Control Mean = 115.4)			
	-8.5	-8.9	-10.4**	-12.2	-12.7***	-10.1***
	(9.0)	(5.7)	(5.1)	(8.1)	(4.5)	(3.0)
p-val	0.253	0.105	0.027	0.170	0.008	0.001
N	2149	2149	2149	2149	2119	2149
			Ages 30-39 (Control Mean = 110.9)			
	-17.5	-9.3	-10.5*	-2.7	-0.5	-2.0
	(16.0)	(7.9)	(6.5)	(8.8)	(5.8)	(4.9)
p-val	0.238	0.264	0.099	0.751	0.813	0.591
N	2149	2149	2149	2149	2119	2149
			Ages 40-49 (Control Mean = 108.7)			
	-16.7*	-5.4	-6.1	3.7	5.3	0.6
	(10.9)	(5.1)	(4.7)	(7.1)	(3.7)	(3.2)
p-val	0.097	0.370	0.222	0.679	0.154	0.899
N	2149	2149	2149	2149	2119	2149
			Ages 50-59 (Control Mean = 117.7)			
	-11.8	-2.0	-1.7	3.8	5.2	6.3*
	(10.8)	(7.1)	(6.0)	(7.0)	(5.0)	(3.6)
p-val	0.204	0.869	0.853	0.408	0.296	0.079
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 D2: Effects of Narrow Republican Prosecutor Election on Firearm Mortality Among Men

	(1)	(2)	(3)	(4)	(5)	(6)
			Ages 20-29 (Control Mean = 38.3)			
	-2.6	-3.3	-3.8	-4.1	-8.7***	-6.7***
	(4.6)	(2.6)	(2.7)	(6.0)	(2.3)	(1.8)
p-val	0.453	0.248	0.170	0.438	0.001	0.001
N	2149	2149	2149	2149	2119	2149
			Ages 30-39 (Control Mean = 27.4)			
	-1.6	-1.1	-1.2	-6.9*	-2.1	-1.3
	(3.5)	(2.1)	(1.7)	(4.1)	(2.1)	(1.4)
p-val	0.541	0.650	0.486	0.088	0.261	0.274
N	2149	2149	2149	2149	2119	2149
			Ages 40-49 (Control Mean = 24.0)			
	-2.7	-1.0	-1.1	0.8	-0.2	-0.6
	(2.5)	(1.4)	(1.1)	(3.4)	(1.3)	(0.9)
p-val	0.238	0.601	0.474	0.904	0.912	0.633
N	2149	2149	2149	2149	2119	2149
			Ages 50-59 (Control Mean = 23.4)			
	-2.0	-0.5	-0.6	-1.4	1.5	1.5*
	(2.2)	(1.3)	(1.3)	(3.2)	(1.4)	(1.0)
p-val	0.261	0.797	0.757	0.701	0.242	0.098
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 firearm-related 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 D3: Placebo Exercise Using Pre-Election Mortality Rates

	(1)	(2)
	Real Estimate	Pre-Treatment Placebo Estimate
External	-10.130	0.210
	(3.770)	(3.390)
p-value	0.010	0.950
N	2149	1556
Firearms	-6.720	-1.640
	(2.450)	(1.540)
p-value	0.010	0.290
N	2149	1556

Notes: This table presents estimated obtained by performing the placebo exercise described in Section D. 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 D4: Effects of Narrow Republican Prosecutor Election on Mortality from Internal Causes

Panel A: Men

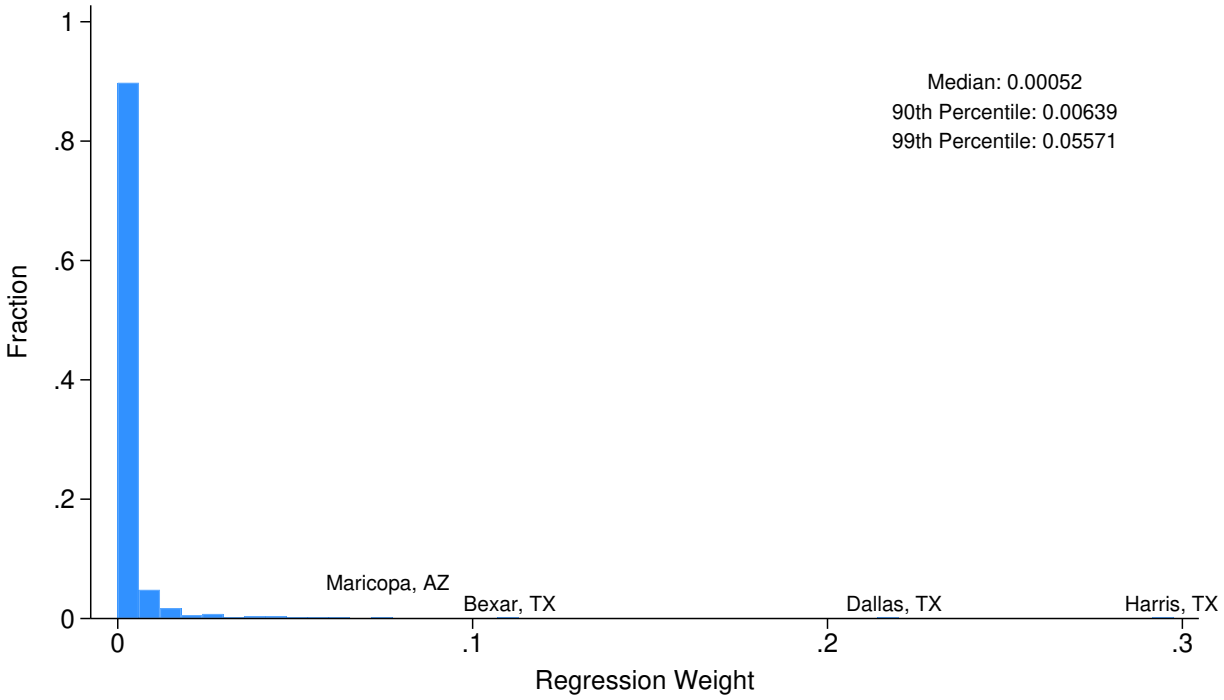
	(1)	(2)	(3)	(4)
	20-29 Year Olds	30-39 Year Olds	40-49 Year Olds	50-59 Year Olds
	0.4	-0.7	-6.3	-13.5
	(1.2)	(1.9)	(5.8)	(11.0)
p-value	0.591	0.744	0.278	0.242
Control Mean	29.7	79.6	232.0	690.0
N	2149	2149	2149	2149

Panel B: Women

	(1)	(2)	(3)	(4)
	20-29 Year Olds	30-39 Year Olds	40-49 Year Olds	50-59 Year Olds
	0.5	2.7**	-1.8	-10.7
	(0.7)	(1.3)	(5.4)	(8.3)
p-value	0.486	0.048	0.926	0.208
Control Mean	23.2	65.0	168.6	441.5
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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

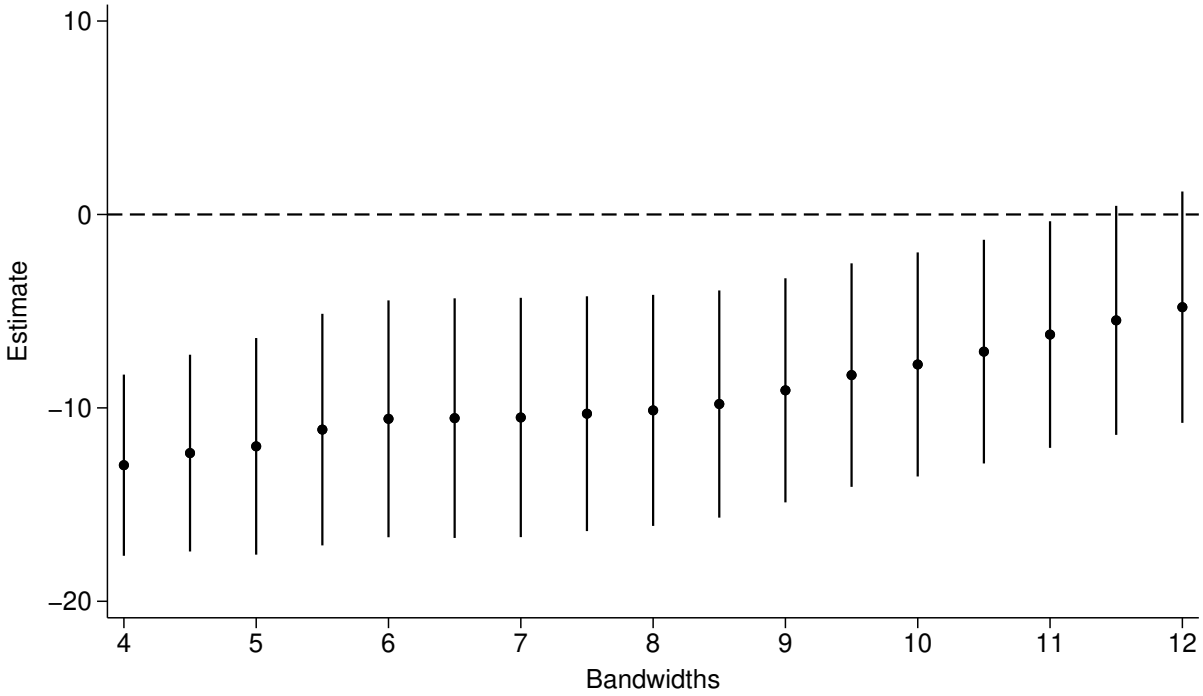
Figure D1: Distribution of Regression Weights Across Counties



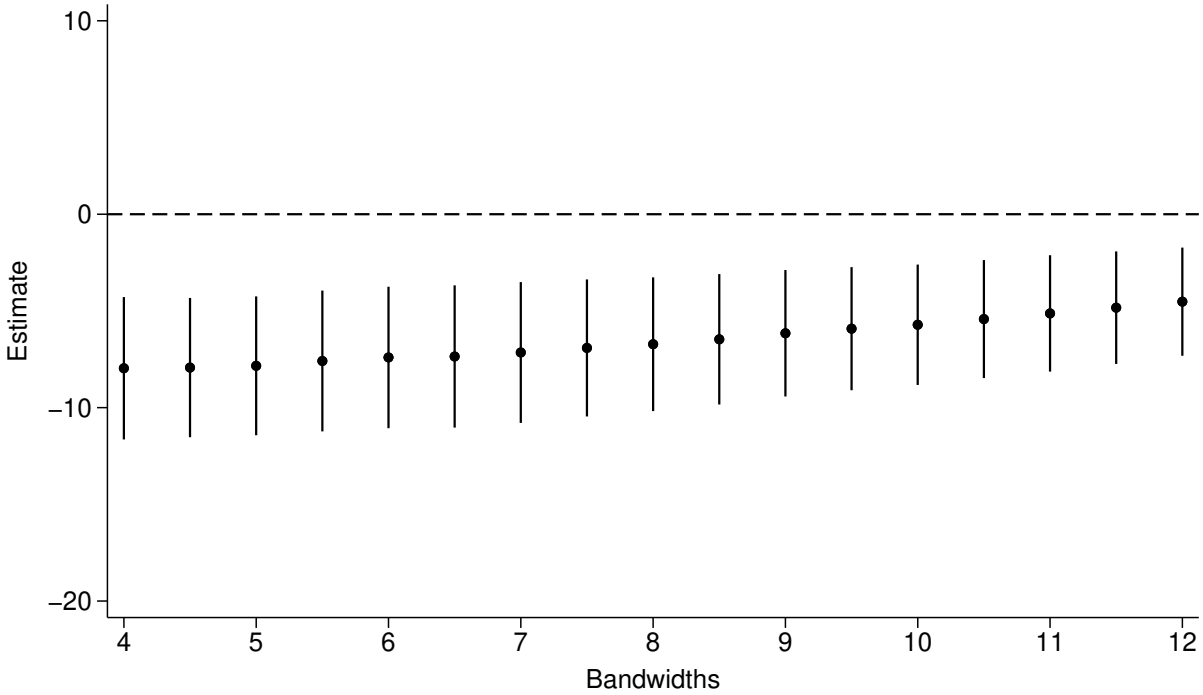
Notes: This figure plots the distributions of regression weights assigned to counties in our regression analysis. These weights correspond to the ones used estimate effects on all-cause mortality among 20-29 years olds. This figure aggregate weights across observations from the same county to the county-level.

Figure D2: Varying Bandwidths Used in Estimation

Panel A: Men Ages 20 to 29, External Causes



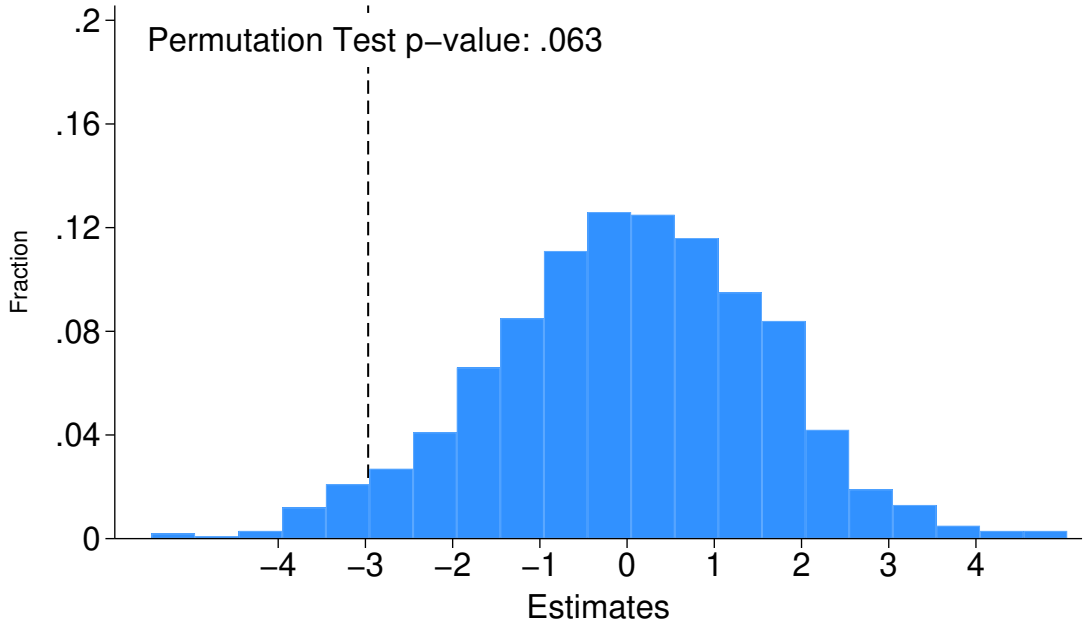
Panel B: Men Ages 20 to 29, Firearms



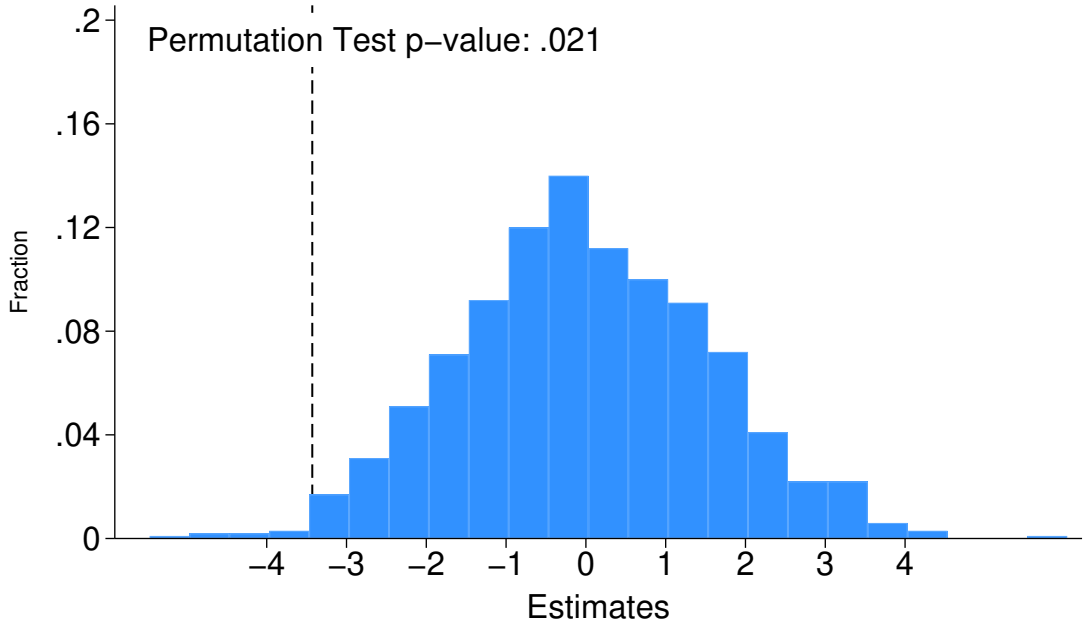
Notes: These figures plot the point estimates and conventional 95 percent confidence intervals from separate RDD regressions on the same outcome that vary the bandwidth by 1 percentage point at a time.

Figure D3: Distributions of Estimates Obtained by Randomly Permuting Vote Shares Across Districts

Panel A: Men Ages 20 to 29, External Causes



Panel B: Men Ages 20 to 29, Firearms



Notes: Each figure visualizes the distribution of test statistics obtained through a permutation test. This test begins with the collection of all districts that possess a Republican prosecutor vote share value between 42% and 58% in at least one year from 2010 to 2019. We then fix the yearly outcome and control variable values for counties in these districts but randomly permute the observed vote share values across districts 1,000 times and plot the distribution of test statistics produced by our preferred specification.

Appendix E Back of the Envelope Calculations

E.1 Incarceration and Mortality

In this section, we calculate whether the mortality reductions among young Black men could be driven by the direct effect of incarceration-based incapacitation on the incarcerated themselves. Panel D from Figure 5 shows increased annual prison admissions among Black men ages 15 to 64 of 339.3 admissions per 100,000. We next adjust this estimate for age differences. 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. This yields an estimate of $(339.3)(\frac{0.4}{0.2}) = 678.6$ additional annual prison admissions per 100,000 among young Black men.

This calculation next requires an assumption about the length of prison stays. The most generous approach assumes that each prison admission lasts several years and that mortality reductions accumulate over time. In this vein, consider a local prosecutor elected for a four year term, and that each prison admission included by a Republican prosecutor lasts at least four years. Here, admissions in year one generate four years of exposure, admissions in year two generate three years, etc.

Norris et al. (2022a) find that incarceration reduces the likelihood of death in a given year by 0.55 percentage points.[34] This would imply that the direct incapacitation effect should reduce cumulative mortality across all four years by $678.6(4 + 3 + 2 + 1)(0.0055) \approx 37.3$ deaths per 100,000. Table 5 shows an annual reduction in external cause mortality among young Black men of 30 fewer deaths per 100,000, implying a cumulative $(4)(30) = 120$ fewer deaths across this four year term. Thus, even under generous assumptions about prison stay length, direct incapacitation effects cannot explain our results.

Incarceration may also reduce mortality through indirect effects on the non-incarcerated. These channels include criminal deterrence spillovers to others in the incarcerated's close

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

networks, families, and neighborhoods (Bhuller et al., 2018; Billings and Schnepel, 2022; Norris et al., 2021). Additionally, individuals at risk of gun violence 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 may thus reduce both perpetrators' and potential victims' mortality risk. However, indirect effects would need to be twice as large as direct effects to explain remaining mortality reductions, which seems unlikely. Several aspects of this back-of-the-envelope calculation make the conclusions suggestive at best, but it suggests that incapacitation explains a substantial but incomplete share of the mortality decline.

E.2 Criminal Convictions, Firearm Restrictions, and Mortality

In this section, we estimate the number of additional firearm possession restrictions implied by our conviction results and their effect on firearm mortality. Felony convictions restrict legal firearm possession under the Gun Control Act of 1968 which prohibits anyone convicted of a crime "punishable by imprisonment for a term exceeding one year" from possessing a firearm. Exceptions to this rule (e.g., pardons and expungement) are rare enough to be immaterial and ignored in this analysis.

A felony conviction generates a new firearm restriction only if the individual lacks a prior felony conviction or other disqualifying circumstance. Olson and Stemen (2019) find approximately half of felony convictions involve first-time offenders, suggesting roughly half of our observed convictions generate new firearm restrictions. This share is very likely to be higher in our setting because marginal convictions under aggressive prosecutors likely involve defendants with less extensive criminal histories. We therefore conservatively assume that 60 percent of new convictions generate new restrictions.

Domestic violence misdemeanor convictions also restrict firearm possession. Nationally representative data are unavailable, but evidence from Washing State suggests 73 percent of

violent misdemeanor cases involve domestic violence charges (Schleimer et al., 2025). Our data show that about ten percent of all misdemeanor conviction are violent offenses. Assuming violent misdemeanor defendants lack prior restrictions (otherwise they would likely face felony charges), approximate (0.73)*(0.10) = 7.3% of all misdemeanor convictions impose a legal firearm restriction.

We calculate separately by race given heterogeneous mortality and conviction effects. Table 6 shows that misdemeanor and felony convictions increase by 2,900.7 and 1,866.5 per 100,000 Black individuals (see Columns 1-2). Applying the scaling above yields an additional $(0.6)(1,866.5) + (0.073)(2,900.7) = 1,332$ additional gun restrictions per 100,000 Black individuals. Using the same logic, Columns 3 and 4 imply an additional $(0.6)(368.3) + (0.073)(1072.9) = 299$ additional gun restrictions per 100,000 White individuals.

What do these estimates, when combined with our mortality findings, then imply about the effect of firearm restrictions on firearm mortality? Among young White men, where incarceration explains none of the effect, we observe 3.3 fewer firearm deaths per 100,000. If effects operative solely through gun restrictions, then each restriction prevents $\frac{3.3}{299} \approx 0.011$ firearm deaths; framed another way, there is about one firearm death prevented for every 91 additional gun restrictions imposed.

For Black individuals, Section E.1 suggests that incarceration explains one-third of the firearm mortality effect. If the remaining $(2/3)(24.4) = 16.27$ fewer deaths are due to gun restrictions, then each restriction prevents $\frac{16.27}{1,145} \approx 0.0122$ firearm deaths. Equivalently, one firearm death is prevented for every 82 additional gun restrictions imposed.