Local Prosecutors and Public Health*

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Abstract

This paper investigates how local prosecutors affect community-level mortality. Using exogenous variation from close partisan elections in a regression discontinuity design, we find that narrow election of a Republican prosecutor reduces mortality among young men by 6.6 percent. This decline is driven by firearm deaths, including large homicide reductions among Black men and firearm suicide/accident reductions among White men. Increased incarceration explains less than half of the effect among Black men, and remaining effects are unexplained by detention or general deterrence. Instead, Republican prosecutors secure additional non-carceral convictions triggering gun restrictions, suggesting reduced firearm access as the primary mechanism.

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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). Together, deaths from firearms and drug overdoses grew from 12% of all deaths among Americans ages 20-59 in 2010 to 18% in 2019;[1] meanwhile, nearly 2 million Americans were arrested each year for violent or drug offenses (Kennedy and Lopez, 2025).

These facts raise 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 powerful yet understudied criminal justice actors: local prosecutors. Prosecutors make key decisions — whether to file criminal charges, the severity of these charges, and how hard to fight for a given sentence — with potential implications for important mortality determinants like firearm access and drug supply. Legal scholars have long noted that prosecutors are "the criminal justice system's real lawmakers" (Stuntz, 2001) given their power and discretion (Misner, 1995; Bibas, 2009; Bellin, 2019). Prosecutors also handle hundreds of thousands of cases annually, impacting millions of Americans directly and indirectly (Cook County State's Attorney, 2018; Becker, 2022; Gramlich, 2023). While the practices of police and judges are well-studied (Chalfin and McCrary, 2017; Chyn et al., 2025), we know little about how prosecutors affect core community outcomes or the mechanisms through which they operate.

To understand how prosecutors affect community health, we leverage 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

[1] Authors' calculations using CDC Mortality Data.

than their Democratic counterparts.[2] To see whether this approach to criminal prosecution translates into 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 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 but still sizable 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 prison admissions among Black men, and we estimate that prison-based incapacitation explains roughly 15 to 36 percent of the mortality reduction among this group.[3] We find no increases in incarceration among White men, nor any clear effects

[2]This matches the Republican Party's high-level political platform. For example, their 2008 Presidential platform stated: "Criminals behind bars 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 contrast, the Democratic Party's platforms tend to emphasize deterrence and social programs over incapacitation-oriented 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."

[3]These back-of-the-envelope estimates account for both the reduced mortality risk among the incarcerated

on admissions to local jails, suggesting little change in policing behavior or general criminal deterrence.

Instead, the firearm mortality effects appear to operate predominantly through some non-carceral aspect of criminal conviction. We find evidence of large increases in criminal convictions issued across all severity levels and offense types, driven by Republican prosecutors filing more charges rather than achieving higher conviction rates for charges filed. The demographic patterns in these conviction results — large increases among Black individuals and smaller but still substantial increases among White ones — mirror our mortality results more completely than the incarceration effects, pointing to some channel outside of detention.

Further results suggest that a specific feature of many convictions drives our mortality effects: legal restrictions on firearm access. Many of the convictions induced by Republican prosecutors prevent formal gun purchases, severely punish gun possession, and establish circumstances under which existing gun possession, including illegal gun possession, is easier to detect. Consistent with this, we observe reductions in one of the best available proxies for county-level gun ownership: the share of suicides committed with a firearm (Azrael et al., 2004; Kleck, 2004; Cook and Ludwig, 2006; Briggs and Tabarrok, 2014; Evans et al., 2022). The decline in the number of firearm suicides and accidents among young White men points to this channel as well. Since White men do not experience an increase in detention under a Republican prosecutor, this observed decline in self-inflicted gun mortality cannot operate through incarceration and must instead reflect reduced gun access.[4] Taken together, these findings suggest that criminal convictions meaningfully reduce gun access for some on the

themselves and among would-be victims of the incarcerated. The exact estimate depends on assumptions about the length of prison stays induced by a Republican prosecutor.

[4]This effect is also plausible given elevated suicide mortality risk among the criminally involved, and the potential presence of criminal involvement among those who die by suicide. Even with causal research indicating that incarceration reduces suicide mortality risk (Hjalmarsson and Lindquist, 2022; Norris et al., 2022), suicide mortality among those in state prison is about 32% higher than in the general population (Carson, 2021), implying greatly elevated risks preceding detention. Individuals on the margin of conviction plausibly possess similar increased suicide risk. At the same time, reporting by the CDC finds that 10% of men who die by suicide had a "recent criminal legal problem" that was a direct "precipitating circumstance" of the suicide itself (Jack et al., 2018).

margin of gun violence or self-harm.

We validate our results in several ways. We show that geographic characteristics and baseline demographics are balanced across the vote share threshold. Our results are also not contaminated by differences in the partisan affiliation of other local officeholders across the prosecutor vote share cutoff. Placebo tests using pre-election mortality rates show no effects before the election, and we find no effects on mortality from internal causes (i.e., medical conditions and diseases), which prosecutors should not affect. Results are also robust to dropping high-weight counties, varying bandwidths, and using permutation-based inference.

This paper makes several contributions. First, we expand the literature on how local criminal justice practice shapes core community-level non-crime outcomes. Recent attention has been paid to policing and its effects on outcomes like human capital, employment, social trust, and health (Ang, 2021; Ang et al., 2025; González and Prem, 2024; DeAngelo and Hansen, 2014), but prosecutors remain largely understudied. Stashko and Garro (2021) show that prosecutor turnover (regardless of party) decreases deaths caused by police, and Agan et al. (2025) find that election of reform-minded prosecutors has no effect on overall drug overdose mortality.[5] We provide novel evidence that local prosecutors substantially affect a core, broad measure of social wellbeing: all-cause mortality among young adults.[6]

Second, we contribute to debates about the costs and benefits of criminal prosecution. Recent work demonstrates that prosecution can be counterproductive for marginal misdemeanor defendants (Agan et al., 2023; Kamat et al., 2024), and negative effects are observed among felons as well (Mueller-Smith and T. Schnepel, 2021), but often only among those who

[5]The context of our study differs meaningfully from Agan et al. (2025). They examine a sample of 57 progressive-minded local prosecutors and employ a difference-in-differences design, whereas we analyze 368 closely contested elections using a regression discontinuity framework that compares Republican and Democratic prosecutors. In addition, their mortality analysis focuses exclusively on overall drug overdose mortality, while we examine all-cause mortality as well as cause-specific deaths, including firearms.

[6]Existing work on local prosecutors instead focuses almost entirely on criminal outcomes. A handful of unpublished working papers find that Republican prosecutors increase conviction rates (Fischer and Ludwig, 2023) or incarceration rates (Arora, 2019; Krumholz, 2020), with particularly large effects among Black defendants (Yntiso, 2022). Some of these papers (Krumholz, 2020; Fischer and Ludwig, 2023) along with Agan et al. (2025) also investigate the relationship between prosecutorial approaches and crime rates and find no clear relationship. Our mechanisms analysis advances these patterns by using data from more districts and connects these findings to our mortality effects.

receive non-carceral convictions (Rose and Shem-Tov, 2021; Kamat et al., 2024; Humphries et al., 2025). We complement this work by examining effects on community-wide outcomes, which include those not directly tied to the criminal justice system, when entire jurisdictions shift prosecutorial practices. We find that jurisdiction-wide increases in prosecution can meaningfully reduce firearm mortality, a benefit outside the scope of what individual-level research designs can capture.

Third, we contribute to the literature on incarceration and health. While correlational studies often find that incarceration is associated with worse health outcomes (Binswanger et al., 2007; Daza et al., 2020), the two existing quasi-experimental papers on the topic show that incarceration reduces mortality risk during confinement, with these reductions persisting or even growing after release (Norris et al., 2022; Hjalmarsson and Lindquist, 2022). However, neither study has clear external validity for the US broadly—Norris et al. (2022) study only three large Ohio counties, and Hjalmarsson and Lindquist (2022) examine a Nordic country with a fundamentally different criminal justice system. Our results reinforce the credibility of these findings—we find that mortality falls most among the demographic group (Black Americans) that experiences substantially increased incarceration when a Republican local prosecutor is narrowly elected, and we see declines in some of the same causes of death. Our findings, obtained using data from over 500 geographically dispersed counties, also point to broader external validity throughout the US.

Fourth, we contribute to the literature on US gun violence, where our findings point to a stronger link between criminal prosecution and gun access than previously recognized. On paper, many criminal convictions restrict gun access: felons and perpetrators of domestic violence are legally barred from owning firearms, and probationers are often forbidden from possessing weapons while under supervision.[7] These restrictions, discussed in detail in Section IV.3, are enforced through a national background check system at purchase, harsh penalties for illegal possession, and monitoring while on parole or probation. Yet, research

[7]Several states also impose further restrictions for those convicted of certain violent misdemeanor offenses (Giffords Law Center, 2025).

shows that restricted individuals can often still obtain a gun through secondary markets and that many firearms used in criminal activity are acquired this way (Cook, 2018). Therefore, the extent to which criminal convictions in practice reduce gun access has been unclear.

Our findings suggest that criminal conviction-based firearm restrictions do meaningfully reduce gun access and mortality. Our back-of-the-envelope calculations imply that for roughly every 80 newly imposed restrictions per year among those on the margin of receiving one, about one firearm death is prevented. Broader forms of gun control can generate similar effects 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 an 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 estimates shed light on the benefits of one channel for reducing firearm deaths in a context with several policy options, each with its own tradeoffs and limitations.

Last, this paper 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, also called district attorneys (DAs) or county attorneys, are public officials tasked with representing the state in criminal judicial proceedings. Local prosecutors are *local* officials who serve districts comprised of either a single county or a collection of counties.[8] Local prosecutors lead offices consisting of tens to thousands of employees, including assistant district attorneys (ADAs) who handle most day-to-day prosecution decisions under the prosecutor's direction. The largest prosecutor's offices handle tens of thousands of cases annually; for example, Cook County (Chicago) and Suffolk County (Boston) handle about 25,000 to 33,000 cases each year (Cook County State's Attorney, 2018; Becker, 2022).

The local prosecutor's office exercises substantial discretion at each stage of criminal case proceedings. First, they 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 control over justice system experiences and outcomes. Discretion over prosecuting lower-level offenses in particular has generated national debate and sparked efforts to remove prosecutors from office or circumvent their decisions (Hessick and Su, 2023). The intensive margin choice of the number, type, and severity of initial charges

[8]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," though the positions are functionally identical. In Texas, some counties divide prosecution between county attorneys (misdemeanors and civil cases) and district attorneys (felonies). Our analysis focuses on district attorneys in these cases, but results are robust to alternate treatments and to omitting Texas entirely.

levied is also left to the prosecutor's office. This choice is meaningful, as a wide range of crimes (e.g., assault, domestic violence, theft, drug crimes, burglary, and vandalism) can typically be charged as either a misdemeanor or felony (McDuffey, 2023).

Conditional on the decision to file and pursue criminal charges, prosecutors next have input on bail and pre-trial detention decisions. These decisions are ultimately made by judges, but prosecutors typically make recommendations, and evidence suggests these recommendations are often followed (Coalition to End Money Bond, 2017) or are at minimum influential (Miller, 2013). The bulk of the work and influence of the prosecutor then occurs during plea bargaining, a process by which the prosecution negotiates with the defense for at least one guilty/no contest plea in exchange for certain benefits.[9] 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.

By necessity, local prosecutors delegate many tasks and decisions to ADAs among whom decision-making varies (Agan et al., 2023); that said, local prosecutors still exert strong top-down influence on ADA behavior through specific directives and final say on important matters. Local prosecutors have been known to produce lists of offenses the office will decline to prosecute (New York Civil Liberties Union, 2017; Cramer, 2018; Caine, 2024), clear guidelines on recommending cash bail to judges (Ouss and Stevenson, 2023), and even names of police officers whose testimony should not be relied on in court (Balboni and Grometstein, 2020). Local prosecutors also control ADA staffing, and evidence reveals significant turnover when a new local prosecutor takes office, meaning that recalcitrant ADAs are typically not an impediment for local prosecutors.[10] In short, as one legal scholar notes, "it is commonplace for a [local] prosecutor to declare a policy on an important topic and to make the policy

[9]Criminal cases in the US rarely go to trial. Estimates suggest that close to 97% of cases resolve with either a plea bargain or a dismissal (Johnson, 2023; Vera Institute of Justice, 2020).

[10]For example, over half of Santa Fe's ADAs resigned when a new DA took office in 2016 (Haywood, 2018), roughly a third of San Francisco's ADAs departed within two years of DA turnover in 2020 (Shaban et al., 2021), and Philadelphia's current DA forced 31 ADAs to resign upon taking office in 2018 (Palmer, 2019). Rapid ADA turnover is also common even absent leadership changes: ADAs typically serve fewer than five years (Dresang et al., 2011), and about half of surveyed ADAs say they have seriously considered leaving within the past month (National District Attorneys Association, 2024).

stick" (Levine and Wright, 2012).

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.[11] 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.[12] When a candidate runs unopposed in the general election, the vote share variable will be coded as 0 (unopposed Democrat) or 1 (unopposed Republican).[13]

[11] 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.

[12] 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.

[13] Some districts do not require unopposed general election candidates to appear on the election ballot. If not handled carefully, this phenomenon could lead to misleading values for our vote share variable. To illustrate, suppose a Republican candidate wins a closely contested election with 55% of the vote in 2012 and runs for reelection uncontested and without appearing on the ballot in 2016. In this case, we code the 2017 Republican vote share variable as "1" because the Republican candidate essentially won the election unopposed, even though the election did not 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

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 to our analysis. These counties reside within a total of 368 electoral districts and contain about 23% of the total US population.[14] The first several rows of Table 1 show no discernible differences between in- and out-of-sample counties in terms of race, age, and urban/rural populations. Counties with closely contested prosecutor elections had a slightly lower baseline unemployment rate (9.16 vs 9.89), but similar poverty rates and fractions of individuals without health insurance. Education levels are similar in both sets of counties and any dif-

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).

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

ferences are statistically indistinguishable from zero at the 5% level. Last, in-sample counties received more shipments of opioid pills than the rest of the US, 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 US 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 primarily on mortality among men, the group ex-ante most likely to be affected by a change in criminal justice policy, and analyze mortality rates in ten-year age groupings (20–29, 30–39, etc.).[15]

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.[16] 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.[17] We also explore effects on three types

[15]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.

[16]Deaths from unknown causes make up about 1% of all deaths.

[17]We combine suicides and accidents for two reasons. First, it can be difficult for medical examiners/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 makes them relatively rarer, which in our county-by-year-by-age group analysis can lead to excess zeros and issues with statistical inference.

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 US as a whole.

I.4 Local Jail and Prison Data

Our data on local jails and prisons come from the Vera Institute of Justice's Incarceration Trends (VIJIT) dataset, which uses several sources to calculate county-by-year correctional facility admission and population rates. The VIJIT jail data are 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 are derived from the National Corrections Reporting Program (NCRP) and—when NCRP data are unavailable or unreliable—data collected directly from state departments of correction. Importantly, this prison data links sentences to the court which issued them, allowing us to track the number of people sentenced to prison by the courts operating in a given county and year. This structure is well-suited for our analysis since we seek to connect prison admissions to prosecutors, and data that instead tracked prison admissions in terms of the county of the prison facility itself would make this difficult.

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 show, the results convey the same narrative when facility populations are used. Data coverage for the counties in our estimating sample is nearly complete—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.[18]

Facility admission rates are also available by race and sex in some cases. Jail admissions can be broken down by sex (but not race), and prison admissions are available by race, sex, and race-sex combinations. Data coverage is sparser for these demographic specific rates; for example, prison admission rates among Black individuals and Black men are only available for about 48% of our estimating sample observations. However, the missing data are concentrated among smaller counties, and measuring data coverage in terms of the fraction of the total population within our estimating sample covered reveals much higher rates. For example, the county-by-year observations available for Black prison admissions cover 90.1% of the total Black population within our estimating sample, and since our analysis weights by population, the loss of information for smaller counties should not meaningfully affect the results.[19]

I.5 Criminal Conviction Data

Our data on criminal conviction rates come from the Criminal Justice Administrative Records System (CJARS) Justice Outcome Explorer (JOE) dataset. 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.

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

[19] 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 are also sparse in West Virginia and Wisconsin.

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 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.

Our conviction estimating sample consists of over 200 county-by-year observations from across eight states. This smaller sample as compared to our mortality and incarceration analysis stems from the JOE itself, as data are unavailable for several states and for counties with populations smaller than 80,000 people.[20] That said, the counties in our estimating sample contain over 32 million individuals, and we are powered to detect effects on conviction rates of approximately 30%.[21] 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. The population coverage in our conviction results is also four times larger than the median population covered by recent causal studies on criminal justice, implying, if anything, greater external validity than the typical paper.[22] The size of the covered population, in conjunction with the power calculations, suggests that the conviction estimates we derive are likely to be informative.

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.

[20]See Appendix Tables A5 and A6 for more information on data coverage. We also conduct tests to show that data unavailability is uncorrelated with treatment in our study; those tests are discussed in Section II.

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

[22]See Appendix Figure A1, which visually captures how our covered population size compares to the covered population in the census of recently published journal articles in the two largest volume publication journals published by the American Economic Association.

We use a vote share regression discontinuity design, an approach commonly used to uncover causal estimates in settings with elected officials, to identify the causal effect of a change in prosecutorial approaches. We use Republican vote share as the running variable, but the results are essentially the same (with the sign reversed) if we use Democratic candidate vote share instead. This design assumes that the average potential mortality outcomes as a function of the running variable are continuous across the 50 percent vote share cutoff. Under this assumption, the observed post-election mortality rates in counties which narrowly fail to elect a Republican candidate provide a credible estimate of the counterfactual outcomes for districts which narrowly do elect one.

Our primary regression specification is given by Equation 1. The outcome variable, Y_{cdst}, is a mortality rate in county c in electoral district d in state s and year t. The 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.[23] We include these baseline county characteristics and fixed effects to explain variation in mortality rates across counties and thus improve the

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

precision of our estimates.[24] In our mechanisms analysis of prison and jail admissions, X_c contains the same variables as our mortality analysis but swaps the baseline mortality rates for baseline outcomes averaged over 2004-2006.[25] 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.[26] In tables reporting

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

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

[26]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 those used

estimates across several subgroups or outcomes, we additionally report Benjamini-Hochberg q-values to adjust for multiple hypothesis testing. Our key results remain statistically significant after these adjustments throughout the study.

We perform a number of tests to probe the validity of this empirical strategy throughout the paper. To begin, Table 2 shows that the 50% vote share cutoff is not associated with discontinuous changes in any geographic or 2010 county-level characteristics, which reinforces the notion that the potential mortality outcomes of counties close to either side of the cutoff are similar. Appendix Figure A2 also reveals no clear discontinuity in the density of our running variable across the cutoff, and Appendix Figures A3 and A4 show that data missingness for the prison and conviction variables in our mechanisms analyses is balanced across the cutoff. Altogether, these tests imply that the latent characteristics of observations in our estimating samples are smooth across the vote share threshold.

Furthermore, in Appendix Section C, we show both theoretically and empirically that our strategy does not confound the effects of the local prosecutor's political affiliation with that of other local offices. We first show that, under weak assumptions in a continuity-based RDD framework, the prosecutorial cutoff generates no discontinuity in the probability of a Republican holding another office, even in the presence of extremely strong (but imperfect) correlation between vote shares. We also show that the Republican prosecutorial vote share is very weakly correlated with that of other offices, including the county's sheriff, demonstrating that our results are not explained by other differences in local governance. Additional robustness 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 all-cause mortality among men by age group. Column 1 shows a precisely estimated reduction in mortality among men ages 20 to 29 (here forward referred to as "young men")

for the point estimates, a choice we then consistently use for all estimates.

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.[27] The Columns 2-4 estimates for the older age groups are neither statistically significant nor meaningful in magnitude relative to their baseline rates, suggesting no effect on mortality for men 30 years old or older. In Appendix Table A8, we show that estimates for women are also null, again consistent with disproportionate criminal justice involvement among young men, who make up some 75-92% of the US criminal justice system (Office of Juvenile Justice and Delinquency Prevention, 2022; U. S. Government Accountability Office, 2024).[28]

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 declines 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 violence and access as important mechanisms.

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

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

[28]That said, the effects among young men are large enough to generate a precisely estimated reduction in mortality among all 20-29 year olds of 5.91 fewer deaths per 100,000 (see Appendix Table A9).

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 adults 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, with these changes fully offsetting each other.[29]

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 of a Republican local prosecutor reduces external cause mortality among this group by 30 deaths per 100,000 (an 18% reduction).[30] 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 partially explained by firearm suicides/accidents (1.9 fewer deaths, Column 6).[31]

Among White men (Panel B), the pattern noticeably differs. 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.

[29]Meanwhile, fentanyl overdose mortality remains unchanged (see Figure A5).

[30]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.

[31]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).

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 still notable 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 incarceration and convictions across these groups.

IV Mechanisms

IV.1 Prison Admissions

We now explore mechanisms underlying our mortality results. Specifically, we examine effects on outcomes plausibly connected to both prosecutorial behavior and firearm mortality, noting that a convincing mechanism account should be consistent with our heterogeneity findings (large mortality effects among Black men and smaller but nonzero effects among White men). We first investigate whether Republican prosecutors secure more prison sentences. Preexisting evidence shows that prison-based incapacitation reduces mortality risk among marginal inmates in a smaller-scale US and Nordic settings (Norris et al., 2022; 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 100,000 (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.[32]

[32]Appendix Section E, which contains all mechanisms results not shown in the main text, shows additional results: there are no effects among men or women in general (Appendix Figure E1); effects remain similar when prison populations are analyzed instead of prison admissions (Appendix Figure E2); and young Black

How much of the mortality reduction among young Black men can be attributed to increased prison-based incapacitation? In Appendix Section F, we perform a back-of-the-envelope calculation that incorporates effects on both the incarcerated's mortality risk and that of potential victims. Our estimates, which depend upon the assumed average length of the prison sentences induced by a Republican prosecutor, suggest incarceration explains approximately 15 to 36 percent of the mortality reduction among Black men. The absence of prison admission effects among White individuals, who also experience firearm mortality reductions, further indicates that prison-based incapacitation cannot fully explain our findings, motivating our exploration of additional mechanisms.

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.[33]

Our findings on local jails are presented in Appendix Figure E3. 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

male mortality effects are present among the sample of counties with available prison data, confirming that both the mortality decrease and incarceration increase are occurring in the same population (Table E1).

[33]To illustrate, police may increase (or decrease) arrests under a more aggressive prosecutor with the belief that those arrested are more likely to be prosecuted, which would increase (decrease) jail admissions. Conversely, jail admissions could also fall if more aggressive prosecution or increased pretrial detention lengthens 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" (Bureau of Justice Statistics, 2025; Warner and Kramer, 2008) under a more aggressive prosecutor.

that our mortality findings are not explained by changes in policing behavior, general criminal deterrence, or jail-based detention.

IV.3 Criminal Convictions

Having ruled out detention and general criminal deterrence as comprehensive explanations, we next consider whether criminal convictions, even when not resulting in a prison or jail sentence, could lead to reduced gun mortality risk. We first test for changes in conviction rates. Figure 6 shows the effect of narrow Republican prosecutor election on the annual per-capita rate of criminal convictions issued among Americans ages 25 to 40. We find large increases across severity levels and offense types: misdemeanor convictions increase by 1,615 per 100,000 (Panel A) and felony convictions increase by 650 per 100,000 (Panel B), with increases occurring for both drug and violent offenses (Panels C and D). In Appendix Section E, we provide additional results: these conviction increases stem from Republican prosecutors filing more charges rather than achieving higher conviction rates for charges filed (Figure E4), property crime convictions increase as well (Figure E5, Panels A and B), and the drug and violent conviction effects hold for both misdemeanors and felonies (Figure E5, Panels C through F).

Next, Table 6 shows that these conviction effects align with the demographic patterns in our mortality findings. Conviction effects are seen for both Black and White Americans, but the effects are noticeably larger for Black individuals. These mirror the noticeably larger mortality reductions for young Black men and smaller but nonzero effects for young White men.[34] These results point towards some non-carceral aspect of convictions as a key mechanism in reducing firearm mortality.

Ex ante, there is a clear channel through which many of these convictions can reduce gun mortality: legal restrictions on firearm ownership. These restrictions come in two broad

[34]We also show in Appendix Table E2 that, despite the loss of power from narrowing the sample to about one tenth of its original size, mortality effects are still detectable among young Black men in the sample of counties used in this conviction analysis, providing evidence that the mortality and conviction effects are happening within the same population.

forms: long-term/permanent bans mandated by the type of conviction, and temporary restrictions imposed as a condition for probation. We now lay out the nature of these restrictions and highlight the ways in which they prevent, punish, and monitor gun access. Here, we do not attempt to argue that restricted individuals have no means of firearm access, nor that threats of punishment always deter illegal ownership, but simply that these restrictions create meaningful frictions and disincentives for gun ownership.

To start, federal law prohibits gun possession among convicted felons and those convicted of misdemeanor domestic violence offenses. Several states extend these restrictions to violent misdemeanants as well (Giffords Law Center, 2025). Only about 40 percent of felony convictions carry a prison sentence (Durose et al., 2009), so most of the additional felony convictions secured by a Republican prosecutor trigger gun restrictions but no incarceration. These numbers alone suggest that convictions could plausibly affect mortality more through firearm restrictions than through incarceration.

Restricted individuals cannot legally purchase or own firearms. To help prevent illegal purchases, federally licensed firearm dealers must conduct a background check on potential buyers through the FBI. While it contains loopholes (Wintemute et al., 2010), this system catches and prevents a large volume of illegal purchases; e.g., between 2015 and 2019, nearly 500,000 individuals failed a background check when attempting to purchase a gun due to a conviction (Everytown Research & Policy, 2020).[35] Gun owners are expected to relinquish their weapons after conviction, and some states take particularly active steps to ensure this occurs (Oliphant and Zeoli, 2024). For example, prosecutors in some districts use firearm sales databases, victim reports, and social media to identify whether the convicted person owns a gun and, if so, require verification of dispossession at the risk of additional jail time (Frattaroli et al., 2021).

The consequences for being caught possessing a gun while restricted are severe. Illegal possession among convicted felons can be prosecuted by either state or federal courts and

[35]Failed background check counts may also understate the deterrent effect by excluding individuals who never attempt a purchase due to their criminal records.

almost always consists of a felony charge, carrying up to 10 years in federal prison (18 U.S.C. § 924). Penalties also exist to disincentivize others from illegally providing them with a firearm. For example, a "straw purchase" of a gun for someone who is restricted is typically a felony offense (18 U.S.C. §§ 922(d), 932), and even simply possessing an unlocked and accessible firearm in a home with a restricted individual can carry criminal charges for both parties (Blocher and Wilson, 2022). Since 46% of gun owners (15% of the total population) keep a loaded and unlocked gun at home (University of Connecticut, 2025), a firearm restriction would require changes within a wide swath of households, with steep costs for noncompliance.

This discussion so far focuses on restrictions mandated by the type of conviction, but there is another avenue through which convictions restrict gun access: as a condition of probation. In at least twenty-one states, criminal statutes note that probationers in general can be barred from possessing weapons while under supervision, and seven of these states impose this as a baseline condition rather than an optional one.[36] While these restrictions only last as long as probation itself, the average probation in the US lasts nearly two years (Horowitz, 2021), making it a significant restriction for many individuals.

Finally, while not inherently tied to firearm restrictions, it is important to note that the community supervision which typically comes with conviction increases the likelihood that illegal firearm possession is detected and prosecuted. About 70% of individuals released from state prison are subject to post-release supervision (Carson, 2020), which entails in-person meetings with parole officers and even unannounced home searches. Probationers are similarly monitored.[37] Together, these channels make gun possession far more likely to be

[36]See Appendix A10 for a state-by-state breakdown. This list may understate the use in practice, as some states' statutes use vague language which could be consistent with these restrictions. For example, the Massachusetts code states that the court may impose "such [probation] conditions as it deems proper," which in practice seems to include firearm restrictions.

[37]Interactions with Child Protective Services (CPS) provide another channel of monitoring. Among fathers with a child in the household, 28% experience a CPS investigation within three years of a criminal conviction, compared to 5% of fathers without a criminal charge (Gifford et al., 2020). These investigations nearly always involve home visits—typically unannounced—in which caseworkers may search the home without plain-view limitations, including cabinets and closets (Arons, 2023; U.S. Department of Health and Human Services, 2003; Hager, 2022).

observed among the convicted.

Returning to our results, several additional pieces of evidence indicate that much of our mortality effect operates through the gun restriction channel. First, Appendix Figure A6 shows that narrow election of a Republican prosecutor affects one of the best proxies for gun ownership rates at local levels: the fraction of suicides committed with a gun (Azrael et al., 2004; Kleck, 2004; Cook and Ludwig, 2006; Briggs and Tabarrok, 2014; Evans et al., 2022). This finding can already be inferred from our main results, but it is worth emphasizing that previous work validates this metric as one of the strongest measures of gun access. For convenience, Figure A7 provides two simple figures based on this existing work. Here, we use two of the best sources of survey data on gun ownership — the Behavioral Risk Factor Surveillance System and the General Social Survey — to plot and estimate the correlation between household gun possession and the gun suicide percentage at the state and census-division levels. The raw correlation coefficients are 0.85 and 0.89, supporting the use of this proxy.

Another key piece of evidence in favor of the gun restriction mechanism is found in the conviction effects among White individuals in Table 6. Since this demographic group does not experience an increase in incarceration, reduced gun access through conviction-based restrictions provides the most plausible explanation for their firearm mortality reductions.

Third, a back-of-the-envelope calculation suggests the implied effect size of conviction-based gun restrictions on firearm mortality is reasonable. The full calculations are found in Appendix Section F.2. Since probation-based restrictions are difficult to quantify, we only count restrictions generated by felony and domestic violence convictions; including these other restrictions would raise both the restriction count and the implied number of restrictions per death prevented. Narrow election of a Republican prosecutor generates roughly 1,331 additional gun restrictions per 100,000 Black individuals and 299 per 100,000 White individuals. Combined with our mortality estimates and after netting out incarceration effects among Black men, this implies one firearm death prevented per 73 restrictions among

Black individuals and per 91 restrictions among White individuals. These magnitudes are plausible given that restrictions apply to individuals with elevated risks of firearm violence and firearm suicide.[38]

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 two main mechanisms: prison-based incapacitation explains roughly 15 to 36 percent of the large mortality reductions among young Black men, and conviction-based restrictions on legal gun access account for the remaining effects among both racial groups. 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 con-

[38]We explored whether effects vary across state gun law regimes (e.g., states with/without violent misdemeanor gun restrictions), but we cannot confidently distinguish between noise and true heterogeneity in such results. Appendix Figure A8 shows that randomly splitting states into two equally-sized groups (an amenable scenario power-wise) and estimating separate effects on firearm mortality produces a wide sampling distribution of effect differences, with the 1st and 99th percentile values around ± 14 deaths per 100,000. This sampling variation is sufficiently large that we cannot distinguish true heterogeneity from noise and therefore do not report heterogeneity tests by state characteristics.

viction 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 from 2010 to 2019. Column (2) presents 2010 averages for all remaining counties, and Column (3) presents the difference between the two averages. Averages are weighted by county population. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Balance in Baseline Characteristics Across the 50% 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 percentage 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 Male Mortality from All Causes

	(1)	(2)	(3)	(4)
	Men	Men	Men	Men
	20-29 Years Old	30-39 Years Old	40-49 Years Old	50-59 Years Old
	-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
q-value	0.012	0.853	0.853	0.853
Control Mean	147.6	193.5	345.3	815.7
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. q-values are Benjamini-Hochberg adjusted p-values that control the false discovery rate across the four age groups. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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
q-value	0.002	0.002	0.011	0.008	0.002	0.002	0.765	0.400
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.3 and Appendix B 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. q-values are Benjamini-Hochberg adjusted p-values that control the false discovery rate across the eight causes of death. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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
q-value	0.002	0.002	0.002	0.002	0.002	0.217	0.233	0.233
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
q-value	0.274	0.053	0.736	0.974	0.012	0.008	0.892	0.974
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

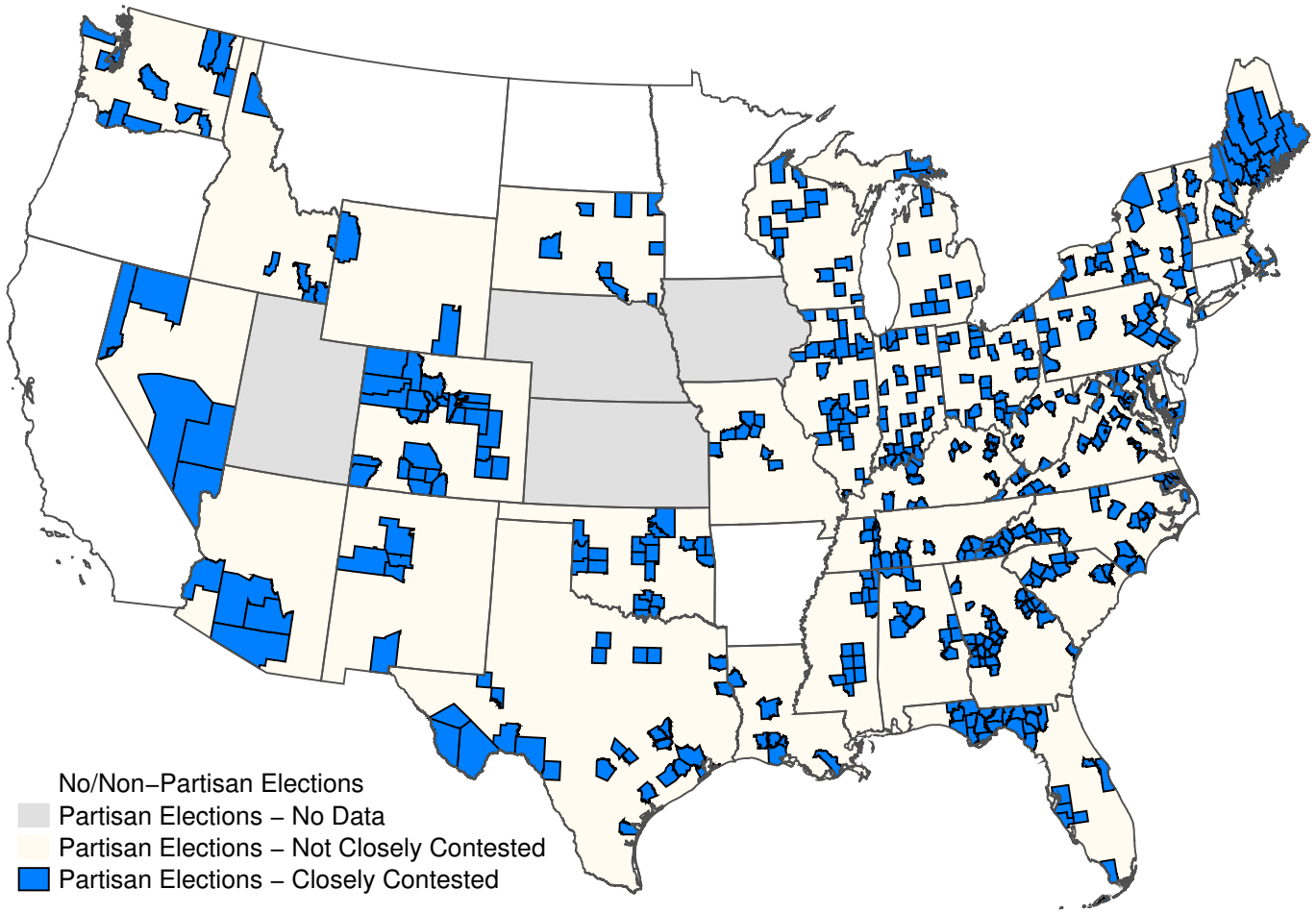
Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates among men ages 20 to 29. Panel A focuses on Black men and Panel B focuses on White men. Each column examines effects on mortality due to the cause category noted at the 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.3 and Appendix B 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. q-values are Benjamini-Hochberg adjusted p-values that control the false discovery rate across the eight causes of death within each panel. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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-value	<0.001	<0.001	<0.001	<0.001
q-value	0.001	0.001	0.001	0.001
Control Mean	5279.3	3007.6	2431.7	1310.4
N	213	199	217	209

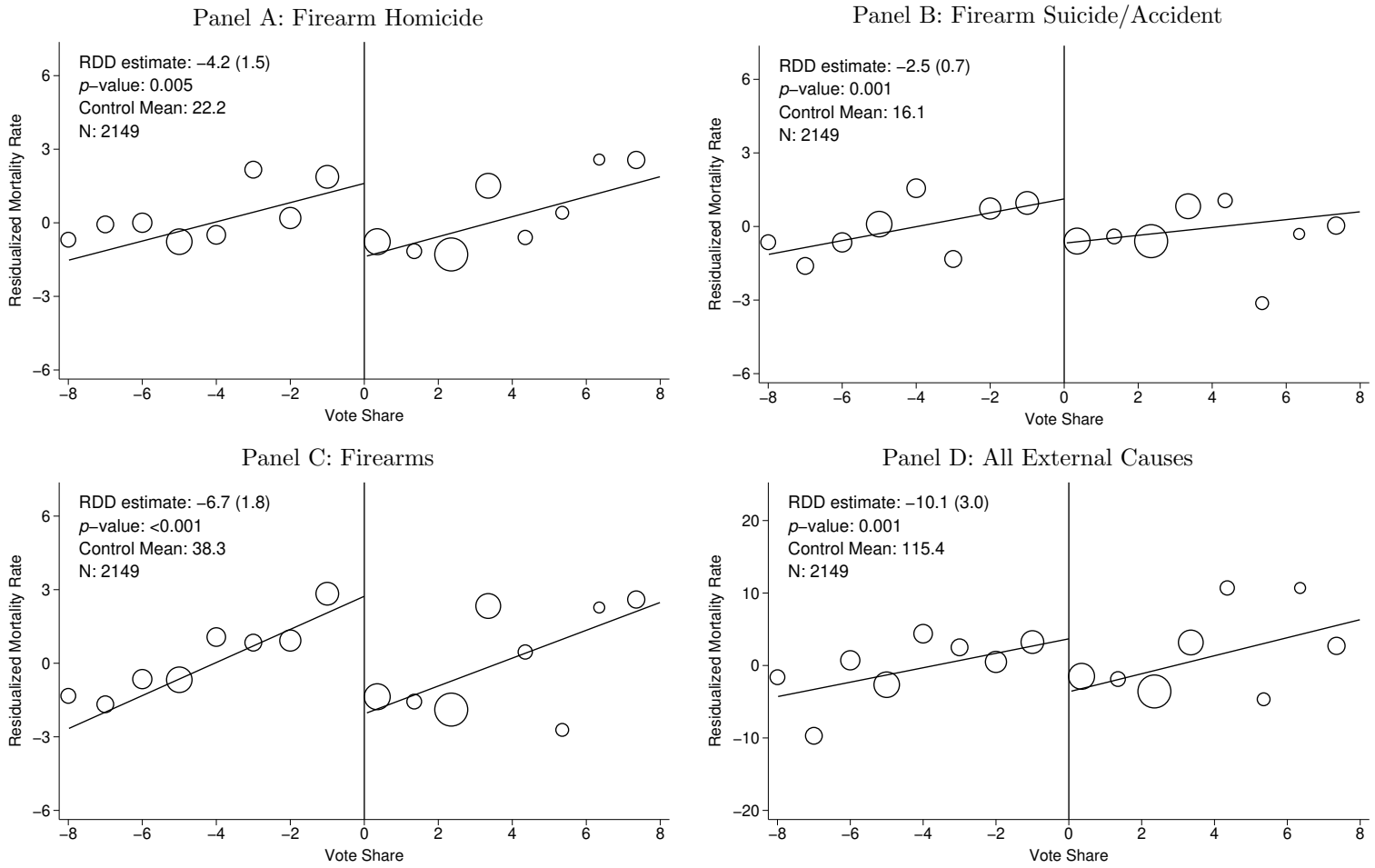
Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level rates of criminal convictions issued per 100,000 residents. Columns (1) and (2) examine effects on the misdemeanor and felony conviction rates among Black Americans ages 25-40. Columns (3) and (4) examine 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. q-values are Benjamini-Hochberg adjusted p-values that control the false discovery rate across the four outcomes within the table. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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



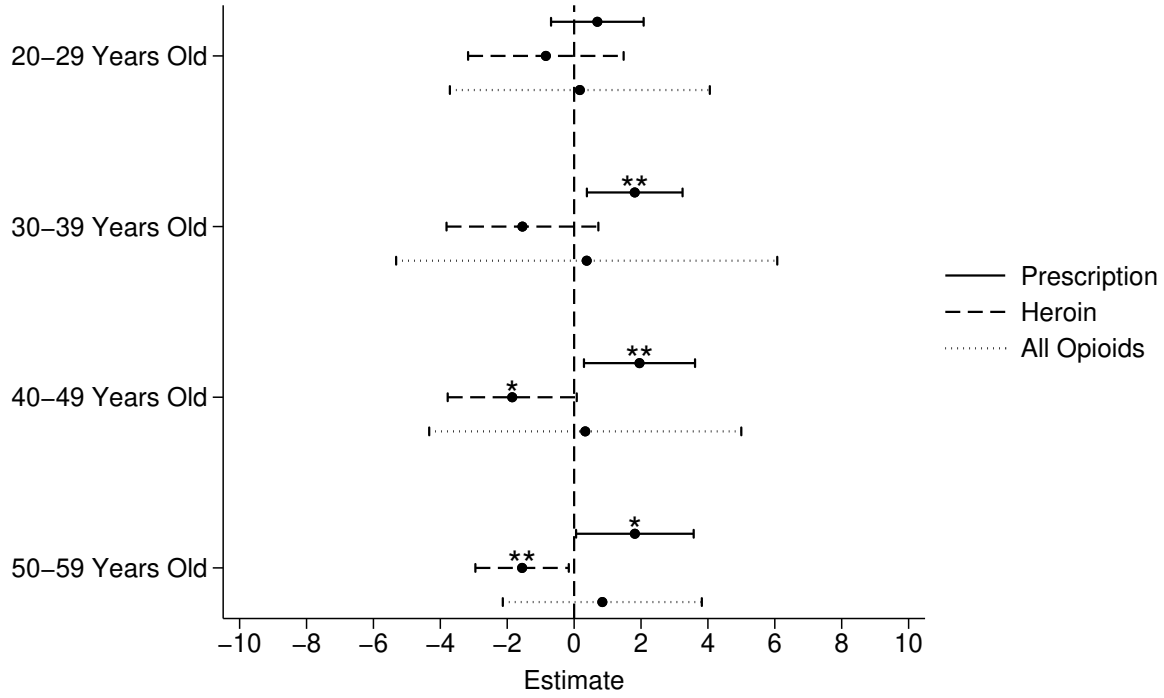
Notes: This map visualizes the counties which contribute to our regression samples. Counties shaded blue are counties in districts that elect local prosecutors in partisan elections and where the Republican vote share from the most recent prosecutor election fell between 42% and 58% in at least one year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

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



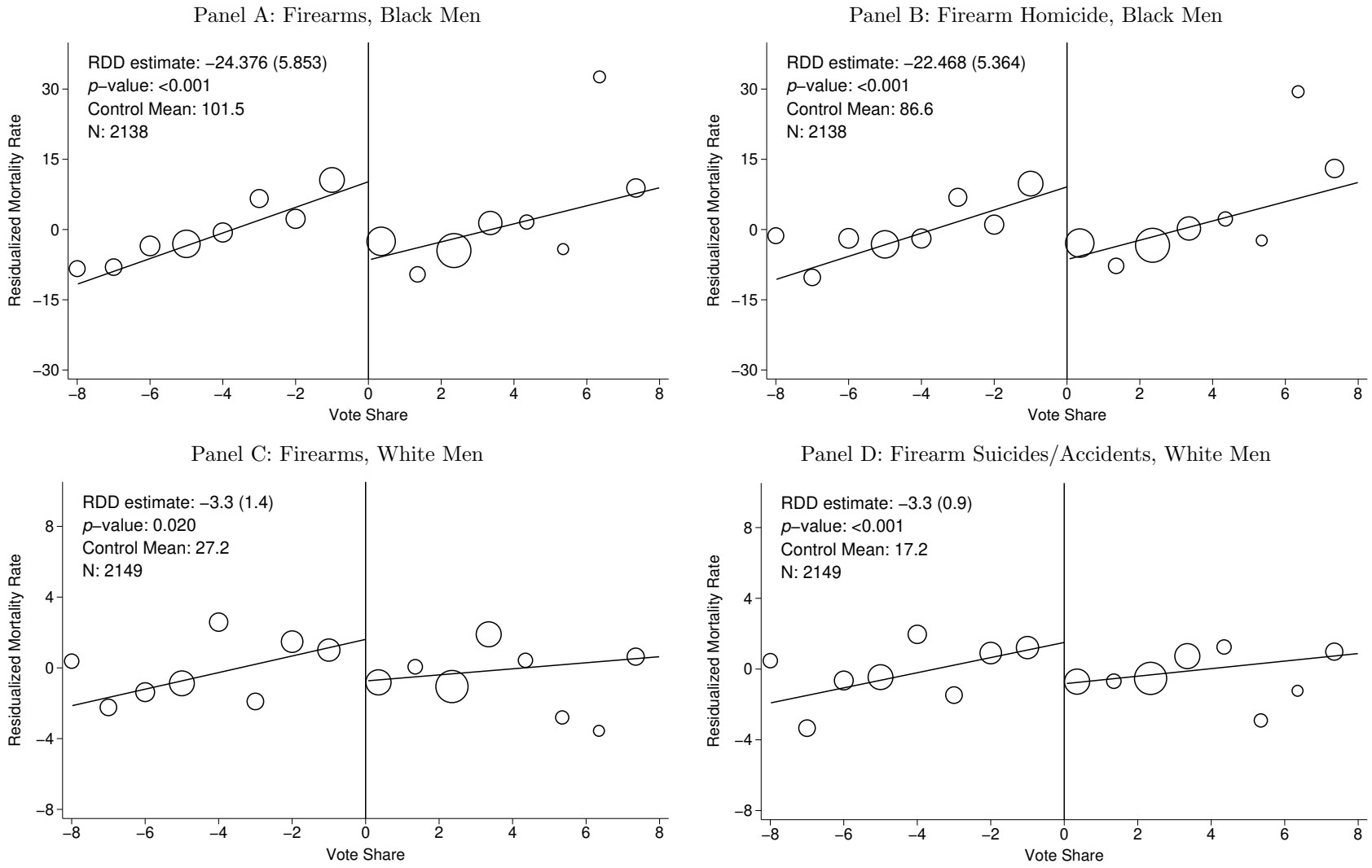
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality rates among men ages 20-29 years old. The information in the corner of each panel comes from estimation of Equation 1, and each plot visualizes RDD estimates 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 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



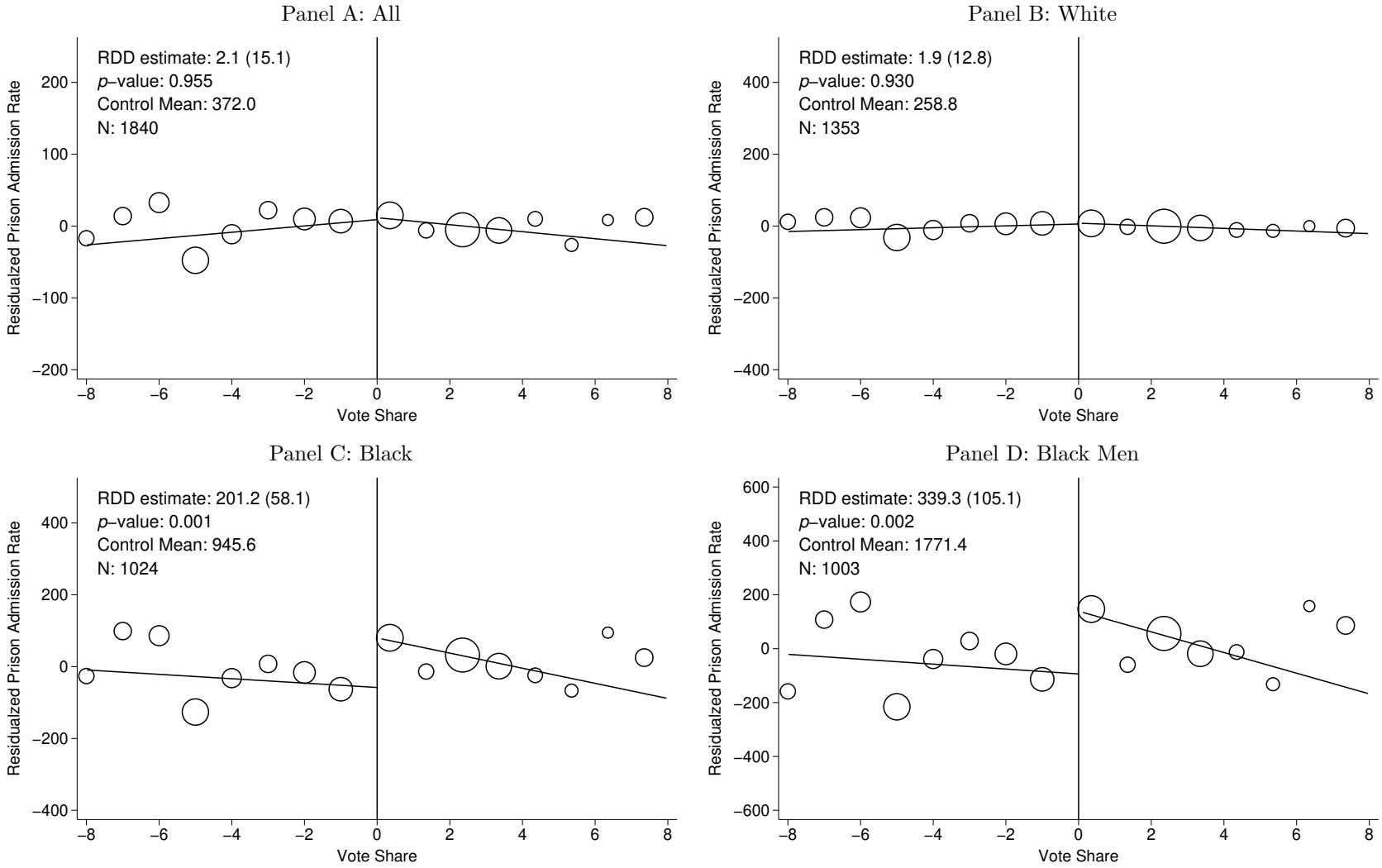
Notes: This coefficient plot conveys RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality due to opioid overdoses across ten year age groups. Each coefficient and corresponding conventional 95% confidence interval comes from a separate regression estimated using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



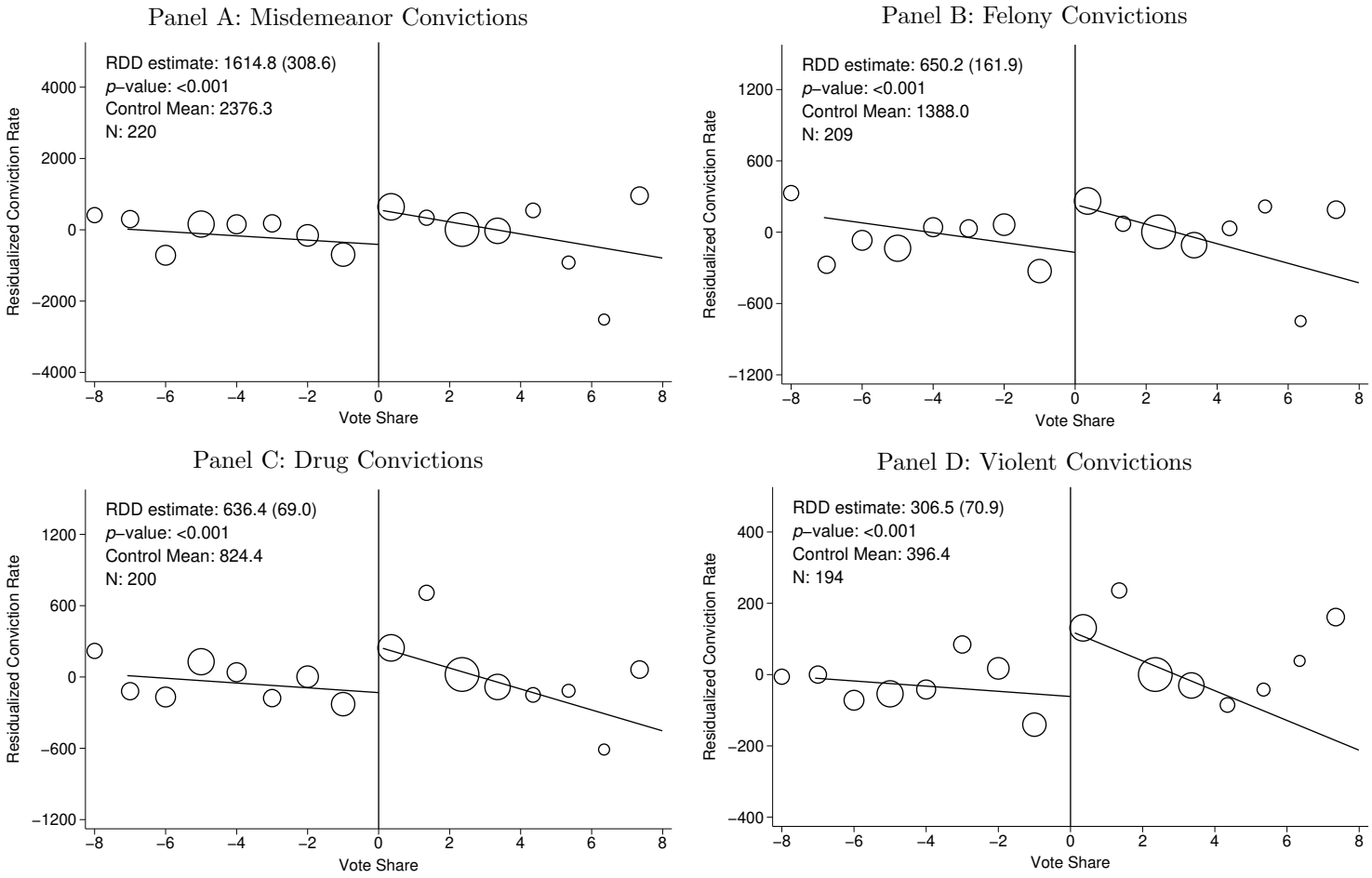
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality rates among Black and White men ages 20-29 years old. The information in the corner of each panel comes from estimation of Equation 1 and each plot visualizes 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 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	Off-Cycle	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 elections in the states which elect them in partisan elections. "Presidential" and "Midterm" states are ones where local prosecutor elections consistently coincide with each respective national election. "Staggered" states are ones with term lengths different than four years and thus swap between presidential and midterm years. "Mixed" states are ones where the timing of local prosecutor elections varies across districts within the state.

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

In Sample Rank	County Name	2010 Middle-Aged Population
1	Harris, TX	1192213
2	Maricopa, AZ	1051565
3	Dallas, TX	695896
4	Clark, NV	572316
5	Tarrant, TX	529860
6	Bexar, TX	471329
7	Nassau, NY	357847
8	Hillsborough, FL	352019
9	Oakland, MI	338710
10	Fairfax, VA	333636
11	Franklin, OH	327845
12	Mecklenburg, NC	289570
13	Wake, NC	287169
14	Salt Lake, UT	285272
15	Marion, IN	250358
16	Pima, AZ	239841
17	Erie, NY	230496
18	Montgomery, PA	220661
19	Baltimore, MD	211981
20	Will, IL	207033

Notes: This table lists the twenty largest counties (in terms of population) that contribute at least one county-by-year observation to our estimating sample, along with the 2010 middle-aged population.

Table A3: Prison and Jail 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 of Justice's 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 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 and Jail Data Availability, By State

	Jail Admissions	Prison Admissions	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 of Justice's 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 share of the total estimating sample population contained in counties available in the JOE data. 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 are 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 Female Mortality from All Causes

	(1)	(2)	(3)	(4)
	Women	Women	Women	Women
	20-29 Years Old	30-39 Years Old	40-49 Years Old	50-59 Years Old
	-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
q-value	0.455	0.455	0.540	0.455
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 among women 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. q-values are Benjamini-Hochberg adjusted p-values that control the false discovery rate across the four age groups. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A9: 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
q-value	0.020	0.845	0.756	0.756
Control Mean	102.9	150.3	281.2	651.7
N	2149	2149	2149	2149

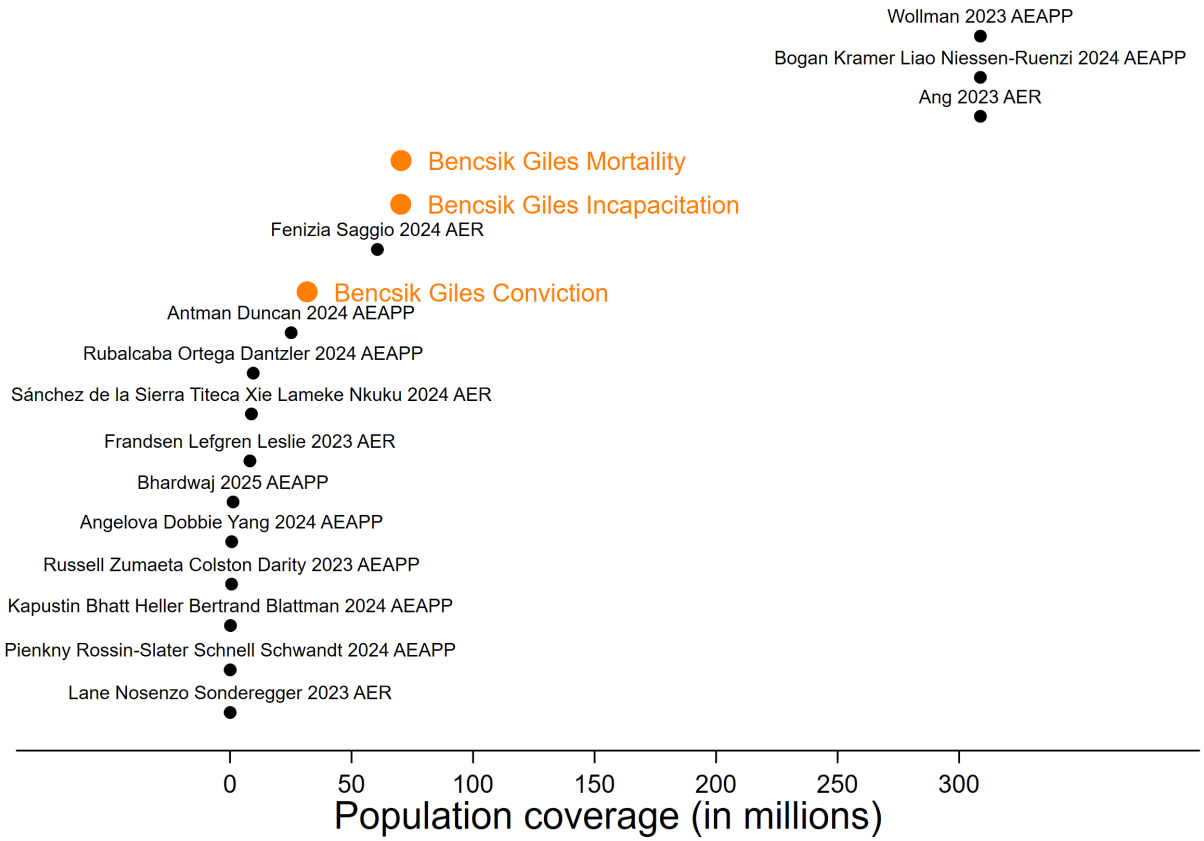
Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates from all causes. Each column examines effects among a specific ten year age group. Mortality rates are calculated per 100,000 residents and are age-adjusted. All estimates 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. q-values are Benjamini-Hochberg adjusted p-values that control the false discovery rate across the four age groups. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A10: States That Allow or Require Firearm Restrictions for All Probationers

State	**Section of Legal Code**	**Imposed as Baseline**
Arkansas	AR Code § 5-4-303	No
California	PEN § 29815	No
Colorado	CO Rev Stat § 18-1.3-204	No
Florida	Fla. Stat. § 948.03(1)	Yes
Hawaii	HI Rev Stat § 706-624	No
Indiana	IN Code § 35-38-2-2.3	No
Louisiana	La. C.Cr.P. art. 895(A)	No
Maine	17-A M.R.S. § 1807	No
Montana	§ 46-18-202(1)	No
Nebraska	Neb. Rev. Stat. § 29-2262(2)	No
Nevada	NRS 176A.400	No
New Jersey	N.J.S.A. 2C:45-1(b)	No
North Carolina	G.S. § 15A-1343(b)(5)	Yes
North Dakota	N.D.C.C. § 12.1-32-07(3)	Yes
Oregon	ORS 137.540(1)(j)	Yes
Pennsylvania	42 Pa.C.S. § 9763(b)	No
Tennessee	T.C.A. § 40-35-303(d)	No
Vermont	28 V.S.A. § 252(b)	No
Virginia	§ 19.2-306.1	Yes
Washington	RCW 9.41.045	Yes
Wisconsin	Wis. Admin. Code DOC 328.04(3)(m)	Yes

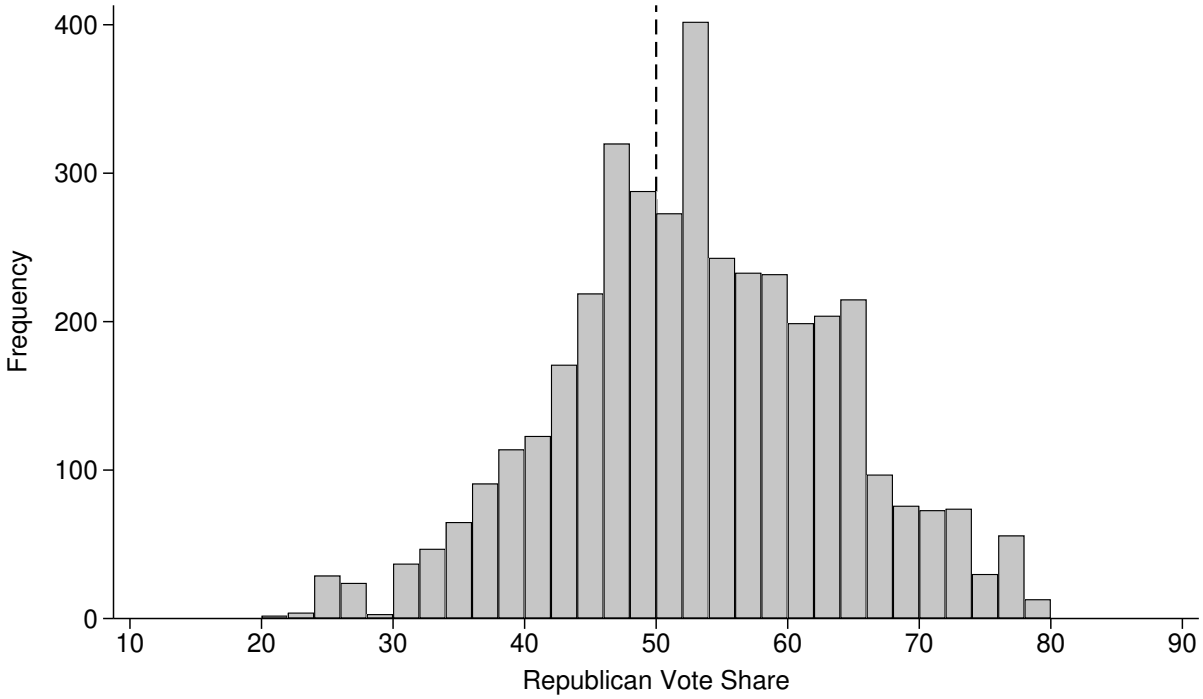
Notes: This table lists the states whose criminal statutes, as of 2025, explicitly lay out that probationers generally may be barred from possessing firearms while under supervision. Column (2) gives the section of the state code containing the rule, and Column (3) denotes whether the restriction is phrased as an optional condition the court must impose ("No") or as a baseline condition that applies by default ("Yes").

Figure A1: Study Representativeness in Terms of Population Covered



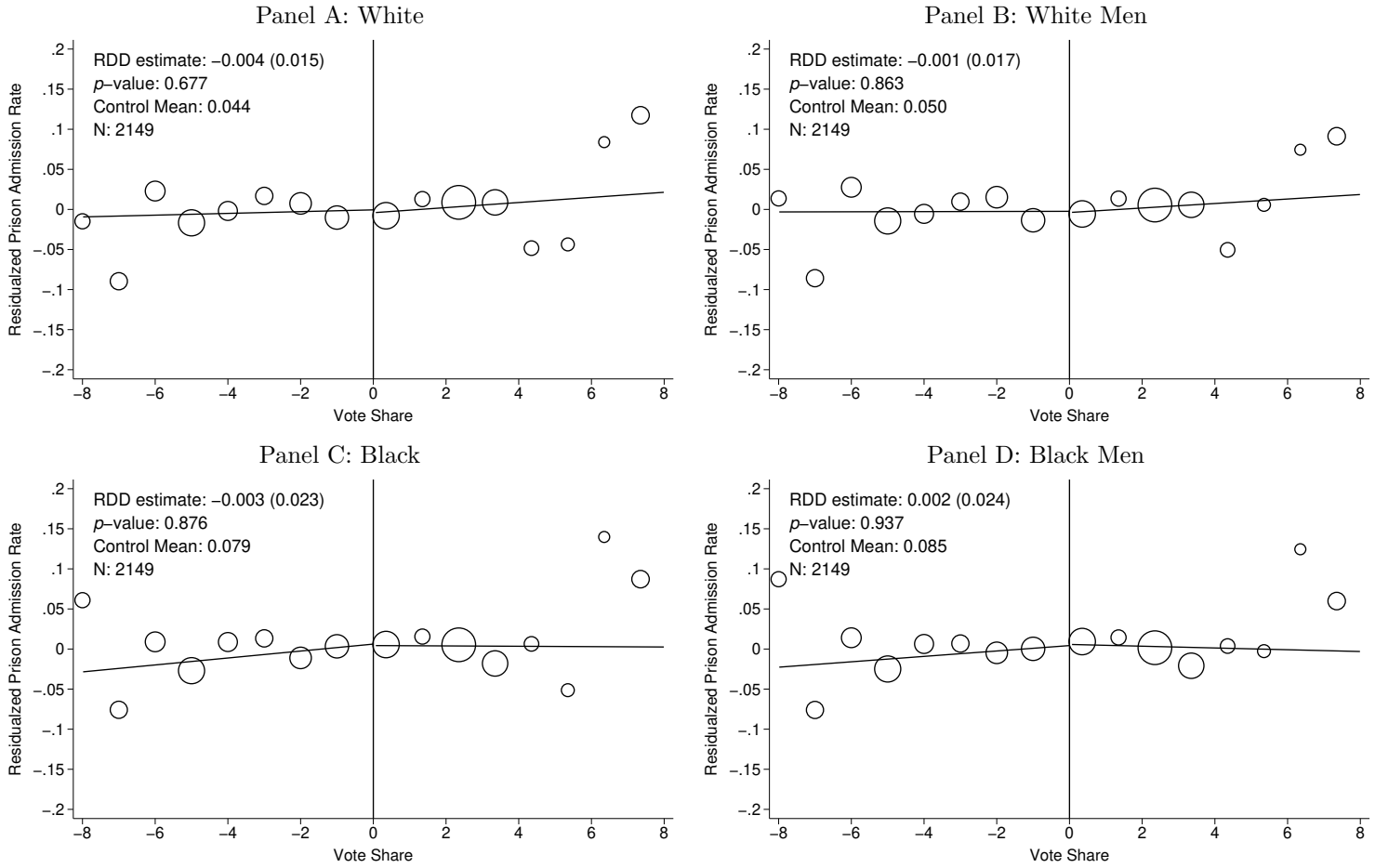
Notes: This figure visualizes the comparative size of the population covered by the current study with that of recent existing studies in criminal justice. To do so, we gathered the census of all papers published associated with the K4 JEL code (Legal Procedure, the Legal System, and Illegal Behavior) in the top two largest volume publication journals by the American Economic Association, the American Economic Review and the American Economic Association Papers and Proceedings, for the past three complete years, 2023-2025. In total, 14 papers were published associated with the K4 JEL code across the journals and years, and we gathered information on the population (as of 2010) covered by or representative of each study. We combine these with the population coverage of our three main outcomes — mortality, incarceration, and conviction — for a total of 17 data points. Comparing our covered population size to these papers, we observe that both our mortality outcomes and our incapacitation outcomes are among the top five largest population covering analyses, followed by one other study, and then our conviction results. Compared to the median population coverage of the census of the studies (8.5 million), our mortality and incapacitation results are based on eight times as much population covered, while our conviction estimates are based on four times as much population covered.

Figure A2: Distribution of Republican Vote Share, 2010 to 2019



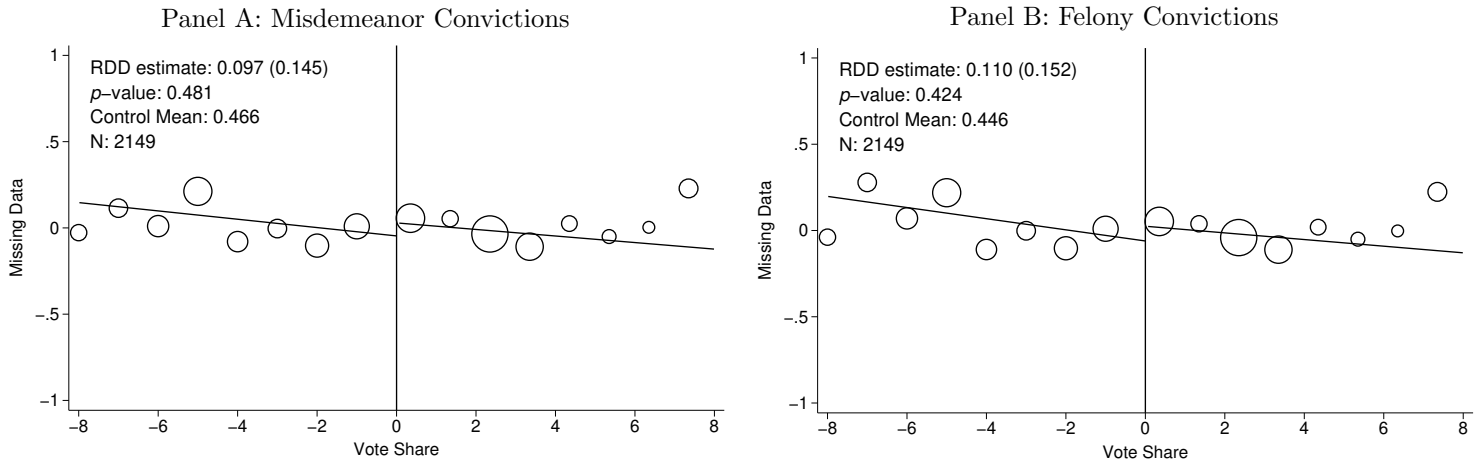
Notes: This figure visualizes the distribution of the Republican vote share variable in our sample. Bins are two percentage points wide.

Figure A3: Balance in Prison Admissions Data Availability Across the 50% Vote Share Threshold



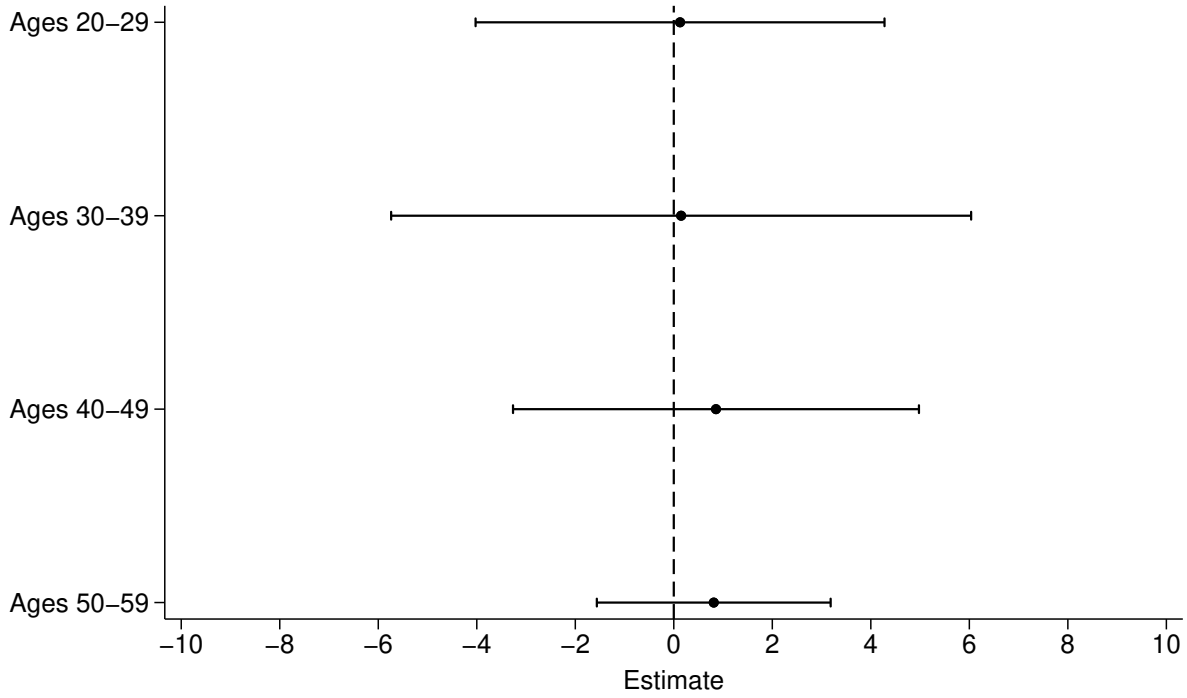
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on whether the given county-by-year observation has available prison admission data. See Section I.4 for more information on the prison admission data. Each panel examines availability for a different demographic group. 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 A4: Balance in Criminal Conviction Data Availability Across the 50% Vote Share Threshold



Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on whether the given county-by-year observation has available criminal conviction data. See Section I.5 for more information on the criminal conviction data. Each panel examines availability for a different type of conviction. 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.

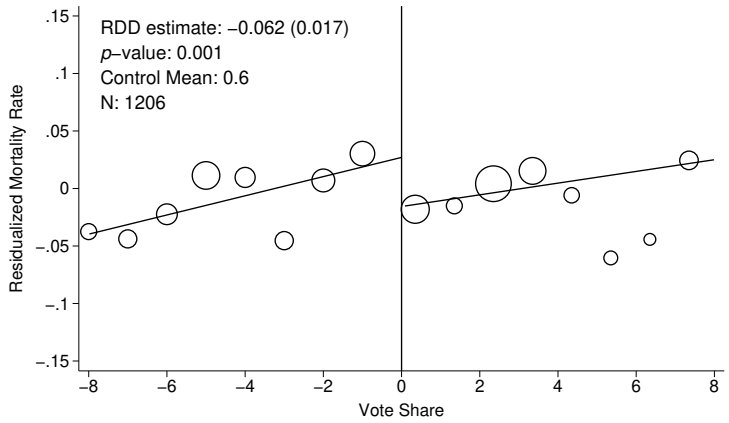
Figure A5: Effects of Narrow Republican Prosecutor Election on Fentanyl Overdose Mortality



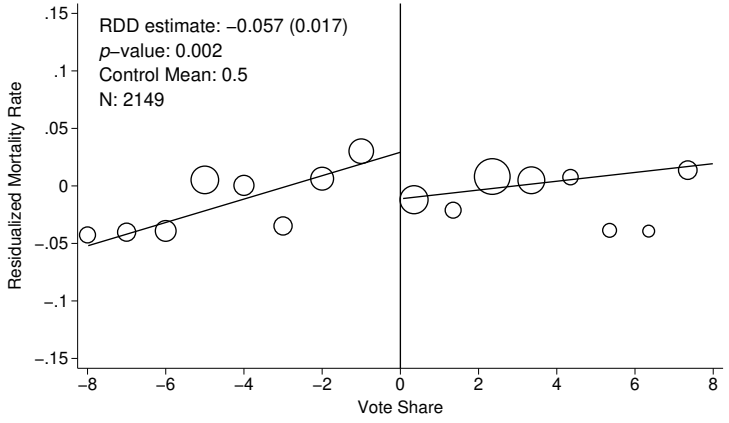
Notes: This coefficient plot conveys RDD estimates of the effect of narrow election of a Republican local prosecutor on mortality due to fentanyl overdoses across ten year age groups. Each coefficient and corresponding conventional 95% confidence intervals comes from a separate regression estimated using 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 A6: Effects of Narrow Republican Prosecutor Election on The Fraction of All Suicides Committed Using A Firearm

Panel A: Drop Observations with Zero Suicides



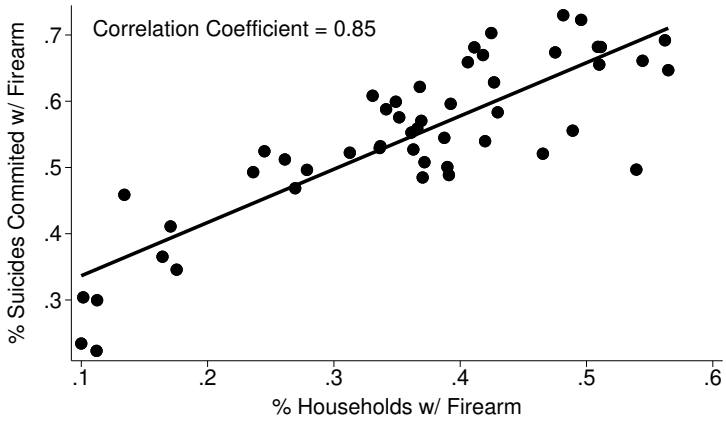
Panel B: Code Observations with Zero Suicides as Zero



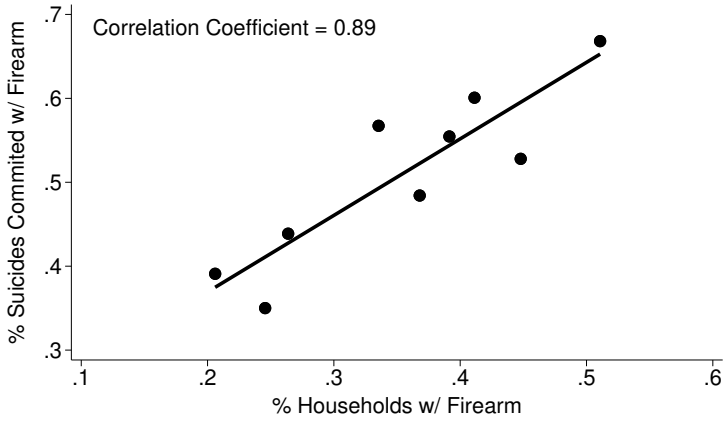
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on the fraction of suicides committed that involve a firearm. Panel A drops county-by-year observations with zero suicides, and Panel B includes these observations as zero 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 A7: Correlation Between Household Firearm Possession and the Fraction of Suicides Committed with a Firearm

Panel A: Behavioral Risk Factor Surveillance System, State Level

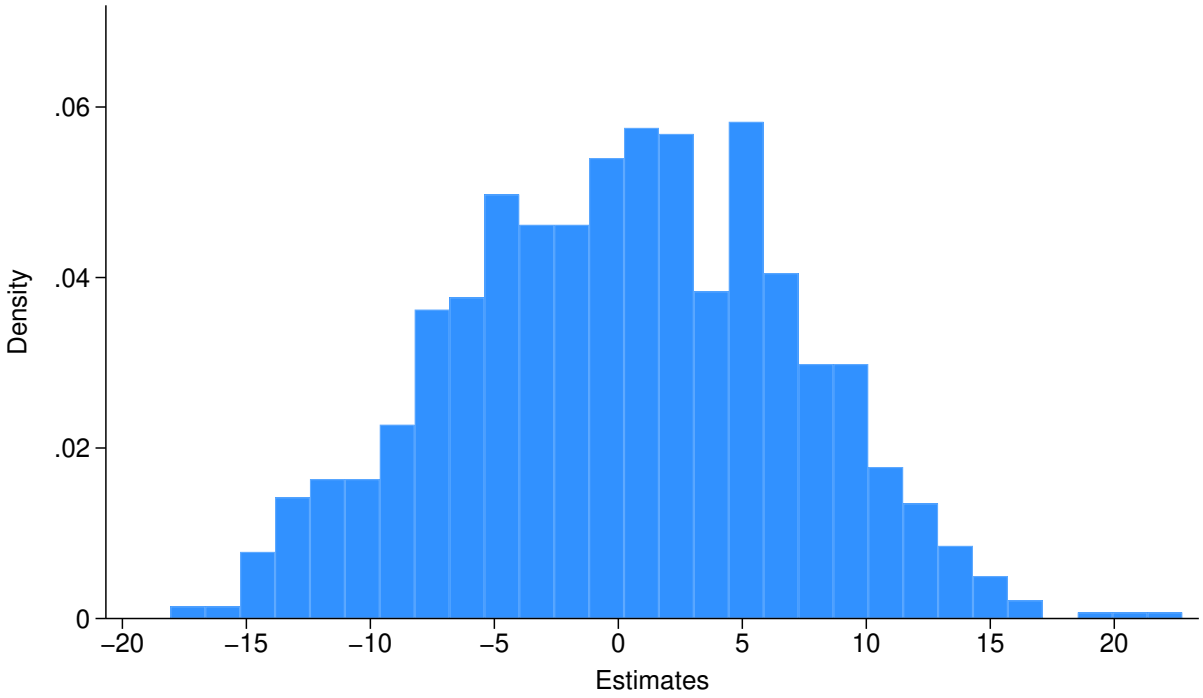


Panel B: General Social Survey, Census Division Level



Notes: Each figure plots the raw correlation between the fraction of households in a geographic area that possess a firearm and the fraction of suicides committed with a firearm. Panel A uses the 2001, 2002, and 2004 waves of the Behavioral Risk Factor Surveillance System (BRFSS) to estimate gun possession at the state level. These are the only waves that include the household firearm question in the core survey. Panel B uses nine waves of the General Social Survey (GSS) (2000, 2002, ..., 2016) to estimate gun possession at the census division level.

Figure A8: Distribution of Placebo Heterogeneity Estimates Across Random State Splits



Notes: This figure shows the distribution of differences in estimated effects across 1,000 random splits of states into two equal-sized groups. For each iteration, we randomly assign in-sample states to one of two equally-sized groups and estimate the effect of narrow Republican prosecutor election on firearm mortality separately for each group. The histogram displays the difference between the two group-specific estimates. The 1st and 99th percentile values are -14.25 and 14.63, respectively.

Appendix B Mortality Data and Trends

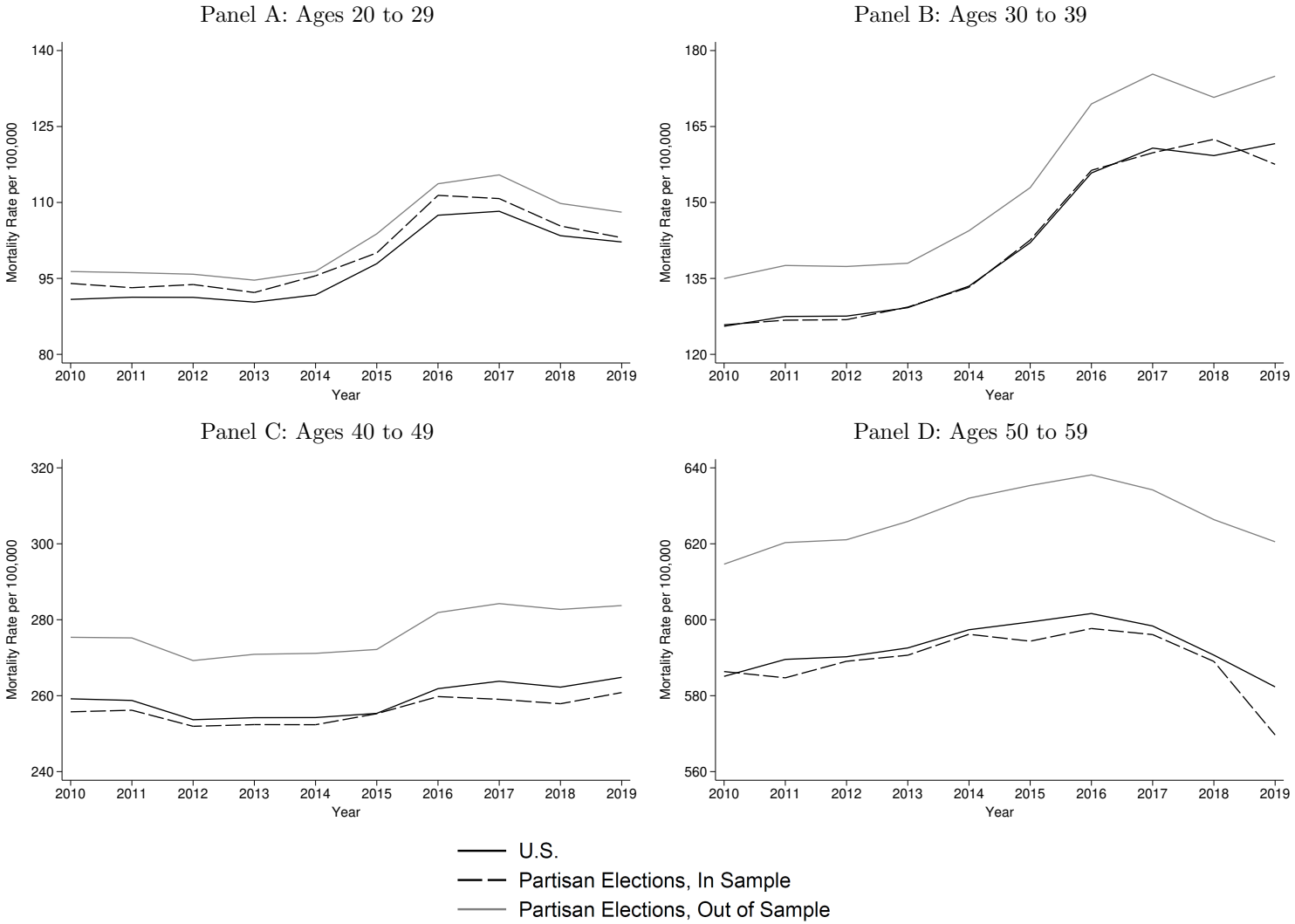
The list below gives the ICD-10 codes used to identify each cause of death in our analysis.

- 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: W32-W34, X72-X74, X93-95, Y22-Y24, Y35.0
- Firearm Homicides: 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 rose in the beginning of the 2010s and was then overshadowed by the explosion of fentanyl halfway through the decade.[39] 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.

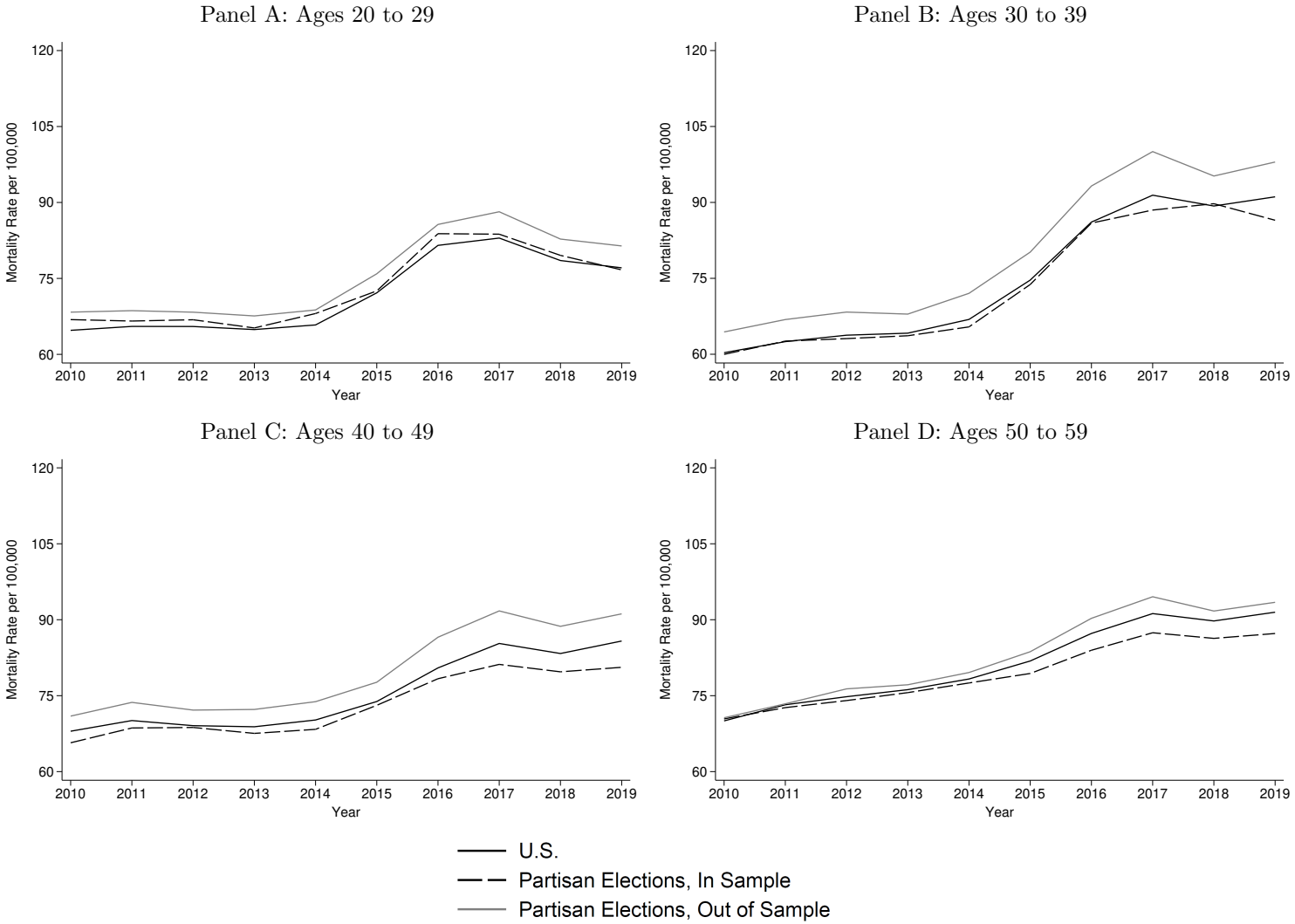
[39]More specifically, the opioid crisis is known to be concentrated among White middle-aged Americans without a college degree (Case and Deaton, 2015, 2017). However, the most recent trends in opioid mortality are different—the crisis is now spreading to younger and non-White Americans (Gibbons et al.,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

Figure B1: Trends in Mortality from All Causes



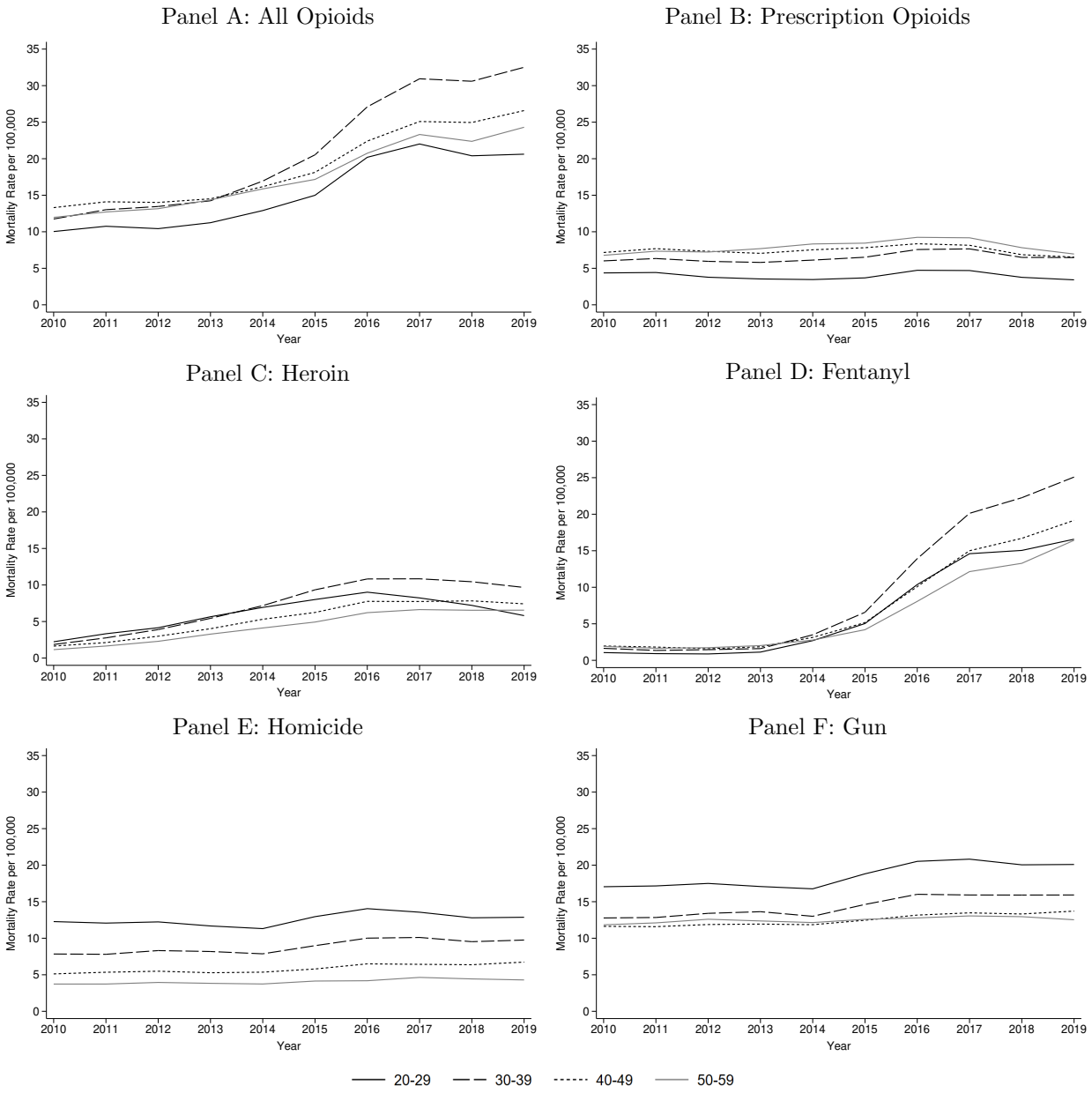
Notes: These figures show trends in all-cause mortality from 2010 to 2019. Each panel focuses on a different age group. Separate trend lines are shown for the U.S. as a whole (black solid line), for counties where the Republican vote share from the most recent prosecutor election fell between 42% and 58% in at least one year from 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 from 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 shows 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 Association Between Local Prosecutor and Other Local Office Vote Shares

C.1 Theory

One possible concern with our empirical strategy is that the Republican prosecutorial vote share may be sufficiently correlated with the vote share for Republicans in other local offices that the 50% prosecutorial cutoff also picks up discontinuous changes in the partisan affiliation of other officeholders. We first address this concern theoretically. Under weak assumptions in a continuity-based RDD framework, we show that the prosecutorial cutoff generates no discontinuity in the probability of a Republican winning another office, even in the presence of extremely strong (but imperfect) correlation between vote shares.

To illustrate, let X_d denote the Republican prosecutorial vote share in district d, and suppose X_d is continuously distributed with a density function $f_X(x)$ which is non-zero in a window around $x = 50$. Let $Y_d = X_d + \varepsilon_d$ denote the Republican vote share for a different local office, where ε_d has a continuous CDF, $Var[\varepsilon_d] = \sigma_\varepsilon^2 > 0$, and the joint density function $f_{X,\varepsilon}(x, e)$ exists and is continuous. This, in an agnostic way, models Y_d as the prosecutor vote share plus some noise, and σ_ε^2 determines the strength of the correlation between X_d and Y_d (smaller = more strongly correlated).

Let τ denote the RDD estimand for the discontinuity in the probability of a Republican winning the other office at the prosecutorial cutoff:

$$\tau = \lim_{x \to 50+} P(Y_d \geq 50 | X_d = x) - \lim_{x \to 50-} P(Y_d \geq 50 | X_d = x)$$

In this setup, it can quickly be shown that τ is exactly zero. To see why, let $F_{\varepsilon|X}(e|x)$ denote the conditional CDF of ε_d given $X_d = x$, and notice that:

$$P(Y_d \geq 50 | X_d = x) = P(\varepsilon_d \geq 50 - x | X_d = x) = 1 - P(\varepsilon_d \leq 50 - x | X_d = x)$$

$$= 1 - F_{\varepsilon|X}(50 - x|x)$$

Plugging this result back into the equation for τ, we have:

$$\tau = \lim_{x \to 50+} [1 - F_{\varepsilon|X}(50 - x|x)] - \lim_{x \to 50-} [1 - F_{\varepsilon|X}(50 - x|x)]$$

$$= \lim_{x \to 50-} F_{\varepsilon|X}(50 - x|x) - \lim_{x \to 50+} F_{\varepsilon|X}(50 - x|x)$$

Thus, $\tau = 0$ when $F_{\varepsilon|X}(50 - x|x)$ is continuous in x at $x = 50$. By definition, notice that:

$$F_{\varepsilon|X}(50 - x|x) = \int_{-\infty}^{50-x} \frac{f_{X,\varepsilon}(x, e)}{f_X(x)} de$$

Since $f_{X,\varepsilon}(x, e)$ is continuous by assumption and $f_X(x)$ is continuous and non-zero around $x = 50$, $F_{\varepsilon|X}(e|x)$ is a composition of continuous functions and is thus continuous in x at $x = 50$. Therefore $\tau = 0$.

Two features of this result are worth noting. First, it does not require that X_d and ε_d be independent; i.e., the noise term may be arbitrarily correlated with the prosecutorial vote share, as long as the conditional distribution of the noise term does not discontinuously change across the prosecutor vote share threshold. Such a discontinuity would be a violation of the standard continuity-based RDD assumption that latent characteristics vary smoothly across the threshold, and Table 2 strongly supports this assumption. Second, the result does not hinge on σ_ε^2 being sufficiently big; as long as $\sigma_\varepsilon^2 > 0$, the imperfect correlation in vote shares leads to no discontinuity in the probability of winning the other office.

The intuition for this result is also straightforward. A continuity-based RDD compares observations asymptotically close to the prosecutorial cutoff such that, in the limit, their values of X_d are the same. Therefore, whether Y_d falls above or below 50 for these observations is determined entirely by the noise term ε_d, which varies continuously through the cutoff.

C.2 Empirics

The theoretical result above establishes the prior belief that, under the standard RDD assumptions already invoked in this paper, correlated vote shares across offices are not a concern. Nevertheless, one might still worry that in a finite sample with limited observations arbitrarily close to cutoff and extreme correlation in vote share variables, this could be a concern. We therefore directly show that in elections with closely contested prosecutor races, the Republican prosecutor vote share is very weakly correlated with vote shares for Republicans in other races.

We begin with the correlation between the Republican prosecutor vote share and the Republican presidential vote share. While not a local office, the presidential race is consistently on the ballot (and contested) nationwide in presidential election years, making it a solid choice to get a sense of how often voters may be split ticketing in times/places with closely contested prosecutor elections. Figure C1 plots, at the prosecutorial district level, the Republican presidential vote share against its Republican prosecutor vote share in the presidential election years in our sample. Here we see a very weak positive correlation, with an R^2 in the bivariate regression of only 0.0175. This result is consistent with recent work on the prevalence of split ticketing in local elections (Kuriwaki, 2026) and running counter to concerns that vote shares are highly correlated.

We next consider other local offices. Some cannot be analyzed because their jurisdictional boundaries do not align with prosecutorial districts. For example, mayors serve cities and municipalities, while prosecutorial districts are typically defined at the county level or span multiple counties.[40] Many judicial offices similarly do not align with prosecutorial district boundaries, and only five states elect all of their judges in partisan elections.

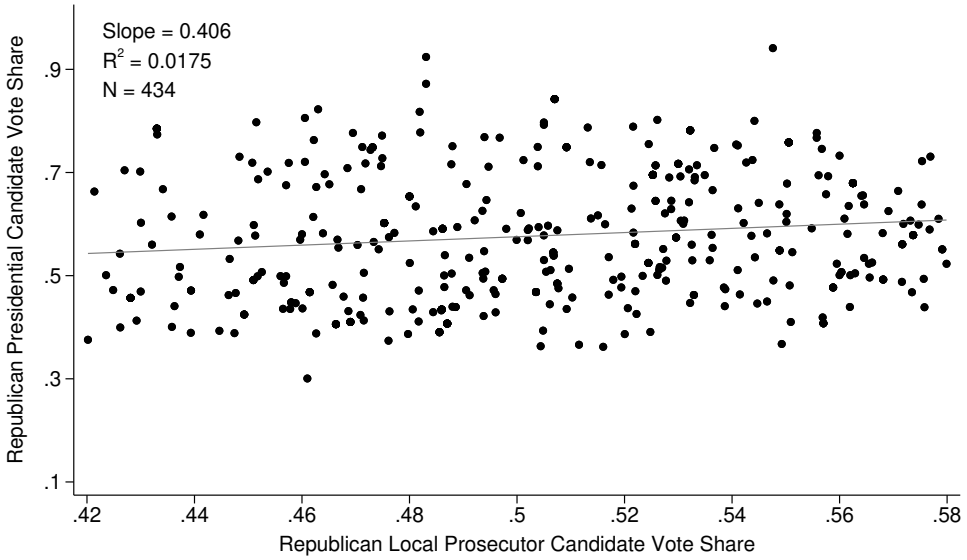
The office best suited for this analysis is sheriff. Sheriffs are county-level officials, meaning we can map single-county prosecutorial districts to their sheriffs. Sheriffs are also the

[40]The partisan affiliation of mayors has also been shown to have no effect on police spending, arrests, and crime (de Benedictis-Kessner et al., 2025).

county officials with the most overlap with criminal justice. 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 C2. As with the Republican presidential vote share, the Republican sheriff vote share is very weakly correlated with the Republican prosecutor vote share in these closely contested elections. More directly to this point, we estimate an RDD using an indicator for whether a Republican sheriff is in office in a given county and year as an outcome. This estimate is visualized in Panel B of Figure C2.[41] The estimate is small and statistically insignificant, conveying that the party affiliation of sheriffs is not confounding our estimates. These results also imply that our main effects are not explained by broad shifts in electoral preferences across the board, in which case we would expect prosecutor and sheriff election results to be strongly correlated.

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

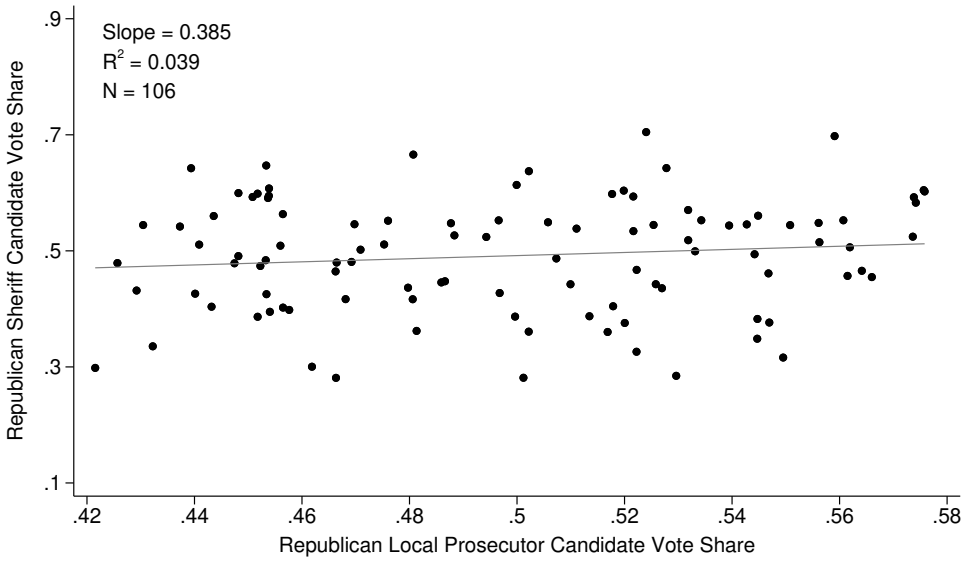
Figure C1: Association Between Republican Local Prosecutor and Presidential Candidate Vote Shares



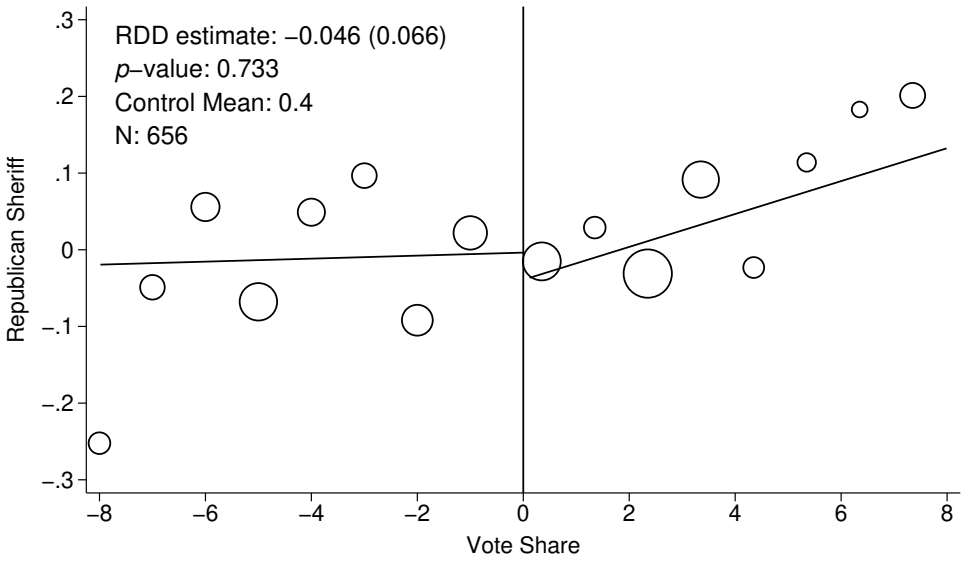
Notes: This figure visualizes the relationship between vote shares for Republican local prosecutors and presidential candidates at the prosecutorial district level.

Figure C2: Association Between Republican Local Prosecutor and Sheriff Candidate Vote Shares

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 of 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. 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 are 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 olds remain precisely estimated and of a similar magnitude with these counties excluded. Last, Column 6 shows that, after accounting for multiple hypothesis testing across the four age groups, our preferred specification estimates for 20-29 year olds are highly statistically significant while all others are statistically indistinguishable from zero.

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 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) among men by age group. None of the estimates are statistically distinguishable 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-value	0.2529	0.1052	0.0270	0.1703	0.0083	0.0007
q-value	0.2529	0.4208	0.1080	0.6812	0.0332	0.0028
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-value	0.2377	0.2643	0.0987	0.7511	0.8128	0.5913
q-value	0.2529	0.4940	0.1974	0.7511	0.8128	0.7884
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-value	0.0970	0.3705	0.2216	0.6791	0.1544	0.8995
q-value	0.2529	0.4940	0.2955	0.7511	0.3088	0.8995
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-value	0.2043	0.8693	0.8527	0.4078	0.2963	0.0791
q-value	0.2529	0.8693	0.8527	0.7511	0.3951	0.1582
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 include 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 (6) 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-value	0.4529	0.2475	0.1695	0.4384	0.0002	0.0001
q-value	0.5415	0.7966	0.6477	0.8768	0.0008	0.0004
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-value	0.5415	0.6504	0.4858	0.0881	0.2610	0.2744
q-value	0.5415	0.7966	0.6477	0.3524	0.3480	0.3659
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-value	0.2378	0.6013	0.4740	0.9042	0.9119	0.6326
q-value	0.5226	0.7966	0.6477	0.9042	0.9119	0.6326
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-value	0.2613	0.7966	0.7572	0.7013	0.2424	0.0976
q-value	0.5226	0.7966	0.7572	0.9042	0.3480	0.1952
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 include 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 (6) 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	-10.13***	0.21
	(3.77)	(3.39)
p-value	0.01	0.95
N	2149	1556
Firearms	-6.72***	-1.64
	(2.45)	(1.54)
p-value	0.01	0.29
N	2149	1556

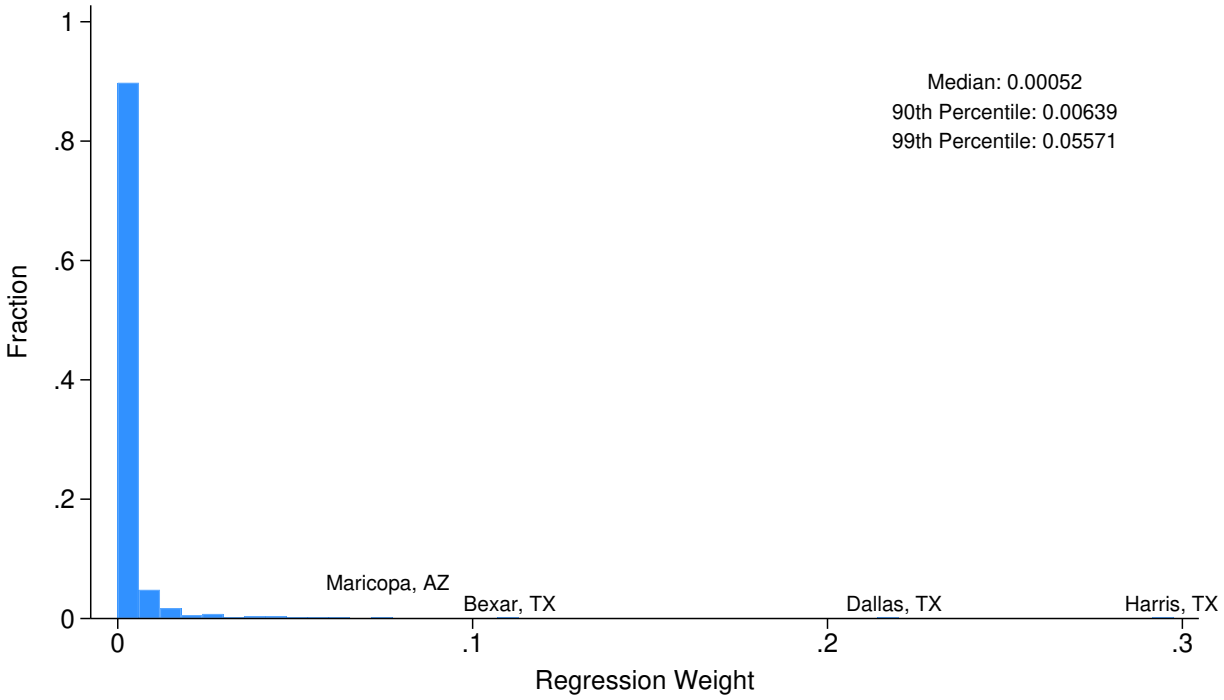
Notes: This table presents estimates obtained by performing the placebo exercise described in Section D. For counties that have a closely contested election during our time frame, we assign their closely contested vote share value to county-by-year observations *prior* to the closely contested election, 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

	(1)	(2)	(3)	(4)
	Men	Men	Men	Men
	20-29 Years Old	30-39 Years Old	40-49 Years Old	50-59 Years Old
	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
q-value	0.744	0.744	0.556	0.556
Control Mean	29.7	79.6	232.0	690.0
N	2149	2149	2149	2149

Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates from internal causes. 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. q-values are Benjamini-Hochberg adjusted p-values that control the false discovery rate across the four age groups. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

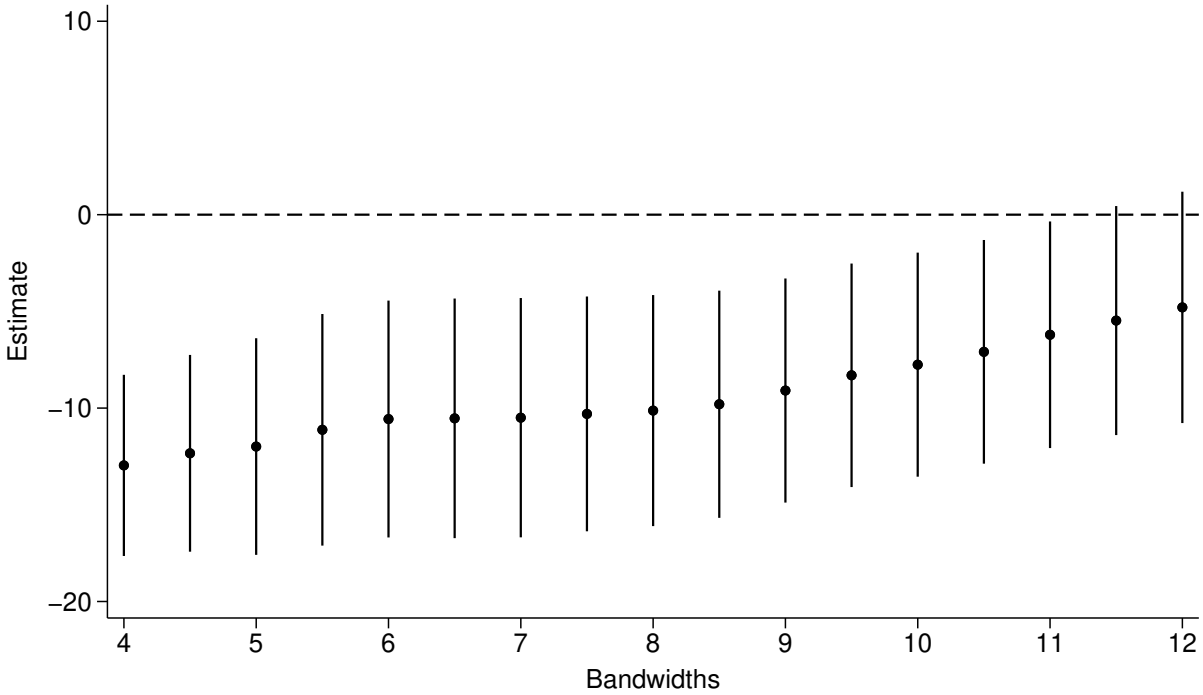
Figure D1: Distribution of Regression Weights Across Counties



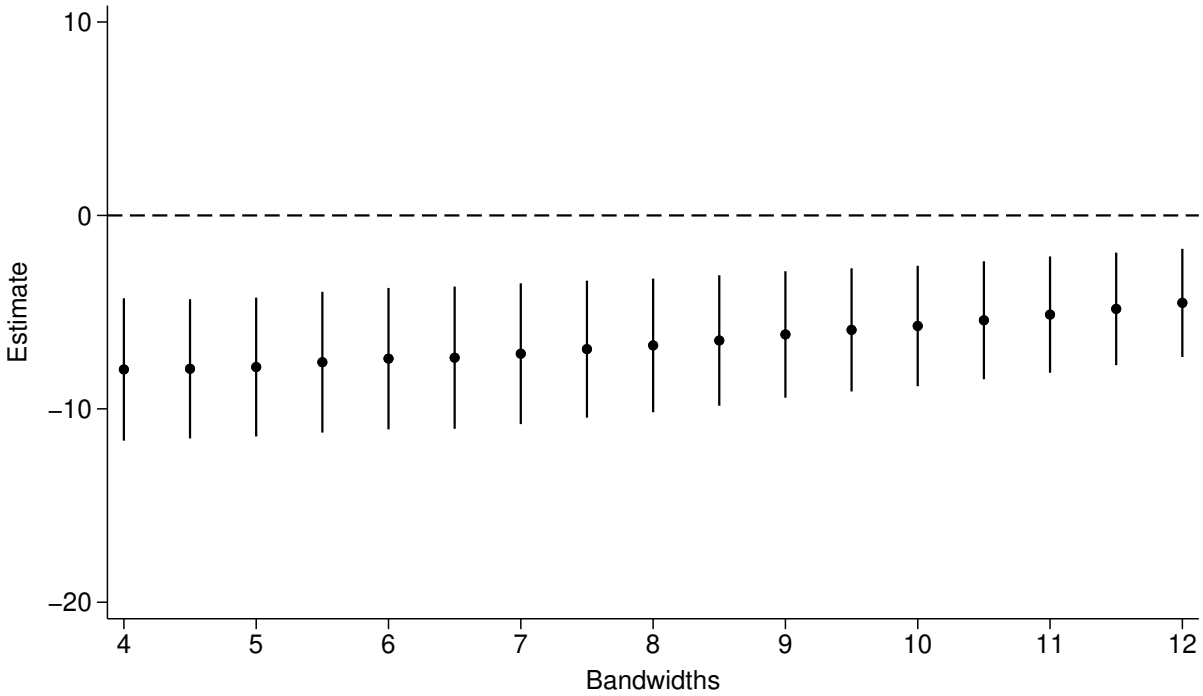
Notes: This figure plots the distributions of regression weights assigned to counties in our regression analysis. These weights correspond to the ones used to estimate effects on all-cause mortality among 20-29 years olds. This figure aggregates weights across observations from the same county to the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

Figure D2: Varying Bandwidths Used in Estimation

Panel A: Men Ages 20 to 29, External Causes



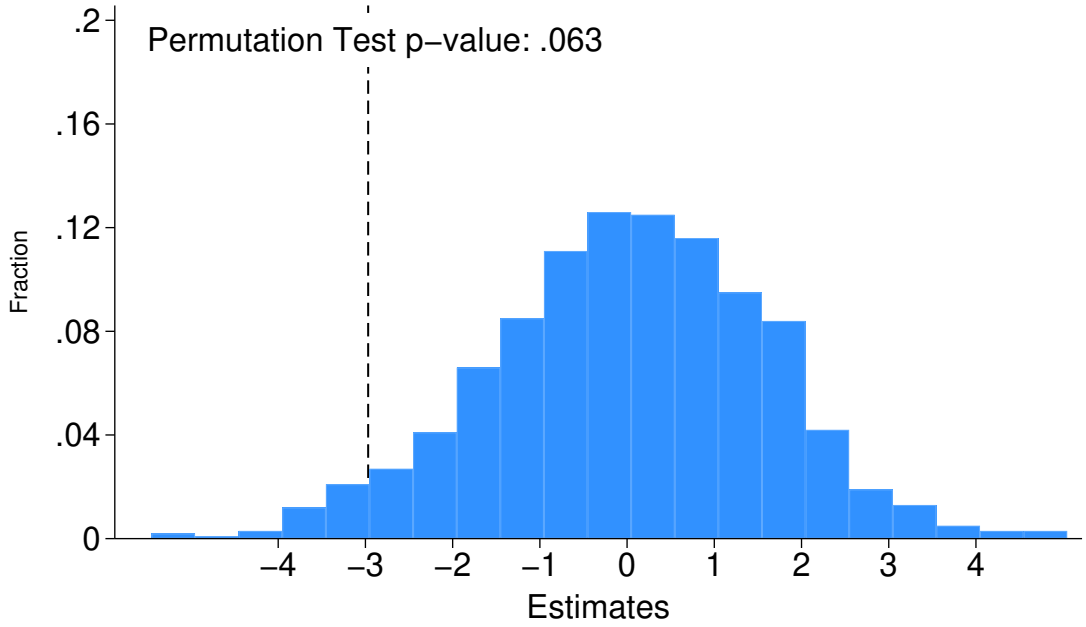
Panel B: Men Ages 20 to 29, Firearms



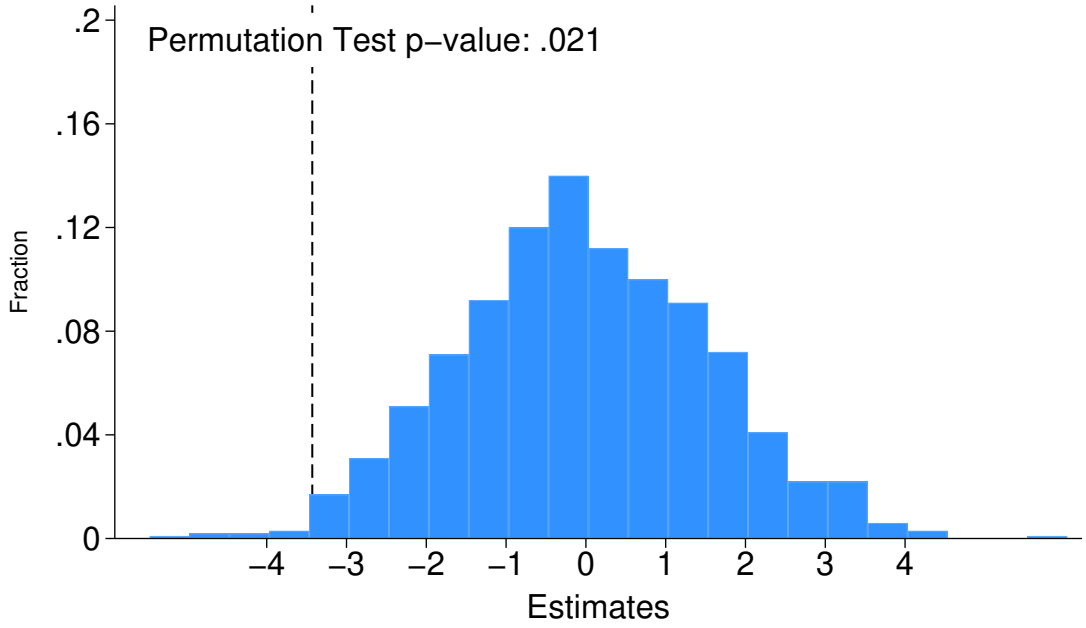
Notes: These figures plot the point estimates and conventional 95 percent confidence intervals from separate RDD regressions on the same outcome that vary the bandwidth by 0.5 percentage point at a time.

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 Additional Mechanisms Results

This section contains additional mechanisms results. Figure E1 shows no effect on prison admissions among all men (Panel A) or women (Panel B) ages 15 to 64. Figure E2 shows that the prison effects remain similar when prison populations are analyzed instead of prison admissions. Figure E3 shows effects on jail admissions and populations. Figure E4 shows effects on the number of charges filed and on the success rate of filed convictions. Figure E5 breaks down the conviction estimates by severity and crime type. Tables E1 and E2 show that mortality effects are still detectable among young Black men in the samples of counties used in the prison admission and conviction analyses, respectively.

Table E1: Mortality Effects Among Young Black Men Using Observations with Available Prison Data

	(1) All External Causes	(2) Firearm Deaths	(3) Firearm Homicides
	-26.6***	-24.9***	-22.9***
	(7.2)	(5.8)	(5.2)
p-value	0.001	0.001	0.001
Control Mean	169.0	104.1	88.5
N	1038	1038	1038

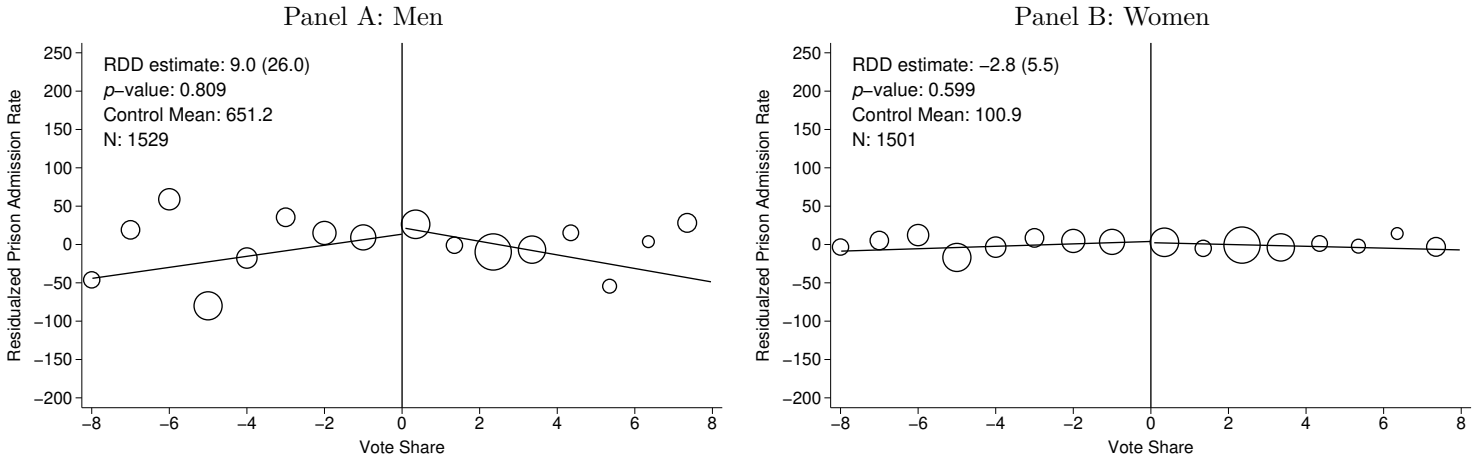
Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates among Black men ages 20 to 29. Each regression uses only county-by-year observations with available prison data. Each column examines effects among a specific cause of death. 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 E2: Mortality Effects Among Young Black Men Using Observations with Available Conviction Data

	(1) All External Causes	(2) Firearm Deaths	(3) Firearm Homicides
	-11.9**	-7.9*	-9.0**
	(5.2)	(4.4)	(4.0)
p-value	0.0156	0.0721	0.0410
Control Mean	131.2	78.5	66.8
N	220	220	220

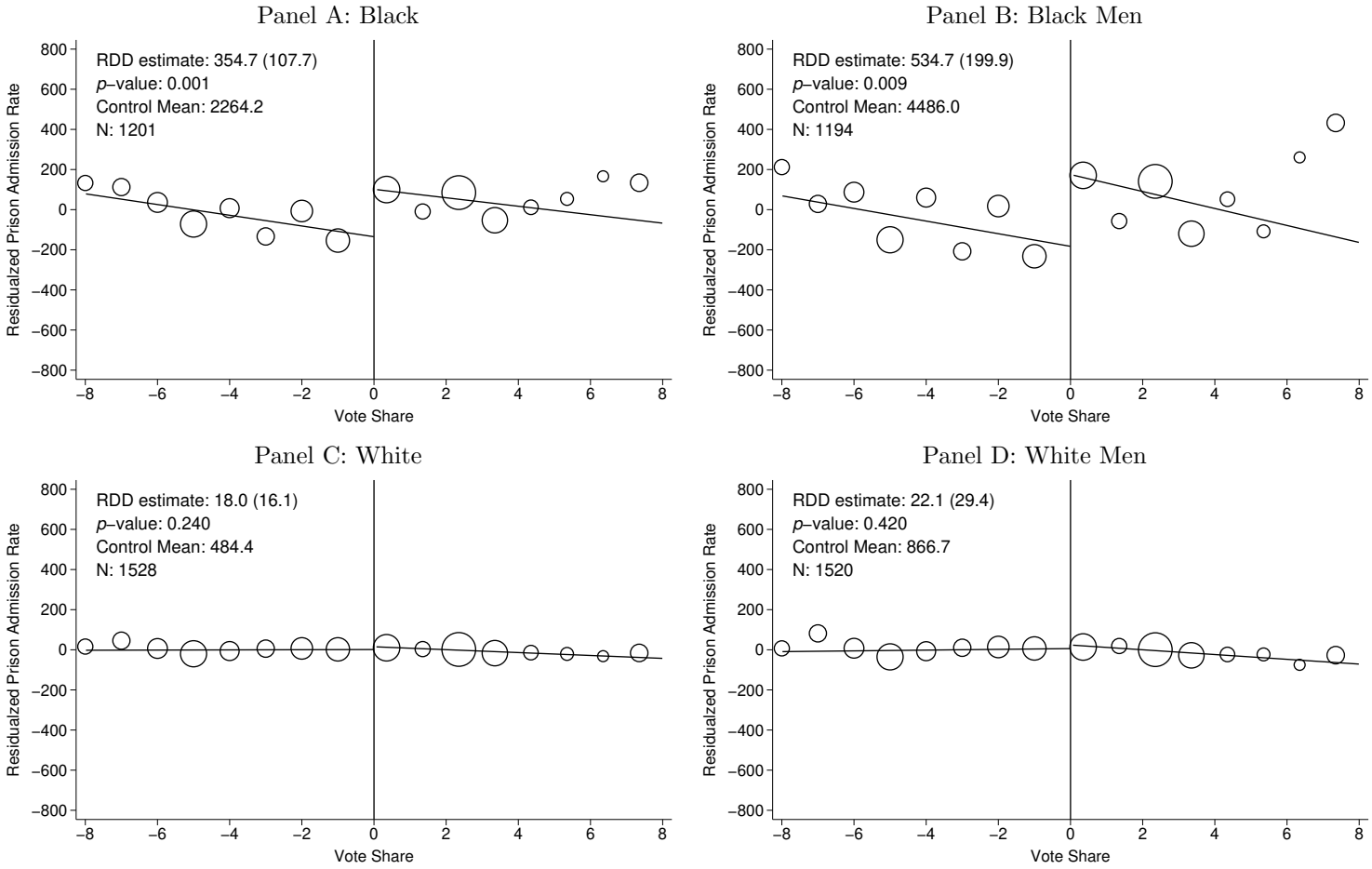
Notes: This table presents RDD estimates of the effect of narrow Republican prosecutor election on annual county-level mortality rates among Black men ages 20 to 29. Each regression uses only county-by-year observations with available conviction data. Each column examines effects among a specific cause of death. 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 E1: Effects of Narrow Republican Prosecutor Election on Prison Admission Rates, By Sex



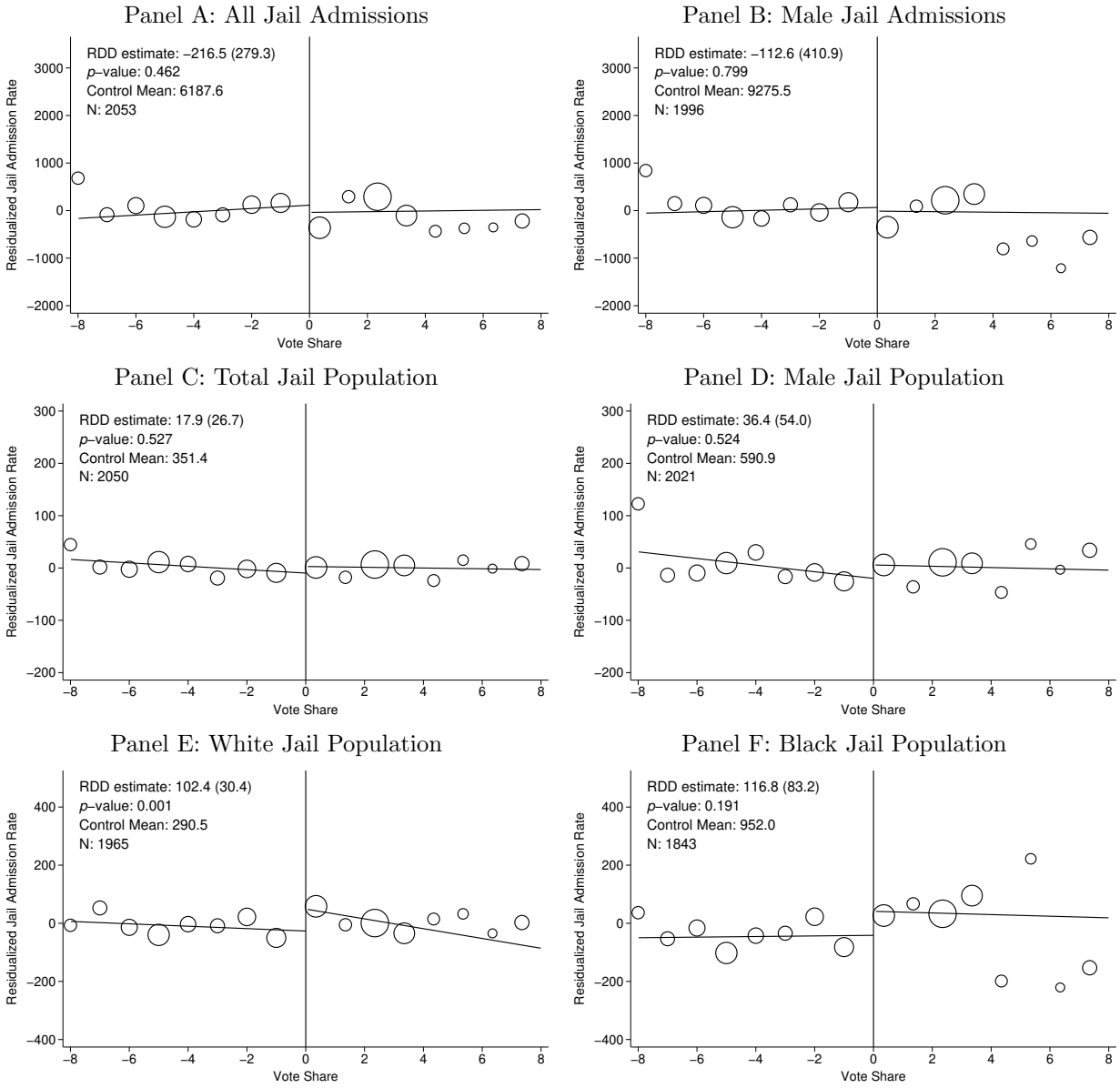
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on prison admission rates among different demographic groups. The information in the corner of each panel comes from estimation of Equation 1 and each plot visualizes RDD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

Figure E2: Effects of Narrow Republican Prosecutor Election on Prison Population Rates



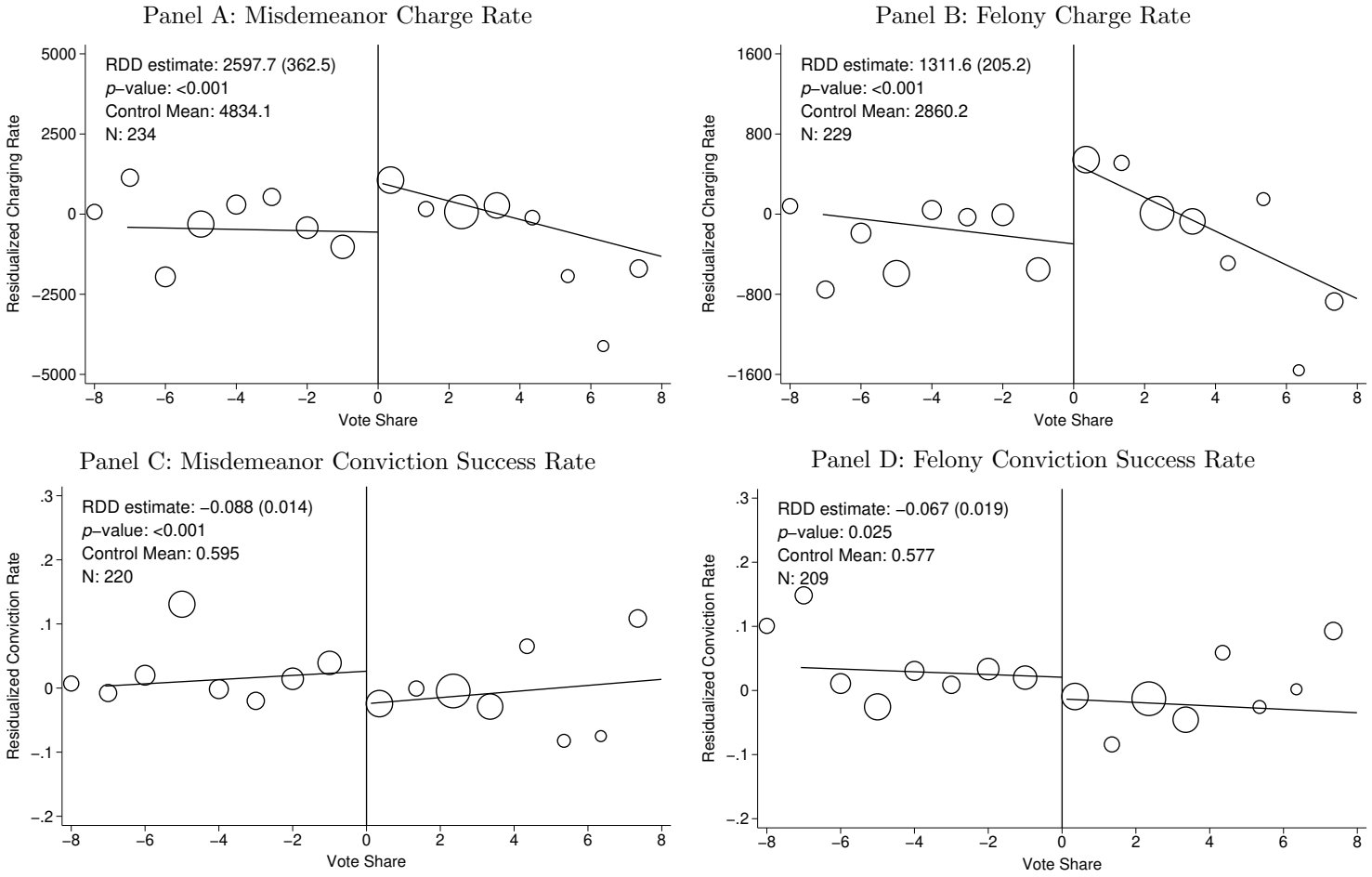
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on prison population rates among different demographic groups. The information in the corner of each panel comes from estimation of Equation 1 and each plot visualizes RDD estimates obtained by first residualizing the outcome 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 E3: Effect of Narrow Republican Prosecutor Election on Jail Admission Rates



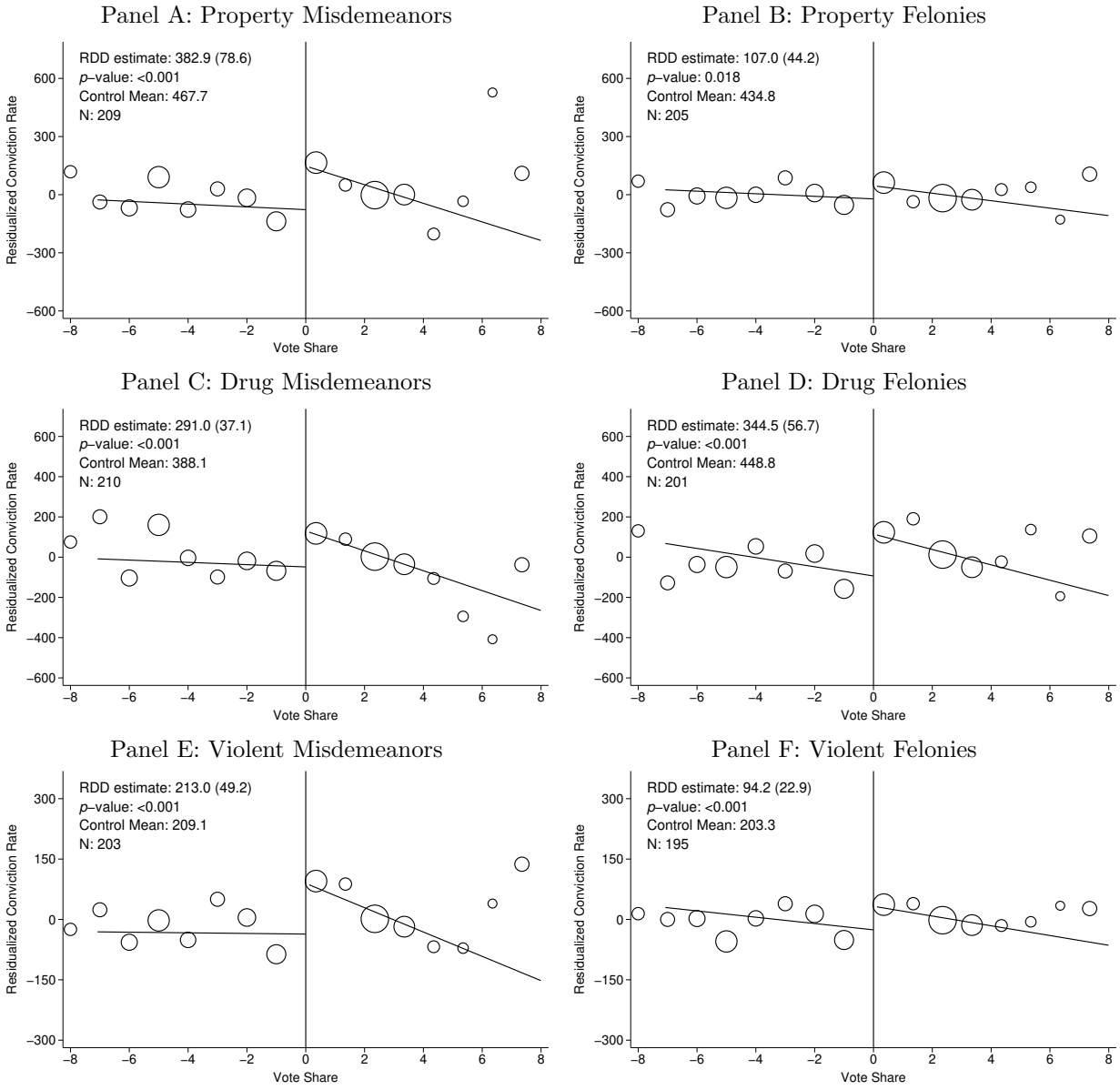
Notes: These figures visualize covariate-adjusted RDD estimates of the effect of narrow election of a Republican local prosecutor on jail admission and population rates among different demographic groups. The information in the corner of each panel comes from estimation of Equation 1 and each plot visualizes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

Figure E4: Effects of Narrow Republican Prosecutor Election on Criminal Charging and Success 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 criminal charges issued per 100,000 residents ages 25 to 40 (Panels A and B) and the fraction of criminal charges issued that result in conviction (Panels C and D). 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 E5: Effects of Narrow Republican Prosecutor Election on Criminal Conviction Rates, By Severity and Offense Type



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 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 F Back of the Envelope Calculations

F.1 Incarceration and Mortality

In this section, we estimate the extent to which the mortality reductions among young Black men caused by narrow election of a Republican prosecutor can be attributed to increased prison-based incapacitation. We account for two distinct channels through which incarceration affects community-level mortality: effects on the incarcerated individual's own mortality risk (the "direct channel") and effects on the mortality risk of potential victims in the community (the "indirect channel"). Since effects from both channels accumulate with time served, these calculations require assumptions about the length of prison stays induced by narrow election of a Republican prosecutor. Accordingly, we present estimates under two different assumptions: a conservative assumption that each induced prison stay lasts one year, and a more expansive assumption that each stay lasts four years. To facilitate these calculations, we consider a prosecutor elected to a four-year term and cumulate effects across the term.

We rely on several parameters from existing work. For the direct channel, we use the estimate from Norris et al. (2022) that incarceration reduces annual mortality risk by 0.55 percentage points (Table 4, Column 7). For the indirect channel, we estimate the likelihood that an incarcerated individual would commit a homicide if not incarcerated, which we obtain through several steps. First, Rose and Shem-Tov (2021) estimates that individuals on the margin of incarceration have a 9.15% violent reoffense rate within three years of release. We assume this translates to an annual violent offense rate of 3%. Second, based on FBI crime data, we assume 1.4% of violent offenses are homicides (Federal Bureau of Investigation, 2019). Third, since about roughly 50% of homicides go unsolved (Li and Lartey, 2022), we assume the true homicide rate is twice the homicide arrest rate. Combined, these assumptions imply an annual homicide rate of $(3)(0.014)(2) = 0.084$ percentage points per year for the

marginal incarcerated individual.[42] When combined, the direct and indirect channels imply that for every 1,000 individuals incarcerated for one year, approximately 6.34 deaths are prevented.

We combine this estimate with our prison admission findings. Panel D of Figure 5 shows that Republican prosecutors increase annual prison admissions among Black men ages 15 to 64 by 339.3 per 100,000. To estimate the admission rate specific to ages 20-29, we make the following adjustment: approximately 40% of individuals receiving felony sentences are ages 20-29 (Durose et al., 2009), and this age group comprises about 20% of the 15-64 population. Assuming our estimated increase is similarly concentrated, we calculate an admission rate of $(339.3)(0.40)(2) = 678.6$ per 100,000 among Black men ages 20-29 specifically.

We begin with the conservative assumption that each of these prison stays lasts a year. Then, across a four year term, these additional prison admissions prevent $(678.6)(4)(0.00634) \approx 17.21$ deaths per 100,000 individuals. 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. So, under conservative assumptions about prison stay length, mortality risk reductions among the incarcerated and any potential victims can only explain about 14% of the total mortality reduction.

We now switch to the more expansive assumption 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 induced 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. In this view, the cumulative mortality reduction due to incarceration becomes $(678.6)(4 + 3 + 2 + 1)(0.00634) \approx 43.02$ deaths per 100,000, explaining about 36% of the effect. Thus, we conclude that prison-based incapacitation explains roughly 15 to 36 percent of the mortality reduction among young Black men.

[42]Following recent evidence, we assume incarceration has no effect on post-release criminal behavior (Humphries et al., 2025; Kamat et al., 2024) nor morality risk (Norris et al., 2022).

F.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 Washington 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 convictions are violent offenses. Assuming violent misdemeanor defendants lack prior restrictions (otherwise they would likely face felony charges), approximately (0.73)*(0.10) = 7.3% of all misdemeanor convictions impose a legal firearm restriction.

We perform these calculations 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 individ-

uals.

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 operate 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 F.1 suggests that incarceration explains roughly 25% of the firearm mortality effect (we use the average of the lower and upper bound estimates). If the remaining $(0.75)(24.4) = 18.3$ fewer deaths are due to gun restrictions, then each restriction prevents $\frac{18.3}{1{,}332} \approx 0.0137$ firearm deaths. Equivalently, one firearm death is prevented for every 73 additional gun restrictions imposed.

Taken together, these calculations imply that conviction-based firearm restrictions could reasonably explain much of our mortality findings. It is especially encouraging that the race-specific calculates, despite relying on different estimates, are qualitatively similar. These estimates rest on several assumptions and should be treated as approximate, but the resulting magnitudes are plausible given that these restrictions target individuals with elevated firearm-violence risk.