Is the mortality gap between red and blue states caused by government?

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Abstract

In the US, age-adjusted mortality rates are higher in "red" states, i.e., states with high support for the Republican Party. We ask whether this is attributable to state-level policies as opposed to confounding variables such as culture. Using a variety of empirical approaches, we find that state government explains between 0 and 20% of the mortality gap. Scaling this by the size of the (large) gap, red state policies increase mortality risk by 0-3% relative to blue state policies.

1 Introduction

A recent literature (Bor, 2017; Goldman et al., 2019; Curtis et al., 2021; Warraich et al., 2022) documents that mortality is substantially higher in US counties and states which vote Republican in presidential elections—so-called "red" areas, as opposed to "blue" areas that vote for Democrats.[1] This literature documents correlations but does not make claims about causation.[2]

These findings have been widely publicized, often in ways implying causation. For instance, the lead story on the Washington Post website on October 5, 2023 was "How red-state politics are shaving years off American lives," with the accompanying article highlighting these correlations and attributing them to policies in areas like gun safety, public health, and criminal justice. In our personal interactions, we have found that a large fraction of educated laypeople are aware of this mortality difference, and most often

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[1]The literature also documents correlations with specific causes of death such as opioids and deaths from COVID-19 (Morris, 2021; Krieger et al., 2022).

[2]The most closely related causal claims in the literature are made by Millimet and Whitacre (2025), who study effects of elected officials' ideology on mortality in the US, but do not use this to explain the mortality gap between red and blue states.

interpret it as being due to policy. If the red-blue mortality gap is due to policy, this would also bolster the case that low life expectancy in the US compared with economic peer countries might be related to America's conservatism.

There is evidence that state-level policies affect mortality. For example, Miller, Johnson, and Wherry (2021) find that Medicaid expansion—a policy with greater Democratic support—reduced mortality. Montez et al. (2022) investigate 57 policies which vary at the state level and might affect life expectancy, and find that, conditional on controls, all but two policy positions favored by Democrats are associated with lower mortality. Millimet and Whitacre (2025) find that, in the US, mortality falls after the election of ideologically liberal candidates, and there is correlational evidence from other countries that egalitarian parties may decrease mortality (e.g., Navarro and Shi, 2001; Navarro et al., 2006; Bambra and Eikemo, 2009).

On the one hand, this gives reason to believe that a state government elected by Democratic voters could increase life expectancy. On the other hand, voters' partisan preferences are closely related to their culture. Because Republicans and Democrats differ in their diets, trust in medical advice, interest in guns, proximity to hospitals, and many other dimensions affecting mortality, we cannot assume that correlations between partisanship and mortality are caused by policy instead of by individual choices.

In this paper, our goal is to assess to what extent the red-blue mortality gap (i.e., the cross-sectional correlation between state partisanship and mortality) in recent years has been due to things done by state governments. That is, we estimate the causal contribution of state partisanship to mortality that occurs via state government. We define partisanship at the state level (since we are interested in state-level government) and mortality at the county level. We use several research designs.

As a baseline analysis, we implement regressions and find that a novel set of rich controls absorbs nearly 80% of the mortality gap. Yet we find evidence that this may still overstate the causal relationship. We therefore turn to quasi-experimental methods.

Our main two quasi-experimental designs exploit the fact that confounders likely operate through characteristics of the county itself (e.g., local culture) but the state government experienced in a county depends on the rest of the state. Of course, local characteristics might be correlated with the partisanship of nearby areas, but not in a way which necessarily corresponds with state boundaries. The goal is therefore to ask how local mortality is correlated with "coincidences" of how state boundaries are drawn, i.e., scenarios where the other places which a county is grouped with administratively are redder or bluer than the places it is near to in general.

We exploit two types of coincidences. First, there is a "distant coincidence": Borders of the state may have been drawn such that the far parts of the state are redder or bluer than the general region. For instance, southern Illinois has little cultural or physical connection with Chicago yet experiences bluer state government than other nearby places because (heavily Democratic) Chicago happens to have been included in Illinois instead of some other state. This translates into an instrumental variables approach where the source of variation is the partisanship of the far part of the state relative to the partisanship of the region. There are many ways one could implement the details of such a design and we obtain a preferred estimate using a data-driven procedure for aggregating specifications.

Second, there is a "nearby coincidence": Perhaps the county in question itself might have easily been placed in a different state instead. We implement this as a border county design, and once again aggregate specifications to obtain a preferred estimate.

Our final empirical approach uses a regression discontinuity (RD) design to study the effects of the partisan affiliation of the governor and legislative majorities on mortality. This answers a slightly different question from our main question, since voter partisanship (our main treatment) does not have a one-to-one mapping with partisan affiliation of elected officials, and may also affect policy stances of candidates conditional on party or policy implementation by other public officials. For these reasons, as well as the imprecision of the resulting estimates, we consider the RD approach the least informative.

Our results show that the mortality gaps between red and blue states are almost entirely due to factors other than state government. Our estimates suggest that the most likely scenario is that about 10% of the mortality gap is due to state government. Values between about 0% and 20% cannot be excluded, but we can confidently rule out values appreciably outside that range. This means that the correlation between partisanship and mortality is almost certainly not mostly due to state policy. We leave it to future research to uncover exactly what factors account for the remaining correlation, such as differences in diet or lifestyle.

Nonetheless, this does not mean that voters should assume that selecting Republicans and Democrats is equally beneficial to their health. Because the mortality gap is so large, the differences in mortality which are most likely attributable to policy are still quantitatively important. For instance, if 10% of the mortality gap is due to policy, then, holding constant the age distribution, the average state would experience nearly 1000 more deaths per year with typical red state government than with typical blue state government. Another way to interpret this is that it is an increase equal to 2% of baseline mortality risk. A third interpretation is that, applying a value of a statistical life of $7 million, the mortality reduction from blue state government as opposed to red state government is most likely worth about $1000 per resident per year—a number similar in magnitude to tax differences between red and blue states. In other words, the effects which are the most likely based on our results are still large enough to be an important consideration for voters.

Our paper is related to several literatures. The central contribution is to the literature documenting a mortality gap between red and blue states described at the start of the introduction. Our contribution to this literature is to use quasi-experimental methods to study to what extent the mortality gap is causally related to state government.

A paper investigating a closely related topic with attention to causation is Millimet and Whitacre (2025). They study effects of ideology of elected representatives on mortality in the US by correlating dynamic changes in these variables, and find a pattern suggesting that more liberal elected officials somewhat decrease mortality in subsequent years. Although they conceive of treatment differently from us—for them, treatment is the ideology of elected officials, while for us it is the partisan composition of the voters selecting the state government—their conclusions and ours are similar.

There are also some papers studying the relationship between governing party ideology and health internationally (e.g., Navarro and Shi, 2001; Navarro, Muntaner, et al., 2006; Bambra and Eikemo, 2009). These papers rely on regression with controls rather than quasi-experimental methods, and typically find that egalitarian political parties improve health.

Our paper is also related to a larger literature studying the causal impact of the partisanship of state government on state-level policy and outcomes. For instance, previous studies using regression discontinuity designs on gubernatorial elections have found that the partisanship of state leaders affects spending on education and healthcare (Beland

and Oloomi, 2017), tax rates (Besley and Case, 2003), and some labor market outcomes (Beland, 2015).

Lastly, our paper is related to a literature documenting that place of residence affects health (Finkelstein, Gentzkow, and Williams, 2016; Finkelstein, Gentzkow, and Williams, 2021; Bor et al., 2024). The current literature establishes that health varies across places in part because of causal effects of place on health. Our findings suggest that, while state-level policies likely matter, they might have limited explanatory power for these place effects. However, we cannot rule out that policy matters greatly on dimensions not systematically related to state partisanship.

The rest of the paper proceeds as follows. Section 2 describes our data sources. Section 3 reports baseline regression results and describes the endogeneity problem. Section 4 describes the far part research design and results. Section 5 describes the border county design and results. Section 6 describes the RD approach and results. Section 7 discusses and concludes.

2 Data

Our primary source of data is the Centers for Disease Control and Prevention's Wide-ranging ONline Data for Epidemiologic Research (CDC WONDER) database. We use their age-adjusted mortality rate (AAMR) data from 1999 to 2020 (Centers for Disease Control and Prevention, 2000–2020). AAMRs are weighted averages of age-specific death rates, designed to address the fact that places with many elderly people tend to have higher mortality rates due to their age composition. Specifically, the weights are equal to the fraction of Americans in the year 2000 who were within that age range, such that the resulting AAMR for a county is an estimate of what the mortality rate would be in that county if it had the same age composition as the US as a whole did in the year 2000. The unit of measurement is the number of deaths per 100,000 residents per year.

Note that the AAMR typically varies more across places than the crude (i.e., unadjusted) mortality rate. For instance, the AAMR in the highest-mortality states is nearly twice the AAMR in the lowest-mortality states. This is because, in places where mortality rates are high, fewer people survive to older ages; so, the population in these places tends to be younger. Age adjustment therefore typically increases the reported mortality rate for high-mortality places, and decreases it for low-mortality places.

We link these data with US presidential election data at the county level from the MIT (Massachusetts Institute of Technology) Election Data and Science Laboratory from 2000 to 2020 (MIT Election Data and Science Lab, 2020). Linkage was successful for 3135 of 3143 (99.7%) counties or county equivalents in the US. The missing counties or equivalents were based in Alaska, where some election data were unavailable. County and state partisanship are measured using the percentage of votes for the Republican candidate in the most recent presidential election. For example, if 46% of votes in a state were for George W. Bush (the Republican candidate) in November 2000, then that state's partisanship would take the value 46 for the years 2001–2004.

We use gubernatorial and state legislative election data between 1990 and 2020 to implement the RD design. For state legislatures, we use data from the National Conference of State Legislatures (NCSL), which maintains data on party control of state legislatures from 2009 to 2020 (National Conference of State Legislatures, 2025).

Covariates We use a number of covariates in our analysis. They are constructed as follows.

We use data from the 2010 US Census to include covariates measuring the proportion of Whites, Blacks, Asians, and Hispanics within a county. We measure urbanicity using the National Center for Health Statistics (NCHS) Urban-Rural Classification Scheme from 2013.

A key source of confounding in our context is culture, and specifically mortality-related aspects of culture. We construct three proxies for mortality-related aspects of culture in a county, each of which uses mortality rates in closely-connected counties which are not in the same state. The first proxy defines closely-connected counties using facebook friendships. We obtain the number of facebook friendships between people in any two counties using data from the Social Connectedness Index (SCI) of Bailey et al. (2018). Let $n^{fb}_{cc'}$ denote the number of facebook friendships where one person lives in county c and the other lives in county c', let $O(c)$ denote the set of all counties not in the same state as c, and let $LocalMortality_{ct}$ denote the AAMR in county c in year t. Then we construct a "facebook friend mortality" proxy for mortality risk as

$$fb_{ct} := \frac{\sum_{c' \in O(c)} n^{fb}_{cc'} LocalMortality_{c't}}{\sum_{c' \in O(c)} n^{fb}_{cc'}}.$$

That is, it is the weighted average of the AAMR in counties outside the same state as c, weighted by how many facebook friendships there are between that county and county c. The reason we exclude counties in the same state as c is to ensure that this variable is not affected by treatment.

Our other two proxies for culture define closely connected counties as counties which are connected through either in-migration or out-migration. These proxies are constructed exactly analogously to the facebook proxy, but replacing $n^{fb}_{cc'}$ with $n^{in}_{cc'}$, defined to be the number of people who moved from county c' to county c during the period 2016-2019, or with $n^{out}_{cc'}$, defined as the number of people who moved from c to c'. We will refer to these two measures as "sending county mortality" and "receiving county mortality," respectively. We obtain migration data using the US Census's County-to-County Migration Flows from 2016-2019 (US Census Bureau, 2016–2020).

We provide summary statistics for all covariates in Table 1.

3 OLS results and the endogeneity problem

We begin our empirical analysis by simply describing the correlation between county-level mortality and state-level partisanship, and the reasons why correlation might not be causation.

To characterize the correlation between county mortality and state partisanship, we estimate regressions of the form

$$LocalMortality_{ct} = \pi_0 + \pi_1 StatePart_{s(c)t} + X'_{ct}\phi + \nu_{ct}, \tag{1}$$

where c indexes counties, t indexes years, $s(c)$ is the state which county c is in, $StatePart$ denotes state partisanship, and X is a vector of controls.

Results from this regression, with no controls, are reported in Column 1 of Table 2. Standard errors are clustered by state. The coefficient of 6.24 means that each one-percentage point increase in the statewide Republican vote share in the last presidential election (e.g., if the Republican candidate receives 54% of the vote instead of 53%) is associated with 6.24 additional deaths per 100,000 residents per year in a county where the age distribution matches the national average.

Some simple math helps to understand the magnitude of this difference. The US population was roughly 330 million people near the end of our sample period, which is 6.6 million people per state—equal to 66 times the number of residents (100,000) used to express mortality rates. A partisanship value of 60 is typical for a solidly red state, and 40 for a solidly blue state; for instance, in the 2020 election, the Republican nominee (Donald Trump) won more than 60% of the vote in 10 states, and less than 40% in 10 other states. The coefficient of 6.24 therefore implies a difference of $6.24 * (60 - 40) * 66 = 8237$ excess deaths per year in an average-sized state if partisanship is like a typical red state as opposed to a typical blue state, holding the age structure constant at the national average. (Of course, in practice, changing mortality rates would result in the age structure *not* being held constant.)

Another way to interpret the magnitude is by comparison with baseline risk. The regression results imply that the difference in AAMR between a typical blue and a typical red state is $6.24 * (60 - 40) = 125$. From Table 1, the mean county AAMR is 856. In other words, the coefficient reported in Column 1 represents a difference in mortality equal to 15% of baseline mortality rates. For comparison, mortality risk for sedentary individuals is 20-30% higher than people who exercise regularly (e.g., Löllgen, Böckenhoff, and Knapp, 2009). So, the coefficient reported in Column 1 represents a quantitatively important difference.

However, there are strong reasons to doubt that this correlation reflects causation. The central endogeneity problem we face is as follows. The treatment effect of interest is that state partisanship affects state-level policy which affects local mortality—i.e., we are only interested in the effects of state partisanship that occur through state-level government. However, partisanship is also a proxy for the overall cultural environment, including but not limited to attitudes about diet, risky behavior, and perhaps an overall willingness to make tradeoffs to increase longevity. We will refer to such confounders broadly as "culture." Local culture (i.e., culture in the county of interest) affects local mortality, and is also correlated with state culture to the extent that culture is spatially correlated. In turn, state culture is a key determinant of state partisanship. This creates confounding between local mortality rates and state partisanship via the pathway

$$\text{state partisanship} \leftarrow \text{regional culture} \rightarrow \text{local culture} \rightarrow \text{local mortality.}$$

Although we expect endogeneity to primarily operate via local characteristics (like local culture in the confounding chain above), an additional possibility is that endogeneity may occur through exclusively non-local channels—i.e., through characteristics not of the county in question, but of other nearby places. For example, mortality in a given county may be affected by proximity to a major city, if this increases access to higher-quality medical care. This is a potential source of endogeneity because states with large cities are more likely to vote Democratic. We will refer to this as "non-local confounding."

As a preliminary assessment of the potential for endogeneity, we add controls to the regression from Equation 1. These controls include Census region-by-year dummies, a cubic of the partisanship of the county itself, urbanicity, quadratics of racial shares, quadratics of all three measures (facebook, sending, and receiving) of age-adjusted mortality rates in closely-connected counties outside the state, racial shares within the facebook, sending, and receiving counties, and interactions between own county and closely connected county racial shares.[3] See Section 2 for details about construction of these variables.

Results with the full set of controls are reported in Column 2 of Table 2. Including controls cuts the coefficient of interest by nearly 80%, from 6.24 to 1.38. This suggests that most of the correlation between state partisanship and county mortality is due to confounding. However, it is not clear whether this rich set of controls has fully addressed endogeneity.

One way to assess whether there is still endogeneity due to regional culture and/or non-local confounding is to ask whether, conditional on controls, county mortality has a different relationship with the partisanship of nearby counties than it does with the partisanship of more distant counties within the same state. If the correlation between county mortality and other counties' partisanship arises through state policy, then there should be an equally strong connection between other counties' partisanship and own county mortality, regardless of where those other counties are located within the state, since all counties contribute to the election of state officials. By contrast, confounding will be stronger when looking at the partisanship of nearby counties than when looking at the partisanship of more distant counties, since both channels of endogeneity are strengthened when comparing nearby counties: Nearby counties are more culturally similar, and amenities in nearby counties have more direct influence than amenities in distant counties. Therefore, the difference in correlations should tell us the sign of endogeneity: For instance, if there is a more positive correlation between local mortality and nearby counties' Republican share than there is with distant counties' Republican share, this would suggest that the coefficient in Column 2 is biased upwards due to endogeneity.

In Columns 3 and 4, we assess this by replicating the results of Column 2, but splitting the variation in state partisanship into two sources: the average partisanship of counties in the nearest half of the same state as a given county, and the average partisanship of counties in the furthest half of the same state. To determine which counties belong to the near half versus the far half of the state as a given county, we calculate the distance between the centroids of a given county and each other county in the rest of the state. If there are N other counties in the same state, we assign the closest $\frac{N}{2}$ counties as being in the near half, and the remaining $\frac{N}{2}$ counties as being in the far half.[4] Columns 3 and 4 then instrument for state partisanship using the half of the state that we would like to exploit variation from, while controlling for the partisanship of the other half of the state plus all of the controls used in Column 2.

The results from Columns 3 and 4 show that the correlation is very different for near vs. far counties. In fact, the result in Column 2 seems *entirely* driven by the fact that local mortality is correlated with the partisanship of nearby counties. This implies that the set of controls in Column 2 is still insufficient to address endogeneity, and that the coefficient in Column 2 overestimates the effect of state policy on county mortality.

It is tempting to ask whether the results in Column 4 might be taken as an estimate of the effect of interest. In the next section, we make explicit what assumptions such a design is making and assess to what extent they are likely to hold. Then in Section 5 we describe the border county design, and finally we discuss the regression discontinuity

[3]For example, we include the interaction of the share of whites within a county with (i) the weighted average share of whites in the receiving counties (ii) the weighted average share of whites in the sending counties and (iii) the weighted average share of whites in the facebook counties.

[4]When N is an odd number, we assign the median distance county to the near half.

design in Section 6.

4 Far part design

As described in the introduction, the far part design exploits the "distant coincidence" that the overall partisanship of a particular state is determined in part by anomalies in the partisanship of counties in the far part of the state. Therefore we use the partisanship of faraway counties within the same state as exogenous variation in state partisanship, controlling for the partisanship of the counties in between and, in some specifications, for the partisanship of nearby states. Variation in the instrument conditional on controls can be understood as capturing that the places the county in question is grouped with administratively are redder or bluer than the places the county is grouped with geographically. The reason this variation is likely to be exogenous is that controlling for the partisanship of counties near the county of interest addresses spatial correlation of culture (i.e., controls for regional culture in the confounding chain of Section 3), such that, conditional on this control, the partisanship of the far part of the state is not informative about local culture. Non-local confounding is also likely to become quantitatively unimportant when the counties in question are far enough away from each other.

Both this analysis and the border county design in Section 5 measure the effects of being grouped together in a state government with Democratic vs. Republican voters. This effect necessarily operates solely through state government, but the interpretation of the parameter being identified is as the effect of sharing a state government with Democrats as they actually exist vs. Republicans as they actually exist—as opposed to the change in mortality which would happen if voters changed their partisan affiliation holding constant all other characteristics of the voters. For instance, suppose that Democrats live in urban areas, and leaders of either party would choose different policies in response to constituents living in urban areas; then replacing Republican voters as they are with Democrats as they are entails there being more urban voters, and our estimate incorporates the effect of any resulting policy changes, too. Nonetheless, these effects still operate through policy and resulting mortality gaps would be attributable to the actions of state government—as opposed to the mechanisms of endogeneity described in Section 3.

4.1 Econometrics

We estimate instrumental variables models of the form

$$\begin{aligned} LocalMortality_{ct} &= \beta_0 + \beta_1 StatePart_{s(c)t} + \beta_2 Near_{ct} + X_{ct}'\zeta + \epsilon_{ct} \\ StatePart_{s(c)t} &= \gamma_0 + \gamma_1 Far_{ct} + \gamma_2 Near_{ct} + X_{ct}'\xi + \eta_{ct}, \end{aligned} \tag{2}$$

where we divide the rest of counties in the same state as county c into two bins: counties in the near part of the state to county c and counties in the far part of the state to county c. We define the partisanship of the near part as $Near_{ct}$ and the partisanship of the far part as Far_{ct}.[5] β_1 measures the causal impact of state partisanship on local (i.e., county) mortality and is our coefficient of interest. To estimate this coefficient, the variable Far is used as an instrument for $StatePart$.

[5]We define the distance between two counties as the distance between the centroids of the two counties.

We estimate specifications with a variety of cutoffs. For instance, the rest of the state can be partitioned such that $Near$ is the average partisanship in the near two-thirds of the state and Far is the average partisanship of the far one-third; or we can split the rest of the state into a near one-fourth and a far three-fourths; or we can choose some other cutoff.

X is a vector of additional controls. We implement specifications where X either includes Census region dummies, the average partisanship of states neighboring state $s(c)$, or no additional regional controls. Furthermore, we vary whether X includes controls for own county characteristics and/or whether it includes the average characteristics of near-part counties, where these remaining characteristics are the controls used in our OLS specification (i.e., partisanship, mortality in connected counties, urbanicity, racial shares, and interactions).

4.2 Cutoff selection and model tests

The identifying assumption is that far-part partisanship is only correlated with a county's mortality through (i) its effect on state government (i.e., treatment effect) and (ii) general regional cultural correlations which are absorbed by controlling for near-part partisanship and the additional controls in X.

We next consider validity tests which will give us evidence about whether this assumption is likely to hold, and whether there are some cutoffs (i.e., ways of dividing the state between a far and near part) or choices of controls where performance is better than others.

Neighboring state placebo tests Our first test is a border county placebo test. To illustrate, consider Chautauqua County, which is in New York State and borders Pennsylvania. If using a given cutoff fails to address general either local or non-local confounding, we would expect to find a non-zero estimated impact of the state policies of Pennsylvania on mortality in Chautauqua County and counties like it.

In general, for each border county, we find an adjacent county on the other side of the border, which we call the neighboring county, and calculate the partisanship in the near and far parts of the neighboring state for the neighboring county using each potential cutoff.[6] We then estimate

$$\begin{aligned} LocalMortality_{ct} &= \Psi_0 + \Psi_1 BorderStatePart_{s(c)t} + \Psi_2 BorderNear_{ct} + \varepsilon_{ct} \\ BorderStatePart_{s(c)t} &= \delta_0 + \delta_1 BorderFar_{ct} + \delta_2 BorderNear_{ct} + \varsigma_{ct}, \end{aligned}$$

where $BorderStatePart$ represents the state partisanship of the neighboring state, instrumented by the partisanship in the far part of the neighboring state $BorderFar$ and controlling for the partisanship of the near part of the neighboring state $BorderNear$. Our coefficient of interest is Ψ_1, which should be zero if we select an appropriate cutoff, and not equal to zero if there is remaining confounding.

We present the results of the neighboring state placebo in Table 3. Panel A does not include regional controls, Panel B includes US Census Region dummies, and Panel C includes the weighted average of state partisanship in neighboring states. We exclude any other controls from X in this table, except for year fixed effects, to focus on the

[6] For border counties with multiple adjacent counties in one or more neighboring states, we randomly select one such county. Our results are not sensitive to alternative approaches.

basics of the design. Column titles indicate the proportion of other counties included in the near part of the state. In the first column ("OLS"), we report results for simply regressing $LocalMortality_{ct}$ on $BorderStatePart_{s(c)t}$ with no instrument but with year fixed effects.

For the closest cutoffs without regional controls, this placebo test gives estimates which are economically and statistically significant—i.e., we have failed to adequately control for regional partisanship. But as regional controls are added and a longer cutoff is used, both the magnitude and statistical significance of the placebo estimates disappear. By cutoffs of one-half, the bias is likely close to zero even without additional regional controls; and, from Panels B and C, closer cutoffs look acceptable when regional controls are added. This suggests that our broad approach works as expected: Adding sufficiently rich controls for the general regional propensity to support Republicans addresses endogeneity.

Balance tests We additionally assess the validity of the instrument using balance tests. We test for correlations between the instrument (defined using various cutoffs) and local characteristics by estimating Equation 2 but replacing $LocalMortality_{ct}$ with other local characteristics.

One such key covariate is local (i.e., own-county) partisanship. This is a particularly informative variable to look at because confounding due to regional culture implies that

$$\text{state partisanship} \leftarrow \text{regional culture} \rightarrow \text{local culture.}$$

In turn, we expect that local culture → local partisanship because, whatever the aspects are of culture such that *regional* realizations of those aspects affect *regional* partisanship, so too *local* realizations of those aspects of culture should affect *local* realizations of partisanship. This creates a causal chain connecting state partisanship with local partisanship which closely resembles our concern about local confounding:

$$\text{state partisanship} \leftarrow \text{regional culture} \rightarrow \text{local culture} \rightarrow \text{local partisanship.}$$

Because the two middle links of this chain are identical to the middle links of local confounding, the same controls which address this correlation would also be expected to address local confounding.

We also test for balance on the other covariates used in our analysis: the three measures of mortality in closely-linked counties (facebook, sending, and receiving), urbanicity, and racial shares.

Because these balance tests use local characteristics as dependent variables, we focus our balance tests on specifications which don't control for local variables. (Obviously, controlling for any variable will cause our instrument to be conditionally uncorrelated with that variable, but we wish to test the general principle that the instrument is as good as randomly assigned.) However, all specifications include year fixed effects.

We report the results of the balance tests in Table 4. Row titles indicate the local characteristic used as the dependent variable in Equation 2, and column titles indicate the proportion of other counties included in the near part of the state. Column 1 (OLS) indicates the results of an OLS regression of the local characteristic on state partisanship with year fixed effects. As discussed above, local partisanship is a key covariate and we observe balance for cutoffs larger than one-third. For cutoffs larger than two-fifths, we observe balance on all local characteristics except for Share White. With a Bonferroni correction for multiple hypothesis testing, the coefficient for Share White would also be insignificant.

4.3 Results

As discussed above, we estimate specifications while varying the cutoff and choice of controls. In order to present the results of all these estimates, we present the results as a specification curve (Simonsohn, Simmons, and Nelson, 2020) in Figure 1. The figure presents the coefficient and 95% confidence interval obtained for every combination of cutoff, regional controls (none, control for Census region, or control for neighboring states' average partisanship), and whether or not we include controls for local and/or near part observed characteristics. All specifications include year fixed effects. Lastly, the figure reports a selection ratio estimate—a way of aggregating specifications which will be discussed in Section 4.4. Specifications are ordered from the smallest estimate to the largest. The characteristics of each specification are indicated with dark dots in the lower half of the figure.

Most estimates are statistically insignificant and smaller than the results obtained via OLS with controls (Column 2 of Table 2). The majority of estimates are positive. Almost no estimate is even half as large as the OLS relationship without controls of 6.24 (see Column 1 of Table 2), and almost all specifications rule out such values. A handful of estimates are noticeably larger and less precise than other specifications. These large estimates come from specifications which use comparatively close cutoffs and do not include controls for local or near part characteristics—the same specifications which generally produced border state placebo test results indicating the possibility of (upwards) bias. In general, more distant cutoffs produce smaller estimates, the inclusion of near part controls produces smaller estimates, and the inclusion of local controls gives estimates in the middle of the specification curve, i.e., reduces outlying results.

4.4 Preferred estimate

The balance and placebo tests fail to reject many specifications. Following a purely binary accept/reject approach to specifications would therefore leave us little way to choose between the specifications in Figure 1, and we do not have strong a priori reasons to favor some specifications over others.

To construct our single best estimate using the far part approach, we instead combine estimates across specifications using the selection ratio approach of Slichter (2023). This approach is as follows. We first obtain effect estimates from many different specifications, each of which is characterized by a choice of cutoff and a choice of region control (Census region dummy, neighboring state, or none). In these specifications, we do not control for local or near part characteristics, except for near part partisanship. We also obtain an estimated violation of covariate balance for each specification. An ideal specification would be one in which there is exact covariate balance. To work with covariates collectively, we aggregate the controls into an index with weights equal to their regression coefficients in a regression of our outcome (county-level mortality) on treatment (state-level partisanship) and controls. The controls are the full set of local controls—i.e., we are testing for balance on the estimated value of $X_{ct}'\phi$ from Equation 1 obtained in the regression reported in Column 2 of Table 2.

Figure 2 plots the estimated effect of interest against the degree of covariate balance (i.e., the estimated effect on this index of observables) for each specification. Each point in the graph represents a specification. The vertical coordinate is the estimated effect of state partisanship on mortality in that specification. The horizontal coordinate is the covariate balance test, i.e., the estimated effect of state partisanship on the index of controls. The key assumptions of the selection ratio method of Slichter (2023) are that (i) this relationship is exactly linear up to sampling error, and (ii) a specification in which the covariates were exactly balanced would be unbiased. In essence, (i) allows us to extrapolate from the observed specifications to determine what main estimate we would have obtained if we had observed a specification with exact covariate balance, and (ii) assumes that this is the specification we would want. In Figure 2, the resulting estimate is the value at which the line of fit through the specifications (red line) intersects the vertical axis (green line).

Figure 2 shows that there is, in fact, a tight relationship between the point estimate obtained from a specification and the degree of covariate imbalance in that specification. We can see that a specification which had exact balance on the index of observables would be predicted to produce an estimate which was just barely positive.

The selection ratio estimate is preferred to simply implementing IV with controls because it uses a strictly weaker assumption. IV with controls relies on the assumption that the controls fully address any remaining endogeneity. By contrast, as discussed by Slichter, if this is true, the assumptions for the selection ratio approach will be satisfied; but it is also possible for the selection ratio approach's assumptions to be satisfied *without* the controls being sufficient to address endogeneity, provided that any bias due to unobserved variables is proportional to bias on observables.[7]

We obtain an estimate of 0.32 under this extrapolation procedure, with a bootstrapped 95% confidence interval of [-1.19, 1.83]. This estimate is highlighted in blue in Figure 1. From the specification curve, we can see that our preferred estimate is typical of estimates obtained using the far part approach, and especially of those specifications which include controls for local characteristics.

4.5 Mechanisms

To seek more clarity about how mortality effects might occur, we also estimate specifications where the outcome variable is cause-specific or age-specific mortality. Unfortunately, we have many missing values for this analysis because WONDER suppresses mortality counts when they are small. This renders these results unreliable due to sample selection. Nonetheless, we report results from these specifications in Appendix B. We find that Republican state policies may increase gun-related deaths, homicides (which includes homicides involving guns), and drug-related deaths, and that Republican state policies might increase mortality in people under the age of 35. However, these are variables for which sample selection is especially severe, so these results should be taken with a grain of salt.

[7] In the case where bias is solely through observables, the slope of the line in Figure 2 would be 1. We can see that the slope is incrementally larger than 1, suggesting that specifications which produce better balance on observables also likely address balance failures on some unobservables too, though most endogeneity is addressed with observables.

5 Border county design

5.1 Econometrics

Our second method for estimating the causal impact of state policies on mortality exploits counties on the border between two states. Local culture should be similar between two neighboring counties, as should non-local factors such as proximity to health care in large cities, thereby minimizing the potential for confounding described in Section 3. We estimate a causal impact of state partisanship on local mortality using a first difference estimation among border pairs. Specifically, we estimate

$$\Delta Mortality_{bt} = \Lambda_0 + \Lambda_1 \Delta RestState_{bt} + \Delta X'_{bt}\Gamma + \mu_{bt},$$

where we now index observations by a border county pair b in year t. For example, one border county pair is Chautauqua County in New York and Warren County in Pennsylvania. We include all border county pairs. $\Delta Mortality$ is the difference in AAMR between these counties, and $\Delta RestState$ is the difference in partisanship of the state, excluding the counties in the border pair. ΔX is the difference in the vector of covariates X used in OLS and the far part design. We calculate each difference as the value for the more Republican county minus the value for the less Republican county.

While we expect the use of border pairs to limit the issue of confounding, we do not expect it to fully eliminate this problem. In the border county design, confounding via culture can occur if the characteristics of the counties within a border county pair are correlated with the partisanship of the remainder of the state. Confounding via non-local effects can also occur if, for instance, the county which is in the same state as a major metro area is slightly closer to that metro area.

To further minimize the potential for confounding, we additionally apply the far part design to the border county pairs. We do this by instrumenting $\Delta RestState_{bt}$ using ΔFar_{bt} while controlling for $\Delta Near_{bt}$, where ΔFar and $\Delta Near$ represent the difference in the partisanship of the far part and near part for the two states pertaining to border county pair b in year t.

To assess whether this approach addresses confounding, we replace $\Delta Mortality_{bt}$ with differences in covariates. As in Section 4, because controlling for the difference in a covariate would mechanically produce a zero estimate, we apply this model test only to specifications which do not control for any variables X.

We report the results for these balance tests in Table A1. We observe a statistically significant coefficient on local partisanship in Column 1, which indicates that differences in state partisanship predict differences in own-county-partisanship within a border county pair. This difference becomes statistically insignificant when we use the far part IV method in Column 2. (We use the far half as the IV.) There are no differences on other covariates. However, using the IV once again results in a balance failure for Share White.

5.2 Results

We present the results of the border county analysis using a specification curve in Figure 3. In contrast with the far part design, these estimates are mostly statistically significant. All border county estimates are positive. While these estimates are on average larger than those in Figure 1, no estimate is as much as half as large as the OLS relationship without controls of 6.24 (Column 1 of Table 2), and the majority of specifications exclude such values.

5.3 Preferred estimate

As in Section 4.4, we produce a preferred estimate using the selection ratio approach to aggregate results from different specifications. We report a plot of point estimates against balance test failures for the border county design in Figure 4. In Figure 4, the horizontal coordinate represents the estimated effect of differences in state partisanship on differences in the index of controls, and the vertical coordinate represents the estimated effect of differences in state partisanship on differences in mortality.

Compared with Figure 2, we can see that the balance failures are far smaller and the effect estimates are far more similar to each other. This is consistent with the view that border county specifications are, as a general rule, effective at addressing confounding.

We obtain an estimate of 1.41 based on this analysis, with a 95% confidence interval of [0.01, 2.80]. From Figure 3, we can see that this is typical of results from border county specifications.

6 Regression discontinuity design

The final analysis utilizes RD to study the effects of the partisan affiliation of the governor and legislative majorities on mortality. The design is implemented using gubernatorial and state legislature elections, where we look for a discontinuity in mortality risks as a function of Republicans narrowly winning versus losing political power. For gubernatorial races, the treatment variable is the party of the governor, which is a discontinuous function of the margin of victory in an election. For state legislatures, we estimate effects of party control of the upper house and the lower house of the legislature, where control is a discontinuous function of the fraction of seats which is controlled by each party.[8]

6.1 Econometrics

For each election type (e.g., gubernatorial), we estimate:

$$StateMortality_{st} = \theta_0^b + \theta_1^b Rep_{st}^b + g^b(MOV_{st}) + \xi_{st}^b.$$

where $StateMortality_{st}$ represents the age-adjusted mortality rate in state s in year t. We use the superscript b to index election types. Rep_{st}^b takes the value of one if the winner of the most recent election of type b at state s and year t is a Republican and zero if the winner is a Democrat for gubernatorial, Senate, and House election types. θ_1^b is the coefficient of interest which shows the effect of a Republican governor or legislature on the age-adjusted mortality rate of the state. MOV_{st} represents the margin of victory of the Republican Party in the most recent election. In gubernatorial elections, this is defined as the percent of the vote received by the Republican candidate minus the percent received by the Democratic candidate. In legislative elections, this is defined as the percent of seats held by Republicans minus the percent held by Democrats.

The identifying assumption, as in Lee (2008), is that, in narrowly decided elections, it is as good as random which party wins. Density tests (McCrary, 2008) indicate no discontinuity in the density of the margin around the cutoff for either gubernatorial or

[8]Nebraska has a unicameral legislature, so we exclude it from our RD analysis.

House control (Figures A1 and A2), but do indicate discontinuity for Senate control, with Republicans more likely to have extremely narrow control of a legislature. We also test for continuity of observables and find no discontinuity (Table A2), and show that results are not particularly sensitive to choice of bandwidth (Figures A3, A4, and A5).

The use of discontinuities for gubernatorial races, or other single elections, is standard (e.g., Lee, 2008; Beland, 2015; Beland and Oloomi, 2017) but the legislative control RD is not. It is possible that legislatures are able to exercise fine control over their exact composition, e.g. through gerrymandering, in which case states barely to the left of the discontinuity would not be comparable to states barely to the right. To be a source of bias, this fine control must also be correlated with potential mortality in a state. The McCrary test rejection for Senate composition suggests that legislative elections could be subject to fine control. However, the balance test results suggest that any such fine control is not correlated with observed mortality-related characteristics; and furthermore, from Figure A2, the McCrary test rejection seems to be driven only by the lack of extremely narrow Democratic majorities, while the bandwidth sensitivity estimates show that we obtain very similar results using wider bandwidths which deemphasize those observations right at the threshold. On balance, we interpret this to mean that the potential for bias from fine control is not likely large, and we choose to include the legislative results in our set of preferred estimates. However, we also report results for gubernatorial races only.

Notice that treatment is defined differently in the RD than in the other empirical approaches. In OLS and the far part and border designs, treatment is the partisanship of the overall state operating via state policy. In the RD, the treatment variables are instead which party the governor belongs to and which party holds majorities in the state legislature.

There are two important gaps between these notions of treatment. One gap likely leads the RD treatment to have smaller effects than the previous notion of treatment, while the other likely leads it to have larger effects.

The reason RD may understate effects of the treatment as defined earlier in the paper is that, while the RD treatments are obvious mechanisms by which voters' political preferences influence state policy, they are not the *only* such mechanisms. For one, elected officials of the same party take different political stances depending on the partisan environment of their state. Therefore, having a heavily Republican electorate will lead both to the election of more Republicans *and* to officials of both parties having more Republican-like policy positions than candidates of the same party in other parts of the US. Furthermore, for legislatures, the RD treatment does not include information about the margin of control, even though surely a legislature with a large Democratic majority will set policy differently from one with a narrow majority.

On the other hand, RD estimates will be *larger* in magnitude than the previous estimates because they are estimates of the effects of gubernatorial/legislative control, as opposed to effects of unit changes in the electorate's composition. Each unit change in the electorate's composition produces much less than a 100 percentage point change in the probability of gubernatorial or legislative control, so the units on the right-hand side of the RD will tend to produce parameter values with much larger magnitudes.

This second problem is more straightforward to address. We do this by rescaling the RD estimates by the correlation between state partisanship and gubernatorial/legislative control. Specifically, we estimate regressions of the form

$$Rep_{st}^{b} = \gamma_0^b + \gamma_1^b StatePart_{st} + \iota_{st}^{b}.$$

Then we report a "rescaled" RD estimate equal to the product of our estimates, $\widehat{\gamma_1^b}\widehat{\theta_1^b}$.

We construct our preferred RD estimate by taking the sum of nine different rescaled estimates. These nine estimates are every combination of each of the three election types (gubernatorial, House, and Senate), and each of three time periods in which to measure *Rep* (the most recent election, the prior election, or the election prior to that).[9] That is, this summed estimate represents how much state partisanship is estimated to influence mortality through the combined influence of legislative and gubernatorial control over the last three election cycles. We include the specifications with longer time frames on the principle that policy changes enacted in recent previous legislative sessions likely still influence current conditions. Standard errors are bootstrapped.

6.2 Results

The results of the RD analysis are presented in Table 5. The first column presents the estimates from the initial estimation for each type of election. The second column shows the estimates from using the prior round of elections and the third column uses elections that occurred two rounds prior.

There are two estimates within each column. The first estimate is the raw RD estimate (i.e., an estimate of θ_1^b) while the second estimate is the rescaled estimate. Sums for each row and column are reported, and our preferred estimate (which sums all nine rescaled values) appears at the bottom right.

Our preferred estimate is of an effect of 0.73. This explains just over 10% of the OLS coefficient without controls in Column 1 of Table 2. However, this estimate is very imprecise; the standard error is 2.86.

Focusing only on the gubernatorial RDs (in case the legislative RD is biased due to fine control), we obtain a combined estimate of 0.11 with a standard error of 1.57. We also consider this result imprecise; it allows us to statistically reject that recent gubernatorial composition explains more than half of the OLS coefficient without controls in Column 1 of Table 2, but of course this is a narrower mechanism which would anyhow be unlikely to explain the entirety of the mortality gap that is due to state policy.

7 Discussion

We have explored four lines of evidence: OLS, far half, border county, and RD. Of these four lines of evidence, we believe that OLS may systematically overestimate policy effects due to confounding, while RD may systematically underestimate them due to the discrepancy in definition of treatment.

Our preferred estimates were as follows. OLS suggests that 22% of the mortality gap is due to policy (95% CI: between 7 and 37%).[10] The far half design gave an estimate of 5% for our preferred estimate from the selection ratio approach (95% CI: between -19% and 29%). The border county design using the selection ratio approach gave estimates of 23% (95% CI: between 0% and 45%). Lastly, the aggregated RD gave estimates of 12% (95% CI: between -78% and 101%), though it is probably coincidental that this

[9] In almost all states, governors serve for four years. However, in Vermont and New Hampshire, terms are two years.

[10] We will express the confidence intervals as a fraction of the baseline OLS estimate without controls, which is 6.244 (see Column 1 of Table 2), without accounting for uncertainty in the estimate of 6.244.

point estimate is so similar to the estimates obtained with the other approaches, since the RD estimate is so imprecise and anyhow is measuring effects of a somewhat different treatment.

Taking the estimates together, the scenario most consistent with the data is that something like 10% of the mortality gap is due to policy. Our preferred point estimates lie between 5% and 23%. Values appreciably outside of this range are rejected by at least one of our approaches: Values larger than 29% are rejected with $p < .05$ by our preferred far half approach, while values of 0% and smaller are rejected by the border county approach. Because the OLS estimate of 22% is likely to overestimate effects of policy, we consider the range approximately between 0 and 20% to be the most plausible.

Bayes factors help illustrate the comparative consistency of the data with various hypotheses. Comparing the hypotheses that 0% of the mortality gap is explained by policy to the midpoint of the far part and border county preferred estimates (14%), the likelihood of obtaining the far part estimate is 0.84 times as large under the 14% hypothesis as under the hypothesis that 0% of the gap is explained by policy—i.e., the far part estimate is about equally consistent with these two views of the world. But the likelihood of the border county estimate is 5.4 times larger if 14% of the mortality gap is due to policy than if 0% is. In other words, 14% is substantially more consistent with the data, though not categorically so, depending on how one weights between these models. Comparing instead between 14% and 50%, both estimates are vastly more consistent with the smaller number: The likelihood of the far part estimates is 578 times greater, and the likelihood of the border county estimates is 14 times greater, if the effect is 14%.

Summarizing these results in qualitative terms, we find very strong evidence that mortality differences between red and blue states should not simply be interpreted as being due to state policy. Instead, it is extremely likely that these differences are *primarily* driven by other factors, and we cannot firmly rule out that these differences are *entirely* driven by non-policy factors.

Nonetheless, we also cannot rule out that there are important effects of state policy, and in fact our preferred estimates are substantially more consistent with this scenario than with no effects. Recall from Section 3 that the OLS relationship gives an additional 8237 deaths per year in a typical red state as opposed to a blue state, holding constant the age distribution. If 14% of this relationship is due to policy (the midpoint of our preferred estimates), that is over 1000 excess deaths per year. Taking the smallest point estimate from our four designs gives that red state policies cause over 400 excess deaths per year. Again, these numbers assume that the age distribution is held constant at the national average, but of course implementing policies which affect mortality will tend to affect the age distribution.

Another way to translate this estimate is as a percent of baseline mortality risk. The mean AAMR by county in our sample is 856 (see Table 1). If 14% of the mortality gap is due to policy, then the difference between a typical red vs. blue state (i.e., state partisanship of 60 vs. 40) is $\frac{.14*6.244*20}{856} = 0.02$. That is, the scenario most consistent with the data is that, relative to blue state policies, red state policies increase mortality risk by 2% of average baseline risk. Performing an analogous translation for the broader range that 0-20% of the mortality gap is due to policy gives that red state policies increase mortality risk by 0-3% of average baseline risk. In other words, even though the mortality gap is not primarily due to policy, the mortality gap is so large to begin with that the fraction of the gap most likely attributable to policy is still quantitatively important.

A final way to translate our preferred estimate is to convert it to a dollar value.

Suppose we use a value of a statistical life of \$7 million. If 14% of the gap is due to policy, then the dollar value of the annual mortality risk averted by blue state policies as opposed to red state policies is $.14 * \frac{6.244}{100{,}000} * 20 * 7,000,000 = \$1136$. That is, the mortality benefits of blue state policies are worth a little over \$1000 per year per resident.

To illustrate the size of this dollar value, we can compare it to the difference in tax burdens between red and blue states. State and local taxes are a larger share of GDP in blue states than in red states. Regressing Tax Foundation estimates of combined state and local tax burdens[11] on 2020 state-level election results, the average tax burden in a blue state (i.e., Republican support of 40%) is 11.8% of output, while the average tax burden in a red state (i.e., Republican support of 60%) is 9.3% of output. Multiplying this gap of $11.8 - 9.3 = 2.5\%$ by a GDP per capita of \$50,000—which is approximately national GDP per capita at the midpoint of years in our sample—gives that tax burdens are approximately \$1250 higher on average in blue states.[12] In other words, the most likely mortality benefit of blue state policies is valuable enough to approximately offset the additional tax cost of living in a blue state. However, we have only limited confidence in the comparability of true (i.e., not just estimated) mortality and tax benefits because, for instance, we can only argue with moderate confidence against a scenario in which none of the mortality gap is explained by state policy.

[11] See https://taxfoundation.org/data/all/state/tax-burden-by-state-2022/.

[12] Note that this ignores that average incomes are different by state. If tax laws were identical in every state but higher earners pay a larger fraction of their income in taxes, then wealthier states will tend to have higher tax burdens as a percent of output.

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Table 1: Summary Statistics

	Mean	Std. Dev	Min	Max
Age-Adjusted Mortality Rate (AAMR)	856.61	159.70	100.03	3331.24
State Partisanship	52.47	7.84	30.27	72.79
County Partisanship	59.23	13.85	7.19	94.58
Own-County Share White	86.02	15.75	3.12	99.48
Own-County Share Black	9.19	14.59	0	85.49
Own-County Share Asian	1.17	2.14	0	44.49
Own-County Share Hispanic	8.28	13.09	0.31	95.71
FB Friend Counties Share White	85.78	6.82	49.75	96.54
FB Friend Counties Share Black	8.97	7.13	0.34	47.66
FB Friend Counties Share Asian	1.01	0.36	0.27	2.88
FB Friend Counties Share Hispanic	7.05	3.30	1.81	33.95
FB Friend Counties AAMR	828.04	74.34	188.02	1126.26
Sending Counties Share White	80.42	12.87	0	98.74
Sending Counties Share Black	11.06	7.31	0	69.31
Sending Counties Share Asian	2.93	1.84	0	25.36
Sending Counties Share Hispanic	11.72	6.99	0	60.68
Sending Counties AAMR	774.85	125.22	0	1239.07
Receiving Counties Share White	81.32	6.42	16.57	98.00
Receiving Counties Share Black	11.45	6.11	0.37	49.98
Receiving Counties Share Asian	3.23	1.68	0.30	31.48
Receiving Counties Share Hispanic	12.68	5.74	1.20	48.97
Receiving Counties AAMR	784.15	61.98	518.56	1239.07
Urbanicity Index	4.62	1.51	1	6

Notes: Age-Adjusted Mortality Rate (AAMR) reports the age-adjusted mortality rate per 100,000 people. State and County Partisanship reports the percentage of votes for the Republican presidential candidate in the most recent presidential election. Racial shares represent percentages for a given county in 2010 as reported by the US Census Bureau. FB Friend, or Facebook Friend Counties, variables are calculated using weighted averages of county-level variables from 2016-2019 with weights based on number of Facebook friend connections to a given county. Sending and Receiving Counties variables are calculated as the weighted average of county-level variables from 2016-2019 with weights based on the number of people immigrating (receiving) or emigrating (sending) to/from a given county. Urbanicity comes from the NCHS Urban-Rural Classification Scheme in 2013. A full explanation of covariates can be found in Section 2.

Table 2: Initial OLS Results

	(1) OLS	(2) OLS Controls	(3) IV: Near Half	(4) IV: Far Half
State partisanship	6.2440***	1.3874**	2.8343**	-0.1677
	(1.3798)	(0.4801)	(0.9282)	(0.7366)
N	60628	60627	60627	60627
Controls	No	Yes	Yes	Yes

Notes: The dependent variable in all columns is age-adjusted mortality per 100,000 people. Controls include a cubic control for own-county partisanship, quadratic controls for own-county racial shares of Whites, Blacks, Asians, and Hispanics, and quadratic controls for average age-adjusted mortality rates for counties sending and receiving migrants to a given county and Facebook-connected counties. Controls also include own-county racial shares interacted with (i) sending counties' average racial shares (ii) receiving counties' average racial shares and (iii) Facebook-connected counties' average racial shares, in addition to controlling for urbanicity. All columns include year fixed effects. Columns 2-4 include year by US Census Region fixed effects. Columns 3-4 instrument state partisanship using partisanship in the half of the state denoted by the column title controlling for the other half of the state. We divide the rest of the counties within the same state as a given county based on the distance between county centroids, with the half of counties furthest from a given county defined as the far half of the state, and the remaining counties defined as the near half of the state. In the case of an odd number of rest of state counties, the near half of the state has one more county than the far half of the state. Cluster-robust standard errors at the state level are reported in parentheses. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$.

Table 3: Placebo Border County Model Test

	(1) OLS	(2) 1/5	(3) 1/4	(4) 1/3	(5) 2/5	(6) 1/2	(7) 3/5	(8) 2/3
Panel A: Baseline								
Border State Partisanship	4.7923***	3.2449**	2.7422*	2.3457	2.5669	0.7020	-0.0467	-0.3791
	(0.9129)	(1.1522)	(1.1907)	(1.2918)	(1.4398)	(1.6002)	(2.1154)	(2.4532)
N	23073	22913	22913	23073	23073	23073	23073	22913
R^2	0.1162	0.1180	0.1161	0.1146	0.1140	0.1148	0.1116	0.1118
Panel B: US Census Region								
Border State Partisanship	2.6741***	1.9025**	1.4248	1.3904	1.4228	0.2047	-0.4810	-0.9775
	(0.6511)	(0.7178)	(0.7813)	(0.9138)	(1.0603)	(1.2407)	(1.5686)	(1.7780)
N	23073	22913	22913	23073	23073	23073	23073	22913
R^2	0.2835	0.2872	0.2872	0.2850	0.2848	0.2879	0.2866	0.2889
Panel C: Neighboring State Partisanship								
Border State Partisanship	3.0970***	1.5001	0.9298	0.3550	0.4573	-1.4443	-2.7711	-3.0509
	(0.8755)	(1.0790)	(1.1793)	(1.2015)	(1.3257)	(1.5327)	(2.0762)	(2.4113)
N	23033	22873	22873	23033	23033	23033	23033	22873
R^2	0.1366	0.1382	0.1360	0.1335	0.1333	0.1335	0.1304	0.1311

Notes: The dependent variable in all columns is own-county age-adjusted mortality rate (AAMR) for counties on the border between two states. All columns include year fixed effects. The main independent variable is neighboring state partisanship. Column 1 reports the OLS estimate. Columns 2-8 instrument neighboring state partisanship with partisanship in the far part of the neighboring state, controlling for partisanship in the near part of the neighboring state. The column titles in columns 2-8 indicate the proportion of counties included in the near part of the neighboring state. In cases where the number of counties in the near part is not an integer we include an extra county in the near part of the neighboring state. Panel A does not include additional regional controls. Panel B includes US Census Region dummies, and Panel C separately includes the average partisanship of the neighboring states as a given county. We report clustered robust standard errors at the state level in parentheses. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$.

Table 4: Far Part IV Balance Test for Local Characteristics

	(1) OLS	(2) 1/5	(3) 1/4	(4) 1/3	(5) 2/5	(6) 1/2	(7) 3/5	(8) 2/3
Local Partisanship	0.8530***	0.1826**	0.1748*	0.1200	0.0851	-0.0473	-0.2274	-0.3372
	(0.0791)	(0.0611)	(0.0736)	(0.0802)	(0.0899)	(0.1214)	(0.1825)	(0.2328)
Share White	-0.1425	-0.1899	-0.1724	-0.2204	-0.2363	-0.3288*	-0.4792*	-0.5745*
	(0.1497)	(0.1241)	(0.1224)	(0.1324)	(0.1417)	(0.1600)	(0.2173)	(0.2762)
Share Black	0.1556	0.0957	0.0931	0.1270	0.1551	0.2222	0.3605	0.4720
	(0.1492)	(0.1375)	(0.1365)	(0.1485)	(0.1572)	(0.1800)	(0.2351)	(0.2917)
Share Hispanic	-0.0615	-0.1022	-0.0938	-0.1090	-0.0937	-0.0952	-0.0211	-0.0242
	(0.1964)	(0.1147)	(0.1216)	(0.1269)	(0.1284)	(0.1335)	(0.1504)	(0.1798)
Share Asian	-0.0790**	-0.0148	-0.0175	-0.0157	-0.0147	-0.0066	0.0015	-0.0059
	(0.0244)	(0.0182)	(0.0188)	(0.0206)	(0.0207)	(0.0227)	(0.0264)	(0.0307)
AAR: Receiving Counties	2.3174***	-0.0469	-0.1438	-0.1378	-0.0551	-0.5630	-1.0434	-1.2210
	(0.5806)	(0.5509)	(0.5583)	(0.5771)	(0.6213)	(0.6868)	(0.7650)	(0.8736)
AAR: Sending Counties	1.5954*	-0.5303	-0.5104	-0.3428	-0.1458	-0.4379	-1.0602	-1.4248
	(0.6478)	(0.6468)	(0.6757)	(0.7388)	(0.8131)	(0.9530)	(1.0761)	(1.2644)
AAR: Facebook Counties	1.5594	-0.8797	-0.8488	-0.8828	-0.7597	-1.1701	-1.6385	-1.8590
	(0.9886)	(0.7927)	(0.8055)	(0.8503)	(0.8867)	(0.9685)	(1.0783)	(1.1892)
Urbanicity	0.0394***	-0.0149	-0.0136	-0.0092	-0.0059	-0.0165	-0.0222	-0.0188
	(0.0080)	(0.0112)	(0.0108)	(0.0104)	(0.0111)	(0.0128)	(0.0155)	(0.0195)

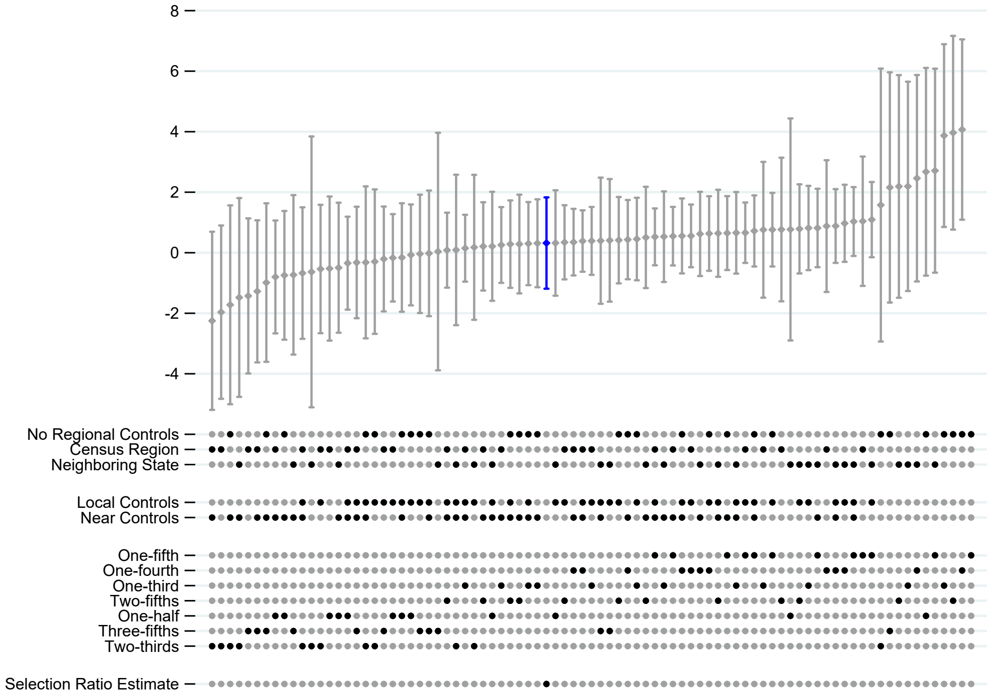
Notes: Row titles represent the dependent variable estimated using 2. We describe the covariates included in Section 2. All columns include year fixed effects. The main independent variable is neighboring state partisanship. Column 1 reports the OLS estimate. Columns 2-8 instrument state partisanship with partisanship in the far part of the state, controlling for partisanship in the near part of the state. The column titles in columns 2-8 indicate the proportion of counties included in the near part of the state. In cases where the number of counties in the near part is not an integer we include an extra county in the near part of the state. Panel A does not include additional regional controls. Panel B includes US Census Region dummies, and Panel C separately includes the average partisanship of the neighboring states as a given county. We report clustered robust standard errors at the state level in parentheses. $p < 0.1^{*}\, p < 0.05^{**}\, p < 0.01^{***}$.

Table 5: RD Estimates

	t		t-1		t-2		Total	
Governor	-10.652	-0.165	7.714	0.084	25.398	0.192	**17.508**	**0.111**
	(51.641)	(0.815)	(44.328)	(0.516)	(41.386)	(0.370)	(133.064)	(1.571)
Observations	152	152	194	194	194	194		
Bandwidth	14.019	14.019	17.750	17.750	15.558	15.558		
Senate	23.483	0.649	26.928	0.601	21.974	0.334	**72.386**	**1.584**
	(29.490)	(0.798)	(30.734)	(0.703)	(35.567)	(0.535)	(83.007)	(1.718)
Observations	735	735	539	539	343	343		
Bandwidth	15.915	15.915	19.102	19.102	19.663	19.663		
House	-18.479	-0.512	-12.837	-0.273	-12.204	-0.183	**-43.520**	**-0.968**
	(31.365)	(0.862)	(37.788)	(0.777)	(27.934)	(0.420)	(82.920)	(1.771)
Observations	735	735	539	539	343	343		
Bandwidth	18.464	18.464	19.959	19.959	20.858	20.858		
Total		**-0.028**		**0.412**		**0.343**		**0.727**
		(1.263)		(1.189)		(0.795)		**(2.860)**
Rescaled?	N	Y	N	Y	N	Y	N	Y

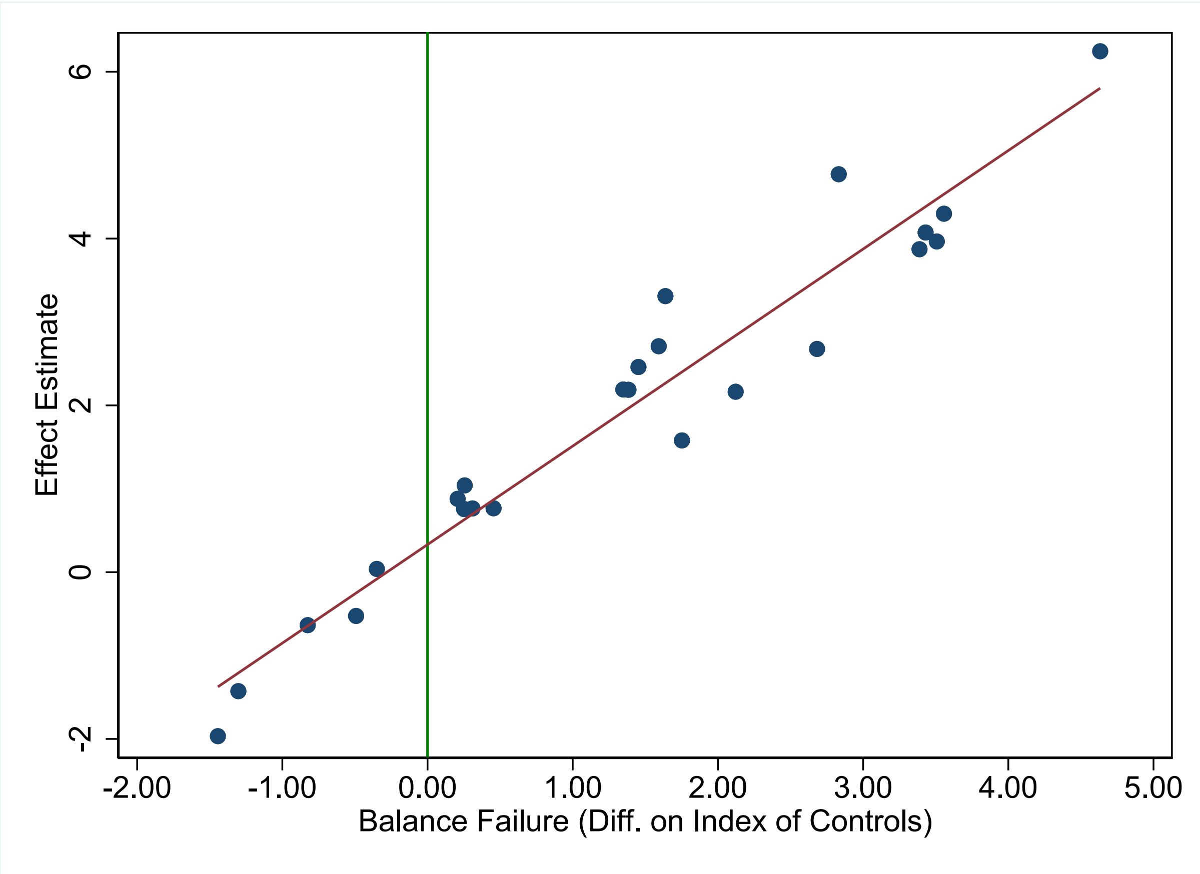
Notes: The dependent variable in all columns is age-adjusted mortality per 100,000 people. The first column presents the estimates for the outcome of the most recent election. The second column shows the estimates using the elections that occurred in the last four years, and the third column uses elections that occurred in the last eight years. In each column, the second estimate reports the rescaled values, which have been adjusted to ensure that the units of the regression discontinuity (RD) design are comparable to those used in the far-half and border county design. The last column presents the summed estimates. The estimate on the bottom right sums the rescaled estimates for all types of elections from the three most recent elections, and is our main estimate. Bootstrapped standard errors are provided in parentheses. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$.

Figure 1: Specification Curve for Far Part Design



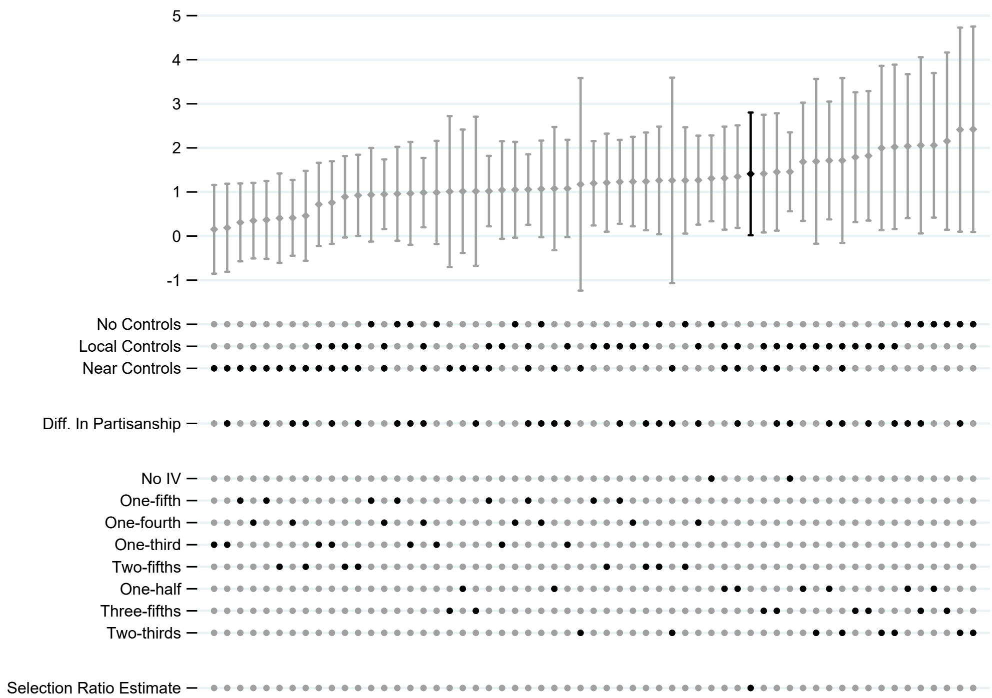
Notes: Each dot represents an estimate for a given specification of the far half design, with bars representing the 95% confidence interval.. We highlight the result of our selection ratio approach (our preferred estimate) in blue; this approach is described fully in Section 4.4. No IV indicates an OLS regression. All specifications include year fixed effects and use robust standard errors clustered at the state level.

Figure 2: Far Half Selection Ratio Extrapolation



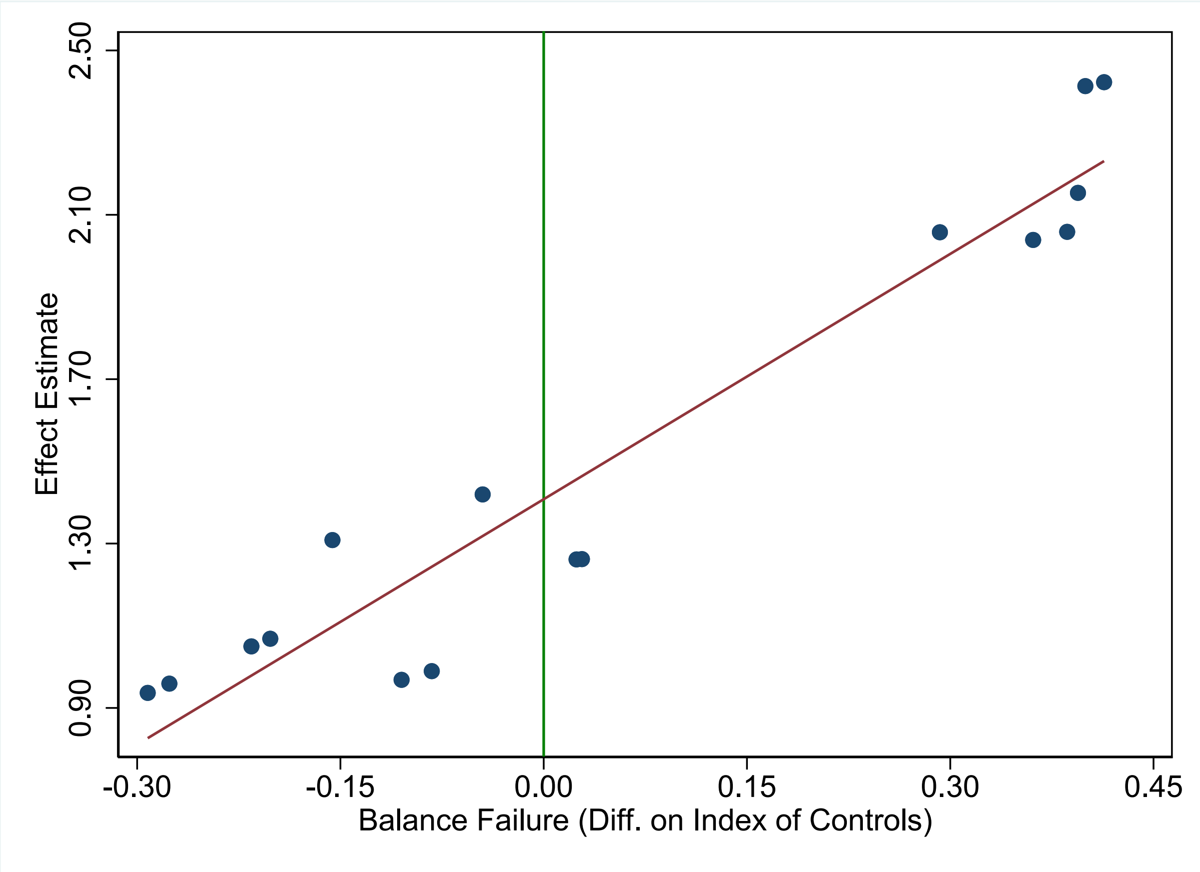
Notes: Each dot in this scatter plot represents a combination of cutoff and regional controls. The y-axis represents the estimate from our far half design. The x-axis represents the estimate from regression age-adjusted mortality on an index of controls. We generate this index by first regressing age-adjusted mortality on state partisanship and our full set of controls. We then generate our predicted age-adjusted mortality from this regression, removing the portion predicted by state partisanship.

Figure 3: Specification Curve for Border County Design



Notes: Each dot represents an estimate for a given specification of the border county design, with bars representing the 95% confidence interval. Our preferred estimate from the border county design is highlighted in black, which includes differences in local and near characteristics, the difference in local partisanship, and instruments the difference in rest of state partisanship with the difference in far half partisanship. We highlight the result of our selection ratio approach (our preferred estimate) in black; this approach is described fully in Section 4.4. No IV indicates an OLS regression. All specifications include year fixed effects and use robust standard errors clustered at the state level.

Figure 4: Border County Selection Ratio Extrapolation



Notes: Each dot in this scatter plot represents a combination of cutoff and regional controls. The y-axis represents the estimate from our border county design instrumenting the difference in rest of state partisanship with the difference in far part partisanship, controlling for the difference in near part partisanship. The x-axis represents the estimate from regressing the difference in age-adjusted mortality on our index of controls. We generate this index by first regressing age-adjusted mortality on the difference in rest of state partisanship and our full set of controls. We then generate our predicted difference in age-adjusted mortality from this regression, removing the portion predicted by the difference in rest of state partisanship.

A Appendix A: Additional Tables & Figures

A.1 Border County Balance Test

Balance test results for the border county design are reported in Table A1. Each row represents a covariate used as the left-hand side variable in Equation 5.1. In Column 1, we do not instrument for the difference in partisanship. The results indicate balance on all covariates except the difference in own-county partisanship. In Column 2, we instrument for the difference in rest-of-state partisanship using the partisanship of the far half of the state. This produces balance on local partisanship, but a failure of balance on Share White. Note that these balance tests are implemented without any controls.

Table A1: Border County Balance Test Table: Local Characteristics

	(1) Diff in Outcome	(2) Diff in Outcome (IV)
Local Partisanship	0.0586**	-0.0133
	(0.020)	(0.037)
Share White	-0.1275	-0.2619**
	(0.070)	(0.102)
Share Black	0.0096	0.0573
	(0.038)	(0.058)
Share Hispanic	-0.0511	0.0383
	(0.054)	(0.062)
Share Asian	-0.0011	-0.0074
	(0.009)	(0.015)
AAR: Receiving Counties	4.5385	31.1998
	(24.466)	(37.169)
AAR: Sending Counties	0.3405	22.1122
	(26.023)	(38.882)
AAR: Facebook Counties	0.3523	0.6701
	(0.412)	(0.801)
Urbanicity	0.0053	-0.0003
	(0.007)	(0.009)

Notes: The dependent variable in each row is the difference in the covariate indicated by the row title. We define the difference as the variable in the redder county minus the variable in the bluer county. Column 1 reports the OLS coefficient of the difference in the outcome variable on the difference in rest of state partisanship. Column 2 instruments the difference in rest of state partisanship with the difference in far half partisanship, controlling for the difference in near half partisanship. Both columns include year fixed effects and report cluster robust standard errors at the state level in parentheses. A full description of each covariate can be found in Section 2. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$.

A.2 Additional RD Results

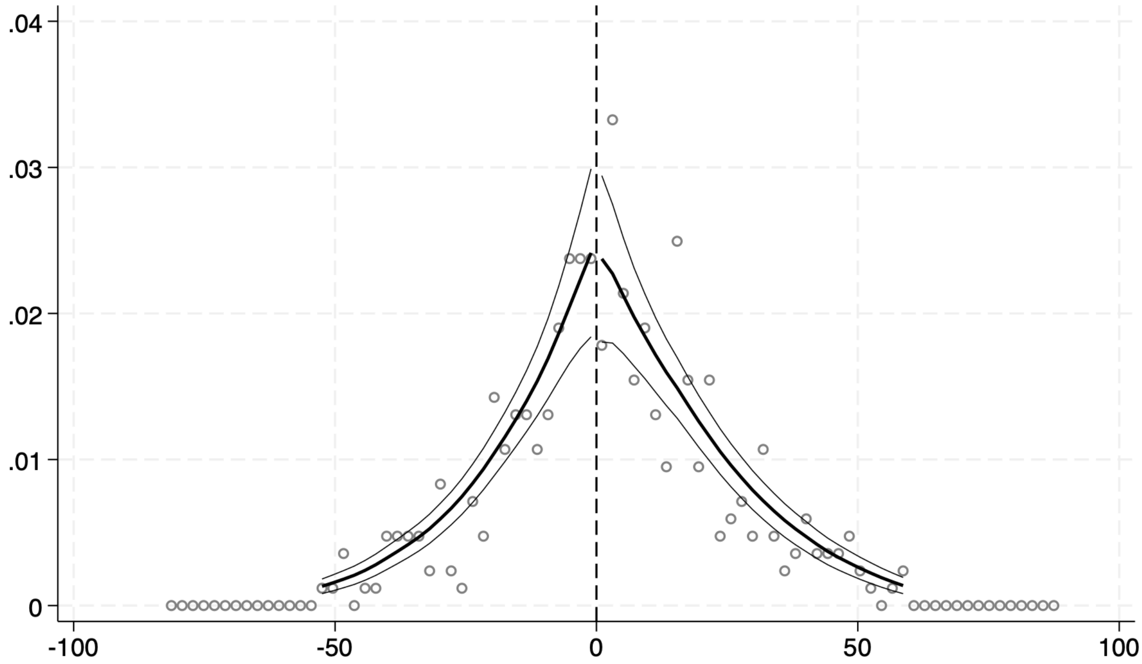
In this section, we present additional figures related to our RD analysis. A central assumption for a valid RD is that there is no sorting of observations to just one side or the other of the cutoff. We test this by assessing whether the density of observations is the same on both sides of the threshold (McCrary, 2008). Figures A1 and A2 present the results of the McCrary test for gubernatorial, Senate, and House discontinuities. The figures show that the density of observations is continuous at the threshold for gubernatorial (p=0.939) and House (p=0.140) elections, consistent with the hypothesis of no sorting. We do not observe continuity for Senate (p=0.004) elections, suggesting the possibility of sorting. From Figure A2, we can see that this is driven by a lack of observations barely to the left of 0, i.e., a lack of instances where Democrats had extremely narrow legislative majorities.

Figures A3, A4, and A5 show the sensitivity of the (unrescaled) RD estimates to the choice of bandwidth. Each dot represents a point estimate, with the error bars representing the 95% confidence interval. Our preferred estimate is shown in black, while estimates under alternative bandwidths are shown in red.[13] Results are broadly similar across choices of bandwidth, though the smallest bandwidths produce results which are less precise and more variable.

Finally, in Table A2, we test for continuity of observables. The estimates reported in this table are the equivalents of our main RD estimate (i.e., they are the sum of rescaled estimated effects from each combination of election type and time frame), but replacing the actual outcome variable (state AAMR) with the variable listed in each row. These observable characteristics appear to be continuous.

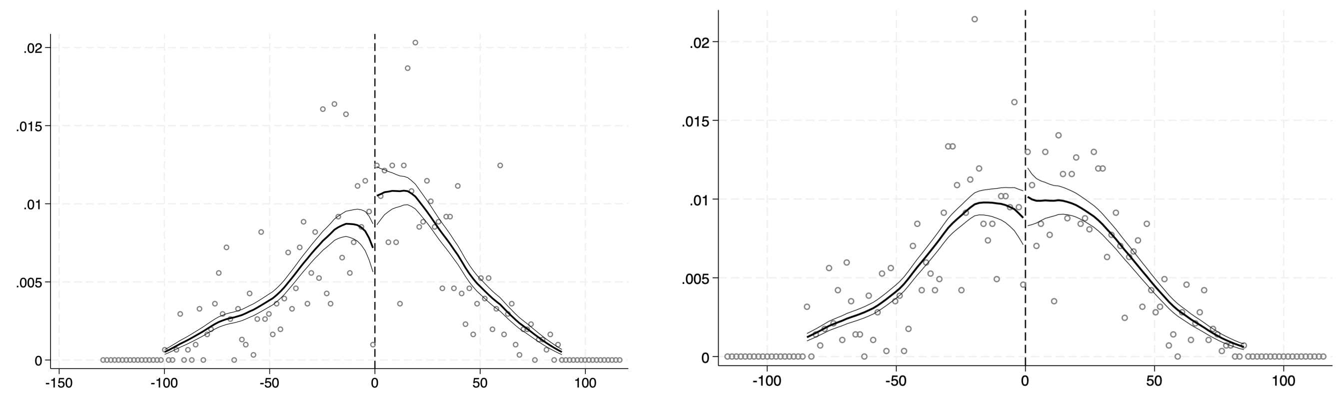
[13]We use the bandwidth selectors in Calonico, Cattaneo, and Titiunik (2014).

Figure A1: McCrary (2008) Test for Gubernatorial Elections



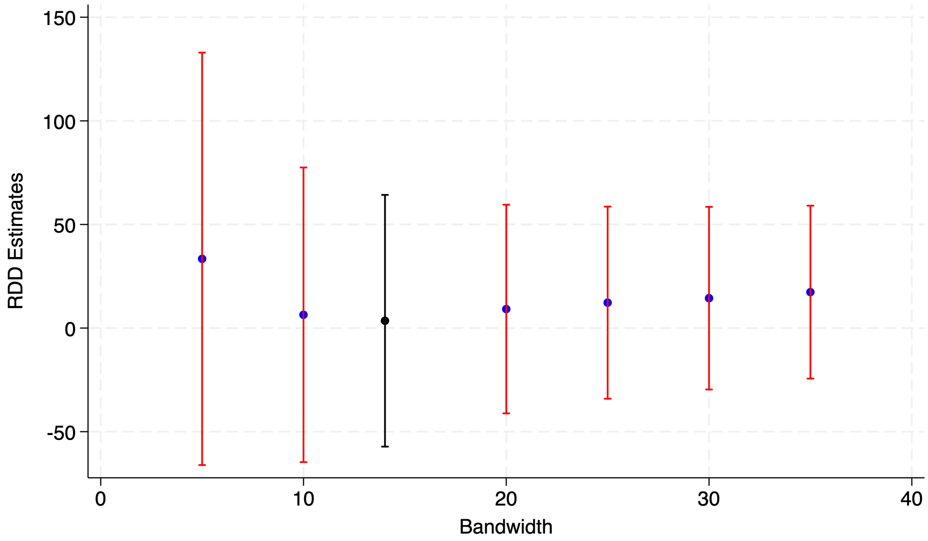
Notes: The running variable is the margin of victory of the Republican candidate at the most recent gubernatorial election, and the y-axis represents the density of the running variable. The cutoff value is 0.

Figure A2: McCrary Test for Senate Elections (Left) and House Elections (Right)



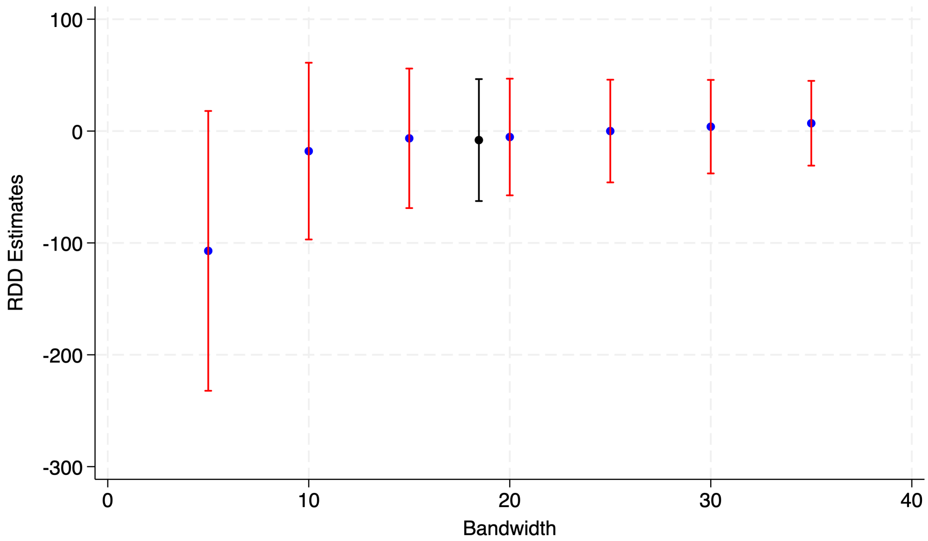
Notes: The running variable is the margin of victory of the Republican Party in the most recent House/Senate election, measured as a percent of possible seats, and the y-axis represents the density of the running variable. The cutoff value is 0.

Figure A3: Bandwidth Sensitivity for Gubernatorial Election Estimates



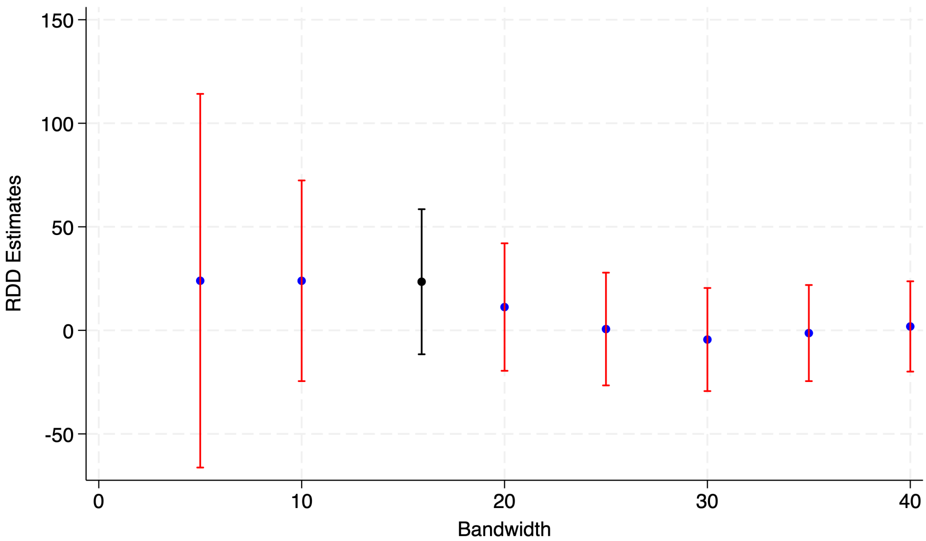
Notes: Each dot represents an estimate using gubernatorial elections for a given bandwidth of the RD design, with bars representing the 95% confidence interval. Our preferred estimate is highlighted in black.

Figure A4: Bandwidth Sensitivity for House Election Estimates



Notes: Each dot represents an estimate using the House elections for a given bandwidth of the RD design, with bars representing the 95% confidence interval. Our preferred estimate is highlighted in black.

Figure A5: Bandwidth Sensitivity for Senate Election Estimates



Notes: Each dot represents an estimate using the Senate elections for a given bandwidth of the RD design, with bars representing the 95% confidence interval. Our preferred estimate is highlighted in black.

Table A2: Continuity Analysis

	Rescaled Estimates
AAR: Facebook Counties	-0.204
	(1.852)
AAR: Receiving Counties	-0.527
	(1.360)
AAR: Sending Counties	-0.000
	(1.362)
Share White	-0.206
	(0.461)
Share Black	0.110
	(0.462)
Share Hispanic	0.256
	(0.274)
Share Asian	0.076
	(0.122)
Urbanicity	-0.029
	(0.041)
Prior State Partisanship	-0.057
	(0.311)

Notes: The dependent variable is displayed in each row. The table presents the summed total estimates for all types (Governor, Senate, and House) of elections from the three most recent elections and rescales them to make them comparable to those used in the far-half and border county design (similar to the RD design). Bootstrapped standard errors are provided in parentheses. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$.

B Appendix B: Age and cause specific analyses

We investigate the extent to which we observe heterogeneity by 10-year age groups and cause of death. Note that the CDC suppresses mortality rate information for any county-year-age group or county-year-cause of death combination in which the number of deaths is less than 20. For many ages and causes of death, this results in a large number of missing observations. This problem is especially exacerbated for the border county analysis, since that design requires information for *both* counties in the pair, so we only report estimates from the far part design. For simplicity, we report estimates using a distance cutoff of one-half, the full set of local and near part controls, and controls for Census region.

Results by age are reported in Table B1. The estimates for young age groups are precise and statistically significant. However, note the bottom row of the table, which reports the number of counties: For younger age groups, mortality information is suppressed in the large majority of counties. Because observations are missing not at random (due to suppression when counts are below 20), these estimates are almost certainly biased due to sample selection.

Table B1: Far Half Results By Age Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	15-24	25-34	35-44	45-54	55-64	65-74	75-84	85+
State Partisanship	0.8985**	1.1889*	0.4846	0.4067	-0.2535	1.1690	-0.0647	1.9459
	(0.3042)	(0.4731)	(0.7622)	(0.9493)	(1.1691)	(2.2912)	(3.6681)	(7.8025)
N	6590	9546	15540	29459	41918	48677	53361	54868
R^2	0.5958	0.6476	0.6426	0.5839	0.5632	0.4726	0.3486	0.1646
Stage 1 F Stat.	158	231	276	331	283	270	260	265
Number of Counties	526	780	1128	1917	2495	2739	2878	2932

The dependent variable in each column is the mortality rate for the 9-year age group indicated in the column title. All columns represent the estimate of our preferred far half specification in which we control for local and near characteristics, US Census Region dummies, year fixed effects, and in which we use a cutoff of one half. All columns include cluster robust standard errors at the state level in parentheses. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$.

We similarly report estimates of the far half design using a one-half cutoff for distinct causes of death in Table B2. This table contains some major causes of death that are presumably more tangentially related to state policy decisions, such as cancer and heart-related deaths, and other causes of death that have a more direct connection with state policy decisions, such as homicides, gun-involved, and drug-related deaths. We identify specific causes of death using the ICD 10 113 Cause List Code in the CDC WONDER data. We identify causes of death related to drugs and alcohol based on the Drug and Alcohol Induced code, also from the CDC WONDER database.[14]

For causes of death more tangentially related to policy, like heart disease, we find no impact of state partisanship through state policy. For causes of death related to the availability of guns, we observe coefficients which are unambiguously different from zero. As with the age-specific analysis, observations are missing not at random, and sample selection is particularly severe for uncommon causes of death—notably including gun-related causes of death.

[14]Note that a single death may count for multiple categories. We define causes of death as follows: Gun deaths: ICD10 113 codes GR113-128, GR113-132, and GR113-119. "Bottle" deaths (alcohol-related deaths): Drug and Alcohol Induced codes A9 and A1. "Drug" deaths (drug-related deaths): Drug and Alcohol Induced codes D1, D9, D3. "Heart" deaths (cardiovascular related deaths): ICD10 113 code GR113-053. Homicides: ICD10 113 code GR113-127. Suicides: ICD10 113 codes GR113-124, GR113-125, Drug and Alcohol induced code D2. Vehicle deaths are defined as any IC10 113 Code descriptions containing the word "Transport." Cancer deaths are defined as any IC10 113 Code descriptions containing the word "Malignant." Respiratory deaths are defined as any IC10 113 Code descriptions containing the word "Lower Res."

Table B2: Far Half Results By Cause of Death

	(1) Gun	(2) Bottle	(3) Drug	(4) Heart	(5) Homicide	(6) Suicide	(7) Vehicle	(8) Cancer	(9) Respiratory
State Partisanship	0.5341***	0.0575	0.4099*	0.2897	0.3610**	-0.0379	-0.0114	-0.0167	-0.0505
	(0.1385)	(0.0664)	(0.1877)	(0.3276)	(0.1262)	(0.0425)	(0.0682)	(0.1553)	(0.1226)
N	2289	5718	7497	55330	3242	18123	9953	51006	28138
R^2	0.7415	0.7440	0.6160	0.5580	0.7397	0.5406	0.7220	0.3446	0.4759
Stage 1 F Stat.	151	135	234	282	148	183	197	287	340
Number of Counties	201	537	741	2930	260	699	850	2796	1944

The dependent variable in each column is the mortality rate for the cause of death indicated in the column title. Note that a single death may count for multiple categories. We define causes of death as follows: Gun deaths: ICD10 113 codes GR113-128, GR113-132, and GR113-119. “Bottle” deaths (alcohol-related deaths): Drug and Alcohol Induced codes A9 and A1. “Drug” deaths (drug-related deaths): Drug and Alcohol Induced codes D1, D9, D3. “Heart” deaths (cardiovascular related deaths): ICD10 113 code GR113-053. Homicides: ICD10 113 code GR113-127. Suicides: ICD10 113 codes GR113-124, GR113-125, Drug and Alcohol induced code D2. Vehicle deaths defined as any IC10 113 Code descriptions containing the word “Transport”. Cancer deaths defined as any IC10 113 Code descriptions containing the word “Malignant”. Respiratory deaths defined as any IC10 113 Code descriptions containing the word ”Lower Res”. The CDC suppresses data for counties which had fewer than 20 deaths for a given cause of death in a given year. All columns represent the estimate of our preferred far half specification in which we control for local and near characteristics, US Census Region dummies, year fixed effects, and in which we use a cutoff of one half. All columns include cluster robust standard errors at the state level in parentheses. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$.