

# The Targeting and Impact of Partisan Gerrymandering: Evidence from a Legislative Discontinuity

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## Abstract

A party's influence over redistricting increases discontinuously when its seat share in the state legislature exceeds 50 percent. We apply bunching tests to show that, in the election before redistricting, parties systematically win narrow majorities in legislatures of states where they have lost recent U.S. House races. This trend of losses is reversed after redistricting despite no change in overall House vote shares in states near the cutoff. The pre-to-post-redistricting change in regression discontinuity estimates implies that the party that controls redistricting engineers an 11 percentage point increase in its probability of winning a House race.

*JEL Codes:* D72, D78, H10, K00

*Keywords:* close elections, sorting, state legislature, electoral competition, redistricting

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# 1 Introduction

After decades of obscurity, Congressional redistricting has become a subject of fear and fascination in the public imagination. Major news sources are tracking the 2020 redistricting in real time on the premise that the resulting maps will tip the partisan balance and even harm democracy.<sup>1</sup> But the academic literature has drawn more nuanced conclusions about the impact of partisan redistricting, ranging from studies that suggest it is relatively inconsequential (Chen and Rodden, 2013), to those that find it creates the potential for sizable unfairness (McGhee, 2014), to older studies that suggest it may even enhance democracy (Gelman and King, 1994a).

Any attempt to measure the impact of partisan redistricting faces two key challenges. The more obvious challenge is to somehow find a proper “control” group, an otherwise identical state that either was not gerrymandered or was gerrymandered by the party out of power. But even leaving aside questions of causal inference, the implications of gerrymandering for democracy rest on more than the size of the advantage gained by whichever party draws the map. They also depend on the electoral trends within the states where parties consciously seek to gain this advantage—or rather, those where they ultimately are left in control as a result of their conscious actions. If parties seek and gain control of redistricting in states where they have lost recent elections, the implications would be more serious than if control were allocated at random. In the former case, parties would in effect be gerrymandering to slow or undo swings in the electorate, diluting voters’ power to change their representation.

This paper aims to answer these two questions: do parties consciously prioritize controlling redistricting in states where electoral trends have turned against them? And if so, what is the impact on post-redistricting U.S. House Races? We answer these questions using a novel approach that hinges on a natural experiment created by the rules of redistricting. Whichever party controls the state assembly has great influence—at least a veto—over the

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<sup>1</sup>See, for example: FiveThirtyEight, <https://projects.fivethirtyeight.com/redistricting-2022-maps/?cid=rrpromo> ; the New York Times, <https://www.nytimes.com/2021/09/16/us/politics/redistricting-gerrymandering.html> ; and Politico, <https://www.politico.com/news/2021/11/18/whos-winning-congressional-redistricting-how-we-calculated-our-figures-522831> .

state's redistricting plan.<sup>2</sup> Control switches discontinuously from Republicans to Democrats when the Democrats' percentage of assembly seats exceeds 50 percent. Each party has a strong incentive to ensure the number of seats it wins in the state assembly election just before redistricting (call this the "redistricting election") is above the cutoff.

Our approach to estimating where parties seek and gain control, which we call the "Selection Effect," draws on the literature on bunching and sorting. This literature infers the preferences and abilities of agents by testing how they adjust some continuous outcome to ensure it falls on one side of an arbitrary cutoff. In the absence of precise control any pre-determined characteristic should be a continuous function of the election outcome. A state where Democrats win 49 percent of seats in the assembly should be similar to one where they win 51 percent. Any discontinuity will arise only as a result of conscious efforts by parties to win the majority before redistricting. If a characteristic determined before the redistricting election increases discontinuously at the cutoff, states with this characteristic must be either easier or more attractive for parties to "sort" onto the side of the cutoff where they control redistricting. The key pre-determined characteristic in our study is the outcome of U.S. House races decided *before redistricting*.

We then measure the discontinuity in the outcomes of House races after redistricting. Since these outcomes *are* affected by redistricting (the "Causal Effect" of redistricting), this post-redistricting discontinuity is a combination of the Selection Effect and the Causal Effect. We can isolate the Causal Effect by netting out the Selection Effect. Since the Selection Effect is simply the discontinuity in the outcomes of House contests that occur before redistricting, we net out the Selection Effect by taking the difference in the discontinuities measured for outcomes before and after redistricting.

The key assumption behind this difference-in-discontinuities approach is that the Selection Effect does not abruptly change from before to after redistricting. We show that in states in the neighborhood of the cutoff, while the average number of seats won by the ruling party

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<sup>2</sup>Some states, such as Florida, are at least in principle barred by their state constitution from adopting maps that favor one party over another. To the extent that these rules bind, our results may actually underestimate the impact of partisan gerrymandering.

increases after redistricting, its average vote share does not. Since the number of seats won depends only on the overall vote share and the distribution of those votes across districts, the lack of any average change in the overall vote share implies the average increase in seats must be caused by the redrawing of district boundaries.

We apply this method to elections from the 1970–2010 redistricting cycles in states where the legislature controlled redistricting. We find that parties systematically win majorities (even in very close legislative elections) in the assemblies of states where the opposition has made recent gains in U.S. House elections. The opposition loses its gains in the election immediately after redistricting. The probability a Republican candidate wins a contest for the U.S. House falls by 11 percentage points when Democrats control the assembly during redistricting. Yet this anti-opposition Causal Effect is short-lived and has largely faded by the next election.

We find evidence that gerrymandering is the mechanism behind these effects. After redistricting there is a sharp decline in how efficiently each vote for the opposition is converted into a seat. A gerrymandered map can achieve this by “packing” the other party’s voters into the smallest possible number of districts. We find that Republican legislatures are roughly 15 percentage points more likely than Democratic legislatures to move majority-black census tracts to new districts, and conditional on being moved they are more likely to be moved into districts that are already overwhelmingly black. These differences are not obviously explained by any objective need for Republican-controlled states to disproportionately redistrict black census tracts.

Our results suggest that, at least in highly competitive states, gerrymandering has a substantial impact. Moreover, the struggle to control redistricting leaves it in the hands of whichever party has recently lost votes and elections, suggesting that gerrymandering works to forestall changes in public sentiment.

## 1.1 Relation to the Empirical Literature on Partisan Redistricting

The literature on partisan redistricting has generally taken two approaches: using simulations to evaluate the fairness of a redistricting plan, and comparing actual election outcomes under different redistricting plans.

One branch of the simulation literature measures the responsiveness and partisan bias of a redistricting plan by simulating how the number of seats won by a party changes as its vote share changes (e.g. Gelman and King, 1990, 1994a,b; Engstrom, 2006a). The most influential of these studies conclude that redistricting actually makes the number of seats won more responsive to changes in a party's support. Another branch of this literature takes a geographical approach, holding fixed the (predicted) votes cast within each precinct and comparing how outcomes would have differed under the old and new redistricting plan (e.g. Glazer et al., 1987) or under the actual plan versus simulated non-partisan plans (e.g. McCarty et al., 2009; Chen and Rodden, 2013; Chen and Cottrell, 2016). Several of these studies have concluded that the actual plans are no more favorable than would have arisen by chance. Finally, in response to (ultimately unsuccessful) litigation aimed at ruling gerrymandering unconstitutional, there has been more recent literature (e.g McGhee, 2014) that defines measures of partisan fairness that could theoretically be used to evaluate a redistricting plan.

These studies implicitly assume voting behavior would be similar under an alternative district map. If being gerrymandered into an uncompetitive district makes certain voters less likely to turn out (Fraga, 2016) or deters strong candidates from challenging incumbents (Williamson, 2019), then this assumption may no longer hold. Our study complements the simulation literature by estimating the impact of gerrymandering using a different approach. We estimate the counterfactual by comparing outcomes across the legislative discontinuity. Our claim is not that our assumption is “right” while the prior literature is “wrong,” only that taking a different approach can yield a fresh perspective on an area of extraordinary public interest.

The rest of the literature compares actual outcomes under plans proposed by Democrats,

Republicans, or independent commissions. Several studies compare outcomes over time (Brunell and Grofman, 2005), over the course of the redistricting cycle (Hetherington et al., 2003), or under plans set by different redistricting authorities (Grainger, 2010). Other work estimates the responsiveness of seat shares to vote shares empirically (Cox and Katz, 2002; Engstrom, 2006b). Several studies estimate the effect of redistricting using some form of difference-in-differences (Ansolabehere and Snyder Jr, 2012; Carson et al., 2007; McCarty et al., 2009; Lo, 2013). Comparing actual outcomes is valid only if the comparison group—different states, different election cycles—is an accurate counterfactual. The counterfactual is invalid if there are differential trends in the attitude of the electorate or if parties actively seek control of certain states in anticipation of redistricting. Our research design is able to account for both omitted confounders and the Selection Effect.<sup>3</sup>

Finally, there is a distinct literature that studies incumbent gerrymandering, the bipartisan attempt to help incumbents get reelected (e.g. Abramowitz et al., 2006; Carson et al., 2014). Of these, Friedman and Holden (2009) is most relevant because they use a regression discontinuity design. They take the election year as their running variable and test for a discontinuous change in the incumbent reelection rate in the first election after the Census in each redistricting cycle. Their approach cannot be used to test for partisan redistricting, which is why they focus solely on incumbent gerrymandering.

## 2 When and How is Redistricting Done?

Most states redraw their Congressional boundaries by passing a law. The state legislature, which comprises a lower and upper house, approves a bill. This bill, if signed by the governor, becomes law. The next election to the U.S. House is contested under the redrawn district

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<sup>3</sup>There is also a theoretical literature that identifies how a party should gerrymander. The earliest theoretical work (e.g. Owen and Grofman, 1988) finds that the optimal gerrymander would “pack” and “crack” opponents to minimize their influence. More recent work (e.g. Friedman and Holden, 2008; Puppe and Tasnádi, 2009; Cox and Holden, 2011) has found that the optimal gerrymander may be more sophisticated if the party has a different set of information or faces additional constraints (although Gul and Pesendorfer, 2010, is a more recent affirmation of packing and cracking). Our results suggest actual gerrymandering is consistent with packing and cracking.

map.

Though control over a single chamber does not grant complete control over redistricting, it does grant a veto. When Democrats gain control of the lower house they are able to vote down any unfavorable plan, giving them a strong incentive to take control of the legislature just before the redistricting process begins.<sup>4</sup> Control switches discontinuously away from Republicans when Democrats win at least 50 percent of seats. Assuming that Democrats can maintain strict party discipline, this logic suggests the redistricting plan should become discontinuously more favorable to them when they achieve a majority.

Figure 1 suggests that this assumption is valid. Using data from several states, we plot the fraction of Democrats and Republicans voting yes on the 2011 redistricting bill against the percentage of seats in the state assembly won by Democrats in the previous election.<sup>5</sup> When Democrats gain control of the assembly they switch from near-universal opposition to near-universal support for the redistricting bill. Republicans are slightly less unified but still sharp in their response. This reversal of support suggests that control of the assembly triggers a sharp change in the type of plan proposed. Moreover, it suggests there is strong party unity—just below the cutoff, 100 percent of Republicans and 0 percent of Democrats vote for the bill. Such unity implies winning 50 percent of the seats really does grant a measure of control over the redistricting plan passed by the lower house. If the party controls no other branches of state government, control of the lower house lets it block a severe partisan gerrymander and haggle for a more balanced map. If it also controls the other branches, it gains the power to impose a partisan gerrymander on the other party. Either way, parties gain enormous benefit by winning a majority in the lower house in years when they have the opportunity to redraw district maps.

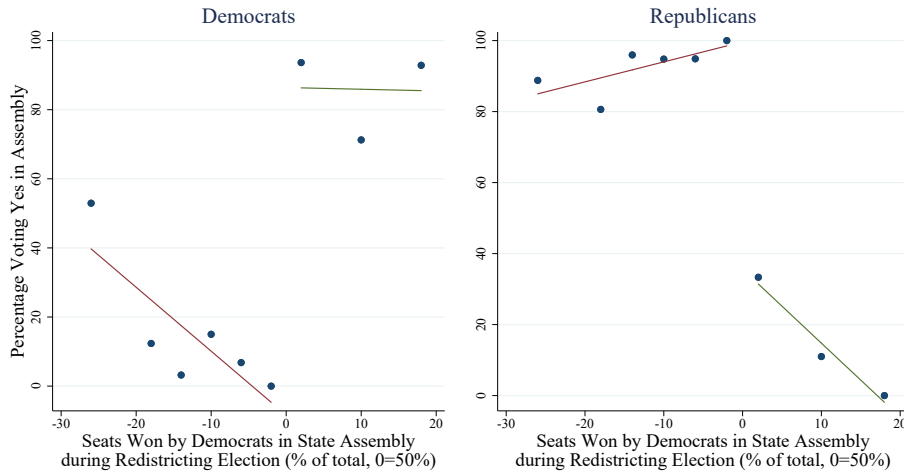
That opportunity arrives every ten years with the decennial census. Aside from making it

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<sup>4</sup>We focus on the lower house because most states stagger the terms of members of the upper house (much like the U.S. Senate). Only a fraction of seats are contested in the election before redistricting, meaning the threshold for the number of contested seats that need to be won will vary by state and may in some cases exceed 100 percent.

<sup>5</sup>The roll call votes were constructed from Vote Smart (2016), which has roll call votes on 51 bills from 21 states for the 2011 redistricting cycle.

**Figure 1**  
State Assembly Members Vote for the Redistricting Bill  
when their Party Holds a Majority



*Note:* The figure shows the fraction of members in the lower house of the assembly who voted in favor of the redistricting bill during the 2011 redistricting cycle.

possible to create districts with equal populations, the census helps the party in power gerrymander on demographics. As shown in Figure 2, the census is completed in years ending in 1.<sup>6</sup> Whichever party wins the election to the state legislature just before this year has the opportunity to pass its own redistricting plan.<sup>7</sup> These key state elections, labeled onward as “redistricting elections,” create the variation we exploit to estimate the Selection Effect and Causal Effect of redistricting.

### 3 Motivating the Research Design: Visual Evidence

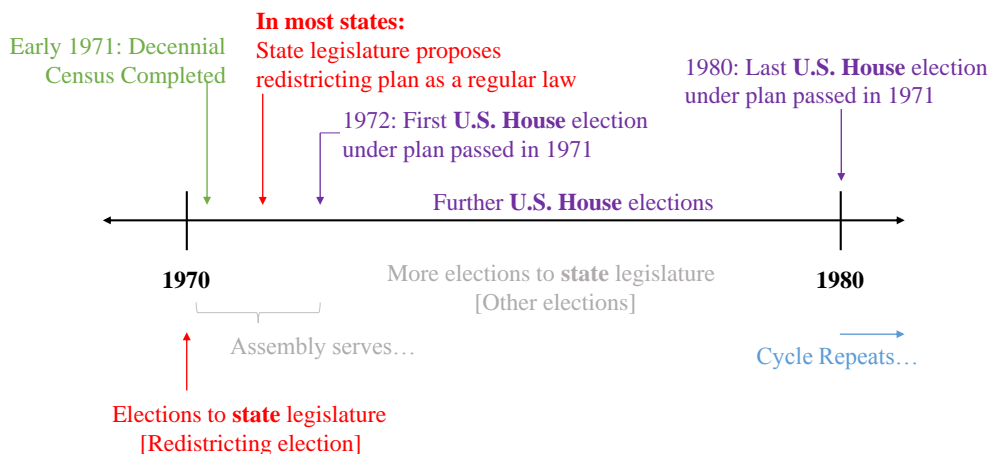
The reasoning behind our research design is most easily explained through a series of figures familiar from the literature on regression discontinuity design. In each panel of Figure 3 we

<sup>6</sup>The redistricting bill might not be passed in the year ending in 1 if, for example, the legislature is divided and the bill is particularly contentious. As a result, the date of passage is both unpredictable and endogenous to our outcome of interest. Instead we focus on the opportunity to redistrict, which comes with the completion of the census.

<sup>7</sup>In many states the election is in years ending in 0, but a few states are irregular. We define the most recent election before a year ending in 1 as the redistricting election.



**Figure 2**  
Schedule of Redistricting



*Note:* The figure shows the redistricting cycle for a typical state (i.e. a state with lower house elections in even years).

plot on the horizontal axis the share of seats won by Democrats *in the state assembly during the redistricting election*.<sup>8</sup> We divide the range into bins of 3 percentage points and plot on the vertical axis the fraction of U.S. House races won by the Republican within each bin. All four figures have the same horizontal axis and differ only in the time frame of the U.S. House races plotted on the vertical axis.

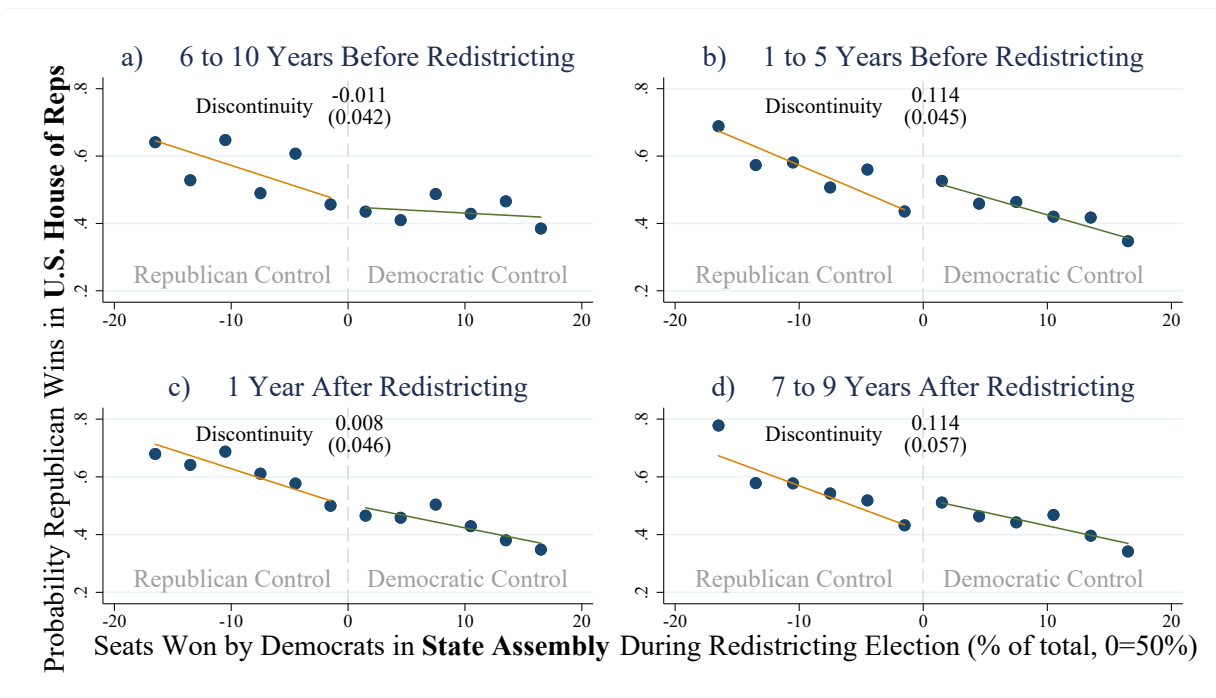
Panels a and b are constructed using the outcomes of U.S. House races that occur *before* the redistricting election. Since these outcomes are predetermined at the time of redistricting, they cannot be affected by it. These figures are thus similar to standard tests for “sorting” or precise control (see Lee, 2008, for example). The motivation behind such regressions is that in the absence of precise control, states where Democrats barely win a majority just before redistricting should be similar to those where they barely lose. Since the 50 percent cutoff for a majority is arbitrary, any confounding factor that makes Democrats more likely to win in both the state assembly and the U.S. House should, in expectation, be the same just above and below the cutoff.

Panel a, which focuses on U.S. House races many years before redistricting, shows no evidence of a discontinuity. But there is a large discontinuity in Panel b, races just before

<sup>8</sup>The average number of U.S. House races per bin is 230, 338, 177, and 115 for each panel (clockwise).

**Figure 3**

The Selection Effect and Causal Effect are Visible in the Data



*Note:* Each panel plots the fraction of U.S. House races won by Republicans against the percentage of seats (relative to 50%) won by Democrats in the state assembly election that determines control of redistricting. The unit of observation is a U.S. House race. Each dot shows the average of the outcome within a bin of width 3. We report the regression discontinuity estimate implied by a local linear regression within a bandwidth of 18 percentage points. Standard errors are clustered by state-redistricting event.

redistricting. We estimate the size of the discontinuity using a local linear regression within a bandwidth of 18, though the results are similar at other bandwidths (see below). States where Democrats subsequently win a narrow majority in the state assembly were 11 percentage points more favorable to Republican U.S. House candidates before redistricting. Since these elections cannot be affected by (future) redistricting and cannot arise naturally, the figure suggests parties have taken conscious actions to “sort” desired states onto the winning side of the cutoff. Democrats (Republicans) barely win enough seats to hold a majority in state assemblies where Republicans won (lost) relatively more seats in U.S. House elections 1 to 5 years before the redistricting election. Since this sorting does not arise by chance, it is the first clear evidence of what we call the Selection Effect.

Panels c and d are constructed using U.S. House elections that occur *after* the redistricting election, meaning these outcomes may have been affected by redistricting (what we call

the Causal Effect of redistricting), but they also have been selected as discussed in the previous paragraph. The key to separating these effects is to recall that the running variable is the same across all of these figures. The states represented by the “dot” just to the right of the discontinuity in Panel c are the same as those represented by the dot in that position in Panel b. Any change in the height of the dot between these figures is caused by a change in election outcomes in these same states from before to after redistricting.

Panel b suggests the states “sorted” to the right of the cutoff were, before redistricting, relatively favorable to Republicans. Panel c suggests that after redistricting *these same states* are suddenly much less favorable relative to those to the left of the cutoff. The Selection Effect that was visible just before redistricting vanishes immediately afterwards.

The change in outcomes from Panels b to Panel c suggests a difference-in-discontinuities estimator will isolate the Causal Effect of redistricting. Assuming the Selection Effect does not abruptly change from before to after redistricting, states sorted to one side of the cutoff should be as favorable to Republicans before as after redistricting. Any change in their probability of winning must be caused by redistricting.

Then we can estimate the Causal Effect of redistricting by differencing the discontinuity estimated in Panel b from that estimated in Panel c. This difference-in-discontinuities is clearly negative. States that were formerly 11.4 percentage points more likely to elect a Republican (Panel b) are, after redistricting, no more likely to elect a Republican. Democratic control of redistricting transforms relatively red states into neutral states (and vice-versa) in the U.S. House election immediately after redistricting.

But Panel d, which shows the outcomes of elections many years after redistricting, looks very much like Panel b. The states where Democrats took control of redistricting are, again, 11.4 percentage points more likely to elect a Republican to the U.S. House, implying the original Selection Effect visible in Panel b has reappeared. The difference in discontinuities between panels b and d is zero, from which we infer the Causal Effect of redistricting has vanished by this time.

## 4 Precise Control of Redistricting: Is it Feasible, and How Does it Arise?

### 4.1 Feasibility of Precise Control

Panel b of Figure 3 suggests parties are somehow able to systematically win the state-level redistricting election in states where they have sustained recent losses in the U.S. House. Precise control, sometimes called “precise sorting” or “complete manipulation,” arises when an agent has both a means and an incentive to guarantee that some continuous outcome falls on one side of an arbitrary cutoff.

It may seem that prior work rules out precise control. Eggers et al. (2015), for example, find no evidence of precise control in U.S. state assembly races. But they and others who study this issue focus on precise control of individual *races*, e.g. whether State Assemblyman Mark Stone wins reelection. Figure 3 suggests only that parties can exert precise control over the outcome of the state assembly *election*, e.g. how many seats do Democrats win in the California State Assembly. The key difference is that while it may be impossible to ensure 50% + 1 voters vote for Mark Stone (a few people may get sick or be caught in traffic on Election Day), it may be possible to almost guarantee 50% + 1 seats fall to the Democrats.

The skeptical reader may justifiably wonder if the discontinuities showing precise control in Figure 3 are driven by an over-wide bandwidth. Figure 4, which estimates the same 4 discontinuities at a range of bandwidths, suggests that is not the case. The discontinuity in pre-determined outcomes (upper-right panel) is almost unchanged at bandwidths as wide as 22 and as narrow as 8. The result is not a statistical anomaly.

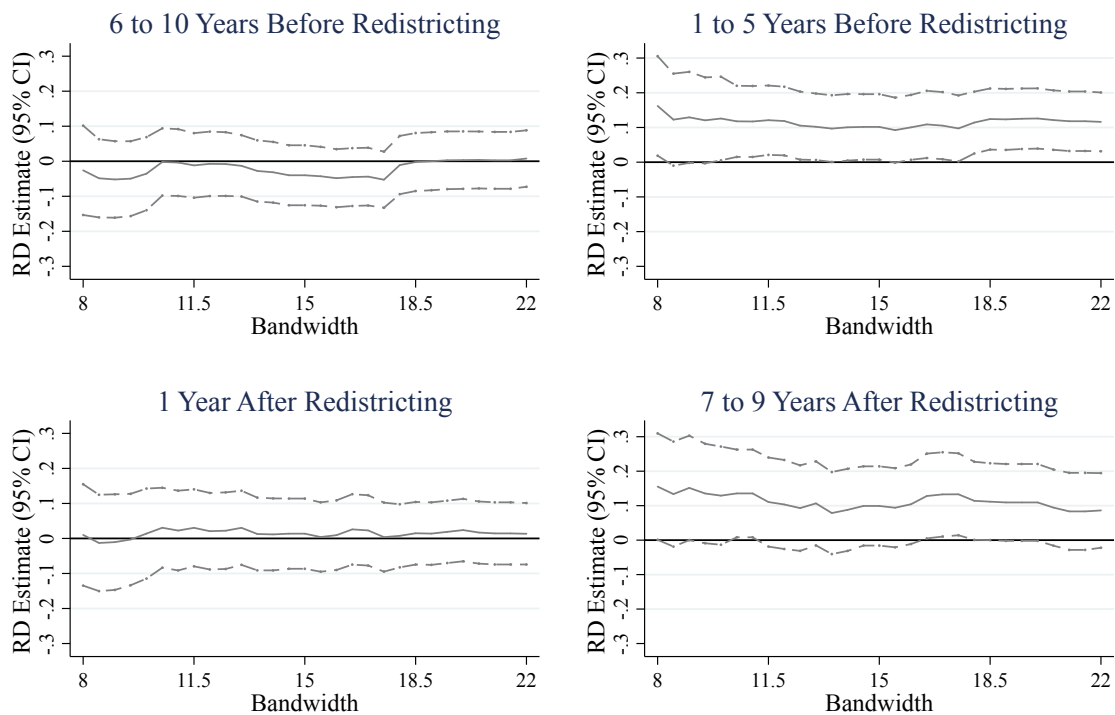
### 4.2 The Tactics and Trends Behind Precise Control

A discontinuity in a pre-determined outcome cannot arise through any purely natural phenomenon. Only the conscious actions of the political parties, guided by accurate forecasting of the state legislative election, can create non-random differences in the states that are

sorted to either side of the 50% cutoff for control of the state assembly.

**Figure 4**

Figure 3 is Not Driven by Choice of Bandwidth



That the parties are taking conscious action is no secret. Prior to the 2010 redistricting cycle, a Democratic operative described his party's extensive preparations:

"It's pretty clear that we're well ahead of them [the Republicans]," said Michael Sargeant, executive director of the Democratic Legislative Campaign Committee (DLCC). He notes the party has been building an infrastructure to handle this redistricting effort for more than six years. (D'Aprile, The Hill, 2010)

The Republican effort was even more comprehensive. Their effort, called REDMAP, left extensive descriptions of their strategic objectives and the resources they marshalled to achieve them:

As the 2010 Census approached, the RSLC began planning for the subsequent election cycle, formulating a strategy to keep or win Republican control of state legislatures with the largest impact on congressional redistricting as a result of reapportionment... To fund the initiative, the RSLC raised more than \$30 million in 2009-2010, and invested \$18 million after Labor Day 2010 alone.

(Republican State Leadership Committee, Jan 3, 2013)

Though the specific objective in this quote may not have been common across parties and time, it suggests that the parties did have broader strategic objectives in deciding which states to target.

But while objectives and resources alone might make a party more likely to win a majority, they cannot fully explain how a party *systematically* wins *a bare majority*. Aside from raising more funds, parties also change the way they allocate those funds during state elections that determine control of redistricting. Makse (2014) and Jeong and Shenoy (2020) show parties switch from the “seat-maximizing” tactics used in typical state elections to “defensive” or “majority-seeking” tactics in redistricting elections. The distinction between these two approaches, first analyzed in a model of legislative competition by Snyder (1989), lies in which districts get the most resources. When the goal is to maximize the probability of holding a majority, the optimal strategy is to channel funds to pivotal districts most likely to put them over the 50 percent threshold. For a party that already holds the majority, the pivotal districts will be held by incumbents.

Jeong and Shenoy (2020) show that Democrats—the majority party in most states during our sample period—do indeed concentrate resources on incumbents. Crucially, they show using simulations that this tactic becomes drastically more effective as the number of incumbent candidates increases. Previously holding a large majority is thus a key mechanism for exerting precise control. Using sorting tests similar to those depicted in Figure 3, they find that in the sample of elections that determine control of redistricting—and *only* in this sample—the party that entered the election with a majority is roughly 4 times as likely to barely retain its majority than to barely lose the majority.

The interplay between the parties’ strategic objectives and their capacity in any given state to exert precise control determined which competitive state elections (and thus which states) were sorted to either side of the cutoff. But broader political trends in part determined which states were competitive to begin with. The major post-war political trends—such as the enfranchisement of blacks and the resulting backlash of Southern whites (Kuziemko and

Washington, 2018), and de-industrialization across broad swathes of the country (Baccini and Weymouth, 2021)—caused major shifts in the partisan lean of states around the country. These trends were big enough to overwhelm district maps that had been gerrymandered in previous rounds of redistricting (Goedert, 2017), making once-secure party strongholds increasingly competitive. Democrats in particular went from controlling over 70 percent of state houses in the 1970s and 1980s to controlling under 40 percent in 2014.

The pattern visible in Panel b of Figure 3 arises from the convergence of these three forces. Broader political trends made previously one-party states competitive at the state and national level. The two parties—especially the Republicans, who were the minority-party in most states during this period—capitalized on these trends by financing concerted efforts to take control of redistricting in several of these newly competitive states. The national parties had to choose which of their own strongholds to barely retain, as it was in areas where they already held a majority that they could successfully execute the majority-seeking tactics that enable precise control. These tactics are costly because the majority party will on average lose seats (by deviating from seat-maximizing tactics). Parties may thus deliberately choose to concentrate these efforts in states where losing control of the state house during redistricting would be most damaging: either areas where their losses in recent federal elections were worse than their losses in state elections, or where broader trends were so unfavorable that they lost the state senate and needed to retain the state house to avoid losing all influence over redistricting. This reasoning explains why the Selection Effect estimated in Panel b of Figure 3 is negative. We now lay out a method to more rigorously analyze that pattern.<sup>9</sup>

## 5 Research Design

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<sup>9</sup>Another possibility is that, within the subset of states controlled by a party, it is only those where it faces negative trends that it even comes close to losing its majority in the state house and must engage in the costly tactics that create precise control. But if this explanation were true, we would expect the losses in federal elections to be concentrated in the election just prior to redistricting. As we show in Section 6.1, the losses in the U.S. House that drive the Selection Effect appear 5 years before redistricting and do not grow any larger in the election just before redistricting.

## 5.1 Regression Equations

Let  $s$  index a state-redistricting event—for example, California’s 1981 redistricting. Define the margin of seats won by Democrats in the state assembly as

$$X_s = \frac{[\text{Democrats in State Assembly}]_s - \frac{1}{2}[\text{Total Assembly Members}]_s}{[\text{Total Assembly Members}]_s} \times 100\% \quad (1)$$

and let  $R_s = \mathbb{I}(X_s \geq 0)$  be a dummy for whether Democrats hold at least 50% of the seats in the assembly. Let  $W_{ist}$  be a dummy for whether the Republican wins the U.S. House race in district  $i$  and House election year  $t$  (for example, CA-20 in 1982). Assume  $t = 0$  is the year of redistricting, so in the prior example 1982 would be  $t = 1$ .

Our first step is to identify the Selection Effect, defined as

**Definition 1 (Selection Effect of Redistricting)** *The electoral lean of states where Democrats get control of redistricting through conscious efforts of the two parties.*

Simply comparing the pre-determined characteristics of states where  $R_s = 1$  to those where  $R_s = 0$  would yield biased estimates because the difference between states need not arise through conscious efforts to control redistricting. A very liberal state like California would likely have a Democratic assembly regardless of whether Democrats actively seek to control redistricting or Republicans consciously forgo control. Our first identifying assumption is that in the absence of precise control, any such confounders vary continuously with  $X_s$ . This assumption, standard in any regression discontinuity design (see Lee, 2008, for example), is plausible because it is unlikely that any natural random process would create discontinuities in the distribution of voter sentiment, demographic characteristics, or any other pre-determined state-level factors that would give Democrats more assembly seats.

This assumption implies that in the absence of precise control there should be no discontinuity in any pre-determined outcome (see Lee, 2008, Proposition 2). Conversely, if there is a discontinuity in some pre-determined outcome—in particular,  $W_{is,k}$  for any  $k < 0$ , a U.S. House race before redistricting—it implies there has been precise control.

Let  $C$  be a row vector of controls. We estimate the Selection Effect by running the regres-



sion

$$W_{ist} = \sum_{k=-9,-7,\dots,-1} \mathbb{I}(t = k) \left\{ \pi_{t0} + \pi_{t1}X_s + \pi_{t2}X_sR_s + \rho_tR_s \right\} + C_{ist}\pi_3 + \nu_{ist} \quad (2)$$

for  $|X_s| < h, \quad t = \{-9, -7, \dots, -1\}$

Equation 2 simultaneously estimates 5 regression discontinuities  $\hat{\rho}_{-9}, \hat{\rho}_{-7}, \dots, \hat{\rho}_{-1}$ , one for each election prior to redistricting. Each estimate comes from a local linear regression within the bandwidth  $h$ . This approach allows the Selection Effect to be fully time-varying, letting us test for a time trend.

Next we turn to estimating the Causal Effect of Redistricting:

**Definition 2 (Causal Effect of Redistricting)** *The change in the probability a Republican wins a U.S. House race when Democrats control the state assembly during redistricting.*

Aside from having to deal with the same confounders that would bias estimates of the Selection Effect, any estimate of the Causal Effect must also be purged of the Selection Effect itself. As described in Section 3, our approach is to strip out our estimate of the Selection Effect using a difference-in-discontinuities estimator.

Consider a new assumption, which we test below: the Selection Effect does not change from before to after redistricting. Though our estimates of the Causal Effect are valid *regardless of whether there is precise control and a non-zero Selection Effect*, we do require that the Selection Effect is constant. Under this assumption we can difference away the Selection Effect by subtracting  $\rho_{k<0}$ , our regression discontinuity estimate from any election just before redistricting (Panel b of Figure 3), from  $\rho_{k>0}$  for any election  $k$  after redistricting.

We first estimate the Causal Effect using a flexible difference-in-discontinuities. For this we must unambiguously assign each House race to a single redistricting event even though most races could be treated as coming either after one redistricting event or before the following event. We assign to an event all elections starting 5 years before through 3 years afterwards. We estimate

$$\begin{aligned}
W_{ist} &= \alpha_0^{base} + \alpha_1^{base} X_s + \alpha_2^{base} X_s \cdot R_s + \rho^{base} R_s \\
&+ \sum_{k=-3,-1,\dots,3} \mathbb{I}(t=k) \left\{ \alpha_0^k + \alpha_1^k X_s + \alpha_2^k X_s \cdot R_s + \rho_k^\Delta \cdot R_s \right\} + \nu_{ist} \\
&\text{for } |X_s| < h, \quad t = \{-5, -3, \dots, 3\}
\end{aligned} \tag{3}$$

The estimates of  $\{\rho_t^\Delta\}_{-5 < t < 0}$  give the Selection Effect relative to  $t = -5$ . If the Selection Effect is constant within the window  $t = \{-5, -3, \dots, 3\}$  then we would expect  $\widehat{\rho}_{-3}^\Delta = \widehat{\rho}_{-1}^\Delta = 0$ . The estimates  $\{\widehat{\rho}_t^\Delta\}_{0 < t \leq 3}$  equal the Causal Effects for 1 year and 3 years after redistricting.

In the results section we find that only  $\widehat{\rho}_1^\Delta$  is nonzero. In our primary specification we maximize the power of our estimate by imposing that the other difference-in-discontinuity estimates are zero:

$$\begin{aligned}
W_{ist} &= \alpha_0 + X_s \alpha_1 + X_s \cdot R_s \alpha_2 + R_s \alpha_3 \\
&+ \mathbb{I}(t=1) \cdot \left[ \alpha_4 + \alpha_5 X_s + \alpha_6 X_s R_s + \beta R_s \right] + \mathbf{C}_{ist} \boldsymbol{\alpha}_7 + \nu_{ist} \\
&\text{for } |X_s| < h, \quad t = \{-5, -3, \dots, 3\}
\end{aligned} \tag{4}$$

where  $\widehat{\beta}$  is our estimate of the Causal Effect of redistricting in the election immediately after redistricting. This specification assumes the Selection Effect is constant, but for robustness we also allow for a time trend in the Selection Effect. That specification adds a linear trend in the discontinuities to (4) and tests for whether there is a deviation from the trend in the election immediately after redistricting.

The choice of bandwidth  $h$  is complicated because the panel specifications of Equations 2–4 simultaneously estimate several regression discontinuities. We make a reasonable choice of bandwidth (guided by standard methods of bandwidth selection) and show that the results are similar using other choices.<sup>10</sup> In our baseline specifications we choose

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<sup>10</sup>When applied to the pooled sample, several methods of optimal bandwidth choice (e.g. Ludwig et al., 2007; Imbens and Lemieux, 2008; Calonico et al., 2014) suggest the proper bandwidth lies in the range of 8 to 20. Hence we take roughly this range for our robustness checks.

a bandwidth of 18, which yields conservative estimates. We show in the results section that the main result is similar for a range of choices from 6 to 22, and the estimates lie within each other's confidence intervals. We also show in Online Appendix C.2 that other results in the main text are not sensitive to the choice of bandwidth. In all specifications we cluster the standard errors by state-redistricting event  $s$  to account for both state-level shocks and the cross-time correlation in the error term.

## 5.2 Data

We draw on data compiled by Klarner (2013) on elections for the lower house of the state legislature, restricting our sample to the years after 1962 (the year of *Baker v. Carr* 369). Our sample includes the redistricting elections for the 1970, 1980, 1990, 2000, and 2010 redistricting cycles. We discard all elections (and thus any state-redistricting event) after a state adopts a redistricting commission (as so marked by Levitt, 2016). We also discard states that have a single at-large district. Maine presents an unusual case because unlike other states it has occasionally redistricted in years ending in 3 rather than 1. In our main sample we treat it like the other states (taking years ending in 1 as the redistricting year) to avoid any problem that may arise because the year of redistricting is endogenous. We show in Online Appendix C.4 that the main results do not change if we drop Maine from the sample. Finally, we exclude Nebraska from all analysis because its unicameral and nominally nonpartisan legislature make it institutionally distinct from the other states. Appendix D.3 shows the full set of state-redistricting-cycles included in the sample.

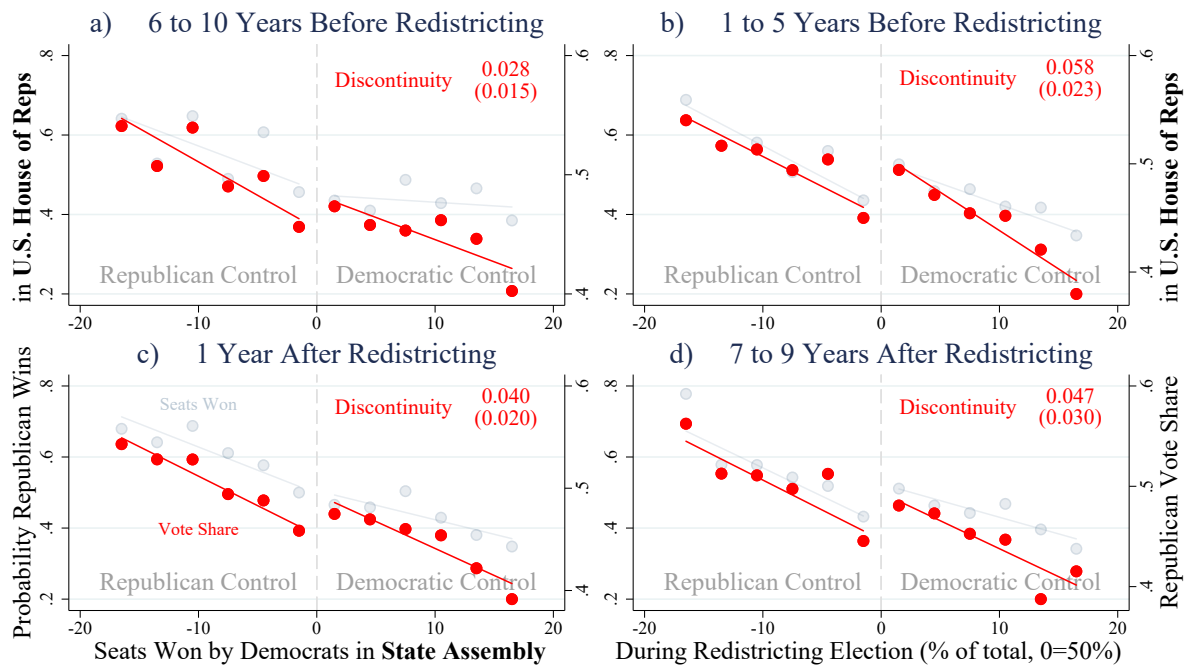
This dataset is merged to data on the outcomes of individual races for the U.S. House. We combine the data from the Inter-university Consortium for Political and Social Research (1995), which covers 1964 through 1990, with data from Kollman et al. (2016), which covers 1991 through 2012.<sup>11</sup> We measure racial gerrymandering using tract-level census data (Minnesota Population Center, 2011) merged to Congressional district boundaries Lewis et

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<sup>11</sup>We verify that the main results hold using the dataset of Lee et al. (2004) for the years 1972 to 1992, where the two datasets have slightly different coverage (see Appendix C.3).

**Figure 5**

The Selection Effect on Vote Shares Does Not Vanish after Redistricting



*Note:* This figure is identical to Figure 3 except that the RD plot of the seats won by Republicans (faint) is overlaid with the RD plot of the Republican vote share (dark). The coefficients and standard errors are for the vote share. Each dot shows the average of the outcome within a bin of width 3. Standard errors are clustered by state-redistricting event.

al. (2013). We assign each tract to whichever pre- and post-redistricting district that contains its centroid. In Online Appendix D we give more details and report descriptive statistics for the data.

### 5.3 Testing the Identifying Assumption

Our difference-in-discontinuities estimator is valid only if everything else that might determine the outcomes of U.S. House elections before redistricting remains roughly unchanged after redistricting. (This can be relaxed to allow for a smooth trend, though we show in the results section doing so does not change the results.) The most obvious concern is mean reversion. States were sorted to the left of the threshold because they had become less favorable to Republicans just before redistricting. If they subsequently revert back to the mean just after redistricting, it would falsely appear that the pre-existing discontinuity is closed by

redistricting. Such mean reversion would have to be implausibly fast to explain the change from Panel a to Panel b of Figure 3. Nevertheless we show in this section that there is no evidence of any change in the Selection Effect.

Our test exploits the fact that election outcomes depend on just two things: the share of votes won by a party, and the distribution of those votes across districts. All other factors—e.g. the partisan lean, the popularity of the candidate—affect outcomes through their effect on the share of votes. If parties are winning narrow majorities in states where the opposition party is relatively more popular, there should be a Selection Effect not only on which party wins U.S. House elections but on the vote share. If we can show that this effect—the change at the cutoff in the share of votes won by Republicans—is unchanged from before to after redistricting, it suggests the change in U.S. House election outcomes must be driven by a change in the distribution of votes.

Figure 5 is constructed analogously to Figure 3, the only difference being that in addition to plotting the average share of seats won by Republicans (faintly colored dots and lines), we also plot the average Republican vote share (darkly colored dots and lines). Panel b of Figure 5 shows a similarly sized discontinuity in both the seat share and the vote share. Democrats (Republicans) are systematically winning bare majorities in the lower houses of states where Republican candidates for the U.S. House win a larger (smaller) share of both seats and votes. But Panel c of Figure 5 shows that this discontinuity in vote shares remains even after redistricting, while the discontinuity in seat shares vanishes. In other words, states sorted to the right of the cutoff would, before redistricting, give Republicans a higher share of both votes and House seats. After redistricting these same states still give Republicans a higher share of votes, but that no longer translates to a higher share of seats.<sup>12</sup>

Since there is a reduction in seats won despite no change in the overall share of votes won, it must be that the distribution of votes across districts has abruptly become less favorable to

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<sup>12</sup>Panel d in Figure 5 resembles its analog in Figure 3, though it is noisier. Surprisingly, Panel a shows some evidence of a discontinuity even though there was no such evidence in Figure 3. That may suggest the Selection Effect is truly constant. But it is also possible that sampling error makes 5.a appear to have a discontinuity by chance, or to hide a similar discontinuity in 3.a.

the opposition party. Barring a massive and sudden migration across district lines (which did not occur in the U.S. during our sample), such a change is only possible if district boundaries are changed in a way that hurts the opposition party.

## 6 Main Results

### 6.1 Selection Effect

Table 1 shows the results from estimating Equation 2. The table shows the estimated discontinuity in the probability the Republican wins a U.S. House race *before redistricting*. Since redistricting has not yet happened, these estimates are suggestive of whether parties select to control redistricting based on outcomes in Year  $t$ . The last three columns estimate the same parameters controlling for year and state-redistricting event fixed effects. In the specifications that control for event fixed-effects the discontinuity 9 years before redistricting is the excluded category (meaning all other estimates give the size of the discontinuity in year  $t > -9$  relative to the discontinuity at  $t = -9$ ).

These estimates add nuance to the pattern visible in Figure 3. There is no evidence of selection on outcomes 7 to 9 years before redistricting, possibly because parties ignore elections so far in the past in deciding where to contest for redistricting. The outcomes of U.S. House contests become relevant for their decision starting 5 years before redistricting. Since the coefficients are positive they suggest the parties' actions leave them in control of redistricting in states where the opposition has been winning.

After the Selection Effect becomes positive it remains constant. The row labeled "Test: ..." tests for whether we can reject that the discontinuities at  $t = -5, -3, -1$  are all of the same size. In no specification can we reject that the Selection Effect remains constant after  $t = -5$ . If the Selection Effect is constant, a difference-in-discontinuities estimator will give consistent estimates of the Causal Effect of partisan redistricting. That logic would fail if the effect is only constant until after redistricting, but Section 5 shows that the electoral bias of

**Table 1**  
Estimates of the Selection Effect

	(1)	(2)	(3)	(4)
	Rep. Win	Rep. Win	Rep. Win	Rep. Win
[Democrats Control Assembly During Redistricting]				
× $\mathbb{I}(t = -9)$	-0.044 (0.052)	-0.028 (0.050)		
× $\mathbb{I}(t = -7)$	0.024 (0.049)	0.001 (0.054)	0.066 (0.056)	0.026 (0.058)
× $\mathbb{I}(t = -5)$	0.119** (0.049)	0.126** (0.050)	0.157*** (0.054)	0.146*** (0.044)
× $\mathbb{I}(t = -3)$	0.113** (0.053)	0.137** (0.056)	0.146*** (0.056)	0.147*** (0.049)
× $\mathbb{I}(t = -1)$	0.106** (0.052)	0.122** (0.050)	0.142** (0.066)	0.135** (0.058)
Test: $\hat{\rho}_{-5} = \hat{\rho}_{-3} = \hat{\rho}_{-1}$	.96	.9	.93	.95
Event FEs			X	X
Year FEs		X		X
Observations	6820	6820	6820	6820
Events	135	135	135	135

*Note:* We estimate Equation 2. Each row gives the estimated discontinuity in U.S. House elections held some years *before* redistricting ( $\hat{\rho}_t$  in Equation 2). The values reported in the row labeled “Test...” are p-values of the test for equality of the discontinuities in the elections 5, 3 and 1 year before redistricting. Standard errors are clustered by state-redistricting event. “Event FEs” are state-redistricting event fixed-effects.

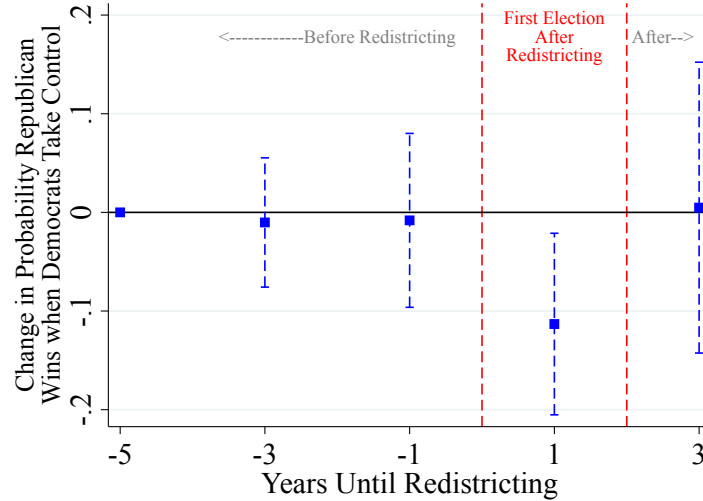
states, as measured by the Republican vote share, does not change after redistricting. That suggests our assumption of a constant Selection Effect is not unreasonable.

## 6.2 Causal Effect

We estimate Equation 3, the flexible difference-in-discontinuities, taking the U.S. House election 5 years before redistricting as the reference year. If the Selection Effect is constant, as suggested by the results of the research design section and the previous section, the difference-in-discontinuities estimates should equal zero for the years before redistricting. Figure 6 plots the difference-in-discontinuities estimates  $\{\widehat{\rho}_t^\Delta\}$  with their 95 percent confidence intervals. Since  $t = -5$  is the reference year its estimate is zero by construction. But the estimates for  $t = -3$  and  $t = -1$  are also close to zero, suggesting the estimator has controlled for the Selection Effect.

In the U.S. House election after redistricting the estimate turns sharply negative. Switch-

**Figure 6**  
Difference-in-Discontinuity Estimate  
of Causal Effect of Redistricting



*Note:* We estimate Equation 3 and plot the coefficients  $\{\hat{\rho}_t^{\Delta}\}$  with their 95 percent confidence intervals. Standard errors are clustered by state-redistricting event.

ing the state assembly from Republican to Democratic control lowers a Republican's chance of winning a U.S. House contest by 11 percentage points. This pattern is unlikely to be explained by mean-reversion in voter sentiment because, as shown in the research design section, the Republican vote share is unchanged from before to after redistricting. The impact must arise from how district boundaries are drawn. But by  $t = 3$  the estimate is again zero, suggesting the impact of redistricting is short-lived. As we argue in the discussion section below and Online Appendix A, such transience is not entirely surprising.

Figure 6 implies that the Causal Effect appears only in the year immediately after redistricting. By imposing this restriction, Equation 4 maximizes the power of our estimates. Panel A of Table 2 shows both the baseline estimates and those that arise after controlling for different fixed-effects. Columns 1 through 4 show that controlling for state-redistricting event fixed-effects and year fixed-effects barely changes the estimates. In Columns 5 and 6 we also allow for a linear trend in the Selection Effect. If our assumption of a constant Selection Effect is invalid, this trend might absorb some of the bias and shrink our estimates. But



**Table 2**  
**Main Results: Difference-in-Discontinuity Estimates**  
**Causal Effect of Dem. Control in the State Assembly on**  
**First U.S. House Election After Redistricting**

<b>Panel A: Main Results</b>						
	(1)	(2)	(3)	(4)	(5)	(6)
	Rep. Win	Rep. Win	Rep. Win	Rep. Win	Rep. Win	Rep. Win
Dif-in-Disc Estimate	-0.108*** (0.036)	-0.111*** (0.039)	-0.112*** (0.035)	-0.105*** (0.037)	-0.110** (0.043)	-0.096*** (0.036)
Event FEs		X		X		X
Year FEs			X	X		X
Trends					X	X
Observations	6541	6541	6541	6541	6541	6541
Events	135	135	135	135	135	135

<b>Panel B: Specification Tests</b>						
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	No Ind. Assemblymen	Drop VRA States	Republican Margin	Drop Special Elections	Placebo
Dif-in-Disc Estimate	-0.108*** (0.036)	-0.093** (0.036)	-0.097** (0.038)	0.096*** (0.035)	-0.107*** (0.036)	-0.015 (0.036)
Observations	6541	6133	5861	6433	6532	6272
Events	135	120	118	133	135	128

<b>Panel C: Robustness to Bandwidth</b>					
	(1)	(2)	(3)	(4)	(5)
	h=22	h=18	h=14	h=10	h=6
Dif-in-Disc Estimate	-0.107*** (0.035)	-0.108*** (0.036)	-0.098** (0.040)	-0.128** (0.049)	-0.181** (0.073)
Observations	6812	6541	5513	3766	2539
Events	149	135	111	82	48

*Note:* Each column shows a different estimate of  $\hat{\beta}$  from Equation 4. Panel A gives the baseline estimate and several estimates that control for various fixed effects (“Event FEs” are state-redistricting event fixed-effects). “Trends” controls for a linear time trend in the size of the discontinuity. Panel B checks the specification. “No Ind. Legislators” drops cases in which independent legislators are elected to the state assembly during the redistricting election. “Drop VRA States” drops states that require pre-clearance from the Justice Department for any change in election law. “Republican Margin” defines the running variable as the Republican rather than Democratic margin of seats in the assembly. “Drop Special Elections” drops all U.S. House elections in odd years. “Placebo” uses the Democratic margin in the election *before* the redistricting election as the running variable. Panel C estimates Equation 4 using several different choices of bandwidth ( $h = 18$  is the bandwidth used in the baseline specifications). Standard errors are clustered by state-redistricting event.

the estimates in Columns 5 and 6 are largely unchanged, suggesting our assumption is not unreasonable.

Panel B shows the results of several specification tests. Column 2 shows that the estimate is little changed by discarding state-redistricting events where independent legislators won seats in the assembly. Column 3 shows that the results are not sensitive to excluding the so-

called pre-clearance states. During our sample these states were required to submit changes to their voting rules for pre-clearance to the U.S. Department of Justice (as per Section 5 of the 1965 Voting Rights Act).<sup>13</sup> Column 3 shows that they are not driving our results. Column 4 shows that changing the running variable from the Democrats' margin to the Republicans' margin of seats won in the assembly gives an estimate of similar magnitude and opposite sign, as expected. Column 5 shows that dropping U.S. House elections in off-years does not change the results. Finally, in Column 6 we report the results of a placebo test. We take as the running variable not the margin won by Democrats in the redistricting election, but in the state election before that. The party that wins this earlier election has no power over redistricting. As expected, the placebo estimates in Column 6 are small (roughly one-seventh the size of our actual estimates) and statistically insignificant.

Finally, Panel C confirms that our estimates are not driven by the choice of bandwidth. Column 2 repeats the estimate with our preferred bandwidth of 18. Column 1 reports the results of a wider bandwidth of 22. Columns 3 through 5 show that the estimates are largely unchanged (or larger) at narrower choices of bandwidth.

## **7 Mechanism: Is It Really Caused by Partisan Redistricting?**

### **7.1 The Conversion Rate Turns Against the Opposition After Redistricting**

A redistricting plan is favorable to Republicans if, holding their share of statewide votes fixed, it yields a larger share of the state's U.S. House seats. Gelman and King (1994b) measure the "responsiveness" of a redistricting plan to swings in vote shares using simulations, while (Cox and Katz, 2002) and Engstrom (2006b) do a conceptually similar calculation by estimating the correlation between statewide vote shares and seat shares. Our difference-in-discontinuities approach lets us measure responsiveness directly and causally. Let  $V_{st}^R$  be the share of votes won and  $W_{st}$  the fraction of seats won in election  $t$  during state-redistricting

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<sup>13</sup>These are Alabama, Alaska, Arizona, Georgia, Louisiana, Mississippi, South Carolina, Texas, and Virginia.

cycle  $s$ . Define the vote-to-seat conversion rate as  $W_{st}/V_{st}^R$ . A higher conversion rate implies Republicans are able to convert the same number of votes into more seats.

We apply a state-level version of Equation 4 to the statewide U.S. House Republican vote share, the fraction of seats won, and the conversion rate. The left-hand panel of Figure 7 shows that there is no statistically significant effect on the Republican vote share. This estimate is not surprising given that Figure 5 shows the Selection Effect in the Republican vote share does not change after redistricting. Since the Selection Effect is stable over the redistricting cycle, the difference in discontinuity estimates will be uniformly zero.<sup>14</sup> By contrast, the center panel of Figure 7 shows that there is a large and statistically significant decrease in the share of U.S. House seats won by Republicans when Democrats control the assembly during redistricting. This result is simply the state-level analog of Figure 6.

The right-hand panel shows the effect on the conversion rate. It is unchanged until after redistricting, when it turns against the opposition party with a point estimate of roughly -0.36. The point estimate implies that if Republicans hypothetically won half the votes in a state, they would win 59 percent of the seats under the redistricting plan drawn by a Republican assembly, but only 41 percent of the seats under the plan drawn by a Democratic assembly.<sup>15</sup> This difference can only arise if the statewide Republican vote, which is the same in both cases, has been distributed across districts less favorably under the Democratic plan.

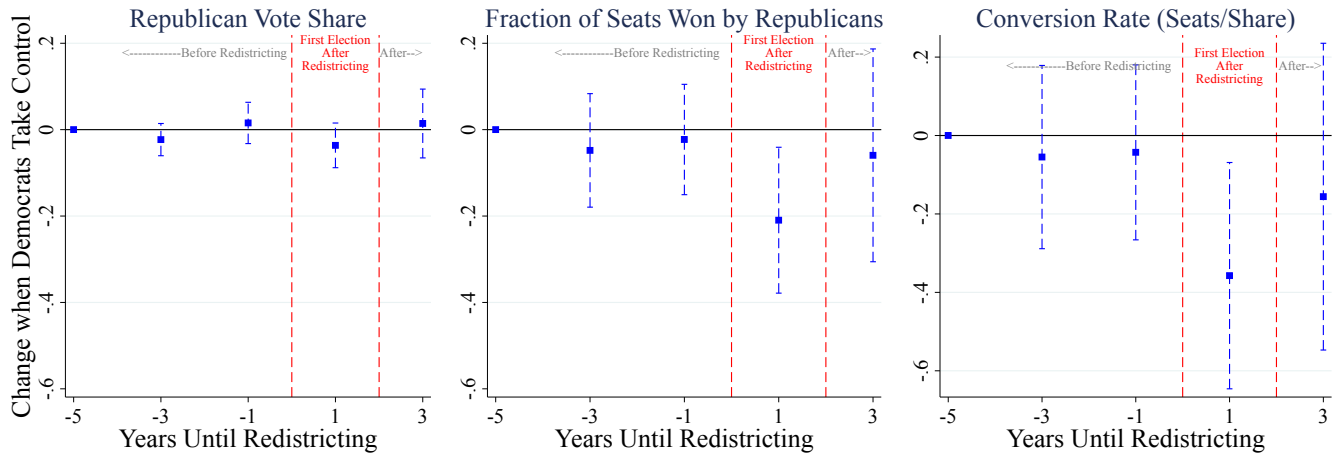
## 7.2 The Party in Power Packs Supporters of the Opposition: The Case of African Americans

To lower the rate of conversion from votes to seats, the party in power can “pack” opposition voters into a smaller number of districts (see, for example, Owen and Grofman, 1988).

<sup>14</sup>Though Figure 5 takes individual House races rather than the total statewide vote share as the unit of analysis, in Online Appendix C.1.2 we show that the results for the statewide vote share are similar. It may seem surprising that packing opposition voters into uncompetitive districts would not deter them from the polls. But until recently, redistricting was not salient to voters. It is possible it may have had little impact on their decision to vote.

<sup>15</sup>This example assumes a conversion rate of 1 in a neutral environment. In reality the political geography of most states naturally favors Republicans. Regardless of what the neutral conversion rate is, Republican control would imply they win 18 percentage points more of the state’s seats.

**Figure 7**  
 Republican Votes are Converted to Seats at a Less  
 Favorable Rate when Democrats Control the Assembly



*Note:* We apply a state-level version of Equation 4 to the statewide Republican vote share, the fraction of seats won, and the conversion rate (the ratio of the two). The unit of observation is the state-election year. We plot the coefficients  $\{\hat{\rho}_t^{\Delta}\}$  with their 95 percent confidence intervals. Standard errors are clustered by state-redistricting event.

Since the census that triggers redistricting also reports population counts by race, gerrymandering racial groups that reliably vote for the opposition is a straightforward method for lowering the conversion rate. African Americans are the demographic group whose party preference is most easily identified. In the 2014 election, 89 percent of African Americans voted Democratic for the U.S. House—support comparable to that of registered Democrats (92 percent).<sup>16</sup> Since an African American is likely to support Democrats, Republicans may try to redistrict African Americans to minimize their influence.<sup>17</sup>

We say a voter has been “moved” if her new Congressional district contains many voters that were not in her old Congressional district. To be precise, for each census tract we define the fraction of the population in the new Congressional district that is “unfamiliar,” meaning the fraction not in the original district. A census tract is marked as having been moved if this fraction exceeds 0.5. The benefit of this measure is that by definition it reflects the act of redistricting; a census tract is counted as “moved” only if district boundaries are changed.

<sup>16</sup>According to CNN (2016), whose data are based on National Election Pool exit polls.

<sup>17</sup>Bullock (1995), Cameron et al. (1996), Lublin (1999), and Swain (1995) have argued that such “packing” makes surrounding districts less friendly to Democrats. For a broader overview of how legal rulings have increased the scope for racial gerrymandering, see Edmundson (2016), Soffen (2016), and Tofighbakhsh (2020).

**Table 3**  
Evidence of Racial Gerrymandering

	Prob. of Being Moved		Conditional on Moving	Pre-Redistricting Char.	
	(1) Black Tracts	(2) Other Tracts	(3) New District > 75% Black	(4) Fraction Black	(5) District Size Deviation
Dem. Control	-0.150** (0.063)	-0.027 (0.050)	-0.230*** (0.054)	0.006 (0.018)	0.012 (0.018)
Observations	19478	185111	2842	1550	19614
Events	117	138	41	137	116

*Note:* Columns 1 and 2 estimate the discontinuity (using a local linear regression) at the level of the census tract on a dummy for whether the tract is “moved” during redistricting (see text for definition). Column 1 restricts to tracts that are majority black; Column 2 restricts to all other tracts. Column 3 estimates a similar specification on the subset of majority black tracts that are moved. The outcome is a dummy for whether the tract is moved into a district whose population is more than 75 percent black. Column 4 tests for a discontinuity in the black population share of districts on either side of the cutoff. For Column 5 we assign majority black census tracts the absolute percentage difference between the population of the district it is located in and the median district in the state. We test for a discontinuity in this deviation. All standard errors are clustered by state-redistricting event. Visual representations of these specifications are in Online Appendix C.1.3.

We test for a discontinuity in the measure using tract-level census data. Column 1 of Table 3 restricts the sample to tracts in which African Americans are a majority; Column 2 uses all other tracts. Majority black census tracts are 15 percentage points more likely to be moved under Republican versus Democratic control. This holds only for African American tracts; Column 2 shows that there is no discontinuity when we restrict the sample to census tracts that are not majority black.<sup>18</sup>

Conditional on moving African Americans, are Republicans more likely than Democrats to move them into districts that minimize their influence? One way to minimize their influence would be to “pack” them into districts in which they form the overwhelming majority. Though these few districts are lost with certainty, the number of contests in which the unfriendly voters may be pivotal is minimized.<sup>19</sup> We restrict the sample to African American tracts that have been moved as per our measure. Column 3 of Table 3 tests for whether majority black tracts are moved into districts in which African Americans form an overwhelming (more than 75 percent) majority. The estimate suggests a large decrease in packing when

<sup>18</sup>Visual representations of all tests described in this section can be found in Online Appendix C.1.3.

<sup>19</sup>A recent Supreme Court ruling struck down two North Carolina districts because, as described by the New York Times (May 22, 2017), “the Supreme Court has insisted that packing black voters into a few districts—which dilutes their voting power—violates the Constitution.” The decision’s syllabus notes of one of the unconstitutional districts that “regardless of party, a black voter in the region was three to four times more likely than a white voter to cast a ballot within District 12’s borders” (Cooper v. Harris, 2017).

Republicans lose control of redistricting.<sup>20</sup>

Can these estimates necessarily be interpreted as the Causal Effect of partisan redistricting? It is difficult to test for a Selection Effect using the exact approach of 6.2 because our measures of how African Americans are moved during redistricting are, by construction, undefined before redistricting. Instead we test for more basic differences in the demographics of districts in states on either side of the cutoff. Our aim is to test whether there is any difference in the objective need to redistrict African Americans.

The most obvious confounder would be if states barely controlled by Republicans on average contain more African Americans, making it almost mechanical that they would be more likely to be moved during redistricting. Taking the district as the unit of analysis we test for whether at the threshold there is a discontinuity in the fraction of a district's population that is African American. We use the old district boundaries to avoid contaminating the estimates with the effect of redistricting. Column 4 of Table 3 shows that there is no evidence of a discontinuity.

Though African Americans may comprise a similar portion of the total population near the threshold, is it possible that they are distributed less evenly than the rest of the population? For example, if migration patterns differ across the threshold, it is possible that in Republican-controlled states African Americans have segregated themselves into heavily over- or under-populated districts. These districts would have to be broken up during redistricting. To test this hypothesis we compute the absolute percentage deviation of the population of each district from the median of all districts in the state prior to redistricting. We assign the district's population deviation to each tract within it. We then test whether majority-black census tracts have higher district deviations on one side of the cutoff. Column 5 of Table 3 suggests there is no difference at the cutoff, implying African Americans are no more likely to live in malapportioned districts in Republican-controlled states. Though we cannot categorically rule out all explanations, prior work has suggested that in practice

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<sup>20</sup>The regression in Column 3 of Table 3 and those used to construct Figure 16.B narrow the bandwidth to 10 because there is essentially no racial "packing" further away from the discontinuity.

objective criteria usually do not produce majority-black districts.<sup>21</sup>

## 8 Discussion and Conclusion

### 8.1 Why Are the Effects of Partisan Redistricting So Short-Lived?

In Online Appendix A we argue that the effect of partisan redistricting is short-lived because there are swings in the electorate that undermine a favorable redistricting plan. Drawing a favorable district requires an accurate prediction of who votes and how they vote. But even if it is possible to predict how a district will vote in the near future, any prediction will become meaningless over the ten-year lifespan of a district map. We calculate that the standard deviation of the aggregate swing—the change between elections in each state’s average Republican vote share—is roughly 6.7 percentage points, implying a one-standard deviation shock is all it takes to change a comfortable 10-point Republican margin to a narrow win for Democrats. The idiosyncratic component—the swing in a district’s Republican share between elections after controlling for the state-wide swing—is even larger. A set of districts gerrymandered to give 10-point margins to Republican candidates could, in the next election, become a catastrophic wave of defeats. These simulation results are consistent with research by Goedert (2017), who finds that partisan redistricting has historically backfired when national trends turn against the gerrymandering party. Gopoian and West (1984) shows in the specific case of the 1980 redistricting cycle, and Scarrow (1981) in the case of three districts drawn in New York in 1966, that partisan redistricting earned short-term gain at the cost of long-term insecurity of individual districts.<sup>22</sup>

That said, we provide some evidence in Online Appendix A that in more recent years

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<sup>21</sup>(Barabas and Jerit, 2004), for example, find that compactness is the only provision from the 1964 Voting Rights Act that has big impacts. Given that all states are required to respect compactness, there could only be a difference at the discontinuity if there were differences in the size or residential patterns of black voters. Table 3 shows little evidence to support this.

<sup>22</sup>Some authors have argued that packed districts deter high-quality candidates from running (Lublin, 1999), thereby creating persistent advantages not easily undone by slight shifts in voting patterns. Our results suggest that, at least for the period we consider, any incumbency advantage created was not big enough to create persistent advantages.

gerrymandering has become more persistent. It is possible that new technology allows more accurate predictions of how people vote. These computer-assisted technologies, still rarely used in prior decades, are growing in popularity (Altman et al., 2005). . The pattern may suggest gerrymandering will have larger and more persistent effects in the long run.

## **8.2 Summary and Directions for Future Research**

We propose and apply a method that lets us measure both where political parties consciously seek to control Congressional redistricting, and the subsequent impact of redistricting. We find that parties' actions leave them in control of states where their influence is declining, and that they use redistricting to at least temporarily reverse the decline. We present further results showing that partisan redistricting is the mechanism. In our overall sample the effect of partisan redistricting is short-lived. But we also find evidence consistent with an increase in its size and persistence in recent years, which may suggest it is becoming more pernicious.

One key caveat of our results is that, by design, our estimates are based on states where one party only barely won a majority in the lower house of the legislature. These are likely to be highly competitive states. On the one hand, that could imply that our estimates of the impact of partisan gerrymandering are an upper-bound because a closely divided state may have the greatest scope for packing and cracking the electorate. On the other hand, it could be a lower bound because these are the states where it is least likely that one party controls all the veto points in redistricting. Given that a House seat in California or Alabama counts as much as a seat in Wisconsin or Pennsylvania, future work should explore whether the impact of gerrymandering is of similar size in these less competitive states, and whether it is as likely to run contrary to trends in statewide voter sentiment.

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# Appendices

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## A Why Are the Effects of Partisan Redistricting So Short-Lived? (For Online Publication)

This appendix provides an example to illustrate how shifts in the electorate can undo a gerrymander, and shows suggestive evidence that as the technology for gerrymandering has improved the effects have grown more persistent.

### A.1 Simple Numerical Example

Suppose a state has two Congressional districts. In the absence of gerrymandering, 65 percent of voters in District 1 are Republicans while the rest are Democrats. In District 2, 45 percent are Republicans. If Republicans control redistricting they shift voters so that both districts are 55 percent Republican.<sup>23</sup> If we assume everyone votes as expected in the first election after redistricting, Republicans win an extra seat (the fraction of seats won is 50 percentage points higher) in states where they control redistricting.

But suppose that in the subsequent election there is a normally distributed mean-zero aggregate shock  $u$  to the Republican vote share in both districts. For simplicity assume there is no idiosyncratic shift, which would only reinforce our conclusions (we show simulations in Online Appendix B that allow both types of uncertainty). If  $-5 < u < 5$  the Republicans still win more seats under the gerrymandered district map. Assuming  $u$  has a standard deviation of 6.7 (as in the data), this event happens with roughly 54 percent probability. But if  $u > 5$ , which happens with 23 percent probability, Republicans win both districts even under the non-gerrymandered plan. Likewise they lose both districts under both plans in the 1 percent chance that  $u < -15$ . And if  $-15 < u < -5$ , which happens with 22 percent probability, they are actually *worse off* under the gerrymandered plan because they have turned District 1 from a very safe to only a somewhat safe Republican seat. Weighting these four scenarios by

---

<sup>23</sup>To make this reallocation concrete, suppose both districts have 100 voters. Republicans replace 10 of District 1's 65 Republicans with Democrats from District 2, leaving 55 Republicans and 45 Democrats in each district.



their probabilities, Republican gerrymandering on average raises the fraction of seats won by only 16 percentage points, meaning nearly two-thirds of the gains from gerrymandering vanish from the first to the second election.

Stark though this example may seem, it actually ignores mitigating factors that would further reduce the longevity of gerrymandering. The district planner may not be able to predict how people will vote even in the election just after redistricting. Moreover, the true uncertainty, which has both an aggregate and an idiosyncratic part, is somewhat larger than in our example. The difficulty in predicting how people will vote many years in the future is a likely explanation for why we find such transient effects.

## **A.2 Is Gerrymandering More Potent in Recent Years?**

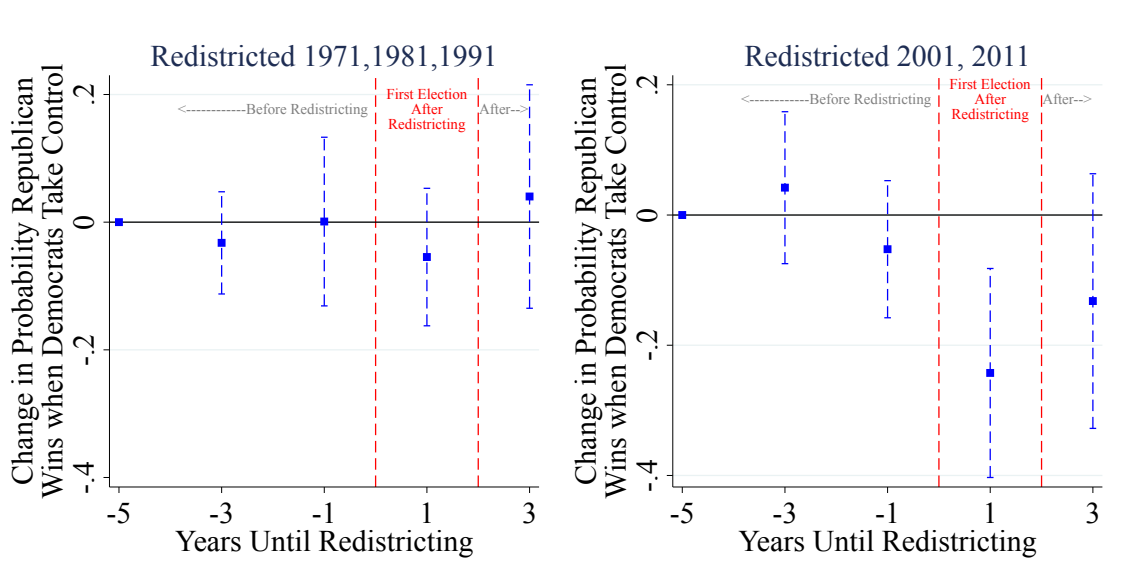
If gerrymandering is short-lived because voters are hard to predict, the effects should become bigger and more persistent as the technology of prediction improves. It is widely perceived that the technology of gerrymandering has improved in recent cycles. The New York Times (May 30, 2017) suggests that restrictions on gerrymandering must be enacted “as voter data and computer-mapping technologies become more sophisticated.”

One such technology is Maptitude, a software product sold by Caliper Corporation starting in 1995 and now used by over 100 state legislatures and state political parties. Maptitude now has a specific add-on called Maptitude for Redistricting. Its brochure boasts that the user can build maps in an interface that can easily “Add political data and election results” or “identify communities of interest” such as “racial or ethnic enclaves that tend to have similar interests and vote as a bloc” (Caliper Corporation, 2016). One might expect that gerrymandering would be easier and more sophisticated with such tools.

The data also suggest that swings in party vote shares are growing smaller over time. During the 1971 redistricting cycle the standard deviation of the state-wide swing is 7.6 percent, but by the 2001 cycle it has fallen to 6.1 percent. The standard deviation of the idiosyncratic component has fallen from 8.5 percent to 6.1 percent. Though this decline in volatility could

**Figure 8**

The Effect of Gerrymandering Grows Larger and More Persistent



*Note:* Each panel is constructed analogously to Figure 6, but with a restricted sample. The left-hand panel restricts to state-redistricting events in 1971, 1981, and 1991. The right-hand panel restricts to more recent redistricting events.

have any number of causes, what matters here is that it might make it easier to design a long-lived gerrymander.

Each panel of Figure 8 is constructed like Figure 6. The left-hand panel restricts the sample to the 1971, 1981, and 1991 redistricting cycles while the right-hand panel restricts to the 2001 and 2011 cycles. The figure suggests that the immediate effect of redistricting is much larger in the later period—an anti-opposition effect of 24 percentage points versus 5.5 percentage points. In the later period the sign of the effect remains large and negative in the next election, though it is not statistically significant. The estimate provides some evidence that, unlike in the earlier period, the effect in the later period is persistent. This analysis is only suggestive, as splitting the sample leaves relatively few elections. We offer it only as some evidence consistent with our explanation: that the consequences of partisan gerrymandering depend largely on how well the party in control can predict how people will vote. At the very least Figure 8 suggests it would be hasty to conclude the effects of gerrymandering will forever remain short-lived.

## B Simulation Appendix (For Online Publication)

We run a simple simulation to demonstrate how swings in the electorate can cause the effect of gerrymandering to fade. Suppose there are  $I$  districts to be drawn in state-redistricting event  $s$ . In the absence of gerrymandering the Republicans would win vote shares  $\{V_{ist}^R\}_{\forall i}$ . If the Republicans gerrymander they may, subject to some constraints, reallocate votes across districts to create a new set of shares  $\{\tilde{V}_{ist}^R\}_{\forall i}$ . We assume the gerrymandered districts are constructed as follows:

1. Republicans choose a threshold  $\bar{V}$ . They aim for all districts to have this vote share.
2. They draw 10 percent of Republican voters out of each district to be reallocated, meaning in the absence of further transfers each district's Republican vote share is now  $\{0.9V_{ist}^R\}_{\forall i}$ .<sup>24</sup> Their "budget" for gerrymandering is then  $B = \sum_i 0.1V_{ist}^R$ .
3. They order districts in descending order by the Republican vote share

$$\{V_{i(I),st}^R, V_{i(I-1),st}^R, \dots, V_{i(1),st}^R\}$$

where  $i(k)$  is the  $k$ -th order statistic over  $i$ .

4. For each  $k$ , if  $V_{i(k),st}^R < \bar{V}$  and  $\bar{V} - V_{i(k),st}^R < B$  they set  $\tilde{V}_{i(k),st}^R = \bar{V}$ . Otherwise they set  $\tilde{V}_{i(k),st}^R = 0.9V_{i(k),st}^R + B$ . The budget is lowered by the Republican vote share moved into the district.

The assumption of the "budget" is an ad hoc but simple way to impose a constraint on the scope of gerrymandering. Geography and the threat of a legal challenge prevent too much reallocation. This constraint ensures the effect on the election immediately after redistricting is of roughly the same size as found in the results section. We vary the threshold  $\bar{V}$  to show how different gerrymandering strategies play out.

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<sup>24</sup>To be precise, 10 percent of the voters are replaced by an equal number of Democrats from a different district.

After the first election there is a shock to the Republican vote share of each district:

$$k_{is,t+1} = \theta_{t+1} + \bar{k}_{s,t+1} + \tilde{k}_{is,t+1}$$

where  $\theta_{t+1}$  is a national shock,  $\bar{k}_{s,t+1}$  a statewide shock, and  $\tilde{k}_{is,t+1}$  an idiosyncratic shock. The statewide and idiosyncratic shocks are both normally distributed with mean zero and variance calibrated to match the data. The national shock is set to three different values to see whether gerrymandering is more persistent when the national mood is hostile, neutral, or friendly.

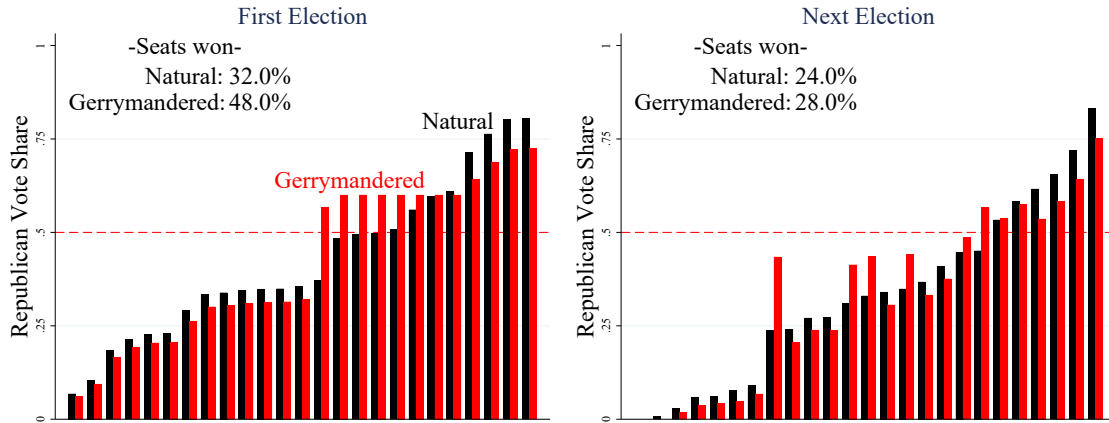
We set  $I = 25$ . We draw the natural vote share  $\{V_{ist}^R\}_{\forall i}$  from a Beta(2,2) distribution rescaled by 0.9 to mimic the pro-opposition Selection Effect. For each threshold  $\bar{V} = .5, .51, \dots, .8$  and each national shock  $\theta = -0.05, 0, 0.05$  we run 200 simulations.

Figure 9 shows one state-redistricting event assuming  $\bar{V} = 60$  and  $\theta = 0$ . Each bar shows the Republican vote share in a single district. The black bars show the natural distribution while the red bars show the distribution after gerrymandering. The left-hand panel shows the outcome in the election immediately after redistricting. Gerrymandering wins Republicans 48 percent of all seats. They would have won only 32 percent had they been unable to gerrymander. The right-hand panel shows the subsequent election ( $t + 1$ ).

In this example the Republicans have sustained a negative statewide shock  $\bar{k}_{s,t+1}$ , making it similar to the case where  $-15 < u < -5$  in the example described in the text. Several of the gerrymandered districts have now swung against the Republicans, leaving them little better off than they would have been under the natural distribution. The figure captures a fundamental tension between the two possible objectives of a partisan gerrymander: maximizing the number of seats versus making the seats already held safer.

Figure 10 shows that this intuition holds more generally. For each threshold we graph the average percentage of seats won by Republicans across all 200 simulations. We show only the case of the neutral national shock, as the others look similar. The red line in the left-hand panel—the average seats won in the first election after gerrymandering—is downward

**Figure 9**  
Simulation Example: A Negative Shock to Support for Republicans  
Nullifies the Gains from Gerrymandering



*Note:* Each bar shows the (simulated) fraction of votes won by Republicans in a district. The black bar shows the fraction that would have been won in the absence of gerrymandering. The red bar shows how many are won assuming Republicans gerrymander as described in the text. The left-hand figure shows the first election after redistricting, and the right-hand figure shows the subsequent election.

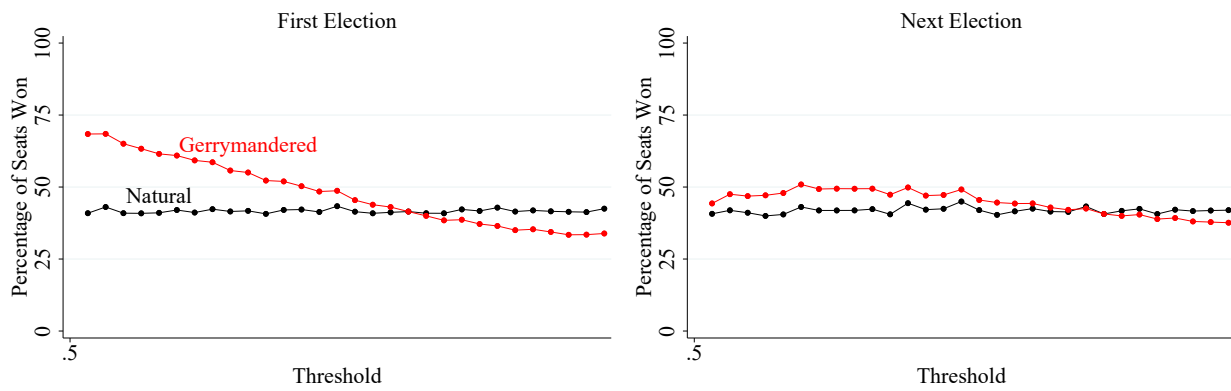
sloping. It captures the trade-off between having a bigger threshold (making safer seats) and winning more seats. Not surprisingly, the gains from gerrymandering—the gap between the red line and the black line, which shows the seats won without gerrymandering—follows a similar pattern.

This gap narrows in the right-hand panel, which shows the subsequent election. At any threshold the gap narrows, and it narrows the most in cases where it was widest to begin with (when the threshold was small). Given that our estimates imply a gap of roughly 11 percentage points in the first election, the threshold should be roughly 0.6. That implies a gap in the next election of around 6 percentage points—somewhat smaller than the original effect and well within the confidence interval of our estimate shown in Figure 6 for the election 3 years after redistricting.

## C Empirical Appendix (For Online Publication)

This appendix shows additional figures, tables, and analyses referenced in the main text.

**Figure 10**  
Simulated Effects: Neutral National Shock



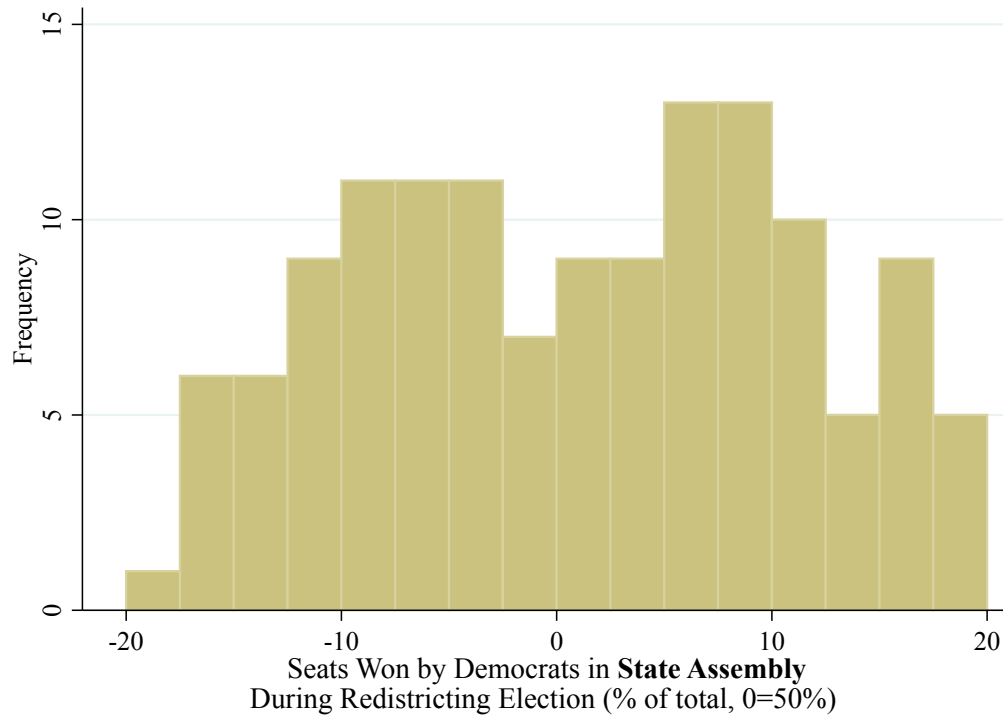
*Note:* The figure plots the average across all simulations of the percentage of seats won by Republicans as a function of the threshold used for gerrymandering (see text).

## C.1 Additional Tables and Figures Referenced in the Text

### C.1.1 Histogram of Seats Won by Democrats by State-Redistricting Events

**Figure 11**

Histogram of Seats Won by Democrats by State-Redistricting Events

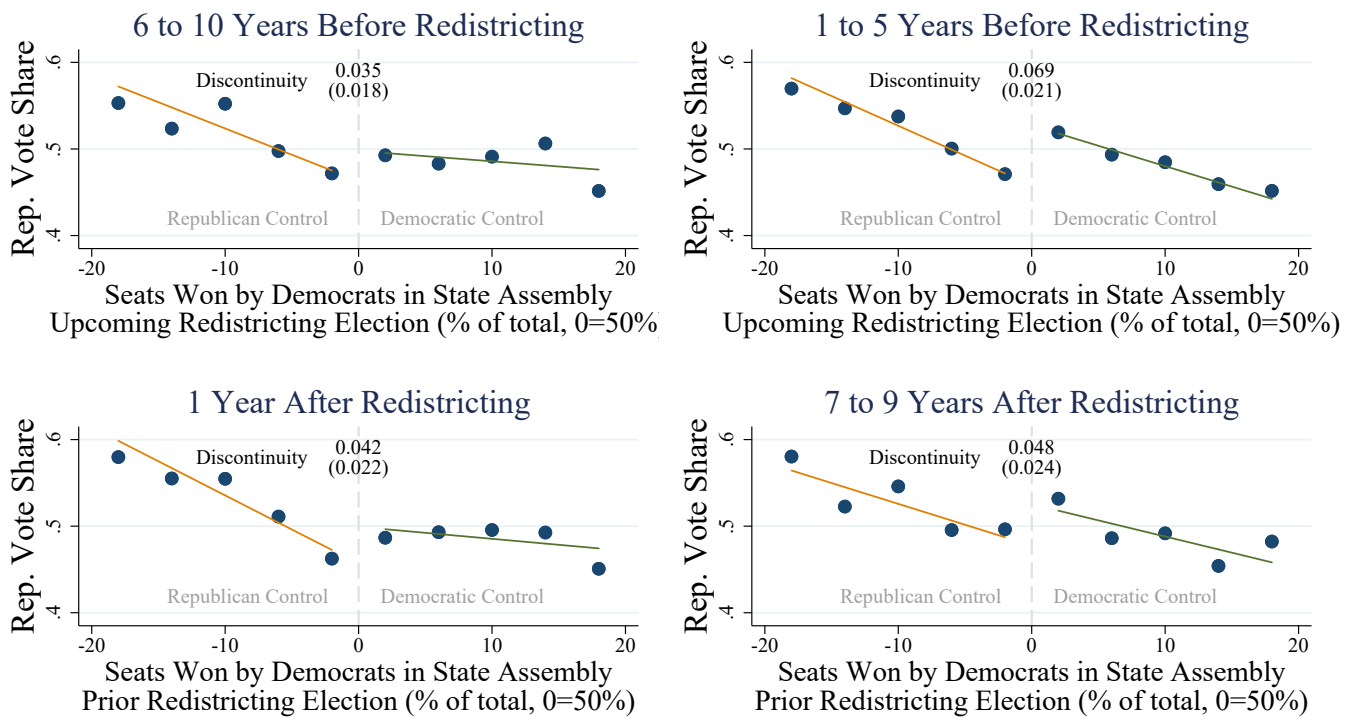


*Note:* This is the histogram of the running variable.

### C.1.2 Selection Effect of Republican Vote Share (the first part of the mechanism section)

Figure 12 is analogous to Figure 5 in the research design section. Unlike Figure 5, which uses an individual U.S. House race as the unit of observation, Figure 12 uses the share of votes won by Republicans throughout the entire state.

**Figure 12**  
The Selection Effect on Vote Shares Does Not Vanish after Redistricting  
(State-Level Regressions)



*Note:* See text for description. Unit of observation is a state-election year. Each dot shows the average of the outcome within a bin of width 4. Standard errors are clustered by state-redistricting event.

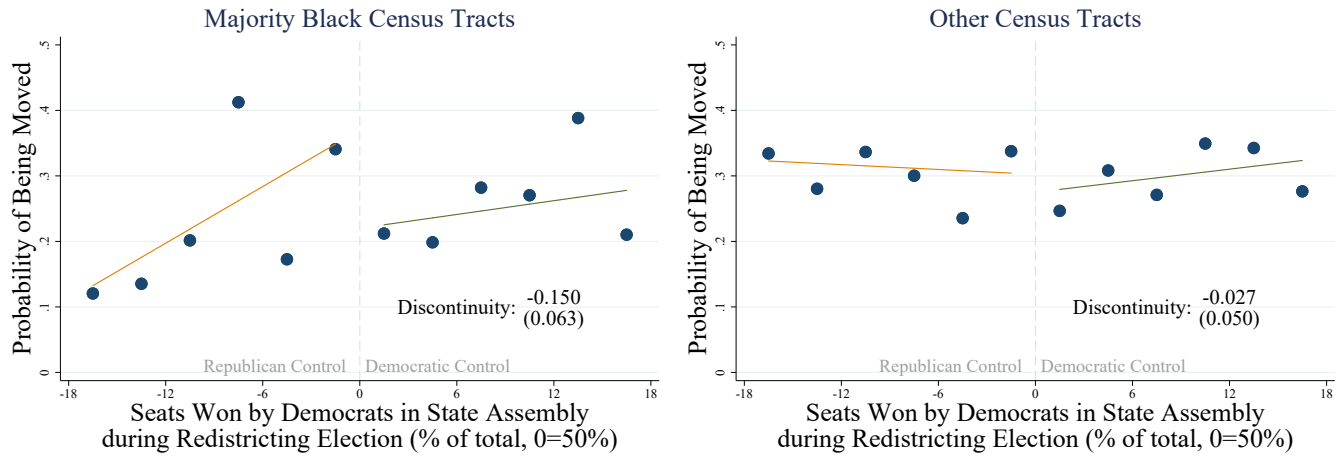
### C.1.3 Racial Gerrymandering (the second part of the mechanism section)

Figure 13 gives the visual representation of Columns 1 and 2 of Table 3. Figure 14 represents Column 3, and Figure 15 represents Columns 4 and 5. See the text of the mechanism section for details on the outcomes and the specifications.

Figure 16 gives a more complete description of how Democrats versus Republicans redistrict African Americans by studying the probability density of the districts into which they

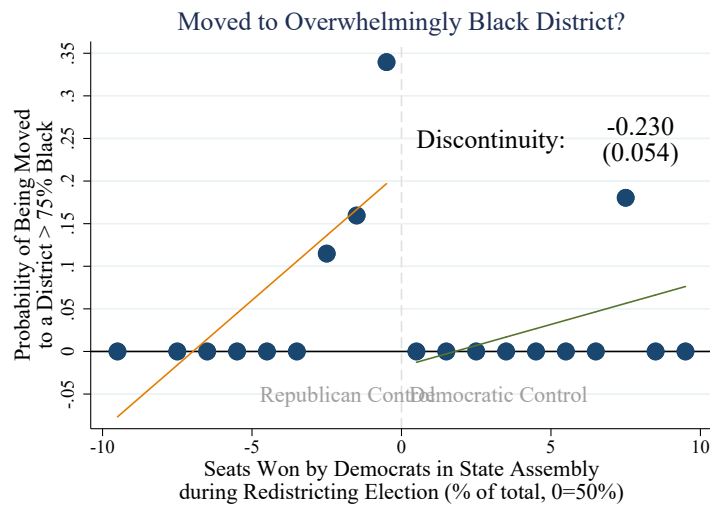


**Figure 13**  
African Americans are More Likely



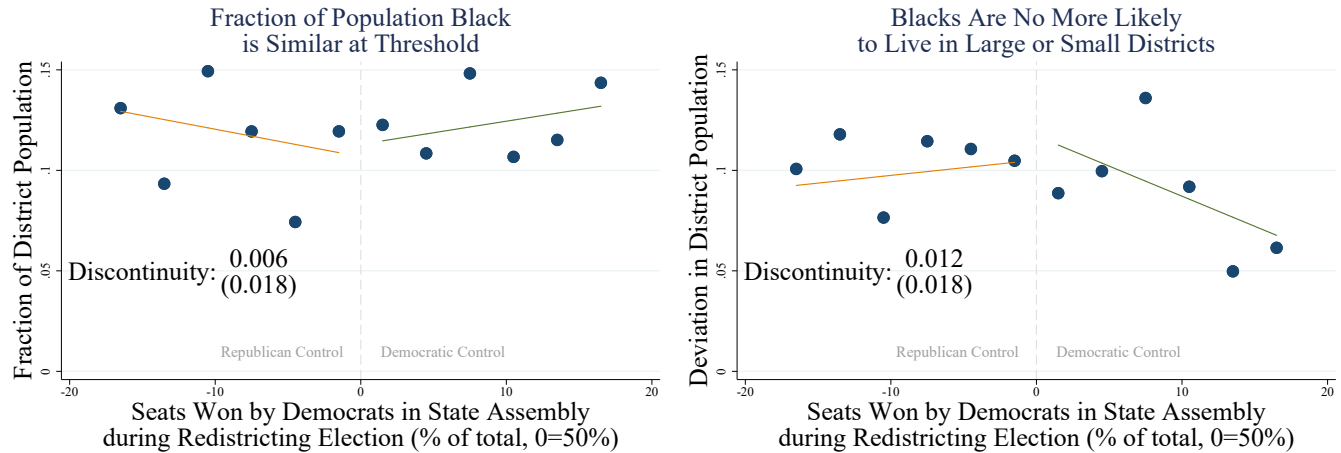
Note: See the text describing Table 3 in the mechanism section for details. Each dot gives the conditional mean within a bin of width 3.

**Figure 14**  
Conditional on Being Moved,  
Where Are African Americans Moved?



Note: See the text describing Table 3 in the mechanism section for details. Each dot gives the conditional mean within a bin of width 1.

**Figure 15**  
 No Evidence of a Greater Need to Redistricting  
 African Americans on One Side of the Threshold



*Note:* See the text describing Table 3 in the mechanism section for details. Each dot gives the conditional mean within a bin of width 3.

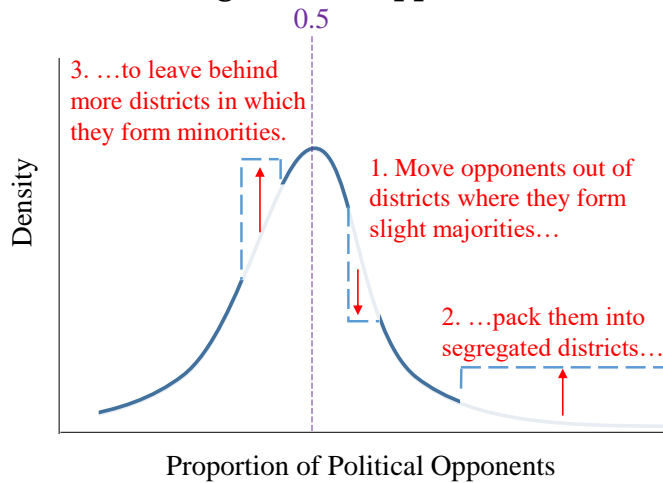
are moved. Figure 16.A shows how a hypothetical gerrymander would move hostile voters to minimize their influence. First, they would be moved out of districts in which they form a slight majority, reducing the mass of districts just above 0.5. They would then combine these opponents into districts where they form the overwhelming majority, increasing the mass at the top of the distribution. Though these districts would be lost with certainty, there would be an increase in the number of districts in which opponents are a minority. As a result, mass shifts from just above to just below 50 percent.

Figure 16.B shows what actually happens to the density of the African American population when Republicans take control. We restrict the sample as described for the regression reported in Column 3 of Table 3. We estimate the discontinuity (using the same specification as in Table 3) on dummies for whether the district into which a tract is moved has an African American population share within some range. By varying the center of this range from 0 to 1 we estimate the change in the probability density. To make the figure directly comparable to the prediction in Figure 16.A we adjust the estimates to show the change when Republicans take control from Democrats.<sup>25</sup> As predicted, there is an increase in the density

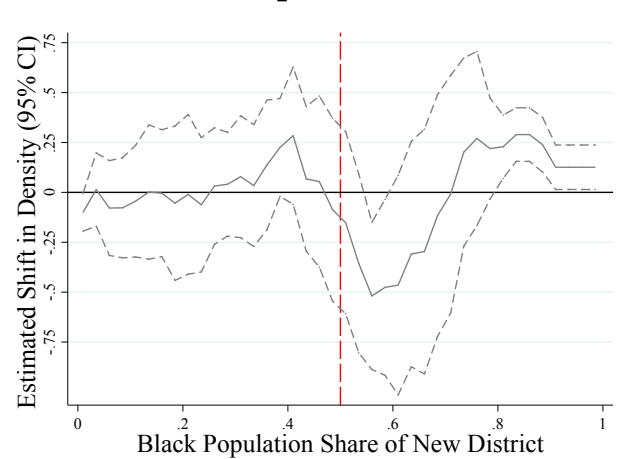
<sup>25</sup>For this figure we restrict the sample to include only states in which no independents are elected to the assembly. This restriction makes Republican control exactly the converse of Democratic control. The results

**Figure 16**

Effect of Packing and Cracking on Distribution of Political Opponents

**A. Predicted Effect of Packing and Cracking Political Opponents**

Note: See text for description.

**B. Actual Change in Distribution of African Americans when Republicans Take Control**

of overwhelmingly African American districts and districts in which African Americans are barely outnumbered. Meanwhile, the density of districts in which African Americans are a slight majority decreases. In summary, the results suggest African Americans are moved into districts that minimize the number of elections they sway.

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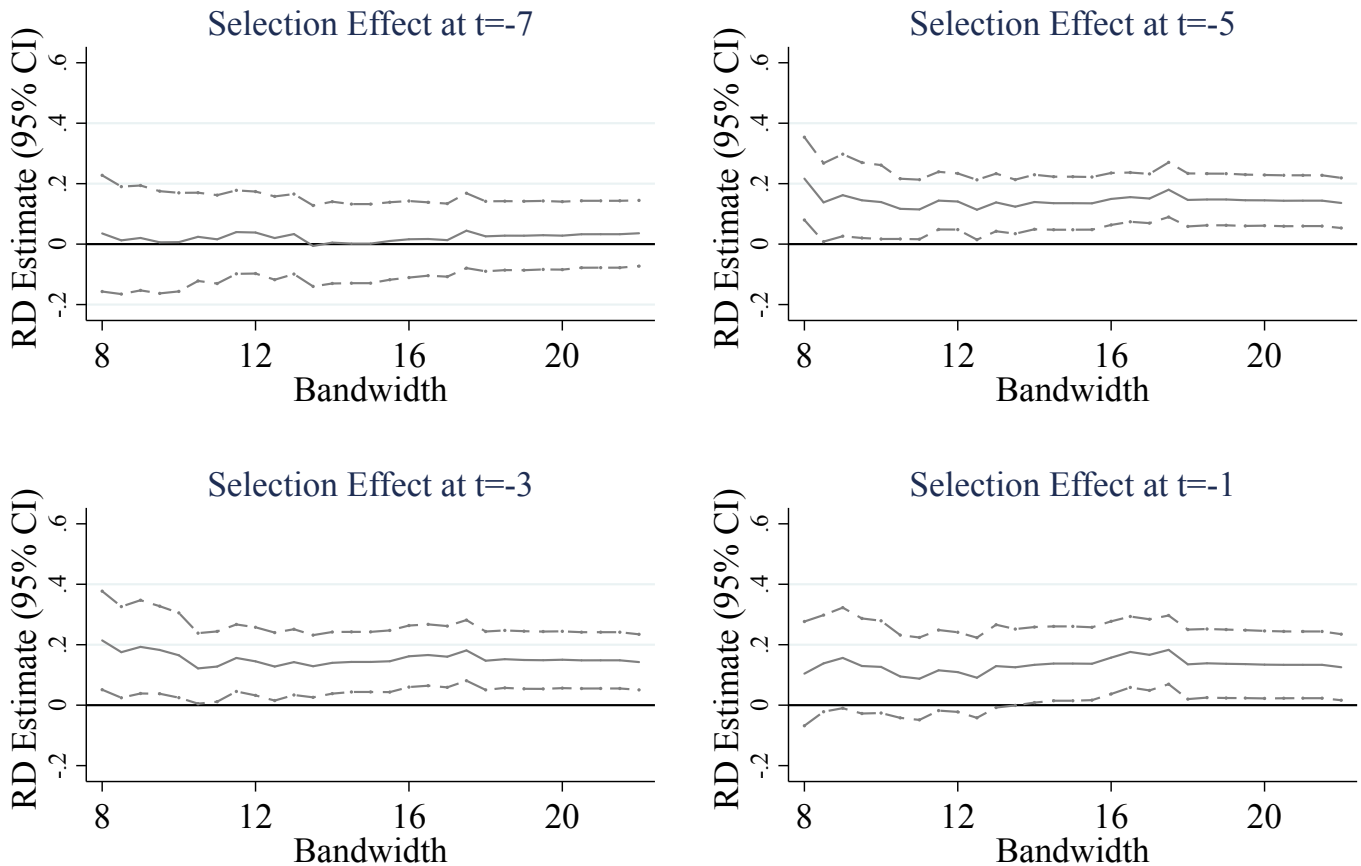
without making this restriction look similar.

## C.2 Verifying the Results are Not Driven by Choice of Bandwidth

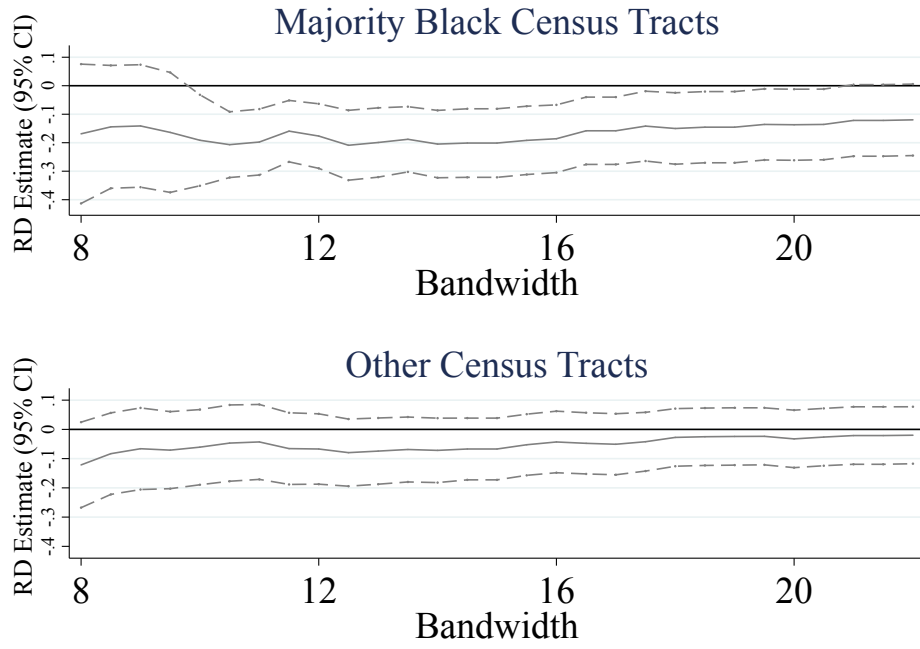
In this appendix we show that the results are robust to the choice of bandwidth. Table 2.C shows that the main results are robust—here we focus on several other results throughout the paper. Figure 17 shows the robustness of the estimates of the Selection Effect from Specification 6 of Table 1. Figures 18—21 show the robustness of the results from the mechanism section. In all cases we plot the size of the estimate and the 95 percent confidence interval against the bandwidth used to make the estimate.

**Figure 17**

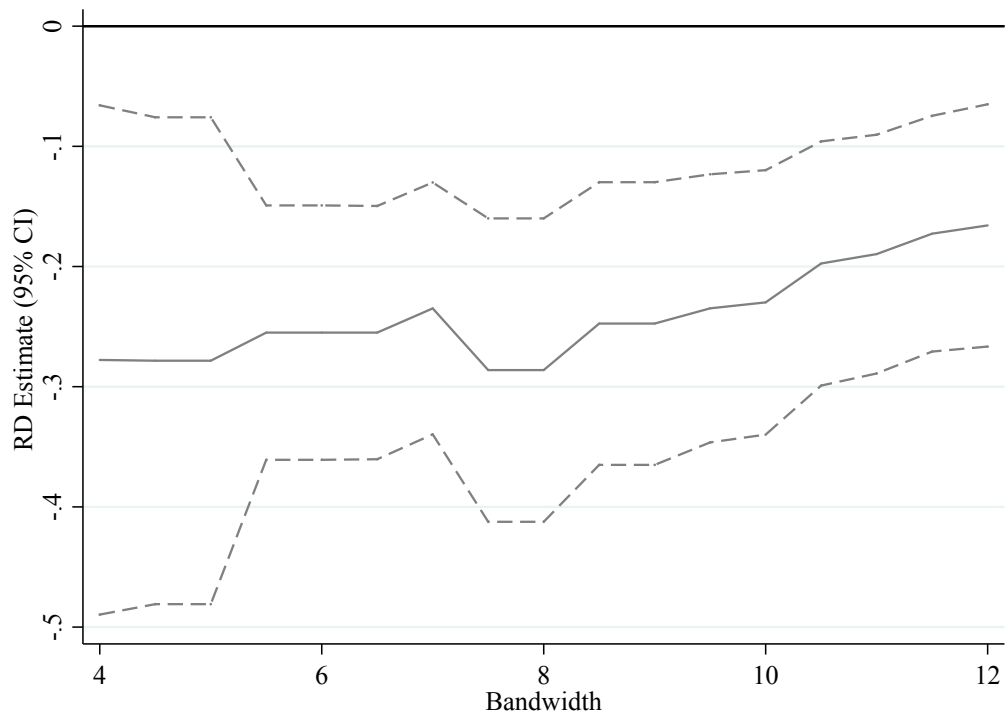
Robustness to Bandwidth: Table 1, Specification 4



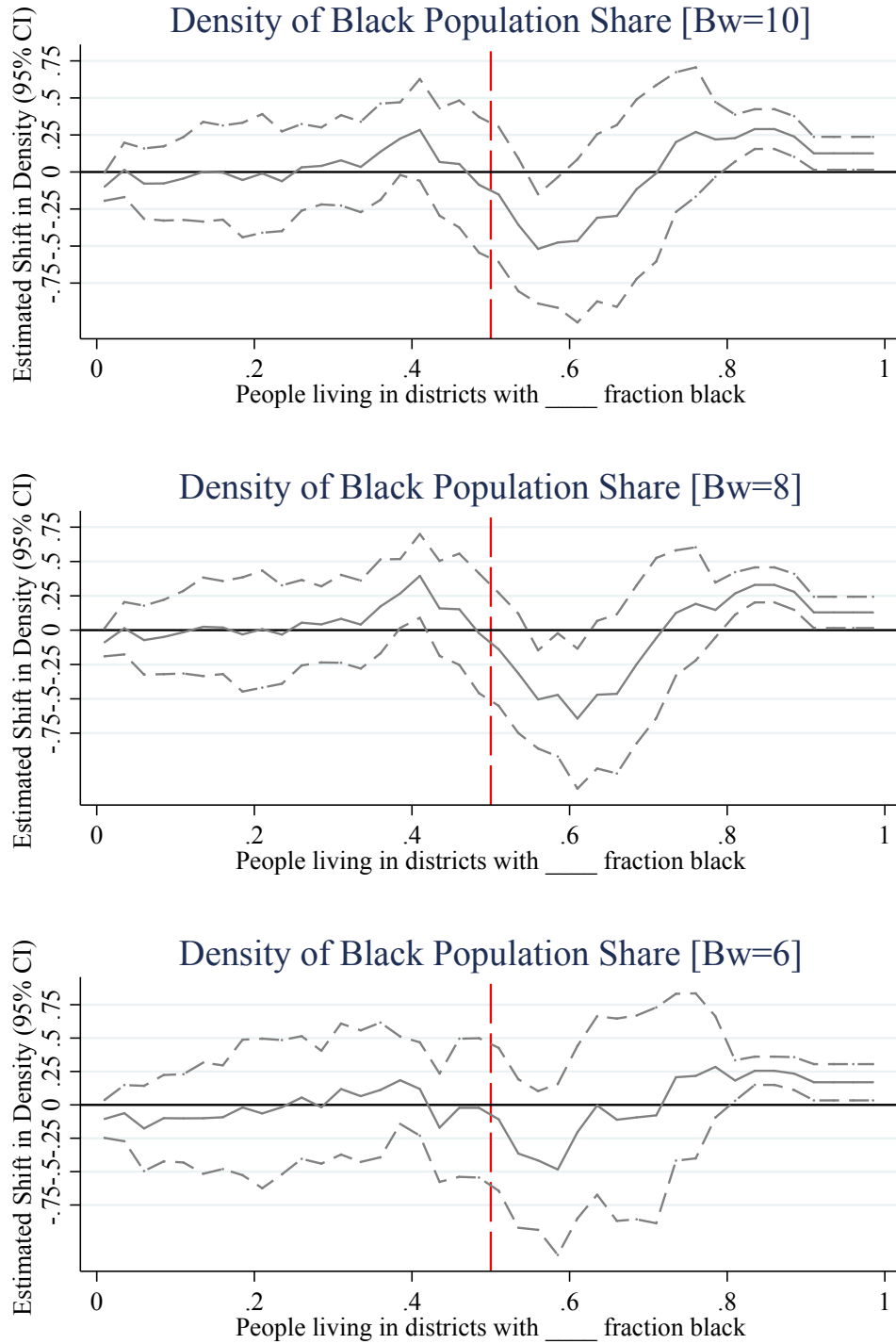
**Figure 18**  
Robustness to Bandwidth: Table 3, Columns 1 and 2



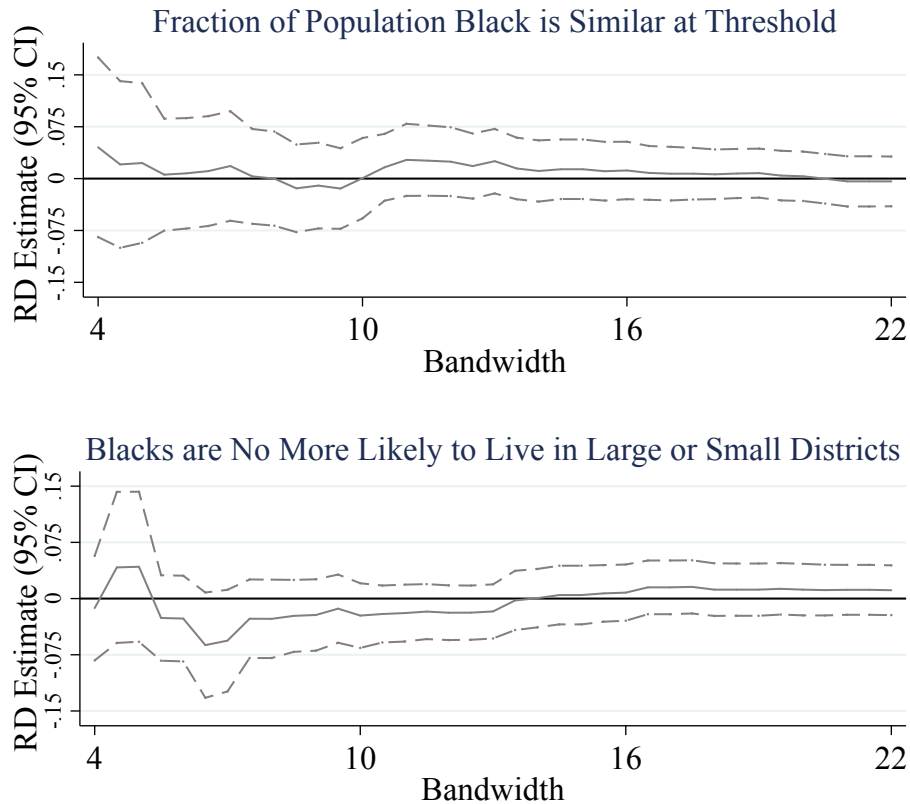
**Figure 19**  
Robustness to Bandwidth: Table 3, Column 3



**Figure 20**  
 Robustness to Bandwidth: Figure 16.B



**Figure 21**  
Robustness to Bandwidth: Table 3, Columns 4 and 5

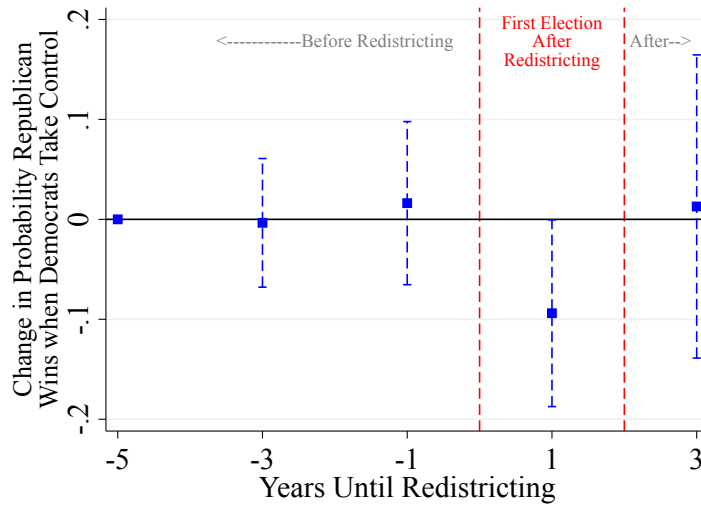


### C.3 Verifying the Results with an Alternative Dataset of U.S. House Races

As noted in the data section, the ICPSR Constituency data we use for our analysis has some disadvantages. We check the ICPSR data against that of Lee et al. (2004) for the years 1972 to 1992 (the years for which the two datasets overlap). The two datasets have in common 4,544 House races. Their data contain 114 races not contained in the ICPSR data, whereas the ICPSR dataset contains 186 races not contained in theirs. Among the races contained in both the two datasets agree on the outcome of 99.45 percent.

To verify that the results are not driven by these minor discrepancies, we redo our analysis by replacing our data with that of Lee et al. (2004) for the years that they overlap. Figure 22 shows that the difference-in-discontinuities estimates are unchanged when we use the alternative dataset.

**Figure 22**  
The Main Results Hold with the Alternative Dataset



## C.4 Verifying the Results without Maine

As noted in the front matter, Maine is an unusual case in that it has in some cycles redistricted in the year ending in 3. In this appendix we verify the results hold after dropping Maine from the sample. Figure 23 confirms that dropping Maine has little effect on the results.

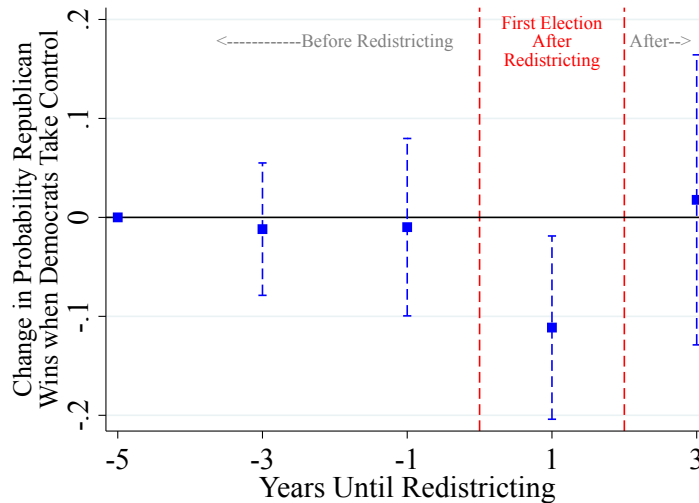
## D Data Appendix (For Online Publication)

### D.1 Election Outcomes Dataset

The Inter-university Consortium for Political and Social Research (1995) dataset of federal election outcomes gives results for each candidate within each race. We collapse it to the level of the race by summing vote shares within each party and assigning Democrats (Republicans) to win the race if one of their candidates wins. As noted in the main text we merge these federal election outcomes to the number of seats won by Democrats in the lower house



**Figure 23**  
The Main Results Hold after Dropping Maine



of the state assembly in the state election before the year the Census is completed (the year ending in 1). To restrict outcomes to open seats we merge our dataset of election outcomes to the data compiled by Bonica (2013). As noted in the text, these data are not available before 1980 or for odd-year elections (which are typically special elections). Table 4 shows descriptive statistics for election outcomes in all and in open districts.

## D.2 District Demographics Dataset

Our data on district demographics comprise two datasets, one at the level of the census tract and one at the level of the district. As noted in the text, we formed both datasets by linking tracts from each census (for example, 1980) to district boundaries both before (1980) and after (1982) the year ending in 1, which we treat as the year of redistricting. As noted in the text, for each tract we compute the fraction of the population in its new district that was not in its old district. If this fraction exceeds 50 percent we mark the tract as having been “moved” to a new district. We also compute the total district population and the fraction of the population that is African American. Finally, we compute the absolute deviation of the

**Table 4**  
Descriptive Statistics, Election Outcomes

Share of seats won in lower house of state assembly ( $X$ )			7.75	
			(17.99)	
Democrats control lower house of state assembly? ( $X \geq 0$ )			0.64	
	$X \geq 0$		$X < 0$	
	All Seats	Open Seats	All Seats	Open Seats
Republican Wins?	0.41	0.51	0.55	0.74
Republican Vote Share	0.44	0.48	0.50	0.53
	(0.18)	(0.14)	(0.17)	(0.11)
Observations	5457	409	2627	143
State-Redistricting Events	126	101	72	50

*Note:* This table reports the mean and standard deviation for election outcomes in the dataset of all outcomes and the dataset of open seats. Standard deviations are not reported for binary variables.

pre-redistricting district from the median district in the state, which is assigned to all tracts contained in the district (see main text for more details). Table 5 shows descriptive statistics for tract- and district-level outcomes.

**Table 5**  
Descriptive Statistics, District Demographics

Fraction of District Population Black:	0.13	
	(0.16)	
	Majority Black	Others
Fraction of Census Tracts Redistricted	0.24	0.29
Average Absolute Population Deviation	0.11	0.10
	(0.13)	(0.15)
Tract-Years	25279	224254
State-Redistricting Events	169	201

*Note:* This table reports the mean and standard deviation for demographics in the dataset of district demographics. Standard deviations are not reported for binary variables.

### D.3 States and Years in Sample

**Table 6**  
States and Years in Full Sample and In Bandwidth

State	Cycles in Sample	Cycles in BW
Alabama	1971,1981,1991,2001,2011	2001,2011
Arizona	1971,1981,1991	1971,1991
Arkansas	1971,1981,1991,2001,2011	2011
California	1971,1981,1991,2001	1971,1981,1991,2001
Colorado	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Connecticut	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Florida	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Georgia	1971,1981,1991,2001,2011	2001,2011
Idaho	1971,1981,1991	1971,1991
Illinois	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Indiana	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Iowa	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Kansas	1971,1981,1991,2001,2011	1971,1981,1991,2001
Kentucky	1971,1981,1991,2001,2011	2001,2011
Louisiana	1971,1981,1991,2001,2011	2011
Maine	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Maryland	1971,1981,1991,2001,2011	
Massachusetts	1971,1981,1991,2001,2011	
Michigan	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Minnesota	1991,2011	1991,2011
Mississippi	1971,1981,1991,2001,2011	2011
Missouri	1971,1981,1991,2001,2011	1981,1991,2001,2011
Montana	1971,1981,1991	1971,1981,1991
Nevada	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
New Hampshire	1971,1981,1991,2001,2011	1971,1981,2001
New Jersey	1971,1981,1991	1981,1991
New Mexico	1971,1981,1991,2001,2011	1981,2001,2011
New York	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
North Carolina	1971,1981,1991,2001,2011	1991,2001,2011
North Dakota	1971	1971
Ohio	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Oklahoma	1971,1981,1991,2001,2011	1991,2001
Oregon	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Pennsylvania	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Rhode Island	1971,1981,1991,2001,2011	
South Carolina	1971,1981,1991,2001,2011	1991,2001,2011
South Dakota	1971,1981	1971
Tennessee	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011
Texas	1971,1981,1991,2001,2011	1991,2001,2011
Utah	1971,1981,1991,2001,2011	1971,1991,2001
Virginia	1971,1981,1991,2001,2011	1991,2001,2011
Washington	1971,1981	1971,1981
West Virginia	1971,1981,1991,2001,2011	2011
Wisconsin	1971,1981,1991,2001,2011	1971,1981,1991,2001,2011

*Note:* This table reports each state-redistricting cycle included in the sample. States that meet the inclusion criteria described in Section 5.2 are in “Cycles in Sample,” while those that additionally fall within the bandwidth of Figure 3 and the other results are in the column “Cycles in BW”. A state that does not appear in this table (e.g. Nebraska) is excluded entirely from the sample.