Endogenous Institutions: The Case of U.S. Congressional Redistricting

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Abstract

We propose a method that simultaneously identifies where parties take control of U.S. Congressional redistricting, and how they use it. Our method exploits the discontinuous change in a party’s control of redistricting triggered when its share of seats in the state legislature exceeds 50 percent, and the timing of the redistricting calendar. We find that parties capture redistricting in states where they are losing popularity. They use redistricting to temporarily reverse their decline. Opposition votes are less efficiently converted to seats and, under Republican redistricting, African Americans are more likely to be segregated into overwhelmingly black districts. (JEL Codes: D72, D78)

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1 Introduction

Once every decade, U.S. Congressional districts must be redrawn. Their boundaries determine how many left- or right-leaning voters a candidate will face, potentially swaying an election. But in most states the boundaries are drawn by the state legislature, which is controlled by whichever party holds a majority. It is widely believed the majority party skews districts to favor its own candidates. Partisan redistricting, or gerrymandering, has been attacked in the popular press as “corrosive to a representative democracy” (New York Times, Editorial, May 30, 2017) and a means by which to “rig an election” (Economist, 2002). The practice has also come under intense legal scrutiny. As of this writing, the U.S. Supreme Court is scheduled to affirm or overturn a lower court’s decision to strike down Wisconsin’s 2011 redistricting plan, which was ruled an unconstitutional partisan gerrymander.

Some academics have countered the public furor by arguing that partisan control of redistricting is relatively inconsequential (Chen and Rodden, 2013) or may even enhance democracy (Gelman and King, 1994a). But the theory of endogenous political institutions predicts that such optimism may be misplaced. District boundaries can be viewed as an electoral rule. In states where the rule-setters expect to subsequently lose influence, the theory suggests they should adjust the rules to prevent or at least forestall their losses (see, for example, Aghion et al., 2004). This hypothesis has two parts: it predicts how agents should write the rules, and where they would want to be the rule-setters. Congressional redistricting offers a unique means to test both predictions. Since it happens at frequent and regular intervals, we can observe not only whether parties draw favorable districts but whether they actively seek to control redistricting in states where they expect they might otherwise lose influence. If true the hypothesis suggests that, far from enhancing democracy, redistricting is a means by which political parties can distort electoral institutions to reverse the desires of the electorate.

But testing either of these two predictions raises a host of challenges. Omitted variables would confound any test for whether parties seek to control redistricting in states where they are losing influence. If Republicans win control of redistricting in conservative states, is it because they actively sought it? Or are such states simply more likely to elect Republican majorities? Meanwhile, a test for whether parties use redistricting to reverse their losses suffers from the same problem of omitted variables, but is further complicated by each party’s choice of where to control re-
districting. Any estimate of the Causal Effect of partisan redistricting must control not only for omitted variables but this Selection Effect.

This paper designs and applies an approach that plausibly estimates both the Selection Effect and the Causal Effect. Our approach 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 state’s redistricting plan. 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 just above the cutoff.

Our approach to estimate the Selection Effect is rooted in 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. We apply this method to the outcomes of state assembly elections that determine which party controls redistricting. As we show in a companion paper (Jeong and Shenoy, 2017) the party that holds a majority before the redistricting election is with high probability able to win precisely enough seats to retain its majority—a phenomenon called “precise control.”

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 a conscious effort to seize 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 races for the U.S. House that were decided before redistricting. These races cannot be affected by redistricting, but parties can look to these past outcomes in deciding where to control it. If parties seek to control redistricting in places where they have sustained recent losses, then there should be a jump at the cutoff in the number of House races won by their opponents.

To illustrate the approach, consider a simple example. Suppose Democrats are planning their strategy for the state assembly elections that determine which party controls redistricting in Pennsylvania and Ohio. In Pennsylvania, the more liberal of the two, Democrats have been winning recent U.S. House races. Ohio is the oppo-
site. In both states Democrats expect to win roughly half the seats in the assembly. They have enough resources to barely tip the scales—that is, exert precise control—in one but not both. Which do they choose? The hypothesis predicts they should seek to control Ohio while Republicans, making the same calculation, should seek Pennsylvania. Then in states like Ohio—those where Republicans have won relatively more House seats—the number of Democrats elected to the assembly should just exceed the cutoff. In states where Republicans have lost House seats, the number of Democrats in the assembly should fall just below. This sorting across the cutoff creates a discontinuity in the outcomes of past House races, which we define as the Selection Effect. Though its presence does not prove that parties consider only past U.S. House outcomes in choosing where to control redistricting, it suggests the partisan lean of a state is among the factors they consider.

We then measure the discontinuity in the outcomes of House races after redistricting. Since these outcomes are affected by redistricting, the 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. To continue the earlier example, we would test whether House races in Ohio, which were relatively unfavorable to Democrats before redistricting, suddenly become more favorable after redistricting. Such a shift would suggest Democrats took control of redistricting in a relatively hostile state and redrew districts to make it more favorable.

The key assumption behind this difference-in-discontinuities approach is that the Selection Effect does not abruptly change from before to after redistricting. That is, we must rule out that Ohio does not abruptly become less likely to elect Republicans for reasons unrelated to district boundaries (such as the preferences of the electorate). But whether a Republican is elected depends only on the overall share of votes received by Republicans, and the distribution of those votes (i.e. the district boundaries). Any confounding factor, such as whether Ohioans grow tired of Republicans, would impact election outcomes only through the vote share. If the Selection Effect on the Republican vote share remains unchanged from before to after redistricting—if Republicans win 5 percentage points fewer votes in Pennsylvania than in Ohio both before and after redistricting—it suggests that, as far as election outcomes are concerned, the other factors remain fixed. We show that the Selection
Effect on vote shares is indeed unchanged after redistricting, implying the change we observe must be driven by a change in district boundaries.

Our results are consistent with the theory of endogenous institutions. We find that each party chooses to capture redistricting in states where the opposition has made recent gains. These gains are reversed 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.

Next we show evidence that gerrymandering is the mechanism behind these effects. We show that after redistricting there is a sharp decline in the rate at which opposition party votes are converted into seats. We also show that the Causal Effect is largest in open districts. As these districts have no incumbent, their partisan bias is more likely to determine the outcome. The probability a Republican wins an open district falls from 80 percent to 30 percent when Democrats take control. Using a measure of vote efficiency cited in the aforementioned Wisconsin court case (Gill v. Whitford, 2016), we show that Republican votes in these open districts are more efficiently distributed when Republicans control the lower house during redistricting.

We also show that the demographic composition of redrawn districts changes discontinuously at the cutoff. Compared to Democrats, Republican legislatures are roughly 15 percentage points more likely to move majority-black census tracts, which overwhelmingly support Democrats, to new districts. There is no difference in the treatment of census tracts that are not majority-black. Conditional on moving an African American voter, Republicans are more likely than Democrats to redistrict her in a way that reduces her electoral influence. This difference in treatment is not obviously explained by any objective need to redistrict African Americans differently in states where Republicans control redistricting. There is no evidence that, before redistricting, there is any difference at the cutoff in the African American population share of districts, or that African American census tracts are more likely to be located in districts with too many or too few residents.¹

To our knowledge this is the first study that interprets Congressional redistricting through the lens of the endogenous institutions hypothesis. We provide not only a

¹We cannot distinguish whether black voters are treated differently because they are black or because they typically vote Democratic. The results are equally consistent with both taste-based and statistical discrimination.
novel interpretation but more credible estimates. To our knowledge no prior work has used a regression discontinuity or difference-in-discontinuities design to estimate the effect of partisan redistricting.\(^2\)

In doing so our study also provides one of the few tests of the endogenous institutions hypothesis. Prior work has either studied cross-country correlations (Aghion et al., 2004; Ticchi and Vindigni, 2010) or looked at how a uniformly dominant group of rule-setters responds to a blanket reform that increases political competition (Trebbi et al., 2008; Drometer and Rincke, 2014; Baskaran and da Fonseca, 2016). These approaches make it difficult to rule out confounding factors, and also make it difficult to credibly estimate the effectiveness of the new rules in shutting out opposition. By studying a situation where different parties can seize control of the rule-setting process, and by credibly estimating where they take control, we use revealed preference to measure the Selection Effect. This measurement also lets us estimate the Causal Effect, confirming that the rules are indeed being set to disadvantage the opposition.

1.1 Applying the Model of Endogenous Institutions to Congressional Redistricting

The model of Aghion et al. (2004) offers the clearest lens through which to study Congressional redistricting. Their model has two stages. In Stage 1 an electoral rule is chosen. This rule sets the threshold of support needed to enact an unknown redistributive policy that will be voted on in the next stage. In Stage 2 the policy, and the identities of the citizens who will be harmed or helped, becomes known; then the public votes on whether to enact it. The key insight is that if in Stage 1 those who set the rule expect to remain the majority, they choose a rule that allows majority rule. But if they suspect they will be in the minority, they manipulate the rule to ensure they can override the majority.

Applying this logic to our context, in each state the district boundaries are effectively a rule that determines how a party’s level and distribution of statewide support is translated into the fraction of U.S. House seats it wins. Holding support for the Republicans fixed at 50 percent, for example, the district boundaries may determine whether Republicans win half the seats, more than half, or less than half (see Section 2.1 for an example). By this interpretation, the party that controls the state assembly

\(^2\)As we describe below, Friedman and Holden (2009) study the separate phenomenon of incumbent gerrymandering using a temporal regression discontinuity.
during redistricting effectively determines the threshold of support needed for each electoral outcome.

Our innovation is to imagine adding a “Stage 0” in which parties choose the U.S. states in which they want to manipulate the rules. The results of our companion paper suggest that the majority party—whichever party holds a majority in the assembly before the election that determines control—can exert precise control over the outcome (Jeong and Shenoy, 2017). The majority party has a unique ability to shift its electoral tactics in a way that drastically reduces the probability it loses its majority (see Section 2.3). The minority party cannot fully neutralize these tactics and thus cannot prevent the majority party from exerting precise control.

But though holding a majority lets the State Republican or State Democratic Party engage in these tactics, they cannot be used successfully without leveraging campaign resources on a scale available only to the national parties. The National Republican and National Democratic Parties raise funds and other resources over the years leading to the redistricting election, then decide how to allocate these finite resources. As per the logic above, they would prefer to expend resources to manipulate the rules in states where candidates for national office are growing increasingly embattled, as reflected in the outcomes of recent U.S. House races. Hence the two predictions stated formally in Section 3.1: parties seek to control redistricting in places where they have sustained recent losses, and they use redistricting to reverse those losses.

1.2 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, 2006). 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, 2016). Several of the most recent studies have concluded that the actual plans are no more favorable than would have arisen by chance.

Overall the simulation literature seems to refute the theory of endogenous institutions. But any simulation approach works only to the extent that the simulated outcome is an accurate reflection of the true counterfactual. Parties adapt their electioneering to reflect the district map. By holding their behavior—and that of the voters—fixed, the simulation may fall short of being a true counterfactual. By exploiting a natural experiment to estimate the counterfactual, our study does not have to make this assumption.

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 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 estimates may be biased if there are trends in the attitude of the electorate, or if states and years in which redistricting is done by different parties are systematically different in other ways. Our research design is able to account for both omitted confounders and the Selection Effect.\footnote{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.}

Finally, there is a distinct literature that studies incumbent gerrymandering, the bipartisan attempt to help incumbents get reelected. 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. Since incumbents always control redistricting, it is impossible to test the endogenous institutions hypothesis by observing the choice of where they seek control.\(^5\)

## 2 Background

### 2.1 What is Redistricting and Why is it Worth Controlling?

Political parties have engaged in partisan redistricting, or “gerrymandering,” since the first days of the republic.\(^6\) To understand why, consider a state that contains 6 likely Democratic voters and 3 likely Republican voters. A redistricting plan must divide these voters into 3 equally sized Congressional districts. In Plan 1 each district contains 2 Democrats and 1 Republican. In Plan 2 all the Republicans are put into a single district while the other voters are put into the other districts. Though the total number of Democratic and Republican voters is held fixed, under Plan 1 the Democrats win all three seats while under Plan 2 the Democrats win only two seats. Clearly Democrats prefer Plan 1 while Republicans prefer Plan 2.

As parties have worked harder to enact favorable plans, district boundaries have grown longer and more contorted. Figure 1.A shows that whereas the Kansas 3rd district (drawn in 1941) was roughly rectangular, the Texas 18th district (drawn in 1991) is almost fractal. By one measure, the ratio of the district’s perimeter to the square root of its area, the Texas 18th is 7 times more contorted. Figure 1.B plots the median perimeter-area ratio of all districts over time. In the early part of the past century, many state legislatures left district lines unchanged to avoid making incumbents face new voters. As a result, the ratio changes little up through the 1960s.

States only began regular redistricting when it was ruled mandatory by the Supreme

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\(^5\)Abramowitz et al. (2006) and Carson et al. (2014) also study incumbent gerrymandering using other research designs.

\(^6\)The first target was James Madison, the mastermind of the U.S. Constitution, who was forced to run for office in a district drawn by his Anti-Federalist enemies (Weber, 1988). Madison won despite their efforts. Ironically it was Madison’s future running mate, Elbridge Gerry, who as governor of Massachusetts signed into law the politically favorable but salamander-shaped district that was first called the “Gerrymander.”
Court in *Baker v. Carr* 369 (1962) and *Wesberry v. Sanders* 376 (1964). Nearly all states that were apportioned more than one Congressional district started redrawing their districts in the year after the decennial census.\(^7\) Starting in 1972, the perimeter-area ratio jumps in the elections just after the census. By and large it jumps upward, suggesting districts have become increasingly contorted.\(^8\)

### 2.2 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.\(^9\) This bill, if signed by the governor, becomes law. The next election to the U.S. House is contested under the redrawn district map.\(^10\)

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\(^7\)States may redistrict in the year after the census, but may not always succeed in passing a bill. There are some cases (e.g. Texas in 2003) when states have chosen to redistrict again later in the cycle. We do not exploit this variation, as the decision to redistrict may itself be endogenous.

\(^8\)The election after the 1990 census is an exception. This may be because in that year states tried to make their districts more compact, or it may be a shortcoming of the perimeter-area ratio as a measure of contortion.

\(^9\)Nebraska, which has a unicameral nonpartisan legislature, is excluded from this study.

\(^10\)For example, on 23 February 2001 Bob Hertzberg introduced to the California State Assembly “An act to add Chapter 5 (commencing with Section 21040) to Division 21 of the Elections Code, \(\sqrt{\text{Area}}\).
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.\textsuperscript{11} That gives them a strong incentive to take control of the legislature just before the redistricting process begins. 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 2 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 won by Democrats in the previous election.\textsuperscript{12} 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 no less 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. It is thus critical to have a majority in the lower house in years when the opportunity to redistrict arrives.

That opportunity arrives every ten years with the decennial census. Aside from making it possible to create districts with equal populations, the census helps the party in power gerrymander on demographics. As shown in Figure 3, the census is completed in years ending in 1.\textsuperscript{13} Whichever party wins the election to the state leg-

\textsuperscript{11}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.

\textsuperscript{12}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. (Unfortunately we do not have these votes for any earlier cycle.) We link the roll call votes to the percentage of seats won by Democrats in the previous election (relative to 50 percent). Based on this percentage we divide the observations into bins of width 5, then compute the average fraction of Democrats and Republicans that vote in favor of the redistricting bill within each bin.

\textsuperscript{13}The redistricting bill may 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.
Figure 2
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.

islature just before this year has the opportunity to pass its own redistricting plan.14 These key elections, labeled from here onwards as “redistricting elections,” create the variation we exploit to estimate the Selection Effect and Causal Effect of redistricting.

2.3 Can Parties Exert Precise Control Over the Outcomes of Redistricting Elections?

As described in the introduction and in the next section, the key to identifying the Selection Effect is that parties must be able to exert precise control over the outcomes of elections. 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 is clear that each political party has an incentive to ensure the number of seats it wins falls and endogenous to our outcome of interest. Instead we focus on the opportunity to redistrict, which comes with the completion of the census.

14In 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.
just above the 50 percent cutoff. But does it have the means?

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. Our identification rests on whether 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), it may be possible to almost guarantee 50% + 1 seats fall to the Democrats.

As we show in Jeong and Shenoy (2017), the majority party can achieve precise control by switching to what political scientists call “defensive” or “majority-seeking” tactics (Makse, 2014). As shown by Snyder (1989), these tactics heavily favor the party that already has an advantage—in this case, the party that previously held a majority and thus contests the election with more incumbents. These tactics can drastically increase the chance of retaining a majority at the cost of potentially shrinking it. The majority party is willing to accept this trade-off when the outcome of the election determines which party controls the assembly during Congressional redistricting—what we have labeled “redistricting elections.” The minority party,
Figure 4
Density Tests Show Strong Evidence of Precise Control in State Assembly Elections that Determine Which Party Controls Redistricting

![Figure 4](image)

Note: Adapted from Jeong and Shenoy (2017). We run a McCrary test on the share of seats won by the party that held a majority before the election.

even when responding optimally, cannot neutralize these tactics and thus cannot prevent the majority party from exerting precise control.

Figure 4, which is taken from Jeong and Shenoy (2017), applies a McCrary test (2008) to the fraction of seats won in the state assembly by whichever party previously held a majority. In the absence of precise control the probability density of election outcomes should be continuous. The majority party should have a similar probability of winning 49 percent of seats versus 51 percent. Although it would much prefer to win the extra one or two seats that retain its majority, there is a reasonable probability they fall to the opposition. This pattern is precisely what happens in a typical election (right-hand panel)—there is no evidence of a discontinuity at the 50 percent cutoff. But the left-hand panel shows that there is a large discontinuity in redistricting elections. The majority party is somehow able to almost guarantee it wins those last few seats needed to retain its majority.
3 Research Design

From a simple empirical model, Section 3.1 derives consistent estimators for the Selection Effect and Causal Effect. We assume each party is able to exert precise control over some fraction of elections. We show that a regression discontinuity estimator applied to pre-redistricting elections yields an estimate of the average partisan lean of states where Democrats choose to control redistricting, as compared to those chosen by Republicans. We then show how applying a difference-in-discontinuities estimator to the entire sample of elections nets out the Selection Effect and yields a consistent estimate of the Causal Effect. Section 3.2 describes the regressions we estimate, and Section 3.3 describes the data.

3.1 Estimating the Selection Effect and the Causal Effect of Partisan Redistricting

Let $s$ index a state-redistricting event—for example, California’s 1981 redistricting. Each $s$ has a partisan drift towards the Republicans equal to $\theta_s \in \mathbb{R}$, which has an absolutely continuous distribution $H^{\theta}$. This drift reflects how favorable the state is to Republicans running for the U.S. House in the years just before and after that redistricting event.

Suppose that in the absence of precise control the margin of seats won by Democrats in the state assembly during the redistricting election for $s$ is

$$X_s^* = x(\theta_s) + v_s$$

(1)

where $x$ is a continuously differentiable function and $v_s$ a shock with an absolutely continuous distribution $F$ and density $f$. The function $x(\theta_s)$ captures how a state that is favorable to Republicans running for Congress may also be favorable to those running for the state assembly.

Precise control allows a party to adjust the number of seats won by Democrats in the assembly. Assume that Democrats have the chance to exert precise control over $\frac{1}{2}$ of all redistricting elections. Republicans can control the other $\frac{1}{2}$. Conditional on having the chance to exert control, each party has the resources to actually control only a fraction $\mu$. If Democrats exert control, the actual outcome of the election equals a random variable $\tilde{X}_s$ with density $\chi$. We assume $\chi(\tilde{X}_s) = 0$ for $\tilde{X}_s < 0$, ...
meaning Democrats do not lose elections they control; that $\chi(0) > 0$, meaning there is some chance they only barely retain control; and that $\chi(\tilde{X}_s)$ is right-continuous at $\tilde{X}_s = 0$. When Republicans exert control the outcome equals $-\tilde{X}_s$.

As per the endogenous institutions hypothesis, assume that both parties decide whether they want to control redistricting in a state based on its partisan drift. Democrats want to control elections where $\theta_s \in \Theta_D$, while Republicans want to control those where $\theta_s \in \Theta_R$. (Think of $\Theta_D$ and $\Theta_R$ as choice sets.) By construction, $H^\theta(\Theta_D) = H^\theta(\Theta_R) = \mu$. Then the actual margin of seats won by Democrats is

$$X_s = \begin{cases} X^*_s & \text{w/prob } 1 - \mu \\ \tilde{X}_s & \text{w/prob } \frac{1}{2}\mu \\ -\tilde{X}_s & \text{w/prob } \frac{1}{2}\mu \end{cases}$$

(2)

The parties choose to exert control over the redistricting election because redistricting helps them win U.S. House elections. Let $W_{ist}$ be a dummy for whether Republicans win House seat $i$ during the election in year $t$ in the state-redistricting cycle $s$. Assume

$$W_{ist} = \theta_s w(t) + \beta^t R_{st} + \eta_{ist}$$

(3)

where $w$ is a continuous function whose properties are discussed below; $\mathbb{E}[\eta_{ist} | X_s]$ is continuous in $X_s$; and $R_{st}$ is a dummy equal to 1 if Democrats control the lower house of the state legislature and the election takes place after redistricting. To be precise, if 0 is the year of redistricting then $R_{st} = \mathcal{I}(X_s \geq 0) \times \mathcal{I}(t > 0)$. Then $\beta^t$ is the effect in year $t$ of having had Democratic control of the legislature during redistricting. The term $\theta_s w(t)$ reflects the time-varying partisan bias of each state.

Conditional on the outcome of a redistricting election, Republicans are expected to win with probability $\mathbb{E}[W_{ist} | X_s] = \mathbb{E}[\theta_s | X_s] w(t) + \beta^t R_{st} (X_s) + \mathbb{E}[\eta_{ist} | X_s]$. As we show in Online Appendix A.1,

$$\mathbb{E}[\theta_s | X_s] = a(X_s) + b(X_s) \cdot \begin{cases} \mathbb{E}[\theta_s | \theta_s \in \Theta_D] & \text{if } X_s \geq 0 \\ \mathbb{E}[\theta_s | \theta_s \in \Theta_R] & \text{if } X_s < 0 \end{cases}$$

(4)

for some functions $a(X_s)$ and $b(X_s)$ that are continuous at 0 with $b(X_s) = 0$ if $\mu = 0$.\footnote{\label{footnote:b} $b(X_s)$ is actually the probability the outcome $X_s$ has been controlled.}
Define the regression discontinuity estimate for year $t$ as

$$\rho_t = \lim_{\varepsilon \to 0} \left\{ \mathbb{E}[W_{ist} \mid X_s = \varepsilon] - \mathbb{E}[W_{ist} \mid X_s = -\varepsilon] \right\}$$

$$= \lim_{\varepsilon \to 0} \left[ a(\varepsilon) - a(-\varepsilon) + b(\varepsilon)\mathbb{E}[\theta_s \mid \theta_s \in \Theta_D] - b(-\varepsilon)\mathbb{E}[\theta_s \mid \theta_s \in \Theta_R] \right] w(t)$$

$$+ \mathbb{E}[\eta_{ist} \mid X_s = \varepsilon] - \mathbb{E}[\eta_{ist} \mid X_s = -\varepsilon] + \beta^t \cdot \mathbb{I}(t > 0)$$

$$= b(0) \left[ \mathbb{E}[\theta_s \mid \theta_s \in \Theta_D] - \mathbb{E}[\theta_s \mid \theta_s \in \Theta_R] \right] w(t) + \beta^t \cdot \mathbb{I}(t > 0) \quad (5)$$

The first term in Equation 5 is the Selection Effect, which is informative about where Democrats versus Republicans seek to control redistricting. The second term is the Causal Effect of redistricting.

We can estimate the Selection Effect in year $t$ as

$$\rho_{t\mid t<0} = b(0) \left[ \mathbb{E}[\theta_s \mid \theta_s \in \Theta_D] - \mathbb{E}[\theta_s \mid \theta_s \in \Theta_R] \right] w(t) \quad (6)$$

which is simply the regression discontinuity estimate in a year before redistricting. For any function $w(t)$ a time-varying regression discontinuity estimator gives a valid estimate of the Selection Effect. This setup is quite flexible, in that if $w(t)$ is a constant we can infer that parties select based on criteria that are relatively invariant, whereas if $w(t)$ is a linear trend we can infer that parties select based criteria that are changing. In Section 5.1 we estimate $w(t)$ nonparametrically using a fully flexible set of time dummies and find that it appears to be constant.

The Selection Effect measures the relative popularity of Republicans in states where Democrats choose to take control (or equivalently, the relative popularity of Democrats in states where Republicans choose to take control). It is informative about whether parties capture redistricting in states where their opponents are strong or weak. The endogenous institutions hypothesis predicts that parties seek to control redistricting in states where the opposition has won recent U.S. House races before redistricting (a “pro-opposition Selection Effect”), implying that $\rho_{t\mid t<0}$ estimated in Equation 6 should be positive.

**Prediction 1 (Selection Effect)** The endogenous institutions hypothesis predicts the Selection Effect is positive (pro-opposition).

To estimate the Causal Effect, first define the difference-in-discontinuities esti-
mate for year $t$ (relative to year $k$) as

$$\rho_t^\Delta = \rho_t - \rho_k$$  \hspace{1cm} (7)

Suppose that $w(t)$ is approximately constant in the neighborhood $\bar{t} \leq t < \bar{r}$ for negative $\bar{t}$ and positive $\bar{r}$ (which, as we show below, is what our estimates of the Selection Effect suggest). Then $\rho_t^\Delta$ is a consistent estimate of $\beta^t$, the Causal Effect of redistricting in year $t$, as long as $\bar{t} \leq k < 0 < t < \bar{r}$. If $w(t)$ is instead a parametric function of $t$—for example, a linear trend—we can recover $\beta^t$ by controlling for a trend in the size of the discontinuity.

Since we estimate the impact of Democratic control on the probability a Republican wins, $\rho_t^\Delta$ effectively measures the impact on the opposition party. The endogenous institutions hypothesis predicts that when one party controls the assembly during redistricting, the opposition party loses seats (an “anti-opposition Causal Effect”). Then $\rho_t^\Delta$ estimated in Equation 7 should be negative for $t > 0$.

**Prediction 2 (Causal Effect)** The endogenous institutions hypothesis predicts the Causal Effect is negative (anti-opposition).

### 3.2 Regression Equations

Define the margin of seats won by Democrats as

$$X_s = \frac{[\text{Democrats in State Assembly}]_s - \frac{1}{2}[\text{Total Assembly Members}]_s}{[\text{Total Assembly Members}]_s} \times 100\%$$  \hspace{1cm} (8)

If there is an uneven number of seats in the assembly we round $\frac{1}{2}[\text{Total Assembly Members}]_s$ up to the next integer. Rounding up ensures $X_s = 0$ is the fewest number of seats Democrats can win without becoming the minority.\(^\dagger\)

Let $t$ be the year of a U.S. House race relative to a redistricting event $s$. For example, if $s$ is the 1981 redistricting of California then a 1980 House race happens in $t = -1$ and a 1986 race happens in $t = 5$. If we fix $t = T$ we can estimate the

\(^\dagger\)In states where there is an even number of seats, a value of zero implies neither party is either the majority or the minority party. Democrats effectively have a veto over redistricting. For example, after the 2000 election left Washington with a perfectly divided house the two parties elected co-speakers and assigned each committee co-chairs from the two parties.
regression discontinuity (5) by running a local linear regression

\[
[\text{Outcome}]_{isT} = \gamma_0 + \gamma_1 X_s + \gamma_2 X_s \cdot I(X_s \geq 0) + \rho T \cdot I(X_s \geq 0) + \nu_{ist} \tag{9}
\]

for \( |X_s| < h \)

where \( h \) is the bandwidth and \( \nu_{ist} \) the error term. For \( t < 0 \) the estimate \( \hat{\rho}_{t} \) is informative about the Selection Effect, and comparing \( \hat{\rho}_{-1} \) to \( \hat{\rho}_{1} \) is informative about the Causal Effect of redistricting.

But to fully estimate the Selection Effect we apply Equation 6 to a panel of elections. Let \( C \) be a row vector of controls, and let \( w \) be a row vector of the set of terms that define \( w(t) \). Recall that \( W \) is a dummy for whether the Republican U.S. House candidate wins. We estimate the Selection Effect by running the regression

\[
W_{ist} = w \pi_0 + X_s w \pi_1 + X_s \cdot I(X_s \geq 0) w \pi_2 + I(X_s \geq 0) w \cdot \rho + C_{ist} \pi_3 + \nu_{ist} \tag{10}
\]

for \( |X_s| < h, \ t = \{-9, -7, \ldots, -1\} \)

In Section 5.1 we let \( w \) be a full set of year dummies. The corresponding estimates \( \hat{\rho} \) are fully flexible estimate of the time variation in the Selection Effect.

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 follow the convention of assigning to an event all elections starting 5 years before through 3 years afterwards. We estimate

\[
W_{ist} = \alpha^{\text{base}}_0 + \alpha^{\text{base}}_1 X_s + \alpha^{\text{base}}_2 X_s \cdot I(X_s \geq 0) + \rho^{\text{base}} \cdot I(X_s \geq 0)
\]

\[
+ \sum_{k=-3, -1, \ldots, 3} I(t = k) \left\{ \alpha^k_0 + \alpha^k_1 X_s + \alpha^k_2 X_s \cdot I(X_s \geq 0) + \rho^k \cdot I(X_s \geq 0) \right\} + \nu_{ist} \tag{11}
\]

for \( |X_s| < h, \ t = \{-5, -3, \ldots, 3\} \)

The estimates of \( \{\rho^t\}_{-5 < t < 0} \) give the Selection Effect relative to \( t = -5 \). If the Selection Effect is constant within the window \( t = \{-5, -3, \ldots, 3\} \) then we would expect \( \hat{\rho}_{-3} = \hat{\rho}_{-1} = 0 \). The estimates \( \{\rho^t\}_{0 < t \leq 3} \) equal the Causal Effects \( \{\beta^t\}_{0 < t \leq 3} \).

In Section 5.2 we find that only \( \hat{\rho}_{1} = \hat{\beta}_{1} \) is nonzero. In our primary specification we maximize the power of our estimate of \( \beta^t = \beta \) by imposing that the other
difference-in-discontinuity estimates are zero:

\[
W_{ist} = w\alpha_0 + X_s w\alpha_1 + X_s \cdot \mathbb{I}(X_s \geq 0)w\alpha_2 + \mathbb{I}(X_s \geq 0)w\alpha_3 \\
+ \mathbb{I}(t = 1) \cdot \left[ \alpha_4 + X_s \alpha_5 + X_s \cdot \mathbb{I}(X_s \geq 0)\alpha_6 + \beta \cdot \mathbb{I}(X_s \geq 0) \right] \\
+ C_{ist}\alpha_4 + \nu_{ist}
\]

(12)

for \( |X_s| < h, \ t = \{-5, -3, \ldots, 3\} \)

In our baseline specification we assume \( w \) is a constant, as the results of Sections 4.2 and 5.1 suggest. For robustness we also allow for a linear trend in the discontinuities, which effectively controls for a time trend in the Selection Effect.

The choice of bandwidth \( h \) is complicated because the panel specifications of Equations 10–12 simultaneously estimate several regression discontinuities. Our approach is to make a reasonable choice of bandwidth and show that the results are similar using other choices. In our baseline specifications we choose a bandwidth of 18, which yields conservative estimates (the estimates of our main result are larger and noisier for smaller choices). We show in Section 5.2 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 Appendix D.2 that other results in the main text are not sensitive to the choice of bandwidth.\(^{17}\) 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.

### 3.3 Data

We draw on data compiled by Klarner (2013) on the number of Democrats, Republicans, and independents elected to 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. Not all states allow their Congressional districts to be drawn by the state legislature. The exceptions are generally independent or appointed commissions. We discard all elections (and thus any state-redistricting event) after a state adopts a

---

\(^{17}\)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.
commission (as so marked by Levitt, 2016).\textsuperscript{18} We also discard states that have only a single House representative, as these states have a single district that consists of the entire state.\textsuperscript{19} 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 D.4 that the main results do not change if we drop Maine from the sample. Finally, we exclude Nebraska (which has a nonpartisan state legislature) from all analysis.

These data on the outcomes of state assembly elections are merged to data on the outcomes of individual races for the U.S. House. We compile a dataset on the vote share and party of each candidate that ran for each district of the U.S. House from 1964 through 2012. 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.\textsuperscript{20}

To measure racial gerrymandering we combine tract-level census data with Congressional district boundaries. The census data come from the National Historical Geographic Information System (Minnesota Population Center, 2011). District boundaries for each U.S. Congress come from Lewis et al. (2013). We assign each tract to whichever district contains its centroid; we do this for the district boundaries both before and after redistricting to get the old and new district of each tract.

We draw on data for incumbency and open seats, which are based on official filings with the Federal Election Commission, from Bonica (2013). These data are only available after 1980, and only available for general elections in even years (as opposed to special elections); thus the results in Section 6.2 use only these elections. In Online Appendix E we give more details and report descriptive statistics for the data.


\textsuperscript{19}Alaska, Delaware, Vermont, and Wyoming are excluded. North Dakota is excluded after the 1972 reapportionment, Montana after the 1991 reapportionment, and South Dakota after the 1981 reapportionment.

\textsuperscript{20}The ICPSR’s dataset includes the vast majority of House races but, like any dataset, is incomplete. However, it also contains several races not contained in other data, such as that of Lee et al. (2004). For that reason we choose the ICPSR data over other options. Nevertheless, these two datasets agree on the vast majority of races. Using the data of Lee et al. (2004) for the years 1972 to 1992 (the years it covers) does not change the main results (see Appendix D).4.
4 Illustrating and Testing the Identification Strategy

4.1 Illustrating the Identification

Though Section 3.1 gave the math behind the identification, the intuition (as well as the main results) can be illustrated in a series of figures like those used to illustrate a standard regression discontinuity design. Each panel of Figure 5 shows estimates of Equation 9 for a subsample of House races restricted to a specific time frame relative to the redistricting event, using as the outcome a dummy for whether the Republican won. In all of these panels the variable on the horizontal axis is the same: the share of seats won by Democrats in the state assembly during the redistricting election. This share is plotted against the probability a Republican wins in the U.S. House during the indicated time frame, which may come before or after the redistricting election.

Panels a and b are constructed using the outcomes of elections that occur before the redistricting election. As these outcomes are predetermined at the time of redistricting, they cannot be affected by it. Our key assumption is that in the absence of precise control all predetermined variables are smooth at the cutoff. The assumption holds as long as the 50 percent cutoff has no inherent meaning beyond determining which party controls the state assembly. Under this assumption, Panels a and b are informative about the Selection Effect.

Though we cannot reject that the outcomes are smooth in Panel a, there is a large discontinuity in Panel b. This discontinuity is the Selection Effect, denoted $\rho_{|t<0}$ in Equation 6.21 It arises because Democrats (Republicans) have ensured that they barely win enough seats to hold a majority in state assemblies where Republicans have won (lost) relatively more seats in U.S. House elections 1 to 5 years before the redistricting election. By exerting precise control over the outcome in the state elections, parties are able to “sort” states with desired characteristics (such as past U.S. House outcomes) onto the winning side of the cutoff. The states that Democrats sort onto their side of the cutoff are on average 11.4 percentage points more likely to have elected a Republican than those that Republicans choose for their side of the cutoff. Finally, the lack of evidence for a discontinuity in Panel a might imply the parties do not decide which states they want to win based on outcomes so far in the past, instead focusing on recent election outcomes that are most predictive of future

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21Assuming $w(t)$ equals 1 for $-3 \leq t$ and 0 for $t < -3$, which the results of Section 5.1 suggests may be true.
outcomes.

**Figure 5**
The Selection Effect and Causal Effect are Visible in the Data

![Figure 5](image_url)

**Note:** Each panel estimates Equation 9 on U.S. House races restricted to the range of years given in the title. The outcome is a dummy for whether the Republican won, which is plotted against the percentage of seats (relative to 50%) won by Democrats in the state assembly election that determines control of redistricting. Each dot shows the average of the outcome within a bin of width 3. Standard errors are clustered by state-redistricting event.

Panels c and d are constructed using elections that occur after the redistricting election, meaning these outcomes may have been affected by redistricting (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.
We make the key identifying assumption that the Selection Effect, or the general favorability of a state towards Republican House candidates, does not suddenly change from before to after redistricting. As we show in the next section this assumption seems to hold in the data. Then the states sorted onto one or the other side of the cutoff should be as favorable to Republicans before as after redistricting, meaning any change in their probability of winning must be caused by redistricting. Then the Causal Effect of redistricting, denoted $\rho_t^\lambda$ in Equation 7, is simply the difference between the discontinuity estimated in Panel b and 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 red states into neutral states (and vice-versa) in the 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, implying the original selection 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.

Figure 5 not only illustrates the research design but previews the results. The parties seek to capture redistricting in states where they have sustained recent losses in the U.S. House. These losses are temporarily reversed by redistricting, but the gains do not persist. That pattern is consistent with the endogenous institutions hypothesis. Each party should prioritize controlling the choice of institutions in states where it is losing influence, then use its control to lock in its influence.

### 4.2 Testing the Identifying Assumption: Does the Selection Effect Change after Redistricting?

To identify the Causal Effect of partisan redistricting, we must assume that everything else that might determine the outcomes of elections before redistricting remains roughly unchanged after redistricting. (This can be relaxed to allow for a smooth trend, though we show in Section 5 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 5. Nevertheless we give evidence in this section that any such change in the Selection Effect does not occur.

One benefit of studying election outcomes is that, by construction, they 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 choosing to win in states where the opposition party is relatively more popular, there should be a Selection Effect not only on which party wins elections but on the vote shares won. 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 election outcomes must be driven by a change in the distribution of votes.

**Figure 6**
The Selection Effect on Vote Shares Does Not Vanish after Redistricting

![Graph](image)

**Note:** This figure is identical to Figure 5 except that the outcome is the Republican vote share rather than a dummy for whether the Republican won.

Figure 6 is constructed analogously to Figure 5, the only difference being the outcome on the vertical axis: the Republican vote share rather than a dummy for
whether the Republican won. Panel b of Figure 6 looks much like Panel b of Figure 5. There is a discontinuity in the Republican vote share, implying Democrats (Republicans) choose to barely win state assembly elections in states where Republican candidates for the U.S. House win a larger (smaller) share of the vote. But Panel c shows that this discontinuity in vote shares remains even after redistricting. That is in stark contrast to Panel c of Figure 5, which shows the discontinuity in the probability of a Republican win vanishes after redistricting. This pattern suggests that states selected because they cast many votes for Republicans do not suddenly stop voting for Republicans.22

Then by the argument above, the only way for the discontinuity shown in Figure 5.b to vanish immediately after redistricting is for the distribution of votes to abruptly become less favorable to 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.

5 Main Results

5.1 Selection Effect: Do Parties Seize Control in States Where they are Losing Seats?

The figures in Section 4 suggest that, as predicted by the endogenous institutions hypothesis, there is a pro-opposition Selection Effect. This section confirms that fact more systematically. We estimate the Selection Effect in each year to trace out its time pattern \( w(t) \). As noted in Section 3, this time pattern must be controlled for to glean a consistent estimate of the Causal Effect of partisan redistricting.

Table 1 shows the results from estimating Equation 10 allowing \( w(t) \) to be a fully flexible set of time dummies, with the last three columns controlling for year and state-redistricting event fixed effects. In the specifications that control for event fixed-effects the dummy for \( t = -9 \) is the excluded category. The estimates in all four columns suggest that there is little or no selection on U.S. House outcomes 7

\[ \text{Panel d in Figure 6 resembles its analog in Figure 5, though it is noisier. Surprisingly, Panel a shows some evidence of a discontinuity even though there was no such evidence in Figure 5. That may suggest the Selection Effect is truly constant. But it is also possible that sampling error makes 6.a appear to have a discontinuity by chance, or to hide a similar discontinuity in 5.a.} \]
**Table 1**
Prediction 1: Estimates of the Selection Effect

<table>
<thead>
<tr>
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<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\times [t=-9]$</td>
<td>-0.044</td>
<td>-0.028</td>
<td>(0.052)</td>
<td>(0.050)</td>
</tr>
<tr>
<td>$\times [t=-7]$</td>
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<td>0.001</td>
<td>0.066</td>
<td>0.026</td>
</tr>
<tr>
<td>$\times [t=-5]$</td>
<td>0.119**</td>
<td>0.126**</td>
<td>0.157***</td>
<td>0.146***</td>
</tr>
<tr>
<td>$\times [t=-3]$</td>
<td>0.113**</td>
<td>0.137**</td>
<td>0.146***</td>
<td>0.147***</td>
</tr>
<tr>
<td>$\times [t=-1]$</td>
<td>0.106**</td>
<td>0.122**</td>
<td>0.142**</td>
<td>0.135**</td>
</tr>
</tbody>
</table>

Test: $[t=-5]=[t=-3]=[t=-1]$ .96 .9 .93 .95
Event FEs X X
Year FEs X
Observations 6820 6820 6820 6820
Events 135 135 135 135

*Note:* We estimate Equation 10 allowing $w(t)$ to be a fully flexible set of time dummies. The terms labeled $[t=-T]$ refer to dummies for whether the election is $T$ years before redistricting. The values reported in the row labeled “Test…” are p-values of the test for equality of the coefficients listed. Standard errors are clustered by state-redistricting event. “Event FEs” are state-redistricting event fixed-effects.

to 9 years before redistricting, confirming Panel a of Figure 5. But 5 years before redistricting the effect becomes positive and significant. The size of the effect remains unchanged for elections 3 years and 1 year before redistricting. The row labeled “Test: …” shows that in no specification can we reject that the Selection Effect remains constant after $t = -5$.

These estimates confirm the pattern visible in Figure 5 while adding nuance. 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 capture 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 parties make pro-opposition selection, meaning they aim to capture redistricting in states where the opposing party has been winning.

Most crucial, however, is that after the estimated Selection Effect becomes positive it remains constant. If the true Selection Effect is indeed constant the derivations of Section 3.1 suggest 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 the results of Section 4.2 sug-
5.2 Causal Effect: Do Parties Use Control of Redistricting to Harm Their Opponents?

We estimate Equation 11, the flexible difference-in-discontinuities, taking the election 5 years before redistricting as the reference year. If the Selection Effect is constant, as suggested by the results of Section 4.2 and Section 5.1, the difference-in-discontinuities estimates should equal zero for the years before redistricting. The endogenous institutions hypothesis predicts they should turn negative, or anti-opposition, after redistricting—that is, we would expect Republicans are less likely to win when Democrats gain a veto over redistricting.

Figure 7 shows exactly that. We plot the estimates \( \{ \hat{\rho}^t \} \) with their 90 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 election after redistricting...
the estimate turns sharply negative. Switching the state assembly from Republican
to Democratic control lowers a Republican’s chance of winning by 11 percentage
points. This damage to the opposition is the Causal Effect of partisan redistricting.
Yet the estimate for \( t = 3 \) is again zero, suggesting the anti-opposition power of re-
districting is short-lived. As shown in Section 4.2, this pattern is unlikely to be ex-
plained by mean-reversion, suggesting the effect is genuinely transient. As we argue
in Section 7.2 and Online Appendix B, such transience is not entirely surprising.

We maximize the power of our estimates by imposing the restriction implied by
Figure 7: that the Causal Effect appears only in the year immediately after redis-
istricting. We estimate Equation 12 using many different controls, run a battery of
specification tests, and verify that the results are not sensitive to the bandwidth of
the local linear regression. The results are reported in Table 2.

Panel A shows how our results change as we add controls. Columns 1 through 4
show that controlling for state-redistricting event fixed-effects and year fixed-effects
makes little difference to the estimates. In Columns 5 and 6 we also allow for a lin-
ear trend in the Selection Effect. If our assumption of a constant Selection Effect is
invalid, this trend might absorb some of the bias. The estimates in Columns 5 and 6
are reassuringly unmoved, suggesting our assumption is valid.

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 sensi-
tive 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). 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 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 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.

\[\text{These are Alabama, Alaska, Arizona, Georgia, Louisiana, Mississippi, South Carolina, Texas, and Virginia.}\]
Table 2
Main Results: Difference-in-Discontinuity Estimates
Causal Effect of Dem. Control on First Election After Redistricting (Prediction 2)

Panel A: Main Results

<table>
<thead>
<tr>
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<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
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<tr>
<td></td>
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<td>(0.035)</td>
<td>(0.037)</td>
<td>(0.043)</td>
<td>(0.036)</td>
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<td>Event FEs</td>
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<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
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</tr>
<tr>
<td>Year FEs</td>
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<td>X</td>
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<td>Trends</td>
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Panel B: Specification Tests

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<th></th>
<th>(1) Baseline</th>
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<th>(4) Republican Margin</th>
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<td>Dif-in-Disc Estimate</td>
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<td>-0.097**</td>
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<td>120</td>
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<td>133</td>
<td>135</td>
<td>128</td>
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</table>

Panel C: Robustness to Bandwidth

<table>
<thead>
<tr>
<th></th>
<th>(1) h=22</th>
<th>(2) h=18</th>
<th>(3) h=14</th>
<th>(4) h=10</th>
<th>(5) h=6</th>
</tr>
</thead>
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<tr>
<td>Dif-in-Disc Estimate</td>
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<td>-0.108***</td>
<td>-0.098**</td>
<td>-0.128**</td>
<td>-0.181**</td>
</tr>
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<td>5513</td>
<td>3766</td>
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<tr>
<td>Events</td>
<td>149</td>
<td>135</td>
<td>111</td>
<td>82</td>
<td>48</td>
</tr>
</tbody>
</table>

Note: Each column shows a different estimate of \( \hat{\beta} \) from Equation 12. 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 12 using several different choices of bandwidth (\( h = 18 \) is the bandwidth used in the baseline specifications). Standard errors are clustered by state-redistricting event.
Finally, Panel C confirms that our estimates are not driven by the choice of bandwidth. Column 2 reiterates 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.

To summarize, the Causal Effect of redistricting is anti-opposition, as predicted by the hypothesis. Democratic control of the assembly during redistricting lowers the probability a Republican wins a contest for the U.S. House by roughly 11 percentage points in the election one year after redistricting. This steep anti-opposition effect does not last long. It has vanished by the next regular election.

6 Mechanism: Is It Really Caused by Partisan Redistricting?

6.1 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. Our difference-in-discontinuities approach allows a more direct measure. Let $V_{st}^R$ be the share of votes won and $W_{st}$ the fraction of seats won in election $t$ during state-redistricting 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 12 to the statewide Republican vote share, the fraction of seats won, and the conversion rate. The left-hand panel of Figure 8 shows that there is no statistically significant effect on the Republican vote share. This estimate is not surprising given that Figure 6 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.\textsuperscript{24} By contrast, the center panel of Figure 8 shows that there is

\textsuperscript{24}Though Figure 6 takes individual House races rather than the total statewide vote share as the unit of analysis, in Online Appendix D.1 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
a large and statistically significant decrease in the share of seats won by Republicans when Democrats control the assembly during redistricting. This result is simply the state-level analog of Figure 7.

The right-hand panel shows the effect on the conversion rate. It is unchanged until after redistricting, when it turns sharply anti-opposition 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. 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.

Footnote: 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.
6.2 The Ruling Party is Far More Likely to Win Open Seats Immediately After Redistricting

Open seats—those in which there is no incumbent—arise either because an incumbent retires or because a state is apportioned more representatives, which requires creating entirely new districts. There are several reasons to expect the effects of partisan redistricting will be most pronounced in open seats. The first is that voters are more likely to vote for an incumbent regardless of their party or ideology. Lee (2008) finds that this incumbency advantage may be on the order of 40 percentage points. The party in control of redistricting may find it relatively difficult to unseat an opposition incumbent through gerrymandering. It would likewise feel no need to prop up its own incumbents through gerrymandering. By this logic there is less to gain from gerrymandering a district that is not open.

The incumbency advantage also may make demographics and party registration less informative about how someone will vote. Ansolabehere et al. (2000) argues that a large part of the incumbency advantage is the “personal vote,” which is based on the relationship an incumbent builds with her constituents. A registered Republican may nevertheless vote for a Democrat if she has represented him for many years and if her office has helped him get access to government services. Removing such voters from the district would not necessarily make the incumbent more secure.

Finally, the assembly members charged with redrawing Congressional districts may have purely selfish reasons for spending more effort on open districts. A stint in the state legislature is often a stepping stone to a seat in the U.S. House. Among members of the state assembly who won office in 2002, roughly 15 percent sought higher office over the next 10 years, of whom over 10 percent ran for the U.S. House. It is much easier for these ambitious assembly members to win a House race in an open district than to challenge an incumbent, especially if the open district has been engineered with a favorable partisan bias. The Economist (2002) reports one such example where the chairman of North Carolina’s 2001 redistricting commission stood for office in a Congressional district he himself created.

We estimate Equation 9 for the subsample of open U.S. House districts. We run these estimates for the year before redistricting (to measure the Selection Effect) and then for the year immediately afterwards. The regression discontinuity is graphed in the top-left and top-right panels of Figure 9, which were created analogously to

---

26In Online Appendix D.1 we show that there is also little evidence of selection in earlier years.
Figure 5. The top-left panel shows that there is no evidence of selection on the outcomes of open seats. The probability a Republican wins in an open seat is smooth around the cutoff. But by the election after redistricting, shown in the top-right panel, a massive discontinuity has opened. In states where Democrats barely miss taking control the Republican candidate for an open seat wins with probability 80 percent. When Democrats barely win control the probability drops to 30 percent. If there is no selection on the outcomes of open districts, as implied by the top-left figure, then the Causal Effect of partisan redistricting is an anti-opposition effect of 50 percentage points. It seems unlikely such a stark difference in outcomes would be an accident.

We can also test whether the allocation of votes across districts is consistent with efficient gerrymandering. McGhee (2014) proposes measuring vote efficiency by counting the number of wasted votes: votes cast for a candidate that loses, or votes cast in excess of what is needed to elect a candidate. Let $V^R$ be the share of votes cast for the Republican, and let $V^{[2]}$ be the share cast for the runner-up (regardless of party). Define Republican vote efficiency as

$$[Vote\;Efficiency]_{ist} = 1 - \begin{cases} V^R_{ist} & \text{if } W_{ist} = 0 \\ V^R_{ist} - V^{[2]}_{ist} & \text{if } W_{ist} = 1 \end{cases}$$

which is simply the unwasted Republican votes as a fraction of all votes cast. The bottom-left panel of Figure 9 shows that, as in the case of the election outcome, there is no evidence of selection on Republican vote efficiency in open districts. But the bottom-right panel confirms that there is strong evidence that Republican votes are allocated less efficiently in states where Democrats control the assembly during redistricting. The estimates must be interpreted with some caution, as open seats may arise because of redistricting (say, because an incumbent steps down after being gerrymandered into an impossible district). But either way, it supports the claim that redistricting is the mechanism.

---

27This may be because political parties find it more alarming, and thus more salient, when their incumbents lose office. Aside from alarming the party establishment (which is run by the very officials whose seats are at stake), the defeat of incumbents is also a stronger signal that the party’s position is eroding. If a Republican loses a district that she won just two years ago, it suggests the state’s electorate may be turning against Republicans.
Figure 9
Open Seats Have the Most Clearly Visible Effect

Note: The figure shows estimates of Equation 9. We restrict the sample to races in open districts (those in which there is no incumbent). The left-hand panels estimate the equation after restricting the sample to the election just before redistricting (the election ending in 0). The right-hand panels restrict to elections just after (the election ending in 2). The outcome in the top panels is a dummy for whether the Republican won, while that of the bottom panels is the measure of vote efficiency defined in Equation 13. Each dot shows the average of the outcome within a bin of width 4. Standard errors are clustered by state-redistricting event.

6.3 African Americans are Less Efficiently Distributed Across Districts under Republicans versus Democrats

The clearest sign of partisan redistricting is a systematic difference in the types of voters who form majorities in districts drawn by the two parties. Of the data sources that might guide their gerrymandering, the decennial census is the most comprehensive. The census reports population counts of each racial and ethnic group within each census tract. If race is informative about how someone will vote, the party in power might move voters of the less friendly race—those very likely to vote for the other party—to minimize their influence.
African Americans are the demographic group whose party preference is most easily identified. In the 2014 election, 89 percent of African Americans voted for a Democrat running for Congress—support comparable to that of registered Democrats (92 percent).\(^{28}\) Since an African American is likely to support Democrats, Republicans may try to redistrict African Americans to minimize their influence.\(^{29}\)

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.

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.\(^{30}\)

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.\(^{31}\) 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 Republicans lose control of

\(^{28}\)According to CNN (2016), whose data are based on National Election Pool exit polls.

\(^{29}\)The ideal test would be to look at actual registered Democrats and registered Republicans. But we do not have historical data on the number of registered Republicans and Democrats by precinct or census tract.

\(^{30}\)Visual representations of all tests described in this section can be found in Appendix D.1.

\(^{31}\)In a recent (as of this writing) Supreme Court case, the justices struck down two districts drawn in North Carolina. The New York Times (May 22, 2017) writes that “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).
Table 3
Evidence of Racial Gerrymandering

<table>
<thead>
<tr>
<th>Prob. of Being Moved</th>
<th>Conditional on Moving</th>
<th>Pre-Redistricting Char.</th>
</tr>
</thead>
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<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td></td>
<td>Black Tracts</td>
<td>Other Tracts</td>
</tr>
<tr>
<td>Dem. Control</td>
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<td>-0.027</td>
</tr>
<tr>
<td></td>
<td>(0.063)</td>
<td>(0.050)</td>
</tr>
<tr>
<td>Observations</td>
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<td>185111</td>
</tr>
<tr>
<td>Events</td>
<td>117</td>
<td>138</td>
</tr>
</tbody>
</table>

Note: Columns 1 and 2 estimate Equation 9 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 Equation 9 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 estimates Equation 9 on districts before redistricting, where the outcome is the black population share of the district. Column 5 estimates Equation 9 on majority black tracts. Each tract is assigned the absolute percentage difference between the population of the district it is located in and the median district. All standard errors are clustered by state-redistricting event. Visual representations of these specifications are in Appendix D.1.

32 The regression in Column 3 of Table 3 and those used to construct Figure 10.B narrow the bandwidth to 10 because there is essentially no racial “packing” further away from the discontinuity.

33 For this figure we restrict the sample to include only states in which no independents are elected.

redistricting.

Figure 10 gives a more complete description of how Democrats versus Republicans redistrict African Americans by studying the probability density of the districts into which they are moved. Figure 10.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 10.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 Equation 9 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 10.A we adjust the estimates to show the change when Republicans take control from Democrats. As predicted, there is an increase in the
Figure 10
Effect of Packing and Cracking on Distribution of Political Opponents

A. Predicted Effect of Packing and Cracking Political Opponents

1. Move opponents out of districts where they form slight majorities…

2. …pack them into segregated districts…

3. …to leave behind more districts in which they form minorities.

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

Density

Proportion of Political Opponents

Note: See text for description.

density 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.

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 Section 3.1 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 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 to the assembly. This restriction makes Republican control exactly the converse of Democratic control. The results without making this restriction look similar.
of analysis we estimate Equation 9 to 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.

7 Discussion and Conclusion

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

In Online Appendix B 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.

If true, this conjecture cuts to the theoretical core of the endogenous institutions
hypothesis. Aghion et al. (2004) argue that inefficient institutions arise when the rule-setters can predict whether they will subsequently be in the minority. If forced to set rules from behind a “veil of ignorance,” they would choose institutions that maximize welfare. Indeed, we provide some evidence in Online Appendix B that, as new technology has made predictions more accurate and the veil has been lifted, gerrymandering has become more persistent. If that is true it suggests leaving control of electoral rules in the hands of politicians will only become more corrosive to democracy.

7.2 Summary

We test whether partisan redistricting in the U.S. is consistent with the theory of endogenous institutions. We propose and apply a method that lets us test the two key predictions of the hypothesis: that political parties should seek to control Congressional redistricting in states where their influence is declining, and that they use control of redistricting to at least temporarily reverse the decline. Our results support both predictions, and suggest 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.

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A  Theoretical Appendix (For Online Publication)

A.1  Derivation of Conditional Expectation

This appendix derives Equation 4. Let $D_+$ be the event that Democrats had the opportunity to exert control and took it, and $D_-$ the event that they had control but chose not to take it. Define $R_+$ and $R_-$ analogously for the Republicans. By the law of iterated expectations,
First we derive the conditional expectations on the right-hand side of this equation. These are most straightforward in the cases where there has been precise control. Assuming $X_s$ is within a range consistent with precise control (as we discuss below),

$$
\mathbb{E}[\theta_s \mid X_s] = \mathbb{E}[\theta_s \mid X_s, D_+] \mathbb{P}(D_+ \mid X_s) + \mathbb{E}[\theta_s \mid X_s, D_-] \mathbb{P}(D_- \mid X_s)
$$

$$
+ \mathbb{E}[\theta_s \mid X_s, R_+] \mathbb{P}(R_+ \mid X_s) + \mathbb{E}[\theta_s \mid X_s, R_-] \mathbb{P}(R_- \mid X_s)
\tag{14}
$$

To derive the conditional expectation in the cases where there has been no precise control we first solve for the conditional density of the partisan drift of an election that has not been controlled. If there were no precise control the conditional density of $\theta_s$ is

$$
H^\theta(\theta \mid X_s) = \mathbb{P}(\theta_s < \theta)
$$

$$
= \mathbb{P}(x^{-1}(X_s - v_s) < \theta)
$$

$$
= \mathbb{P}(X_s - x(\theta) > v_s)
$$

$$
= 1 - F\left[X_s - x(\theta)\right]
\Rightarrow h^\theta(\theta \mid X_s) = f\left[X_s - x(\theta)\right] x'(\theta)
\tag{15}
$$

Suppose Democrats had the opportunity to control the outcome but chose not to take it. Then it must be that $\theta_s \notin \Theta_D$, implying the density of $\theta$ after conditioning on $D_-$ is

$$
h^\theta(\theta \mid X_s, D_-) = \begin{cases} 
0 & \text{if } \theta \in \Theta_D \\
\frac{h^\theta(\theta \mid X_s)}{1-\mu} & \text{otherwise}
\end{cases}
\tag{16}
$$

where the $1 - \mu$ in the denominator follows because conditional on Democrats being able to exert control, $\mathbb{P}(\theta \notin \Theta_D) = 1 - \mu$. Then the conditional expectation can be
written as

\[ E[\theta_s \mid X_s, D_-] = \bar{\theta}^D(X_s) = \int_{\theta \notin \Theta_D} \theta \frac{h^\theta(\theta \mid X_s)}{1 - \mu} d\theta \]

with a similar expression for \( E[\theta_s \mid X_s, R_-] = \bar{\theta}^R(X_s) \).

It remains only to compute the conditional probabilities in (14). Let \( E_0 \in \{D_+, D_-, R_+, R_\} \) be one of the events. As these events partition the event space,

\[ P(E_0 \mid X_s) = \frac{g(X_s \mid E_0)P(E_0)}{\sum_{E \in \{D_+, D_-, R_+, R_\}} g(X_s \mid E)P(E)} \quad (17) \]

Again, it is most straightforward to solve for the terms conditioned on precise control:

\[ g(X_s \mid D_+) = \chi(X_s) \]
\[ g(X_s \mid R_+) = \chi(-X_s) \]

In the absence of precise control—say, in the case where Democrats had the opportunity but chose not to take it—we have

\[ g(X_s \mid D_-) = \tilde{g}^D(X_s) = \int g(X_s \mid \theta, D_-)h^\theta(\theta \mid D_-)d\theta = \int f(X_s - x(\theta_s)) \frac{h^\theta(\theta_s)}{1 - \mu} d\theta \]

The expression for \( g(X_s \mid R_-) = \tilde{g}^R(X_s) \) is defined analogously.

Define

\[ \tilde{\chi}(X_s) = \begin{cases} \chi(X_s) & \text{if } X_s \geq 0 \\ \chi(-X_s) & \text{if } X_s < 0 \end{cases} \]

and

\[ b(X_s) = \frac{\mu \tilde{\chi}(X_s)}{\mu \tilde{\chi}(X_s) + (1 - \mu) [\tilde{g}^D(X_s) + \tilde{g}^R(X_s)]} \]

Since \( \chi \) is right-continuous at 0, \( \tilde{\chi} \) is continuous at 0. The conditional densities \( \tilde{g}^D, \tilde{g}^R \)
and the conditional expectations $\tilde{\theta}^D, \tilde{\theta}^R$ are also continuous. Therefore $b$ is continuous. Finally, the expression vanishes when $\mu = 0$.

Now define

$$a(X_s) = \tilde{\theta}^D(X_s) \frac{(1 - \mu)\tilde{g}^D(X_s)}{\mu \tilde{\chi}(X_s) + (1 - \mu)\tilde{g}^D(X_s) + \tilde{g}^R(X_s)}$$

$$+ \tilde{\theta}^R(X_s) \frac{(1 - \mu)\tilde{g}^R(X_s)}{\mu \tilde{\chi}(X_s) + (1 - \mu)\tilde{g}^D(X_s) + \tilde{g}^R(X_s)}$$

$$= \mathbb{E}[\theta_s | X_s, D_-] \mathbb{P}(D_- | X_s) + \mathbb{E}[\theta_s | X_s, R_-] \mathbb{P}(R_- | X_s)$$

which is a continuous function. Then Equation 4 follows because

$$\mathbb{P}(D_+ | X_s) = \begin{cases} b(X_s) & \text{if } X_s \geq 0 \\ 0 & \text{if } X_s < 0 \end{cases}$$

$$\mathbb{P}(R_+ | X_s) = \begin{cases} 0 & \text{if } X_s \geq 0 \\ b(X_s) & \text{if } X_s < 0 \end{cases}$$

□

B 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.

B.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.\textsuperscript{34} 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 C 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 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.

### B.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 cy-

\textsuperscript{34}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.
Figure 11
The Effect of Gerrymandering Grows Larger and More Persistent

Note: Each panel is constructed analogously to Figure 7, 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.

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 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 11 is constructed like Figure 7. 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 11 suggests it would be hasty to conclude the effects of gerrymandering will forever remain short-lived.

C 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 $\{V_{ist}^R\}$ for all $i$. If the Republicans gerrymander they may, subject to some constraints, reallocate votes across districts to create a new set of shares $\{\tilde{V}_{ist}^R\}$. We assume the gerrymandered districts are constructed as follows:

1. Republicans choose a threshold $\tilde{V}$. 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 $\{0.9V_{ist}^R\}$. 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

\[\{V_{i(I)}^R, V_{i(I-1)}^R, \ldots, V_{i(1)}^R\}\]

\(^{35}\)To be precise, 10 percent of the voters are replaced by an equal number of Democrats from a different district.
where \(i(k)\) is the k-th order statistic over \(i\).

4. For each \(k\), if \(V^R_{i(k),st} < \bar{V}\) and \(\bar{V} - V^R_{i(k),st} < B\) they set \(\bar{V}^R_{i(k),st} = \bar{V}\). Otherwise they set \(\bar{V}^R_{i(k),st} = 0.9V^R_{i(k),st} + B\). 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 Section 5.2. We vary the threshold \(\bar{V}\) to show how different gerrymandering strategies play out.

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

\[
\kappa_{i,s,t+1} = \theta_{t+1} + \bar{\kappa}_{s,t+1} + \tilde{\kappa}_{i,s,t+1}
\]

where \(\theta_{t+1}\) is a national shock, \(\bar{\kappa}_{s,t+1}\) a statewide shock, and \(\tilde{\kappa}_{i,s,t+1}\) 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 \(\{V^R_{i,s,t}\}_{i}\) from a Beta(2,2) distribution rescaled by 0.9 to mimic the pro-opposition Selection Effect. For each threshold \(\bar{V} = 0.5, 0.51, \ldots, 0.8\) and each national shock \(\theta = -0.05, 0, 0.05\) we run 200 simulations.

Figure 12 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 \(\tilde{\kappa}_{s,t+1}\), 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
Figure 12
Simulation Example: A Negative Shock to Support for Republicans Nullifies the Gains from Gerrymandering

<table>
<thead>
<tr>
<th></th>
<th>Seats won</th>
</tr>
</thead>
<tbody>
<tr>
<td>Natural:</td>
<td>32.0%</td>
</tr>
<tr>
<td>Gerrymandered:</td>
<td>48.0%</td>
</tr>
</tbody>
</table>

First Election

Next Election

<table>
<thead>
<tr>
<th></th>
<th>Seats won</th>
</tr>
</thead>
<tbody>
<tr>
<td>Natural:</td>
<td>24.0%</td>
</tr>
<tr>
<td>Gerrymandered:</td>
<td>28.0%</td>
</tr>
</tbody>
</table>

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.

The seats already held safer.

Figure 13 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 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 7 for the election 3 years after redistricting.
Figure 13
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).

D Empirical Appendix (For Online Publication)

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

D.1 Additional Tables and Figures Referenced in the Text

D.1.1 Selection Effect of Republican Vote Share (Section 6.1)

Figure 14 is analogous to Figure 6 in Section 4.2. Unlike Figure 6, which uses an individual U.S. House race as the unit of observation, Figure 14 uses the share of votes won by Republicans throughout the entire state.

D.1.2 Selection Effect in Open Seats (Section 6.2)

Figures 15 and 16 estimate and plot Equation 9 on election outcomes for open seats in the years $t = -7, -5, -3, -1$ (in the main text we show only $t = -1$). The figures show that although the estimates are noisy (there are not many open seats), there is little evidence of a trend or a persistent discontinuity. We take this as suggestive evidence that there is no systematic Selection Effect on open seats.
Figure 14
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.

D.1.3 Racial Gerrymandering (Section 6.3)

Figure 17 gives the visual representation of Columns 1 and 2 of Table 3. Figure 18 represents Column 3, and Figure 19 represents Columns 4 and 5. See the text of Section 6.3 for details on the outcomes and the specifications.
Figure 15
Selection Effect on Open Seats

7 Years before Redistricting

Note: See text for description. Unit of observation is a U.S. House race for an open district. Each dot shows the average of the outcome within a bin of width 4. Standard errors are clustered by state-redistricting event.
\textbf{Figure 16}  
Selection Effect on Open Seats (cont.)

\textit{3 Years before Redistricting}

\begin{align*}
\text{Open Seat: Republican Wins} \\
\text{Discontinuity: } & -0.367 \\
& (0.252) \\
\text{Seats Won by Democrats in State Assembly during Redistricting Election (% of total, 0=50\%)} \\
\text{Republican Control} & \quad \text{Democratic Control}
\end{align*}

\begin{align*}
\text{Open Seat: Republican Vote Efficiency} \\
\text{Discontinuity: } & -0.081 \\
& (0.103) \\
\text{Seats Won by Democrats in State Assembly during Redistricting Election (% of total, 0=50\%)} \\
\text{Republican Control} & \quad \text{Democratic Control}
\end{align*}

\textit{1 Year before Redistricting}

\begin{align*}
\text{Open Seat: Republican Wins} \\
\text{Discontinuity: } & 0.019 \\
& (0.191) \\
\text{Seats Won by Democrats in State Assembly during Redistricting Election (% of total, 0=50\%)} \\
\text{Republican Control} & \quad \text{Democratic Control}
\end{align*}

\begin{align*}
\text{Open Seat: Republican Vote Efficiency} \\
\text{Discontinuity: } & -0.004 \\
& (0.064) \\
\text{Seats Won by Democrats in State Assembly during Redistricting Election (% of total, 0=50\%)} \\
\text{Republican Control} & \quad \text{Democratic Control}
\end{align*}

\textit{Note:} See text for description. Unit of observation is a U.S. House race for an open district. Each dot shows the average of the outcome within a bin of width 4. Standard errors are clustered by state-redistricting event.
**Figure 17**

African Americans are More Likely

- **Majority Black Census Tracts**
  - Discontinuity: -0.150 (0.063)
  - Seats Won by Democrats in State Assembly during Redistricting Election (% of total, 0=50%)

- **Other Census Tracts**
  - Discontinuity: -0.027 (0.050)
  - Seats Won by Democrats in State Assembly during Redistricting Election (% of total, 0=50%)

*Note:* See the text describing Table 3 in Section 6.3 for details. Each dot gives the conditional mean within a bin of width 3.

---

**Figure 18**

Conditional on Being Moved, Where Are African Americans Moved?

- **Moved to Overwhelmingly Black District?**
  - Discontinuity: -0.230 (0.054)
  - Seats Won by Democrats in State Assembly during Redistricting Election (% of total, 0=50%)

*Note:* See the text describing Table 3 in Section 6.3 for details. Each dot gives the conditional mean within a bin of width 1.
Figure 19
No Evidence of a Greater Need to Redistrict
African Americans on One Side of the Threshold

Note: See the text describing Table 3 in Section 6.3 for details. Each dot gives the conditional mean within a bin of width 3.
D.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 20 shows the robustness of the results from Section 4 and Figure 21 shows the robustness of the estimates of the Selection Effect from Specification 6 of Table 1. Figures 22—26 show the robustness of the results from Section 6. In all cases we plot the size of the estimate and the 90 percent confidence interval against the bandwidth used to make the estimate.

**Figure 20**
Robustness to Bandwidth: Figure 5
Figure 21
Robustness to Bandwidth: Table 1, Specification 4

Selection Effect at t=-7

Selection Effect at t=-5

Selection Effect at t=-3

Selection Effect at t=-1
**Figure 22**
Robustness to Bandwidth: Figure 9

**Open Seat: Republican Wins**

Selection: Election Before Redistricting

Causal Effect: First Election After Redistricting

**Open Seat: Republican Vote Efficiency**

Selection: Election Before Redistricting

Causal Effect: First Election After Redistricting
Figure 23
Robustness to Bandwidth: Table 3, Columns 1 and 2

Figure 24
Robustness to Bandwidth: Table 3, Column 3
Figure 25
Robustness to Bandwidth: Figure 10.B

Density of Black Population Share \([Bw=10]\)

Density of Black Population Share \([Bw=8]\)

Density of Black Population Share \([Bw=6]\)
D.3 Verifying the Results with an Alternative Dataset of U.S. House Races

As noted in Section 3.3, 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 27 shows that the difference-in-discontinuities estimates are unchanged when we use the alternative dataset.
D.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 28 confirms that dropping Maine has little effect on the results.

E Data Appendix (For Online Publication)

E.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 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.

### E.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 has 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 pre-redistricting dis-
Table 4  
Descriptive Statistics, Election Outcomes

<table>
<thead>
<tr>
<th></th>
<th>$X \geq 0$</th>
<th>$X &lt; 0$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All Seats</td>
<td>Open Seats</td>
</tr>
<tr>
<td>Republican Wins?</td>
<td>0.41</td>
<td>0.51</td>
</tr>
<tr>
<td>Republican Vote Share</td>
<td>0.44</td>
<td>0.48</td>
</tr>
<tr>
<td></td>
<td>(0.18)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Observations</td>
<td>5457</td>
<td>409</td>
</tr>
<tr>
<td>State-Redistricting Events</td>
<td>126</td>
<td>101</td>
</tr>
</tbody>
</table>

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.

trict 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

<table>
<thead>
<tr>
<th></th>
<th>Majority Black</th>
<th>Others</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fraction of Census Tracts Redistricted</td>
<td>0.24</td>
<td>0.29</td>
</tr>
<tr>
<td>Average Absolute Population Deviation</td>
<td>0.11</td>
<td>0.10</td>
</tr>
<tr>
<td></td>
<td>(0.13)</td>
<td>(0.15)</td>
</tr>
<tr>
<td>Tract-Years</td>
<td>25279</td>
<td>224254</td>
</tr>
<tr>
<td>State-Redistricting Events</td>
<td>169</td>
<td>201</td>
</tr>
</tbody>
</table>

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.