

Jeong, Dahyeon; Shenoy, Ajay

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

Dahyeon Jeong*

Ajay Shenoy[†]

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Abstract

We measure where and to what end parties take control of Congressional redistricting, which lets them redraw districts to favor their own candidates. We exploit the discontinuous change in a party's control of redistricting triggered when its share of seats in the state legislature exceeds 50 percent. Parties capture redistricting in states where they have suffered recent losses, which are temporarily reversed by redistricting. Opposition candidates are 11 percentage points less likely to win House elections just after redistricting. Consistent with recent Supreme Court rulings, African Americans are more likely to be segregated into overwhelmingly black districts under Republican redistricting. (JEL Codes: D72,D78)

*University of California, Santa Cruz; email at dajeong@ucsc.edu

[†]University of California, Santa Cruz; Corresponding author: email at azshenoy@ucsc.edu. Phone: (831) 359-3389. Website: <http://people.ucsc.edu/~azshenoy>. Postal Address: Rm. E2455, University of California, M/S Economics Department, 1156 High Street, Santa Cruz CA, 95064. We are grateful to Gianluca Casapietra, Afroviti Demolli, Benjamin Ewing, Samantha Hamilton, Nicole Kinney, Lindsey Newman, Erica Pohler-Chapman, Abir Rashid, Kevin Troxell, Auralee Walmer, and Christina Wong for excellent research assistance on this paper. We thank George Bulman, Carlos Dobkin, Justin Marion, Jon Robinson, Alan Spearot, Jeremy West, and seminar participants at U.C. Santa Cruz and for helpful suggestions.

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 more

conservative, having delivered wins to Republicans in recent House races. 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.

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 to see if 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.

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 there is no Causal Effect on the statewide share of the vote won by Republicans, suggesting there is no random or mean-reverting shift in political preferences that coincides with redistricting. Nevertheless there is a change in the share of seats won, implying that redistricting changes the rate at which 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

We then consider why the effects of partisan redistricting are so short-lived. Maximizing a party's seat total also requires reallocating its voters to narrow its winning margins in at least a few districts. Shifts in the sentiments and composition of the electorate can erode those margins, erasing the benefits of partisan redistricting. But this logic implies that any improvement in the ability to predict how a district will vote might make the effects larger and more persistent. Such predictions would be easier with better technology for constructing districts or a reduction in the variability of voting behavior, both of which occurred in time for the 2000 and 2010 redistricting cycles. We find some suggestive evidence of larger effects and greater persistence in these more recent redistricting cycles. This evidence, though hardly definitive, is at least consistent with our explanation.

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 novel interpretation but more credible estimates of the effects of redistricting. To

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.

our knowledge no prior work has used a regression discontinuity or difference-in-discontinuities design to estimate the effect of partisan redistricting.

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 determines the threshold of support necessary 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 subsequently remain in 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 during redistricting effectively determines the threshold of support needed for each electoral outcome.

Our approach is to imagine adding a “Stage 0” in which parties choose (at some cost) the U.S. states in which they want to manipulate the rules. As per the logic above, they would prefer to expend resources to manipulate the rules in states where they expect to become the minority.² These are the states where they have been losing support, as reflected in the outcomes of recent 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.³

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 in-

²Why would a party not expend equal effort to keep its opponents from controlling states where it is expected to become the majority? One possibility, as implied in the model of Section 3.1, is that states are partitioned into those where Democrats can exert precise control, and those where Republicans can. This would be the case if, as implied in Section 2.3, only the party that holds a majority before the redistricting election is able to exert precise control. Then each party is choosing to control states where it is expected to become the minority, and “gambling” that it may or may not remain the majority in other states. Another possibility is that parties are loss averse, as would be the case if embattled incumbents pressure party leadership to put disproportionate effort into protecting them.

³There is a related but distinct literature on incumbent gerrymandering. Abramowitz et al. (2006), Friedman and Holden (2009), and Carson et al. (2014) study whether politicians redraw districts not to favor one party but to favor incumbents of all parties.

stitutions. 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.⁴

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.⁵ To understand why, consider a state that contains 6 likely Democratic voters and 3 likely Republican voters. A redistricting plan must

⁴There 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.

⁵The 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.”

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 after forced to by the Supreme 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.⁶ Starting in 1972, the ratio jumps in the elections just after the census. By and large it jumps upward, suggesting districts have become increasingly contorted.⁷

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.⁸ This bill, if signed by the governor, becomes law. The next election to the U.S. House is contested under the redrawn district map.⁹

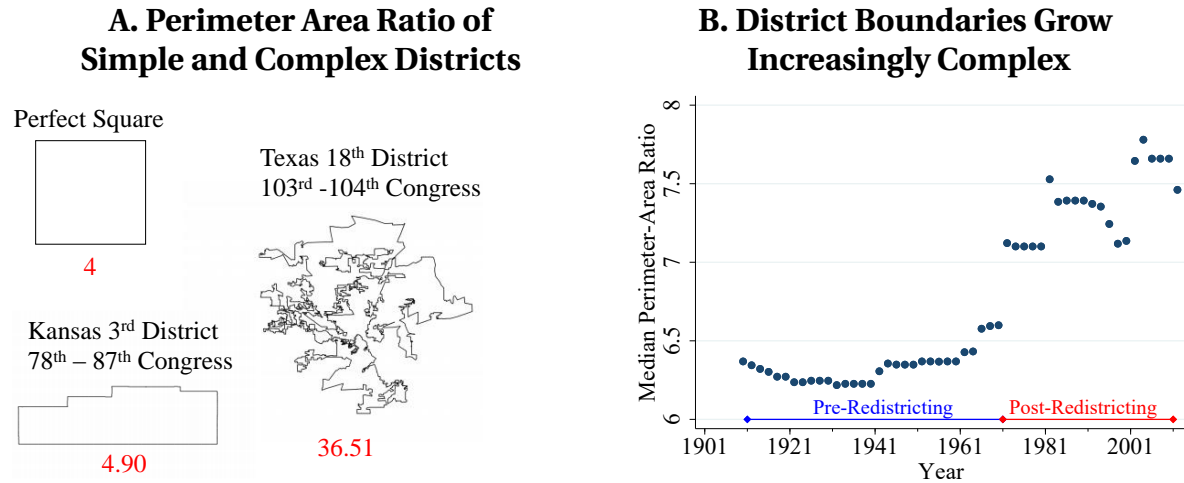
⁶States may redistrict in the year after the census, but may not always succeed in passing a bill. There are some cases (e.g. Texas in 2003) when states have chosen to redistrict again later in the cycle. We do not exploit this variation, as the decision to redistrict may itself be endogenous.

⁷The 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.

⁸Nebraska, which has a unicameral nonpartisan legislature, is excluded from this study.

⁹For 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, relating to redistricting." This bill was amended in the California Senate on 18 June and returned to the Assembly for reconciliation. Had the bill been successful it would have passed to Governor Gray

Figure 1
Simple and Complex Districts



Note: Perimeter-area ratio is defined as $[Perimeter] / \sqrt{Area}$.

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.¹⁰ 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.

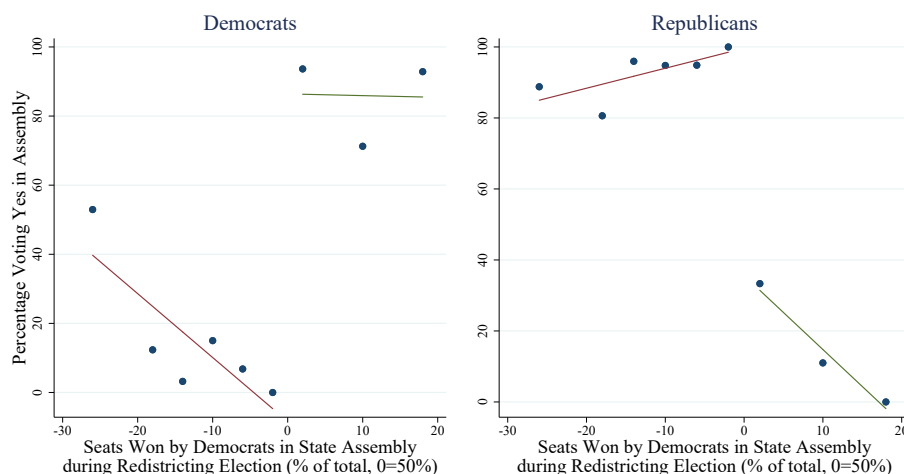
Figure 2 suggests that this assumption is valid. Using data from several states, we plot the fraction of Democrats and Republicans voting yes on the 2011 redistricting bill against the percentage of seats won by Democrats in the previous election.¹¹

Davis to sign into law. In this case the bill ultimately died in committee.

¹⁰We 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.

¹¹The 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

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.

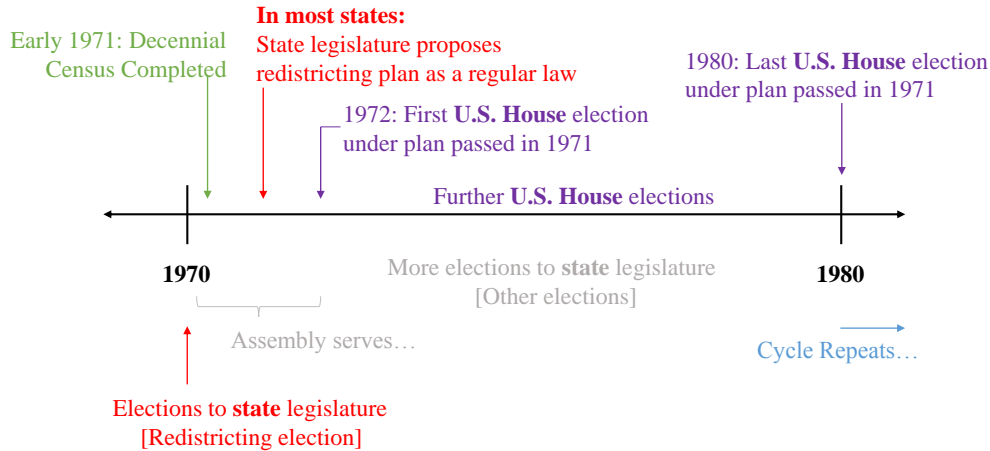
When Democrats gain control of the assembly they switch from near universal opposition to near universal support for the redistricting bill. Republicans are slightly less unified but no less sharp in their response. This reversal of support suggests that control of the assembly triggers a sharp change 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 1.¹² Whichever party wins the election to the state le-

redistricting bill within each bin.

¹²The 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 and endogenous to our outcome of interest. Instead we focus on the opportunity to redistrict, which comes with the completion of the census.

Figure 3
Schedule of Redistricting



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

gislature just before this year has the opportunity to pass its own redistricting plan.¹³ 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?

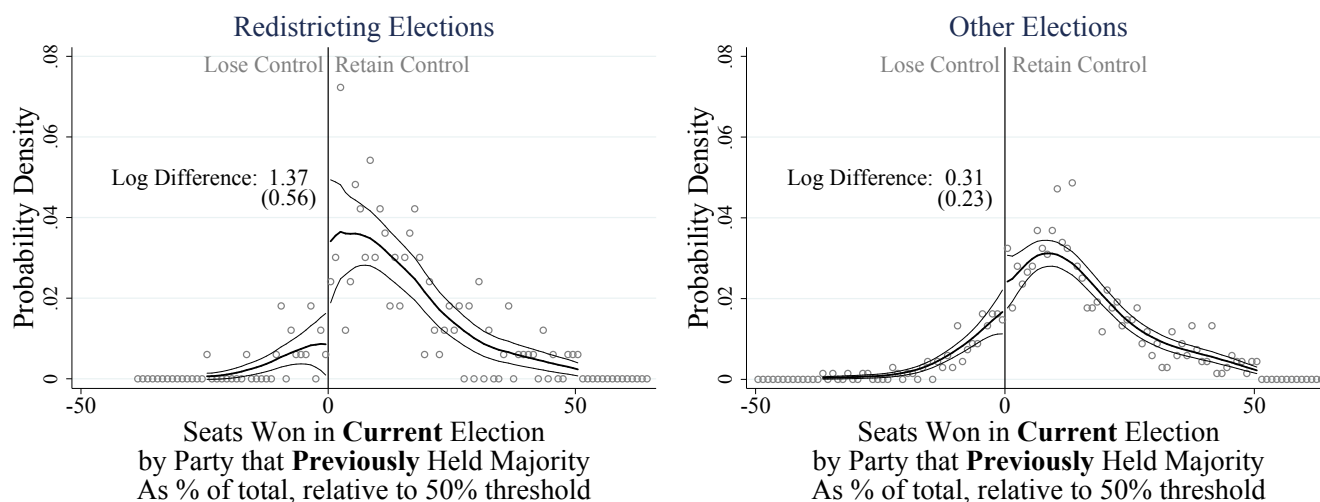
As described in the introduction and in the next section, the key to identifying the Selection Effect is the parties’ ability to exert precise control over the outcomes of elections. Precise control, sometimes called “precise sorting” or “complete manipulation,” arises when an agent has both a means 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 in the redistricting election falls just above the 50 percent cutoff. But does it have the means?

We show in a companion paper that the answer is yes. By changing their campaign tactics, political parties are able to exert precise control over the outcomes of elections to the lower house of the state legislature (Jeong and Shenoy, 2017). In the

¹³In many states the election is in years ending in 0, but a few states are irregular. We define the most recent election before a year ending in 1 as the redistricting election.

Figure 4

Density Tests Show Strong Evidence of
Precise Control in Redistricting Elections



Note: Adapted from Jeong and Shenoy (2017).

absence of precise control the probability density of election outcomes should be continuous, and any predetermined variable should be a continuous function of the outcome. A state where Democrats win 49 percent of seats in the assembly should be similar to one where they win 51 percent. Any discontinuity will arise only as a result of conscious effort by political parties to seize control of redistricting.

Jeong and Shenoy (2017) shows that the party that holds a majority before the redistricting election is 4 times as likely to barely retain control of the assembly as to barely lose control. It achieves this not by poll rigging but 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.”

Figure 4, which is taken from the companion paper, applies a McCrary test (2008)

to the fraction of seats won in the state assembly by whichever party previously held a majority. The left-hand panel shows that in a redistricting election the majority party is able to choose the precise set of outcomes just below the cutoff and drastically reduce their likelihood.

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 $\theta_s \in \mathfrak{R}$, which has an absolutely continuous distribution H^θ . 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

$$X_s^* = x(\theta_s) + v_s \tag{1}$$

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 μ . If Democrats exert control, the actual outcome of the election equals a random variable \tilde{X}_s with density χ . 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 Θ_D and Θ_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

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

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

$$W_{ist} = \theta_s w(t) + \beta^t R_{st} + \eta_{ist} \quad (3)$$

where w is a continuous function whose properties are discussed below; $\mathbb{E}[\eta_{ist} \mid X_s]$ 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 the Census. 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 β^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 district.

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

show in Online Appendix A.1,

$$\mathbb{E}[\theta_s | X_s] = a(X_s) + b(X_s) \cdot \begin{cases} \mathbb{E}[\theta_s | \theta_s \in \Theta_D] & \text{if } X_s \geq 0 \\ \mathbb{E}[\theta_s | \theta_s \in \Theta_R] & \text{if } X_s < 0 \end{cases} \quad (4)$$

for some functions $a(X_s)$ and $b(X_s)$ that are continuous at 0 with $b(X_s) = 0$ if $\mu = 0$.¹⁴

Define the regression discontinuity estimate for year t as

$$\begin{aligned} \rho_t &= \lim_{\varepsilon \rightarrow 0} \left\{ \mathbb{E}[W_{ist} | X_s = \varepsilon] - \mathbb{E}[W_{ist} | X_s = -\varepsilon] \right\} \\ &= \lim_{\varepsilon \rightarrow 0} \left[a(\varepsilon) - a(-\varepsilon) + b(\varepsilon)\mathbb{E}[\theta_s | \theta_s \in \Theta_D] - b(-\varepsilon)\mathbb{E}[\theta_s | \theta_s \in \Theta_R] \right] w(t) \\ &\quad + \mathbb{E}[\eta_{ist} | X_s = \varepsilon] - \mathbb{E}[\eta_{ist} | X_s = -\varepsilon] + \beta^t \cdot \mathbb{I}(t > 0) \\ &= b(0) \left[\mathbb{E}[\theta_s | \theta_s \in \Theta_D] - \mathbb{E}[\theta_s | \theta_s \in \Theta_R] \right] w(t) + \beta^t \cdot \mathbb{I}(t > 0) \end{aligned} \quad (5)$$

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

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

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. In Section 4 we let $w(t)$ be a constant, a linear trend, and a fully flexible set of time dummies.

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 $\rho_{t|t<0}$ estimated in Equation 6 should be positive.

Prediction 1 (Selection Effect) *The endogenous institutions hypothesis predicts the*

¹⁴ $b(X_s)$ is actually the probability the outcome X_s has been controlled.

Selection Effect is positive (pro-opposition).

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

$$\rho_t^\Delta = \rho_t - \rho_k \quad (7)$$

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

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

If there is an uneven number of seats in the assembly we round $\frac{1}{2}[Total\ Assembly\ Members]_s$ up to the next integer. Rounding up ensures $X_s = 0$ is the fewest number of seats Democrats can win without becoming the minority.¹⁵

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

¹⁵In 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.

in $t = -1$ and a 1986 race happens in $t = 5$. If we fix $t = T$ we can estimate the regression discontinuity (5) by running a local linear regression

$$[Outcome]_{isT} = \gamma_0 + \gamma_1 X_s + \gamma_2 X_s \cdot \mathbb{I}(X_s \geq 0) + \rho_T \cdot \mathbb{I}(X_s \geq 0) + \nu_{isT} \quad (9)$$

for $|X_s| < h$

where h is the bandwidth and ν_{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

$$W_{ist} = w\pi_0 + X_s w\pi_1 + X_s \cdot \mathbb{I}(X_s \geq 0) w\pi_2 + \mathbb{I}(X_s \geq 0) w \cdot \rho + C_{ist} \pi_3 + \nu_{ist} \quad (10)$$

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

under different assumptions about w . In Section 4.2 we allow w to be a constant, a linear trend, and a full set of year dummies. The corresponding estimates $\hat{\rho}$ are informative about 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

$$W_{ist} = \alpha_0^{base} + \alpha_1^{base} X_s + \alpha_2^{base} X_s \cdot \mathbb{I}(X_s \geq 0) + \rho^{base} \cdot \mathbb{I}(X_s \geq 0) + \sum_{k=-3, -1, \dots, 3} \mathbb{I}(t = k) \left\{ \alpha_0^k + \alpha_1^k X_s + \alpha_2^k X_s \cdot \mathbb{I}(X_s \geq 0) + \rho_k^\Delta \cdot \mathbb{I}(X_s \geq 0) \right\} + \nu_{ist} \quad (11)$$

for $|X_s| < h, \quad t = \{-5, -3, \dots, 3\}$

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

In Section 4.3 we find that only $\hat{\rho}_1^\Delta = \hat{\beta}^1$ is nonzero. In our primary specifica-

tion we maximize the power of our estimate of $\beta^1 = \beta$ by imposing that the other difference-in-discontinuity estimates are zero:

$$\begin{aligned}
 W_{ist} = & \mathbf{w}\alpha_0 + X_s \mathbf{w}\alpha_1 + X_s \cdot \mathbb{I}(X_s \geq 0) \mathbf{w}\alpha_2 + \mathbb{I}(X_s \geq 0) \mathbf{w}\alpha_3 \\
 & + \mathbb{I}(t = 1) \cdot \left[\alpha_4 + X_s \alpha_5 + X_s \cdot \mathbb{I}(X_s \geq 0) \alpha_6 + \beta \cdot \mathbb{I}(X_s \geq 0) \right] \\
 & + C_{ist} \alpha_4 + \nu_{ist}
 \end{aligned} \tag{12}$$

for $|X_s| < h, \quad t = \{-5, -3, \dots, 3\}$

In our baseline specification we assume \mathbf{w} is a constant. 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 (which yields conservative estimates) 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 4.3 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 C.3 that other results in the main text are not sensitive to the choice of bandwidth.¹⁶ 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

¹⁶When 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).¹⁷ We also discard states that have only a single House representative, as these states have a single district that consists of the entire state.¹⁸ Maine presents an unusual case because unlike other states it has occasionally redistricted in years ending in 3 rather than 1. In our main sample we treat it like the other states (taking years ending in 1 as the redistricting year) to avoid any problem that may arise because the year of redistricting is endogenous. We show in Online Appendix C.5 that the main results do not change if we drop Maine from the sample.

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 2012.¹⁹

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 5.2 use only these elections. In Online Appendix D we give more details and report descriptive statistics for the data.

¹⁷Hawaii adopted a commission in 1968, Washington in 1982, Idaho in 1994, New Jersey in 1995, Arizona in 2000, and California in 2012.

¹⁸Alaska, 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.

¹⁹The 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 C).

4 Main Results

4.1 Evolution of Outcomes

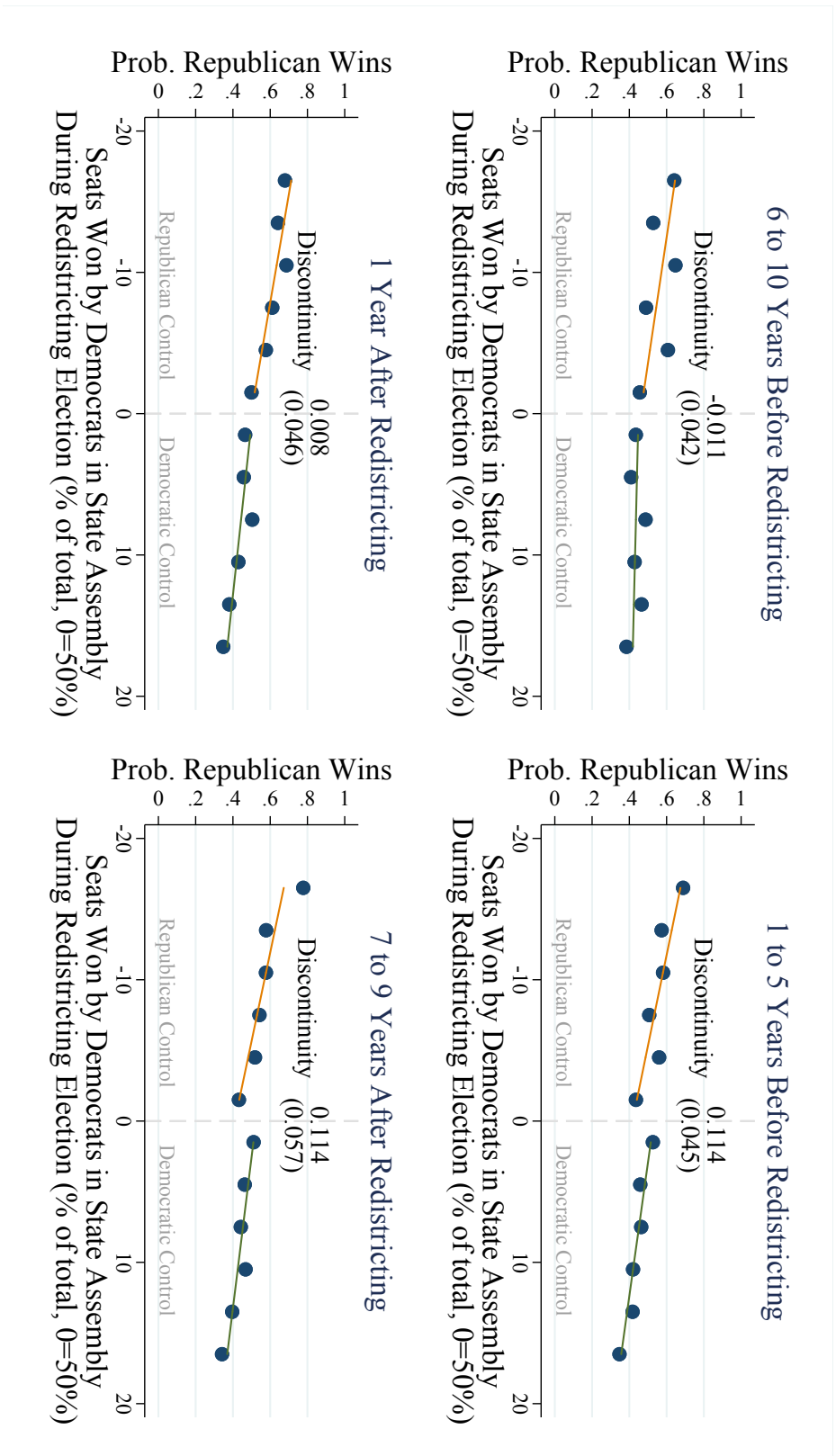
Before estimating our main specifications we present a visual summary of the results. We estimate Equation 9, using as the outcome a dummy for whether the Republican won. We make estimates for different samples of elections based on how far in the future or the past lies the redistricting event.

The top-left panel of Figure 5 shows the discontinuity—or lack thereof—in elections 6 to 10 years before redistricting. Though the probability a Republican wins is decreasing in the seats won by Democrats in the (future) redistricting election, the probability is smooth at the cutoff. That suggests parties are not looking at outcomes so far in the past when deciding where to capture redistricting. But in races just 1 to 5 years before redistricting (the top-right panel) there is a large and statistically significant discontinuity. Democrats choose to control redistricting in states where Republicans have won a higher fraction of recent U.S. House races (relative to states where Republicans choose to control redistricting). The pattern is consistent with the endogenous institutions hypothesis, which predicts each party should prioritize maintaining control in states where it is losing influence.

Yet the bottom-left figure suggests these states immediately become less favorable to the opposition. States that had previously been unfavorable to Democrats—which are also the states where they control redistricting—suddenly become neutral. This reversal suggests the Causal Effect of Democratic control of the assembly during redistricting, which is roughly the difference between the discontinuity in the bottom-left figure and that in the upper-right figure, is to reduce the chance a Republican wins. We show in Section 5.1 that this reversal is not simple mean reversion, as there is no similar change in the statewide share of votes won by Republicans. As predicted by the endogenous institutions hypothesis, the party in control of the assembly uses redistricting to harm its opponents and reverse its losses.

But any such advantage is short-lived. The bottom-right panel suggests the original partisan drift—which, according to the top-right panel, goes against the party in control of the assembly—has returned. By the time the state redistricts again it seems the effects of gerrymandering have eroded. To summarize, 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

Figure 5
Evolution of the Discontinuity Over the Redistricting Cycle



Note: The figure shows estimates of Equation 9, where the unit of observation is a U.S. House race. Each panel estimates the equation after restricting the sample to elections that occur within 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 election just before redistricting. Each dot shows the average of the outcome within a bin of width 3. Standard errors are clustered by state-redistricting event.

persist. Aside from providing evidence consistent with the theory of endogenous institutions, Figure 5 shows the importance of adjusting for the Selection Effect. Had we tried to estimate the Causal Effect without taking into account that parties capture redistricting in states with a hostile partisan drift, we might have erroneously concluded the gains from partisan gerrymandering are zero or even negative.

4.2 Pre-Trends in the Discontinuity: Selection Effect

The figures in Section 4.1 give evidence that, as predicted, there is a pro-opposition Selection Effect. This section confirms that fact more systematically and tests for whether the Selection Effect changes in the lead-up to redistricting. As noted in Section 3, the time pattern of the Selection Effect $w(t)$ 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 under three different assumptions about $w(t)$. Column 1 assumes the Selection Effect is constant. Column 2 assumes it is a constant and a linear trend. Columns 3 through 6 allow it to be fully flexible (a 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.

Column 1 shows that the estimate in the constant specification is noisy, suggesting either there is no selection or that there is heterogeneity in the Selection Effect that is not being captured by the constant alone. In Column 2 the constant remains statistically insignificant but the trend is positive and highly significant, suggesting the Selection Effect is growing over time. But the fully flexible specifications in Columns 3 through 6 show that the actual pattern of selection is not a linear trend. There is little to suggest the coefficients, which are ranked in chronological order, are growing linearly. They suggest there is little or no selection on U.S. House outcomes 7 to 9 years before redistricting. 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 none of the four flexible specifications 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; parties likely ignore elections so far in the past in deciding where to capture redis-

Table 1
Prediction 1: Estimates of the Selection Effect

	(1) Rep. Win	(2) Rep. Win	(3) Rep. Win	(4) Rep. Win	(5) Rep. Win	(6) Rep. Win
[Dem. Control]						
× Constant	0.063 (0.039)	-0.035 (0.049)				
× Trend		0.020*** (0.007)				
× $[t = -9]$			-0.044 (0.052)	-0.028 (0.050)		
× $[t = -7]$			0.024 (0.049)	0.001 (0.054)	0.066 (0.056)	0.026 (0.058)
× $[t = -5]$			0.119** (0.049)	0.126** (0.050)	0.157*** (0.054)	0.146*** (0.044)
× $[t = -3]$			0.113** (0.053)	0.137** (0.056)	0.146*** (0.056)	0.147*** (0.049)
× $[t = -1]$			0.106** (0.052)	0.122** (0.050)	0.142** (0.066)	0.135** (0.058)
Test: $[t = -5]=[t = -3]=[t = -1]$.96	.9	.93	.95
Event FEs					X	X
Year FEs				X		X
Observations	6820	6820	6820	6820	6820	6820
Events	135	135	135	135	135	135

Note: We estimate Equation 10 under different assumptions about $w(t)$. 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.

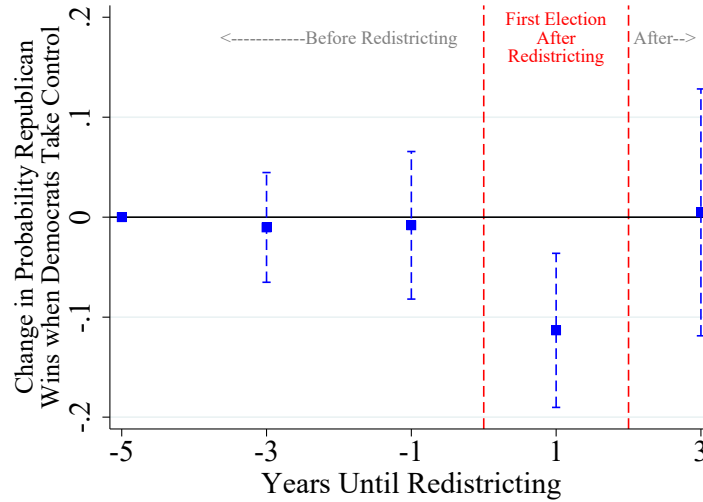
tricting. The outcomes of U.S. House contests become relevant for their decision starting 5 years before redistricting. Since the coefficients are positive they suggest parties make pro-opposition selection, meaning they aim to capture redistricting in states where the opposing party has been winning.

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. Is it possible that the effect is constant only until redistricting? One simple check is to test whether regression discontinuity estimates of the change in the Republican vote share, which should be relatively unaffected by redistricting, are similar before and after redistricting.²⁰ As we show in Section 5.1 and in greater detail in Online Appendix C.2, these estimates are indeed similar, suggesting our assumption of a constant Selection Effect is not unreasonable.

²⁰It is possible that voters would respond to new district lines—for example, supporters of the opposition party might be unwilling to turn out if fewer districts are competitive. We find little evidence of any such effect.

Figure 6

Prediction 2: Difference-in-Discontinuity Estimate
of Causal Effect of Redistricting



Note: We estimate Equation 11 and plot the coefficients $\{\hat{\rho}_t^\Delta\}$ with their 90 percent confidence intervals. Standard errors are clustered by state-redistricting event.

4.3 Difference-in-Discontinuity Estimates: Causal Effect

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, 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 6 shows exactly that. We plot the estimates $\{\hat{\rho}_t^\Delta\}$ 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, implying a Republican is roughly 11 percentage points less likely to win the election after redistricting when Democrats switch to holding a majority in the assembly during redistricting. 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 redistricting is short-lived. We show in Section 5 that this pattern is unlikely to be explained by mean-reversion, suggesting the effect is genuinely transient. As we argue in Section 6, such transience is not entirely surprising.

We maximize the power of our estimates by imposing the restriction implied by Figure 6: that the Causal Effect appears only in the year immediately after redistricting. 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 linear 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 sensitive to excluding the so-called pre-clearance states. During our sample these states were required to submit changes to their voting rules for pre-clearance to the U.S. Department of Justice (as per Section 5 of the 1965 Voting Rights Act).²¹ 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 second 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.

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

²¹These 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						
	(1) Rep. Win	(2) Rep. Win	(3) Rep. Win	(4) Rep. Win	(5) Rep. Win	(6) Rep. Win
Dif-in-Disc Estimate	-0.108*** (0.036)	-0.111*** (0.039)	-0.112*** (0.035)	-0.105*** (0.037)	-0.110** (0.043)	-0.096*** (0.036)
Event FEs		X		X		X
Year FEs			X	X		X
Trends					X	X
Observations	6541	6541	6541	6541	6541	6541
Events	135	135	135	135	135	135

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

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

Note: Each column shows a different estimate of $\hat{\beta}$ from Equation 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.

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.

5 Mechanism: Is It Really Caused by Partisan Redistricting?

Are the difference-in-discontinuities estimates of Section 4.3 really the Causal Effect of partisan redistricting? Given its transience and its timing—it comes immediately after a pro-opposition Selection Effect—one may wonder whether the anti-opposition effect that appears in the year after redistricting is reversion to the mean or caused by some force unrelated to redistricting. Though it is impossible to definitively prove that redrawn boundaries are the mechanism, we present three pieces of evidence to suggest it would be a remarkable coincidence if redistricting is wholly unrelated.

5.1 Conversion Rate

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 7 shows that there is no statistically significant effect on the Republican vote share. Though there is a dip in the year after redistricting, it is not much larger than one visible three years before redistricting and is likely just random variation. As we

show in Appendix C.2, there is in fact a pro-opposition Selection Effect visible in the statewide vote share, as would be expected given the results of Section 4.2. But the size of the effect remains unchanged during the redistricting cycle. This result supports our assumption that the Selection Effect is stable over the redistricting cycle.

By contrast, the center panel of Figure 7 shows that there is 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 6. The contrast between this large effect on the share of seats won and the non-effect on the share of votes won suggests our result is unlikely to be driven by mean-reversion, which would appear in the vote shares.

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.²² 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.

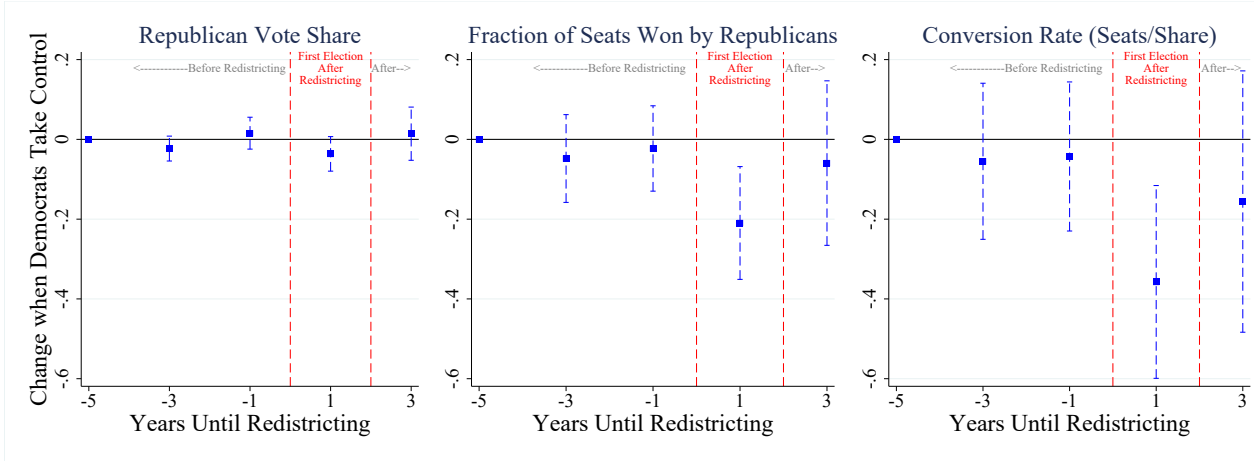
5.2 Open Seats

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 enemy incumbent through gerrymandering. It would likewise find it unnecessary to use gerrymandering to prop up its own incumbents. 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

²²This example assumes a conversion rate of 1 in a neutral environment. In reality the political geography of most states naturally favors Republicans. Regardless of what the neutral conversion rate is, Republican control would imply they win 18 percentage points more of the state's seats.

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



Note: We apply a state-level version of Equation 12 to the statewide Republican vote share, the fraction of seats won, and the conversion rate (the ratio of the two). The unit of observation is the state-election year. Standard errors are clustered by state-redistricting event.

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 who has represented him for many years and whose office may have 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.²³ The regression discontinuity is graphed in the top-left and top-right panels of Figure 8, which were created analogously to 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.²⁴ 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

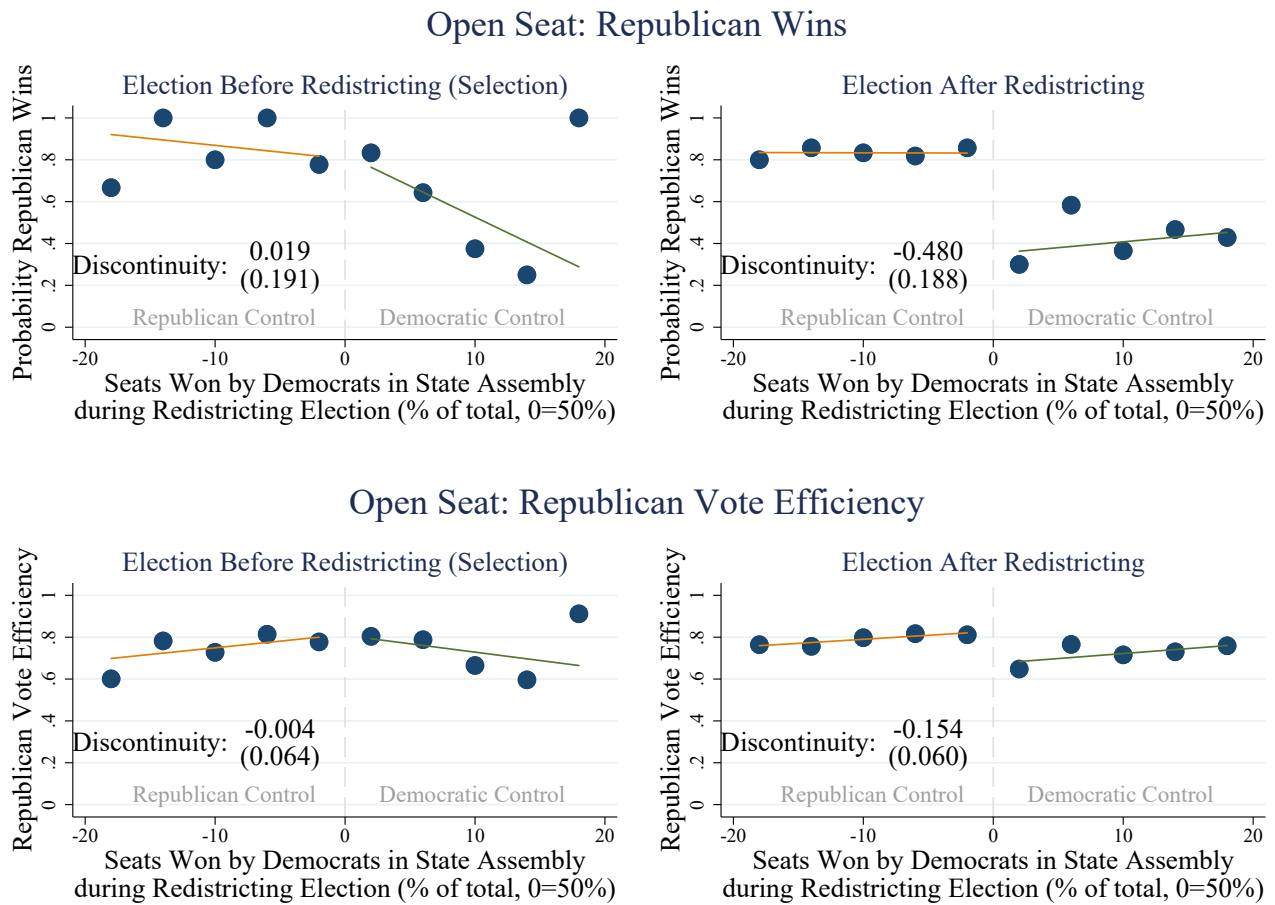
$$[Vote\ Efficiency]_{ist} = 1 - \begin{cases} V_{ist}^R & \text{if } W_{ist} = 0 \\ V_{ist}^R - V_{ist}^{[2]} & \text{if } W_{ist} = 1 \end{cases} \quad (13)$$

which is simply the unwasted Republican votes as a fraction of all votes cast. The bottom-left panel of Figure 8 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.

²³In Online Appendix C.2 we show that there is also little evidence of selection in earlier years.

²⁴This 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 8
Open Seats Have the Most Clearly Visible Effect



Note: The figure shows estimates of Equation 9, where the unit of observation is a U.S. House race. 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. The outcomes are plotted against the percentage of seats (relative to 50%) won by Democrats in the election just before redistricting. Each dot shows the average of the outcome within a bin of width 4. Standard errors are clustered by state-redistricting event.

5.3 Demographics of Redrawn Districts

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 more hostile 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).²⁵ Since an African American is likely to support Democrats, Republicans may try to redistrict African Americans to minimize their influence.²⁶

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.²⁷

Conditional on moving African Americans, are Republicans more likely than Democrats to move them into districts that minimize their influence? One way to minimize the influence of some group of voters is to “pack” them into districts in which they form the overwhelming majority. Though these few districts are lost with cer-

²⁵According to CNN (2016), whose data are based on National Election Pool exit polls.

²⁶The 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.

²⁷Visual representations of all tests described in this section can be found in Appendix C.2.

Table 3
Evidence of Racial Gerrymandering

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

Note: Columns 1 and 2 estimate 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, testing for whether they are more likely to be in districts with too large or too small a population. All standard errors are clustered by state-redistricting event. Visual representations of these specifications are in Appendix C.2.

tainty, the number of contests in which the hostile voters may be pivotal is minimized.²⁸ 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 redistricting.²⁹

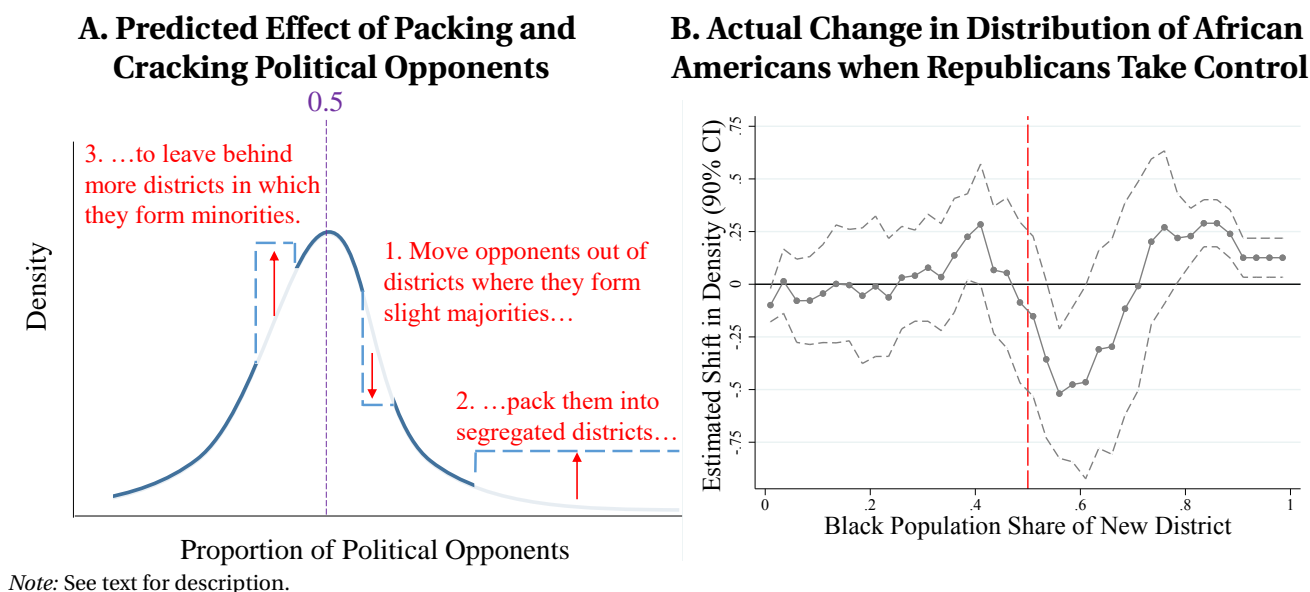
Figure 9 gives a more complete description of how Democrats versus Republicans redistrict African Americans by studying the probability density of the districts into which they are moved. Figure 9.A shows how a hypothetical gerrymander would move hostile voters to minimize their influence. First, they 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

²⁸In 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).

²⁹The regression in Column 3 of Table 3 and those used to construct Figure 9.B narrow the bandwidth to 10 because there is essentially no racial “packing” further away from the discontinuity.

Figure 9

Effect of Packing and Cracking on Distribution of Political Opponents



from just above to just below 50 percent.³⁰

Figure 9.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 9.A we adjust the estimates to show the change when Republicans take control from Democrats.³¹ As predicted, there is an increase in the density of overwhelmingly African American districts and districts in which African Americans are barely outnumbered. Meanwhile, the density of districts in which

³⁰The “optimal” way to gerrymander, as described in Friedman and Holden (2008), is actually rather more sophisticated than this. It requires using the party’s most ardent supporters to neutralize its most ardent foes. However, the common perception is that parties do not attempt this more complex approach. It is possible that constraints of both geography and information—the would-be gerrymanderer might not be able to identify the strength of a voter’s left-right bias—prevents optimal gerrymandering.

³¹For this figure we restrict the sample to include only states in which no independents are elected to the assembly. This restriction makes Republican control exactly the converse of Democratic control. The results without making this restriction look similar.

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 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 then test whether majority-black census tracts are more likely to be in high-deviation districts on one side of the threshold. Column 5 of Table 3 suggests there is no difference, implying African Americans are no more likely to live in malapportioned districts.

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

This section argues that the effect of partisan redistricting is short-lived because there are swings in the electorate that undermine a favorable redistricting plan. De-

signing a favorable district is possible only if the designer has 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 such 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 in the state-election 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.

Aside from helping explain our results, this conjecture is important because it 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. We provide evidence in Section 6.2 that, as new technology has made predictions more accurate and the veil has been lifted, gerrymandering has become more persistent.

6.1 Example: Shocks Undo Gerrymandering

Consider a simple numerical example (we make the point more systematically in simulations presented in Appendix B). 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.³² 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

³²To 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.

(the simulations in Appendix B 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.

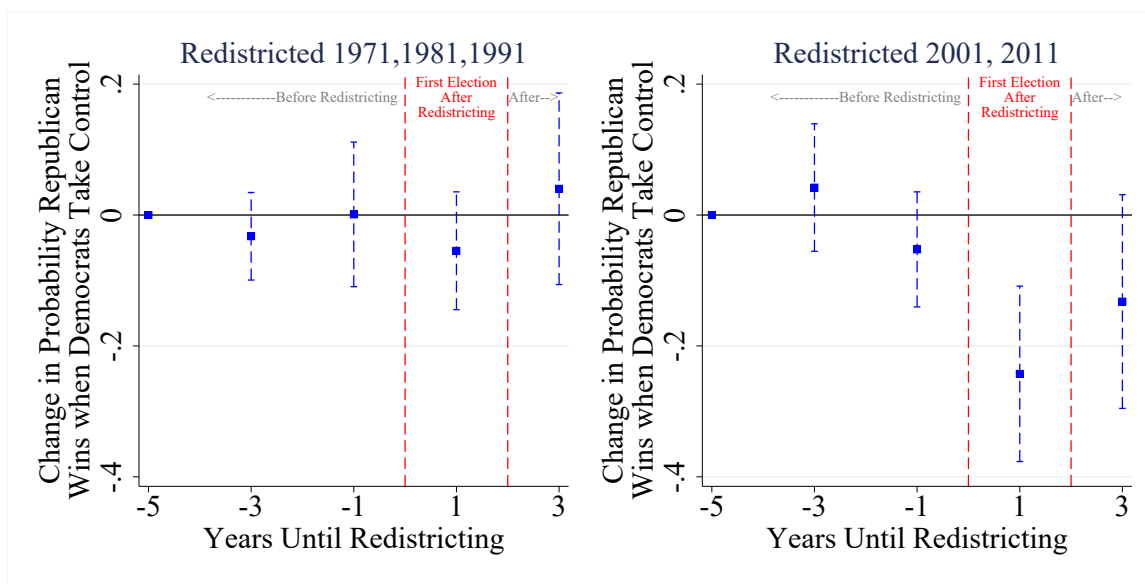
Stark though this example appears, it actually ignores mitigating factors that would further reduce the longevity of gerrymandering. The district planner may not be able to predict how people will vote even in the election just after redistricting. Moreover, the true uncertainty, which has both an aggregate 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.

6.2 Is Gerrymandering More Potent in Recent Years?

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

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

Figure 10
The Effect of Gerrymandering Grows Larger and More Persistent



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

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 for our analysis is that it might make it easier to design a long-lasting gerrymander.

Each panel of Figure 10 is constructed like Figure 6. The left-hand panel restricts the sample to the 1971, 1981, and 1991 redistricting cycles while the right-hand panel restricts to the 2001 and 2011 cycles. The figure suggests that the immediate effect of redistricting is much larger in the later period—an anti-opposition effect of 24 percentage points versus 5.5 percentage points. In the later period the sign of the effect remains large and negative in the next election, though it is not statistically significant. The estimate provides some evidence that, unlike in the earlier period, the effect in the later period is persistent. This analysis is only suggestive, as splitting the sample leaves relatively few elections. We offer it only as some evidence consistent with the idea that the consequences of partisan gerrymandering depends

largely on how well the party in control can predict how people will vote. At the very least this result suggests it would be hasty to conclude the effects of gerrymandering will forever remain short-lived.

7 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.

We provide evidence to support our claim that partisan redistricting is the mechanism behind our results. We show that the party that controls redistricting is able to win a larger share of a state's U.S. House delegation despite winning the same share of the vote. We also show that the effect we find is especially large in open seats, where a district's partisan lean is likely most important, and that the boundaries are adjusted in a way consistent with racial gerrymandering.

Finally, we find that the effects of partisan redistricting are short-lived, possibly because the natural variability in elections makes it difficult to create an enduring gerrymander. But we find evidence consistent with an increase in the size and persistence of the effects of partisan redistricting in more recent times, 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,

$$\begin{aligned}\mathbb{E}[\theta_s \mid X_s] &= \mathbb{E}[\theta_s \mid X_s, D_+] \mathbb{P}(D_+ \mid X_s) + \mathbb{E}[\theta_s \mid X_s, D_-] \mathbb{P}(D_- \mid X_s) \\ &\quad + \mathbb{E}[\theta_s \mid X_s, R_+] \mathbb{P}(R_+ \mid X_s) + \mathbb{E}[\theta_s \mid X_s, R_-] \mathbb{P}(R_- \mid X_s)\end{aligned}\tag{14}$$

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),

$$\begin{aligned}\mathbb{E}[\theta_s \mid X_s, D_+] &= \mathbb{E}[\theta_s \mid \theta_s \in \Theta_D] \\ \mathbb{E}[\theta_s \mid X_s, R_+] &= \mathbb{E}[\theta_s \mid \theta_s \in \Theta_R]\end{aligned}$$

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 θ_s is

$$\begin{aligned}H^\theta(\theta \mid X_s) &= \mathbb{P}(\theta_s < \theta) \\ &= \mathbb{P}(x^{-1}(X_s - v_s) < \theta) \\ &= \mathbb{P}(X_s - x(\theta) > v_s) \\ &= 1 - F[X_s - x(\theta)] \\ \Rightarrow h^\theta(\theta \mid X_s) &= f[X_s - x(\theta)] x'(\theta)\end{aligned}\tag{15}$$

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 θ after conditioning on D_- is

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

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

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

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

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,

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

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

$$\begin{aligned} g(X_s \mid D_+) &= \chi(X_s) \\ g(X_s \mid R_+) &= \chi(-X_s) \end{aligned}$$

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

$$\begin{aligned} g(X_s \mid D_-) &= \tilde{g}^D(X_s) = \int g(X_s \mid \theta_s, D_-) h^\theta(\theta_s \mid D_-) d\theta \\ &= \int_{\theta \notin \Theta_D} f(X_s - x(\theta_s)) \frac{h^\theta(\theta_s)}{1 - \mu} d\theta \end{aligned}$$

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

Now define

$$\begin{aligned} a(X_s) &= \bar{\theta}^D(X_s) \frac{(1 - \mu)\tilde{g}^D(X_s)}{\mu\tilde{\chi}(X_s) + (1 - \mu)[\tilde{g}^D(X_s) + \tilde{g}^R(X_s)]} \\ &\quad + \bar{\theta}^R(X_s) \frac{(1 - \mu)\tilde{g}^R(X_s)}{\mu\tilde{\chi}(X_s) + (1 - \mu)[\tilde{g}^D(X_s) + \tilde{g}^R(X_s)]} \\ &= \mathbb{E}[\theta_s \mid X_s, D_-] \mathbb{P}(D_- \mid X_s) + \mathbb{E}[\theta_s \mid X_s, R_-] \mathbb{P}(R_- \mid X_s) \end{aligned}$$

which is a continuous function. Then Equation 4 follows because

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

□

B Simulation Appendix (For Online Publication)

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

1. Republicans choose a threshold \bar{V} . They aim for all districts to have this vote share.
2. They draw 10 percent of Republican voters out of each district to be reallo-

cated, meaning in the absence of further transfers each district's Republican vote share is now $\{0.9V_{ist}^R\}_{\forall i}$.³³ Their “budget” for gerrymandering is then $B = \sum_i 0.1V_{ist}^R$.

3. They order districts in descending order by the Republican vote share

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

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

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 4.3. 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:

$$\kappa_{is,t+1} = \theta_{t+1} + \bar{\kappa}_{s,t+1} + \tilde{\kappa}_{is,t+1}$$

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

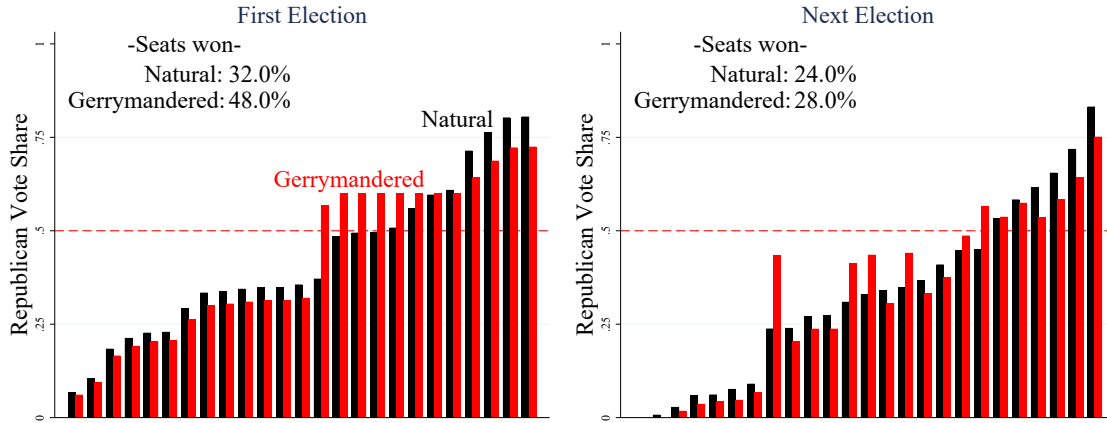
We set $I = 25$. We draw the natural vote share $\{V_{ist}^R\}_{\forall i}$ from a Beta(2,2) distribution rescaled by 0.9 to mimic the pro-opposition Selection Effect. For each threshold $\bar{V} = .5, .51, \dots, .8$ and each national shock $\theta = -0.05, 0, 0.05$ we run 200 simulations.

Figure 11 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 ($t + 1$).

³³To be precise, 10 percent of the voters are replaced by an equal number of Democrats from a different district.

Figure 11

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



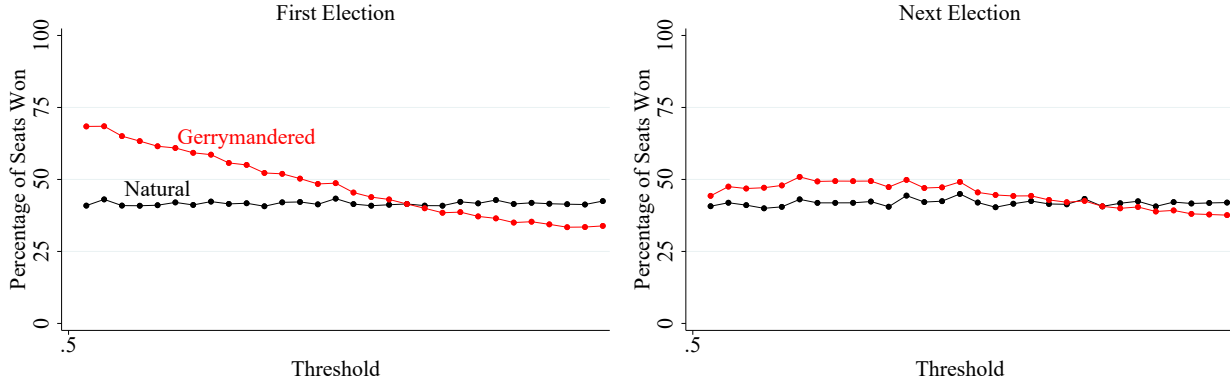
Note: Each bar shows the (simulated) fraction of votes won by Republicans in a district. The black bar shows the fraction that would have been won in the absence of gerrymandering. The red bar shows how many are won assuming Republicans gerrymander as described in the text. The left-hand figure shows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

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

Figure 12 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

Figure 12
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).

a gap of roughly 11 percentage points in the first election, the threshold should be roughly 0.6. That implies a gap in the next election of around 6 percentage points—somewhat smaller than the original effect and well within the confidence interval of our estimate shown in Figure 6 for the election 3 years after redistricting.

C Empirical Appendix (For Online Publication)

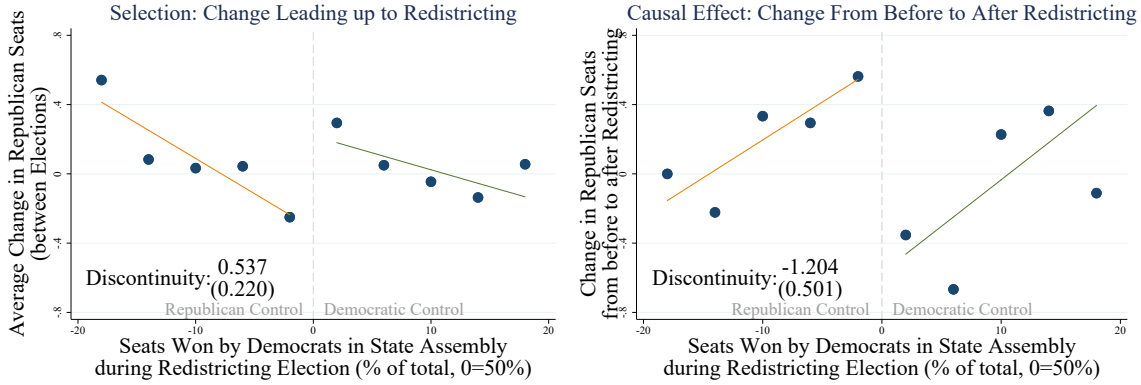
This appendix shows additional figures, tables, and analyses referenced in the main text.

C.1 State-Level Trends in Outcomes: House Versus Senate

In this appendix we take the state-redistricting event as the unit of observation. We estimate Equation 9 on the average change in the total number of Republicans elected to the House in the years leading up to redistricting.³⁴ This estimate gives a rough sketch of the Selection Effect. The left-hand panel of Figure 13 shows the regression discontinuity. Each dot represents the average of the outcome within a four-percentage point bin, and the lines are the predictions based on estimating Equation 9 on the state-level data.

³⁴To be precise, we take $[Outcome]_s = \frac{1}{4} \sum_{t=-7}^{-1} (\sum_i W_{ist} - \sum_i W_{i,s,t-2})$.

Figure 13
Selection and Causal Effect of Redistricting at the State Level



Note: The figure shows estimates of Equation 9 at the state-level. The outcome in the left-hand panel is the average change in the number of U.S. House seats won by Republicans leading up to redistricting. The outcome in the right-hand panel is the total change in Republican seats from the election just before redistricting to the election just after. These outcomes are plotted against the percentage of seats (relative to 50%) won by Democrats in the election just before redistricting. Each dot shows the average of the outcome within a bin of width 4. Standard errors are clustered by state-redistricting event.

The change in seats won by Republicans is decreasing in the margin of seats won in the assembly by Democrats—not surprising, as a state that in the future elects many Democrats to the assembly is becoming more hostile to Republicans. But there is a positive jump at the point where Democrats gain control of the assembly, suggesting Democrats aim to capture redistricting in states where Republicans are increasing their strength in Congress. Republicans aim to do the opposite, choosing to take control in states where their strength is waning.

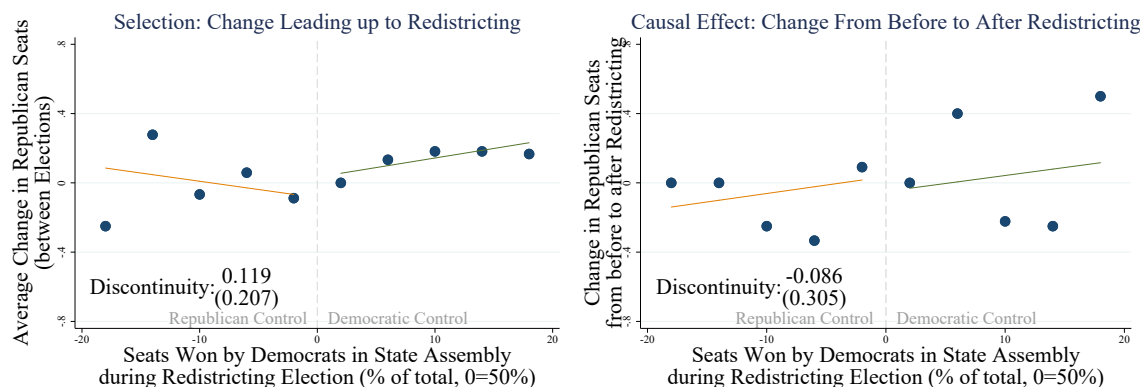
But these gains by the opposition party are partially *reversed* after redistricting. The right-hand panel of Figure 13 shows estimates of Equation 9 when the outcome is now the change in the total number of Republicans elected to the House from before to after redistricting.³⁵ This figure is the mirror image of the left-hand panel, suggesting the fortunes of the Republicans turn sharply negative in states where Democrats control the assembly. This figure suggests each political party seeks to control redistricting in states where it has sustained recent losses. It uses redistricting to reverse those losses.

Is it possible this result is completely unrelated to Congressional redistricting or driven by some form of mean reversion? We assess that possibility by running an

³⁵To be precise, we take $[Outcome]_s = \sum_i W_{i,s,1} - \sum_i W_{i,s,-1}$.

Figure 14

Senate Placebo: There is Neither Selection nor Causal Effect on Senate Outcomes



Note: This figure is analogous to Figure 13, but using the outcomes of U.S. Senate elections rather than U.S. House elections.

identical analysis on the outcomes of elections to the U.S. Senate. Since Congressional redistricting has no effect on the boundaries of a senator’s “district” (as U.S. senators represent the entire state), we would expect the Causal Effect to be zero. We would also expect the Selection Effect to be smaller or even zero, as a political party has no incentive to condition its actions on Senate outcomes. The Selection Effect would exist only to the extent that Senate outcomes are correlated with House outcomes.

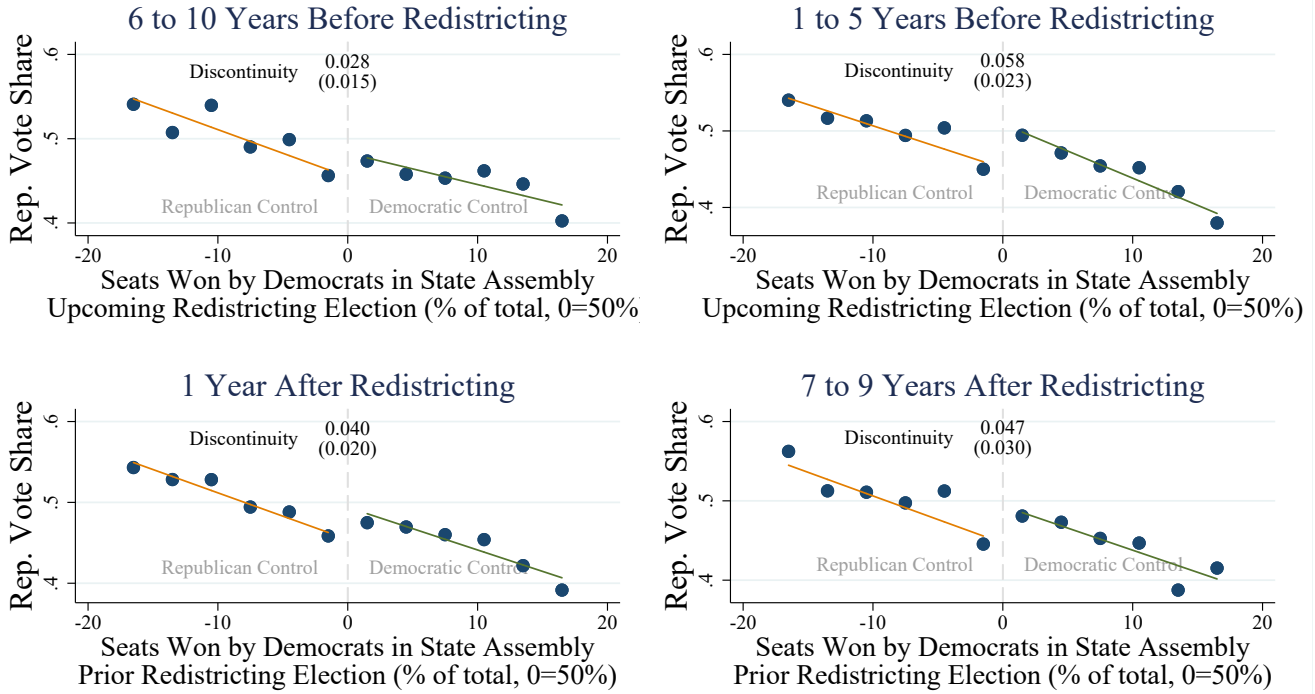
Figure 14 suggests the Selection Effect is small or perhaps zero, while the Causal Effect is close to zero. Neither estimate is statistically significant. The right-hand panel must be treated with some caution, as the number of observations is relatively small (only about a third of state-redistricting events have a senator up for election in both the year ending in 0 and the year ending in 2). But though noisy, the pattern suggests there is no discontinuity in Senate outcomes at the cutoff, making it less likely the results of Figure 13 are biased or the result of mean reversion.

C.2 Additional Tables and Figures Referenced in the Text

C.2.1 Selection Effect of Republican Vote Share (Sections 4.2 and 5.1)

Figures 15 and 16 show that there is a pro-opposition Selection Effect on the Republican vote share, much as there is for the primary outcome of interest (whether the

Figure 15
The Selection Effect on Vote Shares Does Not Vanish after Redistricting
(Race-Level Regressions)



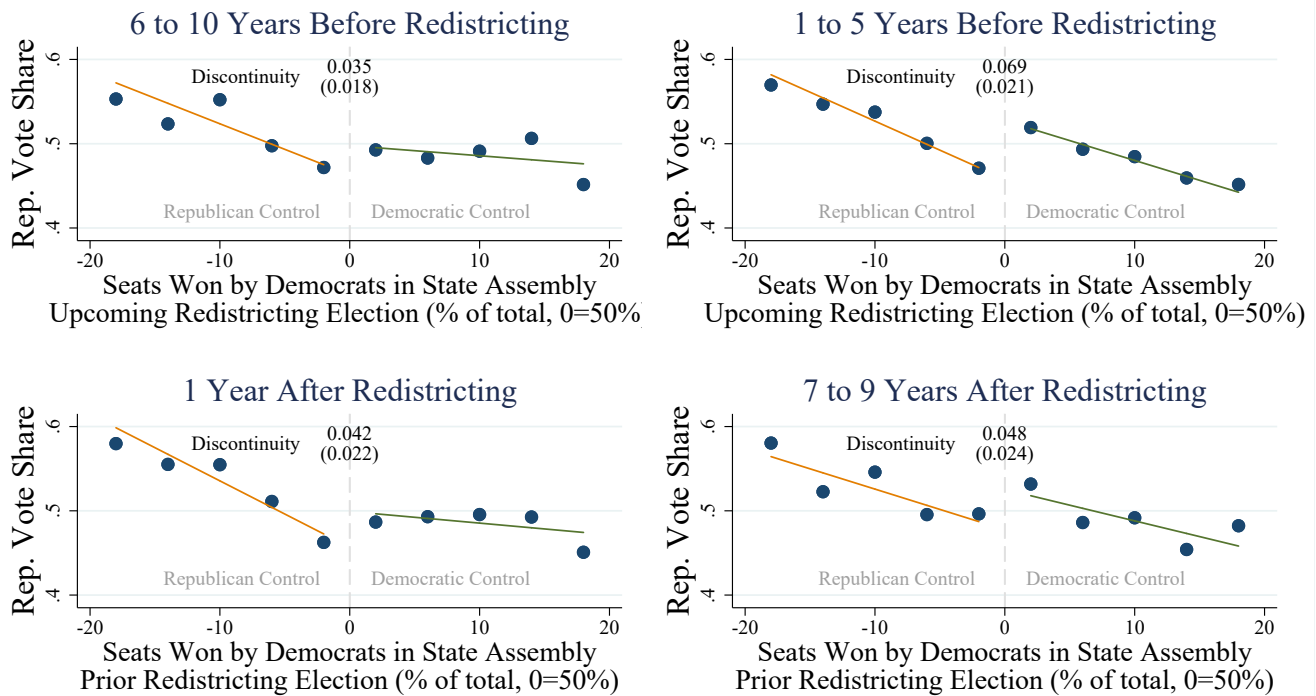
Note: See text for description. Unit of observation is the race. Each dot shows the average of the outcome within a bin of width 3. Standard errors are clustered by state-redistricting event.

Republican wins). But as gerrymandering should affect only the distribution of votes and not average support for the Republicans, there should be no Causal Effect. If our identification assumption of a constant Selection Effect is valid then estimates of Equation 9 should show little or no change in the estimated discontinuities $\{\hat{\rho}_t\}$ between $t \in \{-5, -3, -1\}$ and $t > 0$. This is precisely what Figure 15 suggests about the race-level average Republican vote share and Figure 16 about the state-wide Republican vote share.

C.2.2 Selection Effect in Open Seats (Section 5.2)

Figures 17 and 18 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 the although the estimates are noisy (there are not many open seats), there

Figure 16
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.

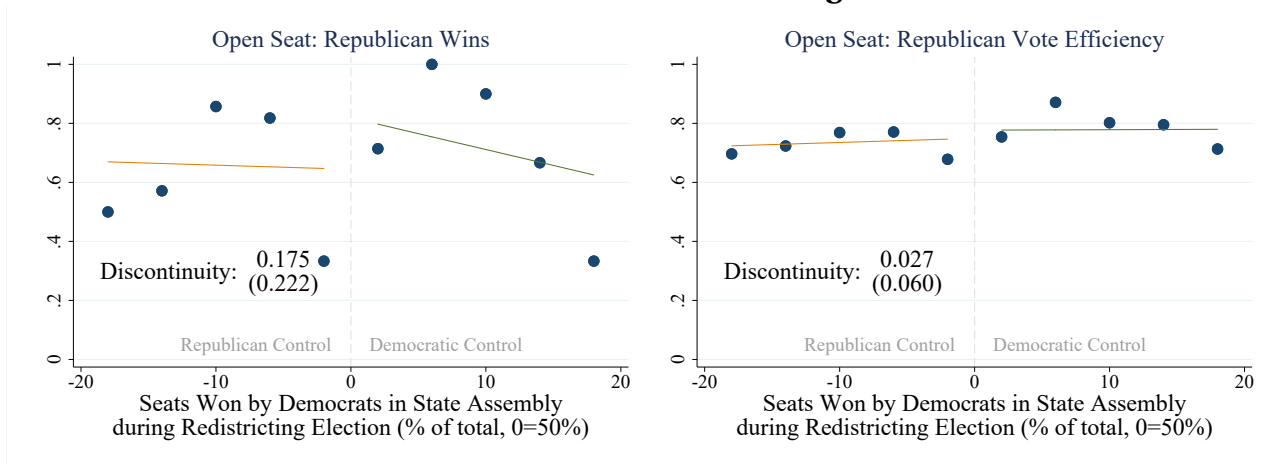
is little evidence of a trend or a persistent discontinuity. We take this as suggestive evidence that there is no systematic Selection Effect on open seats.

C.2.3 Racial Gerrymandering (Section 5.3)

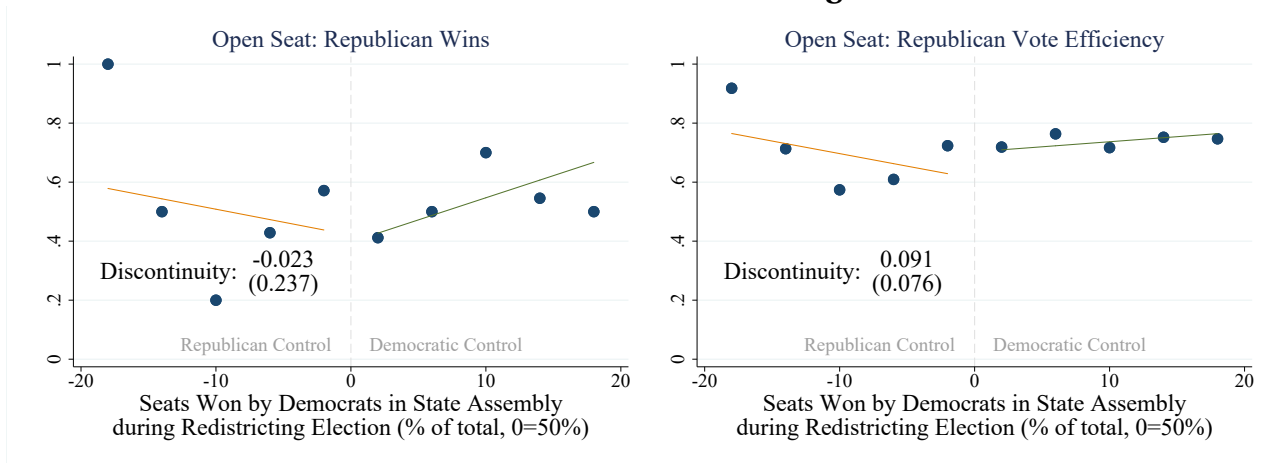
Figure 19 gives the visual representation of Columns 1 and 2 of Table 3. Figure 20 represents Column 3, and Figure 21 represents Columns 4 and 5. See the text of Section 5.3 for details on the outcomes and the specifications.

Figure 17
Selection Effect on Open Seats

7 Years before Redistricting



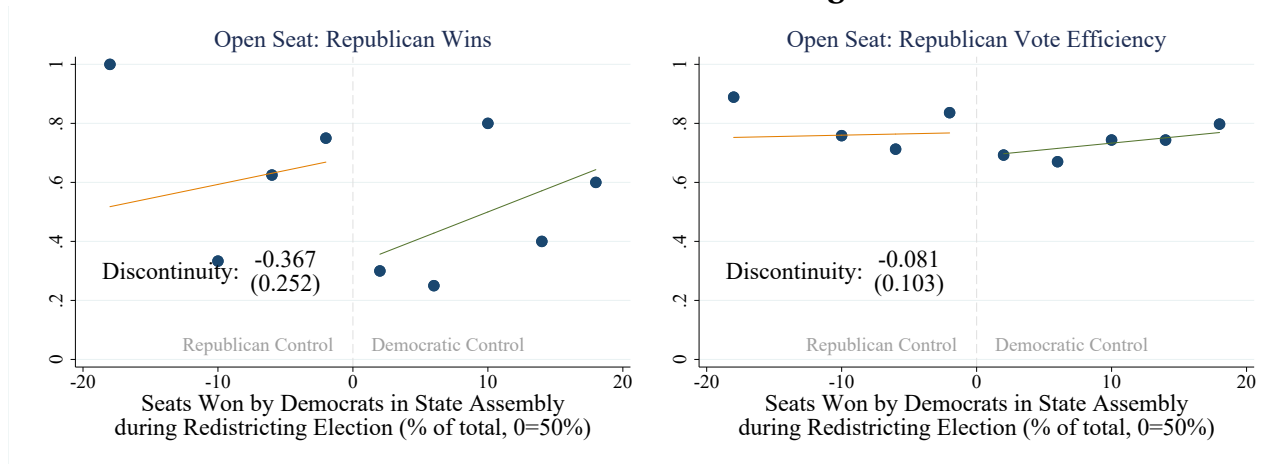
5 Years before Redistricting



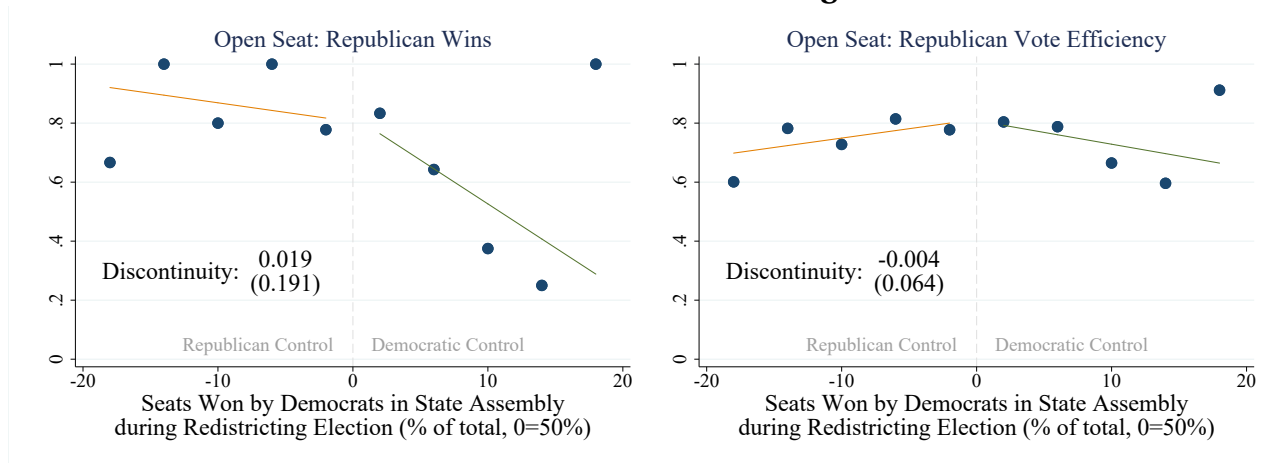
Note: See text for description. Unit of observation is a U.S. House race for an open district. Each dot shows the average of the outcome within a bin of width 4. Standard errors are clustered by state-redistricting event.

Figure 18
Selection Effect on Open Seats (cont.)

3 Years before Redistricting

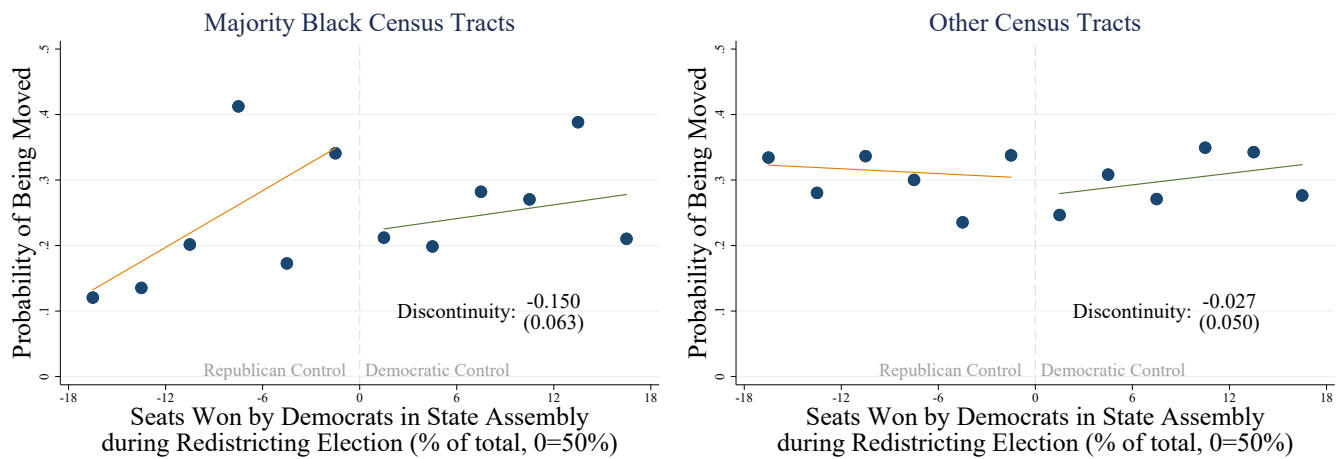


1 Year before Redistricting



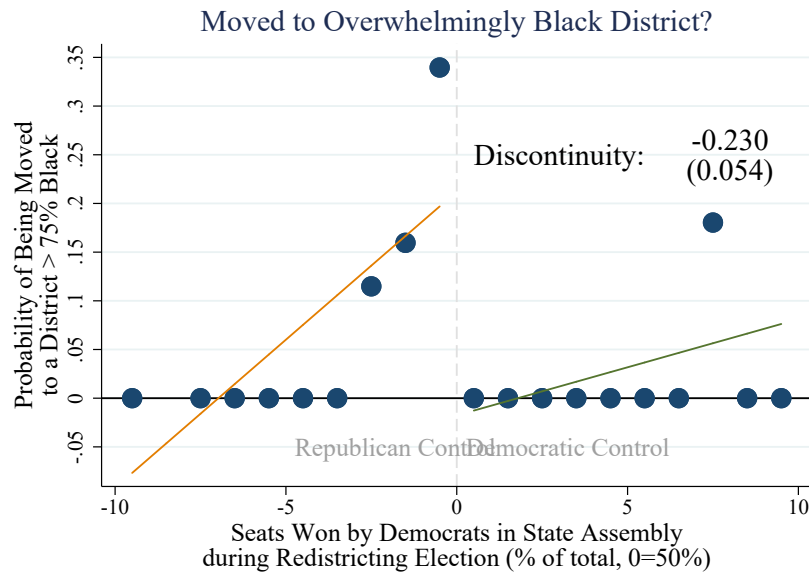
Note: See text for description. Unit of observation is a U.S. House race for an open district. Each dot shows the average of the outcome within a bin of width 4. Standard errors are clustered by state-redistricting event.

Figure 19
African Americans are More Likely
to Be Moved to Unfamiliar Districts



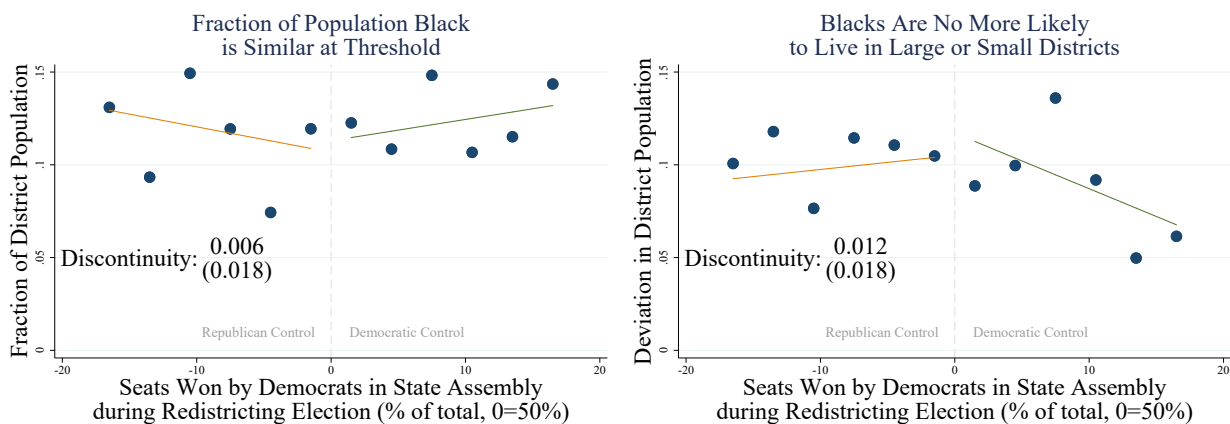
Note: See the text describing Table 3 in Section 5.3 for details. Each dot gives the conditional mean within a bin of width 3.

Figure 20
Conditional on Being Moved,
Where Are African Americans Moved?



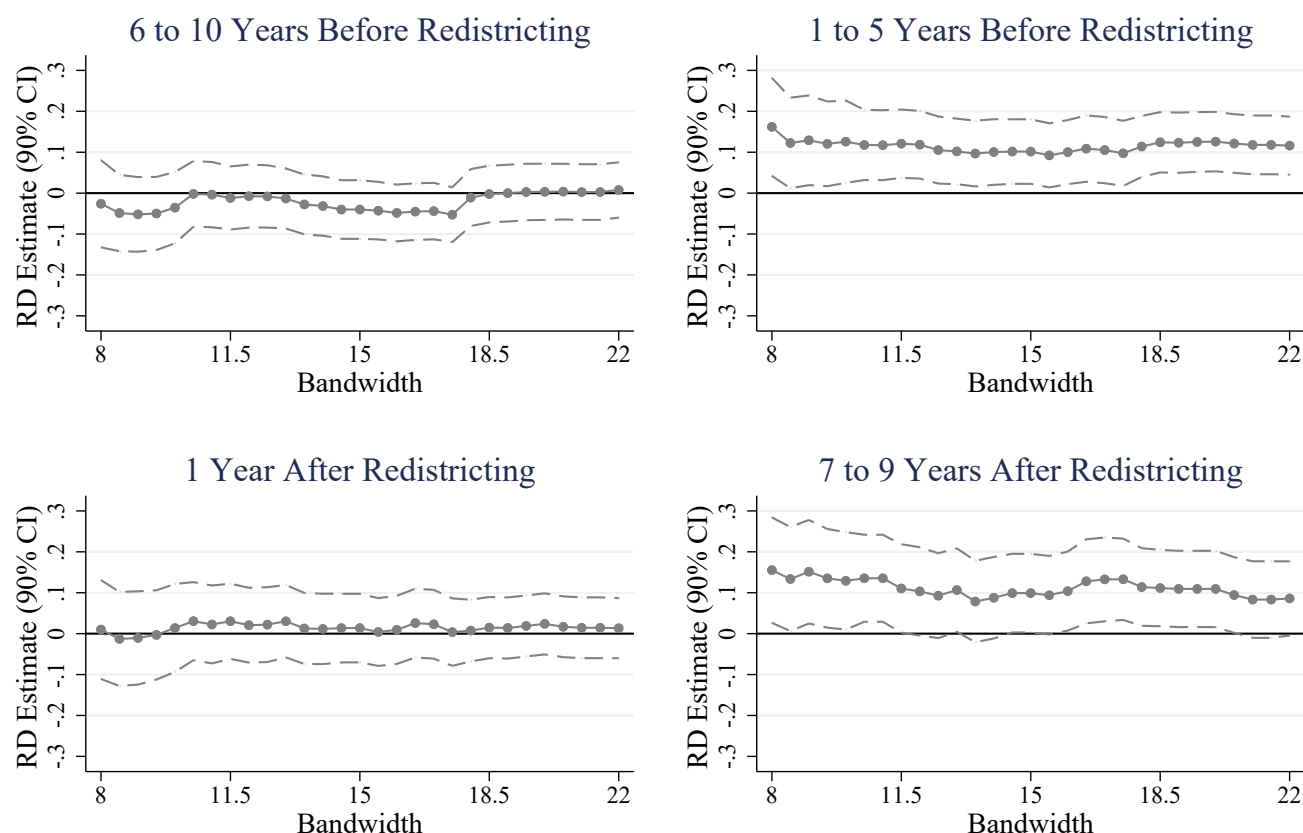
Note: See the text describing Table 3 in Section 5.3 for details. Each dot gives the conditional mean within a bin of width 1.

Figure 21
No Evidence of a Greater Need to Redistricting
African Americans on One Side of the Threshold



Note: See the text describing Table 3 in Section 5.3 for details. Each dot gives the conditional mean within a bin of width 3.

Figure 22
Robustness to Bandwidth: Figure 5



C.3 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 22 shows the robustness of the results from Section 4.1 and Figure 23 shows the robustness of the estimates of the Selection Effect from Specification 6 of Table 1. Figures 24–28 show the robustness of the results from Section 5. Figure 29 shows the robustness of the results from Online Appendix C.1. 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 23
Robustness to Bandwidth: Table 1, Specification 6

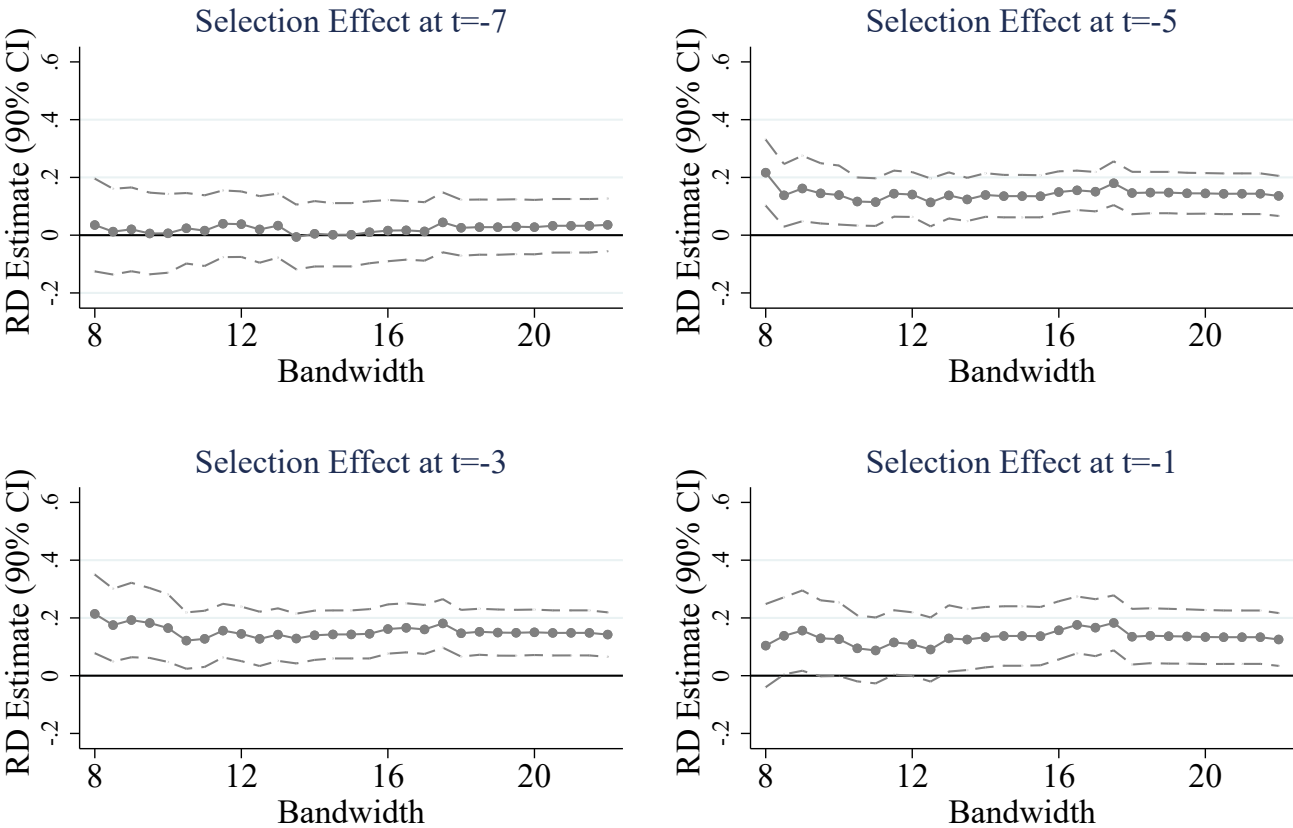
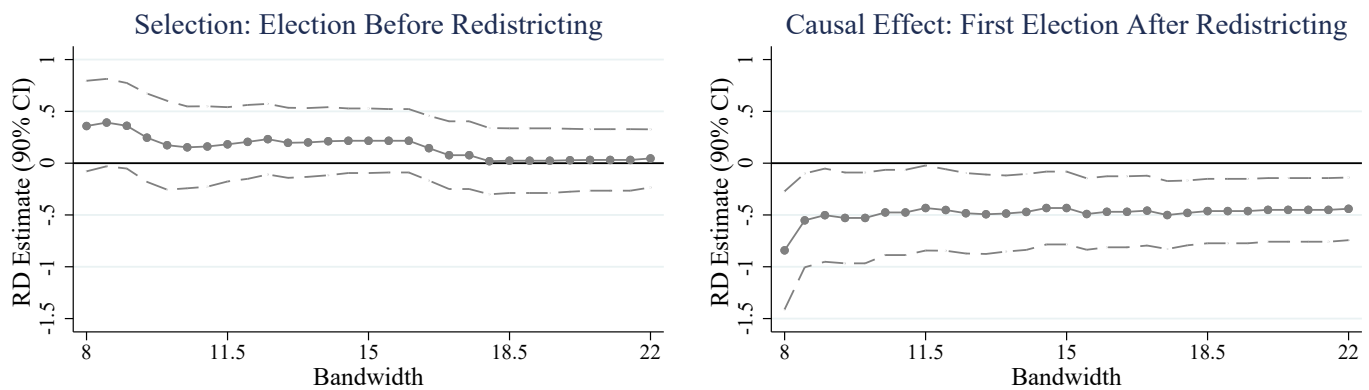


Figure 24
Robustness to Bandwidth: Figure 8

Open Seat: Republican Wins



Open Seat: Republican Vote Efficiency

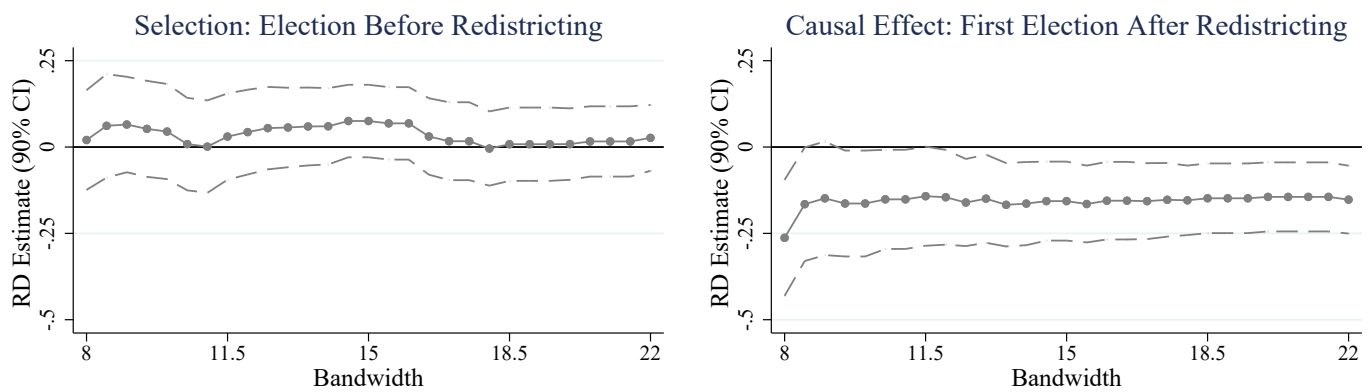


Figure 25
Robustness to Bandwidth: Table 3, Columns 1 and 2

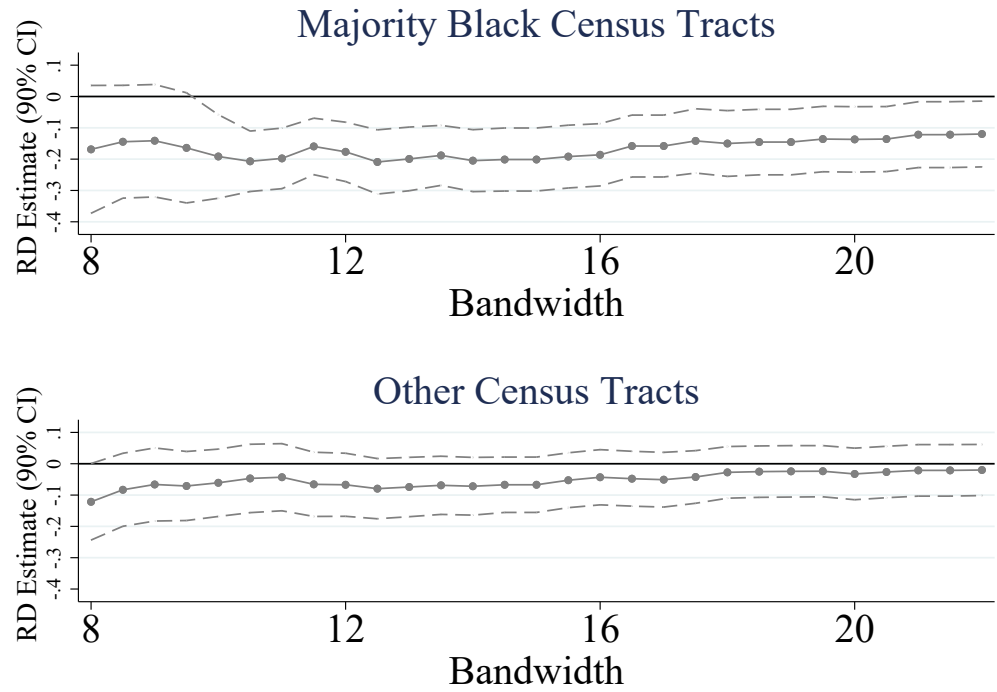


Figure 26
Robustness to Bandwidth: Table 3, Column 3

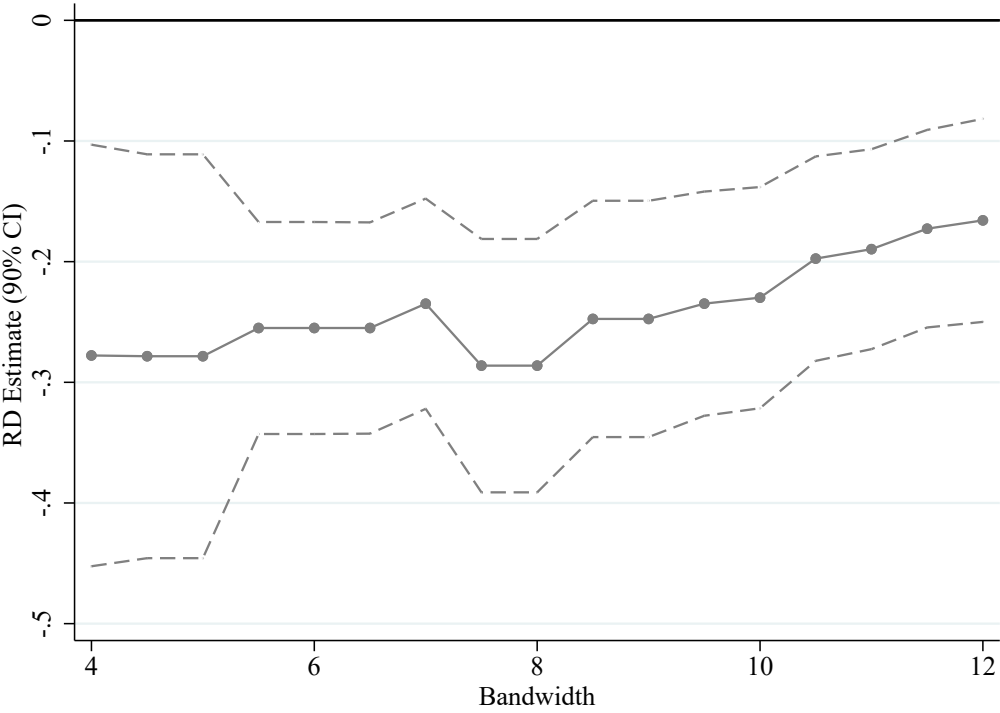


Figure 27
Robustness to Bandwidth: Figure 9.B

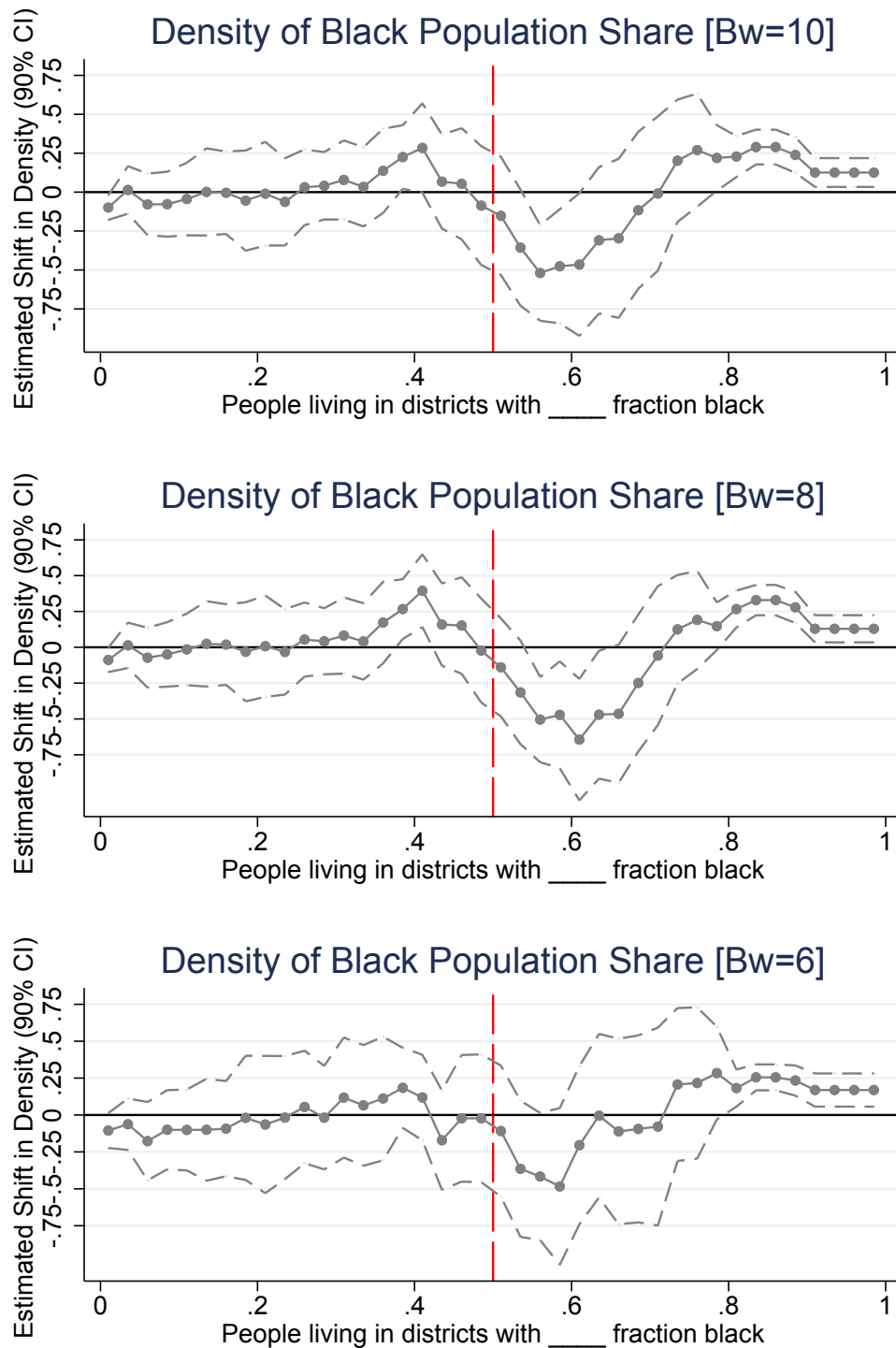


Figure 28
Robustness to Bandwidth: Table 3, Columns 4 and 5

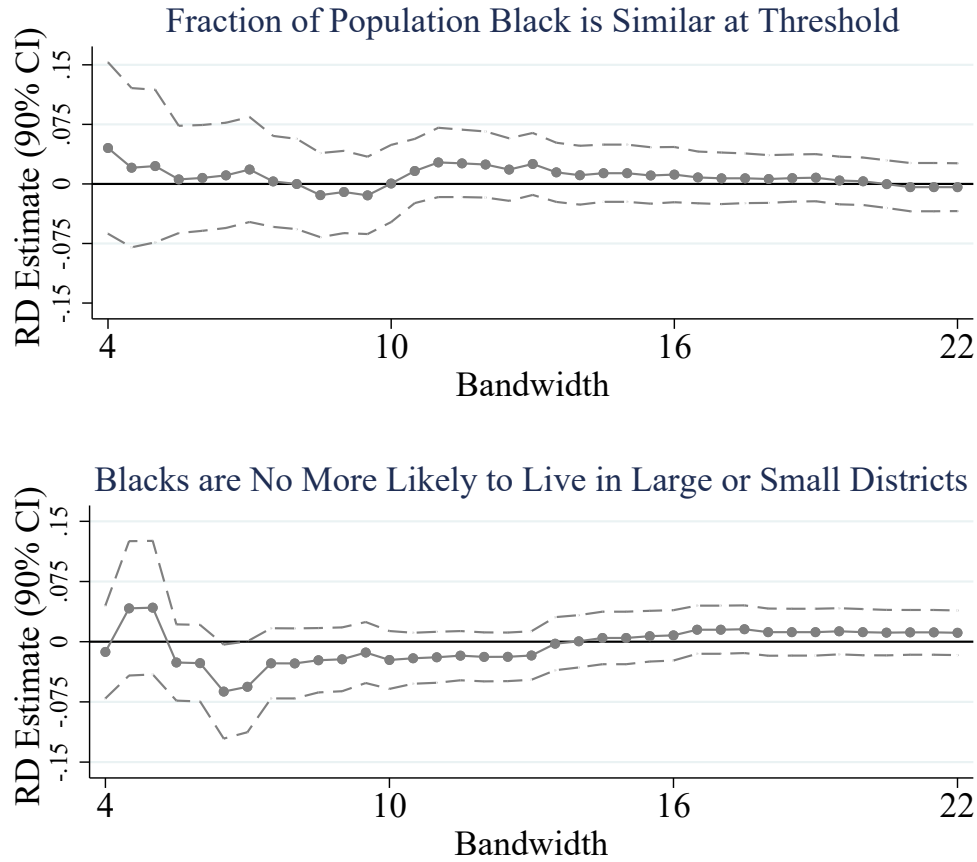


Figure 29
Robustness to Bandwidth: Figure 13

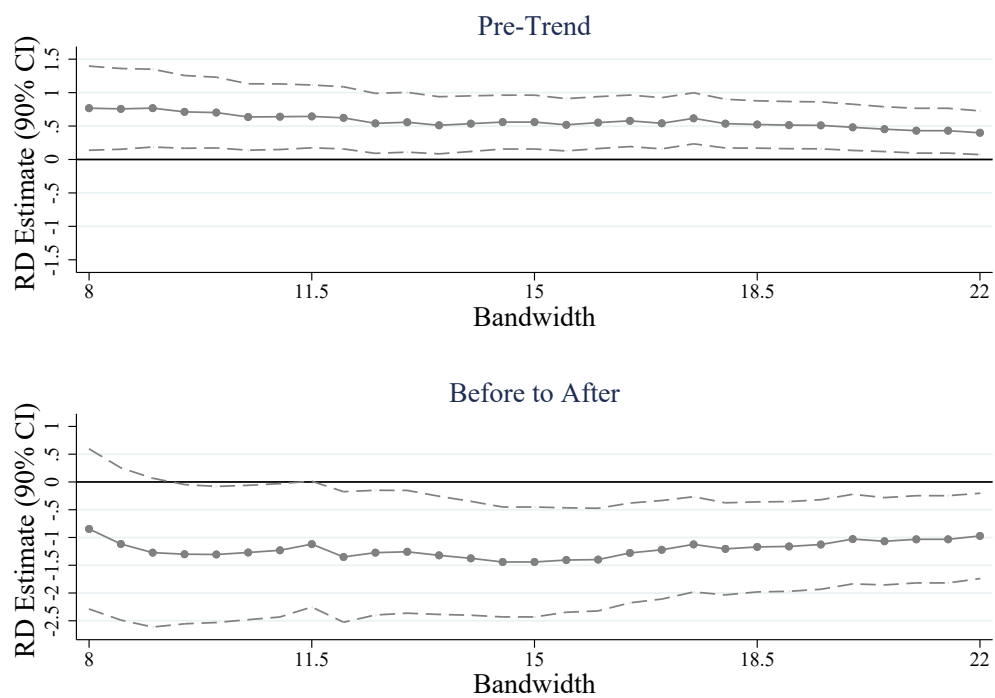
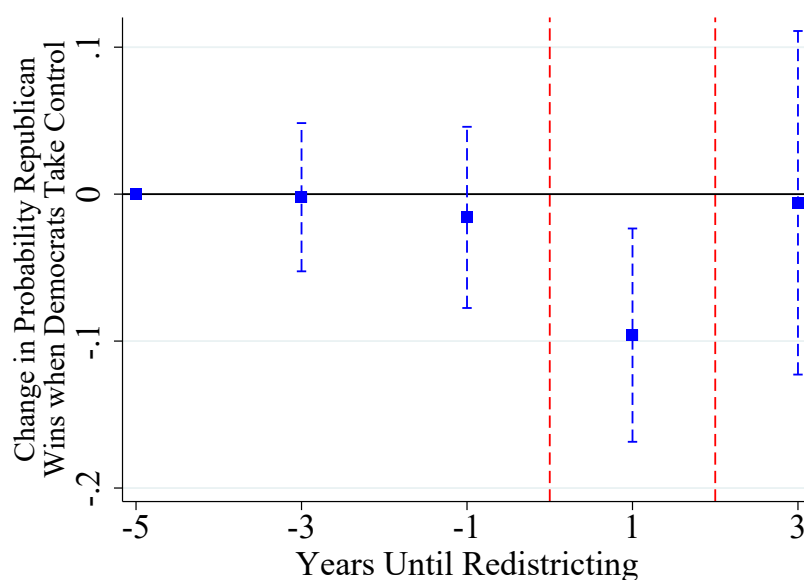


Figure 30
The Main Results Hold with the Alternative Dataset

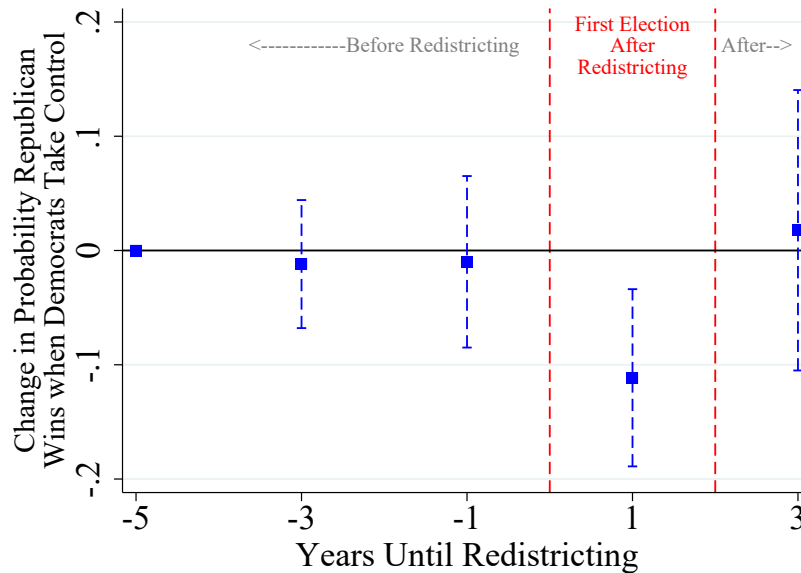


C.4 Verifying the Results with an Alternative Dataset of U.S. House Races

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 30 shows that the difference-in-discontinuities estimates are unchanged when we use the alternative dataset.

Figure 31
The Main Results Hold after Dropping Maine



C.5 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 31 confirms that dropping Maine has little effect on the results.

D Data Appendix

D.1 Election Outcomes Dataset

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

Table 4
Descriptive Statistics, Election Outcomes

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

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

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

D.2 District Demographics Dataset

Our data on district demographics comprise two datasets, one at the level of the census tract and one at the level of the district. As noted in the text, we formed both datasets by linking tracts from each census (for example, 1980) to district boundaries both before (1980) and after (1982) the year ending in 1, which we treat as the year of redistricting. As noted in the text, for each tract we compute the fraction of the population in its new district that was not in its old district. If this fraction exceeds 50 percent we mark the tract as having been “moved” to a new district. We also compute the total district population and the fraction of the population that is African American. Finally, we compute the absolute deviation of the pre-redistricting district from the median district in the state, which is assigned to all tracts contained in the district (see main text for more details). Table 5 shows descriptive statistics for tract- and district-level outcomes.

Table 5
Descriptive Statistics, District Demographics

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

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