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

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Abstract

We propose a method that simultaneously identifies where parties take control of Congressional redistricting, and how they use it to win U.S. House races. Our method exploits the discontinuous change in a party's control of redistricting triggered when its share of seats in the state legislature exceeds 50 percent during redistricting. In the election before redistricting, parties systematically win narrow majorities in legislatures of states where they have lost recent House races. We use a difference-in-discontinuities estimator to control for this systematic difference in pre-redistricting U.S. House outcomes. We estimate that whichever party controls the state legislature during redistricting is 11 percentage points more likely to win House races immediately after redistricting. These gains effectively reverse the party's pre-redistricting losses. Opposition votes are less efficiently converted to seats and, under Republican redistricting, African Americans are more likely to be segregated into overwhelmingly black districts.

JEL Codes: D72, D78, H10, K00

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

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1 Introduction

Labeled "corrosive to a representative democracy" and a means by which to "rig an election," gerrymandering has few friends in the popular press.[1] The academic literature has drawn more nuanced conclusions, ranging from studies that suggest it is relatively inconsequential (Chen and Rodden, 2013), to those that find it creates the potential for sizable unfairness (McGhee, 2014), to older studies that suggest it may even enhance democracy (Gelman and King, 1994a).

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

This paper aims to address both challenges at once using a novel approach. It 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 estimating where parties seek and gain control, which we call the "Se-

[1] New York Times, Editorial (May 30, 2017) and Economist (2002).

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

We then measure the discontinuity in the outcomes of House races after redistricting. Since these outcomes *are* affected by redistricting (the "Causal Effect" of redistricting), this post-redistricting discontinuity is a combination of the Selection Effect and the Causal Effect. We can isolate the Causal Effect by netting out the Selection Effect. Since the Selection Effect is simply the discontinuity in the outcomes of House contests that occur before redistricting, we net out the Selection Effect by taking the difference in the discontinuities measured for outcomes before and after redistricting. The key assumption behind this difference-in-discontinuities approach is that the Selection Effect does not abruptly change from before to after redistricting. We show that while the number of seats won by the ruling party changes sharply after redistricting, its vote share does not, suggesting the change cannot be explained by a shift in popular sentiment towards the ruling party. It must be caused by the redrawing of district boundaries.

We find that parties systematically win majorities (even in very close legislative elections) in the assemblies of states where the opposition has made recent gains in U.S. House elections. The opposition loses its gains in the election immediately after redistricting. The probability a Republican candidate wins a contest for the U.S. House falls by 11 percentage points when Democrats control the assembly during redistricting. Yet this anti-opposition Causal

Effect is short-lived and has largely faded by the next election.

We find evidence that gerrymandering is the mechanism behind these effects. After redistricting there is a sharp decline in how efficiently each vote for the opposition is converted into a seat. 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 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—a difference in treatment not obviously explained by any objective need to redistrict African Americans differently.

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

1.1 Relation to the Empirical Literature on Partisan Redistricting

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

One branch of the simulation literature measures the responsiveness and partisan bias of a redistricting plan by simulating how the number of seats won by a party changes as its vote share changes (e.g. Gelman and King, 1990, 1994a,b; Engstrom, 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 and Cottrell, 2016). Several of these studies have concluded that the actual plans are no more favorable than would have arisen by chance. Finally, in response to (ultimately unsuccessful) litigation aimed at ruling gerrymandering unconstitutional, there has been more recent literature (e.g McGhee, 2014) that defines measures of partisan fairness that could theoretically be used to evaluate a redistricting plan.

These studies implicitly assume voting behavior would be similar under an alternative district map. If parties channel resources, or voters decide whether to turn out, based on whether their district is competitive, this assumption may no longer hold. Our study complements the simulation literature by estimating the impact of gerrymandering using a different approach. We estimate the counterfactual by comparing outcomes across the legislative discontinuity. Our claim is not that our assumption is "right" while the prior literature is "wrong," only that taking a different approach can yield a fresh perspective on an area of extraordinary public interest.

The rest of the literature compares actual outcomes under plans proposed by Democrats, Republicans, or independent commissions. Several studies compare outcomes over time (Brunell and Grofman, 2005), over the course of the redistricting cycle (Hetherington et al., 2003), or under plans set by different redistricting authorities (Grainger, 2010). Other work estimates the effect of redistricting using some form of difference-in-differences (Ansolabehere and Snyder Jr, 2012; Carson et al., 2007; McCarty et al., 2009; Lo, 2013). Comparing actual outcomes is valid only if the comparison group—different states, different election cycles—is an accurate counterfactual. The counterfactual is invalid if there are differential trends in the attitude of the electorate or if parties actively seek control of certain states in anticipation of redistricting. Our research design is able to account for both omitted confounders and the Selection Effect.[2]

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

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

2 When and How is Redistricting Done?

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

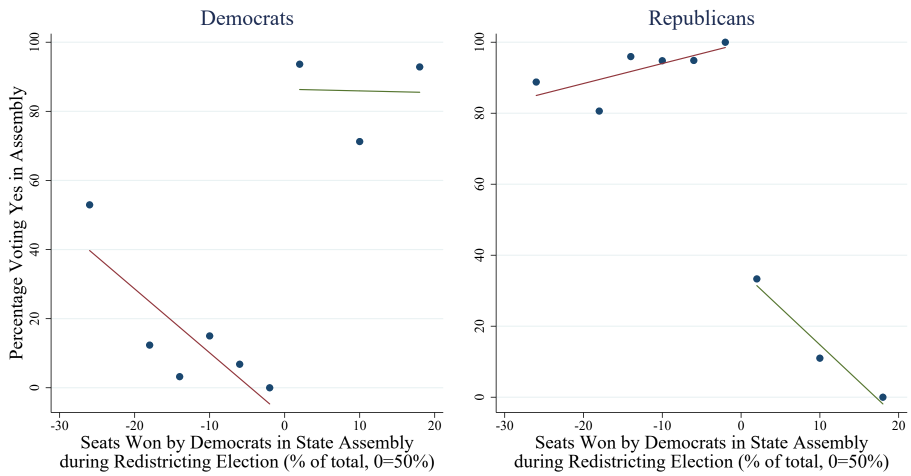
Though control over a single chamber does not grant complete control over redistricting, it does grant a veto. When Democrats gain control of the lower house they are able to vote down any unfavorable plan, giving them a strong incentive to take control of the legislature just before the redistricting process begins.[3] Control switches discontinuously away from Republicans when Democrats win at least 50 percent of seats. Assuming that Democrats can maintain strict party discipline, this logic suggests the redistricting plan should become discontinuously more favorable to them when they achieve a majority.

Figure 1 suggests that this assumption is valid. Using data from several states, we plot the fraction of Democrats and Republicans voting yes on the 2011 redistricting bill against the percentage of seats in the state assembly won by Democrats in the previous election.[4] When

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

[4]The roll call votes were constructed from Vote Smart (2016), which has roll call votes on 51 bills from 21 states for the 2011 redistricting cycle.

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



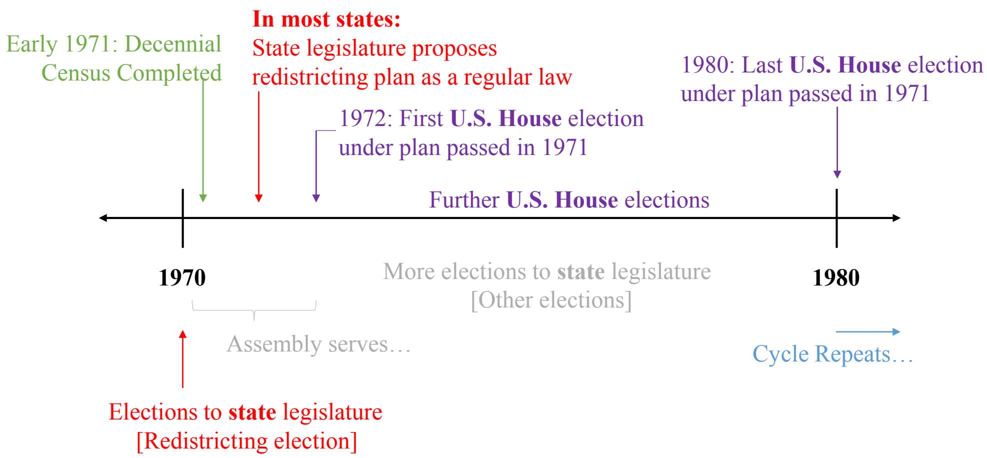
Note: The figure shows the fraction of members in the lower house of the assembly who voted in favor of the redistricting bill during the 2011 redistricting cycle.

Democrats gain control of the assembly they switch from near-universal opposition to near-universal support for the redistricting bill. Republicans are slightly less unified but still sharp in their response. This reversal of support suggests that control of the assembly triggers a sharp change in the type of plan proposed. Moreover, it suggests there is strong party unity—just below the cutoff, 100 percent of Republicans and 0 percent of Democrats vote for the bill. Such unity implies winning 50 percent of the seats really does grant a measure of control over the redistricting plan passed by the lower house. 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 2, the census is completed in years ending in 1.[5] Whichever party wins the election to the state legislature just before this year has the

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

Figure 2
Schedule of Redistricting



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

opportunity to pass its own redistricting plan.[6] These key state elections, labeled onwards as "redistricting elections," create the variation we exploit to estimate the Selection Effect and Causal Effect of redistricting.

3 Motivating the Research Design

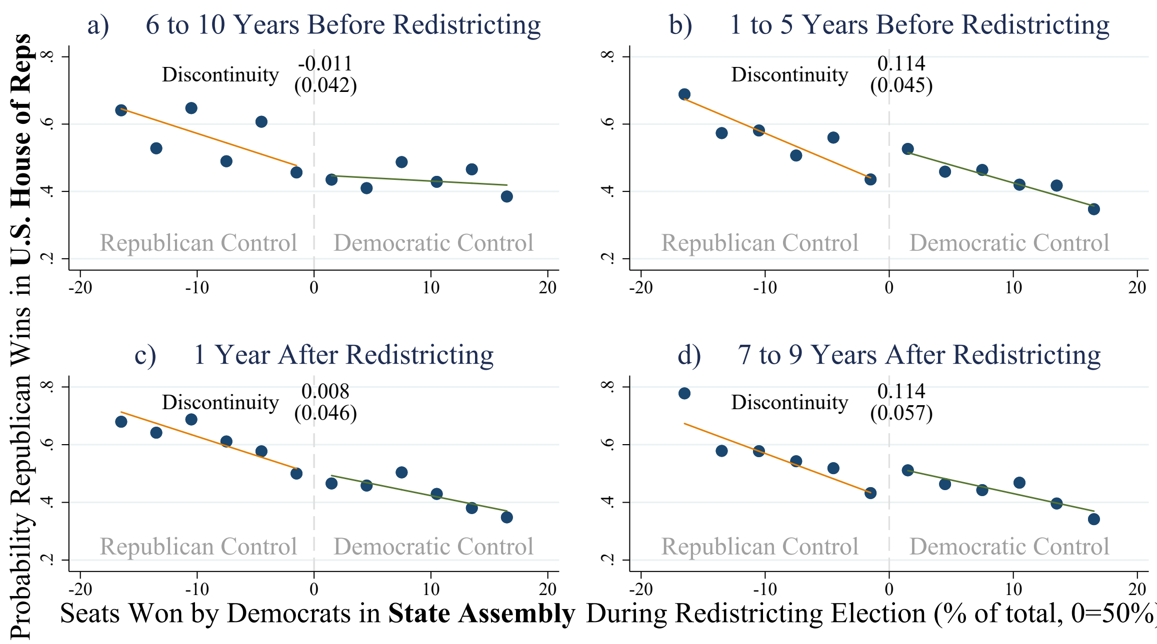
3.1 Visual Evidence

The reasoning behind our research design is most easily explained through a series of figures familiar from the literature on regression discontinuity design. In each panel of Figure 3 we plot on the horizontal axis the share of seats won by Democrats *in the state assembly during the redistricting election.*[7] We divide the range into bins of 3 percentage points and plot on the vertical axis the fraction of U.S. House races won by the Republican within each bin. All four figures have the same horizontal axis and differ only in the time frame of the U.S. House races plotted on the vertical axis.

Panels a and b are constructed using the outcomes of U.S. House races that occur *before*

[6]In many states the election is in years ending in 0, but a few states are irregular. We define the most recent election before a year ending in 1 as the redistricting election.
[7]The average number of U.S. House races per bin is 230, 338, 177, and 115 for each panel (clockwise).

Figure 3
The Selection Effect and Causal Effect are Visible in the Data



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

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

Panel a, which focuses on U.S. House races many years before redistricting, shows no evidence of a discontinuity. But there is a large discontinuity in Panel b, races just before redistricting. We estimate the size of the discontinuity using a local linear regression within a bandwidth of 18, though the results are similar at other bandwidths (see below). States where Democrats subsequently win a narrow majority in the state assembly were 11 percentage points more favorable to Republican U.S. House candidates before redistricting. Since these elections cannot be affected by (future) redistricting and cannot arise naturally, the figure suggests parties have taken conscious actions to "sort" desired states onto the winning side of the cutoff. Democrats (Republicans) barely win enough seats to hold a majority in state assemblies where Republicans won (lost) relatively more seats in U.S. House elections 1 to 5 years before the redistricting election. Since this sorting does not arise by chance, it is the first clear evidence of what we call the Selection Effect.

Panels c and d are constructed using U.S. House elections that occur *after* the redistricting election, meaning these outcomes may have been affected by redistricting (what we call the Causal Effect of redistricting), but they also have been selected as discussed in the previous paragraph. The key to separating these effects is to recall that the running variable is the same across all of these figures. The states represented by the "dot" just to the right of the discontinuity in Panel c are the same as those represented by the dot in that position in Panel b. Any change in the height of the dot between these figures is caused by a change in election outcomes in these same states from before to after redistricting.

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

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

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

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

3.2 Feasibility of Precise Control

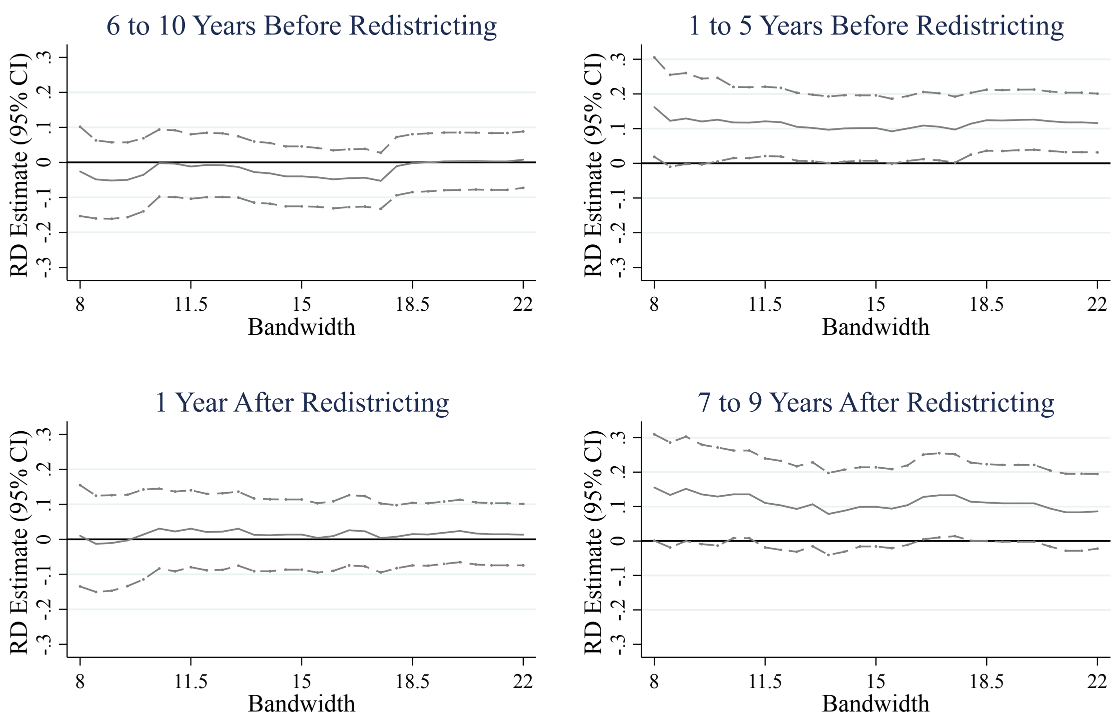
Panel b of Figure 3 suggests parties are somehow able to systematically win the state-level redistricting election in states where they have sustained recent losses in the U.S. House. Precise control, sometimes called "precise sorting" or "complete manipulation," arises when an agent has both a means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

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

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

Jeong and Shenoy (2020) show direct evidence of precise control by testing for a discontinuity in the probability density of the share of seats won by the majority party at the 50% cutoff. They find that the majority party is roughly 4 times as likely to barely win than to lose a close election. They show that the discontinuity appears only in redistricting elections and is not present in regular elections, suggesting it is not a mechanical consequence of discreteness in the running variable. Makse (2014) has shown that parties actively change their tactics in anticipation of redistricting elections. They switch from the "seat-maximizing" tactics used in typical state elections to "defensive" or "majority-seeking" tactics in redistricting elections.

Figure 4
Figure 3 is Not Driven by Choice of Bandwidth



Whatever the mechanism, Figure 4 suggests it is not a fluke. Since a discontinuity in a pre-determined outcome should not arise through any natural phenomenon, it must arise through the conscious actions of the political parties. As a consequence it creates non-random differences in states on either side of the 50% cutoff for control of the state assembly. These differences may be informative about where the actions of political parties leave them in control of redistricting, and—as we now explain—must be controlled for to estimate the Causal Effect of partisan gerrymandering.

4 Research Design

4.1 Regression Equations

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

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

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

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

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

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

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

Let $\boldsymbol{C}$ be a row vector of controls. We estimate the Selection Effect by running the regression

$$W_{ist} = \sum_{k=-9,-7,\ldots,-1} \mathbb{I}(t=k)\Big\{\pi_{t0} + \pi_{t1}X_s + \pi_{t2}X_sR_s + \rho_t R_s\Big\} + \boldsymbol{C}_{ist}\boldsymbol{\pi}_3 + \nu_{ist} \tag{2}$$
$$\text{for } |X_s| < h, \quad t = \{-9,-7,\ldots,-1\}$$

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

Next we turn to estimating the Causal Effect of Redistricting:

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

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

Consider a new assumption, which we test below: the Selection Effect does not change from before to after redistricting. Though our estimates of the Causal Effect are valid *regardless of whether there is precise control and a non-zero Selection Effect*, we do require that the Selection Effect is constant. Under this assumption we can difference away the Selection Effect by subtracting $\rho_{k=-1}$, our regression discontinuity estimate from the election before redistricting (Panel b of Figure 3), from $\rho_{k>0}$ for any election k after redistricting.

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

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

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

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

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

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

The choice of bandwidth h is complicated because the panel specifications of Equations 2–4 simultaneously estimate several regression discontinuities. We make a reasonable choice of bandwidth (guided by standard methods of bandwidth selection) and show that the results are similar using other choices.[8] In our baseline specifications we choose a bandwidth of 18, which yields conservative estimates. We show in the results section that the main result is similar for a range of choices from 6 to 22, and the estimates lie within each other's confidence intervals. We also show in Online Appendix C.2 that other results in the main text are not sensitive to the choice of bandwidth. In all specifications we cluster the standard errors by state-redistricting event s to account for both state-level shocks and the cross-time correlation in the error term.

4.2 Data

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

This dataset is merged to data on the outcomes of individual races for the U.S. House. We combine the data from the Inter-university Consortium for Political and Social Research

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

(1995), which covers 1964 through 1990, with data from Kollman et al. (2016), which covers 1991 through 2012.[9] We measure racial gerrymandering using tract-level census data (Minnesota Population Center, 2011) merged to Congressional district boundaries Lewis et al. (2013). We assign each tract to whichever pre- and post-redistricting district that contains its centroid. In Online Appendix D we give more details and report descriptive statistics for the data.

4.3 Testing the Identifying Assumption

Our difference-in-discontinuities estimator is valid only if everything else that might determine the outcomes of U.S. House elections before redistricting remains roughly unchanged after redistricting. (This can be relaxed to allow for a smooth trend, though we show in the results section doing so does not change the results.) The most obvious concern is mean reversion. States were sorted to the left of the threshold because they had become less favorable to Republicans just before redistricting. If they subsequently revert back to the mean just after redistricting, it would falsely appear that the pre-existing discontinuity is closed by redistricting. Such mean reversion would have to be implausibly fast to explain the change from Panel a to Panel b of Figure 3. Nevertheless we show in this section that there is no evidence of any change in the Selection Effect.

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

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

change in the distribution of votes.

Figure 5 is constructed analogously to Figure 3, the only difference being the outcome on the vertical axis: the Republican vote share rather than a dummy for whether the Republican won. Panel b of Figure 5 looks much like Panel b of Figure 3. There is a discontinuity in the Republican vote share, implying Democrats (Republicans) are systematically winning bare majorities in the lower houses of states where Republican candidates for the U.S. House win a larger (smaller) share of the vote. But Panel c of Figure 5 shows that this discontinuity in vote shares remains even after redistricting. That is in stark contrast to Panel c of Figure 3, which shows the discontinuity in the probability of a Republican win vanishes after redistricting. This pattern suggests that states selected because they cast many votes for Republicans do not suddenly stop voting for Republicans.[10]

Then by the argument above, the only way for the discontinuity shown in Figure 3.b to vanish immediately after redistricting is for the distribution of votes to abruptly become less favorable to the opposition party. Barring a massive and sudden migration across district lines (which did not occur in the U.S. during our sample), such a change is only possible if district boundaries are changed in a way that hurts the opposition party.

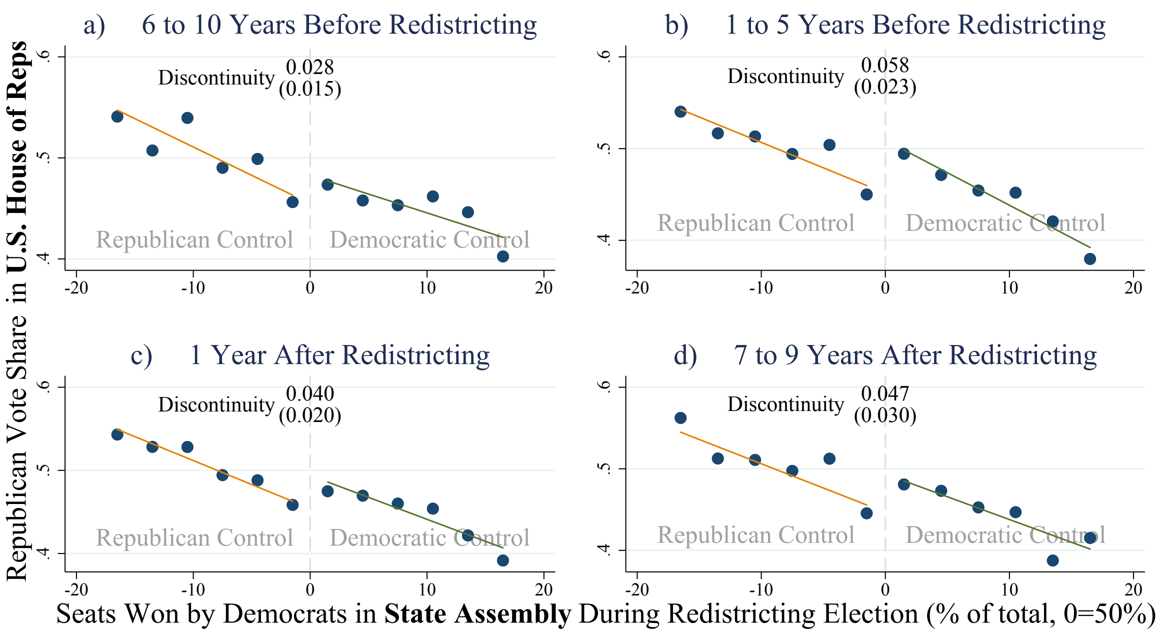
5 Main Results

5.1 Selection Effect

Table 1 shows the results from estimating Equation 2. The table shows the estimated discontinuity in the probability the Republican wins a U.S. House race *before redistricting*. Since redistricting has not yet happened, these estimates are suggestive of whether parties select to control redistricting based on outcomes in Year t. The last three columns estimate the same parameters controlling for year and state-redistricting event fixed effects. In the spec-

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

Figure 5
The Selection Effect on Vote Shares Does Not Vanish after Redistricting



Note: This figure is identical to Figure 3 except that the outcome is the Republican vote share rather than a dummy for whether the Republican won. Each dot shows the average of the outcome within a bin of width 3. Standard errors are clustered by state-redistricting event.

ifications that control for event fixed-effects the discontinuity 9 years before redistricting is the excluded category (meaning all other estimates give the size of the discontinuity in year $t > -9$ relative to the discontinuity at $t = -9$).

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

After the Selection Effect becomes positive it remains constant. The row labeled "Test: …" tests for whether we can reject that the discontinuities at $t = -5, -3, -1$ are all of the same size. In no specification can we reject that the Selection Effect remains constant after $t = -5$. If the Selection Effect is constant, a difference-in-discontinuities estimator will give

Table 1
Estimates of the Selection Effect

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

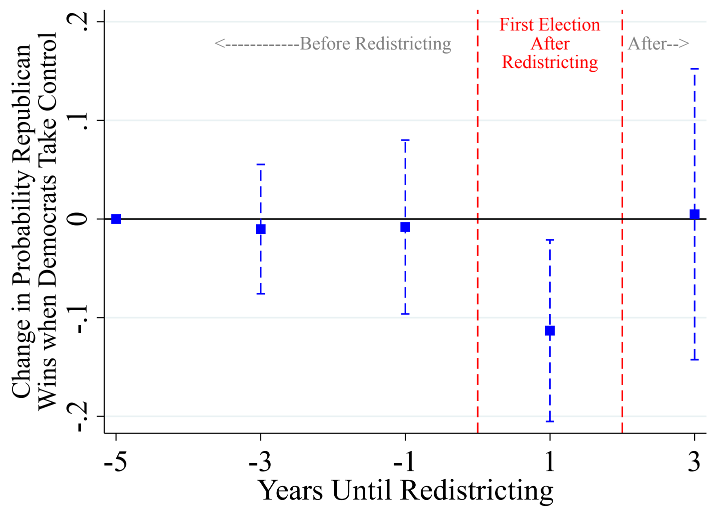
Note: We estimate Equation 2. Each row gives the estimated discontinuity in U.S. House elections held some years *before* redistricting ($\hat{\rho}_t$ in Equation 2). The values reported in the row labeled "Test..." are p-values of the test for equality of the discontinuities in the elections 5, 3 and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

consistent estimates of the Causal Effect of partisan redistricting. That logic would fail if the effect is only constant until after redistricting, but the prior section shows that the electoral bias of states, as measured by the Republican vote share, does not change after redistricting. That suggests our assumption of a constant Selection Effect is not unreasonable.

5.2 Causal Effect

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

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



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

controlled for the Selection Effect.

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

Figure 6 implies that the Causal Effect appears only in the year immediately after redistricting. By imposing this restriction, Equation 4 maximizes the power of our estimates. Panel A of Table 2 shows both the baseline estimates and those that arise after controlling for different fixed-effects. Columns 1 through 4 show that controlling for state-redistricting event fixed-effects and year fixed-effects barely changes the estimates. In Columns 5 and 6

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

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***	-0.111***	-0.112***	-0.105***	-0.110**	-0.096***
	(0.036)	(0.039)	(0.035)	(0.037)	(0.043)	(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***	-0.093**	-0.097**	0.096***	-0.107***	-0.015
	(0.036)	(0.036)	(0.038)	(0.035)	(0.036)	(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***	-0.108***	-0.098**	-0.128**	-0.181**
	(0.035)	(0.036)	(0.040)	(0.049)	(0.073)
Observations	6812	6541	5513	3766	2539
Events	149	135	111	82	48

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

we also allow for a linear trend in the Selection Effect. If our assumption of a constant Selection Effect is invalid, this trend might absorb some of the bias and shrink our estimates. But the estimates in Columns 5 and 6 are largely unchanged, suggesting our assumption is not unreasonable.

Panel B shows the results of several specification tests. Column 2 shows that the estimate is little changed by discarding state-redistricting events where independent legislators won seats in the assembly. Column 3 shows that the results are not sensitive to excluding the so-called pre-clearance states. During our sample these states were required to submit changes to their voting rules for pre-clearance to the U.S. Department of Justice (as per Section 5 of the 1965 Voting Rights Act).[11] Column 3 shows that they are not driving our results. Column 4 shows that changing the running variable from the Democrats' margin to the Republicans' margin of seats won in the assembly gives an estimate of similar magnitude and opposite sign, as expected. Column 5 shows that dropping U.S. House elections in off-years does not change the results. Finally, in Column 6 we report the results of a placebo test. We take as the running variable not the margin won by Democrats in the redistricting election, but in the state election before that. The party that wins this earlier election has no power over redistricting. As expected, the placebo estimates in Column 6 are small (roughly one-seventh the size of our actual estimates) and statistically insignificant.

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

6 Mechanism: Is It Really Caused by Partisan Redistricting?

6.1 The Conversion Rate Turns Against the Opposition After Redistricting

A redistricting plan is favorable to Republicans if, holding their share of statewide votes fixed, it yields a larger share of the state's U.S. House seats. Gelman and King (1994b) measure the "responsiveness" of a redistricting plan to swings in vote shares using simulations. Our difference-in-discontinuities approach lets us measure responsiveness directly. Let V^R_{st} be the share of votes won and W_{st} the fraction of seats won in election t during state-redistricting

[11] These are Alabama, Alaska, Arizona, Georgia, Louisiana, Mississippi, South Carolina, Texas, and Virginia.

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

We apply a state-level version of Equation 4 to the statewide U.S. House Republican vote share, the fraction of seats won, and the conversion rate. The left-hand panel of Figure 7 shows that there is no statistically significant effect on the Republican vote share. This estimate is not surprising given that Figure 5 shows the Selection Effect in the Republican vote share does not change after redistricting. Since the Selection Effect is stable over the redistricting cycle, the difference in discontinuity estimates will be uniformly zero.[12] By contrast, the center panel of Figure 7 shows that there is a large and statistically significant decrease in the share of U.S. House seats won by Republicans when Democrats control the assembly during redistricting. This result is simply the state-level analog of Figure 6.

The right-hand panel shows the effect on the conversion rate. It is unchanged until after redistricting, when it turns against the opposition party with a point estimate of roughly -0.36. The point estimate implies that if Republicans hypothetically won half the votes in a state, they would win 59 percent of the seats under the redistricting plan drawn by a Republican assembly, but only 41 percent of the seats under the plan drawn by a Democratic assembly.[13] 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.

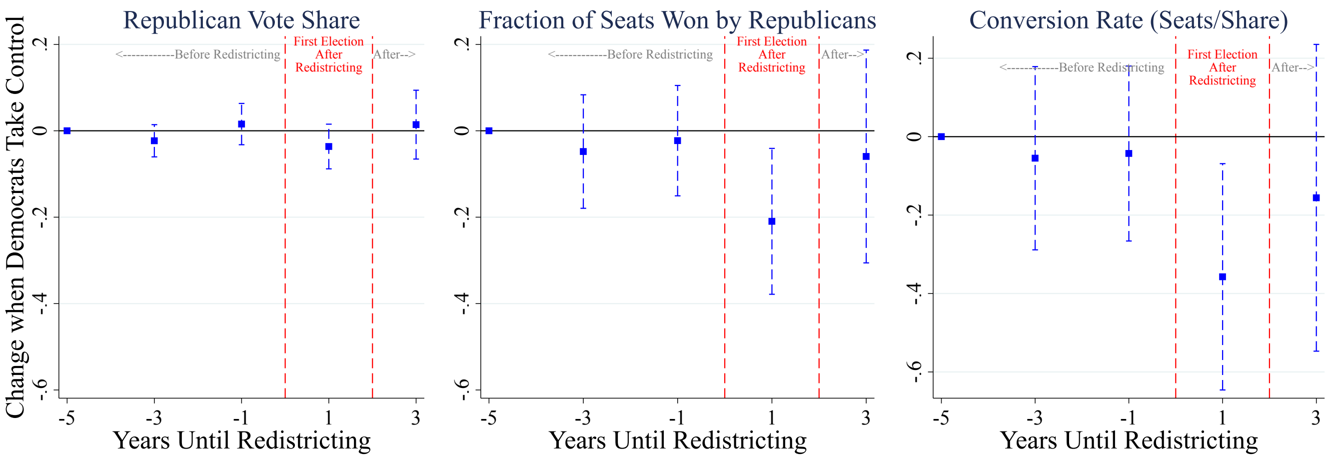
6.2 African Americans are Packed under Republican Redistricting

The clearest sign of partisan redistricting is a systematic difference how each party treats the other party's voters. Of the data sources that might guide their gerrymandering, the decennial census is the most comprehensive. The census reports population counts of each

[12] Though Figure 5 takes individual House races rather than the total statewide vote share as the unit of analysis, in Online Appendix C.1.1 we show that the results for the statewide vote share are similar. It may seem surprising that packing opposition voters into uncompetitive districts would not deter them from the polls. But until recently, redistricting was not salient to voters. It is possible it may have had little impact on their decision to vote.

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

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



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

racial and ethnic group within each census tract. If race is informative about how someone will vote, the party in power might redistrict voters of races that support the opposition 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 Democratic for the U.S. House—support comparable to that of registered Democrats (92 percent).[14] Since an African American is likely to support Democrats, Republicans may try to redistrict African Americans to minimize their influence.[15]

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

[14] According to CNN (2016), whose data are based on National Election Pool exit polls.
[15] The ideal test would be to look at actual registered Democrats and registered Republicans. But we do not have historical data on the number of registered Republicans and Democrats by precinct or census tract.

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**	-0.027	-0.230***	0.006	0.012
	(0.063)	(0.050)	(0.054)	(0.018)	(0.018)
Observations	19478	185111	2842	1550	19614
Events	117	138	41	137	116

Note: Columns 1 and 2 estimate the discontinuity (using a local linear regression) at the level of the census tract on a dummy for whether the tract is "moved" during redistricting (see text for definition). Column 1 restricts to tracts that are majority black; Column 2 restricts to all other tracts. Column 3 estimates a similar specification on the subset of majority black tracts that are moved. The outcome is a dummy for whether the tract is moved into a district whose population is more than 75 percent black. Column 4 tests for a discontinuity in the black population share of districts on either side of the cutoff. For Column 5 we assign majority black census tracts the absolute percentage difference between the population of the district it is located in and the median district in the state. We test for a discontinuity in this deviation. All standard errors are clustered by state-redistricting event. Visual representations of these specifications are in Online Appendix C.1.2.

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.[16]

Conditional on moving African Americans, are Republicans more likely than Democrats to move them into districts that minimize their influence? One way to minimize their influence would be to "pack" them into districts in which they form the overwhelming majority. Though these few districts are lost with certainty, the number of contests in which the unfriendly voters may be pivotal is minimized.[17] 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 overwhelm-

[16]Visual representations of all tests described in this section can be found in Online Appendix C.1.2.

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

ing (more than 75 percent) majority. The estimate suggests a large decrease in packing when Republicans lose control of redistricting.[18]

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 the results section because our measures of how African Americans are moved during redistricting are by construction undefined before redistricting. Instead we test for more basic differences in the demographics of districts in states on either side of the cutoff. Our aim is to test whether there is any difference in the objective need to redistrict African Americans.

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

Though African Americans may comprise a similar portion of the total population near the threshold, is it possible that they are distributed less evenly than the rest of the population? For example, if migration patterns differ across the threshold, it is possible that in Republican-controlled states African Americans have segregated themselves into heavily over- or under-populated districts. These districts would have to be broken up during redistricting. To test this hypothesis we compute the absolute percentage deviation of the population of each district from the median of all districts in the state prior to redistricting. We assign the district's population deviation to each tract within it. We then test whether majority-black census tracts have higher district deviations on one side of the cutoff. Column 5 of Table 3 suggests there is no difference at the cutoff, implying African Americans are no more likely to live in malapportioned districts in Republican-controlled states.

[18]The regression in Column 3 of Table 3 and those used to construct Figure 15.B narrow the bandwidth to 10 because there is essentially no racial "packing" further away from the discontinuity.

7 Discussion and Conclusion

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

In Online Appendix A we argue that the effect of partisan redistricting is short-lived because there are swings in the electorate that undermine a favorable redistricting plan. Drawing a favorable district requires an accurate prediction of who votes and how they vote. But even if it is possible to predict how a district will vote in the near future, any prediction will become meaningless over the ten-year lifespan of a district map. We calculate that the standard deviation of the aggregate swing—the change between elections in each state's average Republican vote share—is roughly 6.7 percentage points, implying a one-standard deviation shock is all it takes to change a comfortable 10-point Republican margin to a narrow win for Democrats. The idiosyncratic component—the swing in a district's Republican share between elections after controlling for the state-wide swing—is even larger. A set of districts gerrymandered to give 10-point margins to Republican candidates could, in the next election, become a catastrophic wave of defeats.

That said, we provide some evidence in Online Appendix A that in more recent years gerrymandering has become more persistent. It is possible that new technology allows more accurate predictions of how people vote. The pattern may suggest gerrymandering will have larger and more persistent effects in the long run.

7.2 Summary

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

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