Political Capture: The Case of U.S. Congressional Redistricting

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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,D73,J11)

Preliminary and incomplete.
Comments and suggestions welcome.

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1 Introduction

A competition is most easily won when the rules are bent in one’s favor. When these rules govern competition in the marketplace, the process by which they are suborned is called regulatory capture. The rules that govern competition in politics are no less crucial, and the consequences of their subversion are no less costly. Besley et al. (2010), for example, find that a reduction in political competition causes U.S. states to adopt policies that slow economic growth.\(^1\) It is often assumed that political capture, though common in developing countries, is only a marginal problem in mature democracies. Robust political institutions are supposed to make it difficult for politicians to capture the institutions that set the rules of an election.

We show that even in a democracy as mature as the United States, political parties are remarkably successful in capturing a key institution that governs political competition: Congressional redistricting. The U.S. Supreme Court mandates that Congressional districts be redrawn every ten years to ensure all districts contain the same number of people. The boundaries of a district determine how many left- or right-leaning voters a candidate will face, potentially swaying an election. But in most states these boundaries are drawn by state legislatures controlled by whichever party holds a majority. This practice gives political parties a means by which they can capture the redistricting process, potentially swaying the outcomes of future U.S. House elections.

Partisan redistricting, or gerrymandering, has gotten much attention from the popular press and the courts.\(^2\) Yet academics remain divided over where parties seek to capture redistricting and whether they actually use it to their advantage. Answering these questions is made difficult by the usual problem of omitted variables. If Republicans win majorities in conservative state legislatures during redistricting, is it because they actively sought to capture redistricting in these states? Or are such states simply more favorable to Republican control? Estimating the effect of partisan redistricting suffers from the same

\(^1\)Many other studies find that societies without political competition suffer worse outcomes in both the economy and in government—Acemoglu et al. (2014), Bernecker (2014), De Paola and Scoppa (2011), Galasso and Nannicini (2011), and Ashworth et al. (2014) are just a few examples.

\(^2\)See the (as of this writing) recent editorial in the New York Times (May 30, 2017) and the Supreme Court decision on North Carolina’s Congressional districts (Cooper v. Harris, 2017).
problem of omitted variables, but is further complicated by each party’s choice of where to control redistricting. For example, a party may choose to capture redistricting in states where it has sustained recent gains or losses in House elections. 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—the difference in U.S. House election outcomes in states where Democrats versus Republicans choose to capture redistricting—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 redistricting. 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 election just before redistricting (call this the “redistricting election”) is just above the cutoff.

As we show in a companion paper (Jeong and Shenoy, 2017) the party that holds a majority before the redistricting election is able to ensure it holds precisely enough seats to retain its majority—a phenomenon the literature on bunching and regression discontinuity design calls “precise control.” In the absence of precise control any omitted 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.

We apply this approach to estimate the Selection Effect. Any discontinuity in a predetermined outcome must be induced by the actions of political parties. An outcome that jumps at the cutoff where Democrats win control of the legislature must make it either easier or more attractive for Democrats to exert precise control. By testing how the outcomes of past U.S. House elections differ on either side of the cutoff we can infer whether parties aim to control redistricting in places where they have sustained recent gains or losses. We can then net out the Selection Effect from the outcomes of U.S. House elections that come after redistricting to measure the Causal Effect. We show that a difference-in-discontinuities estimator can control for both omitted variables and the Se-
lection Effect, yielding a consistent estimate of the Causal Effect.

We find a strong pro-opposition Selection Effect. 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. We estimate that 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 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. We also 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. Republican legislatures are roughly 15 percentage points more likely to move African Americans, who vote overwhelmingly for Democrats, to new districts. There is no difference in the treatment of other groups. Conditional on being moved, African Americans are more likely to be redistricted by Republicans in a way that minimizes their influence. This difference in treatment is not obviously explained by any special need to redistrict African Americans 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.

Finally, we consider the question of why the effects of partisan redistricting are so short-lived. We run simple simulations to show that, given how much each party’s vote share tends to vary between elections, partisan redistricting should not be expected to have persistent effects. Yet this logic implies that any improvement in the ability to predict how a district will vote would 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 persistent in these more recent redistricting cycles. This evidence, though hardly definitive, is at least consistent with our explanation.

This paper makes two contributions, one specific and one general. Our specific contribution is to the vast and somewhat divided literature on partisan redistricting in the U.S. To our knowledge this is the first paper to measure both the Selection Effect and the Causal Effect of partisan redistricting. Our results suggest that failing to account for the Selection Effect would yield highly misleading estimates of the Causal Effect.

We are able to estimate these effects by bringing a novel research design to this literature. To our knowledge, no prior work has used a regression discontinuity design or a difference-in-discontinuities estimator to estimate the effect of partisan redistricting. To our knowledge, we are also the first to test for direct evidence of gerrymandering using census data. Aside from letting us directly test for racial gerrymandering, the census data let us infer the intent of gerrymandering by measuring which voters were moved to new districts during redistricting.

Our more general contribution is to the empirical literature on elite capture of democracies. This literature has found that incumbents redirect spending and public goods to maximize their chances of reelection. But it has also found that these tricks are less likely to work in the context of mature democracies (Brender and Drazen, 2008), in the presence of independent mass media (Akhmedov and Zhuravskaya, 2004), and on an electorate that is relatively educated (Akhmedov and Zhuravskaya, 2004) or has participated in reasoned debate (Fujiwara and Wantchekon, 2013).

Our results suggest that even in a mature democracy, political parties can find and exploit opportunities to capture institutions that set the rules of elections. Nevertheless, we find that the benefits of such capture may in some circumstances be short-lived in dynamic societies with a changing electorate. Whether those circumstances will continue to hold in U.S. elections remains uncertain.
1.1 Related Literature

This paper most directly contributes to a vast empirical literature on partisan gerrymandering in the U.S.\(^3\) The literature 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). Another branch 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). The most recent wave of work compares the actual plan to simulated non-partisan plans (e.g. McCarty et al., 2009; Chen and Rodden, 2013; Chen, 2016). Studies of more recent elections have found some evidence that plans drawn by a party will favor it, but have concluded that the overall effect is small. But any simulation approach works only to the extent that the simulated outcome is an accurate reflection of the true counterfactual. By holding voter behavior fixed, it cannot allow for a change in the composition or the behavior of the electorate.

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.

\(^3\)There 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.
This paper is also related to theoretical work 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, this paper contributes to the literature on how leaders use their office to hang onto power. This literature has found that incumbents will increase spending in election years (Nordhaus, 1975; Drazen and Eslava, 2010); allocate jobs, public goods or popular reforms to swing districts (Folke et al., 2011; Bhardhan and Mookherjee, 2006; Baskaran et al., 2015; Nagavarapu and Sekhri, 2014); or exploit the control of one level of government to increase the odds of winning at another (Curto-Grau et al., 2011). These studies have found such abuse of power to be effective.

But as noted earlier, other work has shown that the attempt may fail or even backfire in mature democracies and in the presence of independent institutions (Peltzman, 1992; Akhmedov and Zhuravskaya, 2004; Brender and Drazen, 2008; Matsusaka, 2009; Fujiwara and Wantchekon, 2013). Other work has shown that voters themselves will respond to attempts to subvert democratic institutions, such as the media (Durante and Knight, 2012). Our work suggests it is possible for politicians to capture the institutions that govern elections even in mature democracies.

2 Background

4 Meanwhile, Shotts (2001) and Shotts (2002) study how much less optimal the optimal gerrymander becomes when the party in power is forced to create majority-minority districts.

5 There is also a literature on socially optimal redistricting (effectively the opposite of gerrymandering). Examples include Coate and Knight (2007), Bracco (2013), and Puppe and Tasnádi (2015).
2.1 What is Redistricting and Why is it Worth Controlling?

Partisan redistricting, or “gerrymandering,” is at least as old as the Republic. Its 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 political opponents (Weber, 1988).\textsuperscript{6} 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.”

Why would a political party want to control Congressional Redistricting? Consider a state that contains 6 likely Democratic voters and 3 likely Republican voters. These voters must be divided evenly into 3 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 would prefer Plan 2.

Enacting a favorable plan often requires drawing longer and more contorted boundaries to enclose the same area of land. One measure of such contortion is the ratio of the perimeter to the square root of the area. (Taking the square root ensures both numerator and denominator have comparable units.) Figure 1.A shows several examples of this perimeter-area ratio. A square has a perimeter-area ratio of 4. A relatively simple district, like the rectangular Kansas 3rd district, has a perimeter-area ratio of 4.9—not much more complex. But the Texas 18th district, with its irregular lines and gaps, has a ratio of over 36.

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. Only after the Supreme Court ruled in \textit{Baker v. Carr 369} (1962) and \textit{Wesberry v. Sanders 376} (1964) that their failure to redistrict was unconstitutional did states start redistricting regularly. After the ruling nearly all states that were apportioned more than one Congressional

\footnote{The district was drawn by the Anti-Federalists, led by Patrick Henry, as punishment for Madison’s defense of the Constitution. Henry’s efforts were in vain; Madison won his election.}
district started redrawing their districts in the year after the decennial census.\textsuperscript{7}

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.\textsuperscript{8} Though these contortions do not prove there is gerrymandering, it is clear that the party in power can and does make major revisions in each redistricting. It is hard to imagine this party would not be tempted to skew the revisions in its favor.

### 2.2 When and How is Redistricting Done?

Most states redraw their Congressional boundaries by passing them into law. The state legislature, which comprises a lower house and upper house (anal-

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

\textsuperscript{8}The election after the 1990 census is an exception. This may be because in that year states tried to make their districts more compact, or it may be a shortcoming of the perimeter-area ratio as a measure of contortion.
gous to the U.S. House and Senate), 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.\(^9\)

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. 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.\(^{10}\) 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 the 2011 redistricting cycle—the only one for which we have consistent roll call votes—we divide the running variable into 5 percentage point bins.\(^{11}\) We plot the average fraction of Democrats and Republicans that vote in favor of the redistricting bill. When Democrats gain control of the assembly they switch from near universal opposition to near universal support for the redistricting bill. The response of the Republicans, though slightly less extreme, is stark nevertheless. 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 arises every ten years with the decennial census. Aside

\(^9\)For example, on 23 February 2001 Bob Hertzberg introduced “An act to add Chapter 5 (commencing with Section 21040) to Division 21 of the Elections Code, relating to redistricting” to the California House State Assembly. 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 Davis to sign into law. In this case the bill ultimately died in committee.

\(^{10}\)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.

\(^{11}\)These data were constructed from Vote Smart (2016), which has roll call votes on 51 bills from 21 states for the 2011 redistricting cycle.
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.

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.\footnote{The redistricting bill may not be passed in the year ending in 1 if, for example, the legislature is divided and the bill is particularly contentious. As a result, the date of passage is both unpredictable and endogenous to our outcome of interest. Instead we focus on the opportunity to redistrict, which comes with the completion of the census.} Whichever party wins the election to the state legislature just before this year has the opportunity to pass its own redistricting plan.\footnote{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.} 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 falls just above the 50 percent cutoff. But do they 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 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. As we show in Jeong and Shenoy (2017), the party

Note: The figure shows the redistricting cycle for a typical state (i.e. a state with lower house elections in even years).
that holds a majority 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 both increasing the total campaign funds available to state elections, and by switching to what political scientists call “defensive” or “majority-seeking” tactics (Makse, 2014). It is willing to do so when the outcome of the election determines which party controls the assembly during Congressional redistricting—what we have labeled “redistricting elections.”

3 Research Design

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

Let $s$ index a state-redistricting event—for example, California during the 1981 redistricting. Each $s$ has a partisan drift towards the Republicans equal to $\theta_s$, which has an absolutely continuous distribution $H^\theta$. Suppose that in the absence of precise control the margin of seats in the state assembly won by Democrats during the redistricting election for state-redistricting cycle $s$ is

$$X^*_s = x(\theta_s) + v_s$$

where $x$ is a continuously differentiable function and $v_s$ a shock with an absolutely continuous distribution $F$ and density $f$.

Democrats have the chance to exert precise control over half of all elections, and Republicans may control the rest. Conditional on having the chance to exert control, each party has the resources to control only a fraction $\mu$. If Democrats exert control, the actual outcome of the election equals a random variable $\tilde{X}_s$ with density $\chi$. We assume $\chi(\tilde{X}_s) = 0$ for $\tilde{X}_s < 0$, meaning Democrats do not lose elections they control, $\chi(0) > 0$, 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 $-\tilde{X}_s$.

Assume both parties choose to control elections based on the partisan drift of the state. Democrats want to control elections where $\theta \in \Theta_D$ and Republicans the set $\theta \in \Theta_R$. By construction, $H^\theta(\Theta_D) = H^\theta(\Theta_R) = \mu$. Then the actual margin
of seats won by Democrats is

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

(2)

As we show in Appendix A.1, conditional on observing the outcome of a state legislative election the expected partisan drift is

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

(3)

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

Suppose the probability that Republicans win Congressional seat \(i\) during the election in year \(t\) in the state-redistricting cycle \(s\) equals

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

(4)

where \(w\) is a continuous function whose properties are discussed below 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 \(\beta^t\) is the causal effect in year \(t\) of Democratic control of the legislature during redistricting. The term \(\theta_s w(t)\) reflects the time-varying partisan bias of each district.

Define the regression discontinuity estimate for year \(t\) as

\[
\omega_t = \lim_{\varepsilon \to 0} \left\{ E[W_{ist} | X_s = \varepsilon] - E[W_{ist} | X_s = -\varepsilon] \right\}
\]

\[
= \lim_{\varepsilon \to 0} \left[ a(\varepsilon) - a(-\varepsilon) + b(\varepsilon)E[\theta_s | \theta_s \in \Theta_D] - b(-\varepsilon)E[\theta_s | \theta_s \in \Theta_R] \right] w(t) + \beta^t \cdot \mathbb{I}(t > 0)
\]

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

(5)

The first term in Equation 5 is the Selection Effect, which is informative about

\(^{14}\)b(X_s) is actually the probability the outcome \(X_s\) has been controlled.
where Democrats versus Republicans seek to control redistricting. The second term is the *Causal Effect* of redistricting.

Then we can estimate the Selection Effect using

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

which is simply the regression discontinuity estimate in years before redistricting. For any function \(w(t)\) a time-varying regression discontinuity estimator gives a valid estimate of the Selection Effect. In Section 5 we let \(w(t)\) be a constant, a linear trend, and a fully flexible set of time dummies.

The Selection Effect measures how favorable towards Republicans are the states where Democrats choose to take control (or equivalently, how favorable towards Democrats are the states where Republicans choose to take control). It is informative about whether parties capture redistricting in states where their opponents are strong or weak. If the estimate of Equation 6 is positive there is pro-opposition selection. If it is negative there is anti-opposition selection.

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

\[
\omega_{t}^{\Delta} = \omega_{t} - \omega_{k}
\]

Suppose that \(w(t)\) is approximately constant in the neighborhood \(t \leq t < \bar{t}\) for negative \(\bar{t}\) and positive \(\bar{t}\), which is what our estimates in of the Selection Effect suggest. Then \(\omega_{t}^{\Delta}\) is a consistent estimate of the Causal Effect of redistricting \(\beta^t\) for \(t \leq k < 0 < t < \bar{t}\). If \(w(t)\) is instead a parametric function of \(t\)—for example, a linear trend—we can control for the trend in the discontinuity to recover \(\beta^t\).

As with the Selection Effect, a positive estimate implies a pro-opposition Causal Effect while a negative estimate implies an anti-opposition Causal Effect. Unlike in the case of the Selection Effect, which could conceivably be either pro- or anti-opposition, it is hard to imagine the Causal Effect could be anything other than anti-opposition. When a party takes control of redistricting it is surely with the aim of helping itself and hurting its opponent.
3.2 Regression Equations

Define the margin of seats won by Democrats as

$$X_s = \frac{[Democrats \ in \ State \ Assembly]_s - \frac{1}{2}[Total \ Assembly \ Members]_s}{[Total \ Assembly \ Members]_s} \times 100\%$$ \hspace{1cm} (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. This ensures that the margin turns non-negative when the Democrats switch from losing to winning the election.\(^\text{15}\)

Let $t$ be the year of a U.S. House election relative to a redistricting event $s$. For example, if $s$ is the 1981 redistricting of California then the 1980 House election is $t = -1$ and the 1986 election is $t = 5$. If we fix $t = T$ we can estimate the regression discontinuity (5) by running a local linear regression

$$[Outcome]_{ist} = \gamma_0 + \gamma_1 X_s + \gamma_2 X_s \cdot I(X_s \geq 0) + \omega_T \cdot I(X_s \geq 0) + \nu_{ist} \hspace{1cm} (9)$$

for $|X_s| < h$

where $h$ is the bandwidth. For $t < 0$ the estimate $\hat{\omega}_t$ is informative about the Selection Effect, and comparing $\hat{\omega}_{-1}$ to $\hat{\omega}_1$ 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 an indicator for whether the Republican candidate for the House wins. We estimate the Selection Effect by running the regression

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

for $|X_s| < h, \ t = \{-9, -7, \ldots, -1\} \hspace{1cm} (10)$

under different assumptions about $w$. In Section 5.2 we allow $w$ to be a constant, a linear trend, and a full set of year dummies. The corresponding estimates $\hat{\omega}$ are informative about the Selection Effect.

\(^{15}\)In states where there is an even number of seats, a value of zero implies neither party has a majority. 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.
To estimate the Causal Effect we first estimate a flexible difference-in-discontinuities:

\[ W_{ist} = \sum_k \mathbb{I}(t = k) \left\{ \alpha_0^k + \alpha_1^k X_s + \alpha_2^k X_s \cdot \mathbb{I}(X_s \geq 0) + \omega_k^\Delta \cdot \mathbb{I}(X_s \geq 0) \right\} + \nu_{ist} \]  

(11)

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

The estimates of \(\{\omega_t^\Delta\}_{-5 < t < 0}\) 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 \(\hat{\omega}_{-3}^\Delta = \hat{\omega}_{-1}^\Delta = 0\). The estimates \(\{\hat{\omega}_t^\Delta\}_{0 < t \leq 3}\) equal the Causal Effects \(\{\beta_t\}_{0 < t \leq 3}\).

In Section 5.3 we find that only \(\hat{\omega}_{-1}^\Delta = \hat{\beta}_1\) is nonzero. In our primary specification we maximize the power of our estimate of \(\beta_1 = \beta\) by imposing that the other difference-in-discontinuity estimates are zero:

\[ W_{ist} = w_0 + X_s w_1 + X_s \cdot \mathbb{I}(X_s \geq 0) w_2 + \mathbb{I}(X_s \geq 0) w_3 + \mathbb{I}(t = 1) \cdot \left[ \alpha_4 + X_s \alpha_5 + X_s \cdot \mathbb{I}(X_s \geq 0) \alpha_6 + \beta \cdot \mathbb{I}(X_s \geq 0) \right] \]  

(12)

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

In our baseline specification we assume \(w\) 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 even for the simplest specification (9). The unit of observation is an individual U.S. House election whereas the running variable does not vary within \(s\). That may explain why different methods for selecting the “optimal bandwidth” disagree. Cross-validation (Ludwig et al., 2007; Imbens and Lemieux, 2008) suggests the optimal bandwidth lies between 15 and 20 percentage points, whereas the method of Calonico et al. (2014) prefers a bandwidth closer to 10.\(^{16}\) These complications are compounded in the panel specifications of Equations 10–12, which simultaneously estimates several regression discontinuities.

We opt instead to make a reasonable choice of bandwidth and show that the

\(^{16}\)The method of Imbens and Kalyanaraman (2011) favors a bandwidth close to 1, which leaves only a few dozen observations in the sample.
results are similar using other choices. In our baseline specifications we choose a bandwidth of 18, which yields conservative estimates (the estimates of our main result are larger and noisier for smaller choices). We show in Section 5.3 that the essence of the main result holds for a range of choices from 6 to 22, and the resulting estimates largely lie within one another’s confidence intervals. We also show in Appendix B.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 aggregate shocks and the cross-time correlation in the error term.

4 Data

We draw on data compiled by Klarner (2013b) on the number of Democrats, Republicans, and independents elected to the lower and upper 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 commission (as per 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 B.4 that the main results do not change if we drop Maine from the sample.

These data on the outcomes of state elections are merged to data on the

---


18 Alaska, Delaware, Vermont, and Wyoming are excluded. North Dakota is excluded after the 1972 reapportionment, Montana after the 1991 reapportionment, and South Dakota after the 1981 reapportionment.
outcomes of individual contests 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.\footnote{The ICPSR’s dataset includes the vast majority of House elections but, like any dataset, is incomplete. However, it also contains several elections 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 elections. 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 B).}

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 every 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; thus the results in Section 6.1 use only elections after 1980.

5 Main Results: How Does Partisan Redistricting Affect Election Outcomes?

5.1 Evolution of Outcomes

Before estimating our main specifications we present a visual summary of the results. We first 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.\footnote{To be precise, we take \[ \text{Outcome}_s = \frac{1}{4} \sum_{t=-7}^{t=-1} \left( \sum_i W_{i,t} - \sum_i W_{i,s,t-2} \right). \]}

This estimate gives a rough sketch of the Selection Effect. The left-hand panel of Figure 4 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.
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. 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. This result is consistent with the study by Makse (2014), who shows that political parties who hold a majority choose defensive strategies in states where the partisan drift is moving against them.

But these changes are partially reversed after redistricting. The right-hand panel of Figure 4 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. This figure is the mirror image of the right-hand panel, suggesting the fortunes of the Republicans turn sharply negative in states where Democrats control the assembly. This figure, which gives a rough estimate of the Selection Effect (left-hand panel) alongside the Causal Effect (right-hand panel), 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 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 5 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

\[ \text{Outcome}_s = \sum_i W_{i,s,1} - \sum_i W_{i,s,-1}. \]
Figure 4
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.

though noisy, the pattern suggests there is no discontinuity in Senate outcomes at the cutoff, making it less likely the results of Figure 4 are biased or the result of mean reversion.

The state-level changes, though a useful summary, obscure the timing of the Selection and Causal Effects. The timing is clearer when we study levels rather than changes and take individual contests as the unit of analysis. We estimate Equation 9 taking a dummy for whether the Republican won as the outcome. 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 6 shows the discontinuity—or lack thereof—in elections 6 to 10 years before redistricting (we now include all elections, including special elections). Though there is some evidence that 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 elections just 1 to 5 years before redistricting (the top-right panel) there is a large and statistically significant discontinuity. In states con-
Figure 5
Placebo 1: There is Neither Selection nor Causal Effect on Senate Outcomes

![Figure 5](image)

Note: This figure is analogous to Figure 4, but using the outcomes of U.S. Senate elections rather than U.S. House elections.

trolled by Democrats, Republicans are roughly 11 percent more likely to have won elections just before redistricting. Since these elections occur before redistricting, the pattern suggests Democrats (Republicans) are aiming to take control of redistricting in states where Republicans have had recent wins (losses) in the U.S. House. In other words, there is pro-opposition selection; each party seeks to control redistricting in places where it has recently lost seats in the House.

Yet the bottom-left figure suggests this pro-opposition drift vanishes in the election immediately after redistricting. States that had previously been unfavorable to Democrats—which are also the states where they control redistricting—suddenly become neutral. Then the Causal Effect of Democratic control of 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. As expected, the Causal Effect of partisan redistricting is anti-opposition. The party in control of the assembly uses redistricting to harm its opponents.

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,
Figure 6
Selection and Causal Effect in Contest-Level Outcomes

Note: The figure shows estimates of Equation 9, where the unit of observation is a U.S. House contest. 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.
the parties seek to capture redistricting in states where they have sustained recent losses in the U.S. House. These losses are temporarily reversed by redistricting, but the gains do not persist. Aside from suggesting there are clear (if short-lived) gains to controlling redistricting, Figure 6 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.

5.2 Pre-Trends in the Discontinuity: Selection Effect

The figures in Section 5.1 give evidence of a Selection Effect. Aside from being of interest in its own right, the Selection Effect must be estimated and mitigated before we can 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 its time pattern \(w(t)\). 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 election 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 constant specification is ill-fitted to the data—the constant is statistically insignificant. In Column 2 the constant remains 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 trend. There is little to suggest the coefficients, which are ranked in chronological order, are growing linearly. They suggest there is 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 changes after \(t = -5\).
Table 1
Estimates of the Selection Effect

<table>
<thead>
<tr>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
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<tbody>
<tr>
<td>[Dem. Control] × Constant</td>
<td>0.063</td>
<td>-0.035</td>
<td>(0.039)</td>
<td>(0.049)</td>
<td></td>
</tr>
<tr>
<td>× Trend</td>
<td>0.020***</td>
<td>(0.007)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>× [t=-9]</td>
<td>-0.044</td>
<td>-0.028</td>
<td>(0.052)</td>
<td>(0.050)</td>
<td></td>
</tr>
<tr>
<td>× [t=-7]</td>
<td>0.024</td>
<td>0.001</td>
<td>0.066</td>
<td>0.026</td>
<td>(0.049)</td>
</tr>
<tr>
<td>× [t=-5]</td>
<td>0.119**</td>
<td>0.126**</td>
<td>0.157***</td>
<td>0.146***</td>
<td>(0.049)</td>
</tr>
<tr>
<td>× [t=-3]</td>
<td>0.113**</td>
<td>0.137**</td>
<td>0.146***</td>
<td>0.147***</td>
<td>(0.053)</td>
</tr>
<tr>
<td>× [t=-1]</td>
<td>0.106**</td>
<td>0.122**</td>
<td>0.142**</td>
<td>0.135**</td>
<td>(0.052)</td>
</tr>
</tbody>
</table>

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 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.

These estimates confirm the pattern visible in Figure 6 while adding nuance. There is no evidence of selection on on outcomes 7 to 9 years before redistricting; parties likely ignore elections so far in the past in deciding where to capture redistricting. The outcomes of U.S. House contests become relevant for their decision starting 5 years before redistricting. Since the coefficients are positive they suggest parties make pro-opposition selection, meaning they aim to capture redistricting where the opposing parties has been winning.

Most crucial, however, is that after the estimated Selection Effect becomes positive it remains constant. Assuming 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.

5.3 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
Main Result: Difference-in-Discontinuity Estimate of Effect of Redistricting

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

is constant, as we assume and as is suggested by the results of Section 5.2, the difference-in-discontinuities estimates should equal zero for the years before redistricting. We would expect them to turn negative, or anti-opposition, after redistricting—that is, we would expect Republicans are less likely to win when Democrats gain a veto over redistricting.

Figure 7 shows exactly that. We plot the estimates \{\hat{\omega}^t\} with their 90 percent confidence intervals. Since \( t = -5 \) is the reference year its estimate is zero by construction. But the estimates for \( t = -3 \) and \( t = -1 \) are also 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. The damage to the opposition is the Causal Effect of partisan redistricting. Yet the estimate for \( t = 3 \) reverts to zero, suggesting the anti-opposition power of redistricting is short-lived. As we argue in Section 7, such transience should not be surprising.
We maximize the power of our estimates by imposing the restriction implied by Figure 7: that the Causal Effect appears only in the year immediately after 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 different 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, contrary to our assumption, the Selection Effect is not constant this trend might absorb some of the bias. The estimates in Columns 5 and 6 are reassuringly unmoved, which may be evidence that 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). 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 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-tenth 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 6 show that the estimates are largely unchanged (or larger) at narrower choices.

\[22\text{These are Alabama, Alaska, Arizona, Georgia, Louisiana, Mississippi, South Carolina, Texas, and Virginia.}\]
Table 2
Main Results: Difference-in-Discontinuity Estimates
Causal Effect of Dem. Control on First Election After Redistricting

<table>
<thead>
<tr>
<th>Panel A: Main Results</th>
<th>(1)</th>
<th>(2)</th>
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<th>(4)</th>
<th>(5)</th>
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<tbody>
<tr>
<td>Dif-in-Disc Estimate</td>
<td>-0.108***</td>
<td>-0.111***</td>
<td>-0.112***</td>
<td>-0.105***</td>
<td>-0.110**</td>
<td>-0.096***</td>
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<tr>
<td></td>
<td>(0.036)</td>
<td>(0.039)</td>
<td>(0.035)</td>
<td>(0.037)</td>
<td>(0.043)</td>
<td>(0.036)</td>
</tr>
<tr>
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<td>X</td>
<td>X</td>
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<td>X</td>
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<tr>
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<td>X</td>
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<table>
<thead>
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<th>Panel B: Specification Tests</th>
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<tr>
<td>Dif-in-Disc Estimate</td>
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<td>-0.093**</td>
<td>-0.097**</td>
<td>0.096***</td>
<td>-0.107***</td>
<td>-0.015</td>
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<td></td>
<td>(0.036)</td>
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<td>(0.038)</td>
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<tr>
<td>Observations</td>
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<td>5861</td>
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<td>Events</td>
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<td>120</td>
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<th>Panel C: Robustness to Bandwidth</th>
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<th>(4)</th>
<th>(5)</th>
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<tbody>
<tr>
<td>Dif-in-Disc Estimate</td>
<td>-0.107***</td>
<td>-0.108***</td>
<td>-0.098**</td>
<td>-0.128**</td>
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<tr>
<td></td>
<td>(0.035)</td>
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<td>5513</td>
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<td>Events</td>
<td>149</td>
<td>135</td>
<td>111</td>
<td>82</td>
<td>48</td>
</tr>
</tbody>
</table>

Note: Each column shows a different estimate of \( \hat{\beta} \) from Equation 12. Panel A gives the baseline estimate and several estimates that control for various fixed effects ("Event FEs" are state-redistricting event fixed-effects). "Trends" controls for a linear time trend in the size of the discontinuity. Panel B checks the specification. "No Ind. Legislators" drops cases in which independent legislators are elected to the state assembly during the redistricting election. "Drop VRA States" drops states that require pre-clearance from the Justice Department for any change in election law. "Republican Margin" defines the running variable as the Republican rather than Democratic margin of seats in the assembly. "Drop Special Elections" drops all U.S. House elections in odd years. "Placebo 2" 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.
of bandwidth.

To summarize, the Causal Effect of redistricting is anti-opposition. 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 year after redistricting. This steep anti-opposition effect does not last long. By the election 3 years after redistricting it has vanished.

6 Mechanism: Is It Really Caused by Partisan Redistricting?

Are the difference-in-discontinuities estimates of Section 5.3 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 in the year after redistricting is reversion to the mean. To allay such concerns we test for more direct evidence of partisan gerrymandering. We first study the outcomes of open districts (those without any incumbent). We then test for systematic differences in the demographic composition of districts drawn by each party. Though it is impossible to prove that redrawn boundaries are what cause a party to win or lose, our results suggest it would be a remarkable coincidence if they are wholly unrelated.

6.1 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 gerrymander in a way that unseats an enemy incumbent, and would find it unnecessary to prop up its own incumbents.

The incumbency advantage also may make demographics and party regis-
tion 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 lower house members who won office in 2002, roughly 15 percent sought higher office over the next 10 years, of whom over 10 percent ran for the U.S. House. It is much easier for these ambitious assembly members to win a House race in an open district than to challenge an incumbent, especially if the open district has been engineered with a favorable partisan bias. The Economist (2002) reports one such example where the chairman of North Carolina’s 2001 redistricting commission stood for office in a Congressional district he himself created.

We estimate Equation 9 for the subsample of open U.S. House districts. We run these estimates for the year before redistricting (to measure the Selection Effect) and then for the year immediately afterwards. The regression discontinuity is graphed in the top-left and top-right panels of Figure 8, which created analogously to Figure 6. The top-left panel shows that there is no evidence of selection on the outcomes of open seats. The probability a Republican wins in an open seat is smooth around the cutoff. But by the election after redistricting, shown in the top-right panel, a massive discontinuity has opened. In states where Democrats barely miss taking control the Republican candidate for an open seat wins with probability 80 percent. When Democrats barely win control the probability drops to 30 percent. If there is no selection on the outcomes of open districts, as implied by the top-left figure, then the Causal Effect of partisan redistricting is an anti-opposition effect of 50 percentage points. It seems unlikely such a stark difference in outcomes would be an accident.

We can also test whether the allocation of Republican votes across districts is consistent with efficient gerrymandering. McGhee (2014) proposes measuring
Figure 8
Open Seats Have the Most Clearly Visible Effect

Open Seat: Republican Wins

Selection: Election Before Redistricting

Causal Effect: First Election After Redistricting

Open Seat: Republican Vote Efficiency

Selection: Election Before Redistricting

Causal Effect: First Election After Redistricting

Note: The figure shows estimates of Equation 9, where the unit of observation is a U.S. House contest. We restrict the sample to contests for 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.
vote efficiency by counting the number of wasted votes: votes cast for a candidate that loses, or votes cast in excess of what is needed to elect a candidate. Let $V^{R}$ be the share of votes cast for the Republican, and let $V^{[2]}$ be the share cast for the runner-up (regardless of party). Define Republican vote efficiency as

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

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

### 6.2 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 tools available to them 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 may use racial gerrymandering to bolster its candidates. It would move its opponents—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).\(^{23}\) Since an African American is likely to support Democrats, Republicans may try to redistrict African Americans to minimize their influence.\(^{24}\)

---

\(^{23}\)According to CNN (2016), whose data are based on National Election Pool exit polls.

\(^{24}\)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

<table>
<thead>
<tr>
<th></th>
<th>Prob. of Being Moved</th>
<th>Conditional on Moving</th>
<th>Pre-Redistricting Char.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Black Tracts</td>
<td>(2) Other Tracts</td>
<td>(3) New District &gt; 75% Black</td>
</tr>
<tr>
<td></td>
<td>(4) Fraction Black</td>
<td>(5) District Size Deviation</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Observations</td>
<td>Events</td>
<td></td>
</tr>
<tr>
<td>Dem. Control</td>
<td>-0.150**</td>
<td>-0.027</td>
<td>-0.230***</td>
</tr>
<tr>
<td></td>
<td>(0.063)</td>
<td>(0.050)</td>
<td>(0.054)</td>
</tr>
<tr>
<td>Observations</td>
<td>19478</td>
<td>185111</td>
<td>2842</td>
</tr>
<tr>
<td>Events</td>
<td>117</td>
<td>138</td>
<td>41</td>
</tr>
</tbody>
</table>

Note: Columns 1 and 2 estimate Equation 9 at the level of the census tract on a dummy for whether the tract is “moved” during redistricting (see text for definition). Column 1 restricts to tracts that are majority black; Column 2 restricts to all other tracts. Column 3 estimates Equation 9 on the subset of majority black tracts that are moved. The outcome is a dummy for whether the tract is moved into a district whose population is more than 75 percent black. Column 4 estimates Equation 9 on districts before redistricting, where the outcome is the black population share of the district. Column 5 estimates Equation 9 on majority black tracts, 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 B.1.

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 it by definition measures intent; 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. When Republicans control redistricting, majority black census tracts are 15 percentage points more likely to be moved. This holds only for African American tracts; other tracts show no discontinuity.25

Are they being moved to 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 certainty, the number of contests in which the hostile voters may be pivotal is minimized.26 We restrict the sample to African American tracts that

25Visual representations of all tests described in this section can be found in Appendix B.1.
26In 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
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.  

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 districts in which opponents are a minority. As a result, mass shifts from just above to just below 50 percent.

The righthand panel of 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.B we adjust the estimates to show the effect of Republican control. As predicted, there is an increase in the density of overwhelmingly African Ameri-
Figure 9
Effect of Packing and Cracking on Distribution of Political Opponents

A. Predicted Effect of Packing and Cracking Political Opponents

1. Move opponents out of districts where they form slight majorities...
2. ...pack them into segregated districts...
3. ...to leave behind more districts in which they form minorities.

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

Note: See text for description.

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

Can these estimates necessarily be interpreted as the Causal Effect of partisan redistricting? It is difficult to test for a Selection Effect using the exact approach of Section 3.1 because our measures of how African Americans are moved during redistricting are by construction undefined before redistricting. Instead we test for more basic differences in the demographics of states on either side of the cutoff. Our aim is to test whether there is any difference in the objective need to redistrict African Americans.

The most obvious confounder would be if states barely controlled by Republicans on average contain more African Americans, making it almost mechanical that they would be more likely to be moved during redistricting. Taking the district as the unit 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. We test this hypothesis by calculating for each census tract the absolute deviation from the mean in the population of its old district. If African Americans in Republican states are concentrated in malapportioned districts before redistricting, majority-black census tracts should have a higher absolute deviation on the left of the threshold. Column 5 of Table 3 suggests no such deviation exists.

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

7.1 Simulations: Shocks Undo Gerrymandering

Change is the bane of a gerrymander. Designing 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.

We run a simple simulation to demonstrate how such volatility 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 $\{V_{ist}^R\}_{vi}$. If the Republicans gerrymander they may, subject to some constraints, reallocate votes to create a new set of shares $\{\tilde{V}_{ist}^R\}_{vi}$. We assume the gerrymandered districts are constructed as follows:

1. Republicans choose a threshold $\bar{V}$. A gerrymandered districts will never be left with fewer votes than this.

2. They draw 10 percent of Republican voters out of each district to be reallocated, meaning in the absence of further transfers each district’s Republican vote share is now $\{0.9V_{ist}^R\}_{vi}$. This is their “budget” for gerrymandering $B = \sum_i 0.9V_{ist}^R$.

3. They order districts in descending order by the Republican vote share $\{V_{i(I),st}^R, V_{i(I-1),st}^R, \ldots, V_{i(1),st}^R\}$

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 5.3. 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 $\theta_{t+1}$ 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.

---

30 To be precise, 10 percent of the voters are replaced by an equal number of Democrats from a different district.
with mean zero and variance calibrate to match the data. The national shock is set to three different values to see whether gerrymandering is more or less persistent when the national mood is hostile.

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

Figure 10 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 lefthand panel shows the outcome in the election immediately after redistricting. Gerrymandering wins Republicans 44 percent of all seats. They would have won only 32 percent had they been unable to gerrymander. The righthand panel shows the subsequent election ($t + 1$).

In this example the Republicans have sustained a negative statewide shock $\bar{k}_{s,t+1}$, making it similar to the example described above. Several of the gerrymandered districts have now swung against the Republicans, leaving them no better off than they would have been under the natural distribution (in both cases they win 24 percent of seats). 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 11 shows that this intuition holds more generally. For each threshold we graph the average across all 200 simulations of the number of seats won by Republicans. We show only the case of the neutral national shock, as the others look similar. The red line in the left-hand panel—the average seats won in the first election after gerrymandering—is downward sloping. It captures the trade-off between having a bigger threshold (making safer seats) and winning more seats. Not surprisingly, the gains from gerrymandering—the gap between the red line and the black line, which shows the seats won without gerrymandering—follows a similar pattern.

This gap narrows in the right-hand panel, which shows the subsequent election. At any threshold the gap narrows, and it narrows the most in cases where it was widest to begin with (when the threshold was small). Given that our estimates imply a gap of roughly 11 percentage points in the first election, the threshold
**Figure 10**
Simulation Example: A Negative Shock to Support for Republicans Nullifies the Gains from Gerrymandering

<table>
<thead>
<tr>
<th>Election Type</th>
<th>Seats Won</th>
<th>Natural</th>
<th>Gerrymandered</th>
</tr>
</thead>
<tbody>
<tr>
<td>First</td>
<td>Seats</td>
<td>32.0%</td>
<td>44.0%</td>
</tr>
<tr>
<td>Natural</td>
<td>Seats</td>
<td>32.0%</td>
<td>24.0%</td>
</tr>
<tr>
<td>Next</td>
<td>Seats</td>
<td>24.0%</td>
<td>24.0%</td>
</tr>
</tbody>
</table>

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

should be roughly 0.6. That implies a gap in the next election of around 6 percentage points—somewhat smaller than the original effect and well within the confidence interval of our estimate shown in Figure 7 for the election 3 years after redistricting.

### 7.2 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. The New York Times (May 30, 2017) suggests that restrictions on gerrymandering become ever more necessary “as voter data and computer-mapping technologies become more sophisticated.”

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

The data also suggest that swings in party vote shares are growing smaller over time. During the 1971 redistricting cycle the standard deviation of the state-wide swing is 7.6 percent, but by the 2001 cycle it has fallen to 6.1 percent. The standard deviation of the idiosyncratic component falls from 8.5 percent to 6.1 percent. Though this decline in volatility could have many potential explanations, what matters for our analysis is that it might make it easier to design a long-lasting gerrymander.

Each panel of Figure 12 is constructed like Figure 7. The lefthand panel restricts the sample to the 1971, 1981, and 1991 redistricting cycles while the righthand 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 percent versus 5.5 percent. 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

Figure 11
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).
Figure 12
The Effect of Gerrymandering Grows Larger and More Persistent

Note: Each panel is constructed analogously to Figure 7, but with a restricted sample. The left-hand panel restricts to state-redistricting events in 1971, 1981, and 1991. The right-hand panel restricts to more recent redistricting events.

suggestive, as the 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 are guaranteed to be short-lived.

8 Summary

We propose and apply a method to estimate where political parties seek to control Congressional redistricting and what they do with their control. Parties seek to control redistricting in states where they have recently sustained losses in the U.S. House. These losses are temporarily reversed in the election immediately after redistricting, suggesting the party in control uses redistricting to hurt the opposition. We show that the effect is especially large in open seats, and that the boundaries are adjusted in a way consistent with racial gerrymandering. Both suggest partisan redistricting is the mechanism for our results. 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.

References


A Theoretical Appendix

A.1 Derivation of Conditional Expectation

This appendix derives Equation 3. 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{equation}
\mathbb{E}[\theta_s \mid X_s] = \mathbb{E}[\theta_s \mid X_s, D_+]\mathbb{P}(D_+ \mid X_s) + \mathbb{E}[\theta_s \mid X_s, D_-]\mathbb{P}(D_- \mid X_s) + \mathbb{E}[\theta_s \mid X_s, R_+]\mathbb{P}(R_+ \mid X_s) + \mathbb{E}[\theta_s \mid X_s, R_-]\mathbb{P}(R_- \mid X_s)
\end{equation}

First we derive conditional expectations on the right-hand side of this equation. These are most straightforward in the cases where there has been precise control. Assuming \( X_s \) is within a range consistent with precise control (as we discuss below),

\[
\mathbb{E}[\theta_s \mid X_s, D_+] = \mathbb{E}[\theta_s \mid \theta_s \in \Theta_D] \\
\mathbb{E}[\theta_s \mid X_s, R_+] = \mathbb{E}[\theta_s \mid \theta_s \in \Theta_R]
\]

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. Suppose \( X_s \) is observed and it is known not to have been precisely controlled. Then the conditional density of \( \theta_s \) is solved for as follows:

\[
H^\theta(\theta \mid X_s) = \mathbb{P}(\theta_s < \theta) = \mathbb{P}(x^{-1}(X_s - v_s) < \theta) = \mathbb{P}(X_s - x(\theta) > v_s) = 1 - F\left[X_s - x(\theta)\right] \\
\Rightarrow h^\theta(\theta \mid X_s) = f\left[X_s - x(\theta)\right]x'(\theta)
\]

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

\[
h^\theta(\theta \mid X_s, D_-) = \begin{cases} 
0 & \text{if } \theta \in \Theta_D \\
\frac{h^\theta(\theta \mid X)}{1-\mu} & \text{otherwise}
\end{cases}
\]
where the $\mu_D$ in the denominator follows because $\mathbb{P}(\theta \notin \Theta_D) = 1 - \mu$. Then the conditional expectation can be written as

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

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,

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

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

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

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

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

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

Define

$$\tilde{\chi}(X_s) = \begin{cases} \chi(X_s) & \text{if } X_s \geq 0 \\ \chi(-X_s) & \text{if } X_s < 0 \end{cases}$$
and
\[b(X_s) = \frac{\mu \tilde{\chi}(X_s)}{\mu \tilde{\chi}(X_s) + (1 - \mu)\left[\tilde{g}^D(X_s) + \tilde{g}^R(X_s)\right]}\]

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

Now define
\[a(X_s) = \frac{(1 - \mu)\tilde{g}^D(X_s)}{\mu \tilde{\chi}(X_s) + (1 - \mu)\left[\tilde{g}^D(X_s) + \tilde{g}^R(X_s)\right]} \tilde{\theta}^D(X_s) + \frac{(1 - \mu)\tilde{g}^R(X_s)}{\mu \tilde{\chi}(X_s) + (1 - \mu)\left[\tilde{g}^D(X_s) + \tilde{g}^R(X_s)\right]} \tilde{\theta}^R(X_s)\]
\[= \mathbb{E}[\theta_s \mid X_s, R_+]P(R_+ \mid X_s) + \mathbb{E}[\theta_s \mid X_s, R_-]P(R_- \mid X_s)\]

which is a continuous function. Then Equation 3 follows because
\[\mathbb{P}(D_+ \mid X_s) = \begin{cases} b(X_s) & \text{if } X_s \geq 0 \\ 0 & \text{if } X_s < 0 \end{cases}\]
\[\mathbb{P}(R_+ \mid X_s) = \begin{cases} 0 & \text{if } X_s \geq 0 \\ b(X_s) & \text{if } X_s < 0 \end{cases}\]

\[\blacksquare\]

**B Empirical Appendix**

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

**B.1 Additional Tables and Figures Referenced in the Text**
Figure 13
African Americans are More Likely to Be Moved to Unfamiliar Districts

Note: See the text describing Table 3 in Section 6.2 for details. Each dot gives the conditional mean within a bin of width 3.

B.1.1 Racial Gerrymandering (Section 6.2)

Figure 13 gives the visual representation of Columns 1 and 2 of Table 3. Figure 14 represents Column 3, and Figure 15 represents Columns 4 and 5. See the text of Section 6.2 for details on the outcomes and the specifications.

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

In this appendix we show that the results are robust to the choice of bandwidth. Table 2.C shows that the main results are robust—here we focus on several other results throughout the paper. Figures 16 and 17 show the robustness of the results from Section 5.1 and Figures 18—22 the results from Section 6. In all cases we plot the size of the estimate against the bandwidth used to make the estimate.
**Figure 14**
Conditional on Being Moved, Where Are African Americans Moved?

Note: See the text describing Table 3 in Section 6.2 for details. Each dot gives the conditional mean within a bin of width 1.
Figure 15
No Evidence that African Americans Need to be Moved More on One Side of the Threshold

Note: See the text describing Table 3 in Section 6.2 for details. Each dot gives the conditional mean within a bin of width 3.
**Figure 16**  
Robustness to Bandwidth: Figure 4
Figure 17
Robustness to Bandwidth: Figure 6
Figure 18
Robustness to Bandwidth: Figure 8

Open Seat: Republican Wins
Selection: Election Before Redistricting
Causal Effect: First Election After Redistricting

Open Seat: Republican Vote Efficiency
Selection: Election Before Redistricting
Causal Effect: First Election After Redistricting
**Figure 19**
Robustness to Bandwidth: Table 3, Columns 1 and 2

<table>
<thead>
<tr>
<th>Bandwidth</th>
<th>RD Estimate (90% CI)</th>
<th>Black Census Tracts</th>
<th>Non-Black Census Tracts</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>8</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>16</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>20</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

The figure shows the robustness of the RD estimate to bandwidth for both Black and Non-Black Census Tracts.
Figure 20
Robustness to Bandwidth: Table 3, Column 3
Figure 21
Robustness to Bandwidth: Figure 9.B

Density of Black Population Share [Bw=10]

Density of Black Population Share [Bw=8]

Density of Black Population Share [Bw=6]
Figure 22
Robustness to Bandwidth: Table 3, Columns 4 and 5

Fraction of Population Black is Similar at Threshold

Blacks are No More Likely to Live in Large or Small Districts
Figure 23
The Main Results Hold with the Alternative Dataset

B.3 Verifying the Results with an Alternative Dataset of Contests for the U.S. House

As noted in Section 4, the ICPSR Constituency data we use for our analysis is not a complete dataset of all elections. To validate that these missing elections do not bias the results we merge our data with 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 elections. Their data contain 114 elections not contained in ours, whereas our dataset contains 186 elections not contained in theirs. Among the elections contained in both the two datasets agree on the outcome of 99.45 percent of elections.

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 23 shows that the difference-in-discontinuities estimates are unchanged when we use the alternative dataset.
Figure 24
The Main Results Hold after Dropping Maine

B.4 Verifying the Results without Maine

As noted in the front matter, Maine is an unusual case in that it has in some cycles redistricted in the year ending in 3. In this appendix we verify the results hold after dropping Maine from the sample. Figure 24 confirms that dropping Maine has little effect on the results.