Political Control Over Redistricting and the Partisan Balance in Congress

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We estimate the impact of a political party's legal ability to unilaterally redistrict Congressional seats upon partisan seat share allocations in the U.S. House of Representatives. Controlling for stateXdecade and year effects, we find an 8.2 percentage point increase in the Republican House seat share in the three elections following Republican control over redistricting in the past two decades. We find no significant or sizable effect for Democrats. We explain the gap between Democratic and Republican states as due to commissions. Effects are largest in populous states and Democrats have outsourced redistricting to neutral commissions whereas Republicans have not. We find similar effects of legal control due to marginal gubernatorial control. We additionally introduce novel methods to estimate the impact of legal control due to marginal legislative control. Effect sizes over the past five decades in aggregate are small and insignificant for both parties. It is only in the past two decades that these effects are sizable though not pivotal for control of Congress. *Keywords: Gerrymandering, redistricting, voting*

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"The partisan gerrymanders in these cases deprived citizens of the most fundamental of their constitutional rights: the rights to participate equally in the political process, to join with others to advance political beliefs, and to choose their political representatives." - Justice Elena Kagan, Dissent, Rucho et al. v. Common Cause et al.

"Extreme partisan gerrymandering is a real problem for our democracy" - Justice Brett Kavanaugh

I. Introduction

"Representatives and direct Taxes shall be apportioned among the several States which may be included within this Union, according to their respective Numbers, which shall be determined by adding to the whole Number of free Persons, including those bound to Service for a Term of Years, and excluding Indians not taxed, three fifths of all other Persons. The actual Enumeration shall be made within three Years after the first Meeting of the Congress of the United States, and within every subsequent Term of ten Years, in such Manner as they shall by Law direct." - Article 1 Section 2, U.S. Constitution

In majoritarian single member-district political systems, representatives are elected in geographical districts. As population imbalances across districts accrue increasingly over time, new district boundaries need to be drawn. In many countries such as Australia, Canada, Mexico and the United Kingdom, maps of political districts are drawn by non-partisan, independent bodies. In the United States, the drawing of district boundaries is delegated to partisan actors: namely state legislators. Article 1 Section 2 of the U.S. constitution requires the federal government to undertake a census of the population and use it as a basis for reapportioning the numbers of districts across states. State legislators then are responsible for drawing district boundaries within states. This process, unfortunately, allows politicians to redraw political boundaries in order to affect partisan control over both federal and state legislatures. This paper estimates the impact of partisan control over the redistricting process on partisan seat shares.

Partisan interest in redistricting became apparent as early as 1812, when the Massachusetts State Senate redrew electoral boundaries, as mandated by the U.S. constitution. The redrawn districts benefited the Democratic-Republican party over the opposition Federalist party. Governor Elbridge Gerry of Massachusetts, a Democratic-Republican, signed the redistricting bill into law though he personally lamented the highly partisan process. The redistricting resulted in some oddly shaped districts. One Federalist newspaper, the Boston Gazette, noted that one of the state senate districts looked like a salamander. The Gazette coined the term "Gerry-mander" in a political cartoon from March, 1812.

In recent years, there has been increased concern over whether or not redistricting leads to gerrymandering: i.e. whether or not parties redistrict in order to increase their share of legislative seats. A body of theoretical work shows that self-interested political parties will redistrict by *cracking* opposition districts with a narrow majority and *packing* the opposition into lop-sided districts in an attempt to increase own-party seat share (Shotts, 2001; Gilligan and Matsusaka, 1999). More recent theoretical work points out that packing is beneficial but when a party is uncertain of partisan leanings and voter turnout, it is usually not optimal to crack (Friedman and Holden, 2008).

There also has been an empirical literature on the impact of redistricting. Much of the best empirical work to date simulates counterfactual maps subject to legal or norms-based constraints such as requirements that districts be connected and that they be compact (Chen, Rodden et al., 2013; Chen and Rodden, 2015; Stephanopoulos and McGhee, 2015). This literature then computes probabilities of a redistricting outcome at least as partisan as the actual outcome. Though this work tends to find that in many states, few alternative ways of drawing districts yield greater imbalance in the relationship between voting behavior and representation, it is nonetheless possible districts which confer partisan advantage to one party or another reflect natural geographical boundaries or reflect natural political communities (Rodden, 2019). (McGann et al., 2016) shows that there is greater skewness (and thus wasted votes) in the vote shares of Democratic as opposed to Republican districts; it interprets greater skewness as evidence of net partisan redistricting in favor of the Republicans.

Some of the work on redistricting, however, does suggest that partisan advantage due to asymmetries in clustering across districts is due to the clustering of like-minded individuals (Chen, Rodden et al., 2013) rather than the intentional design of parties. A large literature has noted increased political sorting over time (Bishop, 2009; Hopkins, 2017; Kaplan and Sullivan, 2018).

Instead of comparing actual maps to counterfactual maps, we estimate the impact of partisan legal control over the redistricting process on the maps that are drawn and on the resulting seat shares in the House of Representatives. In so doing, we provide comprehensive evidence on the prevalence of partisan gerrymandering over 50 years of American history. We show our estimates by party, by size of the state and by time period. The only other work which has estimated the impact of partisan legal control are (Stephanopoulos and McGhee, 2015; Stephanopoulos, 2017) which have been heavily used in recent Supreme Court cases. (Stephanopoulos and McGhee, 2015) introduces a measure of wasted votes called the efficiency gap and, in a follow-up paper (Stephanopoulos, 2017) estimates the impact of unified control on the efficiency gap using a two-way fixed effects model. We go well beyond (Stephanopoulos and McGhee, 2015) and (Stephanopoulos, 2017) in a number of ways. Most important, we show that the two way fixed effects estimates are upwards biased due to a rising impact of partisan legal control over seat shares. However, we also correctly measure legal partisan control over redistricting (as opposed to the proxy of unified control over state government).

We first develop a measure of the amount of redistricting as the fraction of a state which changes districts. We show empirically that almost all redistricting happens once a decade by legislatures in power in years that end in 1. We then estimate the impact of the ability of one party to pass a redistricting bill without votes from the opposition party on that party's fraction of seats in Congress in subsequent elections.

We estimate our effects in three different ways and get very similar estimates. First, controlling for year effects as well as state X decade effects, we find a statistically significant positive impact of 4.7 percentage points of Republican legal control on the Republican seat share in Congress in the subsequent election. The effect is 9.1 percentage points restricted to the past two decades. The average effect over the next three federal elections is positive but not statistically significant for the full five decade sample but increases in size to 8.2 percentage points and becomes statistically significant at conventional levels when we restrict our data to the past two decades. In contrast, we do not find statistically significant effects or sizes as large with Democratic control except for large state delegations restricted to the past two decades. Our estimates for the impact of Republican control over redistricting are relatively stable regardless of state delegation size. However, we do find statistically significant increases in the Democratic seat share following Democratic control over redistricting only when we restrict to the past two decades and only in states with more than five seats. Second, we compare state-decades with unified government versus those with a unified legislature and a governor of a different party due to small differences in the gubernatorial vote share. When using this source of variation, we restrict our sample to the large majority of states where both chambers of the state legislature and the signature of the governor are required for a redistricting bill, We find remarkably similar results.

We also estimate the impact of legal control due to marginal legislative control. Gerrymandering means that states with small difference in seat shares may not be different just due to random chance. We thus introduce two novel methods to estimate impacts of marginal legislative control based upon vote share shocks. To do this, we build upon work by (Folke (2014); Kirkland, Phillips et al. (2018); Kirkland and Phillips (2020)) We first estimate a random effects model with idiosyncratic and statewide shocks. We then simulate electoral outcomes as the stateXdecade level to come up with a probability of Republican legal control, Democratic legal control and absence of partisan legal control. Finally, we use these probabilities of legal control at the state-decade level to construct regression discontinuity as well as matching estimates of the marginal legal control due to legislative unification. These estimates are also consistent with our stateXdecade and year fixed effects estimates and our gubernatorial RD estimates. We see the methods we develop as useful for estimating partisan legal control beyond our particular context.

In a second component of the paper, we investigate why we see differences between the parties in redistricting behavior. We consider three common explanations: (1.) Republicans have been undoing solid control by Democrats, (2.) Democrats have made greater use of non-partisan commissions to redistrict particularly in large states where legal control of redistricting is more effective, and (3.) Democrats have pursued the creation of majority-minority districts when they have had legal control in lieu of maximizing seat shares. Our evidence points strongly towards the second of these explanations: Democrats have tied their hands from partisan redistricting by delegating to non-partisan commissions.

Overall, simple back of the envelope calculations suggest that partisan redistricting can on net account for less than 5% of the gap between Republicans and Democrats in the House during each of the 1970s, 1980s and 1990s. However, the same calculations show that it can account for 57% of the gap in the 2000s and 51% in the 2010s.

In the next section, we discuss important institutional features of the U.S. redistricting process. In section II, we discuss our empirical methods. In section III, we describe our empirical methods. In section IV, we give an overview of the data we use for our estimation. In section V, we present our main results. In section VI, we perform an exercise in which we compute aggregate impacts of the rights to redistrict upon the partisan balance in Congress. In section VII, we provide evidence on the mechanisms that explain the differences in behavior across the Democratic and Republican parties. Finally, in section VIII, we conclude.

II. Institutional Background

The use of drawing district maps to influence elections goes back to the period before the Constitution when the Articles of Confederation was law. Patrick Henry, along with other anti-Federalists, purportedly altered Virginia's 5^{th} Congressional District in an attempt to prevent the strong Federalist, James Madison, from returning to Congress (Labunski, 2006). In 1789 (the following year), the states ratified decadal redistricting. Since the adoption of the Constitution, redistricting has happened within the first three years of the decade in almost all states in every decade with the exception of the 1920s.[1]

The process of redrawing districts happens in two phases. In the first phase, reapportionment, the U.S. Congress uses recently collected data from the Population Census and by January 25^{th} of the year following the completion of the Census, assigns numbers of seats in the House of Representatives to each state. Though there are multiple possible methods to apportion seats, Congress uses the Hamilton-Hill method which minimizes deviations in number of representatives per person across states. Though the first Congress had 105 members and an average of approximately 33,000 individuals per representative, the size of Congress grew over time until it was capped in 1911 at 435 [2]. This cap was reauthorized

[1] In the 1920s, reapportionment would have led to a shift of 11 seats away from rural areas towards urban areas which had grown in size dramatically due to migration. A coalition of representatives from rural areas made sure that the reapportionment was blocked until 1929 (Anderson, 2015).

[2] This followed the addition of the states of Arizona and New Mexico to the United States in 1912.

in 1929 and has been in place continuously since then except for a temporary increase in 1959 when Alaska and Hawaii joined the United States and the number of representatives rose temporarily to 437.

After reapportionment, Congress notifies the states of the number of representatives that they are apportioned. The federal government historically has given individual states wide latitude to redistrict as they see fit. Currently reapportionment results in relative balance across states in representation in the House of Representatives. However, until the 1960s, individual states often created districts with a high degree of population imbalance. For example, in Georgia, the largest districts had 2-3 times the population of the smallest districts [3] In the early 1960s, the Warren court handed down three rulings. First, in 1962, *Baker v. Carr* established that redistricting was subject to judicial review. Then, in 1964, *Wesberry v. Sanders* mandated equal population in federal Congressional districts. *Reynolds v. Sims* also in 1964, then extended equal representation to state legislative districts. In subsequent decisions (*Karcher v. Daggett*, 1983; *Vieth v. Jubelirer*, 2003), the Supreme Court clarified that Congressional Districts should be exactly equal in size to the degree possible whereas for state legislative districts deviations of up to 10% across districts would be allowed (*Brown v. Thomson*, 1983) (Ansolabehere and Snyder, 2008).

More recently, the Supreme Court has further ruled that as long as districts are sufficiently compact, redistricting in order to create majority minority Congressional districts is legal but other racially-based reasons are not legal (Thornburg v. Gingles, 1986; Shawn v. Reno, 1993; Miller v. Johnson, 1995). The courts have been reluctant to disallow redistricting for partisan gain (Davis v. Bandemer, 1986; Vieth v. Jubelirer, 2004). In Vieth v. Jubelirer (2004). However, they allowed for the possibility that in the future they might prevent redistricting on partisan grounds. In 2019, the Supreme Court decided in Rucho v. Common Cause that the Supreme Court did not have the authority to intervene in order to limit redistricting on partisan grounds. However, court battles are ongoing at the state level. Of course, even if partisan redistricting is deemed legal by both federal and state courts, morals as well as longer-run strategic considerations may restrict parties from using political power to gain a future seat advantage.

Not all states provide useful variation for our analysis. States differ in their redistricting laws and processes. Seven states did not redistrict federal Congressional boundaries through most of the late 1900s and early 2000s because they only have one federal representative: Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont and Wyoming. We drop these states from our main analysis as they do not participate in redistricting. Moreover, since these districts have 100% partisan seat shares unless they elect an independent, including these high variance observations decreases our precision.

[3] Imbalance across state legislative districts was even larger. One state house district in Tennessee represented 2,340 people and another in the same state represented 42,298 people. The worst example of representational imbalance was in the Nevada state legislature where one district contained 568 voters and another approximately 127,000.

We also drop Nebraska because since 1934, Nebraska has not allowed political parties to operate at the state level. Thus, it is difficult to tell whether or not Democrats or Republicans have control over legislative bodies and thus whether one party has legal control over the redistricting process[4].

Over the fifty years which our data set encompasses, 12 states have used a commission to draw maps and implement redistricting in at least some decades. The composition of the redistricting commissions varies by state but many attempt to appoint a bipartisan commission by balancing the number of Democratic appointees and Republican appointees to the commission (Arizona, California, Hawaii, Idaho, Maine, Montana, New Jersey and Washington). [5] Three other states (New York, Ohio, and Rhode Island) use commissions without partisan balance. Finally, in Iowa, non-partisan staff draw maps every decade and the maps are then sent for approval to the legislature and the governor. Commissions vary from state to state in the extent of their redistricting powers and restrictions. We treat all state-decades with a commission as being of a singular "commission" type. We include these states in our benchmark specification but also show robustness to dropping them as well as recoding them. In the rest of the states, the legislature and/or the governor draw the maps. When one party has a trifecta, they have the capacity to draw the maps and pass a redistricting bill without help from the other party. In Connecticut and North Carolina, control over both chambers of the legislature is sufficient because the governor cannot veto the redistricting bill.

We estimate the impact of the legal ability of a party to redistrict upon subsequent federal partisan seat shares. This depends upon the laws in the state, the number of seats, the distribution of legislative seats across both chambers and usually the control of the governorship. We use variation across decades and across states to estimate the impact of partisan control over redistricting on partisan seat shares. The details of the laws and how they vary by states over time are explained in greater detail in the Data Appendix. In Figure 1, we color code states by decade with blue for Democratic control, red for Republican control, and gray if neither party had control. We code neither party as having control if the government was divided and required approval by all chambers plus the governor, if the legislature was divided and only the legislature was required to pass a redistricting bill, or if redistricting was delegated to a commission. In the 1970s through 1990s, the Democrats had a much higher share of states with legal control. However, in the 2000s, control was largely balanced across parties and in the 2010s, the Republicans maintained partisan control in a substantially higher fraction of states.

[4] In addition, Nebraska also became the only state to have a unicameral state legislature with the passage of the same 1934 law

[5] Montana uses the commission in years in our data set where they have more than one Representative.

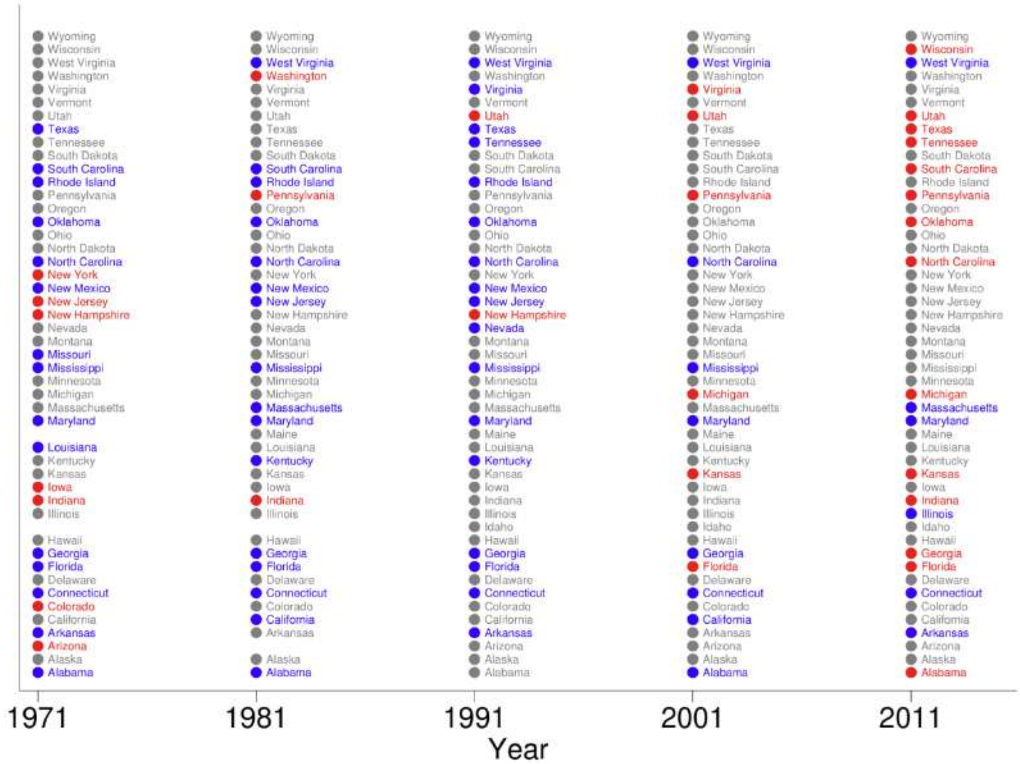

FIGURE 1. TREATMENT DEFINITION BY DECADE

III. Empirical Methods

In this section, we present the empirical methods that we will use to estimate our main effects. We present three main estimation methods. The three designs are aimed to estimate (1.) the overall average impact of legal control over redistricting by a party, (2.) the impact due to marginal control over the governorship and (3.) the impact due to marginal control over the state legislature respectively. The first of these methods uses variation across state-decades in legal control over redistricting, controlling for year effects. This strategy estimates an average effect for all states under a parallel trends assumption between the states with one-party legal control over redistricting and those without legal control by a party. The second approach uses a regression discontinuity estimator in the vote share of the governor given unified legislative control in redistricting years. This identifies the impact of legal control due to unification of an already unified legislature with the governor. The third strategy uses a simulation estimator where we first estimate the shock structure of partisan vote shares for state legislative districts, then use the estimated shocks to simulate the probability of unified control, and finally use a propensity score-type matching estimator to estimate the impact of legal control due to unification of the legislature.

A. StateXDecade and Year Effects Estimation

In our first and main specification, we regress an electoral outcome variable, $O_{s,d,y}$, on a measure of partisan legal control conditional upon stateXdecade ($\gamma_{s,d}$) and year (δ_{dXy}) effects. In terms of notation, we denote by s the state, by d the decade and by y the year within the decade. For example, if $d = 1980$ and $y = 2$, then the year is 1982. Therefore, in our notation, a year is a decade X year-within-a-decade: dXy. Our main electoral outcome variable is the Republican House of Representative seat share; however, we do estimate impacts upon fraction of land area of switching districts, fraction of people switching districts, minority fraction of a state delegation, differential fraction of wasted votes (the efficiency gap), and a measure of legislative polarization (DW-Nominate). The outcomes are indexed by the year in which Congress is elected.

Our main treatment variables are *DemControl* and *RepControl*. They are dummy variables which take on a value of 1 if either the Democrats or the Republicans respectively have the legal ability to pass a redistricting bill solely based upon votes from their own party in that decade [6]; the dummy variable takes on a value of 0 if neither party controls the redistricting process unilaterally. Neither party controls redistricting if different parties control the two chambers of the state legislature, if the governor is from a different party from a unified legislature and redistricting bills require a gubernatorial signature, or if maps are drawn by a commission with the legal authority to force use of the maps they draw. We also note that whereas the main treatment variables (legal control variables) are determined in years ending in one, our main outcome variables (seat shares) are determined in even years (i.e. election years). We interact our treatment variables with a set of election year dummies. Our specification is given by:

$$O_{s,d,y} = \alpha + \mu_y^D DemControl_{s,d} + \mu_y^R RepControl_{s,d} + \gamma_{s,d} + \delta_{dXy} + \epsilon \quad (1)$$

We regress the outcome on our main treatment variables, *DemControl* and *RepControl* interacted with year-within-decade dummies. These, jointly saturate the decade and allow us to trace the dynamic path of our effect over five elections.

[6] Redistricting usually occurs and is supposed to occur in years ending in 1. In a small number of cases, usually due to legal delays, districts are redrawn later in the decade. Due to endogeneity concerns as well as concerns about aggregation of cohort effects over time, we base our definition of legal control off of the composition of state legislatures and governorships in years ending in 1. However, we also show that our results are nearly identical if we instead define our treatment status variable based upon the year when redistricting actually occurs.

In the paper, we report the μ coefficents normalized relative to the year of the last elected Congress before redistricting. Our main reported coefficients are thus:

$$\begin{aligned} \beta_y^R &= \mu_y^R - \mu_0^R \\ \beta_y^D &= \mu_y^D - \mu_0^D \end{aligned} \tag{2}$$

where β_0^P for party P is normalized to zero and does not have a standard error.

Finally, we note that we use an unconventional notion of decade. For us, decades begin in years ending in 8 and continue through years ending in 6. For example, the 1990s include the elections in 1988, 1990, 1992, 1994 and 1996. With this definition of decade, we estimate three effects of redistricting after redistricting happens and two before it happens. Given that we are normalizing our coefficients relative to the last election before redistricting (that which occurs in years ending in 0), we end up with one 'lead' effect (β_{-2}) and three ($\beta_2, \beta_4, \beta_6$) of redistricting.

B. Gubernatorial Regression Discontinuity Estimator

In a second specification, we estimate a regression discontinuity model in the gubernatorial vote share. We limit our sample to states with (1.) a unified legislature which also (2.) require redistricting bill passage with a gubernatorial signature. In such a sample, a narrow electoral victory for a governor narrowly confers legal control. Our regression discontinuity estimator compares outcomes in state-decades where a party has narrowly achieved legal control to others where legal control was not obtained due to a small difference in the vote share for governor. The running variable for the regression discontinuity is thus the top-two party vote share for the party with unified control in both chambers of the state legislature in the election which determines the governor in years ending in 1.

Since the randomization (the close election determining legal control over redistricting) happens at the level of the state-decade, five observations are simultaneously randomized (i.e. the same value of the running variable) by each state-decade in the sample. Additionally, we assume a common functional relationship between the outcome variable and the running variable across all decades. We do this both below and above the discontinuity. We estimate separately for unified Republican legislatures and unified Democratic legislatures. We use a local linear regression with a triangular kernel. We note that our sample size shrinks both because we restrict to states which require unified control to pass a redistricting bill and because we further restrict state-decades based upon bandwidth. Our estimation equation is given by:

$$O_{s,d,y} = \alpha + \mu_y Control_{s,d} + \gamma_{d,y} + \delta_y + g_{Lose}(GovVS_{d,s}) + win_{d,s} X g_{Win}(GovVS_{d,s}) + \epsilon_{s,d,y} \tag{3}$$

Again, we estimate separate coefficients for each year and then normalize by coefficients for years ending in zero. Note that we estimate separate coefficients for each year. Ultimately, we estimate our four parameters of interest: $\beta_{-2}, \beta_2, \beta_4, \beta_6$. In other words, we report coefficients for $\beta_2, \beta_4, \beta_6$ and β_8 by subtracting off the coefficient β_0. This makes our coefficient directly comparable to our panel estimates using state-decade and year fixed effects.

For both the fixed effects estimator given in equation (1) and the gubernatorial regression discontinuity estimator given in equation (3), we cluster all of our results by state-decade. We do this for two reasons. First, our data is highly heteroskedastic. Variances in state delegation shares are substantially higher in smaller states for mechanical reasons. However, since delegation sizes do not change much over time, errors are only heteroskedastic within a state. second, clustering accounts for serial correlation within states over a decade. However, because realignment led to large reversals in partisanship, we do not cluster on decade; doing so decreases our standard errors though not by much [7]. Clustering at the state-decade level accounts both for the patterns of serial correlation and the patterns of heteroskedasticity present in the data.

C. Simulation Estimator

Our final method estimates the impact of legal control due to marginal unified legal control over the state legislature. In comparison with estimating the impact of legal control due to a marginal victory of a governor aligned with a unified legislature, estimating the impact of legal control due to marginal legislative control is much more difficult. The analog to comparing close gubernatorial elections might be to compare narrow legislative seat margins in at least one chamber. However, constituents may sort into districts and states may gerrymander districts. This means that states with a narrow majority of Democratic (Republican) seats might be more than marginally different from states with similar seat shares where Democrats (Republicans) do not control the legislature. Comparing states which differ marginally in terms of seats shares might entail comparing states on different political trajectories.

Instead, we prefer to compare states which differ in legal control resulting from randomness in voting outcomes. In other words, we use differences in seat allocations due to differences in voting outcomes as opposed to generic differences in seat allocations.

[7]Estimates with clustering by state as opposed to state-decade are available from the authors upon request

In order to compare differences across state-decades in legal control due to vote share shocks, we first estimate the shock structure for state legislative districts. In particular, we simultaneously estimate the variances of a state-specific common shock and a district-specific idiosyncratic shock. We do this using an MLE-estimated random effects estimator:

$$VS_{s,d,c,j} = \eta_{s,d} + \epsilon_{s,d,c,j} \tag{4}$$

where $VS_{s,d,c,j}$ is the vote share for district j in chamber c and decade d in state s, $\eta_{s,d}$ is an i.i.d. stateXdecade specific shock to all districts across both chambers of a legislature, and $\epsilon_{s,d,c,j}$ is an i.i.d. shock that is idiosyncratic to a legislative district. We assume that the distribution of state-decade shocks is identical across all state-decades and that the idiosyncratic shock is identical across all districts. We estimate these variances for all legislative elections over the full sample that lead to a legislator being in power in a year ending in 1. We pool across states and decades in order to increase statistical power of our estimation. Of course, this comes at the sacrifice of assuming that vote shock distributions are in fact identical across state-decades (for $\eta_{s,d}$) and across districts (for $\epsilon_{s,d,c,j}$) respectively.

Once we obtain the variances σ^2_η and σ^2_ϵ, we simulate the probability of legislative control by the Democrats and by the Republicans for each state-decade. We do this assuming that the idiosyncratic and state-decade shock distributions are both normal[8]. We do this at the state-decade level by simultaneously but independently drawing a set of idiosyncratic shocks (one for each district) and one state-decade shock from the two distributions and then adding them to the baseline actual vote in the district in that chamber-state-decade. In other words, we simulate:

$$VS_{s,d,c,j,i} = VS_{s,d,c,j} + \eta_{s,d,i} + \epsilon_{s,d,c,j,i} \tag{5}$$

where $VS_{s,d,c,j}$ is the actual vote share in the district, $\eta_{s,d,i}$ and $\epsilon_{s,d,c,j,i}$ are the simulated shocks in the i^{th} simulation for the state-decade and $VS_{s,d,c,j,i}$ is the simulated vote share for the district. We do this for all district in both chambers simultaneously and compute, given the law in the state-decade, the fraction of times the simulation results in legal control for Republicans and the fraction of time it results in legal control for Democrats. For each, state-decade we simulate 10,000 times in order to compute the probability of legal control. We thus simulate the ex-ante probability of legal control for the Democrats and the

[8]We present histograms of the error terms to verify this. These figures can be seen in Appendix Figure A2

ex-ante probability of legal control for the Republicans for each state-decade.

We then choose a sample of state-decades within 3% of legal control and implement a regression discontinuity estimator identical to equation (3). Following (Kirkland, Phillips et al. (2018); Kirkland and Phillips (2020)), we define the running variable as the minimum aggregate shock necessary to shift the legal control status. We both estimate the impact of legal control due to marginal legislative control and, as a check on our methods, due to marginal gubernatorial control. In the former, we compare state-decides with and without legal control due to a small difference in vote shocks determining unification of the legislature with the governor. In the latter, we compare state-decades with a unified legislature where small differences in voting outcomes lead to gubernatorial unification with the legislature.

We differ from (Kirkland, Phillips et al. (2018); Kirkland and Phillips (2020)) in a number of respects. First, we use normally-distributed shocks rather than uniform distribution shocks. Second, we validate that vote shocks in fact are normally distributed. Third, we base our computations of probabilities on both statewide shocks and idiosyncratic district shocks whereas (Kirkland, Phillips et al. (2018); Kirkland and Phillips (2020)) assume all shocks are aggregate. Fourth, we estimate our shock structure empirically using a random effects model (estimated using Maximum Likelihood) rather than imposing a uniform [0,20] shock structure.

We additionally develop a second estimator based directly upon our simulated probabilities of legal control. In particular, we match on our probabilities of legal control and compare within probability bins federal seat share outcomes for those with and without legal control respectively, Due to small samples, we limit ourselves to two thick bins: (1.) "moderate" state-decades with between 40% and 75% probabilities of legal control and (2.) "extreme" state-decades with between 75% and 100% probabilities of legal control.

Our outcome variable for these estimators is the average seat share of the Congressional delegation in the state in the three elections following redistricting.

Since our regression discontinuity estimator contains two generated regressors (the probability of Democrat control and the probability of Republican control), we bootstrap our estimates in two steps. First, we bootstrap the probabilities and second, given the bootstrapped probabilities, we block bootstrap our regression discontinuity estimates at the state-decade level. Block bootstrapping in the second stage at the state-decade level is consistent with clustering at the state-decade level in the fixed effects and gubernatorial regression discontinuity estimators. We bootstrap with 100 replications.

IV. Data

A. Vote Shares and Seat Shares

Our main dependent variable is the Republican two-party seat share of a state's delegation in a given Congress. We collect this data from Congressional Quarterly at the State-Congress level. We additionally collect the Republican two-party vote share for use as a control variable in a robustness check. The vote share data also comes from Congressional Quarterly.

B. Legal Control and Unified Control

Our main independent variable is legal control by a party over redistricting. We compute this using state partisan control data from Klarner et al. (2013). We obtain this back to 1968. This data is available through 2011. From 2012 onward we collect state partisan control data from the National Conference of State Legislatures' legislative partisan composition tables. We thus have a balanced panel of states from 1968 through 2016.

In order to determine whether a party controls the redistricting process in each state-decade, we collect data on how redistricting is conducted. For each state, we collect the state's statutory and constitutional rules for the redistricting process, including any changes to the rules over time. We code each state-decade as one of (1.) Single district state, (2.) Legislature + Governor state, (3.) Legislature only state, or (4.) Commission. If the state has a commission, we furthermore classify it as an advisory commission if it merely provides a recommendation or a statutory commission if it has legal authority to pass a redistricting plan. We classify each commission as partisan or non-partisan depending upon whether a majority of commission members can be appointed in a partisan manner. In our main specification, we treat all commission types as selected in a non-partisan manner. We combine requirements for passing redistricting bills with information on seats shares in years ending in 1 to classify state-decades into Democratic Control, Republican Control and Bipartisan Control. We use further sub-classifications of commission types for robustness checks in Table 4.

From 2000 onward, data on rules comes from Justin Levitt's website. For the pre-2000 period, we employed a team of undergraduates to collect documents from individual state legislatures and from the National Conference of State Legislatures. We present and document our main treatment variable in the Data Appendix.

As a robustness check, we also use the data on legal control from (Friedman and Holden, 2009). This data goes from 1969 through 2004. It was assembled by Friedman and Holden based upon prior work by Cox and Katz (ICPSR 6311) and subsequent work by Gary Jacobson [9].

[9] Richard Holden graciously provided us with the data

Finally, we also estimate the impact of legal control over redistricting on the share minority of the state's delegation. We compute minority share of a state's delegation from lists of all current and historical minority legislators maintained by the House of Representatives on its website.

V. Main Results

In this section, we present our main results. We begin by documenting (1.) that districts almost exclusively change boundaries between federal elections happening in years ending in 0 and those happening in years ending in 2 and (2.) that substantially more redistricting occurs when the Republican party has legal control over the redistricting process. We then show our main estimates of the effects of legal control on a state delegation's partisan seat share, starting with our state-decade and year fixed effect model followed by the regression discontinuity and simulation-based models.

A. Measuring the Extent of Redistricting

We begin by quantitatively measuring the extent of redistricting in a state-decade. For this purpose, we assume that redistricting happens during years ending in 1 and thus is in effect for Congresses elected in years ending in 2. To document that this is almost always the case, we create a quantitative measure of the extent of redistricting. We use ARC-GIS to geocode every congressional map from every state for every Congress between 1968 and 2018. When there is a change in district boundaries between two Congresses, we compute the geographical overlap between each pre-existing Congressional district and each new district. For each pre-existing district, we assign to it a unique new district with which it has maximum geographical overlap. We then sum over all pre-existing districts and compute the fraction of overlap as a share of all land. We thus compute:

$$\sum_{i=1}^{N} \frac{max_{j(i)} \left(|D^A_{j(i)} \cap D^B_i| \right)}{|D_{Total|}} \tag{6}$$

where N is the number of districts before redistricting, $|D^A_{j(i)} \cap D^B_i|$ is the land area in square miles of the intersection between the j^{th} district after redistricting and the i^{th} before redistricting, and $|D_{Total}|$ is the square mileage of the state.

Thus, we compute the change in land area in every district[10]. Our results are presented in Figure 2. We show our measure separately for Congresses elected in

[10]We compute this measure based upon land area rather than population since census tracts, which are population-based, because census tracts were only introduced across the entirety of the United States for the 2000 census.

years ending in 2 and for Congresses elected in years not ending in 2. Moreover, we do this for both the full sample as well as the more recent sample incorporating only the past two decades. Both over the full sample and in the recent sample, almost all state-decades in our sample do redistrict and almost all do it between elections in years ending in 0 and elections in years ending in 2. For elections ending in 2, approximately 30% shift between 1% and 10% of their land across districts; approximately 25% shift between 10% and 20% of their land; well over 90% of state-decades shift less than 40% of their land and all shift less than 50% of their land. By contrast, in other years, almost 100% of states have no change in district boundaries relative to the election two years prior. This is even true in the past two decades when there have been more legal challenges to redistricting.

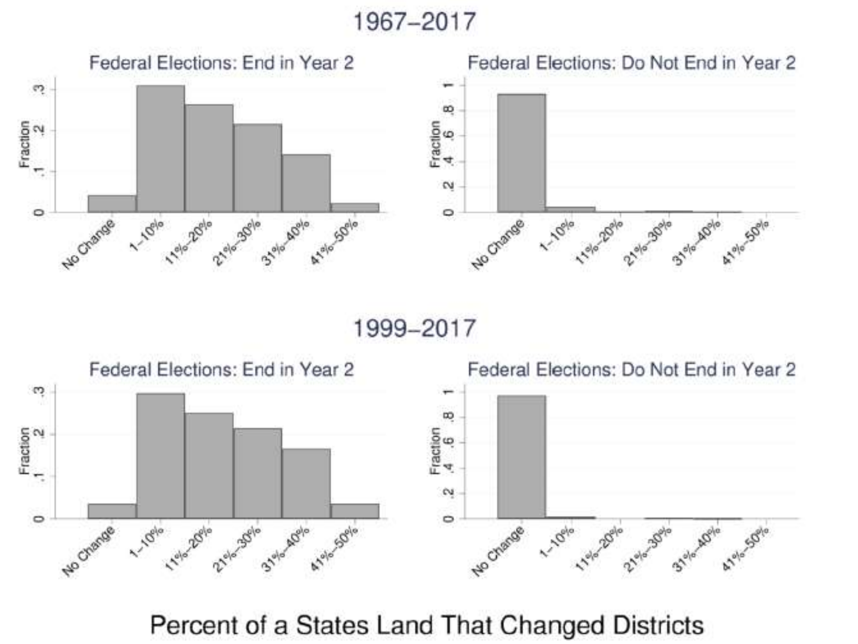

Figure 2. Changes in Land

We then ask whether a higher fraction of land is redistricted when there is unified control. We follow our main specification in Equation (1) and regress our measure of the extent of redistricting on our two partisan control variables, stateXdecade fixed effect, and year fixed effects. We show our results in Table 1. Our preferred specifications, in columns 2 and 5, use the full sample and the 2000+ sample respectively. The full five-decade sample show that 6.3% more land is redistricted when Republicans have control relative to no party having legal control; these results are significant with a standard error of 0.031. The coefficient for Democrats, by contrast, is less than $\frac{1}{4}^{th}$ the size at 1.4% and very far from statistically significant at conventional levels. In the past two decades, the coefficient for Republicans is even larger; Republicans shift 9.8% more land when they control redistricting than when no party does. The coefficient for Democrats is also larger; it is 4.5% but not statistically significant at conventional levels.

Table 1—District Changes from the Prior Election Period

	(1)	(2)	(3)	(4)	(5)	(6)
Republican Control: Effect on District Change						
Control x Election Ending in 2	0.070**	0.063*	0.097**	0.084*	0.098**	0.084*
	(0.029)	(0.031)	(0.040)	(0.043)	(0.040)	(0.043)
Control x Election Ending in 4		-0.012			-0.005	-0.006
		(0.009)			(0.005)	(0.006)
Control x Election Ending in 6		-0.009			0.001	0.000
		(0.008)			(0.002)	(0.002)
Control x Election Ending in 8		-0.005			0.006	0.005
		(0.013)			(0.004)	(0.004)
Democrat Control: Effect on District Change						
Control x Election Ending in 2	0.009	0.014	0.040	0.017	0.045	0.021
	(0.022)	(0.024)	(0.040)	(0.044)	(0.042)	(0.046)
Control x Election Ending in 4		0.002			-0.010	-0.014
		(0.008)			(0.008)	(0.010)
Control x Election Ending in 6		0.005			0.015	0.016
		(0.010)			(0.020)	(0.023)
Control x Election Ending in 8		0.013			0.016	0.016
		(0.010)			(0.016)	(0.017)
Sample	All	All	2000+	2000+	2000+	2000+
Outcome Basis	Land	Land	Land	Pop	Land	Pop
Number of Observations	1060	1060	420	420	420	420
R2	0.641	0.642	0.667	0.681	0.668	0.683’

Each column display coefficients from a single regression. The dependent variable is the fraction of land within a state changing districts since the prior decade in Columns 1 and 3, the fraction of land within a state changing districts since the prior election in Columns 2 and 5, the fraction of population within a state changing districts since the prior election in Column 4 and the fraction of the population within a state changing districts since the prior election in Column 6. The treatment variable is legal control in the years ending 1. State-year level regressions are conditional upon state-decade and year fixed effects. State-decade level regressions are conditional upon decade and state fixed effects. Columns 1-2 estimate over the full sample. Columns 3-6 estimate over the 2000+ sample.

Of course, it is possible that Republicans have legal control and are more dominant in more rural areas where larger shifts in land do not substantively translate into larger shifts in population. To address this concern, we also show the same results with a population-based rather than land-based measure of district change. These estimates show the percentage of people rather than land who switch districts as a result of redistricting. We show these population-based results in Column 6. Due to data constraints, we only show results for the past two decades. Overall, the estimates are of similar magnitude though slightly less precise. Nonetheless, effects are still statistically significant for Republicans at well below a 10% and barely above a 5% level.

B. Effects on Seat Shares: Fixed Effect Estimates

We now present our main results of the impact of partisan legal control on partisan seat shares, estimated using stateXdecade and year fixed effects. Our variation comes from comparing differences within states over time in seat shares across state-decades where a party has legal control to other states where it does not. We additionally control for electoral waves with year effects. It is crucial for our results that we have enough instances of legal control. In Figure 3, we show four histograms which display the number of instances of partisan control separately for each party by size of state delegation. We show histograms for both Democrats and Republicans for the full sample as well as separately restricted to the past two decades. In fact, Republicans only had 12 instances of partisan control in the first three decades of our sample. Seven of these 12 instances are from the 1970s.

Once we drop single district states as well as the state of Nebraska, which did not have partisan elections at the state level, we end up with 212 state-decades in our sample. Out of these, we find 62 instances of Democratic control over redistricting. In contrast, we find only 32 instances of Republican control. This is due to the dominance of Democrats in the earlier portion of our sample. In fact, when we restrict to the past two decades, we see 20 instances of Republican partisan control but only 14 of Democratic control. In part, this more recent Republican dominance is due to historic losses of control by the Democratic party in the 2010 elections.

In general, the size distribution of states skews slightly larger for Republican control than it does for Democratic control. This may seem somewhat surprising. However, many of the larger Democrat-dominated states either have had Republican governors (California, New York) during redistricting or have used redistricting commissions (California, New Jersey, Washington).[11]

Our main estimates are presented in Table 2. The results are split into two panels: a top panel for the effect of Republican control and a bottom panel for the effect of Democratic control. The coefficients are jointly estimated in a single regression for a given column across panels. Different columns represent different regressions, estimated using Equation (1). The first four columns show estimates from the full sample and the second four columns show estimates restricted to the past two decades. For each sample, we show results for states with more than one representative, more than two representatives, more than five representatives and more than 10 representatives respectively.[12] We do not ever include single representative states because they do not redistrict.

[11] California moved to a commission system during our study period.

[12] We compute the number of representatives in a state-decade as given by the number of representa-

Table 2—Main Specification

	All Years				2000 Onward			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Republican Control: Effect on Republican Representative Seat Share								
Control x Election Ending in 2	0.047**	0.048**	0.063**	0.080***	0.091**	0.065***	0.076**	0.078*
	(0.023)	(0.023)	(0.027)	(0.024)	(0.035)	(0.024)	(0.028)	(0.038)
Control x Election Ending in 4	0.002	0.024	0.022	0.040	0.059*	0.054*	0.071**	0.075
	(0.043)	(0.039)	(0.040)	(0.050)	(0.034)	(0.029)	(0.029)	(0.048)
Control x Election Ending in 6	0.034	0.058	0.044	0.064	0.094	0.071*	0.084**	0.078
	(0.048)	(0.046)	(0.038)	(0.047)	(0.058)	(0.039)	(0.039)	(0.059)
Average Effect	0.028	0.043	0.043	0.061	0.082**	0.063**	0.077**	0.077
	(0.035)	(0.032)	(0.034)	(0.039)	(0.040)	(0.028)	(0.031)	(0.046)
	[0.452]	[0.190]	[0.223]	[0.129]	[0.029]	[0.035]	[0.024]	[0.107]
Control x Election Ending in 8	-0.000	-0.008	-0.048**	-0.014	0.022	0.002	-0.047*	-0.010
	(0.041)	(0.040)	(0.020)	(0.021)	(0.045)	(0.039)	(0.026)	(0.033)
Democrat Control: Effect on Republican Representative Seat Share								
Control x Election Ending in 2	-0.018	-0.009	-0.014	-0.027	-0.003	-0.022	-0.096**	-0.127**
	(0.023)	(0.021)	(0.017)	(0.023)	(0.045)	(0.041)	(0.040)	(0.042)
Control x Election Ending in 4	-0.010	0.020	0.030	0.024	-0.015	-0.019	-0.105**	-0.131**
	(0.031)	(0.026)	(0.024)	(0.029)	(0.050)	(0.049)	(0.039)	(0.053)
Control x Election Ending in 6	0.016	0.050	0.049	0.028	0.027	0.004	-0.066	-0.119**
	(0.033)	(0.031)	(0.029)	(0.039)	(0.066)	(0.052)	(0.041)	(0.051)
Average Effect	-0.004	0.021	0.022	0.008	0.003	-0.013	-0.089**	-0.126**
	(0.025)	(0.023)	(0.019)	(0.028)	(0.049)	(0.043)	(0.038)	(0.044)
	[0.875]	[0.354]	[0.273]	[0.763]	[0.954]	[0.754]	[0.077]	[0.033]
Control x Election Ending in 8	-0.055**	-0.041	-0.015	-0.016	-0.035	-0.050	0.024	0.008
	(0.026)	(0.026)	(0.018)	(0.016)	(0.051)	(0.050)	(0.025)	(0.031)
Sample	All	All	All	All	2000+	2000+	2000+	2000+
Size Restriction >	1	2	5	10	1	2	5	10
Republican Treatments	32	30	22	14	20	20	15	10
Democrat Treatments	62	57	42	18	14	14	8	4
Number of Observations	1060	900	675	295	420	370	260	120
R2	0.778	0.840	0.851	0.846	0.869	0.877	0.922	0.856

Each column presents coefficients from a single regression. Each observation is a state-year. Treatment is unilateral legal control of a political party over redistricting. The dependent variable is the state's Republican share of seats in federal House of Representatives. All specifications include state-decade and year fixed effects. Columns 1-4 use data from 1967-2017. Columns 5-8 use data from 1999-2017. Size restrictions along columns restrict the sample to states with more than the listed number of representatives. Rows show estimates of the effect of legal control. Average effect reports the average of the effects for elections for federal elections ending in 2, 4, and 6 respectively. Average effects represent the mean of the coefficients on elections year ending in 2, 4, and 6. Control X Election Ending in 8 represents the coefficient on control for elections in the year 8 from the previous decade. Standard errors clustered by state are in parentheses. P-values from the Wild Cluster bootstrap are presented in square brackets for average effects.

We begin by analyzing the full five-decade sample. In the first election after redistricting, we find a 4.7 percentage point increase in a state delegation's Republican seat share following Republican control over redistricting. The effect sizes rise to 8.0 percentage points when we restrict to states with more than 10 representatives. These results are all statistically significant with at least a 95% level; effect sizes for recent decades are larger. However, the coefficients for later elections are smaller and statistically insignificant.

In columns 5-8, we present estimates restricted to the past two decades, these estimates are larger in magnitude. In the full sample of states with more than one representative, Republican control leads to a 9.1 percentage point increase in a state delegation's Republican share following Republican redistricting control. Estimates with state delegation size restrictions are smaller but all estimates lie at 6.5 percentage points or above and all are statistically significant with a 0.06 p-value or less.[13]

In the second-to-bottom row of the panel, we average effects over the three elections following redistricting. Since we normalize our estimates to the outcomes of the last election before redistricting (elections in years ending in zero) and we define decades as beginning in the election before that (elections in years ending in eight), we can at most estimate the impact on the subsequent three elections after redistricting. The full sample estimates show a statistically insignificant positive coefficient of 2.8 percentage points for Republican control. Independent of size restrictions, we do not see statistically significant increases in the Republican share of a state's delegation following Republican-controlled redistricting. However, in the past two decades, we do see large and statistically significant average increases in the Republican seat share of a delegation following Republican redistricting. Our benchmark estimate yields an 8.2 percentage point increase on average over the following three elections. These estimates are statistically significant at conventional levels. Due to concerns over cluster sizes[14], we also present Wild-Cluster bootstrapped p-values in brackets for average effects[15]; results are similar. Estimates are 6.3 percentage points in states with more than two seats and 7.7 percentage points restricted to both states with more than five as well as more than ten seats respectively.

In contrast to the effects we find of Republican control, we do not find statistically significant or large decreases in Republican seat share after Democratic-controlled redistricting. This is true whether we estimate over the full fifty year sample or whether we restrict to recent years. The reduction in the Republican seat share even in the initial election after Democratic control over redistricting is

tives elected in years ending in 2 - i.e. just after federal reapportionment.

[13]Note that the t-statistic for the 10 or more size restriction is above 2 but because of the substantial degrees of freedom correction, the p-value is 0.06.

[14]There are 44 states in the main sample and 14 states in the sample where we restrict to states with more than 10 representatives

[15]We do not show wild cluster bootstrap p-values for individual year coefficients in order to limit the size of the table. However, wild cluster bootstrap p-values are available from the authors upon request.

1.8 for the full sample and 0.3 in the recent period. We do see large, statistically significant effects only in the recent period when looking at large Democratic states. Restricting to the eight instances in the past two decades with Democratic control in states with more than five seats, we find an average decrease in the Republican seat share of 8.9 percentage points; restricting to states with more than ten seats, we find a decrease of 12.6 percentage points. These estimates are statistically significant with a 95% level of confidence using conventional standard errors. Using the wild cluster bootstrap, both have a p-value less than 0.1 and the more than five restriction yields a p-value less than 0.05. Though these large estimates are estimated off of a small number of clusters: 8 for state-decades with more than 5 seats and 4 for state-decades with more than 10 seats. We will return to the implications of these findings later.

In the last row of each panel, entitled "Control X Election Ending in 8", we show the coefficients on partisan legal control for elections ending in 8. These coefficients are for the elections which took place before the last election before redistricting. Since all coefficients are normalized to the last election prior to redistricting, the coefficient reflects a pre-trend before redistricting. The results yield mostly small and statistically insignificant coefficients with the exception of the estimates restricted to states with more than five seats for the impact of Republican legal control and the main sample for Democratic control. The small number of statistically significant pre-redistricting effects are consistent [16] with what one would expect by random chance.

C. *Effects on Seat Shares: Regression Discontinuity Estimates*

We now present an alternative estimation strategy of the effect of Democrat and Republican legal control: a discontinuity estimate in the vote share for the gubernatorial election. In particular, we estimate off of a sample where small differences in the vote share for the governor led to a differences across state-decades in legal control by a party over redistricting. Of course, in order for gubernatorial races to be pivotal in determining legal control over redistricting, two things must be true. First, states must require both a gubernatorial signature and legislative approval in order to pass a redistricting bill. A sizable majority of states have this requirement in all decades. However, it does exclude commission states because parties do not control the redistricting process when map-making authority is delegated to a commission. It also excludes the states of Connecticut and North Carolina because they do not require a gubernatorial signature. Second, one party must have control over both chambers of the state legislature[17]. That way, a gubernatorial win for the party aligned with the unified state legislature will translate into legal control. We estimate separately by party for both the full

[16]Three of sixteen estimates are statistically significant at conventional levels.
[17]The only state that has only one chamber: Nebraska, is excluded from our sample because it also does not have partisan elections at the state level.

sample and for the recent period. We use a bandwidth of 0.1 across all samples though we show robustness to bandwidth in Appendix Figure A1. Our sample size is substantially smaller: 46 state-decades over the full sample for Republicans, 75 over the full sample for Democrats, 23 over the recent period for Republicans, and 22 over the recent sample for Democrats[18].

Overall, estimates are broadly similar to those using state-decade and year effects. We report our estimates in 3. We find large, statistically significant effects of Republican control in the past two decades but otherwise we find relatively small and statistically insignificant effects. Legal control by Republicans in the past two decades increases the Republican seat share in Congress by an average of 12.3 percentage points over the subsequent three elections. This is almost 50% larger than the same estimate using the state-decade and year fixed effects. However, the two estimates are far from statistically distinguishable at even a 90% confidence level. The similarity in the results are surprising for three reasons. First, the sample size for the regression discontinuity estimates are small and it is surprising that there is sufficient precision. Second, the samples are quite different and therefore it is not clear that the estimates would be the same. Even more important, the identification for the regression discontinuity estimation is from marginal gubernatorial elections[19] rather than all elections.

In Appendix Figure A1, we show four panels. Each panel shows a graph of RD estimates by bandwidth for each of our four samples. The overlaid blue line shows the cluster size (i.e. the number of state-decades) in the sample for the bandwidth. The estimates are quite robust, particularly for the effect of Republican control over the most recent two decades. The one exception is that estimates are substantially larger and even statistically significant at conventional levels for very low bandwidth estimates of Republican control over the full sample. These estimates are only statistically significant for a cluster size below ten where the standard errors are not asymptotically valid. Moreover, the estimates are large because the full sample only reflects the last two decades for small bandwidths.

Overall, we find robust evidence of an 8 percentage point to 12 percentage point increase in Republican representation in Congress following Republican legal control over redistricting in the 2000+ time period but we otherwise find little effect of legal control.

D. Effects on Seat Shares: Simulation Estimator

In this subsection, we present our estimates from our novel simulation estimator. We present estimates of the impact of legislative control where we fix the party of the governor in Columns 1, 2 and 3 of Table 4 and we present estimates of

[18]Our sample size is five times the number of state-decades since we have five observations per decade. However, the number of state-decades does determine our cluster size given that we cluster at the state-decade level.

[19]Note that since the vote share fully determines unified control, the local average treatment effect is the average treatment effect at the discontinuity.

Table 3—Gubernatorial Regression Discontinuity: Analytical Weights

	All Years		2000 Onward	
	(1)	(2)	(3)	(4)
Republican Control				
Twoway Vote Share	-0.924	-1.025	0.153	0.564
	(0.602)	(0.685)	(1.245)	(1.149)
Control x Election Ending in 2	0.058	0.069	0.106*	0.158**
	(0.073)	(0.078)	(0.060)	(0.072)
Control x Election Ending in 4	-0.012	-0.001	0.134*	0.186**
	(0.077)	(0.086)	(0.068)	(0.079)
Control x Election Ending in 6	0.019	0.031	0.128*	0.180**
	(0.086)	(0.096)	(0.070)	(0.081)
Average Effect	0.022	0.033	0.123*	0.174**
	(0.055)	(0.066)	(0.066)	(0.076)
Control x Election Ending in 8	0.010	0.021	-0.100	-0.050
	(0.087)	(0.094)	(0.072)	(0.079)
Bandwidth	0.100	0.100	0.100	0.097
Number of Observations	230	230	115	110
R2	0.026	0.037	0.095	0.136
Democrat Control				
Twoway Vote Share	-1.558**	-1.512*	-0.054	-0.065
	(0.595)	(0.894)	(2.228)	(2.086)
Control x Election Ending in 2	0.003	0.022	0.009	0.015
	(0.052)	(0.057)	(0.140)	(0.131)
Control x Election Ending in 4	0.010	0.022	0.048	0.053
	(0.061)	(0.062)	(0.148)	(0.137)
Control x Election Ending in 6	0.024	0.037	0.029	0.034
	(0.054)	(0.055)	(0.143)	(0.133)
Average Effect	0.012	0.027	0.029	0.034
	(0.054)	(0.056)	(0.143)	(0.133)
Control x Election Ending in 8	-0.003	0.012	-0.091	-0.081
	(0.052)	(0.053)	(0.136)	(0.129)
Bandwidth	0.076	0.074	0.078	0.078
Number of Observations	375	320	90	90
R2	0.038	0.059	0.033	0.027
Decade FE		X		X

Each column presents the average of coefficients from a specification. Running variable is state governor's vote share. Treatment is legal control based on state governor's vote share. The dependent variable is the state's Republican seat share in the federal House of Representatives. Top panel shows estimates of the effect of legal control by Republican on seat shares in federal elections. Bottom panel shows estimates of the effect of legal control by Democrat on seat shares in federal elections. Average effect reports the mean of the coefficients on elections year ending in 2, 4, and 6. Column 1 and 3 are regular OLS regressions. Column 2 and 4 add decade fixed effects. Standard errors clustered by state$_{d}ecadeareinparentheses.BandwidthsofregressiondiscontinuityaretunedusingLOOCV.$

gubernatorial control where we fix control of the legislator in Column 4 of Table 4.

Our full sample estimates are very similar to what we estimate using the the state-decade and year F.E. model, which is reassuring. Also, the simulation-based gubernatorial RD estimates are overall similar to the standard gubernatorial RD model whose results we presented in Table 3. Combined, these provide validation both for our prior results and simultaneously also for our simulation-based estimation method.

Table 4—Impacts of Legal Control Due to Legislative Control

	(1)	(2)	(3)	(4)
All Years				
Rep Average Effect	0.038	0.029	0.055	0.018
	(0.048)	(0.059)	(0.069)	(0.039)
Dem Average Effect	-0.006	0.010	-0.030	-0.038
	(0.036)	(0.060)	(0.033)	(0.037)
Number of Observations	750	225	610	605
2000 and Onward				
Rep Average Effect	0.129**	0.054	0.147**	0.139**
	(0.054)	(0.050)	(0.067)	(0.065)
Dem Average Effect	0.057	-0.087***	-0.038	-0.000
	(0.055)	(0.026)	(0.051)	(0.062)
Number of Observations	270	110	270	225
Specification	Matching	Matching	RD	RD
Legislatures	Extreme	Moderate	All	All
Randomization	Legis	Legis	Legis	Governor

Each column within a panel presents the average of coefficients from a single regression. The top panel presents coefficients over the full sample from 1969-2017. The bottom panel presents coefficients from a restricted panel spanning 1999-2017. All specifications include state-decade and year fixed effects. Treatment is unilateral legal control of a political party over redistricting. In columns 1-2 uses binned matching. In column 1 extreme legislatures are those with a unfication probability between 75% and 100%. In column 2 moderate legislaturesare those with unfication probability between 40% and 75%. Column 3 uses an RD based on the minimum vote shock needed to induce a change in unification with a bandwidth of 3%.

Turning to the recent decade sample, we show the results for our simulation-based matching estimator in Columns 1 and 2 where Column 1 presents results for 'extreme' legislatures with probability of legal control between 75% and 100% and Column 2 presents estimates for moderate legislatures with probability of legal control between 40% and 75%. We find a modest and statistically insignificant 5.4 percentage point effect of legislative control for moderate Republican legislatures and a large 12.9 percentage point effect for extreme legislatures which is statistically significant at a 95% level. One possible explanation of these results is that in marginal, moderate legislatures, a larger fraction of legislators have to approve of gerrymanders and the majority party is more moderate on average.

This makes passing a gerrymander more difficult than in a more uniform legislature. However, it is also possible that moderate legislatures might redistrict more because they are less likely to have the opportunity in the future. For Democratic control, we find estimates consistent with this latter theory. Our results follow the opposite pattern of what we found for Republican legislatures. We find a statistically significant increase of 8.7% in the Democratic seat share with moderate Democratic control but a statistically insignificant reduction with extreme Democratic control.

E. Robustness

In Table 5, we show the robustness of our results to alternative specifications and data. Our estimates are largely robust. In column 1, we repeat our baseline estimates. In column 2, we replace our legal control variable with unified control as our main treatment variable. [20] In column 3, we use the treatment variable from the Friedman-Holden data (Friedman and Holden (2009)). The Friedman-Holden data end in 2004 and thus we extend their cutoff year past 2004 using their method of classifying partisan redistricting.[21] The estimates for the impact of Republican control over the full sample are slightly more than double those in the main sample but remain statistically indistinguishable from zero at conventional levels. In column 4, we control linearly for the statewide vote share for House of Representatives races to account for time-varying political preferences of the electorate. In other words, we control linearly for the seat-share/vote-share map. We do this because we are concerned that legal control may be endogenous to partisan preference shocks at the state level. Our estimates decline slightly to 6.9 percentage points and statistical significance falls slightly to just below the 95% level of confidence. We do not see any sizable or statistically significant estimates for Democrats.

In column 5, we present estimates in which we drop commission state-decades from our sample. The coefficient on Republican control for the recent sample rises by 3.6 percentage points when we drop states with an electoral commission for all specifications; in particular, the Republican effect in the recent sample becomes statistically significant at the 10% level. This potentially suggests that commission states redistrict in a slightly more partisan manner than states with divided government. However, the differences are not large. In fact, the other estimates including the estimates restriction to the prior two decades are within one percentage point of the benchmark estimates.

In column 6, we show our two way fixed effects estimates. The estimates are substantially larger though still statistically insignificant at conventional levels

[20] Unified control is usually used in the political science literature to look at the impact of control over redistricting because of the costs of collecting the legal control variable.

[21] Even though we impute Friedman-Holden past 2004 for the purposes of main sample estimation, we do not report estimates using the Friedman-Holden data for the 2000+ time period since their actual data only covers 20% of the time period.

Table 5—Robustness and Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All Years								
Rep Average Effect	0.028	0.032	0.062	0.032	0.031	0.113***	0.036	0.096***
	(0.035)	(0.030)	(0.038)	(0.032)	(0.038)	(0.031)	(0.041)	(0.033)
Dem Average Effect	-0.004	0.000	0.007	-0.014	-0.007	-0.038	-0.007	-0.019
	(0.025)	(0.025)	(0.023)	(0.021)	(0.029)	(0.037)	(0.030)	(0.047)
Number of Observations	1060	1060	1060	1060	895	1060	1060	1060
R2	0.778	0.778	0.778	0.838	0.757	0.495	0.794	0.778
2000 and Onward								
Rep Average Effect	0.082**	0.072*		0.069*	0.107*	0.121***	0.096*	0.086**
	(0.040)	(0.042)		(0.036)	(0.055)	(0.036)	(0.051)	(0.038)
Dem Average Effect	0.003	0.002		-0.007	0.021	0.049	-0.005	0.025
	(0.049)	(0.049)		(0.040)	(0.059)	(0.073)	(0.066)	(0.068)
Number of Observations	420	420	420	420	320	420	420	420
R2	0.869	0.871	0.867	0.885	0.840	0.753	0.903	0.869
Legal Control	X							
Unified Control		X						
Holden Data			X					
Vote Share Control				X				
Exclude Commissions					X			
2-Way FE						X		
State Linear Trends							X	
Change in Control								X

Each column within a panel presents the average of coefficients from a single regression. The top panel presents coefficients over the full sample from 1969-2017. The bottom panel presents coefficients from a restricted panel spanning 1999-2017. All specifications include state-decade and year fixed effects. Treatment is unilateral legal control of a political party over redistricting. The dependent variable is the state's Republican share of seats in the federal House of Representatives. Rows show estimates of the average effect of legal control by a party on seat-shares in federal elections ending in 2, 4, and 6. Column 2 replaces legal control with unified control. Column 3 replaces our legal control variable with a similar measure due to Holden-Friedman. Column 4 adds in statewide Republican vote share in elections for the House of Representatives as a control. Column 5 excludes states with electoral commissions from the sample. Column 6 replaces state-decade fixed effects with state fixed effects. Column 7 adds state-specific linear time trends to the baseline model in Column 1. Column 8 drops decades with legal control where the same party had legal control in the prior decade. Standard errors clustered by state are in parentheses.

for Democrats. The effect size increases by 50% for Republican control in the recent period and by 300% in the full sample. Since effect sizes for Republicans are increasing over time, this is exactly what we would expect given the recent work in the new panel effects estimation literature. The differences between the two-way fixed effects design and the baseline state-decade fixed effects and year effects design precisely validate the need for our baseline design.

We then show results for our very taxing specification state-specific linear trends baseline model. We estimate this model out of concerns that state specific trends such as the realignment of the parties may induce a correlation both between legal control and increases in the dominant party's seat share. These could even happen within decades. Our estimates become less precise, likely due to over-fitting given the limited degrees of freedom. However, the estimates remain remarkably similar given the large number of covariates added. All estimates are within 1.4 percentage points of our baseline estimates.

Finally, we reclassify partisan legal control as non-partisan where the same party also had legal control in the prior decade. Since parties with ongoing legal control may not gerrymander much if their redistricting goals have already been achieved, we expect our coefficients to increase in magnitude, potentially by a sizable amount. In most cases of legal control over redistricting in our five decade sample, that same party did not have legal control in the prior decade. In column 8, we estimate on this sample of new legal control and we precisely find that all coefficients are larger in magnitude as well as of the same sign. The increase in the effect for Republicans is particularly large. Thus, our results are consistent with a time-invariant effect of new Republican control; the difference between the earlier and later time periods then is due to the greater prevalence of new Republican control in the recent period.

We also show robustness to our definition of legal control and present the results in Table 4. We consider two different types of robustness. In columns 2-3, we reclassify types of commissions and allow different ways of defining commissions as instances of partisan legal control. In the last three columns, we reclassify instances of a unified legislature with a governor of the opposite party as instances of partisan legal control. We do this because legislatures can often over-ride gubernatorial vetos with a strong enough majority. Moreover, legislatures can threaten governors to over-ride vetos on unrelated legislation. Since veto thresholds can vary by type of bill, we consider different levels of minimal legislative majority in columns 4-6 as instances of partisan legal control by the legislature in the cases of divided government with a unified legislature.

Returning to commissions, we define commission types and then discuss our different codings for the purposes of our robustness checks. A partisan commission is a commission which can be appointed with a net partisan balance. These commissions are often appointed by the Governor or the majority leaders of the state legislatures. Some commissions are appointed in a non-partisan or a bi-partisan (i.e. balanced in partisanship) manner.[22] An advisory commission draws maps and submits them to the legislature and governor for legislative and gubernatorial approval. However, advisory commissions have no legal authority to redistrict. A non-advisory commission draws maps and the maps are automatically accepted as law. Non-advisory commissions do not need gubernatorial or legislative approval. We consider redefining commissions as being party-controlled depending upon how they are selected and whether they have the legal ability to directly implement the maps that they draw.

In the first column, we present our baseline results. Then, We consider redefining advisory commissions as instances of partisan legal control when they are appointed in a partisan manner (column 2). We also estimate a model (column 3) where we redefine partisan legal control to include commissions when they are merely advisory (i.e. they do not have the ability to directly implement the maps they draw).[23] Column 2 shows estimates of partisan legal control with net partisan-appointed commissions redefined as instances of partisan legal control. The estimates barely change from our baseline estimates. In column 3, we show estimates with advisory commissions reassigned as partisan in the case of unified control.[24] All estimates show an increased positive impact on the Republican seat share, mostly by approximately two percentage points. These results suggest that advisory commissions do not necessarily reduce partisan redistricting though the differences in the estimates are due to a small number of state-decades and results are only sizable for the case of Republican unified control.

In the last three columns of Table 4, We also redefine legal control as partisan when the governor is a different party from the legislature but the legislature is unified and has over 60% (column 5) and over 66.7% (column 6) majority in both houses respectively. We do this because this may give the legislature the ability to pass a redistricting bill over a Gubernatorial veto. For Connecticut and Maine, which have legislative redistricting thresholds of 60% to pass a bill, regardless of gubernatorial approval, we redefine partisan control to those levels (column 4). None of these changes make a substantial difference in our estimates. Across all definitions, the variation in estimates are similarly small. We thus conclude from Table 4 that our estimates do not depend upon our particular definition of legal control.

[22]For example, in some states, commissions are composed of five members, one member appointed by each of the majority and minority leaders in each of the two chambers. The fifth member is then appointed by a majority of the four directly appointed members.

[23]We do not show estimates based upon samples where we redefine combinations of advisory/non-advisory and partisan/non-partisan appointment as partisan appointment because all non-advisory commissions are also appointed in a bi-partisan or non-partisan manner. Thus, redefining advisory commissions as partisan legal control when government is unified is the same as redefining only advisory commissions with partisan appointment under the same circumstances. Also, redefining partisan appointment state-decades as instances of partisan legal control is akin to redefining partisan appointment with advisory commissions. Since, in all of these cases, results are identical, we limit redefinitions based solely upon changing advisory commissions as well as partisan-appointed commissions individually.

[24]All the advisory commission states require passage of redistricting by a majority of each legislative chambers and the signature of the Governor.

One issue with our estimates is that observations are naturally heteroskedastic. A state-decade with two seats (i.e. Maine over the full sample) inherently changes the seat share by 0.5 when one seat changes party. However, a state-decade with 53 seats (i.e. California in the 2000s and 2010s) changes the seat share by less than 0.02 when one seat changes party. We thus re-estimate Tables 3 and 4, weighting them by delegation size. These estimates appear in Tables A1 and A2 in the Online Appendix respectively. We notice that the estimates are largely similar; however the standard errors are approximately 20% lower on average. The reduction in the standard errors is consistent with a reduction in heteroskedasticity.

Estimates of Democratic control do increase in Table A1 relative to Table 3 since larger states are more heavily weighted and effect sizes are larger for Democrats in larger states during recent period.[25] We note that since we include our baseline estimates in Tables 3 and 4, these estimates also appear in the Online Appendix Tables. Table A2, showing the weighted version of the legal control definition robustness table yields relatively similar estimates to Table 4. Also, estimates are similar across the different alternative definitions of legal control.

We additionally perform four quasi-placebos for the full sample as well as for the most recent two decades. For each sample period, these placebos lead to four placebo coefficients each for Republicans and for Democrats. In Table 5, we re-estimate effects as if redistricting were done by the state governments in power in years ending in 3, 5, 7, and 9 respectively. These are not actual placebos. If legal control in the treatment year persists, we may be picking up actual lags and leads of treated effects. Thus, it is all the more striking that we find only one statistically significant coefficient for Democrats or Republicans out of the eight quasi-placebo coefficients in the full sample and none in the 2000+ sample. Moreover, the true effect estimated for Republicans in the 2000+ sample is the largest in magnitude of the 20 coefficients. That would happen by random chance if all the estimates were independent (as noted above, they are not) with a probability of 5%. Also 15 out of the 20 estimates are less than half the size of our estimate of Republican control in the past two decades.

Overall, our robustness and heterogeneity tests find substantial support for partisan gerrymandering by the Republican party in recent decades and also for the Democratic party in recent decades only for large states.

We additionally show that following Republican legal control, Republican wasted votes in House elections went down in comparison with wasted votes for the Democrats. We compute the efficiency gap for House elections in presidential years, following (Stephanopoulos, 2017), and use it for our dependent variable. The wasted votes for a party in a given election is the sum across districts in the number of wasted votes. If a party loses in the district, the number of wasted votes

[25] Additionally, unified control estimates in the recent period become substantially smaller and statistically indistinguishable from zero. Since one of the contributions of our paper is to code and use legal control, we see the robustness of our legal control measure as opposed to the traditional unified control measure as further validation of the benefits of using legal control.

is the number of votes for the party. If a party wins in the district, the number of wasted votes is the number of votes above what they would have needed to win the district. If a party wins districts narrowly and loses by a large margin, then there will be a small number of wasted votes. However, if a party wins districts by a large margin and loses by a small one, then wasted votes will be very high. A party which gerrymanders should reduce wasted votes for its party and increase wasted votes for the opposing party. We compute wasted votes at the state level by summing wasted votes across districts within states for each party. If the district is sufficiently lop-sided, a race be uncontested in which case there will be no wasted votes for the losing party. In these cases, we follow (Stephanopoulos, 2017) and compute wasted votes for the district using the vote share for president. For this reason, we restrict our sample to presidential election years when presidential vote shares are available. Our results are presented in

VI. Aggregate Effects

We have so far estimated the impact of party legal control over redistricting on subsequent seat shares. What has the been the impact of this upon the aggregate balance in the House of Representatives? We now translate our estimates of average seat share impacts by party into aggregate partisan effects and compare them in size to partisan seat margins.

We do this by year and party. In particular, we use decade and party specific estimates for each year for both Democrats and Republicans and compute implied seat share changes, rounding to the nearest seat. We then multiply by the number of treated states and the average number of seats in each treated state. We also note when the changes would have resulted in a shift in the balance of the House of Representatives. Analytically, we compute:

(7)
$$\Delta DemSeats_{y,d} = 2XInt\left[\beta_y^D N_{d,y}^D I\left(Control_d^D\right)\right] - Int\left[\beta_y^R N_{d,y}^R I\left(Control_d^R\right)\right]$$

where $\beta_{y,d}^P$ is the effect of party P control on a state's seat share fraction for party P in year$y$ and decade d, $N_{d,y}^P$ is the number of seats in that state's delegation in decade and year, and $I\left(Control_d^P\right)$ is a dummy which takes on a value of 1 if party P has legal control over redistricting in decade d.

We show the results of these computations in Table **??**. Overall, we find little evidence of a sizable shift in partisan balance in the House of Representatives until the 2000s. Before the 2000s, net effects are no more than five seats. In the 2000s, we compute that seats shifted by 12 seats towards the Republicans and in the 2010s, we see a shift of 27 seats towards the Republican party. The reason for the small net effects through most of the past 50 years but much larger recent effects is due to a combination of two factors. First, the effect of partisan control upon seat shares has increased over time. We can see this in the difference between the effects for the 2000+ period and in the full period. We can also this by looking at the differences in partisan control in the 1970s and in 2000s. Second, state legislatures have shifted from overall Democratic dominance to overall Republican dominance. This is partly due to realignment and the shift of the South of the United States to the Republican party as well as to the poor performance of the Democratic party in the 2010 election which were critical for redistricting. This has been consequential because this switch in dominance has been from a party with a low impact of legal control on seat shares to one with a high impact of legal control on seat shares. For example, in the 2000s, the Democrats had legal control in two more states than the Republican party. Moreover, the average delegation size in both Republican and in Democratic legal control states was 13. However, because of the greater impact of Republican legal control, our estimates imply a net shift of 12 seats to the Republican party.

Table 6—Aggregate Partisan Effects by Decade

	1970s	1980s	1990s	2000s	2010s
States with Dem Control	15	16	17	8	6
Average Seats, Dem Control	8	10	9	13	8
States with Rep Control	7	3	2	6	14
Average Seats, Rep Control	12	14	3	13	12
Seat Share Effect: Dems	1	1	1	1	0
Seat Share Effect: Reps	5	2	0	13	27
Net Effect	4	1	1	12	27
Average Margin	95	86	62	21	53
Net Effect as % of Avg Margin	4%	1%	2%	57%	51%

Each column presents numbers for a particular decade. States with Dem Control and States with Rep Control show the number of states with Democratic and Republican legal control in the decade respectively. Average Seats is the average number of seats after redistricting in states with Democratic and Republican legal control respectively. Seat Share Effect presents a back-of-the-envelope computation of the gross number of seats gained from legal control over redistricting, broken down by party. Net effect is the absolute value of the net change in seats as a result of redistricting. Average margin is the average of the absolute value of the difference between Republican seats and Democratic seats in the Congresses elected in the years ending with 2, 4 and 6 in the decade.

Interestingly, we do not find that greater imbalance in legal control over redistricting plays an important role. In fact, imbalances were much larger earlier in the sample. In the 1970s, Democrats held control in 8 more seats than the Republicans; in the 1980s, this increased to 13 and in the 1990s, it increased to 15. In the past two decades, precisely when legal control has become more consequential for net partisan balance, these gaps have fallen to 2 in the 2000s and 8 in favor of the Republicans in the 2010s.

Overall, we find an increased role for legal control over redistricting in recent decades. This increased impact is driven by two main factors: (1.) the rise in the effect of legal control and (2.) the switch from the lower impact Democratic party to the higher impact Republican party.

VII. Mechanisms and Discussion

In this section, we discuss and provide empirical evidence on three commonly espoused theories of the differential rise in partisan redistricting: (1.) Democrats have chosen to pack minority voters in order to ensure minority representation in Congress even at the sacrifice of overall seats, (2.) Southern realignment has meant undoing the solid Democratic South and (3.) Democrats have disproportionately delegated redistricting to commissions in large states where impacts of partisan redistricting would have been high. We provide evidence on each of these three theories in Table VII.

We first consider the theory that Republicans are undoing Democratic gerrymanders but that Democrats do not have Republican gerrymanders to undo. The Democrats had historically retained near complete control over Southern politics. In fact, the South was referred to as the 'Solid South' because of the uniformity of its support for the Democratic party. In the 1990s, that support began to wane in Congress and subsequently began to wane in the state legislatures. One possible reason for the difference in impact between Republican and Democrat legal control is that the Republicans were undoing Democrat gerrymanders whereas the Democrats were not undoing Republican gerrymanders. We explore this by estimating the impact of legal control restricted to cases where the party in control had not had legal control in the prior decade. This would, for example, be true of the all cases of Republican legal control in the South. If this theory is correct, the impact of Republican legal control should not change much but the impact of Democrat legal control should rise dramatically. We present our estimates in Column 2 of Table VII. Overall, our estimates in Column 2 look very similar to our baseline estimates in Column 1 for both parties. We do not find support for the theory that the differential rise of Republican gerrymandering is driven by the prior history of Democratic gerrymandering.

We now turn toward our second theory of the differential rise of partisan redistricting: Democratic preference for minority representation. One possible reason for the differential change, which has received attention in the press, is the rise of preference for minority representation within the Democratic party. The argument for this theory is that Democrats have concentrated minorities into districts in order to increase minority representation but that this comes at the expense of overall Democrat representation. We address this theory directly by estimating the impact of legal control for both Democrats and Republicans on the minority share of the delegation. Table VII shows that in the full sample, Democratic legal control increased by a statistically significant 2 percentage points. However, there are a few problems with this explanation. First, the increase in minority representation is roughly the same in the recent period as in the full sample and thus does not explain the differential change over time. Second, it is possible that Democratic redistricting leads to more minority representation because the Democrats are only moderately successful at gerrymandering, not good enough to detect it in aggregate seats but able to detect it in lower variance minority representation. Third, and most important, when we break up the impact into minority representation for Republicans and minority representation for Democrats, we do not find statistical significance. However, we do see *negative* and sometimes sizable estimates on Democratic minority representation and *positive* for Republicans. This is exactly the opposite of what the standard minority representation theory would suggest. We also note that we see no evidence for the related theory that Republicans have redistricted so that Democrats gain greater minority representation as a way to increase Republican seat shares. We see very small and statistically insignificant impacts on minority representation both in the full and in the recent samples. The evidence we present is in columns 3, 4, and 5 of Table VII.

Table 7—Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All Years							
Rep Average Effect	0.028	0.096***	-0.015	0.005	-0.004	0.026	0.018
	(0.034)	(0.030)	(0.012)	(0.021)	(0.008)	(0.106)	(0.109)
Dem Average Effect	-0.004	-0.019	0.020**	0.027	0.004	-0.024	-0.022
	(0.029)	(0.046)	(0.009)	(0.024)	(0.006)	(0.060)	(0.061)
2000 and Onward							
Rep Average Effect	0.082**	0.086**	-0.022	0.009	-0.002	0.069*	0.066*
	(0.037)	(0.036)	(0.019)	(0.034)	(0.014)	(0.036)	(0.039)
Dem Average Effect	0.003	0.025	0.021	-0.052	0.022	-0.050	-0.014
	(0.043)	(0.067)	(0.020)	(0.033)	(0.017)	(0.040)	(0.040)
Specification	Main	Undoing	Minority1	Minority2	Minority3	Rep Weights	Dem Weights

Each column within a panel presents the average of coefficients from a single regression. The top panel presents coefficients over the full sample from 1969-2017. The bottom panel presents coefficients from a restricted panel spanning 1999-2017. All specifications include state-decade and year fixed effects. Treatment is unilateral legal control of a political party over redistricting. In columns 1-2 and 6-7, the dependent variable is the state's Republican share of seats in the federal House of Representatives. In column 3-5 the dependent variable is the state's share of federal (all, Democratic, and Republican) Congressional Representatives that are not white. Rows show estimates of the average effect of legal control by a party on seat-shares in federal elections ending in 2, 4, and 6. Column 1 are the effects from table 2. Column 2 removes observations with legal control where the same party had legal control in the prior decade from treatment. Column 3 uses the fraction of a states representatives that are minority as the dependent variable. Column 4 uses the fraction of a states Democratic representatives that are minority as the dependent variable. Column 5 uses the fraction of a states Republican representatives that are minority as the dependent variable. Columns 6-7 jointly estimates effects by the size of state and reassigns equivalent weights. In column 6 states with Democratic control are assigned weights based on the distribution of states with Republican control. In column 7 states with Republican control are assigned weights based on the distribution of states with Democratic control.

We finally consider a third possible theory: that partisan control is more consequential in larger states and that in larger states, Democrat-dominated states have put replaced partisan redistricting with non-partisan and bi-partisan institutions, namely electoral commissions. First, we note that particularly in the past two decades, Republican legal control is more concentrated in large states than is Democratic legal control. This may seem somewhat surprising given that there are more large Democratic states (California, New York) than large Republican ones. However, a high fraction of large Democratic states redistrict using electoral commissions. In fact, in large non-commission states, estimates of the impact of legal control is higher for Democratic legal control than for Republican control. In order to test whether this an important mechanism to explain the differences in the average effects of Democratic and Republican legal control in the past two decades, we first estimate effects for each party and time period within size bins of legislative delegation size. We separately estimate effects for Democrats and Republicans for states with 2-5 legislators, 5-10 legislators and 11+ legislators. We do this both for the full and for the recent sample. We then aggregate estimates across bins using the size distribution of Republican and Democratic dominated legislatures. Since we do find that there is more scope for larger impacts of redistricting in larger states, we ask the question, what would the effects of Republican redistricting have looked like if Democratic states had the same distribution as Republican ones? We show these results in Column 6. What would the effects of Democratic redistricting look like if Republican states had the same size distribution as Democratic ones? We show these results in Column 7. We also show estimates of Democrat redistricting using Democrat size distribution weights as well as estimates of Republican redistricting using Republican size distribution weights. These are consistency checks. As is well known (Solon, Haider and Wooldridge (2015)), in the presence of effect heterogeneity, OLS does not give a sample-weighted average of treatment effects but rather a variance of treatment multiplied by sample proportion weighted treatment effect. The party effects with own party weights are similar to our baseline estimates and thus we are not concerned. We do find, however, that the effects of Democratic redistricting with Republican weights (5.0) look very similar to the Republican-weighted effects of Republican redistricting (6.9). With re-weighting, the two sets of estimates are not statistically distinguishable. Interestingly, we see less convergence with Democratic weights. We explain our findings by noting two important facts. First, effect sizes are larger for larger states. There is more scope for gerrymandering in larger states and that leads to larger effects as seen in Table V. Second, we note that many of the large Democratic states (California, New Jersey, New York, Washington) have delegated redistricting to electoral commissions. This has not happened on the Republican side. However, where Democrats have retained partisan redistricting, they have not behaved that differently from Republicans.

VIII. Conclusion

In this paper, we have shown that parties sometimes act in their own political interest by reshaping districts to increase their party's representation in Congress when they have the power to do so. The estimated size of the effects are large. In the past two decades, Republican control over redistricting has led to an increase of 8.2 percentage points in the average of a state delegation's Republican seat share in the subsequent three elections. We do not, however, find a similar effect of Democratic control except for a small number of large, Democratic states. We show that the differences in behavior between the Democrats and Republicans is largely explained by a number of large Democratic states opting to bind themselves to non-partisan or bipartisan redistricting commissions. Since larger Democratic states have more ability to gerrymander, legal delegation of authority over redistricting has dramatically reduced average redistricting by Democratic states.

It may not be surprising that parties manipulate vote aggregation to benefit themselves. However, there are reasons why they might not. First, there may be a moral sense of fairness in political competition which may restrain parties from engaging in manipulative behavior. Second, parties in non-competitive environments may not feel the need to gerrymander. Third, parties in competitive states may worry about future retribution. Fourth, parties may limit themselves for fear of incurring court involvement in redistricting.[26] Unfortunately, we cannot distinguish between these different motives to the degree they exist. However, they provide unanswered questions for future research. Finally, though currently there is not enough sample size to directly look at the impact of independent commissions using our methodology, given the increasing numbers of states who have switched to independent or bipartisan commissions, future research on their efficacy would complementary to the research presented here.

[26]The reasons behind why partisan legal control sometimes leads to partisan redistricting and sometimes does not are both interesting and amenable to empirical analysis but beyond the scope of the current paper.

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IX. Appendix Figures and Tables

Table A1—Varying Definitions of Legal Control

	(1)	(2)	(3)	(4)	(5)	(6)
All Years						
Rep Average Effect	0.028	0.034	0.051*	0.031	0.026	0.023
	(0.035)	(0.034)	(0.029)	(0.035)	(0.032)	(0.035)
Dem Average Effect	-0.004	0.009	0.013	0.008	-0.018	-0.022
	(0.025)	(0.025)	(0.025)	(0.026)	(0.025)	(0.024)
Number of Observations	1060	1060	1060	1060	1060	1060
R2	0.778	0.779	0.779	0.779	0.777	0.778
2000 and Onward						
All Years						
Rep Average Effect	0.082**	0.083**	0.104**	0.081**	0.082**	0.076**
	(0.040)	(0.039))	(0.039)	(0.039)	(0.037)	(0.036)
Dem Average Effect	0.003	0.006	0.013	0.002	0.001	-0.040
	(0.049)	(0.025)	(0.049)	(0.055)	(0.025)	(0.035)
Number of Observations	420	420	420	420	420	420
R2	0.869	0.870	0.871	0.870	0.869	0.870
Baseline Legal Control	X					
Partisan Appointed Commissions Included in Treatment Pool		X				
Advisory Commissions Included in Treatment Pool			X			
Super Majority ME, CT				X		
Super Majority 60%					X	
Super Majority 66%						X

Each column within a panel presents the average of coefficients from a single regression. The top panel presents coeffcieints over the full sample from 1969-2017. The bottom panel presents coefficients from a restricted panel spanning 1999-2017. All specifications include state-decade and year fixed effects. Treatment is unilateral legal control of a political party over redistricting. The dependent variable is the state's Republican share of seats in federal House of Representatives. Rows show estimates of the average effect of legal control by a party on seat-shares in federal elections ending in 2, 4, 6. In column 2, partisan legal control includes states with commmisions with net partisan appointment. In column 3, partisan legal control treatment includes states with advisory commisions with unified legislative and gubernatorial control. In column 4, partisan legal is modified to reflect the 60% thresholds used for redistricting bills in Connecticut and Maine. In column 5, partisan legal control treatment includes states with a unified legislature and divided governor where both legislative chambers are above a 60% threshold. In column 6, partisan legal control treatment includes states with a unified legislature and divided governor where both legislative chambers are above a 66% threshold.

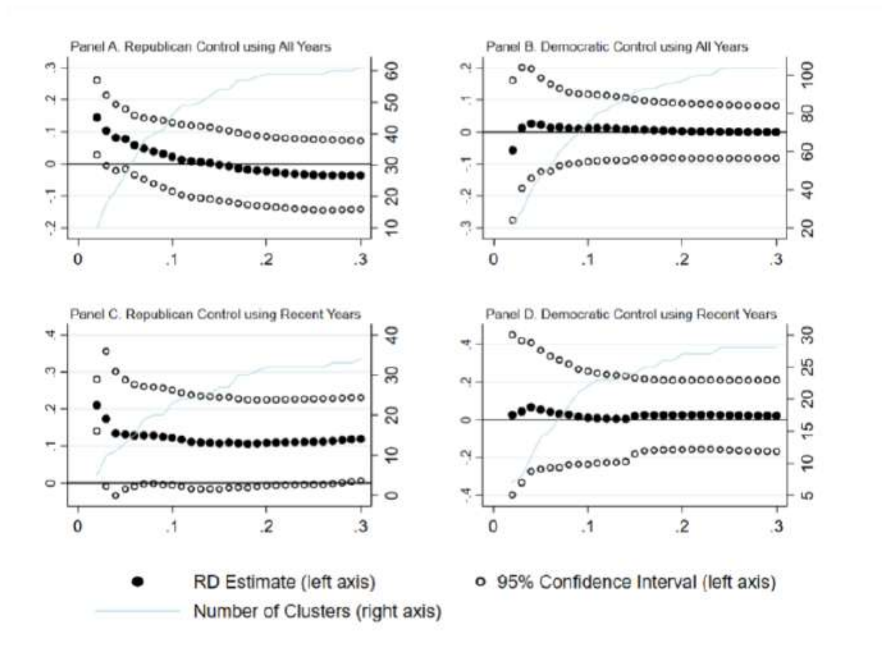

Figure A1. Gubernatorial RD Estimates: Bandwidth Robustness

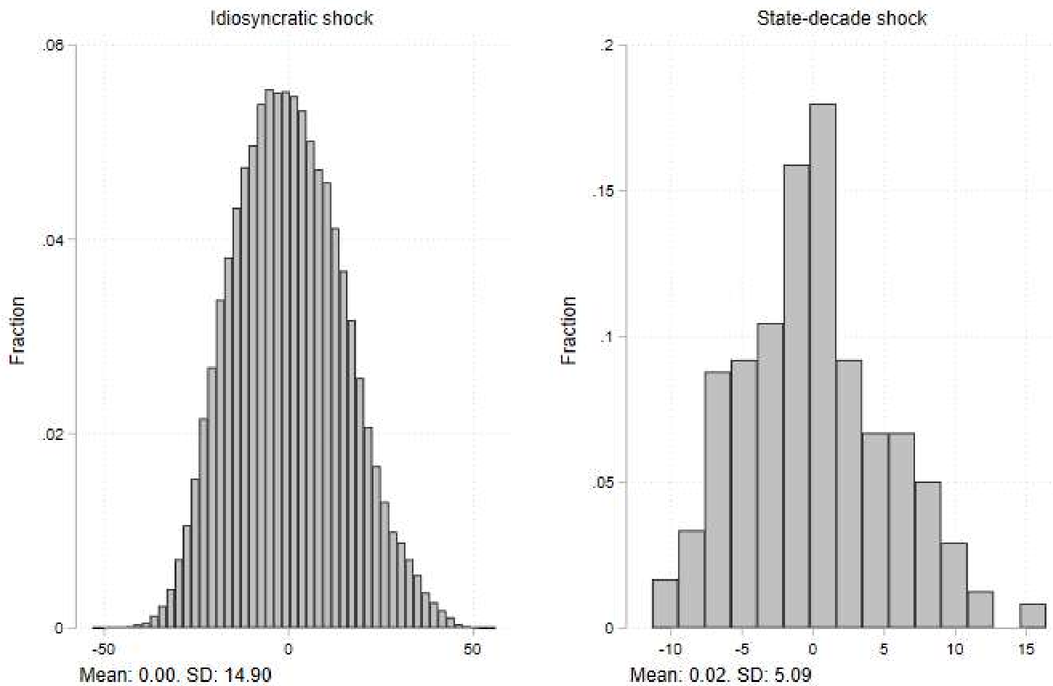

Figure A2. Distributions of Idiosyncratic and State-Decade Shocks

X. Data Appendix

We compile a novel data set on the legal rules that states use to create Congressional district lines from 1968 to 2012. We coded types of legal systems for redistricting across states over 5 decades. We grouped state-decades into one of six categories: (1.) Single district states not eligible for redistricting, (2.) States where redistricting bills are passed by state legislatures and are not subject to a Gubernatorial veto, (3.) States where redistricting bills are passed by state legislatures but where the Governor has veto rights, (4.) States where potentially-partisan advisory commissions (i.e. commissions that are not appointed in a bi-partisan or non-partisan manner) draw the maps but the legislature needs to pass a redistricting bill in order for it to become law, (5.) States where advisory commissions, appointed in a non-partisan or balanced partisan manner, draw the maps but the legislature needs to pass a redistricting bill in order for it to become law, and (6.) States with an independent commission which is appointed in a non-partisan or bi-partisan manner and which has the legal authority to implement a redistricting plan without legislative or gubernatorial approval.

In the 2000+ time period, we rely upon descriptions from Justin Levitt's website: `https://redistricting.lls.edu/2010districts.php`. In the pre-2000 period, we rely upon a combination of sources. First, the National Conference of State Legislatures has documented all historical commissions. Second, we rely upon state legislative documents for each non-single-district state. Third, we rely on law.justia.com. Finally, we also make use of academic articles in some cases. Our sources are documented in greater detail in: `https://docs.google.com/spreadsheets/d/1nZuugxJe09PfCHVIsLyXjGx5cnlKTVnvtDYivDNFdiM/edit?usp=sharing`.

In this document, we point out general patterns, a few anomalies and coding decisions. Most states are of the legislative + gubernatorial veto type. Only Connecticut and North Carolina do not allow for a Gubernatorial veto. In addition, two states, Connecticut and Maine, set a 2/3 majority threshold for passage of a redistricting bill. Five states are one-district states throughout the five-period decade spanning our data. Two others, Montana and South Dakota, start as 2-district states and change to a 1-district state during our time span, while Nevada starts as a 1-district state and eventually reaches 4-districts in our time span. Some states transition to commission states during the time period spanned by our data. However, no states revert from a commission back to legislative redistricting. Montana does transition from a commission state to a 1-district state. For our main specification, we code any state with a commission of any type (4, 5 or 6) as not having legal control by either party. We show robustness to redefining commissions of type 4 as under partisan legal control depending upon the composition of the state legislature in Appendix Table 1.

For all states, we estimate an intention to treat estimate. Thus, we code based upon the law for the decade that was in place in years ending in 1 when redistricting normally happens. Hawaii, in 1968, passed a constitutional amendment which called for redistricting in 1969, 1973 and then every ten years starting in 1981. It also called for a commission system as of 1973. We thus code Hawaii in the 1970s as a commission state.

Table D.A1—State-Level Congressional Redistricting Laws By Decade

State	1970s	1980s	1990s	2000s	2010s
Alabama	3	3	3	3	3
Alaska	1	1	1	1	1
Arizona	3	3	3	6	6
Arkansas	3	3	3	3	3
California	3	3	3	3	6
Colorado	3	3	3	3	3
Connecticut	2	2	2	2	2
Delaware	1	1	1	1	1
Florida	3	3	3	3	3
Georgia	3	3	3	3	3
Hawaii	6	6	6	6	6
Idaho	3	3	3	6	6
Illinois	3	3	3	3	3
Indiana	3	3	3	3	3
Iowa	3	5	5	5	5
Kansas	3	3	3	3	3
Kentucky	3	3	3	3	3
Louisiana	3	3	3	3	3
Maine	3	5	5	5	5
Maryland	3	3	3	3	3
Massachusetts	3	3	3	3	3
Michigan	3	3	3	3	3
Minnesota	3	3	3	3	3
Mississippi	3	3	3	3	3
Missouri	3	3	3	3	3
Montana	6	6	1	1	1
Nebraska	3	3	3	3	3
Nevada	1	3	3	3	3
New Hampshire	3	3	3	3	3
New Jersey	3	3	3	6	6
New Mexico	3	3	3	3	3
New York	3	4	4	4	4
North Carolina	2	2	2	2	2
North Dakota	1	1	1	1	1
Ohio	3	3	4	4	4
Oklahoma	3	3	3	3	3
Oregon	3	3	3	3	3
Pennsylvania	3	3	3	3	3
Rhode Island	3	3	3	3	4
South Carolina	3	3	3	3	3
South Dakota	3	1	1	1	1
Tennessee	3	3	3	3	3
Texas	3	3	3	3	3
Utah	3	3	3	3	3
Vermont	1	1	1	1	1
Virginia	3	3	3	3	3
Washington	3	3	6	6	6
West Virginia	3	3	3	3	3
Wisconsin	3	3	3	3	3
Wyoming	1	1	1	1	1

Note: Numbers represent different legal systems for redistricting: 1: Single District - The state was apportioned a single congressional district and thus there was no need for districting. 2: Legislature Only - The State Legislature has full control over the redistricting process with no possibility of a Gubernatorial veto. 3: Legislature and Governor: The State Legislature is in charge of developing a Congressional Redistricting plan but the Governor has veto rights. 4: Advisory Commission: An advisory commission draws redistricting maps and presents them to the legislature for passage. Advisory commissions of this type are appointed in a manner that lacks partisan balance. 5: Non-Partisan Advisory Commission: An advisory commission which is appointed in a non-partisan manner or on a bi-partisan basis so as to maintain partisan balance on the commissions. 6: Independent Commission - Independent commissions are appointed on a non-partisan basis and have the legal authority to draw and implement a redistricting plan without gubernatorial or legislative approval. For the 2000s and 2010s redistricting cycles data was collected from a website by Justin Levitt. For the 1980s and 1990s cycles the majority of the data came from court cases whose summaries were aggregated by the National Conference of State Legislatures website. The full documentation of the cases were then examined, often via law.justia.com. For the 1970s redistricting cycle, a variety of sources were used. The primary ones were state specific sites either documenting the history of redistricting in the state or documenting historical state constitutional amendments as well as a paper on the 1970s redistricting cycle in which the processes were characterize