Political Control Over Redistricting and the Partisan Balance in Congress*

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July 11, 2024

We estimate the impact of a political party's 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 only find significant effects for Democrats in large states. Effects are one half of the average seat gap between the parties in the past decades.

* We thank Barry Burden, Allan Drazen, Garance Genicot, Richard Holden, Daniel Jones, David Karol, Mateo Uribe-Castro, Jorg Spenkuch, Nick Stephanopoulos, John Wallis, Mark Wilkerson, Haishan Yuan and members of the University of Maryland Political Economy Group for helpful comments. We thank Richard Holden for providing data on legal control over redistricting and Jim Snyder for providing data on presidential vote share by House district. We also thank Maxine Asenso, Matthew Firth, Ziqiao Liu, Jorge Perilla and Drew White for excellent research assistance. All mistakes are our own.
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"Left unchecked, as the court does today, gerrymanders like these may irreparably damage our system of government." - Justice Elena Kagan.

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

I. Introduction

In majoritarian single member district political systems, representatives are elected in geographical districts. As population imbalances across districts accrue 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, specifically 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. The state legislators are then responsible for drawing district boundaries within states, allowing 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 in Congress.

In recent years, there has been increased concern over whether or not redistricting leads to gerrymandering, i.e., parties redistrict in order to increase their share of legislative seats. There exists a substantial body of theoretical work on how self-interested political parties can optimally redistrict. The fundamental idea is that, in order to increase their own-party seat share, parties will *crack* opposition districts with a narrow majority by adding supporters and *pack* opposition districts with large majorities by adding more opponents (Gilligan and Matsusaka, 1999; Shotts, 2001; Gul and Pesendorfer, 2010). Further theoretical work has argued that packing is beneficial, but when a party is uncertain about

partisan leanings and voter turnout, cracking is typically suboptimal (Friedman and Holden, 2008).

Recent research has further explored how a party's choice between packing and cracking is affected by heterogeneity in the turnout rates both of supporters and of opponents (Bouton et al., 2023) as well as (Kolotilin and Wolitzky, 2023) as well as heterogeneity in the degree of extremity (Kolotilin and Wolitzky, 2023).

A large empirical literature primarily in political science tries to test for gerrymandering by simulating counterfactual maps subject to legal-or norms-based constraints. These constraints include requirements that districts be connected and that they be compact (Chen, Rodden et al., 2013; Chen and Rodden, 2015; Kenny et al., 2023; Stephanopoulos and McGhee, 2015). This literature considers counterfactual maps and computes probabilities of a redistricting outcome at least as partisan as the actual outcome. Although 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 that districts confer partisan advantage to one party or another reflect natural geographical boundaries or reflect natural political communities (McGann et al., 2016; Rodden, 2019). Some of the work on redistricting, however, suggests that partisan advantage due to asymmetries in clustering across districts results from 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, Spenkuch and Sullivan, 2022).

Instead of comparing actual maps to counterfactual maps, we estimate the impact of partisan legal control over the redistricting process on the drawn maps and the resulting seat shares in the House of Representatives. We also evaluate a policy-relevant question: what is the impact of allowing partisan politicians to redistrict? In answering our question, we provide comprehensive evidence of the

prevalence of partisan gerrymandering over 50 years of American history. We present our estimates by party, by the size of the state, and by time period. To identify the impact of partisan redistricting, (Stephanopoulos and McGhee, 2015) and (Stephanopoulos, 2017), which have been heavily used in recent Supreme Court cases, estimate the impact of a political party having unified control of a state government on the efficiency gap using a two-way fixed-effects model. We differ from Stephanopoulos (2015) and Stephanopoulos (2017) in several ways. Most importantly, we demonstrate that the two-way fixed-effects estimates are upwards-biased due to a growing influence of partisan legal control over seat shares. Furthermore, we accurately measure legal partisan control over redistricting, as opposed to using the proxy of unified control over the state government.

More recently, Jeong and Shenoy (2024) estimates the impact of marginal legislative control over the lower house of a state legislature on subsequent seat shares. However, they use a seat share RD, which we show in Appendix G suffers from endogeneity issues. Consistent with our estimates, they find temporary effects of partisan control on seat shares; however we find persistent effects lasting at least until the next redistricting cycle. Additionally, they do not break down effects separately by time period and their method is unable to differentiate effects across political parties. This is critical since we only find effects of Republican legal control overall. We find effects of Democratic legal control only in large states. Additionally, we find these effects only in the past two decades.

We first develop a measure of the amount of redistricting as the fraction of a state that undergoes district changes. We empirically demonstrate that nearly all redistricting takes place once a decade, carried out by the legislatures in power during years that end in 1. We then estimate the impact of the ability of the Democratic or Republican party to pass a redistricting bill without votes from the opposition party on the fraction of Republican seats in Congress in subsequent elections.

We evaluate our effects using three different approaches and obtain highly consistent estimates. First, we employ a panel model, controlling for year and state-decade effects. We find a statistically significant positive impact of 4.8 percentage points resulting from Republican legal control on the Republican seat share in Congress during the subsequent election. This effect increases to 9.1 percentage points when restricted to the past two decades. While the average effect across the next three federal elections is positive, it does not reach statistical significance in the full five-decade sample. However, this effect grows to 8.3 percentage points and becomes statistically significant at conventional levels when our analysis is confined to the past two decades. In contrast, we do not find statistically significant effects or magnitudes as substantial in the case of Democratic control, except for large state delegations within the confines of the past two decades. Our estimations regarding the influence of Republican control over redistricting remain notably consistent in the past two decades, irrespective of the size of the state delegation.

We validate these results using a better identified but more local and less statistically powerful difference-in-discontinuities estimate in the vote share for governor. Improving upon work done by Folke (2014); Kirkland, Phillips et al. (2018); Kirkland and Phillips (2020); we also develop a novel simulation-based binned matching (SBBM) estimator where we estimate the shock structure of state legislative elections and then simulate probabilities of legal control to form well matched treatment and control groups for legal control over redistricting. We then estimate treatment effects using a propensity score type design. This estimator provides better and more explicit matching between state-decades with legal control and those without yet retains more statistical power than the gubernatorial RD estimator. We feel that this method will have interest well beyond the scope of our paper and is an important contribution of the paper.

We also investigate reasons behind the observed differences between parties' redistricting behavior. We consider two common explanations: (1.) Republicans

have been undoing solid control by Democrats as part of the process of political realignment and (2.) Democrats have pursued the creation of majority-minority districts when they have had legal control in lieu of maximizing seat shares. We find that neither of these explanations can completely account for the observed differences. In recent work, (Sabet and Yuchtman, 2023) show that Democrats packed Latino voters into districts when they had control over the redistricting process in 1990. This came after the 1986 Immigration Reform and Control Act, which provided a pathway to citizenship for existing undocumented immigrants. In particular, they show that in states with Democratic control of redistricting, Democrats put Latinos into oddly shaped districts in order to create minority representation. This suggests that a political party's objective may be something other than increasing their number of seats in Congress. Instead, they may seek to increase minority representation. Of course, court challenges and randomness in turnout may mute the impact of redistricting and thus its benefits (Owen and Grofman, 1988; Sabouni and Shelton, 2022). Similar to (Sabet and Yuchtman, 2023), we do find that Democrat legal control leads to an increase in majority-minority districts and minority legislators. However, this only occurs in large states where the impact of Democratic and Republican legal control have similar impacts. We thus show that the preferences over ethnic representation do not explain the recent partisan gap in the effect of partisan legal control.

However, our estimates indicate that approximately 25% of the difference can be attributed to variations in delegation sizes between states with Republican and Democratic control over redistricting. The remaining is explained by differences in effects of partisan control across the two parties.

Overall, rough calculations suggest that partisan redistricting contributed to less than 10% of the seat disparity between Republicans and Democrats in the House during each of the 1970s, 1980s, 1990s, and 2000s. However, these calculations also indicate that it could explain 54% of the gap in the 2010s, mainly driven but high impacts of partisan control in recent years combined a near absence of

partisan legal control in large Democratic states.

In the next section, we discuss important institutional features of the U.S. redistricting process. 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 provide evidence on the mechanisms that explain the differences in behavior across the Democratic and Republican parties. In section VII, we perform an exercise in which we compute aggregate impacts of the rights to redistrict upon the partisan balance in Congress. Finally, in section VIII, we conclude.

II. Institutional Background

The process of redrawing districts happens in two phases. In the first phase, known as reapportionment, the U.S. Congress uses data from the decennial Population Census to assign each state a specific number of seats in the House of Representatives. This assignment is completed by January 25th of the year following the Census. Though there are multiple possible methods to apportion seats, Congress uses the Huntington-Hill method which minimizes deviations in numbers of representatives per person across states.

After reapportionment occurs, the federal government has historically given individual states wide latitude to divise district boundaries as they see fit. Overall, reapportionment results in relative balance of the number of House representatives across states. However, there were few rules guiding the balance of representatives within states until the 1960s. Before then, states often created districts with a high degree of population imbalance. However, in a sequence of rulings (*Baker v. Carr (1962), Wesberry v. Sanders (1964)*), the U.S. Supreme Court mandated the principle of "one person, one vote" in representation.

In recent years, the Supreme Court has further ruled that as long as districts are sufficiently compact, redistricting in order to create majority-minority Congres-

sional districts is legal but other racially-based reasons are not legal (*Thornburg v. Gingles*, 1986; *Shawn v. Reno*, 1993; *Miller v. Johnson*, 1995). 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 over whether partisan gerrymandering violates state constitutions. We discuss institutional details concerning redistricting in greater detail in Appendix C.

We now move from a general discussion of federal law concerning redistricting to state law and how it impacts the construction of our treatment variable. Our primary treatment variable, legal control by a political party over redistricting, is assessed at the state-decade level. This leads to estimation strategies that compare state-decades. Treatment status depends upon (1.) the size of a state, (2.) state law, and (3.) the partisan composition of government. Not all states provide useful variation for our analysis. Seven states did not redistrict federal Congressional boundaries through most of our sample period because they only had one federal representative: Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont and Wyoming. We drop single district state-decades from our main analysis. 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.

Over the fifty years covered by our dataset, 11 states have employed commissions for redistricting purposes, either to directly formulate and implement plans or to provide advisory input. Independent Commissions have the force of law to implement the maps they draw. We code these state-decades as not having legal control by either party even if the state has a trifecta at the time of redistricting. The other commissions are advisory commissions, which draft or provide input on maps where the legislature is ultimately responsible for passing a redistricting bill. We code these state-decades according to the legal requirement for passing

a redistricting bill in the state-decade. We consider alternative codings in one of our robustness tables which we discuss in Section V. We provide more detail about our coding in Appendix F.

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 under our baseline definition of legal control. We consider the laws in a given state that outline which bodies will draft and pass a redistricting plan as well as the political affiliation of those bodies[1].

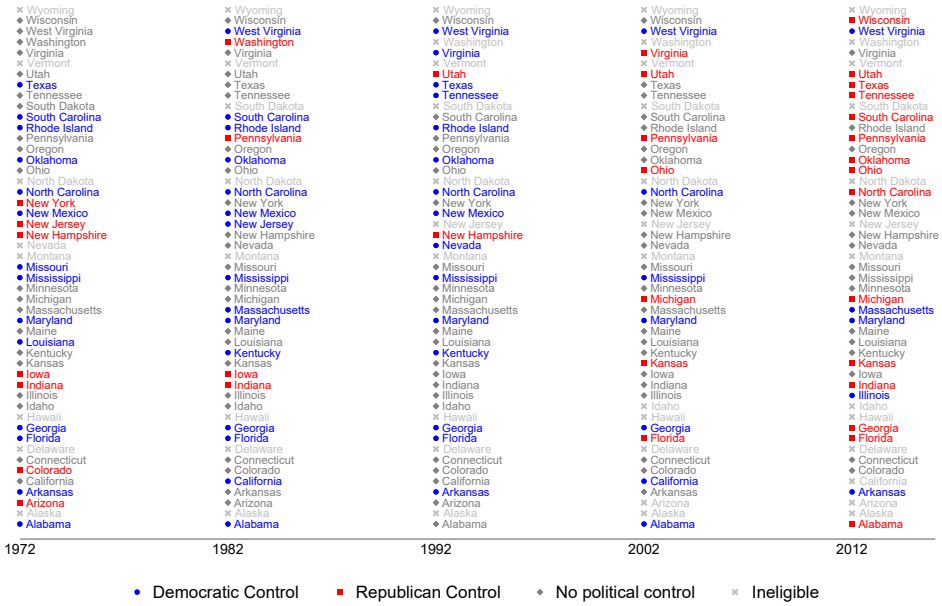
For a political party to have legal control of redistricting, that party needs to be able to pass a redistricting plan without a single vote from another party. Therefore, in most states that do not utilize an independent commission, a party needs majority control over both chambers plus the governor's office (a trifecta), in order to wield legal 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 an independent 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.

III. Empirical Methods

In this section, we discuss the empirical methods that we will use to estimate our main effects. We present three main estimation methods. 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

[1]The details of the laws and how they vary by states over time are explained in greater detail in the Data Appendix.

Figure 1. : Treatment Definition by Decade



Note: This figure displays the main treatment variables: Democratic legal control and Republican legal control by decade. Democratic legal control in a decade is denoted by a blue circle next to the state name in the column for the decade. Republican legal control in a decade is denoted by a red square next to the state name in the column for the decade. When neither party has legal control either because the government is divided or because district boundaries are drawn by a commission, the diamond next to a state in a decade's column is gray. States labeled with an x have a single representative, they are ineligible and excluded from our sample.

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 identification is better for the gubernatorial RD but (1.) the estimate is more specific and (2.) the power is substantially reduced. The third strategy uses the simulation-based binned matching (SBBM) 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

estimate a propensity-score-type matching estimator to estimate the impact of legal control due to marginal unification of the legislature. This last method makes less strong identifying assumptions than the dynamic panel model but has greater power than the gubernatorial RD.

A. StateXDecade and Year Effects Estimation

In our first and main specification, we regress an electoral outcome variable, $O_{s,d,y}$, on measures of partisan legal control conditional upon stateXdecade ($\gamma_{s,d}$) and year ($\delta_{d,y}$) fixed effects. In our notation, a year within the dataset is identified by two components: d, representing the decade, and y, the year's final digit within that decade. For example, if $d = 3$ and $y = 2$, the observation corresponds to the year 1992, categorized as the third decade and the year ending in 2 within that decade.

Our main electoral outcome variable is the Republican House of Representatives seat share; however, we also estimate effects upon the fraction of state land area which switches districts as a result of redistricting, the similar fraction of people switching districts, the minority share of the House delegation from a state in a given Congress, the fraction of minorities who changed districts due to redistricting in a decade, and the differential fraction of wasted votes (the efficiency gap). We index outcomes by the year in which the Congress is elected.

We separately define treatments by political party, $DemControl_{s,d}$ and $RepControl_{s,d}$. Each are dummy variables which take on a value of 1 if (1.) the respective party has the legal ability to pass a redistricting bill solely relying upon votes from their party in that state-decade and (2.) the election occurs after the time normally alotted for redistricting within the decade. 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. Table 1 shows the structure of our decades. Decade 3

consists of decade=3 and year=8 (1988), year=0 (1990), year=2 (1992), year=4 (1994), and year=6 (1996). For a given decade, $DemControl_{s,d}$ takes on a value of 0 in years ending in 8 or 0 and takes on a value of 1 in years 2, 4, and 6 if Democrats had legal control over redistricting in that state-decade. If Democrats did not have legal control in that state-decade, then $DemControl_{s,d}$ takes on a value of zero for all years in that decade. $RepControl_{s,d}$ is analogously defined. Neither party controls redistricting if different parties control the two chambers of the state legislature, the governor is from a different party from a unified legislature and redistricting bills require a gubernatorial signature, or maps are drawn by a commission with the legal authority to implement maps they draw. This means that we assess legal control based upon the commission status, the partisan composition of both state legislative chambers, and typically the party of the governor in order to determine our legal control variables[2].

Table 1—: Decade Structure and Treatment Timing

	Prior Map		New Map		
$d =$	In Use		Implemented		
1	1968	1970	**1972**	**1974**	**1976**
2	1978	1980	**1982**	**1984**	**1986**
3	1988	1990	**1992**	**1994**	**1996**
4	1998	2000	**2002**	**2004**	**2006**
5	2008	2010	**2012**	**2014**	**2016**
$y =$	8	0	2	4	6

Pre-period Post-period

Note: Decades, d, are listed in columns, and the last digit of the year, y, are listed in rows. These correspond to subscripts throughout the remainder of the paper. When $d = 1$ and $y = 8$, this corresponds to observations from 1968.

[2]The main treatment variables (legal control variables) are determined by the partisanship of governments in January of years ending in 1, our main outcome variables (seat shares) are determined in even years (i.e. election years). We make this distinction due to the fact that the majority of redistricting plans are passed in years that end in 1.

The identifying assumption we make in this model is that seat shares would have evolved within a decade in states with legal control similarly to states without legal control. Our specification is outlined as follow:

$$
\begin{aligned}
O_{s,d,y} = \alpha + \beta_y^D DemControl_{s,d} + \beta_y^R RepControl_{s,d} + \gamma_{s,d} + \delta_{d,y} + \epsilon_{s,d,y} \\
\text{for } y = \{8, 0, 2, 4, 6\} \text{ and } d = \{1, 2, 3, 4, 5\} \\
\text{where } y \neq 0 \text{ for } \beta_y^D \text{ and } \beta_y^R
\end{aligned} \tag{1}
$$

The coefficients of interest, β, represent legal control treatments interacted with year-within-decade dummies. The dummy for election years ending in 0 is excluded, allowing the remaining interacted year-within-decade dummies to fully capture dynamics across the decade. This exclusion implies that all β coefficients are normalized relative to the baseline year just before redistricting, years ending in 0, similar to an event study.

With this definition of decade, we estimate three lagged effects of redistricting and two leads before redistricting 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 pre-trend (β_8) and three ex-post dynamic effects ($\beta_2, \beta_4, \beta_6$) of redistricting. Using a definition of decade as beginning in years ending in 8 allows us to estimate a pre-trend. We report the dynamic effects, and average effects according to equation 2.

$$
\begin{aligned}
\beta_{avg}^R &= \frac{\beta_2^R + \beta_4^R + \beta_6^R}{3} \\
\beta_{avg}^D &= \frac{\beta_2^D + \beta_4^D + \beta_6^D}{3}
\end{aligned} \tag{2}
$$

This first research design relies on the existence of redistricting variation within

a decade. Given that redistricting occurs every 10 years, decades structured to start in years ending in 2 and end in years ending in 0 would leave no identifying variation. Table 1 outlines the redistricting timing we impose. Our structure relies on 2 pre-period elections and 3 post-period elections. District boundaries are expected to change between congressional elections from years ending in 0 and those ending in 2. This setup is naturally suited for a difference-in-difference analysis, comparing states where a political party can unilaterally pass a redistricting bill (treated states) to states that require bipartisan agreement (control states). In this setup both treatment and control states are expected to redistrict, however, treatment states can redistrict without the approval of the opposing political party. A state may redistrict at other points in time; however, we show in Appendix Figure A.2 that the vast majority of redistricting occurs as expected. Nonetheless, our estimates should be interpreted as an intention to treat specification.

Finally, 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 at the state-decade level accounts for serial correlation within states over a decade. We do not cluster at the state level because substantial changes have occurred over our sample and given our limited cluster size, we opted for state-decade clustering; nonetheless, clustering at the state level decreases our standard errors slightly [3].

B. Gubernatorial Difference-in-Discontinuities Estimator

In a second specification, we estimate a difference in discontinuities model in the gubernatorial vote share. We limit our sample to states with (1.) a unified

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

legislature which also (2.) require a redistricting bill to pass with a gubernatorial signature. In this sample, a narrow electoral victory for a governor of the majority party confers legal control, whereas a narrow loss denies it. The running variable for the regression discontinuity is the top-two party gubernatorial vote share for the party with unified control in both chambers of the state legislature. The vote share running variable is for the election which determines the party of the governor in years ending in 1[4].

To match our other specifications, we estimate impacts on the average outcomes in years ending in 2, 4 or 6 (our treatment effect), and years ending in 8 (our pre-period placebo). This results in coefficients that are once again relative to a base year just before redistricting (years ending in 0). Thus we estimate the difference in vote share outcomes relate to years ending in zero of legal control relative to no partisan control due to small difference in the gubernatorial vote share. Using two regressions, we estimate separate treatment effects for unified Republican and Democratic legislatures. We use a local linear regression with a triangular kernel. Our sample size shrinks when estimating a gubernatorial regression discontinuity because we restrict to states which require unified control to pass a redistricting bill, do not pool Democrat and Republican effects, and because we further restrict state-decades based upon bandwidth. Our estimation equation is given by:

(3)

$$\begin{aligned} O_{s,d,y} = I\left[y=8\right]\left[\alpha + \beta_{Pre} Govwin^P_{s,d} + f_{lose}(VS^P_d) + f_{win}(VS^P_d) * Govwin^P_{s,d}\right] \\ + I\left[y=2,4,6\right]\left[\alpha + \beta_{Treat} Govwin^P_{s,d} + f_{lose}(VS^P_d) + f_{win}(VS^P_d) * Govwin^P_{s,d}\right] \\ + \alpha + \beta_{Base} Govwin^P_{s,d} + f_{lose}(VS^P_d) + f_{win}(VS^P_d) * Govwin^P_{s,d} + \epsilon_{s,d,y} \\ \text{for } y = \{8,0,2,4,6\} \text{ and } d = \{1,2,3,4,5\} \end{aligned}$$

[4]In states with elections in even years, this means legislators elected in years ending in 0 or 8; In states with elections in odd years, this means legislators elected in years ending in 1 or 9. Either way, the election is the one ending in 2.

where $I[y = k]$ is an indicator variable that equals 1 if the final digit of the year in the seat share observation is k, and 0 otherwise. VS_d^P is the vote share for the governor belonging to the political party P of a unified legislature, who is in power for redistricting in decade d. Specifically, we use two separate regressions; one where P denotes unified Republican legislatures and another where P denotes unified Democratic legislatures. We do not interact with a year zero dummy variable because we estimate a difference-in-discontinuities with a base year of election years ending in zero. We report β_{Treat} and β_{Pre} from two separate regressions, which are comparable to the average effects and pre-trends we report from Equation 1.

C. Simulation-Based Binned Matching (SBBM) Estimator

Our final method estimates the impact of legal control by comparing state-decades with and without legal control restricted to states with a similar probability of partisan legal control. It is similar in spirit to a propensity score estimation strategy in that we compare treated state-decades with control state-decades controlling for the probability of treatment. This strategy is less powered than the panel estimator but the matching between treatment and control is superior. Similarly, it is more powered than the regression discontinuity estimator and estimates a less local parameter. We believe that it has wider applicability in estimating the effects of legislative majorities. The difficulty in the estimation is computing the probability of treatment.

In order to compare differences across state-decades in legal control due to random vote share shocks, we first estimate the shock structure for state legislative districts. We do this by estimating a random effects model using maximum likelihood where we regress the Democratic two-party vote share in a district on a state-decade random effect, year fixed-effect and an idiosyncratic error term. Under the assumption of normality, we estimate both the variance of the state-

decade shock as well as the variance of the idiosyncratic shock. We thus run the following regression:

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

where $VS_{s,d,c,j,y}$ is the two-way Democratic vote share for district j in chamber c and year y in 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, λ_y is a year fixed effect and $\epsilon_{s,d,c,j,y}$ is an i.i.d. shock that is idiosyncratic to a state legislative district at a point in time[5]. Implicitly, we assume that the distribution of state-decade shocks is identical across states and over time and we assume that the distribution of idiosyncratic shocks is identical across districts both within and across states as well as over time. We estimate the variances by pooling all legislative and gubernatorial elections over the full sample that lead to a legislator or governor being in power in the year prior to the federal elections in years ending in 2. We treat votes for governor as just another district[6]. We pool across states and decades in order to increase statistical power of our estimation.

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 maintaining the normality assumption[7]. We simultaneously but independently draw a set of idiosyncratic shocks: one for each chamber x district, one for the governor and one aggregate state-decade shock for each state-decade.

The presence of multi-member districts in 10 states complicates this calcula-

[5]In our vote shock estimation we exclude uncontested districts.

[6]We compared the estimated shock structure for shocks for governor and it was very similar to the shock structure for individual congressional district

[7]We present histograms of the error terms to validate our parametric use of the normal distribution. These figures can be seen in Appendix Figure A.1. We have also run simulations using the non-parametric distribution of shocks. Results are similar but slightly noisier due to the discrete nature of the shock structure. We discuss this in greater detail in Section V.

tion[8]. To include these districts, we calculate placebo vote shares for every candidate in a district. When drawing shocks, a positive shocks represent positive shocks to the Democratic two-party vote share. Thus, we add shocks to a Democratic candidate's vote share and subtract the shock from Republican candidates. We then rank candidates by the placebo vote share received and determine the number of representatives from each party in each state's legislative chamber and the governor's political party. If a district has one representative, the candidate with the highest placebo vote share wins; if there are two representatives, the parties of the top two candidates based on placebo vote shares win. In other words, we simulate:

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

where $VS_{s,d,c,j,p}$ is the actual vote share candidate p received, $\eta_{s,d,i}$ and $\epsilon_{s,d,c,j,i}$ are the simulated shocks in the ith simulation for the state-decade and $\hat{VS}_{s,d,c,j,p,i}$ is the simulated vote share for the candidate[9]. We then determine the majority party in each chamber and the governor, and calculate the fraction of time the simulation results in legal control for Republicans, for Democrats, and when no party has legal control. For each state-decade we compute 10,000 simulations. We thus simulate the ex-ante probability of legal control for the Democrats and the ex-ante probability of legal control for the Republicans for each state-decade.

[8]Louisiana is excluded due to its unique jungle primary system.

[9]For example, in 2010 District 1 in The Ohio House of Representatives had a Republican and Democratic candidate receiving 52.6% and 47.4% of the vote respectively. To calculate placebo vote shares, we take 1 draw from N(0, σ_η^2) and apply this to only District 1 in OH. We would also take one draw from N(0, σ_ϵ^2) and apply this to all state elections in Ohio during the 2010 redistricting cycle. If we drew +5% for the first shock and -1% for the second, this would mean that there was an idiosyncratic 5% Democratic vote shock specific to District 1 in Ohio in 2010, and there was a 1% Republican vote shock specific to all Ohio state elections during the 2010 redistricting cycle. The placebo vote shares for the Republican candidate would thus be 48.6% (52.6% - 5% + 1%) and 51.4% for the Democratic candidate (48.6% + 5% - 1%). The simulated OH District 1 would be Democratic in this case. This process is repeated for all 33 Senate districts in 2010, 99 House districts in 2010, and the 2010 governor election in Ohio.

We then restrict observations to those (either in treatment or control) with between a 20% and an 80% probability of legal control for either of the two parties. We perform our estimations jointly using the sample of states with between at 20% and an 80% probability of Republican or Democratic legal control. We then run a bin-matching regression where we regress the two-party seat share on two treatment variables, Republican and Democratic partisan legal control, controlling for decade X 10 percent probability bins for each political party. Within decade x probability bins there exists overlap in terms of treatment and control[10]. However, there is almost no overlap across the Democratic and Republican states. A state-decade with a moderate probability of Democratic control is one that likely has a low probability of Republican control. We thus estimate:

(6)

$$\begin{aligned} O_{s,d,y} = \alpha &+ \delta^D DemControl_{s,d} + \delta^R RepControl_{s,d} \\ &+ I[y = 2,4,6]\left[\mu + \beta^D DemControl_{s,d} + \beta^R RepControl_{s,d}\right] \\ &+ I[y = 8]\left[\nu + \eta^D DemControl_{s,d} + \eta^R RepControl_{s,d}\right] + \gamma^D_{d,p} + \gamma^R_{d,p} + \epsilon_{s,d,y} \\ &\qquad \text{for } y = \{8,0,2,4,6\} \text{ and } d = \{1,2,3,4,5\} \end{aligned}$$

where we introduce the probability-bin matching parameters, $\gamma^D_{d,p}$ and $\gamma^R_{d,p}$, which represent fixed effects for the Democratic and Republican parties for decade d and probability bin p. Following Equation 3's notation, $I[y = k]$ is an indicator variable for the year's final digit. The coefficients β^D and β^R are comparable to our other estimates of average effects, and η^D and η^R are comparable to our other pre-trend estimates. We cluster our standard errors at the state-decade level[11].

[10]This is not true when the probability of control is less than 20% or greater than 80%.

[11]Since our SBBM estimator contains generated regressors, probability bins for Democratic control, and probability bins for Republican control, we could bootstrap our estimates in two steps. First, bootstrap the probabilities, and second, given the bootstrapped probabilities, block bootstrap our two-way fixed effects 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

Our approach borrows heavily from Kirkland, Phillips et al. (2018) and Kirkland and Phillips (2020). However, we also differ 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 estimate our shock structure empirically using a random effects model (estimated using Maximum Likelihood) rather than imposing a uniform [0,20] shock structure. Fourth, 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. This last difference is particularly important as assuming only aggregate shocks attenuates the probability of unified government towards 50%. This increases the sample size but also leads to comparisons between state-decades that are systemically different.

D. Summary

The challenge when estimating the impact of legal control on the partisan balance of Congress is that voter preferences are related to both the composition of a state's government (treatment assignment) and the congressional representation (outcome). These preferences may be a function of demographic changes, a response to policies, or the fielded candidates. This means that the naive regression is susceptible to omitted variable bias. In our baseline estimation, the implicit parallel trends assumption means we assume that trends in voter preferences are the same in treatment and control states. This is a troubling assumption without any justification, but we utilize treatment leads to justify this specification. Even with leads, there is still some concern that we can never fully prove our parallel trends assumption. We thus introduce the gubernatorial difference-in-discontinuity estimator. This estimator relies on quasi-random variation in close governor elections and has limited omitted variable bias concern, the tradeoff being a substantial loss in power and in addition to a highly specific parameter.

regression discontinuity estimators.

Finally, the SBBM estimator is similar to the baseline estimate, but the match between treatment and control units is improved. In this specification, we compare states with similar probabilities of legal control but different realizations. In these specifications, it is more reasonable to believe that states with, for example, 60% probability of Democratic control have similar trends in voter preferences.

Later, we will further augment our main specification to try to assess the omitted variable threat. First, we directly incorporate a state-wide control for Republican vote share, which, to some extent, directly captures voter preferences within a state-decade. Additionally, we show that prior government control is a poor replacement for control during redistricting. For instance, prior state governments may enact policies that citizens like or dislike, which can be done in a partisan manner when states have a unified government. In appendix table A.4, we show that treatment derived from unified governments in years that do not end in 0 results in null results.

IV. Data

A. Vote Shares and Seat Shares

Our main dependent variable is the Republican seat share of a state's delegation in a given Congress[12]. We collect this data from Congressional Quarterly at the State-Congress level. We additionally collect from Congressional Quarterly the stateXyear level Republican two-party vote share for the House of Representatives for use as a control variable in a robustness check.

B. Legal Control and Unified Control

Our main independent variable is legal control by a party over redistricting. This variable is constructed using a combination of laws and election outcomes.

[12]We could alternatively use the Republican two-party seat share; however, this only impacts our estimating sample in 3 instances from 1968-2016.

We obtain state-decade partisan control data from Klarner et al. (2013) 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. Using these two sources, we 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 non-advisory commissions types as redistricting in a non-partisan manner since all non-advisory commissions are also appointed in a bipartisan manner. If a commission is merely advisory, we code legal control based upon partisan control over the state legislative chambers and the governor's office. We do additionally perform robustness checks for alternative translations of commission codings into treatment status.

From 2000 onward, data on redistricting law comes from Doug Spencer's website[13]. 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

[13]Originally, this website was created and maintained by Justin Levitt

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 [14]. We also estimate the impact of legal control over redistricting on the non-White share of the state's delegation. We compute the 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. We similarly estimate the impact of legal control on the racial composition of a district. To do this, we merge in data from the 1990, 2000, and 2010 population censuses conducted by the Department of the Census at the census block level. We also estimate the impact of legal control on wasted votes. For the purposes of comparability across districts within a state as well as across states at a point in time, we look at voting outcomes for president. James Snyder graciously provided us with historical district-level vote shares for president Hirano and Snyder Jr (2019). Finally, as a robustness check, we define legal control as present when the legislature is unified and has a veto-proof majority even when the governor is of the opposite party. We get the thresholds for legislative vetoes from Ballotpedia: `https://ballotpedia.org/Veto_overrides_in_state_legislatures`.

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 estimates of the effects of legal control on a state delegation's partisan seat share using our three estimation methods. We also show that our estimates are robust to a number of alternative estimation strategies and data set choices as well as a placebo exercise.

[14] Richard Holden graciously provided us with the data

A. Measuring the Extent of Redistricting

We begin by quantitatively measuring the extent of redistricting in a state-decade. We use ARC-GIS to geocode every congressional map from every state for every Congress between 1968 and 2018. We compute the geographical overlap between each pre-existing Congressional district within a state 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:

$$\sum_{i=1}^{N} \frac{max_{j(i)} \left(|D^A_{j(i)} \cap D^B_i| \right)}{|D_{Total|}} \tag{7}$$

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 jth district after redistricting and the ith before redistricting, and $|D_{Total}|$ is the square mileage of the state.

Thus, we compute the change in land area in every district and compute the fraction of landing changing district as a fraction of total land in the state[15]. Appendix Figure A.2 shows a bar graph with the fraction of states changing from between 0% and 10% of land up until 40% to 50% of land respectively. We show separate bar graphs for congresses elected in 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, we see evidence of redistricting in almost all state-decades. Almost all states do redistrict and almost all do it

[15] We compute this measure based upon land area rather than population since census tracts, which are population-based, were only introduced across the entirety of the United States for the 1990 census.

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. These patterns hold even in the past two decades when there have been more delays due to legal challenges to redistricting.

We then ask whether a higher fraction of land is redistricted when the potential redistricting government is unified[16]. We follow our main specification in Equation 1 and regress our measure of the extent of redistricting on our two partisan legal control variables, stateXdecade fixed effects, and year fixed effects. We show our results in Table 2. Column 1, the full five-decade sample, shows that 8.4% more land is redistricted when Republicans have control relative to no party having legal control. The coefficient for Democrats, by contrast, is less than $\frac{1}{4}^{th}$ the size at 2.0% and very far from statistically significance. In the past two decades, the coefficient for Republicans is even larger; Republicans shift 11.8% more land when they control redistricting than when no party does. The coefficient for Democrats is also larger; it is 5.9% but not statistically significant at conventional levels.

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 4. Due to data constraints, we only show results for the past two

[16]In this case only we allow for control to vary within the decade.

Table 2—: District Changes from the Prior Election Period

	(1)	(2)	(3)	(4)
Republican Control: Effect on District Change				
Control x Election Ending in 2	0.084***	0.042	0.118***	0.104***
	(0.023)	(0.033)	(0.032)	(0.033)
Control x Election Ending in 4	0.003	-0.014	0.015	0.025
	(0.013)	(0.013)	(0.020)	(0.022)
Control x Election Ending in 6	-0.002	0.002	0.001	0.015
	(0.009)	(0.011)	(0.014)	(0.015)
Control x Election Ending in 8	-0.013	0.014	-0.021*	-0.022*
	(0.010)	(0.011)	(0.012)	(0.012)
Democrat Control: Effect on District Change				
Control x Election Ending in 2	0.020	0.002	0.059	0.039
	(0.018)	(0.021)	(0.038)	(0.040)
Control x Election Ending in 4	-0.007	-0.012	0.000	0.001
	(0.008)	(0.011)	(0.009)	(0.009)
Control x Election Ending in 6	0.007	0.008	0.001	0.007
	(0.009)	(0.011)	(0.017)	(0.020)
Control x Election Ending in 8	-0.015*	-0.016	-0.014	-0.019
	(0.008)	(0.010)	(0.012)	(0.013)
Sample	l968-2016	l968-1996	l998-2016	l998-2016
Outcome Basis	Land	Land	Land	Pop
Number of Observations	1060	640	420	420
R2	0.649	0.631	0.686	0.694

Note: Each column provides estimates for the impact of political control over redistricting in various elections on district changes from the previous election. The term *Control x Election Ending in t* calculates the influence of a political party's control over redistricting for election years ending in t on district alterations between the election years ending in t and t-2. In columns 1 to 3, the dependent variable is the proportion of land within a state that has shifted districts since the previous election. Column 4 utilizes the percentage of a state's population that changes districts as the dependent variable. The estimates for the effects of Republican and Democratic control are calculated simultaneously and depend on state-decade and year fixed effects. Standard errors, clustered by state-decade, are noted in parentheses.

decades. Overall, the estimates are of similar magnitude.

B. Effects on Seat Shares: Fixed Effect Estimates

We now present our baseline results of the impact of partisan legal control on partisan seat shares, estimated using stateXdecade and year fixed effects[17]. Our

[17]We also replace the dependent variable with numbers of seats rather than seat shares. Our estimates imply very similar seat share effects and the patterns of significance mirror the estimates on seat shares.

final sample consists of 212 state-decades (1060 elections). Out of these, we find 56 instances of Democratic control over redistricting. In contrast, we find only 35 instances of Republican control. This is due to the dominance of Democrats in the earlier portion of our sample. Actually, 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.

Our main estimates are presented in Table 3. 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 column shows estimates for the full sample, the second for the first three decades of the sample (1968-1996), and the third for the recent two decades (1998-2016). In each panel, the first three rows show the effects on the first three elections after redistricting respectively, the fourth shows the average effect across the first three elections following redistricting and row five shows a pre-trend[18]. Since the dependent variable is the Republican seat share, a positive coefficient reflects a relative *increase* in Republican seats and a negative coefficient reflects a relative *decrease* in Republican seats or relative *increase* in Democratic seats.

Overall, we do not see average impacts of legal control by a party on subsequent seat share over the full sample for either party. Average effects for Democrats are near zero in all periods. There are pre-trends for Democrats in both the full sample (significant at below a 10% level) and for the early period (significant at below a 5% level). The pre-trends are 20% smaller in the recent period and statistically insignificant at conventional levels. This means that Democrats have

Estimates are available from the authors upon request.

[18]Following Equation **??**, all coefficients are normalized to the coefficient for elections in years ending in 0

Table 3—: Main Specification

	(1)	(2)	(3)
Republican Control: Effect on Republican Representative Seat Share			
Control x Election Ending in 2 (β_2^R)	0.048*	-0.010	0.091***
	(0.025)	(0.033)	(0.034)
Control x Election Ending in 4 (β_4^R)	0.010	-0.058	0.057*
	(0.039)	(0.079)	(0.033)
Control x Election Ending in 6 (β_6^R)	0.047	-0.024	0.101**
	(0.045)	(0.085)	(0.048)
Average Effect ($\frac{\beta_2^R+\beta_4^R+\beta_6^R}{3}$)	0.035	-0.031	0.083**
	(0.033)	(0.059)	(0.036)
Control x Election Ending in 8 (β_8^R)	0.001	-0.014	0.013
	(0.033)	(0.058)	(0.041)
Democrat Control: Effect on Republican Representative Seat Share			
Control x Election Ending in 2 (β_2^D)	-0.026	-0.034	-0.016
	(0.024)	(0.028)	(0.046)
Control x Election Ending in 4 (β_4^D)	-0.011	-0.012	-0.022
	(0.036)	(0.046)	(0.051)
Control x Election Ending in 6 (β_6^D)	0.030	0.016	0.054
	(0.040)	(0.050)	(0.059)
Average Effect ($\frac{\beta_2^D+\beta_4^D+\beta_6^D}{3}$)	-0.002	-0.010	0.005
	(0.030)	(0.038)	(0.049)
Control x Election Ending in 8 (β_8^D)	-0.052*	-0.055*	-0.045
	(0.027)	(0.033)	(0.049)
Sample	l968-2016	l968-1996	l998-2016
Republican Treatments	35	13	22
Democrat Treatments	56	44	12
Number of Observations	1060	640	420
R2	0.778	0.690	0.871

Note: Each column presents coefficients from a single regression with each observation representing a state-year. The first column employs data spanning 1968-2016. The second column uses data from 1968 to 1996. The third column utilizes data from 1998 to 2016. The treatment variable is the unilateral legal control of a political party over redistricting for election years ending in 2. Row estimates display the impact of a political party having legal control over the redistricting process during years ending in 2 on a state's proportion of congressional representatives who are Republican in election years ending in 2, 4, 6, and 8. *Control X Election Ending in 8* indicates the coefficient for control in elections occurring in years ending in 8 from the previous decade. All specifications include state-decade and year fixed effects. Standard errors, clustered by state-decade, are noted in parentheses.

tended to do worse in the elections right before redistricting in the state-decades where they achieved legal control.

There are no statistically significant average impacts for Republicans in the full sample or in the early period except in the first election after redistricting in the full sample. However, in the recent period, there is an average increase in the Republican seat share for a state with Republican legal control of 8.3 percentage points in the three elections following redistricting. Moreover, effects are individually statistically significant with a 10 percent level of confidence or smaller for each of the three elections following redistricting. For Republicans, pre-trends are small and statistically insignificant in all three samples.

C. Effects on Seat Shares: Regression Discontinuity Estimates

We now present our difference-in-discontinuities estimates. We show these estimates in Table 3. We estimate our regressions separately by party for the full sample, the early sample and for the recent period. Following Jeong and Shenoy (2024), we use a bandwidth of 0.18 across all samples though we show robustness to bandwidth in Appendix Figures A.5 and A.6. The RD requirements reduce our sample size substantially to 52 state-decades over the full sample (of which 32 are treated) for Republicans and 77 for Democrats (of which 42 are treated). Broken down by time period, the samples are smaller still.

Mirroring our panel estimates, we find that the only estimates which are statistically significant are for Republican legal control in the recent period. We estimate an increase of 19.7 percentage points in the state delegation's Republican seat share due to unification resulting from a narrow Republican gubernatorial win. The RD point estimates are larger in magnitude than the panel point estimates. However, the standard errors are also sizable and the panel estimate lies well within a 95% confidence interval for the RD estimates. The recent period effects of Republican legal control yield only the second largest magnitude of the six estimates we present; the largest are for Republicans in the early period (with

Table 4—: Average Effects: Governor RD Conditional on Unified Legislature Party

	Republican Governor Vote Share			Democratic Governor Vote Share		
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Period Party Effect (β_{pre})	-0.037	-0.075	0.000	-0.032	-0.015	0.040
	(0.102)	(0.133)	(0.135)	(0.066)	(0.081)	(0.172)
Avg Party Control Effect (β_{Treat})	-0.116	-0.401	0.197*	-0.054	-0.042	-0.079
	(0.179)	(0.284)	(0.116)	(0.054)	(0.067)	(0.080)
Sample	l968-2016	l968-1996	l998-2016	l968-2016	l968-1996	l998-2016
Legislature	R	R	R	D	D	D
Treatments	32	13	19	42	32	10
Number of Observations	260	130	130	385	295	90

Note: Each column shows regression discontinuity estimates for the effect of a political party's legal control over the redistricting process on the proportion of a state's congressional representatives who are Republican. Both the average effect and the pre-period effect are reported. All estimates employ a triangular weighted kernel with an 18% bandwidth. Columns 1 to 3 assess the impact of a Republican governor, conditional on a Republican state legislature, in states where unified government leads to legal control of redistricting. Columns 4 to 6 evaluate the effect of a Democratic governor, given a Democratic state legislature, in states where unified governance results in legal control of redistricting. Columns 1 and 4 provide coefficients for the entire sample period from 1968 to 2016. Columns 2 and 5 offer coefficients for the period from 1968 to 1996. Columns 3 and 6 present coefficients for the period from 1998 to 2016. Standard errors, clustered by state-decade, are noted in parentheses.

the opposite sign effect)[19]. However, the early period Republican effect estimate is based upon a sample of only 13 state-decades and is far from statistical significance at conventional levels. Appendix Figures A.3 (Demoratic legal control) and A.4 (Republican legal control) show four regression discontinuity plots each. For each party, we show the average of the two pre-period Republican seat shares on the left. The top row shows results for the full sample and the bottom row for the recent sample. The two appendix figures visually validate the results in the table that the only statistically meaningful change in seat shares from unification is for Republicans in the recent period.

Because of the small sample, the estimates are noisier than our panel estimates. Given the small number of events that form our sample, our results are remarkably stable. Appendix Figures A.5 and A.6 show the robustness of our estimates to bandwidth choice. We show estimates at every percentage point of bandwidth 0.05 to 0.20 and then every 0.05 from 0.20 to 0.35. Estimates are slightly lower for low bandwidths for Republican legal control in the modern era. However, these are estimated off of a very small number of states rises above 20, the estimates

[19] A downside of this approach is the sensitivity to outliers. Excluding 1970s ME results in estimates of .046(.118) and -.128(.199) for columns 1 and 2 in Table A.9, respectively.

are quite stable.

Overall, we see the gubernatorial RD evidence as consistent with the evidence from our main specification. We find evidence of a sizable impact of legal control upon state delegation seat shares; however, we see it only for the modern period and for the Republican party.

D. Effects on Seat Shares: Simulation-Based Binned Matching Estimator

In this subsection, we present estimates from our SBBM estimator. We first show that our two samples of state-decades with between 20% and 80% probability of Democratic legal control and Republican legal control respectively only overlap in one state-decade. Appendix Figure A.7 shows this in a scatter plot of the probability of Democratic legal control on the probability of Republican legal control where each point is a state-decade in the sample.

We present estimates following Equation 6 reporting η and β, which are respectively comparable to the pre-period and average effects from Table 3. To obtain estimates of the impact of legal control, we jointly estimate the effect for the Republican and Democratic party. The SBBM estimator restricts the identifying variation such that treatment and control observations are comparable in their likelihood of being under political control of the same party.

In all samples, the estimates in Table 5 are very similar to what we estimate using the the state-decade and year F.E. model, which is reassuring. In Appendix Table A.1 we show results of the SBBM design calculating the probability that a governor matches an existing unified legislatures and find results very similar to the standard gubernatorial RD model whose results we presented in Table 3.

Since sampling-based standard errors would have to account for error in our estimation of probabilities of legal control, we opt for design-based inference (Abadie et al. (2020)) for the SBBM estimator. As a result, we show randomization inference figures and compute p-values for these estimates. We consider two different

Table 5—: Simulation Based Binned Matching Design

	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Period Party Effect (η)	-0.019	-0.016	0.011	-0.047	-0.004	0.016
	(0.041)	(0.036)	(0.062)	(0.043)	(0.054)	(0.060)
Avg Party Control Effect (β)	0.023	-0.011	-0.058	-0.030	0.109***	-0.001
	(0.038)	(0.036)	(0.068)	(0.046)	(0.040)	(0.053)
Party	Rep	Dem	Rep	Dem	Rep	Dem
Sample	1968-2016		1968-1996		1998-2016	
Number of Observations	800		490		310	
Republican Treatments	31		18		18	
Democrat Treatments	44		33		11	

Note: Each column shows a different estimate for the average effect of a political party having legal control of the redistricting process after redistricting has occurred on the fraction of a state's congressional representatives that are Republican. Columns 1-2, 3-4 and 5-6 show coefficients from a pooled regression which includes states that had a 20%-80% probability of having a Republican or Democratic trifecta. Columns 1,3,5 show estimates of the effect of Republican control of redistricting. Columns 2,4,6 show estimates of the effect of Democratic control of redistricting. Columns 1 and 2 are jointly estimated with data from 1968-2016. Columns 3 and 4 are jointly estimated with data from 1968-1996. Columns 5 and 6 are jointly estimated with data from 1998-2016. All specifications are conditional on Republican and Democratic probability bins of political control-decade fixed effects. There are 6 probability bins for each political party, with each bin capturing 10 percentage points. Standard errors, clustered by state-decade, are noted in parentheses.

types of randomization: (1.) fully randomizing the 35 Democrat and 56 Republican legal control treatments across state-decades and (2.) fully randomizing Democrat and Republican legal control treatments across state-decades within panels[20]. The inference looks similar to conventional standard errors except that we lose significance for the gubernatorial RD in the modern period and our p-values decline to below the 5% level for the SBBM estimator. These results are shown in Appendix Table B.1 with randomization figures in Appendix B[21].

[20]For the 1998-2016 panel this would re-assign 22 Republican and 12 Democratic treatments across state-decades. For the 1968-1996 panel this would re-assign 13 Republican and 44 Democratic treatments across state-decades.

[21]The interpretation of sampling-based inference is not completely clear in our context since our data consists of almost the entire population of states with redistricting over the 50 years of U.S. history. As a result, we have also computed randomization inference-based estimates for our other estimation methods.

E. Robustness

In Table 6, we show the robustness of our results to alternative specifications. In Table A.2, we show the robustness of our results to treatment as defined by Friedman and Holden (2009). In Table A.3, we additionally show the robustness of our results to alternative ways of defining legal control using our data. These are all done with our panel estimation strategy. Our estimates are largely robust. We report the average of the coefficients for the three elections following redistricting and we break up our table into three panels: the full sample, the early sample, and the later sample. In column 1 of Table 6, we repeat our baseline panel estimates. In column 2, we replace our legal control variable with unified control as our main treatment variable[22]. This lowers the coefficient on Republican control in the recent period, consistent with the view of unified government as legal control measured with error; however, the coefficient remains significant at a 95% level of confidence. The coefficients from other time periods and parties are not substantively impacted. In column 3, we control linearly for the statewide vote share for the 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 6.7 percentage points, but remain statistically significant at the 95% level of confidence. We again do not see any sizable or statistically significant estimates for Democrats.

In column 4, we show our two way fixed effects estimates by replacing our state-decade effects with state effects. The estimates are substantially larger though still statistically insignificant at conventional levels for Democrats. Since effect sizes for Republicans are increasing over time, this is exactly what we would expect

[22]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.

Table 6—: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. 1968 - 2016						
Rep Average Effect	0.035	0.031	0.032	0.105***	0.045	0.025
	(0.033)	(0.033)	(0.028)	(0.031)	(0.034)	(0.046)
Dem Average Effect	-0.002	-0.001	-0.020	-0.045	-0.009	-0.039
	(0.030)	(0.030)	(0.026)	(0.034)	(0.028)	(0.041)
Number of Observations	1060	1060	1060	1060	1060	1060
R2	0.778	0.778	0.839	0.495	0.794	0.822
Panel B. 1968 - 1996						
Rep Average Effect	-0.031	-0.029	-0.004	-0.031	-0.055	-0.025
	(0.059)	(0.060)	(0.048)	(0.035)	(0.063)	(0.089)
Dem Average Effect	-0.010	-0.005	-0.025	-0.030	-0.023	-0.060
	(0.038)	(0.038)	(0.031)	(0.028)	(0.035)	(0.052)
Number of Observations	640	640	640	640	640	640
R2	0.690	0.690	0.795	0.547	0.723	0.755
Panel C. 1998 - 2016						
Rep Average Effect	0.083**	0.072*	0.067**	0.123***	0.092**	0.078**
	(0.036)	(0.039)	(0.033)	(0.030)	(0.040)	(0.037)
Dem Average Effect	0.005	0.002	-0.010	0.053	-0.016	0.019
	(0.049)	(0.043)	(0.044)	(0.061)	(0.050)	(0.058)
Number of Observations	420	420	420	420	420	420
R2	0.871	0.871	0.885	0.754	0.904	0.891
Legal Control	X					
Unified Control		X				
Vote Share Control			X			
2-Way FE				X		
State Linear Trends					X	
Division x Year FE						X

Note: Each column shows a different estimate for the average effect of a political party having legal control of the redistricting process after redistricting has occured on the fraction of a state's congressional representatives that are Republican. Panel A presents coefficients over the full sample from 1968-2016. Panel B presents coefficients over the full sample from 1968-1996. Panel C presents coefficients over the full sample from 1998-2016. Unless stated otherwise, all specifications include state-decade and year fixed effects and treatment is defined as unilateral legal control of a political party over redistricting. Column 1 is the baseline average estimate shown in main table. Column 2 uses unified control of a state government in the year before a redistricting event occurs as treatment. Column 3 includes a statewide Republican vote share in elections for the House of Representatives as a control. Column 4 replaces state-decade fixed effects with state fixed effects. Column 5 adds state-specific linear time trends to the baseline model. Column 6 replaces year fixed effects with census division-year fixed effects. Standard errors clustered by state-decade are in parentheses.

given the recent work in the new panel effects estimation literature (Goodman-Bacon (2021)). 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 where cross-sectional fixed effects are only taken over the limited time period of a decade. In Column 5, we show results for our very taxing state-specific linear trends specification where we add state-specific trends to the 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 2.4 percentage points of baseline and all but two of the coefficients change by less than 2 percentage points of our baseline estimates. Finally, in Column 6, we replace year dummies with Census Division X year dummies. This has little impact on the estimates for Republicans but does lower the estimates for Democrats somewhat in the early period though without a change in statistical significance.

We now turn to Table A.2 where we take our treatment variable from data compiled by Friedman-Holden (Friedman and Holden (2009)). The Friedman-Holden data end in 2006 and are largely missing for the 1970s. Thus, we only estimate effects over the three decades for which they have data. Friedman and Holden have two different methods of classification: one based upon who drew the maps that were implemented and a second based upon who had control over the government. We present estimates using both of their methods and compare it to our estimates. We first estimate our baseline specification on our data limited to the time period 1978-2006. The estimate for Republican legal control is 10.1 percentage points and is highly statistically distinguishable from zero. For Democrat legal control, it is 2.4 percentage points and far from statistically distinguishable

from zero. In Column 2, we use Friedman-Holden's classification of legal control but we place court ordered maps in the no legal control group. In Column 3, we follow Friedman-Holden and allow court ordered maps to be classified as partisan. In Column 4, we use Friedman and Holden's government control variable which is veto-proof unified control. All of the estimates of Republican control vary by up to 2 percentage points. Given the sample size and standard errors, our results are quite robust to using the Friedman-Holden data.

We also show robustness to our definition of legal control and present the results in Appendix Table A.3. Column 1 shows our baseline results. In Column 2, we allow non-advisory commissions to count as treated if the state-decade has a trifecta. In Column 3, we go in the opposite direction and recode advisory commissions as neither party ever having legal control. In Column 4, we drop all commission state-decades from our sample entirely. The results are remarkably robust. In Column 5, we expand our treatment group to reclassify state-decades with a unified legislative supermajority and a governor of the opposite party as instances of partisan control. We find these estimates are substantially lower suggesting that state legislatures do not use supermajority power to gain leverage on redistricting.

We address one additional identification concern: that legal control over redistricting usually entails unified control of the legislature by a single party and almost always also unified control over the state government. Thus, subsequent gains in the House of Representatives could merely reflect popular policies implemented by the trifecta in the state. As a result, we estimate a set of time 'placebos' where we reassign the year of redistricting to just before election years ending in 8, 0, 4 and 6 respectively in addition to our baseline of years ending in 2. We thus look at whether we see gains in House seat shares with state-level trifectas following control in years other than those ending in 2. Appendix Table A.4 shows our estimates. Out of 30 coefficients estimated (2 parties X 5 years of placebo or actual redistricting X 3 time periods), only two are statistically

significant at below a 10% level. Moreover, the largest size coefficient is that for Republican control in the recent period. Thus, we do not think that our estimates reflect the effect of other policies implemented with unified legislative legislative plus gubernatorial control.

We now go beyond effects on seat shares. When political parties redistrict to their advantage, it is by packing the opposition into concentrated districts and cracking districts where the opposition has a modest majority (Friedman and Holden (2008)). The purpose is to minimize risk-adjusted expected wasted votes for one's own party and maximize the same for the opposition. What are wasted votes? They are the votes for a political party that either exceed the number necessary for a candidate's victory in a district or are cast for candidates who ultimately lose in other districts. In a pair of legally influential papers, Stephanopoulos and McGhee (2015) and Stephanopoulos (2017) introduce and use the concept of the efficiency gap. The efficiency gap has been widely used in court cases, including it has played a prominent role in recent Supreme Court decisions. The efficiency gap measures the difference in wasted votes across the parties as a fraction of the electorate[23]. We describe it in more detail in Appendix D.

Using our main panel estimator, we replace the dependent variable with the two party efficiency gap, computed at the stateXelection level. We thus capture the differential changes in the efficiency gap due to legal control over redistricting. We focus on the vote share in the district for the president to avoid candidate selection issues. Since in some decades, there are three presidential elections and in others two, we limit our analysis to the last presidential election before redistricting and the first presidential election after redistricting within each decade. We report the static estimates of the impact of Democratic legal control and Republican legal control on the efficiency gap. As the efficiency gap decreases, this represents

[23] We calculate the efficiency gap as $\frac{\text{Wasted Rep Votes - Wasted Dem Votes}}{\text{Total Votes}}$.

an increase in wasted Democratic votes. We break the results into three panels: one for the full sample, one for the early sample and one for the recent sample. Results are in Appendix Table A.5.

The results look similar to our main estimates. We find no significant change in the efficiency gap for Democrats following Democratic legal control but sizable reductions in the efficiency gap for Republicans following Republican legal control. In the next two columns, we show in two different ways that legal control reduces political competition. We find that winning vote shares increase for both parties by 1-2 percentage points following legal control. Effects are only significant for Republicans, partly due to a slightly higher estimated impact and partly due to lower standard errors. We also find similarly sized and significant effects of Democratic control in the early period. Moreover, following legal control, there is a decline in the fraction of states where the winning party gets less than 60% of the two party vote share. Actually, the estimate is twice the size for Democratic legal control as it is for Republican but the Republican legal control is statistically significant and the Democrat estimate is not. This is likely due to the larger fraction of states with Republican legal control in the recent period. In the last two columns, we estimate the impact of legal control on the average Democratic vote share and the average Republican vote share separately. We find that in the recent period, legal control by a party increases the average winning vote share of the opposite party. In contrast, the own party effect is null or negative. This cross-party effect is about twice as large for the Republican impact on Democrats: 5.4 percentage points as opposed to a 2.3 percentage point impact of Democratic legal control on winning Republican vote shares.

We provide more non-parametric evidence in Appendix Figures A.8 (early period) and A.9 (recent period). We estimate the effect of legal control restricting ourselves to districts with below a given presidential vote share. We start with a sample of all districts with less than a 55% presidential vote share for the party with legal control and then expand the sample and re-estimate for every percent

up to 80%. The evidence from the early period is noisy and small in magnitude. However, in the recent period, we see that Republican legal control has little impact in competitive Democratic districts but increases the Democratic vote share by about 5 percentage points in safe Democratic districts. This could be due to cracking competitive districts or packing uncompetitive ones.

We also have an extended discussion of racial gerrymandering in Appendix E. We find no evidence that either party disproportionately relocates minority voters. We do find that Republicans move minorities to more extreme districts. We also find that both Republicans and Democrats are slightly more likely to elect a minority to the House following legal control by the Democrats but not following legal control by the Republicans.

VI. Mechanisms and Discussion

In this section, we show that the difference in the average effect of legal control between the Republican and Democratic parties is due to (1.) a difference in behavior in small states and (2.) a surprising greater prevalence of large states with legal control for Republicans. The greater prevalence of legal control in large states for Republicans is, in turn, due to a large rate of denied trifectas in Democratic states as well as a greater fraction large Democratic states with commissions. This latter reason for the discrepancy has increased in importance over time.

We begin our investigation by looking at the role of delegation size. Is it possible that it is just more difficult to gerrymander in small states than large and that Democrats have more small states? Appendix Table A.6 shows the impact of size distribution of state-decades on the estimates. In Column 1, we show our benchmark results. In Column 2, we show estimates for states with 9 or fewer representatives ('small states'). In Column 3, we show states with 10 or more representatives ('large states'). Again, we break down our estimates into three

panels: full sample, early sample, and recent sample.

We see three things of note. First, the effect in large states is 7.2 percentage points and significant at below the 10% level for Republicans even in the full sample. Second, in recent years, the effects for both Democrats and Republicans are statistically significant in large states and for both, with greater than a 95% level of confidence. In fact, the point estimate is larger in magnitude for Democrats (11.1%) than for Republicans (8.6%). Third, the standard errors are notably smaller for the larger state sample. This is because seat share variance is quite large for small states. Seat shares can easily fluctuate across years by 50 or 100 percentage points. We also note that the large state estimates are almost always larger than their small state counterparts and are never statistically significant.

Appendix Figure A.10 shows estimates and 95% confidence intervals for Republicans and Democrats broken up into states with 10 or more delegates and those with 9 or fewer. The figure also shows on the right Y-axis the fraction of state-decades in each bin by the height of the red and blue bars. Appendix Figure A.11 shows the same breaking state-decades into 7 size categories. Results look similar. Our findings show that neither party seemed to benefit much either in small or large states in the early period. However, both parties seemed to benefit from legal control in large states in the later period. Republicans also benefited from legal control in the small states whereas Democrats did not.

In Columns 4 and 5 of Appendix Table A.6, we reweight our estimates so that both parties have the same number of state-decades with legal control. We reweight separately for each sample. Note that the coefficients do change (though only slightly) even for the party not being reweighted. This is because the weighting affects the year fixed effects. We note that Republican weights increase the average magnitude of the estimates for Democrat legal control and Republican weights decrease the average magnitude of the estimates for Republican legal control. This is what we expect since Republican legal control is disproportionately

in larger states. However, even after reweighting, large differences remain between the effects of Republican and Democratic legal control, underscoring that the gaps do reflect a different distribution of delegation sizes as well as different effectiveness of legal control in small states. In Appendix Table A.7, we also re-estimate our main panel estimates weighted by number of representatives. We additionally show a district-level specification where we estimate Equation 1 altering the dependent variable to take on a value of 1 if the legislator is a Republican and 0 if a Democrat. These findings confirm our prior findings: differential effects by party are closer when weighted by number of seats but still Republican estimates are larger.

We also present a Oaxaca decomposition of our panel estimates in Table 7. In particular, we decompose the gap between the main estimate for Republicans and that for Democrats into differences in the size distribution and differences in the coefficients. In the earlier period and overall, the gaps are substantially smaller than in the recent period and over 95% is attributable to the coefficients. In the recent period, the gap is 7.2 percentage points, 27% of which is attributable to Republicans having more large states with legal control. This is for two main reasons. First, many large Democratic states have often elected Republican governors just before the redistricting cycle. Examples in 2001 alone include Illinois (George Ryan), Massachusetts (Paul Cellucci), and New York (George Pataki). In the past two decades, there were 12 instances of Democratic legal control and 10 of denied Democratic legal control due to a Republican governor. In contrast, on the Republican side, there were 22 instances of Republican legal control and 7 of denied legal control due to a Democratic governor. A second source of the smaller number of large states with Democratic legal control is that large Democratic states have been more likely to delegate to commissions. As an example, in 2011, California had a trifecta but also had a redistricting commission. Over the past 2 decades there have been 26 instances of Democratic unified government, five of which did not result in legal control due to a commission. During

the same time period, there were 34 instances of Republican unified government, four of which did not result in legal control due to a commission. Overall, 12% of instances of unified control were blocked on the Republican side and 19% were blocked on the Democratic side. Moreover, three of the five commissions on the Democratic side were in large states whereas all of the Republican commissions were in small states.

Table 7—: Oaxaca Decomposition

	Beta Republicans	Beta Democrats	Share Republicans	Share Democrats
		Sample: 1968-2016		
Between 2-9 Representatives	0.011	-0.013	0.514	0.643
Over 10 Representatives	0.072	0.021	0.486	0.357
Aggregate Difference: -0.040	Beta Contributions: 0.959		Share Contributions: 0.041	
		Sample: 1998-2016		
Between 2-9 Representatives	0.064	0.050	0.455	0.667
Over 10 Representatives	0.086	-0.111	0.545	0.333
Aggregate Difference: -0.072	Beta Contributions: 0.732		Share Contributions: 0.268	

Note: This table presents average party control effects for Republicans and Democrats in states with between 2-9 representatives and 10 or more representatives. Estimates are provided for the entire sample period (1968-2016) and the most recent two decades (1998-2016). Beta Republicans (Beta Democrats) indicates the average effect of Republican (Democratic) control. Share Republicans (Share Democrats) shows the percentage of Republican (Democratic) control among states with 2-9 and 10 or more representatives. Aggregate difference refers to the difference in average effects; it is calculated by taking the absolute value of the average Democrat effect minus the average Republican effect. Beta contributions estimates what percent of the aggregate difference is attributable to variations in the beta estimates. Share contributions estimates what percent of the aggregate difference is due to differences in the distribution of treatment delegation sizes.

We also consider one additional theory which could possibly explain the larger impact of Republicans in the modern era. It is possible that Republicans have been undoing longstanding Democrat gerrymanders from many decades of the "Solid South". More generally, maybe legal control only leads to a shift in seat shares when there is a change in legal control from the prior decade and Republicans legal control has been more in states with prior Democratic control.

Legal control in the absence of a change in legal control status from the prior decade may act like attenuation. If Republicans are more likely to inherit legal control from Democrats than Democrats are to inherit from Republicans, that could potentially explain the differences in the coefficients across the two parties.

Since legislative realignment happened at the federal level in the 1990s and at the state level largely in the 2000s and even 2010s, this theory is also capable of explaining the timing of our effect. Appendix Table A.8 displays the number of instances of legal control by party and decade. It further breaks these instances of partisan control down by continuity in power (same party partisan control) or change in power (opposite party legal control or no partisan control). This table shows that in fact Republican legal control is more often accompanied by a transition in power. Out of 22 instances of Republican legal control, 15 (68%) were cases of new legal control; however, out of 15 instances of Democratic legal control over the same time period, only 5 (41%) were cases of new legal control.

Of course, the partisan gap in "new" legal control between Democrats and Republicans in the modern period would only explain the gap between average Democratic effect in the recent period if there was an effect of Democratic legal control to attenuate. Appendix Table A.9 estimates treatment effects separately by type of party and by transition type. The first panel replicates our baseline panel estimates. The second throws out state-decades with legal control where legal control status was different in the prior decade. It thus estimates the effect of "continuity-in-power" legal control. The last panel does the converse. It throws out state-decades with legal control where legal control status was the same in the prior decade. It thus estimates "Change in Partisan Alignment" legal control. Focusing on the recent period, whose estimates are in the third column, we see noisy but smaller and statistically insignificant effects when a party retains legal control. However, restricting to instances where both parties experienced a change in legal control, we see statistically insignificant coefficients of the wrong sign for the effects of Democratic legal control while we see a significant 9.6 percentage point effect for Republicans. Thus, we conclude that differences across parties in the effect of legal control in the modern period are not attributable to greater continuity in power for Democrats.

While we we are not able to definitively say what has caused the gaps between

Democrats and Republicans in small states in the modern period, we now show evidence that points towards two separate but plausible theories: (1.) a norms shift which did not impact Democrats in small states but did affect Republicans and large state Democrats, and (2.) greater use of new technology (GIS software) subject to geographical constraints. In Appendix Table A.10, we show, consistent with our prior results, that states under Republican legal control and large states under Democratic control redistrict more land but small Democratic states do not. One possible explanation of our results is that since Democrats live in greater concentrations Rodden (2019), it may be technically more difficult to create Democratic gerrymanders. One explanation that we think is unlikely is that is the common view that it is due to a rise in racial gerrymanders. In this theory, Republicans cooperate with minority Democrats to create majority-minority strongholds at the expense of Democrat seat share. However, Republicans do not create more majority-minority districts or representatives with Republican legal control (see Appendix E). Also, though Democrats *do*, consistent with the findings in Sabet and Yuchtman (2023), increase the number of minority representatives, we find that the effects on minority representation of Democratic control are relegated only to large Democratic states where there is not a gap between the parties in effectiveness of legal control on representation.

VII. Aggregate Effects

We have so far estimated the impact of legal control over redistricting on subsequent seat shares. In this section, we look at the aggregate implications of the the abilities of parties to redistrict on the partisan seat distribution in the House of Representatives. We translate our estimates of average seat share impacts by party into aggregate partisan effects and compare them to partisan seat margins in Congress.

We use estimates by party, state size (small: < 10, large: 10+) and time period

(first three decades, recent two decades) and compute implied seat share changes from partisan redistricting, 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:

$$\Delta DemSeats_{y,d} = 2 \sum_{s} \left[\beta_d^{D,s} N_d^{D,s} A_d^{D,s} - \beta_d^{R,s} N_d^{R,s} A_d^{R,s} \right] \tag{8}$$

where $\beta_d^{P,s}$ is the effect of party P control on the seat share fraction for party P in decade d in a state of size s, $N_d^{P,s}$ is the number of states with party P control in decade d of size s, and A_d^P is the average number of seats for states of size s with party P control in decade d.

We show the results of these computations in Table 8. 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 8 seats. In the 2000s, we compute that seats shifted by an average of zero seats on net but because both parties shifted around 8 seats from legal control. In the 2010s, the Republicans increased their seat share in Congress. With an average seat seat margin in the 2010s of 53 and an average net increase towards the Republicans of 28 seats, Republicans on net shifted 54% of the average gap between the parties towards themselves. 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. 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.

Table 8—: Aggregate Partisan Effects by Decade

	1970s	1980s	1990s	2000s	2010s
Small States with Dem Control	9	9	10	4	4
Average Seats	5.333	5.333	4.800	5.500	6.000
Control Avg. Effect	-0.040	-0.040	-0.040	0.050	0.050
Large States with Dem Control	5	6	5	3	1
Average Seats	14.000	18.333	17.400	26.333	18.000
Control Avg. Effect	0.055	0.055	0.055	-0.111	-0.111
Small States with Rep Control	4	2	2	2	8
Average Seats	4.250	7.000	2.500	3.500	6.625
Control Avg. Effect	-0.043	-0.043	-0.043	0.064	0.064
Large States with Rep Control	3	2	0	5	7
Average Seats	21.667	16.500	0	17.600	19.429
Control Avg. Effect	0.011	0.011	0.011	0.086	0.086
Seat Share Effect: Dems	1.953	4.171	2.896	-7.679	-0.796
Seat Share Effect: Reps	-0.021	-0.241	-0.215	7.976	15.038
Net Effect	1.932	3.930	2.681	0.296	14.242
Average Margin	95	86	62	21	53
Net Effect as % of Avg Margin	4.068%	9.140%	8.648%	2.822 %	53.742%

Note: 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.

VIII. Conclusion

In this paper, we have shown that historically neither of the two main US political parties used legal control over redistricting to benefit their party's representation in Congress. However, in recent years this has changed. On average Republican legal control increases the Republican seat share in Congress by 8.2 percentage points. Though there is no significant average impact of Democratic

legal control on the Republican seat share, the null effect masks substantial heterogeneity. Large (10+ representatives) Democratic states with legal control operate similarly to Republican states and use legal control to benefit their party. However, small Democratic states (9- representatives) do not redistrict to benefit Democrats. We also note that in recent years, Democratic states have been more more often denied legal control by electing a Republican governor; they have also been more likely to delegate the drawing of maps to a bi-partisan or non-partisan commission. In addition to our findings, we think our paper will also be of use to the literature in that we provide a novel method to estimate the causal effects of control over a state legislative body.

One limitation of our paper is that we do not explain why large states under partisan control and small states under Republican control have changed their behavior over time. In contrast to the public, economists may not be surprised that parties manipulate vote aggregation to benefit themselves. However, there are reasons why they might not and each of these reasons provides a potential explanation for why party behavior has changed. 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. Fifth, gerrymandering may require detailed spatial information which has improved over time. Unfortunately, distinguishing between these different motives goes beyond the scope of this paper. 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 be complementary to the research presented here.

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A. Appendix Figures and Tables

Table A.1—: Binned Matching Design

	Republican Control			Democratic Control		
	(1)	(2)	(3)	(4)	(5)	(6)
Avg Party Control Effect	-0.033	-0.212	0.197	-0.018	-0.009	-0.038
	(0.101)	(0.134)	(0.141)	(0.042)	(0.051)	(0.050)
Sample	l968-2016	l968-1996	l998-2016	l968-2016	l968-1996	l998-2016
Party Control Bin x Decade FE	X	X	X	X	X	X
Party	Rep	Rep	Rep	Dem	Dem	Dem
Treatments	29	12	17	37	28	9
Number of Observations	235	115	120	335	260	75

Note: Each column shows a different estimate for the average effect of a political party having legal control of the redistricting process after redistricting has occured on the fraction of a state's congressional representatives that are Republican. Columns 1-3 show estimates of the effect of Republican control of redistricting. Columns 4-6 show estimates of the effect of Democratic control of redistricting. Columns 1 and 4 are estimated with data from 1968-2016. Columns 2 and 5 are estimated with data from 1968-1996. Columns 3 and 6 are estimated with data from 1998-2016. All specifications are conditional on a unified legislature and include a fixed effect based on decade x probability bins of the governor matching the legislature. There are 10 probability bins for each political party, with each bin capturing 10 percentage points. All specifications limit the sample to observations with probability of partisan governors between 20% and 80%. Standard errors are clustered at the state-decade level.

Table A.2—: Alternate Treatment Definitions

	(1)	(2)	(3)	(4)
Panel A. 1978 - 2006				
Rep Average Effect	0.101***	0.116**	0.100**	0.081*
	(0.029)	(0.045)	(0.040)	(0.045)
Dem Average Effect	-0.024	-0.008	0.002	-0.001
	(0.035)	(0.027)	(0.030)	(0.029)
Number of Observations	635	585	585	635
R2	0.758	0.784	0.784	0.756
CKK Legal Control	X			
HF Plan Type		X		
HF Plan Type Include Courts			X	
HF Government Type				X

Note: Each column shows a different estimate for the average effect of a political party having legal control of the redistricting process after redistricting has occurred on the fraction of a state's congressional representatives that are Republican. All specifications include state-decade and year fixed effects. The estimating sample, 1978-2006, is based on Holden-Friedman (2009). Column 1 is the baseline average estimate shown in the main table. Column 2 replaces our legal control treatment with Holden-Friedman's definition of which political party had control of drawing a redistricting map. Column 3 additionally allows for redistricting plans that were imposed by the courts to be defined as political according to data provided by Holden. Column 4 replaces our legal control treatment with a classification of the type of government from data provided by Holden. Standard errors clustered by state-decade are in parentheses.

Table A.3—: Alternative Treatment Definitions

	(1)	(2)	(3)	(4)	(5)
Panel A. 1968 - 2016					
Rep Average Effect	0.035	0.035	0.031	0.034	0.002
	(0.033)	(0.033)	(0.033)	(0.038)	(0.037)
Dem Average Effect	-0.002	-0.003	-0.003	-0.006	-0.012
	(0.030)	(0.030)	(0.030)	(0.033)	(0.030)
Number of Observations	1060	1060	1060	905	1060
R2	0.778	0.778	0.778	0.762	0.778
Panel B. 1968 - 1996					
Rep Average Effect	-0.031	-0.031	-0.047	-0.055	-0.028
	(0.059)	(0.059)	(0.061)	(0.063)	(0.060)
Dem Average Effect	-0.010	-0.011	-0.012	-0.021	-0.016
	(0.038)	(0.038)	(0.038)	(0.041)	(0.041)
Number of Observations	640	640	640	570	640
R2	0.690	0.690	0.691	0.682	0.690
Panel C. 1998 - 2016					
Rep Average Effect	0.083**	0.083**	0.083**	0.104**	0.024
	(0.036)	(0.036)	(0.036)	(0.045)	(0.046)
Dem Average Effect	0.005	0.005	0.005	0.020	-0.012
	(0.049)	(0.049)	(0.049)	(0.054)	(0.036)
Number of Observations	420	420	420	335	420
R2	0.871	0.871	0.871	0.852	0.869
Legal Control	X				
Partisan Commissions Treated		X			
Advisory Commissions Control			X		
Exclude Commissions				X	
Legislature Supermajorities					X

Note:. Each column in this table represents coefficients from a single regression analysis. Panels A, B, and C use data from different periods: 1968-2016, 1968-1996, and 1998-2016, respectively. The treatment definition varies across columns. Column 1 uses the baseline definition of legal control. Column 2 considers non-advisory commissions where one party has a trifecta as a form of control. Column 3 redefines advisory commissions as never giving legal control. Column 4 omits states using redistricting commissions in their estimations. Column 5 assigns states with legislative veto-authority as treated. All specifications include state-decade and year fixed effects. Standard errors, clustered by state-decade, are shown in parentheses. The coefficient change in Column 5 is attributable to a significant Republican shift in New Hampshire in 2010, transforming a Democratic legislature (55D-45R) to a predominantly Republican one (25D-75R), this wave affected congressional representation as well. This shift was reversed during the 2010 decade. Excluding this state-decade from the analysis yields an average estimate for Republican control of .060** (.0285).

Table A.4—: State Government Control in Non-Redistricting Years

	(1)	(2)	(3)	(4)	(5)	(6)
Panel B. 1968 - 2016						
Rep Average Effect	0.035	0.009	0.002	0.058*	0.040	0.012
	(0.033)	(0.037)	(0.040)	(0.035)	(0.040)	(0.032)
Dem Average Effect	-0.002	-0.011	-0.018	-0.012	-0.018	0.005
	(0.030)	(0.028)	(0.029)	(0.031)	(0.031)	(0.031)
Panel B. 1968 - 1996						
Rep Average Effect	-0.031	0.017	-0.035	0.079	0.098	0.014
	(0.059)	(0.067)	(0.072)	(0.065)	(0.073)	(0.062)
Dem Average Effect	-0.010	-0.002	-0.022	-0.016	0.008	0.010
	(0.038)	(0.036)	(0.037)	(0.042)	(0.046)	(0.040)
Panel C. 1998 - 2016						
Rep Average Effect	0.083**	-0.000	0.027	0.046	-0.016	0.007
	(0.036)	(0.030)	(0.050)	(0.041)	(0.043)	(0.030)
Dem Average Effect	0.005	-0.027	-0.010	0.002	-0.072**	-0.006
	(0.049)	(0.046)	(0.044)	(0.030)	(0.029)	(0.048)
Year of Control Basis	0	8	2	4	6	Random

Note: Each column presents coefficients from a single regression analysis. Panels A, B, and C utilize data from 1968-2016, 1968-1996, and 1998-2016, respectively. The state government elections used for determining treatment status differs across columns. Column 1 uses our baseline definition of legal control, considering state governments from elections ending in 0. Column 2 defines control based on unified state governments from elections ending in 8. Column 3 defines control with unified state governments from elections ending in 2. Column 4 defines control using unified state governments from elections ending in 4. Column 5 defines control according to unified state governments from elections ending in 6. Column 6 defines legal control based on a random selection of state governments within the decade, excluding those from elections ending in 0. All models include state-decade and year fixed effects. Standard errors, clustered by state-decade, are noted in parentheses.

Table A.5—: Presidential Vote Share Efficiency Gap and Competitiveness

	(1)	(2)	(3)	(4)	(5)
Panel A. 1968 - 2016					
Rep Average Effect	-0.023	0.004	-0.032	0.046***	-0.009
	(0.033)	(0.007)	(0.058)	(0.011)	(0.006)
Dem Average Effect	-0.016	0.017**	-0.115**	0.011	0.024***
	(0.035)	(0.007)	(0.057)	(0.011)	(0.006)
Mean Control	-0.054	0.604	0.550	0.613	0.592
Number of Observations	424	424	424	276	368
R2	0.756	0.853	0.761	0.855	0.913
Panel B. 1968 - 1996					
Rep Average Effect	0.083	-0.011	0.030	0.008	-0.016
	(0.053)	(0.014)	(0.122)	(0.027)	(0.010)
Dem Average Effect	-0.024	0.017*	-0.100	0.026*	0.023***
	(0.046)	(0.009)	(0.071)	(0.015)	(0.008)
Mean Control	-0.097	0.605	0.550	0.610	0.600
Number of Observations	256	256	256	132	222
R2	0.668	0.815	0.706	0.796	0.890
Panel C. 1998 - 2016					
Rep Average Effect	-0.094***	0.015***	-0.079*	0.054***	-0.003
	(0.034)	(0.005)	(0.042)	(0.011)	(0.006)
Dem Average Effect	0.032	0.012	-0.145	-0.021	0.023***
	(0.037)	(0.008)	(0.090)	(0.013)	(0.007)
Mean Control	0.008	0.603	0.550	0.615	0.580
Number of Observations	168	168	168	144	146
R2	0.899	0.941	0.873	0.924	0.951
E-gap	X				
Winning TWVS		X			
Prob(Win TWVS < .6)			X		
Winning Dem TWVS				X	
Winning Rep TWVS					X

Note: Each column presents coefficients from a single regression analysis, with Panels A, B, and C analyzing data from 1968-2016, 1968-1996, and 1998-2016, respectively. All specifications use a balanced panel and are based on presidential elections with a single pre and post-period. The outcome of interest varies by column. Column 1 shows results for the efficiency gap, defined by summing wasted votes in presidential elections at the district level; positive values indicate a higher number of wasted Republican votes compared to Democratic ones. Column 2 shows results for the average winning presidential twoway vote share (TWVS) in a district. Column 3 shows results using the share of districts with a winning twoway voteshare of less than 60

Table A.6—: Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. 1968 - 2016						
Rep Average Effect	0.035	0.011	0.072*	0.037	0.034	0.066**
	(0.031)	(0.046)	(0.036)	(0.031)	(0.031)	(0.032)
Dem Average Effect	-0.002	-0.013	0.021	-0.005	-0.001	-0.019
	(0.026)	(0.039)	(0.026)	(0.024)	(0.026)	(0.044)
Number of Observations	1060	715	345	1060	1060	1060
Panel B. 1968 - 1996						
Rep Average Effect	-0.031	-0.043	0.011	-0.029	-0.020	0.032
	(0.044)	(0.067)	(0.026)	(0.044)	(0.044)	(0.052)
Dem Average Effect	-0.010	-0.040	0.055*	-0.010	-0.010	-0.041
	(0.034)	(0.050)	(0.031)	(0.034)	(0.034)	(0.049)
Number of Observations	640	425	215	640	640	640
Panel C. 1998 - 2016						
Rep Average Effect	0.083**	0.064	0.086**	0.084**	0.079**	0.088**
	(0.038)	(0.055)	(0.038)	(0.037)	(0.037)	(0.036)
Dem Average Effect	0.005	0.050	-0.111**	-0.022	0.005	0.026
	(0.055)	(0.075)	(0.042)	(0.048)	(0.054)	(0.068)
Number of Observations	420	290	130	420	420	420
Baseline	X					
2-9 Rep States Only		X				
10+ Rep States Only			X			
Republican Weights				X		
Democratic Weights					X	
Change in Legal Control						X

Note: Each column shows a different estimate for the average effect of a political party having legal control of the redistricting process after redistricting has occured on the fraction of a state's congressional representatives that are Republican. Panel A presents coefficients over the full sample from 1968-2016. Panel B presents coefficients over the full sample from 1968-1996. Panel C presents coefficients over the full sample from 1998-2016. All specifications include state-decade and year fixed effects and unless otherwise stated treatment is defined as unilateral legal control of a political party over redistricting. Column 1 is the baseline average estimate shown in main table. Column 2 restricts the sample to include states with between 2 and 9 congrssional representatives. Column 3 restricts the sample to include states with 10 or more congrssional representatives. Column 4 applies Republican weights to estimates according to the fraction of Republican treatments in each panel's sample which come from small (2-9 representative) and large (10 or more representatives) states. Column 5 applies Democratic weights to estimates according to the fraction of Democratic treatments in each panel's sample which come from small (2-9 representative) and large (10 or more representatives) states. Standard errors clustered by state-decade are in parentheses.

Table A.7—: District Based Outcomes

	(1)	(2)	(3)
Panel A. 1968 - 2016			
Rep Average Effect	0.035	0.048**	0.045*
	(0.032)	(0.022)	(0.024)
Dem Average Effect	-0.002	0.014	0.015
	(0.030)	(0.021)	(0.022)
Number of Observations	1060	1060	10580
Panel B. 1968 - 1996			
Rep Average Effect	-0.031	-0.005	0.014
	(0.059)	(0.029)	(0.034)
Dem Average Effect	-0.010	0.033	0.040
	(0.038)	(0.025)	(0.027)
Number of Observations	640	640	6330
Panel C. 1998 - 2016			
Rep Average Effect	0.083**	0.073**	0.055*
	(0.035)	(0.028)	(0.033)
Dem Average Effect	0.005	-0.041	-0.051
	(0.049)	(0.035)	(0.036)
Number of Observations	420	420	4250
State Averages	X		
Weighted		X	
District Level			X

Note: Each column presents a different estimate of the average effect of a political party's legal control over the redistricting process post-redistricting. Columns 1 and 2 use the fraction of a state's congressional representatives who are Republican. Column 2 applies analytic weights based on the number of representatives in each state. Column 3's outcome is a binary variable, equal to 1 if a Republican candidate won a district. Panel A uses data from 1968-2016, Panel B from 1968-1996, and Panel C from 1998-2016. All specifications include state-decade and year fixed effects. Standard errors, clustered by state-decade, are in parentheses.

Table A.8—: Transitions: Prior Decade

	1970	1980	1990	2000	2010
Republican Treatments	7	4	2	7	15
Prior Decade:					
Republican	3	2	0	1	6
Democrat	1	0	0	2	3
Divided	3	2	2	4	6
Democratic Treatments	14	15	15	7	5
Prior Decade:					
Republican	0	1	0	0	0
Democrat	13	10	10	5	2
Divided	1	4	5	2	3

Note: Treatments are broken down by decade across columns and into Republican legal control treatments on top and Democratic legal treatments on the bottom. Subrows display the number of times treatment in a decade was preceded by Republican, Democrat and divided control in the prior decade.

Table A.9—: Average Effects: By Transition Type

	(1)	(2)	(3)
All Treatments Included			
Pre-Period Republican Effect	0.001	-0.014	0.013
	(0.033)	(0.058)	(0.041)
Avg Republican Control Effect	0.035	-0.031	0.083**
	(0.032)	(0.059)	(0.035)
Pre-Period Democratic Effect	-0.052*	-0.055*	-0.045
	(0.027)	(0.033)	(0.049)
Avg Democratic Control Effect	-0.002	-0.010	0.005
	(0.030)	(0.038)	(0.049)
Continuity in Partisan Alignment			
Pre-Period Republican Effect	0.006	-0.058	0.057
	(0.048)	(0.045)	(0.072)
Avg Republican Control Effect	-0.026	-0.133	0.058
	(0.058)	(0.097)	(0.055)
Pre-Period Democratic Effect	-0.064**	-0.067*	-0.067
	(0.031)	(0.036)	(0.053)
Avg Democratic Control Effect	0.001	0.000	-0.014
	(0.035)	(0.042)	(0.061)
Change in Partisan Alignment			
Pre-Period Republican Effect	-0.002	0.004	-0.006
	(0.040)	(0.086)	(0.037)
Avg Republican Control Effect	0.067**	0.026	0.096**
	(0.031)	(0.056)	(0.037)
Pre-Period Democratic Effect	-0.019	-0.019	-0.019
	(0.049)	(0.062)	(0.081)
Avg Democratic Control Effect	-0.018	-0.044	0.034
	(0.044)	(0.056)	(0.069)
Sample	l968-2016	l968-1996	l998-2016

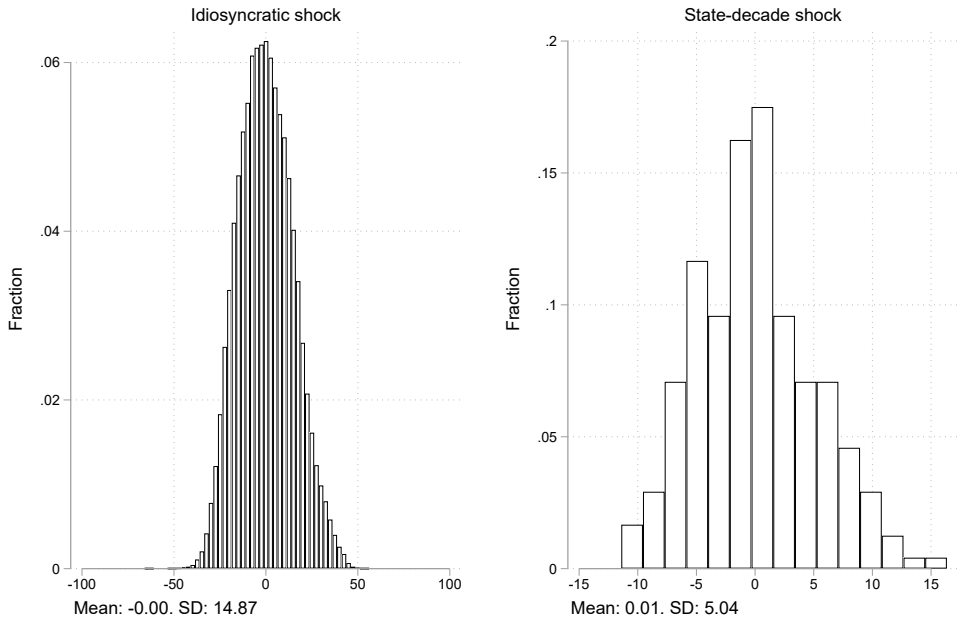
Note: Within a panel, each column presents coefficients from a single regression with each observation representing a state-year. The first column employs data spanning 1968-2016. The second column uses data from 1968 to 1996. The third column utilizes data from 1998 to 2016. The treatment variable is the unilateral legal control of a political party over redistricting for election years ending in 2. Row estimates display the impact of a political party having legal control over the redistricting process during years ending in 2 on a state's proportion of congressional representatives who are Republican in election years ending in 2, 4, 6, and 8. Both the average effect and the pre-period effect are reported. The first panel shows estimates on the full sample, replicating results presented in Table 3. The second panel drops state-decades with legal control where the legal control status is different from the prior decade. Treated state-decades in this panel are thus ones where the same party had legal control in the prior decade. The third panel drop state-decades with legal control where the legal control status is the same as in the prior decade. Treated state-decades in this panel are thus ones where either a different party was in power or no party was in power in the prior decade.

Table A.10—: District Changes from the Prior Election Period (Weighted)

	(1)	(2)	(3)	(4)
Republican Control: Effect on District Change				
Control x Election Ending in 2	0.059***	0.001	0.108***	0.084**
	(0.021)	(0.023)	(0.029)	(0.036)
Control x Election Ending in 4	0.009	-0.071**	0.051	0.070
	(0.038)	(0.029)	(0.047)	(0.050)
Control x Election Ending in 6	0.002	-0.007	0.016	0.029*
	(0.011)	(0.012)	(0.016)	(0.016)
Control x Election Ending in 8	-0.004	0.013	0.000	-0.000
	(0.011)	(0.008)	(0.014)	(0.015)
Democrat Control: Effect on District Change				
Control x Election Ending in 2	0.022	-0.012	0.103**	0.061*
	(0.021)	(0.018)	(0.044)	(0.036)
Control x Election Ending in 4	-0.017	-0.023	-0.004	0.007
	(0.015)	(0.020)	(0.010)	(0.011)
Control x Election Ending in 6	0.019	0.027**	-0.011	-0.007
	(0.011)	(0.013)	(0.018)	(0.026)
Control x Election Ending in 8	-0.010	-0.009	-0.010	-0.015
	(0.009)	(0.011)	(0.015)	(0.017)
Sample	l968-2016	l968-1996	l998-2016	l998-2016
Outcome Basis	Land	Land	Land	Pop
Number of Observations	1060	640	420	420
R2	0.741	0.731	0.787	0.798

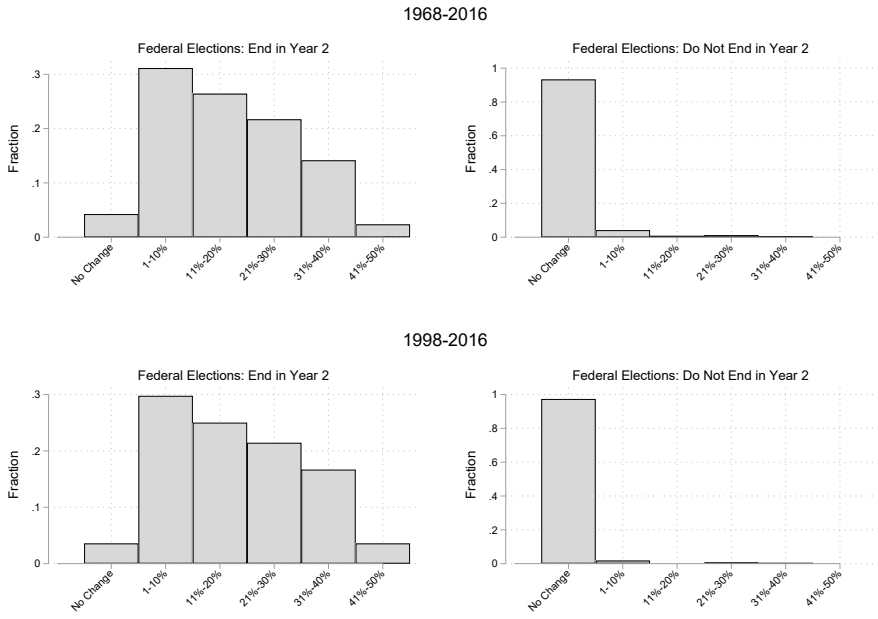
Note: Each column provides estimates for the impact of political control over redistricting in various elections on district changes from the previous election. Analytic weights are applied based on the number of representatives in each state. The term *Control x Election Ending in t* calculates the influence of a political party's control over redistricting for election years ending in t on district alterations between the election years ending in t and t-2. In columns 1 to 3, the dependent variable is the proportion of land within a state that has shifted districts since the previous election. Column 4 utilizes the percentage of a state's population that changes districts as the dependent variable. The estimates for the effects of Republican and Democratic control are calculated simultaneously and depend on state-decade and year fixed effects. Standard errors, clustered by state-decade, are noted in parentheses. Observations are weighted by number of representatives in a state.

Figure A.1. : Distributions of Idiosyncratic and State-Decade Shocks



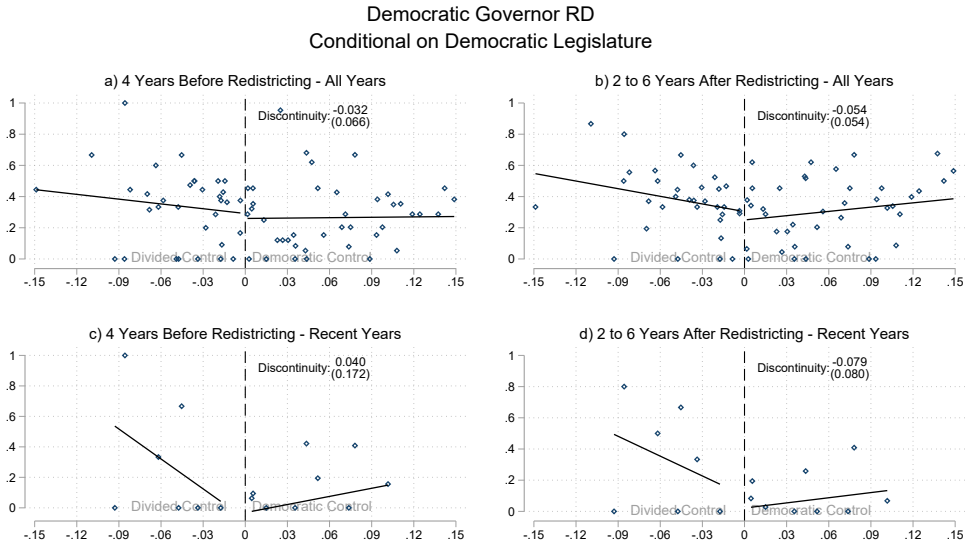
Note: Figure plots the distribution of idiosyncratic and state-decade shocks as estimated in Equation 4.

Figure A.2. : Redistricting In Non-Redistricting Years



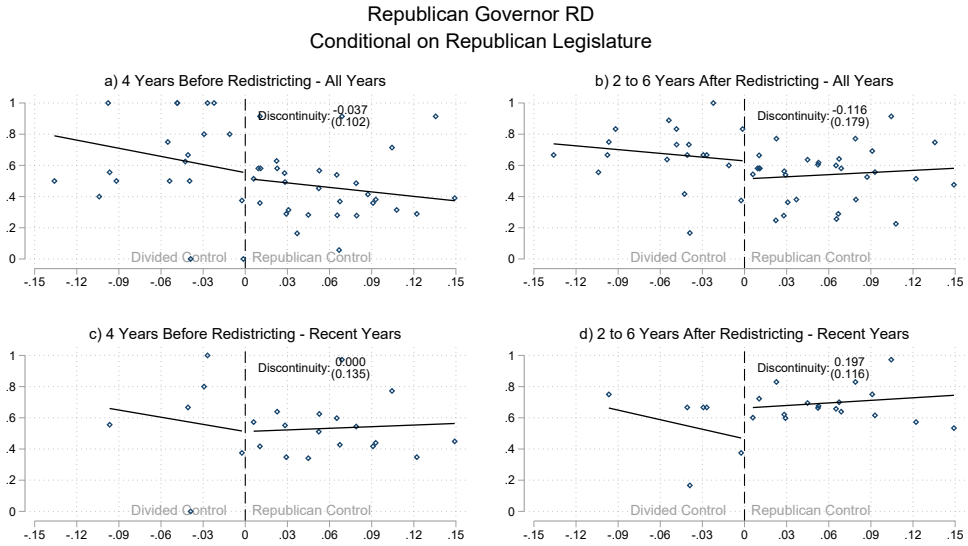
Note: This figure shows the distribution of percentages of land whose district changed in a state since the prior federal election. The top panel shows changes over the entire sample period. The bottom panel restricts the sample to the 1998-2016 time period. Graphs on the left show the distribution of changes for federal election years ending in 2; graphs on the right show the distribution of changes for federal elections ending in all other years.

Figure A.3. : Democratic Gubernatorial RD Conditional Upon Unified Democratic Legislature



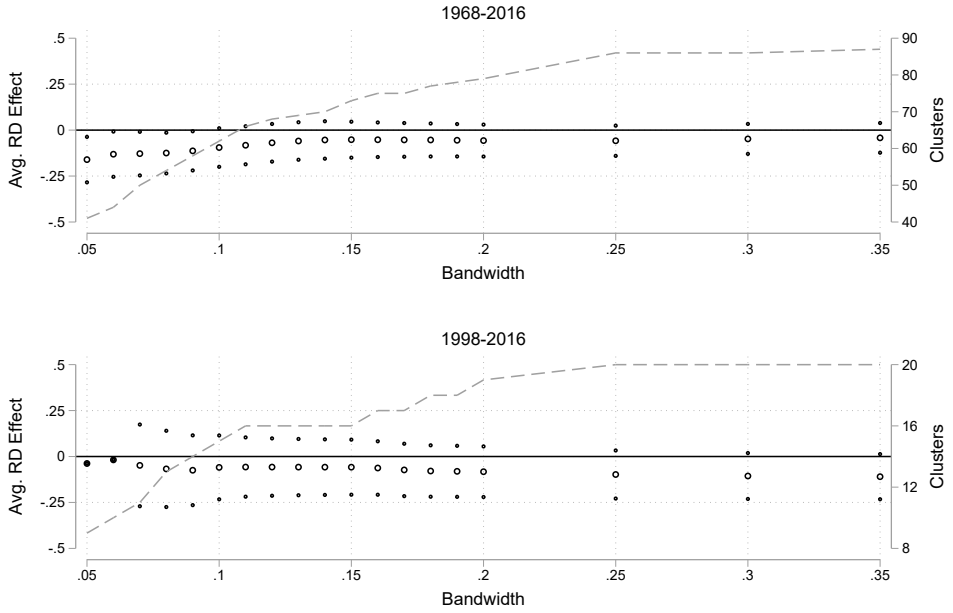
Note: Figure shows estimates from the Democratic gubernatorial regression discontinuity, using states with unified Democratic legislatures. Panels A and C present regression discontinuity estimates based on election outcomes four years prior to redistricting (years ending in 8), representing a pre-period effect. Panels B and D display average estimates based on election outcomes two to six years post-redistricting (years ending in 2, 4, and 6). Estimates in Panels A and B are derived from data spanning 1968-2016, while those in Panels C and D are based on data from 1998-2016. Each scatter-point represents a single state-decade.

Figure A.4. : Republican Gubernatorial RD Conditional Upon Unified Republican Legislature



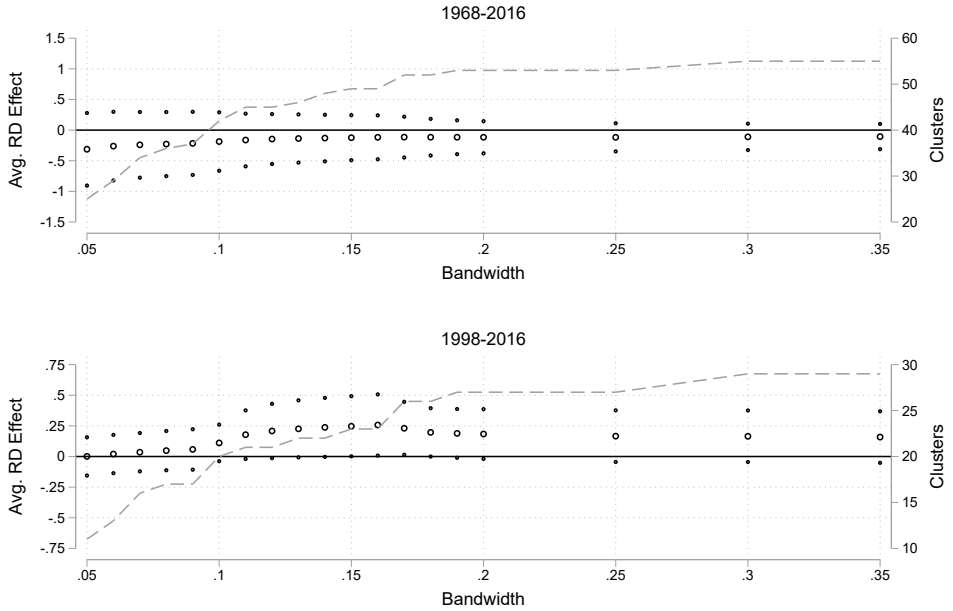
Note: Figure shows estimates from the Republican gubernatorial regression discontinuity, using states with unified Republican legislatures. Panels A and C present regression discontinuity estimates based on election outcomes four years prior to redistricting (years ending in 8), representing a pre-period effect. Panels B and D display average estimates based on election outcomes two to six years post-redistricting (years ending in 2, 4, and 6). Estimates in Panels A and B are derived from data spanning 1968-2016, while those in Panels C and D are based on data from 1998-2016. Each scatter-point represents a single state-decade.

Figure A.5. : Gubernatorial RD Estimates: Bandwidth Robustness - Democrats



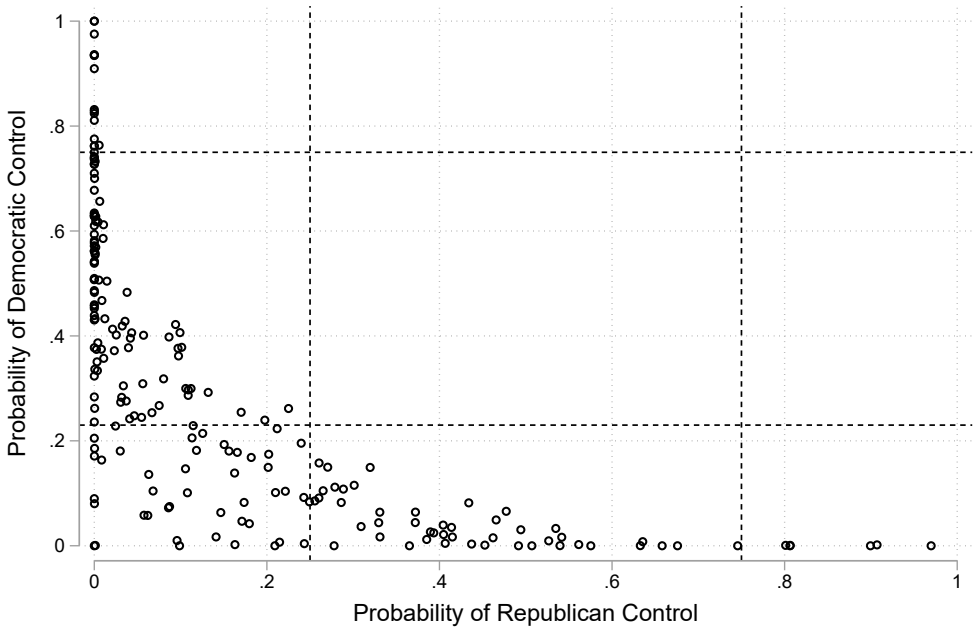
Note: This figure displays the average Democratic gubernatorial regression discontinuity estimates, using state's with a unified Democratic legislature under varying bandwidths. These bandwidths increment by 1 percentage point, ranging from 5% to 20%, and also include 25%, 30%, and 35%. Additionally, a dashed line depicts the quantity of state-decades utilized for each specified bandwidth.

Figure A.6. : Gubernatorial RD Estimates: Bandwidth Robustness - Republicans



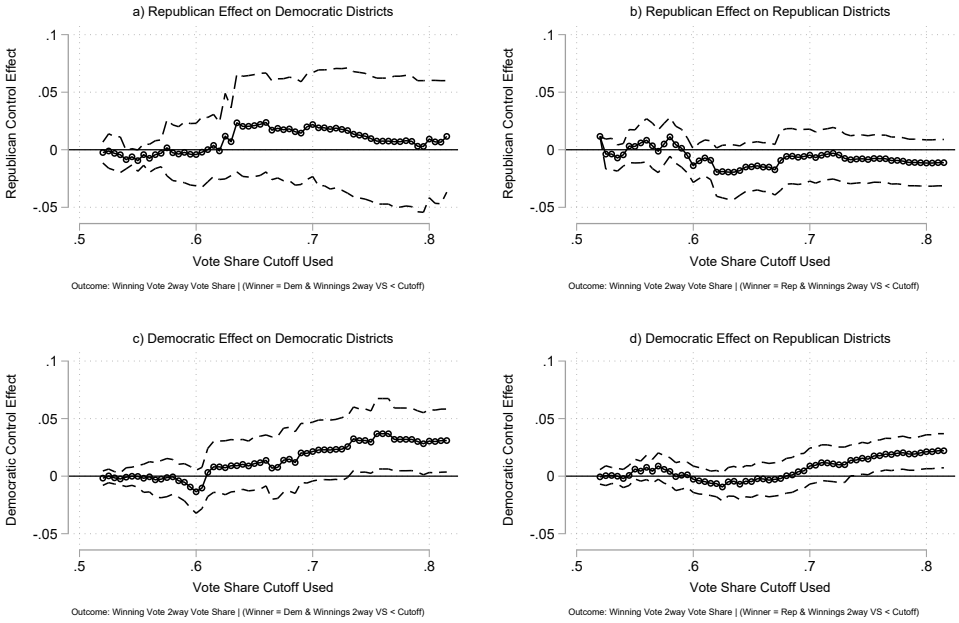
Note: This figure displays the average Republican gubernatorial regression discontinuity estimates, using state's with a unified Republican legislature under varying bandwidths. These bandwidths increment by 1 percentage point, ranging from 5% to 20%, and also include 25%, 30%, and 35%. Additionally, a dashed line depicts the quantity of state-decades utilized for each specified bandwidth.

Figure A.7. : Probability Scatter of Democratic vs. Republican Legal Control by State-Decade



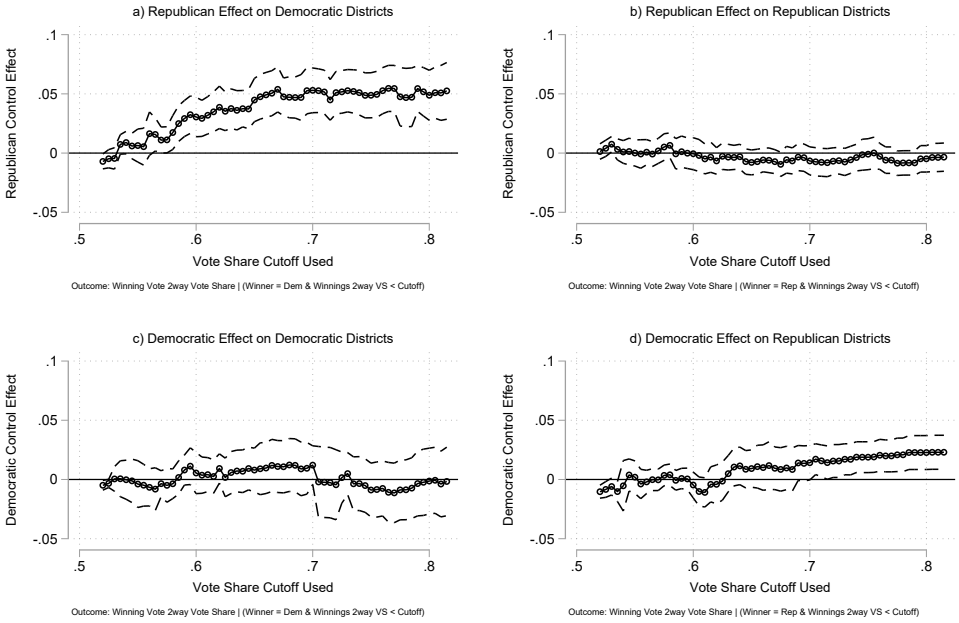
Note: Figure plots the computed probability that a state's redistricting government would be Democrat (on the y-axis) or Republican (on the x-axis). Reference lines are included at 25% and 75%.

Figure A.8. : Effects on Average District Winning Vote Share: 1968-1996



Note: Figure shows the average effects of political control on district average winning vote share from 1968 to 1996 following redistricting by Republicans and Democrats. The district average winning vote share is calculated using the two-way vote share in congressional districts for presidential candidates, using the presidential election before and after redistricting, creating a balanced panel. The graph presents regression coefficients reflecting the post-period impact of redistricting for both Republicans and Democrats. Panel A depicts the influence of Republican-controlled redistricting in districts with at least 50% Republican two-way presidential vote share. Panel B highlights the impact in districts with at least 50% Democratic vote share under the same Republican control. Conversely, Panels C and D showcase the effects of Democratic-controlled redistricting on districts with a minimum of 50% Republican and Democratic vote shares, respectively. The panels further distinguish effects based on varying levels of competitiveness: regressions closer to 0.5 focus on the most competitive districts, while those closer to 0.8 encompass the majority of districts, the vote share cutoff increases by .005 for each regression. Dashed lines represent the 95% confidence interval, with standard errors clustered at the state-decade level.

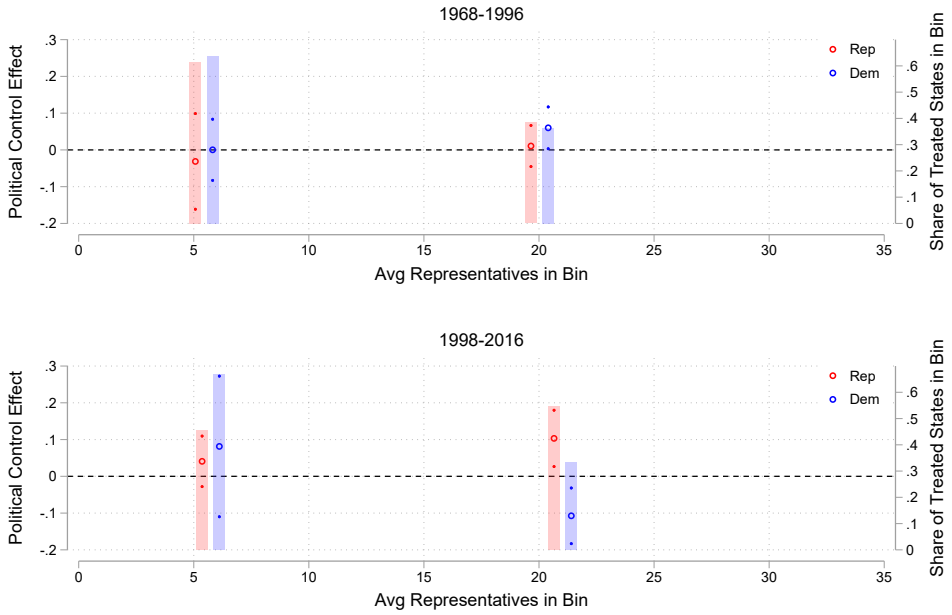
Figure A.9. : Effects on Average District Winning Vote Share: 1998-2016



Note: Figure shows the average effects of political control on district average winning vote share from 1998 to 2016 following redistricting by Republicans and Democrats. The district average winning vote share is calculated using the two-way vote share in congressional districts for presidential candidates, using the presidential election before and after redistricting, creating a balanced panel. The graph presents regression coefficients reflecting the post-period impact of redistricting for both Republicans and Democrats. Panel A depicts the influence of Republican-controlled redistricting in districts with at least 50% Republican two-way presidential vote share. Panel B highlights the impact in districts with at least 50% Democratic vote share under the same Republican control. Conversely, Panels C and D showcase the effects of Democratic-controlled redistricting on districts with a minimum of 50% Republican and Democratic vote shares, respectively. The panels further distinguish effects based on varying levels of competitiveness: regressions closer to 0.5 focus on the most competitive districts, while those closer to 0.8 encompass the majority of districts, the vote share cutoff increases by .005 for each regression. Dashed lines represent the 95% confidence interval, with standard errors clustered at the state-decade level.

Figure A.10. : Effects of Legal Control on Republican Seat Shares:

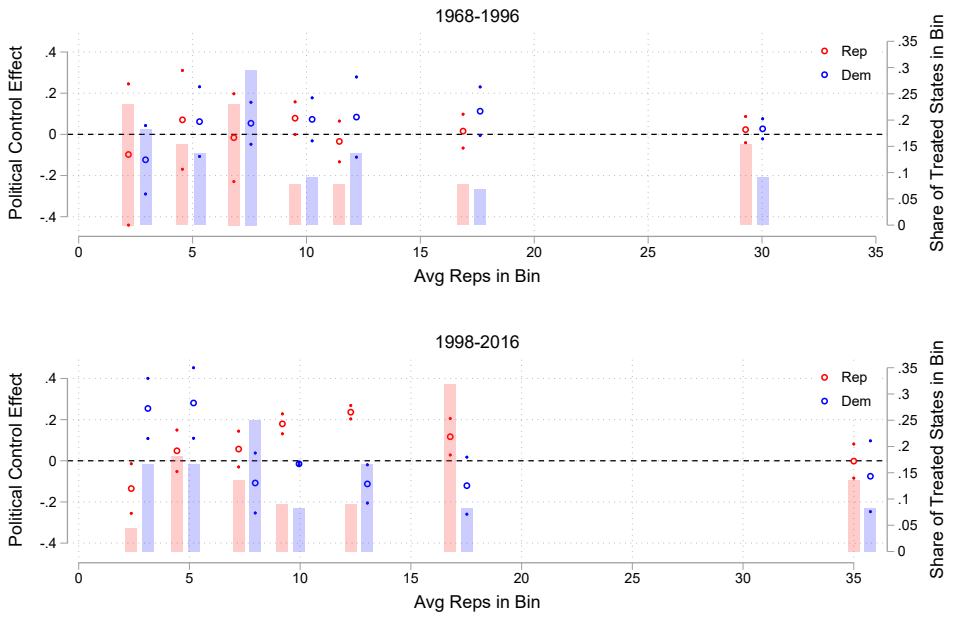
By Delegation Size (Two Groups)



Note: Figure shows the average effect of political control post-redistricting for Republicans and Democrats, categorized by the number of representatives in a state. It displays two estimates for two groups of states: those with 2-9 representatives and those with 10 or more representatives. The x-axis corresponds to the average number of representatives within a bin which includes Republican, Democratic and control state-decades. Scatter-points represent the average effect (left-axis), while bars indicate the share of political control treatments for a political party (right-axis). Estimates are provided for two time periods: 1968-1996 and 1998-2016.

Figure A.11. : Effects of Legal Control on Republican Seat Shares:

By Delegation Size (Seven Groups)



Note: Figure shows the average effect of political control after redistricting for Republicans and Democrats, segregated based on the number of representatives in each state. It offers two estimates across seven groups of states, categorized as those with 2-3, 4-5, 6-8, 9-10, 11-13, 14-19, and 20 or more representatives. The x-axis corresponds to the average number of representatives within a bin which includes Republican, Democratic and control state-decades. Scatter-points denote the average effect (left-axis), and bars reflect the proportion of political control treatments for each political party (right-axis). The estimates cover two distinct periods: 1968-1996 and 1998-2016.

B. Randomization Inference Appendix

Table B.1—: Randomization Inference P-values

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. 1968 - 2016						
Rep Average Effect	0.035	0.023	-0.116	0.035	0.023	-0.116
	[0.160]	[0.280]	[0.869]	[0.149]	[0.143]	[0.784]
Dem Average Effect	-0.002	-0.011	-0.054*	-0.002	-0.011	-0.054*
	[0.479]	[0.374]	[0.087]	[0.474]	[0.243]	[0.089]
Panel B. 1968 - 1996						
Rep Average Effect	-0.031	-0.058	-0.401	-0.031	-0.058	-0.401
	[0.716]	[0.874]	[0.996]	[0.679]	[0.843]	[1.000]
Dem Average Effect	-0.010	-0.030	-0.042	-0.010	-0.030	-0.042
	[0.425]	[0.263]	[0.199]	[0.405]	[0.248]	[0.198]
Panel C. 1998 - 2016						
Rep Average Effect	0.083**	0.109**	0.197	0.083***	0.109**	0.197
	[0.025]	[0.028]	[0.122]	[0.007]	[0.013]	[0.142]
Dem Average Effect	0.005	-0.001	-0.079*	0.005	-0.001	-0.079*
	[0.528]	[0.469]	[0.089]	[0.508]	[0.462]	[0.075]
Specifcation	TWFE	SIMUL	RD	TWFE	SIMUL	RD
RI Type	1	1	1	2	2	2
Replications	10000	10000	10000	10000	10000	10000

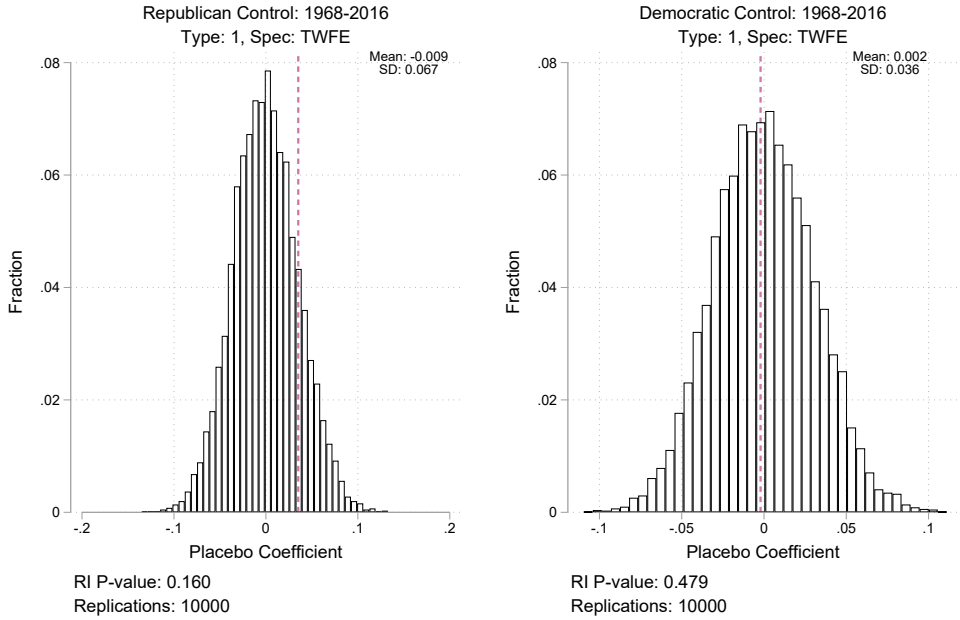
Note: Each column shows a different estimate for the average effect of a political party having legal control of the redistricting process after redistricting has occured on the fraction of a state's congressional representatives that are Republican. Monte carlo based randomization inference p-values are shown in brackets for 3 different time periods, 3 different regression specifications, and 2 different ways of randomizing treatment. Panel A presents coefficients over the full sample from 1968-2016. Panel B presents coefficients over the full sample from 1968-1996. Panel C presents coefficients over the full sample from 1998-2016. Columns 1 and 4 use specification TWFE, which corresponds to our twoway fixed effects estimates. Columns 2 and 5 use specification SIMUL, which corresponds to our simulated matching estimates. Columns 3 and 6 use specification RD, which corresponds to our conditional Gubernatorial regression discontinuity estimates. Columns 1-3 use randomization type 1, which randomly re-assigns the Republican and Democratic treatment, holding fixed the aggregate number of treatments. Columns 4-6 use randomization type 2, which randomly re-assigns the Republican and Democratic treatment, holding fixed the aggregate number of treatments within each panels time period. In all cases, randomization inference is based on 10,000 replications.

Table B.2—: Randomization Inference P-values

	(1)	(2)	(3)
Panel A. 1968 - 2016			
Rep Average Effect	0.035	0.035*	0.044**
	[0.189]	[0.057]	[0.042]
Dem Average Effect	-0.002	-0.002	0.008
	[0.491]	[0.456]	[0.789]
Panel B. 1968 - 1996			
Rep Average Effect	-0.031	-0.031	-0.010
	[0.684]	[0.587]	[0.376]
Dem Average Effect	-0.010	-0.010	0.004
	[0.409]	[0.496]	[0.767]
Panel C. 1998 - 2016			
Rep Average Effect	0.083**	0.083**	0.082**
	[0.016]	[0.041]	[0.015]
Dem Average Effect	0.005	0.005	0.006
	[0.526]	[0.323]	[0.512]
Specifcation	TWFE	TWFE	TWFE
RI Type	3	4	5
Replications	1000	1000	1000

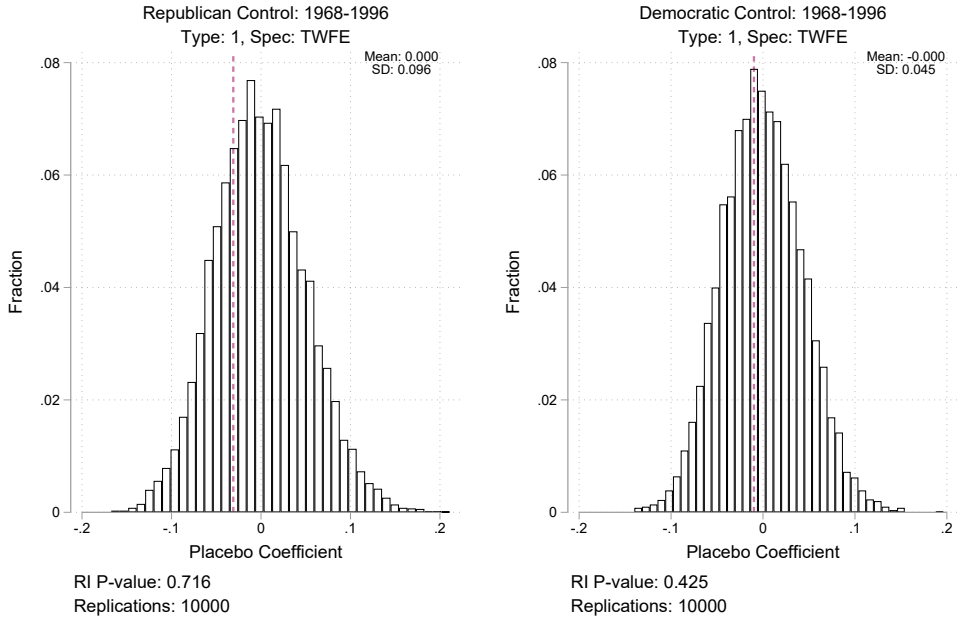
Note: Each cell shows a different estimate for the average effect of a political party having legal control of the redistricting process after redistricting has occured on the fraction of a state's congressional representatives that are Republican. Monte carlo based randomization inference p-values are shown in brackets for 3 different time periods and 3 different ways of randomizing treatment. All columns use the specification TWFE, which corresponds to our twoway fixed effects estimates. Panel A presents coefficients over the full sample from 1968-2016. Panel B presents coefficients over the full sample from 1968-1996. Panel C presents coefficients over the full sample from 1998-2016. Column 1 uses randomization type 3, which randomly reassigns Republican and Democratic treatment, holding fixed the number of treatments per decade. Column 2 uses randomization type 4, which randomly re-assigns Republican and Democratic treatment, holding fixed the number and types of treatments each state receives. Column 3 uses randomization type 5, which randomly re-assigns full state-decade Republican and Democratic treatment assignments to a different state. In column 3 the sample is restricted to only include states which are eligible for redistricting in all 5 decades. In all cases, randomization inference is based on 1,000 replications.

Figure B.1. : Randomization Inference: Full Sample, Full Randomization



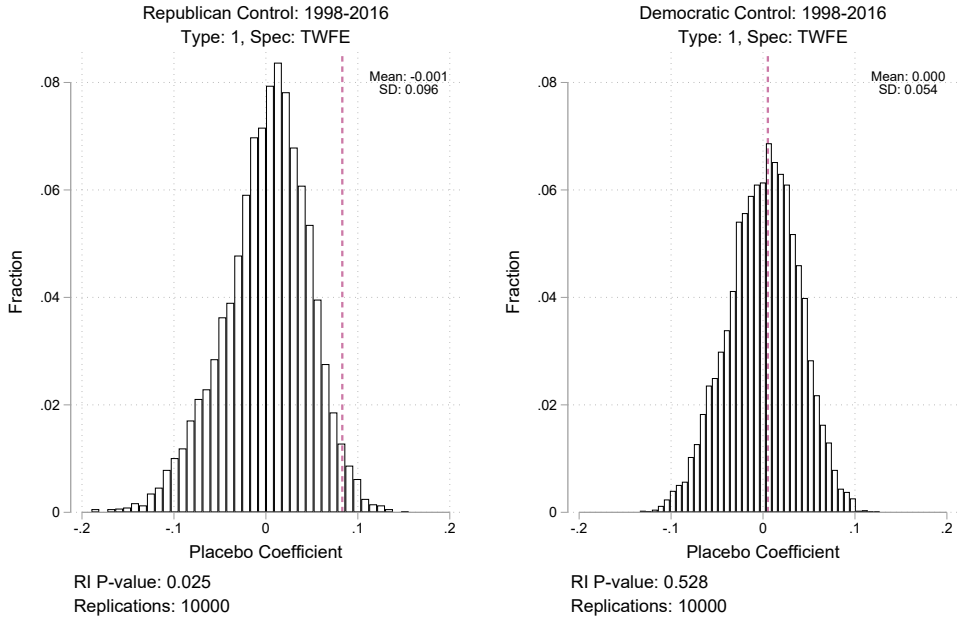
Note: Figure displays the distribution of coefficients from the first randomization inference procedure using 10,000 replications. In this specification, Republican and Democratic treatments are randomly reassigned while maintaining the fixed number of each treatment across the sample from 1968-2016. Estimates correspond to the average effect of Republican and Democratic control as per Equation 1. The p-value is determined by calculating the percentage of placebo coefficients that exceed the estimates from Table V. The mean and standard deviation of the placebo coefficients are reported in the top right corner of each panel.

Figure B.2. : Randomization Inference: Early Sample, Full Randomization



Note: Figure displays the distribution of coefficients from the first randomization inference procedure using 10,000 replications. In this specification, Republican and Democratic treatments are randomly reassigned while maintaining the fixed number of each treatment across the sample from 1968-1996. Estimates correspond to the average effect of Republican and Democratic control as per Equation 1. The p-value is determined by calculating the percentage of placebo coefficients that exceed the estimates from Table V. The mean and standard deviation of the placebo coefficients are reported in the top right corner of each panel.

Figure B.3. : Randomization Inference: Recent Sample, Full Randomization



Note: Figure displays the distribution of coefficients from the first randomization inference procedure using 10,000 replications. In this specification, Republican and Democratic treatments are randomly reassigned while maintaining the fixed number of each treatment across the sample from 1998-2016. Estimates correspond to the average effect of Republican and Democratic control as per Equation 1. The p-value is determined by calculating the percentage of placebo coefficients that exceed the estimates from Table V. The mean and standard deviation of the placebo coefficients are reported in the top right corner of each panel.

C. Institutional Background Appendix

In the United States, drawing district maps in order to influence elections goes back to the period before the Constitution when the Articles of Confederation were law. Patrick Henry, along with other anti-Federalists, purportedly altered Virginia's 5th Congressional District in an attempt to prevent the strong Federalist, James Madison, from returning to Congress (Labunski, 2006). This highlighted a period where states oscillated between using single-member districting and at large general elections (elections which produce a single party winning the entirety of a state's delegates). It wasn't until the Apportionment Act of 1842 that Congress required states use single-member districts, although states were slow to abide. This led to a period of relative district stability in the early 19th century, where redistricting changes were rare and largely driven by changes in state population leading to an increase or decrease in delegates[24]. During this period, there were no rules governing the size of districts. By the 1960s urban population centers had begun to outpace rural population growth, resulting in district populations that were terribly lopsided. In 1964, the Supreme Court established the one-person, one-vote standard requiring districts to be of equal size. Combined with Article 1, section 2 of the Constitution, this has formed the basis of modern redistricting.

The first Congress had 105 members and an average of approximately 33,000 individuals per representative, the size of Congress grew over time with population growth until it was capped in 1911 at 435 representatives[25]. This cap was reauthorized 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.

For example, in Georgia, the largest districts had 2-3 times the population of

[24]For instance Connecticut's redistricting map in 1912 lasted until 1962 and Louisiana's 1912 map last until 1966

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

the smallest districts[26]. 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 decided 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 have been allowed (*Brown v. Thomson*, 1983) (Ansolabehere and Snyder, 2008).

[26]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.

D. Appendix on the Efficiency Gap

In this appendix, we provide more background on the efficiency gap. Whereas an efficiency gap of zero does not indicate that no votes are wasted, it does indicate that there is no difference across the parties in wasted votes. The wasted votes for a party in a state is summed across districts.

For each district, the wasted votes for a party are the number of votes if the party lost its election and the number of votes above one more than the second highest candidate if the party won the election. We measure the efficiency gap as the difference between Republican and Democrat wasted votes divided by total votes. Thus an efficiency gap of 5% says that the Republicans wasted 5 percent of total votes more than the Democrats. An efficiency gap of -3% says that Republicans wasted 3 percent of total votes less than the Democrats. The efficiency gap in a state can be computed as:

$$Egap_{st} = \frac{\sum_d (WastedRep_{dst} - WastedDem_{dst})}{TotVotes_{st}} \tag{D.1}$$

where $Egap_{st}$ is the efficiency gap in state s at time t, $WastedRep_{dst}$ is the wasted Republican votes in district d in state s at time t, $WastedDem_{dst}$ is the wasted Democrat votes, and $TotVotes_{st}$ is the total votes in state s and time t.

The efficiency gap by itself is not a measure of the impact of gerrymandering. For example, if a party has 100% of the votes in a state, it will have a -50% efficiency gap because it wastes 50% of its votes whereas the opposition doesn't waste any votes. In general, it is impossible merely from computing the efficiency gap to distinguish an inherently unfavorable spatial distribution for a party from the impact of gerrymandering. This is further complicated by candidate selection. States may differ in their efficiency gaps due to differences in candidate quality.

For these reasons, we combine the efficiency gap with identification strategies to estimate the impact of legal control on wasted votes.

E. Appendix on Racial Gerrymandering

In this section, we discuss racial gerrymandering. The law and the courts have disallowed redistricting based upon race except for the purposes of creating greater representation for minorities in accordance with the 1965 Voting Rights Act. We ask four questions: (1.) do parties move minorities more when they have legal control (2.) do parties move minorities to more extreme districts when they have legal control, (3.) are minorities in more extreme districts, and (4.) does legal control result in a different number of minority legislators being elected?

To analyze these questions, we combine the U.S. census which has data on race of individuals at the Census block level with Congressional District shape files. Census blocks are a useful level of aggregation because they almost never cross congressional district lines. Unfortunately, race at the Census block level is only available back to the 1990 Census. Thus, we restrict our analysis to the 1988 to 2016 time period. We also do separately show the recent period from 1998 to 2016. At the state-decade level, we compute the fraction of individuals whose districts change due to redistricting separately by race. We then regress the outcome variable on a dummy variable for Republican control and another for Democratic control. Our dependent variables are first differences (change in location, change in the vote share of their district). Thus, these level estimates implicitly control for locational fixed effects.

$$O_{sXd} = \alpha + \beta_R RepControl_{sXd} + \beta_D DemControl_{sXd} + \epsilon_{sXd} \tag{E.1}$$

In columns 1 and 2, we look at the fraction moved. We find that Democrats do not move either Whites or Blacks more than in states without legal control. This is not surprising given that we did not find that Democrats moved people overall

Table E.1—: District Level Race Based Gerrymandering: All House Elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. 1988 - 2016								
Rep Average Effect	0.078**	0.061*	0.015	0.064***	0.049*	0.016	0.031**	0.018*
	(0.034)	(0.036)	(0.024)	(0.023)	(0.025)	(0.021)	(0.013)	(0.010)
Dem Average Effect	0.031	0.039	0.030	0.024	-0.007	-0.002	-0.033*	-0.027
	(0.038)	(0.042)	(0.025)	(0.016)	(0.029)	(0.021)	(0.018)	(0.020)
Mean Control Outcome	0.173	0.163	0.651	0.643	-0.008	-0.011	-0.006	-0.008
Number of Observations	124	124	124	124	124	124	126	126
R2	0.052	0.028	0.016	0.100	0.036	0.006	0.086	0.051
Panel B. 1998 - 2016								
Rep Average Effect	0.103**	0.086**	0.048**	0.084***	0.035	-0.002	0.013	-0.006
	(0.039)	(0.042)	(0.024)	(0.023)	(0.026)	(0.023)	(0.013)	(0.010)
Dem Average Effect	0.052	0.028	0.036	0.030	-0.006	0.006	-0.019	-0.001
	(0.052)	(0.046)	(0.028)	(0.022)	(0.034)	(0.015)	(0.017)	(0.013)
Mean Control Outcome	0.163	0.148	0.627	0.634	0.007	0.005	0.010	0.014
Number of Observations	83	83	83	83	83	83	84	84
R2	0.106	0.065	0.069	0.183	0.027	0.001	0.034	0.003
Race	W	B	BM	BM	BM	WM	BS	WS
Share Moved	X	X						
Pre winning voteshare			X					
Post winning voteshare				X				
Post - pre voteshare					X	X	X	X

Note: Each column shows a different estimate for the difference in district based outcomes between Republican, Democratic and no political control states. Observations are at the state-decade level. Panel A presents coefficients over the sample from 1988-2016. Panel B presents coefficients over the sample from 1998-2016. In columns 1 and 2, the outcome is based on the percentage of population which changes districts defined using the maximum overlap of pre and post redistricting boundaries. In column 1 the sample population is white (W). In column 2 the sample population is black (B). In column 3 the outcome is the average vote share of the winning candidate, weighted by the black mover (BM) population, prior to redistricting. In column 4 the outcome is the average vote share of the winning candidate, weighted by the black mover (BM), after to redistricting. In columns 5-8 the outcome is the difference in the average vote share of the winning candidate after redistricting less the average vote share of the winning candidate prior to redistricting weighted by the population of black movers (BM), white movers (WM), black stayers (BS) and white stayers (WS). Standard errors clustered by state are in parentheses.

more than in states without legal control. We also find that Republican-controlled states move both Whites and Blacks more than states without legal control. This is also consistent with our prior findings that Republicans shift both more land and people. However, we find that there is a higher probability of a White getting moved than a Black getting moved.

In columns 3 and 4, we show that prior to redistricting, minorities in Democratic-controlled states are in districts which are 3 percent more extreme and 1.5% more extreme for Republicans. However, the coefficients are not statistically distinguishable from zero. In the recent two decades, the coefficients are similar for Democrats but higher for Republicans at 4.8% more extreme. Moreover, the coefficients for Republicans are statistically distinguishable from zero at conventional levels. In the election right after redistricting, coefficients for Democrats do not change significantly or substantively.

In columns 5-8, we look at whether Whites or Blacks are redistricted to more or less extreme districts. We find statistically significant effects in the full sample that Blacks are placed in more extreme districts by Republicans. The estimates are of a similar magnitude but not statistically significant in the recent period. We also find that the districts of Blacks who remain become more extreme on average though this coefficient is not only statistically insignificant in the recent period, it is also smaller. Additionally, our findings amount to a 10%-significant reduction in the extremity of Black districts with Democratic legal control over the full sample. This coefficient is two thirds as large and statistically insignificant in the recent period.

In some cases, redistricting may create districts which are sufficiently non-competitive that the opposition does not field a candidate. When restricting to competitive races, the coefficients for Republican legal control's impact upon Black movers lessens and Black stayers lessens sufficiently as to become statistically indistinguishable from zero. These results are available upon request from

the authors. Thus, creating non-competitive minority districts does seem to be one consequence of Republican legal control. Nonetheless, we do see a decline in competitiveness for both Black movers and stayers in presidential elections following Republican legal control. These results are available in Appendix Table E.2.

Table E.2—: District Level Race Based Gerrymandering: Presidential Elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. 1988 - 2016								
Rep Average Effect	0.078**	0.061*	0.019	0.057***	0.038**	0.031*	0.024*	0.037***
	(0.034)	(0.036)	(0.015)	(0.018)	(0.018)	(0.016)	(0.013)	(0.014)
Dem Average Effect	0.031	0.039	0.009	-0.012	-0.021	-0.016	-0.011	-0.017
	(0.038)	(0.042)	(0.013)	(0.017)	(0.018)	(0.017)	(0.013)	(0.016)
Mean Control Outcome	0.173	0.163	0.595	0.588	-0.007	-0.026	-0.028	-0.034
Number of Observations	124	124	124	124	124	124	126	126
R2	0.052	0.028	0.021	0.082	0.063	0.043	0.032	0.056
Panel B. 1998 - 2016								
Rep Average Effect	0.103**	0.086**	0.019	0.053***	0.034***	0.013	0.011	0.013*
	(0.039)	(0.042)	(0.018)	(0.017)	(0.011)	(0.009)	(0.008)	(0.007)
Dem Average Effect	0.052	0.028	0.017	0.014	-0.003	0.016	-0.001	0.013*
	(0.052)	(0.046)	(0.022)	(0.012)	(0.018)	(0.009)	(0.012)	(0.008)
Mean Control Outcome	0.163	0.148	0.594	0.613	0.019	0.015	0.006	0.012
Number of Observations	83	83	83	83	83	83	84	84
R2	0.106	0.065	0.027	0.142	0.120	0.047	0.030	0.076
Race	W	B	BM	BM	BM	WM	BS	WS
Share Moved	X	X						
Pre winning voteshare			X					
Post winning voteshare				X				
Post - pre voteshare					X	X	X	X

Note: Each column shows a different estimate for the difference in district based outcomes between Republican, Democratic and no political control states. Observations are at the state-decade level. Panel A presents coefficients over the sample from 1988-2016. Panel B presents coefficients over the sample from 1998-2016. In columns 1 and 2, the outcome is based on the percentage of population which changes districts defined using the maximum overlap of pre and post redistricting boundaries. In column 1 the sample population is white (W). In column 2 the sample population is black (B). In column 3 the outcome is the average vote share of the winning candidate, weighted by the black mover (BM) population, prior to redistricting. In column 4 the outcome is the average vote share of the winning candidate, weighted by the black mover (BM), after to redistricting. In columns 5-8 the outcome is the difference in the average vote share of the winning candidate after redistricting less the average vote share of the winning candidate prior to redistricting weighted by the population of black movers (BM), white movers (WM), black stayers (BS) and white stayers (WS). Standard errors clustered by state are in parentheses.

Finally, we look at the consequences for minority representation in Congress. Interestingly, despite the effect of Republican legal control on the extremity of Black districts, we do not see an increase in minority representation from Repub-

lican redistricting. Thus, we do not find evidence that Republicans and minority Democrats work together to create more majority minority districts at the expense of overall Democrat seats. In contrast, we do see an increase in minority representation following Democratic legal control. These are statistically significant only in the full sample; however, point estimates are even larger in the recent decades. In the full sample, Democratic legal control is associated with an increase of 2.4% in the minority share of representation or 0.26 more minority representatives. Interestingly, we find impacts upon minority representation for both Democrats and Republicans resulting from Democrat legal control though the effect sizes are 3 times larger for Democrat minority representation. Likely, this is because Democrats may move minority candidates into somewhat competitive previously Republican districts where fielding a minority candidate can help Republicans win a seat. We show these results in Appendix Table E.3.

Table E.3—: Nonwhite Representatives

	(1)	(2)	(3)	(4)
Panel A. 1968 - 2016				
Rep Average Effect	-0.014	-0.170	-0.054	-0.116
	(0.011)	(0.141)	(0.046)	(0.125)
Dem Average Effect	0.024***	0.266**	0.066**	0.200*
	(0.009)	(0.112)	(0.032)	(0.105)
Number of Observations	1060	1060	1060	1060
R2	0.898	0.967	0.842	0.969
Panel B. 1968 - 1996				
Rep Average Effect	-0.006	-0.144	0.002	-0.145*
	(0.008)	(0.091)	(0.016)	(0.085)
Dem Average Effect	0.023**	0.231*	0.047	0.184
	(0.010)	(0.138)	(0.037)	(0.121)
Number of Observations	640	640	640	640
R2	0.902	0.954	0.798	0.959
Panel C. 1998 - 2016				
Rep Average Effect	-0.019	-0.179	-0.088	-0.091
	(0.017)	(0.187)	(0.074)	(0.171)
Dem Average Effect	0.028	0.373	0.131	0.241
	(0.018)	(0.232)	(0.078)	(0.193)
Number of Observations	420	420	420	420
R2	0.883	0.972	0.852	0.972
Fraction of Representatives Nonwhite	X			
Number of Nonwhite Representatives		X		
Number of Republican Nonwhite Representatives			X	
Number of Democrat Nonwhite Representatives				X

Note: Each column displays estimates for the average effect of political party control over redistricting. Panel A presents coefficients for the entire period from 1968 to 2016. Panel B focuses on 1968-1996, and Panel C covers 1998-2016. Column 1 reports average effects on the proportion of non-white representatives. Column 2 covers average effects on the number of non-white representatives. Column 3 focuses on the average effects on the number of non-white Republican representatives. Column 4 addresses the average effects on the number of non-white Democratic representatives. All models include state-decade and year fixed effects. Standard errors, clustered by state, are presented in parentheses.

F. 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 balanced 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 (now maintained by Doug Spencer): `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 addi-

tion, 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 an independent commission (type 6) as not having legal control by either party. In column 2 of Table A.3, we show robustness to redefining independent commissions (type 6) to partisan legal control if the state-decade has a trifecta. In column 3 of the same table, we go in the opposite direction and recode all advisory commissions as non-partisan.

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 F.1—: 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

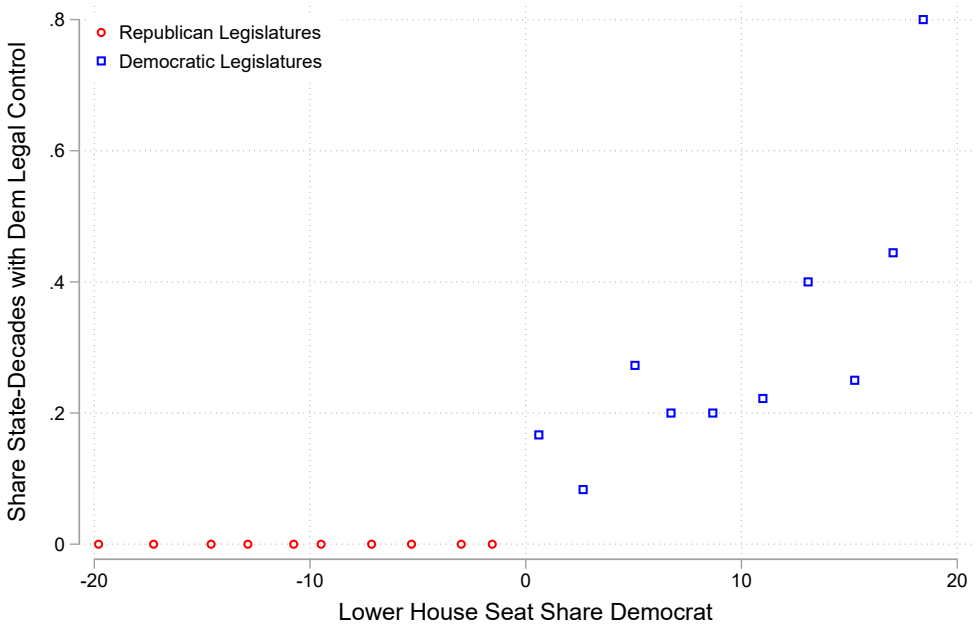
G. Jeong and Shenoy (2024) Discussion Appendix

The paper most similar to ours is Jeong and Shenoy (2024) (hencforth JS2024). In this appendix, we discuss the differences between our paper and theirs. JS2024 estimate the impact of political control over the lower house of a state legislature on subsequent seat shares. They use a two party seat share RD. They find similar sized estimates. However, they find effects are temporary. By contrast, we find permanent effects but only for one party and only in recent decades. In this Appendix, we account for the different results across the two papers and attribute them largely to differences in estimation strategy. We also argue that there are fundamental differences in the main parameter that we estimate and in the main parameter that they estimate.

As can be seen in Appendix Figure G.1, Democratic legal control is zero percent to the left of the discontinuity but state-decades with marginal legislative control over the lower chamber only have approximately a 20% probability of having Democratic partisan legal control. This is sensible since the states with a bare majority in the lower house are unlikely to be heavily partisan and thus unlikely to satisfy their states' requirements for partisan legal control.

A second difference between our specifications and the JS2024 specification is in the construction of the counterfactual. By using a seat share RD for the lower house only, state-decades to the left of the discontinuity include all state-decades with less than a 50% two party Democratic seat share. This includes both state-decades with Republican legal control as well as other state-decades without partisan legal control. Similarly, to the right of the discontinuity, state-decades are either under Democratic legal control or neither party has legal control. The fraction of Republican legal control just to the left of the seat share discontinuity is also around 20%. Thus, the JS2024 estimator estimates:

Figure G.1. : Two Party Dem Seat Share RD: Impact on Dem Legal Control Over Redistricting



Note: Figure plots the percentage of state-decades with Democratic legal control against the Lower House Democratic seat share. Estimates are binned using 1 percentage point increments, ranging from 30% (-20) to 70% (+20) Democratic seat share.

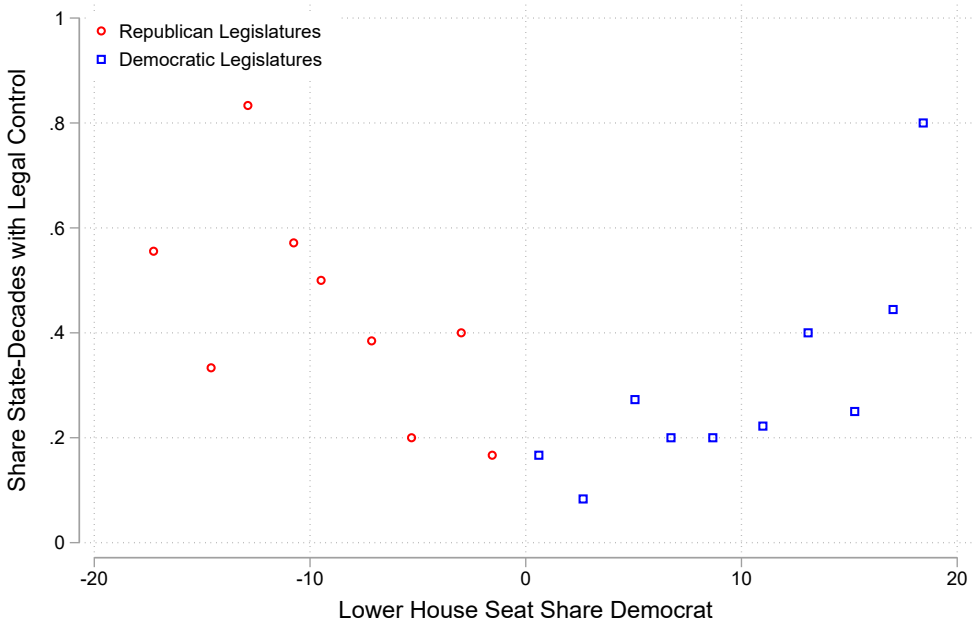
$$lim_{v \to .5^-} \omega \beta_{Rep} + [1-\omega]\beta_{None} - lim_{v \to .5^+} \omega \beta_{Dem} - [1-\omega]\beta_{None} = \omega[\beta_{Rep} - \beta_{Dem}] \tag{G.1}$$

where ω is the fraction of state-decades with partisan legal control, β_{Rep} is the effect on the subsequent seat share in the House of Representatives of Republican legal control, β_{Dem} is the similar effect of Democratic legal control, β_{None} is the effect of no partisan legal control, and v is the Democratic two party vote share.

From Equation G.1, we see that the effect of no partisan legal control cancels out across the discontinuity. This is because the fraction with no partisan legal

controls does not change across the discontinuity. The JS2024 estimator thus effectively estimates the impact of Republican legal control using a counterfactual of Democratic legal control. As an estimate of the effect of partisan legal control, the use of the seat share RD produces an upward bias because the control group incorporates the effect of partisan legal control by the other party. However, the estimator is also weighted by the fraction of marginal partisan legal control (20%) which attenuates the estimate. On net, the sign of the bias is unclear. In G.2, we show the fraction of Democratic and Republican legal control on each side of the discontinuity. Though derived from theory, we show in Figure G.2 that the share of states without legal control does not change across the discontinuity.

Figure G.2. : Two Party Dem Seat Share RD: Impact on Any Legal Control Over Redistricting



Note: Figure plots the percentage of state-decades with either Democratic or Republican legal control against the lower house Democratic seat share. Estimates are binned using 1 percentage point increments, ranging from 30% (-20) to 70% (+20) Democratic seat share.

A third difference between our paper and JS2024 is that we use stateXdecade and year effects, a gubernatorial RD conditional upon a unified legislature, and our SBBM estimator which estimates the impact of partisan legal control relative to states with a similar ex-ante probability of partisan legal control. The comparison group for Democratic legal control, therefore, is not Republican legal control but rather state-decades without legal control. In the case of the gubernatorial and SBBM estimators, we further restrict the comparison group to be state-decades without partisan legal control who nearly had legal control but didn't due to random vote shocks. We think that these comparisons, particularly the latter two face substantially lower endogeneity concerns than comparing Democratic legal control to Republican legal control[27].

A fourth difference is that the JS2024 reliance upon a seat share RD restricts them to more competitive states where the lower house is under contention. Our gubernatorial RD is similar in that regard. However our panel estimator and our SBBM estimator both estimate effects over a broader swathe of states and we find a surprising amount of similarity in the estimates. In fact, JS2024 note that the literature should try to estimate effects over a broader cross-section of states in their suggestions for the literature.

A fifth difference is also due to the use of a seat-share RD. Since the counterfactual for Democratic control over the lower house is Republican control over the lower house, inherently, it is impossible to separately estimate effects of control by party. In contrast, in each of our specifications, the control group consists of state-decades without partisan legal control. As a result, we *are* able to separately estimate effects by party.

There are a number of other differences between our specification and that of JS2024. In Appendix Table G.1 below, we replicate the main estimate from Jeong and Shenoy (2024) and compare our estimates. We estimate three basic

[27]Interestingly, the probability of legal control does not increase much with an increase in the lower house seat share as can be seen in both Appendix Figure G.2 and Appendix Figure G.1.

ways: using our stateXdecade and year fixed effects model, using their lower house seat share RD, and using an upper house seat share RD. These results are presented in columns 1, 2, and 3. For our specification, we can and do separately estimate effects for Democrats and Republicans. In columns, 2 and 3, we are using chamber-specific seat share RDs and thus we do not estimate effects separately by party. In the first super row, we alter our timing to match that of JS2024. This adds the years from 1962 to 1966 to our sample. In super row 3, we drop commission state-decades from the sample. In super row 4, we estimate at the district-year level rather than the state-year level. This gives much higher weight to large states. In super row 5, we estimate at the district level and additionally exclude all uncontested races. Finally, in the last super row, we estimate the JS2024 model in its entirety. In other words, we consider all the changes simultaneously.

Most of the changes do not yield large, statistically significant results in the two way fixed effects model over the full sample. Using district-level outcomes does as this weights large states much more heavily and large states have larger sized effects. Estimating at the district level and dropping uncontested races yields the largest coefficient in our panel estimator. However, the decision not to field a candidate is endogenous to the redistricting decision. If, for example, Democrats pack Republican voters into non-competitive districts which subsequently don't field Democratic candidates, the seat share of the remaining districts will be higher even if the overall seat share is unchanged. Most of the estimates are robust though not statistically different from zero with the lower house RD model. However, the upper House seat share RD yields small and statistically insignificant results in all but one specification including the full JS2024 specification. The one statistically significant result is of the wrong sign. It estimates the that winning the upper chamber reduces a party's seat share by 7.2 percentage points.

JS2024 also looks at racial gerrymandering as do we. They show that majority black census tracts are more likely to be moved in the redistricting process. By

Table G.1—: Transition Table: Main Effects

		(1)	(2)	(3)
CKK Method	Rep Effect	0.035		
		(0.032)		
	Dem Effect	-0.002	-0.060	-0.041
		(0.030)	(0.056)	(0.065)
Alternate Timing	Rep Effect	0.052**		
		(0.026)		
	Dem Effect	0.003	-0.068	0.072*
		(0.021)	(0.044)	(0.038)
Exclude Commissions	Rep Effect	0.030		
		(0.036)		
	Dem Effect	-0.005	-0.066	-0.032
		(0.032)	(0.061)	(0.071)
District Outcome	Rep Effect	0.041*		
		(0.023)		
	Dem Effect	-0.019	-0.077*	-0.050
		(0.025)	(0.039)	(0.041)
District Outcome[1]	Rep Effect	0.059**		
		(0.025)		
	Dem Effect	0.001	-0.039	-0.046
		(0.026)	(0.046)	(0.047)
All Changes	Rep Effect	0.067***		
		(0.021)		
	Dem Effect	-0.033*	-0.105***	0.004
		(0.017)	(0.036)	(0.030)
Treatment Definition		Control	LH RD	UH RD

Note Each cell presents an estimate for political control using various treatment specifications, sample, and panel constructions. Every specification incorporates state-decade and year fixed effects. The first column displays estimates using the baseline definition of legal control. The second column employs a Lower House seat share regression discontinuity design for estimates. The third column utilizes an Upper House seat share regression discontinuity design. Rows represent changes in sample or panel construction. The CKK method applies to the 1968-2016 sample from Table 3. Alternate timing measures effects for the first election post-redistricting, specifically elections ending in 2, as opposed to results from elections concluding in 8, 0, and 6. The exclusion of commissions omits any state with any form of redistricting commission. District outcome switches the outcome of interest to a binary variable at the district level, indicating whether a Republican won (1) or not (0). District Outcome[1] omits districts lacking one Democrat and one Republican candidate. All changes encompass alternate timing, commission exclusion, district outcomes, and the exclusion of uncontested elections. Standard errors, clustered by state-decade, are shown in parentheses.

contrast, our method finds that both Whites and Blacks are more likely to be moved across districts as a part of redistricting with Republican legal control because in general Republican legal control entails more shifting of land (and people). However, we do not see a differences across races in the probability of a Black versus a White individual being moved. We do, by contrast, see that Black voters are more likely to be moved to more extreme districts where their votes will be less pivotal under Republican legal control. We do not find this to be true for Whites nor for Democratic legal control.

Finally, though there are a large number of other differences between our respective papers, we focus on one other major difference. Since we find large partisan differences as well as differences over time in the effect of partisan legal control, we devote the latter portion of the paper to providing evidence on mechanisms as to why we see differences across the parties in political behavior.