The Effects of Voter Partisanship on Economic Redistribution: Evidence from Gerrymandering*

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Abstract

We study how voter partisanship affects economic redistribution. We model that partisan alignment between voters and their legislative representative reduces the representative's incentive to serve her constituents' economic interests. To identify shifts in partisan alignment, we exploit U.S. congressional redistricting and show that partisan gerrymandering produces predictable shifts in district-level voter partisanship. Comparing districts where the gerrymandering party's candidate narrowly won and narrowly lost the pre-redistricting election, we find representatives insulated by favorable gerrymandering vote more frequently with their party on congressional bills and bring less discretionary federal spending to their districts relative to representatives exposed by unfavorable gerrymandering.

JEL codes: D72, H73, P16

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1 Introduction

What are the economic consequences of voter partisanship in a representative democracy? Considerable attention has been placed on partisanship's social and political consequences, especially as the partisan divide between Democrats and Republicans in the United States has grown to historic levels in recent years.[1] Given that elected legislators wield significant influence over the allocation of federal resources, increasing voter partisanship may also have implications for public spending and economic redistribution. In this paper, we investigate how voter partisanship affects economic redistribution by influencing legislator incentives.

Voters evaluate political candidates based on many potential factors, with two important ones being a) the perceived ability of the candidate to serve the voter's interests and b) the degree to which the candidate's ideology aligns with the voter's own. When voters are more partisan—i.e., align more closely to one particular party—the partisan-advantaged candidate will find it easier to win elections by burnishing their partisan credentials rather than serving their constituents' best interests.[2] Ultimately, this increase in the partisan alignment between a district's voters and its representative may result in fewer federal resources allocated to that district due to a weakening of the representative's economic accountability to her constituents.

We formalize this intuition in a stylized model, in which a local shift in voters' partisan preferences toward a district's incumbent representative's party affects that incumbent's incentives in two ways. First, the incumbent is pushed toward her party's base to accommodate her constituents' ideological shift in the spirit of Downs (1957), manifesting in the incumbent being more likely to vote along partisan lines on prospective legislation. Second, a tighter partisan alignment between the representative and her constituents increases her electoral

[1] See `https://www.pewresearch.org/politics/2014/06/12/political-polarization-in-the-american-public/` to see recent polls that show Republicans and Democrats being more divided than ever on ideological issues.

[2] Anecdotal evidence suggests that partisanship may even induce voters to vote against their own economic interests. See `https://www.nytimes.com/2017/04/12/upshot/why-americans-vote-against-their-interest-partisanship.html`.

advantage and thus reduces the marginal electoral benefit to serving her constituents' economic interests. This leads the legislator to reduce her effort in bringing federal resources to her district.

We look to test our model's predictions but face an identification challenge. Specifically, a district's degree of partisan lean is likely correlated with socioeconomic factors that also affect the amount of federal spending allocated to the district. We address this challenge by exploiting the politically motivated redistricting ("gerrymandering") of U.S. congressional districts. In states where one party controls the redistricting process, the redistricting party has the incentive to redraw district boundaries to maximize the number of districts under its control. Based on the well-known strategy of "packing" opponents' voters into a few unwinnable districts and "cracking" one's own voters across many winnable districts (Owen and Grofman, 1988; Gilligan and Matsusaka, 1999), the redistricting party can achieve this by reallocating partisan supporters from non-competitive districts to competitive ones and reallocating opposing partisan voters in the opposite manner.

We use this insight to implement a novel regression discontinuity design (RDD) in which the "forcing" variable is the redistricting party's electoral margin of victory/defeat in the election immediately before redistricting. We expect a discontinuous jump at the zero margin cutoff in the incumbent candidate's partisan support in the subsequent election. Below the cutoff, the redistricting party narrowly loses the district in the pre-redistricting election and shifts the district's partisan balance against the incumbent. Above the cutoff, the redistricting party narrowly wins the district and shifts the district's partisan balance in favor of the incumbent.

We find evidence in support of this predicted discontinuity using U.S. House of Representatives district-level election data. Over the past two redistricting cycles, narrow-victory incumbents from the redistricting party have a post-redistricting vote share that is 19.4 percentage points higher than narrow-victory incumbents from the non-redistricting party. Using this exogenous shift in partisan alignment between voters and incumbent representatives, we find empirical evidence in support of our model predictions. First, we find that incumbents insulated by partisan gerrymandering become more partisan in their congressional roll call voting following redistricting. Second, we find that districts of insulated incumbents experience a relative decrease in the amount of discretionary spending they receive from the federal government. We further find that this drop in spending is accompanied by a decrease in regional output and hiring growth.

We evaluate alternative explanations for our empirical results using two sets of counterfactual tests. First, we examine states where partisan incentives should *not* affect redistricting, either because redistricting is controlled by a bipartisan or independent commission, or because a state contains only one district. Second, we examine alternative time periods (i.e., mid-decade years) in which no redistricting occurs. In both tests, we define our forcing variable as the vote margin of the party that *would* be in charge of defining district boundaries *if* redistricting were to occur under partisan rules and estimate our RDD accordingly. We find that our benchmark findings cannot be replicated in these counterfactual tests.

Our paper relates to the literature on electoral competition and economic redistribution. While our main focus is on voter partisanship, partisanship only affects federal transfers in our model by insulating incumbents from electoral competition. Prior studies have consistently found a positive connection between congressional electoral competition and local federal spending (Stein and Bickers, 1994; Levitt and Snyder Jr, 1995; Bickers and Stein, 1996; Lazarus, 2009), but these studies generally lack a clear causal identification strategy. In particular, a weak regional economy may produce voter dissatisfaction with the local incumbent while also attracting more federal transfers due to economic need. In our empirical design, variation in electoral competition derives from gerrymandering-induced shifts in voters' partisan preferences, a phenomenon likely to be orthogonal to potentially confounding socioeconomic factors.

Prior research that explicitly examines partisan politics and redistribution largely focuses on how a local representative's party affiliation affects federal spending in their district. For example, many studies find that having a representative affiliated with the President's party or the congressional majority party leads a region to receive more federal dollars (Levitt and Snyder Jr, 1995; Berry, Burden, and Howell, 2010; Albouy, 2013). While we focus on the partisan alignment between voters and their representative rather than the representative's affiliation with a particular party, our empirical strategy does exploit differences in representatives' affiliation with the within-state majority party. To ensure our results are not driven by direct effects related to party affiliation, we employ a first-differenced RDD (as used in Lemieux and Milligan (2008)) in which we limit our sample to districts where the incumbent party won the pre-redistricting election. This allows us to difference out any direct party affiliation effects by examining changes in outcomes in districts where the same party maintains control over consecutive terms.

We also contribute to the literature on how electoral incentives affect congressional voting. Mayhew (1974) argues that legislators will be more hesitant to vote along partisan lines on roll call votes when they face strong reelection challenges. However, Mann (2006) notes that the relationship between electoral competition and legislator partisanship likely depends on whether the competition (or lack thereof) stems from voters' preferences for a *particular party* or for a *particular candidate*. For example, an incumbent insulated from competition due to a strong personal reputation for serving constituent interests may not feel compelled to vote with her party, while an incumbent insulated from competition due to a tight partisan alignment with her constituents likely will feel pressure to vote in a more partisan manner. In this paper, we examine shifts in voter partisan preferences to study the latter prediction.

In support of Mayhew (1974), prior empirical studies have documented a negative relationship between electoral competition and legislator partisanship (Ansolabehere, Snyder Jr, and Stewart III, 2001; Canes-Wrone, Brady, and Cogan, 2002), but these generally establish correlations rather than causation. In contrast, Lee, Moretti, and Butler (2004) use an identification strategy based on close elections to *reject* Downsian convergence by showing that more competitive elections do *not* induce legislators to vote in a less partisan manner. Our paper differs from Lee et al. (2004) in two important respects. First, we examine the effect of *anticipated* electoral shifts, while they examine differences in backward-looking electoral performance.[3] Second, they use the well-documented phenomenon of the *incumbency advantage* to identify variation in electoral strength, while we use gerrymandering to identify shifts in partisan alignment. Under Mann's argument, greater electoral strength that derives from the incumbency advantage may not necessarily induce more partisan congressional voting if incumbents develop their advantage by forging a personal connection with their constituents.

Lastly, Polborn and Snyder Jr (2017) explicitly model party polarization in which voters care about candidates' local appeal (which they term "valence") and the national party positions. They find that when voters place less weight on valence and more weight on national party positions, polarization between the parties increases. Our model is similar in also allowing voters to care about national parties and non-partisan local issues. However, we keep the weights on the two components fixed while allowing for shifts in the partisan alignment between voters and national party positions. Moreover, rather than examining overall differences between national parties, we keep national party positions fixed and focus on shifts in local representatives' incentives.

2 Model

We construct a stylized model of electoral incentives in the presence of a partisan electorate. We distinguish between two channels of electoral support in the model: through cultivating the *personal vote* by catering to the economic interests of her constituents,[4] and cultivating the *partisan vote* by catering to the partisan preferences of her constituents.

[3] Specifically, they are interested in whether candidates credibly keep their campaign promises. For example, a Republican candidate facing a close election may be pressured to moderate her policy platform on the campaign trail, and be held to these promises in the following term if she wins. The question of whether candidates credibly commit to their campaign promises does not factor into our paper.

[4] Cain, Ferejohn, and Fiorina (2013) provides an in-depth discussion and history of the personal vote in American politics, and Jacobson (2015) provides evidence that the importance of the personal vote has declined due to the rise of partisan polarization. In political economy models, "local valence" is often used to describe a congressional candidate's personal appeal to voters.

In a single period, an incumbent politician simultaneously makes two choices that will determine her reelection prospects. First, she chooses her level of effort e to boost her personal appeal, where greater effort results in larger allocations of federal funds to her district. Voters reward effort by casting $y(e)$ worth of expected personal votes for the incumbent, but the incumbent incurs a personal cost of $c(e)$.[5] The personal vote and personal cost are both strictly increasing in e (i.e., $y' > 0$ and $c' > 0$), but the marginal personal vote is decreasing in effort ($y'' < 0$) while the marginal personal cost is increasing in effort ($c'' > 0$).

In the spirit of Downs (1957), the incumbent chooses a partisan position x, which encapsulates her voting record and public statements on partisan issues, to generate $f(x-b)$ worth of expected partisan votes, where b represents her constituents' partisan preference. The incumbent can maximize her partisan votes by choosing a partisan position to exactly match her constituents' preferences, formalized by f being a twice-differentiable concave function such that $f'(0) = 0$, $f'(x-b) < 0$ if $x > b$, $f'(x-b) > 0$ if $x < b$, and $f'' < 0$. Without loss of generality, we assume b is positive. We normalize the incumbent party's partisan preference to zero, and impose a cost to deviating from her party through the political cost function $g(x)$, a twice-differentiable convex function such that $g'(0) = 0$, $g'(x) > 0$ if $x > 0$, $g'(x) < 0$ if $x < 0$, and $g'' < 0$. We can think of this as the cost of losing party support during future election campaigns or facing primary challenges from the party's base.[6]

We assume that partisan and personal politics are separable—i.e., the incumbent's choice of e does not directly affect f or g, and her choice of x does not directly affect y or c. Thus, the incumbent's choice of e and x will yield $v = f(x-b)+y(e)$ worth of total expected votes. Given that voting is non-deterministic, having a sufficient number of expected votes does not guarantee victory. Instead, the incumbent's expected votes translate into a win probability of $w(v)$, where w is a strictly increasing concave function such that $w' > 0$ and $w'' < 0$. Here,

[5] One can think of the personal cost of effort as the political capital required to shape legislation in her constituents' favor and direct government transfers to her district or the time and resources spent in responding to constituent demands.

[6] The convexity assumption captures the idea that small deviations from the party line are likely to be overlooked, but large deviations will more likely be punished by party leaders or primary challenges.

concavity captures the idea that the marginal effect of an additional vote on the election outcome declines as the incumbent's electoral advantage grows. More generally, we can think of $w''(v) < 0$ if $v > \bar{v}$ and $w''(v) > 0$ if $v < \bar{v}$, where $\bar{v}$ represents the threshold for winning reelection. By assuming $w'' < 0$, we presume that the incumbent enjoys a baseline electoral advantage (i.e., $v > \bar{v}$), consistent with prior empirical evidence (Lee, 2008; Eggers, Fowler, Hainmueller, Hall, and Snyder Jr, 2015).

To understand the consequences of changing voter partisan preferences (i.e., shifts in b) we obtain comparative statics by solving the incumbent's maximization problem:

$$\max_{e,x} w(v) - c(e) - g(x),$$

in which she takes her constituents' partisan preference b as given. One can interpret $w(v)$ as the incumbent's expected benefit from winning the election, where the realized benefit of winning is normalized to one.

We show from first-order conditions that the incumbent's optimal partisan position lies between her party's and her constituents' preferences (see Appendix A for the proof):

Lemma 1. *Given $b > 0$, the incumbent's optimal choice of x^* must satisfy $0 < x^* < b$.*

Since we normalize the incumbent's party preference to zero, Lemma 1 implies that an *increase* in b can be interpreted as a partisan shift *against* the incumbent's party (e.g., turning a Democratic representative's constituency more Republican). Moreover, it follows from our initial assumptions that $f'(x^* - b)$ and $g'(x^*)$ are both positive,[7] allowing us to determine how the incumbent's optimal choices of e^* and x^* vary with b (see Appendix A for the proof):

Proposition 1. *Given $b > 0$, the incumbent's optimal choices and expected vote total will have the following relationship with b:*

[7] More generally, x^* must lie between 0 and b. If b is negative, then $b < x^* < 0$ implies that $f'(x^* - b)$ and $g'(x^*)$ are both negative, which will ultimately lead to the same set of empirical predictions.

(a) $\frac{\partial v^*}{\partial b} < 0$ – *i.e., the incumbent's vote share* $v^* = v(e^*, x^*)$ *is decreasing in* b.

(b) $\frac{\partial x^*}{\partial b} > 0$ – *i.e., the incumbent's partisan position* x^* *is increasing in* b*, and*

(c) $\frac{\partial e^*}{\partial b} > 0$ – *i.e., the incumbent's personal effort* e^* *is increasing in* b.

Proposition 1 formalizes a set of intuitive predictions about voters' partisan preferences shifting *toward* the incumbent's party. 1(a) implies the incumbent will increase her vote share, which we test directly using elections data. 1(b) implies she will shift her partisan position toward her party's base, which we test by examining congressional roll call votes. Lastly, 1(c) implies she will reduce her efforts to acquire federal resources, which we test by examining district-level federal spending. In the following section, we describe our strategy for identifying shifts in voter partisan preferences.

We note that we frame our predictions as *decentralized* responses from incumbent representatives reacting to shifts in voter partisan preferences. An alternative interpretation for our predictions about federal spending, following theoretical work by Lindbeck and Weibull (1993) and Dixit and Londregan (1996), is that the national party makes a *centralized* decision to tactically allocate federal resources to "swing districts". In this case, an unfavorable partisan shift that destabilizes an incumbent representative would lead the national party to allocate more resources to her district.

While we cannot completely disentangle whether our predictions come from a centralized ("push") or decentralized ("pull") response, we find some suggestive evidence to support a pull mechanism in our analysis. First, our finding that representatives change their roll call voting behavior suggests that local legislator incentives are indeed affected. Second, we examine different categories of federal spending, and find that our predicted effects are concentrated in spending categories for which local representatives possess greater discretion and influence.

3 Empirical Strategy

To identify exogenous shifts in voter partisanship, we exploit the predictability of the congressional redistricting process when it is under the control of a single party. Before describing our identification strategy in detail, we first describe the congressional redistricting process and discuss partisan gerrymandering theory.

3.1 Congressional Redistricting

The United States House of Representatives consists of 435 legislative seats, in which each seat represents a congressional district containing an approximately equal population of voters. Every two years, each district holds an election in which voters choose their representative to Congress. Following the decennial census in years ending in a '0', district boundaries are redrawn to account for population changes. The newly-drawn map first takes effect in the following election, in the year ending in a '2'.

Those in charge of redrawing district maps often manipulate redistricting to favor specific candidates, political parties, or population groups, a practice known as gerrymandering.[8] Gerrymandering can be motivated by a variety of interests. For example, *bipartisan* gerrymandering occurs when redistricting entrenches existing incumbents, regardless of partisan affiliation. Alternatively, *racial* gerrymandering refers to redistricting that either dilutes or concentrates the voting power of minority communities. To study shifts in voter partisanship, we focus on *partisan* gerrymandering, in which redistricting is motivated by partisan interests.

Partisan gerrymandering in the U.S. is facilitated by one particularly notable institutional practice. In most states, the *state legislature* is responsible for redrawing all congressional districts within the state, which means that the party with majority control of the state

[8] The term "gerrymander", a portmanteau of Gerry and salamander, originated from a satirical cartoon in 1812 following the redrawing of unusually shaped congressional districts in Massachusetts by then-Governor Elbridge Gerry.

legislature also directly controls redistricting. In such cases, the redistricting party ("gerrymanderer") has the incentive to redraw the congressional map to maximize the number of seats under its control, resulting in a biased electoral map that gives it a disproportionate number of seats relative to its share of the state-wide vote.

The alternative to legislature-controlled redistricting is to appoint an independent or bipartisan commission to oversee redistricting, which a minority of U.S. states have implemented to avoid partisan bias. A few low-population states contain only one district per state and therefore are not subject to redistricting bias concerns. While commission-based redistricting has become more common over time, most states still appoint their legislatures to pass redistricting plans. In 2020, state legislatures maintained primary control over redistricting in 39 out of 50 states.[9]

3.2 Predictability in Partisan Gerrymandering

How does the partisan gerrymanderer maximize its number of expected seats through redistricting? Owen and Grofman (1988) formulate an optimal strategy of "cracking-and-packing", in which one "packs" the opposing party's voters into a few unwinnable districts and "cracks" the remaining voters by spreading them over winnable districts. Intuitively, the gerrymanderer aims to create a disparity in the number of "wasted" votes between its opponent and itself (McGhee, 2014).

Existing theoretical models typically frame redistricting as a blank slate exercise, without considering the *pre-existing* electoral map. However, pre-existing electoral maps impose practical *constraints* on gerrymandering. Each pre-existing district is represented by an incumbent member of Congress who will likely object to extreme changes to her voter base. Moreover, many states have adopted redistricting principles that effectively constrain the gerrymanderer from making extreme changes to the electoral map. These principles include, among others, maintaining a compact district shape, preserving counties and other political

[9] See `https://redistricting.lls.edu/redistricting-101/who-draws-the-lines/`.

subdivisions, avoiding the pairing of incumbents against one another, and preserving the cores of prior districts.[10] Redrawn maps do not always comply with every redistricting principle, but egregious violations are more likely to attract negative media attention, face legal challenges, and be struck down by court rulings.

Our insight is that, given it is *costly* to make significant changes to the pre-existing electoral maps, the partisan gerrymanderer should prioritize winning districts that were closely contested prior to redistricting. Under cracking-and-packing, it is crucial to turn close losses, in which maximal votes are wasted, into close wins, in which minimal votes are wasted, and to prevent the opposite from happening. Intuitively, the gerrymanderer can pursue a crack-and-pack strategy while *making minimal changes to the electoral map* by "trading" friendly voters from uncompetitive districts for unfriendly voters from competitive ones.

We illustrate this idea in Figure I, with the horizontal axis representing the gerrymanderer's vote margin in the pre-redistricting election (in a year ending in '0') and the vertical axis representing the incumbent candidate's vote margin in the post-redistricting election (in a year ending in '2'). We first consider the benchmark case of *no gerrymandering*, i.e., district boundaries remaining fixed, represented by the blue lines. Assuming persistence in voter preferences,[11] we expect that a competitive pre-redistricting election should predict a competitive post-redistricting election and that an uncompetitive pre-redistricting election should predict an uncompetitive post-redistricting election. In the absence of gerrymandering, the persistence of voters' preferences should not depend on whether the redistricting party won or lost the pre-redistricting election, as reflected by the symmetry between the left and right sides of the plot.

The red lines in Figure I illustrate the consequences of a *cost-minimizing* crack-and-pack gerrymandering strategy. Just to the left of the central axis, the gerrymanderer shifts the partisan balance against the incumbent from the opposing party, pulling the red line

[10] See `https://www.ncsl.org/research/redistricting/redistricting-criteria.aspx`.

[11] We discuss this assumption in more detail in the following section.

below the blue line. Just to the right side of the central axis, the gerrymanderer shifts the partisan balance in favor of its own incumbent, pushing the red line above the blue line. The gerrymanderer accomplishes this through a combination of packing its opponent's stronghold districts (represented by the red line rising above the blue line on the far left) and cracking its own stronghold districts (represented by the red line falling below the blue line on the far right).

If the gerrymanderer were unconstrained in its ability to redefine districts, it would be free to crack and pack pre-existing districts in an infinite number of ways. However, the strategy illustrated in Figure I requires minimal deviation from the status quo (i.e., the blue lines) relative to alternative pack-and-crack strategies. For example, packing the gerrymanderer's stronghold districts on the far right side of the plot or cracking the opponent's stronghold districts on the far left side would require making much larger changes to the pre-existing electoral map. Therefore, we use Figure I as a basis for our empirical predictions.

3.3 Rising Polarization and Optimal Gerrymandering Strategy

We note that cracking-and-packing does not constitute the only possible partisan gerrymandering strategy. Notably, Friedman and Holden (2008) construct a model of partisan gerrymandering in which the gerrymanderer follows a "slice-and-match" rather than crack-and-pack strategy. In their framework, the gerrymanderer observes a noisy signal of voter preferences from a continuous distribution and will optimally group "slices" of the voter distribution together in a way that maximizes the number of districts where the median voter tilts its way. They find that, under certain conditions, the optimal strategy is to group one's most extreme supporters with the opposing party's most extreme supporters, contrary to the traditional packing intuition.

Under the slice-and-match framework, it is unclear whether the partisan gerrymanderer will trade friendly voters from uncompetitive districts for unfriendly voters from competitive districts in the manner illustrated in Figure I. An important assumption of Friedman and

Holden (2008) is that the gerrymanderer's posterior distribution about voters' preferences follows a *unimodal* distribution where the mode lies at the mean. This assumption implies that voter preferences may not persist over time in the manner illustrated in Figure I. Intuitively, the gerrymanderer may lack a sufficient "supply" of partisans to reallocate between competitive and uncompetitive districts under a unimodal distribution.

We posit that the secular trend of rising polarization in American politics has resulted in a distribution of voter preferences that is more *bimodal* in nature. Boxell, Gentzkow, and Shapiro (2020) find that, over the last several decades, the U.S. experienced the largest increase in party polarization among twelve OECD countries. As the median voter in each party drifts further apart, the traditional intuition of cracking-and-packing is more likely to apply.[12] Moreover, the phenomenon of *geographic polarization* makes geographic voting patterns more persistent over time, which means that the closeness of the pre-redistricting election should more closely predict the closeness of the post-redistricting election as illustrated in Figure I.

Given that political polarization has increased over time, we expect the predicted relationship in Figure I to be more pronounced in more recent decades. Lang and Pearson-Merkowitz (2015) provide evidence that the geographic sorting of voters into "red counties" and "blue counties" emerged in the mid-1990s. Therefore, we consider 2000 as the first "post-polarization" redistricting cycle in our empirical tests. However, we note that increasing polarization may not be the only explanation for any differences in cross-decade comparisons. For example, advances in computation techniques have made gerrymandering techniques more sophisticated over time, potentially leading to more pronounced partisan effects.[13]

[12] At the extreme, Gilligan and Matsusaka (1999) finds cracking and packing to be optimal when every voter prefers one party with certainty and their preference is observed by the gerrymanderer.

[13] See `https://www.theatlantic.com/politics/archive/2017/10/gerrymandering-technology-redmap-2020/543888/` for a discussion of how gerrymandering techniques have progressed over time.

3.4 Regression Discontinuity Design

We use the predicted discontinuity around the central axis from the red lines in Figure I to construct a regression discontinuity design (RDD) in which the pre-redistricting vote margin acts as the "forcing" variable according to the following specification:

$$
\begin{aligned}
IncumbMargin_{i,c+2} = \; & RedistWin_{i,c} \cdot p_{win}(RedistMargin_{i,c}) \\
& + (1 - RedistWin_{i,c}) \cdot p_{loss}(RedistMargin_{i,c}) \\
& + \beta \cdot RedistWin_{i,c} + \gamma_c + \epsilon_{i,c},
\end{aligned} \tag{1}
$$

where i indexes districts and c indexes redistricting cycle years that end in a '0', *IncumbMargin* represents the difference in the vote shares received by the incumbent party candidate and the best-performing challenger candidate, *RedistMargin* denotes the difference in vote shares received by the redistricting party and the non-redistricting party, $p_{loss}(\cdot)$ and $p_{win}(\cdot)$ represent independent polynomial splines on either side of the discontinuity, *RedistWin* denotes a dummy variable indicating whether $RedistMargin > 0$ (i.e. whether the redistricting party wins), γ_c denotes redistricting cycle (i.e., decade) fixed effects, and ϵ represents the residual error term.

We expect β to be positive, as it represents the difference between a partisan shift toward the incumbent's party and a partisan shift away from the incumbent's party. We define $p_{loss}(\cdot)$ and $p_{win}(\cdot)$ as independent spline functions on either side of the $RedistMargin = 0$ threshold to account for predicted differences in slope and curvature. In our benchmark specification, we use independent cubic splines to account for potential non-linearities,[14] but we also explore alternative parametric assumptions in robustness tests.

Based on Figure I, we expect the slopes on the two sides of the threshold to have op-

[14] While the plots in Figure I are linear for illustrative purposes, we remain agnostic about the *curvature* of the relationship between pre-redistricting and post-redistricting vote margins. It is possible, for example, for the blue lines representing the status quo to be concave to reflect the upper-bound in electoral margins, and for the gap between the blue and red lines to follow a non-linear pattern to account for the fact that the gerrymanderer may want to target a broader range of districts in the center of the plot due to uncertainty about which districts are truly competitive.

posite signs due to the difference in alignment between the incumbent and the redistricting party, and the slope to be steeper on the left side relative to the right side to reflect packing and cracking. This motivates us to implement a *global* RDD that uses all available data points. Gelman and Imbens (2019) advise against using global high-order polynomial regressions for placing significant weights on points far from the discontinuity threshold, but such observations matter in our setting because a crack-and-pack strategy involves trading votes between competitive and uncompetitive districts. According to Lee (2008), a global parametric approach using all available data points should produce a more efficient estimate than a non-parametric local approach when the parametric assumptions are well-motivated. Moreover, Pei, Lee, Card, and Weber (2020) argue that the common intuition that RD designs should only use observations close to the discontinuity threshold can be misleading in finite samples where the optimal bandwidth may be relatively large. Maximizing efficiency is crucial in our setting as the infrequency of redistricting limits our sample size and statistical power. In robustness tests, we show that our benchmark results are even stronger under a local linear design with Imbens and Kalyanaraman (IK) bandwidths.

3.5 Estimation Framework for Congressional Voting and Spending

We are ultimately interested in testing our model's prediction about congressional voting behavior and district-level federal spending. Since our predictions relate to *anticipated* partisan shifts, we focus on outcomes during the post-redistricting period, defined as the two years between the pre-redistricting election in year c and the post-redistricting election in year $c+2$. During this period, the incumbent knows whether she is aligned with or against the redistricting party, but the new district shapes have not yet taken effect.[15] For example, in the 2000 redistricting cycle ($c = 2000$), we examine the incumbent's roll call votes and her districts' federal financial assistance during the 2001-2002 congressional session. Let

[15] We implicitly assume that incumbent representatives can *anticipate* gerrymandering that targets their district. For example, we expect a Democrat representative who narrowly won her seat in 2000 in a state where the Republican party controls redistricting should be able to anticipate that the Republicans will gerrymander against her for the next election.

$Y_{i,c}$ denote the post-redistricting outcome of interest (congressional voting or district-level spending), and consider the following model of $Y_{i,c}$:

$$Y_{i,c} = \pi b_{i,c} + \mu_{i,c}, \tag{2}$$

where $b_{i,c}$ denotes the anticipated partisan position of district i's constituents (i.e., b from our model), π parameterizes $\partial Y^*/\partial b$ from our model, and $\mu_{i,c}$ denotes the component of Y that is unrelated to b.

In our model, an increase in b represents a partisan shift against the incumbent's party. Therefore we predict π to be positive when Y represents either a) the amount of federal resources allocated to district i, or b) the frequency at which district i's representative votes *against* her party. We cannot estimate Eq. 2 directly because we do not observe b, but we can use the discontinuity between narrow-redistrictor-win districts and narrow-redistrictor-loss districts to identify shifts in b:

$$\begin{aligned} E(Y_{i,c}|RW_{i,c}=1) - E(Y_{i,c}|RW_{i,c}=0) = \pi[E(b_{i,c}|RW_{i,c}=1) - E(b_{i,c}|RW_{i,c}=0)] \\ + E(\mu_{i,c}|RW_{i,c}=1) - E(\mu_{i,c}|RW_{i,c}=0), \end{aligned} \tag{3}$$

where *RedistWin* from Eq. 1 is abbreviated to *RW* for brevity, and $E(b_{i,c}|RW_{i,c} = 1) - E(b_{i,c}|RW_{i,c} = 0)$ represents the expected difference in voter partisan preferences between districts where the redistricting party won and lost the pre-redistricting election. Under our predictions about partisan gerrymandering, we expect this difference to be negative in closely-contested districts. Given our model predicts $\pi > 0$, we should therefore also expect $\pi[E(b_{i,c}|RW_{i,c} = 1) - E(b_{i,c}|RW_{i,c} = 0)]$ to be negative.

It is clear from Eq. 3 that the causal effect of b on Y is identified under the assumption $E(\mu_{i,c}|RW_{i,c} = 1) - E(\mu_{i,c}|RW_{i,c} = 0) = 0$. In our RDD, this means assuming congressional voting and district-level spending outcomes are not affected by whether the redistricting party narrowly won or narrowly lost the pre-redistricting election in any way *ex-*

cept through b. If this assumption holds, then we should interpret finding $E(Y_{i,c}|RW_{i,c} = 1) - E(Y_{i,c}|RW_{i,c} = 0) < 0$ (i.e., a sharp decrease in Y at our RDD cutoff) as evidence in support of our model.

However, the very definition of RW poses a potential threat to identification. Since redistricting is controlled by the party that holds power over the state legislature (and, in non-supermajority states with gubernatorial veto power, the governorship as well), RW reflects the incumbent representative's alignment with the party that controls the state "trifecta" (both legislative chambers and the governorship). This creates the potential for RW to affect Y in ways other than through gerrymandering-induced shifts in b. For example, being aligned with the state trifecta may allow a district to receive more in-state transfers and therefore be less dependent on federal transfers.

We take two steps to address this identification threat. First, following the first-difference RD estimator methodology from Lemieux and Milligan (2008), we define our dependent variables as $\Delta Y_{i,c}$, the time-series *difference* between post-redistricting outcomes and pre-redistricting outcomes, where the pre-redistricting period is defined as the two years leading up to the year c election (e.g., 1999 and 2000 for $c = 2000$). For closely-contested districts, it is not yet known in the pre-redistricting period whether the representative will be aligned with the redistricting party in the post-redistricting period. Therefore, we can use it as a benchmark to difference out any district-level characteristics that are time-invariant within a redistricting cycle.

Second, we restrict our sample to districts where the incumbent party wins the pre-redistricting election in year c. For example, if Republicans held a district during the post-redistricting term, we include it in the sample only if Republicans also held the district during the pre-redistricting term. Intuitively, conditioning on "same-party" districts allows us to difference out potentially confounding political factors, including the incumbent's party affiliation. Our estimation framework can thus be expressed as follows:

$$
\begin{aligned}
&E(\Delta Y_{i,c}|RW_{i,c}=1, IW_{i,c}=1) - E(\Delta Y_{i,c}|RW_{i,c}=0, IW_{i,c}=1) \\
&= \pi[E(\Delta b_{i,c}|RW_{i,c}=1, IW_{i,c}=1) - E(\Delta b_{i,c}|RW_{i,c}=0, IW_{i,c}=1)] \\
&+ E(\Delta \mu_{i,c}|RW_{i,c}=1, IW_{i,c}=1) - E(\Delta \mu_{i,c}|RW_{i,c}=0, IW_{i,c}=1),
\end{aligned}
\tag{4}
$$

where $IW_{i,c}$ denotes an indicator for the incumbent party winning the pre-redistricting election in year c.

Here, $E(\Delta b_{i,c}|RW_{i,c}=1, IW_{i,c}=1) - E(\Delta b_{i,c}|RW_{i,c}=0, IW_{i,c}=1)$ represents the expected difference in voter partisan preference *shifts* between districts narrowly won by an incumbent from the redistricting party and districts narrowly won by an incumbent from the non-redistricting party. We should still expect this difference to be negative to reflect diverging shifts in partisan alignment between voters and their representatives. In particular, conditioning on $IW = 1$ should not affect our predictions about partisan gerrymandering, as the redistricting party should have the incentive to "protect" its own narrowly-held districts and "attack" the opposing party's narrowly-held districts, regardless of whether the incumbent party held the district the previous term.

In this modified estimation framework, identification requires $E(\Delta \mu_{i,c}|RW_{i,c}=1, IW_{i,c}=1) - E(\Delta \mu_{i,c}|RW_{i,c}=0, IW_{i,c}=1) = 0$. This assumption should be satisfied as long as local *trends* in legislator partisanship and federal spending do not depend on whether an incumbent from the redistricting party or an incumbent from the non-redistricting party narrowly won the pre-redistricting election, in which case we can interpret a sharp decrease in ΔY at our RDD cutoff as evidence in support of our model's predictions. Importantly, by conditioning on $IW = 1$, we ensure the sitting representative's affiliation does not change between the pre-redistricting and post-redistricting periods, and thus difference out any effects related to the incumbent's party affiliation, as well as any other district-level characteristics that are time-invariant within a redistricting cycle.

4 Data

4.1 Defining the Redistricting Party

We construct our sample using the 1980, 1990, 2000, and 2010 redistricting cycles, which yield 1,740 district-decade records (435 districts per decade). To restrict the sample to states in which one party has legislative control of redistricting, we apply the following filters. First, we eliminate any state that contains only one district or appoints an independent or bipartisan commission to redraw district boundaries. Out of the remaining states, we eliminate any state that is not under the unified majority control of one party at the time of redistricting. We then check whether the governor is a member of the "redistricting party", and eliminate any state in which a) the governor has veto power over redistricting, b) the governor's political affiliation is in opposition to the redistricting party, and c) the redistricting party does not have a super-majority (i.e., a two-thirds majority) that can override the governor's veto. Applying these filters results in 756 district-decade observations.

We provide details on the construction of this set of "vetoproof" states in Table I. For each decade, a given state is indicated to be vetoproof if the entries under "LegControl" and "GubVeto" are both blue (indicating Democratic control) or both red (indicating Republican control). We use data from The Council of State Governments' Book of the States to construct the vetoproof sample for 1980 and 1990,[16] and data from Professor Justin Levitt's website to construct the vetoproof sample for 2000 and 2010.[17] Note that there are two exceptions to the sample filters described above. First, Nebraska maintains a non-partisan state legislature and so never enters our sample. Second, Connecticut and Maine require a legislative super-majority for the legislature to control redistricting, which neither party ever achieved in the four decades of our sample.

We obtain data on U.S. House of Representatives election outcomes from the MIT Elec-

[16] See `https://knowledgecenter.csg.org/kc/category/content-type/content-type/book-states`.

[17] See `http://redistricting.lls.edu/who.php`.

tion Lab (MIT Election Data and Science Lab, 2017). For each decade, we construct our explanatory variables (*RedistMargin* and *RedistWin*) based on the redistricting party's vote margin in the pre-redistricting election (in years ending in a '0'). In Figure II, we present maps illustrating the geographic distribution of *RedistMargin*, the forcing variable in our RDD, for each decade. Yellow districts represent decisive victories by the redistricting party, red districts represent decisive victories by the non-redistricting party, and orange districts represent closely-contested districts.[18] We note that closely-contested orange districts are spread out and not clustered in any one region, which is important for two reasons. First, a lack of clustering alleviates potential concerns of broad regional trends driving our empirical findings. Second, the "trading" of voters between competitive and uncompetitive districts requires the two types of districts to share boundaries.

Following Lee and Lemieux (2010), we verify that the density of our RDD's forcing variable, $RedistMargin$, is continuous at the discontinuity threshold. Figure III illustrates the distribution of $RedistMargin$, with subfigure (a) providing a kernel density plot and subfigure (b) providing a density plot corresponding to the McCrary density smoothness test from McCrary (2008). The plots show that the distribution of our forcing variable is smooth across the discontinuity threshold. This is reinforced by a formal McCrary density smoothness test, which produces a t-statistic of 0.07, as well as a more recent manipulation test from Cattaneo, Jansson, and Ma (2019), which produces a robust t-statistic of 0.16.[19]

4.2 Linking Pre-Redistricting and Post-Redistricting Districts

We also use the MIT Election Lab data to construct our election outcome variable of interest, *IncumbMargin*, defined as the incumbent candidate's vote margin in the post-redistricting election. We define the incumbent as the post-redistricting election candidate who is *a member of the same party* as the winner of the pre-redistricting election, which allows us

[18] Districts from states with non-partisan gerrymandering or not controlled by a unified state legislature are left blank.

[19] Both statistics are in reference to the null hypothesis that the underlying distribution is smooth across the threshold.

to include cases where the winner of the pre-redistricting election does not participate in the post-redistricting election. This means the incumbent candidate can be either a) a representative who won the pre-redistricting election, b) a replacement representative who fills a vacant seat midway through the term, or c) a replacement candidate who fills in for a representative who does not run in the post-redistricting election.

We focus on the incumbent *party* rather than the incumbent *candidate* because our predictions concern shifts in voter preferences in relation to national parties rather than to local candidates. For example, a rightward shift in the electorate toward the Republican party should affect a Democratic candidate regardless whether she is the sitting incumbent or a replacement candidate. However, we obtain similar findings if we restrict our sample to candidates who run in both the pre-redistricting and post-redistricting elections.

Given that redistricting changes how districts are defined, the task of matching the appropriate $IncumbMargin_{c+2}$ for each $RedistMargin_c$ is not trivial.[20] We apply the following rules to ensure we link pre-redistricting and post-redistricting districts in a consistent manner:

1. We link pre-redistricting district i to post-redistricting district j if the winner of the pre-redistricting election in i runs in the post-redistricting election in j.
2. We link pre-redistricting district i to post-redistricting district j if the winner of the pre-redistricting election in i is replaced by another sitting representative who runs in the post-redistricting election in j.
3. From the remaining unlinked districts, we link pre-redistricting district i to post-redistricting district j if the geographic overlap between i and j is greater than the overlap between i and any other unlinked post-redistricting district. Such links reflect cases where the sitting representative for district i does not run for reelection.

After applying these linkages, we obtain 735 district-decade observations over four decades.

[20] In particular, we cannot simply rely on district labels due to the possibility of label changes. For example, Adam Schiff represented California's 29th district during the 2011-2012 term and ran in California's 28th district in the 2012 election, but the "change" was simply due to CA-29 being "redistricted" (i.e., slightly redrawn and relabeled) as CA-28 during the 2010 redistricting cycle.

In rare instances, multiple incumbents are linked to a single post-redistricting district. This occurs when reapportionment reduces the number of seats within a state and incumbents from different districts are forced to run against one another.[21] As this does not fit into our prediction framework, we eliminate all districts where two incumbents are paired against one another in the post-redistricting primary or general election. In a few cases, incumbents from eliminated seats do not run and therefore are not matched to a post-redistricting election outcome. Similarly, the creation of new seats from reapportionment results in some post-redistricting elections that cannot be linked to a pre-redistricting incumbent. Applying these filters results in a sample of 695 district-decade observations.

We present descriptive statistics for our pre-redistricting and post-redistricting election variables in Panel A of Table II. The top half of Panel A provides statistics for the full sample that includes all four decades. The first two rows show that the redistricting party achieves an average vote margin of 17.9 percentage points and wins the pre-redistricting election 66 percent of the time. The next row shows that the incumbent achieves an average vote margin of 34.3 percentage points. This large average incumbency advantage is well-documented, but the large standard deviations indicate significant variation across elections.

In the bottom half of Panel A, we present statistics for a subsample limited to the 2000 and 2010 redistricting cycles. As discussed in Section 3.3, we expect the predicted discontinuity in our RDD to be more pronounced in later decades due to greater polarization and geographic sorting making a crack-and-pack strategy more viable. To maintain consistency with the sample we use to test our predictions on roll call votes and federal financial assistance, we further restrict this subsample to districts where the incumbent party retains power from the pre-redistricting period (years $c-1$ and c) to the post-redistricting period (years $c+1$ and $c+2$). We see that our summary statistics are not significantly affected by these sample restrictions.

We note that the redistricting party's average vote margin, while not as large as the

[21] In the last two reapportionments, incumbents faced each other 13 times either in a primary election or general election.

average incumbent margin, is positive and significant in both samples. This suggests that alignment with the redistricting party (which holds legislative power within a state) may *directly* confer an electoral advantage. We address this potential alternative explanation for post-redistricting differences in incumbency advantage by conducting counterfactual tests in which we apply our RDD in cases where partisan gerrymandering should not occur.

4.3 Congressional Voting, Redistribution, and Economic Outcomes

We construct our congressional voting, redistribution, and economic performance variables by taking the difference between post-redistricting and pre-redistricting averages as described in Section 3.4. We limit our analysis of these differenced outcomes to the 2000 and 2010 redistricting cycles, partially due to the lack of available data for earlier decades (for redistribution and GDP measures, in particular). More importantly, as we will discuss in greater detail in Section 5, we limit our analysis to later decades because we find no evidence of a discontinuity in $IncumbMargin_{c+2}$ during earlier redistricting cycles, suggesting that a crack-and-pack strategy was less viable during this earlier period of low polarization and relatively unsophisticated gerrymandering techniques.

We note that all our differenced variables are defined according to *pre-redistricting* district definitions. Due to the length of the typical redistricting process, new district boundaries are usually not finalized until a few months before the year $c+2$ election. During the post-redistricting period (coinciding with the congressional term from $c+1$ to $c+2$), the incumbent representative still serves constituencies defined according to the "old" districts. Therefore, to capture the incentive effects from *anticipated* partisan gerrymandering, we keep district definitions static within a redistricting cycle. This also means that we do not need to link pre-redistricting and post-redistricting districts as we do for election outcomes.

We obtain data on representative-level roll call votes from the Congressional Roll-Call Votes Database from `www.voteview.com` (Lewis, Poole, Rosenthal, Boche, Rudkin, and Sonnet, 2021). We construct two variables that measure a representative's propensity to vote along partisan lines. For *VoteDev*, we calculate the absolute difference between a representative's vote and the average vote by their party (where "yea" is coded as one and "nay" is coded as zero) and average this vote-level difference across all votes within a two-year congressional term. For *VoteProb*, we take *prob*, a Voteview-constructed measure that reflects the ex-post likelihood of a representative's recorded vote given their DW-NOMINATE ideology score,[22] and average it across all votes within a congressional term. Since this latter measure is derived from legislators' personal congressional records, we can use it to detect *within-legislator* changes in voting behavior.

In Panel B of Table II, we present summary statistics for our vote measures in the pre-redistricting period ("Pre" columns), the post-redistricting period ("Post" columns), and the difference between the two ("Δ" columns). We create separate measures of *VoteDev* and *VoteProb* based on subcategories of legislative issues from Peltzman (1984) that Voteview uses to classify congressional voting. Given that we are interested in economic redistribution, we focus on *economic* issues, which Peltzman divides into the following four categories:

(a) **"BGI"** indicates Budget General Interest bills related to the debt limit, budget targets, revenue sharing, unemployment insurance, tax rates, continuing appropriations, etc.

(b) **"BSI"** indicates Budget Special Interest bills related to authorization/appropriations for agencies and departments, public works, subsidized housing, food stamps, etc.

(c) **"RGI"** indicates Regulation General Interest bills related to general tariff, minimum wage, gasoline rationing, auto emissions, water pollution, etc.

(d) **"RSI"** indicates Regulation Special Interest bills related to union regulations, coal mine regulations, export/import controls, fish and wildlife, etc.

We also create *VoteDev* and *VoteProb* measures that encompass all legislative issues (suffix "All"), including both economic and non-economic issues.[23] The "Pre" and "Post"

[22] Poole and Rosenthal developed the NOMINATE (NOMINAl Three-step Estimation) numerical estimation methodology, which calculates the probability of a legislator voting yea or nay on a given roll call vote, conditional on that legislator's ideological "ideal point," which is also calculated in the estimation method. The DW-NOMINATE (Dynamic, Weighted NOMINATE) method is their further refinement of NOMINATE.

[23] Non-economic issues include domestic social policy, defense and foreign policy, and government organization.

means from Panel B indicate that legislators are more likely to vote along partisan lines (i.e., exhibit lower *VoteDev* and higher *VoteProb*) on general interest issues relative to special interest issues.

For our analysis on distributive politics, we focus on the following major categories of federal financial assistance:

(a) **Grants** provide funding for projects that benefit the general public and stimulate the economy. We examine two major categories of grants: formula grants, which are awarded based on statistical criteria (e.g., job training grants allocated based on the number of unemployed individuals), and discretionary grants, which are awarded through a merit-based selection process by federal agencies (e.g., innovation grants awarded based on R&D project design).

(b) **Loans** involve the U.S. government subsidizing financial lending. There are two main categories of loans: direct loans, in which the government itself acts as the lender, and guaranteed loans, in which the government covers part or all of the default risk on loans made by private-sector intermediaries.

(c) **Direct payments** are a form of non-reimbursable transfer of cash from the federal government to an individual, private firm, or private institution. Payments are generally governed by statute (e.g., agricultural subsidies specified in the Farm Bill) rather than annual appropriations.

(d) **Contracts** involve the U.S. federal government acquiring goods or services from a non-federal entity. Examples of government contracts include purchase contracts for military equipment, construction contracts for federal infrastructure projects, and service contracts to maintain government websites.

We obtain data on federal financial assistance from USASpending.gov, a government website that reports federal awards of more than $25,000.[24] Since USASpending data is available only starting in 2008, we supplement it with data from the U.S. Census' Consolidated Federal

[24] We collect district-level data on loans, grants, government contracts, and direct spending from USASpending's API (`https://api.usaspending.gov`), specifically through the "spending by geography" endpoint.

Funds Reports (CFFR), which we obtain from the National Institute for Computer-Assisted Reporting (NICAR). The CFFR data is available up to 2007 and provides similar categories of federal financial assistance but is less detailed than USASpending. Certain categories of federal assistance are only available from USASpending, limiting coverage for those measures to the 2010 redistricting cycle.

For our analyses on economic performance, we obtain county-level data on regional GDP from the Bureau of Economic Analysis (BEA) and county-level data on employment, earnings, and hires from the Census Quarterly Workforce Indicator (QWI). Since county-level GDP data is unavailable before 2001, our analysis of GDP growth is limited to the 2010 redistricting cycle. We convert all county-level measures into district-level measures by applying a population-weighted mapping method using the Missouri Census Data Center (MCDC) geographic correspondence engine,[25] in which we define districts according to their pre-redistricting boundaries.

In Panel C of Table II, we present summary statistics for redistribution and economic performance measures in the pre-redistricting period ("Pre" columns), the post-redistricting period ("Post" columns), and the log difference between the two ("Δlog" columns).[26] All dollar figures are expressed in millions of USD, and all employment figures are expressed in thousands. Contracts and formula grants constitute the largest categories of federal financial assistance (each accounting for approximately 2% of district GDP). In contrast, direct and guaranteed loans constitute the smallest (each accounting for less than 0.1% of GDP).

Since we compare two years before and two years after the year c election, we can interpret the Δlog measures as *biennial* growth rates. Comparing the upper and lower rows, we observe significantly more variability in growth rates for federal financial assistance measures relative

[25] Specifically, we aggregate totals for all counties within a district, and for any county that straddles multiple districts, we proportionally allocate that county's total across the districts based on population. For example, if a county has 50% of its population in district A and 50% in district B, we split that county's GDP evenly across A and B when aggregating district-level GDP. Population weights come from the MCDC's "allocation factor" which measures the proportion of one geographic sub-unit's population that resides within another geographic sub-unit.

[26] Note that the number of observations for GDP and for grants are smaller than for other measures since data is unavailable for the 2000 redistricting cycle.

to GDP and employment measures. For example, we see that *FormulaGrants* grew almost 200% from the pre-redistricting period to the post-redistricting period, on average. This is primarily explained by the American Recovery and Reinvestment Act (ARRA), which saw large amounts of stimulus administered through formula grants in response to the Great Recession of the late 2000s (Boone, Dube, and Kaplan, 2014).

5 Results

5.1 Partisan Gerrymandering and Election Outcomes

We test whether partisan gerrymandering affects election outcomes in the manner described in Section 3 and illustrated in Figure I. We present binned scatterplots that illustrate the relationship between *RedistMargin* and various post-redistricting election outcomes in Figure IV. In each plot, the solid lines represent fitted polynomial splines from estimating Eq. 1 and the dashed lines represent the 95% confidence interval around the fitted values. We look for a discontinuity at the $RedistMargin = 0$ threshold (marked by the vertical dashed red line) and interpret the change from the left side of the threshold to the right side (i.e., the coefficient on *RedistWin*) to be statistically significant at the 5% level if the solid line on one side falls outside the dashed boundaries on the opposite side.

In the first three subfigures, we present the relationship between *RedistMargin* and *IncumbMargin* for a) all four decades, b) the 1980 and 1990 redistricting cycles, and c) the 2000 and 2010 redistricting cycles. It is clear from subfigure (a) that the fitted relationship between *RedistMargin* and *IncumbMargin* exhibits the same general pattern as the red lines in Figure I. Consistent with the pack-and-crack strategies described in Section 3.2, *IncumbMargin* is higher on the right side of the threshold relative to the left side, and the slope of the fitted relationship is steeper on the left side relative to the right side. We interpret the discontinuity at the $RedistMargin = 0$ threshold as the difference between gerrymandering that insulates the incumbent through a favorable partisan shift and gerrymandering that exposes the incumbent to an unfavorable partisan shift.

Subfigures (b) and (c) reveal that this pattern is detectable only for later decades. We offer three potential explanations for this finding. First, as noted in Section 3.3, a pack-and-crack strategy is theoretically more viable when voters' partisan preferences follow a *bimodal* distribution (i.e., when the electorate is more polarized), and as Lang and Pearson-Merkowitz (2015) document, the partisan geographic sorting of voters is a relatively recent phenomenon that emerged in the late 1990s.[27] Second, improved computing power greatly improved the sophistication of gerrymandering techniques. Until the 1990s, most legislators lacked access to computerized mapping software and often drew districts using colored pens on acetate sheets overlaid on top of big maps on the floor.[28] Third, the earlier decades in our sample were marked by a long period of legislative dominance by the Democratic party,[29] which suggests that gerrymandering may have been used to solidify the dominant party's majority rather than maximizing the number of seats. Therefore, we focus on the 2000 and 2010 redistricting cycles throughout the remainder of the paper to identify shifts in voter partisanship.

In the second row of figures (subfigures (d) through (f)), we illustrate the relationship between *RedistMargin* and *WinnerMargin*, defined as the *absolute* difference in vote shares between the top two candidates in the post-redistricting election. *WinnerMargin* serves as a direct measure of electoral competition, where a higher margin indicates a *less* competitive election. We see very similar patterns in the second row as in the first row, with the discontinuity in subfigures (d) and (f) indicating a drop in *WinnerMargin* at the $RedistMargin = 0$ threshold. This serves to validate the assumption in our model that a partisan shift toward the incumbent's party results in less competitive elections and consequently decreases the value of the marginal vote (i.e., $w''(v) < 0$).

[27] They attribute the "tipping point" nature of the 1990s to the Republican party's "Contract with the American Family" movement and the introduction of Fox News in 1996.

[28] See `https://www.economist.com/united-states/2002/04/25/how-to-rig-an-election`.

[29] The Democrats held the House of Representatives for 40 years before 1994, often claiming more than 60% of seats. Since then, control of the House has flipped several times, and no party has claimed over 60% of seats.

In Table III, we present the results from estimating Eq. 1 using the late-decades sample only. For robustness, we include additional district-level control variables to the specification from Eq. 1. These consist of the log population size (*lnPop*), the median population age (*MedAge*), the percent of the population that is male (*PctMale*), the percent of the population that is non-Hispanic white (*PctWhite*), the log median house value (*lnHouseVal*), and the unemployment rate (*UnempRate*). All control variables, obtained from U.S. Census Bureau's Decennial Census, are measured immediately before redistricting (i.e. in years ending in a zero). Following Calonico, Cattaneo, Farrell, and Titiunik (2019), we impose the same covariate adjustment above and below the discontinuity cutoff (i.e., we do not interact control variables with *RedistWin*), as we later show that *RedistWin* has no mean effect on the control variables. Unless otherwise indicated, all tables report results from regressions that include these control variables.

In the first two columns, we present estimates based on the sample of *all* districts in vetoproof states (i.e., not restricted to the incumbent party winning the pre-redistricting election), the same sample used to produce the fitted plots in Figure IV. In column (1), the point estimate for *IncumbWin* indicates a difference in incumbent vote margin of 19.4 percentage points. In column (2), we see a similar albeit slightly lower estimate for *WinnerMargin*, confirming that a partisan shift toward the incumbent's party results in more competitive elections.

In the last two columns, we present estimates based on the sample restricted to districts where the incumbent party wins reelection in the pre-redistricting election. We do this to make our election analysis consistent with our analysis of congressional voting and economic redistribution. As discussed in Section 3.4, these restrictions serve to net out any effects related to partisan differences and partisan alignment with the state legislature. We observe that the estimated discontinuities in *IncumbMargin* and *WinnerMargin* are even larger for the restricted sample. A potential explanation for the larger point estimates is that first-term incumbents, who are eliminated by this sample restriction, are less likely to be the target of gerrymandering because their future electoral performance is *less predictable* given their limited track record.

5.2 Test of Balanced Covariates

Following Lee and Lemieux (2010), we check to ensure that observable covariates are "balanced" across our RDD cutoff. In Figure V, we provide scatterplots illustrating the relationship between our forcing variable *RedistMargin* and the district-level characteristics that we used as control variables in our table regressions. The top row of figures represents analysis on the sample of all districts from vetoproof states, and the bottom row represents analysis on the restricted sample of districts where the incumbent party retains power from the pre-redistricting to the post-redistricting period. As in all subsequent analyses, we focus on the 2000 and 2010 redistricting cycles only.

We observe that the covariates consistently exhibit balance across the $RedistMargin = 0$ threshold. Specifically, the fitted splines on either side of the RDD cutoff point lie within the 95% confidence interval bands of the opposing side at the threshold. This is confirmed by the estimates reported in Table IV,[30] which shows that covariates do not differ significantly between districts where the redistricting party narrowly loses and districts where the party narrowly wins the pre-redistricting election. These findings support the identifying assumption of quasi-random assignment above and below the RDD cutoff for both the restricted and unrestricted samples.

5.3 Effects on Congressional Voting

In Figure VI, we present scatterplots illustrating the relationship between *RedistMargin* and congressional vote measures described in Section 4.3. According to Proposition 1(b) from our model, we should expect a partisan shift toward the incumbent's party to result in the incumbent voting more frequently with her party on roll call votes.

[30] The regressions represented by the table estimates do not contain additional control variables, as the control variables themselves constitute the dependent variables of interest.

In the top row of figures, we examine $\Delta\, VoteDev$, which measures the change in a representative's propensity to deviate against her party on roll call votes. We see from subfigure (a) that, consistent with our model predictions, incumbents insulated by favorable gerrymandering deviate less frequently from their party on roll call votes relative to incumbents exposed by unfavorable gerrymandering. The next four subfigures ((b) through (e)) show that this difference is more pronounced for bills on special interest issues, both on budgetary matters (BSI) and regulatory matters (RSI).

In the bottom row of figures, we examine $\Delta\, VoteProb$, which captures changes in a representative's tendency to vote in accordance with her NOMINATE ideology score. Subfigure (f) shows that insulated incumbents are *more* likely to vote as predicted by their NOMINATE score following redistricting. In the next four subfigures, we see the pattern is again more pronounced for BSI and RSI bills. Since the NOMINATE score is based on a legislator's personal voting record, these findings also indicate that increased partisanship exhibited by insulated incumbents comes from *within-legislator* changes in their voting behavior.

In Table V, we show that our findings are not qualitatively affected by the addition of control variables. In Panel A, which presents estimates for $\Delta\, VoteDev$, we see from the point estimate in column (1) that insulated incumbents deviate from their party by 2.73 percentage points (25 percent relative to the pre-redistricting mean) less following redistricting relative to vulnerable incumbents. The subsequent columns in Panel A reveal no detectable difference between insulated and vulnerable incumbents in votes related to general interest issues (columns (2) and (4)), and that the vote deviation gap is more significant (both economically and statistically) for RSI bills (column (5)) than for BSI bills (column (3)).

We observe the same pattern in Panel B as in Panel A, with inverted signs due to opposing interpretations of $\Delta\, VoteDev$ and $\Delta\, VoteProb$. In column (1), we interpret the point estimate as insulated incumbents being 2.57 percentage points (2.92 percent relative to the pre-redistricting mean) more likely to vote as predicted by their NOMINATE score relative to vulnerable incumbents following redistricting. Consistent with the wide confidence intervals we observe in Figure VI, the estimates for $\Delta\,VoteProb$ are noisier than those for $\Delta\,VoteDev$, potentially due to measurement errors in the construction of NOMINATE scores.

We provide two interpretations for the stronger results on special interest issue bills relative to general interest issue bills. First, as shown in Table II, votes on general interest issue bills tend to be significantly more partisan than votes on special interest issue bills on an overall basis. Therefore, special interest bills provide more margin for a representative to shift in a partisan direction. Second, special interest bills are more likely to be relevant to the *local* interests in an incumbent's district, and a partisan shift that insulates her from electoral competition will make her less likely to serve those interests. This may also explain why we observe larger effects for regulatory issues than budgetary issues, as there may be greater cross-district variation in regulatory policy preferences due to differences in local industry composition.

5.4 Effects on Distributive Politics

We test whether a partisan shift that insulates the incumbent representatives from electoral competition affects the amount of federal resources her district receives. In Figure VII, we present scatterplots illustrating the relationship between *RedistMargin* and measures of federal financial assistance described in Section 4.3. According to Proposition 1(c) from our model, we should expect an insulated incumbent to reduce her effort in serving her constituents' interests, resulting in fewer federal resources directed toward her district. In the first three subfigures, we observe evidence supporting this prediction in the form of lower growth in project grants, formula grants, and guaranteed loans on the right side of the $RedistMargin = 0$ threshold. However, the next three figures show that we do not observe this pattern for direct loans, contracts, and direct payments.

In Table VI, we show that our findings are not qualitatively affected by the addition of control variables. The point estimates are large, indicating a 56.7 and 96.1 percentage point difference, respectively, in project grant growth (column (1)) and guaranteed loan growth (column (5)). The difference in growth rates for formula grants is even larger, but the estimate is no longer statistically significant with the inclusion of control variables. As mentioned in Section 4.3, the American Recovery and Reinvestment Act (ARRA) of 2009 resulted in significant increases in formula grants, and our analysis on grants is limited to the 2010 redistricting cycle due to data availability. When we include coarser data from the CFFR to examine *overall* grant growth for both the 2000 and 2010 cycles, we still observe a significant difference between insulated-incumbent-districts and vulnerable-incumbent-districts, as seen in column (3).

What explains the differences in our findings across different categories of federal financial assistance? Prior studies (Rich, 1989; Alvarez and Saving, 1997) have found that the distribution of federal grants is susceptible to *local* political influence, and Stein and Bickers (1994) find similar evidence of federal grants being directed toward vulnerable congressional incumbents. Project grants, in particular, make a natural target for political influence given the discretionary nature of their awarding process. Many representatives offer services to help their constituents through the competitive grant application process and the Congressional Research Service even provides a guide for representatives to help their constituents obtain grants (Gerli, 1997).[31] Formula grants, awarded based on statistical formulas rather than agency discretion, should be less susceptible to political influence in principle, but Alvarez and Saving (1997) surprisingly find that formula grants are even more politically sensitive than project grants. In our sample period, the large influx of formula grants brought on by the ARRA may have been particularly enticing for incumbents weakened by gerrymandering.

The contrasting findings for guaranteed loans and direct loans may also be explained by political expediency. Unlike direct loans, guaranteed loans are distributed through private-sector intermediaries and do not require the federal government to provide up-front capital. This speeds up capital distribution, making guaranteed loans a more attractive target for

[31] See `https://carson.house.gov/help-from-andre/additional-services-and-resources/grants`, for example.

congressional representatives facing two-year election cycles.[32] In the case of Small Business Administration (SBA) loan guarantee programs, congressional offices often write letters to the SBA advocating directly for individual loan applicants from their district,[33] consistent with a decentralized "pull" mechanism" (i.e., incumbent incentives) rather than a centralized "push" mechanism (i.e., party strategy). Notably, prior research by Bickers and Stein (1996) finds that the distribution of SBA loans tends to be particularly sensitive to district-level electoral competition.

Finally, government contracts and direct payments are relatively inflexible forms of spending. Government contracts are strictly regulated under the Competition in Contracting Act (CICA), and contract approvals are governed by a rigid approval process that often requires independent cost estimates. Direct payments are generally governed by statute rather than annual appropriations, making legislation the sole avenue for congressional influence. For Congressional incumbents facing two-year election cycles, it is generally more expedient to target the distribution of existing spending and transfer programs rather than wait for the passage of new legislation.

5.5 Local Economic Performance

We explore how changes in federal resource allocation arising from gerrymandering-induced shifts in voter partisanship ultimately affect local economic performance. Given the possibility of public spending crowding out private-sector economic activity, it is not obvious that increased federal spending will lead to detectable higher regional growth. Ramey (2011) provides a summary of papers that study regional fiscal multipliers, and show a wide variety of estimated values, including negative multipliers. Notably, Cohen, Coval, and Malloy (2011) finds that increases in local congressional spending are associated with cutbacks in corporate

[32] The attractiveness of using private banks to accelerate the distribution of government funds is illustrated by the central role of SBA loan guarantees in the economic stimulus plan in response to the global coronavirus pandemic. See `https://www.wsj.com/articles/sba-to-oversee-vast-lending-program-under-federal-economic-stimulus-plan-11585167856`.

[33] Examples of return correspondence from the SBA can be found on its website, `https://www.sba.gov/document/report--congressional-correspondence`.

investment.

In Figure VIII, we present plots that illustrate the relationship between *RedistMargin* and growth in local output and employment outcomes. We see that insulated incumbent districts to the right of the cutoff indeed experience lower growth across a variety of economic performance measures, but only the differences in GDP growth (subfigure (a)) and labor hiring growth (subfigure (b)) appear statistically significant. These findings suggest an overall positive fiscal multiplier, in that any negative crowding-out effect is insufficient to offset the direct positive effect of increased spending on output and employment growth. Moreover, the employment, earnings, and hiring measures are based on private-sector figures, and our finding that growth in these measures is *not higher* for insulated incumbents to the right of our RDD cutoff further suggests evidence *against* a crowding-out effect.

An examination of Table VII reveals that, while all coefficients estimates are negative, only the estimated effect on hiring growth (a 6.8 percentage point difference between exposed-incumbent districts and insulated-incumbent districts) is statistically significant. The lack of precision in these estimates may potentially stem from the noisiness of our district-level output and employment measures. Recall that these outcomes are constructed from county-level measures that are mapped to district-level measures in a process that involves population-weighted imputations when a county overlaps multiple districts.

We note that we are also limited in our ability to make quantitative inferences about a multiplier effect because we do not observe *all* forms of federal benefits and transfers that incumbents weakened by partisan gerrymandering potentially direct toward their districts. For example, a weakened incumbent may attempt to shape regulatory legislation in a manner favorable to firms in her district (consistent with our congressional voting analysis) or target other forms of federal transfers not covered by the limited USAspending and CFFRA data. From this perspective, we view these findings on regional growth as providing a more holistic view of how voter partisanship affects regional economies.

5.6 Alternative Explanations

We evaluate whether our findings may be explained by factors other than gerrymandering-induced shocks to voter partisanship. Since we restrict the sample to districts where the same party retained power in the pre-redistricting and post-redistricting periods, our first-differenced RD estimator should allow us to account for any time-invariant effects within a redistricting cycle (including affiliation with the state trifecta). However, it is possible that alignment with the redistricting party could affect time-*varying* trends in local federal spending and congressional voting. This would violate our identifying assumption from Eq. 4—i.e., $E(\Delta\mu_{i,c}|RW_{i,c} = 1, IW_{i,c} = 1) \neq E(\Delta\mu_{i,c}|RW_{i,c} = 0, IW_{i,c} = 1)$.

To ensure that confounding explanations do not drive our main findings, we conduct two separate counterfactual tests. First, we repeat our benchmark tests on an alternative sample of states that are *not* subject to partisan redistricting due to containing only one district or redistricting being assigned to an independent/bipartisan commission. We define the "pseudo-redistricting party" in the same way we define the redistricting party in our main sample and estimate our benchmark RDD (without control variables) with the pseudo-redistrictor's vote margin as the forcing variable. We focus on the 2000 and 2010 redistricting cycles, in which our main findings are concentrated.

In Panel A of Table VIII, we present the results of our counterfactual tests on four key outcome variables: *IncumbMargin*, Δ*VoteDevAll*, Δ*lnProjGrants*, and Δ*lnGuarLoans*. The positive coefficient in column (1) suggests that alignment with the state trifecta may improve a representative's election performance, but the effect is significantly smaller than our main estimate (14.7 vs. 34.9 percentage points) and statistically insignificant. In the last three columns, we see that the estimated effects on congressional voting behavior and federal assistance growth are in the opposite direction relative to our main findings, although only the estimate for Δ*VoteDevAll* is statistically significant.

The use of non-partisan states for a counterfactual test is potentially problematic for two reasons. First, there are relatively few non-partisan states, resulting in a small sample size and large standard errors. Second, even in states with commission-based redistricting, the redrawing of boundaries may be influenced by partisan forces. In particular, Coriale, Kaplan, and Kolliner (2020) find evidence that advisory commissions do not necessarily reduce partisan redistricting, which suggests that our coefficient estimate from column (1) may still be capturing the effects of gerrymandering-induced partisan shifts.

To address these issues, we employ an alternative counterfactual test in which we repeat our analysis using mid-decade "pseudo-redistricting years." Specifically, we define the redistricting party based on who controlled the state legislature and governorship immediately following years ending in a '4' or a '6' (instead of a '0'), and use that party's vote margin as the forcing variable in our RDD.[34] We then estimate our benchmark tests (without control variables) with the pseudo-redistricting year as a reference point. For example, given a pseudo-redistricting year of 2004, we examine the change in outcomes from the 2003-2004 term to the 2005-2006 term. We still focus on the 2000s and 2010s, but the sample here is larger because we have up to two observations per decade instead of one. Note that we limit the pseudo-redistricting years to the *mid-decade* in order to avoid overlap with our main sample.

We present the results of this second counterfactual test in Panel B of Table VIII, which shows the same set of outcome variables as Panel A. The coefficient estimate in column (1) for *IncumbMargin* is still positive and statistically insignificant, but it is much closer to zero and more precisely estimated. The remaining columns provide estimates with opposing signs to our first counterfactual tests, but none of the estimates are statistically significant. These counterfactual test findings indicate that our main findings indeed capture the effects of gerrymandering-induced partisan shifts rather than the effect of being aligned with or against the state trifecta.

[34] For example, if the Democrats controlled the state legislature and held the governorship (or had a two-thirds legislative supermajority or the Governor lacked veto authority) following the 2014 election, we use the Democratic candidate's vote margin in the 2014 district election as the forcing variable.

5.7 Long-Run Effects

In Table IX, we present analysis of partisan gerrymandering's *long-run* effects through the inclusion of outcome variables from later election cycles. Each row represents a different regression, in which the explanatory variables remain the same (i.e., defined according to the redistricting party's performance in the pre-redistricting election in year c), but the dependent variable varies across the columns. Specifically, the outcome variable for row τ is defined based on the τth two-year period following the pre-redistricting election. Thus, the first row presents our benchmark estimates for outcomes in the first post-redistricting period (years $c+1$ to $c+2$), the second row presents estimates for outcomes in the second post-redistricting period (years $c+3$ to $c+4$), etc.

In column (1), we define *PartisanMargin* as the vote margin of the party that won the pre-redistricting election in year c. By fixing the partisan reference relative to the gerrymanderer's party, we isolate gerrymandering's long-run partisan effects. For example, suppose the Democrats narrowly won a district in 2000 and the Republicans gerrymandered against the incumbent for the 2002 election. In that case, we track the Democratic party's performance in subsequent elections even if the seat subsequently flipped back to the Republicans. We see that gerrymandering's partisan effects are persistent, with positive coefficients in all rows and statistical significance in three of the four terms.

In column (2), we observe that the estimated effect of partisan gerrymandering on *IncumbMargin* is not statistically significant following the first post-redistricting election. This can be explained by the fact that the incumbent's alignment with the redistricting party may change following the first post-redistricting election. For instance, if the gerrymanderer successfully unseats a vulnerable incumbent from the opposing party, the *new* incumbent will find the original redistricting to be in her favor. Therefore, the direction of the partisan shift *relative to the incumbent representative's party* becomes unstable at longer horizons.

Given our model predictions are all based on partisan shifts relative to the incumbent's party (which we cannot predict at longer horizons), we should not be able to detect any effects on congressional voting and redistribution at longer horizons. In the last three columns of Table IX, we examine select measures of congressional voting (*VoteDevAll*) and distributional politics (*lnProjGrants* and *lnGuarLoans*). Each Δ outcome variable is defined as the change from the pre-redistricting period to the τth period following redistricting. The estimates show little evidence of any effects at longer horizons, where we can no longer predict the direction of the partisan shift in relation to the incumbent's party. The positive effect on guaranteed loan growth exhibits some persistence, but only for one additional period.

5.8 RDD Specification Robustness

In all analyses presented thus far, we use a global cubic polynomial specification in our RDD. For robustness, we replicate key regressions using local linear regression discontinuity methods with Imbens and Kalyanaraman (IK) bandwidths and global regression discontinuity methods with alternative polynomial orders (quadratic and quartic). As in our main analysis, we include pre-redistricting district characteristics as control variables and decade fixed effects.

Table X presents the results of using alternative specifications, where all reported estimates represent coefficients for the indicator variable *RedistWin*. The first row provides estimates from the local linear RDD design, in which we limit the sample to observations in which the forcing variable *RedistMargin* falls within the IK bandwidth as defined according to Imbens and Kalyanaraman (2012), and allow for independent linear relationships between *RedistMargin* and the outcome variable on either side of the RDD threshold.[35] The bottom two rows provide estimates from global RDD designs that are identical to the global polynomial approach we employ in the main paper, except with quadratic and quartic splines, respectively, instead of cubic ones.

Each column corresponds to a different dependent variable we examine in our main paper. We see that employing alternative specifications does not qualitatively change our main find-

[35] We use a triangular kernel function to construct our estimator for the local linear RDD, following guidance provided in Fan and Gijbels (1996).

ings: incumbents are more electorally insulated in narrow-redistrictor-win districts relative to narrow-redistrictor-loss districts, and this is accompanied by more partisan roll call voting behavior, fewer federal assistance dollars, and weaker local economic performance. The local linear specification produces estimates that are particularly significant, both economically and statistically, with the estimate for GDP growth particularly notable. We note that given the bandwidth restrictions in the local linear specification, the sample size is small (ranging from 25 observations for $\Delta lnGDP$ to 76 observations for $IncumbMargin$).

6 Conclusions

In this paper, we study how voter partisanship affects distributive politics. We construct a stylized model to show that a district-level shift in voter partisan preferences toward their legislative representative's party should incentivize the local representative to prioritize partisan politics over serving her constituents' economic interests. Using partisan gerrymandering to identify shifts in partisan alignment, we find evidence in support of our model's predictions. Specifically, our first-differenced RD estimator shows that representatives insulated by a favorable partisan shift vote more frequently along party lines and obtain fewer federal transfers for their constituents relative to representatives exposed to an unfavorable partisan shift.

We refrain from providing welfare implications on the last point regarding distributive politics, but our findings raise the normative question of whether electoral politics *should* affect the allocation of federal resources. In our setting, districts of insulated and vulnerable representatives are ex-ante similar across a wide range of dimensions, suggesting that distributive politics does not direct resources towards where they are most needed. However, this may not be the case in a general context outside of our specific empirical setting designed to identify partisan shifts. For example, a district neglected by its representative may suffer economically, leading to more competitive elections that motivate the representative to direct needed resources to her constituents. Whether electoral pressures generate positive or negative feedback loops with respect to regional economic *needs* forms an important follow-up question for future study.

Appendix: Proofs

A. Proof of Lemma 1

Proof. We take the first-order conditions of the incumbent's maximization problem (Eq. 2) to characterize the optimal effort e^* and optimal partisan position x^*:

$$w'(v^*)y'(e^*) - c'(e^*) = 0 \tag{A.1}$$

$$w'(v^*)f'(x^* - b) - g'(x^*) = 0 \tag{A.2}$$

where $v^* = f(x^* - b) + y(e^*)$.

We note that given our assumptions of $w'(x) > 0$, then (A.2) implies that $g'(x)$ and $f'(x^* - b)$ must have the same sign. Given our assumption that $b > 0$, it must follow that $0 < x^* < b$, otherwise $g'(x^*)$ and $f'(x^* - b)$ will have opposite signs.

$\square$

B. Proof of Proposition 1

Proof. We differentiate $v^* = f(x^* - b) + y(e^*)$ with respect to b to obtain:

$$\frac{\partial v^*}{\partial b} = -f'(x^* - b) + f'(x^* - b)\frac{\partial x^*}{\partial b} + y'(e^*)\frac{\partial e^*}{\partial b}, \tag{A.3}$$

and differentiate (A.1) and (A.2) with respect to b to obtain:

$$\frac{\partial e^*}{\partial b} = \frac{w''(v^*)\frac{\partial v^*}{\partial b}y'(e^*)}{c''(e^*) - w'(v^*)y''(e^*)} \tag{A.4}$$

$$\frac{\partial x^*}{\partial b} = \frac{w''(v^*)\frac{\partial v^*}{\partial b}f'(x^* - b) - w'(v^*)f''(x^* - b)}{g''(x^*) - w'(v^*)f''(x^* - b)}. \tag{A.5}$$

From our assumptions, $w''(v^*) < 0$, $y'(e^*) > 0$, $c''(e^*) > 0$, $w'(v^*) > 0$, $y''(e^*) < 0$, $f''(x^* - b) < 0$, and $g''(x^*) > 0$. Furthermore, $0 < x^* < b$ from Lemma 1 implies that $f'(x^* - b) > 0$. Therefore, it suffices to show that $\frac{\partial v^*}{\partial b} < 0$ to prove that $\frac{\partial e^*}{\partial b} > 0$ and $\frac{\partial x^*}{\partial b} > 0$.

To this end, we substitute (A.4) and (A.5) into (A.3) to obtain:

$$\frac{\partial v^*}{\partial b} = \frac{-f'(x^* - b)g''(x^*)}{\alpha^* \beta^*}, \tag{A.6}$$

where

$$\alpha^* = g''(x^*) - w'(v^*)f''(x^* - b), \tag{A.7}$$

$$\beta^* = 1 - \frac{f'(x^* - b)^2 w''(v^*)}{\alpha^*} - \frac{y'(e^*)^2 w''(v^*)}{c''(e^*) - w'(v^*)y''(e^*)}. \tag{A.8}$$

It follows from our assumptions that $\alpha^* > 0$ and $\beta^* > 0$, and therefore $\frac{\partial v^*}{\partial b} < 0$, which proves part (c) of Proposition 1. It then follows from (A.4) and (A.5) that $\frac{\partial e^*}{\partial b} > 0$ and $\frac{\partial x^*}{\partial b} > 0$, which proves parts (a) and (b) of Proposition 1.

□

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Table I: Redistricting Control

This table presents data on partisan control of redistricting in each state across the last four redistricting cycles. *LegControl* indicates which party controls legislative oversight of redistricting and *GubVeto* indicates which party controls the gubernatorial veto over redistricting plans. "D" indicates Democratic control, "R" indicates Republican control, "SPLIT" indicates split control, "BC" indicates bipartisan commission control, "IC" indicates independent commission, "OD" indicates one-district states with no need for redistricting, "NP" indicates a non-partisan state legislature, "NSM" indicates that neither party has the state-required super-majority to control redistricting, "NV" indicates that the governor does not possess veto power, and "(OR)" indicates that the legislature has a super-majority to override a gubernatorial veto. Red represents Republican control and blue represents Democratic control.

	1980		1990		2000		2010	
State	LegControl	GubVeto	LegControl	GubVeto	LegControl	GubVeto	LegControl	GubVeto
Alabama	D	D	D	R	D	D	R	R
Alaska	OD		SPLIT		OD		SPLIT	
Arizona	R	D	SPLIT		SPLIT		IC	
Arkansas	D	R (OR)	D	D	D	R (OR)	D	D
California	D	D	D	R	D	D	IC	
Colorado	R	D	R	D	SPLIT		SPLIT	
Connecticut	NSM		NSM		NSM		NSM	
Delaware	OD		OD		OD		OD	
Florida	D	D	D	D	R	R	R	R
Georgia	D	D	D	D	D	D	R	R
Hawaii	BC		BC		BC		BC	
Idaho	R	D	SPLIT		IC		IC	
Illinois	SPLIT		D	R	SPLIT		D	D
Indiana	R	R	SPLIT		SPLIT		R	R
Iowa	R	R	D	R	R	D	SPLIT	
Kansas	R	D	SPLIT		R	R	R	R
Kentucky	D	D	D	D	SPLIT		SPLIT	
Louisiana	D	R	D	D	D	R (OR)	R	R
Maine	SPLIT		NSM		NSM		NSM	
Maryland	D	D	D	D	D	D	D	D
Massachusetts	D	D	D	R	D	R (OR)	D	D
Michigan	D	R	SPLIT		R	R	R	R
Minnesota	D	R	D	R	SPLIT		R	D
Mississippi	D	D	D	R (OR)	D	D	D	R
Missouri	D	R (OR)	D	R	SPLIT		R	D
Montana	IC		IC		IC		IC	
Nebraska	NP		NP		NP		NP	
Nevada	D	R	SPLIT		SPLIT		D	R
New Hampshire	R	D	R	R	R	D	R	D (OR)
New Jersey	D	R	R	D	SPLIT		BC	
New Mexico	D	D	D	D	D	R	D	R
New York	SPLIT		SPLIT		SPLIT		SPLIT	
North Carolina	D	D	D	NV	D	D	R	NV
North Dakota	OD		OD		OD		OD	
Ohio	SPLIT		SPLIT		R	R	R	R
Oklahoma	D	D	D	D	D	R	R	R
Oregon	D	R	SPLIT		R	D	SPLIT	
Pennsylvania	R	R	SPLIT		R	R	R	R
Rhode Island	D	D	D	D	D	R (OR)	D	I (OR)
South Carolina	D	D	D	R	R	D	R	R
South Dakota	OD		OD		OD		OD	
Tennessee	D	R	D	D	D	R	R	R
Texas	D	R	D	D	SPLIT		R	R
Utah	R	D (OR)	R	R	R	R	R	R
Vermont	OD		OD		OD		OD	
Virginia	D	D	D	D	R	R	R	R
Washington	R	R	IC		IC		IC	
West Virginia	D	D	D	D	D	D	D	D
Wisconsin	D	R	D	R	SPLIT		R	R
Wyoming	OD		OD		OD		OD	

Table II: Summary Statistics

This table presents summary statistics for four sets of data used in our analyses: U.S. House of Representatives election results, House legislators' voting patterns, federal assistance, and local economic outcomes. In Panel A, we present mean, standard deviation, median, 25th percentile, 75th percentile, and total number of observations for election outcomes for the redistricting party in the year of the U.S. Census (c) and for the incumbent in the election following the Census ($c+2$). In the top half of Panel A, we present statistics across the last four complete Census cycles (1980s, 1990s, 2000s, and 2010s) for all vetoproof congressional districts and, in the bottom half, we present statistics for the 2000 and 2010 Census cycles for vetoproof congressional districts where the incumbent party retained power in the Census year election. In Panels B and C, we present mean and standard deviation for the pre-redistricting term ($c-1$ to c), the post-redistricting term ($c+1$ to $c+2$), and the change between the two terms, as well as the number of observations for each variable. In both of those panels, our sample is vetoproof congressional districts in the 2000 and 2010 Census cycles where the incumbent party retained power in the Census year election. In Panel B, we report the change as the raw difference between the two terms whereas, in Panel C, we report the natural log of the change (for consistency with our usage of the variables). Panel B provides summary statistics for House legislators' voting patterns for five categories of bills: All, Budget General Interest (BGI), Budget Special Interest (BSI), Regulatory General Interest (RGI), and Regulatory Special Interest (RSI). We present two measures of voting patterns: deviation of the legislator's vote from the party ($VoteDev$) and probability of legislators' votes based on DW-NOMINATE methodology ($VoteProb$, see Poole and Rosenthal, 1985). In Panel C, we provide summary statistics on federal assistance and local economic performance, with federal assistance and GDP amounts reported in millions of dollars and employment amounts reported in thousands.

Panel A: Elections

	Mean	SD	Median	25th %ile	75th %ile	N
All decades						
RedistMargin (c)	0.179	0.406	0.197	-0.130	0.435	735
RedistWin (c)	0.660	0.474	1.000	0.000	1.000	735
IncumbMargin ($c+2$)	0.343	0.268	0.315	0.174	0.474	695
2000s & 10s						
RedistMargin (c)	0.178	0.414	0.279	-0.187	0.450	368
RedistWin (c)	0.649	0.478	1.000	0.000	1.000	368
IncumbMargin ($c+2$)	0.382	0.263	0.366	0.236	0.500	355

Panel B: Congressional Voting

	Pre		Post		Δ		
	Mean	SD	Mean	SD	Mean	SD	N
VoteDevAll	0.11	0.04	0.12	0.06	0.0141	0.0625	378
VoteProbAll	87.89	4.94	88.21	5.05	0.3189	4.5783	378
VoteDevBGI	0.09	0.05	0.10	0.08	0.0106	0.0512	378
VoteProbBGI	92.57	5.49	92.68	6.12	0.1098	4.9038	378
VoteDevBSI	0.13	0.05	0.14	0.05	0.0115	0.0564	378
VoteProbBSI	87.87	5.25	87.36	4.93	-0.5124	4.6641	378
VoteDevRGI	0.14	0.06	0.13	0.17	-0.0081	0.1556	364
VoteProbRGI	90.61	6.48	92.68	12.99	1.9695	11.3675	376
VoteDevRSI	0.15	0.06	0.13	0.08	-0.0164	0.0808	377
VoteProbRSI	87.07	6.12	89.27	6.71	2.2010	6.0930	377

Panel C: Distribution and Economic Performance

	Pre		Post		Δ log		
	Mean	SD	Mean	SD	Mean	SD	N
ProjGrants	105.5	174.1	86.3	160.5	-0.2616	0.3978	197
FormulaGrants	636.9	2,241.0	707.9	2,572.8	1.9653	1.7640	189
DirLoans	11.7	42.1	7.8	29.1	-0.1733	1.5138	317
GuarLoans	5.8	13.0	8.4	20.7	0.0363	0.8959	358
Contracts	727.1	1,758.1	712.8	1,806.1	0.0434	0.5366	368
DirPayments	62.8	96.8	66.8	81.8	0.0870	0.5065	357
GDP	34,073.7	10,659.6	32,912.5	11,153.1	0.0400	0.0360	197
Emp	249.0	59.9	251.8	61.2	0.0132	0.0290	358
Earn	16.8	20.8	18.0	22.4	0.0526	0.0324	358
Hires	53.6	20.1	52.1	15.5	-0.0079	0.1273	358
Seps	52.3	18.9	50.6	15.5	-0.0210	0.0929	358

Table III: Narrow Redistrictor Losses and Election Outcomes

This table presents the regression results from estimating Equation 1 on two election outcomes: incumbent win margin and winner win margin in the election after the year of the U.S. Census ($c+2$). Each RDD regression uses two independent cubic polynomial splines of the pre-redistricting vote margin as the "forcing" variables for districts where the redistricting party won and lost the pre-redistricting election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. The regression sample in the first two columns includes all vetoproof congressional districts in the 2000 and 2010 Census cycles and, in the last two columns, vetoproof congressional districts in the 2000 and 2010 Census cycles where the incumbent party retained power in the pre-redistricting election. Each regression is performed at the district-decade level, controls for all demographic and socioeconomic variables described in Figure V, and includes decade fixed effects. Standard errors are clustered at the district level.

	All Districts		Same Party	
	(1) IncumbMargin	(2) WinnerMargin	(3) IncumbMargin	(4) WinnerMargin
RedistWin	0.194**	0.158**	0.349**	0.322**
	(0.0872)	(0.0728)	(0.143)	(0.130)
R^2	0.274	0.306	0.248	0.274
Observations	407	407	355	355

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table IV: Narrow Redistrictor Losses and Pre-Redistricting District Characteristics

This table presents the regression results from estimating Equation 1 on district characteristics measured in the year of the U.S. Census (c). These district-level characteristics are control variables in our primary analyses. The characteristics are: natural log of population, median age of population, percent of population that is male, percent of population that is non-Hispanic white, natural log of median house value, and percent of labor force that is unemployed. Each RDD regression uses two independent cubic polynomial splines of the pre-redistricting vote margin as the "forcing" variables for districts where the redistricting party won and lost the pre-redistricting election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. In Panel A, all vetoproof congressional districts in the 2000 and 2010 Census cycles are included in regressions. In Panel B, only vetoproof congressional districts in the 2000 and 2010 Census cycles where the incumbent party retained power in the pre-redistricting election are included in regressions. Each regression is performed at the district-decade level and includes decade fixed effects. Standard errors are clustered at the district level.

Panel A: All Districts

	(1) lnPop	(2) MedAge	(3) PctMale	(4) PctWhite	(5) lnHouseVal	(6) UnempRate
RedistWin	-0.0360	-0.181	-0.000463	-0.0457	-0.0346	0.00715
	(0.0369)	(1.281)	(0.00234)	(0.0531)	(0.181)	(0.00677)
R^2	0.231	0.129	0.106	0.179	0.104	0.320
Observations	428	428	428	428	428	428

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Same Party

	(1) lnPop	(2) MedAge	(3) PctMale	(4) PctWhite	(5) lnHouseVal	(6) UnempRate
RedistWin	-0.0234	-0.411	-0.00259	-0.0689	0.0607	0.00948
	(0.0574)	(1.865)	(0.00378)	(0.0762)	(0.231)	(0.0108)
R^2	0.237	0.103	0.112	0.176	0.126	0.322
Observations	371	371	371	371	371	371

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table V: Narrow Redistrictor Losses and Congressional Voting

This table presents the regression results from estimating Equation 1 on changes from the pre-redistricting term ($c-1$ to c) to the post-redistricting term ($c+1$ to $c+2$) in U.S. House of Representatives legislator voting patterns. In Panel A, a legislator's voting pattern in a term is measured as *VoteDev*, defined as the within-term mean absolute difference of the legislator's votes from the mean votes of their party. In Panel B, the legislator's voting pattern is measured as *VoteProb*, defined as the within-term mean of the estimated probability of each vote, based on the DW-NOMINATE methodology (see Poole and Rosenthal, 1985). In each panel, we present regression results for legislator votes for all bills (All), budget general interest bills (BGI), budget special interest bills (BSI), regulatory general interest bills (RGI), and regulatory special interest bills (RSI). Each RDD regression uses two independent cubic polynomial splines of the pre-redistricting vote margin as the "forcing" variables for districts where the redistricting party won and lost the pre-redistricting election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. The regression sample includes all vetoproof congressional districts in the 2000 and 2010 Census cycles where the incumbent party retained power in the pre-redistricting election. Each regression is performed at the district-decade level, controls for all demographic and socioeconomic variables described in Figure V, and includes decade fixed effects. Standard errors are clustered at the district level.

Panel A: Δ VoteDev

	(1) All	(2) BGI	(3) BSI	(4) RGI	(5) RSI
RedistWin	-0.0273**	-0.0112	-0.0306*	-0.0304	-0.0901***
	(0.0114)	(0.0152)	(0.0181)	(0.0858)	(0.0270)
R^2	0.864	0.439	0.711	0.555	0.641
Observations	378	378	378	364	377

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Δ VoteProb

	(1) All	(2) BGI	(3) BSI	(4) RGI	(5) RSI
RedistWin	2.567**	-2.032	2.619	-0.463	4.618***
	(1.163)	(1.621)	(1.777)	(6.632)	(1.757)
R^2	0.762	0.522	0.596	0.441	0.559
Observations	378	378	378	376	377

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table VI: Narrow Redistrictor Losses and Federal Assistance

This table presents the regression results from estimating Equation 1 on changes from the pre-redistricting term ($c-1$ to c) to the post-redistricting term ($c+1$ to $c+2$) in the natural logs of seven federal assistance spending amounts: project grants, formula grants, total grants, direct loans, guaranteed loans, contracts, and direct payments. Each RDD regression uses two independent cubic polynomial splines of the pre-redistricting vote margin as the "forcing" variables for districts where the redistricting party won and lost the pre-redistricting election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. The regression sample includes all vetoproof congressional districts in the 2000 and 2010 Census cycles where the incumbent party retained power in the pre-redistricting election. Each regression is performed at the district-decade level, controls for all demographic and socioeconomic variables described in Figure V, and includes decade fixed effects. Standard errors are clustered at the district level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ΔlnProjGrants	ΔlnFormulaGrants	ΔlnGrants	ΔlnDirLoans	ΔlnGuarLoans	ΔlnContracts	ΔlnDirPayments
RedistWin	-0.567**	-1.031	-0.307*	0.650	-0.961***	0.114	0.0426
	(0.268)	(1.458)	(0.183)	(0.825)	(0.288)	(0.286)	(0.261)
R^2	0.153	0.144	0.053	0.041	0.390	0.147	0.162
Observations	197	189	370	317	358	368	357

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table VII: Narrow Redistrictor Losses and Local Economic Performance

This table presents the regression results from estimating Equation 1 on changes from the pre-redistricting term ($c-1$ to c) to the post-redistricting term ($c+1$ to $c+2$) in the natural logs of local economic performance measures: GDP, private-sector employment, private-sector wages, and private-sector hires. Each RDD regression uses two independent cubic polynomial splines of the pre-redistricting vote margin as the "forcing" variables for districts where the redistricting party won and lost the pre-redistricting election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. The regression sample includes all vetoproof congressional districts in the 2000 and 2010 Census cycles where the incumbent party retained power in the pre-redistricting election. Each regression is performed at the district-decade level, controls for all demographic and socioeconomic variables described in Figure V, and includes decade fixed effects. Standard errors are clustered at the district level.

	(1) ΔlnGDP	(2) ΔlnEmp	(3) ΔlnEarn	(4) ΔlnHires
RedistWin	-0.0281	-0.00601	-0.00984	-0.0680**
	(0.0220)	(0.00929)	(0.0112)	(0.0303)
R^2	0.223	0.324	0.199	0.747
Observations	197	358	358	358

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table VIII: Counterfactual Tests using Non-Redistricting States and Non-Redistricting Years This table presents the regression results from estimating Equation 1 using states without partisan redistricting in Panel A and using 2004, 2006, 2014, and 2016 as pre-redistricting election years in Panel B. In Panel A, we define states where there is only one congressional district, a bipartisan or non-partisan commission conducts congressional redistricting, or the redistricting party does not have vetoproof ability to conduct redistricting as counterfactual redistricting states and define the regression sample as districts where the incumbent party retained power in the pre-redistricting election in those states. In Panel B, we define c to be mid-decade years (2004, 2006, 2014, and 2016) instead of 2000 and 2010 as counterfactual pre-redistricting election years and define the regression sample as vetoproof congressional districts for each pseudo-redistricting cycle where the incumbent party retained power in the pre-redistricting election. Each table presents pseudo-redistricting results on incumbent win margin in the first post-redistricting election ($c+2$) and changes from the pre-redistricting term ($c-1$ to c) to the post-redistricting term ($c+1$ to $c+2$) in vote deviation in all bills, project grants, and guaranteed loans. Each RDD regression uses two independent cubic polynomial splines of the pre-redistricting vote margin as the "forcing" variables for districts where the redistricting party won and lost the pre-redistricting election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. Each regression is performed at the district-pre-redistricting election year level and includes redistricting cycle fixed effects. Standard errors are clustered at the district level.

Panel A: Non-Redistricting States

	(1) IncumbMargin	(2) ΔVoteDevAll	(3) ΔlnProjGrants	(4) ΔlnGuarLoans
RedistWin	0.147	0.0639**	0.0932	0.506
	(0.112)	(0.0274)	(0.335)	(0.497)
R^2	0.405	0.680	0.063	0.240
Observations	107	111	88	107

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Non-Redistricting Years

	(1) IncumbMargin	(2) ΔVoteDevAll	(3) ΔlnProjGrants	(4) ΔlnGuarLoans
RedistWin	0.0392	-0.0192	-0.0943	-0.617
	(0.0820)	(0.0136)	(0.299)	(1.283)
R^2	0.372	0.287	0.307	0.355
Observations	719	720	410	544

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table IX: Long-Run Effects of Narrow Redistrictor Losses

This table presents the regression results from estimating Equation 1 on six long-run outcomes: pre-redistricting term incumbent party win margin, incumbent win margin, vote deviation for all bills, natural log of project grants, and natural log of guaranteed loans. For the first two columns, the outcomes are the win margins in each post-redistricting election and, for the remaining columns, the outcomes are changes from the pre-redistricting term to each post-redistricting term. Each RDD regression uses two independent cubic polynomial splines of the pre-redistricting vote margin as the "forcing" variables for districts where the redistricting party won and lost the pre-redistricting election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. The four rows present this discontinuous difference in impact for regressions for each of the four post-redistricting terms, $\tau \in 1, 2, 3, 4$. For instance, the second row, $\tau = 2$, presents the discontinuous difference in the impact of the win margin on the second post-redistricting election $(c+4)$ for the first two columns and on the change from the pre-redistricting term to the second post-redistricting term ($c+3$ to $c+4$) for the other columns. The regression sample includes all vetoproof congressional districts in the 2000 and 2010 Census cycles where the incumbent party retained power in the pre-redistricting election. Each regression is performed at the district-decade level, controls for all demographic and socioeconomic variables described in Figure V, and includes decade fixed effects. Standard errors are clustered at the district level.

Term	(1) PartisanMargin	(2) IncumbMargin	(3) ΔVoteDevAll	(4) ΔlnProjGrants	(5) ΔlnGuarLoans
$\tau = 1$	0.190**	0.192**	-0.027**	-0.567**	-0.961***
	(0.083)	(0.082)	(0.011)	(0.268)	(0.288)
$\tau = 2$	0.230**	0.098	0.014	0.508	-1.091***
	(0.094)	(0.070)	(0.020)	(0.490)	(0.353)
$\tau = 3$	0.147	-0.053	0.032*	0.598	-0.858
	(0.121)	(0.077)	(0.017)	(0.527)	(0.551)
$\tau = 4$	0.278***	0.041	0.019	-0.397	0.461
	(0.105)	(0.076)	(0.028)	(0.663)	(1.338)

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table X: Alternative Specifications

This table presents the regression results from estimating Equation 1 using alternative specifications for the regression discontinuity methodology on six key outcomes: incumbent win margin, vote deviation for all bills, project grants, guaranteed loans, private-sector hires, and GDP. For the first column, the outcome is the incumbent win margin in the election in the post-redistricting year ($c+2$) and, for the remaining columns, the outcomes are changes from the pre-redistricting term ($c-1$ to c) to the post-redistricting term ($c+1$ to $c+2$). The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. The three rows provide the estimated impact using three alternative RD specifications: local linear RDD, global quadratic (2nd degree) polynomial RDD, and global quartic (4th degree) polynomial RDD. The first row provides estimates from a local linear RDD, where we limit the sample to observations in which the "forcing" variable falls within the IK bandwidth (Imbens and Kalyanaraman, 2012) and allow for independent linear relationships between the "forcing" variable and the outcome variable on either side of the RD threshold. The second and third rows present the impact of the win margin using RDD regressions with two independent quadratic and quartic polynomial splines, respectively, of the pre-redistricting vote margin as the "forcing" variables for districts where the redistricting party won and lost the pre-redistricting election. The regression sample includes all vetoproof congressional districts in the 2000s and 2010s where the incumbent party retained power in the pre-redistricting election. Each regression is performed at the district-decade level, controls for all demographic and socioeconomic variables described in Figure V, and includes decade fixed effects. Standard errors are clustered at the district level.

	(1)	(2)	(3)	(4)	(5)	(6)
RDD Specification	IncumbMargin	ΔVoteDevAll	ΔlnProjGrants	ΔlnGuarLoans	ΔlnHires	ΔlnGDP
Local Linear	0.370***	-0.018**	-0.608**	-0.903***	-0.051*	-0.131***
	(0.111)	(0.008)	(0.275)	(0.223)	(0.028)	(0.029)
Global 2nd Deg Poly	0.170*	-0.028***	-0.233	-0.582***	-0.045**	-0.014
	(0.103)	(0.008)	(0.202)	(0.215)	(0.022)	(0.015)
Global 4th Deg Poly	0.427**	-0.026*	-0.672*	-0.818**	-0.081**	-0.058*
	(0.170)	(0.015)	(0.353)	(0.321)	(0.041)	(0.031)

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

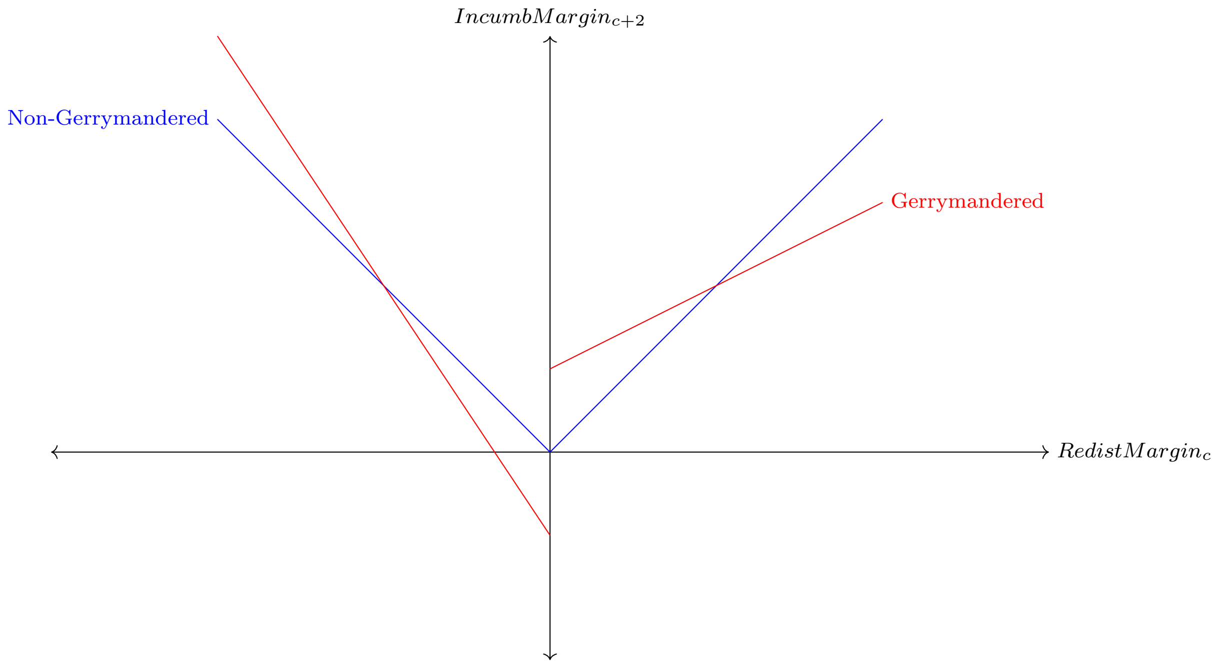
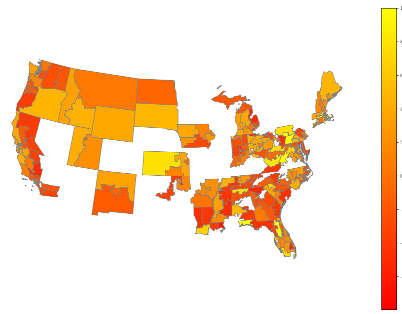
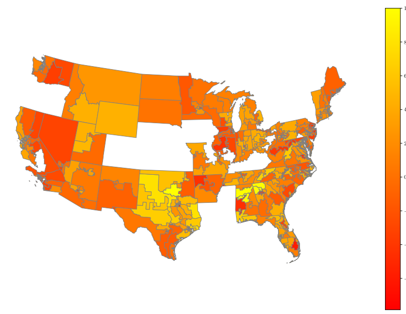


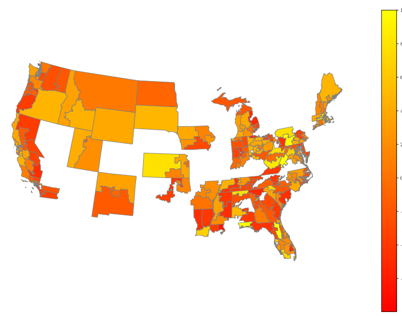
Figure I: Predicted Incumbent Vote Margins vs. Redistricter Votes
This figure represents the predicted relationship between the margin of victory in a district for the redistricting party in the pre-redistricting election, represented on the horizontal axis, and the margin of victory for the incumbent candidate in the same district in the post-redistricting election, represented on the vertical axis.



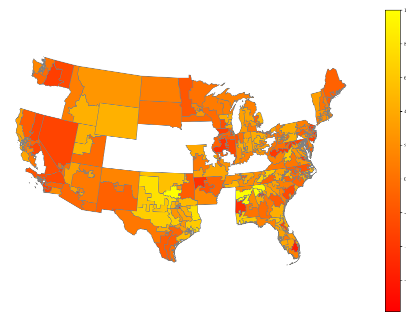
(a) 1980 elections



(b) 1990 elections

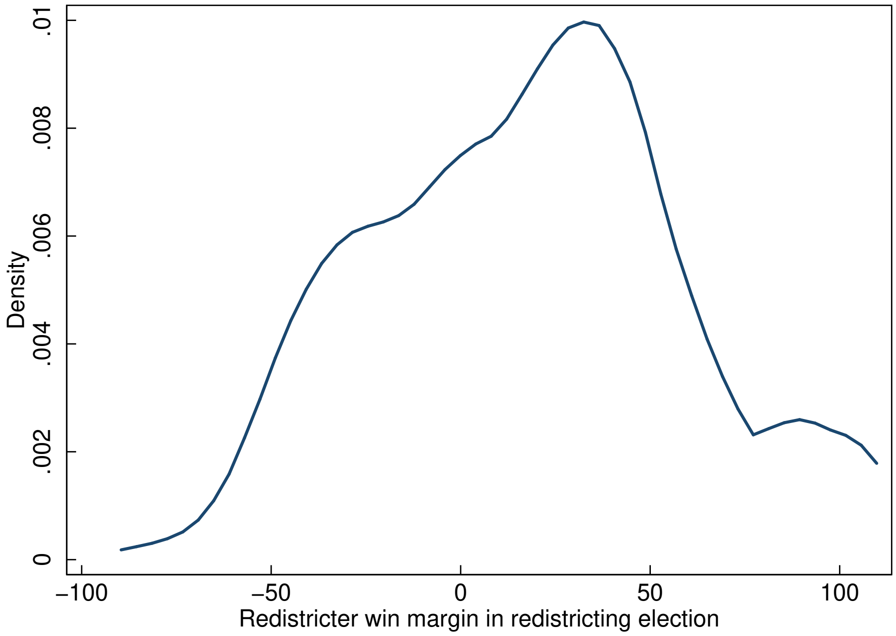


(c) 2000 elections

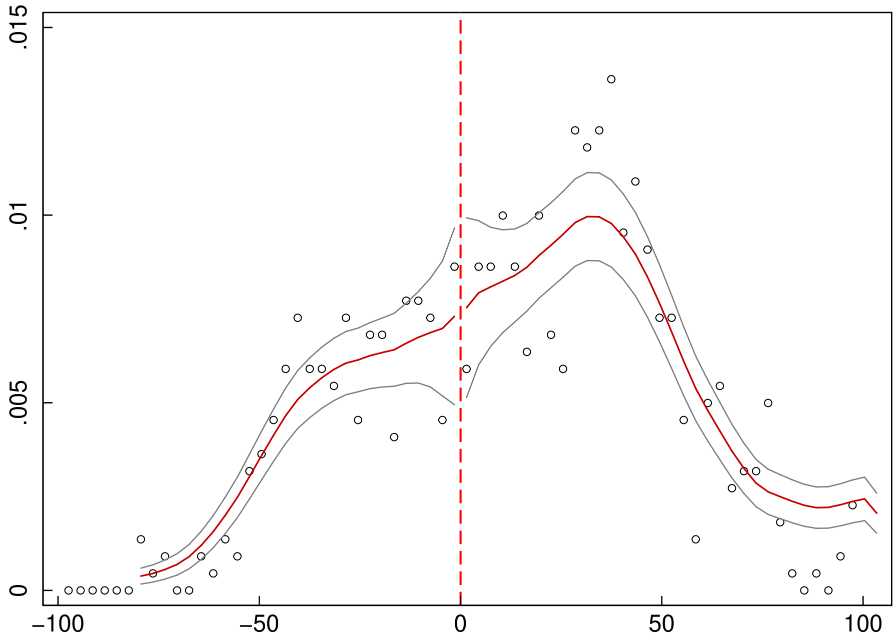


(d) 2010 elections

Figure II: Redistrictor Electoral Margins for U.S. House of Representatives Elections
These figures present the electoral margins for the redistricting party in the 1980, 1990, 2000, and 2010 U.S. House of Representatives elections. Yellower districts are those where the redistricting party won with a larger margin of votes. Redder districts are those where the non-redistricting party won with a larger margin of votes. In the middle between yellow and red, orange represents closely-contested elections. Districts from states with non-partisan gerrymandering or states not controlled by a unified state legislature are left blank.

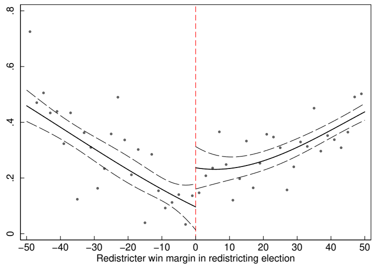


(a) Kernel Density Plot

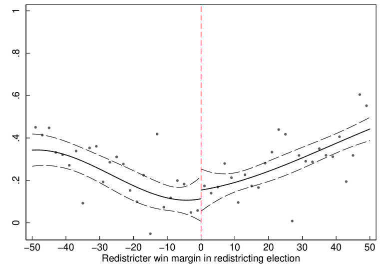


(b) McCrary Density Smoothness Test

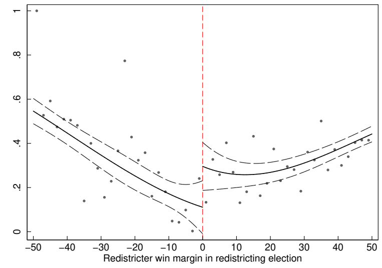
Figure III: Distribution Density of Pre-Redistricting Election Outcomes
These figures illustrate density smoothness for the redistricting party win margin (*RedistWin*) for all veto-proof congressional districts in 1980, 1990, 2000, and 2010. Figure (a) presents kernel density plots of *RedistWin* and figure (b) presents the McCrary density plot of *RedistWin* (see McCrary, 2008).



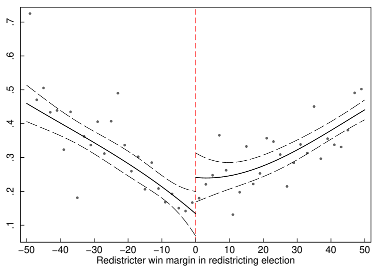
(a) IncumbMargin (all decades)



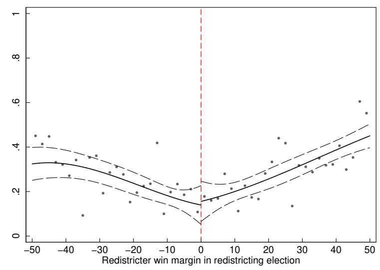
(b) IncumbMargin (1980/1990)



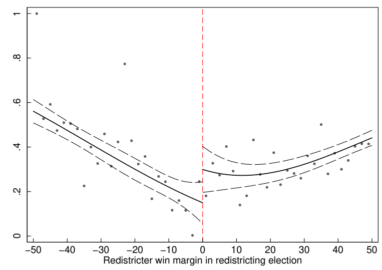
(c) IncumbMargin (2000/2010)



(d) WinnerMargin (all decades)



(e) WinnerMargin (1980/1990)

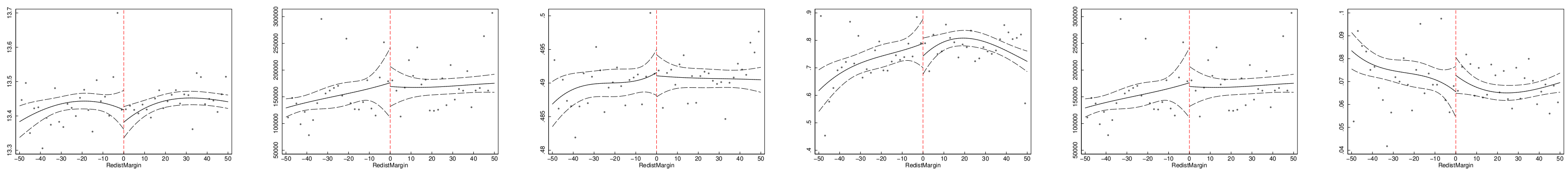


(f) WinnerMargin (2000/2010)

Figure IV: Redistrictor Electoral Margin and Post-Redistricting Election Outcomes

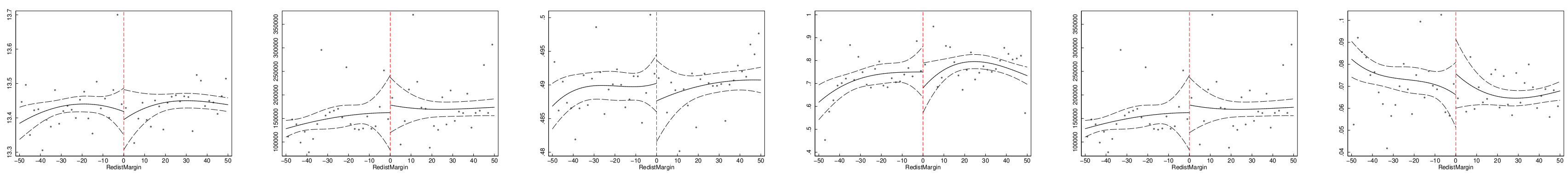
These figures illustrate the results from estimating Equation 1 on incumbent win margin (figures (a), (b), and (c)) and winner win margin (figures (d), (e), and (f)). The figures present results from vetoproof congressional districts from all decades (figure (a) and (d)), from the 1980s and 1990s (figures (b) and (e)), and from the 2000s and 2010s (figures (c) and (f)). The solid lines represent the mean estimated change in election outcomes for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. These estimates are based on regressions with decade fixed effects and standard error clustering at the district level.

All Districts



(a) lnPop (b) MedAge (c) PctMale (d) PctWhite (e) lnHouseVal (f) Unemp

Same Party

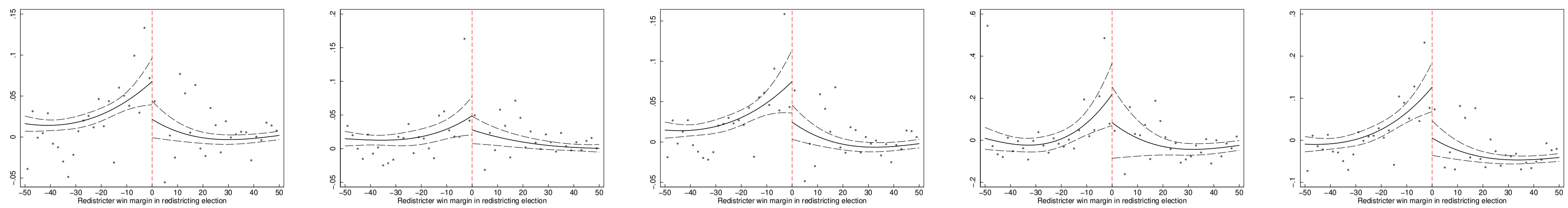


(g) lnPop (h) MedAge (i) PctMale (j) PctWhite (k) lnHouseVal (l) Unemp

Figure V: Narrow Redistrictor Losses and Pre-Redistricting District Characteristics

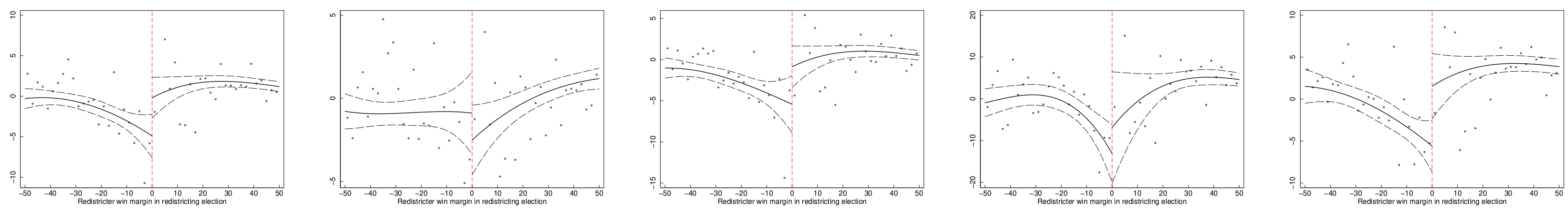
These figures illustrate the results of estimating Equation 1 on district characteristics measured in the pre-redistricting election year (c). The district characteristics are: natural log of district population, median age, percent of population that is male, percent of population that is non-Hispanic white, natural log of median house value, and percent of labor force that is unemployed. The figures in the top row present results from all vetoproof congressional districts in all decades and the figures in the bottom row present results from vetoproof congressional districts in the 2000s and 2010s where the incumbent party retained power in the pre-redistricting election. The solid lines represent the mean estimated outcome for every level of redistrictor win margin in the pre-redistricting election. The dashed lines represent the 95% confidence intervals for the estimated outcome. These estimates are based on regressions with decade fixed effects and standard errors clustered at the district level.

Δ VoteDev



(a) All bills (b) BGI (c) BSI (d) RGI (e) RSI

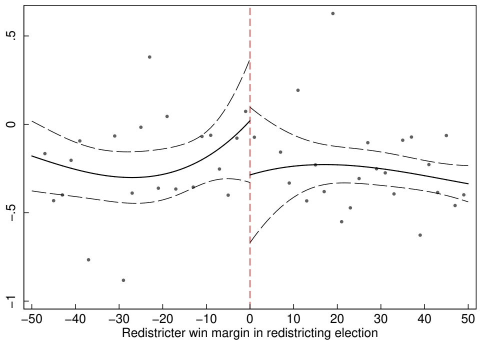
Δ VoteProb



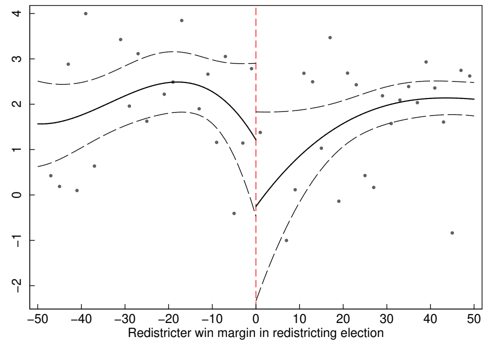
(f) All bills (g) BGI (h) BSI (i) RGI (j) RSI

Figure VI: Narrow Redistrictor Losses and Congressional Voting

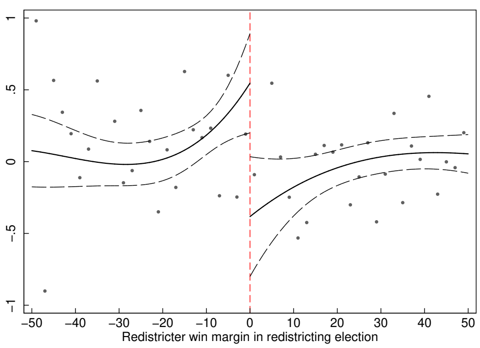
These figures illustrate the results of estimating Equation 1 on changes from the pre-redistricting term ($c-1$ to c) to the post-redistricting term ($c+1$ to $c+2$) in U.S. House of Representatives legislator voting patterns. The top row of figures (figures (a) through (e)) measure voting patterns as *VoteDev*, defined as the within-term mean absolute difference of each legislator's votes from the mean votes of their party. The bottom row of figures (figures (f) through (j)) measure voting patterns as *VoteProb*, defined as the within-term mean of the probability of each of the legislator's votes, based on DW-NOMINATE methodology (see Poole and Rosenthal, 1985). Each column represents results for a set of bills based on their subject areas: all bills (figures (a) and (f)), budget general interest (figures (b) and (g)), budget special interest bills (figures (c) and (h)), regulatory general interest bills (figures (d) and (i)), and regulatory special interest bills (figures (e) and (j)). The solid lines represent the mean estimated change for every level of redistrictor win margin in the pre-redistricting election. The dashed lines represent the 95% confidence intervals for the estimated change. These estimates are based on regressions for vetoproof congressional districts in the 2000s and 2010s where the incumbent party retained power in the pre-redistricting election. All regressions include decade fixed effects and standard error clustering at the district level.



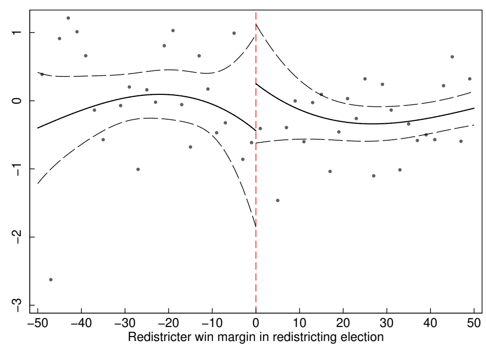
(a) ΔlnProjGrants



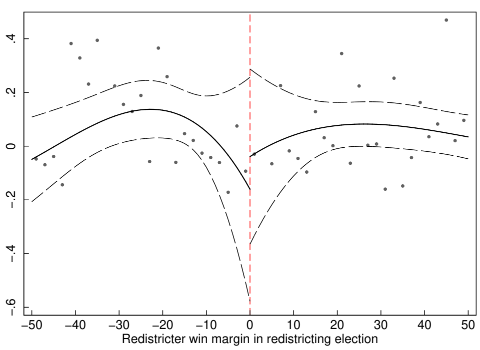
(b) ΔlnFormulaGrants



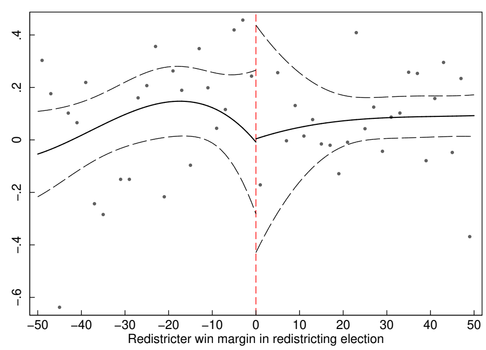
(c) ΔlnGuarLoans



(d) ΔlnDirLoans



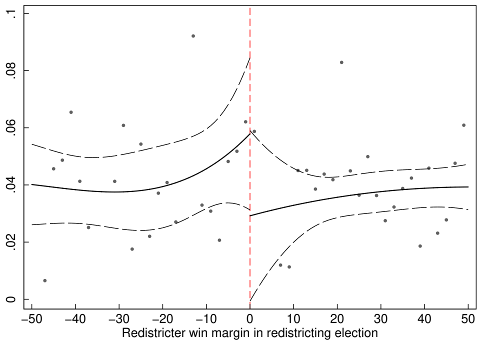
(e) ΔlnContracts



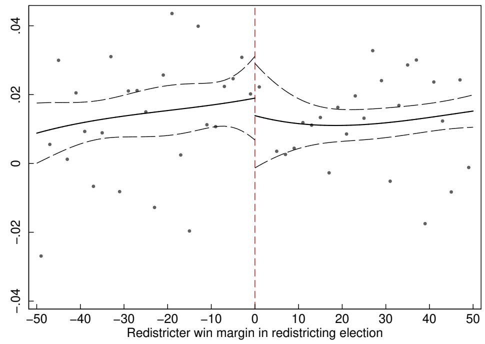
(f) ΔlnDirPayments

Figure VII: Narrow Redistrictor Losses and Federal Assistance

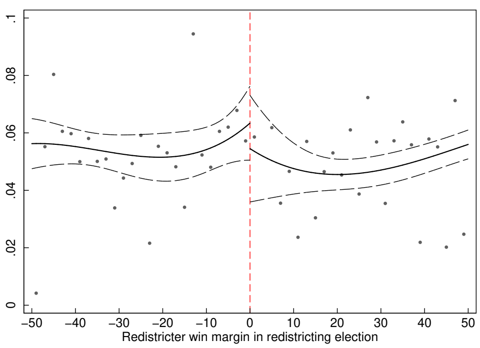
These figures illustrate the results of estimating Equation 1 on changes from the pre-redistricting term ($c-1$ to c) to the post-redistricting term ($c+1$ to $c+2$) in the natural logs of six forms of federal assistance: (a) project grants, (b) formula grants, (c) guaranteed loans, (d) direct loans, (e) contracts, and (f) direct payments. The solid lines represent the mean estimated change for every level of redistrictor win margin in the pre-redistricting election. The dashed lines represent the 95% confidence intervals for the estimated change. These estimates are based on regressions for vetoproof congressional districts in the 2000s and 2010s where the incumbent party retained power in the pre-redistricting election. All regressions include decade fixed effects and standard error clustering at the district level.



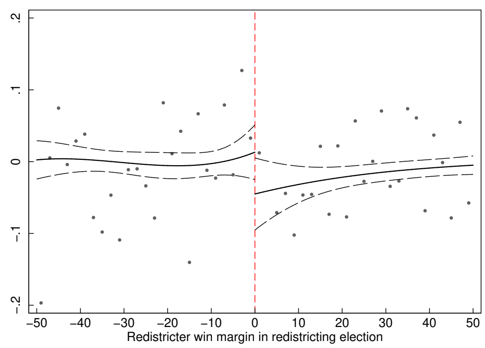
(a) ΔlnGDP



(b) ΔlnEmps



(c) ΔlnEarn



(d) ΔlnHires

Figure VIII: Narrow Redistrictor Losses and Local Economic Outcomes

These figures illustrate the results of estimating Equation 1 on changes from the pre-redistricting term ($c-1$ to c) to the post-redistricting term ($c+1$ to $c+2$) in the natural logs of four economic outcomes: (a) district-level GDP, (b) private-sector employment, (c) private-sector wages, and (d) number of private-sector hires. The solid lines represent the mean estimated change for every level of redistrictor win margin in the pre-redistricting election. The dashed lines represent the 95% confidence intervals for the estimated change. These estimates are based on regressions for vetoproof congressional districts in the 2000s and 2010s where the incumbent party retained power in the pre-redistricting election. All regressions include decade fixed effects and standard error clustering at the district level.