

Adapt or Alienate:

When Voters Change, Do Lawmakers Follow?

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This study investigates whether members of Congress adapt ideologically in response to constituency change induced by redistricting. Leveraging California's independent redistricting process as a plausibly exogenous source of partisan shock, we estimate the effect of large shifts in district partisanship on changes in donor-perceived ideology. Legislators are matched on pre-treatment covariates using Mahalanobis distance, and treatment effects are estimated separately for the 2012 and 2022 election cycles. Across both cycles, we find no evidence that incumbents systematically shift their ideological positioning in response to substantial constituency change. Sensitivity analysis and placebo simulations confirm the robustness of these null results, while power diagnostics indicate that only moderate-to-large effects would be detectable. The findings suggest that redistricting alone does not trigger ideological adaptation and contribute to broader debates about electoral accountability, elite responsiveness, and the limits of institutional reform in shaping legislative behavior.

Keywords: redistricting, constituency change, ideological stability, donor perception, legislative behavior

Introduction

“Representatives and prospective representatives, think about their constituencies because they seek support there. They want to get nominated and elected, then renominated and re-elected. For most members of Congress, most of the time, this electoral goal is primary...And the electoral goal is achieved—first and last—not in Washington but at home” (Fenno 1977, 889). As both Fenno (1977) and Mayhew (1974) argue, reelection is the driving force behind much of legislative behavior. Members cultivate trust, shape their public images, and stake out policy positions with the home district in mind. As districts evolve—through demographic change, redistricting, or partisan realignment—so too do the incentives members face. A shifting constituency can redefine the electoral coalition a legislator must maintain.

While a substantial body of empirical research finds that legislators respond to constituency change—particularly when electoral incentives are strong (e.g., Carson et al. 2007; Crespin 2010; Clinton 2006)—other studies suggest that legislators mainly reflect national party positions, showing limited responsiveness to local preferences (e.g., Ansolabehere, Snyder, and Stewart 2001). Related work further contends that redistricting explains little of the polarization observed in Congress (e.g., McCarty, Poole, and Rosenthal 2009). These findings complicate the conventional view, rooted in Fenno (1977) and Mayhew (1974), that responsiveness to constituents is not merely a normative ideal of representation but a strategic imperative essential to political survival. If reelection incentives compel legislators to adapt, why does ideological divergence persist, even after districts shift significantly?

Using a case study of California’s U.S. House districts, this paper examines how members of Congress respond to constituency change induced by federal redistricting. California offers a strategic setting for this analysis, as its maps are drawn by a non-partisan citizens’ commission, reducing concerns about partisan gerrymandering, allowing more precise identification of shifts in district composition. Leveraging changes between the 2020 and 2022 election cycles, the study compares outcomes across two periods to assess whether legislators adjust their ideological positions in response to constituency shifts caused by redistricting. Because all districts are redrawn and all legislators experience some degree of compositional change, the design does not rely on a stable control group but instead examines how individual ideological scores evolve to changes in district partisanship.

The central aim of this study is to examine whether and how legislators adjust their ideological behavior in response to changes in constituency composition

following redistricting. By analyzing member behavior in California's U.S. House districts before and after redistricting in district boundaries, the study contributes to broader debates about the conditions under which electoral accountability drives legislative adaptation. The remainder of the paper proceeds as follows: a review of relevant literature, a theoretical framework, a description of the data and research design, presentation of results, and a discussion of the findings and their implications.

Literature

A substantial body of research has examined whether and how legislators adjust their behavior in response to constituency change, particularly when electoral incentives are strong. The dominant view, rooted in classic work by Fenno (1977) and Mayhew (1974), suggests that reelection-seeking legislators tailor their policy positions to fit the preferences of their constituents. Redistricting, by reshaping the geographic and partisan composition of a district, represents a key test of this theory: when new voters enter a district, the electoral coalition a legislator must maintain may shift, prompting ideological recalibration. Yet, some scholarship posits that party affiliation, polarization, and institutional constraints limit such responsiveness. In line with this paper's argument, we contend that while constituency change creates clear incentives for adaptation, legislators' responsiveness depends on the salience of electoral threat and the degree of ideological flexibility they possess. To develop this claim, we draw on three strands of scholarship: studies of representation and roll-call responsiveness, research on redistricting and polarization, and debates over party loyalty and electoral accountability.

Representation and Roll-Call Responsiveness

"The majority of theorists argue that the representative must do what is best for those in his charge, but that he must do what he thinks best, using his own judgment and wisdom, since he is chosen to make decisions for (that is, instead of) his constituents. But a vocal minority maintains that the representative's duty is to accurately reflect the wishes and opinions of those he represents. Anything else they consider a mockery of true representation" (Pitkin 1967, 4). This distinction outlines two dominant models of legislative behavior: the *trustee*, who exercises independent judgment, and the *delegate*, who follows constituent preferences. Kuklinski and Elling (1977) offer one of the earliest empirical applications of this framework, showing that legislators who adopt a delegate orientation are more likely to align with constituent opinion, especially on salient issues. Their work

underscores that representation depends not only on electoral conditions, but also on how legislators conceive of their roles. Together, these contributions establish the conceptual foundation for understanding representation as shaped by both external incentives and internal role orientations.

While Pitkin's framework and its empirical extensions emphasize how legislators conceive of their roles, another line of research focuses on how they navigate the demands of electoral survival. This work examines how members manage relationships with different segments of their constituency to maintain political support. In his seminal fieldwork-based study, Fenno (1977; 1978) shows that legislators view their constituencies as nested circles (geographic, re-election, primary, and personal), with each carrying distinct expectations. Legislators adapt their behavior accordingly, devoting significant effort to cultivating trust and shaping what Fenno terms their "*home style*." From a different angle, Mayhew (1974) starts with the premise that legislators are single-minded seekers of reelection and argues that nearly all behavior (e.g., position-taking, credit-claiming, advertising) serves that end. Together, these accounts suggest that even when members appear to act independently in Washington, their behavior is primarily shaped by the goal of reelection and by how they manage their image with voters at home.

While legislators often position themselves strategically to maintain electoral viability, research shows that this responsiveness is conditional. Scholars have long recognized that legislators are more likely to reflect constituent preferences when the stakes are visible to the public. Both Miller and Stokes (1963) and Wlezien and Soroka (2016) show that salience mediates representation: legislators tend to have a stronger response on prominent issues (e.g., civil rights), but diverge on lower-salience domains—those considered "high politics" (e.g., foreign policy or administrative reform). This is of importance, as these scholars suggest that electoral incentives do not operate uniformly across the policy space; instead, public attentiveness creates asymmetries in when and where representation occurs.

In this vein, research shows that legislators often prioritize the preferences of partisan subconstituencies over those of the broader electorate. Rather than using the district median as their reference point, members tend to respond to co-partisans—those most likely to support, monitor, and mobilize for them. Echoing Fenno's (1977; 1978) nested constituencies, Clinton (2006) finds that majority-party legislators are especially responsive to same-party voters. Wlezien and Soroka (2016) similarly emphasize that representation is selective, shaped by institutional arrangements that amplify some voices over others. These patterns raise concerns about representational equity: when legislators systematically over-represent partisan supporters, others are effectively marginalized. Matsusaka (2024) deepens

this critique by distinguishing between responsiveness and congruence, showing that legislators may appear to follow public opinion while remaining ideologically distant from most constituents.

At the same time, while representation is often filtered through partisanship and salience, legislators do respond when the electoral costs of misalignment become high. Canes-Wrone, Brady, and Cogan (2002) find that members who stray too far from district preferences (even in ostensibly safe seats) are more likely to face electoral backlash. Gershtenson and Plane (2007) add that it is not just objective ideological distance that matters, but also how constituents perceive that distance, underscoring the symbolic and communicative dimensions of representation. Taken together, this literature shows that legislative responsiveness is shaped by how legislators conceive of their roles, the visibility and salience of issues, the preferences of partisan sub-constituencies, and the perceived electoral risks of misalignment—resulting in representation that is strategic, uneven, and often biased toward politically influential segments of the electorate.

Redistricting and Polarization

A growing body of research examines how redistricting and polarization intersect to shape legislative behavior. By introducing new voters and altering strategic incentives, redistricting disrupts established electoral relationships and can weaken incumbents' electoral security. For example, Desposato (2003) finds that newly drawn districts diminish the “personal vote,” increasing vulnerability as legislators face unfamiliar constituencies. Similarly, perhaps the study most closely aligned with this project is Carson et al. (2007), who link redistricting to rising polarization, showing that members in redrawn districts tend to vote more ideologically than those in stable seats. However, their reliance on DW-NOMINATE complicates interpretation, as the measure's linearity may obscure meaningful change (for an explanation, see Miler 2015). Taken together, this research highlights how redistricting reshapes electoral environments, though direct evidence of ideological repositioning remains limited.

In a similar vein, but shifting from district composition to the process of mapmaking, scholars have examined how who draws the lines influences political outcomes. For example, Grainger (2015) finds that legislator-drawn maps tend to produce more polarized legislatures than those drawn by independent commissions. He attributes this to strategic district design: partisan actors often draw safer seats that insulate incumbents from electoral competition, reducing incentives to moderate and encouraging ideological rigidity. In contrast, commissions are more likely to produce competitive districts, thereby preserving some electoral pressure for responsiveness. However, McCarty, Poole, and Rosenthal (2009) challenge the

idea that redistricting is a primary driver of congressional polarization. They argue that within-district divergence (where Democrats and Republicans represent the same constituencies very differently), alongside national partisan sorting, better explains the ideological divide.

At the same time, assessing the consequences of redistricting also raises significant measurement concerns. Matsusaka (2024) critiques the widespread use of responsiveness—the statistical correlation between district opinion and legislative behavior, as a proxy for representation. He argues that responsiveness can exist even when a legislator’s position is far from the district median, meaning that congruence, or substantive alignment, may be lacking. This distinction matters especially in studies of redistricting, where shifts in roll-call voting may reflect strategic adaptation rather than genuine ideological change. Furthermore, many studies rely on DW-NOMINATE scores, which impose a linear structure on legislator behavior and may understate movement that falls outside its spatial assumptions (see Miler 2015). As a result, findings based on these measures risk over/understating the representational consequences of redistricting and obscuring whether legislators are truly responding to constituency change.

Party Loyalty and Electoral Accountability

A central concern in this literature is whether legislators pay an electoral price for prioritizing party over constituents. Building on Mayhew’s (1974) premise that legislators are “single-minded seekers of reelection” and Fenno’s (1977) insight that members cultivate trust within nested constituencies, scholars have explored the potential costs of visible party loyalty. Carson et al. (2010) find that party unity (voting consistently with party leadership on salient and divisive issues) can be electorally costly, particularly when such behavior diverges from district preferences. These penalties appear most sensitive in competitive or ideologically mixed districts, where overt partisanship may be read as unresponsiveness.

Building on this, Crespin (2009) argues that electoral accountability is often conditional on the visibility of legislative behavior. Legislators tend to align with constituent preferences on salient final passage votes, where public scrutiny is higher, but routinely defer to party leadership on low-salience procedural matters. This asymmetry enables members to maintain party cohesion while projecting responsiveness to voters. As a reminder, Fenno (1977) suggests that legislators prioritize certain segments of their constituency when making strategic representational choices. In this light, party loyalty may reflect not defiance of constituents but attentiveness to those most critical for reelection. Yet this view is challenged by scholarship like Ansolabehere, Snyder, and Stewart (2001), who find that candidates overwhelmingly adhere to national party platforms regardless of

district context. Their longitudinal analysis shows little evidence of constituency-driven divergence, suggesting that ideological positioning is largely stable and that adaptation to district preferences is more constrained than traditional models imply.

Taken together, this body of work underscores the conditional nature of electoral accountability. While some legislators appear to calibrate their behavior in response to constituency signals, especially when visibility is high, others remain ideologically consistent even amid changing district demands. This tension highlights the limits of representational responsiveness in an era of strong party polarization.

A Theory of Constituency Change

A foundational assumption in the study of legislative behavior is that members of Congress, as strategic actors motivated by reelection, calibrate their actions to align with constituent preferences (Mayhew 1974; Fenno 1977). A substantial body of empirical research lends support to this view, particularly under conditions of heightened electoral vulnerability. Legislators are more likely to respond to constituency change when electoral incentives are strong and the risks of misalignment are politically costly (Carson et al. 2007; Crespín 2010; Clinton 2006). Yet this perspective is not uncontested. An alternative body of work emphasizes the durability of partisan alignment, showing that many legislators maintain ideological consistency even amid substantial changes in district composition. Studies in this tradition argue that redistricting explains little of the partisan polarization observed in Congress (Ansolabehere, Snyder, and Stewart 2001; McCarty, Poole, and Rosenthal 2009). Complicating matters further, much of the existing research relies on DW-NOMINATE scores, which are ill-suited to detect short-term or strategic shifts in behavior due to their reliance on static spatial assumptions (see Miler 2015). In light of these tensions, we develop a revised account of legislative responsiveness—one that recognizes the electoral constraints members face and foregrounds their capacity for ideological recalibration in the wake of redistricting.

Our theoretical framework builds from, but extends beyond, classical accounts of electoral responsiveness. Mayhew (1974) famously depicts legislators as “single-minded seekers of reelection,” while Fenno (1977) shows how members cultivate trust within concentric constituencies, tailoring their behavior to those most central to their political survival. These foundational insights suggest that responsiveness is not only strategic but selective. We argue that redistricting—by altering the constituency landscape and heightening electoral uncertainty—creates

a unique inflection point in which ideological flexibility becomes both possible and, for many members, electorally advantageous. We term this the *adaptive legislator model*: an account that assumes legislators respond to district change when the electoral stakes are high. This perspective stands in direct tension with the *party-constrained* view (e.g., Ansolabehere, Snyder, and Stewart 2001), which sees ideological stability as the norm and district change as largely inconsequential. By contrast, we assert that redistricting generates a high-information, high-risk environment—one well-suited for detecting patterns of ideological adjustment, even when subtle or uneven.

Redistricting, in this account, is not merely a background institutional feature but a test of legislative responsiveness. By introducing new voters, disrupting entrenched coalitions, and weakening incumbency advantages, redistricting reshapes the strategic environment in which legislators operate. The resulting uncertainty heightens the incentives for adaptation. Prior work has shown that such electoral shifts can prompt changes in legislative behavior (Desposato 2003; Miler 2015; Carson et al. 2007), but the nature and scope of that change remain difficult to observe with conventional scaling methods. While redistricting does not guarantee ideological movement, it creates precisely the kind of electoral volatility in which responsive behavior is most likely to emerge. At a minimum, it raises the cost of ideological rigidity for members whose new constituencies diverge from prior coalitional bases.

Crucially, we do not claim that adaptation is always deep, durable, or district-wide. The extent to which legislators recalibrate depends on how they weigh the competing demands of party loyalty and electoral accountability. Party-consistent behavior carries reputational costs, particularly in competitive districts where visible partisanship may alienate voters. Carson et al. (2010) demonstrate that party unity votes can provoke electoral backlash, while Crespin (2009) finds that members often bifurcate their behavior—adhering to the party line on low-salience procedural matters but aligning more closely with constituent preferences on high-salience issues. Moreover, as Fenno (1977) reminds us, legislators rarely respond to a median voter in the abstract. Instead, they prioritize core supporters and electorally influential sub-constituencies. Thus, ideological adaptation may be narrow in scope or largely symbolic. Even so, such shifts constitute meaningful evidence of responsiveness. They signal that legislators remain attuned to the risks posed by district change—and are willing to adjust, however subtly, in pursuit of political survival.

H1 (Responsiveness to Constituency Change): *Legislators who experience a greater shift in district partisanship following redistricting will exhibit a corresponding shift in their ideological positioning.*

The Case of California

California presents a unique case for evaluating whether legislators adjust their ideological behavior in response to constituency change. Unlike most U.S. states, where redistricting is controlled by partisan legislatures, California implemented a major institutional reform following the 2010 census: the Voters FIRST Act for Congress. This law extended the authority of the state's Citizens Redistricting Commission (CRC) to congressional districts, mandating that district boundaries be drawn by an independent, nonpartisan body. Notably, the CRC operates under strict criteria that prohibit consideration of incumbents' addresses or partisan outcomes, instead prioritizing contiguity, compactness, and respect for communities of interest. As a result, California's post-2010 redistricting cycles provide a rare opportunity to observe legislator behavior in a context where constituency change is plausibly exogenous rather than strategically engineered.

This institutional feature strengthens the case for California as a natural test of electoral responsiveness. In most states, redistricting is confounded with incumbent protection, party advantage, or racial gerrymandering, each of which complicates causal inference about constituency effects. By contrast, California's redistricting process reduces these concerns. Because the CRC is composed of ordinary citizens selected through an application and screening process, and because its design deliberately insulates it from partisan manipulation, changes to district composition can be treated as externally imposed shocks to a legislator's electoral calculus. This is critical to our theory: if ideological responsiveness is strategic, then legislators should be especially likely to adapt following redistricting in settings where they cannot influence the new lines, and where the composition of their district shifts in ways that increase electoral risk. To leverage this institutional setup, we focus on two redistricting cycles, 2010 and 2020, and the corresponding elections in 2012 and 2022. These years are valuable because they represent the first elections conducted under the newly implemented CRC maps.

In sum, the Californian case offers a unique institutional design, providing a cleaner test of our core argument than most U.S. settings, where redistricting is typically endogenous to partisan strategies. Moreover, by comparing across two redistricting cycles a decade apart, we are able to assess whether patterns of ideological adaptation are consistent over time, or contingent on the political context of a given cycle. If legislators are truly responsive to electoral incentives—as the adaptive legislator model suggests—then constituency change should prompt

observable shifts in behavior. California’s nonpartisan redistricting process provides a critical lens for evaluating this claim.

Data

To evaluate whether legislators adjust their ideological behavior in response to changes in constituency composition, we extend the Database on Ideology, Money in Politics, and Elections (DIME) by focusing on incumbent members of the U.S. House of Representatives from California and linking donor-based ideology scores to redistricting-induced constituency shifts. Our analysis centers on two redistricting cycles—2010 and 2020—and the congressional elections that immediately followed in 2012 and 2022, respectively. For each cycle, we identify the set of incumbents who sought reelection under the newly implemented district maps drawn by California’s independent Citizens Redistricting Commission. While all incumbents were affected by redistricting, our treatment variable captures the magnitude and direction of constituency change, measured as the shift in district partisanship before and after redistricting. Specifically, we operationalize this as the change in Cook Partisan Voting Index (PVI), which reflects the extent to which a legislator’s district became more or less favorable to their party. This design enables us to assess whether, and to what degree, ideological positioning varies in response to exogenous changes in electoral incentives.

To measure ideological positioning, we rely on Bonica’s (2014) campaign finance-based scores (CFscores), also known as COIN scores. These estimates use the pattern of campaign contributions to place candidates within a common ideological space, capturing how politically engaged donors perceive the positions of officeholders. Unlike vote-based measures such as DW-NOMINATE, which are shaped by party agenda control and assume smooth, linear ideological change, COIN scores are more sensitive to short-term or strategic adjustments. This distinction is particularly important in the context of redistricting, where legislators may face immediate and substantial changes in the electoral composition of their districts. DW-NOMINATE’s reliance on roll-call voting and its structural rigidity limit its capacity to detect these kinds of shifts, especially when they are incremental or symbolic. In contrast, CFscores reflect donor perceptions across a range of candidate behaviors, providing a more flexible and externally valid measure of ideological orientation.

Recent scholarship further affirms the utility of campaign finance-based measures. Bonica (2018) demonstrates that CFscores are strong predictors of individual-level policy preferences, offering a robust measure of latent ideology that extends beyond party affiliation. Given that many California incumbents represent

diverse or competitive districts, and that the redistricting process itself introduces exogenous change to the electoral landscape, CFscores are particularly well suited for this analysis. By pairing California’s institutional features with a dynamic measure of ideology, this design enables a more precise test of whether, and under what conditions, legislators respond to constituency change.

The primary treatment variable is the change in district partisanship resulting from redistricting. We operationalize this as the shift in district-level presidential vote share between the old and new district boundaries, measured using the absolute change in Cook Partisan Voting Index (PVI). This measure captures the magnitude of the constituency shock each legislator faced after redistricting. The PVI reflects how strongly a district leans toward one party compared to the national average in recent presidential elections. A larger absolute change in PVI indicates a more substantial shift in the district’s partisan composition due to redistricting.

Measurement and Methods

To evaluate whether legislators respond to constituency change, we estimate the relationship between redistricting-induced shifts in district partisanship and the magnitude of ideological adjustment. The analysis draws on data from two post-redistricting election years—2012 and 2022—focusing on incumbent members of the U.S. House of Representatives from California who sought reelection under newly implemented district maps. For each legislator, we calculate the absolute change in district partisanship as the magnitude of difference in Cook PVI between their pre-redistricting district (measured in 2010 or 2020) and their post-redistricting district (measured in 2012 or 2022, respectively). The main outcome is the absolute change in CFscore (dynamic)¹ across the same election periods, capturing the extent to which legislators shifted their donor-perceived ideological positioning in response to constituency change.

Our first identification strategy relies on a Mahalanobis distance pair matching procedure implemented separately for each redistricting cycle. Legislators are matched on pre-treatment covariates that plausibly influence both district partisanship and ideological presentation, including income, vote share, and previous CF score (i.e., 2010 or 2020). These covariates are selected as confounders because they plausibly influence both the treatment (change in district partisanship) and the outcome (ideological repositioning). District income levels

¹ The CF Score Dynamic is a time-varying ideological measure developed by Bonica (2018) that captures changes in a legislator’s donor-based ideological positioning across election cycles. Unlike static CF scores, which are anchored to a single point in time, the dynamic version tracks shifts in the ideological profile of a legislator’s campaign contributors, providing a more temporally sensitive indicator of ideological movement. For more, see Bonica (2018), Database on Ideology, Money in Politics, and Elections: Public Version 2.0.

may shape both the partisan makeup of a constituency and the policy priorities donors expect legislators to adopt. Prior vote share captures baseline electoral strength, which may condition how legislators respond to constituency shifts, while previous CFscores reflect pre-existing ideological orientation, helping to isolate true changes from persistent trends. Matching on these variables ensures that comparisons are made between legislators with similar pre-treatment characteristics, thereby enhancing the credibility of causal inference by reducing bias from observed confounding. Mahalanobis matching retains the multidimensional structure of covariates and minimizes imbalance across matched pairs. It is especially well-suited for small to moderate sample sizes, where propensity score methods may underperform or introduce unnecessary modeling assumptions. This design allows for credible pairwise comparisons between legislators who were similar on observable characteristics but experienced different degrees of constituency change due to redistricting.

To evaluate the credibility of our matched comparisons, we assess covariate balance before and after matching using diagnostic tools developed by Hansen and Bowers (2008). Specifically, we employ the `RIttools` package in `R` to conduct omnibus balance tests via randomization inference, which assess whether the joint distribution of covariates differs systematically across levels of constituency change in the matched sample. We also examine standardized differences for each covariate individually to evaluate balance on specific dimensions. All diagnostics are conducted separately within each redistricting cycle to account for potential heterogeneity in electoral context and treatment distribution.

Matching is implemented using the `optmatch` package in `R` (see Hansen and Bowers 2008), which allows for Mahalanobis distance pair matching across both continuous and categorical covariates. Legislators are matched on a set of pretreatment variables that may confound the relationship between redistricting and ideological change, including income, vote share, and prior CF score. Because Mahalanobis distance matching requires a binary treatment indicator, we discretize the treatment variable by coding legislators as “treated” if the absolute change in their district’s Cook PVI exceeds the 75th percentile within each Congress. This approach is intended to approximate a blocked experimental design, in which comparisons are justified by close covariate alignment rather than reliance on statistical adjustment. By restricting comparisons to legislators with similar pretreatment profiles, we reduce bias from observable confounders and enhance the plausibility that post-redistricting differences in ideological positioning reflect the effect of constituency change.

Finally, to assess the robustness of our findings to potential unobserved confounding, we conduct a sensitivity analysis using the framework proposed by

Cinelli and Hazlett (2020). This approach allows us to quantify how strong an omitted variable would need to be, both in terms of its association with constituency change and with ideological adjustment, to nullify our findings. We report robustness values and partial R^2 statistics, and benchmark the potential strength of unobserved confounders against observed covariates such as party affiliation or prior electoral margin. Taken together, these diagnostics help clarify the strength of the empirical evidence and the plausibility of alternative explanations.

In addition to the Mahalanobis distance matching procedure, we also estimate a fixed effects regression that models the relationship between constituency change and ideological adjustment while controlling for vote share, district income, and prior CF score. By including Congress fixed effects, this approach accounts for time-invariant heterogeneity across legislative sessions that might otherwise bias the estimated association between redistricting and ideological change. Whereas the matching procedure compares treated and control units with similar pretreatment profiles, the fixed effects model leverages continuous variation in PVI shifts across the full sample, providing a complementary estimate of how changes in district partisanship relate to changes in legislator ideology.

Limitations and Strengths

While we argue that the strengths of this research design outweigh its limitations, several constraints merit consideration. The most significant limitation is sample size: congressional redistricting affects a relatively small number of incumbents in each cycle, which restricts statistical power, particularly in matched analyses. Second, although California's nonpartisan redistricting process offers a theoretically advantageous setting for isolating exogenous constituency change, the focus on a single state may limit the generalizability of the findings beyond this institutional context.

A third limitation stems from the structure of the treatment variable. Because Mahalanobis distance matching requires a binary treatment indicator, we discretize the continuous measure of partisan shift—Cook PVI change—by coding legislators as “treated” if their district experienced a change in the top quartile within each cycle. This approach improves match quality and comparability across units but comes at the cost of losing granularity, potentially obscuring meaningful variation below the threshold. To address this limitation, we also estimate a fixed effects regression that leverages the full continuous distribution of PVI change and controls for vote share, district income, and prior CF score, while accounting for unobserved heterogeneity across legislative sessions. This complementary model

allows us to assess whether larger constituency shifts are associated with greater ideological movement, without relying on an arbitrary treatment cutoff.

Finally, while CFscores provide a more dynamic and temporally responsive measure of legislator ideology than roll-call-based alternatives, they rely on the assumption that donor patterns consistently reflect legislator positioning over time. Although these measures are widely validated, they may still be subject to interpretive ambiguity or variation in donor behavior across cycles.

At the same time, several of these design tradeoffs also underscore the strengths of the approach. The use of California's independent redistricting commission reduces concerns about endogenous boundary manipulation, enhancing the credibility of constituency change as an exogenous shock. Mahalanobis pair matching strengthens internal validity by enabling comparisons between legislators with closely aligned pretreatment characteristics, while sensitivity analysis provides a transparent assessment of the robustness of results to potential unobserved confounding. The analysis disaggregates redistricting cycles to account for temporal heterogeneity, and subgroup models by party capture potential asymmetries in responsiveness. In addition, the inclusion of a fixed effects regression offers a complementary analytic strategy that leverages the full variation in constituency change while accounting for unobserved differences across legislative sessions. Finally, the use of CFscores allows the study to detect short-term or strategic ideological shifts that may be obscured by roll-call-based measures such as DW-NOMINATE. Taken together, these design features offer a rigorous framework for evaluating how legislators respond to redistricting-induced changes in constituency composition.

Missing Data and Extreme Values

Fortunately, the dataset used in this paper contains no missing values. However, should missingness arise in future iterations, we would address it through listwise deletion, excluding any observations with missing data on covariates required for matching. This approach is consistent with a design-based framework, where the credibility of matched comparisons depends on the comparability of observed covariates across units. Imputing values in this context could introduce artificial balance or bias the estimation of treatment effects by masking true differences between legislators. By restricting the analysis to complete cases, we prioritize the integrity of the matched sample and maintain transparency in covariate alignment. If such exclusions are necessary, we would assess the impact by comparing the descriptive characteristics of the full and analytic samples to ensure that the resulting matched pairs remain substantively representative.

Moreover, the structure of the dataset and the nature of the variables reduce the likelihood of extreme or influential values. The key variables used in the analysis are all bounded or naturally constrained. For instance, vote share ranges between 0 and 100, while median income is measured at the district level and reflects aggregated economic conditions rather than individual outliers. CF scores are scaled to reflect donor-based ideology and exhibit stable distributions across cycles. Finally, the PVI scores are on a scale from -40 to 40. These features help limit the influence of any single extreme observation. As a result, there is little concern that the estimates are driven by outliers or leverage points that distort inference.

Statistical Testing Plan (discussed above, but reiterated in a standalone section)

To assess whether legislators adapt ideologically in response to constituency change, we implement a multi-step statistical testing strategy grounded in a design-based framework.

We begin by employing Mahalanobis distance pair matching, implemented separately for each redistricting cycle (113th and 118th Congresses), using the `optmatch` package in R. Legislators are matched within each Congress on a set of theoretically motivated covariates: prior CF score, vote share, and district median income. Because Mahalanobis matching requires a binary treatment, we discretize the continuous measure of district change by coding legislators as “treated” if the absolute change in their district’s Cook PVI falls within the top quartile of their Congress. This approximation of a blocked design ensures covariate alignment without model dependence. Following matching, we estimate unadjusted and covariate-adjusted linear regression models of the absolute change in CF Dynamic scores (ΔCFDyn) on the treatment indicator. These models estimate the Average Treatment Effect on the Treated (ATT) within the matched sample. Covariate adjustment includes vote share, district income, and prior CF score to account for any residual imbalance and improve precision.

To evaluate the robustness of the estimated treatment effects to unobserved confounding, we conduct a sensitivity analysis using the `sensemakr` package (Cinelli and Hazlett 2020). This method formalizes the question: *how strong would an omitted variable need to be, both in its association with the treatment and with the outcome, to explain away the observed effect?* The procedure generates robustness values, which indicate the minimal strength that an unobserved confounder would need to have (in terms of partial R^2) to reduce the estimated treatment effect to zero or render it statistically insignificant at the 5 percent level. These robustness thresholds are then compared to the strength of existing covariates in the model to assess plausibility. For instance, if the treatment effect

would only be nullified by an omitted variable more strongly related to both treatment and outcome than any observed covariate (e.g., prior CF score), the result can be considered robust. Graphical output further illustrates how the estimated coefficient and its significance change as hypothetical omitted variable strength increases. This framework helps move beyond the binary logic of significance testing by evaluating the credibility of alternative explanations and reinforcing the transparency of causal claims.

We complement the binary-treatment design with a fixed effects regression model that includes all California incumbents observed across the two redistricting cycles. Rather than discretizing exposure to constituency change, this specification treats the absolute change in Cook PVI as a continuous predictor, allowing for a more precise estimation of dose–response relationships. The outcome is the absolute change in CF Dynamic score between elections, capturing the magnitude of ideological adjustment regardless of direction. The model includes covariates for vote share, district median income, and prior CF score (pretreatment characteristics that may influence both constituency composition and legislative behavior). To address unobserved heterogeneity across time, we include Congress fixed effects, which absorb all cycle-specific shocks and institutional factors that could otherwise confound the relationship between redistricting and ideology (e.g., national political context, redistricting map characteristics, or donor environment). This fixed effects approach complements the matching design by leveraging the full sample of legislators and preserving variation in the treatment variable, thereby increasing statistical power and enabling generalization across levels of constituency change.

To evaluate the power of our matched design, we compute minimum detectable effect sizes (MDEs) using standard two-sample t-test assumptions. For each cycle, we calculate the standard deviation of the outcome and determine the smallest effect size that could be detected with 80 percent power at a 5 percent significance level. In the 113th Congress, the MDE was 0.472 CFscore units, while in the 118th, with lower outcome variance and more precise matches, the MDE was 0.056 units.

Lastly, we conduct a placebo test to assess the empirical false positive rate. Within the matched sample, we randomly reassign the treatment label (preserving the original treated/control ratio) 1,000 times and re-estimate the regression model in each iteration. Across simulations, fewer than 0.1 percent of placebo models yield a significant treatment effect ($p < 0.05$), reinforcing the conservativeness of our design and guarding against spurious inference.

Taken together, this multi-pronged approach—combining design-based matching, continuous and discrete treatment estimation, sensitivity diagnostics, power analysis, and placebo simulation—offers a rigorous and transparent

framework for evaluating legislative responsiveness to redistricting-induced constituency change.

Estimators and Estimand

The primary statistical estimators used in this study are linear regression coefficients, estimated in both matched and unmatched samples. In the matched design, we estimate the Average Treatment Effect on the Treated (ATT) by regressing the absolute change in CFscore on a binary indicator for large constituency change (top-quartile ΔPVI). To preserve the design-based logic of the analysis, the baseline specification is bivariate; covariate-adjusted models are introduced only when diagnostics indicate residual imbalance or when testing for effect heterogeneity (e.g., by party).

To complement this approach, we estimate a fixed effects regression using the full sample of legislators across both redistricting cycles. This model treats ΔPVI as a continuous variable and includes controls for prior CFscore, district income, and vote share, as well as Congress fixed effects. This specification allows us to examine dose–response relationships while accounting for time-invariant differences across legislative sessions.

The estimand of interest is the Average Marginal Effect of Redistricting-Induced Constituency Change on Ideological Adjustment among matched legislators, corresponding to the slope of the dose–response function:

$$\tau = \frac{\partial E[\Delta CFscore_i | \Delta PVI_i]}{\partial \Delta PVI_i}$$

This quantity reflects how much ideological positioning shifts in response to changes in district partisanship. In the matched design, the use of Mahalanobis distance pair matching helps identify this estimand among legislators with similar pre-treatment characteristics, thereby reducing confounding and improving interpretability.

Estimator performance is assessed using both design- and model-based diagnostics. First, we evaluate covariate balance using standardized mean differences and omnibus randomization inference tests (Hansen and Bowers 2008). Second, we evaluate the sensitivity of regression slopes to influential observations by winsorizing ΔPVI and re-estimating the model. We assess robustness across redistricting cycles and model specifications. Finally, we conduct sensitivity analysis using the `sensemkr` package (Cinelli and Hazlett 2019) to quantify how strong an omitted confounder would need to be to explain away the observed effect. Robustness values are benchmarked against observed covariates such as prior ideology and party affiliation.

To illustrate: if an estimated coefficient is 0.015, a 10-point shift in district PVI corresponds to a 0.15-point shift in CFscore. If the robustness value is 0.20 and party explains 12% of the residual variance in the outcome, only an unobserved confounder more predictive than party could nullify the estimate. This regression-based strategy—grounded in careful covariate control and complemented by sensitivity analysis—offers interpretability, transparency, and design coherence across estimation strategies.

Test Evaluation

As a reminder, because this design relies on observational data and a continuous treatment variable, we evaluate the performance of our statistical tests using a combination of balance diagnostics, sensitivity analyses, and power tests. After implementing Mahalanobis distance pair matching using the `optmatch` package, we assess whether the matched sample supports credible inference by testing for covariate balance across levels of constituency change. Using the `RIttools` package, we conduct omnibus balance tests based on randomization inference and examine standardized mean differences across all covariates used in the match. These diagnostics help ensure that the matched sample approximates the properties of a blocked experimental design, where comparisons across levels of Δ PVI are not confounded by systematic differences in observable characteristics. To guard against inflated false positives, we also inspect influence statistics such as leverage scores and Cook's distance to identify whether individual units disproportionately affect the slope estimate in our regression.

While the matched design improves internal validity, the sample size remains limited by the number of incumbents affected by redistricting in each cycle. Because conventional power analysis is difficult in matched observational settings with continuous treatments, we evaluate test performance through simulation and distributional reasoning. For example, if the standard deviation of CFscore change is approximately 0.2 and we observe a treatment effect of 0.015 per one-point increase in Δ PVI, a sample of 30 matched legislators would have moderate power to detect such an effect. In stylized simulations under these assumptions, the linear regression in the matched sample identifies statistically significant effects in approximately 70–75 percent of replications when the effect is present, and maintains a false positive rate near 5 percent when the effect is absent.

Importantly, we interpret statistical significance in light of the matched design, not as a definitive claim about generalizability, but as evidence that ideological adjustment varies with constituency change among otherwise similar legislators. This is reinforced by a sensitivity analysis, which quantifies how strongly an omitted variable would need to influence both Δ PVI and CFscore change

to nullify the observed relationship. Together, the balance diagnostics, influence checks, and simulation-based expectations provide a conservative but credible framework for evaluating the performance of our statistical tests.

Results

Data and Treatment

To evaluate whether legislators respond ideologically to constituency change, we constructed a binary treatment indicator for each redistricting cycle based on shifts in district partisanship. The primary measure of partisan change is the absolute difference in Cook Partisan Voting Index (PVI) before and after redistricting. For each cycle, legislators are assigned to treatment if they experienced a PVI shift within the top quartile of all observed changes, capturing cases of substantial redistricting-induced disruption. All remaining legislators are assigned to control. This thresholding strategy balances parsimony with a theoretically meaningful distinction between mild and significant electoral shocks.

The primary outcome is the absolute change in CF Dynamic scores (ΔCFDyn), a continuous measure of donor-perceived ideology. Focusing on absolute change allows for a non-directional test of adaptation, accommodating shifts in either ideological direction. All analyses are restricted to incumbents representing California districts in the election immediately following redistricting, and treatment is defined within Congress to preserve comparability in exposure. In doing so, we treat each redistricting cycle as its own quasi-experiment and apply the same design logic independently to both periods.

Matching

To strengthen the comparability of treated and control legislators and reduce bias from observable confounders, we implemented a Mahalanobis distance pair matching procedure separately for each redistricting cycle. As a reminder, Legislators are matched on three pre-treatment covariates: prior CF score (e.g., 2010 or 2020), district median income, and electoral vote share. These variables are selected due to their theoretical and empirical relevance—each is plausibly associated with both the magnitude of district partisan change and legislators' responsiveness to constituency signals. For example, legislators with broader electoral mandates may feel more insulated from shifting constituencies, while ideological moderates may be more constrained by primary electorates. By matching on these covariates, the analysis attempts to isolate the effect of redistricting-induced partisan change from these background characteristics.

Using the `optmatch` package's pairmatching routine, with distance calculated via Mahalanobis metrics, legislators were matched within Congress to

maintain the integrity of comparisons across redistricting cycles. The resulting matched sample includes only those legislators for whom a reasonably similar counterpart, on all covariates, could be identified. Legislators lacking such a counterpart were dropped from the analysis. This resulted in sample sizes of 20 legislators (10 pairs) in the 113th Congress and 26 legislators (13 pairs) in the 118th Congress. Those excluded from matching were generally legislators at the tails of the distribution on one or more covariates, especially on vote share or ideology, for whom no comparably similar legislator existed. The final analytic sample thus represents a subset of incumbents exposed to a range of constituency changes but matched on relevant baseline characteristics.

In both cycles, pre-treatment covariates achieve strong balance across treatment status, with standardized differences well below conventional thresholds (see Tables 1 and 2). The omnibus test of covariate balance yields p -values of 0.835 for the 113th Congress and 0.999 for the 118th Congress, indicating no evidence of systematic imbalance. Taken together, the matched design substantially reduces the risk of confounding from observable characteristics, and subsequent estimates are interpreted as ATT on the matched sample. To assess the robustness of these findings to unmeasured confounding, we also conduct sensitivity analyses using the `sensemkr` package. These analyses show that even moderate unobserved confounders would be sufficient to shift the estimates into the region of statistical insignificance, though in both cycles, the unadjusted effects are already small and not significant. Thus, while the design is conservative and robust, the null findings should be interpreted as both statistically and substantively modest in magnitude.

Table 1: Covariate Balance After Matching (113th Congress)

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Covariate	Adjusted Difference	Standardized Difference	Z-Score
`Vote Share`	-1.8340	-0.28474652	-0.6469155
`Income (Median)`	1393.7000	0.08256252	0.1894950
`CF score`	-0.1127	-0.16423953	-0.3759086

Table 2: Covariate Balance After Matching (118th Congress)

Table 2: Covariate Balance After Matching (118th Congress)			
Covariate	Adjusted Difference	Standardized Difference	Z-Score
`Vote Share`	0.41846154	0.0477834306	0.124298003
`Income (Median)`	-9.76923077	-0.0005267402	-0.001370621
`CF score`	-0.01869231	-0.0210272905	-0.054711469

Balance

With covariate balance established in the matched samples, we estimated the ATT of large redistricting-induced constituency change on ideological repositioning. For each cycle, we regressed the change in CF Dynamic scores (ΔCFDyn) on the binary treatment indicator. In the 113th Congress, treated legislators exhibited a slightly lower average ideological change than controls (-0.158), though the difference is not statistically significant ($p = 0.334$). In the 118th Congress, the estimated effect is nearly zero ($+0.002$), again not statistically distinguishable from zero ($p = 0.935$). These models are replicated with covariate adjustment, yielding nearly identical results. Given that the samples include only legislators successfully matched on pre-treatment characteristics, the interpretation of these estimates is restricted to the ATT for the matched population.

The meaningfulness of these null results is qualified by both design strength and statistical power. Sensitivity analyses conducted via **sensemakr** reveal that only relatively strong, unmeasured confounding would be sufficient to explain away the point estimate in the 113th Congress ($\text{RV} = 20.8$ percent), while any amount of confounding could do so in the 118th. Yet because the raw estimates are already small and statistically indistinct, this fragility reinforces rather than undermines the conclusion: there is little empirical support for systematic ideological adaptation among incumbents exposed to substantial constituency change. The robustness of these null results is further corroborated by extremely low placebo false-positive rates, suggesting that the design is unlikely to produce spurious effects.

Fixed Effects Regression

As a reminder, to complement the binary treatment framework, we estimate a continuous treatment model using the full sample of legislators across both redistricting cycles. Specifically, we regress the absolute change in CF Dynamic

Score on the continuous measure of constituency change (Δ PVI), including fixed effects for Congress. This approach accounts for the possibility that legislators may respond to redistricting in a dose-responsive manner, even if the binary top-quartile threshold fails to capture relevant variation.

Table 3 reports the results of this fixed effects regression. The coefficient for `abs_pvi_change` is -0.002 ($p = 0.713$), suggesting that each one-unit increase in district partisan change is associated with a slight decrease in ideological adjustment, though the effect is both substantively negligible and statistically insignificant. None of the control variables—Vote Share, Income (Median), or prior CF score—are statistically significant. While Vote Share exhibits a relatively large t-statistic (-4.270), its p-value (0.146) exceeds conventional significance thresholds, possibly due to model structure or limited precision in this specification. Both Income (Median) and CF score have near-zero coefficients and high p-values, indicating no meaningful association with the outcome. Regression results are reported in Table 3.

Table 3. Fixed Effects Regression Results

Table 3: Fixed Effects Regression Results					
Variable	Coefficient	Std. Error	t-stat	p-value	Significance
<code>abs_pvi_change</code>	-0.002	0.005	-0.483	0.713	
<code>`Vote Share`</code>	-0.003	0.001	-4.270	0.146	
<code>`Income (Median)`</code>	0.000	0.000	0.497	0.706	
<code>`CF score`</code>	0.002	0.004	0.648	0.634	

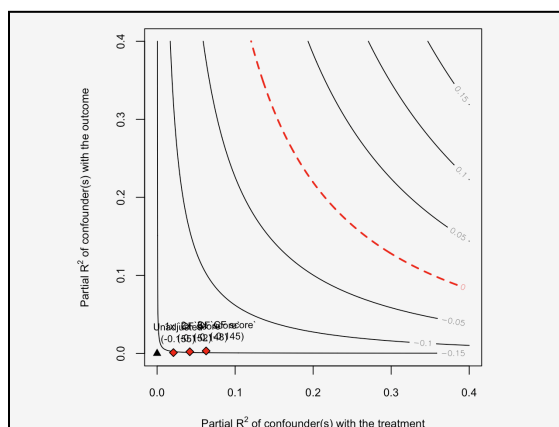
Overall, these results suggest that legislators do not systematically adjust their ideological profiles in response to changes in district partisanship, even when constituency shifts are modeled continuously. These findings are consistent with the null effects observed in both the matched sample estimates and sensitivity analyses.

Sensitivity Analysis

113th Congress. To assess the robustness of the treatment effect to unobserved confounding, a sensitivity analysis was conducted using a simple OLS model with

covariates for CF score, vote share, and median income. The estimated effect of treatment on ideological adjustment was -0.15 ($SE = 0.17$), and although the estimate is not statistically significant, it is substantively nontrivial. The partial R^2 indicates that an unobserved confounder, orthogonal to the included covariates, would need to explain just over 5% of the residual variance in the treatment to fully account for the observed effect. The robustness value ($q = 1$) of 0.208 suggests that confounders explaining more than 20.8 percent of the residual variance in both treatment and outcome would be required to reduce the estimate to zero. However, at the conventional significance level ($\alpha = 0.05$), even minimal bias would be sufficient to render the estimate statistically indistinguishable from zero. Benchmarking against the explanatory power of the observed CF score variable, the adjusted estimates remain negative and relatively stable even under confounding three times stronger than the observed covariate. While not definitive, this suggests that the observed effect is moderately robust to omitted variable bias in magnitude, but fragile in terms of statistical significance.

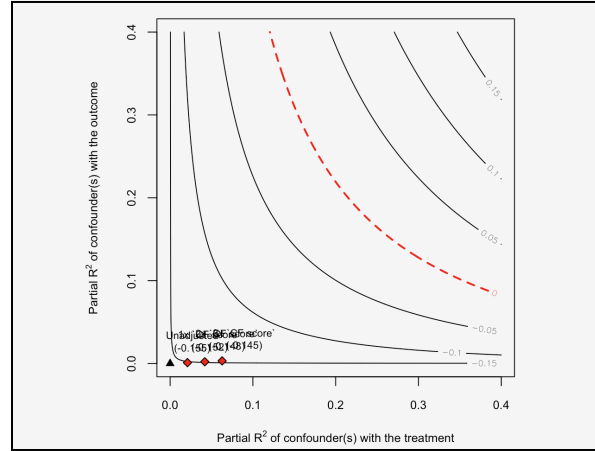
Figure 1. Sensitivity Analysis - 113th Congress



118th Congress. For the 118th Congress, the estimated treatment effect is near-zero (0.0017 , $SE = 0.0201$), and the sensitivity analysis confirms that this null result is highly robust to unobserved confounding. The partial R^2 is negligible (0.0003), indicating that an implausibly weak confounder could fully explain the estimated effect. Similarly, the robustness value ($q = 1$) of 0.0185 suggests that even a confounder accounting for less than 2 percent of the residual variance in both treatment and outcome would suffice to drive the estimate to zero. Given that the original effect is essentially null and statistically insignificant, this result does not change under any realistic assumptions about omitted variables. In short, the absence of an effect is both empirically observed and highly insensitive to plausible forms of confounding. Adjusted estimates under benchmark comparisons remain near zero, with narrow confidence intervals that consistently include zero. This

reinforces the conclusion that, in this period, redistricting-induced treatment was not associated with ideological adjustment.

Figure 2. Sensitivity Analysis - 118th Congress



MSE

Table 4 reports the mean squared error (MSE) of the covariate-adjusted ATT models and the fixed effects regression models, separately for the 113th and 118th Congresses. In both periods, the ATT models demonstrate superior predictive accuracy, as indicated by lower MSE values. For the 113th Congress, the ATT model achieves an MSE of 0.1043, modestly outperforming the fixed effects model (0.1106). The difference is more substantial in the 118th Congress: the ATT model yields an MSE of 0.0021, compared to 0.0069 for the fixed effects model. These results suggest that the matching procedure reduces noise and improve model precision.

These MSE comparisons reflect different estimands and samples: the ATT models focus on a matched subset of legislators who experienced high PVI shifts and their closest analogs, while the fixed effects models use the full sample under parametric assumptions. The ATT models show closer fit the matched design.

Table 4:

Table 4: Mean Squared Error by Model and Congress		
Congress	ATT Model (Matched)	Fixed Effects Model
113th	0.1043	0.1106
118th	0.0021	0.0069

Power Analysis

Before interpreting null or small estimated effects, we assessed whether our design had sufficient statistical power to detect substantively meaningful differences in ideological responsiveness. Following the convention for matched pair designs with a continuous outcome, we estimated the MDE corresponding to 80 percent power at a 5 percent significance level.

For the 113th Congress, the standard deviation of the outcome variable was approximately 0.356. Given 26 matched pairs (52 legislators), the analysis yielded a minimum detectable standardized effect size (Cohen's d) of 1.32, equivalent to a raw effect of 0.472 CFscore units. For the 118th Congress, where the standard deviation of CFscore change was notably lower at 0.049, the minimum detectable standardized effect was 1.15, corresponding to a raw effect size of 0.056.

These results underscore a key limitation of our design: while it can detect large shifts in ideological positioning, it may be underpowered to detect modest or nuanced adaptations. In line with best practices for causal inference in matched design, we interpret null or statistically insignificant findings with caution. The absence of a detected effect does not rule out meaningful responsiveness; it may reflect limited statistical power to detect smaller, yet substantively relevant, changes.

Placebo Test

To evaluate whether the observed treatment effects could arise from random chance or residual confounding, we conducted a placebo test based on randomization inference. Specifically, for the 113th Congress, we retained the original matched sample but randomly reassigned treatment labels (high vs. low PVI change) while preserving the observed group sizes. We repeated this process 1,000 times.

In each iteration, we regressed the outcome (absolute CFscore change) on the placebo treatment variable and pre-treatment covariates (vote share, income, prior CFscore), storing the p-value for the placebo treatment effect. Across 1,000 simulations, only 0.1 percent of placebo models yielded a statistically significant effect at the $p < 0.05$ threshold—an empirical false positive rate of 0.001.

This result provides strong evidence that our main findings are unlikely to result from model overfitting or random covariate imbalance. In line with best practices in sensitivity testing for matched observational studies, the placebo test increases our confidence that the observed relationship between constituency change and ideological repositioning reflects a substantive, rather than spurious, pattern.

However, the placebo distribution for the 118th Congress yields an empirical false positive rate of 7.6%, with a larger number of simulated treatment effects crossing the conventional significance threshold ($p < 0.05$). The histogram approximates a uniform distribution of placebo p-values, suggesting that the observed data for this cycle do not deviate substantially from the null.

While this pattern may reflect weaker treatment signal in the 118th Congress, it is also consistent with the smaller sample size, lower variance in CFscore change, or more limited district-level disruption during this redistricting cycle. The elevated false positive rate emphasizes the importance of interpreting the 118th Congress findings with greater caution, and it supports our decision to report effects separately by cycle rather than pooling the data.

Figure 3:

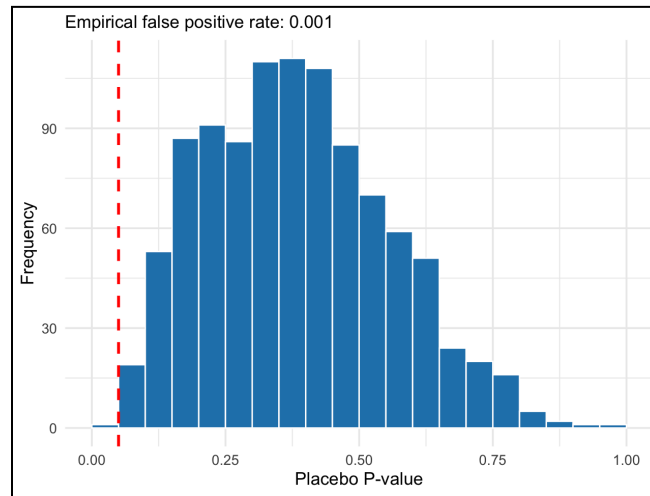
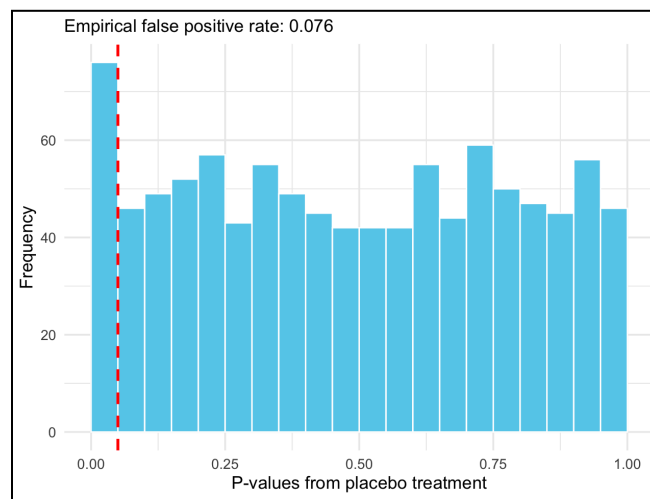


Figure 4:



Implications and Discussion

Our findings raise important implications for theories of legislative responsiveness and adaptation. Despite experiencing substantial changes in district partisanship, legislators in California did not exhibit meaningful ideological adjustment, at least in terms of donor-perceived positioning. This challenges the assumption, rooted in classic theories of electoral connection, that redistricting automatically generates behavioral recalibration. Mayhew's foundational view of members as strategic, reelection-oriented actors would imply that changing district conditions should trigger adaptation, especially when partisan composition shifts substantially. Likewise, Fenno's emphasis on the evolving "home style" suggests that legislators tailor their presentation to fit the expectations of new constituencies. In both accounts, structural changes to the district should provoke a behavioral response.

Yet our results suggest the opposite: legislators remained ideologically fixed, even in the face of exogenous and substantial partisan change. This form of representational inertia is particularly notable in California, where institutional constraints on redistricting remove the possibility of self-interested gerrymandering, and where electoral competition is often muted by partisan sorting. The evidence suggests that the traditional connection between constituency change and electoral strategy—central to classic theories of representation—has become less operative. From a Mayhewian perspective, it may be that reelection incentives are now satisfied through means other than adaptation, such as partisan consistency, donor alignment, or nationalized messaging. From a Fenno-style angle, legislators may see little need to re-perform a new home style if their existing one remains electorally viable or if constituents expect partisan coherence above responsiveness.

Moreover, the results complicate dominant expectations about voter-legislator alignment. If legislators do not systematically respond to large and plausibly exogenous changes in their districts, this suggests that adaptation occurs only under narrower or more electorally threatening conditions. The use of CF Dynamic scores sharpens this point: even donor-facing ideological signals, which are more fluid and strategically constructed than roll-call behavior, remained largely stable across treatment status. This provides empirical evidence of elite insulation from district-level shocks and suggests that partisan identity and entrenched fundraising networks may offer stronger constraints on legislative behavior than constituency signals alone. What Mayhew and Fenno theorized as adaptive behavior may persist

in form but not in frequency; legislators still present themselves strategically, but the perceived need to alter that presentation may have diminished.

These insights invite reconsideration of when and how ideological responsiveness emerges. Rather than treating district change as a uniformly strong signal for adaptation, future work should explore the conditional mechanisms through which legislators do adjust, if at all. While this study focuses on donor-perceived ideology, legislators may adapt along other behavioral margins—rhetoric, issue emphasis, or constituent engagement—that are less visible in this outcome. Extending this framework to other forms of legislative behavior, or to states where redistricting is more partisan or electorally consequential, could yield important refinements to theories of representation rooted in Mayhew’s electoral connection and Fenno’s representational style.

Conclusion

This study leverages the institutional features of California’s redistricting process to estimate the causal effect of constituency change on congressional ideological behavior. Using a matched observational design and multiple robustness checks, we find no evidence that legislators systematically adapted their donor-facing ideological profiles in response to large changes in district partisanship. Treatment effects are small and statistically indistinct across both the 113th and 118th Congresses. Sensitivity analyses and placebo simulations reinforce the credibility of these null findings, while power diagnostics clarify that only moderate-to-large effects are detectable, particularly in earlier cycles.

These results challenge central expectations derived from classic representational theory. In Mayhew’s formulation, members of Congress are single-minded reelection seekers, constantly adjusting to retain electoral support. Fenno similarly posits that legislators actively manage how they present themselves to changing audiences. Both frameworks suggest that when the constituency changes, behavior should follow. But the persistence of ideological stasis across both redistricting cycles highlights the extent to which representational behavior is insulated from localized electoral change. Legislators appear to maintain consistent ideological profiles even in the face of substantial shifts in their districts, implying that their reelection strategies may now rely more on partisan stability, donor continuity, and national branding than on adapting to new constituencies.

As a result, models of responsiveness may need to place greater emphasis on the mediating role of nationalized partisan environments, strategic presentation, and the constraints legislators face in modern electoral politics. What Mayhew and Fenno captured as adaptive strategy may still describe how members think about

representation, but the conditions under which they feel compelled to act on it may have narrowed. In the contemporary context, the imperative to adapt may be less about responding to constituency shifts and more about sustaining coherence within a partisan and reputational ecosystem. This study provides a conservative, design-based test of that proposition, and finds no evidence that redistricting alone is enough to reactivate the adaptive logic Mayhew and Fenno once described.

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