

When Do Legislators Represent Their Constituents? Evidence from Roll-Call and Referendum Votes

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This article develops a measure of representation that uses referendum returns to capture constituent opinion and applies it to 4,192 roll-call votes on 33 laws in 10 states. Roll-call votes were congruent with majority/median opinion in a district 66% of the time. Roll-call votes can be explained primarily by legislator ideology, with constituent opinion of secondary importance. The data do not show a reliable connection between congruence and competitive elections, term limits, media attention, and party pressure. The evidence generally supports the predictions of representation theories that emphasize selection of legislators that share constituent ideology and provides little support for theories that emphasize re-election incentives.

The purpose of this article is to explore the conditions under which legislators do and do not represent the preferences of their constituents. This is a longstanding issue in the literature—the innovation here is an empirical strategy that uses previously unexploited data from state referendums. Representation is measured by matching a legislator’s roll-call vote on a specific law with the votes cast by his or her constituents on that law in a referendum election. Unlike representation studies that compare ideal points of legislators and constituents, in the approach here, a legislator can be congruent with constituents on one issue and non-congruent on another, allowing discrimination of key explanatory factors by controlling for legislator-specific factors.

Twenty-three American states allow citizens to challenge state laws that have been approved by the legislature and governor using what is often called the veto referendum process. In these states, by collecting a predetermined number of signatures from eligible voters, citizens can trigger an election in which voters have the option to uphold or repeal the law. I construct a dataset covering 33 laws in 10 states and determine if each of the 4,192 roll-call votes on those laws were congruent with majority opinion in the district.

Numerous studies have shown that policy is correlated with citizen ideology (Caughey and Warshaw 2022; Erikson, Wright, and McIver 1993; Tausanovitch and Warshaw 2013,

2014), but direct measures of congruence find that specific policies match the preferences of the majority only about half of the time (Lax and Phillips 2012; Lax, Phillips, and Zelizer 2019; Matsusaka 2010, 2020; Simonovitz, Guess, and Nagler 2019). I find that legislators voted congruent with majority opinion 66% of the time, in the range of other roll-call congruence estimates (Giger, Klüver, and Witko 2020; Krimmel, Lax, and Phillips 2016; Lax et al. 2019). Because my sample contains laws that were challenged by referendum, and thus were likely to have had failed representation, this congruence estimate is likely to be a lower bound on the amount of congruence in the universe of laws.

The study’s primary goal is to identify factors that explain variation in congruence across legislators. I explore several factors, motivated by selection and electoral accountability theories. “Selection” theories, at their core, hold that voters choose candidates committed to a particular set of policies (ideologies), which they implement if elected, and representation occurs through the selection of like-minded representatives (Besley and Coate 1997; Downs 1957; Osborne and Slivinski 1996). In contrast, “accountability” theories hold that legislators represent constituent interests in order to increase their chance of reelection (Barro 1973; Ferejohn 1986). Both theories are surely relevant in practice; the goal here is to quantify their relative importance.

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I find that a legislator's ideology is by far the most important factor in explaining congruence. This can be seen in simple nonparametric comparisons—in cases where a legislator's ideology pointed in the opposite direction from citizen preferences, legislators voted according to their ideology 72% of the time—and appears in more demanding regression models with various fixed effects and controls. On the other hand, it is difficult to detect an important role for reelection incentives. Legislators were no more likely to cast a congruent than noncongruent vote if they represented a competitive district, if they were not subject to term limits, or if media attention was high, all of which should predict congruence according to accountability theories. I find some suggestive evidence for a third explanation, that some noncongruent voting stemmed from party pressure, using a test developed by Snyder and Groseclose (2000), but the findings are not entirely robust, and the overall explanatory power is modest.

The picture that emerges is one in which elections select candidates who are usually ideologically aligned with their district's voters and who follow their ideology when voting, for the most part regardless of constituent preferences. This is not to suggest that reelection incentives do not exist but that at the level of individual roll-call votes, a legislator's ideology appears to drive voting much more than the legislator's concern about losing reelection.

THEORIES: SELECTION, REELECTION, AND PARTY PRESSURE

The analysis is motivated by several theories of representation. According to "selection" theories, elections allow voters to select representatives that share their preferences. Downs (1957) developed an early model of this type in which candidates declare platforms that they implement once elected. A theoretical limitation of the model is that candidate promises are not credible once elected. The "citizen-candidate model" addresses this criticism by assuming that candidates compete but that voters assess them based on their personal ideologies, not promises, expecting that they will pursue their ideological goals once in office (Besley and Coate 1997; Osborne and Slivinsky 1996). Selection leads to representation through voters choosing legislators that share their ideologies.

According to accountability or principal-agent models, elections are a reward-and-punishment (incentive) mechanism by which voters settle up with incumbents when they stand for reelection. The core premise of accountability models is that representatives are more likely to advance voter interests if they face a risk of being turned out of office. Barro (1973) and Ferejohn (1986) developed early models in this

vein, which is also associated with Mayhew (1974). A theoretical limitation of these models is that voters' accountability strategies are not time consistent—once an election is at hand, it might not be credible to punish and remove an incumbent who performed poorly if the challenger is even worse. One response to this criticism is to blend in a selection dimension by assuming that candidates have abilities or preferences that are imperfectly observable but partially revealed by past votes, so that it can be rational for voters to condition reelection on past behavior (Ashworth 2012; Banks and Sundaram 1993; Fearon 1999).

Both of these theories enjoy empirical support, but views on their relative importance differ. After reviewing a variety of findings, Fearon (1999, 82) concluded that voters "think about elections much more as opportunities to select good types than as sanctions to deter shirking by future incumbents." Similarly, based on analysis of congressional voting, Poole and Rosenthal (1991, 1996) concluded that representatives adopt an ideological position and maintain it consistently throughout their careers—legislators "die in their ideological boots" (Poole 2007, 435).

In contrast, Kousser, Lewis, and Masket (2007, 828) observed that "[a] central tenet of American legislative scholarship over the last 30 years holds that legislators are predominantly, if not single-mindedly, motivated by electoral incentives." Several studies have tested if constituents are aware of legislator votes and if legislators pay an electoral cost from disregarding constituent preferences, typically finding statistically detectable costs from voting against constituents but too small to generate strong incentives (Ansolabehere and Kuriwaki 2022; Hanretty, Mellon, and English 2021; Rogers 2017; Tausanovitch and Warshaw 2018). Several studies have used term limits as an exogenous shifter since legislators lose reelection incentives in their last term, typically finding that the ideological content of roll-call votes does not change in the last term. A third approach, for example, Kousser et al. (2007) and Jenkins (2000), is to test if an incumbent's voting changes when constituent opinion changes, with most studies finding some support. At the subnational level, the "nationalization" of local elections (Rogers 2016) undercuts the idea that voters hold local legislators accountable for their roll-call votes.

The two theories offer relatively clear predictions that can be tested. For the selection model, the core prediction is that legislators vote according to their ideological preferences, regardless of constituent preferences. For reelection incentives, the core prediction is that legislators are more likely to vote according to constituent preferences when reelection pressure is high, which I measure by electoral competition, term limits, and media attention.

A different line of argument attributes noncongruent roll-call voting to party pressure: to build a winning coalition, party leaders or the party caucus may reward or punish members based on their votes. Leaders can reward members through committee assignments, campaign funds, and treatment of favored bills. To assess this idea, I draw on the observation of Snyder and Groseclose (2000) that party pressure is more likely to be applied on close versus lopsided votes because leaders are more willing to pay the cost of pressing when they need a member's vote to win than when they have already secured a large majority.

A final explanation for noncongruent roll-call voting is that legislators misread constituent opinion and make “honest mistakes” when trying to represent them. Because legislators must vote on hundreds of bills and represent thousands of people, sometimes they are going to misread constituent preferences. Consistent with this idea, research shows that legislators sometimes misperceive citizen preferences (Broockman and Skovran 2018; Pereira 2021; Walgrave et al. 2023) and alter their voting when presented with evidence on constituent preferences (Butler and Nickerson 2011; Kogan 2016). I assess this explanation for misrepresentation by comparing congruence in situations where legislators were more or less likely to make mistakes.

METHODS AND DATA

Empirical approach

Citizens are “represented” if their legislator casts a roll-call vote congruent with the vote they would have cast themselves if they were informed about the law. I measure this by comparing a legislator's roll-call vote $ROLL \in \{yes, no\}$ with the majority (median) position in the district on the referendum $REF \in \{yes, no\}$. A legislator's vote is defined to be “congruent” if $ROLL = REF$ and “noncongruent” if $ROLL \neq REF$. Referendum elections take place after roll-call votes; this sequence is useful because it means that legislators must estimate constituent preferences at the time of the roll call, just as for normal votes.

Congruence is not without limitations as a measure of representation, as discussed below; here I discuss its potential advantages compared to the alternatives. One alternative is to collapse individual votes into a single spatial measure representing legislator and constituent ideal points and compare the distance between the two.¹ Such estimates are grounded in theory and central to research on representation, but they require a bridging assumption to place ideal

points on the same scale. Broockman (2016) showed that even with a valid bridging assumption, such comparisons might capture consistency of beliefs held by legislators and ordinary citizens rather than ideological congruence. Also, when comparing ideal points, the distance exists in an abstract space, so the units lack a real-world interpretation. Congruence as measured in this article does not require a bridging assumption, and the units have a natural interpretation. Another important advantage is that congruence captures representation at the level of an individual vote, as opposed to ideal points, which aggregate a large number of votes. With vote-specific measures of congruence, it is possible to explore why a legislator voted congruently on one issue and not another and to strip out legislator-specific effects.

Another approach is to regress roll-call votes on proxies for constituent preferences and ideology, with the size of the coefficient on preferences interpreted as the degree of representation (Peltzman 1984). Representation using this method is a parameter of the entire dataset—it does not reveal representation at the level of an individual legislator or policy vote. Also, there are theoretical difficulties in using such coefficients to identify variance in representation across subgroups (Achen 1977; Matsusaka 2001).

Congruence can also be measured using opinion surveys, instead of referendums, to capture constituent preferences.² The strengths and limits of the two approaches are the following. First, referendum returns indicate opinion on exactly the law approved by the legislature, while opinion surveys usually ask about the concept of a law; if “the devil is in the details,” opinion polls may omit legal details that matter to voters. Second, referendum elections actually make law—citizen opinions may be more informed when they have real consequences than when they talk to a pollster. In theory, referendum votes reflect information acquired during public discussion during election campaigns, and the provision of election shortcuts such as media and interest group endorsements. Early polls on ballot measures are unreliable predictors of the final vote (Matsusaka 2018), suggesting that opinion polls of uninformed voters can be misleading about their true preferences. On the other hand, the number of policies that can be studied with referendum data is smaller than when using opinion surveys. Also, referendum data may be less representative of the general population than opinion surveys because not every person votes (although this could be a virtue from a theoretical perspective since both selection and

1. Examples: Bafumi and Herron (2010); Clinton (2006); Gerber and Lewis (2004); Kousser, Phillips, and Shor (2018); and Maskett and Noel (2012).

2. Studies using opinion data include Krimmel et al. (2016) and Lax et al. (2019). Giger et al. (2020), Portmann, Stadelmann, and Eichenberger (2012); and Stadelmann, Portmann, and Eichenberger (2013, 2014) use ballot measure returns to calculate congruence.

incentive theories predict that only the preferences of those who actually vote should matter). In the end, the two approaches seem complementary in painting an overall picture of representation.

Referendums and roll-call votes

Twenty-three states allow citizens to challenge laws passed by the legislature and signed by the governor. The process always takes the following form: a citizen (petitioner) can challenge a recently adopted law by collecting signatures from fellow citizens; if the petitioner is able to collect enough signatures to meet the state's threshold number, then an election is called; and if a majority votes against the law, then the law is nullified.³ To construct the sample, I began by identifying all state-level referendums during 2000–2022. I then collected election returns by legislative district; these were available for 33 referendums. For each law, I identified the (final) roll-call votes in the legislature, giving a final sample of 4,192 roll-call votes. If districts changed between the roll call and the referendum election, then the referendum was dropped. The individual laws are listed and described in appendix A.

The referendums were held in Alaska, California, Colorado, Maine, Maryland, Michigan, North Dakota, Ohio, South Dakota, and Washington, a mix of urban and rural and “blue” and “red” states. The challenged laws covered fiscal, political, and social issues and included topics of national interest such as same-sex marriage and the minimum wage, as well as issues of primarily local interest such as Alaska's law on aerial wolf hunting and North Dakota's law allowing the state university to change the name of its mascot from “Fighting Sioux.” The ideological orientation of the laws was also mixed, some proposing progressive policies (e.g., allowing same-sex marriage or permitting the state to use racial preferences/affirmative action) and others proposing conservative policies (e.g., allowing charter schools or limiting collective bargaining by public employees). Voters approved 18 of the 33 laws.

All representation studies focus on a subset of all policies, and therefore, it is important to understand their potential selection biases. For example, studies that measure representation using polling data are restricted to issues that are sufficiently controversial to prompt a poll; we might expect congruence to be lower on such policies than on routine noncontroversial policies. By construction, all of the laws in my sample were challenged by citizen groups. We might

expect roll-call votes to have been less congruent on these laws than others, in which case the level of congruence in my sample would be a lower bound of overall congruence. Selection is less obviously a concern when seeking to identify the factors that predict noncongruent voting—while we expect more noncongruent factors in my sample, there is no obvious reason for their effects to have been different.

Ideological alignment

I measure legislator ideology using National Political Awareness Test (NPAT) common space scores constructed by Shor and McCarty (2011; July 2020 version). Each legislator's score is a time-invariant scalar that is negative for relatively progressive positions and positive for relatively conservative positions. The mean score is -1.19 for Democrats and 0.94 for Republicans.

The use of ideal point estimates as a proxy for ideology is a common practice, but not without controversy. Technically, the scores are simply a low-dimensional representation of a legislator's history of roll-call votes; as such, they could be capturing not just ideology but also external influences such as constituent opinion, reelection incentives, and party pressure. Several arguments have been offered for interpreting them as ideology: the estimates are fairly stable throughout a legislator's career; a legislator's ideal point changes little when his or her constituency changes; US Senators from the same state often have quite different ideal points (implying that ideal points are not entirely induced by constituents); and same-party replacements of an incumbent legislator can have different ideal points (McCarty 2011; Poole 2007; Stratmann 2000).

To mitigate the concern that ideal points are noisy estimates of ideology, for parts of the analysis, I assign legislators to one of two broad ideology groups: “conservative” if a legislator's NPAT is positive and “progressive” if the NPAT is negative (the findings are quite similar for any cutoff in a large neighborhood of zero). This dichotomization also mitigates the possibility that ideology impounds constituent preferences because the two groups essentially correspond to Democrats and Republicans, which have known ideological differences (and the article's findings hold if ideology is measured by party instead).

A central question for testing the theories is how legislators voted on laws for which their ideological leanings were contrary to constituent opinion. This requires measuring whether a legislator's ideology was aligned with constituent opinion on that law and assigning an ideological orientation to each district on each law. I first constructed each law's ideological orientation in three ways (for robustness): (i) regressing roll-call votes on legislator party; (ii) regressing

3. For example, in California, petitioners have 90 days after the governor signs a law to collect signatures from eligible voters equal to 5% of the number of votes cast in the previous gubernatorial election. For institutional details across the states, see Gerber (1999).

referendum approval rates on the percentage of votes for the Democratic candidate in the preceding legislative election; and (iii) examining a bill’s sponsors. A law was classified as conservative or progressive if there was a significant connection between voting and party or based on the partisan affiliation of the sponsors.⁴ The estimated classifications conform to expectations—for example, allowing same-sex marriage is classified as a progressive law and restricting labor unions is classified as a conservative law—and the main findings are robust across the three classifications. Using the ideological orientation of a law and a district’s vote on the law, we can infer the district’s ideological orientation on that law. This information can then be used to construct a dummy variable for whether a legislator was ideologically aligned with his or her constituents on a given law:

$$ALIGNED = \begin{cases} 1 & \text{if (Legislator was conservative) \& (District voted conservative);} \\ 1 & \text{if (Legislator was progressive) \& (District voted progressive);} \\ 0 & \text{if otherwise (= NOT ALIGNED).} \end{cases}$$

The *ALIGNED* variable is missing if a law could not be classified ideologically.⁵

Other data

For each district, I collected returns from the previous legislative election. A district’s competitiveness is measured by the vote margin, defined as the difference between votes received by the winner and runner up, divided by their combined votes. For multimember districts, the vote margin is the difference between the incumbent’s vote and the losing candidate with the most votes, divided by their combined votes.

If a state had term limits for state legislators, I determined whether a legislator was in the statutorily mandated final term. California and Michigan have lifetime limits; the other term-limit states allow members to serve again after remaining out of office for one term. I accounted for a 2012 rule change in California.

I captured media attention based on coverage in the state’s leading newspaper. If a law was featured on the front page, it was classified as “high” attention; if it was covered on other than the front page, it was classified as “medium” attention; and if it was not covered it was classified as “low” attention. Some laws received heavy attention, such as the same-sex marriage laws in Maryland and Washington; other were largely ignored by media, such as the election procedure laws in Maryland and South Dakota.

4. Laws were omitted if they had no partisan orientation. Where not otherwise noted, the article uses (i).

5. App. B discusses a continuous version of the alignment variable.

FINDINGS

Overall congruence

Table 1 describes overall congruence. Across all 4,192 votes, congruence was 66.3%. To put this in perspective, note that if legislators voted by flipping a coin, they would have sided with the majority 50% of the time; if they always voted in accord with majority opinion, congruence would have been 100%. The estimate exceeds the 58% that Lax et al. (2019) found for a sample of US Senate votes is just below the 68% that Krimmel et al. (2016) found for a sample of US House votes on gay rights issues and is well below the 79.2% that Giger et al. (2020) found for state legislators. Incomplete congruence between roll-call votes and public opinion may go partway toward explaining the low congruence between policy and public opinion (Lax and Phillips 2012; Matsusaka 2010; Simonovitz et al. 2019).

As mentioned above, congruence is probably lower on referendum laws than others. To gauge the magnitude of the selection effect, we can compare laws that voters upheld with laws they rejected, with the idea that upheld laws are closer to “normal” than repealed laws. Consistent with this conjecture, congruence was 79.6% on upheld laws (similar to Giger et al. [2020]) compared to 48.8% on repealed laws.

Table 1 also reports congruence for other samples. Congruence was almost identical in the upper and lower chamber, contrary to the intuition that lower House members are more attuned to constituent opinion. This is true for both repealed and upheld laws. Congruence was more than 10 percentage points higher for Democratic than Republican legislators, but this was the case only for upheld laws.

On specific laws (not shown in the table), congruence was lowest for South Dakota’s 2015 law reforming the candidate nomination process (23.2%), California’s 2013 gambling law (27.9%), and South Dakota’s 2015 law creating a subminimum wage for youth (30.7%), all three of which voters repealed. Congruence was highest for California’s health insurance law of 2003 (92.4%), which, interestingly, voters also repealed.

Table 1. Percent of Roll-Call Votes that Were Congruent

| | All Laws | Repealed Laws | Upheld Laws | N (All) |
|---------------|----------|---------------|-------------|---------|
| All votes | 66.3 | 48.8 | 79.6 | 4,192 |
| Lower chamber | 66.3 | 49.0 | 79.1 | 2,922 |
| Upper chamber | 66.4 | 48.4 | 80.8 | 1,270 |
| Democratic | 71.4 | 49.9 | 84.7 | 2,291 |
| Republican | 60.3 | 47.8 | 72.2 | 1,900 |

Table 2. Congruence and Alignment of Legislator Ideology and District Preference

| Sample | ALIGNED | NOT ALIGNED | | N |
|---|---------|-------------|------------|-------|
| All roll-call votes (roll-call classification) | 92.9 | 27.9 | $z = 42.7$ | 3,994 |
| All roll-call votes (referendum classification) | 90.1 | 26.3 | $z = 39.7$ | 3,693 |
| All roll-call votes (sponsor classification) | 94.9 | 15.8 | $z = 38.7$ | 2,311 |
| Democratic legislators only | 92.0 | 30.6 | $z = 29.9$ | 2,178 |
| Republican legislators only | 94.4 | 25.7 | $z = 29.9$ | 1,816 |
| Robustness | | | | |
| District majority > 55% | 93.4 | 28.8 | $z = 38.9$ | 3,317 |
| District majority > 60% | 94.6 | 28.6 | $z = 34.2$ | 2,413 |
| District majority > 70% | 97.6 | 24.1 | $z = 24.1$ | 966 |
| Ideology > 0.5 | 93.6 | 25.8 | $z = 42.1$ | 3,589 |
| Ideology > 0.75 | 94.2 | 24.3 | $z = 39.3$ | 2,933 |
| Ideology > 1.0 | 95.3 | 23.5 | $z = 33.8$ | 2,040 |
| District majority > 55% and Ideology > 0.5 | 94.0 | 27.2 | $z = 37.9$ | 2,963 |
| District majority > 60% and Ideology > 0.75 | 95.9 | 24.6 | $z = 30.8$ | 1,686 |
| District majority > 70% and Ideology > 1.0 | 98.7 | 6.3 | $z = 19.8$ | 460 |

Note. The main entries are the percentage of roll-call votes congruent with majority opinion in the district. ALIGNED means that a legislator’s ideological leaning on a law was aligned with the district majority’s opinion. z-statistics test if congruence was the same for ALIGNED and NOT ALIGNED.

The predictive power of legislator ideology

If legislators largely vote according to constituent opinion, then congruence should be the same whether legislators are ideologically in favor or against a law. Table 2 shows to the contrary that legislators’ votes are heavily dependent on their ideologies. In the first row, which classifies the orientation of laws based on roll-call regressions, congruence was 92.9% when legislators agreed with constituent opinion on a law but only 27.9% when legislators preferred a different law than their constituents. The second and third rows, which use different classifications of a law’s orientation, show a similar pattern. For all classifications, legislators usually followed their ideology when it conflicted with constituent opinion. The fourth and fifth rows show congruence separately for Democrats and Republicans. The propensity to vote according to ideology rather than constituent opinion was bipartisan.

The remaining rows examine robustness to potential mis-measurement of district opinion and legislator ideology. The district majority is more likely to be misidentified when district opinion was evenly divided (say 51-49) than one-sided. Rows 6–8 report congruence after deleting districts with 10%, 20%, and 40% margins. Even in the most one-sided districts, legislators usually voted according to their ideologies even if it ran against district opinion. In terms of legislator ideology, misclassification is most likely for legislators with NPAT scores near the dividing line of zero. Rows 9–11 report congruence after deleting legislators with NPAT scores in different neighborhoods around zero—the findings are robust to this as well.

Finally, the bottom three rows apply the district majority and legislator ideology filters simultaneously, with each successive row removing more observations that may be mismeasured. The pattern becomes even more pronounced with these filters: in the bottom row, congruence was 98.7% when a legislator and district were ideologically aligned and only 6.3% when they were not aligned.

The correlation between roll-call votes and ideology could be spurious if some legislators had a general tendency to vote noncongruently, and those legislators happened to represent nonaligned districts. To allay this concern, we can compare the votes of the same legislator on two issues, one in which the legislator was aligned and one in which the legislator was not aligned with district opinion, using fixed-effect regressions of the form:

$$CONG_{il} = \alpha_0 \cdot ALIGNED_{il} + \alpha_1 \cdot DISTRICT_MAJORITY\%_{il} + \gamma_i + \lambda_l, \tag{1}$$

where i indexes a law, l indexes a legislator, the dependent variable is congruence, $ALIGNED$ is a dummy defined above, $DISTRICT_MAJORITY\%$ is the percentage of referendum votes on the winning side, and γ and λ are legislator and law-chamber fixed effects, respectively.⁶ The legislator fixed effect

6. Although both $CONG$ and $ALIGNED$ are functions of the district majority, this does not mechanically induce a sign or magnitude for α because district majority can increase or decrease both variables.

Table 3. Fixed Effects Regressions of Congruence on Aligned

| | All Laws | Laws Upheld | Laws Repealed |
|----------------------|------------------|------------------|------------------|
| ALIGNED | 63.7*** (4.3) | 66.4*** (6.2) | 68.3*** (7.8) |
| Size of majority (%) | .26 (.23) | -.23 (.20) | -.04 (.25) |
| N | 3,028 | 1,517 | 809 |

Note. Each column is a regression of model 1. The dependent variable is a dummy = 1 if the roll-call vote was congruent. *ALIGNED* = 1 if the legislator’s ideology and district opinion were aligned on the law. Regressions include both legislator and law-chamber fixed effects. Standard errors clustered at the law-chamber level are in parentheses. Coefficients are scaled by 100 to represent percentages.

* $p < .10$.
 ** $p < .05$.
 *** $p < .01$.

also alleviates the concern that ideology, and hence alignment, incorporates induced district preferences, district competition, and so forth. Table 3 shows the estimates for three samples: all laws, only upheld laws, and only repealed laws.⁷ Ideological alignment increased congruent voting by 63.7%–68.3%, depending on the sample.⁸

An alternative approach is to regress a dummy for a roll-call vote in favor of a law (*ROLL_YES*) on the legislator’s and district’s ideological preferences and an interaction term:

$$\begin{aligned}
 ROLL_YES_{it} = & \beta_0 \cdot IDEO_YES_{it} + \beta_1 \cdot DISTRICT_YES_{it} \\
 & + \beta_3 \cdot IDEO_YES_{it} \cdot DISTRICT_YES_{it} \quad (2) \\
 & + \gamma_i + e_{it},
 \end{aligned}$$

where *IDEO_YES* is a legislator’s NPAT score expressed as a positive number if ideologically in support and a negative number if ideologically opposed, and *DISTRICT_YES* is the percent of votes cast in favor of the law in the referendum. A legislator fixed effect is not included because there is no reason to believe that legislators have law-invariant tendencies to vote yes. This specification uses the full distribution of ideology and district opinion rather than compressing them into dummy variables. Because the regression coefficients are difficult to interpret individually, table 4 reports the implied probability of voting yes for different values of the explana-

7. One can also construct a continuous alignment variable, although the coefficients then lack a natural interpretation. See app. B for a discussion and version of table 3 with a continuous variable.

8. The number of observations in columns 2 and 3 do not sum to column 1 because the fixed effects estimator drops singletons, and there are more singletons in the subsamples.

tory variables (the regression itself is in app. C). For example, the top left cell means that a legislator had a 20.2% probability of voting in favor of a law if 40% of the district supported it and the legislator was ideologically opposed with $|NPAT| = 1$.

Looking down the columns gives a sense of the importance of district opinion for a legislator’s vote. A legislator was more likely to support a law, holding constant his or her own preferences, as constituent support rose. The magnitude of the effect was strongest if the legislator was ideologically opposed to the law: an increase in district favorable support from 40%–60% (approximately the 25th and 75th percentiles) made an ideologically opposed legislator 29.7% more likely to vote yes. A similar increase in district favorable made a legislator ideologically aligned with the law only 16.6% more likely to vote for it. Looking across the rows gives a sense of the importance of ideology. We see sizable effects for all levels of constituent support—holding constant district opinion, a switch from ideological opposition to support with $|NPAT| = 1$ (approximately the mean for both parties) increased the likelihood of voting yes by about 50%. These estimates reinforce the conclusion that legislator ideology was far more important than constituent opinion in explaining roll-call votes.

Logically, noncongruent roll-call votes could happen if legislators are ideologically out of step with their constituents or if they are ideologically in step but constituent preferences on the law are out of step with what their own ideologies would normally prescribe. To determine which of these cases is most common, I classify each district ideologically as conservative or progressive according to the majority preference in the preceding presidential election. I then compare each district’s ideology so measured with its legislator’s ideology and with the district’s referendum vote.

Table 4. Estimated Probability (%) of a Roll-Call Vote in Favor of a Law

| | | Legislator Ideology | | |
|--------------------|-----|---------------------|---------------|---------------|
| | | -1 (Opposed) | 0 | 1 (In Favor) |
| District favorable | 40% | 20.2 (1.3) | 49.0 (1.2) | 77.8 (2.7) |
| | 50% | 35.1 (3.3) | 60.6 (1.1) | 86.1 (1.3) |
| | 60% | 49.9 (5.9) | 72.1 (2.9) | 94.4 (1.2) |

Note. The entries show the probability (expressed as percent) of a yes vote from a legislator with a given ideological score and district opinion. Estimates are linear combinations of coefficients from model 2.

Table 5. District Ideology and District Voting

| Percent of Observations in Which: | All | | Not |
|---|-----------------|-----------------|---------------|
| | Votes | Congruent | Congruent |
| District ideology = legislator ideology | 87.9 [3,053] | 87.6 [2,219] | 88.6 [834] |
| District referendum vote = district ideology | 61.8 [3,010] | 77.5 [2,195] | 19.5 [815] |

Note. District ideology (progressive, conservative) was measured by whether it cast a majority of its presidential vote for the Democratic or Republican candidate. Roll-call votes were congruent if they matched the district’s majority opinion. The number of observations is in square brackets.

The top row of table 5 shows that districts were ideologically aligned with their legislators 87.9% of the time, and this percentage was essentially the same for congruent and noncongruent votes. Noncongruence stemmed from an ideological mismatch between legislators and constituents. The second row shows that districts voted according to their ideology 61.8% of the time overall, but only 19.5% of the time on issues with noncongruent roll-call votes. This tells us that noncongruent roll-call voting most often occurred in districts that deviated from their traditional ideological positions (e.g., Republican districts favoring the minimum wage). In other words, much noncongruence appears to have been due to ideological “impurity” among voters coupled with elected representatives sticking to their ideological guns.

The predictive power of reelection and party pressure

The dependence of roll-call voting on a legislator’s ideology is consistent with predictions from selection models. This section explores how well the data conform to predictions of election-incentives models. I begin with electoral competitiveness, an important potential source of pressure on legislators (Pildes 2021). Figure 1 shows the relation between congruence and vote margin nonparametrically using kernel regressions. If competition is important for congruence, we expect congruence to increase as the vote margin declined, especially when the legislator and district were not aligned. To the contrary, when they were not aligned, congruence was actually higher in the less competitive districts, and the congruence gap between aligned and not-aligned legislators did not close even for the most competitive districts with a margin ≈ 0 .⁹

9. All of the findings in this section are similar if districts with unopposed incumbents (vote margin = 1) are deleted; see app. D for regressions.

Table 6 reports regressions of congruence on several variables that are motivated by election-incentive models. Regression 1 excludes and regression 2 includes legislator fixed effects. Because the media variables are law-specific, law-specific fixed effects are not included. The ideological alignment variable is included in all regressions, and its coefficient is sizable and statistically significant in all cases. The first pressure variable, vote margin, is close to statistical significance only in regression 2. Statistical significance is less important than the magnitude, which in regression 1 indicates that a 1-percentage-point increase in vote margin was associated with 0.02% lower probability of a congruent roll-call vote. This implies, for example, that a change in vote margin from 0%–40%—substantial by any reckoning—was associated with less than a 1% change in the probability of a congruent vote. The coefficient is larger in column 2 but still quantitatively trivial. The magnitudes are tiny for any values in the confidence intervals.

Another way to assess the importance of electoral pressure is by examining how roll-call votes change when a legislator is ineligible to run for reelection (Besley and Case 2003; Fourniaies and Hall 2022; Poole and Romer 1993). Ashworth (2012, 194) observed that “[t]he most elementary prediction of the accountability model is that a term-limited incumbent, who cannot derive any benefit from impressing the voter, will not be responsive to voters.” To capture this, the regressions include a dummy variable equal to 1 if a legislator was unable to stand for reelection because of term limits. The coefficients are negative, indicating that congruence was lower in the last term, but again the magnitudes are quite small and never statistically significant. Congruence was 1.4% lower in the last term in regression 1 and 2.8% lower in regression 2. These estimates do not offer much support for the idea that term limits significantly release reelection pressures that otherwise induce congruent voting.¹⁰

The vote margin and term limits evidence suggests that reelection pressure was not a major influence on individual roll-call votes. Giger et al. (2020) similarly found little connection between roll-call vote congruence and vote margin and found that legislators who did not stand for reelection (which compounds term limits and voluntary decisions) were 4% less likely to cast a congruent vote.¹¹

10. These findings are robust to distinguishing states that impose lifetime bans from those with waiting periods. Kogan (2016) argues that waiting periods do not provide effective limits but rather induce a form of musical chairs among office holders.

11. In unreported results, I found no connection between election proximity and congruence, contrary to expectation if voters remember actions taken shortly before an election better than those years earlier.

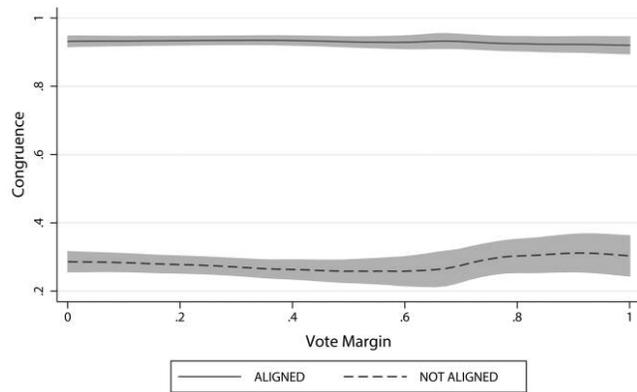


Figure 1. Congruence and vote margin. Kernel regressions. A legislator’s roll-call vote was congruent if it matched the district’s majority opinion. Vote margin is the percentage difference in votes received by the top two candidates in the previous election. Regressions differ according to whether a legislator’s ideology was aligned with district opinion on an issue. The 95% confidence intervals are shaded.

Another prediction of election-incentive theories is that public attention enhances congruence or, put the other way around, that noncongruence happens when voters are not watching. There is some causal evidence on this: Berry and Howell (2007) found that South Carolina voters held school board members more accountable for test scores when local newspapers devoted attention to test scores, and Snyder and Stromberg (2010) found that Congressmen worked harder when they were covered more often by local newspapers. The regressions include dummy variables for high and medium media attention, with low media attention the omitted category. Consistent with theoretical predictions, congruence was 16.9 percentage points higher on high-attention issues and 10.7 percentage points higher on medium-attention issues, statistically significant in regression 1. In regression 2, with legislator fixed effects, the coefficients are smaller in magnitude and statistically insignificant. Overall, this offers mixed support for the importance of attention effects.

Another form of pressure may come from party leaders. Snyder and Groseclose (2000) argued that parties are more likely to apply pressure when the roll-call vote is close than when it is lopsided. Intuitively, if it is costly for party leaders to persuade a legislator to vote against constituent preferences, then leaders would be more inclined to pay the cost when a vote is pivotal than when they already have a lopsided majority. Figure 2 shows congruence by roll-call margin for aligned and not-aligned legislators. For aligned legislators, there is not much of a relation, but for not-aligned legislators, there is a huge decline in congruence when the roll call is close to failure. This fits the story that party leaders call on legislators who already are ideologically disposed to vote against their constituents when the vote is close.

Returning to table 6, the regressions include a variable equal to the roll-call margin on the bill. Regression 1 indicates, again consistent with theory, that congruence was lower for laws that narrowly passed: a change in the roll-call vote margin from 10% to 0% was associated with 2.9-percentage-points lower congruence. This coefficient is statistically significant in regression 1 but smaller and statistically insignificant in regression 2, again mixed evidence for the effect of pressure. The evidence for party pressure is weaker in table 6 than in figure 2 because the regressions do not condition on ideological alignment.

Columns 3 and 4 estimate the regressions separately for laws that were upheld and repealed, to allow for the possibility that the factors determining congruence were different for repealed than approved laws. Regression 3 contains an odd finding that close roll-call votes were associated with higher congruence, statistically significant; but overall, the basic picture of small and insignificant effects for the pressure variables, and a large statistically significant coefficient for ideological alignment remains.

Table 6. Congruence Regressions

| | (1) | (2) | (3) | (4) |
|-----------------------|------------------|------------------|------------------|------------------|
| ALIGNED | 65.6*** (3.8) | 69.5*** (3.6) | 66.5*** (5.6) | 71.0*** (6.2) |
| | -.02 | -.09* | -.07* | .03 |
| Vote margin (%) | (0.03) | (0.05) | (.03) | (.11) |
| | -1.4 | -2.8 | -13.7 | 3.8 |
| Last term | (2.3) | (4.3) | (11.8) | (2.9) |
| | 16.9*** | 4.6 | -1.7 | 6.5* |
| High attention | (4.4) | (3.7) | (1.9) | (3.5) |
| Medium attention | 10.7** | -1.4 | -.1 | -3.6 |
| | (4.3) | (3.7) | (2.2) | (4.5) |
| Roll-call margin (%) | .29*** (.09) | .09 (.13) | -.24*** (.07) | -.05 (.17) |
| District majority (%) | .23 (.16) | -.42** (.18) | -.36* (.19) | -.02 (.30) |
| N | 3,992 | 3,027 | 1,517 | 807 |
| Legislator FE | No | Yes | Yes | Yes |
| Sample laws | All | All | Upheld | Repealed |

Note. Each column is a regression in which the dependent variable is a dummy = 1 if a legislator’s roll-call vote was congruent with district opinion. Coefficients and standard errors are scaled by 100. Standard errors are clustered at the law-chamber (column 1) and law-chamber and legislator level (columns 2-4). FE, fixed effects.

* $p < .10$.
 ** $p < .05$.
 *** $p < .01$.

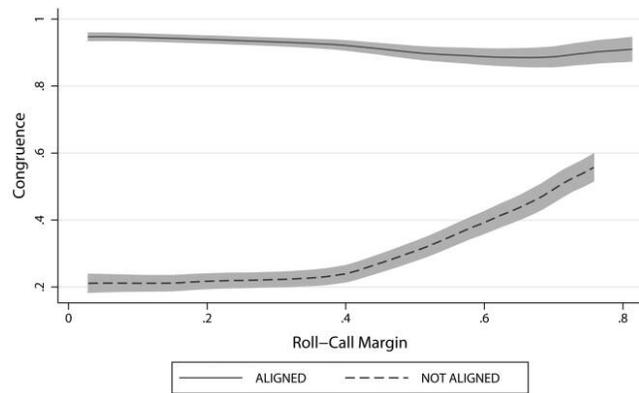


Figure 2. Congruence and roll-call vote margin. Kernel regressions. A legislator’s roll-call vote was congruent if it matched the district’s majority opinion. Roll-call margin is the ratio of votes for and against the law as a fraction of total votes. Regressions differ according to whether a legislator’s ideology was aligned with district opinion on an issue. The 95% confidence intervals are shaded.

Honest mistakes

Noncongruence could also happen because legislators misread constituent preferences. “Honest mistakes” are more likely when constituent preferences are difficult versus easy to ascertain. Because the majority position is more difficult to determine when district opinion is evenly divided than when it is one-sided (imagine a legislator randomly sampling a subset of constituents—the sample mean is less precise when opinion is divided), noncongruent voting should occur primarily in districts with closely divided opinion if honest mistakes are important. However, table 2, which includes a variable for size of the district majority, shows that noncongruence is common even in districts with very one-sided opinions.

Alaska’s wolf hunting law of 2000 (SB 267) is an interesting illustration of noncongruent voting that was not a mistake. The law passed with large majorities in both the Senate (14-5) and House (27-11), yet voters rejected the law statewide and in 29 of 40 House districts. Congruence was only 53% in the House and 47% in the Senate. For this law, we can be confident that noncongruent voting was not an honest mistake because SB 267 overrode an existing law banning precisely this form of hunting that voters had approved by initiative less than four years earlier. When voting, each legislator had a clear expression of his or her constituent’s opinion on the issue from the previous initiative election.

PUTTING THE PIECES TOGETHER

This article develops a measure of representation based on referendum election returns and uses it to revisit some central questions in the representation literature. The main innovation is measuring congruence by comparing a legis-

lator’s roll-call vote with the votes cast by the legislator’s constituents when a referendum was held on the law. Descriptive evidence is common in representation research; to this literature I add the estimate that 66% of roll-call votes were congruent with majority opinion, a somewhat underwhelming number that is nevertheless consistent with other studies. My estimates, like others in the literature, come from a set of issues that are likely to be more controversial than the universe of issues, so they are best seen as a lower bound on the overall representativeness of laws.

By far the most powerful explanatory variable in my data is legislator ideology: when legislators’ ideology was aligned with district opinion, they cast congruent votes 92.9% of the time; when their ideology was not aligned, they cast congruent votes only 27.9% of the time. Digging deeper, I find that noncongruence was not rooted in a core ideological divergence between legislators and their districts—legislators and their constituents were usually aligned ideologically—but occurred when citizens deviate from the conventional prescription of their ideology on a specific policy—their representatives tended to remain ideologically pure.

In contrast, variables traditionally associated with reelection pressure—electoral competition, term limits, media attention—do not reliably predict congruence in my data. This suggests that fear of losing reelection is not a primary driver of roll-call votes, and squares with other evidence that the consequences of individual roll-call votes for reelection is small (Ansolabehere and Kuriwaki 2022; Hanretty et al. 2021; Rogers 2017; Tausanovitch and Warshal 2018). However, the absence of a connection between reelection incentives and *roll-call votes* does not imply that reelection pressure is unimportant for other aspects of legislative behavior; as Fournaies and Hall (2022) show, reelection pressure influences

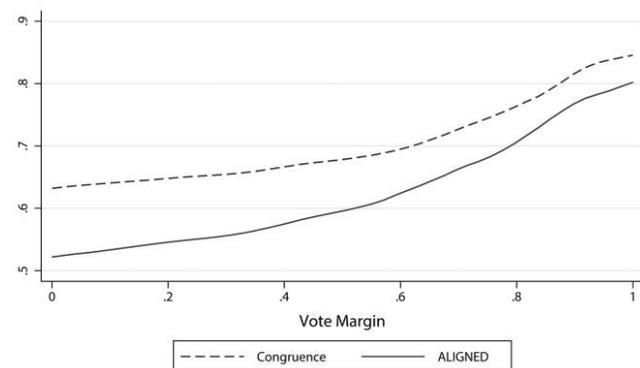


Figure 3. Congruence and alignment by vote margin. Kernel regressions. A legislator’s roll-call vote was congruent if it matched the district’s majority opinion. ALIGNED means that a legislator’s ideology agreed with district majority opinion. Vote margin is the difference in votes received by the top two candidates in the previous election as a fraction of top two votes. Legislators with uncontested elections are omitted.

metrics of legislative effort such as bill sponsorship and attendance. The other factor that displays some hints of explanatory power is party pressure: on close votes party leaders may “whip” legislators to support the party’s policy, even against their constituents’ preferences. I find some support for the prediction that congruence is higher when roll-call margins are narrow, but only for legislators that are not aligned with their constituents on the issue, and even those effects are modest in magnitude.

Despite the absence of reelection incentive effects, roll-call votes usually do represent citizen preferences in my sample. This happens because of a selection effect—voters select ideologues with whom they are usually aligned (Fearon 1999). A conclusion of this sort has implications for how we understand the process of policy change. McCarty, Poole, and Rosenthal (2013) argue that if elections are used to select ideologues more than induce pressure on incumbents, then major policy changes will require replacing legislators not by changing the minds of incumbents. This is what Fedaseyeu, Gilje, and Strahan (2019) found for energy policy after the development of fracking technology.

This perspective on representation also has implications for how we think about election reform, especially electoral competition. Consider figure 3, which plots congruence and ideological alignment against vote margin. The upper line shows that congruence was actually higher in uncompetitive than competitive districts. The lower line shows why: legislators were more likely to be ideologically aligned with constituents in uncompetitive districts. This is because one-sided districts were more likely to select legislators who shared the district’s preferences.

If elections are mainly about selection, then the value of competitive districts does not lie in inducing incumbents to pay more attention to constituents but in choosing ideologues that are aligned with constituents. Competitive districts might even undermine representation from this perspective: in a one-sided district (say, one composed entirely of Democrats), the winner almost surely will be ideologically aligned with the district; on the other hand, in a competitive district, the chance is higher that the winner will be ideologically out of step with the district’s majority. Somewhat counterintuitively, then, creating a map of one-sided (all Democratic or all Republican) districts could enhance representation by ensuring that each district gets a representative that shares its ideological position. Creating a map of 50-50 districts could result in many citizens ending up with ideologically misaligned representatives. Which map would lead to better representation overall is less obvious than conventional intuition suggests and may depend on the equilibrium of the entire legislature, not just the quality of the connection between

individual districts and their representatives (Gilligan and Matsusaka 2006).

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