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

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This paper develops a measure of representation that uses referendum returns to capture constituent opinion, and applies it to 4,094 roll-call votes on 32 laws in nine states. Roll-call votes were congruent with majority/median opinion in a district 66 percent 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.

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1. Introduction

The purpose of this paper 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 using 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 noncongruent 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 data set covering 32 laws in nine states, and determine if each of the 4,094 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 (Erikson et al. 1993; Tausanovitch and Warshaw 2013, 2014; Caughey and Warshaw 2022), but direct measures of congruence find that specific policies match the preferences of the majority only about half the time (Matsusaka 2010, 2020; Lax and Phillips 2012; Lax et al. 2019; Simonovitz et al. 2019). I find that legislators voted congruent with majority opinion 66 percent of the time, in the range of roll-call congruence estimates produced by other studies (Krimmel et al. 2016, Lax et al. 2019, Giger et al. 2020). Because my sample contains laws that were challenged by

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referendum, and thus were likely to have a representation failure, 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 the factors 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 (Downs 1957; Osborne and Slivinsky 1996; Besley and Coate 1997). In contrast, "accountability" theories hold that legislators represent constituent interests in order to increase their chance of re-election (Barro 1973; Ferejohn 1986). Both theories are surely relevant empirically; the goal here is quantify their relative importance.

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 percent of the time – and appears in more demanding regression models with various fixed effects and controls. On the other hand, re-election incentives play almost no detectable role. Legislators were no more likely to cast a congruent than noncongruent vote if they represented a competitive district, were not subject to term limits, or media attention was high, all of which should predict congruence according to incentive theory. 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 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 re-election 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 re-election.

2. Theories: Selection, Re-election Incentives, 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 (Osborne and Slivinsky 1996; Besley and Coate 1997). 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 re-election. 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 for voters 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 re-election on past behavior (Banks and Sundaram 1993; Fearon 1999; Ashworth 2012).

Both of these theories enjoy empirical support, but views on their relative importance differ. After reviewing a variety of findings, Fearon (1999, p. 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," in the words of Poole (1997, p. 435).

In contrast, Kousser et al. (2007, p. 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, typically finding that there are statistically detectable costs from voting against constituents but too small to generate meaningful incentives (Rogers 2017; Tausanovitch and Warshal 2018; Hanretty et al. 2021; Ansolabehere and Kuriwaki 2022). Several studies have used term limits as an exogenous shifter since legislators lose re-election 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

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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.

My research strategy is to assess how well each theory's core predictions fit the data. For the selection model, the key prediction is that legislators vote according to their ideological preferences, regardless of constituent preferences. For re-election incentives, the key prediction is that legislators are more likely to vote according to constituent preferences when re-election 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 pressure is costly to exert, leaders are more likely to pay the cost when they need a member's vote to win than when they have already secured a large majority.

A final possibility is that legislators sometimes misperceive what constituents want, and make "honest mistakes" when voting. Because legislators have to vote on hundreds of bills and represent thousands of people, its understandable that they may occasionally mistake constituent preferences. Consistent with this idea, research shows that legislators 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.

3. Methods and Data

A. 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 proposed law. I approximate 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 have to estimate constituent preferences at the time of the roll call, just like 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 capturer consistency of beliefs held by legislators and

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[1] Examples: Gerber and Lewis (2004), Clinton (2006), Bafumi and Herron (2010), Masket and Noel (2012), Kousser et al. (2018).

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 paper 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 data set – it does not reveal representation at the level of an individual legislator or policy vote. There are theoretical difficulties in using the coefficients to identify variance in representation across subgroups (Achen 1977; Matsusaka 2001).

Much of what we know about congruence comes from studies that use opinion surveys to capture constituent preferences instead of referendums.[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 if they have real consequences as opposed to "cheap talk" to a pollster. Referendum votes are informed by

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

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 with opinion surveys. Also, referendum data may be less representative of the general population than opinion surveys because not every person votes – but this could be a virtue from a theoretical perspective since both selection and incentive theories predict that only the preferences of those who actually vote should matter. In the end, research using opinion and referendum data seem complementary, and there is value in having both types of studies to cross-check each other, and be able to explore different sets of policies.

B. *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; 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

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

returns by legislative district; these were available for 32 referendums. For each law, I identified

the (final) roll-call votes in the legislature, giving a final sample of 4,094 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 the Appendix A.

The referendums were held in Alaska, California, 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 high-profile

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 hunting of wolves 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 repealed 18 of the 32 laws in question.

All representation studies, to the best of my knowledge, 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 something of a lower

bound of overall congruence. Selection is less obviously a concern when seeking to identify the

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factors that predict noncongruent voting – we expect to find more noncongruent factors in my sample, but do not expect their effects to have been different.

C. Ideological Alignment

I measure legislator ideology using 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 numbers for relatively progressive positions and positive for relatively conservative positions. The mean score is -1.19 for Democrats and 0.97 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 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; U. S. 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 2008).

To mitigate the concern that ideal points are noisy estimates of ideology, for part 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 paper's findings hold if ideology is measured by partisan affiliation instead.)

A central question 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 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 a progressive law and restricting labor unions is 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. Then a dummy variable for whether a legislator was ideologically aligned with his or her constituents on a given law can be constructed:

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

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

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[4] Laws were omitted if they had no partisan orientation. Where not otherwise noted, the paper uses (i) to measure ideology.

D. 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; 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. Findings

A. Overall Congruence

Table 1 describes overall congruence. Across all 4,094 votes congruence was 65.8 percent. For perspective on this number, note that if legislators voted by flipping a coin, they would have sided with the majority 50 percent of the time; if they always voted in accord with

majority opinion congruence would have been 100 percent. The estimate exceeds the 58 percent that Lax et al. (2019) found for a sample of U.S. Senate votes; is just below the 68 percent that Krimmel et al. (2016) found for a sample of U.S. House votes on gay rights issues; and is well below the 79.2 percent that Giger et al. (2020) found for state legislators. The incomplete congruence between roll-call votes and public opinion may go part way toward explaining the low congruence that has been found between *policy* and public opinion in the states (Matsusaka 2010; Lax and Phillips 2012; 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 congruence on 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 79.2 percent on upheld laws (same as in Giger et al. (2020)) compared to 48.8 percent on repealed laws.

Table 1 also reports congruence for other samples. Congruence was almost the same 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.

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

	All Laws	Repealed Laws	Upheld Laws	N (All)
All votes	65.8	48.8	79.2	4,094
Lower chamber	65.8	49.0	78.6	2,859
Upper chamber	65.7	48.4	80.4	1,235
Democratic	70.8	49.9	84.4	2,232
Republican	59.6	47.8	71.5	1,861

On specific laws (not shown in the table), congruence was lowest for South Dakota's

2015 law reforming the candidate nomination process (23.2 percent), California's 2013 gambling

law (27.9 percent), and South Dakota's 2015 law creating a subminimum wage for youth (30.7

percent, all three of which voters repealed. Congruence was highest for California's health

insurance law of 2003 (92.4 percent), which, interestingly, voters also repealed.

B. *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 vote regressions, congruence was 92.8 percent

when legislators agreed with constituent opinion on a law, but only 28.0 percent 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 mismeasurement of district

opinion and legislator ideology. The district majority is more likely to be misidentified when

district opinion is evenly divided (say 51-49) than one-sided. Rows 6-8 report congruence after

deleting districts with a 10 percent, 20 percent, and 40 percent margin. Even in the most one-

sided districts, legislators usually voted according to their ideologies even if it ran against

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

Sample	ALIGNED	NOT ALIGNED		N
All roll call votes (roll call classification)	92.8	28.0	$z = 42.2$	3,926
All roll call votes (referendum classification)	86.6	29.1	$z = 35.1$	3,626
All roll call votes (sponsor classification)	94.8	15.7	$z = 38.1$	2,244
Democratic legislators only	92.0	30.5	$z = 29.8$	2,146
Republican legislators only	94.3	25.8	$z = 29.4$	1,787
Robustness:				
District majority > 55%	93.2	28.8	$z = 38.5$	3,270
District majority > 60%	94.6	28.6	$z = 33.9$	2,384
District majority > 70%	97.6	24.1	$z = 23.9$	957
|Ideology| > 0.5	93.6	25.8	$z = 41.6$	3,522
|Ideology| > 0.75	94.2	24.2	$z = 38.8$	2,867
|Ideology| > 1.0	95.2	23.4	$z = 33.3$	1,979
District majority > 55% and |Ideology| > 0.5	93.9	27.2	$z = 37.5$	2,916
District majority > 60% and |Ideology| > 0.75	95.8	24.6	$z = 30.5$	1,658
District majority > 70% and |Ideology| > 1.0	98.7	6.3	$z = 19.6$	457

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.

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 neighborhood 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 that may be mismeasured. The pattern becomes even more pronounced with these filters: in the bottom row, congruence was 98.7 percent when a legislator and district were ideologically aligned, and only 6.3 percent when they were not aligned.

The correlation between roll-call votes and ideology could be spurious if, for example, 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*

Table 3. Fixed Effects Regressions of Congruence on Aligned

	All Laws	Laws Upheld	Laws Repealed
ALIGNED	63.7***	66.4***	68.3***
	(4.3)	(6.2)	(7.8)
Size of majority (%)	0.26	-0.23	-0.03
	(0.22)	(0.20)	(0.25)
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 referendum level are in parentheses. Coefficients are scaled by 100 to represent percentages. Significance levels: * = 10 percent, ** = 5 percent, *** = 1 percent.

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:

(1) $CONG_{il} = \alpha_0 \cdot ALIGNED_{il} + \alpha_1 \cdot SIZE\_OF\_MAJORITY\%_{il} + \gamma_i + \lambda_l,$

where l indexes a legislator, i indexes a law, the dependent variable is congruence, ALIGNED is a dummy defined above, SIZE_OF_MAJORITY% is the percentage of referendum votes on the winning side, and γ and λ are legislator and law-chamber fixed effects, respectively.[5] The legislator fixed effect 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. Ideological alignment increased congruent voting by 63.6 to 68.3 percent, depending on the sample.

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:

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[5] 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.

(2) $ROLL\_YES_{il} = \beta_0 \cdot IDEO\_YES_{il} + \beta_1 \cdot DISTRICT\_YES_{il} + \beta_3 \cdot IDEO\_YES_{il} \cdot DISTRICT\_YES_{il} + \gamma_i + e_{il}$,

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 explanatory variables (the regression itself is in Appendix B). For example, the top left cell means that a legislator had a 0.200 probability of voting in favor of a law if 40 percent 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 to 60 percent (approximately moving from the 25th to the 75th percentile) made an ideologically opposed legislator 30.3 more likely to

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

		Legislator Ideology		
		-1 (opposed)	0	1 (in favor)
	40%	20.0	49.1	78.2
		(1.4)	(1.2)	(2.7)
District Favorable	50%	35.1	60.8	86.5
		(3.4)	(1.1)	(1.3)
	60%	50.3	72.5	94.8
		(6.0)	(3.0)	(1.2)

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

vote yes. A similar increase in district favorable made a legislator ideologically in favor of the law only 16.6 percent more likely to vote for it. Looking across the rows gives a sense of the importance of ideology. We see sizeable effect 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 percent. 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 use the majority vote in the preceding presidential election as a proxy for district ideology. I then compare each district's ideology so measured with its legislator's ideology and with the district's referendum vote. The top row of Table 5 shows that districts were ideologically aligned with their legislators 87.9 percent of the time, and this percentage was essentially the same for congruent and noncongruent votes – noncongruence was not usually the result of an ideological mismatch between legislators and their constituents. The second row shows that districts voted according to their ideology 61.8 percent of the time overall, but only 19.5 percent of the time on issues with noncongruent roll-

Table 5. District Ideology and District Voting			
Percent of observations in which:	All votes	Congruent	Not 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 vote were congruent if they matched the district's majority opinion. The number of observations is in square brackets.

call votes. This tells us that noncongruent roll-call voting primarily happened when districts

deviated from their traditional ideological positions (e.g., Republican districts supporting a

minimum wage increase). In other words, the root cause of much noncongruence appears to

have been ideological "impurity" among voters coupled with elected representatives sticking to

their ideological guns.

C. The Predictive Power of Re-Election Incentives 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

Figure 1. Congruence and Vote Margin

Note. 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. 95% confidence intervals are shaded.

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.[6]

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

Table 6. Congruence Regressions				
	(1)	(2)	(3)	(4)
ALIGNED	65.4***	69.5***	66.5***	71.0***
	(3.8)	(3.6)	(5.6)	(6.2)
Size of majority (%)	0.23	-0.42**	-0.36*	-0.02
	(0.16)	(0.18)	(0.19)	(0.30)
Vote margin (%)	-0.02	-0.09*	-0.07*	0.03
	(0.03)	(0.05)	(0.03)	(0.11)
Last term	-1.5	-2.8	-13.7	3.8
	(2.3)	(4.3)	(11.8)	(2.9)
High attention	17.1***	4.6	-1.7	6.5*
	(4.5)	(3.7)	(1.9)	(3.5)
Medium attention	10.3**	-1.4	-0.1	-3.6
	(4.4)	(3.7)	(2.2)	(4.5)
Roll-call margin (%)	0.30***	0.09	-0.24***	-0.05
	(0.09)	(0.13)	(0.07)	(0.17)
N	3,925	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 (1) and law-chamber and legislator level (2)-(4). Statistical significance: * = 10 percent; ** = 5 percent; *** = 1 percent.

[6] The pattern is similar if districts with unopposed incumbents (Vote Margin = 1) are deleted.

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 is sizeable and statistically

significant in all cases. The first pressure variable, vote margin, is close to statistical significance

only in (2). Statistical significance is less important than the magnitudes, which in regression (1)

indicate that 1 percent point increase in vote margin was associated with 0.02 percent lower

probability of a congruent roll-call vote. This implies that a change in vote margin from zero

percent to 40 percent – substantial by any reckoning – was associated with less than a 1 percent

change in the probability of a congruent vote. The coeffficient 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 changes when a legislator is ineligible to run for re-election (Poole and Rosenthal 1993;

Besley and Cast 2003; Fournaies and Hall 2022). Ashworth (2012, p. 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 in include a dummy variable equal to one if a legislator was unable

to stand for re-election 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 1.5 percent lower in the last term in (1) and 2.8 percent
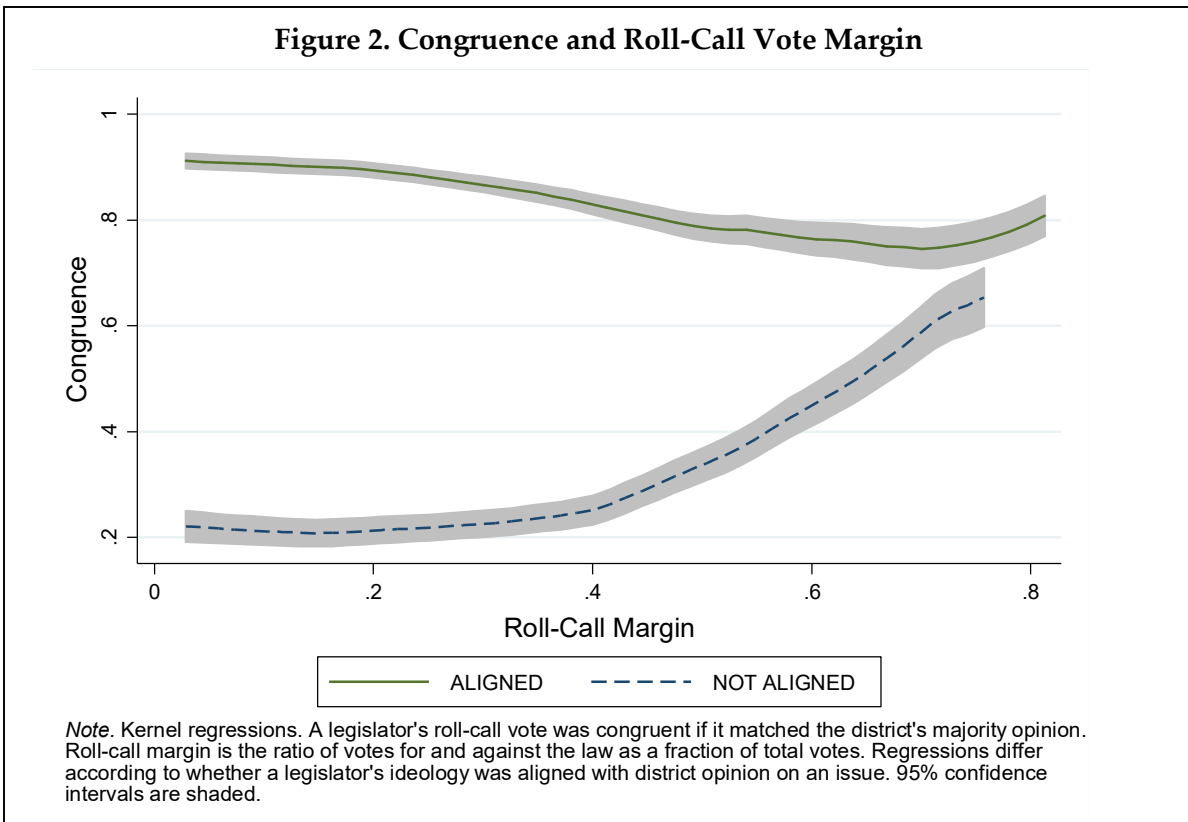
lower in (2). These estimates do not support for the idea that term limits significantly release re-election pressures that otherwise induce congruent voting.[7]

The vote margin and term limits evidence suggests that re-election pressure was not a major influence on individual roll-call votes. Giger et al. (2020) similarly find little connection between roll-call vote congruence and vote margin; and they find that legislators who did not stand for re-election (which compounds term limits and voluntary decisions) were 4 percent less likely to cast a congruent vote.[8]

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 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 17.1 percentage points higher on high-attention issues and 10.3 percentage points higher on medium-attention issues, statistically significant in regression (1). In regression (2), with

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[7] 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.

[8] 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 2. Congruence and Roll-Call Vote Margin

Note. 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. 95% confidence intervals are shaded.

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) argue 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 the vote was pivotal than when they already had 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 the theory, that congruence was lower for laws that were close votes: a change in the roll-call vote margin from 10 percent to zero percent was associated with 3 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 repealed laws were somehow fundamentally different those typical 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 typically insignificant effects for the pressure variables, and a large statistically significant coefficient for ideological alignment remains.

D. Honest Mistakes

Noncongruence could also happen because legislators make mistakes about 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 shows that noncongruence is common even in districts with very one-sided opinion.

Alaska's wolf hunting law of 2000 (SB 267) provides an interesting example 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 percent in the House and 47 percent in the Senate. We can be confident that the 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.

5. Putting the Pieces Together

This paper 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 legislator's roll-call vote on a law with the votes cast by the legislator's constituents when a referendum was held on that law. Descriptive evidence is common representation research; to this literature I add the estimate that 66 percent 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 are best thought of 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 92.8 percent of the time; when their ideology was not aligned they cast congruent votes only 28.0 percent of the time, meaning that they did not represent constituent preferences 72.0 of the time. Digging deeper, I find that noncongruence is not rooted in a core ideological divergence between legislators and their districts – legislators and their constituents are usually aligned ideologically – but occurs when citizens sometimes deviate from their ideology on a specific policy – their representatives tend to remain ideologically pure.
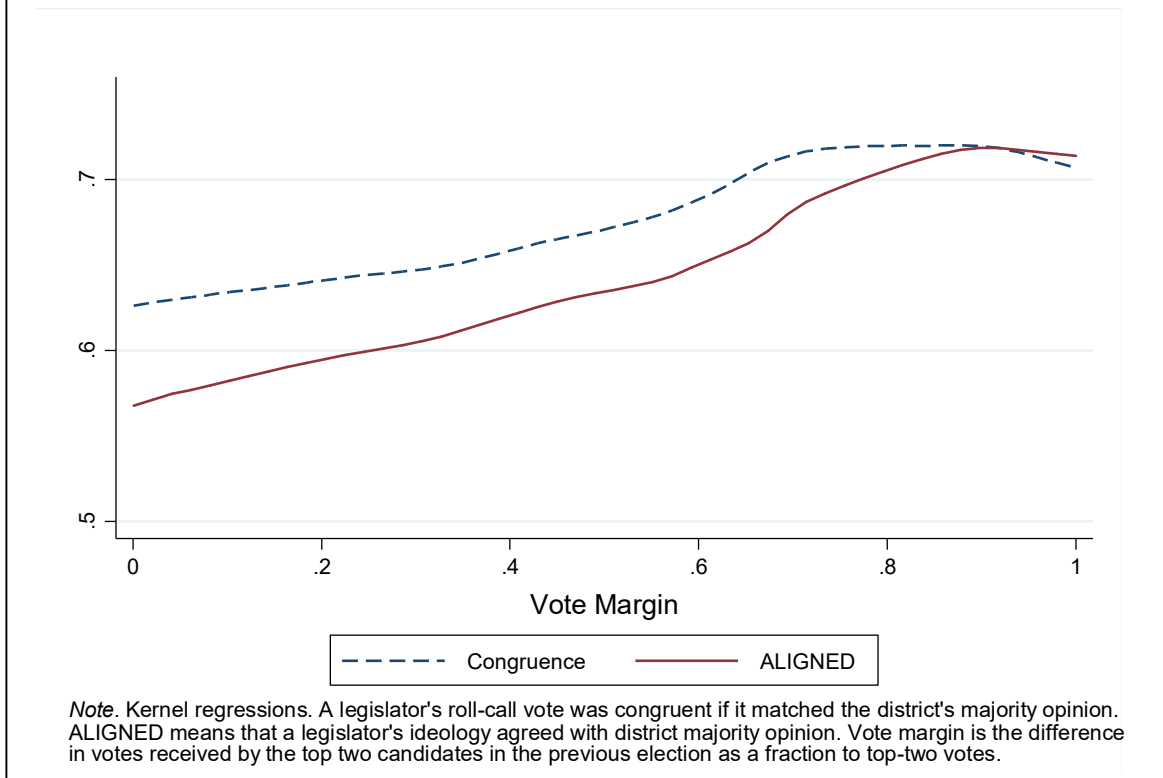
In contrast, variables traditionally associated with re-election pressure – electoral competition, term-limits, media attention – do not reliably predict congruence in my data. This suggests that fear of losing re-election is not a primary driver of roll-call votes, and squares with other evidence that the consequences of individual roll-call votes for re-election is small (Rogers 2017; Tausanovitch and Warshal 2018; Hanretty et al. 2021; Ansolabehere and Kuriwaki 2022). However, the absence of a connection between re-election incentives and *roll-call votes* and does not imply that re-election pressure is unimportant for other aspects of legislative behavior; as Fouirnaies and Hall (2022) show, re-election pressure does influence metrics of legislative effort such as bill sponsorship and attendance. The other factor that displays some hints of explanatory power is party pressure: on some key issues party leaders may "whip" legislators to vote in support of the party, 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 these effects are not large in magnitude.

Despite the absence of re-election incentive effects, roll-call votes usually do represent citizen preferences in my sample. This happens because of 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 et al. (2013) argue that if elections are used to select ideologues more than induce pressure on incumbents, then major policy changes will have to come through replacing legislators not by changing the minds of incumbents. This is what Fedaseyeu et al. (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 agreement against vote margin. The upper line shows that congruence is actually higher in uncompetitive than competitive districts. The lower line shows why: legislators are more likely to be ideologically aligned with constituents in uncompetitive districts. This happens because one-sided districts are more likely to select legislators who share the district's preferences.

If elections are mainly about selection, then the value of competitive districts is does not lie in inducing incumbents to pay more attention to constituents, but in choosing ideologues that are aligned with constituents. One can even imagine competitive districts harming 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

Figure 3. Congruence and Alignment by Vote Margin

Note. 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 to top-two votes.

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, 2005).

Online Appendix A. Description of Laws

State	Law	Bill	Roll Call (Y-N-A)	Referendum	Date	Vote (Y-N)	Outcome
Alaska	Permits hunters to use airplanes to hunt wolves	SB 267	Senate 14-5-1 (3/23/2000) House 27-11-2 (4/4/2000)	Measure 6	Nov. 7, 2000	47% - 53%	Repealed
California	Permits Pala tribe to operate video lottery terminals	SB 287	Senate 21-7-12 (8/27/1998) Assembly 52-24-4 (8/28/1998)	Prop. 29	Mar. 7, 2000	53% - 47%	Approved
California	Allows third parties to sue insurance companies for unfair claim practices	SB 1237	Senate 22-16-2 (6/2/1999) Assembly 43-26-11 (7/8/1999)	Prop. 30	Mar. 7, 2000	32% - 68%	Repealed
California	Allows third parties to sue insurance companies (modifies Prop 30 on same ballot)	AB 1309	Senate 22-14-4 (9/7/1999) Assembly 43-32-5 (9/7/1999)	Prop. 31	Mar. 7, 2000	28% - 72%	Repealed
California	Requires large companies to provide health care coverage	SB 2	Senate 25-15-0 (9/12/2003) Assembly 46-32-2 (9/13/2003)	Prop. 72	Nov. 2, 2004	49% - 51%	Repealed
California	Authorizes gambling compact with Pechanga tribe	SB 903	Senate 23-8-9 (4/19/2007) Assembly 61-9-10 (6/28/2007)	Prop. 94	Feb. 5, 2008	56% - 44%	Approved
California	Authorizes gambling compact with Morongo tribe	SB 174	Senate 23-10-7 (4/19/2007) Assembly 50-13-17 (6/28/2007)	Prop. 95	Feb. 5, 2008	56% - 44%	Approved
California	Authorizes gambling compact with Sycuan tribe	SB 175	Senate 22-10-8 (4/19/2007) Assembly 61-9-10 (6/28/2007)	Prop. 96	Feb. 5, 2008	56% - 44%	Approved
California	Authorizes gambling compact with Agua Caliente tribe	SB 957	Senate 23-9-8 (4/19/2007) Assembly 52-11-17 (6/28/2007)	Prop. 97	Feb. 5, 2008	55% - 45%	Approved
California	Allows North Folk tribe casino in Central Valley	AB 277	Assembly 41-12-26 (5/2/2013) Senate 22-11-6 (6/27/2013)	Prop. 48	Nov. 4, 2014	39% - 61%	Repealed
California	Bans plastic bags at grocery stores	SB 270	Assembly 45-31-4 (8/28/2014) Senate 22-15-3 (8/29/2014)	Prop. 67	Nov. 8, 2016	53% - 47%	Approved

California	End use of cash bail	SB 10	Assembly 42-31-7 (8/20/2018) Senate 26-12-2 (8/21/2018)	Prop. 25	Nov. 3, 2020	44% - 56%	Repealed
Maine	Replaces health insurance claims tax with beverage tax	LD 2247	House 75-64-12 (4/15/2008) Senate 18-17-0 (4/15/2008)	Question 1	Nov. 4, 2008	35% - 65%	Repealed
Maryland	Changes voting procedures	HB 1368	House 94-43-4 (3/29/2006) Senate 29-3-14 (3/29/2006)	Question 4	Nov. 7, 2006	71% - 29%	Approved
Maryland	Allows illegal immigrants to pay in-state tuition rates	SB 167	Senate 27-19-1 (4/7/2011) House 74-65-2 (4/8/2011)	Question 4	Nov. 6, 2012	59% - 41%	Approved
Maryland	Congressional redistricting plan	SB 1	House 91-46-4 (10/19/2011) Senate 32-13-2 (10/20/2011)	Question 5	Nov. 6, 2012	64% - 36%	Approved
Maryland	Allows same-sex marriage	HB 438	House 72-67-2 (2/17/2012) Senate 25-22-0 (2/23/2012)	Question 6	Nov. 6, 2012	52% - 48%	Approved
Michigan	Allows hunting of mourning doves.	HB 5029	Senate 22-15-1 (3/31/2004) House 65-40-15 (6/8/2004)	Proposal 06-03	Nov. 7, 2006	31% - 69%	Repealed
North Dakota	Ends use of "Fighting Sioux" college nickname	SB 2370	Senate 39-7-1 (11/8/2011) House 63-31-0 (11/9/2011)	Referred Measure 4	Jun. 12, 2012	67% - 33%	Approved
North Dakota	Allows corporate dairy and swine farms	SB 2351	House 56-37-1 (3/16/2015) Senate 29-16-2 (3/19/2015)	Referred Measure 1	Jun. 14. 2016	24% - 76%	Repealed
Ohio	Limits interest rate charged by payday lenders	HB 545	Senate 29-4-0 (5/14/2008) House 70-24-4 (5/20/2008)	Issue 5	Nov. 4, 2008	64% - 36%	Approved
Ohio	Limits collective bargaining by public employees	SB 5	House 53-44-2 (3/30/2011) Senate 17-16-0 (3/31/2011)	Issue 2	Nov. 8, 2011	38% - 62%	Repealed
South Dakota	Bans smoking in restaurants and bars	HB 1240	Senate 21-14 (3/4/2009) House 46-23 (3/9/2009)	Referred Law 12	Nov. 2, 2010	64%-36%	Approved

South Dakota	Reforms candidate petition process, makes qualification more difficult for independents	SB 69	House 59-16-4 (3/13/2015) Senate 26-7-2 (3/13/2015)	Referred Law 19	Nov. 8, 2016	29% - 71%	Repealed
South Dakota	Creates a sub-minimum wage for workers under the age of 18	SB 177	Senate 26-7-2 (2/18/2015) House 44-24-2 (3/4/2015)	Referred Law 20	Nov. 8, 2016	29% - 71%	Repealed
Washington	Increases taxes for unemployment insurance	HB 2901	House 66-29-3 (3/11/2002) Senate 35-14-0 (3/13/2002)	R-53	Nov. 5, 2002	41% - 59%	Repealed
Washington	Allows charter schools	HB 2295	House 51-46-1 (3/10/2004) Senate 27-22-0 (3/10/2004)	R-55	Nov. 2, 2004	42% - 58%	Repealed
Washington	Prohibits insurers from denying certain claims	SB 5726	Senate 30-17-2 (3/13/2007) House 59-38-1 (4/5/2007)	R-67	Nov. 6, 2007	57% - 43%	Approved
Washington	Grants domestic partners same rights as married persons	SB 5688	Senate 30-18-1 (3/10/2009) House 62-35-1 (4/15/2009)	R-71	Nov. 3, 2009	53% - 47%	Approved
Washington	Allows same-sex marriage	SB 6239	Senate 28-21-0 (2/1/2012) House 55-43-0 (2/8/2012)	R-74	Nov. 6, 2012	54% - 46%	Approved
Washington	Allows government to use racial preferences/affirmative action	I-1000	House 56-42-0 (4/28/2019) Senate 26-22-1)4/28/2019)	R-88	Nov. 5, 2019	49% - 51%	Repealed
Washington	Requires schools to teach sex education	SB 5395	House 56-40-2 (3/5/2020) Senate 27-21-1 (3/7/2020)	R-90	Nov. 3, 2020	58% - 42%	Approved

Note. Roll-call numbers are (in order): votes in favor, votes against, and abstentions (or vacant seats), followed by the date of the vote. For referendum outcomes, "Y" is votes to uphold the law and "N" is votes to repeal the law (Alaska and Maine frame their questions in reverse, denoting "yes" as a repeal the law; the table restates numbers so that "yes" means approval.)

Online Appendix B. Roll-Call Vote Regression

	All Laws	Upheld Laws	Repealed Laws
IDEO (YES)	42.7***	35.8***	50.4***
	(4.6)	(8.0)	(3.9)
DISTRICT(YES)	1.17**	1.27***	1.00***
	(0.20)	(0.25)	(0.25)
IDEO(YES) x DISTRICT(YES)	-0.34***	-0.22	-0.53***
	(0.11)	(0.16)	(0.10)
CONSTANT	2.2	-10.9	17.2**
	(8.6)	(13.8)	(8.4)
N	3,927	2,148	1,779

Note. Each column reports a regression in which the dependent variable is a dummy = 1 if a legislator voted in favor of a law. Each regression includes law-chamber fixed effects. Significance: * = 10 percent, ** = 5 percent, *** = 1 percent.

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