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The Precarious Link between Legislators and
Constituent Opinions: Evidence from Matched
Roll Call and Referendum Votes

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The Precarious Link between Legislators and Constituent Opinion: Evidence from Matched Roll Call and Referendum Votes

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The Precarious Link between Legislators and Constituent Opinion: Evidence from Matched Roll Call and Referendum Votes

1. Introduction

Representation by elected officials is the core of modern democracy, yet the nature of the relationship is a matter of dispute. Scholars have long debated whether legislators should vote their conscience or the preferences of their constituents, sometimes called the trustee-versus-delegate debate. These views are instantiated in two broad political economy theories: in citizen-candidate models voters choose legislators who implement their personal preferences when in office (Osborne and Slavinsky, 1996; Besley and Coate, 1997), while in political agency models voters induce legislators to follow constituent preferences by threatening not to re-elect them (Barro, 1973; Ferejohn, 1986; Banks and Sundaram, 1993; Maskin and Tirole, 2004). No doubt both views have practical relevance, but we would like to know their relative importance, in part because they lead to different prescriptions for electoral reform.

This paper offers a new approach to quantifying the importance of the citizen-candidate versus political agent model, and applies it to data on 3,242 roll call votes by state legislators. While both theories imply that legislators usually vote in accordance with constituent opinion – citizen-candidates, because they share their constituents' views, and political agents, because they wish to be re-elected – the theories give different predictions of how legislators vote when they face an issue on which their ideological preferences *conflict* with district preferences. The citizen-candidate theory predicts that legislators will follow their personal preferences, while the political agency theory predicts that legislators will suppress their ideology and follow constituent opinion, at least in cases where electoral pressure is substantial. One of the paper's key tests is to estimate how often legislators follow their own preferences versus constituent preferences when they conflict.

The challenge in implementing this test is determining if legislator votes are congruent with constituent preferences. My approach is to use referendum election returns to measure district-level constituent preferences. At present, 23 American states allow citizens to challenge state laws that have been approved by the legislature and governor, using what is typically

called the referendum process.¹ In these states, if citizens collect a predetermined number of signatures from fellow citizens, an election is held in which voters have the option to approve or repeal the law. I construct a new data set that includes 25 laws in nine states for which district-level referendum election returns are available, and determine whether or not each roll call vote was congruent with majority opinion in the district.

I find that representation “works” more often than not – legislators cast congruent votes 67 percent of the time – but that representation appears to “fail” one-third of the time. To assess the citizen-candidate versus political agency models, I compare each legislator’s ideological preference on a law, as measured by Shor and McCarty’s (2011) NPAT common space scores, with district opinion. When a legislator’s ideological preference disagrees with district opinion (which happens for 38 percent of roll call votes), I find that legislators vote their ideology 72 percent of the time and follow district opinion only 28 percent of the time. By comparison, when ideology and district opinion agree, legislators vote congruent with district opinion 92 percent of the time. For the most part, legislators in the sample behave like citizen-candidates rather than political agents.


A second test explores the effectiveness of re-election pressure in bringing about congruent voting. Electoral pressure can be induced in several ways. Although theoretical predictions are mixed, it is an article of faith among many scholars and reformers that making elections more competitive is the key to increasing the quality of representation (e.g. see the various contributions in McDonald and Samples (2006)). I find only a modest connection between the congruence of a legislator’s roll call votes and the competitiveness of his or her district: legislators representing the most competitive districts, with a vote margin between the top two candidates of less than 2.5 percent, are only 6 to 8 percent more likely to cast a congruent vote than legislators who run unopposed. Another source of electoral pressure may

¹ Direct democracy terminology is not standardized; I follow common practice and refer to a citizen-initiated proposal for a new law as an “initiative”, a citizen-initiated proposal to repeal an existing law as a “referendum”, a proposal placed on the ballot by the legislator as a “legislative proposal”, and I use “measure” and “proposition” interchangeably as an umbrella term for any proposal that voters decide directly. See Lupia and Matsusaka (2004) or Matsusaka (2005) for a discussion of direct democracy terms and institutional details.

be imminence of an election. I find that legislators are 8 to 12 percent more likely to vote congruently if a law is being considered in an election year than in years without an election. Finally, congruence drops modestly (at most) for legislators in their last term in a term-limited state, consistent with the idea that removal of re-election pressure reduces attention to constituent interests. While there is some evidence that legislators are sensitive to electoral pressures, the magnitudes of the effects are modest, and much smaller than the effect of ideology, reinforcing the conclusion that representation mainly works through selection and not through electoral incentives.

I also explore several extensions of the basic estimates in order to check for robustness and identify other factors that influence roll call behavior. Legislators may cast noncongruent votes by accident, rather than intentionally, if they misunderstand their constituents' preferences. To assess this possibility, I focus on laws in which public opinion was strongly one-sided so that honest mistakes are unlikely, and find similar results. In order to determine if the laws studied in the paper are unrepresentative because they were challenged by referendum, I examine voting behavior separately on laws that were approved and repealed. Even laws that were approved by the voters show a dominance of ideological voting. Finally, I consider if roll call voting is different for high than low salience issues, and in low as opposed to high population districts. I find some evidence that legislators heed constituent preferences more on high salience issues, but the effect is not enormous. Somewhat surprisingly, I find that ideological voting is stronger in districts with few voters than in districts with many voters.

Because the literature on representation is so extensive, it may help the reader to situate this paper in the literature in order to highlight its main innovations. At the risk of oversimplifying, the literature has revolved around two main empirical strategies, each of which has come under significant criticism. One approach has been to regress roll call votes on proxies for constituent preferences and ideology, with the size of the coefficient (or correlation) on preferences taken as a proxy for the degree of representation.² While this approach can reveal if there is a connection between votes, citizen preferences, and ideology at the margin,

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² "Classic" references in this vein are Kau and Rubin (1979), Kalt and Zupan (1984), and Peltzman (1984).

several studies have shown that the magnitude of the coefficient cannot be interpreted as a proxy for the *degree* of representation (Achen, 1977; Romer and Rosenthal, 1979; Erikson et al., 1993, chapter 4; Matsusaka, 2001). A more recent approach has been to estimate the ideal point of each legislator (from roll call votes) and his or constituents (from opinion surveys or election returns) in a spatial model, and compare the distance between the two.³ Such estimates are appealing because of their grounding in theory, but require a bridging assumption to place ideal points on a similar scale, and the numbers lack a natural interpretation. More troubling, Broockman (forthcoming) shows that even with a valid bridging assumption, such comparisons might not indicate ideological congruence, but rather the relative consistency of beliefs held by legislators and ordinary citizens. My paper's approach of comparing roll call and referendum votes on individual issues avoids the problems associated with the regression method by calculating congruence directly, and with the ideal point method by comparing individual votes rather than aggregating. The basic strategy of using referendum votes is inspired by Gerber (1996) and Gerber and Lewis (2004) and is closely related to recent work by a team of researchers studying Swiss legislators (Portmann et al. (2012), Stadelmann et al. (2013, 2014)).⁴ One goal of the paper is to suggest this approach as a viable alternative to more common existing methods.⁵

The paper attempts to advance our understanding of the representation process by introducing a new data set, by employing a relatively unexploited method for measuring

³ Recent examples include Gerber and Lewis (2004), Bafumi and Herron (2010), Masket and Noel (2011), and Kousser et al. (2014).

⁴ Brunner et al. (2013) use ballot proposition votes in California to measure the congruence of legislators with their poorest and richest constituents.

⁵ Lee et al. (2004) and Levitt (1998) develop completely different approaches that do not involve measuring constituent preferences. Lee et al. (2004) assume that re-election motivated politicians should respond to an incumbency advantage by moving their roll call votes in the direction of the incumbent's ideal point. Using a regression discontinuity strategy based on close elections, they find no evidence of such a move, which they interpret as purely ideological voting. A limitation of their approach is that the assumption that incumbency causes policy shifts is not a general property of political agency models. Levitt (1998) estimates a structural model of a representative senator's utility function, finding a heavy weight on ideology in the voting decision. His approach requires the strong assumption that voter preferences can be proxied by the roll call voting behavior of elected representatives.

constituent preferences, and by offering new tests to distinguish between the citizen-candidate and political agency theories. The paper's main substantive findings – that legislators usually cast congruent votes, that ideology usually trumps district opinion when the two conflict, and that competition only modestly mitigates this pattern – together lend support to a core assumption of the citizen-candidate theory. The picture that most naturally fits the evidence is that elections serve to select candidates who are more-or-less ideologically aligned with the district's voters, but once in office legislators mainly follow their ideologies when voting, even when they are in a tenuous electoral position. The evidence does not imply that legislators ignore re-election considerations – indeed, approximately one-quarter of the time they defer to constituent preferences over their own ideological inclinations – but that the incentives provided by elections may be less important than their selection function. In this regard, the paper's conclusions support Besley's (2005) call for increased attention on the selection role of political markets.

The paper also sheds light on the broader issue of popular dissatisfaction with elected officials. In the United States today, opinion surveys show high levels of dissatisfaction with the quality of representation; only 36 percent of respondents in a recent survey disagreed with the statement, "People like me don't have any say about what the government does."⁶ One possible explanation for the current state of public opinion is provided by two recent studies finding that state policies are often not congruent with majority opinion (Matsusaka (2010) finds a congruence rate of 59 percent while Lax and Phillips (2012) find a congruence rate of 48 percent, neither measurably better than the 50 percent congruence that would attain if policies were chosen by flipping a coin), that is, often voters are not getting the policies want. There are many potential causes of policy congruence, such as court overrides and legislative gridlock; the evidence here suggests that one cause of policy congruence may be ideological voting by legislators without regard to constituent preferences.

⁶ Source: American National Election Survey, 2012.

2. Constructs and Data

A general definition of congruence between legislator n and his or her constituents is $-|Y_n - Y_n^*|$, where Y_n is the legislator's vote (or voting record) and Y_n^* is the vote (or voting record) preferred by his or her constituents.⁷ As noted above, some previous research has estimated Y_n and Y_n^* as points on the real line based on a sample of roll call votes and a measure of constituent opinion. Broockman (forthcoming) argues that this approach is flawed because middling ideological scores arise not only if a person is a moderate but also if he or she takes extreme positions on both the conservative and liberal side of different issues; calculating congruence with ideal points then may capture differences in consistency rather than differences in ideology.

My approach in this paper is to estimate congruence between roll call votes and referendum returns on individual laws, where $Y_n \in \{yes, no\}$ is the legislator's vote on a particular law, $Y_n^* \in \{yes, no\}$ is the majority view in the district based on referendum election results,⁸ and

$$Congruence_i = \begin{cases} 1 & \text{if } Y_n = Y_n^*; \\ 0 & \text{if } Y_n \neq Y_n^*. \end{cases}$$

Using roll call votes coupled with referendum returns offers some advantages over previous estimates: because these decisions involve only two outcomes, approve or reject, they are naturally on the same scale and thus directly comparable without a bridging assumption. Also, they have a natural interpretation: *Congruence* = 1 means that the legislator is representing majority opinion in the district, and *Congruence* = 0 means the legislator is not representing district opinion. Finally, they do not suffer the aggregation problem identified by Broockman (forthcoming).

⁷ While the focus here is on congruence between roll call votes and constituent opinion, congruence can also be defined at the policy level, that is, in terms of whether the policy choices in a political unit correspond to majority opinion (Matsusaka, 2010; Lax and Phillips, 2012).

⁸ With only two outcomes, the majority view in the district is unambiguous and equal to the median outcome.

A. Referendums and Constituent Preferences

At present, 23 American states allow citizens to use the referendum process to challenge state laws approved by elected officials (passed by both houses of the legislature and approved by the governor). Implementation details differ, but in these states, if citizens collect a predetermined number of signatures from fellow citizens, an election is held involving the electorate at large in which voters have the option to confirm or repeal the law.⁹ I use district level referendum election returns to measure the majority opinion of constituents in each legislator's district on a law.

To construct the sample, I began by identifying all state-level referendums during the period 2000-2014 using the database maintained by the Initiative and Referendum Institute. From this list of 54 ballot measures, I examined official election returns provided by each state's election division (typically the office of the secretary of state) to determine if returns were available by legislative district, or could be constructed from precinct-level data.¹⁰ The necessary data are available for 25 referendums. For each referendum, I identified the roll call votes cast in each house of the state legislature on the law. Laws are voted on several times en route to approval; I used the final roll call vote cast in each house. The 3,242 roll call votes associated with these referendums form the core of the study (abstentions are omitted). The 25 referendums are listed in Table 1, along with summary and descriptive information.

The referendums took place in nine states: Alaska, California, Maine, Maryland, Michigan, North Dakota, Ohio, South Dakota, and Washington. These states represent a mix of urban and rural, and include both "blue" and "red" states in terms of ideological orientation: In the sample period, Republicans typically controlled the legislatures of Alaska, Michigan, North Dakota, Ohio, and South Dakota; and Democrats usually controlled California, Maine,

⁹ For example, in California, petitioners have 90 days after approval of a law to collect signatures from eligible voters equal to 5 percent of the number of votes cast in the previous gubernatorial election (as of 2015, roughly 505,000 signatures). For institutional details across the states, see Gerber (1999).

¹⁰ Some referendums had to be excluded because states do not report sufficiently disaggregated data. Others were excluded because the state changed its district lines between the time of the roll call vote and the time of the referendum election. One California referendum was abandoned by its sponsors after qualifying for the ballot.

Maryland, and Washington. The subject matter of the challenged laws covered fiscal, political, and social issues, and included hot-button topics of national interest such as same-sex marriage as well as issues of primarily local interest such as Alaska's law allowing aerial hunting of wolves and North Dakota's law allowing the University of North Dakota to stop using the name "Fighting Sioux" for its mascot. The ideological orientation of the laws was also mixed, with some proposing to move policy in a liberal direction (e.g. allowing same-sex marriage or granting tuition to illegal immigrants) and others proposing to move policy in a conservative direction (e.g. allowing charter schools or limiting collective bargaining by public employees). Voters repealed 10 of the 25 laws in question.

An alternative approach to identifying constituent opinion would be to use opinion surveys. Opinion surveys on specific laws are seldom available at the district level. Even when available, referendum election results offer some advantages: First, election returns indicate opinion on exactly the same law approved by the legislature, while opinion surveys usually summarize the law in question when polling voters. Because "the devil is in the details," the summaries may fail to capture elements of the law that turn out to be important to voters. Second, votes in referendum elections actually make law – citizens are not giving off-the-cuff opinion on a matter over which they have no control but are casting votes that aggregate into an actual law. Third, election returns come closer to giving the informed opinion of citizens because the votes are cast after a campaign in which contending groups publicize the benefits and costs of the law, as they see them, and opinion leaders (such as newspapers and interest groups) give their endorsements.¹¹ A possible limitation of election returns is that not every person votes so the numbers may not give an unbiased estimate of district opinion. The severity of this limitation depends on the question to be asked; if the goal is to understand how electoral incentives influence legislator behavior, then it may be desirable to focus on the views of those citizens who vote and not the abstainers.

¹¹ To illustrate how opinion can change in the course of a campaign, I compared the initial opinion survey with final election returns for a sample of 242 California ballot propositions during the period 1958-2014. Opinion data were collected by the Field Poll. The mean absolute change in the percentage of votes in favor was 15 percent.

One issue that is relevant for the external validity of the roll call votes studied in this paper is whether referendums are anticipated, causing legislators to vote differently on these laws than other laws. If legislators expect a law to be put to a vote of the people, their decision calculus may be different; they may vote no in order to avoid being overruled or may vote yes simply to give voters the final decision. As a practical matter, referendums are extremely rare. As noted, there have been only 54 state-level referendums in the 21st century, compared to many thousands of laws that have been passed in the 23 states that allow referendums. Aggrieved group may threaten a referendum, but the cost of collecting petitions in a compressed time period is typically prohibitive. It is plausible to assume that when legislators cast their votes on the laws studied in this paper, they did not expect the law to go to a referendum, and so their voting behavior on these laws is likely to have been similar to their behavior on other laws.¹²

B. Ideology and Disagreement

I capture legislator ideology using the NPAT common space scores constructed by Shor and McCarty (2011) for state legislators during the period 1993-2013.¹³ These scores assign each legislator a scalar (ranging from -2.69 to 2.95 in the sample); negative numbers are naturally interpreted as relatively liberal positions and positive numbers as relatively conservative positions. By construction, the scores do not vary over time. There is a close connection between ideology and partisan affiliation: the mean score for Democrats is -1.18 and the mean for Republicans is 0.98, with the difference statistically significant at the 1 percent level.

¹² The argument given here for external validity would not apply to laws that are known to *require* voter approval, such as bond proposals and constitutional amendments in some states. When voting on such proposals, it is not uncommon for legislators to vote yes in order to let voters decide issue, even though they are not necessarily in favor themselves. Thus, one needs to be careful in generalizing from roll call votes on issues that require popular approval, such as bond proposals or most propositions considered in studies of Switzerland.

¹³ More precisely, I use the July 2014 updated scores, available online at <http://americanlegislatures.com>. I thank Nolan McCarty and Boris Shor for helping me navigate through the data.

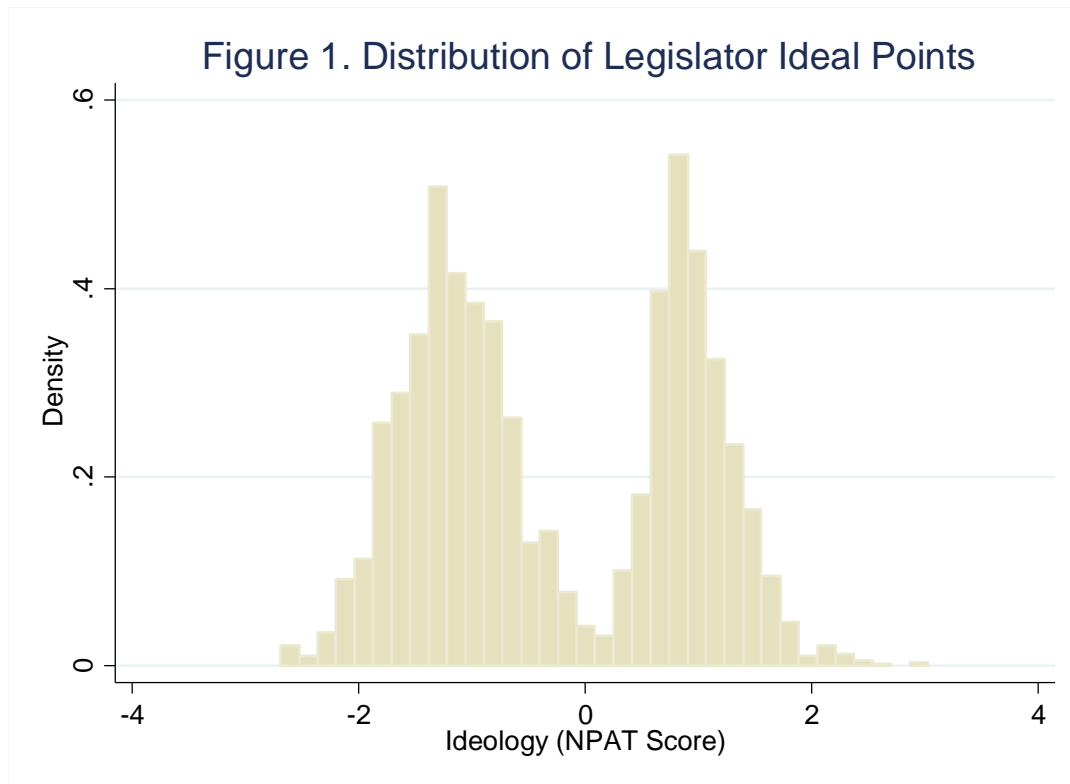
The use of NOMINATE and similar ideal point scores as a proxy for ideology is a common practice. Technically the scores are simply a low dimensional representation of a legislator's history of roll call votes, meaning they could be determined not only by ideology, but also by external influences such as party membership, interest group pressure, and constituent opinion. Several arguments have been offered for interpreting these ideal point estimates as ideological preferences: the estimates are fairly stable throughout a legislator's career; a legislator's ideal point estimate changes little when his or her constituency changes, such as when the legislator moves from one chamber to another or when major redistricting occurs; Senators from the same state often have quite different estimated ideal points, suggesting that ideal points are not simply induced by constituents; and same-party replacements of an incumbent legislator can have very different ideal points from their successors.¹⁴

Despite evidence suggesting that ideal point estimates are effective proxies for ideology, they may incorporate other information as well. To mitigate the possibility of patterns being driven by these other factors, my estimates do not rely on the precise estimate of a legislator's ideal point, but only on whether it is on the "left" or "right" side of the distribution. That is, in the main estimates, I collapse NPAT scores into two categories, called "liberal" and "conservative." Figure 1, which reports the distribution of NPAT scores, shows that dividing legislators into two broad groups like this is a natural way to organize the data. The two-group classification scheme, which typically corresponds to Democrat and Republicans, loses some information but turns out to display a fair amount of explanatory power.

To assess the role of preference disagreement, we need to compare the legislator's and the district's view on an issue. The district's view comes from referendum returns. The legislator's view comes from his or her ideology in conjunction with the ideological orientation of the law in question. I determined the ideological orientation of each law using three approaches: (i) by regressing the percentage of referendum votes in favor on the percentage of votes received by the Democratic candidate in the preceding legislative election; (ii) by

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¹⁴ See McCarty (2011) for a longer discussion. Also see Poole (2007) and Stratmann (2000).



regressing a legislator’s roll call vote on his or her party; and (iii) by identifying the party of each bill’s sponsors. Each law was classified as conservative or liberal if there was a significant connection between voting and party, where Democrat=liberal, or based on the partisan affiliation of the sponsors.¹⁵ The orientations conform to what an informed observer would expect, for example, allowing same-sex marriage (Maryland), granting domestic partnership rights to gay couples (Washington), and requiring employers to provide health insurance (California) are classified as liberal issues, while allowing charter schools (Washington) and restricting public employee collective bargaining (Ohio) are conservative issues. The first classification scheme, which orients an issue based on how people in the district vote, seems most likely to capture how a legislator would personally think about an issue (as opposed to the roll call or sponsorship measures, which may be influenced by party pressure), so I focus on that classification below, but I show that the findings are robust to the other classifications.

¹⁵ Laws were left unclassified if the regression coefficients were statistically insignificant (for the first two approaches); or if the list of sponsors included both Democrats and Republicans or the bill originated with a governor of one party while the sponsor belonged to the other party (third approach).

To determine a legislator's preference on a law, first I classified each legislator as liberal if his or her NPAT score was negative and conservative if his or her NPAT score was positive. Dividing ideologies at the point zero is somewhat arbitrary, especially since the common space scale is arbitrary, but as Figure 1 shows, legislators are polarized with few scores in the vicinity of zero. For robustness, I explore other cutoff points below; as will be seen the main findings are not dependent on the cutoff points. I then compare a legislator's ideological classification (conservative or liberal) with the orientation of a law to determine if the legislator's preference would be to vote in favor or against the law. Finally, I compare the legislator's preference with majority opinion in the district to determine if there is disagreement, and construct a dummy variable DISAGREE that is equal to one if the legislator and district prefer different outcomes. That is, DISAGREE = 1 if a liberal legislator is voting on a liberal law opposed by the district, if a conservative legislator is voting on a conservative law opposed by the district, if a liberal legislator is voting on a conservative law supported by the district, and if a conservative legislator is voting on a liberal law supported by the district.

C. Competition and Other Electoral Pressure

To assess the importance of electoral pressure, I collected data that allow construction of several variables that are linked to re-election concerns. For each district, I collected information on votes received by the top two candidates in the previous legislative election. Following the literature, I measure competitiveness of the district as the vote margin, defined as the difference between votes received by the winner and runner up, divided by their combined votes.

Electoral pressure may also be exerted by proximity of the next election. If voters are myopic, legislators may be more responsive to constituent interests when the next election is imminent than when it is far in the future. Based on state law, I determine the date of the next legislative election in each district.

If a state has term limits for state legislators, I determine if a legislator is currently serving in his or her statutorily mandated final term. Political agency theory implies that legislators should be less attentive to constituent interests when they are ineligible to stand for

re-election.¹⁶ This implication might be softened by the observation that when legislators in term-limit states are forced out of one office they often seek election to another state and local office. Consequently, they may still care about constituent opinion even in their final term (although less so to the extent that their new and old offices have different constituents.)

D. Other Variables

I also collected information on each legislator's party membership and gender. Party membership is potentially relevant because a body of theory suggests that the majority party forms a coalition or cartel to advance the party's interest, and the coalition functions by ensuring that its members vote in accordance with the party's agenda, not necessarily in accordance with constituent interests (Cox and McCubbins, 1993; 2005). Members of the majority party may be pressed to vote in solidarity with their party, resulting in lower congruence, more often than members of the minority party.

Gender is of interest because of an ongoing debate over whether and how female legislators vote differently than male legislators. Anzia and Berry (2011) argue that because of discrimination against women, only the most talented and hard-working female candidates succeed in being elected. Consistent with this idea, they find that female congressional representatives deliver more federal spending to their districts than male representatives. In contrast, Ferreira and Gyourko (2014) find no difference in the policy choices of female and male mayors. The only study that looks at gender differences in representation itself is Stadelmann et al. (2014), which reports that female legislators vote more congruently than male legislators in Switzerland. Table 2 reports summary statistics on the explanatory variables.

3. Basic Patterns of Congruence

I begin with descriptive information on congruence. Figure 2 reports the percentage of congruent roll call votes by issue and overall. The solid dots show congruence based on all roll call votes cast on a law. Overall congruence was 67.0 percent on the 3,242 roll call votes in the

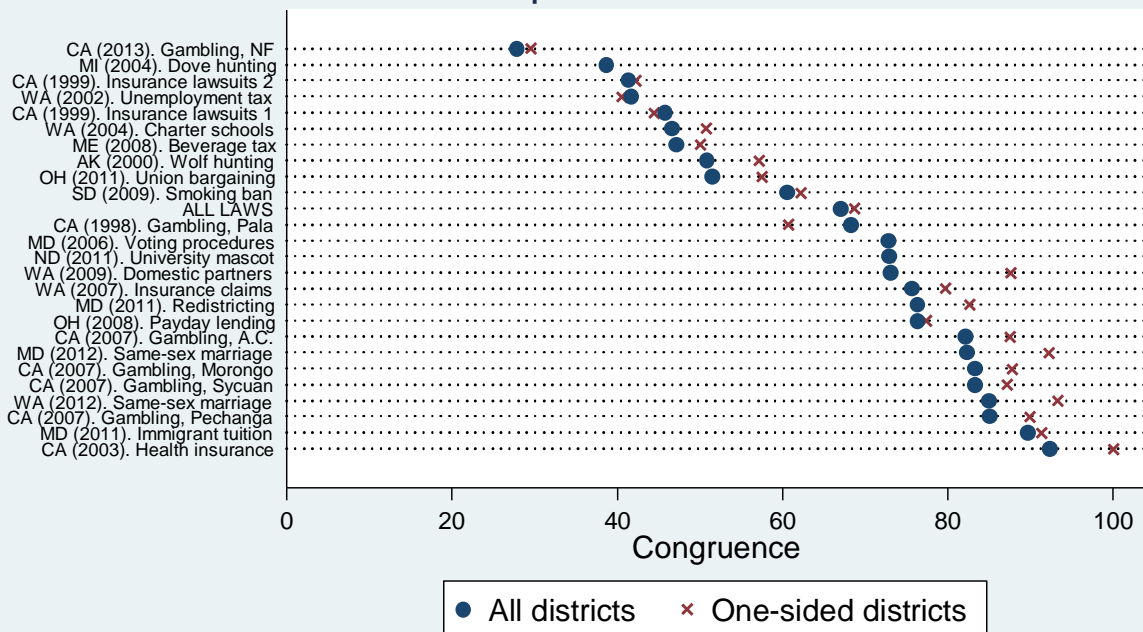
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¹⁶ For theory and evidence on term limits, see the essays in Grofman (1996) and Besley and Case (2003).

full sample: one-third of roll call votes were cast contrary to the preferences of a majority of people in the legislator’s district. Congruence was almost identical in the upper chamber (68.1 percent) and lower chamber (66.5 percent). The lowest congruence was on California’s gambling law of 2013 (27.9 percent), Michigan’s dove hunting law of 2004 (38.7 percent), California’s second insurance law of 1999 (41.4 percent), and Washington’s unemployment insurance tax of 2002 (41.7 percent). All four laws were repealed by the voters. The highest congruence was on California’s health insurance law of 2003 (92.4 percent, including 100 percent in the senate). Interestingly, this law was also repealed.

The premise of the following analysis is that legislators deliberately choose whether to follow constituent interests or not. However, it is possible that legislators deviate from constituent preferences by accident: faced with hundreds if not thousands of votes during a legislative session, and with tens of thousands of constituents to represent, occasionally even a faithful representative may have to act with limited information about constituent views and

Figure 2. Congruence between Roll Call Votes and District Opinion



Note. The figure plots the percentage of roll call votes that were congruent with majority opinion in a district. 'One-sided districts' are those with a majority greater than 55 percent. The full sample includes 3,242 votes; the one-sided district sample includes 2,635 votes.

might make an “honest mistake.”¹⁷ Given limited time to solicit district opinion and a scarcity of district-level polls, legislators might find it difficult to ascertain the preferences of constituents on a given issue.

We can explore this possibility by observing that it is easier to make an honest mistake when voter opinion is evenly divided than when it is one-sided.¹⁸ Based on this observation, Figure 2 also reports congruence only for districts in which opinion was “one-sided” in the sense that the majority was greater than 55 percent (a margin of 10 percent or more, following Broockman and Skovran (2014)); these estimates exclude the most plausible cases of misinterpreted district opinion. In the non-deleted districts, opinion was one-sided so that legislators should have been able to determine the majority view. The figure shows that legislators in one-sided districts voted more congruently overall and for all but six issues than legislators in the other districts. Even so, congruence in this subsample is not all that different from the full sample: for the 2,635 votes in one-sided districts, congruence was 68.7 percent overall, only 1.7 percent higher than the full sample. This suggests that most representation “failures” observed in the full sample are probably not due to honest mistakes about constituent preferences, but rather are deliberate choices.

Alaska’s wolf hunting law of 1999 is an interesting case in point. The law was passed by large majorities in both the senate (14 in favor and 5 against) and house (27 in favor and 11 against), yet it was rejected in 29 of the state’s 40 house districts and repealed statewide. Congruence was only 53 percent in the house and 47 percent in the senate. Was it simply a matter of legislators misunderstanding that their constituents objected to this way of hunting wolves? Possibly, but against this interpretation is the fact that the law in question, SB 267, overrode an existing law banning such hunting that voters had approved by initiative in 1996,

¹⁷ This idea has been explored theoretically in Matsusaka (1992) and Matsusaka and McCarty (2001). Broockman and Skovron (201) provide extensive evidence that state legislators often and systematically misperceive constituent views, typically erring by 10 percentage points or more.

¹⁸ A related possibility is that election returns are noisy and do not accurately represent district opinion. Again, this is more likely to be the case when opinion is evenly divided than when it is one-sided. In the extreme, when district opinion is evenly divided between yes and no voters, the concept of a representative roll call vote is not well defined.

only three years earlier. Thus, each legislator had a fairly clear statement of his or her constituent's opinion on the issue from the previous election. It seems likely that most legislators understood constituent preferences on this issue but chose to proceed anyway.

4. Citizen-Candidates or Political Agents?

The picture that emerges from the preceding section is that legislators represent constituent opinion most of the time, but there are still many instances when they cast votes contrary to constituent opinion. Those contrary votes do not appear to be mistakes, but rather deliberate choices.

This section explores the reason for noncongruent votes. Understanding the causes of noncongruent voting helps distinguish two broad classes of political economy theories. According to the citizen-candidate theory, elections serve to select legislators who share voter preferences, with the understanding that once elected, legislators will follow their personal preferences. According to the political agency theory, elections serve to incentivize legislators to cast congruent votes, and punish them if they do not. The two theories are not mutually exclusive, but their relative importance can be assessed by focusing on situations in which a legislator's personal preferences run opposite to constituent preferences. In these situations, the citizen-candidate view predicts that legislators will follow their own preferences, while the political agency view predicts that legislators will be induced by electoral peril to follow constituent preferences, at least when re-election races are competitive and imminent.

A. The Role of Legislator Preferences

I begin by examining the role of disagreement between legislators and their constituents. Table 3 explores the connection between congruence, constituent interests, and ideology. Each row reports the percentage of congruent roll call votes cast when the legislator's ideology agrees and disagrees with majority opinion in his or her district. The first row includes all roll call votes, and classifies the orientation of laws based on referendum vote regressions. With ideological agreement, congruence is 91.5 percent, meaning that legislators almost always vote in accordance with constituent opinion when they happen to agree with it. Thus, representation

“works” quite well when voters select a legislator whose ideology matches their preferences. In contrast, when a legislator disagrees with district opinion, congruence is only 27.6 percent; which is to say that legislators follow their own preferences 72.4 percent of the time when they disagree with constituents. If elections generate incentives for legislators to adhere to constituent opinion, those incentives appear to be inadequate more than two-thirds of the time. The second row of the table reports the same information but classifies the ideological orientation of laws based on roll call votes; and the third row classifies laws based on the party membership of its legislative sponsors. The basic pattern is the same regardless of how laws are classified.

Next I explore some possibilities that could lead to spurious findings, particularly, that could cause the congruence rate for DISAGREE to appear misleadingly low. One possibility, discussed above, is that majority opinion in a district is misclassified. Because misclassification is more likely when district opinion is evenly divided than when it is one-sided, we can get a sense of this issue by restricting the sample to districts in which opinion was one-sided. The fourth, fifth, and sixth rows report congruence rates when the size of the majority in the districts was 55 percent, 60 percent, and 70 percent, respectively. The pattern remains even for districts with one-sided opinion: legislators almost always vote with district opinion when they agree with it and usually vote against district opinion when they disagree with it.


Another possibility is that legislator opinion is misclassified. Recall that legislators are assigned an ideology based on whether their NPAT common space score is positive or negative. Misclassification is more likely for scores that are near zero. Although Figure 1 shows that such cases are rare, we can allow for the possibility that ideological misclassification drives the results by restricting the sample to legislators whose ideology is far from zero. The seventh, eighth, and ninth rows restrict the sample to legislators with an absolute NPAT score in excess of 0.5, 0.75, and 1.0, respectively. Again, the basic pattern is unchanged even if legislators whose ideology is most likely to be misclassified are omitted.

The bottom three rows of the table apply the district majority and legislator ideology filters simultaneously, that is, they delete observations in which both district opinion and legislator ideology are most likely to be misclassified. Each successive row applies a more

stringent filter; in the bottom row only observations in which the district majority exceeded 70 percent and the legislator's ideology was greater than 1.0 or less than -1.0 are retained. If anything, the basic patterns become more pronounced with these filters. In the bottom row, congruence was 98.8 percent when the legislator and district agreed, and only 2.2 percent when they disagreed.

Table 4 extends the analysis with regressions that allow for possible confounding factors to be controlled. Each column reports coefficients from a linear probability regression in which the dependent variable is a dummy equal to one if a legislator cast a congruent vote.¹⁹ For ease of interpretation, the coefficients are multiplied by 100 to represent percentages. To control for spurious correlation due to law or chamber specific effects, each regression includes law-chamber fixed effects. Standard errors, clustered by law-chamber, are reported beneath the coefficient estimates.

The regression in columns (1)-(3) of Table 4 includes a dummy equal to one if the legislator's ideology differs from the majority view on a given issue, and three other control variables, of each of which is of interest in its own right. A dummy for Republican (as opposed to Democratic or Green) legislators allows for the possibility that one party represents voters better than the other party. A dummy for whether the legislator is a member of the majority party allows for the possibility that the majority party exerts pressure on its members to deliver policies that are optimal for the party (Cox and McCubbins, 1993, 2005). And a dummy equal to one for male as opposed to female legislators allows for the possibility that representatives of different genders represent differently, as suggested by some previous research (Anzia and Berry, 2011). In all regressions, the coefficients on these three variables are tiny in magnitude and never statistically significant. The only coefficients that show any consistency are on the dummy for males, which are always positive, and taken at face value imply that male legislators are 0.4 to 0.7 percent more likely to cast a congruent vote. The absence of statistical significance could be a power issue, but these data give no reason to believe partisan identification, membership in the majority party, or gender have an effect on congruence. This

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¹⁹ The results are essentially the same with logistic regressions, except where noted.

evidence is not necessarily inconsistent with previous evidence showing that female legislators deliver more spending, work harder, and so forth; it only means that female legislators do not differ in their inclination to follow constituent preferences.

The coefficient on DISAGREE indicates the mean effect of disagreement after adjusting for law-chamber differences in congruence rates and controlling for the other three variables. The coefficient in column (1) means that ideological disagreement reduces the probability of congruence by 60.1 percent, comparable to the difference in the means observed in Table 3. The DISAGREE coefficient is different from zero at better than the 1 percent level of statistical significance. Columns (2) and (3) tell essentially the same story.

Regressions (1)-(3) of Table 4 capture ideological disagreement with a dummy variable. Beyond the basic fact of agreement or disagreement, we could imagine degrees of disagreement. Regression (4) includes variables to explore this possibility. On the legislator side, the regression introduces a term that interacts the DISAGREE dummy with the absolute value of the ideology variable. On the constituent side, the regression introduces a variable equal to the size of the majority (for example, if opinion was divided 60-40, then the size of the majority is 60 percent). The coefficients on the extremity of the legislator's ideology are small and statistically insignificant at conventional levels. It could be that ideological extremity does not matter – that the essential fact is simply being conservative or liberal – or that ideology is not estimated precisely enough to detect a connection between congruence and fine gradations in ideology.²⁰ The size of the district majority, on the other hand, clearly matters. The coefficient of 0.88 indicates that a 1 percentage point increase in the size of the majority increases the probability of congruence by 0.88 percent. The coefficient is different from zero at the 1 percent level of significance. Legislators pay attention to majority opinion in their districts, and increasingly so as the size of the majority grows.

Regression (5) in Table 4 repeats regression (4) but restricts the sample to districts with one-sided constituent opinion, defined as a majority greater than 55 percent. As above, the purpose is rule out noncongruent roll call votes that might have occurred because of honest

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²⁰ The interaction term is statistically significant at the 5 percent level with a logistic specification.

mistakes by a legislator. The findings in this regression are substantively unchanged, reinforcing the conclusion that the observed effects represent deliberate choices and not honest mistakes by legislators. The findings also are essentially unchanged if only districts with a majority greater than 60 percent are retained (regression not reported).

The regressions in Table 4 employ 24 to 48 clusters, depending on the sample. With fewer than 50 clusters, we may have a “few-clusters” problem, meaning that standard test statistics will over-reject the null hypothesis. Cameron et al. (2008) show that this problem can be severe. Cameron and Miller (forthcoming) suggest that one strategy for addressing this problem is to use $G - 1$ degrees of freedom for t -tests, where G is the number of clusters. The standard errors reported in the table use this adjustment. Another strategy suggested by Cameron and Miller (forthcoming) is to use the wild bootstrap to estimate the distribution of t -statistics. The basic idea of bootstraps is to generate pseudo-samples from the original sample, use each pseudo-sample to calculate the test statistics, and use the distribution of the test statistic across the pseudo-samples to infer the distribution of the test statistic in question; the wild bootstrap uses a particular algorithm to calculate the pseudo-samples. Cameron et al. (2008) show that the wild cluster bootstrap method can improve on the degrees of freedom approach when the number of clusters is very small (they study cases in which the number of clusters is less than 30). To assess the reliability of the test statistics in the paper, I construct p -values using the wild bootstrap cluster method for key coefficients in Table 4, and compare them to the p -values from the degrees of freedom method. Those numbers are reported in the bottom four rows of the Table 4. As can be seen, the p -values for the two methods are fairly similar, suggesting that the test statistics do not suffer from a few-clusters problem.

Because legislators are not assigned randomly to districts, the results could also be spurious if, for some reason, legislators who are prone to ideological voting also are more likely to disagree with their constituents. One way to get a sense of this is to include legislator-specific fixed effects, which essentially subtract the mean congruence for each legislator and estimate the disagreement effect based on within-legislator comparisons across different votes. The severe limitation of such estimates is that only 659 legislators appear in the sample more than once. Nevertheless, with this caveat in mind, Table 5 reports linear probability regressions of

congruence with legislator fixed effects. In these regressions, propositions that do not include overlapping legislators are omitted. The control variables for party, majority party, and gender are omitted because they are unchanged or almost always unchanged across an individual.

Column (1) of Table 5 reports a simple regression of congruence on disagreement. The coefficient -60.9 is almost unchanged from Table 4, and statistically significant at the 1 percent level. The negative effect of disagreement thus does not appear to be the result of a spurious legislator-specific effect. The regression in column (2) of Table 5 introduces the intensity controls. In contrast to Table 4, ideological intensity here enhances the probability of noncongruence when there is disagreement. To interpret the coefficients, consider a “moderate” legislator with ideology = 0 and an “extreme” legislator with ideology = 1.0. The coefficient estimates imply that when there is agreement, the extreme legislator is 16.3 percent more likely than a moderate legislator to cast a congruent vote; when there is disagreement, the extreme legislator is 3.4 percent less likely to cast a congruent vote. This provides some evidence for the idea that degree of ideological commitment matters for roll call voting. Somewhat puzzlingly, the coefficient on the size of the majority in the district is tiny and statistically insignificant.

To summarize, the natural interpretation of this evidence is that ideology drives the roll call votes of legislators, and that district opinion often is ignored when it conflicts with legislator ideology. When a legislator’s preferences are aligned with majority opinion in the district, the legislator votes with the district 92 percent of the time; when the legislator disagrees with district opinion, the legislator votes with the district only 28 percent of the time. Most of the time, then, legislators behave more like citizen-candidates or trustees than re-election motivated political agents.

B. The Role of Electoral Pressure

The preceding evidence shows that legislators usually follow their own preferences when their views conflict with those of their constituents. This is one of the most direct implications of the citizen-candidate theory. I next explore one of the most direct implications of political agency theory, that electoral competition and reelection incentives help bring about congruence. If legislators behave mainly like citizen candidates, as the previous evidence

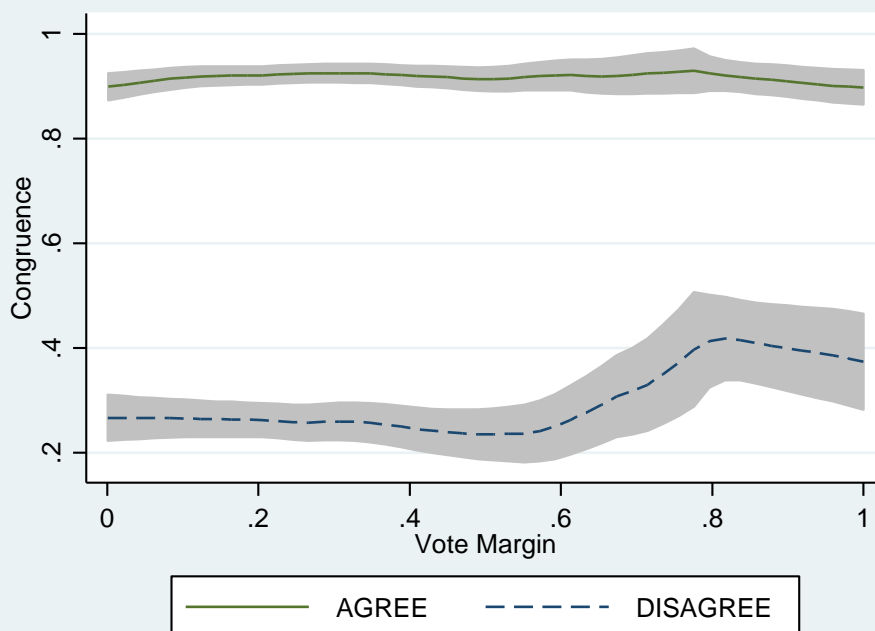
suggests, then we should not observe their roll call votes to be highly sensitive to electoral pressure.

A common measure of electoral competition is the vote margin between the winning and losing candidate. Following the literature, I define the vote margin in a district as the difference between the votes received by the winner and the runner-up in the previous election, divided by the sum of their votes. This inverse measure of competition ranges from zero in the case of a tie, to 1.0 in the case of a candidate running unopposed. Many legislative districts have almost no effective competition: for the full sample, 13 percent of districts had only one candidate, and in 24 percent of districts the vote margin exceeded 50 percent (meaning that the winner received more than 75 percent of the votes.)

We seek to answer two questions: Does congruence rise as electoral competition increases; and does increased competition reduce the effect of ideological disagreement, perhaps entirely erasing the effect of ideology with enough competition? Figure 3 shows the simple nonparametric relation between congruence and vote margin, based on kernel regressions. The figure is included primarily for descriptive purposes as it includes no controls, but it foreshadows two basic patterns that appear in the parametric estimates: a dearth of evidence for the idea that congruence increases in competitive districts; and a lack of evidence that competitiveness chips away at the importance of ideology.

Table 6 reports regressions of congruence on measures of electoral pressure. The regression in column (1) adds two electoral pressure variables to the baseline specification that includes disagreement, the size of the district majority, and the other control variables. One electoral pressure variable is the vote margin; if competition increase congruence then the coefficient on vote margin should be negative. The other electoral variable is the number of years until the next legislative election. Although there is evidence that voters are not as myopic as sometimes believed (Peltzman, 1990), voters may remember actions taken immediately before an election better than actions taken years earlier (Bechtel and Hainmueller, 2011). The number of years to the next election ranges from zero to three years. If an imminent election puts pressure on legislators to attend to district opinion, the coefficient on this variable should be negative. The coefficient on vote margin takes the correct sign, the coefficient on years-to-

Figure 3. Congruence by Vote Margin



Note. The figure plots the relation between congruence and electoral competition based on kernel regressions. Congruence means a legislator's roll call vote is consistent with majority opinion in his or her district. Vote margin is the difference in votes received by the top two candidates in the previous election, divided by their combined votes. The two regressions differ according to whether a legislator's ideology agrees or disagrees with district majority opinion. The regressions are estimated using the Epanechnikov kernel function with bandwidth of 0.1. Shaded areas show 95% confidence intervals.

election takes the wrong sign, and neither are statistically different from zero at conventional levels of significance. Inclusion of these variables results in no material change to the other variables.

The specification in column (1) of Table 6 assumes a linear relation between electoral pressure along the entire range of the vote margin and years-to-election variables. The regression in column (2) considers a nonlinear specification by including dummy variables for the highest pressure situations: highly competitive districts in which the vote margin is less than 2.5%, and roll call votes taken in the year of a legislative election.²¹ This specification can be thought of as the best case scenario for detecting electoral pressure effects. The findings are supportive. The coefficients indicate that congruence is 7.8 percent higher when a legislator

²¹ I explored a large number of alternative specifications, for example, allowing a series of dummy variables for ranges of vote margin or years to next election. The findings in the reported specifications are representative of the patterns in these other specifications.

represents a highly competitive district, and 8.1 percent higher when the roll call vote takes place during an election year. Both coefficients are statistically significant at the 5 percent level or better.

While this specifications reveals that roll call votes respond to electoral pressure – in contrast to other evidence that re-election motives are irrelevant (for example, Lee et al. (2004)) – the magnitude of the effects are modest, and the R-squared increases only slightly. To put the numbers in perspective, the additional congruence associated with representing a competitive district (7.8 percent) or being in an election year (8.1 percent) chips away only about one-eighth of the ideological disagreement effect (55.9 percent). Of course, the coefficients are estimated with error; we can find rough bounds on the true coefficients by adding twice the standard error to the estimate. Doing this, gives an upper bound on the vote margin coefficient of 14.8 percent and on the election year coefficient of 10.9 percent, still far from the size of the disagreement effect. From a policy perspective, increasing competitiveness of districts does not promise a big improvement in representation.

We might suspect that electoral pressure exerts more influence on the legislator when he or she disagrees with constituents than when there is agreement. The regression in column (3) in Table 6 adds two terms that interact DISAGREE with the electoral pressure variables to allow for this possibility. Neither interaction term can be distinguished from zero at conventional levels of significance. The negative sign on the election year interaction implies that disagreement leads to even more noncongruence in election than non-election years, although again the coefficient is not precisely estimated. The regression offers no reason to believe that the effect of electoral competition matters varies with agreement or disagreement.

C. Term Limits

Another way to assess the importance of electoral motives is to consider how roll call voting changes when legislators no longer face the prospect of standing for re-election. The decision to stand for re-election in most cases is endogenous, but in states with term limits, at

some point it is not. During the sample period, five states imposed term limits on state legislators: California, Maine, Michigan, Ohio, and South Dakota.²²

Political agency theory assumes that legislators are motivated to mind constituent interest by the need to stand for re-election. According to this theory legislators should be less attentive to constituent interests as they near the end of a term, and in the extreme, they can fully indulge their personal preferences once they are ineligible to stand for re-election (see Besley and Case (2003)). In practice, politicians often jump from one public office to another when they hit a term limit, for example, they may move from the lower chamber to the upper chamber then to the local city council, so re-election incentives do not totally vanish at the end of a term. Even so, those incentives are muted because any subsequent campaigns would involve a different group of constituents.

Table 7 reports congruence regressions that include term limit variables. The regression in column (1) includes the baseline variables from above, and adds a dummy variable equal to one if a legislator is in the mandatory last term of office. If re-election concerns are important for congruence, the coefficient on the dummy should be negative. The coefficient is negative, but not different from zero at conventional levels of significance. Taken at face value, the coefficient implies that legislators in their last terms are 3.8 less likely to cast a congruent vote. The upper bound estimate of the coefficient, using the two standard deviation rule, is 8.8 percent. While this evidence does not reject a term limit effect, it suggests that if such an effect exists it is not enormous.

Advocates of terms limits argue that they change the basic functioning of a legislature by replacing professional politicians with ordinary citizens. To the extent this is true, how legislators behave in the face of disagreement may be different in term-limit states (as may all of

²² California and Michigan restricted members of the lower chamber to a maximum of three two-year terms and members of the upper chamber to a maximum of two four-year terms. After 2012, California changed its rules to limit members to a maximum of twelve years total in any chamber. Maine and South Dakota restricted members to a maximum of four consecutive two-year terms in either house. Ohio restricted house members to four consecutive two-year terms and senators to two consecutive four-year terms. In California and Michigan the limits were for a lifetime; in the other states members could serve again after remaining out of office for one term.

the other effects captured by the control variables). The regression in column (2) of Table 7 allows for this possibility that restricting the sample to the five term-limit states. The DISAGREE coefficient remains negative and statistically significant, but drops more than a third in magnitude to -34.7 percent. The coefficient on the size of the majority tells a similar story: it almost doubles in size in the term-limit states. Term limits may or may not be the cause of the differences, but for some reason legislators in term-limit states are less inclined to follow their own beliefs when they conflict with constituent preferences. The coefficient on the last-term dummy variable, however, is essentially unchanged in this sample, remaining small and statistically insignificant.

The term-limit restrictions in California and Michigan are lifetime caps – once the years have been served the legislator is forever ineligible to serve in that office – while in the other states, a legislator’s eligibility returns after spending a term out of office. To allow for the possibility that lifetime limits have a larger effect than waiting-period limits, regression (3) of Table 7 includes two last-term dummies, one for the lifetime-limit states and one for the waiting-period states. The magnitude of the coefficients is the reverse of what is expected, although the differences are not statistically significant. The coefficient on the dummy for waiting-period states is different from zero at the 10 percent level of statistical significance, but remains small.²³

The evidence in Table 7 reinforces the general message from the rest of the paper. Legislators pay some attention to constituent preferences, and they are not insensitive to electoral pressures, but their roll call voting is predominantly influenced by their own ideology. They behave mainly like citizen-candidates and only modestly like political agents.

D. Other Considerations

This section explores several issues that are raised by the preceding analysis. The first issue concerns the external validity of the roll call votes studied here. One might wonder if the fact that these laws were challenged by referendum makes them fundamentally different from

²³ I also estimated the regression with legislator fixed effects. The last-term dummy remains small in magnitude and statistically insignificant.

other votes. In particular, it could be that these votes were challenged specifically because of the high prevalence of legislators voting their own opinion rather than constituent preferences, in which case the influence of ideological disagreement would be unusually high for these laws. To evaluate this possibility, Table 8 reports the main regression separately for laws that were subsequently repealed and approved by the voters. If ideological roll call voting is what caused these laws to be challenged, then we should see the ideology effect mainly for laws that were repealed. The disagreement difference is -64.6 percent for repealed laws (column (1)) and -47.7 percent for approved laws (column (2)), both different from zero at the 1 percent level of statistical significance. Ideological voting was stronger on the laws that were repealed than those that were approved, however, the disagreement effect remains large even for those laws that the voters approved. The large disagreement effects found throughout the paper are not likely to be unique to laws challenged in referendums.

A different issue concerns the search for a deeper explanation of why legislators seem to behave like citizen-candidates rather than political agents. It is beyond the scope of the paper to offer a conclusive explanation, but some exploratory evidence can be provided. One possibility concerns salience: voters may not be paying attention to roll call votes, leaving legislators free to pursue their own preferences. To provide a rough assessment of the importance of this issue, I classified each law in terms of its salience based on coverage in the main newspaper in the state following Snyder and Stromberg (2010), which shows that newspaper coverage of Congressmen make them more accountable to voters. If passage of a law was covered on the front page of the newspaper or otherwise received extensive coverage, it was classified as “high” salience. If it was covered elsewhere than the front page (typically in the local politics section), then it was classified as “medium” salience. If it was not reported within a week of passage, it was classified as “low salience.”

Regression (3) in Table 8 introduces three disagreement dummy variables, one for each level of salience.²⁴ If salience accounts for ideological voting, then we expect to see a smaller disagreement coefficient as salience increases. The estimate provide limited support for this

²⁴ Normally, one would also include the three salience levels independently, but because salience is issue-specific, they are collinear with the fixed effects.

idea. Consistent with the idea that low salience leads to ideological voting, the coefficient on DISAGREE for low salience is -79.5; this coefficient is larger in magnitude than the other two disagreement coefficients and statistically different from them at the 1 percent level (medium salience) and 10 percent level (high salience). The disagreement coefficients for medium and high salience are not statistically different from each other ($p = .289$). More to the point, it does not appear that high salience is a big deterrent for ideological voting: the coefficient on high salience issues – issues that were front-page material – remains a sizeable -59.3 and statistically different from zero. We see legislators following their personal views even on extremely high profile issues that are the focus of intense media scrutiny.

The other issue that I briefly explore is the connection between roll call voting behavior and the number of constituents that a legislator represents. The population of districts varies widely in the sample: house districts in Maine and Alaska typically contain less than 5,000 voters, while senate districts in California typically exceed 200,000 voters. One might expect the process of representation to be different in districts with a small versus large population. In particular, we might expect the close connection between legislators and their constituents in small districts to produce greater alignment between roll call votes and district preferences.

Regression (4) in Table 8 examines this hypothesis by introducing three disagreement dummy variables for districts with small (less than 25,000 voters), medium (25,000-75,000 voters), and large (more than 75,000 voters) populations, as well as dummy variables for the small and medium districts (the large-district dummy is collinear with the fixed effects). All three disagreement coefficients remain sizeable and statistically different from zero at the 1 percent level. Somewhat unexpectedly, the disagreement coefficient is largest for legislators representing small districts, and the coefficient of -75.3 is different from the other two at the 1 percent level. The other two coefficients cannot be distinguished from each other statistically ($p = .234$). One conjecture is that in small districts, legislators are closer to ordinary citizens so they feel more comfortable voting their own views without consulting their neighbors.

As mentioned, the evidence on salience and district population is exploratory and much more remains to be done on these subjects. The results do suggest that both salience and district size may be important factors in understanding roll call voting behavior, but neither appears to

offer more than a partial explanation for why legislators predominantly vote their own views when they conflict with constituent preferences.

5. Discussion and Conclusion

This paper evaluates theories of representation based on a comparison of legislator roll call votes and referendum election returns. Such a comparison allows direct measurement of whether or not a legislator's vote is congruent with majority opinion of his or her constituents. In a sample of 3,242 roll call votes on 25 laws in nine states, I find that 67.0 percent of roll call votes were congruent with majority opinion in the district. In this respect, representation appears to "work" more often than not, although the 33 percent "failure" rate is not inconsequential.

The core of the paper is an evaluation of two prominent theories of representation. The citizen-candidate theory (one formalization of the venerable "trustee" view) assumes that legislators vote according to their personal view of what is the best policy; representation occurs by selecting individuals whose personal views correspond with majority opinion in the district. The political agency theory assumes that legislators heed constituent opinion because of the threat of being rejected when standing for re-election. The theories give different predictions about legislator behavior in situations when a legislator's personal view with the constituent preferences: citizen-candidate legislators follows their personal opinion, while political agency legislators follow constituent opinion, as long as there is an electoral consequence from doing otherwise. I find that when a legislator's ideological preference on an issue conflicts with district opinion, the legislator votes his or her own view 72.4 percent of the time. Thus, legislators to act as if they are citizen-candidates rather than political agents.

This conclusion is reinforced by evidence on the connection between electoral pressure and congruence. I find some evidence that legislators are more likely to vote their constituents' preferences when representing a competitive district, as measured by the vote margin in the previous legislative election, but the effect is modest in magnitude and far from offsetting the effect of ideological disagreement. I also find some evidence that congruence is higher in election years than non-election years, suggesting that legislators pay more attention to

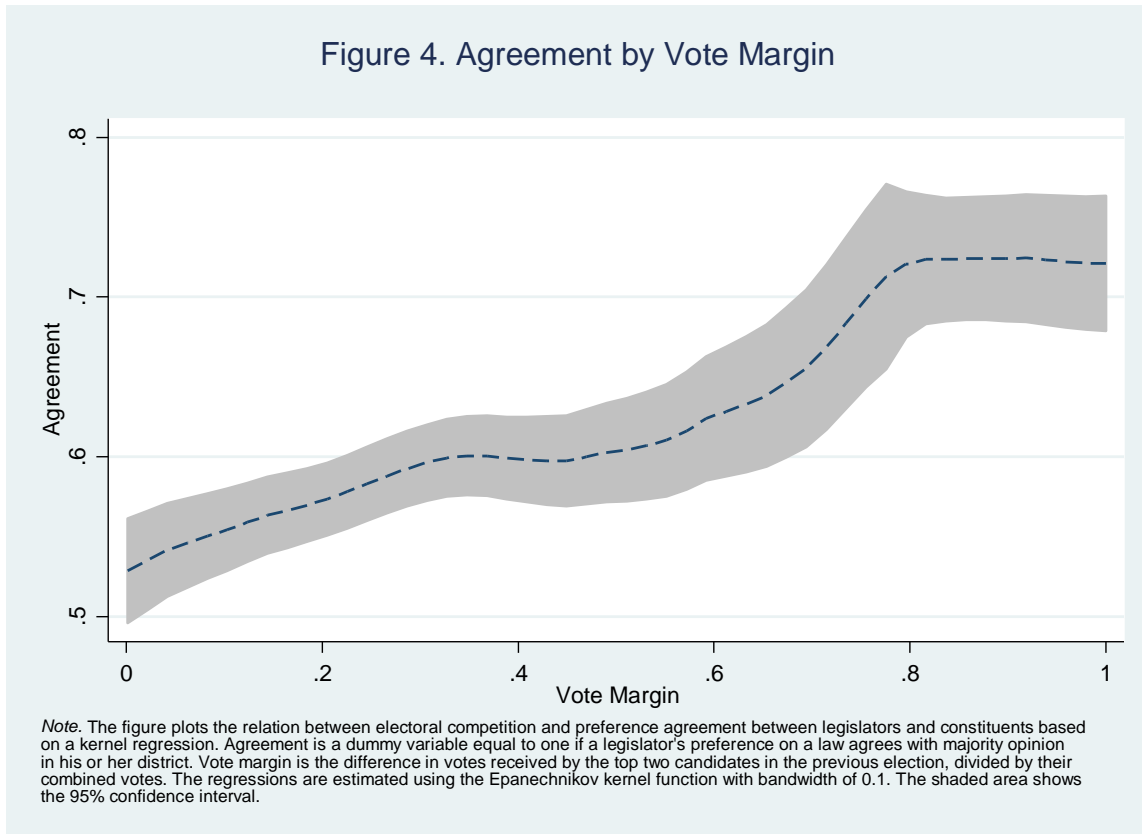
constituent preferences when an election is imminent, but the magnitude of this effect is also modest. I find at best weak evidence that legislators about to be termed out of office are less likely to cast a congruent vote. In short, legislators appear to respond to electoral incentives at the margin, but their responsiveness is quite limited, and even the strongest electoral pressures offset only a small part of the ideology effect.

One message from the evidence is that representation works mainly by selecting legislators who share their constituents' preferences. An important implication is that major policy changes require changes in legislators. If electoral pressure is too weak to force legislators to heed voter preferences, then change will not occur through legislators responding to shifts in public opinion. This supports the argument of McCarty et al. (2013), which contains an extensive argument along these lines.

Another implication is that elections should be analyzed with an eye toward ensuring selection of the right type of legislator rather than only in terms of generating pressure on legislators to sideline their personal views (see Besley (2005) for an extended discussion.) Understanding the structural factors that influence selection of aligned legislators (such as how district lines are drawn) is an important topic for future research, but this line of thinking may challenge some standard intuitions. For example, Figure 4 reports a kernel regression connecting vote margin in a district with the likelihood that a legislator's ideology agrees with constituent preferences. Not surprisingly, one-sided districts are more likely to select legislators who share the preferences of the district majority. However, it follows that drawing district lines so as to make them competitive – often seen as a virtue among electoral reformers – may have a negative effect on representation, counterintuitively, by electing more representatives whose personal views diverge from district opinion.

The same line of thinking suggests that representation might be more effective in high compared to low population districts. In citizen-candidate models with multiple policy dimensions, legislators are chosen from the set of citizens, meaning the policy is the ideal point of some citizen, but the winner's views might not match the ideal point of the majority on every

Figure 4. Agreement by Vote Margin



dimension. Because the choice set is smaller in a low-population district, the winner is more likely to have misaligned preferences on some dimensions.

Political processes are often conceived as involving three groups: constituents, their representatives, and third parties. Third parties such as business and union interest groups play a prominent role in many narratives about contemporary politics. This paper focuses on the first two groups, and shows that a great deal of voting behavior can be understood simply in terms of their relationship, without requiring consideration of interest groups. Indeed, the finding that legislators vote congruently 92 percent of the time when they agree with their constituents, and are very likely to follow their own ideology when they disagree, suggests that interest groups may play a minor role overall.

In terms of political reform, inadequate competition is often blamed for various maladies that are believed to plague American democracy, and many reform proposals focus on increasing competition (e.g. see the various chapters in McDonald and Samples (2006)). For example, nonpartisan primaries are valued as a way to ensure that there are two credible candidates on the general election ballot; independent redistricting is valued in order to prevent

legislators from creating safe districts; and campaign finance regulation is intended to level the playing field and ensure that challengers can compete. The findings in this paper are not encouraging about the ability of heightened competition to increase congruence; competition appears to have at most a modest effect on congruence, and raises questions about the utility of reforms programs dedicated to increasing competition.

Competition also has emerged as a potential organizing principle for election law. Issacharoff and Pildes (1998), among others, argue that judges should move away from thinking about democracy exclusively in terms of rights (of individuals, or groups, of states) and more in terms of creating a competitive environment: the “judiciary should destabilize political lockups in order to protect the competitive vitality of the electoral process and facilitate more responsive representation.” (Issacharoff and Pildes, 1998, p. 649). While this idea has merit, the evidence above suggests that competition ought to be emphasized as a way to select better aligned representatives rather than entirely as a way to induce existing officeholders to place less weight on their personal views. Creating a competitive environment does not necessarily bring about more responsive representation. As illustrated by Figure 4, it could be that noncompetitive districts (in terms of competition between parties) do a better job of electing responsive citizen legislators, even if they provide little electoral pressure.

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Table 1. 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
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
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
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
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Note. Roll call numbers are (in order): votes in favor, votes against, and abstentions, followed by the date of the vote. On Alaska and Maine ballots, a “yes” vote is to repeal the law; the table restates numbers so that “yes” means approval.



Table 2. Summary Statistics

	Mean	SD	Min	Max	N
Ideal point	-0.23	1.14	-2.69	2.95	3,241
Ideal point	1.07	0.46	0.01	2.95	3,241
Ideal point > 1.0	0.53	0.50	0	1	3,241
DISAGREE (laws classified based on referendum votes)	0.38	0.49	0	1	2,837
DISAGREE (laws classified based on roll call votes)	0.41	0.49	0	1	3,137
DISAGREE (laws classified based on sponsorship)	0.38	0.48	0	1	1,710
Vote margin (%)	37.3	30.5	0.1	100	3,242
Dummy = 1 if vote margin < 2.5%	0.04	0.21	0	1	3,242
Dummy = 1 if vote margin 2.5% - 5%	0.05	0.22	0	1	3,242
Dummy = 1 if vote margin 5% - 10%	0.09	0.28	0	1	3,242
Years to next election	1.17	1.09	0	3	3,242
Dummy = 1 if election year	0.33	0.47	0	1	3,242
Dummy = 1 if last term (term-limited)	0.27	0.44	0	1	1,597
Size of majority in district	62.5	8.0	50.01	90.9	3,242
Dummy = 1 if Republican	0.43	0.50	0	1	3,241
Dummy = 1 if member of majority party	0.62	0.48	0	1	3,242
Dummy = 1 if male	0.72	0.45	0	1	3,242

Note. The unit of observation is a legislator/district. Ideal point is the NPAT common space score from Shor and McCarty (2011) (July 2014 version); positive scores can be interpreted as conservative voting records and negative scores represent liberal voting records. DISAGREE is dummy variable equal to 1 if the legislator's ideology conflicts with majority opinion in the district on a law; DISAGREE is calculated three different ways: based on referendum returns, based on roll call votes, and based on party of its sponsors. Vote margin is the difference between the votes received by the winner and runner up, divided by their combined votes, in the previous legislative election. A legislator is classified as term-limited if he or she is in the final term of office in a state with term limits; the term limit variable is only calculated for states with term limits. Size of majority is the fraction of votes cast for the majority position on the referendum.

Table 3. Congruence when Legislator and District Preferences Agree and Disagree

Sample	AGREE	DISAGREE		N
All roll call votes (referendum classification)	91.5	27.6	$z = 35.2^{***}$	2,873
All roll call votes (roll call classification)	91.4	31.7	$z = 34.9^{***}$	3,137
All roll call votes (sponsor classification)	93.7	16.2	$z = 32.5^{***}$	1,710
District majority > 55%	92.0	27.2	$z = 32.4^{***}$	2,341
District majority > 60%	93.7	25.7	$z = 28.5^{***}$	1,629
District majority > 70%	97.6	20.8	$z = 18.9^{***}$	556
Ideology > 0.5	92.1	25.7	$z = 35.1^{***}$	2,624
Ideology > 0.75	93.0	24.9	$z = 33.2^{***}$	2,236
Ideology > 1.0	94.0	26.6	$z = 28.4^{***}$	1,625
District majority > 55% and Ideology > 0.5	92.6	26.1	$z = 32.1^{***}$	2,171
District majority > 60% and Ideology > 0.75	95.0	22.5	$z = 27.0^{***}$	1,299
District majority > 70% and Ideology > 1.0	98.8	2.2	$z = 18.3^{***}$	378

Note. The main cell entries are the percentage of roll call votes that are congruent with majority opinion in the district. AGREE means that the legislator's ideology and majority opinion in the district agree on the law in question; DISAGREE means that legislator's ideology and district opinion disagree. Except where noted, issues are classified based on district referendum results. "District majority>55%" means the sample is restricted to districts in which the majority opinion exceeded 55 percent. "|Ideology| > 0.5" means the sample is restricted to legislators with an absolute NPAT score in excess of 0.5. The z-statistic tests the hypothesis that the proportions for AGREE and DISAGREE are the same. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 4. Linear Probability Regressions of Congruence

		Laws classified by:				
		Referendum	Roll call	Party of	Referendum	Referendum
		votes	votes	sponsors	votes	votes
		(1)	(2)	(3)	(4)	(one-sided districts only)
		(1)	(2)	(3)	(4)	(5)
DISAGREE		-60.1*** (6.3)	-59.8*** (6.1)	-74.0*** (5.9)	-48.5*** (10.1)	-51.1*** (9.7)
Dummy = 1 if Republican		-2.1 (5.9)	1.8 (4.7)	-0.1 (4.9)	-1.2 (5.2)	0.5 (5.5)
Dummy = 1 if member of majority party		-0.3 (6.3)	3.6 (5.2)	1.0 (4.6)	-0.3 (5.7)	1.2 (6.5)
Dummy = 1 if male		0.4 (1.3)	0.5 (1.4)	0.7 (1.4)	0.6 (1.4)	0.4 (1.3)
Ideology		0.5 (4.5)	0.9 (4.5)
DISAGREE × Ideology		-6.8 (8.7)	-5.8 (7.5)
Size of majority in district		0.88*** (0.24)	0.93*** (0.26)
R ²		.510	.476	.640	.525	.552
N		2,837	3,137	1,710	2,837	2,341
DISAGREE	<i>p</i> _{DF}	< .001	< .001	< .001	< .001	< .001
	<i>p</i> _{WILD}	< .001	< .001	< .001	< .001	< .001
Size of majority	<i>p</i> _{DF}001	.001
	<i>p</i> _{WILD}	<.001	.002

Note. Each column reports estimates from a linear probability regression in which the dependent variable is equal to one if a legislator cast a congruent vote and zero if the vote was noncongruent. Coefficients are multiplied by 100 to be interpreted as percentages. Standard errors clustered by law-chamber are in parentheses beneath the coefficient estimates. All regressions include law-chamber fixed effects. DISAGREE is a dummy variable equal to one if a legislator's ideological preference differs from majority opinion in a district on a particular law. The method for classifying the ideological orientation of laws is indicated at the top of each column. One-sided districts are those in which the majority exceeded 55 percent. *p*_{DF} is the *p*-value for the coefficient using the degrees of freedom correction; *p*_{WILD} is the *p*-value using the wild bootstrap cluster method. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 5. Linear Probability Regressions of Congruence with Legislator Fixed Effects

	(1)	(2)
DISAGREE	-60.9*** (2.9)	-41.4*** (7.2)
Ideology	...	16.3 (26.3)
DISAGREE × Ideology	...	-19.7*** (6.9)
Size of majority in district	...	0.02 (0.17)
R ²	.460	.450

Note. Each column reports estimates from a linear probability regression in which the dependent variable is equal to one if a legislator cast a congruent vote and zero if the vote was noncongruent. Coefficients are multiplied by 100 to indicate percentages. Standard errors clustered by legislator are in parentheses beneath the coefficient estimates. All regressions include legislator and law-chamber fixed effects. DISAGREE is a dummy variable equal to one if a legislator's ideology differs from majority opinion in a district on a law, classified by referendum votes. Regressions include 2,521 observations. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 6. Regressions of Congruence on Measures of Electoral Pressure

	(1)	(2)	(3)
DISAGREE	-55.9*** (6.6)	-55.6*** (6.6)	-52.2*** (7.8)
Dummy = 1 if Republican	-0.3 (5.2)	-0.4 (5.2)	1.1 (5.4)
Dummy = 1 if member of majority party	0.1 (5.6)	-0.2 (5.6)	0.6 (5.9)
Dummy = 1 if male	0.7 (1.4)	0.7 (1.4)	0.8 (1.4)
Size of majority in district	0.93*** (0.22)	0.91*** (0.22)	0.93*** (0.22)
Vote margin	-3.9 (3.2)
Years to next election	2.6 (2.3)
Dummy = 1 if vote margin < 2.5%	...	7.8** (3.5)	6.6 (4.7)
Dummy = 1 if election year	...	8.1*** (1.4)	12.1*** (4.3)
Dummy = 1 if DISAGREE & vote margin < 2.5%	1.6 (7.3)
Dummy = 1 if DISAGREE & election year	-10.3 (9.9)
R ²	.525	.525	.527

Note. Each column reports estimates from a linear probability regression in which the dependent variable is equal to one if a legislator cast a congruent vote and zero if the vote was noncongruent. Coefficients are multiplied by 100 to indicate percentages. Standard errors clustered by law-chamber are in parentheses beneath the coefficient estimates. All regressions include law-chamber fixed effects. DISAGREE is a dummy variable equal to one if a legislator's ideology differs from majority opinion in a district on a particular law, classified using referendum votes. Each regression has 2,837 observations. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 7. Regressions of Congruence on Term Limit Variables

	Full Sample (1)	Term Limit States Only	
		(2)	(3)
DISAGREE	-55.7*** (6.6)	-34.7** (13.1)	-34.7** (13.1)
Dummy = 1 if Republican	-0.1 (5.3)	4.3 (5.0)	4.3 (5.0)
Dummy = 1 if member of majority party	-0.01 (5.7)	-13.1 (9.1)	-13.0 (9.1)
Dummy = 1 if male	0.6 (1.4)	0.8 (2.3)	0.9 (2.3)
Size of majority in district	0.91*** (0.21)	1.68*** (0.27)	1.69*** (0.28)
Dummy = 1 if vote margin < 2.5%	7.3** (3.4)	12.0** (4.6)	12.0** (4.6)
Dummy = 1 if election year	7.9*** (1.4)	6.2** (2.6)	5.6* (2.9)
Dummy = 1 if last term	-3.8 (2.5)	-3.6 (2.5)	...
Dummy = 1 if last term (lifetime limit)	-3.0 (2.8)
Dummy = 1 if last term (waiting period)	-5.7* (3.3)
R ²	.526	.537	.537
N	2,837	1,389	1,389

Note. Each column reports estimates from a linear probability regression in which the dependent variable is equal to one if a legislator cast a congruent vote and zero if the vote was noncongruent. Coefficients are multiplied by 100 to indicate percentages. Standard errors clustered by law-chamber are in parentheses beneath the coefficient estimates. All regressions include law-chamber fixed effects. DISAGREE is a dummy variable equal to one if a legislator's ideology differs from majority opinion in a district on a particular law. The sample includes on California and Ohio. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 8. Linear Probability Regressions, Miscellaneous

	Repealed (1)	Approved (2)	(3)	(4)
DISAGREE	-64.6** (7.2)	-47.7** (8.5)
DISAGREE low salience	-79.5*** (8.8)	...
DISAGREE medium salience	-46.0*** (9.1)	...
DISAGREE high salience	-59.3*** (8.9)	...
DISAGREE #voters < 25,000	-75.3*** (5.6)
DISAGREE #voters 25,000 - 75,000	-58.9*** (6.6)
DISAGREE #voters > 75,000	-43.7*** (8.9)
Dummy = 1 if #voters < 25,000	23.1 (14.1)
Dummy = 1 if #voters 25,000 – 75,000	20.0 (13.7)
Dummy = 1 if Republican	2.3 (4.7)	5.8 (3.5)	5.7 (5.2)	-1.3 (4.8)
Dummy = 1 if member of majority party	-7.9 (7.4)	14.2** (5.5)	0.00 (5.1)	-1.1 (5.4)
Dummy = 1 if male	0.6 (2.1)	0.3 (1.9)	0.8 (1.4)	0.9 (1.4)
Size of majority in district	0.66** (0.31)	0.77*** (0.28)	0.90** (0.18)	0.84*** (0.21)
Dummy = 1 if vote margin < 2.5%	3.7 (4.6)	8.4 (5.6)	8.1** (3.6)	7.5*** (3.6)
Dummy = 1 if election year	6.4** (2.6)	5.3 (3.1)	7.6*** (2.0)	7.5*** (1.6)
R ²	.610	.358	.534	.536
N	1,158	1,679	2,837	2,837

Note. Each column reports estimates from a linear probability regression in which the dependent variable is equal to one if a legislator cast a congruent vote, and zero if the vote was noncongruent. Coefficients are multiplied by 100 to indicate percentages. Standard errors clustered by law-chamber are in parentheses beneath the coefficient estimates. All regressions include law-chamber fixed effects. DISAGREE is a dummy variable equal to one if a legislator’s ideology differs from majority opinion in a district on a particular law, classified using referendum votes. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.