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Shirking the Initiative? The Effects of Statewide Ballot Measures on Congressional Roll Call Behavior

Joshua Huder[1], Jordan Michael Ragusa[1], and Daniel A. Smith[1]

Abstract

Do ballot measures affect congressional voting behavior? Examining the issues of gay marriage, campaign finance, and minimum wage, we test if the results of statewide ballot initiatives inform congressional roll call votes on legislation occupying the same issue space. Theoretically, we expect signals from ballot measures—which provide precise information about the preferences of a member's voting constituency—reduce policy "shirking" by members. Our findings across the three issues indicate that ballot initiative outcomes alter the floor votes of members of the House, reducing legislative shirking, but we find that the educative effect of ballot measures is attenuated in the Senate due to institutional factors. We attribute the positive effect in House to the precise signal ballot measures provide members about the preferences of the median voter in their district.

Keywords

Congress, direct democracy, ballot measures, roll call votes

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For over a century, the federal government has responded, either directly or indirectly, to the passage of statewide ballot measures. From tax cuts, euthanasia, school choice, and affirmative action, to medical marijuana, minimum wage, eminent domain, and gay marriage, statewide ballot measures have influenced the policy agenda at the national level. Furthermore, recent studies have shown that ballot measures have "educative" and "spillover" effects that can shape the political landscape by altering the strategies of political parties and interest groups engaged in federal campaigns, as well as the choices of voters (Campbell & Monson, 2008; Donovan, Tolbert, & Smith, 2008; Nicholson, 2005; Smith & Tolbert, 2004). There is little question, then, that the process of direct democracy not only affects the political process within a state, but also the political process at the federal level.

But might ballot measures alter the behavior of members of Congress? Though perhaps nonobvious to congressional scholars, statewide ballot measures may affect congressional vote choice by filling a policy void due to congressional inaction, leading eventually to intergovernmental cooperation. At other times, statewide ballot measures may challenge federal statutes, exacerbating tension between the federal government and the states (Ferraiolo, 2008). In the ongoing dance of federalism, ballot measures in the American states can have a national influence despite their subnational nature (Magleby, 1998). What scholars have yet to examine, though, is whether initiatives and referendums in the states influence (both directly and indirectly) congressional roll call voting behavior.

Drawing on an original data set, we investigate whether statewide ballot measures inform the legislative behavior of members of Congress. Specifically, we are interested in whether the passage (or defeat) of initiatives and referendums in a member's district or state may indirectly influence his or her vote when considering similar pieces of legislation on the floor of the Senate and House. Though the substantive nature of the congressional votes we consider may be of interest to some (votes to regulate campaign finance, raise the minimum wage, and ban same-sex marriage), we are theoretically interested in whether added information about the median voter, derived from statewide ballot measures, may help reduce "shirking" by members of Congress. That is, we are interested in whether having statewide popular votes, which provide inimitable insight to members of Congress as to what the preference is of the median voter in their state or legislative district (McDonagh, 1989, 1993), reduces shirking, or "legislative behavior that differs from what would be observed given perfect monitoring and effective punishment by constituents" (Rothenberg & Sanders, 2000, p. 316; but see also Overby, 1991).

By considering how statewide ballot measures might affect congressional voting behavior, our study pushes the boundaries of research on the indirect effects of direct democracy. It also informs the literature on congressional behavior, specifically legislative shirking, as ballot measures in the American states may provide some members of Congress with salient information which may help them more accurately reflect the median preference of their voting constituents. We think the possibility of ballot measures having a discernable, "educative" impact on congressional vote choice is not fanciful. During the Progressive Era, advocates of direct democracy advanced the plebiscitary mechanism in part to have lawmakers align more closely to the popular opinion held by their constituents (Smith & Tolbert, 2004). Finally, our study has important implications for theories of representation, as we consider how members use precise information from their constituencies to inform their own issue positions. Specifically, we contrast the insight provided by the median voter—a simple indicator of whether a ballot measure passed—with a more detailed indicator of public opinion—the actual rate (a continuous measure) of support the ballot measure received in the member's district.

Policy Congruence, Legislative Shirking, and Direct Democracy

It is important to acknowledge at the outset that we suspect the impact of ballot measures on congressional behavior may be minimal. There is ample reason to expect the voting behavior of most members of Congress will not be influenced by a popular vote on an issue in their home state. Driven largely by considerations to ensure their chances of reelection (Mayhew, 1974), elected officials already seem to have a preternatural electoral incentive to represent the preferences of the median voter in their district (Downs, 1957). It is therefore quite rational for incumbents to have an innate understanding of the policy preference of their median voting constituent and vote accordingly on the floor, irrespective of the results of any plebiscitary statewide action. There is good reason to expect strong policy congruence between public preferences and the behavior of elected officials (Dahl, 1956; Downs, 1957). Beginning with the work of Miller and Stokes (1963), research on congressional voting behavior generally has found that members' roll call votes tend to mirror, or represent dynamically, the preferences of their constituents (Conley, 1999; Peltzman, 1984; Richardson & Munger, 1990; Theriault, 2005).

In theory, then, the desire for reelection predisposes members of Congress to reflect the median voter of their constituency. There is a tension, though,

between the theory of representation and the empirical literature of congressional voting. Although members are assumed to adapt to the changes of the median voter, congressional voting behavior portrays legislators as stable ideologues (Poole, 2007; but see Espino & Canon, 2009). Rarely changing their ideological positions, studies have shown that representatives are generally resistant to alter their voting behavior in correspondence with shifts of the median voter (Grofman, Griffin, & Berry, 1995; Lott, 1987). Despite long-term stability of ideology, moments of electoral threat may induce representatives to respond to their median constituent's preference, especially if they suspect the voting public is attentive. Legislators may especially be susceptible to adjusting their voting preferences during periods of "electoral shock," with lawmakers mirroring any shifts in the preferences of the median voter (Kousser, Lewis, & Masket, 2007). Of course, to complicate matters, the importance of the median voter to an understanding of representation is far from settled (Romer & Rosenthal, 1979). Regardless, clear insight to median voter preferences (and change) may curtail instances of policy shirking.

Undoubtedly, there will be "shirkers"—lawmakers who for ideological reasons, or to curry favor with special interests, or because they lack full information about their constituency—may vote in a way at odds with the preference of their constituency's median voter (Bender & Lott, 1996; Kalt & Zupan, 1984; Lott, 1987). It is certainly possible that these lawmakers would really like to represent the median preference of their constituents, but they do not have complete or accurate information to do so. In assessing legislative shirking, many scholars who study legislative behavior readily admit that it is not particularly easy to determine if there is a gap between a legislator's actions and the median preference of his or her constituency (Burden, 2007; Uslaner, 1999). Moreover, researchers have found that legislators are often poor estimators of their constituents' views on certain issues (Hedlund & Friesema, 1972; Uslaner & Weber, 1979). Thus, when assessing legislative shirking, most studies use proxies to measure the preferences of legislators' constituencies (Kalt & Zupan, 1990; Lawrence, 2007; Lott, 1987; Poole & Romer, 1993; Rothenberg & Sanders, 2000, 2007; but see Garrett, 1999; Smith, 2001).

In contrast to less precise measures of constituency preferences, such as small-N public opinion polls or franked constituent surveys, we rely on the election results of statewide ballot measures. Statewide popular votes provide inimitable insight to the median voter's preference on an issue.[1] As McDonagh (1993, p. 189) puts it, "legislators perceive referenda and initiative decisions more easily than they do the general attitudes of their constituents. As a result, grassroot referenda cues give representatives more reliable information about their constituents' issue positions." Given this additional

information, we are interested in the effect of the results of a ballot measure on the roll call voting behavior of members of Congress. That there may be such a connection between statewide ballot measures and congressional voting behavior should not be dismissed out of hand (Overby, 1991). Members of Congress, if they are interested in accurately representing the views of their constituents, must first know what they want. Ballot measures may provide inimitable insight, inducing legislators to moderate their policy preferences in response to a showing of the public will. Across the 50 states, policy responsiveness to median voter preferences is more likely in states that have the initiative process than those that do not (Gerber, 1996; Matsusaka, 1995; Matsusaka & McCarty, 2001). Ballot measure results, then, may serve as a public opinion thermometer, providing members with newfound awareness of their constituents' policy preferences.

At the individual level of lawmakers, though, there is conflicting evidence that elected officials respond to the median voter in their constituencies after statewide initiatives. Kousser, Lewis, and Masket (2007), in a natural experiment resulting from the recall of Governor Gray Davis in 2003, find that California state lawmakers—especially Democrats in the most competitive legislative districts—modified their ideological position on the issues by shifting to the right, in keeping with the preferences of their constituents. However, another study by Masket and Noel (in press), which examines votes on ballot measures in California over time, finds that state lawmakers—especially those in the majority—tend to be more ideologically extreme than their district's median voter. Earlier studies of state legislators and constituent public opinion have similar conflicting results. Examining the congruence between actual versus perceived constituency opinion, Erikson, Luttbeg, and Holloway (1975, p. 245) find that Florida lawmakers are (on the whole) able to accurately identify the preferences of their constituents, concluding that, "the opinions of the public are at least present in the perceptions of legislators when they make public policy." However, they do not take the next step and examine whether this congruence has a discernable impact on lawmakers' voting patterns. Garrett (1999) finds that though a majority of members of West Virginia House of Delegates voted in line with their core constituencies' position on a 1984 lottery adoption, those who voted against their constituencies did not face political repercussions. In his examination of "countermajoritarian" ballot initiatives passed in the 1990s in Colorado, Smith (2001) finds that representatives often reflect their constituency's median voter, and not that of the statewide electorate, but it depends on the issue. In high-use initiative states, such as California, the constituency-representative link seems to keep legislative voting more in-line with constituent preferences

(Snyder, 1996). At the individual level, then, there is mixed evidence that ballot measures lessen policy shirking among state legislators.

Similarly at issue is the nature of representation between the chambers of Congress. As the Federalist Papers make evident, the two chambers were designed to allow members to react differently to public demands, blending democratic responsiveness with republican deliberation (Riker, 1992; Tsebelis & Money, 1997). Federalist 52 argues that frequent elections of the House of Representatives would create a chamber of "common interest with the people," whereas Federalist 63 contends that the longer, staggered Senate terms would insulate members from popular whims and "suspend the blow meditated by the people against themselves, until reason, justice, and truth can regain their authority" (Hamilton, Madison, & Jay, 1788/1961). This difference is regarded as one of the "merits of multicameralism" (Riker, 1992, p. 101). Given the electoral and institutional differences of the House and Senate, members of the two chambers are likely to respond differently to median voter insight. Heeding the wisdom of the Federalists, we expect representatives and senators to react differently when obtaining information about their constituency's median voter in ballot issue contests.

Data, Method, and Expectations

To test the influence of ballot measures on the voting behavior of members of Congress, we have selected three cases that offer a pair of congressional votes straddling a popular vote on a similar statewide ballot measure. In other words, for each issue we identify a congressional vote preceding (T_1) the ballot measure, and another congressional vote subsequent (T_2) to the ballot measure. Perhaps not surprisingly, there are a limited number of congressional votes and statewide ballot measures on the same issue domain. Furthermore, inherent to any temporal study, factors affecting congressional voting behavior vary across time. Redrawn districts, changing demographics, shifting issue saliency, and fluctuations of members' ideologies limit our ability to compare different congressional votes. It goes without saying that finding perfectly comparable votes across time is impossible. We were, however, able to identify a small set of cases that are highly comparable across time, with the two roll call votes in Congress and the ballot measure occupying the same issue space (Downs, 1957; Miller & Stokes, 1963; Stokes, 1963).[2] After probing several potential policy issues, and assessing their comparability, we settled on the following three cases comprising the following issues: Raising the minimum wage; banning gay marriage; and campaign finance reform.

With each issue, we are primarily interested in isolating any educative effects from the ballot measures on the voting behavior of members of Congress. We argue that statewide direct democracy contests supply some members of Congress with inimitable insight into the preferences of their constituency's median voter. The passage or failure of a statewide ballot measure offers inimitable insight for members of Congress. The function of the median voter in deciding ballot measure outcomes is inherently important to our study. Precise insight into median voter preferences puts members of Congress at a representative advantage. Whether they use that information or not is at the core of the shirking debate. Because ballot measures are an exact instrument of constituent preferences, we expect that the median constituent's vote will affect a member's behavior in Congress. If the median constituent votes to support a ballot measure, we posit that the representative will take this educative cue and vote in-line with that signal. For example, if the member voted against the issue in his or her initial vote, we contend that a statewide ballot measure will provide a precise and unambiguous insight that has the potential of altering the representative's floor vote. The importance of the median constituent's preference, then, is inherent to the operationalization of ballot measures as an educative cue.

To test the possible effect of the three statewide ballot issues on congressional voting behavior and legislative shirking, we assembled two data sets—one for members of the U.S. Senate and one for the California delegation of the U.S. House of Representatives. For each of the three issues, we recorded a lawmaker's roll call votes for the vote preceding the ballot measure (T_1) and after the ballot measure passed or failed (T_2).[3]

We acknowledge that approaching congressional voting behavior in this manner is not without controversy. A primary concern is whether these votes and ballot measures lie within the same issue space. However, to conduct a rigorous analysis of the effects hypothesized, we combine cases so that we have an adequate sample size with which to disentangle competing effects, which allows us to generalize the results. By grouping together the different issues, we are able to test the inimitable insight of ballot measures across a variety of issues. The cases in the model—economic, political, and moral—are a microcosm of issues Congress tackles every session. Members regularly face voting decisions comparable to these cases. Second, though the weakness cited above is an intuitive one, we feel our hypothesized effect transcends this potential limitation. That is, a member's concern with representing his or her district should be unaffected by the issue space. Indeed, prior work shows that ballot measures may prime a member's constituency on the issues of minimum wage (Smith & Tolbert, 2010) and gay marriage (Donovan et al., 2008).

Moreover, by including a member's initial vote at T_1 in our modeling strategy, and clustering the observations by case, we can quite effectively account for variation across these issues. In short, although this is a potential area of concern, we think the negative effects of combining cases are less severe than they appear at face value.

Minimum Wage

On the issue of increasing the minimum wage, between 1999 and 2007, eight states passed a minimum wage measure to counter the lack of action at the national level.[4] Each of these ballot measures fall between our first and second congressional votes. For the Senate, we used floor votes from 1999 and 2007. The vote in 1999 was an amendment attached by Senator Kennedy (D-MA) to the "Bankruptcy Reform Act of 1999."[5] The amendment would have raised minimum wage from US$5.15 an hour to US$6.15 an hour by January 1, 2001. The effort to increase the minimum wage in the Senate was repeated with the consideration of the "Social Security and Medicare Lock-Box Act of 2007." Although Democrats had a majority in the Senate, they were unable to control the legislative agenda, as Republicans stymied the majority's effort to pass the bill in its original form.[6] After an onslaught of amendments, the final vote on the bill was 94-3. Instead of using this final vote, marred with logrolling amendments that changed the tenor of the bill, we use the cloture vote that preceded the flurry of amendments. Cloture was not enacted, but the vote considered the minimum wage increase in its original format.[7] The vote was divided primarily along party lines, with only four Republicans voting to invoke cloture.[8] As such, the two floor votes provide the cleanest comparison on minimum wage in the Senate and reduce multidimensionality.

Our House data set is composed solely of members of the California delegation. Since California passed a statewide initiative (Proposition 210) in 1996 raising the minimum wage, we adjusted slightly the time frame to accommodate this measure. In narrowing the period, we are able to minimize the effect of time across the two congressional votes. Our initial vote in the House is on an amendment to a 1996 bill titled, "To amend the Portal-to-Portal Act of 1947 relating to the payment of wages to employees who use employer owned vehicles."[9] The amendment, attached by Representative Riggs (R-CA), sought to increase the minimum wage by 90-cents in 2 years.[10] The amendment passed 266-162 in May, 1996. For the subsequent floor vote, we used a 2000 House bill titled, "To Amend the Fair Labor Standards Act of 1938 to Increase the Minimum Wage." The bill sought to raise the minimum

wage to US$6.15 an hour by April 1, 2002. The measure passed by a vote of 282-143.[11] As with Senate, we feel that these two roll call votes provide the cleanest comparison in the legislative record.

Same-Sex Marriage

With respect to gay marriage, the evangelical movement began increasingly advocating a ban on same-sex marriage in the 1990s. As this issue became more politicized in the 2004 presidential election, several states responded by enacting initiatives and referendums limiting marriage to the union between a man and a woman. Between 1996 and 2006, 28 states passed statutes or constitutional amendments defining marriage through the initiative or referendum process, including the approval of Proposition 22 in California in 2000. Most of these statewide votes occurred in 2004 or 2006.[12] In assessing any change in members' votes on the question of gay marriage, both preceding and following the state-level initiatives redefining marriage,[13] we use the final floor votes on the 1996 Defense of Marriage Act (DOMA) as our initial vote in both the House and Senate.[14] With no amendments added to the bill, it provided a clean look at members' preferences on this issue domain. Our second vote comes from the 109th Congress.[15] With Republicans still in power in 2006, both the House and the Senate attempted to pass resolutions proposing a constitutional amendment defining marriage as between a man and a woman.[16] Both the House and the Senate resolutions failed on the floor.[17] Regardless of their fate, they provide the second congressional vote for our same-sex marriage case.[18]

Campaign Finance Reform

Our final case is campaign finance reform. The increasing support for campaign finance reform ushered in with President Clinton's 1992 victory brought several legislative efforts to stem the influence soft money and campaign spending. In 1996, the House and the Senate voted on campaign finance reform bills that would have limited the influence of soft money. Republicans, attempting to steal the reform baton, introduced a watered-down bill that guaranteed Democratic opposition. Because this bill was an attempt to undermine the Democrats' reform efforts, it poses substantive deviations from our later vote on the Bipartisan Campaign Reform Act of 2002 (BCRA).[19] Although we use the 1996 bill in the Senate due to its similarity with BCRA,[20] to assess the voting behavior of the California delegation we use the 1993 House vote on the "Campaign Spending Limit and

Election Reform Act,"[21] as it is more similar to BCRA than the 1996 House bill. Thus, although we use different initial votes (T_1) for our House and Senate data sets, we employed the same secondary vote (T_2) on BCRA for both the House and Senate.[22] During the intervening period, there were a handful of ballot measures that tried to limit soft money and campaign spending, including Proposition 208 in California in 1996. Between 1996 and 2002, voters in seven states considered increased regulations on campaign finance activities.

Dependent Variable

The dependent variable in the House and Senate data sets is the lawmaker's second roll call vote (at T_2), following the ballot measure. Of interest is whether a member's subsequent vote is predicted by the result of a successful ballot measure in the member's state or district. We hypothesize that the passage of a ballot measure in a member's state or district will have a positive impact on a subsequent congressional vote, that is, that a member of Congress's future floor vote will be partially determined by whether the median voter in his or her constituency voted in favor of a similar ballot measure.

Of course, one limitation of any study examining patterns of congressional voting behavior over time is that some members are replaced between the two votes (T_1 and T_2). Not only does this reduce our sample size, but member replacement works against our hypothesized effect. Some members, of course, voluntarily decide not to seek reelection; others are defeated or pass away while holding office. Of the members omitted from our House and Senate data sets, most were eliminated because they lost reelection.[23] Had those defeated members won their reelection, and had a measure been on the ballot in their state or district, it is likely that they would have been prime candidates to have modified their initial vote (Canes-Wrone, Brady, & Cogen, 2002). In short, member replacement removes a disproportionate number of cases that likely would bolster our findings, but the phenomenon of electoral defeat actually biases our data against our primary hypothesis.

Independent Variables

The key causal variable in both the House and Senate models is the passage (or rejection) of a ballot measure in a member's district or state. The manner in which we model this effect has important implications for the present study as well as the study of representation more generally. On the one hand, it is possible to model the passage of a ballot measure as the direct insight of

the member's median constituent. The hypothesis is that the passage of a ballot measure (indicated by a dichotomous variable) provides a member with exact information about what at least half his or her constituency prefers. Thus, it does not matter whether the ballot measure passes with 51% of the vote or 80%; both are equivalent in terms of the signal provided by the median voter. The second option is to model the passage of a ballot measure in a continuous fashion. Conceptually, the hypothesis is that a ballot measures passing (failing) by wide margins might provide a stronger signal than narrowly approved (rejected) ballot measures. By utilizing both constructions, we are able to add to the debate on the congruence between public opinion and public policy.

We control for a number of other factors. The principal control variable is a member's vote on the issue at T_1 (denoted by $Vote_{T1}$). Because legislative voting on issues is relatively stable across time and is not likely to change (Poole, 2007), we expect the earlier vote to capture most of the variance of the subsequent vote (T_2). Conceptually, this variable controls for a host of factors not explicitly accounted for in the models, reducing the pitfalls of omitted variable bias. However, using a member's initial vote as a control is yet another fairly high hurdle for the hypothesized effect of a successful ballot measure to overcome. Second, we account for constituent preferences. In the House models, the variable CD Ideology is the proportion of the presidential vote in a member's district during the previous election. For example, with regard to raising the minimum wage and limiting campaign finance (supposedly liberal issues), we utilize the proportion of the vote for the Democratic presidential candidate. Conversely, for banning gay marriage (a conservative issue), we use the proportion of the vote for the Republican presidential candidate. Thus, higher values indicate a district more likely to support their member when voting in favor of the issue. To account for possible shifting constituent preferences in the House, we include a variable for any change in congressional district ideology (denoted *CD Ideology Change*). We measure this variable as the difference in the proportion of the presidential vote (using the same coding scheme as above) from T_1 to T_2.[24] Positive values indicate the district shifted in a direction supporting the issue: negative, against. In the Senate models, we control for the same constituent preference constructs as in the House.[25]

We also expect legislator responsiveness to be contingent on whether the member seeks reelection and occupies a safe seat. Although most legislators are single-minded reelection seekers (Mayhew, 1974) predisposing them to act as delegates as opposed to trustees, retiring members may have more voting leniency. We expect retiring members (Retiring) to be more likely to

shirk the preference of the median voter in their state or district because they are not vying for reelection. We also control for if a member has a safe seat or not (Safe Seat). We expect members from safe seats may be more likely to be policy shirkers because they have little risk of being defeated, come reelection. We determined whether a member hailed from a safe seat using the Cook Political Report's rating just prior to a member's reelection at T_2 with races coded "likely" and "safe" considered secure whereas races classified as "lean" or "toss-up" considered unsecure.[26] Finally, in our Senate models, we include a reelection variable. Theoretically, this factor is a constant in the House models, as every member is up for reelection at the same time. For the Senate models, we include a dichotomous variable for whether a member was up for reelection in the period preceding the vote at T_2 (denoted *Reelection*). All three dummy variables are coded to estimate the likelihood of a member voting against his or her constituency. That is, we code each variable relative to whether a ballot measure passed or failed in a member's district or state.[27] Positive coefficients indicate that the variable increases the likelihood of a member voting with his or her constituency (less likely to shirk), whereas negative coefficients indicate that the variable decreases the likelihood of a member voting with his or her constituency (more likely to shirk).

A number of other member-specific controls are included as well. First, we control for a member's party (Party), as the floor votes on all of these highly salient issues fell with few exceptions along party lines. Because of the recent a resurgence of party loyalty within Congress (Aldrich, 1995; Cox & McCubbins, 2007; Rhode, 1991), we expect that a member's party will have a positive effect on the voting patterns in the models for both the Senate and House. The variable is coded one if the member's party supported passage of the policy.[28] We also coded each member's ideology at the time of his or her roll call vote, T_2 (denoted DW_{T2}). Because changes in a legislator's ideological preferences may account for differences in voting patterns, we coded any change in a member's ideology from T_1 to T_2 (DW Change). Both measures utilize Poole and Rosenthal's DW-Nominate data and were coded in the same directional fashion (i.e., so that higher values indicate ideological support for the tenor of the bill rather than political conservatism) as with the district ideology variables described above.[29]

In the House models, we include a measure of issue salience. We measure issue salience as the "percent rolloff" on a given California ballot measure. That is, we calculate issue salience by dividing the total votes on a ballot measure in a district by the total vote for all the House candidates on the ballot in the same election, and then subtract this total from 1. In the case of

Proposition 22 in 2000 (ban on same-sex marriage), in nearly every California House district, more citizens voted on the ballot measure than for all the House candidates, combined. Unfortunately, we are unable to include a measure of issue salience in Senate models, as not all the states had popular votes on the set of issues. Still, it is important to control for variation in the three issues, even if we are only able to do so in one chamber. We reverse code this variable according to the fate of the ballot measure in a member's district in the same manner as Retiring, Safe Seat, and Reelection.

Finally, we account for lag effects between the date a ballot measure passed and the subsequent vote (denoted *Lag*). It is possible that as time elapses between the passage of the ballot measure and a member's roll call decision, the signal from his or her constituency becomes less salient (because of faulty memory, changes in constituency makeup, exogenous events, etc.). This is especially important given electoral differences between the two chambers and the consequences for representation. This variable is coded as the number of days between the two time periods.

Findings: Predicting Legislative Votes

To examine the effect of state ballot measures on congressional roll call voting, we estimate a series of probit models with the House and Senate data separately. Tables 1 and 2 present the results of multiple model specifications, providing some assurance that the findings are robust. The dependent variable in each model is a member's vote at T_2 (1 if *aye*, 0 if *nay*). Standard errors are clustered by case to correct for within-case correlated errors.

House Models

Table 1 presents the House results. The primary independent variable in Models 1 through 3 is the dichotomous indictor of the median voter's preference in the member's district (denoted *Passage*). The variable assumes the value one whenever the median voter approved the ballot measure (0 if he or she voted to reject it). Models 1 and 2 include the member's vote at the first time period as a control ($Vote_{T1}$). Because this variable captures a large proportion of the variance in a member's vote at the second time period, we think these models provide a rigorous test of the central theory. Also included in these two models are measures of district ideology change (CD Ideology Change) and member ideology change (DW Change). The underlying logic is that while a member's vote at T_1 controls for a number of factors relevant to his or her vote at T_2, it cannot control for shifts in ideology or

Table 1. The Effect of Ballot Measure Passage on Vote Choice at T_2 (House)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Vote$_{T1}$	4.635*** (0.108)	4.204*** (0.079)		4.051*** (0.272)	3.806*** (0.304)	
Passage	3.395*** (0.145)	2.96*** (0.053)	3.053*** (0.410)			
Passage rate				3.973 (4.651)	0.425 (3.201)	−9.191 (10.549)
Safe seat	−2.06*** (0.707)	−1.717* (1.001)	−0.574 (0.519)	−1.163** (0.550)	−0.649 (0.987)	0.244 (0.676)
Retiring	0.191 (1.176)	0.334 (1.39)	−0.843 (1.111)	−0.543 (1.178)	−0.335 (1.318)	−1.121 (0.834)
Time lag	−0.001** (.001)	−0.002*** (0.001)	−0.003*** (0.001)	−0.001*** (0.001)	−0.001 (0.001)	−0.002*** (0.001)
Salience	−4.562 (2.949)		−1.381 (3.578)	−5.650 (3.806)		−0.014 (4.511)
DW change	15.778*** (5.413)	12.161* (7.038)		11.383** (5.630)	7.901 (7.078)	
CD ideology change	−0.048*** (0.009)	−0.050*** (0.017)		−0.034*** (0.005)	−0.036*** (0.014)	
DW$_{T2}$			7.352*** (0.953)			9.161** (3.892)
CD ideology			0.024*** (0.004)			0.074*** (0.024)
Party			−1.480 (1.105)			−2.224 (2.359)
Constant	0.352 (2.189)	1.212 (1.788)	5.043** (1.460)	0.032 (3.032)	2.075 (2.317)	11.108 (7.582)
Observations	100	100	100	100	100	100
R^2	.79	.77	.78	.76	.73	.77

Note: Cell entries are probit coefficients with standard errors in parentheses, clustered by case. The Hosmer-Lemeshow goodness-of-fit statistic for each model is 37.58, 64.86, 27.03, 49.93, 136.89, and 27.46 (respectively), indicating our specifications fit the data very well.
*$p < .10$. **$p < .05$. ***$p < .01$.

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Table 2. The Effect of Ballot Measure Passage on Vote Choice at T_2 (Senate)

	Model 1	Model 2	Model 3
Vote$_{T1}$	2.150*** (0.590)		
Passage	0.1439 (0.332)	−0.113 (0.787)	0.042 (0.649)
Safe seat	−0.030 (1.039)	0.021 (0.581)	0.941 (0.778)
Retiring	−0.099 (0.386)	1.150*** (0.401)	0.678*** (0.142)
Time lag	−0.001 (0.001)	−0.001*** (0.001)	−0.001*** (0.001)
DW change	8.234*** (0.445)		
State ideology change	0.0238 (0.040)		
Reelection	0.466*** (0.172)	0.779*** (0.239)	0.550*** (0.210)
DW$_{T2}$		8.114*** (1.074)	
State ideology		−0.016 (0.022)	−0.010 (0.026)
Party		−2.634*** (0.630)	
Senate index			3.491*** (0.323)
Constant	−1.153 (1.331)	2.147*** (0.717)	−0.704 (1.314)
Observations	166	166	166
R^2	.47	.71	.59

Note: Cell entries are probit coefficients with standard errors in parentheses, clustered by case. The Hosmer-Lemewhow goodness-of-fit statistic for each model is 183.28, 111.69, 114.81, respectively, indicating our specifications fit the model very well.
*$p < .10$. **$p < .05$. ***$p < .01$.

district ideology from T_1 to T_2. Model 3 excludes the vote at T_1 control variable, substituting in its place several constitutive variables.[30] These include a member's ideology at T_2 (DW$_{T2}$), district ideology at T_2 (CD Ideology), and party (Party). Models 4 through 6 in Table 1 provide identical specifications as models 1 through 3, except that the key independent variable is the percentage of voters who voted for the ballot measure in a member's district (Passage Rate). Comparison of the ballot measure variables across the models allows us to assess the strength of the two hypothesized constituent signals and speak to the general questions concerning representation and policy congruence.

Models 1 through 3 reported in Table 1 fit the data well and explain a large proportion of the variation in the dependent variable.[31] Most important, we find a significant, positive relationship between the median voter signal and a representative's vote at T_2 across all three models. This effect confirms our primary hypotheses that successful ballot measures reduce policy shirking among House members by revealing the true preference of a member's

Table 3. Predicted Probabilities for Statistically Significant Covariates (House)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Vote$_{T1}$*	0.98	0.96		0.96	0.94	
Passage* <or> passage rate	0.86	0.83	0.56			
Safe seat*	−0.02	−0.42		−0.02		
Retiring*		−0.03				
Time lag	−0.01	−0.01	−0.01	−0.01	−0.01	−0.01
Salience						
DW change	0.47	0.81		0.63		
CD ideology change	−0.01	−0.01		−0.01	−0.03	
DW$_{T2}$			2.8			3.2
CD ideology			0.01			0.03
Party						

Note: For dichotomous covariates (*), predicted probabilities are calculated for a discrete change (from 0 to 1). For all other covariates probabilities are calculated at their median value.

median district voter. Table 3 provides the predicted probabilities for each significant coefficient in the first three models. In Model 1, a ballot measure passing in a member's district is estimated to increase his or her probability of voting in favor of the same policy at the subsequent time period by about 86 percentage points (setting all other control variables at their median value). In Models 2 and 3, the corresponding predicted probabilities are 83 percentage point and 56 percentage point increases, respectively, holding all else constant.

A number of control variables in Table 1, Models 1 through 3, achieve statistical significance and are in the predicted direction. As expected, a member's vote at T_1 is a strong predictor of the floor vote at T_2. The predicted probabilities in Table 3 approach 100% for Models 1 and 2, lending support to the hypothesis that members of Congress "die in their ideological boots" (Poole, 2007). The significant negative coefficient on the Time Lag variable across all three models indicates that policy shirking increases as time increases between the passage of a ballot measure in the district and a member's subsequent vote (Tsebelis & Money, 1997). In Model 1, there is a significant, positive effect with a member's ideology and voting in favor of the subsequent legislation. This effect approaches statistical significance in Model 2 (.10 level), but not in Model 3. Consistent with our hypothesis, members from safe districts are more likely to vote against their constituents'

policy wishes, controlling for all else, though this variable only reaches significance in Model 1. In Model 3, we find that a member's ideology at T_2 and his or her district ideology are both significant (positive) predictors of roll call behavior. We could not confirm a significant party effect in Model 3. Generally speaking, the three models perform very well, with most coefficients in the expected direction. The only unexpected result is the negative coefficient on district ideology change, suggesting that members from districts that became more ideologically aligned with an issue were less likely to represent the views of their constituents.

The results of Models 4 through 6 in Table 1 largely confirm the results of Models 1 through 3 for each of the control variables. Predicted probabilities for each of the statistically significant coefficients are included in Table 3. The time lag variable is significant and negative (though not in Model 4). In all three models, we find that members from safe seats are more likely to shirk their constituents' preferred policies. Members who became more ideologically predisposed toward the issue after T_1 were more likely to vote in this direction at T_2. In Model 3, we find that lawmakers who had a favorable ideology in relation to the policy and who were from ideologically favorable congressional districts were more likely to vote to pass the bill in Congress. Contrary to expectations, though, we again find that members from districts that shifted in the direction of the ballot measure were less likely to vote to pass subsequent legislation at T_2.

The most telling difference between Models 1 through 3 and Models 4 through 6 in Table 1 is the effect of the primary independent variables. In Models 4 through 6, we find no evidence that the percentage of support for a ballot measure has a significant effect on a member's subsequent roll call voting patterns. In Model 5, in fact, the coefficient is approaching statistical significance (.10 level only), but is negative. We believe this lack of evidence supports our overarching hypothesis regarding the median voter signal. The significant effect of the dichotomous ballot passage measure coupled with the insignificant effect of the passage rate variable indicates that any information beyond the vote of the member's median voter is background noise and actually confounds the effect.[32] This is likely the case for two reasons. First, our binary dependent variable cannot accurately capture the degree to which a member supports legislation. Members may fervently support legislation but are unable to articulate that support through a binary vote choice. Second, constituencies with extraordinary support for a ballot measure minimize members' uncertainty. In short, high levels of constituent support make it easy for members to represent their interests. Inimitable insight to constituency preferences is most influential when constituency opinion is unknown

or uncertain. We think this makes sense conceptually (that is, from the perspective of lawmakers) as the difference between a ballot measure passing with 50% (plus 1 vote) versus failing with 49% of the vote is much different than the difference between a ballot measure passing with 75% versus passing with 74%. In summary, the evidence reported here suggests that it does not matter if a ballot measure passed by a close margin or a landslide—the median constituent is paramount.

Senate Models

In modeling voting behavior and policy congruence in the Senate, a primary goal was consistency with the House specifications. Ultimately, however, we were required to make some adjustments. First, we included a dichotomous variable for whether a member was up for reelection during the period immediately preceding the vote at T_2 (Reelection). As mentioned previously, reelection is a constant in the House models. Second, because not all states (unlike California's districts) voted on ballot measures, there is no way to calculate comparable rolloff data as a proxy of issue saliency for the Senate data set. Also, we encountered problematic levels of collinearity between the party and DW-nominate variables at T_2 ($r = .92$).[33]

Table 2 presents the Senate results, and the predicted probabilities for the statistically significant covariates are highlighted in Table 4. A number of controls are as expected. In Table 2, Model 1, a member's vote at T_1 is a strong predictor of the vote at T_2. The length of time between the ballot measure passing and the subsequent vote (Time Lag) decreases the likelihood of a Senator voting with his or her district in Models 2 and 3. The reelection variable is highly significant across all three Senate models. Holding all other variables at their mean, when a Senator is up for reelection, the estimated increase in the predicted probability he or she will vote "yes" at T_2 is between 11 and 22 percentage points greater in the three models. In Model 2, we find a member's ideology to be a significant (positive) predictor of his or her voting patterns, and in Model 3, the ideology-party (Senate Index) composite index is significant and positive. Contrary to expectations, Models 2 and 3 reveal that retiring members are 13 and 27 percentage points more likely to vote in the direction of their constituents at T_2, respectively.

Unlike the House results, then, we find no evidence that the passage of a ballot measure has any discernable effect on a Senator's subsequent voting behavior. The coefficients in each model are many times smaller than their standard errors. In Model 2, the coefficient is negative, though not statistically significant. There is no reason to suspect the models are misspecified,

Table 4. Predicted Probabilities for Statistically Significant Covariates (Senate)

	Model 1	Model 2	Model 3
Vote$_{T1}$*	0.69		
Passage*			
Safe seat*			
Retiring*		0.13	0.27
Time lag		−0.01	−0.01
DW change	2.60		
State ideology change			
Reelection	0.13	0.11	0.22
DW$_{T2}$		1.80	
State ideology			
Party		−0.80	
Senate index			0.35

Note: For dichotomous covariates (*) predicted probabilities are calculated for a discrete change (from 0 to 1). For all other covariates probabilities are calculated at their median.

as the direction of a number of the control variables are as expected.[34] Although we anticipated finding some evidence for the hypothesized effect of statewide ballot measures in the Senate, there is a good reason why it might be more pronounced in the House. There is considerable scholarship documenting that House members are more reactive to public opinion and reflective of the expressed preferences of their constituents than are Senators (Canes-Wrone et al., 2002; Fenno, 1978; Jacobson, 2004). Thus, we find that the effects of direct democracy on members of Congress appear to fall in line with the Federalist's original bicameral expectation regarding representation and subsequent theories of policy congruence.

Conclusion

Recent work has begun to untangle the effects of direct democracy on state and federal politics. Scholars have shown that ballot measures can have "educative" and "spillover" effects that influence not only the strategies of political parties and interest groups, but the choices of voters as well (Donovan, Smith, & Tolbert, 2008; Nicholson, 2005; Smith & Tolbert, 2004, 2010). There is little question that direct democracy impacts not only the way politics are practiced within a state, but also politics at the federal level. Despite this recent work, scholars have yet to fully examine the linkage

between direct democracy, which is often conceptualized as a state-level phenomenon, and the behavior of members of Congress (but see Ferraiolo, 2008; Magleby, 1998). We hope our research offers a constructive step in filling the gap in the literature.

Our overarching conclusion is that the passage of some statewide ballot measures can have an indirect, educative effect on the voting patterns of members of Congress. Ballot measures provide inimitable insight on policy-specific information about the preferences of a member's median constituent. As information-seekers, there is good reason to think that some lawmakers will value the precise information that ballot measures provide about their constituencies, which is more detailed than that gleaned from polling, prior election results, or a lawmakers' "general intuition." Thus, the information provided by statewide ballot measures may induce congressional lawmakers to be less likely to shirk and vote against their constituencies (Bender & Lott, 1996; Kalt & Zupan, 1984; Lott, 1987). Indeed, as we suggested at the outset, it is entirely possible that (at least some) lawmakers who are branded as "deviants" or "shirkers" merely posses imperfect information about the policy preferences of their home state (or district) voters (Hedlund & Friesema, 1972; Uslaner & Weber, 1979). Although our findings and causal explanation have theoretical utility, methodologically our analysis is uniquely suited to address the question of shirking. Whereas previous studies use imprecise proxies for a member's constituency (Kalt & Zupan, 1990; Lawrence, 2007; Lott, 1987; Poole & Romer, 1993; Rothenberg & Sanders, 2000, 2007; but see Garrett, 1999) we think that statewide ballot measures provide a more direct metric and afford us greater leverage on the question of shirking.

The precision of ballot measures to illustrate constituent policy preferences has an added dimension. Passage of a ballot measure—a function of the median voter's policy choice—provides inimitable insight to members of Congress. Moreover, the strength of this signal does not diminish with respect to the ballot measure's passage rate. In other words, regardless of a ballot measure's margin of victory, the median voter's signal carries the same weight. Therefore, whether direct democracy legislation narrowly or easily passes, the representative's insight is equivalent. One caveat, of course, is that if members do not face reelection, they may be more likely to ignore the insight garnered from the median voter. Our Senate findings suggest that longer congressional terms can mute the willingness of members to reflect the preferences of their constituencies, potentially creating less policy congruence (see Bartels, 2008). In the House, though, we find that this signal is a significant factor in a member's voting calculus.

Some scholars have recently suggested that the congruence between the votes of members of Congress and their constituencies has declined with increased levels of polarization and national party influence on Capitol Hill (Ansolabehere, Snyder, & Stewart, 2001; Cox & McCubbins, 2007). The findings we present here suggest that in some contexts the information gained from statewide ballot measures is able to provide inimitable insight into the median preference of a member's constituency. The educative cues provided by direct democracy, then, may push some legislators, especially members of the House who are constantly running for reelection, away from contemporary party pressures and polarization found in Washington, DC.

Finally, as is often the case in novel studies, our research has raised a number of new questions. First, although our findings on the House are welcome news for a number of scholars—advocates of a "delegate" model of representation, those who emphasize the saliency of ballot measures and state politics and proponents of median voter theories of representation—one main caveat is that this linkage may not reach the upper chamber. However, by limiting our data to three particular issues in search of a parsimonious model, we may be sacrificing some degree of generalizability. It is possible that our sample size is simply too small to uncover such an effect in the Senate. At the same time, we admit that variation across the three issues is an unexplored dynamic in our study. Exploring this dynamic, for example by stratifying the analysis by issue area, is impossible given the very small sample size. Needless to say, this is a first step. Despite these natural limitations, we believe the main findings have powerful implications. A more thorough study with an expanded data set seems to be a fruitful endeavor for future research.

Declaration of Conflicting Interests

Funding

Notes

1. We assume that members are more interested in catering to their median *voter* rather their median *constituent*. One of the inherent limitations of relying on public opinions polls for information on the median voter is the obvious fact

that a large segment of the public does not vote. This underscores our belief that statewide ballot measures provide a cleaner signal than any other source.

2. Bills that dealt directly with the issues we selected were rare and often riddled with amendments that fundamentally altered the substance of the legislation. When this dilemma presented itself, we opted to use amendments or cloture votes that more specifically targeted our issues. Although this was not necessary for all of the issues, we employed this tactic to provide the cleanest comparison between the two congressional votes.

3. One limitation we are unable to account for in the Senate model is variation in the election. That is, in some states, the ballot measures appeared in the primary election whereas in others they appeared in the general election. Because other states have no ballot measures at all, we cannot introduce a variable to properly account for this difference. In the House models, we are confident that this is not an issue. The minimum wage and campaign finance ballot measures appeared in the November general election. The gay marriage ballot measure, though appearing in a primary election, had characteristics more closely resembling a November general election. The March primary was competitive—both parties' primary races were undecided—and had high turnout—53.9%.

4. Arizona, Colorado, Florida, Missouri, Montana, Nevada, Ohio, and Oregon all passed ballot measures raising the minimum wage. The Florida case is omitted from our data set because it did not have a Senator residing in both the 106th and the 110th Congresses.

5. S.625, 106th Congress 1st Session. Amendment to the bill: S.Amdt.2751, "To amend the Fair Labor and Standards Act of 1938 to increase the Federal minimum wage," passed 50-48 on November 9, 1999.

6. H.R.2 of the 110th Congress, 1st Session. Passed 315-116 (Roll Call No. 18) on January 10, 2007.

7. We understand this may be a limitation to our analysis. We opted to use amendments or cloture votes that more specifically targeted our issues. This has been done in several studies attempting to measure particular issue domains (Bartels, 2009).

8. Senate cloture vote on H.R.2, 110th Congress, 1st Session. Cloture not invoked 54-43 (Roll Call No.: 23) on January 24, 2007.

9. H.R.1227 of the 104th Congress, 1st Session. Bill title: "To amend the Portal-to-Portal Act of 1947 relating to the payment of wages to employees who use employer owned vehicles."

10. H.Amdt.1084 of the 104th Congress, 1st Session. Pass 266-162(Roll Call No.: 192) on May 23, 1996. Amendment changed House Bill H.R.1227.

11. H.R.3846 of the 106th Congress 2nd Session. Passed 282-143 (Roll Call No.: 45) on March 9, 2000. There were three exemptions to the minimum wage increase:

computer professionals; certain sales employees; funeral directors. The amendment and bill had more party defections than our Senate case; however, a bivariate correlation of .8938 between the votes at T_1 and T_2 suggests we are capturing a pair of votes occupying the same issue space.

12. Again, not all of these ballot measures are represented in our data set. Only states with representatives voting at both T_1 and T_2 were included in the data set. Due to legislators retiring or losing reelection, the following states' ballot measures were not included in the data set: Arkansas, Colorado, Georgia, Kansas, Louisiana, and Nebraska.

13. The second congressional vote (T_2) in the House and the Senate voted on the bill with the exact same text.

14. H.R.3396 of the 104th Congress, 2nd Session. Passed in the House 342-67 (Roll Call No.:316) on July 12, 1996. Passed in the Senate 85-14 (Roll Call No. 280) on September 10, 1996.

15. In the same-sex marriage case, votes at T_1 and T_2 straddle redistricting in California. During this time several district lines were redrawn. We control for this using multiple district ideology and member ideology measures tracking changes from T_1 to T_2. Redistricting complicates the analysis, but is an inherent difficulty with dynamic data sets.

16. H.J.RES.88 of the 109th Congress 2nd Session. Failed (needed two-thirds majority) 236-187 (Roll Call No.: 378) on July 18, 2006. In the Senate: S.J.RES.1 of the 109th Congress 2nd Session. Cloture failed 49-48 (Roll Call No.: 163) on June 7, 2006.

17. The House vote failed: 236-187. In the Senate, cloture was defeated: 49-48.

18. A statutory regulation defining marriage and a nonbinding resolution to amend the Constitution to define marriage are admittedly different considerations. Legislators that supported the passage of DOMA might find it difficult to support a similar nonbinding resolution supporting a constitutional amendment to ban gay marriage. Despite the inherent differences that these votes represent, we believe they reside within the same issue space. Our control statistics indicate that these votes do not tap different issue dimensions. If anything, the inherent differences between these two roll call votes should work against our primary hypothesis.

19. H.R.2356 of the 107th Congress 2nd Session. Passed in the House, 240-189 (Roll Call No.: 34) on February 14, 2002. Passed in the Senate with a vote of 60-40 (Roll Call No.: 54) on March 20, 2002.

20. S.1219 of the 104th Congress 2nd Session. Cloture was not invoked by a vote of 54-46 (Roll Call No.: 168) on June 25, 1996.

21. H.R.3 of the 103rd Congress 1st Session. Passed the House 255-175 (Roll Call No. 605) on November 22, 1993.

22. In the House, the bivariate correlation between the 1993 vote and the 2002 vote is 0.9372. In the Senate, the bivariate correlation between the 1996 vote and the 2002 vote is 0.7859. Using the 1996 vote in the House would have created a negative correlation between congressional votes at T_1 and T_2.

23. Across the three cases, up to 54 House members and 53 Senators were dropped because they were no longer in office at T_2 to vote on one of the three issues.

24. District presidential vote data available from the California Secretary of State: http://www.sos.ca.gov/elections/election.htm

25. We derived these measures from a series of *CBS/New York Times* national public opinion polls (Erikson, Wright, & McIver, 1993). These data were updated to include party and ideology measures as late as 2003. Data are available from Gerald Wright, http://php.indiana.edu/~wright1/. In two cases, the year of the data at T_2 does not correspond with the legislator's second vote. Due to data limitations, we were forced to use 2003 ideology measures from the Erikson, Wright, and McIver data set to approximate the ideology of a state in 2006; however, because state political ideology is mostly stable over time, we believe this is an acceptable shortcoming (see Brace, Arceneaux, Johnson, & Ulbig, 2004; Erikson, Wright, & McIver, 2001).

26. For our campaign finance case, this information was not available from the Cook Political Report. We employ the same measure as the Cook Political Report to identify safe and unsafe seats during this time. In other words, we coded members who won with less than 55% of the vote with a zero and members winning with more than 55% with a one to identify a safe seat. There was one instance where a member lost his primary election and did not have the opportunity to run in the general election. For obvious reasons, we coded this member as residing in an unsafe seat. Furthermore, for Senators, there is an additional obstacle in coding the safe seat variable, as only one third of the Senate is up for reelection in any given election. We believe it makes theoretical sense to list members not up for reelection at the time of T_2 as residing in safe seats. Regardless of their reelection chances, 2 or 4 years down the road, these Senators are not under any immediate electoral pressure at the time of the second vote.

27. For example, safe seat is coded one if a member's district or state voted to adopt a ballot measure and coded zero if a member's district or state voted to reject a ballot measure. Thus, a negative effect indicates that members from a safe seat are less likely to vote in the same direction as their constituency. (Stated differently, they are more likely to shirk.) By coding variables in this manner, we are effectively creating interaction terms.

28. A series of bivariate correlations shows that the dependent variable had a positive correlation with minimum wage and campaign finance and a negative correlation with same-sex marriage. To make sure that all of our variables were coded in the

same direction, we switched the party identification variables for same-sex marriage in both the House and the Senate data sets. For the same-sex marriage case only, Republicans are coded 1 and Democrats are coded 0. Recoding this variable ensures that a vote T_2 indicates that the lawmaker's party would support the issue.

29. We use only the first dimension Poole and Rosenthal's DW-Nominate data. This dimension measures economic policy preferences of members of Congress. For reasons of data accuracy, the social (second) dimension was not used. We understand that these roll call ideology measures of the first dimension do not perfectly align with all of our cases (i.e. same-sex marriage). Nonetheless, the first dimension is the most reliable data on roll call preferences over the time span we have selected (Poole & Rosenthal, 1997). We also wanted to control for a member's ideology at T_2, but unfortunately there is considerable collinearity among some of the predictors and lawmaker ideology. We choose to remove this variable from the models as there are other analogs we include.

30. Alternatively, we tested a specification where member ideology was included in a model with the $Vote_{T1}$ variable. We uncover the same significant effect on our primary independent variable. We opt not to include this specification because—consistent with our expectations—it introduces problematic levels of collinearity; $Vote_{T1}$ and ideology are correlated at .89.

31. As indicated by Hosmer-Lemeshow goodness-of-fit tests (Lemeshow & Hosmer, 1982). See the note in Table 1 for these statistics.

32. To be clear, we are not implying that members from districts that overwhelming pass a ballot measure *care* about the corresponding issue to the same extent as members from districts that narrowly pass a ballot measure. Rather, we are arguing that in terms of representation, the direction of the median voter is the most important signal.

33. The negative and significant coefficient on party in Table 2, Model 2, is likely a direct outcome of this problem. To properly account for this, Model 3 combines the two offending variables (Cronbach's $\alpha = .95$) into a single index (labeled *Senate Index*).

34. Additional specifications of the Senate model were examined, including splitting the sample based on a member's vote at T_1. This approach, as well, failed to identify the hypothesized effects.

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