The Initiative to Shirk? The Effects of Ballot Measures on Congressional Voting Behavior

Josh Huder

Jordan Ragusa

Daniel A. Smith*

Abstract

For over a century, the federal government has responded, either directly or indirectly, to the passage of statewide ballot measures. But do statewide ballot measures affect congressional voting behavior? Drawing on an original dataset, we investigate whether successful statewide ballot measures might inform the legislative behavior of members of Congress, specifically if the passage of initiatives and referendums may indirectly influence members' floor votes on similar pieces of legislation. Theoretically, we are interested in whether ballot measures—which provide precise information about the median preferences of a member's constituency—help reduce policy "shirking" by members of Congress. Our findings across three issues in both chambers of Congress indicate that the passage of ballot measures by a member's constituency may alter a member's floor vote in the House on parallel legislation, reducing legislative shirking, but that such an effect is attenuated in the Senate by other factors.

*Corresponding author. Associate Professor & Interim Director, Political Campaigning Program, Department of Political Science, University of Florida. dasmith@polisci.ufl.edu

The Initiative to Shirk? The Effects of Ballot Measures on Congressional Voting Behavior

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, ballot measures have helped to set the policy agenda at the national level. Recent studies have shown that ballot measures can have "educative" and "spillover" effects that 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 (Smith and Tolbert 2004; Nicholson 2005; Smith, DeSantis, and Kassel 2006; Smith 2006; Smith and Tolbert 2007; Donovan, Tolbert, and Smith 2008). There is little question that direct democracy impacts not only the way politics are practiced within a state, but also at the federal level.

In this paper we suggest that the passage of statewide ballot measures (initiatives and referendums) may have the indirect, educative effect of altering the voting behavior of members of Congress. While rarely considered, statewide ballot measures may affect congressional politics (Ferraiolo 2008). The policy effects of ballot measures at the federal level may vary. Sometimes statewide ballot measures may fill 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. In the ongoing dance of federalism, ballot measures in the American states can have a national influence despite their subnational nature. Many ballot measures have at once challenged and complemented federal policymaking. What scholars have yet to examine, though, is how direct democracy in the states may affect congressional voting behavior.

Drawing on an original dataset, we investigate whether successful statewide ballot measures might inform the legislative behavior of members of Congress. Specifically, we are interested whether the passage of initiatives and referendums indirectly influence the vote choice of members of Congress 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 statewide ballot measures may help reduce "shirking" by members of Congress. That is, we are interested in whether having statewide popular votes, which provide precise information to members of Congress as to what the preference is of the median voter in their state or legislative district, reduces shirking, or "legislative behavior that differs from what would be observed given perfect monitoring and effective punishment by constituents" (Rothenberg and Sanders 2000: 316). In a word, can statewide ballot measures alter congressional legislative behavior so that it becomes aligned more closely with the median constituent preference?

To determine whether statewide ballot measures may modify legislative behavior at the federal level, we examine how well successful ballot measures in a member's state (Senate) or district (House) predict floor votes by members on similar issues. By considering the effects of successful ballot measures on the voting behavior of members of Congress, we can assess whether members representing states (or districts) where ballot measures have passed are more likely to heed the median constituent preference, or if they might deviate from it. By considering how ballot measures might affect congressional voting behavior, our study pushes the boundaries of research on the indirect effects of direct democracy. The idea that ballot measures may have a discernable, educative impact on vote choice at the congressional level 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 of their constituents (Goebel 2002; Piott 2003; Smith and Tolbert 2004). Our study also informs the considerable 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 constituents.

Direct Democracy and Legislative Shirking

It is important to acknowledge at the outset that we suspect the impact of ballot measures on congressional behavior to be minimal. There are good reasons 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 have a preternatural electoral incentive to represent the preferences of the median voter in the district (Downs 1957). It is likely, then, that incumbent lawmakers have an innate understanding of the policy preference of their median constituent and thus will usually vote accordingly on the floor, irrespective of the results of any plebiscitary statewide action. Indeed, 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 the preferences of their constituents (Peltzman 1984; Richardson and Munger 1990; Conley 1999; Theriault 2005). Scholars have theorized that there is a strong, casual connection between public preferences and the behavior of elected officials (Dahl 1956; Downs 1957).

Undoubtedly, though, there will be "shirkers," lawmakers who for one reason or another vote in a way at odds with the preference of the median preference in their district or state (Kalt and Zupan 1984; Lott 1987; Bender and Lott 1996). 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 whether there is a gap between a legislator's actions and the median preference of his or her constituency (Uslaner 1999; Burden 2007). In assessing legislative shirking, most studies use proxies to measure the preferences of legislators' constituencies (Kalt and Zupan 1990; Rothenberg and Sanders 2000; 2007; Lawrence 2007; Lott 1987; Poole and Romer 1993; but see Garrett 1999). In contrast to less precise measures of constituency preferences, such as haphazard public opinion polls or franked constituent surveys, we rely on the results of statewide ballot measures. Statewide popular votes provide a direct measure of the median voter's preference on an issue.

Provided that a member hails from an initiative state, statewide ballot measures provide a direct measure of the preferences of the member's constituency on a range of salient issues. Given this additional information, we are interested whether members of Congress in states where there have been popular votes on issues are inclined to change their voting behavior on legislation in light of the results from a popular vote.

Scholars, however, have yet to consider whether ballot measures have any indirect effects on 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. Ballot measures can provide an institutional check, serving as a "gun behind the door" (Lascher, Hagen, and Rochlin 1996; Smith and Tolbert 2004), inducing legislators to moderate their policy preferences in response to a showing of the public will. In other words, ballot measures can serve as a public opinion thermometer, offering elected officials a gauge of constituent policy preferences. Empirically, there is mixed macro-level evidence across the states showing that in states with the initiative, legislatures are more likely to heed the median preference of the state. Some studies have shown that policy responsiveness to median voter preferences is more likely in states that have an initiative than states that do not (Gerber 1996; Matsusaka 1995; Matsusaka and McCarty 2001; but see Lascher, Hagen, and Rochlin 1996; Camobreco 1998). At the individual level of state legislators, it is similarly unclear as to whether statewide initiatives effectively pressure politicians to respond to the median voter in their constituencies (Garrett 1999). In his examination of counter-majoritarian measures passed by initiative in Colorado and Idaho, Smith (2001; 2003) finds that representatives sometimes reflect their constituent's interests and not those of a statewide election (Smith 2001, 2003), but it depends on the issue. In high initiative use states, such as California, the constituency-representative link seems to keep legislative voting more in-line with constituent preferences (Snyder 1996), but this finding has been recently challenged (Masket and Noel 2008). At the state-level, then, there is mixed evidence as to whether ballot measures lessen policy shirking. We

suspect the same may be true when examining the effects of ballot measures on the vote choices of members of Congress.

Data, Methods, and Expectations

In order to observe whether ballot measures may alter a member of Congress's voting behavior we first had to find floor votes in both chambers that both preceded and succeeded issues that were also approved by statewide initiatives or referendums. Besides finding issues that were voted upon in both the House and the Senate, the primary challenge we faced was finding issues that had comparable congressional votes at t_1 and t_2. We settled on three issues: raising the minimum wage; banning gay marriage; and campaign finance reform.

There were several difficulties in matching congressional votes across time. 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. While this was not necessary for all of the issues, when it was we employed this tactic to provide the cleanest comparison between the two congressional votes. On all three issues, for both the House and the Senate, we are interested in whether statewide ballot measure affect the subsequent voting behavior of members of Congress. While our Senate data represents a national sample, our House dataset considers only representatives from California (where district level ballot measure results are available), for statewide initiatives on the three issues.

We assembled two datasets—one for the Senate and one for the members of the California delegation—to measure the possible effect of ballot measures on congressional voting behavior. We coded both floor votes across the three issues. The first dataset aggregates two Senate votes (pre- and post- statewide ballot measures) for the three issues. Theoretically, we are interested in whether Senators are more likely to adhere to the results of successful ballot measures in subsequent votes on an issue. The

second dataset also aggregates the three sets of votes (pre-and post statewide ballot measures) in the

House, but only for members of California's delegation. The primary distinctions between these two

datasets are the control variables we collected for constituent preferences. In the House dataset,

members' districts in which the ballot measure received more than 50 percent of the vote are coded 1, and

those receiving less than 50 percent are coded zero. In the Senate, we employ a series of state ideology

measures to control for public opinion in the state. One final difference is that unlike the statewide votes

on the gay marriage ban in the Senate dataset (both initiatives and referendums), there are only initiated

measures in our House dataset.[1]

On the issue of increasing the minimum wage, between 1999 and 2007 several states passed a

minimum wage measure to preempt the lack of activity at the national level. During this time period,

eight states used the initiative and referendum to raise the minimum wage above the existing (at that time)

federal levels.[2] Each of these ballot measures reside 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 (MA) to the "Bankruptcy Reform Act of 1999."[3] The amendment would have raised

minimum wage $5.15 and hour to $6.15 an hour by January 1st, 2001. The effort to increase the

minimum wage in the Senate was repeated in 2007, but under much different circumstances. Democrats

had a majority in the Senate, but they were unable to control the legislative agenda, as Republicans were

able to stymie the majority's effort to pass the bill in its original form.[4] After an onslaught of

amendments, the final vote on the bill was 94-3. Instead of using this final vote, marred with several

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[1] Several Senators as well as California House members were dropped in the two datasets. Although the literature on Congress espouses the large and growing advantage of incumbents over the last century (Polsby 1968; Fiorina 1977; Jacobson 2004), across a period of four to ten years (depending on the issue), many members either retired or lost their seats. Since voting consistency and change in congressional behavior are our primary concern here, those members who were not present for both votes are removed from the two datasets.

[2] Arizona, Colorado, Florida, Missouri, Montana, Nevada, Ohio, and Oregon all passed direct democracy legislation to raise minimum wage. The Florida case is omitted from our dataset because it did not have a Senator that resided in both the 106th and the 110th Congresses.

[3] Senate Bill 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 9th, 1999.

[4] H.R.2 of the 110th Congress 1st Session.

amendments that changed the tenor of the bill, we decided to use the cloture vote that preceded the flurry

of amendments. Cloture was not enacted, but it considered the bill in its original format. This vote

largely was divided along party lines, with only four Republicans voting to invoke cloture.[5] These two

floor votes provide the cleanest comparison on minimum wage in the Senate. Again, we are interested in

whether the information provided by these initiatives spurred Senators to vote any differently from the

first vote (t_1) to the second (t_2).

Our House dataset is composed solely of members of the California delegation. Since California

passed a statewide initiative in 1996 raising the minimum wage, we needed to adjust our time frame to

accommodate this initiative. In making the time period narrower, we are able to minimize the effect of

time across the two congressional votes. Our initial vote 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."[6] The amendment, attached by Representative Riggs (R-CA), sought to increase the

minimum wage by ninety-cents in two years.[7] 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 $6.15 an hour by April

1st, 2002.[8] The measure passed by a vote of 282-143.[9]

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 (Smith, DeSantis, and Kassel 2006). Between 1996 and 2006, twenty-eight states

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[5] Senate cloture vote on H.R.2: Recorded Vote Number: 23. Recorded on January 24th, 2007.

[6] H.R.1227. 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."

[7] H.Amdt.1084, passed on May 23rd, 1996. Roll Call Number: 192. Amended H.R.1227.

[8] H.R.3846. Roll Call Number: 45, 106th Congress 2nd Session. There were three exemptions to the minimum wage increase: computer professionals; certain sales employees; funeral directors.

[9] Both the amendment and bill had more party defections than our Senate case; however, a bivariate correlation of .8938 between vote at t_1 and vote at t_2 suggests we are capturing a similar dynamic in these two votes.

passed statutes or constitutional amendments defining marriage through the initiative or referendum

process. Possibly spurred by the 2004 presidential election, most of these statewide votes occurred

between the 2004 and 2006 elections.[10] We found congressional floor votes that preceded and followed

the state-level impetus to redefine marriage. As it turns out, we were able to use the same votes for the

same-sex marriage case in both the House and the Senate.[11] We use the final floor votes on the 1996

Defense of Marriage Act (DOMA) as our initial vote in both chambers.[12] With no amendments added to

the bill, it provided a clean look at members' preferences. Our second vote comes from the 109th

Congress. With Republicans still in power in 2006, both the House and the Senate attempted to pass

resolutions proposing a constitutional amendment defining marriage between a man and a woman.[13] Both

the House and the Senate resolutions failed on the floor.[14] Regardless of their fate, they provide the

second congressional vote for our same-sex marriage case.[15]

Our final case is campaign finance reform. The increasing support for campaign finance reform

with the election of President Clinton brought several attempts to limit the influence soft money and

campaign spending. In 1996, the House and the Senate considered campaign finance reform bills that

would have limited the roll 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

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[10] Again, not all of these ballot measures are represented in our dataset due to legislators retiring or losing reelection. Arkansas, Colorado, Georgia, Kansas, Louisiana, and Nebraska were removed for this reason.

[11] The second congressional vote (t_2) in the House and the Senate presented their own form of the bill but with the exact same text.

[12] Passed in the House on July 12th, 1996. Roll Call Number: 316, 104th Congress 2nd Session. Passed in the Senate on September 10th, 1996. Record Vote Number: 280, 104th Congress.

[13] In the House: H.J.RES.88 of the 109th Congress 2nd Session. Roll Call Number: 378. In the Senate: S.J.RES.1, cloture vote. Record Vote Number: 163 of the 109th Congress 2nd Session.

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

[15] We admit that a statutory regulation defining marriage and a nonbinding resolution to amend the Constitution to define marriage are very different considerations. However, the fact that these resolutions needed supermajority votes to pass them helps our case. This would give legislators who were initially hesitant to vote for a constitutional amendment room to gain political capital with their constituents given the understanding that a supermajority vote was unlikely.

Bipartisan Campaign Reform Act of 2002 (BCRA).[16] While we used the 1996 bill in the Senate due to its similarity with BCRA,[17] to assess the voting behavior of the California delegation we use the 1993 House vote on the "Campaign Spending Limit and Election Reform Act"[18] because it is more similar to BCRA than the 1996 House bill. Thus, while we use different initial votes (t_1) for our House and Senate datasets, we employed the same secondary vote (t_2) on BCRA for both the House and Senate datasets.[19] Meanwhile, in the states during the intervening period, there were a handful of ballot measures that attempted to limit soft money and campaign spending. Between 1996 and 2002, voters in seven states passed increased regulations on campaign finance activities. It is possible that these efforts in the states may have encouraged some members of Congress to heed the median preference of their constituencies.

Predicting Legislative Votes and Consistency

We use a series of logistic regressions to estimate the likelihood that a member votes in a way that is consistent with the median preference of his or her constituency on a given issue. The extent that members of Congress may actually rely on information gleaned from successful ballot measures to reinforce or alter their voting decisions is the primary question of this study. Our key expectation is that the passage of initiatives or referendums on particular issues provides legislators with educative cues (Smith and Tolbert 2004) that can shape voting behavior on Capitol Hill. To observe any change in voting behavior, or the lack thereof, we code a pair of congressional votes for each legislator on three separate issues. As mentioned previously, the first congressional vote (t_1) takes place prior to a measure being placed on a statewide ballot. The subsequent congressional vote (t_2) is on the same (or very

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[16] See H.R.2356 of the 107th Congress 2nd Session. BCRA was passed in the House, 240-189. Roll Call Number: 34. It was passed in the Senate with a vote of 60-40. Record Vote Number: 54.
[17] Senate bill S.1219 of the 104th Congress 2nd Session. Cloture was not invoked by a vote of 54-46. Record Vote Number: 168.
[18] H.R.3 of the 103rd Congress 1st Session.
[19] In the House, a bivariate correlation between the 1993 vote and the 2002 vote reveals a high correlation of 0.9372. In the Senate, a bivariate correlation between the 1996 vote and the 2002 vote reveals a similarly high correlation of 0.7859. Using the 1996 vote in the House would have created a negative correlation between congressional votes at t_1 and t_2.

similar) issue that takes place after citizens in several states have voted on the issue. Thus, a pair of floor votes for each legislator straddles the successful ballot measures on the three relevant issues.

Dependent Variables

We employ three different dependent variables to measure the possible effect of statewide popular votes on congressional voting behavior. In Model 1, our dependent variable is a legislator's vote at t_2 for both the Senate and the House datasets (denoted by *Vote$_{T2}$*). Of interest is whether a member's subsequent votes (t_2) on the three bills is predicted by the result of a successful ballot measure in the member's state or district that occurred *after* the legislator's three initial votes (t_1). 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. We expect, then, that intervening ballot measures may reduce policy shirking among members of Congress who have information about the preference of the median voter in his or her state or district, compared to other members without such information

There are at least two reasons, though, why we should not expect the substantive impact of our key explanatory variable to be all that large. First, the principle control variable in the model is a member's vote on the issue at t_1. Because legislative voting on issues is relatively stable across time and is not likely to change (Poole 2007), we expect the earlier vote (t_1) to capture most of the variance of the subsequent vote (t_2). Indeed, using a member's initial vote as a control is a fairly high hurdle for the hypothezied effect of a successful ballot measure to overcome. Second, there are many more members of Congress (in both the Senate and House) who represent states or districts where ballot measures on a similar issue are not successfully voted on by citizens.

Our two other dependent variables assess the consistency of a member's vote at t_1 and t_2, given that some members receive additional information regarding what the median voter's preference is on the

issues. The dependent variable in Model 2 is coded as a one if the member voted consistently (i.e., at both t_1 and t_2) *in favor* of a piece of legislation dealing with gay marriage, minimum wage, and campaign finance, and zero otherwise (denoted by *Consistent Support*). The dependent variable in Model 3 is coded as a one if the member voted consistently (at t_1 and t_2) *against* the same three pieces of legislation, and zero otherwise (denoted by *Consistent Opposition*). While both dependent variables are coded to measure voting consistency of members on an issue (for or against), they inherently measure vote change. As such, we are interested in whether the passage of a ballot measure in a member's state or district, which theoretically should enhance that legislator's knowledge of the median preference of his or her constituency, predicts the voting behavior of members who initially supported the legislation, as well as those who initially opposed the legislation. We hypothesize that members who initially supported a piece of legislation—who then obtain additional information that their median constituent favors the same issue—will be more likely to maintain their 'yes' vote when compared to other members who lack such information. In short, we expect that a favorable vote on a ballot measure will increase the likelihood of a member consistently supporting a piece of legislation at t_1 and t_2.

In contrast, we hypothesize that members who oppose a piece of legislation at t_1 and t_2 will be marginally less likely to do so if affirmative votes are contracted at the polls by his or her median constituent's preference.   In short, we expect that a favorable vote on a ballot measure will decrease the likelihood of a member who (knowingly or unknowingly) cast 'no' votes on a bill at t_1 and t_2. In the first model, a successful ballot measure should *increase* the likelihood that a member consistently votes in alignment with the median vote of his or her constituency, relative to other members lacking such knowledge. In the second model, a successful ballot measure should *decrease* the likelihood that a member consistently votes against the median vote of his or her constituency, relative to other members without such knowledge. In both models, we expect the voting behavior of members to minimize policy shirking.

Independent Variables

As mentioned above, since we are interested in the possible effects a statewide ballot measure may have on congressional voting behavior, our key causal variable in all three models is a dummy variable for whether or not there was an intermediary ballot measure on an issue that passed in a legislator's state (Senate) or district (House). Across the three cases, members representing constituencies that approved a ban on gay marriage, an increase in the minimum wage, or regulations on campaign finance are coded one if the popular vote occurred between the pair of related congressional votes. If no initiative or referendum on an issue was approved in the member's state, the dummy variable for a successful ballot measure is coded as zero. Chart 1 displays the causal flow schematically.

[Chart 1 about here]

All three models of legislative voting behavior control for several other factors. First, we control for a member's party (*party*), as the floor votes on all of these highly salient issues fell largely along party lines. Because of the recent a resurgence of party loyalty within Congress, 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.[20] Second, we control for any change in a member's ideology from t_1 to t_2 (*DW-Nominate Change*) using aggregated roll call votes (Poole and Rosenthal 1997).[21] We coded each member's DW-Nominate score at

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[20] After running a series of bivariate correlations we found 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 datasets. For the same-sex marriage case only, Republicans are coded 100 and Democrats are coded 200. Recoding this variable ensures that a one at vote t_2 indicates that the lawmaker's party would support the issue.
[21] 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 in the time span we have selected (Poole and 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. Specifically, we think a member's initial vote (to predict his or her subsequent vote) sufficiently taps ideology. In many ways a lawmaker's initial vote may be a more accurate proxy for ideology, as it is case-specific, whereas the Poole and Rosenthal measure of ideology is aggregated from a wide range of issues. Similarly, there is unquestionably some overlap

t_2 as well as the difference of a member's DW-Nominate score from the t_1 Congress to the t_2 Congress.[22]

Because changes in a legislator's ideological preferences may account for differences in consistent voting

patterns, the change in a member's DW-Nominate score should capture this possibility.

For our Senate models, we control for state ideology at t_2 (*State Liberal)* as well as change in state

ideology from t_1 to t_2 (*State Liberal Change*). These measures were derived from a series of *CBS/New

York Times* national public opinion polls (Erickson, Wright, and McIver 1993).[23] For our t_2 measure, we

use the percent of liberal ideology in a state.[24] We expect the higher the percent of liberal ideology in a

state, the more likely a member will vote for the liberal position of the legislation. The second measure of

state ideology is the change from t_1 to t_2 in the percent liberal ideology in a member's state. We expect as

a state becomes more liberal, the Senator will be more likely to cast a liberal vote and vice-versa if the

state becomes less liberal.[25]

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between party identification and ideology. Thus, we think excluding ideology from the model is not overly problematic.

[22] Again, this data is found in Poole and Rosenthal's (1997) roll call voting dataset found on Keith Poole's website: voteview.com. An example of this measure would be the difference of a legislator's DW-Nominate score in the 104th Congress from their DW-Nominate score in the 109th Congress. Like the partyid variable, the change in a member's DW-Nominate score from t_1 to t_2 had a negative correlation for our same-sex marriage case and positive correlation with our minimum wage and campaign finance cases. Again, to make sure these variables were coded in the same direction, we multiplied each member's DW-Nominate scores for the same-sex marriage case by negative-one (-1). This recoded variable indicates that we would expect a member's ideological change would be in favor of a one vote (yea) at t_2.

[23] This data was updated to include party and ideology measures as late as 2003. This data was found compliments of Gerald Wright's webpage: http://php.indiana.edu/~wright1/

[24] In two of our 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 Erickson, Wright, and McIver dataset 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 Erickson, Wright, and McIver 2001; Brace, Arceneaux, Johnson, and Ulbig 2004).

[25] We decided to use the percent-liberal ideology measure because it provided more stability in our state ideology change variable. Subtracting the conservative ideology from the liberal ideology at t_1 and t_2 led to large shifts in ideological change that was not consistent with the authors' conclusions and did not make theoretical sense in our dataset. For example, in the original computation Delaware had ideological changes of 38 percentage points from 1996 to 2002. While we do not want to underestimate the political dynamics of the state of Delaware, we believe that measuring one ideology across time provided more accurate and stable measures of state ideological change across time. Like our other ideological measures, this variable had a positive relationship with two of our cases and a negative correlation with the third. Again, these variables were recoded for the same-sex marriage case to ensure that the variables were coded in the same direction. Instead of multiplying this variable by negative one, we instead substituted the percentage of conservative political ideology in the state at t_1 and t_2 for the same-sex marriage case.

We expect legislator responsiveness also to be contingent on whether the member seeks reelection. 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 leniency with their votes. We use a dummy variable, coded one for members who are retiring, and zero otherwise (*Retiring*). We expect retiring members to be more likely to shirk the preference of the median voter in their state or district since they are not vying for reelection.

Finally, we control for whether 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. If a member had a close race, we assume he or she is less likely to shirk. Members in "likely" and "safe" seats are coded with a one to signify their relatively secure reelection chances, while members in races that "leaned" either Democrat or Republican or were in a "Toss-Up" seat are coded with a zero.[26]

We add additional controls for cross-sectional heterogeneity by case using a series of dummy variables. This is meant to control for the possibility that observed voting patterns are a function of case selection which might prevent the prospect that observed relationships between the primary independent variables and outcomes are spuriously related. These controls are omitted from our tables for simplicity.

Thus, we expect that a higher ideological scores for our state ideology variables to be more likely to support a one vote (yea) at t_2.

[26] For our campaign finance case, this information was not available from the Cook Political Report. To find the missing data we used the percentage of victory from the elections returns of 2002 for both the House and Senate. In line with Mayhew's (1974) assessment of marginal seats in Congress, we coded members who won with less that fifty-five percent of the vote with a zero and members winning with more than fifty-five percent 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 two or four years down the road, these Senators are not under any immediate electoral pressure at the time of the second vote.

Findings

To examine the effect of ballot measures on lawmakers vote choices we estimate three models for both the House and Senate. The first of these models (*Vote$_{T2}$*) predicts a members vote directly while two subsequent models (*Consistent Support* and *Consistent Opposition*) predict the lawmakers voting patterns overtime. In each case the dependent variable is dichotomous so the models were estimated via maximum likelihood (logit). The three House models are given by:

Logit(Vote$_{T2}$)= alpha+Vote$_{T1}$+BallotMeasure+ΔDW-Nominate+SafeSeat+Retiring+GayMarriage +CampaignFinance

Logit(Consistent Support)= alpha+BallotMeasure+ΔDW-Nominate+SafeSeat+Retiring+GayMarriage +CampaignFinance

Logit(Consistent Opposition)= alpha+BallotMeasure+ΔDW-Nominate+SafeSeat+Retiring+GayMarriage +CampaignFinance

and the three Senate models are:

Logit(Vote$_{T2}$)= alpha+Vote$_{T1}$+BallotMeasure+Party+ΔDW-Nominate+SafeSeat+Retiring +StateLiberal+ΔStateLiberal+GayMarriage+CampaignFinance

Logit(Consistent Support)= alpha+BallotMeasure+Party+ΔDW-Nominate+SafeSeat+Retiring +StateLiberal+ΔStateLiberal+GayMarriage+CampaignFinance

Logit(Consistent Opposition)= alpha+BallotMeasure+Party+ΔDW-Nominate+SafeSeat+Retiring +StateLiberal+ΔStateLiberal+GayMarriage+CampaignFinance

Although there are some limitations to our data,[27] overall our modeling technique fits the data fairly well.[28] As Table 1 reveals, in each of the three House models there is some evidence confirming

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[27] The most significant problem inherent in our data and estimation technique is known in the literature as "separation." Separation occurs when covariate patterns lack variation in the response (for a review of the problem and its appropriate solution see Zorn, 2005). Specifically, when we model our data we encounter what is known as "quasicomplete separation" (Albert and Anderson, 1984); where a number of covariates perfectly predict some, but not all, of the observations. As Zorn (2005: 157) explains "separation is a particularly thorny problem, for a host of reasons. From an estimation perspective, separation leads to infinite coefficients and standard errors." Zorn (2005: 161) writes, "the most widely–used 'solution' is simply to omit the offending variable or variables from the analysis." Still another solution is to simply omit the observations that are perfectly predicted. Indeed, this is the technique automatically implemented in STATA. We are aware of the problem and subsequent work will attempt to implement an alternative methodology known as the "exact logistic regression" (Hirji 2006).

our overarching hypothesis that the passage of state ballot measures has a statistically significant effect on lawmakers' vote choices. In the model predicting a members vote in the subsequent time period ($Vote_{T2}$), the positive coefficient on the variable ballot measures indicates that lawmakers from districts where a majority of voters cast their ballot in favor of the ballot measure were more likely in subsequent roll-calls to vote accordingly. In other words not only does the passage of a ballot initiative provide lawmakers a very precise measure of their constituents' policy views but, most importantly for proponents of the "delegate model" of representation, they respond by adopting their voters' position. Likewise, members who initially supported legislation similar to the successful ballot measure (*Consistent Support*) were more likely in future votes to remain consistent in their support of the policy and (conversely) less consistent in their opposition (*Consistent Opposition*).[29] For example, a discrete change[30] in the passage of an initiative *increases* the probability that a member will remain a consistent supporter of that policy by about 39 percent (holding all else at their mean values). Conversely, a discrete change in the passage of an initiative d*ecreases* the probability that a member will remain a consistent opponent of the policy by about 52 percent (holding all else at their means). Predicted probabilities for each of the covariates are included in Table 2. Perhaps most importantly, these effects obtain statistical significance even when controlling for a number of relevant factors. Most noteworthy in this respect is that in the first model predicting vote choice we use a member's initial vote as a control for their subsequent vote. Certainly, this is a very high hurdle for the hypothesized effect to overcome.[31]

[Table 1 about here]

[Table 2 about here]

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[28] In both the House and Senate models we estimated the Hosmer-Lemeshow (HL) goodness of fit statistic in STATA. (Lemeshow and Hosmer 1982; Long and Freese 2006). The test statistic is insignificant in all three models for both the House and Senate datasets suggesting that the models fit well.

[29] The coefficient just misses conventional levels of statistical significance (p=.07).

[30] That is the difference between a district where a majority of voters cast their ballot in favor of the measure versus a district where a majority of voters cast their ballot against the measure

[31] The correlation between the initial vote and subsequent vote is .86 while the subsequent vote variable (alone) explains 63 percent of the variance in the dependent variable.

Interestingly, in two of the models the coefficient on safe seat is negative and approaching statistical significance (p=.067 in the first model and p=.084 in the second model) whereas the coefficient on retiring is negative and approaching statistical significance (p=.064) in the consistent support model. For example, a member from a safe seat is about 42 percent less likely to consistently support the three policies under investigation while lawmakers who are retiring are 39 percent less likely to be consistent in their support. This provides some (albeit only moderate) evidence that lawmakers from safe seats and those who are retiring are more likely to shirk (Kalt and Zupan 1990; Rothenberg and Sanders 2000; 2007). That is, these members are less likely to vote for policies which their median constituent who voted clearly supported.[32]

Perhaps the case of Representative Elton Gallegly (R, CA 24) will make our findings a bit less abstract. Gallegly, from a moderately conservative district, is himself a fairly moderate conservative lawmaker (ranking as the 109th most Conservative member of the 228 member 106th House).[33] Given this, the fact that a majority of Rep. Gallegly's constituents (59 percent) voted in favor of the 1996 ballot measure raising the minimum wage likely put the lawmaker in a difficult position. He could either remain true to his conservative preferences or attenuate his preferred position in order to match his median constituent's preference. In other words, Gallegly was put in the position of having to "shirk"—vote against his median constituent's preference—or behave as a true delegate carrying out his constituents' wishes. Indeed, only months prior to the 1996 California ballot measure, Rep. Gallegly voted *against* raising the minimum wage.

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[32] This conclusion is a bit shaky as our modeling technique was not suited to directly asses this question (rather the primary function of the retiring and safe seat variables was as a control). Before making this conclusion we reran our three models were every instance when a member's constituents did not vote in favor of the ballot measure were deleted. Therefore our remaining sample included only lawmakers whose constituents voted in favor of the ballot measure. Any lawmaker who voted "no" at the subsequent time period or who consistently opposed these policies are shirkers. In rerunning each of the three models we find the same effects. While (again) none are significant are the conventional .05 level, each is approaching statistical significance (p<.10). Overall we feel confident that there is a *moderate* linkage being retiring and safe seat members and legislative shirking.

[33] Based on the legislator's DW-NOMINATE first dimension coordinate (Poole and Rosenthal, 2007). Poole and Rosenthal's dataset covers all unique members who served. Thus, while there are 228 unique member s the Republican majority numbered only 223.

We examine what Rep. Gallegly would be "expected" to do according to the predicted probabilities for two of the three models. The lawmaker's profile is outlined in Table 3. We can see that, according to our data and modeling strategy, we predict Rep. Gallegly had approximately an 81 percent chance of voting in favor of raising the minimum wage in the subsequent vote and only a 41 percent chance of remaining consistent in his opposition. In the end, Rep. Gallegly sided with his constituents, voting in favor of the 2000 minimum wage increase, and thereby "flip-flopping" on his previous floor vote. Rep. Gallegly decided not to shirk. In other words our predicted voting patterns for Rep. Gallegly, based on the theory of legislative shirking and the educative effects of state ballot measures, matches the observation.

[Table 3 about here]

Unlike the House models, we find no evidence that state ballot measures influence Senators' vote choices (Table 4). There is reason to believe that the models are correctly specified, as most of the control variables are statistically significant and in the expected directions. For example, the strongest predictor of a member's subsequent vote on the issue is his or her initial Senate vote. As Table 5 displays, a lawmaker who voted in favor of the issue in the initial time period was 75 percent more likely to vote in the same direction at the subsequent time period (holding all else at their mean values). Similarly, members of the political party we identify as the strongest supporter of the measure were more likely (by about 47 percent) to subsequently vote in favor of the policy and were more consistent (by about 86 percent) in their support.[34] Also, Senators from states whose constituents tended to be more ideologically aligned with the particular issue were more likely to vote accordingly.

While we expected to find 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. Simply put, we

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[34] For reasons outlined in the "notes" section of table 4 we were unable to estimate the standard errors for the coefficient ton party in the consistent opposition model. Thus, while there is strong theoretical reason to expect that the effect is statistically significant (indeed the coefficient is quite large) we are unable to make that claim at this time.

think House members are more reactive to public opinion and the expressed preferences of their constituents than Senators (Fenno 1978; Baker 2001; Canes-Wrone, Brady, Cogen 2002; Jacobson 2004). The short reelection timeframe for representatives in the House places members in a position far more responsive than their Senate counterparts. Legislators seeking reelection every two years are, as Madison hoped in *Federalist #52* (2004 [1788]), more responsive to their constituents' wishes. We find that the effects of direct democracy on members of Congress appear to fall in line with Madison's original bicameral expectation regarding representation.

[Table 4 about here]

[Table 5 about here]

Conclusion

Recent work has begun to untangle the effects of direct democracy on state and federal politics. Research has 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 (Smith and Tolbert 2004; Nicholson 2005; Smith, DeSantis, and Kassel 2006; Smith 2006; Smith and Tolbert 2007; Donovan, Tolbert, and Smith 2008). 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 for the first step in this direction). This paper, we hope, is a constructive step in filling the gap in the literature.

Our overarching conclusion is that the passage of some statewide ballot measures can to have an indirect, educative effect on the voting patterns of members of Congress. We claim that the causal mechanism is informational; ballot measures provide precise, policy specific information about the preferences of a member's median constituent. Indeed, there is good theoretical reason to believe that ballot measures provide more precise information about a constituency's policy views than polling, prior

election results, or a lawmakers' "general intuition." Thus, statewide ballot measures may act as a plebiscitary mechanism (Goebel 2002; Piott 2003; Smith and Tolbert 2004), whereby congressional lawmakers are less likely to shirk and vote against their constituency (Kalt and Zupan 1984; Lott 1987; Bender and Lott 1996). 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. While 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 and Zupan 1990; Rothenberg and Sanders 2000; 2007; Lawrence 2007; Lott 1987; Poole and Romer 1993; but see Garrett 1999) we believe that statewide ballot measures provide a more direct metric and afford us greater leverage on the question of shirking.

Some scholars have suggested recently that the congruence between members of Congress and their constituencies has declined with increased levels of polarization and national party influence on Capitol Hill (Ansolabehere, Snyder, and Stewart 2001; Cox and McCubbins 2007). We suggest that statewide ballot measures may provide specific information to some members, and unlike public opinion polls, they may give members precise insight into the median preference of their constituencies. 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, while our findings certainly may be welcome news for proponents of a "delegate" model of representation, as well as for scholars who emphasize the saliency of ballot measures and state politics, one main caveat from our findings is that this linkage may not reach the upper chamber. Indeed, we found scant evidence that Senators' roll call voting patterns are influenced by statewide ballot measures. One possibility is that our sample size is simply too small to uncover such an effect. Certainly, House members are likely to be more reactive to public opinion (Fenno 1978; Baker 2001; Canes-Wrone, Brady,

Cogen 2002; Jacobson 2004) than members of the Senate due to their narrow time frame for reelection (Mayhew 1974). Thus, as is often the case in novel studies, our research has perhaps raised more questions as it has resolved. Most notably, our research has not answered the question of whether the effect of ballot measures that we have identified for members of the House is simply non-existent in the Senate, or just too weak to identify with our limited dataset. A more thorough theoretical and empirical study exploring bicameral differences in the effects of direct democracy on voting behavior seems to be a fruitful endeavor for future research.
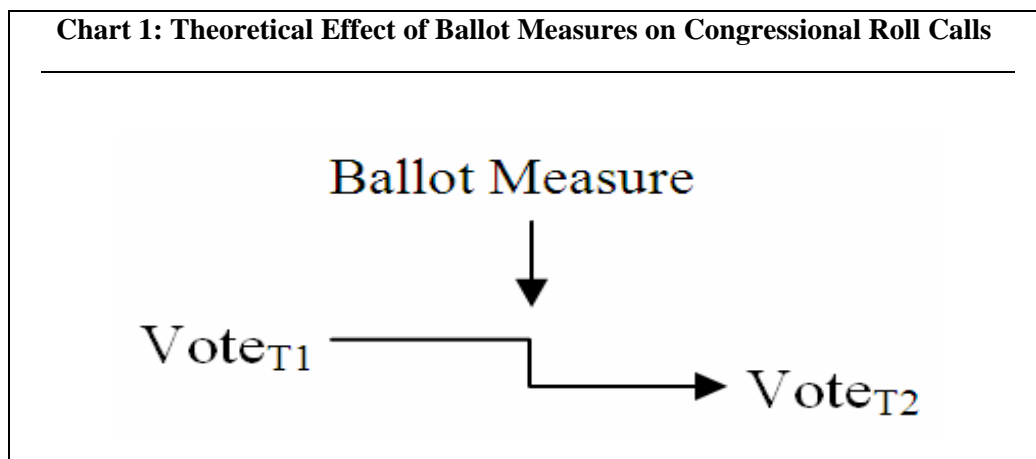
Chart 1: Theoretical Effect of Ballot Measures on Congressional Roll Calls

Table 1. The Effect of Ballot Measures on Lawmakers Vote Choices (House)

	Vote$_{T2}$	*Consistent Support*	*Consistenet Opposition*
Vote$_{T1}$	36.8	-	-
	(1.2)	-	-
Ballot Measures	16.0	1.9	-2.6
	(2.1)	(1.1)	(1.2)
DW-Nominate Change	27.3	12.3	-8.1
	(12.7)	(4.5)	(4.0)
Safe Seat	-3.9	-2.2	1.9
	(2.1)	(1.3)	(1.2)
Retiring	-0.3	-2.1	-0.2
	(3.4)	(1.1)	(1.3)
N	100	101	101
Pseudo-R^2	.80	.17	.14

Notes: Cells highlighted in dark are significant at the .05 level whereas cells with light shading are significant at the .10 level. The dummy variables for each case were included in the models as controls but are excluded from the table. We were forced to exclude party-id due to separation (STATA was unable to calculate standard errors for the coefficient). It is important to point out that even when party-id was included in the model as a control the substantive interpretation regarding ballot initiatives (the primary hypothesis being tested) remains the same.

Table 2. Covariate Predicted Probabilities (House)

	Vote$_{T2}$	*Consistent Support*	*Consistenet Opposition*
Vote$_{T1}$*	1.0	-	-
Ballot Measures*	.99	.39	-.52
DW-Nominate Change	.99	.22	-.14
Safe Seat*	-.01	-.42	.34
Retiring*	-.001	-.39	-.04

Notes: For dichotomous covariates (indicated by an *) predicted probabilities are calculated for a discrete change. For DW-Nominate Change values are calculated for a standard deviation change (from ½ standard deviation below the mean to a ½ standard deviation above the mean). Due to the inherent problems in our dataset and modeling technique (separation) detailed above, the primary independent variables are perfect predictors of the vote at the subsequent time period.

Table 3. Vote Choice Predicted Probabilities for Representative Elton Gallegly

Vote$_{T1}$	Ballot Measures	DW-Nom. Change	Safe Seat	Retiring
Against (coded 0)	Passed (59%)	-.018	No	No

Predicted Probabilities: Pr(Vote$_{T2}$|X)=.81   Pr(Consistent Opposition)=.41
Observed Vote: Vote$_{T2}$=1   Consistent Opposition=0

Table 4. The Effect of Ballot Measures on Lawmakers Vote Choices (Senate)

	$Vote_{T2}$	Consistent Support	Consistenet Opposition
$Vote_{T2}$	3.9	-	-
	(0.9)	-	-
Ballot Measures	-0.02	1.1	0.6
	(0.7)	(0.8)	(0.5)
Party	2.2	5.3	-
	(0.8)	(0.8)	-
DW-Nominate Change	12.1	1.5	-12.4
	(3.8)	(1.7)	(5.1)
Safe Seat	-0.5	0.6	0.8
	(1.5)	(1.0)	(0.9)
Retiring	-0.2	-0.02	0.7
	(1.0)	(1.3)	(0.8)
State Liberal	0.2	0.1	-0.1
	(0.05)	(0.05)	(0.05)
State Liberal Change	0.001	-0.02	-.001
	(0.04)	(0.04)	(0.05)
N	166	166	166
Pseudo-R^2	.65	.62	.23

Notes: Cells highlighted in dark are significant at the .05 level. The dummy variables for each case are included in the models as controls but are excluded from the table. Party-Id was excluded in the model testing consistent opposition due to separation (STATA was unable to calculate the standard error for the coefficient).

Table 5. Covariate Predicted Probabilities (Senate)

	$Vote_{T2}$	Consistent Support	Consistenet Opposition
$Vote_{T1}$*	.75	-	-
Ballot Measures*	-.003	.26	.13
Party*	.47	.86	-
DW-Nominate Change	.25	.03	-.20
Safe Seat*	-.10	.15	.12
Retiring*	-.04	-.005	.14
State Liberal	.30	.24	-.20
State Liberal Change	.002	-.03	.001

Notes: For dichotomous covariates (indicated by an *) predicted probabilities are calculated for a discrete change. For DW-Nominate Change, State Liberal, and State Liberal Change values are calculated for a standard deviation change (from ½ standard deviation below the mean to a ½ standard deviation above the mean).

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