Evaluating Policy Representation with Dynamic MRP Estimates: Direct Democracy and Same-Sex Relationship Policies in the U.S

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Evaluating Policy Representation with Dynamic MRP Estimates: Direct Democracy and Same-Sex Relationship Policies in the United States

Daniel C. Lewis¹ and Matthew L. Jacobsmeier²

Abstract

Does direct democracy strengthen popular control of public policy in the United States? A major challenge in evaluating policy representation is the measurement of state-level public opinion and public policy. Although recent studies of policy responsiveness and congruence have provided improved measures of public opinion using multilevel regression and poststratification (MRP) techniques, these analyses are limited by their static nature and cross-sectional design. Issue attitudes, unlike more general political orientations, often vary considerably over time. Unless the dynamics of issue-specific public opinion are appropriately incorporated into the analyses, tests of policy responsiveness and congruence may be misleading. Thus, we assess the degree of policy representation in direct democracy states regarding same-sex relationship recognition policies using dynamic models of policy adoption and congruence that employ dynamic MRP estimates of attitudes toward same-sex marriage. We find that direct democracy institutions increase both policy responsiveness and congruence with issue-specific public opinion.

Keywords

direct democracy, political behavior, issue preferences, public opinion, politics of sexuality, identity/group politics, representation, same-sex marriage, public policy, multilevel models, quantitative methods, methodology

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An important indicator of the quality of democratic government is the extent to which public policy reflects the preferences of citizens (e.g., Erikson, Wright, and McIver 1993; Key 1961). Most democratic systems rely on elections of government officials to exert popular control over public policy, but some supplement the representative process with direct democracy institutions, such as ballot initiatives and referendums. Indeed, advocates for direct democracy reforms at the turn of the twentieth century sought to increase popular control of U.S. state governments by allowing citizens to circumvent elected officials and create policy themselves. Yet despite reformers’ assertions, the extant literature finds little consensus regarding the connection between direct democracy and policy representation. Studies of salient policies show that states with direct democracy are more responsive to or congruent with public preferences than other states (e.g., Arceneaux 2002; Gerber 1996; Matsusaka 2010). However, other studies—usually examining general policy measures—find no evidence of increased responsiveness or congruence (e.g., Burden 2005; Lascher, Hagen, and Rochlin 1996; Lax and Phillips 2012).

A major challenge in testing whether direct democracy institutions improve policy representation is measuring state-level public opinion in a way that accurately corresponds with public policy. Recent work using multilevel regression and poststratification (MRP) techniques to estimate state-level public opinion has allowed for more direct tests of policy congruence (Lax and Phillips 2009a). Yet studies employing MRP measures of state public opinion find no evidence that direct democracy increases policy congruence with public preferences, even on salient issues such as gay rights (Lax and Phillips 2009a; 2012).

Although MRP techniques provide methodological solutions to address measurement challenges, existing research remains limited due to the use of cross-sectional research designs. The concept of policy responsiveness suggests a dynamic relationship between public opinion and policy, but most studies that employ MRP are static. This is problematic because it treats a state such as Utah, the first state to pass an explicit same-sex marriage ban, as equally responsive as Wisconsin, which passed a comparable law more than 10 years later, even though citizens in both states preferred this policy at least as far back as 1994. Despite the decade lag between Utah and Wisconsin in changing their policies to be congruent with public opinion, a static, cross-sectional analysis would mistakenly assess both states as equally responsive to public opinion. Static analyses are also limited in assessing policy responsiveness because they cannot account for temporal variation in public opinion. Although citizens largely supported same-sex marriage bans from 1995 to 2008, majorities in favor of same-sex marriage have recently emerged in some states (Brewer and Wilcox 2005; Lax and Phillips 2009a; 2012).

In this study, we address these challenges by developing dynamic MRP estimates of attitudes toward homosexuality and same-sex marriage. We then test whether direct democracy institutions increase policy responsiveness to these attitudes using event history analysis of the adoption of policies regarding the recognition of same-sex relationships. The analyses focus on these specific gay rights policies because this allows us to simultaneously examine the negative side of increased policy
responsiveness—“tyranny of the majority.” Indeed, several recent studies find that the rights of minority groups are at higher risk in direct democracy states compared with nondirect democracy states (Lewis 2011a; 2011b; 2013; Haider-Markel, Querze, and Lindaman 2007). Same-sex relationship recognition policies, in particular, provide a good test of dynamic responsiveness because of the significant shifts in public attitudes toward same-sex marriage over the past 30 years and the relative newness of state policies explicitly addressing this issue. In addition to testing policy responsiveness, the study also assesses the extent to which increased policy responsiveness associated with direct democracy translates into policy congruence. The results support the argument that direct democracy improves policy representation through increases in policy responsiveness and congruence.

Policy Representation and Direct Democracy

Since the publication of Statehouse Democracy (Erikson, Wright, and McIver 1993), most scholars agree that American state public policy is, at minimum, somewhat representative of public preferences (e.g., Gray et al. 2004; Hill and Hinton-Anderson 1995; Pacheco 2013). Policy representation is typically understood using two related concepts: responsiveness and congruence. Responsiveness is the degree to which policy reacts to public opinion, often measured by the correlation between public opinion and public policy. Congruence is the degree to which policy matches majority public preferences. Importantly, responsiveness to public opinion does not necessarily ensure that policy is congruent with majority preferences. For example, in studying policies related to gay rights, Lax and Phillips (2009a) find that state policy responsiveness to public opinion is greater with respect to support for housing nondiscrimination law than it is with respect to same-sex sodomy prohibitions. However, housing policy is congruent with majority preferences in 12 fewer states than laws banning same-sex sodomy laws. In other words, while the relationship between the percentage of a state’s citizens who support housing nondiscrimination laws and the presence of nondiscrimination laws in states is stronger than the relationship between percentage support for bans on same-sex sodomy and the presence of sodomy bans in states, majority preferences match state policy in significantly more cases when it comes to same-sex sodomy bans.

Both of these concepts were relevant to the argument of Populist and Progressive reformers that advocated for direct democracy institutions at the turn of the twentieth century. They contended that direct democracy would enhance policy representation by circumventing corrupt and unresponsive state legislators (Sullivan 1893). By allowing citizens to directly make policy, direct democracy institutions, such as ballot initiatives and popular referendums, should not only enhance responsiveness but also create more congruent policy that reflects the “will of the people,” or at least the will of the majority of the electorate (Haskell 2001). In other words, direct democracy would enhance the effect of public opinion on public policy.

Contemporary theoretical analyses extend this argument by recognizing the power of giving citizens agenda-setting power (Gerber 1996; Matsusaka and McCarty 2001). Gerber (1996) demonstrates how the threat of citizen legislation can influence the
behavior of policymakers. Citizen initiatives can serve as a credible threat to pass policy that is relatively distant from the status quo or as a clear signal of public preferences. Either way, this may cause policy makers to shift policies closer to the preferences of the median voter than they otherwise would have done on their own, often resulting in moderate policies in line with public opinion. Thus, there is a strong theoretical basis to expect states with direct democracy institutions to be more responsive to public preferences than nondirect democracy states.

Empirical analyses of direct democracy and policy representation, however, have not consistently found support for these arguments. Several studies, particularly those that examine salient social issues such as gay rights and abortion, have shown significant differences in the responsiveness or congruence of direct democracy and nondirect democracy states (e.g., Arceneaux 2002; Gerber 1996; 1999; Matsusaka 1995; 2010). However, it is far from clear that this responsiveness extends to all policies and all public attitudes. Indeed, Burden (2005) finds that policy in direct democracy states is more responsive to specific issue attitudes but not to general ideological orientations (but see Lewis, Schneider, and Jacoby 2015).

Other research finds no evidence of increased responsiveness or congruence (Camobreco 1998; Lascher, Hagen, and Rochlin 1996; Lax and Phillips 2009a; 2012; Monogan, Gray, and Lowery 2009). Some contend that referendums are imperfect reflections of the public’s ideal policies and provide limited choices that can do little to enhance representation (Camobreco 1998; Lascher, Hagen, and Rochlin 1996). Another potential mitigating factor is the ability of interest groups to use direct democracy institutions to further their own interests rather than the public interest (Gerber 1999; Lascher, Hagen, and Rochlin 1996; Monogan, Gray, and Lowery 2009). Moreover, an underlying assumption of arguments for increased policy responsiveness in direct democracy states is that state legislators and policymakers are inherently unresponsive, but analyses have shown public opinion to influence policy outcomes in all states (Erikson, Wright, and McIver 1993; Gray et al. 2004; Schneider and Jacoby 2006). However, a recent study of Swiss institutions finds that direct democracy use will only enhance policy congruence under the context of “bad” representation, when elite preference diverge from mass preferences (Leemann and Wasserfallen 2016). This suggests that direct democracy states may not always display higher levels of policy congruence but, depending on the representational context, can induce policy congruence.

One of the major obstacles to studying policy responsiveness has been developing measures of state-level public preferences that match with public policies. Many studies rely on broad measures of ideology due to the difficulty of measuring more specific state-level issue attitudes. Until recently, these measures required either pooling several surveys across multiple years and then disaggregating the data by state (Erikson, Wright, and McIver 1993) or an indirect method based on election results and elite behavior (W. D. Berry et al. 1998). Both approaches make linking public opinion to policy difficult (Jacoby and Schneider 2009). In addition, disaggregation methods require many years’ worth of surveys to build appropriate state sample sizes, and can only measure changes in opinion over very coarse units of time. While ideological
orientation may be relatively static over time (Erikson, Wright, and McIver 2006), issue attitudes can be much more dynamic (e.g., Pacheco 2014), and disaggregation methods risk underestimating changes in mass attitudes.

Recent work addresses this obstacle by using MRP (Lax and Phillips 2009a; 2009b; 2012; Park, Gelman, and Bafumi 2004). This approach estimates state-level support for specific policies using multilevel models with individual-level demographic factors and state-level covariates as predictors. Multilevel modeling requires relatively small sample sizes in each state, so multiple surveys may not be required. The multilevel model results are then poststratified using census data to generate state-level estimates of public preferences. Producing state-level estimates of specific issue attitudes with MRP allows researchers to more closely match public opinion with public policies instead of relying on broad ideological orientations. For example, Lax and Phillips (2012) conduct extensive tests of policy congruence and find that states respond to public opinion, but policy congruence is still quite limited. More pertinent to our study, they find no significant effect of direct democracy institutions (Lax and Phillips 2009a; 2012).

Although MRP provides good estimates of state-level issue attitudes that can be matched with specific policies, it has mostly been used in cross-sectional analyses. This presents a limited view of representation. Lax and Phillips, focusing on the lack of policy congruence in the states, note that policymaking occurs slowly over time, and static analyses may underestimate policy congruence, especially in the short term (Lax and Phillips 2012, 157–58). Static approaches may also underestimate differences in responsiveness between the states (Lupia et al. 2010). Again, static analyses ignore the differences between slow adopting states and those that quickly respond to public preferences.

Thus, in addition to assessing responsiveness, incorporation of over-time changes in opinion and policy into analyses of state policy representation has the potential to improve our understanding of how institutions affect policy congruence. Indeed, both policy responsiveness and congruence are critical to understanding the relationship between policy representation and direct democracy, but the effects of direct democracy institutions on policy representation may depend on whether responsiveness or congruency is being analyzed (Achen 1978; Golder and Stramski 2010; Matsusaka 2001). Ideally, states would be both responsive to public opinion and enact policies that reflect majority preferences. But, since responsiveness does not necessarily create policy congruence, a dynamic analysis of policy representation should also address congruence.

By the same token, studies of congruence can also benefit from dynamic analyses. This is particularly true if issues tend to first appear on the legislative agenda when government policy is incongruent with opinion. If state governments are slow to enact policies in line with citizens’ preferences, measures of congruence will count them as incongruent even if they are moving toward congruence. Since the policy process, itself, is dynamic and can take years to produce clear output, a static analysis may not provide an accurate assessment of the propensity of a state’s public policies to be congruent with public preferences. A dynamic analysis, meanwhile, is better equipped to assess the degree of congruence because it allows policies to change over time.
Furthermore, policy congruence can also be driven by changing public preferences that come in line with the status quo policy. As such, it is important for an analysis of congruence to allow for both dynamic shifts in public opinion and public policy. Thus, static analyses, with just snapshots of public opinion and public policy, risks underestimating the degree of policy congruence.

Consequently, the temporal aspect of policy responsiveness is essential for evaluating the quality of democratic institutions, especially when considering an issue’s short life span on the public and/or governmental agenda (e.g., Downs 1972). If states are slow to respond to public preferences, some issues may fall off the agenda before any action can be taken. Direct democracy should give citizens the tools to translate their preferences into policy more quickly than representational democracy. This responsiveness should enable state policies to match majority public opinion more frequently. Thus, accounting for dynamics in public opinion and policymaking is crucial to evaluating policy responsiveness, congruence, and representation.

Measuring Dynamic Issue Attitudes

As noted earlier, using MRP to measure state-level public opinion only requires relatively small sample sizes to produce good estimates (Lax and Phillips 2009b). Lax and Phillips show that MRP typically outperforms disaggregation methods at sample sizes of 1,400, and evidence suggests that MRP performs well in producing estimates of opinion even for states in which as few as 17 respondents complete surveys in a given year. (Pacheco 2011). As Lax and Phillips (2009b) note, this opens up the possibility of generating dynamic estimates of state-level public opinion on specific issues from national surveys. Here, we use MRP to estimate dynamic issue attitudes from national surveys that have asked about gay or same-sex marriage.

Three types of data are necessary for MRP: census data, survey data containing a measure of the attitude one is interested in, and data on state-level variables that may have an impact on attitudes within each state. The survey data must include indicators of the state in which a respondent resides and a sufficient number of individual-level variables thought to affect the attitude of interest that are also available in the census data. We draw on surveys housed by the Roper Center for Public Opinion Research, including surveys by Pew Research Center, the Gallup Organization, and ICR Survey Research Group from 1996 to 2013. We used every available survey available through the Roper Center that asked respondents whether they favored allowing gays and lesbians to marry legally and contained the other variables necessary for our analyses. Respondents indicating they favor allowing same-sex marriage are coded as one, and zero otherwise. For individual-level predictors, we include age, level of education, gender, and race. Age and education are recoded as ordinal variables, so that respondents could be categorized into “types” for poststratification: a respondent might be identified, for example, as a white female, under 30, with a moderate level of education.

We then estimate multilevel models of opinions on same-sex marriage for each survey year. These models include the individual-level variables identified above and state and region random effects, along with state-level variables capturing the
percentage of a state’s population that is Mormon or evangelical Christian (e.g., Fejes 2008; Haider-Markel 2001) and the Democratic share of a state’s two-party vote in the most recent presidential election. Next, using the estimates from the multilevel regression, a predicted probability of favoring same-sex marriage is generated for each type of respondent in each state. In our case, there are 4,896 potential types of respondents. Last, census data is used to determine the actual number of each type of respondent residing in each state, and a weighted average is used to generate the final state-level estimates of support for gay rights.

The resultant measure of annual state-level support for same-sex marriage has considerable face and external validity. As seen in Figure 1, which presents the mean percentage that support same-sex marriage from 1996 to 2013 by census region, more ideologically conservative regions have lower levels of support for same-sex marriage. This regional pattern extends to individual states. The highest levels of support, on average, are in liberal states like Massachusetts and Hawaii, while conservative states such as Utah and Alabama have the lowest levels of support. Furthermore, our measure compares well with Lax and Phillips’ (2009a) static MRP measure, which aggregates survey data from 1999 to 2008. Over these 10 years, our dynamic measure correlates with their static measure at 0.86. The advantage of our measure is that it can track changes in opinion over time. Indeed, we find that attitudes toward same-sex marriage varied considerably over time in each state, regardless of how supportive citizens of a state were. At the high end, Hawaii and Maine witnessed swings of over 30 percentage points from 1996 to 2013. Even at the low end of temporal variation, states like Oklahoma and Arkansas still had shifts of more than 10 percentage points.

Figure 1. Mean annual estimates of support for same-sex marriage by region, 1996–2013.
Figure 1 also shows the timing of these attitude dynamics. In general, attitudes have become more accepting over time in all states and regions, following national trends (Flores 2014) and consistent with arguments that expanding legal recognition of same-sex relationships can signal new social norms (Kreitzer, Hamilton, and Tolbert 2014). However, there are two periods of declining support. In 2004, there appears to be a backlash following increased public attention to gay rights sparked by the 2003 Lawrence v. Texas Supreme Court decision to strike down sodomy laws, the presidential campaign, and the prominence of a slew of same-sex marriage ban initiatives on the ballot. This pattern is consistent with studies that have found a significant relationship between court decisions and public opinion shifts, especially in this case (Stoutenborough and Haider-Markel 2008; Stoutenborough, Haider-Markel, and Mahalley 2006; but see Bishin et al. 2015). In 2008, there is another decline in support for same-sex marriage that may be driven by negative policy feedback in some regions (e.g., Edelman 1971; Pierson 1993). Although all 50 states show a positive trajectory over the time period, there is significant variation across the states.

Dynamic Analyses of Same-Sex Relationship Recognition Policies

With a dynamic measure of state-level support for same-sex marriage in hand, we can examine whether states are responsive to these attitudes and whether direct democracy enhances responsiveness. Using event history analyses, we examine three aspects of same-sex relationship recognition policy—bans on same-sex marriage, same-sex relationship recognition, and legalized same-sex marriage. This policy area was selected because it has been a central issue in the gay rights movement and has witnessed considerable changes in public support over the past three decades, highlighting the importance of public opinion dynamics. Furthermore, this policy area is useful to assess issues of policy responsiveness and congruence since it is a relatively new policy area for the states. Prior to 1994, there were no formal policies directed toward same-sex relationship recognition (Pinello 2006). Although a few states had gendered marriage definitions, enforcement of heterosexual marriage policy was primarily driven by court rulings drawing on religious definitions of marriage. Thus, each state in our sample is addressing this policy anew. Similarly, since public opposition to same-sex marriage in 1994 stands in stark contrast to the de jure (though not de facto) status quo policy, the context of this policy area is ripe for what Leemann and Wasserfallen (2016) call a “democratic effect” of direct democracy to increase policy congruence. In addition, policy in this area is quite uniform across states, ensuring clean comparisons. More practically, this policy area has been addressed in polls fairly consistently in recent decades.

The analyses examine annual data from 49 states from 1994 to 2013. As many states had not yet passed the policy in question as of 2013, the data are right-censored. Event history approaches allow us to account for bias generated by this right-censoring. Furthermore, using a Cox Proportional Hazards analysis allows for a flexible baseline hazard rate, avoiding assumptions about the nature of the temporal effects. Since we are interested in the effects of public opinion and direct democracy, and not
the duration dependency, this approach is well-suited for our purposes.\textsuperscript{11} The dependent variable is a dichotomous indicator of whether a state has adopted the policy. States are then dropped from the analysis in subsequent years once they adopt the policy since they are no longer “at risk” of adopting the policy.

In addition to the estimates of support for same-sex marriage, the primary independent variables of interest are measures of direct democracy and interactions between opinion and direct democracy. By interacting the direct democracy measures with public opinion, we test the extent to which these institutions enhance responsiveness to public opinion. If direct democracy does increase responsiveness, the coefficient for the interaction term should be in the same direction as the public opinion coefficient. Direct democracy is initially measured with a dichotomous indicator of whether the state has direct initiatives, indirect initiatives, or popular referendums. Twenty-four states have at least one of these institutions.\textsuperscript{12} Next, since there is quite a bit of variation among these 24 states in their direct democracy institutions (Bowler and Donovan 2004; Lupia et al. 2010), we also employ a measure of Direct Democracy Impact. This variable is generated with a principle components analysis of three commonly used direct democracy measures: Bowler and Donovan’s (2004) Qualification Difficulty Index, their Legislative Insulation Index, and the natural log of the number of ballot measures considered in the state in the contemporary era (Pippen, Bowler, and Donovan 2002). The Direct Democracy Impact scores range from zero for nondirect democracy states to nearly five for California, with an average of 3.1 for direct democracy states.\textsuperscript{13} For each policy, we estimate one model using the dichotomous direct democracy variable and another using the Direct Democracy Impact variable.

The models also control for several other factors. We include a general measure of citizen ideology: MRP estimates of the percentage of self-identified liberals minus self-identified conservatives in each state (Enns and Koch 2013). The models also include an interaction between direct democracy and citizen ideology, allowing us to account for general ideological responsiveness when assessing the effects of issue-specific public opinion in direct democracy states. Partisanship in the state government is accounted for with two dichotomous indicators of unified Democratic or Republican control, where one party holds majorities in both chambers of the legislature and occupies the Governor’s office. Unified government should make it easier for the majority party to pass their agenda and block the opposing party’s issues. The reference category in most of the models is divided party control. The analyses also control for the level of party competition using a folded Ranney Index (Klarner 2014; Ranney 1976). In states with competitive party systems, the parties may seek to reach out to new constituencies to gain an electoral advantage. We also account for the effects of geographic diffusion with a variable measuring the proportion of neighboring states that have previously adopted the policy (e.g., F. S. Berry and Berry 1990). In addition, the models include the estimated percentage of the state population that identifies as lesbian, gay, bisexual, or transgender (Gates and Newport 2013) as measure of the organizing capacity of lesbian, gay, bisexual, and transgender (LGBT) right groups (Haider-Markel 2001). Last, the models control for the size of states with the natural log of a state’s population.
We do not include several demographic factors, such as education, race, and religious adherence. Although these factors may influence the adoption of gay rights policies, they are hypothesized to work by affecting public opinion (Lewis 2011b; 2013; Lupia et al. 2010; Taylor et al. 2012). For example, higher education levels are expected to increase political tolerance, making the adoption of gay rights protection more likely in states with higher rates of college graduates (McClosky and Brill 1983). However, we already account for this effect by directly measuring attitudes toward same-sex marriage. Thus, we avoid multicollinearity issues that would arise from including them in the models alongside our estimate of public attitudes.

**Same-Sex Marriage Bans**

We begin with an analysis of same-sex marriage bans, extending from 1994, prior to any existing statutory bans, to 2006, the last year in which a new ban was adopted. The dependent variable indicates the adoption of a state’s initial same-sex marriage ban and does not differentiate between statutory provisions and constitutional amendments. In addition to the variables described earlier, we added an indicator of whether the status quo policy in a state had language that could reasonably be interpreted as a ban on same-sex marriage (i.e., “husband and wife”).

The results, presented as hazard ratios, are shown in Table 1. Using the exact partial estimation approach, the hazard ratios should be interpreted as the relative change in the risk of policy adoption conditional on the underlying hazard rate for each year. A hazard ratio less than one indicates a reduction in the relative risk for a one-unit change in the independent variable, while a hazard ratio greater than one indicates an increase in the relative risk. It is clear from the table that public attitudes toward same-sex marriage have significant effects on the likelihood of passing a same-sex marriage ban in a given year. The hazard ratio for the public opinion variable reveals that nondirect democracy states that are more supportive of same-sex marriage are less likely to pass these policies in a given year. A percentage point increase in support for same-sex marriage reduces the hazard of a state adopting a ban by 16.6%. Furthermore, the hazard ratio for the interaction between the direct democracy measures and public opinion indicates that these institutions slightly increase responsiveness to public opinion. In direct democracy states, with a combined hazard ratio of 0.791 ($p = .014$), the hazard of adopting a same-sex marriage ban is reduced by 21% for a one-unit increase in support for same-sex marriage. These public opinion effects are smaller than the estimated impact of unified Republican control—the odds of passing a ban in these states is more than three times higher than in other states—but the magnitude of responsiveness is nonetheless substantively significant. In 1997, for example, an increase of one percentage point over the mean level of public support (34%) reduces the hazard rate from 0.41 to 0.38. For a standard deviation (about 10 percentage points) increase from the mean, the hazard drops all the way down to 0.28. Figure 2 shows the effect of public opinion across the different levels of Direct Democracy Impact, revealing increases in responsiveness as the impact of these institutions grows.
Table 1. Event History Models of Same-Sex Marriage Ban Adoption, 1994–2006.

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1) Direct democracy dummy</th>
<th>(2) Direct democracy impact</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Hazard ratio</td>
<td>SE</td>
</tr>
<tr>
<td>Public Opinion</td>
<td>0.834*</td>
<td>(0.097)</td>
</tr>
<tr>
<td>Opinion × Direct Democracy</td>
<td>0.948</td>
<td>(0.095)</td>
</tr>
<tr>
<td>Citizen Ideology</td>
<td>0.947</td>
<td>(0.083)</td>
</tr>
<tr>
<td>Ideology × Direct Democracy</td>
<td>1.135*</td>
<td>(0.111)</td>
</tr>
<tr>
<td>Direct Democracy</td>
<td>56.123</td>
<td>(235.027)</td>
</tr>
<tr>
<td>Republican Government</td>
<td>3.159**</td>
<td>(1.851)</td>
</tr>
<tr>
<td>Democratic Government</td>
<td>1.410</td>
<td>(0.865)</td>
</tr>
<tr>
<td>Party Competition</td>
<td>1.020</td>
<td>(0.031)</td>
</tr>
<tr>
<td>Geographic Diffusion</td>
<td>0.988</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Prior Language</td>
<td>0.107***</td>
<td>(0.100)</td>
</tr>
<tr>
<td>LGBT Population %</td>
<td>0.937</td>
<td>(0.363)</td>
</tr>
<tr>
<td>Population (logged)</td>
<td>0.838</td>
<td>(0.195)</td>
</tr>
</tbody>
</table>

N 309 309
Log Likelihood −69.556 −69.486

Note. Each model presents the results of a Cox Proportional Hazards where the dependent variable indicates the adoption of a state’s initial same-sex marriage ban. The results are presented as hazard ratios. Standard errors are shown in parentheses.
*p < 0.1. **p < 0.05. ***p < 0.01 (one-tailed tests where appropriate).

Figure 2. Effect of public opinion on the adoption of a same-sex ban (Model 2).
The results suggest that all states are responsive to public preferences on same-sex marriage bans, but there is a small increase in responsiveness in direct democracy states. This is particularly striking since the models control for citizen ideology and its interaction with direct democracy. The results highlight the extent to which state governments are responsive to specific issue attitudes and preferences, not just a general sensitivity to ideological orientations. While state citizen ideology is correlated with support for same-sex marriage ($r = 0.49$), the two measures of public opinion clearly tap distinct constructs.

**Same-Sex Partnership Recognition**

While almost every state ultimately banned same-sex marriage by 2006, some states also passed policies to legally recognize same-sex relationships and provide benefits analogous to marriage. These policies include domestic partnerships, civil unions, and same-sex marriages. Here, we examine the factors that affect a state’s initial adoption of any of the three types of relationship recognition policies from 1994 to 2013. The independent variables are the same as the previous analyses, but the Republican Government variable is omitted since these governments did not adopt any relationship recognition policies during this time period, making it impossible to estimate standard errors for this coefficient.

The results are presented in Table 2. Unlike the previous analyses, support for same-sex marriage in nondirect democracy states does not have a significant effect. However, in direct democracy states, these attitudes have a significant impact. The combined hazard ratio for public opinion and the interaction term from Model 3 produces a marginal hazard ratio of $1.254$ ($p = .028$), indicating a 25% increase in the risk of passing a relationship recognition policy for every percentage point increase in support for same-sex marriage in direct democracy state. A similar finding is evident in Model 4. Public opinion is not a significant factor for nondirect democracy states, but the combined effects of the interaction term and the public opinion variable show that the effect of public opinion in direct democracy states is positive and statistically significant. Figure 3 shows that states with low Direct Democracy Impact scores—those that do not use ballot measures often and insulate the legislature from their effects—have a small, marginally significant effect, increasing the risk of adoption by 16%. In more typical direct democracy states, those with scores of two or greater (all but three direct democracy states), public opinion has a larger, statistically significant impact, with hazard ratios indicating increases in the risk of policy adoption ranging from 18% to 27%. Once again, the significant effect of specific issue attitudes in direct democracy states is especially striking since the models also account for general citizen ideology. While party control of government has a relatively larger effect in these models, the magnitude of the effect of public opinion in direct democracy states is still substantively significant.

**Same-Sex Marriage**

The next set of analyses examines the legalization of same-sex marriage. Policy development in this policy presents a complication that requires a slightly different
Table 2. Event History Models of Same-Sex Relationship Recognition Policies, 1994–2013.

<table>
<thead>
<tr>
<th>Variable</th>
<th>Hazard ratio</th>
<th>SE</th>
<th>Hazard ratio</th>
<th>SE</th>
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</thead>
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<tr>
<td>Direct democracy dummy</td>
<td>1.117</td>
<td>0.133</td>
<td>1.128</td>
<td>0.139</td>
</tr>
<tr>
<td>Opinion × Direct Democracy</td>
<td>1.123</td>
<td>0.131</td>
<td>1.025</td>
<td>0.034</td>
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<tr>
<td>Citizen Ideology</td>
<td>1.105</td>
<td>0.099</td>
<td>1.101</td>
<td>0.096</td>
</tr>
<tr>
<td>Ideology × Direct Democracy</td>
<td>1.072</td>
<td>0.075</td>
<td>1.028</td>
<td>0.024</td>
</tr>
<tr>
<td>Democratic Government</td>
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<td>0.156</td>
<td>0.672</td>
<td>0.923</td>
</tr>
<tr>
<td>Party Competition</td>
<td>1.019</td>
<td>0.051</td>
<td>1.008</td>
<td>0.051</td>
</tr>
<tr>
<td>Geographic Diffusion</td>
<td>0.990</td>
<td>0.014</td>
<td>0.988</td>
<td>0.015</td>
</tr>
<tr>
<td>LGBT Population %</td>
<td>2.169</td>
<td>1.461</td>
<td>2.091</td>
<td>1.385</td>
</tr>
<tr>
<td>Population (logged)</td>
<td>0.780</td>
<td>0.267</td>
<td>0.748</td>
<td>0.249</td>
</tr>
<tr>
<td>N</td>
<td>859</td>
<td></td>
<td>859</td>
<td></td>
</tr>
<tr>
<td>Log Likelihood</td>
<td>−32.067</td>
<td></td>
<td>−31.544</td>
<td></td>
</tr>
</tbody>
</table>

Note. Each model presents the results of a Cox Proportional Hazards where the dependent variable indicates the adoption of a state’s initial same-sex recognition policy (domestic partnerships, civil unions, or same-sex marriage). The results are presented as hazard ratios. Standard errors are shown in parentheses.

*p < .1. **p < .05. ***p < .01 (one-tailed tests where appropriate).

Figure 3. Effect of public opinion on the adoption of a same-sex relationship recognition policy (Model 4).
Table 3. Event History Models of Same-Sex Marriage Recognition, 1994–2013.

<table>
<thead>
<tr>
<th>Variable</th>
<th>Legislative policy adoptions</th>
<th>All policy adoptions</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td></td>
<td>Direct democracy dummy</td>
<td>Direct democracy impact</td>
</tr>
<tr>
<td>Public Opinion</td>
<td>Hazard ratio: 0.975 (0.172)</td>
<td>Hazard ratio: 1.001 (0.185)</td>
</tr>
<tr>
<td>Opinion × Direct Democracy</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Citizen Ideology</td>
<td>1.144 (0.154)</td>
<td>1.111 (0.149)</td>
</tr>
<tr>
<td>Ideology × Direct Democracy</td>
<td>2.055* (0.987)</td>
<td>1.426** (0.254)</td>
</tr>
<tr>
<td>Direct Democracy</td>
<td>0.000 (0.000)</td>
<td>0.000 (0.000)</td>
</tr>
<tr>
<td>Democratic Government</td>
<td>9.315** (11.193)</td>
<td>11.475** (14.577)</td>
</tr>
<tr>
<td>Party Competition</td>
<td>1.062 (0.095)</td>
<td>1.064 (0.096)</td>
</tr>
<tr>
<td>Geographic Diffusion</td>
<td>1.045** (0.020)</td>
<td>1.050** (0.023)</td>
</tr>
<tr>
<td>LGBT Population %</td>
<td>0.288 (0.422)</td>
<td>0.377 (0.584)</td>
</tr>
<tr>
<td>Population (logged)</td>
<td>0.546 (0.333)</td>
<td>0.594 (0.344)</td>
</tr>
</tbody>
</table>

N                                         | 947                          | 947                  | 947                      | 947                      |

Note. Each model presents the results of a Cox Proportional Hazards where the dependent variable indicates the legalization of same-sex marriage. Models 5 and 6 are competing event history models, which treat only legislative adoptions as policy events and judicial adoptions as censoring events. Models 7 and 8 treat all policy adoptions as policy events. The results are presented as hazard ratios. Standard errors are shown in parentheses. *p < .1. **p < .05 (one-tailed tests where appropriate).

For the states that have legalized same-sex marriage, only 11 did so through the legislative process or direct democracy. Indeed, state courts have been quite active in this policy area. Although state courts, like legislatures and governors, may be affected by public opinion and direct democracy, there is relatively little empirical evidence of this kind of relationship, especially for unelected judges (but see Lewis, Wood, and Jacobsmeier 2014; Manweller 2004). Thus, it is not clear that judicial policymaking should be treated as the same as legislative and direct democracy processes.

To address this issue, we first estimate event history models of legislative policy adoption, but allow for multiple censoring events (Models 5 and 6). States that legally recognize same-sex marriage are subsequently dropped from the risk pool, regardless of whether it was through legislative or state judicial processes. However, only policies established by legislation (by the legislature or through direct democracy) are treated as policy events, indicated in the dependent variable with a value of one. Judicial enactments are treated as censoring events, so the dependent variable retains a value of zero but is subsequently removed from the risk pool.

The results from Models 5 and 6 are presented in Table 3. In both models, the effect of public support for same-sex marriage in nondirect democracy states is not statistically significant. However, both of the public opinion interaction hazard ratios are
positive, suggesting an increase in responsiveness to public opinion. In Model 5, the combined hazard ratio is 1.645, indicating that increasing support for same-sex marriage is associated with a 65% increase in the hazard of passing legislation, but it does not quite reach traditional levels of statistical significance ($p = .149$). Model 6, however, does reveal a statistically significant relationship between public opinion and policy adoption for direct democracy states. The first graph in Figure 4 shows that states with a Direct Democracy Impact score greater than one are significantly affected by public attitudes toward same-sex marriage. For each percentage point increase in support for same-sex marriage, the hazard rate in direct democracy states is roughly...
two to six times as high, depending on the impact of those institutions. The magnitude of public opinion effects in direct democracy states in this case is roughly on par or greater than the effect of unified Democratic control.

As seen from Models 7 and 8, treating judicial enactments as policy events slightly dampens the interaction between public opinion and direct democracy, but the effects remain, for the most part, statistically significant. The combined hazard ratio from Model 7 is 1.293 \( (p = .095) \). The interactive effect can be seen in the second graph in Figure 4, revealing an increasing effect of public opinion as the impact of direct democracy increases. This effect is statistically significant for high impact direct democracy states, those with values above three on the Direct Democracy Impact measure. Perhaps not surprisingly, when judicial enactments are included in the model, party control of government is no longer statistically significant. This leaves public opinion in direct democracy states, geographic diffusion, and LGBT group capacity as the primary drivers of policy adoption.

**Policy Congruence**

Each of the previous analyses supports the argument that direct democracy increases responsiveness to public opinion. However, this dynamic responsiveness does not necessarily mean that direct democracy states are more likely than other states to hold policies that are congruent with public opinion. A responsive government may not be representative government (e.g., Lax and Phillips 2009a). To assess the degree to which responsiveness to public opinion in direct democracy states extends to normative ideals of democratic representation, we conduct an analysis of policy congruence. States are considered to be congruent with public opinion when the current policy is in line with the preferences of the public. For same-sex marriage policy, a state’s policy is congruent if they have a ban and less than 50% of the public supports same-sex marriage or if the state recognizes same-sex marriages and more than 50% of the public supports that policy. Note that policy congruence can be achieved either by the state acting to bring policy in line with public opinion or by public opinion shifting to reflect state policy. Similarly, policy incongruence can result either from state action or through changes in public opinion.

From 1994 to 2013, nondirect democracy states were congruent with public opinion 60.2% of the time, while direct democracy states were congruent 71.5% of the time. Both levels of congruency are remarkable since every state began this time period with an incongruent policy because there were no statutory bans or legal recognition laws in place. However, direct democracy states, reflecting our findings of increased responsiveness, were congruent over 10% more often \( (p < .001) \) than nondirect democracy states.

To test this finding further, we estimate two logistic regression models of policy congruence. With cross-sectional time series data, the models are estimated using state random effects and specified with fixed effects for years. Along with the two measures of direct democracy, we also include several other factors: public opinion, party competition, and legislative professionalism. Changes in public opinion from year to year
should make it less likely for state policy to be congruent. The size of the public opinion majority may also affect policy congruence—large majorities apply more pressure on government and send clearer signals about preferences, while small majorities may be less influential and less clear. High party competition, measured using a folded Ranney index, should increase the likelihood of policy congruence as the parties seek to win or maintain control of government. Legislative professionalism, measured using the Squire Index (Squire 1992; 2007), may increase the likelihood of congruence since professional legislators have a greater incentive to seek reelection and more resources at their disposal to respond to constituents (e.g., Lax and Philips 2012; Maestas 2000). At the same time, legislative professionalism may insulate legislators from the electorate, resulting in a lower likelihood of policy congruence.

The results from the models of policy congruence are presented in Table 4. Model 9 shows that the odds of policy congruence in direct democracy states are higher than other states in a given year, with a 12% increase, on average, in the predicted probability of policy congruence. Model 10 reveals similar results. Each unit increase in Direct Democracy Impact is associated with a 5% increase, on average, in the predicted probability of policy congruence. The models also show that public opinion has significant effects in all states. As expected, large changes in public opinion are associated with significantly lower odds of policy congruence. Large majorities are significantly more likely to achieve policy congruence than bare majorities. Each percentage point increase in the majority is associated with a 2% increase in the predicted probability of policy congruence.

In the case of same-sex marriage, it appears that the increased responsiveness of direct democracy state is positively associated with policy congruence. This

<table>
<thead>
<tr>
<th>Variable</th>
<th>(9) Direct democracy dummy</th>
<th>(10) Direct democracy impact</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Odds ratio</td>
<td>SE</td>
</tr>
<tr>
<td>Change in Opinion</td>
<td>0.879**</td>
<td>(0.065)</td>
</tr>
<tr>
<td>Size of Opinion Majority</td>
<td>1.290***</td>
<td>(0.045)</td>
</tr>
<tr>
<td>Direct Democracy</td>
<td>4.317*</td>
<td>(0.916)</td>
</tr>
<tr>
<td>Party Competition</td>
<td>0.969</td>
<td>(0.022)</td>
</tr>
<tr>
<td>Legislative Professionalism</td>
<td>0.941**</td>
<td>(0.028)</td>
</tr>
<tr>
<td>ln(σ_u^2)</td>
<td>2.145</td>
<td>(0.306)</td>
</tr>
<tr>
<td>σ_u</td>
<td>2.923</td>
<td>(0.447)</td>
</tr>
<tr>
<td>p</td>
<td>.722</td>
<td>(.061)</td>
</tr>
<tr>
<td>N</td>
<td>931</td>
<td></td>
</tr>
<tr>
<td>Log Likelihood</td>
<td>−263.273</td>
<td></td>
</tr>
</tbody>
</table>

Note. Models present the results of logistic regressions estimated with random effects by state and fixed effects for years. The results are presented as odds ratios. Standard errors are shown in parentheses.

*p < .1. **p < .05. ***p < .01 (one-tailed tests where appropriate).
relationship makes intuitive sense. On average, as public opinion shifts, more responsive states should be more likely to achieve policy congruence compared with states that tend to be less responsive.

Discussion

Understanding the degree of policy responsiveness and congruence of government requires dynamic approaches to both measuring public preferences and analyzing policy adoptions. By developing dynamic MRP estimates of support for same-sex marriage, we are able to take an initial step toward providing a more complete picture of dynamic representation in both direct democracy states and nondirect democracy states. For each of the three aspects of same-sex relationship policy examined here, the analyses consistently produced compelling evidence of responsiveness to issue-specific public opinion, particularly for states with direct democracy institutions. Further, the dynamic congruence analyses revealed that direct democracy not only enhanced responsiveness but also increased the likelihood that state’s policies matched majority public opinion. Each of the results highlights the importance of accounting for dynamic, issue-specific public opinion in models of policy change. In policy areas where public opinion is fluid and quickly changing, assessing responsiveness without incorporating dynamics may underestimate the democratic nature of government.

Rather than mixed evidence, we find a robust effect of direct democracy in the area of LGBT rights. Contrary to Lax and Phillips’ (2009a; 2012) null findings, the incorporation of dynamic public opinion and policy measures reveals evidence consistent with the argument that direct democracy institutions enhance popular control over public policy. This finding is also consistent with Matsusaka’s (2010) study of congruence on a range of salient public policies. While the latter study uses a cross-sectional design, it does incorporate measures of public opinion and policy congruence across multiple years, likely picking up some of the same dynamic effects we show here. Put alongside recent findings of responsiveness in state spending priorities by direct democracy states (Lewis, Schneider, and Jacoby 2015), this study shows consistent evidence of increased responsiveness when opinion and policy change is accounted for. Furthermore, this study shows a clear connection between increased responsiveness and policy congruence. In all, these dynamic analyses clearly support the argument that direct democracy strengthens the representative ties between government and the governed.

The results of the event history analyses also demonstrate the difference between general political orientations and issue-specific attitudes. Although we expect ideological orientation and underlying political values to be related to issue attitudes, the responsiveness of government to public opinion should arguably be driven by clear manifestations of policy-specific preferences. Our analyses mirror this argument. Even when accounting for citizen ideology, governments displayed substantial responsiveness to issue attitudes. Indeed, attaining statistical significance under conditions of substantial collinearity between the two measures of public opinion further underscores the extent of responsiveness demonstrated in the analyses.
Finally, although evidence of responsiveness to public opinion is generally discussed in positive terms, it is also important to recognize the potentially negative connotations of majority responsiveness. Consistent with the recent literature on direct democracy and minority rights, it seems that direct democracy institutions make state government at least slightly more responsive to public opinion (e.g., Haider-Markel, Querze, and Lindaman 2007; Lewis 2013). In terms of the gay rights issues analyzed here and similar issue areas, such as transgender rights (e.g., Taylor et al. 2012), that result is potentially troubling. For much of the time period, most states showed very low levels of support for same-sex marriage. Thus, responsiveness in most states meant passing antigay rights policies like same-sex marriage bans and not passing policies recognizing same-sex relationships.

This last point highlights the need for further research on direct democracy and representation across a range of specific issue areas. As recent studies have noted, the potential of direct democracy to enhance representation may be context-dependent (Burden 2005; Leemann and Wasserfallen 2016). The case of same-sex relationship recognition policies presents a near-ideal context for enhancing representation through direct democracy—the status quo policy (or lack thereof) was initially unrepresentative of public opinion, the issue was publicly salient, and opinion shifted dramatically over a relatively short period of time. However, in other contexts, direct democracy may not have such a clear effect on representation. Can direct democracy improve representation on less salient issues or in cases that Leemann and Wasserfallen (2016) would describe as “good representation?” Do these institutions enhance representation in areas where public opinion is more stable? While evidence of general policy responsiveness, as demonstrated by Lewis, Schneider, and Jacoby (2015), suggests a more generalizable enhancement of representation under direct democracy institutions, further examination of a range of policies under a variety of representation contexts and opinion dynamics should be pursued in the future.

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Notes
1. See, for example, same-sex marriage policy after Goodridge v. Dept. of Public Health (2003), or state abortion policy in wake of the Roe v. Wade (1973). Lax and Phillips (2012) discuss this possibility, and dynamic analysis allows us to examine it in greater detail.
2. Because the surveys that we use were not administered at regular intervals across time, we cannot pool across multiple survey years when generating our estimates as Pacheco suggests. While having additional data that would allow for pooling across years would be
useful, pooling is not without costs. As Pacheco notes, pooling may mask real short-term changes in state-level opinion. Aside from the issue of data availability, given the well-documented high rate of change in national-level opinion on same-sex marriage, we feel there are substantial benefits to not pooling across years in this case. Moreover, we include state-level covariates that should increase the precision of our estimates.

3. We thank Kastellec, Lax, and Phillips (2010) for their excellent primer on multilevel regression and poststratification (MRP).

4. Buttice and Highton (2013) caution that the quality of MRP estimates can vary substantially. Our model meets both of the two main criteria that they suggest are important if MRP is to perform well. First, both prior research and Buttice and Highton’s own results suggest that the state-level predictors employed here strongly correlate with opinions on same-sex marriage. Second, we find substantial cross-state variation in opinion on same-sex marriage.

5. Individual question wordings and sample sizes are listed in the online appendix.

6. We identified surveys with state geo-codes and questions for 1996, 2000, 2003, 2004, 2006–2010, and 2012–2013. Census data is only collected every 10 years. The state-level religion variable is also only available at 10-year intervals. To match the Census and religion data with each survey year, we use linear interpolation to determine the number of each type of respondent in each state and the percentage of each state’s residents who are Mormon or evangelical Protestants for each year.


8. Use of the census data was facilitated by the Integrated Public Use Microdata Series (IPUMS-USA) database (Ruggles et al. 2015).

9. Trends for all 50 states are presented in the online appendix.

10. Nebraska is excluded from the analyses due to its nonpartisan legislature.

11. Following Box-Steffensmeier and Jones (2004), we use the exact partial estimation method to handle ties since we have discrete time data.

12. Given the strict content restrictions on Illinois’ ballot initiatives, it is often considered to be a nondirect democracy state. Alternative measures that treat them this way produce similar results to those presented here.

13. See the online appendix for state Direct Democracy Impact scores.

14. Subsequent adoptions of constitutional amendments that reinforce existing statutory law are not included. We are interested in the policy change, not the particular form that the policy takes. See Lupia et al. (2010) for analysis of constitutional amendments to ban same-sex marriage.

15. California, Maryland, Wisconsin, and Wyoming all have language in their marriage policies that fit this description.

16. This effect is calculated with all other variables at their mean or modal values. Alternatively, the substantive effect of public opinion is evident in the change in the median predicted time until a ban is passed in a state. Holding all other variables at their mean or modal values, a one standard deviation increase in the support for same-sex marriage, from 26% (the mean value in 1994) to 30%, increases the median time to adoption by roughly 1 year.

17. Repeated and competing events models that treat these policies as distinct produce similar results, showing increased responsiveness in direct democracy states.

18. This includes legalization of same-sex marriage. As such, the Republican Government variable is also omitted from the following analysis of same-sex marriage legalization.

19. We do not include the string of federal court cases in 2013 that struck down same-sex marriage bans across the country.
Supplementary Material

Supplementary material for this article is available online.

References


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