

Direct Democracy and Social Issues*

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This paper explores the connection between the initiative process – the most potent form of direct democracy – and social issues by examining laws on seven social issues in all 50 American states. Initiative states are 18 percent more likely than noninitiative states to choose a conservative than a liberal policy on the median issue after controlling for public opinion, demographic, and regional variables. The conservative shift is majoritarian: initiative states are 8 percent more likely than noninitiative states to choose laws that reflect the majority's preference. The initiative effect does not appear to depend on the institutional features that scholars and reformers often discuss.

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1. Introduction

Ballot propositions are emerging as an important arena in the battle over social issues. Since the Massachusetts Supreme Judicial Court ruled in favor of same-sex marriage in 2004, 25 referendums related to marriage and civil unions have come before the voters in 24 states. Over the last 15 years, voters have decided 12 initiatives on abortion-related topics, a flurry of propositions concerning job discrimination on the basis of sexual orientation, and numerous other issues related to social policy, such as capital punishment, medical marijuana, and physician-assisted suicide. The number of ballot propositions overall is at or near a historical high and the amount of money involved is enormous, with \$358 million spent on ballot propositions in 2006 in California alone, including \$154 million on a single proposition.¹ Some observers even believe that ballot propositions are eclipsing the legislature in some states (Schrag, 2004, p. 195): “While some of the measures on California’s post-Proposition 13 ballots can match almost anything that preceded them for triviality, by almost any measure – cost, length, social and economic importance – many are of such consequence that the real policy decisions are now being made in the plebiscitary process and not in the halls of the legislature or the office of the governor, much less at the school board or city council.”

Despite the growing importance of direct democracy, empirical research on how popular lawmaking affects social policies is scarce. Existing research tends to study fiscal policies, and those studies focused on social issues are mainly interested in voting behavior. Little is known about whether direct democracy changes policy choices, whether those changes are in a liberal or conservative direction, and how direct democracy impacts the “representativeness” of policy choices. The purpose of this paper is to provide an empirical analysis of the impact of the initiative – the most potent form of direct democracy – on the choice of social policies by studying the connection between

¹ Campaign spending numbers calculated from information on the California Secretary of State web site (www.ss.ca.gov). A total of 226 propositions went before the voters in 37 states in 2006, apparently close to or exceeding the highest level historically. The number of initiative ballot propositions – measures placed on the ballot by citizen petition – has set a new record every decade since the passage of California’s tax-cutting Proposition 13 in 1978. See www.iandrinstitute.org for historical information on ballot propositions.

direct democracy and policy choices of all 50 states on seven different issues.²

Methodologically, the paper follows in the path of a growing literature that measures policy effects by comparing outcomes in initiative and noninitiative states. As observed by Gerber and Hug (2002), while most existing research focuses on passage rates and voting on individual ballot propositions, formal models strongly suggest that initiatives and referendums may influence policy even without a successful proposition on the ballot – the threat of a ballot measure may be enough to prod the legislature to adopt policies it would not otherwise choose (Gerber, 1996; Matsusaka and McCarty, 2001). Studies that examine only the measures that reach the ballot and are approved may capture the “direct effect” but cannot capture the “indirect” or “threat” effect of direct democracy. One way to capture the full (direct + indirect) effect is to compare policy choices in states with and without the initiative. Since the final policy choice will be determined by the full effect, the overall impact of the initiative can be inferred by comparing policy choices of initiative and noninitiative states after controlling for other factors that influence policy choices. The strategy of using variation across states to identify the effects of institutions has been used successfully to study fiscal and other policies, but rarely for social issues.³

The analysis focuses on seven policy issues that have received a great deal of attention over the last decade, and is organized around three broad questions:

1. Does direct democracy bring about different policies than representative democracy?

I begin by investigating whether the initiative matters at all for social policy, or is merely a veil that political actors are able to work around. The answer might seem obvious given the proliferation of ballot propositions dealing with social issues, but the

² To define terms, an “initiative” is a new law proposed by citizens and placed on the ballot by petition; a “referendum” is a proposal to repeal an existing law, also placed on the ballot by petition, and a “legislative” or “referred” measure is a new law placed on the ballot by the legislature.

³ Existing studies that exploit differences between initiative and noninitiative states include Gerber and Hug (2002) (on gay and lesbian antidiscrimination laws, English-only laws, and affirmative action laws), Gerber (1999) (on death penalty and parental consent for abortion), and Arceneaux (2002) (on abortion “restrictiveness”). Examples of studies that have used the cross-sectional approach for other issues are Matsusaka (1995, 2000, 2004) for fiscal policies, and Tolbert (1998), Bowler and Donovan (2004), and Matsusaka (2006) for election laws.

approval of ballot measures does not necessarily mean that policy is different as a result. An initiative is just window dressing if the legislature would have approved the policy on its own anyway. For example, while initiatives were used to ban gay marriage in 11 states (through 2006), legislatures placed similar measures on the ballot in 16 other states, raising the possibility that the end result would have been the same with or without the initiatives. In addition, as Gerber et al. (2001) show, even when voters approve an initiative, it may never go into effect because of court challenges or lack of enforcement. To assess the importance of the initiative on final policy choices, I estimate a set of regressions that control for public opinion and demographic, economic, and regional factors. The estimates show that direct democracy results in significantly different laws for the seven issues taken as a group, and the initiative appears to push policy in a conservative rather than liberal direction: for the median issue, initiative states are about 18 percent more likely than noninitiative states to choose a conservative law. Although some observers have concluded that the mix of issues on the ballot are not tilted toward conservative or liberal outcomes (Cronin, 1989; Schmidt, 1989), it seems that the net result of the initiative process has been to push policy in a conservative direction, at least for these seven social issues.

2. Does direct democracy bring about policies that favor the many or the few?

After documenting the ideological direction of the initiative's effect on these social issues, the paper investigates whether the changes bring policy closer to the preferences of the majority or favor a minority. The Progressives who brought the initiative to the United States in the early twentieth century and modern proponents favor the process in the belief that it allows "the public" to gain control over policy when special interests become too influential in the legislature. Yet critics have long contended that the initiative has the opposite effect, increasing the power of special interests and leading to policy *less* consonant with public opinion (Broder, 2000). This could happen if those who turn out to vote on initiatives are not representative of the overall population (Magleby, 1984, Ch. 6), or if people can be misled into casting a vote for a proposition

that implements a policy outcome opposite from what they really want.⁴ To evaluate whether the initiative helps the many or the few, I use opinion data from the American National Election Studies to identify whether the majority is in favor of or opposed to a particular policy outcome for each issue and state. I then compare the majority view with the actual law in each state, and classify each state-issue as “congruent” if the majority’s preference prevails, and “noncongruent” if the majority’s view does not prevail. For the 350 state-issue observations, I find that 61 percent of policies are congruent, and that availability of the initiative increases congruence by a statistically and quantitatively significant 8 percent on the median issue. Thus, a central finding is that direct democracy appears to bring social policies into greater conformity with the majority’s opinion.

3. Which features of the initiative process are important?

Although similar in form, each state with the initiative implements the process in a different way. It is useful to understand which of these institutional differences are important, both for theoretical reasons, and for reformers who are interested in improving the process in their state. Previous research suggests that institutional features such as petition signature requirements, geographic dispersion requirements, and circulation periods may be important, but the results are not always strong (Matsusaka, 1995, 2000, 2004; Matsusaka and McCarty, 2001; Bowler and Donovan, 2004). I explore the importance of institutional features by considering the impact of signature requirements, whether initiatives are allowed to amend the constitution or only to pass statutes, and using two indexes of “qualification ease” and “insulation from the legislature” created by Bowler and Donovan (2004). None of these features appear to have an important effect

⁴ A possible example of misled voters is California’s Proposition 77 in 2005, in which voters rejected 60-40 a measure that would have taken redistricting away from the legislature and put it in the hands of a panel of retired judges, something that voters say they want to happen by about a 2-1 margin (see, for example, a survey by the Rose Institute at Claremont McKenna College at rose.claremontmckenna.edu/redistricting.asp.) Opponents described the proposition as a “power grab by the politicians” in a series of ads that were condemned by many observers as unusually deceptive (for example, “the politicians are lying to you about Prop. 77” in the *Sacramento Bee* (Weintraub, 2005) and “commercials distort redistricting proposition” in the *San Diego Union-Tribune*).

on the social policy decisions in the sample. Whether or not the initiative is available is the only identifiable factor that seems to influence policy choices.

The paper proceeds as follows. The next section identifies the social issues to be studied and discusses the data collection. Section 3 summarizes the social policy initiatives that have appeared on statewide ballots over the last century. Section 4 contains the main empirical results on how the initiative affects policy choices and congruence, as well as evidence on the importance of institutional features. Section 5 concludes with a discussion of implications.

2. Data and Social Issues

The analysis requires information on social policies and public opinion for each state. Since state-by-state public opinion data on individual issues is the primary constraint, I began by searching the codebooks for the American National Election Studies (ANES) from 1988 to 2004, and identified all survey questions that asked about specific social policies that were decided at the state level. This yielded a set of seven social issues, listed in Table 1 together with a summary of public opinion on each issue. It is worth emphasizing that the sample studied in this paper is not a list of all social issues, but rather a list of all social issues with statewide opinion data in the ANES, so the results may or may not generalize to other issues.⁵

After identifying the set of issues with available opinion data, I collected information on each state's policy choice for each issue. For each issue, I consulted at least two sources, and when discrepancies arose, consulted additional sources including state constitutions and statutes. Some states changed their laws in the last decade. This study uses the laws in place at the time the data were collected, the middle of 2006. Because opinion data are usually not available for multiple years and in any case show

⁵ The ANES is designed to be representative at the national but not at the state level. If responses in a predominantly rural state are drawn exclusively from the state's single metropolitan area, the measured opinion may be biased. To alleviate this problem, where possible I relied on responses from the 1988-1992 Senate study that is designed to be representative at the state level.

little variation in the majority's position over time, I do not attempt to exploit the modest amount of time series variation in state policies.

For each state, I calculated the percentage of respondents for and against each policy, ignoring "don't know" and "decline to state responses." When a question was asked in multiple years, I combined all responses into a single sample. This worked well for about two-thirds of the observations. For the remaining observations where the ANES had too few responses to yield a reliable estimate of state opinion, I imputed opinion based on the state's general ideology, using coefficients from a regression that employed data from the other states. The details are reported in the appendix.

The key variable is whether or not a state allows initiatives, taken from Matsusaka (2004, Appendix 1). The only state to change its initiative status in the last 30 years is Mississippi, which adopted the process in 1992. Following standard practice, I classify Illinois as a noninitiative state because initiatives there can only be used to amend one narrow section of the constitution that concerns government structure. Other information on the data sources and variable definitions is reported in the appendix.

3. Propositions Decided by the Voters

To provide context, I searched the Initiative and Referendum Institute's *Initiatives Historical Database* (version 2006-5) that lists all statewide initiatives since the first one in Oregon in 1904, and extracted every initiative related to the social issues identified from the ANES. The measures are listed in Table 2. Since one purpose of collecting this information is to provide background information that might be useful to other scholars, I included measures that were related to the general subject of the ANES issues, even if the connection was somewhat loose. For example, since the ANES contained questions about specific abortion policies, Table 2 lists all initiatives related to abortion, not just those related to abortion issues included in the ANES (partial-birth abortion, parental consent, and public funding). The list includes only initiatives, new laws proposed by citizens, but is a complete historical list of initiatives on these topics. Propositions that were placed on the ballot by the legislature (legislative measures) or were attempts to repeal existing laws (referendums) are not included (no existing database contains the information necessary to compile a list for legislative measures and referendums.)

Table 2 reveals that a total of 68 initiatives have come before the voters since 1904. Of these, 22 concerned abortion, 13 concerned death penalty, 11 concerned English-only, 10 concerned job discrimination based on sexual orientation, and 12 concerned the definition of marriage. Most of these propositions are modern, in the sense that they appeared in the last two decades. The exception is death penalty, which was the subject of an initiative as far back as 1912 (in Oregon). Death penalty initiatives were common early in the twentieth century, and reemerged in the 1970s after the U.S. Supreme Court permitted the practice once again. Initiatives on these social policies appeared in a total of 18 states (out of the 24 that permit initiatives), but most were in Oregon (16), California (16), and Colorado (9), three of the leading initiative states.

Table 3 provides a summary of the ideological orientation of the outcomes. Each proposition was classified according to whether a “conservative” or “liberal” outcome prevailed, using conventional assignments (that is, “conservative” outcomes were defined as opposition to partial-birth abortion, opposition to public funding of abortion, opposition to anti-discrimination laws based on sexual orientation, opposition to same-sex marriage, support for parental consent for a minor’s abortion, support for the death penalty, and support for English-only laws). These assignments are for convenience and could be argued – there may be conservatives who favor anti-discrimination laws or liberals in favor of parental consent for abortion, for example. Table 3 shows that conservative (so-defined) outcomes prevailed 62 percent of the time. Conservative outcomes were most likely on same-sex marriage (92 percent), English-only (91 percent), and death penalty (85 percent). Liberal outcomes were most likely on abortion (62 percent) and anti-discrimination laws pertaining to sexual orientation (60 percent). While Table 3 suggests a rough balance between conservative and liberal outcomes, this information is only suggestive because it does not include indirect effects and does not indicate which outcomes prevailed when initiatives were not available. Moreover, Table 3 does not take into account public opinion – we would like to know how initiatives affect policy after controlling for public opinion.

Table 3 also shows that placing a measure on the ballot does not guarantee it will pass. Only 56 percent of these initiatives passed overall. This is well above the historical

pass rate of 41 for all initiatives during the period 1904-2006 (see *Initiative Use, 1904-2006*, published by the Initiative and Referendum Institute, www.iandrinstitute.org).

4. Empirical Results

A. *How the Initiative Affects Whether Policies Are Conservative or Liberal*

The first set of estimates explores the relation between social policy outcomes and direct democracy with a set of logistic regressions of the form:

$$(1) \quad \ln\left(\frac{\Pr(\text{outcome}_{is} = \text{conservative})}{\Pr(\text{outcome}_{is} = \text{liberal})}\right) = a + bI_{is} + cX_{is} + u_{is},$$

where i indexes one of the seven issues, s indexes one of the 50 states, I is a dummy variable equal to one if the state allows initiatives, X is a set of control variables, u is an error term, and a , b , and c are parameters to be estimated. Each issue has two possible outcomes that are classified as “conservative” or “liberal,” as discussed above.

The model in equation (1) is common in the literature, and approaches the data in a fairly agnostic way. The critical coefficient is b , which indicates whether policy choices are different between initiative and noninitiative states on average, controlling for the X variables. If the initiative pushes policy in different directions in different states (to the left in some states and to the right in other states), then the effects may cancel out in aggregate, resulting in a coefficient of b close to zero. This means that b is biased toward zero, which works against finding a significant effect, and strengthens confidence in an initiative effect if we observe a nonzero coefficient.

For the most part, I use the same set of control variables throughout the paper. Much of the previous literature has relied either on crude economic and demographic control variables or general measures of state ideology that are not directly connected to the issue being studied. One innovation of this study is to include specific measures of opinion for each issue studied together with a set of demographic and regional variables. The first variable in X is the fraction of the population in favor of the conservative outcome on the issue in question. If policy responds to opinion, then conservative outcomes should be more likely as conservative opinion grows, and conversely. The

second variable is the state's population (as a logarithm). Policy outcomes might be different in large states than small states because citizen monitoring of elected officials is subject to greater free rider problems, and because politicians might find it more difficult in large states to determine public preferences due to a greater distance between representatives and their constituents. The third control variable is the fraction of the state's adult population with a high school degree, included to capture the possibility that the educated have different information and preferences, and are more engaged in politics.⁶ The regressions also include a dummy variable for southern states. A southern dummy is standard fare in regressions using states as the unit of observation, and usually has explanatory power, suggesting it captures something not controlled by the other variables, but what that something is, is not clear. Another control variable with a similar motivation is the number of years since the state entered the Union ("age" of the state), also included to capture aspects of the state's political environment that the other variables do not. The last control variable is a dummy equal to one if a state's judges must stand for re-election instead of being appointed for life or reappointed by the governor, legislature, or a commission. As shown in Hanssen (1999) and Besley and Payne (2006), elected judges make different decisions than appointed judges. Finally, to control for issue-specific effects, the regressions include seven dummy variables, one for each issue; those coefficients are not reported in order to conserve space.⁷

Table 4 reports the estimates. Each column is a regression. Column (1) pools the observations for all seven issues and 50 states. The coefficient of 1.07 on the initiative dummy indicates that initiative states are more likely to choose the conservative outcome than noninitiative states, and the effect is different from zero at better than the 1 percent level. The size of the effect is not small. If all control variables are set at their mean

⁶ I also estimated the regressions with an income variable, which did not change the initiative coefficients in a material way, and seemed to be capturing the same thing as the education variable.

⁷ I also estimated the regressions with variables describing the partisan composition of the legislature and legislative professionalism. The initiative coefficients have similar signs and significance. I do not include these variables in the final regressions because they are endogenous with respect to opinion, region, and (in the case of professionalism) availability of the initiative process, and do not lead to conclusions that are different in substance. I also explored variables that captured the racial composition of the state. They were also generally insignificant, perhaps because the issues under investigation are not connected to race..

values, the marginal effect of the initiative ranges from 2 percent when the issue is job discrimination to 27 percent when the issue is death penalty, with a median across all issues of 18 percent (for parental consent laws). These findings are interesting in two respects. First, they show that the initiative is not just a veil – it appears to bring about materially different social policies. Second, the influence of the initiative appears to work in a conservative direction, making a conservative outcome 18 percent more likely for the median issue when the initiative is available than when the legislature alone sets policy.

As for the control variables, public opinion is highly significant, and the positive coefficient indicates that policies respond to opinion at the margin, confirming the findings of Erikson et al. (1993) and others. Large states tend to adopt more liberal policies. Education is associated with more liberal policy choices, but the coefficients are not statistically different from zero. Southern states and older states are significantly more likely to adopt conservative outcomes. Elected judges also are associated with more conservative outcomes. The fact that most control variables are highly significant highlights the importance of including such controls in policy regressions, and suggests that models without such controls are misspecified in a potentially important way.

Because initiative states are disproportionately in the West, it is natural to wonder if the initiative coefficient might be capturing a Western effect instead of an initiative effect. One way to test for this is to estimate the regression without the Western states. Column (2) reports the coefficients and errors when the model is estimated on the subsample of non-Western states. As can be seen, the initiative coefficient is even larger when Western states are not included, and remains distinguishable from zero at better than the 1 percent level. Thus, we can be reasonably confident that the initiative dummy is not a proxy for the West.

Some studies (e.g. Gerber, 1996) expand regression (1) by adding a variable that interacts the initiative dummy with public opinion. Such a specification allows the influence of public opinion to be different (at the margin) in initiative and noninitiative states.⁸ Column (3) reports a model that includes an interaction variable. The coefficient

⁸ Contrary to what is sometimes claimed in the literature, the coefficient on the interaction term does not reveal whether policy is more congruent with public opinion in initiative or noninitiative states. See Erikson et al. (1993), Matsusaka (2001), and the discussion in section B below.

on the interaction variable is small and far from conventional levels of statistical significance, suggesting that including this term does not help explain policy choices on these issues. If b_1 is the coefficient on the initiative dummy and b_2 is the coefficient on the interaction variable, then the full effect of the initiative in this specification is

$$(2) \quad \text{Full Effect} = b_1 + b_2 \times \text{OPINION},$$

where *OPINION* is a specific value of the conservative opinion variable. When (2) is calculated using the coefficients in column (3), the full effect is positive for all opinion levels in the sample, and significantly different from zero at the 10 percent level or better for values of conservative opinion between 30 percent and 90 percent (full statistics not reported). In short, allowing for the interaction term does not change the basic message that the initiative changes social policy, and in a conservative direction.

I also estimated, but do not report, regressions for each issue separately. The initiative coefficient is positive and at least 0.76 for six of seven issues (the exception being -0.13 for partial-birth abortion), and the coefficient can be distinguished from zero at the 10 percent level or better for three issues, parental consent for abortion, public funding of abortion, and English-only. The abortion results update and tend to confirm the findings of Gerber (1996, 1999), Arceneaux (2002), and Bowler and Donovan (2004) that abortion policies are more conservative (or “restrictive”) in initiative than noninitiative states. Public opinion is also positively related to outcomes in every regression, and significantly so in four regressions. With only 50 observations per regression, the models lack power, but the results suggest that the initiative effect seen in the full sample is not driven by a single issue.

To summarize, the evidence suggests that social policy choices are significantly different between initiative and noninitiative states, even after controlling for opinion and other factors that predict policy outcomes. Moreover, the policies in initiative states are more conservative than those in noninitiative states, again holding constant public opinion and other factors. It is important to recognize that the evidence in Table 4 does not reveal whether policies are more congruent with opinion in initiative or noninitiative states. Nor can the evidence be used to conclude that the initiative biases policy in a

conservative direction. One interpretation is that the initiative creates a conservative bias, but an alternative possibility is that legislatures have a liberal bias that the initiative simply “corrects” by pulling policy back (in a conservative direction) toward the median position. To assess the potential policy bias of the initiative requires a different empirical approach, which is the focus of the next section.

B. How the Initiative Affects Congruence

A major controversy surrounding the initiative process is whether it enhances popular control of public policy, or empowers special interests. Some previous studies have attempted to measure the congruence of policy and opinion (or “responsiveness”) with regressions of the form

$$(3) \quad y_{is}^* = a + bI_{is} + cX_{is} + d_I \lambda_s y_{is}^o + d_N (1 - \lambda_s) y_{is}^o + u_{si},$$

where y^* is the policy outcome, y^o is a measure of public opinion on issue i in state s , and λ is an indicator variable equal to 1 if the state allows the initiative and zero otherwise. The coefficients d_I and d_N indicate the marginal responsiveness of policy to opinion in initiative and noninitiative states, respectively, and some studies have argued that $d_I > d_N$ implies that policy is more congruent with opinion in initiative states than noninitiative states.⁹ While seemingly intuitive, such an inference is not valid, as discussed in Erikson, Wright, and McIver (1993, pp. 92-94) and Matsusaka (2001, 2007). Matsusaka (2001) emphasizes the situation where the opinion measure y^o is in a different metric than the policy outcome (for example, when opinion is a measure of general liberalness and y is a specific policy), but the criticism applies to cases where y^o is a direct measure of opinion as well. I will not restate the argument here in detail, but

⁹ The formulation in (3) is formally equivalent to the common practice of including a variable that interacts opinion with the initiative dummy variable, in which case it has been argued that a positive coefficient on the interaction term implies greater responsiveness.

Figure 1 gives the main intuition.¹⁰ The heavy 45 degree line shows the relation that would prevail between opinion and policy if policy choices were completely congruent with opinion, that is, if $y^* = y^o$ (more generally, one can think of (non)congruence as $\sum |y_{is}^* - y_{is}^o|$, as in Gerber (1996)). The cluster of points X represents opinion-policy observations in one group of states and the cluster labeled Z represents observations in another set of states, where one group allows the initiative and the other does not. The policies in group X are clearly less congruent with (more distant from) what the public wants than the policies in group Z . However, if model (3) is estimated for these observations, we would find $d_x > d_z \approx 0$, falsely suggesting that policies are more congruent in group Z . It is straightforward to show that *any* pattern of coefficients for d_x and d_z can arise regardless of whether some group of X states is more or less congruent than some group of Z states. The problem, in words, is that the regression coefficients characterize the *slope* of the policy-opinion relation while congruence depends on the *distance* between policy and opinion. In short, estimates of d from models like (3) cannot be used to compare congruence or responsiveness of policy to opinion across states.¹¹

Since (3) cannot be used to study congruence (or bias, for the same reason), I approach the problem in a different way using a nonparametric method developed in Matsusaka (2007). For each issue i and state s , I construct a measure of “congruence,” C_{is} , where

$$(4) C_{is} = \begin{cases} 1 & \text{if state } s \text{ chooses the outcome preferred by the majority on issue } i; \\ 0 & \text{otherwise.} \end{cases}$$

¹⁰ The following example is adapted from Matsusaka (2001), which essentially elaborates an argument in Erikson et al. (1993). Romer and Rosenthal (1979) observe a related problem in tests of the median voter model that measure preferences with linear combinations of economic and demographic variables. Achen (1977) identifies a different problem with approaches based on (3).

¹¹ Although this example works with a policy variable that takes on a continuum of values, the same point applies if policy outcomes are discrete and model (3) is a probit or logit.

State-by-state opinion data on each issue are taken from the ANES. Because each issue is defined in a dichotomous way in the survey, there is a well-defined majority position and the majority position is also the position of the median citizen. Thus, $C_{is} = 1$ means that state s has adopted the majority/median position on issue i , while $C_{is} = 0$ means the state has adopted the minority's choice, which is not the median position.

Figure 2 reports the fraction of congruent outcomes in initiative and noninitiative states by issue, to provide context for the regressions that follow. Outcomes are more congruent in initiative than noninitiative states for six of seven issues, the exception being laws protecting workers from discrimination on the basis of sexual orientation. For the seven issues overall, congruence is 63 percent in initiative states and 58 percent in noninitiative states. None of the differences are statistically different from zero at conventional levels of significance.

The empirical strategy is to estimate how the initiative and other factors affect the likelihood of a congruent outcome:

$$(5) \quad \ln\left(\frac{\Pr(\text{outcome}_{is} = \text{congruent})}{\Pr(\text{outcome}_{is} = \text{not congruent})}\right) = a + bI_{is} + cX_{is} + u_{is}.$$

Because model (5) utilizes a “direct” measure of congruence, it is not subject to the inference problems associated with model (3). Table 5 reports the estimates of model (5). As before, each column presents estimates from a single regression that pools all seven issues. The control variables are the same as in Table 4, except that instead of conservative opinion, Table 5 includes a variable equal to the size of the majority (regardless of whether it is a conservative or liberal majority). Each regression includes seven issue-specific dummy variables (and no intercept) whose coefficients are not reported.

The estimates in column (1) are based on the full sample. The coefficient on the initiative dummy is 0.57 and different from zero at about the 7 percent level. Converted into a marginal probability at means of the control variables, the estimates imply that initiative states are from 5 percent (parental consent) to 14 percent (partial-birth abortion ban) more likely than noninitiative states to be congruent, with a difference of 8 percent

for the median issue (English only). Column (2) reports the same model estimated on a sample that excludes the Western states to make sure that the initiative effect is not a proxy for the West. The initiative coefficient rises to 0.68, and remains significantly different from zero at about the 7 percent level.¹² For these seven social issues taken as a group, therefore, the evidence suggests that initiatives make public policy more responsive to popular opinion. There is no evidence for the view that initiatives allow special interest groups to bring about policies disliked by the majority. The evidence is thus consistent with less direct evidence on fiscal policies in Matsusaka (2004).

The specification in columns (1) and (2) assume that the initiative's effect on congruence is unbiased in the sense that it does not depend on whether the majority is conservative or liberal. The regression in column (3) allows different initiative coefficients for conservative and liberal majorities. This provides a way to examine if the process displays a bias in one direction. The regression also includes a dummy variable equal to one for a conservative majority to allow for the possibility that conservative or liberal majorities have more influence independent of the initiative. The coefficient on the initiative dummy when there is a conservative majority is 0.91, different from zero at about the 1 percent level of statistical significance. In contrast, the coefficient on the initiative dummy when there is a liberal majority is small and nowhere close to being distinguishable from zero at conventional levels of significance. The two initiative coefficients are different from each other at about the 12 percent level of significance ($p = .118$), outside the normal bounds of statistical significance, but suggesting that the initiative may help conservative majorities more than it helps liberal majorities. The coefficient of 1.25 on the conservative majority dummy implies that congruent policies are more likely for conservative than liberal majorities even in the absence of the initiative. I am not aware of any theoretical reason why this should be so, and it presents an interesting empirical puzzle. In any case, although the initiative effect is less precisely estimated for liberal than conservative majorities (due perhaps to the fact that only 23

¹² As another robustness check, I estimated but do not report the model on a sample that excludes the observations where public opinion was imputed. In this sample, with 236 observations, the coefficient on the initiative dummy was 0.62, and just shy of significance at the 10 percent level.

percent of the observations feature a liberal majority), the estimates seem to suggest that the initiative brings about congruent outcomes mainly when the majority is conservative.

To develop a more textured picture of the connection between congruence and the initiative, I also estimated but do not report regressions for each issue separately. The coefficient on the initiative dummy was positive for six of seven issues, and different from zero at conventional levels of significance for parental consent for abortion and English-only laws. The inability to distinguish the other four coefficients from zero could be because the regression contains only 50 observations or because there is no relation between congruence and the initiative to begin with. In any event, the basic picture remains that the initiative process tends to increase the congruence between public opinion and public policy. The only negative relation between congruence and the initiative appears for anti-discrimination laws in employment. For that sample, the coefficient on the initiative dummy is -1.99, different from zero at better than the 10 percent level of significance. Taken at face value, the coefficient on this issue implies that direct democracy may allow nonmajority groups to bring about policies disliked by the majority. Because all but one state have liberal majorities on this issue, the nonmajoritarian bias pushes in a conservative direction, that is, direct democracy states are less likely to adopt job protection laws even when they are supported by the majority.

The control variables also reveal some interesting things about congruence. Not surprisingly, as the size of the majority increases, congruence is more likely. When opinion becomes sufficiently one-sided on these issues, the majority gets its way. Less obviously, the coefficients on the South dummy and years since a state entered the Union are positive and statistically different from zero. Southern states and older states appear to choose more congruent laws. Congruence is also higher when judges are elected than when they are appointed, suggesting that elected judges are less willing to buck the majority. Population and education are not reliably associated with congruence.

C. Institutional Features

Theory and a healthy empirical literature (e.g. Matsusaka, 2004; Feld and Matsusaka, 2003; Bowler and Donovan, 2004) show that policy outcomes may depend not only on availability of the initiative process but also on features of the initiative

process. The initiative is different in every state, and those differences may determine how big an impact it has on policy. This section examines several institutional features that have interested scholars and reformers to see which are most important in practice.

The feature that has received the most attention is the number of signatures required to place a measure on the ballot. Requirements range from a low of 2 percent of voters in North Dakota to a high of 15 percent in Wyoming. As the signature requirement rises, the initiative is more costly to use, and its impact is likely to decline. Evidence for fiscal policies suggests that initiative states choose increasingly lower taxes and spending than noninitiative states as the signature requirement rises (Matsusaka, 1995, 2004).¹³ There does not appear to be any related evidence on social policies, or on congruence. To test whether the signature requirement is an important institutional feature, Table 6 reports logistic regressions in which the initiative effect is allowed to vary with the signature requirement. The dependent variable in column (1) is the likelihood of a conservative policy choice (as in Table 4) and the dependent variable in column (3) is the likelihood of a congruent outcome (as in Table 5). If a state allows both statutory and constitutional initiatives, I use the lowest signature requirement of the two. The signature requirement variable takes a value of zero for noninitiative states so is essentially an interaction term between the initiative dummy and the signature requirement.

The coefficient on the signature requirement variable is negative in columns (1) and (3), consistent with the idea that higher requirements reduce the initiative's impact. However, the coefficients are quantitatively small and far from statistical significance. With an interaction term, the full effect of the initiative is a linear combination of the initiative dummy and signature requirement coefficients: if b_1 is the coefficient on the initiative dummy and b_2 is the coefficient on the signature requirement variable, then the full effect of the initiative is $b_1 + b_2 \times (\text{SIGNATURE REQUIREMENT})$. The lower rows of Table 6 report the p -value for the hypothesis that the full effect is equal to zero for various signature requirements in the sample. For policy choice, the initiative effect is significantly different from zero for all signature requirements. For congruence, the

¹³ Several studies have found that more initiatives reach the ballot when the signature requirement is low, for example, Magleby (1984) and Matsusaka and McCarty (2001).

initiative effect is different from zero at the 10 percent level or better for a signature requirement of 5 percent and 8 percent (the most popular requirements) but not for a signature requirement of 10 percent. In short, initiative states choose significantly more conservative and congruent policies than noninitiative states, but the difference does not seem to depend on the signature requirement in an important way.

Another difference in initiative rules is whether ballot propositions can be used to amend the constitution or only to pass statutes. We might expect that constitutional initiatives have a larger effect because they cannot be overturned by courts for violating the state constitution. The regressions reported in columns (2) and (4) include separate dummy variables for states that allow constitutional initiatives (17 states) and states that allow only statutory initiatives (6 states). The constitutional and statutory initiative coefficients are similar to each other in magnitude in both regressions. The last row of the table reports the *p*-value for the hypothesis that the coefficients are equal, a hypothesis that cannot be rejected at conventional levels of significance. Here again, it seems that availability of the initiative and not its form is of paramount importance.

Instead of examining specific initiative features, an alternative approach is to employ indexes that combine several features at the same time. The virtue of such an approach is that it allows the researcher to consider numerous features simultaneously without consuming too many degrees of freedom. The down side is that indexes are typically ad hoc – they combine features using arbitrary weights and assume the effects are additive – and they obscure the precise features that might be important, limiting their usefulness to policymakers. Given the negative results in Table 6, it seems worthwhile to explore if stronger effects emerge when indexes are employed.

To do so, I employ two indexes constructed by Bowler and Donovan (2004). The “qualification difficulty index” takes on values from zero to six, where high scores mean it is more difficult to qualify an initiative for the ballot. The “legislative insulation index” takes on values from one to nine where high scores indicate that initiatives are less able to constrain the legislature.¹⁴ Table 7 reports estimates of models specified in equations (1)

¹⁴ Specifically, the qualification difficulty index gives one point for each of the following conditions that hold: (i) the state does not allow both statutory and constitutional initiatives, (ii) the time allowed to collect signatures is limited, (iii) signatures must be geographically distributed across the state, (iv) the signature

and (5) in which the two indexes are added as explanatory variables. The dependent variable in columns (1)-(3) is the likelihood of a conservative policy outcome. Three out of four index coefficients are negative, consistent with the idea that the initiative effect is attenuated as the process becomes more difficult to use, but the coefficients are small in magnitude, and never close to statistical significance. Inclusion of the index variables also has no material effect on the coefficient on the initiative dummy (or the other coefficients, either).¹⁵ A similar picture emerges in columns (4)-(6) where the dependent variable is the likelihood of a congruent outcome. Again three out of four index coefficients are negative, but they are quantitatively small and never close to statistical significance. The other coefficients are not changed in an important way when the indexes are included. In short, neither index seems to offer an improvement in explanatory power. The critical variation between initiative and noninitiative states on these issues appears to be captured by the dummy variable.¹⁶

The evidence in this section as a whole is largely negative: I am unable to discover any evidence that institutional details affect the impact of the initiative on policy outcomes. The only thing that seems to matter is whether or not a state has the initiative

requirement exceeds 7 percent, (v) the signature requirement exceeds 10 percent, (vi) the subject matter of initiatives is limited. The “legislative insulation index” gives one point for each of the following conditions that hold: (i) the state has a single-subject rule, (ii) the subject matter is limited, (iii) fiscal initiatives are limited, (iv) the legislature can amend or repeal a statutory initiative, (v) the legislature can repeal initiative statutes without a waiting period, (vi) the legislature can repeal a statutory initiative without a supermajority, (vii) the state does not allow constitutional initiatives, (viii) the state allows both direct and indirect initiatives, (ix) the state allows indirect but not direct initiatives. See the appendix of Bowler and Donovan (2004).

¹⁵ At first glance, these estimates might seem to contradict the finding in Bowler and Donovan (2004) that the indexes are significantly related to abortion policy “restrictiveness.” However, Bowler and Donovan do not report estimates including both the initiative dummy variable and the indexes at the same time in order to determine if the indexes add information beyond the dummy variable. If I estimate the regressions using the indexes without the dummy variable, the indexes are statistically significant.

¹⁶ I also estimated but do not report regressions that included a term interacting the indexes with public opinion. Those coefficients were never statistically significant either. I should also note that the full effect of the initiative on congruence continues to be positive and statistically different from zero at about the 8 percent level in the congruence regression for some values of the indexes.

at all. One explanation for this finding could be that these institutional details do matter, but the regressions lack the power to identify their effects. Alternatively, these factors may be fairly unimportant in practice. At any rate, pending other evidence, it seems that researchers studying social policies will lose little if they employ the conventional initiative dummy variable approach instead of attempting to make fine distinctions in the institutional features.

5. Discussion

One of the main messages from this study is that the initiative brings about significant changes in state social policies. Based on the policy choices of all 50 states across seven different social issues, I find that states with the initiative process are approximately 18 percent more likely to choose conservative policy outcomes than noninitiative states after controlling for public opinion, demographic, and regional factors. A large empirical literature documents that the initiative process brings about systematic changes in state fiscal policies (Lupia and Matsusaka, 2004; Matsusaka, 2005). The evidence in this paper lays down a complementary empirical foundation for research on social issues, by showing that the initiative matters, and in a systematic way. Similarly, the findings here confirm and extend the handful of previous studies on individual social issues that also suggest the initiative matters.

A second important message is that the changes brought about by the initiative tend to push policy in a majoritarian direction: initiative states are about 8 percent more likely than noninitiative states to select the policy outcome favored by a majority of citizens. While critics have long argued that the initiative allows narrow groups to subvert the public will, the evidence here is consistent with the view that the initiative in fact empowers the majority. This finding reinforces the emerging view that direct democracy promotes the interests of the majority, as the Progressives intended (Matsusaka, 2004; Lupia and Matsusaka, 2004; Matsusaka 2007), and appears to be the first evidence of its kind for state social policies. The argument that direct democracy empowers special interests and allows them to subvert the majority seems increasingly untenable as a general proposition, although it may be true for isolated cases.

The normative implications of these findings are not obvious. The fact that the initiative promotes majority rule would seem to be a good thing, all else equal, since popular control of public policy is a central tenet of democracy (Erikson et al., 1993). However, as is well known, an unchecked majority could abuse its power and “tyrannize” the minority. Whether the initiative endangers minority rights is an empirical question, but one that has proven extremely difficult to examine, largely because of the difficulty distinguishing majority rule from majority tyranny (Matsusaka, 2004, Ch. 8). Is denial of same-sex marriage a legitimate exercise of majority rule or a violation of the civil rights of gays and lesbians? One approach to this issue, pioneered by Hajnal et al. (2002), is to determine how often minorities are overruled by the majority and whether they feel their rights are being undermined by the initiative process. The evidence in Hajnal et al. (2002) is comforting for the initiative process: racial and ethnic minorities are about as likely as white voters to be on the winning side on ballot propositions and a majority of blacks, Latinos, and Asians express support for the initiative process. The evidence in this study cannot reach the underlying issue of whether majority rule is good or bad, but the hope is that it might advance the discussion by providing an empirical groundwork for understanding how the initiative influences policy, and whose interests it advances.

This study also finds that conservative policies are more likely to prevail in initiative than noninitiative states. While at first glance this suggests that the initiative might have a conservative ideological bias, a more subtle interpretation might be appropriate. The evidence on policy congruence shows that while the initiative promotes majoritarian outcomes, it seems to help conservative majorities more than it helps liberal majorities. So it is unclear if the initiative process is loaded in favor of conservative *outcomes* or conservative *majorities*. Theoretical work on direct democracy does not yet offer a reason why the initiative process would display an ideological bias either way, so progress on this issue awaits further research. However, evidence on fiscal policy shows that the changes brought about by the initiative do not have a fixed ideological orientation over time – since the mid-1970s, the initiative has cut taxes and spending, but in the early 20th century it increased taxes and spending (Matsusaka, 2000). This suggests that the initiative’s perceived ideological effect may be induced by the changing ideological position of the majority.

A final set of findings concern the institutional features of the initiative process. Scholars and activists continue to be interested in ways to reform the initiative process. Reforms include changing the petition process, restricting subjects, and making it easier for the legislature to amend successful propositions, among other things (Garrett and McCubbins, 2007). This study fails to turn up evidence that any popular institutional features have a material effect on social policy choices. In particular, I fail to find significant policy effects associated with signature requirements, constitutional versus statutory initiatives, and indexes of petition difficulty and legislative insulation. It would be premature to dismiss the relevance of institutional features, particularly because they have been shown to matter for fiscal policies, but the evidence in this study suggests that they may be of secondary importance. For reformers, this suggests that tweaking the initiative's features is unlikely to result in a material change in policies. For scholars, it suggests that the standard "dummy variable approach" in which states are simply distinguished by whether or not they allow initiatives may be an acceptable approximation for many purposes.

Appendix. Data Sources

Abortion laws. Does the state prohibit certain “late-term” or “partial-birth” abortions? Primary sources were Guttmacher Institute (2006) and Center for Reproductive Rights (2004). Does the state require consent of parents or legal guardian before a minor has an abortion? Primary sources were NARAL Pro-Choice America (2006), Kaufmann (2004), and individual state pages on the web site of the Guttmacher Institute (www.guttmacher.org). If a law was struck down by a court but not replaced by one that passed constitutional muster, the state was coded as not having a law. Does the state use its own funds to pay for all or most medically necessary abortions for Medicaid enrollees? Primary sources were Guttmacher Institute (2006) and NARAL Pro-Choice America (2006).

Death penalty laws. Did the state permit capital punishment for some crimes? States were coded based on information in Peterson (2005a) and Death Penalty Information Center (2006). Illinois has had a moratorium since 2000 and New Jersey has a moratorium until 2007, but they were counted as death penalty states since the moratoriums are presumably temporary.

English-only laws. Did the state have a law declaring English its official language? Primary sources were U.S. English, Inc. (2005) and ProEnglish (no date). Bilingual states Hawaii (English and Hawaiian), Louisiana (English and French), and New Mexico (English and Spanish) were not counted as English-only

Employment anti-discrimination laws based on sexual orientation. Does the state prohibit job discrimination in the private sector on the basis of sexual orientation? The primary data source was Human Rights Campaign Foundation (2005, 2006). Oregon’s law does not contain the phrase “sexual orientation” but courts have interpreted it to prohibit such discrimination since *Tanner v. Oregon Health Sciences University*, 1988, so Oregon is classified as having a prohibition. Washington’s public sector anti-discrimination law was extended to cover private sector employers in June 2006.

Same-sex marriage laws. Did the state have a law or constitutional amendment defining marriage as solely between a man and a woman? Primary sources were Peterson (2005b) and Traditional Values Coalition (2005). State law was consulted directly to resolve discrepancies. Maryland’s ban was struck down by a judge in January 2006, but the decision was stayed pending appeal. I classified it as having a law. Vermont passed legislation recognizing gay civil unions that was called “quasi gay marriage” but at the same time defines marriage as only between a man and a woman. I classified it as having a law.

Public opinion. Public opinion data were assembled from various years of the American National Election Studies, as discussed in the text and Table 1. Missing values were imputed by estimating a regression $I_s^{ANES} = \alpha + \beta I_s^{BERRY} + u$ for those states with reliable opinion information (typically defined as states with 60 or more observations), where I_s^{ANES} is the ANES opinion score of state s and I_s^{BERRY} is the state’s general ideology index as constructed by Berry et al. (1998). Then, for the states with missing ANES information, I used the estimated values of α and β together with the state’s index score from

Berry et al. to impute an ANES score. I also tried imputing scores using the ideology measure in Erikson et al. (1993), with very similar results. See Matsusaka (2007) for more information.

Other control variables. Initiative status and features were taken from Matsusaka (2004, Appendix 1). Population in 2003 is from the Bureau of the Census. Education is the fraction of the population over the age of 25 with a high school degree as of 2000, taken from *Statistical Abstract of the United States*, 2006, Table 218. The South dummy is equal to one for Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia. The Western states, following the census classification, are Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington, and Wyoming. Judicial selection data were taken from American Judicature Society (2004), Hanssen (2004), and Besley and Payne (2006), and describe the situation in 2004. No state changed its procedures in a way that changed its classifications during 1990-2006 except Tennessee, which changed from initial election to initial appointment in 1994. Classifications apply to the court of last resort. Nine states (AL, GA, ID, KY, MN, MT, NV, ND, WI) fill midterm vacancies by appointment and New Mexico fills vacancies by appointment, but all require elections shortly thereafter, typically at the next general election. Following Besley and Payne (2006), I classified these states as using elections to make the initial selection.

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Figure 1. Hypothetical Observations of Policy Choices (y^*) and Public Opinion (y^o)

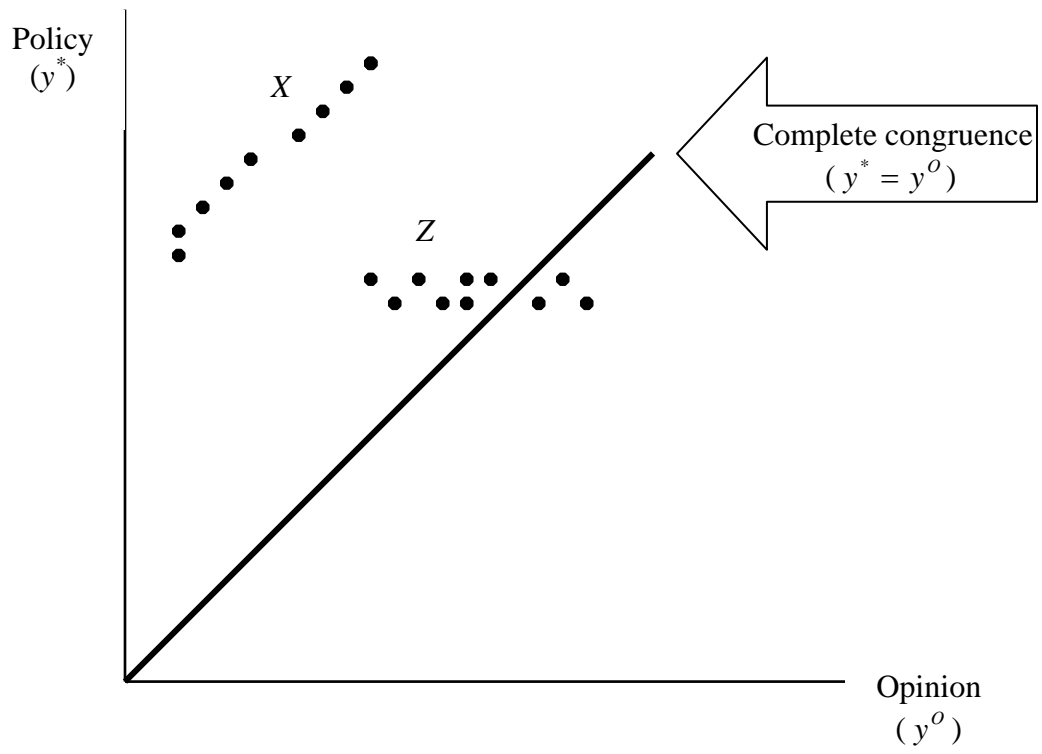


Figure 2. Percent of Initiative and Noninitiative States with Congruent Outcomes

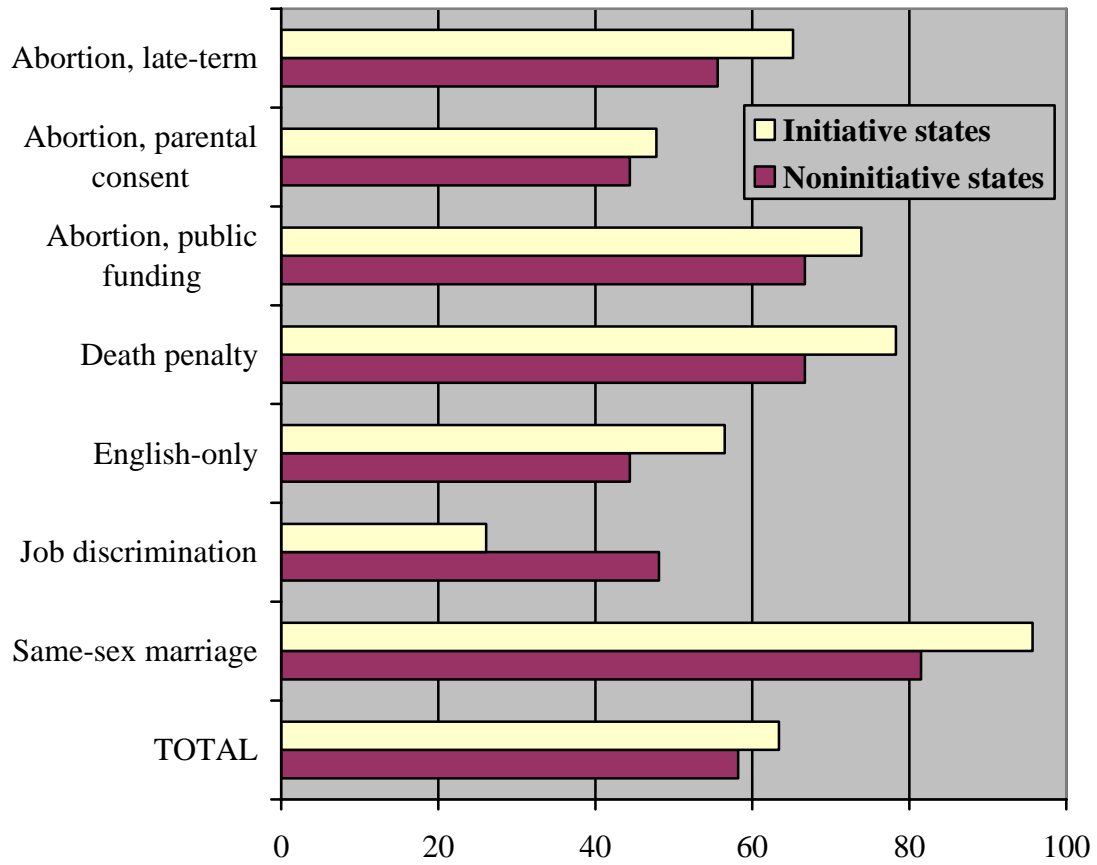


Table 1. Social Issues

Issue	Question	Percent in favor			Years
		Mean	Min	Max	
Abortion, partial-birth / late-term	“There has been discussion recently about a proposed law to ban certain types of late-term abortions, sometimes called partial birth abortions. Do you favor or oppose a ban on these types of abortions?”	67.2	41.5 (NM)	81.4 (IN)	1998, 2000, ‘04
Abortion, parental consent	“Would you favor or oppose a state law that would require parental consent before a teenager under 18 could have an abortion?”	74.4	58.9 (VT)	87.2 (WI)	1988, ‘90, 2000
Abortion, public funding	“Would you favor or oppose a law in your state that would allow the use of government funds to help pay for the costs of abortion for women who cannot afford them?”	49.4	35.7 (WI)	65.6 (NY)	1988
Death penalty	“Do you favor or oppose the death penalty for persons convicted of murder?”	78.0	66.7 (RI)	91.4 (OK)	1988, ‘90, ‘94, ‘96, ‘98, 2000, ‘04
English only	“Do you favor a law making English the official language of the United States, meaning government business would be conducted in English only, or do you oppose such a law?”	70.9	56.5 (OR)	80.8 (MN)	1990, ‘98, 2000
Job discrimination, homosexuals	“Do you favor or oppose laws to protect homosexuals against job discrimination?”	65.4	38.0 (KS)	79.4 (HI)	1990, ‘94, ‘96, 2000, ‘04
Same-sex marriage	“Should same-sex couples be allowed to marry, or do you think they should not be allowed to marry?” Responses: 1=Allowed. 5= Not allowed. 7=Not allowed to marry, but civil unions allowed. (“In favor” = response 1)	32.9	11.5 (AL)	49.1 (IL)	2004

Note. “Question” is the precise question asked in the American National Election Studies survey. “Year” is the study year, except that 1988 refers to the 1988-1992 ANES Pooled Senate File. Statistics for “Percent in favor” were computed with the state as the unit of observation. The identity of extreme states is reported in parentheses beneath the values.

Table 2. Complete List of Abortion, Death Penalty, English-only, Job Discrimination, and Same-Sex Marriage Initiatives, 1904-2006

State	Year	Measure	C/S	Proposal	Result
<i>Abortion</i>					
Alaska	1982	Measure 6	S	Prohibits public funding of abortion	F 41
Arizona	1992	Prop 110	C	Prohibits abortion except to save mother's life, prohibits public funding of abortion	F 31
Arkansas	1986	Amendment 65	C	Limits public funding of abortion	F 49.96
Arkansas	1988	Amendment 3	C	Prohibits public funding of abortion, bans abortion if permitted by federal courts	A 52
California	2005	Prop 73	C	Requires parental notification of a minor's abortion	F 47
California	2006	Prop 85	C	Requires parental notification of a minor's abortion	F 46
Colorado	1984	Amendment 3	C	Prohibits public funding of abortion	A 50.4
Colorado	1988	Amendment 7	C	Restores public funding of abortion	F 40
Colorado	1998	Amendment 11	C	Prohibits partial-birth abortions	F 49
Colorado	1998	Amendment 12	C	Requires parental notification of a minor's abortion	A 55
Colorado	2000	Amendment 25	C	Requires 24-hour wait before an abortion is performed	F 39
Maine	1999	Question 1	S	Prohibits partial-birth abortions	F 44
Michigan	1972	NA	C	Legalizes abortion	F 39
North Dakota	1972	NA	S	Legalizes abortion	F 23
Oregon	1978	Measure 7	C	Prohibits public funding of abortion	F 48
Oregon	1986	Measure 6	C	Prohibits public funding of abortion	F 45
Oregon	1990	Measure 10	S	Requires parental notification of a minor's abortion	F 48
Oregon	1990	Measure 8	C	Prohibits abortion	F 32
Oregon	2006	Measure 43	S	Requires parental notification of a minor's abortion	F 45
Washington	1984	Initiative 471	S	Prohibits public funding of abortion	F 47
Washington	1991	Initiative 120	S	Establishes a right to abortion	A 50.1
Washington	1998	Initiative 694	S	Prohibits abortion.	F 43
<i>Death penalty</i>					
Arizona	1914	NA	S	Abolishes death penalty	F 48
Arizona	1916	NA	S	Abolishes death penalty	A 50.2
Arizona	1918	NA	S	Reinstitutes death penalty	A 66
California	1972	Prop 17	C	Institutes death penalty	A 68
California	1978	Prop 7	S	Death penalty for more crimes ("special circumstances")	A 71
California	1990	Prop 115	C	Death penalty for more crimes, limits discretion of judges, allows hearsay evidence	A 57

(Continued on next page.)

(Table 2 continued)

State	Year	Measure	C/S	Proposal	Result
<i>Death penalty (continued)</i>					
California	2000	Prop 21	S	Death penalty for gang-related murders	A 62
Oregon	1912	Measure 33	S	Abolishes death penalty	F 39
Oregon	1914	Measure 18	C	Abolished death penalty	A 50.04
Oregon	1978	Measure 8	S	Reinstitutes death penalty	A 64
Oregon	1984	Measure 6	C	Allows death penalty.	A 56
Oregon	1984	Measure 7	S	Convicted murderers must be given separate sentencing hearing before trial jury	A 75
Washington	1975	Initiative 316	S	Mandatory death penalty for first degree aggravated murder	A 69
<i>English only</i>					
Alaska	1998	Measure 6	S	Makes English official language of state	A 69
Arizona	1988	Prop 106	C	Makes English official language of state	A 51
Arizona	2000	Prop 203	S	Requires public school instruction be in English (end bilingual education)	A 63
California	1984	Prop 38	S	Requires voting materials to be only in English	A 71
California	1986	Prop 63	C	Makes English official language of state	A 73
California	1998	Prop 227	S	Requires public school instruction be in English (end bilingual education)	A 61
Colorado	1998	Amendment 1	C	Makes English official language of state	A 61
Colorado	2002	Amendment 31	C	Requires public school instruction be in English (end bilingual education)	F 44
Florida	1998	Amendment 11	C	Makes English official language of state	A 84
Massachusetts	2002	Question 2	S	Requires public school instruction be in English (end bilingual education)	A 68
Utah	2000	Initiative A	S	Makes English official language of state	A 67
<i>Job discrimination, homosexuals</i>					
California	1978	Prop 6	S	Permits firing of school teachers for homosexuality	F 42
Colorado	1992	Amendment 2	C	Repeals local laws banning discrimination based on sexual orientation	A 53
Idaho	1994	Prop 1	S	Prohibits laws banning discrimination based on sexual orientation	F 49.6
Maine	1995	Question 1	S	Prohibits laws banning discrimination based on sexual orientation.	F 47
Oregon	1988	Measure 8	S	Repeals law banning discrimination based on sexual orientation.	A 53
Oregon	1992	Measure 9	C	Government required to discourage homosexual "behaviors."	F 44
Oregon	1994	Measure 7	C	Prohibits discrimination based on sexual orientation.	F 43
Oregon	1994	Measure 13	C	Government cannot approve or create classifications based on sexual orientation.	F 48

(Continued on next page.)

(Table 2 continued)

State	Year	Measure	C/S	Proposal	Result
<i>Job discrimination, homosexuals (continued)</i>					
Oregon	2000	Measure 9	S	Public school instruction may not sanction or encourage homosexuality.	F 47
Washington	1997	Initiative 677	S	Prohibits discrimination based on sexual orientation.	F 40
<i>Same-sex marriage</i>					
Arizona	2006	Prop 107	C	Defines marriage as only between a man and a woman.	F 48
Arkansas	2004	Amendment 3	C	Defines marriage as only between a man and a woman.	A 75
California	2000	Prop 22	S	Defines marriage as only between a man and a woman.	A 61
Colorado	2006	Amendment 43	C	Defines marriage as only between a man and a woman.	A 55
Michigan	2004	Proposal 04-2	C	Defines marriage as only between a man and a woman.	A 59
Montana	2004	CI-96	C	Defines marriage as only between a man and a woman.	A 67
Nebraska	2000	416	S	Defines marriage as only between a man and a woman.	A 70
Nevada	2000	Question 2	C	Defines marriage as only between a man and a woman.	A 70
Nevada	2002	Question 2	C	Defines marriage as only between a man and a woman.	A 67
North Dakota	2004	Amendment 1	C	Defines marriage as only between a man and a woman.	A 68
Ohio	2004	Issue 1	C	Defines marriage as only between a man and a woman.	A 62
Oregon	2004	Measure 36	C	Defines marriage as only between a man and a woman.	A 57

Note. C = constitutional amendment, S = statute. Result: A = approved, F = failed. The number is the percent in favor.

Table 3. Initiative Outcomes, 1904-2006

Issue	Number		Percent conservative outcome	Percent failed
	Conservative outcome	Liberal outcome		
Abortion	6	16	38%	82%
Death penalty	11	2	85%	15%
English-only	10	1	91%	9%
Job discrimination, homosexuals	4	6	40%	80%
Same-sex marriage	11	1	92%	9%
TOTAL	42	26	62%	44%

Note. This table summarizes whether outcomes were “conservative” or “liberal” on the 68 initiatives listed in Table 2.

Table 4. Logistic Regressions Explaining Likelihood of a “Conservative” Law

	Full sample (1)	Without West (2)	Full sample (3)
Dummy = 1 if initiative available	1.07*** (0.35)	1.47*** (0.44)	1.07 (1.11)
Public opinion: percent in favor of conservative law	10.61*** (2.37)	15.65*** (3.54)	10.61*** (2.49)
Initiative dummy × conservative public opinion	0.003 (1.69)
Population, log	-0.49*** (0.17)	-0.71*** (0.23)	-0.49*** (0.17)
Education, percent with high school diploma	-2.89 (4.40)	-7.23 (5.49)	-2.89 (4.40)
Dummy = 1 for Southern states	2.36*** (0.54)	2.32*** (0.60)	2.36*** (0.54)
Years since state entered union	1.51*** (0.47)	2.45*** (0.84)	1.51*** (0.47)
Dummy =1 if judges stand for re-election	1.59*** (0.45)	2.00*** (0.59)	1.59*** (0.45)

Note. Each column reports a logistic regression to explain the probability that a state has a conservative law on a given issue rather than a liberal law. Standard errors are in parentheses beneath the coefficient estimates. The full sample contains 350 observations, and the non-West sample contains 266 observations. Each regression included seven issue-specific dummy variables whose coefficients are not reported. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 5. Logistic Regressions Explaining Congruence

	Full sample (1)	Without West (2)	Full sample (3)
Dummy = 1 if initiative available	0.57* (0.32)	0.68* (0.39)	...
Dummy = 1 if initiative available & conservative majority	0.91** (0.36)
Dummy = 1 if initiative available & liberal majority	-0.05 (0.57)
Dummy = 1 if conservative majority	1.25* (0.75)
Public opinion: size of majority %	16.07*** (2.54)	18.78*** (3.32)	16.21*** (2.57)
Population, log	-0.10 (0.15)	-0.26 (0.19)	-0.05 (0.15)
Education, percent with high school diploma	-0.66 (4.24)	-2.67 (5.00)	-0.91 (4.28)
Dummy = 1 for Southern states	1.05** (0.41)	1.03** (0.44)	1.00** (0.41)
Years since state entered Union	0.80** (0.40)	1.53** (0.73)	0.85** (0.41)
Dummy = 1 if judges stand for re-election	0.82** (0.37)	1.14** (0.49)	0.69* (0.38)

Note. Each column reports coefficients from a logistic regression to explain the likelihood of a congruent outcome, defined as an outcome that is supported by a majority of citizens. Standard errors are in parentheses beneath the coefficient estimates. The full sample contains 350 observations and the non-West sample contains 266 observations. Each regression included seven issue-specific dummy variables whose coefficients are not reported. Significance levels are indicated: *=10%, **=5%, ***=1%.

Table 6. Logistic Regressions Differentiated by Signature Requirement and Constitutional/Statutory

	Conservative policy		Congruent policy	
	(1)	(2)	(3)	(4)
Dummy = 1 if initiative available	1.19 [*] (0.61)	...	0.73 (0.58)	...
Signature requirement %	-0.02 (0.08)	...	-0.02 (0.07)	...
Dummy = 1 if constitutional initiative available	...	1.09 ^{***} (0.36)	...	0.54 [*] (0.33)
Dummy = 1 if only statutory initiative available	...	1.00 [*] (0.53)	...	0.72 (0.51)
Public opinion: percent in favor of conservative law	10.63 ^{***} (2.37)	10.61 ^{***} (2.37)
Public opinion: size of majority %	16.09 ^{***} (2.54)	16.08 ^{***} (2.54)
Population, log	-0.49 ^{***} (0.17)	-0.49 ^{***} (0.17)	-0.10 (0.15)	-0.09 (0.15)
Education, percent with high school diploma	-2.83 (4.41)	-2.70 (4.52)	-0.59 (4.25)	-1.00 (4.34)
Dummy = 1 for Southern states	2.37 ^{***} (0.54)	2.37 ^{***} (0.54)	1.07 ^{***} (0.41)	1.03 ^{**} (0.41)
Years since state entered Union	1.49 ^{***} (0.48)	1.50 ^{***} (0.48)	0.78 ^{**} (0.40)	0.82 ^{**} (0.40)
Dummy = 1 if judges stand for re-election	1.59 ^{***} (0.44)	1.59 ^{***} (0.45)	0.82 ^{**} (0.37)	0.84 ^{**} (0.38)
<i>p</i> -value for no initiative effect				
5% signature requirement	.003077	...
8% signature requirement	.003089	...
10% signature requirement	.026176	...
<i>p</i> -value for constitutional initiative equal to statutory initiative856706

Note. Each column reports coefficients from a logistic regression. Standard errors are in parentheses. The dependent variable is indicated at the top of the column, either the likelihood of a conservative policy or the likelihood of a policy supported by a majority of citizens (congruent policy). The signature requirement is the lowest of the signatures required for statutory and constitutional initiatives, expressed as a percent. Each regression is based on seven issues in 50 states (350 observations). All regressions included seven issue-specific dummies whose coefficients are not reported. Significance levels are indicated: * = 10%, ** = 5%, *** = 1%.

Table 7. Logistic Regressions with Qualification Difficulty and Legislative Insulation Indexes

	Conservative policy			Congruent policy		
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy = 1 if initiative available	1.17** (0.55)	1.49** (0.71)	1.50** (0.71)	0.78 (0.52)	0.71 (0.64)	0.70 (0.64)
Initiative × Qualification difficulty (0-6)	-0.04 (0.16)	...	0.09 (0.23)	-0.08 (0.15)	...	-0.11 (0.22)
Initiative × Legislative insulation (0-9)	...	-0.08 (0.12)	-0.13 (0.17)	...	-0.03 (0.11)	0.03 (0.16)
Public opinion: percent in favor of conservative law	10.67*** (2.39)	10.76*** (2.39)	10.71*** (2.39)
Public opinion: size of majority %	16.14*** (2.54)	16.05*** (2.54)	16.20*** (2.56)
Population, log	-0.49*** (0.17)	-0.52*** (0.18)	-0.53*** (0.18)	-0.12 (0.16)	-0.11 (0.16)	-0.11 (0.16)
Education, percent with high school diploma	-2.82 (4.41)	-2.48 (4.44)	-2.38 (4.45)	-0.53 (4.25)	-0.52 (4.28)	-0.64 (4.29)
Dummy = 1 for Southern states	2.38*** (0.54)	2.40*** (0.54)	2.39*** (0.54)	1.09*** (0.42)	1.06*** (0.41)	1.10*** (0.42)
Years since state entered Union	1.50*** (0.47)	1.60*** (0.49)	1.67*** (0.53)	0.80** (0.40)	0.83** (0.41)	0.76* (0.43)
Dummy = 1 if judges stand for re-election	1.59*** (0.45)	1.58*** (0.45)	1.59*** (0.46)	0.82** (0.37)	0.82** (0.38)	0.82** (0.37)

Note. Each column reports coefficients from a logistic regression. Standard errors are in parentheses. The dependent variable is indicated at the heading of the column, either the likelihood of a conservative policy or the likelihood of a policy supported by a majority of citizens (congruent policy). The qualification difficulty index takes on values 0-6 and indicates the difficulty of placing a measure on the ballot. The legislative insulation index takes on values 0-9 and indicates how difficult it is for initiatives to constrain the legislature. Each regression is based on seven issues in 50 states (350 observations). All regressions included seven issue-specific dummies whose coefficients are not reported. Significance levels are indicated: *=10%, **=5%, ***=1%.