

Popular Control of Public Policy: A Quantitative Approach

John G. Matsusaka*

*Marshall School of Business, Gould School of Law, and Department of Political Science,
University of Southern California, Los Angeles, CA 90089, USA; matsusak@usc.edu*

ABSTRACT

The quality of government is often measured by the degree of congruence between policy choices and public opinion, but there is not an accepted method for calculating congruence. This paper offers a new approach to measuring policy-opinion congruence, and uses it to study 10 high-profile issues across the 50 states. For the issues examined, states chose the policy preferred by a majority of citizens (equivalent to the median voter outcome) 59 percent of the time — only 9 percent more than would have happened with random policymaking. Majoritarian/median outcomes were 18–19 percent more likely when direct democracy was available, and 11–13 percent more likely when judges were required to stand for reelection. The likelihood of a majoritarian/median outcome was not correlated with a variety of election laws, including campaign contribution limits, public funding of campaigns, and commission-based redistricting.

In a democracy, citizen preferences are supposed to play an important role in public policy decisions. Indeed, as Erikson *et al.* (1993, p. 1) note, “we often gauge the quality of government by the responsiveness of public policymaking to the preferences of the mass public,” and scholars and activists continue to search for institutions that will enhance responsiveness. The Downsian model (Downs, 1957) shows that competition between

* I received helpful comments from Michael Alvarez, Jonathan Barnett, Christina Gathmann, Tim Groseclose, Andy Hanssen, Arthur Lupia, Mathew McCubbins, Jaffer Qamar, anonymous referees, and seminar participants at Stanford University, UC-San Diego, the University of Chicago, and the University of Southern California.

Supplementary Material available from:

http://dx.doi.org/10.1561/100.00009055_supp

MS submitted 14 August 2009; final version received 20 June 2010

ISSN 1554-0626; DOI 10.1561/100.00009055

© 2010 J. G. Matsusaka

candidates can bring policy decisions into alignment with the preferences of the median voter, but the predominant theme of recent research is the many obstacles that stand in the way of citizen control, such as the limited information of voters and representatives (Campbell *et al.*, 1960; Miller and Stokes, 1963; Lupia and McCubbins, 1998; Groseclose and McCarty, 2000), interest groups (Olson, 1965; Stigler, 1971; Peltzman, 1976; Grossman and Helpman, 2001), and legislative structure (Weingast *et al.*, 1981; Cox and McCubbins, 2005). Opinion surveys consistently reveal that most citizens believe government responds more to powerful interests than the general public.

Despite its importance for the practice and study of democracy, there is little statistical evidence on the amount of congruence between preferences and policy that actually prevails, and little evidence on how institutions affect the amount of congruence. Numerous studies, such as the well-known contributions of Erikson *et al.* (1993), Borcherting and Deacon (1972), Bergstrom and Goodman (1973), and Stimson *et al.* (1995) document a correlation between policy and indirect measures of citizen preferences, such as demographic and economic variables or indexes of ideology along a liberal-conservative continuum.¹ While such evidence shows that policies respond at the margin to changes in opinion, several studies have noted that the correlations (or regression coefficients) generated in such studies are not direct measures of congruence, and cannot be used to compare congruence *across* institutions (Romer and Rosenthal, 1979; Erikson *et al.*, 1993, pp. 92–94; Matsusaka 2001). Therefore, the literature that uses the correlation between policy and opinion (either directly or in a regression framework) to compare the effectiveness of alternative institutions appears to lack foundation.

This paper offers a new, nonparametric approach to measuring policy congruence, and uses it to explore the performance of representative democracy in the American states and identity factors that influence the amount of congruence. As discussed at greater length below, the challenge in measuring congruence is that we seldom have direct information on voter preferences, and even when we do, it is not obvious how a distribution of preferences (say, over tax rates) should be aggregated into a “public preference.” In order to avoid these difficulties, my approach is to focus on a set of issues that have two possible outcomes, for example, capital punishment, which can be allowed or prohibited (as opposed to tax policy, which can be chosen along a continuum). With dichotomous issues, there is a unique outcome that a majority prefers, which is also the median citizen outcome. This majority position can be identified in principle using opinion surveys and then simply compared to the policy that prevails. For each state and policy, then, we can conclude that the actual policy is either “congruent” (the outcome favored by the majority) or noncongruent (the outcome favored by the minority).

¹ A large literature in economics and political science associated with Miller and Stokes (1963), Kau and Rubin (1979), Kalt and Zupan (1984), and Peltzman (1984) studies the correlation between citizen preferences and roll call votes. See Bafumi and Herron (2009) for a good discussion of the state of the literature and new results. These studies speak to the issue of constituent–legislator representation, but only indirectly to the connection between preferences and *policy* because policy outcomes depend on more than individual roll call votes, such as the method for aggregating individual votes, selection of issues to put to a vote, executive veto, and the behavior of courts.

In order to illustrate the potential utility of this approach to quantifying congruence, I examine 10 high-profile issues that were decided by the 50 states in the last two decades. The set of issues includes a variety of policies that received significant popular and scholarly attention. For these 500 state-issue observations, I find that policy choices were congruent 59 percent of the time. Because congruent choices would arise 50 percent of the time with random policymaking, public opinion does not appear to have been the determining factor for these policy choices. One interesting implication is that the popular median voter model does not provide a reliable explanation for these issues.

As a second and perhaps more important application, this paper investigates the connection between congruence and several institutions that are believed by some to influence the degree of popular control of policy:

- Direct democracy: The initiative process, that allows voters to propose and pass laws and constitutional amendments without involving their representatives, has grown in importance over the last 30 years and is now available in 24 states. The Progressives promoted the initiative in order to make government more responsive to “the people” instead of the narrow special interests they believed had a stranglehold on their legislatures, yet from the beginning critics argued that the process actually empowers special interests with access to financial and organizational resources (Broder, 2000; Lupia and Matsusaka, 2004). This paper provides direct evidence on how direct democracy affects congruence, finding that policies are approximately 18–19 percent more congruent in initiative than noninitiative states.
- Judges: Theoretically, judges can reduce or increase congruence (Hansen, 2000; La Porta *et al.*, 2004). Judges may reduce congruence if they overrule legislatures and ballot propositions to protect minorities (perhaps for the best). Judges may increase congruence if they overrule legislators, executives, and agencies that unduly favor special interests. Because recent research has shown that the behavior of judges depends on how independent they are from electoral control (Hanssen, 1999b; La Porta *et al.*, 2004; Klerman and Mahoney, 2005; Lim, 2008), I compare congruence in states where judges must stand for re-election with congruence in states where judges have life terms or are reappointed by the governor or legislature. I find that congruence is approximately 11–13 percent higher when judges must stand for re-election.
- Election institutions: The focus of many reform efforts and much scholarship is not on institutions that can check and override the legislature, but on improving the accountability of the legislature itself.² In order to assess the importance of the legislature, I examine the relation between congruence and a set of election laws favored by reformers, including campaign finance regulation (contributions, disclosure, public funding),

² Scholars have identified a long list of factors that might frustrate accountability in the legislative process, such as the crudeness of elections as a tool for selecting candidates and sending signals to representatives on a multitude of issues (Barro, 1973; Ferejohn, 1986), the influence of parties, agenda control, and logrolling in legislatures (Cox and McCubbins, 2005; Baron and Ferejohn, 1989; Buchanan and Tullock, 1962; Weingast *et al.*, 1981), the executive veto (McCarty, 2000), and independence of administrative agencies (Gerber *et al.*, 2001). For examples that focus on reform, see the various contributions in McDonald and Samples (2006).

primary election laws, redistricting processes, ballot access rules, and recall. Somewhat surprisingly, the data do not display a significant connection between congruence and any of these election laws.

While I believe the method outlined in this paper provides a partial solution to one of the more difficult empirical challenges in assessing democracy, and the applications offer some interesting new insights, there are a number of caveats to the analysis and it remains to be seen to what extent the findings can be generalized. I attempt to highlight the approach's limitations and offer caveats throughout the paper. The paper begins by outlining why the conventional "correlation" approach that uses preference proxies to measure responsiveness does not allow measurement of overall congruence or comparisons of congruence across institutions. It is important to understand why existing methods fall short in order to understand the marginal value of a new approach. It then develops the measure of congruence, describes the data that are explored in the applications, and presents results. Specific caveats and limitations of the analysis are noted throughout, with more general concerns discussed at the end.

MEASURING CONGRUENCE

Because this paper offers an approach to measuring the congruence between public opinion and policy that differs from the preceding literature, it is important to explain the limitations of previous work in this area. A direct, nonparametric measure of "congruence" between policy and preferences on issue i in state s is:

$$|y_{is} - y_{is}^*|, \quad (1)$$

where y_{is} is a state's chosen policy and y_{is}^* is the policy that accords with the public's preferences (more on that in a moment). Smaller values indicate greater congruence. The public's preferred policy y_{is}^* is some aggregation of voter preferences, for example, the ideal point of the median voter in the median voter theory or the policy favored by the majority in a textbook majority-rule system.

While theoretically straightforward, in practice measuring congruence using Equation (1) is difficult and seldom attempted because we cannot quantify y_{is}^* . If y_{is}^* is taken to be the median voter's ideal point and the policy is the income tax rate, we would need to know the ideal tax rate for the median voter. Such information is very hard to come by. Instead, opinion data typically take the form of broad ideology scores. For example, voters may be asked to place themselves along a conservative-liberal scale. When policy and ideology do not share a common metric and the mapping between ideology and policy preferences is unknown, Equation (1) cannot be implemented.³

³ Gerber (1999, ch. 7) provides a clear statement of the general problem, and in some respects was the inspiration for this paper. However, the actual estimates in Gerber (1999) are variants of Equation (2), and thus subject to the limitations discussed in the text.

Instead, most studies have estimated correlations between policy and opinion, or (what is essentially the same) regressions of the form:

$$y_{is} = a + bO_{is} + e_{is}, \quad (2)$$

where O_{is} is an index of opinion presumed to be related to voter preferences over the policy, a and b are coefficients to be estimated, and e is an error term. For example, in a cross-section of states or a time-series, tax rates might be regressed on an index of liberalness or a set of demographic variables. Several studies find nonzero value for b , indicating that changes in opinion are associated with changes in policy.⁴ However, because y and O are in different metrics, the coefficients a and b do not reveal to what extent policy choices are congruent with opinion (“we cannot discern whether any particular state has more liberal or more conservative policies than its electorate wants”; Erikson *et al.*, 1993, p. 92). It could be that policy outcomes are far away from what voters want, even though they respond at the margin to changes in opinion. A significant consequence of this limitation is that it prevents comparison of congruence across jurisdictions. As a result, we cannot use Equation (2) to compare congruence of governments under different political institutions.

At first glance, it might seem that higher values of b represent a greater congruence of policy and opinion, and that one could compare estimates of b under alternative conditions to determine which factors bring policy closer to opinion. And indeed, many studies have made precisely this argument (see Matsusaka, 2001 for examples). However, the argument is not justified. To illustrate the problem, Figure 1 shows a hypothetical mapping from O to y^* , labeled f . The mapping f (unknown to the researcher) indicates the preferred policy of (say) a state with opinion O ; observations that lie on f would be perfectly congruent. The cluster of points X represents policy-opinion observations for one group of states and the cluster-labeled Z represents observations for another group of states. If regression (2) is estimated separately for group X and group Z , we would find $b_X > b_Z$. However, the policies in group X are clearly less congruent with (more distant from) what the public wants than the policies in group Z (in the sense of Equation (1)). Even a finding of $b_X > 0$ and $b_Z = 0$ would not imply that that X is more congruent than Z . It is straightforward to show that any pattern of coefficients from Equation (2) can be consistent with X being more or less congruent than Z . In short, despite their prevalence in the literature, estimates of Equation (2) cannot be used to identify the factors that influence congruence.⁵

⁴ Well-known examples are Erikson *et al.* (1993) in political science, and Borchering and Deacon (1972), and Bergstrom and Goodman (1973) in economics.

⁵ This argument is abbreviated from Matsusaka (2001), which itself is an elaboration of an argument given by Erikson *et al.* (1993, pp. 92–94). Romer and Rosenthal (1979) observe a related problem in tests of the median voter model that use linear combinations of economic and demographic variables as proxies for preferences. Achen (1977) identifies a different problem with approaches based on Equation (2). An older literature proposes to measure congruence by the R^2 of a regression (for example, Pommerehne, 1978), but this is also problematic. A suitably modified version of Figure 1 shows that R^2 — the degree to which observations can be fit to a line — does not reveal how close

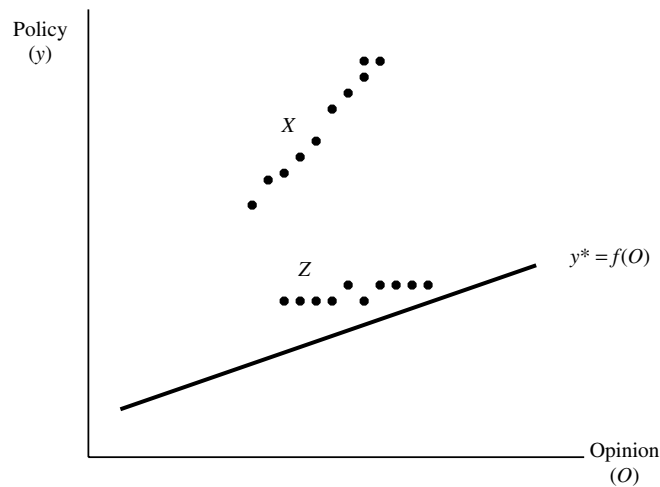


Figure 1. Hypothetical observations of policy (y) and opinion (O).

A central innovation of this study is to suggest that we can escape this thicket by working with Equation (1) instead of Equation (2). The challenge is finding a direct measure of y_{is}^* . The analytical step that allows implementation of Equation (1) is to focus on issues that have two outcomes, $y_{is} \in \{yes, no\}$, rather than a continuum of outcomes.⁶ With only two outcomes, the policy favored by the majority is unambiguous and happens to correspond to the median voter outcome as well.⁷ My approach is to define $y_{is}^* \in \{yes, no\}$ as the policy preferred by the majority/median voter based on opinion surveys. One can think of other definitions of y_{is}^* — and the method is flexible enough to accommodate a variety of other definitions — but majority rule and the median voter are central concepts in theoretical political economy, providing a good starting point for studying congruence. To summarize formally, for issue i and state s ,

the points are to y^* . A question sometimes raised is if the problem identified in Figure 1 can be solved by including intercepts, possibly specific to the individual groups of states. The answer is no, as shown in Matsusaka (2001), but intuitively, with group-specific intercepts, each group would be characterized by two parameters (intercept and slope), and there is no natural way to determine which group is more congruent by comparing *pairs* of parameters. It may be worth noting that this criticism also applies to estimates of dynamic policy responsiveness.

⁶ This distinction “works” for many issues but is obviously a simplification. Every issue I classify as having two outcomes could be thought of as having more dimensions. For example, if capital punishment is allowed, there is still the question of whether it applies to minors, which crimes it applies to, and so on. This simplification introduces no obvious biases in the measurement of congruence.

⁷ The majoritarian/median voter outcome in this context is also the utility maximizing outcome if each person is weighted equally and each person has the same utility from his or her favored outcome relative to his or her disfavored outcome.

congruence is defined as:

$$\text{Congruence}_{is} = \begin{cases} 1 & \text{if } y_{is} = y_{is}^*; \\ 0 & \text{if } y_{is} \neq y_{is}^*. \end{cases}$$

The remainder of this paper provides estimates of congruence so defined, and shows the connection between congruence at the state level and various political institutions.⁸

DATA AND ISSUES

The remainder of this paper applies this measure of congruence to a set of high-profile issues in the states. To identify issues, I searched the codebooks for the American National Election Studies (ANES) from 1988 to 2004 and identified all questions concerning policies that the survey treated as dichotomous (respondents either “favored” or “opposed” one outcome). I eliminated policies that states could not control (such as whether abortion should be legal and foreign policy questions) and questions that were too general to link to specific policy outcomes (such as whether taxes are too high or too low). This left a set of 10 policy questions, listed in Table 1.⁹

For each state, I calculated opinion for and against each policy to determine the majority/median position (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, the ANES had zero or just a handful of responses. For these observations, 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, but for each issue i , the basic procedure was to estimate a regression $O_{is}^{ANES} = \alpha + \beta O_{is}^{BERRY} + u_{is}$ for those states with reliable opinion information (typically defined as states with 60 or more observations), where O_{is}^{ANES} is the ANES opinion score for state s and O_{is}^{BERRY} is the state’s general ideology index as constructed by Berry *et al.* (1998). Then, for states with missing ANES information, I imputed an ANES score using the estimated values of α and β and the state’s index value from Berry *et al.* The empirical results are generally the same if the imputed observations are deleted, as discussed below.

⁸ Since my study began to circulate, two noteworthy papers by Lax and Phillips (2009a, 2009b) have appeared that in effect also provide estimates of Equation (1) for a subset of issues. Their approach differs from mine in that they impute y_{is}^* for each state using national opinion surveys while I use direct survey information for each state, but the similarities outweigh the differences (for example, they focus on dichotomous policy choices) and their methods represent a valuable step forward from the previous literature that relies on estimates of Equation (2). As discussed below, their estimates paint a similar overall picture of congruence but their estimates of institutional effects are different.

⁹ In some cases the literal formulation of the question does not correspond exactly to the policy under investigation. For example, the ANES term limits question asks about *Congressional* term limits (which the Supreme Court has ruled cannot be imposed by the states), while the paper examines term limits on state legislators. In this case, I assume that voters in favor of term limits for Congressmen would also favor term limits for state legislators. See the Appendix for discussion and assumptions.

Table 1. Policies.

Issue	Question	Percent in favor			Years
		Mean	Min	Max	
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?”	70.5	56.5 (OR)	80.8 (MN)	1990, '98, 2000
Estate tax	“There has been a lot of talk recently about doing away with the tax on large inheritances, the so-called ‘[estate/death]’ tax. Do you favor or oppose doing away with the [estate/death] tax?”	73.0	60.4 (DE)	82.7 (AR)	2002
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
Late-term abortions	“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
Parental consent, abortion	“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
Public funding, abortion	“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
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)	32.3	11.5 (AL)	49.1 (MA)	2004
School prayer (moment of silence)	“Which of the following views comes closest to your opinion on the issue of school prayer? 1 = By law, prayers should not be allowed in public schools. 2 = The law should allow public schools to schedule time when children can pray silently if they want to. 3 = The law should allow public schools to schedule time when children, as a group, can say a general prayer not tied to a particular religious faith. 4 = By law, public schools should schedule a time when all children would say a chosen Christian prayer.” (“In favor” = response 2, 3, 4)	86.8	73.6 (UT)	94.6 (IN)	1990, '94, '96, '98
Term limits	“A law has been proposed that would limit members of Congress to no more than 12 consecutive years of service in that office. Do you favor or oppose such a law?”	78.5	69.8 (CT)	88.0 (MO)	1990, '94, '96, '98

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, including imputed values as described in the text. The identity of extreme observation is reported in parentheses beneath the values.

Experts on the ANES will note that my use of the survey goes beyond its intended purposes. Except for the 1988–1992 Senate study (which does provide much of my data), the ANES is designed to be representative only at the national, not at the state level. This raises questions about the validity of my opinion estimates, particularly for small states where all responses might come from a single region of the state. If responses in a predominantly rural state are drawn exclusively from the state’s single metropolitan area, the measured opinion is likely to be skewed. Jones and Norrander (1996) report systematic evidence suggesting that the ANES can be aggregated reliably at the state level, at least with large enough sample sizes, but even so, it has to be conceded that these estimates of citizen preferences are likely to contain significant noise and possibly bias.

However, an important feature of my measure of congruence is that it is robust to potentially large amounts of measurement error. This is because when calculating congruence, only the position, and not the size of the majority matters: congruence is the same if a state’s opinion is 55 percent or 95 percent in favor of a policy. Errors in measuring opinion do not affect congruence unless the error is great enough to cause the majority to flip from one side to the other. It turns out that for the policies studied, opinion is usually lopsided in favor of one position, meaning that an “in favor” state is unlikely to be erroneously classified as an “opposed” state, and conversely. For the same reason, measurement error in the imputed observations is less troubling than it might seem at first. In short, even though the ANES results by state are likely to contain significant measurement error,¹⁰ this should not have a large effect on measured congruence.¹¹

The 10 policies in Table 1 span a broad set of issues but most are social issues rather than economic issues. Opinion tends to be one-sided: for seven policies the national majority exceeded two-thirds, and for seven policies the majority was on the same side in all 50 states. Information on each state’s policy choice for each issue was collected from a variety of sources as detailed in the Appendix.

¹⁰ To provide some corroborating evidence, I compared actual and imputed ANES opinion numbers with survey information from polls that were designed to be representative at the state level and turned up very few cases where the majority position classified incorrectly. On same-sex marriage, I compared the ANES numbers with: (i) state-specific opinion surveys reported in Lupia *et al.* (forthcoming, Appendix 1) and found agreement about the majority view for all but two states, giving 96 percent consistency; and (ii) opinion numbers imputed by Lax and Phillips (2009a, Figure 6) from national surveys for 2003–2004 and found 100 percent consistency. For term limits, I found the consistency in the majority position in the ANES and state-specific opinion surveys in 20 of 20 states.

¹¹ To assess the plausibility of this argument, I conducted several robustness exercises including: (i) deleting all of the imputed observations, (ii) including only states with a number of respondents in excess of a cutoff value (for various cutoffs), and (iii) using only data from the 1988–1992 Senate study when available. None of these changes significantly altered the measured congruence in the sample, suggesting that measurement error in state opinion is not distorting the measurement of congruence in a big way.

SUMMARY OF CONGRUENCE IN THE STATES

As a first step, this section describes the overall level of congruence in the sample and summarizes the variation across states and issues. To provide context for the numbers, we can compare them with two polar cases. At one extreme, what might be called “complete control”, congruence would be 100 percent — the majority rules for every state and every issue. At the other extreme, what might be called “random policymaking,” each policy would be selected by the flip of a coin, and congruence would be 50 percent.

Figure 2 reports the percentage of congruent policies for each issue and for the 10 issues combined (darker bars). Of the 500 state-issue observations in the sample, 59 percent are congruent. Congruence is 57 percent if observations with imputed opinion data are deleted. It seems that policy choices are somewhat connected to majority preferences, but overall the outcomes look closer to random policymaking than complete control.

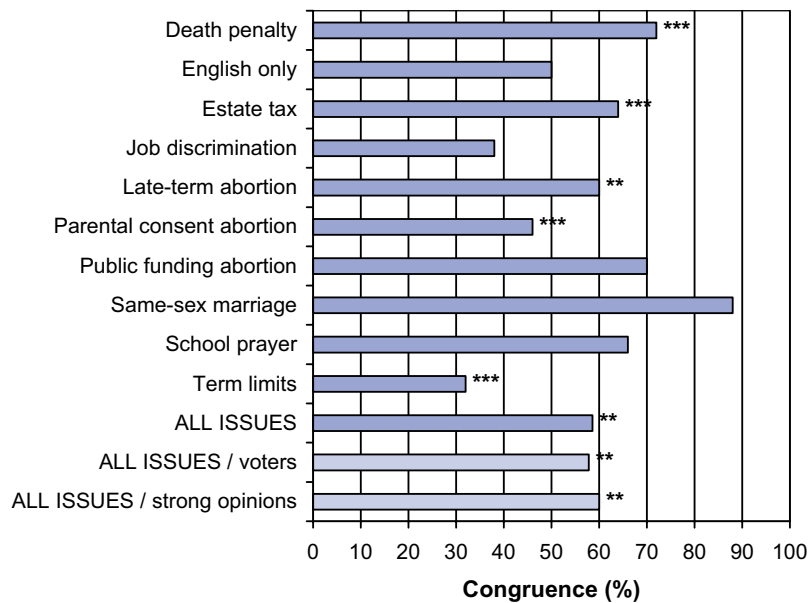


Figure 2. Congruence by issue.

Note. Except for the last two bars, a state’s policy is “congruent” if it corresponds to the preference of a majority of survey respondents in the state. In the penultimate bar, a state’s policy is congruent if it corresponds to the preference of citizens that voted. In the last row, a state’s policy is congruent if it corresponds to the policy of citizens with strong preferences. The issue-specific bars are based on 50 observations. The “all issues” bars are based on 500 observations, except for the bottom bar that is based on 380 observations. Significance levels for the hypothesis that congruence equals 50 percent are indicated as follows: * = 10%, ** = 5%, *** = 1%.

This suggests that the popular median voter model does not work well for these issues. Of course, the sample includes only a selection of particularly salient and controversial issues so the level of congruence here is not representative of American democracy as a whole — congruence is certainly higher on dozens of uncontroversial issues not in the sample, such as whether to fund police and fire protection, allow trial by juries, and so on.¹²

For the full sample, congruence is statistically different from 50 percent at better than the 1 percent level. For individual issues, congruence is greatest for same-sex marriage (88 percent). Congruence is 70 percent or more and statistically different from 50 percent for public funding of abortion and death penalty. Congruence is lowest for term limits (32 percent) and significantly different from 50 percent at nearly the 1 percent level. Congruence is less than 50 percent for laws protecting homosexuals from job discrimination and parental consent for abortion, but not different from 50 percent at conventional levels of significance. Congruence also cannot be distinguished from 50 percent at the 10 percent level for English only, estate tax, and late-term abortion.

An alternative notion of popular control is that policy choices are determined by the opinion of a majority of *voters*, as opposed to all citizens (Griffin and Newman, 2005). If voters have different preferences than nonvoters, and policy responds to voters rather than nonvoters, estimated congruence will be higher for voters than nonvoters. To assess this possibility, I recalculated congruence using opinion of voters rather than all survey respondents. The overall congruence measured in this way (i.e., considering only the opinions of voters) is 58 percent, slightly lower than considering all citizens (lighter bar in Figure 2).¹³ The issue-by-issue pattern (not reported) is virtually identical whether the sample includes all citizens or only voters.

Another possibility is that policy choices are determined by the opinion of citizens with strong preferences. To assess this idea, I recalculated congruence using only opinion data from respondents who indicated they held their opinion “strongly.” Congruence measured in this way is slightly higher — 60 percent — although still closer to random policymaking than complete control (bottom bar in Figure 2). The issue-by-issue pattern (not reported) is essentially the same. In short, the finding of “low” congruence overall is robust to considering the opinions of all citizens, only voters, and only citizens with strong opinions.

The direction of noncongruence is also interesting. For the full sample, 33 percent of observations are noncongruent in the liberal direction and 8 percent are noncongruent in the conservative direction. For individual issues, liberal bias (meaning noncongruence in the liberal direction) is greatest for term limits (68 percent of observations), abortion parental consent (54 percent), and English only (50 percent). Conservative bias is greatest for antidiscrimination in employment on the basis of sexual orientation (62 percent) and public funding for abortions (20 percent). Some caution is in order when interpreting

¹² Lax and Phillips (2009b) also finds low congruence — 48 percent — using state opinion imputed from national surveys with an otherwise similar research design.

¹³ A survey respondent was defined as a “voter” if he or she voted in the most recent presidential election.

these numbers because majority opinion is conservative on 86 percent of these observations, so at most 14 percent of the observations could display a conservative bias, but the pattern does suggest that to the extent that democracy in the states is ineffective, it is mainly because policies are too liberal compared to public opinion.

I also calculated congruence for individual states. Since there are 10 issues, congruence can take one of eleven values (0 percent, 10 percent, . . . , 90 percent, 100 percent). No state achieves 100 percent congruence. Arkansas is the most congruent, with policy outcomes reflecting the majority view for nine of ten issues (the noncongruent issue is a law prohibiting job discrimination on the basis of sexual orientation). Nine states are congruent on 80 percent of the issues, and 11 states are congruent on 70 percent of the issues.¹⁴ At the other extreme, five states are congruent for only three of ten issues — Hawaii, Minnesota, New Mexico, New York, and Vermont — and seven states are congruent on only 40 percent of the issues.¹⁵ Overall, 28 states have congruence above 50 percent while 12 states have congruence below 50 percent. Southern states are 20 percent more congruent than other states (74 percent versus 54 percent).

Congruence rises monotonically with the size of the majority. When the majority is 60 percent or less, congruence is 45 percent, statistically indistinguishable from random policymaking. Congruence increases to 55 percent when the majority is in the 61–70 percent range, 60 percent when the majority is in the 71–80 percent range, 66 percent when the majority is in the 81–90 percent range, and reaches 78 percent when the majority exceeds 90 percent. Congruence may be related to the size of the majority for several reasons. From a statistical perspective, the majority's position is most likely to be misidentified when the size of the majority is close to 50 percent, leading to the appearance of random policymaking in the neighborhood of 50 percent. More substantively, a larger majority has more votes to overcome supermajority requirements that support executive vetoes, constitutional amendments, ballot propositions, and other legislative procedures. Finally, states that have heterogeneous opinion (a small majority) may be more difficult to represent because the majority view is more difficult to identify and significant opposition can materialize on both sides of an issue, leading to more “honest mistakes” (noncongruent policies) by politicians (Matsusaka and McCarty, 2001).

DETERMINANTS OF CONGRUENCE: DIRECT DEMOCRACY AND JUDGES

The preceding evidence shows that the majority often does not rule for these issues. This and the next section seek to identify factors that can explain the variance in congruence across states and issues. The list of potential explanatory factors is enormous, far more than could be addressed in a single paper. I choose to focus on political institutions,

¹⁴ 80%: Alabama, Arizona, Kentucky, Louisiana, Mississippi, Missouri, South Carolina, Tennessee, Utah. 70%: California, Florida, Georgia, Illinois, Indiana, Montana, North Dakota, Ohio, Oklahoma, South Dakota, Wyoming.

¹⁵ 40%: Alaska, Connecticut, Delaware, New Jersey, Oregon, Rhode Island, Washington.

fundamental, and enduring features of a state's political system, because as foundations of democratic government, they seem like a natural point of departure. I begin with two institutions designed to counterbalance the legislature — direct democracy and the courts — and then explore institutions designed to improve responsiveness of the legislature itself.

Direct Democracy

The early twentieth-century Progressives sought to make government more responsive by introducing direct democracy. The most high-powered form of direct democracy is the initiative process that permits voters to propose and approve new laws.¹⁶ Currently, 24 states and about 80 percent of the cities in the country allow initiatives, and over 70 percent of Americans have it available either in their city or state. Although initiatives are often promoted as a way to increase congruence, critics argue that initiatives reduce congruence because they empower wealthy and organized special interests (Broder, 2000). It might seem that holding a popular vote on an issue leads to a majoritarian outcome by definition, but this may not be the case. Special interests could bring about noncongruent outcomes by attracting a disproportionate number of their supporters to the polls, making the voting majority different from the population majority. Or a complex and technically worded proposition could trick voters into supporting a proposition that implements an outcome they do not favor. The question of whether direct democracy enhances or inhibits majority rule has been and remains a central issue surrounding the institution.¹⁷

Panel A of Table 2 reports nonparametric comparisons of congruence in states with and without the initiative process. Congruence is almost 10 percent higher in initiative than noninitiative states (63.9 percent versus 54.1 percent), and the difference is significant at about the 3 percent level.¹⁸ This pattern supports the idea that the initiative is a majoritarian institution, and undermines the hypothesis that initiatives allow special interests to override the majority. The finding of lower congruence in noninitiative states suggests that legislatures tend to choose noncongruent policies (otherwise there would be nothing for initiatives to make congruent), and hints that special interests could be more influential in the legislature than the initiative process.

¹⁶ “Referendums” allow voters to repeal existing laws, but not propose new laws. “Legislative measures” are propositions placed on the ballot by the legislature, and are used in every state but Delaware.

¹⁷ The descriptive numbers in this paragraph are from Matsusaka (2004, 2005, 2009). For theory, see Gerber (1996), Gerber and Lupia (1995), and Matsusaka and McCarty (2001). The latter two identify conditions under which direct democracy may reduce congruence even with rational voters. Matsusaka (2004) contains evidence and a review of the literature. Theory suggests that initiatives can influence outcomes directly — when a proposition is approved — and indirectly — when the threat of a proposition alters the legislature's behavior. To capture both effects, I focus on availability of the initiative rather than the number or content of the measures that actually appear on the ballot.

¹⁸ Without imputed observations, congruence is 54 percent in noninitiative states and 62 percent in initiative states.

Table 2. Congruence, initiative process, and judicial selection.

	Congruence	<i>N</i>
<i>Panel A. Initiative Process</i>		
Initiative unavailable	54.1	270
Initiative available (constitutional or statutory)	63.9	230
Only constitutional initiative available	75.0	20
Only statutory initiative available	55.0	60
Both constitutional and statutory initiative available	66.0	150
<i>Panel B. Judicial Selection (initial/retention)</i>		
Elected/re-elected	61.4	220
Appointed/re-elected	65.0	160
Appointed/reappointed	44.4	90
Appointed for life	46.7	30

Note. The main entries show the percentage of state-issues that are congruent. For example, 54.1 percent of state-issues are congruent in states where the initiative process is not available. Judicial selection systems are classified based on the initial selection procedure (elected or appointed) and the retention procedure (reelection or reappointment). More detailed information on the data is in the Appendix.

A potentially important difference across states is whether initiatives are allowed to amend the constitution or only to make statutory law. Constitutional initiatives are more potent because they cannot be modified by the legislature without voter approval and cannot be struck down by courts as violations of the state constitution. To see if congruence varies with the type of initiative, Panel A of Table 2 reports congruence separately for states with only constitutional initiatives, only statutory initiatives, and both. Congruence is higher in initiative than noninitiative states when constitutional initiatives are available, whether coupled with statutory initiatives (66 percent) or without statutory initiatives (75 percent), but congruence in initiative states (55 percent) is not materially different than congruence in noninitiative states when only statutory initiatives are available. Few states have only constitutional initiatives or only statutory initiatives, and none of the congruence rates between the different types of initiatives are different at the 10 percent level.

Judges

Judges provide a counterbalance to legislatures and agencies. Judges can overrule legislative statutes, agency decisions, and ballot propositions, and judges have had a direct hand in setting policy for several of the issues investigated in this paper (abortion, death penalty, term limits). A priori, the effect of judges on congruence is ambiguous. When courts intervene to protect the rights of minorities that are threatened by “majority

tyranny,” they reduce congruence. When they intervene to counteract the influence of narrow special interests in the legislature, they increase congruence.

To examine the impact of judges, I focus on the degree of judicial independence, following a recent literature that suggests independent courts are more likely to protect fundamental rights and more willing to stand up to other branches of government (Hanssen, 1999b; La Porta *et al.*, 2004). To proxy for independence, I follow Hanssen (1999a, 1999b, 2000) and Besley and Payne (2006) and distinguish whether judges are appointed (by the governor, legislature, or a commission) or elected to office.¹⁹ The most popular system (see Panel B of Table 2) is to require elections for both initial selection and retention; the second most popular system is to appoint judges initially and hold elections for retention, the so-called merit review plan; and the third most popular system is to select and reappoint judges without election. No state elects judges initially and then reappoints them without an election. It is not clear which system leads to more “independent” judges in a global sense, but we can say that appointed judges are more independent from the voters while elected judges are more independent from the governor and legislature.

Panel B of Table 2 shows that congruence is 15–20 percent lower when judges are independent from the voters. When both initial selection and retention decisions are made by the governor, legislature, or a commission without involvement of the voters, congruence is 45 percent. When judges must stand for re-election, congruence is 61 percent or 65 percent, depending on how they initially come to office.²⁰ It appears that the retention procedure is more important than the initial selection procedure, as found in Besley and Payne (2006).

Parametric Results for Direct Democracy and Judges

Each column of Table 3 reports a logistic regression to explain the probability of a congruent issue-state. The explanatory variables include initiative and judicial retention variables and several control variables of secondary interest. The first control variable is size of the majority, motivated by the unconditional correlation between congruence and size of the majority noted above. The second variable is the state’s population (as a logarithm). Congruence might be lower in a large state because citizen monitoring of elected officials is subject to greater free rider problems, and because politicians might find it more difficult to determine public preferences given the 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. This variable is included to capture the effect of information on congruence — more informed voters may be better at

¹⁹ I also explored if the form of election (nonpartisan versus partisan) or length of terms mattered but could not find robust effects.

²⁰ Without imputed observations, congruence is 56 percent for elected/reelected, 62 percent for appointed/reelected, 53 percent for appointed/reappointed, and 53 percent (only 19 observations) for lifetime appointments.

Table 3. Regressions explaining congruence with initiative and judicial selection procedures.

	(1)	(2)	(3)	(4)	(5)	States (6)
<i>Initiative Process</i>						
Dummy = 1 if (any) initiative allowed	0.87*** (0.25)	...	0.79*** (0.26)	...	0.79*** (0.27)	0.63*** (0.16)
Dummy = 1 if constitutional initiative allowed	0.83*** (0.27)
Dummy = 1 if only statutory initiative allowed	0.61 (0.41)
<i>Judicial Selection</i>						
Dummy = 1 if judges stand for re-election	...	0.69* (0.30)	0.52* (0.31)	0.50 (0.31)	...	0.35 (0.22)
Dummy = 1 if judges elected/re-elected	0.33 (0.34)	...
Dummy = 1 if judges appointed/re-elected	0.70* (0.37)	...
Dummy = 1 if judges appointed for life	-0.27 (0.51)	...
Size of majority	14.87*** (2.13)	14.13*** (2.12)	14.36*** (2.14)	14.38*** (2.14)	14.25*** (2.14)	12.80*** (3.38)
Population, log	-0.02 (0.12)	-0.08 (0.12)	-0.09 (0.12)	-0.10 (0.13)	-0.12 (0.13)	-0.07 (0.08)
High school graduates, fraction of population	-6.09* (3.93)	-2.26 (3.33)	-4.91 (3.46)	-4.51 (3.54)	-6.60* (3.71)	-3.80* (2.21)
Dummy = 1 for Southern state	0.92*** (0.33)	0.90*** (0.32)	0.89*** (0.33)	0.91*** (0.33)	0.85** (0.34)	0.65*** (0.19)
Years since state entered Union	0.25 (0.28)	0.24 (0.30)	0.52 (0.33)	0.49 (0.33)	0.60* (0.34)	0.41** (0.20)

Note. Columns (1)–(5) reports coefficients from a logistic regression that predicts the probability of a congruent state-issue using 500 observations (10 issues and 50 states). A state-issue is “congruent” if the state has adopted the policy position favored by a majority in the state. Column (6) reports coefficients from a log-of-the-odds regression that predicts the percent of congruent issues in each state, using 50 observations (one for each state). Standard errors are in parentheses beneath coefficient estimates. The models in columns (1)–(5) included 10 dummy variables (and no constant), one for each of the issues; the coefficients are not reported. In column (6) the size of majority variable is the state average across all 10 issues. Significance levels are indicated as * = 10%, ** = 5%, *** = 1%.

monitoring their representatives and preventing shirking.²¹ The regressions also include a dummy variable for Southern states to capture unobserved factors that might affect congruence. Southern dummies are standard fare in regressions using states as the unit of observation, and usually “work,” suggesting the standard controls are missing something, 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. A dummy for Western states would capture a similar source of variation as the age of the state, but the age variable seems to have slightly more explanatory power.²² Finally, to control for issue-specific effects, the models included 10 dummy variables, one for each issue, although those coefficients are not reported in order to conserve space.²³

The regressions in columns (1)–(3) of Table 3 include the initiative and judicial independence variables separately and then together, in addition to the other controls. The initiative variable is a dummy equal to one if any type of initiative is available in a state, and the judge variable is a dummy variable equal to one if judges must stand for reelection. The estimates show that the initiative and judicial independence effects that appeared in Table 2 are not just proxies for the other control variables, nor are they capturing the same source of variation. The initiative coefficient is significantly different from zero at better than 1 percent level. This is fairly direct evidence that direct democracy does in fact promote majority rule, at least for these issues, and undermines the view that direct democracy allows rich and powerful special interests to subvert the majority. The judicial coefficient is also statistically different from zero, but only at the 10 percent level once the initiative dummy is included. Congruence appears to be higher when judges must stand for election, although the coefficient is not estimated with enough precision to give unshakeable confidence in that conclusion.

The other explanatory variables are not the focus of investigation, but a few interesting patterns emerge. First, congruence is significantly more likely as the size of the majority increases. Second, Southern states are more congruent than other states. Population does not seem to be an important factor. The coefficient on the fraction of high school graduates is consistently negative, although not always distinguishable from noise. This is inconsistent with the view that educated voters do a better job monitoring their

²¹ This is not the only plausible interpretation of the education variable’s coefficient. I ran exploratory regressions including mean income and the poverty rate and found that they essentially capture the same factor as education. So the education variable could be capturing an effect that operates through wealth.

²² To see if the South and age variables are capturing “political culture,” I ran the regressions including dummy variables for “moralistic” and “traditional” political cultures using the Elazar–Sharkansky typology. Neither political culture variable was statistically significant. I also tried including a dummy for Western states out of concern that the initiative variable was capturing a West effect (most initiative states are in the West), but it was insignificant and did not have a material effect on the initiative coefficient.

²³ I use the same set of control variables throughout this paper. I also tried including variables for urbanization, racial heterogeneity, income, and the poverty rate, which were almost always insignificant and did not change the major results.

representatives, thereby increasing congruence. The number of years since a state entered the Union is positively related to congruence, suggesting that the majority is more likely to rule in older states, although the coefficient is not different from zero at conventional levels of significance.²⁴

Column (4) of Table 3 attempts to distinguish the effect of initiative type on congruence. Two initiative variables are included, a dummy equal to one if a constitutional initiative is available, and a dummy equal to one if only a statutory initiative is available. Although the constitutional initiative coefficient is greater than the statutory initiative coefficient, they cannot be distinguished from each other statistically.

Column (5) of Table 3 attempts to distinguish by type of judicial selection procedure. Three dummy variables are included, one each for states that: (i) elect their judges initially and also retain them by election, (ii) appoint their judges initially and then retain them by election, and (iii) appoint their judges for life. The omitted category is states that appoint their judges and leave the reappointment decision to the governor, legislature, or commission. Columns (1)–(4) show that congruence is higher when judges stand for reelection. Column (5) reveals that the effect comes primarily from higher congruence in states that initially appoint their judges, not states that initially elect their judges. The finding of a larger coefficient for appointed/re-elected than elected/re-elected is contrary to expectations, but perhaps not too much should be made of this since the coefficients for the two reelection cases cannot be distinguished from each other statistically. Finally, congruence is lowest of all when judges have lifetime appointments. The basic picture that emerges is that congruence is lower as judges become less accountable to the voters.²⁵

As a final a robustness check, column (6) of Table 3 reports an ordinary least-squares regression in which the unit of observation is a state and the dependent variable for each state is the (log of the odds of the) fraction of congruent issues. This specification parallels column (3) except that each state provides a single observation instead of 10 issue-specific observations. The control variable for the size of the majority opinion here is an average across all 10 issues for that state. If congruence tends to be correlated within a state across issues, the full sample using state-issue observations might overstate the degrees of the freedom. As can be seen, the estimates in column (6) are fairly similar to those in column (3), and in particular, the initiative coefficient remains positive and statistically significant. The judicial selection coefficient remains positive, but falls in magnitude and loses statistical significance.

The coefficient estimates in Table 3 are difficult to interpret. To give a sense of the magnitudes of the effects, Table 4 reports the estimated probability of congruence for

²⁴ When the imputed observations are excluded, the initiative coefficient in regression (3) of Table 3 becomes 0.91 and remains significant at the 1 percent level, while the judge coefficient falls to 0.32 and is no longer distinguishable from zero. As another robustness check, I ran the regressions without the observations for term limits. The initiative coefficient falls in magnitude but remains different from zero at the 3 percent to 8 percent level depending on model specification, suggesting that term limits account for a healthy amount of the initiative effect, but not all of it.

²⁵ Lax and Phillips (2009b) also report evidence of a connection between elected judges and congruence.

Table 4. Predicted probability of congruence conditional on initiative status and judicial retention procedure.

	Judicial retention		Marginal effect: appointed → elected
	Appointed	Elected	
No initiative	43.6	56.5	+12.9
Initiative available	63.0	74.1	+11.1
Marginal effect: no initiative → initiative	+19.4	+17.6	

Note. This table reports the predicted probability of congruence based on the estimated model in column (3) of Table 3. The estimates are for a non-Southern state with average population, education level, and age, where the issue is estate tax and the size of the majority is the sample mean. For example, when the initiative is unavailable and judges are appointed, predicted congruence is 43.6 percent.

different variable configurations using the estimates in column (3) of Table 4. All predictions are for a non-Southern state with the mean age, education, and population, where estate tax is the issue and the size of the majority is the sample mean for that issue. For example, predicted congruence is 43.6 percent for a state without the initiative and with appointed judges, and 74.1 percent for a state with the initiative and elected judges. The last row and column give the marginal effects. Availability of the initiative is associated with 19.4 and 17.6 percent greater congruence, depending on the judicial retention procedure. Judges who stand for reelection are associated with 12.9 and 11.1 percent greater congruence depending on initiative status. After controlling for other factors, the marginal effects of the initiative and judicial elections are larger than indicated in Table 2. I also estimated predicted effects for the most one-sided issue — school prayer — and the most divided issue — public funding of abortion — and found quite similar marginal effects.

The finding that direct democracy increases congruence is consistent with existing theory, previous indirect evidence on direct democracy and congruence (see Matsusaka, 2004 for a review), and numerous studies finding that the institution changes policy outcomes, but it contrasts with Lax and Phillips (2009b), which finds an insignificant negative relation between congruence and the initiative process. I cannot determine why Lax and Phillips find no initiative effect but suspect it is due in part to their inclusion of explanatory variables that are determined by the initiative process. Of most concern is their use of a term limits indicator as an explanatory variable because having the initiative process is almost a necessary and sufficient condition for having term limits, and most states adopted term limits through the initiative process. Therefore, the term limits coefficient is likely to be a strong proxy for the initiative process, and including a term limits variable could absorb the direct democracy effect.²⁶ The same concern arises, but perhaps to a lesser degree, with the legislative professionalization index employed by

Lax and Phillips because the components of the index — days in session, salaries, and staff — can be and have been established by initiatives in direct democracy states.

In research on the effects of institutions, the possible endogeneity of institutions is always a concern. In this context, we might wonder, for example, if states with high congruence are more likely than states with low congruence to adopt the initiative and require judicial elections, that is, if causality runs from congruence to the institutions. While possible, such a relation seems unlikely. Of the 24 initiative states, 18 adopted the process before 1920, 23 had adopted by 1970, and the most recent adopter was Mississippi in 1993 (Matsusaka, 2004, Appendix A.1.3), in almost all cases long before the early 21st policy choices studied in the paper. Similarly, while states have changed some details of their judicial selection procedures (for example, replacing partisan with nonpartisan elections or changing the membership of appointment commissions), no state has changed its retention procedure from appointment to election or vice versa since 1989 (Hanssen, 2004, Table 5; Besley and Payne, 2006, Table 1; American Judicature Society, 2004). This is not conclusive evidence against the endogeneity of these institutions, but it does give the impression that they are fairly durable and may be exogenous to a large extent with respect to the policy choices studied in this paper.

DETERMINANTS OF CONGRUENCE: ELECTION INSTITUTIONS

The previous section studies the connection between congruence and institutions that can restrain or override the legislature. A natural complement to that analysis is to examine institutions that influence the functioning of the legislature itself. The performance of the legislature potentially depends on the process by which representatives are elected (election institutions) and the rules under which they act once elected (legislative institutions). I focus on election institutions in this paper, in part for reasons of space, but also because of their a priori importance.²⁷ Elections are the central institution through which the public exercises control in a democracy. As James Madison wrote in *The Federalist*, No. 52, “[I]t is particularly essential that the [legislature] should have an immediate dependence on, and an intimate sympathy with the people. Frequent elections are unquestionably the only policy by which this dependence and sympathy can be effectually secured.” The institutions, or practices, I study can be grouped into five

²⁶ It could be argued to the contrary that the direct democracy variable is a proxy for term limits. However, given the extensive evidence showing effects of direct democracy on policy compared to a distinct lack of evidence that term limits matter, the a priori case for the direct democracy variable seems stronger. If I include a term limits dummy in the Table 3 regressions (not reported), the initiative coefficient drops by about one-quarter.

²⁷ However, I did run a variety of exploratory regressions in search of connections between congruence and legislative institutions, and failed to find a robust connection between congruence and any variables I examined, including legislative professionalization, the size of the legislature, number of executive officials, and partisan control. Legislative structure and organization can be and often is regulated by the initiative process, so there is an inference problem determining whether those variables are independent factors or proxies for direct democracy.

broad categories:

- **Campaign finance.** Campaign finance can affect congruence if wealthy interest groups are able to “buy” policy outcomes, if large war chests send a signal of candidate quality (Prat, 2002), or if campaign advertising allows the wealthiest candidate to advertise more and communicate more effectively than challengers (Coate, 2004). Reformers seek to limit contributions, provide public funding of campaigns (in exchange for reduced spending), and increase disclosure. These regulations are claimed to level the playing field by reducing the financial advantage of incumbents, and by limiting the role of wealthy individuals and organizations. However, contribution limits could hurt challengers, who lack the name recognition of incumbents, and public funding may reduce the incentive of politicians to respond to their constituents (Samples, 2006). Empirical research on state elections suggests that contribution limits increase competition, measured by the margin of victory or partisan division of the legislature (Besley and Case, 2003 [corporate contribution limits]; Stratmann and Aparicio-Castillo, 2006 [individual contribution limits]; Primo *et al.*, 2006 [individual contribution limits]), but the estimated effects are modest. Primo *et al.* (2006) find no effects of public funding on victory margins in gubernatorial elections while in laboratory experiments, Houser and Stratmann (2008) find that “high quality” candidates are more likely to win with public funding.
- **Primary elections.** Many reformers favor open rather than closed primaries. Closed primaries, in which each party’s nominee is chosen only by those belonging to the party, are said to select extreme candidates that cater to the party faithful rather than moderate/centrist candidates. Existing evidence finds that U.S. Congressmen are more likely to vote in accord with the median constituent’s preferences in open primary states than in closed primary states (Gerber and Morton, 1998; Besley and Case, 2003; Westley *et al.*, 2004). If open primaries also promote median voter candidates in state legislatures (I am not aware of a study on this question), then we might expect greater congruence in states with open than closed primaries.
- **Redistricting.** Traditionally, redistricting is carried out by sitting legislators. This creates a potential conflict of interest because legislators may want to insulate themselves from competition by creating safe districts for themselves.²⁸ To remove the conflict, reformers have pressed for independent redistricting commissions (“voters should choose their legislators, not the other way around”). How this affects competition and congruence depends in part on how the objectives of commissions compare with those of legislators. For example, if legislators draw lines to increase the number of seats held by their party, they may actually create numerous competitive districts, each with a small margin in their favor (Gelman and King, 1994). Even if districts are competitive so that each legislator ends up representing the district’s median voter, district lines can be gerrymandered so that the median legislator is not congruent with the median

²⁸ For a good overview, see Cain and Gerber (2006). Despite the widespread belief that gerrymandering has contributed to the decline in electoral competition over the last several decades, rigorous empirical research generally fails to find a connection. See, for example, Abramowitz *et al.* (2006) and Jacobson (2006).

voter in the population, and even random redistricting will result in noncongruent policies (Gilligan and Matsusaka, 2006).

- **Ballot access.** Ballot access fees and signature requirements required to place a candidate's name on the ballot create barriers to competition by increasing the cost of candidate entry. One set of reform proposals focuses on making ballot access less costly. Stratmann (2005) provides some evidence that easier ballot access leads to more candidates, which is one measure of electoral competition.
- **Recall.** The recall, a seldom-used procedure that allows voters to remove an official before the end of his or her term, received a tremendous amount of attention when California governor Gray Davis was recalled in 2003. The theoretical and empirical literature on the recall is not extensive, but as a first cut, we would expect the possibility of recall to improve congruence.

To anticipate the main finding of this section, I am unable to uncover a robust correlation between congruence and any of these election institutions. Because statistical power becomes a question with statistically insignificant results, the approach in this section is to report a large number of empirical results under a number of plausible empirical specifications, and document that the absence of a congruence–election institution correlation in this sample is pervasive.

Table 5 reports nonparametric comparisons of states with and without various election institutions. The table is arranged so that the “Yes” column represents the institution usually presumed to increase competition and congruence. The first two rows compare states that do and do not limit campaign contributions from corporations and individuals, respectively. I focus on individual and corporate contributions following Besley and Case (2003), Stratmann and Aparicio-Castillo (2006), and Primo *et al.* (2006). As can be seen, congruence is actually 5 percent lower in states that limit corporate contributions, and only 3 percent higher in states that limit individual contributions. Neither difference approaches conventional levels of statistical significance. The other four rows under “Campaign finance” utilize indexes constructed by Witko (2005) that essentially add up the number of specific laws in place. For example, the campaign contribution index gives one point for each contributor that is limited by state law: individuals, corporations, unions, corporate PACs, union PACs, self-financing, and candidate families. I do not know of a theoretical reason to believe the effects should be additive, but the indexes provide a rough cut as we search for effects. For each of the four indexes — contribution limits, public funding, disclosure, and overall — I compare the congruence in states with index values above and below the median. Tougher campaign finance regulations are associated with lower congruence for all four indexes, but only the contribution limits index approaches statistical significance.²⁹

Figure 3 provides a more detailed look at the relation between congruence and the indexes. It shows congruence by individual index values, or groups of values if the

²⁹ I also tried a multiplicative overall index to allow for the possibility that campaign finance regulation only matters if the full menu of reforms is adopted, but the difference remained insignificant.

Table 5. Congruence and election institutions.

Election law	Yes	No	<i>p</i> -Value for difference
Campaign finance			
Limits on contributions from corporations	57.3	62.3	0.319
Limits on contributions from individuals	58.9	56.0	0.695
Contribution limits index, more stringent than median (score >4 on 0–6 scale)	54.5	61.8	0.103
Public funding index, more than median (score >1 on 0–8 scale)	56.7	60.0	0.456
Disclosure index, more stringent than median (score >5 on 0–8 scale)	58.3	58.8	0.908
Overall index, greater than median (score >11 on 0–18 scale)	57.3	59.6	0.594
Open primaries	62.0	53.5	0.059
Redistricting by independent commission	54.0	59.8	0.297
Ballot access			
Major-party filing fees ≤ \$100	56.2	65.4	0.068
No signature requirement for major-party candidates	61.9	53.2	0.053
Recall allowed for statewide offices	55.0	60.6	0.221

Note. A state is congruent on a given issue if its policy choice corresponds to the preference of the majority. The main entries show the percentage of state-issues that are congruent. For example, 57.3 percent of state-issues are congruent when corporate contributions are limited. The last column reports the *p*-value for the hypothesis that the percentages are the same. There are 500 observations. “Open primaries” includes semi-open/closed primaries.

number of observations per value is small. The figures indicate that the lack of significance in Table 5 is not due to the somewhat arbitrary division of the sample into states above and below the median — the relation between congruence and the campaign indexes is simply not pronounced.

The news for reformers is better when it comes to primary election systems. States with open primaries show 9 percent greater congruence, a difference that can be distinguished from zero at about the 6 percent level. The multivariate analysis that follows suggests this difference is spurious, however.

Redistricting by commission does not appear to be associated with greater congruence. Mean congruence in states that redistrict using an independent commission is actually

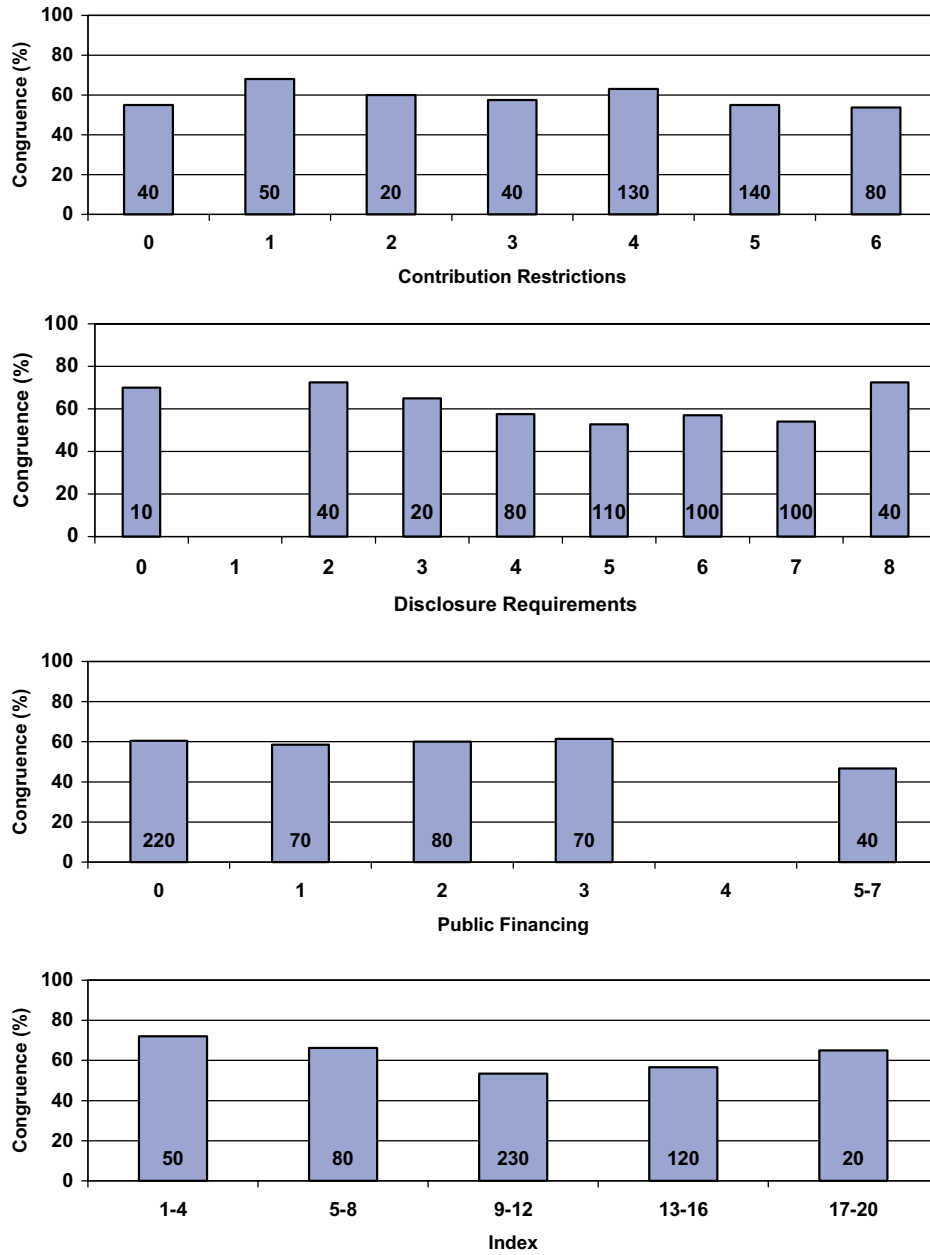


Figure 3. Congruence and campaign finance index values.
Note. Each figure shows the percent congruent by an index value or range of values. A state is congruent for a given issue if its policy choice corresponds to the preferences of the majority. The number of observations is reported inside each bar.

5.8 percent lower than in states where the legislature or other incumbent politicians draw the lines. The difference is not close to statistical significance.³⁰

For ballot access rules, I focus on two measures that Stratmann (2005) showed are correlated with the number of candidates. The first is whether major-party filing fees to place a candidate's name on the ballot are less than \$100. States with low filing fees show almost identical congruence as states with high filing fees, and the difference is not statistically different from noise. The other measure is whether signatures are required for major-party candidates to appear on the ballot. States without signature requirements are 9 percent more congruent, consistent with the reform view, and the difference is different from zero at about the 5 percent level.

The last row compares congruence in states that do and do not allow recall of state officials. The mean congruence is 6 percent lower in states that allow recall, contrary to expectations, but the difference is not statistically significant.

The overall impression from Table 5 and Figure 3 is that election institutions are not strongly associated with congruence. To check the robustness of this conclusion, Table 6 report logistic regressions that control for initiative and judicial retention status, and the other variables used above (size of majority, population, education, South dummy, age of state, and 10 issue dummies). Each column shows coefficients for a single regression. Regressions vary with respect to campaign finance variables: column (1) includes contribution limit dummies; column (2) includes contribution, disclosure, and public funding indexes; and column (3) includes the overall index. The estimates cast further doubt on the existence of a connection between these election institutions and congruence. None of the election laws are significantly associated with congruence in any specification. Primary election and ballot access rules are no longer reliably associated with congruence. However, the initiative continues to be a significant predictor of congruence. The judicial selection coefficient does not change materially, but remains shy of conventional statistical significance. The insignificant results also appear if observations with imputed values of opinion are deleted (not reported).

To summarize, the correlation between this set of election institutions favored by reformers and the 10 issues under investigation is strikingly small. An important limitation of this analysis that should be kept in mind is the endogeneity of election laws. Unlike initiative and judicial retention procedures, changes in state election laws are not rare, and several changes have occurred even in the last 10 years. It is possible that low congruence leads to adoption of certain laws, creating a bias against detection of a congruence effect.

³⁰ In these estimates, a state was defined to have a redistricting commission if there was a commission that had sole authority to draw the lines, that is, I excluded commissions that were advisory to the legislature or had authority only if the legislature failed to act in a timely manner. If the variable is defined to include advisory commissions, the results are unchanged (continue to show no effect). Some states make an attempt to represent both parties on the commission, prohibit membership by current officials, or prohibit commission members from running for office for a specified number of years. I was unable to find commission effects even after taking into account these variations.

Table 6. Logistic regressions of congruence as a function of election institutions.

Variables	(1)	(2)	(3)
Campaign finance			
Dummy = 1 if corporate contributions limited	-0.35 (0.46)
Dummy = 1 if individual contributions limited	-0.12 (0.34)
Contribution limits index (0-6)	...	-0.06 (0.07)	...
Public financing/spending limits index (0-7)	...	0.02 (0.06)	...
Disclosure index (0-7)	...	0.01 (0.07)	...
Overall index	-0.01 (0.03)
Dummy = 1 if open primaries	0.28 (0.25)	0.26 (0.25)	0.24 (0.25)
Dummy = 1 if redistricting by independent commission	0.15 (0.33)	0.10 (0.32)	0.07 (0.31)
Ballot access			
Dummy = 1 if major-party filing fees ≤\$100	-0.07 (0.32)	0.004 (0.32)	-0.03 (0.32)
Dummy = 1 if no signature requirement for major-party candidates	0.07 (0.26)	0.08 (0.28)	0.01 (0.26)
Dummy = 1 if recall allowed for statewide offices	-0.37 (0.26)	-0.38 (0.26)	-0.41 (0.26)
Dummy = 1 if (any) initiative allowed	0.86*** (0.27)	0.81*** (0.27)	0.82*** (0.26)
Dummy = 1 if judges stand for reelection	0.50 (0.36)	0.52 (0.34)	0.54 (0.34)
Size of majority	14.79*** (2.19)	14.64*** (2.19)	14.42*** (2.16)
Population (log)	-0.12 (0.14)	-0.10 (0.14)	-0.08 (0.14)
High school graduates, fraction of population	-6.51* (3.75)	-6.38* (3.83)	-5.67 (3.70)
Dummy = 1 if Southern state	0.62 (0.39)	0.64 (0.40)	0.68* (0.40)
Years since state entered Union (×100)	0.59 (0.37)	0.53 (0.36)	0.48 (0.35)

Note. A state is congruent on a given issue if its policy choice corresponds to the preference of the majority. Each column reports coefficients from a logistic regression that predicts the probability of a congruent state-issue. Standard errors are in parentheses beneath coefficient estimates. Each model was estimated with 500 observations. Each equation also included 10 dummy variables (and no constant), one for each of the issues; the coefficients are not reported. Significance levels are indicated as * = 10%, ** = 5%, *** = 1%.

DISCUSSION

This paper offers an approach to measuring the congruence between policy and public opinion, a central concept for evaluating the performance of democratic governments. In a sense the paper is walking a well-worn path because there are multiple streams of literature interested in the connection between policy and public opinion, both across space and across time. However, as several studies have shown, the method used to measure responsiveness in the existing literature — typically, the coefficient from a regression of policy on opinion — does not in fact reveal the extent to which policies reflect opinion, or allow comparisons of congruence across time or space. The approach developed in the paper is fairly simple to apply, but it does require issues with dichotomous outcomes. For a sample of 10 high-profile issues across the 50 states, I find congruent policies in 59 percent of observations, meaning that states chose policies opposed by a majority of citizens 41 percent of the time. This basic finding appears whether opinion is measured by considering all citizens, only those who vote, or only those with strong preferences. Whether this is surprising or not depends on one's priors about how democracy works in the states, and whether this finding generalizes to other issues remains to be seen, although the evidence in Lax and Phillips (2009b) provides some corroboration.

The results raise questions about the relevance of the median voter model for understanding policy formation when it comes to high-profile issues in the states. The failure to observe median voter outcomes for these particular issues suggest potential challenges for theoretical research in other areas in political economy where the median voter model has served as a workhorse, such as public finance.³¹ The evidence does not tell us what is the “right” model of policy formation for high-profile issues, but it does give some hints what such a model would look like. For one thing, the “right” model would need to deliver nonmajoritarian outcomes with some frequency. Several types of theories seem to fit at this general level, including interest group theories such as Stigler (1971), Peltzman (1976), and Grossman and Helpman (2001) that posit disproportionate influence of organized special interests; models in which politicians choose inefficient policies to signal to voters, such as Coate and Morris (1995) and Groseclose and McCarty (2000); and models in which voter concern over candidates' personal characteristics (“valence” or “character”) swamps concern over positions on issues, such as Groseclose (2001) and Kartik and McAfee (2007).

From a practical point of view, scholars and practitioners are interested in the design of institutions that promote responsiveness. Reformers have advanced a host of election laws designed to increase competition and ultimately responsiveness, such as public funding of campaigns and commission-based redistricting. This paper provides an assessment of the connection between congruence and a variety of institutions for a set of high-profile issues. Two institutions emerge as having a significant effect on congruence: the initiative process appears to increase congruence by 18–19 percent, and requiring judges to stand

³¹ Median voter models have long been used to explain tax rates and the size of government (e.g., Meltzer and Richard, 1981; Krusell and Rios-Rull, 1999) and local provision of public goods (Fischel, 2001, Ch. 4), policy decisions that do not appear in my sample.

for election appears to increase congruence by 11–13 percent (although the judge effect is not statistically significant in all specifications). In contrast, virtually all of the changes sought by reformers have little or no connection with congruence. To keep this study a manageable size, I have not reported results on the connection between congruence and political competition, political parties, legislative structure, and bureaucracies, but exploratory estimates were not encouraging. This would seem to be a natural direction for future research.

The paper considers congruence at a point of time (essentially using 10 concurrent cross-sections). Another interesting direction for future research would be to study the evolution of congruence over time. The issues in the sample are different from many other issues because they are sufficiently controversial or topical to merit inclusion in the ANES. Presumably over time some of these issues become “settled” and congruence increases for them. The finding that some institutions are associated with higher congruence in the sample then could mean that they allow the majority to assert its will over policy more quickly, rather than that the institutions are inherently more favorable to majorities. For versions of this argument as applied to the initiative process, see Matsusaka (2004, Ch. 6) and Lupia *et al.* (2009).

It should be noted in closing that congruence is not the only feature of democracy that we might care about. Campaign contribution limits might not increase congruence but may still serve a useful purpose if they reduce corruption, for example. Moreover, congruence is undesirable in some situations, such as when a majority seeks to exploit a numerical minority or perhaps when the minority has intense preferences and the majority is close to being indifferent about the outcome. Evidence that a particular institution increases congruence does not mean that adoption of the institution is unambiguously desirable. However, congruence — majority rule — is a central value of democracy, and probably should not be forsaken without a clear benefit.

APPENDIX

A. Imputed Opinion

Opinion data were taken from various years of the American National Election Studies (ANES), as indicated in Table 1. For state-issues with few survey responses, opinion was imputed using the following algorithm. (I also imputed Pennsylvania opinion on public funding of abortion because opinion was split exactly 50–50 in the ANES.)

Step 1. For each issue, I estimated exploratory regressions in which the dependent variable was the percentage in favor, and the explanatory variable was a measure of ideology. I considered three different ideology measures, “citizen ideology” and “government ideology” from Berry *et al.* (1998) (calculated as an average over the period 1990–1999, taken from ICPSR Study 1208, “Measuring Citizen and Government Ideology in the United States”) and the “state ideology index” (= percent liberal minus percent conservative) from Erikson *et al.* (1993), kindly provided by Robert Erikson. Initially, I included a state in the regression if it had at least 60 survey responses in the ANES,

but I also considered regressions with 30-respondent and 90-respondent cutoffs. From the nine exploratory regressions for each issue (three ideology measures times three cutoff points), I chose the model with the best fit based on R^2 . The final imputation models were based on a cutoff of 60 respondents except for English only, estate tax, and same-sex marriage, where the cutoff was 30 respondents. The final imputations used the Berry *et al.* “government ideology” measure except for estate tax and term limits, which used “citizen ideology.” Although the Erikson, Wright, and McIver measure provided a slightly better fit for a few issues, the improvement was too small to justify the loss of Alaska and Hawaii when using that measure.

Step 2. I used the estimated coefficients from the final regression together with the appropriate ideology measure to impute scores for states with fewer than 50 observations. The estimates in the paper were repeated using cutoff values of 30, 40, 60, and 70 observations, with very similar findings.

B. Policy Data

For each policy, I consulted at least two sources, and when discrepancies arose, consulted additional sources or the state’s constitution and statutes. The numbers in this paper are as of 2006.

1. *Death penalty.* 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 a moratorium since 2000 and New Jersey has a moratorium until 2007, but they were counted as death penalty states because the moratoriums are presumably temporary.

2. *English only.* 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 Hawaii), Louisiana (English and French), and New Mexico (English and Spanish) were not counted as English-only.

3. *Estate tax.* Did the state levy an estate or inheritance tax? Primary sources were McNichol (2006) and Feldman (2005). Information was double-checked on various tax sites and state web pages.

4. *Job discrimination against homosexuals.* Did the state prohibit private sector job discrimination based on sexual orientation? The primary data source was Human Rights Campaign Foundation (2005, 2006). Oregon’s law did not contain the phrase “sexual orientation” but courts interpreted it to prohibit such discrimination since *Tanner v. Oregon Health Sciences University*, 1988, so Oregon is classified as having a prohibition.

5. *Late-term abortions.* Did the state prohibit certain “late-term” or “partial birth” abortions? Primary sources were Guttmacher Institute (2006) and Center for Reproductive Rights (2004).

6. *Parental notification for minor’s abortion.* Did 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.

7. *Public funding of abortion.* Did the state use public funds to pay for medically necessary abortions for Medicaid enrollees? Primary sources were Guttmacher Institute (2006) and NARAL Pro-Choice America (2006).

8. *Same-sex marriage.* Did state law or the constitution define 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 defined marriage as only between a man and a woman. I classified it as having a law.

9. *School prayer (moment of silence).* Did the state mandate or allow a moment or period of silence during the school day? Primary data sources were Education Commission of the States (2000) and Marshall (2001). Discrepancies were resolved by reading statutes. States that allowed a moment of silence only before the start of the school day (New Hampshire, New Jersey) or allowed only student-initiated moments of silence (New Mexico) were counted as not having a law.

10. *Term limits on state legislators.* Did the state impose term limits on its state legislators? Primary data source was U.S. Term Limits (no date). The ANES question on this issue asks about term limits on congressmen rather than state legislators. I assume that supporters of congressional term limits also support limits for state legislators, and conversely. Individual states cannot limit congressional terms following the Supreme Court decision in *U.S. Term Limits v. Thornton*, 514 US 779 (1995).

C. Election Law Data

1. *Campaign finance.* Campaign finance indexes based on 2002 laws are from Table 1 in Witko (2005). Information on individual and corporate contribution limits is from Federal Election Commission (2002, Chart 2-A) and National Conference of State Legislatures (2005).

2. *Primaries.* A state had a "closed" primary election if only previously declared party members could vote in that party's primary. The other states allowed voters to choose which party's primary to participate in on the day of the election regardless of previously declared affiliation (open primary) or allowed independents and undeclared voters (but not members of other parties) to participate in a party primary of their choice (semi-closed primary). In Utah (since 2002) and West Virginia, the majority party holds a closed primary and the minority party holds an open primary. Both states were classified as having closed primaries. Wyoming has a closed primary, but voters can change their registration on election day, so it is classified as an open primary state. Data were taken from Federal Election Commission (2003), FairVote (no date), and various state election web sites.

3. *Recall.* A state allows recall if citizens can remove a statewide official from office before his or her term ends by petition and then majority voter. Eighteen states (AK, AZ, CA, CO, GA, ID, KS, LA, MI, MN, MT, NV, NJ, ND, OR, RI, WA, WI) allow recalls. Data sources were Council of State Governments (2004, Table 6.16) and National Conference of State Legislatures (2006).

4. *Redistricting by commission.* A state redistricted by commission if district lines for the state legislature were drawn by an independent nonpartisan or bipartisan commission rather than the legislature or other elected officials for the post-2000 Census round of redistricting (AK, AZ, CO, HI, ID, IA, MO, MT, NJ, PA, WA). Commissions that were advisory to the legislature (IA, ME, VT) or responsible only if the legislature failed to redistrict on time (CT, IL, MS, OK, TX) were excluded. Data sources were National Conference of State Legislatures (1999) and the Campaign Legal Center (2006).

5. *Ballot access.* Ballot access was measured in two ways: whether the filing fee for major-party candidates was greater than \$100 (10 percent of the sample), and whether major party candidates were required to collect signatures to qualify for the ballot (41 percent of the sample). The data were from Stratmann (2005), kindly provided by Thomas Stratmann.

D. Other Data

Initiative status is from Matsusaka (2004). Following standard practice, Illinois was coded as a noninitiative state because its process is severely limited in scope.

Judicial selection data as of 2004 were taken from American Judicature Society (2004), Hanssen (2004), and Besley and Payne (2006). Classifications apply to the court of last resort, although appellate courts in the same states tend to use the same selection procedures. No state classification changed during 1990–2006 except Tennessee, which shifted from initial election to initial appointment in 1994. 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 classify these states as using elections to make the initial selection.

Population in 2003 is from the Bureau of the Census. Education is fraction of the population over the age of 25 that has a high school degree as of 2000, taken from *Statistical Abstract of the United States*, 2006, Table 218. South dummy is equal to one for AL, AR, FL, GA, LA, MS, NC, SC, TN, TX, VA.

REFERENCES

- Abramowitz, A. I., B. Alexander, and M. Gunning. 2006. "Incumbency, Redistricting, and the Decline of Competition in U.S. House Elections." *Journal of Politics* 68(February): 75–88.
- Achen, C. H. 1977. "Measuring Representation: Perils of the Correlation Coefficient." *American Journal of Political Science* 21(November): 805–815.
- American Judicature Society. 2004. *Judicial Selection in the States: Appellate and General Jurisdiction Courts*. www.ajs.org/selection/sel_stateselect.asp (April 19, 2010).
- Bafumi, J. and M. C. Herron. 2009. "Leapfrog Representation and Extremism: A Study of American Voters and Their Members of Congress." Dartmouth College. Manuscript.
- Baron, D. P. and J. A. Ferejohn. 1989. "Bargaining in Legislatures." *American Political Science Review* 83(December): 1181–1206.
- Barro, R. J. 1973. "The Control of Politicians: An Economic Model." *Public Choice* 14: 19–42.
- Bergstrom, T. C. and R. P. Goodman. 1973. "Private Demands for Public Goods." *American Economic Review* 63(June): 280–296.
- Berry, W. D., E. J. Ringquist, R. C. Fording, and R. F. Hanson. 1998. "Measuring Citizen and Government Ideology in the American States, 1960–1993." *American Journal of Political Science* 42(January): 327–348.

- Besley, T. and A. Case. 2003. "Policy Institutions and Policy Choices: Evidence from the United States." *Journal of Economic Literature* 41(March): 7–73.
- Besley, T. and A. Abigail Payne. 2006. "Implementation of Anti-Discrimination Policy: Does Judicial Selection Matter?" London School of Economics. Manuscript.
- Borcherding, T. E. and R. T. Deacon. 1972. "The Demand for the Services of Non-Federal Governments." *American Economic Review* 62(December): 891–901.
- Broder, D. S. 2000. *Democracy Derailed: Initiative Campaigns and the Power of Money*. New York: Harcourt, Inc.
- Buchanan, J. M. and Gordon Tullock. 1962. *The Calculus of Consent: Logical Foundations of Constitutional Democracy*. Ann Arbor: University of Michigan Press.
- Cain, B. E. and E. R. Gerber, eds. 2006. *Voting at the Political Fault Line: California's Experiment with the Blanket Primary*. Berkeley: University of California Press.
- Campaign Legal Center. 2006. "Redistricting." www.campaignlegalcenter.org.
- Campbell, A., P. E. Converse, W. E. Miller, and D. E. Stokes. 1960. *The American Voter*. New York: Wiley.
- Center for Reproductive Rights. 2004. "So-Called 'Partial-Birth Abortion' Ban Legislation: By State." http://reproductiverights.org/sites/default/files/documents/pub_bp_pba_bystate.pdf (April 19, 2010).
- Coate, S. 2004. "Pareto Improving Campaign Finance Policy." *American Economic Review* 94(June): 628–655.
- Coate, S. and S. Morris. 1995. "On the Form of Transfers to Special Interests," *Journal of Political Economy* 103(December): 1210–1235.
- Council of State Governments. 2004. *The Book of States*. Volume 36. Lexington, KY: Council of State Governments.
- Cox, G. W. and M. D. McCubbins. 2005. *Setting the Agenda: Responsible Party Government in the U.S. House of Representatives*. New York: Cambridge University Press.
- Death Penalty Information Center. 2006. "Facts about the Death Penalty," May 12, 2006, www.deathpenaltyinfo.org/FactSheet.pdf (May 12, 2006).
- Downs, A. 1957. *An Economic Theory of Democracy*. New York: Harper & Row.
- Education Commission of the States. 2000. "School Prayer, Moment of Silence, Other Policies Concerning Religion." www.ecs.org/clearinghouse/13/38/1338.htm (April 19, 2010).
- Erikson, R. S., G. C. Wright, and J. P. McIver. 1993. *Statehouse Democracy: Public Opinion and Policy in the American States*. Cambridge, UK: Cambridge University Press.
- FairVote. No date. "Primaries: Open and Closed." www.fairvote.org/?page=1801 (April 19, 2010).
- Federal Election Commission. 2002. *Campaign Finance Law 2002: A Summary of State Campaign Finance Laws with Quick Reference Charts*. www.fec.gov/pubrec/cfl/cfl02/cfl02.shtml (April 19, 2010).
- Federal Election Commission. 2003. *NVRA Application Form*. www.fec.gov/votregis/vr.shtml (June 2006).
- Feldman, A. 2005. "Fifty States, A Thousand New Tax Laws," *Inc.com*. pf.inc.com/magazine/20050601/taxes.html.
- Ferejohn, J. 1986. "Incumbent Performance and Electoral Control." *Public Choice* 50: 5–25.
- Fischel, W. A. 2001. *The Homevoter Hypothesis: How Home Values Influence Local Government Taxation, School Finance, and Land-Use Policies*. Cambridge, MA: Harvard University Press.
- Gelman, A. and G. King. 1994. "Enhancing Democracy Through Legislative Redistricting." *American Political Science Review* 88(September): 541–559.
- Gerber, E. R. 1996. "Legislative Response to the Threat of Popular Initiatives." *American Journal of Political Science* 40(February): 99–128.
- Gerber, E. R. and A. Lupia. 1995. "Campaign Competition and Policy Responsiveness in Direct Legislation Elections." *Political Behavior* 17(September): 287–306.
- Gerber, E. R., A. Lupia, M. D. McCubbins, and D. R. Kiewiet. 2001. *Stealing the Initiative: How State Government Responds to Direct Democracy*. Upper Saddle River, NJ: Prentice Hall.
- Gerber, E. R. and R. B. Morton. 1998. "Primary Election Systems and Representation." *Journal of Law, Economics, and Organization* 14(Fall): 304–324.

- Gilligan, T. W. and J. G. Matsusaka. 2006. "Public Choice Principles of Redistricting." *Public Choice* 129(December): 381–398.
- Griffin, J. D. and B. Newman. 2005. "Are Voters Better Represented?" *Journal of Politics* 67(November): 1206–1227.
- Groseclose, T. 2001. "A Model of Candidate Location When One Candidate Has a Valence Advantage." *American Journal of Political Science* 45(October): 862–886.
- Groseclose, T. and N. M. McCarty. 2000. "The Politics of Blame: Bargaining before an Audience." *American Journal of Political Science* 45(1): 100–119.
- Grossman, G. M. and E. Helpman. 2001. *Special Interest Politics*. Cambridge, UK: MIT Press.
- Guttmacher Institute. 2006. "State Policies in Brief: An Overview of Abortion Laws." www.guttmacher.org/statecenter/spibs/spib_OAL.pdf (June 1, 2006).
- Hanssen, F. A. 1999a. "The Effect of Judicial Institutions on Uncertainty and the Rate of Litigation: The Election versus Appointment of Judges." *Journal of Legal Studies* 28 (January): 205–232.
- Hanssen, F. A. 1999b. "Appointed Courts, Elected Courts, and Public Utility Commissions: Judicial Independence and the Energy Crisis." *Business and Politics* 1(April): 179–201.
- Hanssen, F. A. 2000. "Independent Courts and Administrative Agencies: An Empirical Analysis of the States." *Journal of Law, Economics, and Organization* 16(October): 534–571.
- Hanssen, F. A. 2004. "Is There a Politically Optimal Level of Judicial Independence?" *American Economic Review* 94(June): 712–729.
- Houser, D. L. and T. Stratmann. 2008. "Selling Favors in the Lab: Experiments on Campaign Finance Reform." *Public Choice* 136(July): 215–239.
- Human Rights Campaign Foundation. 2005. *The State of the Workplace for Lesbian, Gay, Bisexual and Transgender Americans, 2004*, Washington D.C.
- Human Rights Campaign Foundation. 2006. "Non-Discrimination Laws: State by State." www.hrc.org/Template.cfm?Section=Get_Informed2&Template=/TaggedPage/TaggedPageDisplay.cfm&TPLID=66&ContentID=20650 (June 30, 2006).
- Jacobson, G. C. 2006. "Competition in U.S. Congressional Elections." In *The Marketplace of Democracy: Electoral Competition and American Politics*, M. P. McDonald and J. Samples (eds.), Washington D.C.: Brookings Institution Press.
- Jones, B. S. and B. Norrander. 1996. "The Reliability of Aggregated Public Opinion Measures." *American Journal of Political Science* 40(February): 295–309.
- Kalt, J. P. and M. A. Zupan. 1984. "Capture and Ideology in the Economic Theory of Politics." *American Economic Review* 74: 279–300.
- Kartik, N. and R. Preston McAfee. 2007. "Signaling Character in Electoral Competition." *American Economic Review* 97(June): 852–870.
- Kau, J. B. and P. H. Rubin. 1979. "Self-interest, Ideology and Logrolling in Congressional Voting." *Journal of Law and Economics* 22: 365–384.
- Kaufmann, L. J. 2004. "Parental Consent or Notification for Abortion." National Council of State Legislatures. www.ncsl.org/programs/health/adolabor.htm.
- Klerman, D. M. and P. G. Mahoney. 2005. "The Value of Judicial Independence: Evidence from Eighteenth Century England." *American Law and Economics Review* 7(1): 1–27.
- Krusell, P. and J.-V. Rios-Rull. 1999. "On the Size of U.S. Government: Political Economy in the Neoclassical Growth Model." *American Economic Review* 89(December): 1156–1181.
- La Porta, R., F. Lopez-de-Silanes, C. Pop-Eleches, and A. Shleifer. 2004. "Judicial Checks and Balances." *Journal of Political Economy* 112(April): 445–470.
- Lax, J. R. and J. H. Phillips. 2009a. "Gay Rights in the States: Public Opinion and Policy Responsiveness." *American Political Science Review* 103(August): 367–386.
- Lax, J. R. and J. H. Phillips. 2009b. "Explaining Democratic Performance in the States." Columbia University. Manuscript.
- Lim, C. S. H. 2008. "Turnover and Accountability of Appointed and Elected Judges." Stanford Graduate School of Business. Manuscript.
- Lupia, A., Y. Krupnikov, A. S. Levine, S. Piston, and A. V. Hagen-Jamar. 2009. "Why State Constitutions Differ in Their Treatment of Same-Sex Marriage." *Journal of Politics* Forthcoming.
- Lupia, A. and J. G. Matsusaka. 2004. "Direct Democracy: New Approaches to Old Questions." *Annual Review of Political Science* 7: 463–482.

- Lupia, A. and M. D. McCubbins. 1998. *The Democratic Dilemma: Can Citizens Learn What They Need to Know?* Cambridge, UK: Cambridge University Press.
- Marshall, J. 2001. "Moment of Silence: State by State." Family Research Council. www.frc.org/get.cfm?i=IF01K2 (June 30, 2006).
- Matsusaka, J. G. 2001. "Problems with a Methodology Used to Test the Responsiveness of Policy to Public Opinion in Initiative States." *Journal of Politics* 63(November): 1250–1256.
- Matsusaka, J. G. 2004. *For the Many or the Few: The Initiative, Public Policy, and American Democracy*. Chicago: University of Chicago Press.
- Matsusaka, J. G. 2005. "Direct Democracy Works." *Journal of Economic Perspectives*. 19(Spring): 185–206.
- Matsusaka, J. G. 2009. "Direct Democracy and Public Employees." *American Economic Review*. 99(December): 2227–2246.
- Matsusaka, J. G. and N. M. McCarty. 2001. "Political Resource Allocation: Benefits and Costs of Voter Initiatives." *Journal of Law, Economics, and Organization* 17(October): 413–448.
- Mayhew, D. R. 1974. "Congressional Elections: The Case of the Vanishing Marginals." *Polity* 6(Spring): 295–317.
- McCarty, N. M. 2000. "Presidential Pork: Executive Veto Power and Distributive Politics." *American Political Science Review* 94: 117–129.
- McDonald, M. P. and J. Samples, eds. 2006. *The Marketplace of Democracy: Electoral Competition and American Politics*. Washington D.C.: Brookings Institution Press.
- McNichol, E. C. 2006. "Many States Are Decoupling from the Federal Estate Tax Cut," Center on Budget and Policy Priorities. www.cbpp.org/5-23-02sf.pdf (April 19, 2010).
- Meltzer, A. H. and S. F. Richard. 1981. "A Rational Theory of the Size of Government." *Journal of Political Economy* 89(October): 914–927.
- Miller, W. E. and D. E. Stokes. 1963. "Constituency Influence in Congress." *American Political Science Review* 57(March): 45–56.
- NARAL Pro-Choice America. 2006. "Who Decides? The Status of Women's Reproductive Rights in the United States." www.prochoiceamerica.org/choice-action-center/in_your_state/who-decides (April 19, 2010).
- National Conference of State Legislatures. 1999. *Redistricting Law 2000* (Appendix E). www.senate.leg.state.mn.us/departments/scr/redist/red2000/red-tc.htm (April 19, 2010).
- National Conference of State Legislatures. 2005. "Contribution Limits (50-State Charts)." www.ncsl.org/programs/legman/about/ContribLimits.htm (June 30, 2006).
- National Conference of State Legislatures. 2006. "Recall of State Officials." www.ncsl.org/LegislaturesElections/ElectionsCampaigns/RecallofStateOfficials/tabid/16581/Default.aspx (April 19, 2010).
- Olson, M. 1965. *The Logic of Collective Action: Public Goods and the Theory of Groups*. Cambridge, MA: Harvard University Press.
- Peltzman, S. 1976. "Toward a More General Theory of Regulation." *Journal of Law and Economics* 19 (August): 211–240.
- Peltzman, S. 1984. "Constituent Interest and Congressional Voting." *Journal of Law and Economics* 27(April): 181–210.
- Peterson, K. 2005a. "Death Penalty: 34 States Permit Executions." stateline.org. April 19, 2005. www.stateline.org/live/ViewPage.action?siteNodeId=136&contentId=25995.
- Peterson, K. 2005b. "50-State Rundown on Gay Marriage Laws." stateline.org. November 23, 2005. www.stateline.org/live/ViewPage.action?siteNodeId=136&contentId=15576.
- Pommerehne, W. W. 1978. "Institutional Approaches to Public Expenditure: Empirical Evidence from Swiss Municipalities." *Journal of Public Economics* 9(April): 255–280.
- Prat, A. 2002. "Campaign Advertising and Voter Welfare." *Review of Economic Studies* 69(October): 999–1017.
- Primo, D. M., J. Milyo, and T. Groseclose. 2006. "State Campaign Finance Reform, Competitiveness, and Party Advantage in Gubernatorial Elections." In *The Marketplace of Democracy: Electoral Competition and American Politics*, M. P. McDonald and J. Samples (eds.), Washington D.C.: Brookings Institution Press.

- ProEnglish. n.d. "English as Our Official Language." www.proenglish.org/issues/offeng/states.html (April 19, 2010).
- Romer, T. and H. Rosenthal. 1979. "The Elusive Median Voter." *Journal of Public Economics* 12(October): 143–170.
- Samples, J. 2006. *The Fallacy of Campaign Finance Reform*. Chicago: University of Chicago Press.
- Stigler, G. J. 1971. "The Theory of Economic Regulation." *Bell Journal of Economics and Management Science* 2(Spring): 3–21.
- Stimson, J. A., M. B. MacKuen, and R. S. Erikson. 1995. "Dynamic Representation." *American Political Science Review* 89(September): 543–565.
- Stratmann, T. 2005. "Ballot Access Restrictions and Candidate Entry in Elections." *European Journal of Political Economy* 21(March): 59–71.
- Stratmann, T. and F. J. Aparicio-Castillo. 2006. "Competition Policy for Elections: Do Campaign Contribution Limits Matter?" *Public Choice* 127(April): 177–206.
- Traditional Values Coalition. 2005. "Survey of States with Marriage Amendments." www.traditionalvalues.org/marriage_protection_amendments.php (June 30, 2006).
- U.S. English, Inc. 2005. "Fact Sheets: States with Official English." www.us-english.org/inc/official/fact_sheets/db_stateswithlaws.asp (April 19, 2010).
- U.S. Term Limits. n.d. "State Legislative Term Limits." www.us-english.org/Current_Info/current_info.html (June 30, 2006).
- Weingast, B. R., K. A. Shepsle, and C. Johnsen. 1981. "The Political Economy of Benefits and Costs: A Neoclassical Approach to Distributive Politics." *Journal of Political Economy* 93: 642–664.
- Westley, C., P. T. Calcagno, and R. Ault. 2004. "Primary Election Systems and Candidate Evaluation." *Eastern Economic Journal* 30(Summer): 365–376.
- Witko, C. 2005. "Measuring the Stringency of State Campaign Finance Regulation." *State Politics and Policy Quarterly* 5(Fall): 295–310.