Popular Control of Public Policy[*]

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This paper studies policy choices and public opinion on 10 high profile issues across the 50 states. States chose the median voter outcome (which for these issues is also the majoritarian outcome) for only 59 percent of the 500 state-issue observations. Median voter (majoritarian) outcomes were 17 to 19 percent more likely when direct democracy was allowed, and 12 to 14 percent more likely when judges were required to stand for reelection. The likelihood of a median voter (majoritarian) outcome was not correlated with a variety of election laws, including campaign contribution limits, public funding of campaigns, and commission-based redistricting. Despite its prevalence in political economy theory, the median voter model appears to have limited explanatory power for these issues.

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1. Introduction

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 candidates can bring policy decisions into alignment with the preferences of the median voter, but the predominant theme of political economy research is the many obstacles that stand in the way of citizen control, such as uninformed voters (Campbell et al., 1960), special interest groups (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 very little statistical evidence on the amount of congruence between preferences and policy that actually prevails, and how institutions affect the amount of congruence. Numerous studies, including the well known contributions of Erikson et al. (1993), Borcherding and Deacon (1972), and Bergstrom and Goodman (1973), 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. While such evidence shows that policies respond at the margin to changes in opinion, several articles have shown that it is not possible to determine the overall level of congruence without a direct measure of preferences (for example, Romer and Rosenthal (1979) and Erikson et al. (1993, pp. 92-94)). Moreover, although the magnitude of the correlation between policy and opinion is sometimes used to compare the effectiveness of alternative institutions, such inferences cannot be justified without a direct measure of preferences (Matsusaka, 2001).[1]

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[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 of legislators. These studies speak to the issue of constituent-legislator representation. However, they have less to say about the connection between preferences and *policy*

This paper proposes a simple, nonparametric approach to measuring policy congruence, and uses it to assess the performance of representative democracy in the American states and identify factors that influence the amount of congruence. 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, I 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). I then collect survey information in which citizens register a direct preference on the issue ("Are you in favor of or opposed to the death penalty?") Since the issue is dichotomous, there is a unique outcome that the majority prefers, which is also the median citizen outcome. The majority/median position provides a plausible measure of what "the public" prefers. I then compare the majority/median position with the actual policy that prevails. In each state, the actual policy can be "congruent" (the outcome favored by the majority) or noncongruent (the outcome favored by the minority).

The long-running American National Election Studies survey allows recovery of public opinion information on 10 different issues for all 50 states during the last 15 years. The set of issues is not representative of everything that government does, but spans a variety of policies that have received significant popular and scholarly attention. For these 500 state-issue observations, I find that policy choices were congruent 59 percent of the time. Since congruent choices would arise 50 percent of the time with random policymaking, it seems that public opinion plays a role in policy but perhaps less than expected. The evidence suggests that the popular median voter model is unlikely to be a good approximation when studying such high profile issues like these.

The core of the paper is an investigation into the factors that determine the amount of congruence. Legislatures take the lead in much policymaking, but all states have institutions that can check and override the legislature. One institution that has been growing in importance over the last 30 years is the initiative process, a form of direct

because policy outcomes depend on much more than roll call votes, for example, how those votes are aggregated, which issues are put to a vote, whether the executive exercises a view, and how courts behave.

democracy that allows voters to propose and pass laws and constitutional amendments without involving their representatives (Matsusaka, 2005). The initiative was introduced in the American states in the late nineteenth century by the Progressives, and is now available in 24 states. The Progressives promoted the initiative in the belief that it would 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 have argued that in fact the initiative empowers special interests that are able to mobilize financial and organizational resources in support of ballot proposition campaigns (Broder, 2000; Lupia and Matsusaka, 2004). This paper provides what I believe is the first direct evidence on how direct democracy affects congruence, and shows that policies are approximately 17 to 19 percent more congruent in initiative than noninitiative states.

Courts provide another check on the legislature. Courts can overrule policy decisions of legislators and ballot propositions, and courts have been important for several of the issues investigated in this paper. One function of courts is to counterbalance majorities that threaten the rights of citizens, that is, to prevent congruence when civil rights are at risk. Another function is to control the legislature, executive, and agencies when they overstep their constitutional bounds (Hanssen, 2000; La Porta et al., 2004). If the other branches of government are unduly responsive to special interests, the "counterbalancing" function of the courts would increase congruence. On the other hand, if judges overrule majority decisions in order to protect basic rights (or implement their personal policy preferences), counterbalancing could reduce congruence. 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). In order to assess the importance of judges, I compare congruence in states where judges must stand for re-election to those where they have life terms or are reappointed by the governor or legislature. I find that congruence is approximately 12 to 14 percent higher when judges must stand for re-election. Independent judges appear to be an important source of noncongruence in the states.

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. Persson and Tabellini (2003), for example, document a link between

economic performance and how a legislature is organized and elected in a cross-national sample. 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), 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. From the viewpoint of reformers who want to improve congruence, the evidence seems to suggest that the largest gains in congruence come from improving institutions that check the legislature rather than trying to improve responsiveness of the legislature.

The evidence in this paper is related to several strands of the political economy literature. First, the evidence seems to support theories implying that significant gaps can appear between citizen preferences and public policy. Representation failures have been posited due to interest group influence (Stigler, 1971; Peltzman, 1976; Grossman and Helpman, 2001), 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), gerrymandering (Gilligan and Matsusaka, 2006), the executive veto (Cameron, 2000; McCarty, 2000), and independence of administrative agencies (Gerber et al., 2001). At the same time, the high propensity of non-median voter outcomes (41 percent of the time) raises questions about political economy models that use the median voter to explain policy outcomes, at least for the type of high profile issues contained in the sample.[2]

The paper is also related to the large and growing literature on policy effects of institutions (surveyed by Besley and Case (2003)). For example, Matsusaka (1995, 2004) documents that initiative states spent and taxed less than noninitiative states over the last 30 years and Hanssen (1999b) shows that appointed judges decide cases differently than elected judges. While it is clear that institutions matter for policy outcomes, there is little evidence on the extent to which voter interests are promoted or hindered by the policy

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

changes brought about by the institutions. This paper complements the institutions-policy literature by providing evidence on how well certain institutions advance the interests of the majority, and thus takes some steps toward a normative analysis of institutions.

The paper proceeds as follows. Section 2 outlines 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, and the develops the measure of congruence used in this paper. Section 3 describes the data and policy issues. Section 4 reports the overall level of congruence for the issues in the sample. Section 5 explores the connection between congruence and direct democracy, judicial independence, and election laws. Section 6 concludes.

2. Measuring Congruence

Since this paper is ultimately about measuring the "congruence" between public opinion and policy, it is important to understand the complications involved in measuring congruence, and the limitations of previous work in this area. A direct, nonparametric measure of "congruence" between policy and preferences in state s is

(1) $| y_s - y_s^* |,$

where y_s is a state's chosen policy and y_s^* 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_s^* 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 practice, measuring congruence using (1) is difficult because of the lack of data on y_s^*. If y_s^* 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 ratings. 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. As a result, I am not aware of any statistical study that directly measures congruence as defined in (1).

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

(2) $y_s = a + bI_s + e_s$,

where I_s 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, tax rates might be regressed on an index of liberalness or on a set of demographic variables. Several studies find nonzero value for b, indicating that changes in opinion are associated with changes in policy.[3] However, because y and I are in different metrics, there is no way to tell from the coefficients a and b 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, far away from what voters want, even though they respond at the margin to changes in opinion.

Perhaps more critically, congruence cannot be compared across jurisdictions using (2), so it is not possible to determine what factors enhance congruence. 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. Some studies have taken such an approach (see Matsusaka (2001) for examples) but it is invalid. The reason hinges on the lack of a common metric between policy and opinion, or seen in a different way, the fact that we do not know the precise mapping between y^* and I. To illustrate the problem, Figure 1 shows a hypothetical mapping from I to y^*, labeled f. The mapping f (unknown to the researcher) indicates the preferred policy of (say) a state with opinion I; observations that lie on f would be perfectly congruent. The cluster of points X represents

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[3] Perhaps the best known examples are Erikson et al. (1995), Borcherding and Deacon (1972), and Bergstrom and Goodman (1973).

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policy-opinion observations under one set of institutions and the cluster labeled Z represents observations under another set of institutions. 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 (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 (2) can be consistent with X being more or less congruent than Z. In short, despite their prevalence in the literature, estimates of (2) are not useful in identifying the factors that influence congruence without knowledge of f.[4] This means that we do not appear to have any rigorous empirical evidence that provides a valid estimate of the amount of congruence in practice.

The way I propose out of this thicket, and the central innovation of the study, is to work with equation (1) instead of (2). To do so requires a direct measure of y_s^*. Fortunately, many of the complications associated with measuring y_s^* can be avoided if we focus on issues that have two outcomes ("yes" and "no") rather than a continuum of outcomes.[5] When there are only two outcomes, the policy favored by the majority is

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[4] 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 proxy for preferences using linear combinations of economic and demographic variables. Achen (1977) identifies a different problem with approaches based on (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 the points are to y^*.

[5] This is a somewhat subjective distinction that seems to "work" for many issues but should not be taken literally. Every issue I classify as having two outcomes could be thought of as having more dimensions. For example, proponents of the death penalty might disagree about whether it should apply to minors, which crimes it should be applied to, and so on. It is worth noting that even if people's views on these issues are not dichotomous, there is no reason to expect the raw data (which are dichotomous) to be biased in either direction. Thus, the main effect on measured congruence is likely to be noise, biasing regression coefficients toward zero, and strengthening confidence in the coefficients that are estimated to be nonzero.

unambiguously identified, and corresponds to the median voter outcome.[6] My approach

is to define y_s^* as the policy preferred by the majority/median voter. One can think of

other definitions of y_s^*, but majority rule and the median voter are dominant concepts in

theoretical political economy, and seem to provide a good starting point for studying

congruence. More specifically, I identify a set of issues with dichotomous outcomes for

which opinion data are available, use the opinion data to determine the

majoritarian/median outcome, and then calculate congruence directly by comparing the

majoritarian/median outcome with each state's policy outcome. For each state and each

issue, the policy is either congruent with the majority/median or noncongruent. The mean

congruence over a sample of $i = 1,...,I$ observations is then $\frac{1}{I}\sum_{i=1}^{I}(1- | y_i - y_i^* |)$, where I

have redefined a congruent observation as 1 and a noncongruent observation as 0 (as

opposed to (1), which assigns 1 to a noncongruent outcome and 0 to a congruent

outcome).[7]

3. Data and Policy Issues

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 "supported" 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

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[6] The aim of this paper is positive not normative – the goal is to understand when the majority rules – but it

may be worth noting in passing that the majoritarian/median voter outcome in this context also happens to

be equal to the utility maximizing outcome if we assume that 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. Of

course, it is not obvious that utility maximization is the appropriate welfare criterion, particularly for issues

that are perceived to involve civil rights. I also do not attempt to take into account intensity of preferences,

mainly because of data limitations, but also because the one-person one-vote principle of democracy is

purported to treat each person equally, and my purpose is to examine when the majority does in fact rule.

[7] Note that my definition of congruence does not attempt to take into account intensity of preferences. This

is mainly because of data limitations, but also because the one-person one-vote principle of democracy is

purported to treat each person equally, and my purpose is to examine when the majority does in fact rule.

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.

For each state, I calculated the opinion for and against each policy to determine the majority/median position.[8] 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 no responses or too few responses to yield a reliable estimate of state opinion. 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 the basic procedure was to estimate a regression $I_s^{ANES} = \alpha + \beta I_s^{BERRY} + u$ for those states with reliable opinion information (typically defined as states with 60 or more observations), where I_s^{ANES} is the ANES opinion score of state s and I_s^{BERRY} is the state's general ideology index as constructed by Berry et al. (1998). Then, for the states with missing ANES information, I 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 instead of included, as discussed below.

The ANES is designed to be representative at the national but not at the state level. This raises questions about the validity of opinion data, particularly for small states where all responses might come from a single area. 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. To minimize this risk, where possible I relied on data from the 1988-1992 Senate study that is designed to be representative at the state level.[9]

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. Consequently, errors in measuring opinion are not likely to be important when calculating congruence unless the true opinion is close to 50-50. It turns out that for the policies studied, opinion is usually lopsided in favor of one position, reducing the danger

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[8] Responses other than "support" and "oppose," such as "don't know" and "decline to state," were ignored.

[9] Jones and Norrander (1995) report systematic evidence suggesting that the ANES can be aggregated reliably at the state leve.

that an "in support" state will be erroneously classified as an "opposed" state, and conversely. It also makes measurement error in the imputed observations less of worry. An unsystematic comparison of actual or imputed ANES opinion numbers with survey information from polls that were designed to be representative at the state level turned up very few cases where the majority was mismeasured.[10]

The ten 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. For each policy, I consulted at least two sources, and when discrepancies arose, consulted additional sources or the state's constitution and statutes. Some states changed their policy in the last decade. The numbers reported in this paper are based on the current (mid-2006) policy. Because opinion data are usually not available for multiple years and in any case show little variation in the majority's position over time, I do not attempt to exploit the modest amount of time series variation in state policies.

4. Basic Description of Congruence

To frame the analysis, this section describes the overall level of congruence in the sample and summarizes the variation across states and issues. Figure 2 reports the percentage of congruent policies for each issue and for the 10 issues combined. 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. Considering that purely random policymaking (in the sense of flipping a fair coin to choose the policy) would yield congruence 50 percent of the time, it seems that opinion influences state policy, but is not decisive. It should be kept in mind that the sample includes only a selection of particularly salient and controversial issues so the level of congruence in the sample does

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[10] For example, on gay marriage, the majority position in the ANES coincides with independent surveys and ballot proposition election returns in 34 of 37 states for which data are available. For term limits, 20 of 20 states are in agreement.

not indicate the effectiveness of American democracy as a whole – congruence is certainly much higher on dozens of uncontroversial issues not in the sample, such as whether to levy an income tax, fund police and fire protection, allow trial by juries, and so on. However, the finding that median voter outcomes (or majoritarian outcomes) do not attain for these high profile issues suggests that the popular median voter model is not a good approximation for empirical research on such issues.

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 gay 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.

Table 2 reports congruence for individual states. Since there are 10 issues, congruence can take one of 11 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 10 issues (the noncongruent issue is a law prohibiting job discrimination on the basis of sexual orientation). At the other extreme, five states are congruent for only three of 10 issues: Hawaii, Minnesota, New Mexico, New York, and Vermont. Overall, 28 states have congruence above 50 percent while 12 states have congruence less than 50 percent. Table 2 gives the impression that southern states are more congruent than other states, and in fact the congruence for southern states is 23 percent higher overall (73.6 percent versus 54.4 percent).

Figure 3 reports congruence as a function of the size of the majority. Congruence rises monotonically with the size of the majority. When the majority is 60 percent or less, congruence is 48 percent, statistically indistinguishable from purely random congruence. Congruence increases to 54 percent when the majority is in the 60-70 percent range, and reaches 78 percent when the majority exceeds 90 percent. There are several reasons why congruence is likely to be related to the size of the majority. 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, making congruence likely to approach the purely random outcome of 50 percent as the size of the majority approaches 50 percent. A more substantive consideration is that a larger majority is more likely to have the votes to overcome supermajority requirements that support executive vetoes, constitutional amendments, ballot propositions, and other legislative procedures. Finally, states that have heterogeneous opinion (have a small majority) may be more difficult to represent because the majority is more difficult to ascertain and significant opposition can materialize on both sides of an issue. Politicians may be more likely to make "honest mistakes" (choose noncongruent policies) when the majority is small (Matsusaka, 1992; Matsusaka and McCarty, 2001). All of these considerations suggests the importance of controlling for the size of the majority in the regressions that follow.

5. Explaining Congruence

This section seeks to identify factors that affect the amount of congruence. The strategy is to regress congruence on a variety of institutional factors, following a well established tradition in political economy research (e.g., see the survey by Besley and Case (2003)): if the institutions are exogenous and the other factors influencing congruence are controlled in the regressions, the coefficients on the institutional variables can be given a causal interpretation.[11] A challenge for empirical studies in this tradition is adequately controlling for citizen preferences, and one of the virtues of this study its inclusion of direct measures of citizen preferences on each issue. 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.

A. Direct Democracy and Judges

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[11] Alternatively, for identification we need to assume that the residual errors in congruence are not correlated with the institutions in question. As discussed below, this may be a strong assumption for some institutions – such as election laws – and it may be more cautious to view those results as correlations. For other institutions – such as direct democracy and judges – it is less obvious what would lead to a spurious result.

Twentieth century reformers, notably the Progressives, sought to make government more responsive by introducing direct democracy, which allows voters to make laws directly without involving elected officials. The most high-powered form of direct democracy is the initiative process that permits voters to propose and approve new laws.[12] Currently, 24 states and about half of the cities in the country allow initiatives, and over 70 percent of Americans have it in either their city or state (Matsusaka, 2004). The use of direct democracy has grown in the United States since California's tax-cutting Proposition 13 (Matsusaka, 2005). The main argument for direct democracy is to increase congruence, and some theory suggests that is what it will do. However, critics have long argued that direct democracy results in less congruence because it empowers wealthy and organized special interests (Broder, 2000). At first glance, it might seem that holding a popular vote on an issue would bring about a majoritarian outcome by definition, but this may not be the case for several reasons. Special interests may be able to bring about noncongruent outcomes by attracting a disproportionate number of their supporters to the polls, making the voting majority different from the population majority. Another possibility is that voters may be deceived by complex and technically worded propositions, causing them to a support a proposition that would actually implement the opposite outcome from the one they prefer. The fear that initiatives actually empower special interests and hurt the majority has been one of the central criticisms of the process from the beginning, and remains a central issue in discussions of direct democracy.[13]

I begin with simple nonparametric estimates in Table 3 that compare the congruence of 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),

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[12] "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.

[13] 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. A good example of the special interest view is Broder (2000). 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.

and the difference is significant at about the 3 percent level.[14] This is inconsistent with the hypothesis that initiatives allow special interests to subvert the majority, and suggests that legislatures may be prone to choosing noncongruent policies (otherwise there would be nothing for initiatives to make congruent), that is, special interest influence may be more of a concern in the legislature than the initiative process.

The initiative process differs in many respects across states. A potentially important difference is whether initiatives are allowed to amend the constitution or only 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 3 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 (66.0 percent) or without statutory initiatives (75.0), but congruence in initiative states (55.0) is not materially different than congruence in noninitiative states when only statutory initiatives are available. There are few states with 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.

All state constitutions fragment power among different branches of the government, and courts in particular are designed to provide a counterbalance on legislatures and agencies. Courts can overrule legislative statutes, agency decisions, and ballot propositions, and courts have had a direct hand in setting policy for several of the issues investigated in this paper (abortion, death penalty, term limits). In theory, the counterbalancing actions of courts can increase or decrease congruence. When courts intervene to protect the rights of minorities that are threatened by "majority tyranny," they may reduce congruence. When they intervene to counteract the influence of special interests that are unduly influential in the legislature, they may increase congruence.

To examine the role of courts, I exploit differences across states in the amount of judicial independence. Recent evidence suggests that independent courts are more likely

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[14] Without the imputed observations, congruence is 53.7 percent in noninitiative states and 62.4 percent in initiative states.

to protect fundamental rights and more willing to stand up to other branches of government and powerful special interests (Hanssen, 1999b; La Porta et al., 2004). The amount of judicial independence is believed to be related to the manner in which judges are selected for office. Following Hanssen (1999a, 1999b, 2000) and Besley and Payne (2006), I distinguish whether judges are appointed (by the governor, legislature, or a commission) or elected.[15] As Panel B of Table 3 shows, the most popular system is to use elections for initial selection and reappointment. The second most popular system is to appoint judges initially and hold elections for reappointment (so-called merit review plans). The third most popular system selects and reappoints judges without election. No state elects judges initially and then reappoints them without an election. Congruence is 45.0 percent when both initial selection and retention decisions are made by the governor, legislature, or a commission without involvement of the voters. When judges must stand for election to be retained in office, congruence is 61 percent or 65 percent, depending on how they initially come to office.[16] Thus, like Besley and Payne (2006), it appears that the retention procedure is more important than the initial selection procedure. Which system leads to more "independent" judges is largely a matter of definition – appointed judges are more independent of the voters while elected judges are more independent of the governor and legislature. The correlations in Table 3 suggest that when judges independent of the voters they reduce congruence by 18 percent on these issues.

    With the nonparametric comparisons as a guide, Table 4 turns to regressions. Each column 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 the size of the majority, which Figure 3 shows is strongly correlated with congruence. 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

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

[16] Without imputed observations, congruence is 56.4 percent for elected/reelected, 61.5 percent for appointed/reelected, 53.3 percent for appointed/reappointed, and 52.6 percent (only 19 observations) for lifetime appointments.

problems, and because politicians might find it more difficult in large states 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 should be better able to monitor their representatives and prevent shirking.[17] 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.[18] Finally, to control for issue-specific congruence effects, the models also included 10 dummy variables, one for each issue, although those coefficients are not reported in order to conserve space.[19]

The regressions in columns (1)-(3) include the initiative and judicial independence variables separately and then together, in addition to the other controls. Since Table 3 does not suggest a significant difference between statutory and constitutional initiatives, the initiative variable is a dummy equal to one if any type of initiative is available in a state. Table 3 also suggests that the critical distinction for

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[17] 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.

[18] 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.

[19] I use the same set of control variables throughout the 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.

judges is whether they stand for reelection or not, so the judicial 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 3 are not proxying for any of the 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 at about the 6 percent level in column (3). When judges must stand for election, congruence is higher. Put differently, judges that do not stand for reelection appear to be a source of noncongruence.

The other explanatory variables are not the focus of investigation, but a few interesting patterns emerge. First, as Figure 3 suggests, congruence is significantly more likely as the size of the majority increases. Second, Southern states are more congruent than other states. I am not aware of an obvious explanation for the higher congruence of Southern states.[20] Population does not seem to be an important factor, which suggests that free rider problems might not be more pervasive in large states than small states.[21] 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 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 always different from zero at conventional levels of significance.[22]

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[20] Opinion is not more homogeneous in Southern states than other states on these issues.

[21] This finding thus tends to undermine the argument in Matsusaka and McCarty (2001) and Matsusaka (2004, Ch. 9) that free rider problems explain why initiative effects are different in large and small cities.

[22] When the imputed observations are excluded, in regression (3) the initiative coefficient becomes 0.93 and remains significant at the 1 percent level, while the judge coefficient falls to 0.28 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 it 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.

Column (4) 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) 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/reelected than elected/reelected 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.[23] 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.

Finally, as a robustness check, column (6) 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

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[23] Part of the explanation may also have to do with the fact that the elections in the "appointed/reelected" group are more likely to be merit plan type of elections where the judge runs unopposed and voters can either retain or remove the judge, while the "elected/reelected" elections are more likely to be partisan elections. Unfortunately, it is not possible with these data to go further down the path of making fine grain distinctions by judicial selection procedures, because there are too few cases of each type and procedures tend to be clustered by region (Besley and Payne, 2006).

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 and judicial election effect continue to be positive and statistically significant at about the same levels.

The coefficient estimates in Table 4 are difficult to interpret. To give a sense of the economic magnitudes of the relations, Table 5 reports the estimated probability of congruence for 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, the predicted congruence is 43.9 percent for a state without the initiative and with appointed judges, and 75.1 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 17.0 and 19.1 percent greater congruence, depending on the judicial retention procedure. Judges who stand for reelection are associated with 14.3 and 12.2 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 3. I also estimated predicted effects for the most one-sided issue – school prayer – and the most divided issue – public funding of abortion – and found very similar marginal effects.

It may be worth emphasizing that the pro-congruence effect of the initiative is not hard-wired; allowing citizens to vote on policies does not necessarily make the outcomes congruent. If the pool of voters does not represent the population at large, perhaps because organized interest groups are better at turning out their supporters, then noncongruent propositions can be approved. Some initiative critics also argue that voters can be misled into supporting propositions that actually implement policies that voters would oppose if they were fully informed.[24] Furthermore, even if voters are informed and

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[24] A related possibility is that special interests influence not only views about specific ballot propositions but also underlying opinion as reflected in the ANES. This cannot be ruled out as a general phenomenon, but seems unlikely to explain the initiative effect since ANES opinion seems relatively stable across time, and opinion on the 10 issues is not significantly different in initiative and noninitiative states (for example, the mean conservative opinion on the 10 sample issues is 68.6 percent in initiative states and 67.6 percent in noninitiative states).

representative of the population at large, initiatives can be repealed or amended by legislatures and courts, so there is no guarantee that having the initiative available will have a measurable effect on final policies. The finding that congruence is 17 to 19 percent higher in initiative than noninitiative states suggests that in fact the initiative is highly effective, and the power of special interests in ballot proposition elections may be exaggerated by critics.

The pro-congruence effect of elected judges is not hard-wired either. If special interests are able to dominate elections then they would be able to influence the legislature and, when elected, the judiciary. Independent judges in such an environment could counterbalance special interests, and lead to greater congruence. The finding that elected judges increase congruence by about 12 to 14 percent suggests that the influence of special interests in judicial elections may be modest.

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.

B. Election Laws

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." It is thus natural to ask what role election laws play in congruence.

I focus on election laws that vary across states, and are connected to electoral competition. The apparent decline of competition in legislative elections has been a preoccupation of empirical research in political science at least since Erikson (1971) and Mayhew (1974).[25] Competition has been measured in many ways, such as the incumbency reelection rate or margin of victory, but by most accounts, the amount of competition is low and, if anything, has fallen over time. For example, Niemi et al. (2006) report mean incumbency reelection rates for state lower and upper houses of 94 percent and 92 percent, respectively, over the period 1992-2002. The apparent absence of electoral competition has fueled a variety of reform proposals designed to make it easier to dislodge incumbents, such as public funding of campaigns and redistricting by nonpartisan commissions. Research to date has focused on how election laws affect competition, which is presumed to make government more responsive, but there is no direct evidence on whether particular reforms result in more congruence, that is, the link between competition and congruence is assumed but has not been demonstrated. There are reasons to question whether competition as conventionally measured actually captures the quantity we are interested in. For example, the fact that incumbents routinely enjoy large margins of victory could simply reflect that voters are satisfied with incumbent performance, that is, a high rate of retention in office could be a consequence of a competitive process in which the cream rises to the top (Groseclose, 2001).

In order to provide a rough assessment of the role of elections in creating policy congruence, this section explores the effect on congruence of a large set of election laws, summarized in Table 6, many of which appear to affect competition as conventionally measured. The laws can be grouped into five 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

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[25] See Jacobson (2006) and other essays in McDonald and Samples (2006) for an overview.

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 actually 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 (2006) find that "high quality" candidates are more likely to win with public funding.

- Primary elections. Many reformers favor open rather than closed primaries. They argue that closed primaries, in which each party's nominee is chosen only by those belonging to the party, lead to the election of 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 since legislators may want to insulate

themselves from competition by creating safe districts for themselves (Crain, 1977).[26] 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 objective function of commissions compares to that 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 voter in the population, and even random redistricting will result in noncongruent policies (Gilligan and Matsusaka, 2006). Bradbury and Crain (2005) report some evidence that district lines do introduce such distortions.

- 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. There does not appear to be an extensive theoretical or empirical literature on the recall, 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 laws. Since statistical power

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[26] Despite the widespread belief that redistricting is an important factor in the decline of 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).

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 law correlation in this sample is pervasive.

Table 7 begins with simple nonparametric comparisons of states with and without various election laws. The table is arranged so that the "Yes" column represents the policy that is 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 5.0 percent lower in states that limit corporate campaign contributions, and only 2.9 percent higher in states that limit individual campaign 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. There is no theoretical reason to believe the effects should be additive, but the indexes provide another first cut in the 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.[27]

Figure 4 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 number of observations per value is small. The figures indicate that the lack of significance in Table 7 is not due to the somewhat arbitrary division of the sample into states above and below the median – there is simply no apparent relation between congruence and any of the campaign indexes.

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

The news for reformers is better when it comes to primary election systems. States with open primaries show 8.5 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 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.[28]

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 8.7 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 5.6 percent lower in states that allow recall, contrary to expectations, but the difference is not statistically significant.

The overall impression from Table 7 and Figure 4 is that elections laws are not strongly associated with congruence. To check the robustness of this conclusion, I next report logistic regressions that control for initiative and judicial retention status, and the other variables in Table 4 (size of majority, population, education, South dummy, age of state, and 10 issue dummies).

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[28] 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 these commission, 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 8 shows the estimates. Each column reports 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 laws 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 appear as a significant predictor of congruence, and the judicial selection coefficient hovers at about the 10 percent level of significance, with similar magnitudes to what was found in Table 4.[29]

Election law reforms tend to cluster in certain states. To explore the possibility that some policies might be highly correlated with each other, resulting in insignificant coefficient estimates, Table 9 reports coefficients from 11 separate regressions, each of which includes only a single election law variable together with initiative and judicial retention status and the demographic and regional controls. To conserve space, I only report the coefficients on the election law variable, initiative status, and judicial retention procedure. The last column reports the predicted change in probability of congruence from introducing the election law (assuming all other variables take mean values, the state is not in the South and does not have initiatives and judicial elections, and the issue is abortion parental consent). For example, adding a contribution limit on corporations reduces the predicted probability of congruence by 0.08. Table 9 further reinforces the basic impression that election laws are not correlated with congruence in this sample. None of the election law coefficients are significantly different from zero at the 10 percent level, the predicted probabilities are small in magnitude, and the largest magnitude predicted probabilities go in the oppose direction of what is expected: corporate and individual contributions and recall are associated with less congruence.

None of the election law variables materially change the coefficients on initiative availability and judicial elections. The initiative continues to show a strong connection with congruence and can be distinguished from zero at better than the 1 percent level in

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[29] When the imputed observations are deleted, all of the election laws remain statistically insignificant.

every regression. The judicial election variable also continues to show a positive relation with congruence, and hovers at about the 7 percent level of significance.

To summarize, there is a striking absence of correlation between this set of election laws favored by reformers and the 10 issues under investigation. Perhaps the most 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 there are several instances of changes 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.

6. Discussion

This paper quantifies how often American states choose policies supported by the majority of citizens in order to understand the factors that influence popular control of policy. Previous research has documented a correlation between public policy and broad measures of public opinion, but prevailing methodologies do not reveal whether policy choices are congruent with majority opinion, nor can they be used to identify factors that lead to greater congruence. My approach is to focus on 10 issues with dichotomous outcomes, and use opinion surveys to determine which outcome is preferred by the majority in each state. If a state chooses the outcome favored by the majority, the policy is "congruent," otherwise it is noncongruent. Based on this measure, I find that 59 percent of the 500 state-issue policy choices are congruent. The sample congruence is not indicative of the effectiveness of American democracy in general – if we were to include the many noncontroversial policies such as whether to fund police or ban child labor, congruence would be much higher – but it does suggest that when it comes to the type of emerging, controversial issues contained in the sample, the majority does not always rule and the median voter model is not a good approximation of the policy formation process.

In some respects, the findings of this paper can be seen as negative: they suggest that a class of models (median voter, majority rule) does not accurately characterize the policy formation process in American states on high profile issues. The evidence leaves open the question of which models will perform better. While not pointing to any specific approach, the finding do lend support to the broad group of models that do not imply

median or majoritarian outcomes, such as the interest group theories of Stigler (1971), Peltzman (1976), and Grossman and Helpman (2001). Another explanation for the unimpressive overall congruence on the issues in the sample might be that the social issues studied here take a backseat to economic and fiscal issues. It is rare that English-only, public funding of abortion, and death penalty are the focus of candidate campaigns. Congruence could be much higher on issues that seem to concern the voters more, such as tax policy. Another possibility is that voters' concern over candidates' personal characteristics ("valence" or "character") swamps concern over their positions on these issues, as in Groseclose (2001) and Kartik and McAfee (2007), for example.

A central purpose of this study is to identify factors that influence the likelihood of congruence. 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), yet none of the reforms I examine show a reliable connection to congruence. Based on the evidence from the 10 issues studied in this paper, laws designed to increase competition appear to be an unpromising avenue for increasing democratic responsiveness.

The factors that do seem to contribute to increased congruence are those that counterbalance the legislature. The largest increase in congruence comes from counterbalancing institutions that are under popular control. Availability of initiatives, an institution that allows voters to override the policy decisions of their representatives, increases congruence in this sample by about 17 to 19 percent. Making judges stand for reelection, as opposed to giving them lifetime appointments or allowing them to be reappointed by sitting government officials or commissions, increases congruence by about 12 to 14 percent in this sample, although the estimated effect is somewhat imprecise. One view that would be consistent with most of the findings in the paper, offered speculatively, is that legislatures are somewhat unresponsive to the majority (perhaps because of special interest influence) and popular control of institutions that can override the legislature, such as initiatives and courts, allow the majority to have its way more often. Whether this is an accurate picture, and if so, precisely what factors impede congruence in legislatures seem like important questions for future empirical research.

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, there are situations when congruence is undesirable, such as when a majority seeks to exploit a numerical minority or perhaps when the minority has intense preferences and majority is close to being indifferent about the outcome. Evidence that a particular institution increases congruence – in this study, initiatives and judges that stand for reelection – does not mean that adoption of the factor 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 only a few survey responses, opinions were 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 percentage in favor was the dependent variable, and a measure of ideology was the explanatory variable. I considered three different ideology measures, "citizen ideology" and "government ideology" from Berry et al. (1998) (calculated as average values 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). To be included in the regression, initially I required a state to have had at least 60 survey responses in the ANES, but 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 that provided the best fit based on R^2 using the most observations. The final imputation models were based on a cutoff of 60 respondents except for the estate tax (cutoff of 30 respondents), gay marriage (cutoff of 30 respondents), and English only (cutoff of 90 respondents). The Berry et al. "government ideology" measure was used in the final imputations except for estate tax and term limits, which used the citizen ideology measure. Although the Erikson et al. measure provided as good as or a slightly better for a few issues, the improvement was judged insufficient 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

Policies were checked against at least two, and sometimes three data sources.

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

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

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

6. Late-term abortions. Does the state prohibit certain "late-term" or "partial birth" abortions? Primary sources were Guttmacher Institute (2006) and Center for Reproductive Rights (2004).

7. Parental notification for minor's abortion. Does the state require consent of parents or legal guardian before a minor has an abortion? Primary sources were NARAL Pro-Choice America (2006), Kaufmann (2004), and individual state pages on the web site of the Guttmacher Institute (www.guttmacher.org). If a law was struck down by a court but not replaced by one that passed constitutional muster, the state was coded as not having a law.

8. Public funding of abortion. Does the state use its own funds to pay for all or most medically necessary abortions for Medicaid enrollees? Primary sources were Guttmacher Institute (2006) and NARAL Pro-Choice America (2006).

9. School prayer (moment of silence). Did the state have a law mandating or allowing for 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 is U.S. Term Limits (no date). The ANES question that provides state-by-state opinion 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 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. The data describe procedures as of 2004. No state changed its procedures in a way that changed its classifications during 1990-2006 except Tennessee, which changed from initial election to initial appointment in 1994. 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 the 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. The South dummy is equal to one for AL, AR, FL, GA, LA, MS, NC, SC, TN, TX, VA.

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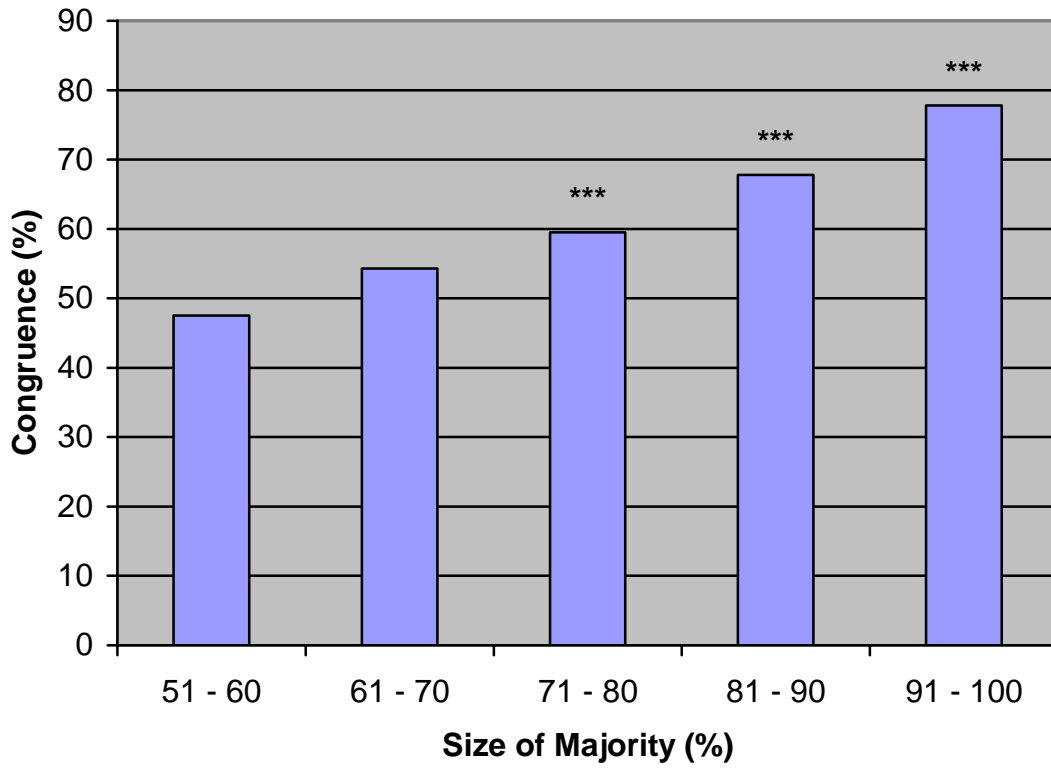**Figure 1. Hypothetical Observations of Policy (*y*) and opinion (*I*).**
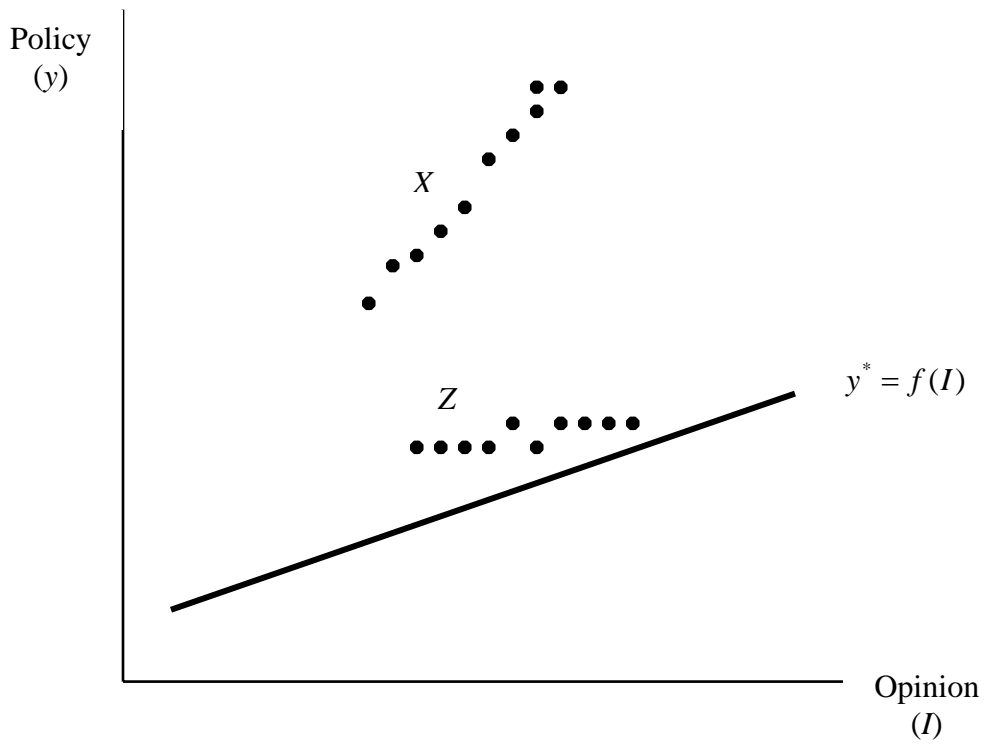
Figure 2. Congruence by Issue
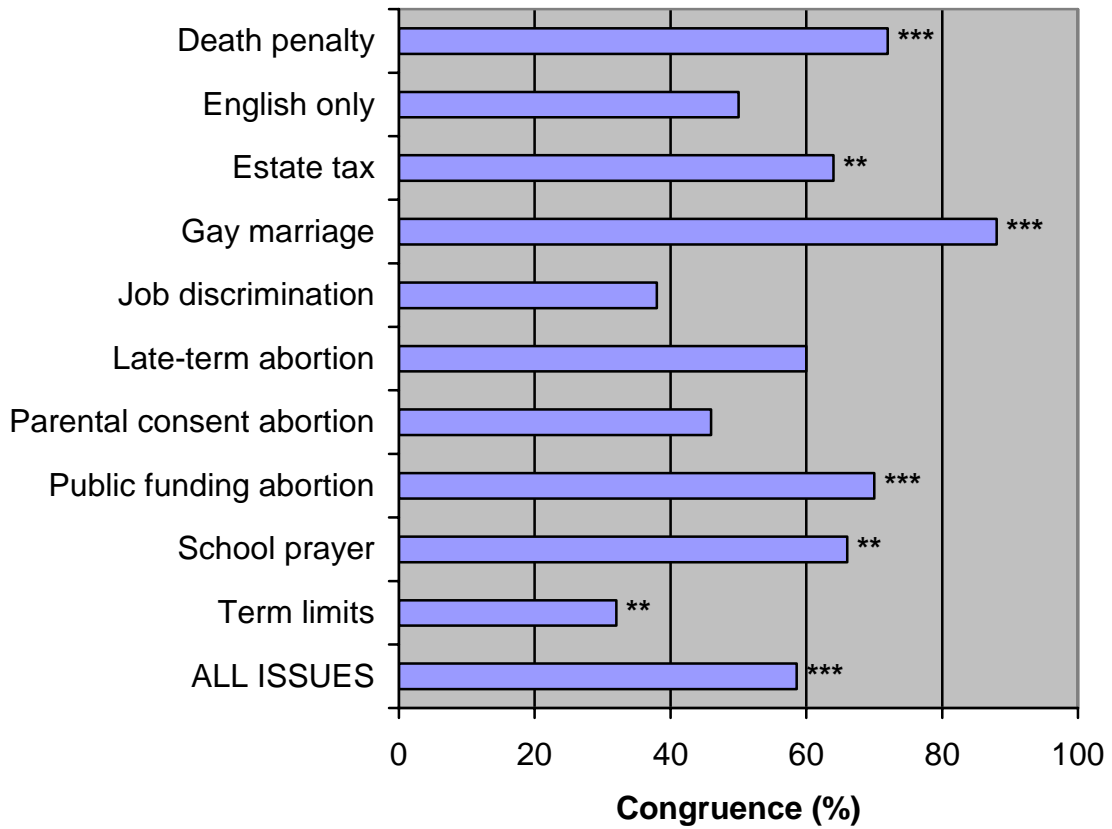
Note. Significance levels for the hypothesis that congruence is equal to 50 percent are indicated as follows: *=10%, **=5%, ***=1%.
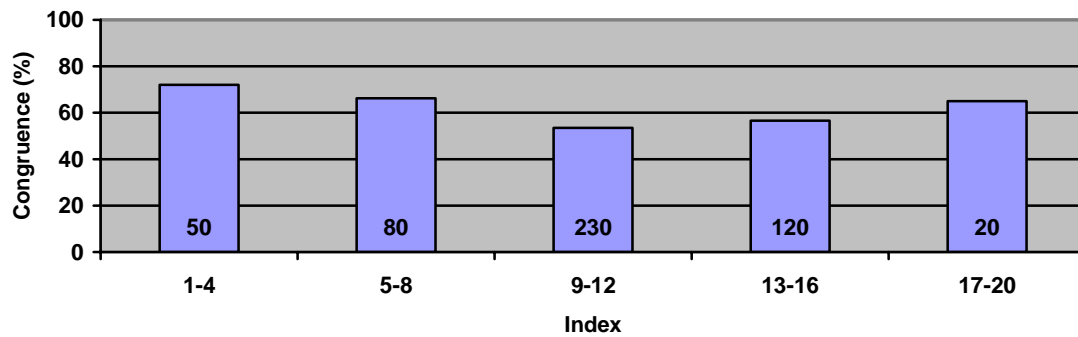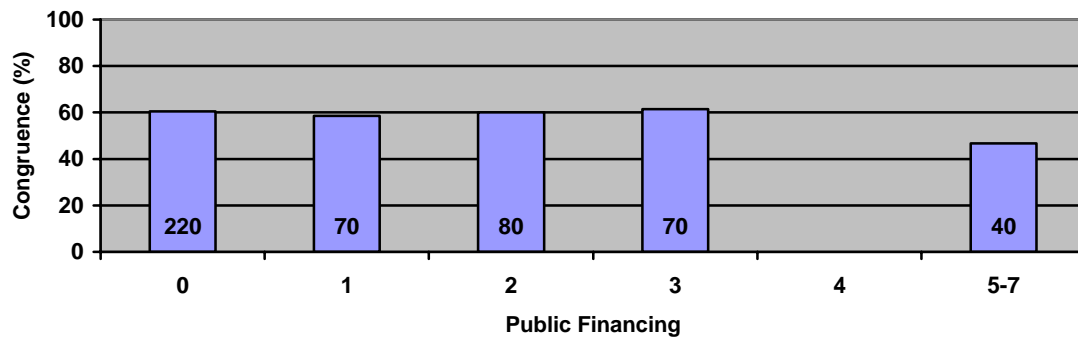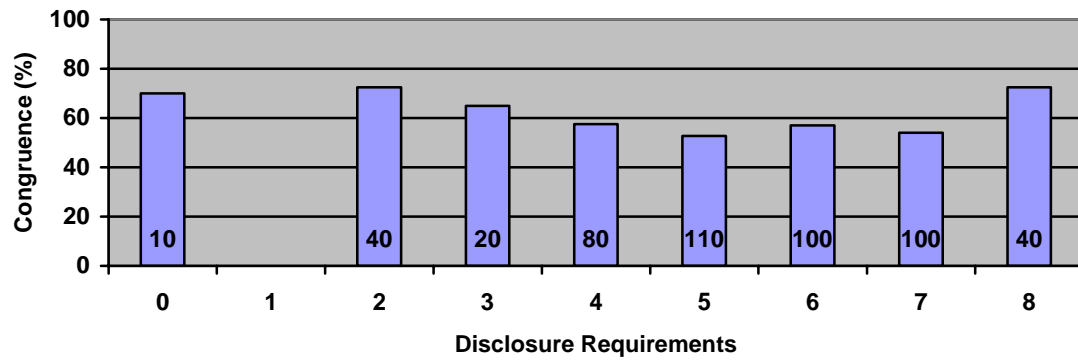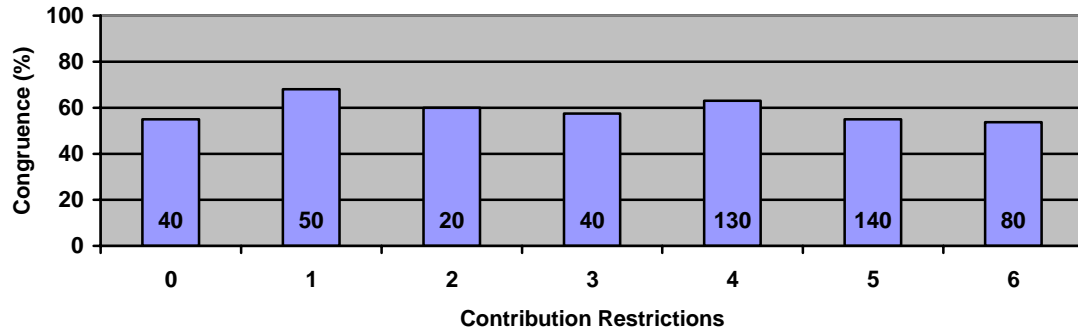
Figure 3. Congruence by Size of Majority

Note. Significance levels for the hypothesis that congruence is equal to 50 percent are indicated as follows: *=10%, **=5%, ***=1%.

Figure 4. Congruence and Campaign Finance Index Values

Note. Each figure shows the percent congruent by an index value or range of values. The number of observations is reported inside each bar.

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?'	70.9	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?"	72.5	59.2 (HI)	84.6 (UT)	2002
Gay marriage	"Should same-sex couples be allowed to marry, or do you think they should not be allowed to marry?" Responses: 1=Allowed. 5= Not allowed. 7=Not allowed to marry, but civil unions allowed. ("In favor" = response 1)	32.9	11.5 (AL)	49.1 (IL)	2004
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 teen-ager 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
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. The identity of extreme states is reported in parentheses beneath the values.

Table 2. Congruence by State

Congruent policies	States
0%, 10%, 20%	…
30%	Hawaii, Minnesota, New Mexico, New York, Vermont
40%	Alaska, Connecticut, Delaware, New Jersey, Oregon, Rhode Island, Washington
50%	Colorado, Idaho, Iowa, Kansas, Maine, Maryland, Massachusetts, New Hampshire, West Virginia, Wisconsin
60%	Michigan, Nebraska, Nevada, North Carolina, Pennsylvania, Texas, Virginia
70%	California, Florida, Georgia, Illinois, Indiana, Montana, North Dakota, Ohio, Oklahoma, South Dakota, Wyoming
80%	Alabama, Arizona, Kentucky, Louisiana, Mississippi, Missouri, South Carolina, Tennessee, Utah
90%	Arkansas
100%	…

Note. There are 10 different policies. The table shows how often each state adopted the policy favored by the majority. For example, Arkansas followed the majority's preference for nine out of 10 policies.

Table 3. Congruence, Direct Democracy, and Judicial Selection Procedures

Structure	Congruence	N
Panel A. Initiatives		
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
Panel B. Judicial Selection (initial/retention)		
Elected/reelected	61.4	220
Appointed/reelected	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 initiatives are 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.

Table 4. Regressions Explaining Congruence with Initiative and Judicial Selection Procedures

	(1)	(2)	(3)	(4)	(5)	States (6)
Initiatives						
Dummy = 1 if (any) initiative allowed	0.86*** (0.25)	...	0.78*** (0.26)	...	0.78*** (0.27)	0.63*** (0.16)
Dummy = 1 if constitutional initiative allowed	...	...	...	0.81*** (0.27)	...	...
Dummy = 1 if only statutory initiative allowed	...	...	...	0.60 (0.41)	...	...
Judicial Selection						
Dummy = 1 if judges stand for re-election	...	0.74** (0.30)	0.58* (0.31)	0.56* (0.31)	...	0.38* (0.21)
Dummy = 1 if judges elected / reelected	...	...	...	...	0.41 (0.34)	...
Dummy = 1 if judges appointed / reelected	...	...	...	...	0.75** (0.37)	...
Dummy = 1 if judges appointed for life	...	...	...	...	-0.25 (0.51)	...
Size of majority	13.20*** (2.00)	12.92*** (1.99)	13.10*** (2.01)	13.11*** (2.01)	12.95*** (2.02)	12.75*** (3.32)
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	-6.27* (3.40)	-2.34 (3.33)	-4.98 (3.46)	-4.59 (3.54)	-6.51* (3.70)	-4.06* (2.21)
Dummy = 1 for southern state	0.87*** (0.33)	0.86** (0.32)	0.84*** (0.33)	0.86*** (0.33)	0.80** (0.34)	0.63*** (0.19)
Years since state entered union	0.24 (0.28)	0.26 (0.30)	0.54* (0.33)	0.51 (0.33)	0.62* (0.34)	0.41** (0.20)

Note. Each column (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). 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%.

Table 5. Predicted Probability of Congruence Conditional on Initiative Status and Judicial Retention Procedure

| | Judicial retention | | Marginal effect: appointed ➔ elected |
	Appointed	Election	
No initiative	43.9	58.2	+14.3
Initiative available	62.9	75.1	+12.2
Marginal effect: no initiative ➔ initiative	+19.1	+17.0	

Note. This table reports the predicted probability of congruence based on the estimated model in column (3) of Table 4. 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 43.9 percent.

Table 6. Election Law Summary Statistics

Variable	Mean	Min	Max
Campaign Finance			
Dummy = 1 if individual contributions are limited	0.74	0	1
Dummy = 1 if corporate contributions are limited	0.90	0	1
Contribution stringency index	3.82	0	6
Public financing index	1.54	0	7
Disclosure index	5.26	0	8
Overall index	10.62	3	20
Dummy = 1 if open primary	0.60	0	1
Dummy =1 of redistricting by independent commission	0.20	0	1
Ballot access			
Dummy = 1 if major-party filing fees less than $100	0.74	0	1
Dummy = 1 if no signature requirement for major-party candidates	0.62	0	1
Dummy = 1 if recall allowed	0.36	0	1

Note. All statistics are calculated using 500 observations. "Open primary" also includes semi-open/closed primaries.

Table 7. Congruence and Election Laws

Election law	Yes	No	*p*-value for difference
Campaign finance			
Limits on contributions from corporations	57.3	62.3	.319
Limits on contributions from individuals	58.9	56.0	.695
Contribution limits index, more stringent than median (score > 4 on 0-6 scale)	54.5	61.8	.103
Public funding index, more than median (score > 1 on 0-8 scale)	56.7	60.0	.456
Disclosure index, more stringent than median (score > 5 on 0-8 scale)	58.3	58.8	.908
Overall index, greater than median (score > 11 on 0-18 scale)	57.3	59.6	.594
Open primaries	62.0	53.5	.059
Redistricting by independent commission	54.0	59.8	.297
Ballot access			
Major-party filing fees ≤ $100	56.2	65.4	.068
No signature requirement for major-party candidates	61.9	53.2	.053
Recall allowed for statewide offices	55.0	60.6	.221

Note. 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.

Table 8. Logistic Regressions of Congruence as a Function of Election Laws

Variables	(1)	(2)	(3)
Campaign finance			
Dummy = 1 if corporate contributions limited	-0.38 (0.46)	…	…
Dummy = 1 if individual contributions limited	-0.08 (0.33)	…	…
Contribution limits index (0-6)	…	-0.05 (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	0.29 (0.25)	0.26 (0.25)	0.16 (0.25)
Dummy = 1 if redistricting by independent commission	0.19 (0.32)	0.14 (0.32)	0.12 (0.30)
Ballot access			
Dummy = 1 if major-party filing fees $\leq$ \$100	-0.06 (0.32)	0.01 (0.32)	-0.01 (0.32)
Dummy = 1 if no signature requirement for major-party candidates	0.09 (0.26)	0.11 (0.28)	0.03 (0.26)
Dummy = 1 if recall allowed for statewide offices	-0.38 (0.26)	-0.38 (0.26)	-0.37 (0.25)
Dummy = 1 if (any) initiative allowed	0.84[***] (0.27)	0.80[***] (0.27)	0.83[***] (0.27)
Dummy = 1 if judges stand for reelection	0.57 (0.35)	0.58[*] (0.34)	0.55 (0.35)

(Continued on next page.)

(Table 8 continued)

Size of majority	13.48^{***}	13.34^{***}	13.14^{***}
	(2.06)	(2.06)	(2.03)
Population (log)	-0.12	-0.10	-0.07
	(0.14)	(0.14)	(0.14)
High school graduates, fraction of population	-6.64^{*}	-6.53^{*}	-5.86
	(3.75)	(3.83)	(3.77)
Dummy = 1 if southern state	0.58	0.59	0.66
	(0.39)	(0.40)	(0.41)
Years since state entered Union ($\times$ 100)	0.63*	0.56	0.47
	(0.37)	(0.36)	(0.34)

Note. 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%.

Table 9. Logistic Regressions of Congruence and Individual Election Law Laws

Regression number / election law variable	Coefficient Estimates			Change in congruence probability from election law
	Election law	Initiative dummy	Elected judges dummy	
(1) Dummy = 1 if corporate contributions limited	-0.38 (0.35)	0.82*** (0.26)	0.58* (0.31)	-0.08
(2) Dummy = 1 if individual contributions limited	-0.32 (0.26)	0.84*** (0.27)	0.48 (0.32)	-0.06
(3) Contribution limits index (0-6)	-0.06 (0.06)	0.79*** (0.26)	0.54* (0.31)	…
(4) Public financing/spending limits index (0-7)	-0.001 (0.06)	0.78*** (0.26)	0.57* (0.32)	…
(5) Disclosure index (0-8)	-0.01 (0.07)	0.78*** (0.27)	0.57* (0.31)	…
(6) Overall index (3-20)	-0.02 (0.03)	0.79*** (0.26)	0.53* (0.32)	…
(7) Dummy = 1 if open primaries	0.17 (0.23)	0.77*** (0.26)	0.57* (0.31)	0.03
(8) Dummy = 1 if redistricting by independent commission	0.01 (0.30)	0.78*** (0.26)	0.58* (0.31)	0.002
(9) Dummy = 1 if major-party filing fees ≤ $100	0.01 (0.30)	0.78*** (0.26)	0.58* (0.31)	0.001
(10) Dummy = 1 if no signature requirement for major-party candidates	-0.04 (0.24)	0.78*** (0.26)	0.60* (0.30)	-0.01
(11) Dummy = 1 if recall allowed for statewide offices	-0.35 (0.24)	0.81*** (0.26)	0.61** (0.31)	-0.07

Note. Each row reports coefficients from a logistic regression that predicts the probability of a congruent state-issue. Each regression contained one election variable, as indicated in the first column, initiative and judicial election variables, size of majority, log of population, high school education, dummy for Southern states, years since the state entered Union, and dummy variables for each issue. Only coefficients on the election law, initiative, and judicial retention variables are reported. Standard errors are in parentheses beneath coefficient estimates. The last column reports the predicted change in probability of congruence from adding the election law. Each model was estimated with 500 observations. Significance levels are indicated: *=10%, **=5%, ***=1%.