

Measuring Representation: Direct Democracy and Policy Responsiveness in the American States^ψ

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Abstract

Institutional design is critical moderate the representational relationship. Previous studies largely validate the link between direct democracy and policy responsiveness by elected officials. Generally concurring with Matsusaka's (2001) critique that modeling responsiveness through interactions with public opinion measures has limited validity; this paper reexamines the theoretical data generating process of these findings and presents an alternative empirical specification for evaluation. This updated method provides a better fit to the underlying theoretical process, as well as permits multivariate controls. I demonstrate the validity of this alternative model with replication of previously published data. Two of the three replications indicate that the initiative results in superior policy representation, and one finding affirms the original null results. All models demonstrate strong confirmation of the validity of the method. A new test with 32 years of data demonstrates greater policy convergence to the median voter as a result of the initiative process.

Introduction

Any democratic political system must determine the process through which the populace translates their collective will into policy. Almost universally, democracy becomes synonymous with *representative* democracy: the public does not directly select policy but selects those who do. The level of separation between the populace and policy is not haphazard, but is instead regulated by the design of the institutions undergirding that relationship.

Although the Founders of the American Constitution established numerous institutional factors to insulate elected officials from the citizenry, the subsequent 200 years saw a slow erosion of these institutions. A key example is the rise of the initiative process in the American states. In almost half the states, constituents can bypass their elected officials to directly establish policy. This ability to circumvent a state legislature is widely thought to bring policy closer to the desires of the public. Although this theory has inspired numerous tests (Romer & Rosenthal 1979, Gerber 1996, 1999, Matsusaka 2005, Burden 2005, Arceneaux 2002, Camobreco 1998, Hug 2001, 2001, Lascher et al. 1996, Pippin et al. 2002, Bowler & Donovan 2004), these studies have also started a debate regarding the proper approach to modeling the effect of the initiative on policy responsiveness.

This paper intends to demonstrate a theoretically and empirically improved method for evaluating the effect of the initiative on the responsiveness of elected officials. The first section reviews the central methodological critique by Matsusaka (2001) and the findings giving rise to that critique. The second section extends a basic formal model of the initiative process (Gerber 1996) in order to derive a more theoretically sound method of empirical evaluation. The third section explicates the statistical method derived in the previous section, the fourth section tests the method by replicating previous results and the fifth section applies the method to a new dataset spanning 32 years.

Findings and Methodological Critique

Over the past two decades, widespread theoretical arguments have concluded that the initiative ought to entail greater convergence between citizens and their representatives. Since Romer and Rosenthal (1979) the dominant explanation for the initiative bringing closer representation has been competition. Gerber (1996, 1999) formally models this process: the availability of the initiative allows an interest group to compete with the legislature for votes of the citizenry. Matsusaka and McCarty (2001) phrase this as the end of the legislature's monopoly over policy due to the entrance of a competitor: the citizen. The competition breeds Downsian convergence toward the median voter.

Matsusaka (2005) follows Kalt and Zupan (1990) in the assumption that the median voter hypothesis breaks down due to free riders in monitoring and disciplining office holders. As elected officials putatively have the option to either accurately represent their constituents, or vote according to their own ideology; constituents must be able to both monitor the behavior of the officials, and discipline any observed shirking. The breakdown of monitoring and discipline provides elected officials leeway to consume their own ideological preferences. The initiative ameliorates this dilemma by stripping issues from the hands of politicians. By restricting the number of issues that are 'bundled' into a politician, citizens are then better able to monitor and punish their officials. Without the initiative, policies are bundled within each individual politician as well as log-rolled within the legislature. Bundled politicians and legislative log-rolls both facilitate policy passage that would be impossible if proposed individually. The initiative provides the clarity needed to strengthen constituent monitoring, and breaks apart policy so that the electoral significance of the median voter is strengthened.

From a quite different school of thought, there are the 'educative' effects of direct democracy. This thread of research has found that direct democracy increases voter turnout,

causes individuals to pursue more information, and breeds political discussion on the issues (Tolbert, Grummel, & Smith, 2001; Bowler & Donovan, 2002a; Bowler & Donovan, 2002b; Smith & Tolbert, 2004; Tolbert & Smith, 2005). Such effects ought to empower the citizenry to ‘tighten the reins’ on their elected officials.

Representatives are educated by direct democracy as well. Burden (2004) theorizes that one reason a legislature without the initiative is not optimally responsive is because it is unable to discern public opinion. The initiative provides a new avenue for citizens to provide information to their representatives.

All these explanations conclude with the same claim: the presence of the initiative ought to make public policy more responsive to constituents. How this should be measured is hotly debated. As responsiveness is about the quality (and to a lesser extent, the presence) of the relationship between constituent opinion and policy enactments, what is being modeled is not the direct effect of the initiative on policy, but the moderating effect of the initiative between constituent opinion and policy. This framework predictably has led to an interaction between the initiative and public opinion:

$$G = \alpha + \beta_i I + \beta_p P + \beta_{ip} IP + \beta_x X + \varepsilon \quad (1)$$

Where G is an observed policy indicator (presence of death penalty or level of education spending for example), I is a dummy for the presence of the initiative process, P is a preference indicator of the populace (e.g. level of support for the death penalty, demand for education spending or a general ideology indicator), IP is an interaction, and X a vector of controls. This method, with minor variants, is used by Arceneaux (2002), Bowler & Donovan (2004), Camobreco (1998), Burden (2004), Gerber (1996, 1999), Hug (2004), Lascher Jr et al. (1996), and even Matsusaka (2004). This approach is criticized by Matsusaka (2001).

Matusaka points out that the differential slopes of this model are lacking a fundamental meaning. Put simply, a higher slope does not indicate greater responsiveness; this higher slope may in fact be *too* high. Matusaka claims that responsiveness is measured by which of the slope coefficients is more like the *true*, but unfortunately unknown, mapping function (slope) between constituent opinion and government policy. Without knowledge of this true relationship, however, empirical estimation does not meaningfully carry into theoretical significance.

This critique is a critical challenge to the study of representation. Even with the best public opinion data, and perfect knowledge of public policy, analysts are unable to demonstrate that any set of political institutions entails better or worse representation. At best, we can demonstrate that two systems provide a *different* quality of representation.

The next section returns to the theoretical data generating process in order to derive a process to better estimate the effect of a convergence inducing institution. Of central importance is the claim that, when we return to the underlying data generating process, we actually know more about the mapping between constituent preferences and representative positions than is often thought.

The Formal Model

This paper looks to an early formal model (Gerber 1996) which is well accepted in the literature. This single dimensional sequential game has three actors: the median member of the state legislator (L), the median voter of the state (V), and a ‘proposer’ which I will consider an interest group (IG). Preferences are single-peaked monotonically decreasing with distance from their ideal policy outcome, and all players act with complete information. The legislator acts first by choosing a policy position, followed by the interest group deciding whether or not to propose an initiative, and a popular vote between the status quo and that initiative (if it is proposed). If

the initiative has no cost, then the predicted effect would be perfect convergence between the legislature and the median voter. To the degree that the initiative process is costly to the interest group, then there will be some degree of slack between constituents and representatives.

In the presence of the initiative, the legislature must anticipate the response of the interest group and voters above and beyond what is typical. The initiative provides the voters with the last move in the policy making sequence, and policy positions therefore must respond to the preferences of the median voter. As legislators without the initiative have all their characteristics bundled into a single entity, the threat of electoral punishment for individual policy divergence is questionable. With the initiative, voters are able to target specific policies to rectify the work of the legislature.

Figure 1 is the extensive game form. In equilibrium, L will set policy as close to the V as needed to pre-empt an initiative from the interest group. This results in policy that is either unchanged or closer to the ideal point of the median voter.

-Insert Figure 1 about here-

First consider the relationship between constituent demands and public policy in the absence of the initiative. Achen (1977, 1978) makes it clear that the expected baseline for comparison, in which representatives face no constraints at all, is not the absence of any relationship between constituents and representatives. In this minimalist relationship, in which the electoral connection is entirely absent, representatives are, at worst, randomly drawn from the population of voters they represent. Therefore, when legislatures implement their preferred policy “without regard for their constituent’s preferences,” there is still a clear relationship

between citizen preferences and state policy when viewed across multiple legislatures. Policy will, *on average*, accurately reflect constituent demands.

Figure 2 illustrates the baseline distribution of all legislative ideal points relative to their district's median voters. Although all districts certainly have different medians, figure 2 is the distribution of legislatures around the set of medians centered to the same point.¹ While the legislature could theoretically be on a far extreme of the political spectrum relative to their median voter, such a positioning is unlikely and the use of a normal distribution follows from that likelihood.

-Insert Figure 2 about here-

If we consider the initiative to be the only constraining institution, this is the baseline relationship between the median voter and the legislature's policy that would be expected in one of four conditions:

1. There is no initiative process in the state.
2. There is no interest group (proposer) in the state.
3. The cost of the initiative is at such a level that no initiative is a credible threat to the legislature.
4. The ability of the legislature to modify the initiatives passed by the voters is of such a degree that they need not modify their behavior to prevent initiatives they do not favor (Bowler & Donovan, 2004).

The first condition is of central importance. The other three are unlikely to exist to this extreme degree, and should instead simply moderate the effect of the initiative. I.e. it is the presence of the initiative process that plays a driving role in instilling greater representative responsiveness, the other factors moderate that effect.

¹ See appendix 1 for an uncentered graphical representation of figure 1.

Figure 3 introduces an interest group in the presence of the initiative process. An interest group in an initiative state changes two types of legislatures: those on the opposite side of the median voter as the IG, and those more extreme than the interest group. In the case of the former, these legislatures on the opposite side of the median voter relative to the IG converge toward the median in Downsian competition. Similarly, legislatures more extreme than the IG but on the same side of the median voter also are forced to compete for support of the median voter. In this case, however, convergence is to the interest group's ideal point. Under the assumption of perfect information, the 'induced' equilibrium does not require the process of the legislature being rebuked by an initiative to actually take place. Given basic foresight, legislatures will, in equilibrium, pass legislation between the median voter and the IG without initiatives being proposed.

Legislatures located between the median voter and the interest group are unaffected by the initiative (Gerber's case 2). Given the IG's extremity relative to the median voter, threats of an initiative are not credible to these moderate legislatures.

Lastly, note the slack present to the left of the median voter and to the right of the IG. As the cost of the initiative, and the ability of the legislature to overturn initiatives increase, so will the slack in the model. In figure 2 this slack means more legislatures producing policy to the left of the median voter and to the right of the interest group. When these two conditioning factors are both zero there should be no laws to the left of the median voter or to the right of the interest group.

-Insert Figure 3 about here-

This dynamic is of course symmetric around the median voter. Figure 4 shows the same relationships on the other side of the median voter, but with an interest group closer to the median voter. With costless initiatives that the legislature is unable to amend or overturn, policy divergence from the median voter is bounded by the distance of the interest group to the median voter.

-Insert Figure 4 about here-

While the intention of these figures is simply to re-specify Gerber's 1996 model as a baseline framework that is compatible with successive theorists, the basic conclusion of this paper should be increasingly clear: given the symmetric nature of convergence, the aggregate effect of the initiative process is to *reduce the variance around the policies demanded by constituents*. This is shown in Figure 5.

-Insert Figure 5 about here-

So while Matsusaka (2001) is concerned that we do not know the proper slope coefficient, and therefore we do not know which of the two slopes is closer to the truth, this analysis demonstrates that a naïve estimate of this slope is an unbiased estimate of the true slope coefficient. The quality of representation, then, is not about which slope is correct, but which slope has the smaller variation around the conditional mean of the estimate.

Measuring Representation

Matsusaka (2001) rightly claims that we could create a dependent variable based upon the sum of squared error, if we could measure the desired policy directly. He clarifies “directly”

as meaning one of two possibilities: either the topic is simple and straightforward like a dichotomous preference for (against) the death penalty; or the preferences of the public are directly evaluated (such as asking how many dollars we should spend per pupil). In both of these cases we know the exact level of divergence between public preferences and government policy. Matsusaka believes that it is largely absurd to collect public opinion data on something akin to exact spending levels, and so the only viable policy indicators are those that are dichotomous. This is currently the dominant solution.

Although the using differential slopes for a dichotomous variable slopes is well accepted, this paper concurs with that approach but the reasoning is vastly different. Typically, it is said that a dichotomous variable is acceptable because we can generate information on constituent opinion that directly translates into policy. In other words, the mapping function is known a priori. This allows a simple congruence measure between constituents and policy. The model presented herein leads to the same conclusion, but through a different approach. Instead of the simplicity of the mapping function, this model argues that what is unique about a dichotomous policy indicator is that there can be no over-representation. The elimination of the over-representation comes through the bounded nature of the policy indicator. This boundary converts the excessive variance (which this paper posits to be the basic indicator of poor representation) into a smaller coefficient. Therefore, a higher coefficient is indicative of better representation, but only because of the bounded nature of the dependent variable which makes that coefficient indicative of reduced variance.

When the dependent variable is continuous, or more accurately, the dependent variable has no realistic boundaries that would transform variance attenuation into coefficient amplification, then the variance must be explicitly modeled independent of the slope coefficient.

Analyzing the variance could be done in three different ways. First, lower variance around the estimate will be seen in a higher R^2 when samples are split. In addition to re-introducing an “Achen problem,” in which extra variance in the independent variable will artificially increase the R^2 , this approach is exceedingly difficult to include controls or make comparisons between institutions. Each factor thought to increase responsiveness would necessitate a new split in the samples. Second, squared residuals measure the variance around the estimate. While such an approach does permit multivariate estimation, the sequential structure does not accurately estimate slope coefficients in light of modified standard errors, and vice-versa. Finally, we can simultaneously estimate slope and variance parameters in the same model. Although rarely done, OLS theoretically provides the means for such a dual estimate (Franklin 2005).

Although OLS estimates are typically substantively interpreted as a single parameter model of beta coefficients, the second parameter (sigma) can be more than a nuisance parameter. In the modified equation below, \mathbf{z} is a vector of estimable factors (with a constant – gamma) affecting the variance. As the initiative has no theoretical reason to affect the conditional mean of y , but should instead affect the variance around that estimate, estimating the beta coefficients is inaccurate. The initiative, or other factors thought to induce convergence, should be estimated through sigma, not beta. Maximizing this function provides a direct test of initiatives constricting legislative outputs closer to the demand function of their constituents. This also has the benefit of exceedingly easy incorporation of proper multivariate controls of variance.

$$y_i \sim \frac{1}{\sqrt{2\pi e^{z_i\gamma}}} e^{-\frac{1}{2} \left[\frac{(y_i - x_i\beta)^2}{e^{z_i\gamma}} \right]} \quad (2)$$

All together, this approach provides a theoretically and empirically sound test to determine if direct democracy brings state policy closer to that of constituent demands. We can map differing scales onto one another, determine the level of divergence, and empirically evaluate the factors that affect divergence in the face of proper multivariate controls.

Replications

I test this approach by replicating results from previous studies. The first is Arceneaux's (2002) piece that distinguishes between a general ideological demand function and a topic specific indicator of constituent preferences. Using the basic interactive dummy approach, Arceneaux finds confirming results both for the distinction between general and specific policy demands, as well as the effect of the initiative through the interaction. Table 1 shows an exact replication of Arceneaux's findings next to the same full model specification with a parameterized variance estimate.

The coefficients of the two models are indistinguishable, but the standard errors in the second model are uniformly smaller. While simultaneously estimation of the variance creates a better model fit overall, the real importance is the parameter estimates on the variance part of the equation. The second column is simply the exact replication with an added indicator variable in the variance equation. This finds, as expected, that the initiative shrinks the variance around the estimated relationship between ideology and public policy. The third column attempts the same replication while replacing the indicator variable for the initiative with the number of initiatives

in 2002. The replication is very similar substantively, but does not include the interaction effects. These are not included because the model would not converge when they are included, but this doesn't affect the key theoretical findings. In addition the presence of the initiative leading to greater convergence, differentiating states by the number of initiatives also finds that a higher number of initiatives entails better convergence between constituent ideology and policy.

-Insert Table 1 about here-

The second test is of the classic findings denying the effect of the initiative: Lascher, Hagen, and Rochlin's 1996 "Gun Behind the Door? Ballot Initiatives, State Policies and Public Opinion." Like with Arceneaux, I first replicate the data (table 2). Although I was unable to perfectly replicate the data², as there are no differences of any significance, I consider this replication to be satisfactory as a baseline test.

-Insert Table 2 about here-

Unfortunately, the effect of the initiative on the variance of the estimate is not as clear in this case. Bolded are the three (out of 9) models where it looks as though initiatives might be having an effect on the variance (two-tailed $p < \sim .12$). Given the small-n of 47 and two-tailed level of significance, this is a far from unreasonable to consider the finding to be at least of some interest. I chose educational expenses to test the specification of the model. In sequential models I drop the interaction term, drop the intercept shift from initiatives, and then add a south dummy to the variance estimate. While these changes had no effect on the variance decrease provided by initiatives; the coefficient was always negative and the p values were consistently about .1.

-Insert Table 3 about here-

-Insert Table 4 about here-

² Much thanks to Michael Hagen for taking the time to dig up data from over a decade ago.

Although consideration of the other evidence may lead to acceptance of a p-value about .1, the dependent variable of Education Expenses was selected as it made the best case for the effect of the initiative. Considering the full set of possible policy areas makes a reversal of Lascher et. al's conclusions quite dubious. At the same time, the data for this test is quite limited.

The last replication also has the limitation of one year's worth of data, but fortunately provides clearer results. This last replication is of Burden's (2005) "Institutions and Policy Representation in the States." While this piece has multiple policy areas: abortion, death penalty, and a general policy index, it is only the policy index dependent variable that is continuous, so this is the only replicated regression. Burden concludes that the discharge petition leads to greater policy responsiveness, as indicated by a statistically significant positive interaction term, but that the initiative has no effect. The methodological critique, however, questions the interpretation of the statistical finding. Fundamentally, it may be the case that this positive interaction term indicates that the discharge petition skews the mapping function (slope) between constituent ideology and public policy *away* from the ideal relationship. To imply that a positive coefficient of a higher value is always better representation necessarily assumes that the perfect slope is infinity.

The alternative posits that public policy will sometimes be too conservative, and sometimes too liberal. Absent strong reason to think that public policy is biased in a certain direction, a naïve estimate of the mapping function will, on average, be correct. The real indicator of superior representation is that public policy within a class of states (like initiative states) has less variation around the estimated policy demanded by the constituents. Using this approach, the replication of Burden finds that the initiative does lead to superior policy

representation. This finding is robust to different models of the relationship between ideology and policy, as well as inclusion of controls for other representation inducing institutions.

-Insert Table 5 about here-

The first column of table 5 replicates the first column of Burden's third table. The following columns vary the control variables included in the variance equation (bolded), as well as the model specification for the slope estimates. Consistently across all specifications, the presence of this initiative shrinks the variance around the conditional mean of the estimated public opinion. This, I argue, indicates that the initiative entails superior policy representation.

The Arceneaux replication confirmed an interaction-based finding that the initiative leads to better policy responsiveness. The replication of Burden's findings, conversely, reveals a robust reversal of the finding that the initiative does not have a convergent effect. These all, however, have severe data limitations. One single year, generating forty-seven (or forty-eight) observations, makes it quite difficult to claim the test is conclusive. What is needed is a test not limited to a single year or haphazard policy areas. The next section provides that test.

Extension

This final test has two elements that are atypical. First, the Berry et al. (1998) data provides a long trend in which constituent as well as state government ideology is measured. Second, the measures are aggregates of general ideology. Such aggregation is more theoretically meaningful, less susceptible to idiosyncratic effects, and is a more conservative test.

The third column of table 6 shows the simultaneous estimation of the slope and variance estimates. Accurate to the theorized data generating process, compare the effects of population, legislative professionalism, and racial diversity. All three "matter" but it is only racial diversity

that affects the conditional mean. Upon controlling for a host of other factors, most notably citizen ideology of course, large and small states do not differ in the ideology of their governments. Similarly, legislative professionalism does not affect mean governmental ideology. These factors do however both affect the variance around those estimates. Populous states are more likely to be heterogeneous and send more conflicted signals to their representatives (Przeworski, Stokes, & Manin, 1999), similar to racial diversity. These both increase the variance around the relationship between constituent preferences and public policy. Legislative professionalism, conversely, provides a greater ability for representatives to respond to constituent demands, and this is reflected by a negative coefficient indicating greater convergence (Maestas 2000 also finds that legislative professionalism leads to greater policy convergence). While legislative professionalism and population only affect responsiveness (variance), racial diversity affects both responsiveness and the conditional mean of governmental ideology. States with greater racial diversity are more liberal.

What is most important, however, is the effect of ballot measures on representation. Quite simply, there is a slight (insignificant) drop for those states with the initiative, but a strong effect from the number of initiatives on the ballot. Note that the intercept shift from the initiative dummy treats California the same as Mississippi. The number of issues on the ballot captures the costs and restrictions that vary by state. Although the negative coefficient indicates reduced variation around the relationship between constituents and representatives, i.e. greater responsiveness, this impact is substantively much smaller than the effect of time, population, racial diversity, or legislative professionalism. It should be noted, however, that the significance of the initiative in a state is something that is fairly easy to modify. States without it can introduce it, and states with heavy restrictions can relax those restrictions. By comparison, there

is not much that can be done about racial diversity or large population size, or, more specifically, there is not much that a state would want to do about those.

-Insert Table 6 about here-

Conclusion

This paper set out with four tasks in mind: (1) explicate the problems of measuring responsiveness through changes in slopes estimates, (2) derive an alternative specification, (3) establish the validity of this specification, (4) use this approach to replicate previous studies, and (5) using new data, test whether or not initiatives act as a ‘gun behind the door’ to induce greater convergence between citizen preferences and the public policy.

The first goal, already well articulated by Matsusaka, was reiterated by returning to the theorized data generating process. This not only demonstrates that slope estimates should be interpreted as an unbiased constituent demand function, but also reveals the solution: that it is the *variance* around the demand function that indicates the level of responsiveness to constituent preferences. Estimation of this variance (goal 3) is fairly simple. A slight modification of the basic OLS estimator allows parameterized estimates of variance. By exploiting the simultaneous estimation of beta & sigma, we can find and compare changes in variance across different slopes even while including controls for different institutional designs.

Finally, this estimator demonstrated the efficacy of the initiative in inducing policy convergence towards constituent preferences. Replicating previous studies of this question, I have found clear agreement with Arceneaux’s affirmation of the hypothesis, agreement with Lascher et. al in the finding of null results, but a clear reversal of Burden’s findings. Moving to my own test of this question, I diverged from previous approaches of looking at individual topics in singular years. Using Berry et. al’s index of citizen and government ideology over 32 years, I am able to conclude that the initiative brings convergence between policy and preferences.

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Figure 1.

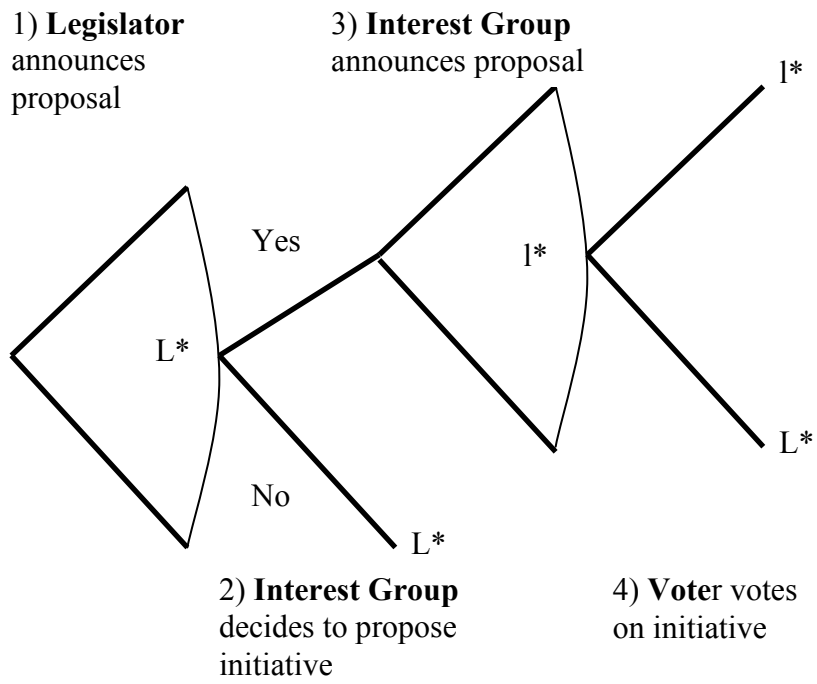


Figure 2.

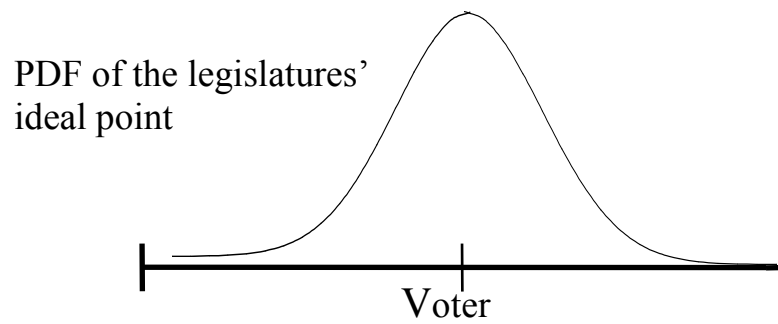


Figure 3.

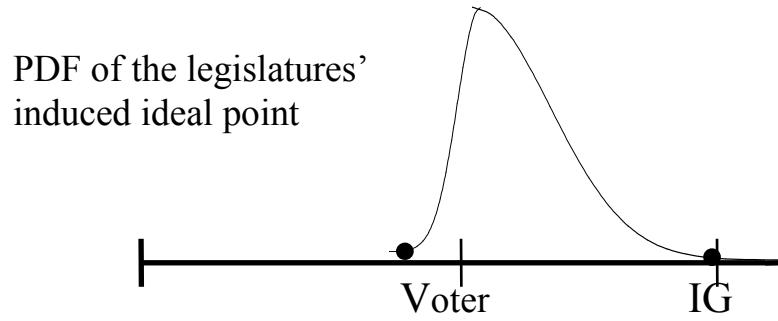


Figure 4.

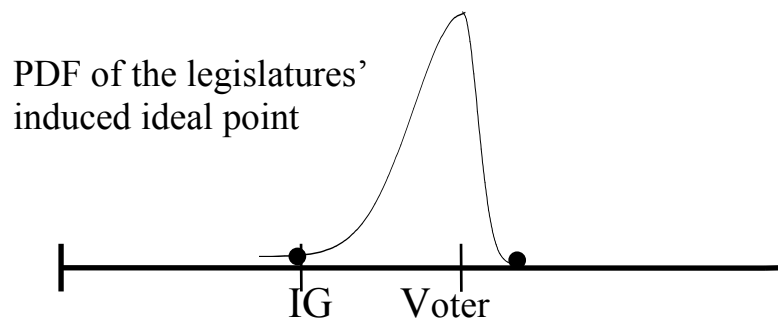


Figure 5

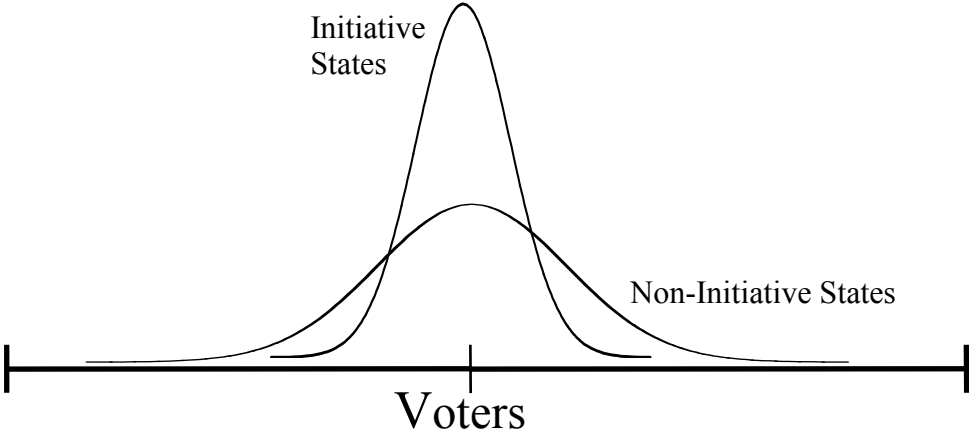


Table 1.

DV: Nara! abortion index			
	Arceneaux Replication	Parameterized variance	Parameterized variance (number of initiatives)
gss abortion attitudes	-33.67** (11.88)	-39.76** (9.49)	-63.02** (8.26)
EWM Ideology	1.67** (0.48)	1.52** (0.46)	0.52 (0.43)
percent fundamentalists	-0.17 (0.40)	-0.23 (0.32)	-0.61 (0.32)
percent female legislators	-1.33* (0.64)	-0.78 (0.50)	-0.69 (0.50)
divided government	0.73 (0.50)	0.94* (0.42)	0.55 (0.53)
initiative/referendum	127.94 (69.10)	114.72* (52.21)	
Abortion attitudes X initiative	-34.15* (16.54)	-30.91* (12.41)	
Ideology X initiative	-0.72 (0.98)	-0.63 (0.85)	
Number of Initiatives			0.31 (1.25)
Constant	201.67** (51.72)	215.04** (41.46)	302.71** (35.10)
Variance effect of initiatives		-7.14* (3.46)	
Variance effect of number of initiatives			-1.60* (0.70)
Variance constant		15.30** (2.13)	16.86** (2.30)
Observations	40	40	40
R-squared	0.75		
Prob > chi2		0.000	0.000
* significant at 5%; ** significant at 1%			

Table 2.

Replication of Table 3 - Lascher, Hagen, and Rochlin										
	AFDC	Consumer Policy	Criminal Justice	Education Expenses	ERA	Gambling	Medicaid	Tax Progressivity	Policy Liberalism	
HS Grad	5.860** (1.42)	0.188* (0.08)	2.094 (3.39)	0.909 (0.71)	0.120* (0.05)	1.264 (3.08)	0.412 (0.38)	0.22 (0.12)	0.038** (0.01)	
Income	0.006 (0.01)	0 (0.00)	0.012 (0.03)	0.017** (0.01)	0 (0.00)	0.009 (0.02)	-0.001 (0.00)	0 (0.00)	0 (0.00)	
Urban	0.049	0.03	0.45	-0.105	-	-0.158	0.092	0	0.001	
Liberal Opinions	(0.43)	(0.03)	(1.04)	(0.22)	0.045** (0.02)	(0.94)	(0.12)	(0.04)	(0.00)	
Init	6.895** (1.72)	0.134 (0.10)	7.097 (4.12)	4.403** (0.86)	0.205** (0.06)	20.771** (3.74)	1.758** (0.46)	0.337* (0.14)	0.104** (0.02)	
Init X Lib. Op.	-66.176 (34.71)	0.829 (2.07)	-29.346 (83.02)	-36.146* (17.24)	-1.774 (1.26)	-51.544 (75.34)	-9.796 (9.33)	-2.83 (2.82)	-0.469 (0.33)	
Constant	-4.133 (2.07)	0.095 (0.12)	-2.204 (4.96)	-2.231* (1.03)	-0.004 (0.08)	-6.747 (4.50)	-0.587 (0.56)	-0.096 (0.17)	-0.024 (0.02)	
	-105.978 (97.77)	0.147 (5.82)	-18.433 (233.88)	96.635 (48.56)	-0.626 (3.55)	379.383 (212.24)	128.065** (26.27)	-8.786 (7.95)	-1.824 (0.94)	
Observations	47	47	47	47	47	47	47	47	47	47
R-squared	0.68	0.46	0.26	0.76	0.57	0.64	0.48	0.32	0.79	
Standard errors in parentheses										
* significant at 5%; ** significant at 1%										

Table 3.

Parameterized Variance Version of Table 3 - Lascher, Hagen, and Rochlin									
	AFDC	Consumer Policy	Criminal Justice	Education Expenses	ERA	Gambling	Medicaid	Tax Progressivity	Policy Liberalism
HS Grad	5.944** (1.152)	0.173* (0.085)	1.478 (3.207)	1.063 (0.592)	0.122** (0.04)	1.169 (2.816)	0.408 (0.426)	0.221* (0.088)	0.038** (0.012)
Income	0.006 (0.009)	0 (0.001)	0.02 (0.026)	0.018** (0.004)	0 (0)	0.007 (0.017)	-0.001 (0.003)	0 (0.001)	0 (0)
Urban	0.041	0.017	0.215	-0.243	-	-0.161	0.087	-0.001	0.001
Liberal Opinions	(0.388)	(0.025)	(1.063)	(0.181)	0.046**	(0.865)	(0.143)	(0.039)	(0.005)
Init	6.955** (1.951)	0.129 (0.078)	6.795 (3.761)	4.493** (0.874)	0.205** (0.071)	21.018** (3.25)	1.761** (0.392)	0.335** (0.124)	0.104** (0.013)
Init X Lib. Op.	-66.566 (35.938)	0.911 (1.812)	-26.166 (70.586)	-36.822* (15.443)	-1.788 (0.948)	-50.724 (59.991)	-9.765 (9.467)	-2.837 (2.465)	-0.469 (0.301)
Constant	-4.139 (2.199)	0.1 (0.096)	-2.078 (4.444)	-2.200* (0.942)	-0.004 (0.062)	-6.765 (3.578)	-0.586 (0.507)	-0.096 (0.143)	-0.024 (0.017)
Variance: Initiative	-103.783 (85.265)	-0.719 (4.24)	-41.578 (245.25)	89.654* (42.772)	-0.828 (3.201)	408.121* (177.474)	128.203** (23.172)	-9.123 (6.537)	-1.849* (0.75)
Variance: Constant	2.395 (10.317)	1.063 (0.691)	36.825 (19.757)	-8.909 (5.195)	-0.253 (0.312)	-25.46 (18.349)	0.412 (2.426)	0.344 (0.678)	0.018 (0.086)
Observations	45.881** (6.359)	2.282** (0.348)	94.531** (11.382)	27.064** (4.466)	1.811** (0.255)	112.555** (13.083)	12.432** (1.556)	3.661** (0.45)	0.441** (0.058)
	47	47	47	47	47	47	47	47	47

* significant at 5%; ** significant at 1%

Table 4.

Parameterized variance. DV: Education Expenses				
HS Grad	1.063 (0.592)	1.048 (0.621)	0.881 (0.581)	1.176 (0.632)
Income	0.018** (0.004)	0.018** (0.004)	0.019** (0.004)	0.017** (0.005)
Urban	-0.243 (0.181)	-0.277 (0.184)	-0.273 (0.186)	-0.248 (0.197)
Liberal Opinions	4.493** (0.874)	2.908** (0.611)	2.969** (0.615)	2.835** (0.714)
Init	-36.822* (15.443)	-5.251 (7.767)		
Init X Liberal Opinions	-2.200* (0.942)			
Constant	89.654* (42.772)	65.199 (43.970)	71.55 (43.689)	61.642 (43.248)
Variance: Initiative	-8.909 (5.195)	-10.665 (5.981)	-10.15 (6.068)	-9.081 (5.440)
Variance: South				-14.056** (4.854)
Variance constant	27.064** (3.949)	29.273** (4.466)	29.101** (4.468)	31.849** (4.592)
Observations	47	47	47	47
Standard errors in parentheses				
* significant at 5%; ** significant at 1%				

Table 5.

		DV: Policy Liberalism				
		Burden				
	Replication	1	2	3	4	
Ideology	0.11*	0.09*	0.095***	0.095***	0.09***	
	(0.06)	(0.056)	(0.012)	(0.012)	(0.02)	
Discharge	0.004	0.015				
	(0.024)	(0.024)				
Initiative	-0.64*	-0.85**				
	(0.37)	(0.35)				
Ideology X Discharge	.003	0.0005				
	(0.127)	(0.001)				
Ideology X Initiative	-0.047**	-0.059***				
	(0.022)	(0.021)				
Legislative Professionalism	2.4***	2.30***				
	(0.69)	(0.62)				
Turnover	1.78**	1.3*				
	(0.815)	(0.75)				
Constant	-0.533	-0.51	1.37***	1.37***	1.27***	
	(1.148)	(1.11)	(0.19)	(0.19)	(0.27)	
Initiative		-0.27**	-0.28*	-0.291*	-0.323**	
		(0.12)	(0.14)	(0.15)	(0.155)	
Avg Number of Initiatives		0.16		0.037	0.0127	
		(0.12)		(0.121)	(0.13)	
Number Initiatives (1988)			0.078	0.07	0.105	
			(0.064)	(0.068)	(0.09)	
Discharge					0.003	
					(0.01)	
Legislative Professionalism					0.21	
					(0.47)	
Constant	0.53***	0.53***	0.66***	0.65***	0.48	
	(0.05)	(0.09)	(0.1)	(0.1)	(0.43)	
Observations	48	48	48	48	48	
Prob > chi2	0	0	0	0	0	

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
 Bolded variables sigma coefficients

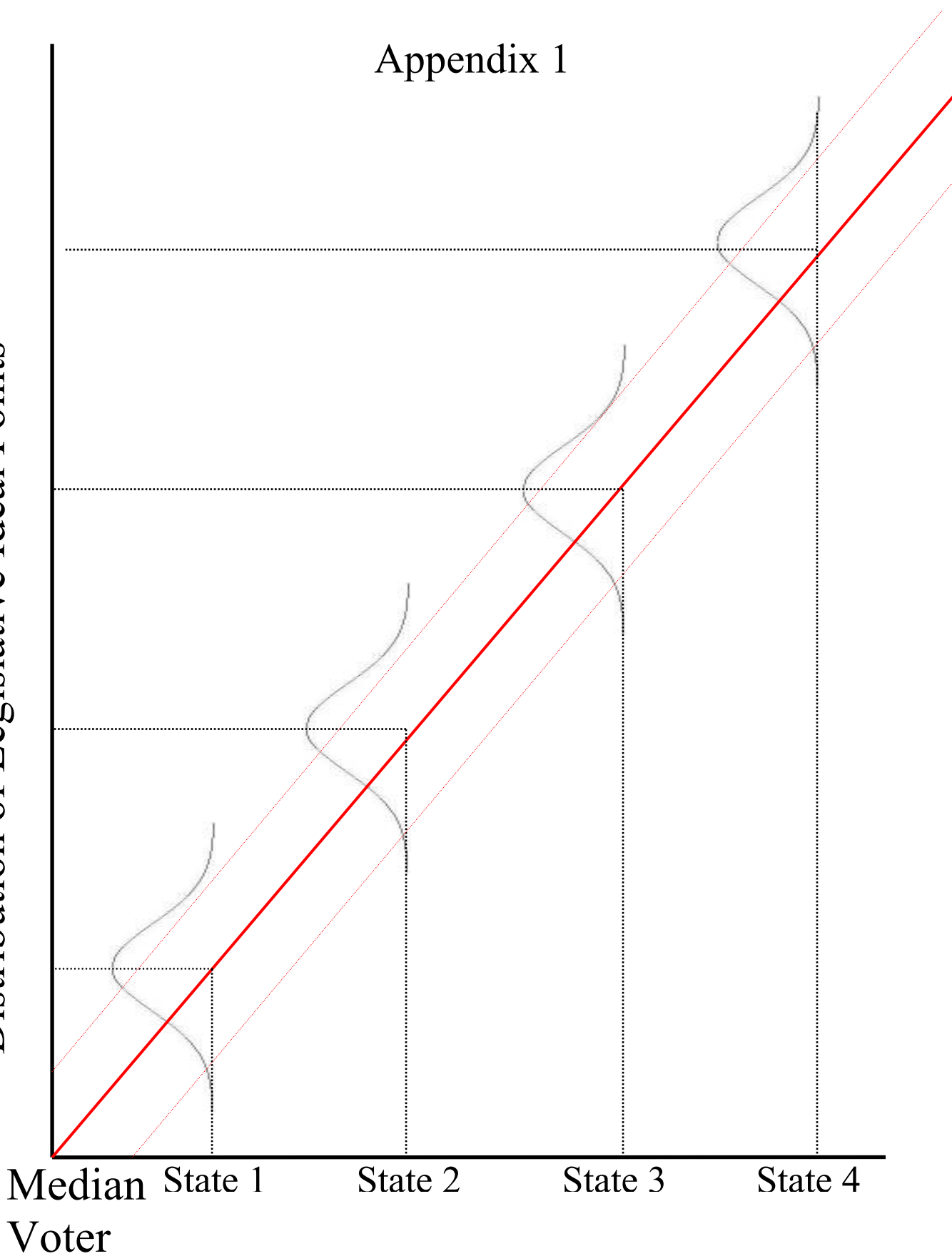
Table 6.

Dependent Variable: Institutional Ideology (Berry)				
	Ballot_Inter	Init_Inter	Slope	Variance
Citizen Ideology (Berry)	1.600***	1.755***	1.815***	
	(0.392)	(0.393)	(0.303)	
Citizen Ideology Squared	-0.0105	-0.0133	-0.0155**	
	(0.00848)	(0.00852)	(0.00690)	
Citizen Ideology Cubed	8.04e-05	9.89e-05*	0.000110**	
	(5.72e-05)	(5.74e-05)	(4.80e-05)	
Initiative		3.336	-0.213	-0.380
		(2.866)	(1.010)	(0.817)
Number of Ballot Issues	0.549		-0.434*	-0.446**
	(1.358)		(0.250)	(0.220)
Citizen Ideology X Initiative		-0.126**		
		(0.0552)		
Citizen Ideology X Ballot	-0.0158			
	(0.0254)			
Year	-0.204***	-0.199***	-0.0655	0.243***
	(0.0499)	(0.0502)	(0.0510)	(0.0428)
Population	-1.77e-07	-1.81e-07	-9.03e-08	3.66e-07***
	(1.17e-07)	(1.16e-07)	(1.10e-07)	(8.97e-08)
Legislative Professionalism	6.151	9.470*	0.881	-9.242**
	(5.276)	(5.433)	(4.724)	(4.269)
Herfindahl Race Index	-6.122	-4.152	-10.40***	13.95***
	(3.916)	(4.002)	(3.966)	(2.842)
Recall	1.315	1.414	0.892	0.359
	(0.966)	(0.963)	(0.912)	(0.826)
South	16.66***	16.55***	13.29***	-2.828***
	(1.333)	(1.329)	(1.310)	(0.831)
West	7.240***	7.812***	7.658***	0.613
	(1.254)	(1.311)	(1.332)	(1.050)
Constant	390.6***	376.2***	118.6	-477.1***
	(100.1)	(100.7)	(102.3)	(86.35)
Observations	1600	1600		1600
R-squared	0.515	0.518		0.51

Standard errors in parentheses *** $p \leq 0.01$, ** $p < 0.05$, * $p < 0.1$

Appendix 1

Distribution of Legislative Ideal Points



Induced equilibrium for legislative output

