Political Institutions and Policy Choices: Empirical Evidence from the United States

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Abstract

The rich array of institutional diversity makes the United States an excellent testing ground for studying the relationship between political institutions and public policy outcomes. This essay has three main aims. First, it reviews existing empirical evidence on the relationship between institutional rules, political representation and policy outcomes. It aims to place the literature into a broader context of theoretical and empirical work in the field of political economy. Second, we develop a parallel empirical analysis which updates some studies in the literature as well re-examing some of the claims in a unified setting (both in terms of policy outcomes and the time period in question). Third, the paper develops some new directions for research, presenting a small number of novel exploratory results.

1 Introduction

In recent years, there has been increased interest in bringing economics and politics together to understand policy. This resurgence of political economy has generated new questions and new models. Insights from game theory, contract theory, and mechanism design theory now permeate our understanding of the political sphere.[1] The recent literature has paid much more attention to theory than empirics.[2] The main point of this essay is to highlight a particular genre of empirical work in the field – that which studies the impact of institutional rules on policy outcomes in the United States.

The essence of the literature is illustrated in the following example. A number of states have recently passed laws that tie voter registration to motor vehicle registration, so-called "motor-voter" laws. The intention is to promote voting, particularly among disadvantaged groups that have low rates of turnout. Here, we might ask three questions. First, does the passage of such laws lead to greater turnout? Second, does this change in turnout (if there is one) change the composition of the legislature, for example, by increasing representation of Democrats? Third, does the increase in Democrat representation (if there is one) have an influence on policy outcomes? In principle, these questions can be addressed empirically using available data, and we discuss the answers below. In thinking through these possible chains of influence (as documented in the data), one is also drawn to thinking about the right theoretical context in which to understand the policy process.

The paper studies a rather heterogeneous group of institutions. Thus, the term institution should be understood in the rather broad sense suggested by North (1990) as "the humanly devised constraints that structure human interaction" (page 3). We use the term institutional choice in preference to the narrower idea of a constitutional rule, which suggests a formally encoded mandate. As observed by a number of commentators (such as Dixit (1996)) it does not make an awful lot of sense to draw hard distinctions between terms like institution, constitutional rule and (even) policies. Moreover,

[1] Persson and Tabellini (2000) bring many of these together in an excellent survey of the field.

[2] The development of the field of political economy is reminiscent of the Industrial Organization literature which, in the nineteen eighties, had a major theoretical surge. However, many of theories were found to have contradictory empirical implications. More recent progress has, therefore, put weight on empirical testing to match the earlier theoretical progress.

in practice, those who study constitutional economics use an equally broad conception of the term constitution. For example, Brennan and Hamlin (1998) characterize constitutional economics as studying the "basic rules under which social orders may operate" (page 401).

The survey will restrict itself mainly to evidence based on cross-state institutional differences in the United States. This has costs and benefits. The main cost is in the range of institutional differences that can be considered. While some interesting and important differences are available, there are a host of common features whose implications cannot be studied in this way. However, the latter is also a benefit as there is a reasonable hope of isolating the true source of differences, rather than attributing to a particular institution some effect that is in reality due to some other source of unobserved heterogeneity. The latter is a constant source of concern in cross-country analyses. A further benefit from the US focus is that there are a significant number of studies available on all aspects of institution differences.

In addition to reviewing the main contributions in the area, we also present new empirical results. By using a consistent set of data and methods, this will provide an additional perspective on the issues in question. It will also help us to illustrate some of the methodological issues involved in making use of cross-state institutional variation to study policy outcomes.

Most of the work that is discussed below is by economists. However, there is also a large body of relevant work coming out of political science departments. Within political science, there is far from universal acceptance of the value of formal and quantitative reasoning. The debate surrounding Green and Shapiro (1994) is indicative of this. Green and Shapiro criticize the willingness of formally inclined political scientists to engage in empirical testing. This essay is partly an effort to promote the work that has been done. However, it is clear from the existing literature that the evolution of theory and empirical evidence on understanding political institutions has been unbalanced. Much of the empirical testing eschews interpretation of results in terms of models, and hence fails to push forward the debates about modeling. At the other end, a good deal of theory seems to be lifting insights from game theory, contract theory and information economics without pushing testable implications.

The remainder of the paper is organized as follows. The next section presents some background discussion of the literature and the evolution of thinking in the field of political economy. Section 3 develops a framework for the study of political competition and discusses its empirical implications for the study of how political institutions affect policy outcomes. Section 4 reviews how institutional arrangements of different kinds vary across US States. Section 5 studies institutions that directly affect the process of political representation, to see whether and how they affect voter turnout and such legislative outcomes as the party identity and political ideology of the legislature and governor. Section 6 then examines how political representation affects state policies, particular those of taxing and spending. Section 7 considers institutional effects on policy outcomes, either through direct influence or through their proximate effect on representation. Section 8 turns to the question of how institutional rules are made and changed. Section 9 concludes.

2 Background

Understanding how institutions change policy is an important intellectual end in itself. However, it is also part of the kit bag that we need to improve the way in which the world works. Among the pioneers of the vision that a just polity involves the design of appropriate rules for policy formation is James M. Buchanan.[3] A great deal of public economics is about the choice of policies – the level of taxation, whether to mandate health insurance, etc. However, there are two reasons to think that this should be augmented with studies that go back one stage prior and evaluate the process on the basis of which policy is made. First, institutional reforms are frequently on the agenda and we need to have a framework (and empirical knowledge) for judging them. Second, the policy advice and insights that economists offer are mediated through the political system. It may be that once the workings of the political system are understood, then we would change the policy advice that we give. Policies that appeared sub-optimal may be desirable because of the way in which they are operated in political equilibrium. But this raises the larger question of whether it is better to change the rules by which policies are formed than to advocate policy changes themselves. To do this, then, requires an understanding of the mapping from institutional rules to policy outcomes.

One rather grand view of policy making, suggested by Buchanan, is to think of there being two stages of analysis. At the first stage, a constitution

[3]This way of thinking is much older. Buchanan credits Wicksell as a key intellectual forefather.

is designed.[4] This has two components. A *procedural constitution* sets the terms by which decisions are made (electoral rules, term limits, the separation of powers etc.). A *fiscal constitution* builds in constraints on the policies that can be adopted within the framework of the procedural constitution. This might, for example, include limits on taxation or particular forms of public spending. After the constitution is determined, policies are chosen. However, these are autonomous, and the key role for the policy advisor is at stage one, anticipating the outcome at stage two from some underlying model.[5]

While a useful benchmark, the reality is likely to be much messier. As argued by Dixit (1996) among others, the distinction between a rule and policy is quite narrow. A good example is the rule that prohibited many US states from levying an income tax, but which has been overturned by most states during the twentieth century. This kind of fiscal rule then looks much closer to what we would ordinarily call a policy than a rule. In practice, there may be larger costs associated with changing some aspects of the policy framework than others – the need for ratification by two thirds majorities is a good example. Thus, it is probably a little dangerous to try to draw a hard-and-fast distinction between the immutable constitution and the pliable policy arena.

The notion of designing an optimal constitution is tinged with hubris. In practice, the optimizing approach to policy analysis can be solved only under very stylized assumptions about the economic environment and incentive problems. Moreover, the bewildering array of different policy issues needing to be solved makes the notion of specifying the optimal constitution a distant dream. On a more practical level, we might hope to understand the workings of particular institutional changes. Much as in policy economics, we can debate how particular interventions – such as minimum wages or publicly funded broadcasting – affect the economy. The contributions surveyed here are more in pursuit of this agenda and make progress for precisely this reason. While it is true that there are probably important interactions between policy decisions – for example increasing the minimum wage may result in adjustments to welfare policy – we need to begin where concrete progress can be made.

[4] See Laffont (2000) for a conception of the optimal constitution problem from a mechanism design perspective.

[5] Besley and Coate (2001) develops a very simple model which illustrates the issues that might arise in the design of a fiscal constitution.

Most of the institutional changes analyzed here are procedural, in Buchanan's terminology, although there will be exceptions (balanced budget restrictions being a key example). Institutional rules are of two broad kinds: (i) electoral rules: restrictions on who can vote, whether proportional representation is used etc., and (ii) decision making rules: the use of line item vetoes, whether certain agencies are independent. The distinction is, however, not a very precise one.

To understand the effect of an institutional change on policy outcomes requires some kind of underlying model of the policy process. The importance of providing theoretical foundations is two-fold. First, the interpretation of a particular effect is normally (consciously or unconsciously) tied to a model. Second, and more importantly, is a concern about the potential generalizability of the findings. If the sole aim of a study is to answer a narrowly-posed question (did a particular historical institutional change have some impact?), then this may not be necessary. However, most authors would like to claim a result of more wide-ranging significance. This can most often be achieved by showing that it illuminates some particular theoretically validated relationship. The studies that are discussed below vary greatly in the extent to which the author(s) spell out the theory behind their results.

The political economy literature has developed a plethora of models that can be employed to address a wide range of questions, and here is not the place to develop any kind of survey of them. However, we will make a few comments about the theoretical literature that will be germane to what follows. For a comprehensive tour of the field see Persson and Tabellini (2000).

A great deal of theoretical political economy literature begins from the notion that policies should line up with the preferences of the median voter. Indeed, for a long period this was almost a caricature of economists' forages into political economy. The motivation comes from two key contributions – (i) the observation (most often attributed to Black (1958)) that restrictions on preferences (most often single-peakedness) imply that the median voter's preferred point is a Condorcet winner,[6] and (ii) the observation due to Downs (1957) that two parties who care only about winning would pick out the Condorcet winner if they could commit to policies during an election campaign. In spite of the model's centrality in the literature, both components provide

[6]A Condorcet winner is a policy that would beat all others in binary comparisons based on majority rule.

a deeply flawed basis for thinking through the implications of political competition for policy determination. First, policy environments with multiple issues rarely have a Condorcet winner. Second, the assumptions about commitment and motivation in the Downsian paradigm are unreasonable and outcomes are highly unrobust to deviations from them. A good example is illustrated by Alesina (1988), who endogenized commitment when parties have policy preferences.

Progress in theoretical political economy has had to wait for modeling approaches that leave the straightjacket imposed by the Downsian paradigm behind them. Political economy thrived in some quarters by throwing out explicit electoral modeling completely as, for example, in Peltzman (1976) and Becker (1981). However their reduced form approaches do not provide a way for thinking about the policy implications of institutional changes. It has also become apparent that modeling needs an approach that allows the simultaneous functioning of elections and interest groups to have a realistic hope of generating predictions that might apply in reality. Progress on this front has been slow but Grossman and Helpman (1996) and Persson and Tabellini (2000, section 7.5) point the way ahead.

Literatures have also sprung up that de-emphasize the spatial choice component of elections, instead concentrating on the role of elections in curbing opportunistic behavior of politicians (see, for example, Barro (1973) and Ferejohn (1986)). Below, we will argue that some of the cross-state literature from the United States speaks to the relevance of this approach for explaining the data.

Among political scientists, a large literature developed that focused on approaches to solving the problems of multi-issue decision making at the level of legislatures, but rarely has this been mapped back to overall electoral consequences. However, a key insight emerges from this that is useful in many other contexts. Part of the difficulty in the Downsian paradigm is the fact that there is little institutional restriction on policy proposals. It is very difficult to get a stable point when any policy can be proposed by any political actor at any time. In their seminal work on legislatures, Shepsle and Weingast (1981) proposed a particular extensive form structure that restricts this.

This general insight can be valuable in other contexts. For example, Besley and Coate (1997) restrict policies to those that are optimal for some citizen. Roemer (1999) restricts proposal power by modeling within-party conflict. All of these restrictions have been shown to provide a much better chance of having a theory that predicts an equilibrium outcome in a particular policy context, and hence the basis for an empirical analysis.

The notion that the structure of the power to propose policy is important is really part of a more generally valuable insight, that the structure of political institutions affects political outcomes. This is the core idea behind the empirical literature we discuss below. It should also be clear that this is likely to rest on departures from the median voter logic that has dominated so much of the political economy literature. If policies we see in the world really are responsive to the median voter's preferred point in any kind of general sense, then there is no good reason to think that the institutional structure of decision making would matter much at all.

One particular theoretical issue that is highlighted by many empirical studies is the need to have tractable frameworks to study the allocation of multiple issues. Most often analysts are interested in studying the impact of institutional rules that may affect only some part of the policy space or, at least, would affect different parts in varying ways. Thus, consider the effect of increased voter registration among minority voters. We would typically want to consider the impact of this on general policies and those that particularly affect the groups in question. However, to think about this requires a model where both issues are considered. It would not, for example, make much sense to consider a world where redistribution towards minorities (in the form, say, of affirmative action) were the only policy being determined in political equilibrium. Political scientists are well aware that changes in the salience of issues is central to understanding the evolution of policy (see, for example, Carmines (1994)). However, economists do not have well-developed frameworks for thinking about this.[7] Some play has been obtained from the multi-dimensional models of Lindbeck and Weibull (1993) and Dixit and Londregan (1998). They envisage two ideologically opposed parties who court groups of voters by promising transfers. While this yields useful insights, it cannot address the question of when the ideological dimension may shift – as arguably has been the case in a number of time periods in the U.S. It also seems odd to invoke such strong assumptions about commitment to transfers without specifying the mechanism by which credibility is achieved.

The post-Downsian literature in political economy suggests the following

[7]Besley and Coate (2000a,b) also provide a way of thinking about salience in a multiple issue framework and how institutions can change the salience of issues.

simple three-way theoretical classification of the importance of institutions. First, institutions may affect policy preferences directly, and their expression at the ballot box. The simplest possibility is their effect on who votes. However, policy preferences may change for a given voting population. For example, a voter may be more willing to tolerate a Democrat bound by a tax and expenditure limitation. Second, in a multi-issue world, institutions can affect policy priorities. Thus, incentives to target transfer programs to particular groups may change if that group is registered to vote. Citizens' initiatives can unbundle a particular issue and hence change its salience to voters. Third, institutions can change the ability of the policy process and particular politicians to commit. For example, a term-limit may reduce the credibility of election promises since an incumbent may not run again for office. These three effects can go a good way towards interpreting many of the findings below.

3 An Organizing Framework

The above scheme for understanding institutional changes emphasized that we could decompose impacts on the policy process into changes in voting, representation and policy. One possible role of theory is to isolate where the effect is likely to come from in any particular context. We now sketch a broad empirical framework that should be useful in variety of contexts. It is based on Besley and Case (1997).

3.1 Legislative and Policy Outcomes

We suppose that there is a large set of voters with preferences that depend on a vector of economic outcomes o_{st} in state s at time t. These outcomes in turn depend upon policy in state s at time t denoted by a vector x_{st}, a vector of economic and demographic characteristics in state s at time t, denoted y_{st}, and a state specific shock ε_{st}. Denote this relationship by

$$o_{st} = g\left(x_{st}, y_{st}, \varepsilon_{st}\right).$$

The preferences of voter i are denoted $u\left(o_{st}, x_{st}, \theta_i\right)$ where $\theta_i \in \Theta$ represents preference parameters of individual i. This specification allows for the policies to enter preferences directly as well as via their effects on outcomes. Let the reduced form preferences be $v(x_{st}, y_{st}, \varepsilon_{st}, \theta_i) \equiv u(g(x_{st}, y_{st}, \varepsilon_{st}), x_{st,}\theta_i)$ for citizen i. The shock ε_{st} is assumed to be realized after the election. However, prior to that, citizens receive a (common) signal σ_{st} about ε_{st}.

We assume that some subset of individuals belong to parties. There are two parties — which we label D and R – that maximize average utility among their party members. Let $\mathcal{M}_j$ denote the set of members of party j. Then

$$V\left(x, \chi_{jst}, y_{st}, \varepsilon_{st}\right) = \sum_{\theta_j \in \Theta, j \in \mathcal{M}_j} v(x, y_{st}, \varepsilon_{st}, \theta)\, \pi\left(\theta; \chi_{jst}\right); \quad j \in \{D, R\}.$$

where $\pi\left(\theta; \chi_{jst}\right)$ is the density of taste characteristics in party j in state s at time t and χ_{jst} are the parameters of that distribution. This specification allows for the same party in different places to have different policy preferences depending upon the economic conditions, history etc.

We assume that the policy enacted in the legislature reflects a compromise reached between the two parties with their "influence" reflecting the relative strength of their numbers. Let ℓ_{st} be a variable that denotes the state of the legislature in state s at time t. This should be thought of as a list of the characteristics of all the candidates who have been elected along with their party affiliations (and any other relevant characteristics). Parties cannot choose x_{st}, but they do choose a list of candidates, one for each district. Let c_{ist} be the candidate list of party i in state s at time t, and X_{ist} be the "platform" of party i in state s at time t,[8] then we suppose that

$$x_{st} = G(\ell_{st}, X_{Dst}, X_{Rst}, I_{st}, \varepsilon_{st})$$

where I_{st} are the institutional rules in force in state s at time t. The function $G(\cdot)$ is intended to capture the reduced form of a potentially complicated bargaining model of legislative decision making. In general, we will say that policy outcomes reflect the numerical strength of each party and the vector of platform choices that the two parties have chosen. This is consistent with a variety of models of legislative choice models.

The number of seats in the legislature is determined in an election. We do not model this explicitly. In reduced form, let $P(\ell; X_{Dst}, X_{Rst}, c_{Dst}, c_{Rst}, z_{st}, I_{st}, \sigma_{st})$

[8] This model is not specific about the nature of X_{ist}. It could be a vector of candidate's characteristics or a vector of promises that are to be carried out ex post.

denote the probability that the state of the legislature is ℓ, when the platforms of the parties are (X_{Dst}, X_{Rst}), the candidate choices are (c_{Dst}, c_{Rst}) and the vector of relevant state characteristics is z_{st} including the distribution of tastes in the whole state population. This could be motivated by a variety of models, including some kind of probabilistic voting framework.

We can write the payoff function of the Democratic party in state s as

$$\sum_{n=1}^{L_s}\{P\left(\ell; X_{Dst}, X_{Rst}, c_{Dst}, c_{Rst}, z_{st}, I_{st}, \sigma_{st}\right)$$
$$\times E\left\{V\left(G\left(\ell, X_{Dst}, X_{Rst}, I_{st}, \varepsilon_{st}\right), \chi_{Dst}, y_{st}, \varepsilon_{st}\right) : \sigma_{st}\right\}\}$$

and that of the Republicans is

$$\sum_{n=1}^{L_s}\{P\left(\ell; X_{Dst}, X_{Rst}, c_{Dst}, c_{Rst}, z_{st}, I_{st}, \sigma_{st}\right)$$
$$\times E\left\{V\left(G\left(\ell, X_{Dst}, X_{Rst}, I_{st}, \varepsilon_{st}\right), \chi_{Rst}, y_{st}, \varepsilon_{st}\right) : \sigma_{st}\right\}\}.$$

This is simply their expected utility taking into account the number of seats won and the resulting policy outcome.

It is useful to note that this approach encompasses within it the standard Downsian approach to policy competition where the choice of candidates is irrelevant – policy is determined ex ante with full commitment. It also includes a Lindbeck and Weibull (1987) type of model. It also encompasses the approach suggested in Besley and Coate (1997), (2000a,b) where competition takes place by candidate proposal. In this encompassing approach, we suppose that policy platforms and candidate lists are chosen simultaneously by each party to maximize their payoffs. We assume that the resulting Nash equilibrium exists and is unique,[9] and denote it by

$$\left(X_D^*\left(z_{st}, y_{st}, \chi_{Dst}, \chi_{Rst}, I_{st,}\sigma_{st}\right), X_R^*\left(z_{st}, y_{st}, \chi_{Dst}, \chi_{Rst}, I_{st}, \sigma_{st}\right)\right)$$

and

[9] It is well known that this is not a trivial matter. Here, it would require sufficient smoothness and concavity assumptions on the payoff function and the function that maps policies into number of seats.

$$\left(c_D^*\left(z_{st}, y_{st}, \chi_{Dst}, \chi_{Rst}, I_{st,}\sigma_{st}\right), c_R^*\left(z_{st}, y_{st}, \chi_{Dst}, \chi_{Rst}, I_{st}, \sigma_{st}\right)\right)$$

Given a particular realization of the number of Democrats and Republicans in the election and the shock ε_{st}, the policy outcome will be

$$x^*\left(\ell, z_{st}, y_{st}, \chi_{Dst}, \chi_{Rst}, I_{st}, \varepsilon_{st}\right) = G\left(\ell, X_D^*\left(\cdot\right), X_R^*\left(\cdot\right), I_{st}, \varepsilon_{st}\right).$$

Policy thus depends on state characteristics relevant to determining preferences over policy, tastes of party members $\left(\chi_{Dst}, \chi_{Rst}\right)$, electoral and decision making institutions, and the number of Democrats. Viewed *ex ante*, the state of the legislature will be determined by the equilibrium candidate choices, the platforms that are chosen, and the signal σ_{st} that has been received about ε_{st}. Note that policy making is also a function of the outcome shock ε_{st} so that, as an empirical matter, it would not be legitimate to condition on ℓ in modeling policy outcomes if σ_{st} and ε_{st} are strongly correlated.

The theory set out here is too broad-brush to generate predictions. In practice, the modeling approach needs to be more specific and tailored to the specific institutional choice being studied. Ideally, theory should offer some comparative statics results of the effect of institutions on either policy or electoral outcomes. These could then be taken to the data. In cases where the theory is not unambiguous, it would ideally give some account of the competing effects that might be present in the data and how, if it all, their presence can be tested.

The model could easily be extended at this level of generality to handle a number of issues. First, we have assumed that there is only one representative institution within a state. However, in reality, there are usually three – two legislatures and a governor. In some states, the party tickets also include an array of other public officials. Second, the payoff function specified is static while electoral competition is an on-going process. This can be rectified. To do so properly would demand setting up a recursive structure which would not make a lot of sense at this level of generality. However, in a reduced form sense, we could think of $V\left(\cdot\right)$ as a dynamic payoff function. Third, we assumed that the structure of party decision making is centralized rather than candidate choice being undertaken at the district level. In reality district level parties can play a role in selecting which candidates stand.

In principle the above approach could give way to a fairly complicated structural model that considers endogeneity of candidates and policy platforms at stage one, followed by a model of the electoral outcome at stage two, and then a policy choice at stage three. This kind of ambitious undertaking has not, to our knowledge, been attempted. There are clear data limitations. First, it is rare to have detailed information on the platforms (X_{Dst}, X_{Rst}). In a very naive model, they would be equated to the policy *outcomes.* However, this does not appear to be very reasonable. Second, it is rare to have very detailed information on candidates. Standard data sources tell us a lot about incumbents, but little about challengers. Third, we do not typically have independent information about party preferences. In principle, we might glean this from surveys of party members.

Given these issues, the literature has tended to pursue more reduced form approaches. There are models that study the effects of political institutions on various electoral outcomes. Most often, this reflects how I_{st} affects who is elected ℓ, e.g. whether the legislature is dominated by Republicans or Democrats. For the kth political outcome, we have the following reduced form model:

$$\ell_{kst} = \zeta_{ks} + \xi_{kt} + \lambda^k I_{st} + \phi^k Y_{st} + \nu_{kst}, \tag{1}$$

where ζ_{ks} is a state indicator, ξ_{kt} is a year indicator and $Y_{st} = (z_{st}, y_{st})$. In practice, Y_{st} are represented by socio-demographic and economic variables.

Most other efforts have gone into modeling how policies respond to institutional choices. For policy, we have the following reduced form model for the jth policy in state s at time t of the form:

$$x_{jst} = \alpha_{js} + \beta_{jt} + \omega_j I_{st} + \gamma^j Y_{st} + \eta_{jst}, \tag{2}$$

where α_{js} is a state indicator variable and β_{jt} is a year indicator. The outcome shock ε_{st} is part of η_{jst} while the signal σ_{st} is part of ν_{kst}. In both cases, the interest is on how I_{st} affects the outcome of interest.

These reduced form approaches make no effort to model the interdependence of policy and political outcomes. However, the theory certainly supports the possibility of such links. There are two main variants of the pure reduced form model that are worth considering. In the first of these the variables in ℓ_{st} are introduced into the policy equation. Thus (2) is modified to

$$x_{jst} = \alpha_{js} + \beta_{jt} + \omega_j I_{st} + \gamma^j Y_{st} + \psi^j \ell_{st} + \eta_{jst}. \tag{3}$$

The main additional interest is now in explaining ψ^j. A good example of this in practice is trying to understand how party control of the legislature or the governors chair affects policy. The difficulty with this formulation is immediately apparent from the theory above. The fact that ε_{st} is part of η_{jst} and σ_{st} is part of ν_{kst}, makes it clear that we should expect correlation between ℓ_{st} and the error in (3). Intuitively this arises because voters and parties may choose policies and candidates to reflect the economic circumstances that they anticipate facing. Thus political outcomes are affected by the same forces that also shape policy directly. The answer lies in finding either an institutional variable (one of the I_{st}) or some other variable (in Y_{st}) that can serve as instrument for ℓ_{st}, i.e. does not belong in the equation explaining x_{jst}. We discuss this further in section 7.6 below.

The model that we laid out above is static. However, it is clear that policy decisions and electoral choices take place in dynamic setting. There are two main issues that need to be raised in relation to this. First, consider the interpretation of the policy outcome function $G(\cdot)$. This may now embody forward looking behavior, whereby incumbents choose policy to affect the probability that they are re-elected in future as, for example, in Besley and Case (1995). Also important is the possibility that policy today is used strategically to influence future policy outcomes as in Tabellini and Alesina (1987) or Persson and Svensson (1988). Policies may also be regarded as state variables when it is costly to make changes. In the election model, we could reinterpret the payoffs above as a value function reflecting the future discounted present value of all future payoffs. Let x_{st-1} be the vector of policies in place at time $t-1$. Then, we can write

$$x_{st} = G(\ell_{st}, X_{Dst}, X_{Rst}, I_{st}, \varepsilon_{st}, x_{st-1})$$

to reflect any time dependence. Dynamics are also important when looking to see how past policy outcomes affect future reelection chances. Thus, consider the support that a party or candidate receives as a function of x_{st}. The function $P(\ell; \cdot)$ could now depend on x_{st-1}. As we shall see below, there is a large literature that worries about the link between policy and electoral success. In terms of the empirics, this would mean modeling something along the lines of

$$\ell_{kst} = \zeta_{ks} + \xi_{kt} + \lambda^k I_{st} + \phi^k Y_{st} + \kappa^k x_{st-1} + \nu_{kst}. \tag{4}$$

This presents econometric problems if x_{st-1} is not strictly exogenous. The particular concern is likely to be that policy is set in relation to some signal about how the election will be fought in the future. We would also modify (2) to

$$x_{jst} = \alpha_{js} + \beta_{jt} + \omega_j I_{st} + \gamma^j Y_{st} + \tau_j x_{st-1} + \eta_{jst}, \tag{5}$$

The presence of a lagged dependent variable in this fixed effect model leads to bias in short panels, or in cases in which there is heterogeneity in the parameters to be estimated.

The above specification assumes that there is time series and cross-sectional variation in the institutional variables (I_{st}). In many cases discussed below, researchers are interested in studying the impact of institutions that do not vary over time. Then, the above model is not estimable. Such studies usually proceed either in cross-section or by omitting the fixed effects, hoping that x_{st} will capture the relevant heterogeneity over the cross-sectional units. For this to be a satisfactory way of estimating the impact of institutions on outcomes, we need to assume, in the usual way, that η_{st} is uncorrelated with the institution in question. This is a very strong assumption.

An alternative way to get mileage out of fixed institutions is to derive (preferably from some underlying theoretical structure) their implications for the response of an outcome to a time-varying regressor. For example, Besley and Coate (2000a) consider the effect of electing versus appointing regulators on the relationship between cost and price. If such implications exist, one can exploit the panel nature of the data even where there is only cross-sectional variation in institutions.

3.2 Institutional Change

Institutions do change over time – this is a simple fact and it motivated the reason why we are interested in seeing what effects they have. This raises the question of whether we can develop some underlying theory of institutional change. If institution building is purposeful, then we would expect societies to choose their political institutions based on a view of how institutions affect policy. If institutions are to be more inflexible than policies, then we would expect some elements that make institutional changes relatively infrequent. We now consider this issue in the model set out above. First consider the preferred institutional structure of either party defined as

$$\begin{gathered}I_{jst}^{*}=\arg\max_{I_{st}}\{\textstyle\sum_{n=1}^{L_s}P\left(\ell;X_D^*\left(\cdot\right),X_R^*\left(\cdot\right),c_D^*\left(\cdot\right),c_R^*\left(\cdot\right),z_{st},I_{st},\sigma_{st}\right)\times\\ E\left\{V\left(G\left(\ell,X_D^*\left(\cdot\right),x_R^*\left(\cdot\right),I_{st},\varepsilon_{st}\right),\chi_{jst},y_{st},\varepsilon_{st}\right):\sigma_{st}\right\}\}.\end{gathered}$$

Note here that institutions can affect the probability that a particular party wins office as well as the rules of the game conditional on a party having won. In a dynamic model, we would expect the payoff to be the discounted present value of all future elections fought under particular institutional conditions.

One possibility is that electoral and decision making institutions are chosen *strategically* to affect future election outcomes. This draws a parallel between the literature on institutional change and strategic policy making as developed, for example, in Tabellini and Alesina (1987) and Persson and Svensson (1988). In such models, an incumbent chooses to constrain future policy makers by changing the level of public debt that they inherit. There are cases where this Machiavellian theory of institution would seem relevant (at least anecdotally). A good example is the case of decentralization. In the UK, Tony Blair's increased decentralization will result in a counterweight to the power of the conservative party. It may also make it less likely that the conservatives will win office in the future. Republican efforts to decentralize welfare policy in the United States could also be viewed strategically as an effort to encourage a race to the bottom with a resultant reduction in welfare spending.

So far, we have not discussed the possibility that institutions are endogenous as a source of correlation between I_{st} and the error. The issue of endogeneity is often discussed in confusing ways. Many state institutions in the U.S. date back more than century while others are a more recent adoption. One might be tempted to argue that the former are less susceptible to concerns about endogeneity than the latter. There is no particular reason to believe that this is the case.

A proper treatment of these issues requires a model of what drives institutional rules. This depends, of course, on the framework in which institutions are adopted which will vary from place to place. However, suppose that every society that we wish to study has its institutions constantly under review. Hence in every period, a decision is made about which institutions to use to make social decisions. Then, we might capture this empirically with the following equation.

$$I_{st} = \alpha_{es} + \beta_{et} + \gamma_e x_{st-1} + \sigma_e w_{st} + \omega_{est}$$

The key additional variable here is w_{st}. These are time varying influences that drive institutional choice. They give a source of exogenous variation in institutions that we would like to exploit to measure the impact of institutions. However, they are rarely measurable. The worry arises if $cov\left(w_{st}, \eta_{st}\right) \neq 0$. Observe that this concern is much greater in cross-sectional than in panel data studies. It is plausible in many instances to think that the variables w_{st} are not time varying and hence would be absorbed in the state fixed effects. Good examples are long-run variations in political culture across regions or countries. However, the author of a cross-sectional study or a panel data study that omits fixed effects, has to assume that there are also no *time-invariant* omitted regressors that drive institutional choice.

3.3 Structure of the Survey

Our analysis will look at the relationship between institutions, legislative outcomes and policy outcomes. The following section will summarize the main variables that we will use:

- **Institutions**: Rules affecting who can run for office and who can vote, including those affecting the costs of registering to vote (such as poll taxes and literacy tests), those regulating campaign contributions in state elections, and those governing the conduct of primary elections. Rules governing whether the state allows some form of direct democracy such as citizens' initiatives. Restrictions on the governor's and legislators' freedoms, including term limits on governors; tax and expenditure limitations; super-majority requirements for tax increases;

the governor's possession of a line item veto; and rules for appointing regulators and judges.

- **Legislative outcomes:** Characteristics of state office holders, including the fractions of Democrats in the state house and senate; party affiliation of the governor; the fraction of women representatives in the upper and lower houses; the ideology of the state government. Voter turnout. Degree of party competition.

- **Policy outcomes:** Taxes, in total and disaggregated into income, sales and corporate. Expenditures, in total and disaggregated to focus on redistributive expenditures (such as family assistance and Medicare).

While it will not always make sense to look at the impact of all institutions on all outcomes, we will try to be as consistent as possible in our choice of time periods, variables and specifications.

Schematically, there are four relationships that we will look at:

- Legislative outcomes as a function of institutions

- Policy outcomes as a function of legislative outcomes

- Policy outcomes as a function of institutions

- Determinants of institutional change

We will organize our discussion of the literature around this. We will also present a number of our own empirical results, both to illustrate a number of findings from the literature and to illustrate some of the broader points that we wish to make.

3.4 A Brief Digression on Empirical Method

One of the contributions of this paper is to estimate relationships, many of which are motivated by contributions in the literature. To do so, we have assembled a cross-state panel data set for the 48 continental US states from 1950-99. While not all variables are available for the entire study period, many are. Of the studies that we review, many use shorter time periods – often for the obvious reason that they were written some time previously. However, a variety of empirical methods are also at large in the literature.

Where possible, we identify the relationships that we seek from a baseline model that includes state and year fixed effects.[10] We believe that this provides credible estimates for many of the relationships in question. The fixed effects, in particular, are crucial to dealing with long-lasting unobservable differences between states that may both determine institutional rules and fiscal outcomes. It will not surprise the reader that many of the (unreported) specifications that do not include state fixed effects yield dramatically different results. When fixed effect estimation is not possible – when, for example, the policy in question varies by state, but not over time – we estimate robust standard errors, allowing for an unspecified form of correlation between observations from the same state over time. (Although, below, we discuss cases where robust standard errors may not be appropriate.) We believe it important that a common method of estimation be used across the specifications that we report, to allow the results to be read as all being cut from the same piece of cloth. It is also a potential source of discrepancy with results in the literature.

4 The Geography of American State Democracy

We first look at institutions that affect the policy process. Our study uses data from the 48 continental states for the post-war period. The background against which institutional features will be highlighted is the growing importance of state government over this period, especially in taxing and spending. In 1950, state governments took on average 3% of state income in taxes, a percentage that roughly doubled in fifty years, rising to 5.7% by 1999. US states had tax revenues of \$161 per capita on average in 1950, which climbed to \$833 per capita in 1999 (all measured in 1982 dollars).[11] The same story holds true on the expenditure side. State spending rose from an average of 6.9% of state income in 1950 to 12.4% in 1999. This constitutes an increase from \$370 to \$1764 in real terms. There is no single factor that has led to this dramatic change, just as no convincing mono-causal force behind Wag-

[10]Results presented below are largely robust to estimation using state and year effects and robust standard errors that allow for an unspecified form of correlation between observations from the same state over time.

[11]Unless otherwise stated, dollar values in the empirical work that follows will be given in 1982 dollars.

ner's law has been shown in the vast literature on this.[12] The change in magnitude of state government can be seen in the bottom panel of Table 4.1, where average state tax revenues and state spending per capita are presented for the decennial years from 1950 to 1990. In the empirical work that follows, we analyze annual data from 1950 to 1999 where possible. Some of our data series begin later. Detailed data from the Center for the American Woman in Politics, for example, publishes data on women legislators only from 1975 to the present. Table 4.1 presents data from the decennial census years, unless otherwise noted, to provide some sense of the pattern in the data over time.

Aggregate tax revenues and expenditures provide only a crude look at the changes observed. Within taxes, the most striking finding is the growth in income and sales taxes. In 1950, 17 states raised no income taxes. By 1999, all but 6 of the continental states had an income tax. The picture is similar for the corporate tax – 18 states were without one in 1950, true of only 3 or 4 states in1999 (depending on how one characterizes the "franchise tax" on earned surplus in Texas). Substantial changes also occurred over this period in the distribution of government spending. Family assistance per capita tripled between 1960 and 1980 (bottom panel, Table 4.1). Since that time, it has fallen in real terms, particularly in the second half of the 1990s. In contrast, state spending on Medicare has increased monotonically since its introduction in the mid-1960s.

Over this period, there has been a reasonable amount of institutional change. All fifty US states have broadly similar constitutions with a bicameral system (Nebraska is an exception here) and an elected governor. However, there is significant variation in electoral processes between states, with differences in the way states organize their legislative districts, and in the way registered voters go about voting for candidates. States also vary in their campaign finance laws, and voter registration and party primary rules. Some of these differences are highlighted in the first panel of Table 4.1. In 1950, 7 Southern states (roughly 15 percent of states in our sample) had poll taxes, which restricted voting among the poor, and roughly 14 percent of state residents were affected by a literacy test or device that severely limited the ability of less well-educated people, or those whose command of English was poor, to vote. These voting restrictions were eliminated in the 1960s, but voter registration continued to be restricted by regulations on place of registration, the timing of registration relative to the next election, and the

[12] See Holsey and Borcherding (1997) for a review of the issues with a U.S. focus.

continuation of a place on the voting rolls for those who missed elections. With the passage of the National Voter Registration Act of 1993 (the "motor voter act"), voter registration through vehicle registration is currently becoming a reality in all but a handful of states. States that were flexible in allowing registration on polling day or that required no registration (MN, ND, WI,WY, NH, ID) were exempt from the NVRA. The fraction of states in compliance with NVRA, and those with flexible registration, has increased over time, as can be seen in the first panel of Table 4.1.

There has been much variation over time and between states in the types of primaries parties have run, with the fraction of states running open primaries doubling between 1960 and 1990. Another important institutional restriction is in the ability of governors and state legislators to stand for reelection. In the 1990s alone, 10 states passed term limits on the number of terms their governors can hold successively and, by the end of the 1990s, three-quarters of all the continental US states had some sort of gubernatorial term limit.

Once elected, officials face different constraints on what they can do. For example, there are differences between states in tax-setting power, and on whether the Governor possesses a line-item veto. There are also important differences between states in the *scope* of policy responsibility. For example, some states directly elect certain office holders while others allow the Governor or legislature to choose them. The responsibility of different elected officials may also vary.

These political institutions may affect outcomes directly – for example, super majority requirements may have a direct effect on tax rates. In other cases, these institutions may affect policy choices through their effect on the character of the legislature. The fractions of seats in the states' lower and upper houses that are held by Democrats, or the number of women elected to the legislature, may depend upon electoral rules. The middle panel of Table 4.1 shows that the proportion of seats held by Democrats varied substantially from decade to decade – with the fraction of seats in the lower house as high as 70 percent in 1960 and as low as 55 percent in 1970 – and that women have gained seats in both houses of the state legislatures through time. Electoral rules may also affect voter turnout, and the degree of competition between parties. Ironically, voter turnout has fallen through time (here we present voter turnout in presidential election years), while party competition in the legislature has risen through time.

An important feature of parties is the way in which they generate competition for political office. There is a view, perhaps most famously articulated by Key (1949) that parties are likely to be most effective as a representative mechanism when they are in a truly competitive environment. There is no unanimously agreed method of measuring this. Authors have variously used differences in seat or vote shares at the last election as a means of quantifying the extent of competition between the parties. The important link, in Key's accounts, is between the degree of competition and the probability that certain groups turn out to vote. The general consequence is that parties will redistribute more to the poor in more competitive states. He writes: "In the two-party states the anxiety over the next election pushes political leaders into serving the interests of the have-less elements of society," (Key (1949), page 307.) An important observation is that a large number of the Southern states did not have a change in party control between 1950 and 1980.

The extent of party competition has been suggested as an important factor in explaining policy outcomes. Table 4.1 uses a very simple measure of party competition equal to –1 times the absolute difference from 0.5 in the fraction of seats held in the lower house by Democrats times the absolute difference from 0.5 in the fraction of seats held by Democrats in the upper house. In this way, larger (less negative) numbers are associated with more competition – that is, with a closer balance in seats between the two parties.

While the U.S. is broadly a two party system, it is well understood that there are differences in the political complexion of the parties in different states. Thus, the political scientists Erickson, Wright and McIver (1989) observe that "the Democratic party of Mississippi is far more conservative than the Democratic party of New York and perhaps the New York Republican party as well." (page 731). There are, therefore, many attempts to measure the ideology of the citizens and parties in different states. Berry et al construct measures of *state citizen ideology* from widely used ideology ratings of the state's Congressional delegation.[13] These are the Americans for Democratic Action (ADA) rating, and the AFL/CIO's Committee on Political Education (COPE) rating. Berry et al assign an ideology rating to the citizens of each Congressional district using a weighted average of the Congressional member's score and his or her election opponent's score, weighting the scores according to the number of votes each received. They then gener-

[13] There is a large political science literature on measuring ideology of citizens and elected officials. Berry, Ringquist, Fording and Hanson (1998) for provide a useful review of the various methods used to construct these as well as some new data on this.

ate a state-wide measure by taking the simple average over all Congressional districts. Berry et al also construct a measure of *government ideology,* by assigning to the governor and major party delegations in the legislature the ratings of the members of Congress from their party. Table 4.1 suggests that, over time, governors have become slightly more liberal, as measured by the governors' COPE scores.

The literature has long recognized that differences exist in institutions, ideology, and legislative and policy outcomes between states in the Southern region of the U.S. and elsewhere. We take a second look at our institutional and outcome variables in Table 4.2, where for 1960 and 1990 we present means for the US South and for all other parts of the U.S. In the early part of our sample, it is clear that the South restricted access to the vote: it was the Southern states that relied on poll taxes and literacy tests in the 1950s and early 1960s. In 1960, Southern states were also significantly less likely to hold open primaries, or to allow citizens' initiatives. Southern states in all parts of our sample have significantly higher fractions of Democrats in both the lower and upper houses of their legislatures. In the early part of the period, their governors were significantly more likely to be Democrats – but were significantly more conservative, when measured using the COPE score. Voter turnout is significantly lower in the South (in part as a result of the restrictions on registration and voting), and party competition in the legislature is lower in the Southern states, perhaps as a result. In the estimation results we present below, we do not generally rely on the geographic differences between states – because our estimation strategy generally includes state fixed effects. However, to the extent that Southern states have changed their institutions and legislative composition over time, these changes help to identify the results we present below.

There is a large literature that has studied aspects of political institutions on policy outcomes. Many of these studies are discussed below, where we also present results from new empirical work that synthesizes findings on the impacts of a number of institutional rules on the characteristics of those elected and the policy choices they make. We begin by outlining the impact of institutional differences on legislative outcomes in the last half century.

5 Institutions and political outcomes

In this section, we consider how institutions affect political outcomes. The main institutions of interest in this section are those that affect voter registration, candidate selection (primary rules), legislative redistricting and campaign finance. We examine their association with measurable political outcomes – such as party control, political competition and ideology.

5.1 Voting

If voting is an effective mechanism for determining policy outcomes, then it requires that individuals show up at the polls to express their views. This has two parts. First, whether individuals have registered to vote and second, whether registered voters turnout at the polls. As we noted in section 4, there are important institutional variations in the way in which states organize voter registration. There are good reasons to expect that these will affect registration and hence turnout.

While some political scientists view turnout as a good in itself – signaling the health and legitimacy of a democracy – it is less obvious whether turnout affects policy outcomes. This depends on whether there are important sources of bias in turnout, in the sense that low turnout favors one party over another. It is widely known that turnout varies across different groups in society,[14] with richer and better educated citizens more likely to vote. There is a long-standing view in political science that biases in turnout bias policy choices – the main expectation being that larger turnout among low income groups will be correlated both with better election performance by the Democrats and with more redistributive policies, as the income of the decisive voter falls. This mechanism is key to some recent papers, such as Benabou (2000). An example of a study along these lines is Hill, Leighley and Hinton-Andersson (1995), which looks at this in panel data from 1978-1990 and finds that turnout among lower-class voters is positively associated with increased welfare spending. This is robust to year effects, although they do not include state effects. However, using turnout as a regressor is

[14] The classic study in political science is Wolfinger and Rosenstone [1980]. They, and a host of subsequent literature, estimate the effects of voter registration laws using micro data with turnout as the outcome variable. They then interact these laws with socio-economic information about the individual finding that registration laws have most bite for low education groups.

problematic since these are likely to be co-determined with factors that also drive policy, as we discuss further below. It is possible that some information about the economy is revealed that leads both to higher welfare spending being desired and to more people wishing to vote.[15]

A promising approach to estimating the impact of turnout on policy choice is to find institutional change that affects registration and turnout independently of the demand for policy. In recent U.S. history there are two main reforms that have affected the probability that citizens will register to vote. First, in the early part of the century, many states were forced by the Federal government to extend the franchise to women. Second, changes to voting rights laws in the 1960s and 1970s led to greater voter registration by minority voters. More recently, states have encouraged voter registration under so-called "motor-voter" laws that tie vehicle license registration to voter registration.

Lott and Kenny (1999) consider the effect of extending the franchise to women, using data from 1870 to 1940. They exploit the fact that some states gave women the vote before it was federally mandated in 1920, which generates state-to-state variation in the timing of women's suffrage. They find that women's participation increased the size of government. While the finding is interesting, they offer no theoretical explanation, but suggest that it may be due to the different policy priorities of men and women vis a vis child rearing issues.

Husted and Kenny (1997) consider the effect of the Federal voting acts of the 1960s and 1970s, which struck down literacy requirements and poll taxes. As we discuss in greater detail below, they find significant correlations with welfare spending. Earlier work by Filer, Kenny and Morton (1991) found these acts to have had a significant impact on turnout.

The ease with which voters may register has also been studied as a source of differences in voter turnout (see Highton (1997) for a review of the political science literature). One of the key institutional variations that has been studied is whether voters can register on election day. Suggestively, Highton points out that in turnout is 10% higher in North Dakota (where no voter registration is required) and other states with election day registration (using data from 1980 and 1992). The biggest difference is at low education levels.

Arguably, studying these institutional changes provides a more satisfactory basis for assessing whether *who* votes affects policy outcomes than does

[15] See Feddersen and Pesendorfer (2000) for a particular example of this.

simply controlling for turnout in a reduced form regression. Moreover, there is both cross-sectional and time-series variation to exploit, as some states changed their laws without prompting from the Federal government.

Table 5.1 examines the impact of a variety of institutions on voter turnout. Specifically, we examine whether literacy tests and poll taxes, voter registration, citizens' initiatives, and restrictions on corporate campaign financing affect voter turnout, while controlling for year effects and a number of time-varying state level variables that may affect turnout (state income per capita and income squared, state population and population squared, the proportion of the population aged 65 and above, and the proportion aged 5 to 17). Turnout is defined here as the number of votes cast for the highest office holding an election in that year divided by the total voting age population in the state. The impact of literacy tests and poll taxes on turnout is immediately apparent in column 1 of Table 5.1. Consistent with the results of Filer, Kenny and Morton (1991,1993), voter turnout was roughly 15 percentage points lower on average in states when poll taxes were in place. Because we control for state fixed effects, the impact of poll taxes is identified from the difference in turnout within states that ever had poll taxes, before and after they were lifted.

Turnout is positively correlated with having a constitution that permits citizens' initiatives: turnout is on average 3 percentage points higher in states where initiatives are allowed by law. This is consistent with the often articulated view among political scientists that direct democracy promotes political participation (see, for example, Butler and Ranney (1978)). However, there is very little time-series variation in our initiative variable, and this regression does not include fixed effects. Hence, the result could be due to some omitted variable such as "political culture" that drives initiatives and voter turnout.

In addition, states with the least cumbersome voter registration–either allowing registration on polling day, or not requiring registration at all – have voter turnout that is 2 to 3 percentage points higher on average. Again, these effects are identified using the differences in turnout within states, before and after they moved to more flexible registration. Voter turnout is also significantly correlated with restrictions on corporate campaign contributions. Controlling for state fixed effects, in those states that adopted restrictions on corporate campaign contributions, turnout was 2 percentage points higher. These results are robust to estimating the impact of poll taxes, literacy tests, voter registration and corporate campaign finance requirements simultaneously (column 5), where these institutional rules are jointly highly significant (F-test = 66.09, p-value=.0000).

That institutional rules may also affect the political composition of the legislature is clear from Table 5.2, where we regress the fraction of the seats in the states' upper houses held by Democrats, whether the governor is a Democrat, and measures of party competition, on indicators that the state has a poll tax, a literacy test, voter registration through vehicle registration, day-of-polling registration, and an indicator for restrictions on campaign contributions. We control for state and year effects and the same time-varying state-level controls introduced above. For the dependent variables in Table 5.2, all controls have been lagged one period, to represent conditions in the state in the year in which these office holders were elected. We find that literacy tests are significantly correlated with the fraction of seats held by Democrats, in those Southern states that had these restrictions on voting. In addition, day-of-polling registration is positively and significantly correlated with the fraction of seats held by Democrats. Restrictions on corporate campaign contributions are also associated with a higher fraction of the states' upper houses held by Democrats: on average, if a state passes restrictions on corporate contributions, the share of the state upper house that is held by Democrats increases by 2.1 percentage points. Jointly, these institutional rules are significant correlates of house composition (F-test=4.47, p-value=.0005). (Results are quite similar when the proportion Democrat in the states' lower houses is regressed on these same controls.)

All of the institutional rules we have identified here have significant effects on party competition in the legislature (columns 4 and 5). Rules that make registering to vote or voting itself costly significantly reduce the balance between Democrats and Republicans in the legislature. Jointly, these institutional rules are significant correlates of party competition. While these institutional rules have a large and significant effect on the state legislatures, they appear to play little role in the election of a governor. None of the institutional controls is significant by itself, nor are they jointly significant.

As we have seen above, turnout is significantly correlated with these election rules but, interestingly, the impact of institutional rules on party composition of the legislature is not working through voter turnout. When we include a control for turnout (column 2), it has no significant effect on the fraction of the upper house held by Democrats, and has no effect on the coefficients attracted by the electoral rules. Where turnout appears to have a significant effect on the legislature is in the degree of party competition (column 5). In those years in which turnout is large, the legislature becomes more evenly divided between Republicans and Democrats.

For each dependent variable, we can test whether institutional rules are affecting legislative composition solely through their effect on turnout. In the third column, we present results for instrumental variables estimation, where voter turnout is instrumented on the institutional rules that appear in columns 1 and 2. The F-test in column 3 compares the fit of the regression using the predicted value in column 3 to that in column 1, where the institutional rules are allowed to enter in an unrestricted fashion. Results in column 6 report an analogous comparison for party competition in the legislature. We reject that these institutional rules are affecting the fraction Democrat and party competition solely through their effect on voter turnout.

Overall, results in this section provide evidence that the cost of registering to vote has real effects on political outcomes – turnout is higher, for example, when registration is simplified, and this is associated with greater political control by Democrats in the legislature. The latter is suggestive of the idea that, on the margin, it is low-income voters who are encouraged to vote when the cost of voting falls. This is in line with a long-standing tradition of work in political science on voter turnout and its consequences. The increase in Democratic representation raises the possibility of real policy consequences when we examine the effect of party representation on policy in section 6.1 below.

5.2 Primaries

Institutional rules may also affect who is selected to run for office. The main vehicle for candidate selection is the primary process, and there are important differences in primary rules across states. Models of electoral competition that include primaries typically divide the action into three stages. At stage one, voters affiliate themselves with a particular party. At stage two, parties nominate from the subset of candidates that choose to stand. At stage three, voters choose between party nominees.

States vary in the primary rules that they use to determine candidate choices. There are basically two institutional variants. In *open* primaries, participants do not need to declare their party allegiance in advance of the primary.[16] In *closed* primaries, participation is limited to voters who have

[16] Within this general cetagory are blanket primaries where voters may participate in all

declared a party affiliation a sufficient time prior to the primary.[17]

Gerber and Morton (1998a) argue that more open primary arrangements will likely lead to more moderate candidates being chosen. Closed primaries restrict influence to the party elites, who are more likely to have strong ideological preferences. By facilitating cross-over voting (i.e., for a party with whom one does not identify), open primaries will tend to lead to more moderate candidates being selected (although Gerber and Morton observe that things are less clear cut if cross-over voting has a strategic element). They cite a significant number of political science studies suggesting that cross-over voting is more common in more open primaries.[18] To look at this empirically, they examine the ideological stances of winners in Congressional races. They use past voting records in Presidential races as a control for the ideology of the districts, and argue that the evidence is consistent with their expectations, that more open primaries spawn less extreme candidates.

We examine the relationship between primary rules and the ideological differences between citizens and elected officials in Table 5.3. The first two columns provide evidence on the relationship between open primaries and voter turnout for the highest office holding an election, in the even years between 1950 and 1998. Open primaries are positively and significantly related to voter turnout, increasing turnout by 1 to 2 percentage points, with or without controls for other institutions that we have found to be related significantly to turnout.

Open primaries are not significantly related to the party-composition of the legislature (columns 3 and 4). However, open primaries are negatively and significantly correlated with the fraction of women elected to state lower houses (columns 5 and 6). Again, because state fixed effects are included in each specification, this effect is identified using changes in the election of women legislators within states that changed their primary rules during the period 1977 to 1999, which occurred for half of all US states. This finding could signify that party elites are keener on women's representation than voters at large. However, clearly it requires a more thorough-going investigation.

To test whether open primaries reduce the ideology gap between citizens

parties' primaries. In non-partisan primaries, voters choose among candidates without declaring any party allegiance.

[17] Primaries are *semi-closed* if new registrants can register for a party on the day of the primary or if independents are allowed to participate.

[18] They also have variation in whether states have runoffs.

and their elected officials, we first generate measures of such a gap, using data from Berry et al (1998). Measuring the ideology gap between elected officials and state citizens as the absolute difference between the ADA-based scores or the COPE-based scores for these two groups, we find that open primaries are associated with reductions in the absolute differences between the ideologies of citizens and elected officials. In Table 5.3 primary rules are shown to be negatively correlated with the gap between citizen and government ideology, here measured using the COPE score. In line with the findings of Gerber and Morton (1998a), this suggests that instituting open primaries may be having a systematic effect on political representation.

5.3 Campaign Finance

The issue of campaign finance reform has been a major political issue of late, given a general popular concern about the level of political spending. It is estimated, for example, that more than $3 billion were spent on political campaigns in the year 2000 elections.[19] Current campaign financing rules raise many difficult issues, including the possibility that public officials may become beholden to special interests, and that the sums of money necessary to launch a campaign may discourage able challengers, to the benefit of incumbents. Levitt (1995) takes a more sanguine view, at least for Congressional elections, suggesting that the most careful recent studies have not found spending by challengers to be more effective than that of incumbents and that, for this reason, placing limits on spending will do little to alter the incumbency advantage. In addition, Levitt reviews work that questions whether Political Action Committees (PACs) have much of an effect on elected officials' behavior once in office. The history of campaign finance reform, details of the current laws and innovations in the states is provided in Corrado et al. (1997).

There has been is a great deal of variation across states and over time in the administration and enforcement of campaign finance laws. Huckshorn (1985) discusses variation across states in four different types of finance regulations: spending limits by candidate; contribution limits by different types of supporters (corporations, unions, individuals); disclosure rules; and monitoring and enforcement mechanisms to stop illegal or unethical expenditures. Differences across states creates a source of institutional variation that may

[19]Public Campaign, www.publicampaign.org.

have political ramifications. As well as creating an interesting source of institutional variation for political testing, the theoretical consequences of these institutional variations are of interest. They may, for example, affect the kinds of candidates who are selected as in Coate (2001). By changing the information provision environment, they could also affect voter attachment to particular parties or candidates.[20]

Jones (1981) examines the impact of the cross-state variation in schemes for public funding of political parties during the 1970s, mainly through tax check-off schemes. She finds that around 20% of the population participate in the tax check off schemes introduced. The data suggest that in terms of financing, the Democrats are disproportionate beneficiaries. However, she does not conduct formal empirical testing of the effect of state funding on outcomes.

While there is a large body of work that tries to explore the impact of campaign spending in federal elections, explorations of the empirical effects of campaign finance laws at the state level is extremely sparse. This is somewhat surprising given the rich institutional variation observed at the state level. To illustrate the possibilities, we explore a particular kind of campaign restriction, namely a limit on corporate campaign financing, The results are in the bottom row of Table 5.3. We find that when a state restricts corporate contributions, turnout is higher, and the fraction of the lower house held by Democrats and by women is significantly higher. It is clear that there is scope for expanding and refining this kind of work in this area in future. Of particular interest will be to explore the determinants of law changes.

5.4 Redistricting

Another important institutional variation across states that can have a significant impact on political (and hence policy) outcomes is the relationship between voting and political control. It is common to characterize an idealized political system as one in which the proportion of seats held is proportional to vote share. Although the normative foundations of this are unclear, it is a useful benchmark. In practice, majoritarian systems like the U.S. do not yield proportionality and, for many years, the so-called cube law was purported to characterize the relationship observed.[21] The relationship between

[20]For an overview of the issues on campaign finance reform, see Morton and Cameron (1992).

[21]This is represented by

seats between votes is considered to be extremely important in the process of designing districts.

Gelman and King (1994) use data on state legislative districts from 1968 to 1988 to examine whether redistricting affects *electoral responsiveness* – the degree to which the party composition of the legislature responds to voter preferences – and *partisan bias* – the fairness with which state-cumulative votes for a party translate into seats in the state legislature.[22] They find that redistricting increases electoral responsiveness, largely by inducing uncertainty in the electoral process. However, when one party controls the redistricting map, Gelman and King find this induces partisan bias. On average, the controlling party gains 6 percent of the seats that would have gone to the other party, had it controlled the redistricting. They conclude that "even though redistricting makes the electoral system substantially fairer overall than if there were no redistricting, the difference between Democratic and Republican control over the drawing of district maps is still one that politicians are rightfully concerned about" (page 553).

We test the extent to which a unified party influences party competition through redistricting, by analyzing the legislative composition of the 48 continental U.S. states between 1952 and 1995. After data on the U.S. population have been released by the Census Bureau following the decennial census, states go to work to reapportion legislative districts. In all but a handful of states (AR, HI, ID, NJ and WA), redistricting begins with the legislature and the governor. If power is divided, agreement is often difficult to reach, and redistricting can end up in the courts. We create an indicator that a party controls redistricting, using information on whether it has unified control of both houses of the legislature and the governor's office in the

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$$\frac{s}{1-s} = \left(\frac{v}{1-v}\right)^3$$

where s is share of seats and v is share of votes.

[22]They propose a generalization of the cube law referred to in the previous footnote where

$$\frac{s}{1-s} = \beta\left(\frac{v}{1-v}\right)^\rho$$

where β measures bias and ρ measures responsiveness.

decennial census year.[23] We then test whether the change in the number of seats held by the Democratic party in the legislature following redistricting (decennial year + 2) is significantly correlated with whether the Democrats or Republicans controlled the state's redistricting following the census.

In Table 5.4, we regress the change in the fraction of seats held by the Democrats between all years (t)and year ($t-2$) on indicators that the Democratic party held unified control, and this Democratic control indicator interacted with an indicator that this election year is immediately after redistricting. We include analogous variables for Republican control. (Thus leaving mixed control as the omitted category.) We find evidence of the same partisan bias discussed by Gelman and King (1994). Controlling for state and year effects and time-varying state-level variables, we find that when the Democrats controlled redistricting, they protect Democratic seats in the lower house of the legislature following redistricting, and that the opposite held when Republicans controlled redistricting. We need to be careful in our wording here: on average, if the Democrats held unified control in a state in year $t-2$, then in year t the Democrats are likely to lose seats, with the opposite holding true for Democrats when Republicans hold unified control. This may be a regression to the mean phenomenon – the political pendulum swings and then swings back – but is not special to redistricting years. What is special to the post-redistricting years is that the Democrats, when in control, can stop the pendulum from swinging back as far as it otherwise would. To take an example from Table 5.4: column 3 presents results on the change in the number of seats held by the Democrats in the state lower house. If Democrats had unified control in year $t-2$, we would expect them to lose 4 to 5 seats in year t on average. However, if year t is just after a redistricting overseen by the Democrats, that loss is cut by 3 to 4 seats.

These results suggest that there is potential for using redistricting as way of trying to deal with the potential endogeneity of political control – since we have an exogenous shock to political control which (due to the effects observed in Table 5.4) varies both across space and time. Space precludes the possibility of developing this here. However, it is clearly ripe for further investigation.

[23]We recognize that this will not yield a perfect measure of where redistricting is partisan, due to court challenges and legislative impasses among other reasons (see Gelman and King (1994) on this point). However, it is likely to be a useful instrument in predicting when partisan redistricting will occur.

5.5 Overview

There is little doubt that variations in institutions affecting voting, primaries, redistricting and campaign finance have significant consequences for political outcomes. Voter turnout, party representation, the degree of competition in the legislature, and the distance between the ideology of voters and their representatives all respond to political institutions. However, for these to have real effects on policy, it must be the case that these political outcomes are drivers of policy. In the next section, we review what is known about the extent to which these measures of legislative outcomes influence policy choice.

As an intermediate step, we discuss whether the institutions that we have investigated in this section, appear to have any "reduced form effect" on policy outcomes. There is some existing evidence that they do. For example, Husted and Kenny (1997) consider whether institutions had an impact on welfare spending, using data from a panel of states over the period 1950-1988. Their results are consistent with the idea that the income of the decisive voter was lowered by the extension of the franchise, leading to higher welfare spending. This effect is identified by the time-series variation in Southern states, where most of the restrictions obtained prior to changes in Federal law.

Racial bias in turnout has been investigated in Radcliff and Saiz (1995), who examine the impact of voter turnout over the period 1979 to 1992 on welfare spending and on policy liberalism measures (a la Wright, Erikson, McIver (1987)). They focus on welfare spending in the 26 U.S. states that have more than 5% black populations in 1990. Using the Current Population Survey to construct measures of bias in turnout, which they measure as the ratio of black to white turnout, Radcliff and Saiz find that welfare spending is lower in states with lower black turnout using panel data where fixed effects are included. They also find a negative association between cross-sectional measures of policy liberalism and their turnout measure. On the right hand side they include a measure of the difference between black and white turnout and find a negative effect, i.e. less policy liberalism and less welfare spending. These finds are interesting. However, they do not deal with the possibility that turnout is endogenous.

We examine the extent to which institutional rules are associated with state taxes, overall spending, and transfers in Table 5.5, where we regress taxes and spending (all in real $1982 per capita terms) on institutional rules, together with the time-varying state level variables discussed above, controlling for state fixed effects and year effects. We find that open primaries are associated with overall lower total taxes and state spending. Open primaries are associated with a reduction in total taxes and spending of roughly $20 per capita. Open primaries also appear to have distributional effects on state spending: total transfers are significantly higher when office holders are elected under open primaries and, in particular, family assistance per capita and Medicare spending is both significantly higher. Less costly voter registration – through motor-voter rules, or through day-of-polling registration – is generally associated with higher taxes, higher spending and larger family assistance payments. Restrictions on corporate contributions are associated with lower overall taxes, but higher transfers in the form of family assistance. For all of these fiscal outcomes, our institutional rules are jointly significant.

These policy correlations raise more questions than providing answers. However, since we know that the institutions in question are correlated with turnout and representation, it provides some reason to think that these aspects of the policy process are important to underlying policy outcomes. It is to identifying these effects in more detail that we now turn.

6 Political Representation and Policy Outcomes

This section investigates the link between political representation as measured mainly by party identity and political competition and measurable policy outcomes. For the sake of consistency, we maintain a small core set of outcomes for the empirical work presented here – total taxes per capita, total state spending per capita, family assistance spending per capita and medicare spending per capita.

6.1 Party Representation

At the heart of the democratic ideal is the notion that government policy should be guided by citizens' preferences. In representative democracies, public opinion affects policy only indirectly by influencing the identity of elected decision makers. These often represent different parties and the different ideologies of the parties are often argued to be the core choices on offer. A key issue is how opinion finds its way into the policy process, and a key role is given to elections in shaping policies that are representative of widely held opinion. This, of course, requires that people vote, the outcome of which is limited by the choices people have available to them – involving generally two candidates (one from each party) in the case of state legislatures. (As we discuss below, some states permit more direct opportunities to influence policy through citizens' initiatives.)

How parties represent preferences is a central area of research among political scientists. Moreover, many of the key approaches to modeling representative democracy hinge on assumptions about parties' strategies and motivations. While parties are frequently characterized as ideologically based organizations with distinct agendas, there remains an important empirical question about whether party control really does deliver measurable policy differences and whether particular policies appear to be more responsive to party identity. In light of its centrality, it is not surprising that a large literature has developed that attempts to gauge how the process of representation works (empirically) and whether party control makes a different in determining policy outcomes.[24] Finding that parties do not matter would deal a blow to the stereotypical characterization of party differences that most commentators take for granted. There is a huge literature in political science using cross-country and within-country evidence on whether parties matter for policy. The literature that has looked at the federal level in the United States is far from conclusive. By exploiting both cross-sectional and time-series variation, the U.S. states are a much more promising testing ground.

In principle, it is straightforward to investigate this issue. Returning to equation (2) above, party control and party "competitiveness" can be thought of as part of the variable ℓ_{st}. Even then, however, there are a number of important issues to be resolved.

At any point in time, the controlling parties may vary – both between the two houses of the legislature and vis a vis the governors' office – raising the interesting question of how policy making differs between unified and divided control of the legislature and the executive. Alt and Lowry (1994) considers the bargaining game that will ensue and its consequences. For the budgetary process, the governor typically has the power to propose, with

[24] It should be borne in mind that this leads to exclusion of Nebraska, since it holds non-partisan elections and, in early years, Minnesota, since its parties were not comparable to Democratic and Republican parties in other states.

the legislature having the power to amend or even throw out the budget. Although their main focus is on the federal level, the whole question of how divided government affects policy outcomes is also tackled by Alesina and Rosenthal (1995). From the point of view of voters, dividing control may provide a means of getting an outcome that is intermediate between the policy outcomes offered by either party under unified control. Once the possibility of divided government is admitted, one needs to be careful about how to measure party control in a given state, which authors have approached in a number of ways.

Beyond the question of measurement lie the issues of endogeneity and omitted variable bias. The circumstances that brought a party to power may also lead to changes in policy for independent reasons. For example, a recession in a state could lead to more Democrats being elected and a larger demand for transfers. Endogeneity also arises with respect to divided government – if voters anticipate that one party or gubernatorial candidate has extreme views, they may split their ticket deliberately to provide the appropriate check. This may make it problematic to include divided government as a control in policy regressions.

Omitted ideological bias is also an issue. This underlies the observation from section 4, that the ideologies of parties vary across regions of the United States. Thus, it may be problematic to take party labels at face value in determining their effects on outcomes. This suggests the need to include separate controls for party ideologies and/or public opinion in addition to party labels.

Much of the early literature finds little evidence that party control matters – see Winters (1976) for a survey. Dye (1984) runs a series of state specific time-series regressions for the period 1950-80 that examine the effect of a change in party control on state welfare spending. Allowing for divided control of the state legislatures and controlling for state income, he finds an effect of party control in fewer than half the states. Winters (1976) looks at the impact of party control on benefits and taxes borne by low income groups for 1961 and 1965 using data from the Tax Foundation. He uses the party identification of the governor, an inter-party competition index (one minus the percent vote difference between the candidates), and the percentage of Democrats in the lower house as political variables. He also controls for a variety of economic variables such as state income, population and percent of employment in manufacturing, Since the dependent variable is observed at two points, he uses a pooled time-series cross-sectional analysis. However, he cannot find any role for the political variables. Plotnick and Winters (1985) look at the empirical determination of AFDC benefits using a five equation structural model that treats party control as endogenous (identified by supposing that voter preferences affect policy only through their effects on party outcomes). They find little role for party control.

Garand (1988) considers a number of explanations for the growth of state government in the post war period, among them the idea that party control (by Democrats) is associated with faster growth of government. He runs separate time series regressions for each state during the period 1945-84 where the dependent variable is the level of state government spending as a proportion of total state output. He uses the party of the governor, the party controlling the legislature and these two interacted as control variables, finding little evidence for the importance of parties. He does not, however, exploit the panel data aspect of the data, preferring to treat each state separately.

Gilligan and Matsusaka (1995) examine five cross-sections at five year intervals between 1960 and 1990 to examine whether government expenditure is affected by party control. They include both state and year dummy variables and control for an array of economic variables such as population and state income. Their political variables are the number of seats in the upper and lower houses and the control of both houses of the legislature and the governors' chair. They also measure the degree of competitiveness in legislative elections. This provides a canonical panel data approach to the problem. Their dependent variable is state and local general expenditures. The results provide very little evidence of party effects with almost all party coefficients being insignificant.

Although not the main point of his analysis, Knight (2000) also provides evidence on this, finding that control of both houses by the Democrats leads to significantly higher tax rates relative to state GDP while control of both houses by the Republicans has the opposite effect. Alt and Lowry (1984) consider how divided government at the state level is related to taxes and expenditures, and the attendant consequences for passing deficit finance. They argue that states with divided government will find it more difficult to respond to a shock in taxes or spending, resulting in deficit finance being used more often. They use data between 1968 and 1987 to test this. They run separate regressions for groups of states, treating the southern group separately. They run a two equation system for taxes and spending identified by supposing that federal government contributions affect revenues but not expenditures and unemployment rates affect expenditures but not revenues. Overall, they find evidence in favor their notion that deficits are more likely under divided government. They also find using their method that Democrats implement higher public spending than Republicans.

Clarke (1998) considers the impact of divided government on the degree of conflict between the executive and legislature by looking at agency level data, specifically the difference between gubernatorial recommendations and actual appropriations from twenty states during the period 1985-94 – a total of 6027 observations. A larger percentage change to the governor's recommendation is taken as evidence of greater conflict. He then models this as a function of political variables and state fixed effects. He finds that a unified legislature and opposition governor yields greater conflict. He also finds that a greater ideological spread and party system liberalism leads to greater conflict.

Grogan (1994) investigates medicaid policy as a function of political and economic variables using a biannual panel of (49) states between 1979 and 1989 using a random effects model. Unlike many of the studies referred to above, she finds that states under Republican party control in the legislature (meaning both houses being controlled by that party) are less generous in their provision of Medicaid, measured in various ways.

We investigate the significance of party control and party competition on the determination of state taxes and spending in Table 6.1. Controlling for state fixed effects and year effects and time-varying state level controls, we find that the higher is the fraction of seats held by Democrats in the states' lower house, all else equal, the higher are state taxes and spending per capita. On average, and all else held equal, a ten percentage point increase in the fraction of seats held by Democrats in both the lower and upper houses is associated with an increase in overall state spending per capita of $10 in 1982 dollars. Roughly half of that increase is attributable to higher spending on family assistance, and roughly half to Medicare. Democratic control of both the lower and upper houses of the legislature is associated with significantly higher taxes (roughly $13 per capita) and a redistribution of state spending in favor of family assistance: overall spending per capita does not change significantly, but spending on family assistance per capita increases by almost $4 in 1982 dollars. This is in line with the idea that the exact structure of political control is important and that divided control of the legislature does provide a check on policy. We find little evidence of Democratic governors spending more or taxing more. However, we do find that greater party competition in the legislature is associated with significantly lower taxes, and significantly higher spending on Medicare. Greater party competition may entice the legislature to reach out to elderly voters.

These indicators of party control and legislative composition are jointly highly significant in our fiscal policy regressions. (F-tests are given at the bottom of each regression column.) It is not clear why we find such large and significant effects when many other studies in the literature do not. Our results are similar when we remove time-varying state level controls, and when we remove state fixed effects. Our panel is longer than many used in the literature, which may account for the difference.

6.1.1 Controlling for Ideology and Party Heterogeneity

Erikson, Wright and McIver (1989) argue that the failure of much of the literature to find a link between party control and policy outcomes is due to the failure to control systematically for the ideological differences in party stances across states (a problem somewhat ameliorated by the inclusion of state fixed effects in many regressions). They see the link as being from liberal public opinion to liberal party elites to liberal policy. Thus, in states with more liberal electorates, both parties will be more liberal and the effect of party control is muted if public opinion is omitted when studying this relationship. In general, they find that the relationship between opinion liberalism and Democratic control is quite weak. They operationalize the study of party elite liberalism by looking at the conservatism of congressional candidates, local party chairmen, national convention delegates and state legislators. Public opinion is measured using CBS/*New York Times* surveys from the period1976-82. Using a range of measures of policy liberalism in key areas, such as AFDC and Equal Rights Amendment ratification, they find a *negative* correlation between policy liberalism and Democratic strength in the legislature after controlling for liberalism in public opinion. While provocative, the empirical models that are used have only small numbers of observations and cannot use state fixed effects.[25]

Brown (1995) disaggregates party support among different sub-groups to reflect the different cleavages between the parties that dominate in different states. He uses polling data to show that there are distinct differences in partisan support among socio-demographic groups across states. Parties are then characterized according to their dominant party cleavage. He suggests a three-way classification of states – a southern group of states where

[25]See Barrilleaux (2000) for further discussion of these results.

the Democrats are predominately supported by black, low-income and rural groups, a New Deal group with Democrat support based on Catholic, low income, union and female groups, and a Post-New Deal Group with Democratic support based on black, Catholic, urban, union, low income, female and Jewish groups. He then regresses welfare spending between 1976 and 1985 on party control interacted with the type of cleavage identified. However, he does not include state or fixed effects. This exercise suggests that Democratic party control does matter. However, it matters less in the Post-New Deal and Southern party cleavage states.

Wright, Erikson and McIver (1987) are among those who have considered the direct links between public opinion and policy outcomes. They find significant correlation between state opinion and state policy when using their measure of ideology based on the CBS/*New York Times* surveys (above) to estimate the cross-sectional relationship between ideology and state policy choices for 47 U.S. states.

We examine the relationship between state policy and state opinion in Table 6.2, where we present regression results on the determinants of state taxes and spending, controlling for state and year effects and time-varying state-level controls. Our measure of public opinion is the Berry et al (1998) state citizens' COPE score, which has a range from 1.43 (South Carolina in 1965) to 96.99 (Massachusetts in 1988) and a median of 50.83 (Minnesota in 1980). This analysis is limited by the fact that the Berry et al measure is available only for the years from 1960 to 1993. Nonetheless, we continue to find that Democratic party variables are significantly correlated with state taxes and spending. In addition, we find that having a liberal state citizenry, as measured using the COPE score, is significantly correlated with state taxes, state spending overall and, in particular, state spending on family assistance programs.

Overall these results suggest that there is still information in the party identities even after controlling for ideology. However, it is clear that party identity and ideology measure somewhat different things in different states, depending on their political culture, history and policy priorities.

6.1.2 Intensity of Party Competition

There are many ways of measuring party competition (see Holbrook and van Dunk (1993) for a review). The most common is the Ranney index, which averages together the proportion of seats won by Democrats in the state

House and Senate elections, the Democratic percentage in the gubernatorial election and the percentage of time that the governorship and the state legislature were controlled by the Democratic party. Holbrook and van Dunk (1993) use district level data, using the percentage of the vote won by the winning candidate and the winning candidate's margin of victory.

Party competitiveness is potentially endogenous too. A party that is systematically behind in a state will try to converge towards the other party, in order to increase its chances of winning. Hence, we would expect the parties to be constantly changing their stances in particular states to respond to voter ideology. Moreover, a state's dominant party may use redistricting to provide the party with small margins of victory in the largest number of districts possible.

Holbrook and van Dunk (1993) argue that there will be greater turnout when outcomes are more competitive. Using their proposed alternative measure, they find statistically significant correlation between a number of political competition and policy outcomes – they find much weaker effects using the conventional Ranney index.

Rogers and Rogers (2000) consider whether party competition across the states is related to growth in government size. They acknowledge that there is no necessary theoretical link between government size and intensity of competition – it seems just as likely that there would be tax cuts as expenditure increases – and they do not put forward a theoretical framework to analyze this. Their measure of political competition is the percentage of the vote won by the current governor in the most recent election. Government size is measured in both revenue and expenditure terms. Using year effects and state effects and panel data from 1950-90, they find no positive link between government size and political competition. The coefficient is either not significantly different from zero or negative and significant – although the latter, where found, is sensitive to the sample period chosen. Unlike many of the studies discussed above, they do find that Democrats in the House are associated with larger government.[26]

Table 6.1 finds a significant effect of party competition on policy outcomes for total taxes per capita, which are significantly lower, and Medicare spending per capita, which is significantly higher when parties are more

[26] They also find that divided government also seems important as a moderating device with a Democratic governor and a Republican House leading to smaller government than a Republican Governor with a Republican House.

competitive. While interesting, the result requires more investigation. One possibility is that intense competition changes the policy priorities of the elected representatives and the policy stances that the party adopts. It is also possible that the nature of legislative coalition formation changes in more competitive environments.

6.2 Non-Party Identity of Representatives

Legislators are characterized by more than just their party label. Of particular issue has been the extent of involvement in the political process by women and minorities, not just as voters, but also as legislators. For this to have a fundamental effect on political representation, it must be the case that citizens find it necessary to delegate authority to citizens of particular kinds to further their policy ends.

In principle the effects of legislature identity on policy outcomes can be studied by including this information in ℓ_{st} for the effect of policy on outcomes.

There has been significant interest in electoral determinants of black representation. (See Sass and Pittman (2000) for a survey.) There have been significant changes in the law to promote black representation, such as the Voting Rights Act of 1965. Also important has been the increased use of legislative redistricting to create so-called "majority-minority" districts rather than having at large elections. Sass and Pittman (2000) looks at the link between election structure and black representation in the South using data between 1970 and 1996. Their data are for city council elections, where they explain the fraction of blacks elected as a function of the percentage black in the population and variables representing the electoral system. They estimate this in levels and first differences finding some evidence in favor of the idea that the move to district elections had some effect on black representation early in the sample period.

The presence of women in state legislatures has had a significant effect on state policy. The Center for the American Woman and Politics find that women on average give higher priority to policies related to children, families and health care. Thomas (1994), in a study of 12 state legislatures, finds that women spend more time and effort on bills related to family issues. Case (1998), using U.S. state panel data from 1978-91, documents the extent to which a state's child support enforcement policies tightened as the number of women legislators in the state grew. Controlling for state fixed effects and year effects, and time-varying state-level economic variables, Case finds that the number of women legislators is significantly correlated with the passage of several child support laws. Besley and Case (2000), using state panel data from 1975-88, find that the fraction of women in state upper and lower houses are highly significant predictors of state workers compensation policy.

We examine whether women in state legislatures have a significant effect on policy making in Table 6.3. We focus on a number of policy variables that researchers have posited are sensitive to the gender composition of the legislature (Thomas 1994), in particular on family assistance per capita, and policies related to child support enforcement – here, withholding of child support immediately after the non-resident parent becomes delinquent in payments, and the ability to establish paternity to the child's 18th birthday. We find, controlling for state and year effects and time-varying state-level controls, with or without controls for the ideology of state citizenry, that women in the legislature apply pressure to increase family assistance, and to strengthen child support laws.

7 Policy and Institutions

In this section we look at the effect of political institutions on policy outcomes directly. For the most part, these can be thought of as picking up the direct policy consequences of the institution in question e.g. as when a line item veto in the hands of the governor changes political bargaining power. However, there may also be important indirect effects when institutions work via changes in ideology, composition and control of the state legislature along the lines discussed in the previous section. This latter possibility, and its implications, are discussed in section 7.6.

7.1 Direct Democracy

At the present time, some twenty-three U.S. states have a provision for some form of direct democracy, typically through an initiative process whereby citizens can place ballot propositions which are subsequently voted on. There is now widespread interest in this type of institution and the possibility that it affects the policy process, and there has been an effort to evaluate its role by exploiting cross-state evidence.

Supporters of initiatives are typically populist, selling them as a device to bring policy into line with public opinion. However, opponents worry that minorities will be unfairly targeted and that citizens are too ill-informed to judge policies. The latter concern, together with the view that citizens are easily influenced by slick advertising campaigns, has led some critics of the initiative process in the United States to argue that initiatives actually enhance the power of special interests (see, for example, Broder (2000)).

There are two ways of thinking about the impact of initiatives on policy. First, there are direct effects – when an initiative leads to a policy change. There are many examples of this – we will be discussing the role of initiatives in implementing tax and expenditure limitations. Second, there are indirect effects, whereby initiatives can change policy outcomes if policy makers preempt their consequences by making policy changes.

The theoretical role of citizens' initiatives has received some attention. Matsusaka (1992) discusses what kinds of issues will be decided by initiative (as opposed to elected representatives). He argues that controversial issues that are not too technical, such as banning bilingual education, will tend to be tackled via initiative–largely to allow legislators to avoid making decisions on controversial issues. However, technical issues are often too complicated to be settled via initiative. Gerber (1996) shows how a legislature that does not represent the median view can be called to account by a citizens' initiative. She makes the important observation that a legislature may act preemptively, passing a majority preferred policy to avoid an initiative. Denzau, Mackay and Weaver (1981) make a similar point concerning the ability of initiatives to constrain agenda setting politicians with non-majoritarian preferences. Matsusaka and McCarty (1999) emphasize the possibility that holding an initiative can reveal information to legislators about policy preferences.

Besley and Coate (2000b) suggest that policy bundling is key to understanding the role of initiatives. It is in the nature of representative democracy that candidates take positions on a diverse array of issues. It is possible that candidates will fail to represent majority preferences on some issues, and there is potential for initiatives to restore the majoritarian outcome on those issues.

If citizen's initiatives do bring policies into closer alignment with voter preferences, then there needs to be divergence between the two in the first place, some of which is attributable to electoral rules, such as the openness of primaries, as discussed in the last section. There is a large empirical literature that investigates the "congruence" between public opinion and public policy which finds significant sources of divergence.[27] These divergences lead naturally to the question of whether citizens' initiatives bring policy into closer alignment with preferences.

In assessing the effect of citizen's initiatives, it might be tempting to look at the intensity of initiative activity as a measure of their importance. However, as emphasized by Gerber (1999) and Besley and Coate (2000b) the political equilibrium may change endogenously to the threat of initiatives. Thus, the effect of initiatives on policy may be much less than is revealed by looking at the initiatives that actually pass. Hence most empirical studies simply contrast the policy experiences of states that do and do not have initiatives available. Since the pre-war period has seen so little change in such institutions, the identification of the effect if cross-sectional. Moreover, it is not possible to control for fixed effects which could deal with other sources of underlying heterogeneity.

On strand of empirical literature has used data from U.S. states to test whether public opinion and policy outcomes are closer together in initiative states. For example, drawing on the work of Wright, Erikson and McIver (1987), Lascher, Hagen and Rochlin (1996) investigate whether the link between aggregate measures of policy outcomes and public opinion is closer when states allow citizens' initiatives. They find no significant effect of allowing initiatives.

With respect to specific policy issues, Gerber (1999) finds that policy outcomes on the death penalty and abortion regulation are closer to public opinion in states that permit citizens' initiatives, even though these policies are not directly determined via initiatives. Gerber uses cross-sectional state variation from the 1990s and compares stances on an array of policies (Table 7.1 page 124). She finds significant differences (at the 10% level) for personal income taxes (initiative states lower); highway, natural resources and hospital spending (initiative states higher in all cases); and the implementation of "three strikes" legislation (initiative states lower). Gerber looks in greater detail at the death penalty and parental consent laws for abortion, using public opinion data to estimate median voter preferences. With cross-sectional

[27] This comes out of a variety of approaches. Weissberg (1976) looks directly at the relationship between policy outcomes and citizens' preferences for specific issues while Wright, Erikson and McIver (1987) look at aggregate measures of policy stance. Miller and Stokes (1963) and Herrera, Herrera and Smith (1992) explore the relationship between citizens' preferences and the voting behavior and/or policy preferences of their representatives. Monroe (1983) tudies the relationship between party platforms and public opinion.

data for 1990, she runs a logistic regression that interacts whether a state has an initiative with public opinion, and finds that states with initiatives mirror public opinion more closely.

Other studies have focused more on level effects of initiatives – whether they are device for reducing the size of government. One motivating theoretical background to this is the possibility that political agency problems are diminished when initiatives are available.

In this spirit, Zax (1989) investigates access to initiatives in a cross-section of fifty states for 1980. His dependent variable is the level of direct state expenditures per capita. He finds that state spending is significant higher in states that permit direct statutory initiatives. Farnham (1990) estimates the cross-sectional effect of citizens' initiatives and referenda using data on 735 communities taking the log of state expenditures as the dependent variable. The study finds little evidence of the importance of these measures.

Perhaps one of the best known studies of the empirical impact of initiatives is Matsusaka (1995) which regresses government expenditures and revenues in 49 states (Alaska is excluded) on a number of control variables for a panel of states sampled over a 30 year period at five year intervals from 1960 to 1990. He includes year effects, but not state fixed effects, to estimate the effect of initiatives on expenditure, and finds a strongly negative effect. He also finds some evidence that the effect is strongest where the signature requirement on expenditures is low. While this is interesting, there is an issue of whether some other state characteristic, correlated with initiatives, is driving this. Bails and Tieslau (2000) present somewhat similar results running a random effects panel data regression of total expenditures on initiatives for the period 1969-94, finding a negative and significant effect of a state permitting an initiative.[28]

This variety of findings makes it interesting to take a fresh look at the issue. We present results on the impact of citizen initiatives on state spending and revenues in Table 7.1. We regress state total taxes, state income taxes, total government spending, and spending on family assistance programs (all in real \$1982 per capita terms) on an indicator that the state allows citizen initiatives, with controls for year effects and for state income per capita and income squared, state population and population squared, the proportion of state residents aged 65 and older, and the proportion aged 5 to 17. We do not control for state fixed effects, because only four states (FL, IL, MS, and WY)

[28]Pommerehne (1990) presents a similar finding for Swiss cantons.

changed their policy on initiatives during the 38 year period 1960 to 1997. In Table 7.1 we present three estimates of the impact of initiatives on state fiscal outcomes. In the first panel, we estimate OLS regressions with robust standard errors, allowing for an unspecified pattern of correlation between the unobservables from the same state over this period. The estimation procedure constructs a 38-by-38 variance-covariance matrix for each state, which may lead to a lack of precision in the standard error estimates. Using this estimation procedure, we find no significant effect of citizens' initiatives on state revenues or state spending. In the second panel, we estimate the impact of initiatives using a random effects specification. Here the estimates suggest that state tax revenues per capita – and income tax revenues in particular – are significantly lower in states that have citizens' initiatives. When we use a between-state estimator in panel three, in which regressions are run on state means, the between-state estimates, like the robust estimates in panel one, suggest that the standard errors are large – and too large to say conclusively that state initiatives have held down state taxes and spending. Note that if nothing changed within the state from year to year, and we estimated OLS regressions using 38 years' worth of data from each state, the standard errors would fall by a factor of 6: $\sqrt{38} = 6.16$. Estimating the effect of state initiatives using *only* the state means ignores information that can make our estimates more precise, and is therefore less than ideal. However, the fact that the observations from a given state each replicate roughly the same information each year makes it difficult to interpret the findings here.

Overall these results do (in their random effects incarnation) provide some evidence in favor of a negative correlation between initiatives and overall taxation levels. However, they also illustrate the great difficulties in providing reliable estimates when institutions vary only across states, and not within states over time. Panel data are only of limited value in this context.

7.2 Electoral Accountability

An important issue is how voters hold politicians to account for their performance while in office. Folk wisdom suggests that deterioration in economic performance and tax increases, in particular, are not conducive to electoral success. This claim was made forcefully by Peltzman (1992) in his assessment of gubernatorial electoral chances.

From a theoretical point of view, these claims are best justified in political agency models where there is private information about an incumbent's type (representing his competence or his willingness to consume rents at the citizens' expense) or else there is uncertainty about the true state of public finances. Models along these lines were first developed by Barro (1970) and Ferejohn (1986). It is straightforward to see how they can generate an aversion to tax increases, if the latter are correlated with greater incompetence or greater likelihood of rent seeking behavior.

A key feature of these models is that voters will condition their voting decision on incumbent behavior, either to curb moral hazard problems or else to sort in politicians with desirable characteristics. Hence, we would expect to see voters punish indicators of poor effort or "type" performance and reward the opposite. Electoral accountability is in large measure about reputation formation. This can be built around look at the historical record of parties and Governors. Besley and Case (1995b) argue that whether Governors are term-limited provides a test agency models as the time horizon varies exogenously with term limits. We discuss their results in greater detail below.

As well as looking at past performance, relative performance evaluation may also be important. Besley and Case (1995a) develop a model in which voters use cross-state comparisons of policy when evaluating office holders. They show that this can generate yardstick competition between incumbents. In a world where neighboring jurisdictions face correlated shocks, there is information about the state of one's own jurisdiction from observing the tax setting decision of another. Thus voters would be rational to use relative performance comparisons. This will, in turn, make tax setting decisions across neighboring states interdependent. In terms of electoral accountability, we should expect to see incumbent success correlated with neighboring states' policies and economic conditions as well as one's own.

One way to look at this is through general indicators of state-level economic health. Chubb (1988) considers the determinants of state elections as a function of the performance of the state economy and other factors using data from 1940-82. He finds very little evidence that changes in state income levels affect election outcomes. However, voters may not view state income per capita as being strongly affected by unobservable incumbent effort or type. This line of reasoning ties into the large literature on political budget cycles where the most persuasive theoretical contributions, such as Rogoff (1990), use electoral accountability based on incomplete information as the foundation. Cross-country evidence such as that developed in Alesina and Roubini (1992) find only limited evidence in support of the existence of such cycles. Table 7.2 looks at this in the context of the US states, where we focus exclusively on the states that have a four year electoral cycle for the sake of easy comparison. The data do not show any marked cycle in either state income or unemployment. This is consistent with Chubb's observation that these are not key indicators for which incumbents are held accountable at the state level.

A more promising approach is to look for evidence of electoral accountability as a function of policy variables. In principle, the idea that electoral success depends on past policy choices can be tested straightforwardly. For governors, electoral success can be represented as a discrete variable measuring whether or not the governor is reelected, as in Besley and Case (1995a), or as the percentage of the vote going to the dominant party, as in Alt, Lowry and Ferree (1998). The latter also contend that the ruling party will be held to greater account when there is unified rather than divided control of state offices. They also argue that the effect should be larger in gubernatorial elections where a single agent can be blamed, rather than in legislative elections where blame is harder to attribute.

The evidence is broadly consistent with these ideas. Alt, Lowry and Ferree (1998) find that there are fewer votes for incumbents who experience a shock when there is unified rather than divided government and that the effect is larger in gubernatorial elections. Besley and Case (1995a) find that a governor is more likely to be defeated if he puts up taxes but is more likely to win if his geographic neighbors do. They find this in a model that includes state fixed effects and year effects to control for macro shocks. Kone and Winters (1993) also find that governors suffer at the polls for raising taxes, using pooled time series and cross-sectional data from 1957-85. Measuring the percentage of the votes cast for the Democratic gubernatorial candidate, they find that Democrats are punished for putting up taxes. However, their results do not include year or state indicator variables.

Niemi, Stanley and Vogel (1995) also test for the importance of tax increases on gubernatorial elections using individual data from exit polls for 34 states in 1986. They model the probability that a respondent voted Republican in a particular state as a function of respondent, state and national variables. They allow the effect of the state-level economic and policy variables to vary according to whether the incumbent governor was a Democrat or a Republican. To measure a tax increase, they look at increases in sales, income and excise taxes in the 1985-86 period. They also use a variable that scores the number of tax increases. Consistent with the results of Besley and Case (1995a), they find that support for the incumbent party falls when taxes are increased. They also find state level income to be an important determinant of voting decisions.

Wolfers (2001) also considers the nature of gubernatorial electoral accountability. He shows that events beyond the control of a governor (specifically the oil price) appear to be correlated with whether or not the governor is re-elected. He interprets this as irrational behavior by voters.[29]

Overall, these results leave little doubt that the electoral process does hold policy makers to account for the policies chosen during their tenure. This cements the link between economic and political outcomes in a way that is consistent with theories based on political agency.

7.3 The Scope of Elective Office

While every US state has an elected legislature (in almost all cases two) and a directly elected chief executive, there are marked differences in the extent to which other kinds of state officers are directly. Good examples of institutional variation are public utility commissioners who are elected in 13 states, high court judges who are elected in 23 states and insurance commissioners who are elected in yy states. In states that appoint officer holders, then it is typically the state Governor who is key (although ratification may sometimes be required in the legislature).

From a theoretical point of view, the key difference between appointment and election of particular state office holders is how it changes the electoral salience of the policy issues handled by those officials. This argument has been widely discussed and is formalized for the first time in Besley and Coate (2000a). The argument has both static and dynamic implications. Besley and Coate (2000a) develop the argument to public utility regulators in a static setting, but its scope is potentially much broader than this. If regulators are appointed, then regulatory policy is bundled with many other policy issues at election times. Since voters have only one vote to cast and regulatory issues are unlikely to be politically salient, the link between regulatory policy and voters' preferences is likely to be weak. Directly electing regulators strengthens this link and hence can produce regulators who are more pro-consumer. This device can also weaken the power of interest groups. To

[29]However, if the management skills of a governor are more in evidence when there are good times rather than bad, then this would be consistent with rational updating on the part of voters.

the extent that these lobby on behalf of the industry, this is also likely to be associated with greater pro-consume outcomes.

The second argument is related to the dynamics of the electoral process and the pursuit of re-election incentives. A directly accountable judge, for example, may have to behave in particular ways to get re-elected by voters because his/her reappointment depends on impressing the voters. If he or she is appointed, then the insulating layer from the voters may make a less popular stance possible as he/she knows that the official who is re-appointing them on the basis of an issue that is electorally salient in a multi-issue election. Moreover, it may therefore be possible for appointed judges to be given a more credible long-run incentive scheme (for a given term in office).

The question of whether elected regulators behave differently from those that are appointed has been widely studied in the empirical literature. A number of studies, including Berry (1979), Costello (1984), Crain and McCormick (1984), Harris and Navarro (1983), Navarro (1982), and Primeaux and Mann (1986), have looked at the evidence from different perspectives.[30] Some of these contributions have looked at rate setting, while others have looked at broader indicators of how favorable is the regulatory climate within a state. Costello (1984)'s review of the evidence concludes that "In summary, it probably makes little difference to the average ratepayer whether a PUC is elected or appointed." (page 104). However, the data and time periods used differ across studies as do the set of controls and institutional measures. More worrisome for convincing empirical testing is the fact that the literature is predominantly cross-sectional, with particular researchers choosing selected years and available controls to report their findings.

Besley and Coate (2000a) exploits panel data to look at these issues. They begin by looking at long-run mean differences between utility prices in states that elect and appoint their regulators. Here, they find a robust negative correlation between electing regulators and the price tariff faced by consumers. However, this still begs the question of whether the decision to elect or appoint regulators is simply correlated with important unobservable differences between states. They suggest another key test, motivated by theory, that prices should respond less to cost shocks when states elect their

[30] The large empirical literature on the effects of regulation in U.S. states begins with the seminal contribution of Stigler and Friedland (1962), and is expertly reviewed in Joskow and Rose (1989).

regulators if this results in more pro-consumer regulation. This prediction is tested using data on electricity prices from a panel of U.S. states. The empirical results strongly support the idea that direct elections produce more pro-consumer regulators. One important feature of this test is that, even though the institution may be relatively fixed over time, the comparative static refers to a variable that varies over states and time (the shock to the cost of producing electricity).[31] Hence, the prediction can be tested even with the inclusion of state fixed effects.

There is a significant body of work on the difference between elected and appointed judges. In line with the theoretical discussion above, there is evidence that appointed judges are more independent than elected judges. On the whole they server longer terms in office (see Hanssen (1999, 2000)). There is significant change

Hanssen (2000) tests the idea that judges will be more independent than elected by looking at staffing levels in three budgetary agencies that are subject to judicial review: public utility commissions, insurance commissions and education bureaucracies. He argues that the kind of defensive activity that more independent judiciaries will give rise to will tend to increase staff size. Using cross-sectional data for 1983, he shows that states with elected judges have significantly larger bureaucracies controlling for a number of other observables. Hanssen (1999) looks at whether states that elect their judges have more or less litigation activity, arguing that this may reflect the degree of uncertainty in the operation of courts. Using data from all 50 states, he tests whether are significantly more Public Utility disputes (1978-83), and High Court and Trial Court Filings (1985-94) in states that elect their judges. The main finding is that appointing states have significantly higher rates of judicial activity in each of the first two cases, but no effect in the third.[32] These are identified from cross-sectional differences but introduce a number of economic and demographic control variables.

7.4 Term Limits

Another institutional variation across U.S. states is in whether they hold elected officials to a term limit. Some kind of term limit is observed for governors in roughly half of the states. A key issue is how we should expect

[31] Cross-secitonal variation comes from the differing production structures across states.

[32] In the case of public utility commission ruilings, these are also higher in states that appoint their utility commissioners.

such limits to affect policy outcomes. Besley and Case (1995b) consider a framework in which term limits change the incentives of politicians to build a reputation. When a term limit binds, there may be a temptation to play the end game, resulting in significant changes in policy.

One way to look at term limits is cross-sectionally, looking at the permanent differences in policies in those states that have such limits. This is the approach in early work on the topic by Crain and Tollison (1977) and (1993), and Crain and Oakley (1995). Crain and Tollison (1977) make the interesting and important point that if political office is a productive asset, one used to produce political outcomes, then candidates for the office should be willing to pay more for the opportunity to serve in states with longer terms, and in states without term limits. They find, using cross-sectional data for races run in 1970, that challengers spend less money when running for two two-year terms than do those running for one four-year term. In addition, challengers spend less in states with term limits. Crain and Oakley examine whether states that allow governors to succeed themselves indefinitely have different public capital stocks and flows than do states where governors are restricted by some sort of term limit. They find, using data from the 1980s, and controlling for a number of state institutions, that the stock of state government capital per capita, the change in the stock, and the percentage change in the stock are all lower in states with term limits. Bails and Tieslau (2000) argue that term limits should lower the rate of growth of spending by "making public-sector decision makers more responsive to the desires of the citizenry" (page 260). They test for this using a random effects model for the period 1969-94 and confirm a negative coefficient on state expenditures. All of these results raise the usual issue of whether such limits are merely proxying for omitted state level characteristics. State income per capita and state population are significantly lower in states with term limits, to name but two important differences between states with and without term limits (Besley and Case 1995b, Table III, p. 778).

A second strand of the term limits literature uses data on the behavior of representatives in the U.S. Congress to predict how state representatives facing term limits would be expected to behave. Lott and Bronars (1993) analyze Congressional voting data from 1975-90, and find no significant change in voting patterns in a representative's last term in office. However, it is far from clear that Congressional representatives who announce they are stepping aside provide an adequate picture of the behavior of state governors who are bound by law not to run again for reelection. A provocative paper on potential end-games in the U.S. Congress is provided by McArthur and Marks (1988), who observe Congressional behavior in a lame duck session of Congress: in post-election sessions, members who have not been reelected are at times called upon to vote on legislation before the swearing in of the new Congress. McArthur and Marks observe that lame duck representatives were significantly more likely in 1982 to vote against automobile domestic content legislation than were members who were returning.

Besley and Case (1995b) identify the effect of a term limit from the difference between first and second terms in office for incumbents that face term limits. Controlling for state fixed effects and year effects, and using annual data from the 48 continental U.S. states from 1950-86, they show that a variety of policy measures are affected by term limits. Specifically, state taxes and spending are higher when term limits bind in states that have them. They show that such limits tend to induce a fiscal cycle with such states having lower taxes and spending in the first gubernatorial term compared to the second.

List and Sturm (2001) apply the Besley and Case methodology to cross state variation in environmental policy. They find that governors in their last term in office are significantly more likely to spend resources on environmental protection over the period 1960-99. However, this term limit effect is muted in states where a larger fraction of citizens belong to environmental organizations. They also show that their term limit effect varies according to the margin of victory in the gubernatorial race – with term limit effects being attenuated when the margin of victory is larger.

We update the results from Besley and Case (1995b) using data from 1950-1997, and present the results in Table 7.3. Controlling for state and year effects and time-varying state-level variables, we continue to find that in those years in which an incumbent governor cannot stand for reelection because of a term limit, on average state spending per capita is significantly higher (roughly $15 per person higher in 1982 dollars). However, our earlier finding that taxes are higher when the governor is a lame duck does not hold in this longer panel. Total taxes per capita are lower, although not significantly so, on average in those states in which the current governor cannot stand for reelection. We examine this further in Figure 1, in which we graph the pattern on the effect of having a lame duck, year by year, from 1950 to 1997, controlling for the same time-varying variables used in the regressions presented in Table 7.3. We find that, over time, the impact of having an incumbent who cannot stand for reelection has changed: from being on average positive and significant in the first half of the period, to being on average negative and significant (and much more variable year-to-year) in the second half. We can offer no simple explanation for this pattern. It continues to emerge when we control for other time-varying state level variables that we think may influence taxes — such as the need for a super majority to change taxes, the fraction Democrats in the lower house of the legislature, the amount of competition between the parties in the legislature, and the level of grants from the federal government. We parameterize the changing pattern in the impact of having a lame duck governor in regression results presented in the 2nd and 4th columns of Table 7.3. We see that for both taxes and state spending, governors in the 1950s spent and taxed more when they could not stand for reelection, but over time this phenomenon has changed. This is an area ripe for future research.

7.5 Restrictions on Incumbent Discretion

Central to much of the public choice literature is the idea that a fiscal constitution should limit the policy choices of elected representatives. A large number of policies are subject to judicial review. However, some states' constitutions embody specific limits and grants particular kinds of veto power in the policy process. Many U.S. states historically prohibited levying particular taxes. A number of states also limit the ability of legislators to finance public expenditures with debt. While the need to limit incumbent discretion is at the heart of a traditional Public Choice view, there is much scepticism about the possibility of designing effective constitutional limits.

7.5.1 Tax and Expenditure Limitations

A number of these issues have been scrutinized in recent work. Restrictions on taxing and spending fall into three broad categories: (i) indexed limits on the growth of revenues or expenditures, for example, to the population growth rate; (ii) requirements that voter approve all new taxes and (iii) supermajority requirements that require anywhere between three fifths and three quarters of the legislature to approve tax increases. There are twenty-four states with indexed limits, 13 allow an override with a supermajority vote, and 5 require a simple majority if the governor has declared a state of emergency.

Most tax and expenditure limitations were introduced in the 1970s, which many believe reflected a general disillusionment with government and a view that spending was out of tune with what a majority of voters preferred. At a theoretical level, the adoption of limits can have either a distributional or efficiency motivation. The former would be germane if the group of citizens or legislators who adopted the measure saw it as way to constrain future policy makers in a way that was favorable to them. The efficiency motivation is strongest if there are circumstances where binding the hands of governments works to everyone's advantage. However, short of that, there is a possible motive in restraining politicians from extracting rents from ordinary voters. This concern runs through Brennan and Buchanan (1980). However, it is developed in a more satisfying way by authors using agency models with imperfect information.

Bails and Tieslau (2000) run a panel data regression which puts both supermajority requirements and expenditure limits on the right hand side of a spending equation. Their random effects specification for the period 1969-94 finds a negative and significant effect of expenditure limits and supermajority requirements.[33]

Rueben (2000) gives a useful overview of the history and content of tax and expenditure limitations. Half the states that have tax and expenditure limitations restrict the growth in state expenditures to the growth rate in personal income averaged over some previous period. Five others restrict the size of appropriations to a specified percentage of state income, while four others restrict growth to an index of population growth and inflation. Three other states restrict the absolute expenditure growth rate. Spending on capital projects is excluded, as are federally funded projects. Half of the limits in place are constitutional with the remaining fraction being statutory.

The early literature on tax and expenditure limitations fueled scepticism by finding only weak responses to their introduction. For example Abrams and Dougan (1986) could not find any effect of the line-item veto or borrowing limits. However, tax and expenditure limitations did seem to have a marginal and negative effect. However, one cannot put much credence in cross-sectional work for the reasons that we have already outlined. Most recent contributions use panel data to assess the effectiveness of constitutional limits.

[33] In fact the latter is interacted with balanced budget amendments which makes it difficult to assess whether the supermajority requirements enters significantly when entered alone. However, results below suggest that it probably does.

The efficacy of these measures has been assessed in a number of recent contributions. Poterba (1994) finds that states tended to raise taxes less in response to fiscal crises when there was a tax or expenditure limitation. A general concern raised in the literature is whether states can find ways around most of the limitations that they face. Rueben (2000) finds that binding state tax and expenditure limits have no effect in the OLS or fixed effects specifications.

Shagbegian (1996) considers the effect of tax and expenditure limitations on the size of government in a panel of states over the period 1972-87. His left hand side variable is the growth of government expenditures. As well as looking for a level effect of tax and expenditure limitations, he also interacts these limitations with income. Consonant with Rueben (2000), he finds no level effect. However, the interaction effect is always significant. This is intriguing, although there is no obvious interpretation of this result from a theoretical point of view.

Rueben (2000) consider what leads to the introduction of tax and expenditure limitations. This is natural as many are fairly recent and it seems unlikely that they could be taken as exogenous. She uses whether a state has direct legislation as an instrument for whether a state has a tax and expenditure limitation. In her analysis, tax revenues as a percentage of personal income fall by around 2% in the two-stage least squares estimates. This contrasts with her findings for the fixed-effect and OLS estimates.(However, because she is using a fixed institution as an instrument, she is unable to employ state effects in the two stage least squares case.) This is a good example of why we might expect to see certain institutional systems being correlated with one another. Moreover, they give a more precise account of why Matsusaka (1995) finds a negative correlation between public spending and the availability of direct legislation.

We present results on the association between tax and expenditure limitations and state taxes and spending in Table 7.4. Controlling for state and year effects, and time-varying state-level variables, we find that non-binding limits (those that are advisory or require only a simple legislative majority to change) are not significantly correlated with either taxes per capita or spending per capita. However, potentially binding tax or expenditure limits are positively (i.e., perversely) correlated with both taxes and spending. This again highlights the problems associated with quantifying the impact of institutions on policy outcomes when the institutions may change in response to policy choices – such as taxes and spending that citizens consider inappropriately high. We return to the issues associated with endogenous institutions in Section 8.

7.5.2 Budgetary Rules

The Reagan era of budget deficits ushered in a period of heightened concern about the causes and consequences of budget deficits. This filtered into state politics too with many states passing measures that restrict the ability of elected officials to use deficit finance. Indeed, all states, with the exception of Vermont, have some measure of this form.

Poterba (1996) looks in detail at rules that restrict the ability of states to run budget deficits. He points out that there are good reasons, *a priori*, to be suspicious of the efficacy of budget rules. This is due to the relative ease with which accounting procedures can be modified and the nominal timing of taxes and expenditures can be changed. Moreover, in many states they apply only to part of the government budget (the "general fund" budget). There are, however, differences in the way in which the laws are structured, which leads one to expect a difference in the impact of these laws across states. The weakest form of restriction requires that the governor submit a balanced budget (44 states). This need not lead to balanced budget being passed by the legislature, but 37 states have a rule requiring this. Stricter still is a prohibition on carrying forward a deficit, which has been passed by 24 states.

Bails and Tieslau (2000) consider the effect of balanced budget amendments on state spending in the period 1969-94. They interact such requirements with whether a state has a supermajority requirement and some form of expenditure limitation. Using a random effects panel model, they find balanced budget requirements are associated with reductions in spending, but only when they are combined with these latter institutions.

There are other rules affecting the budgetary process, including the line item veto whereby a governor can strike out a particular unwanted budgetary item rather than vetoing the budget as a whole. This should in theory enhance the bargaining power of a governor. Restrictions on raising public debt is also an important part of the budgetary process and most states have some form of restriction of this kind. Debt limitations are basically of four kinds (see Kiewert and Szakaly (1996), Table 1). At one extreme are outright prohibitions on guaranteed long-term debt. Even if there is no prohibition, some states require referendum approval. The weakest restrictions are revenue-based limitations or some kind of supermajority voting requirement in the legislature.

Bohn and Inman (1996) examine the effect of budgetary rules on deficits using panel data over the period 1970-91. They model general fund surplus a function of economic and political control variables and the state's budgetary institutions using fixed and random effects. Because the former cannot identify the effects of budgetary institutions that do not vary in the cross-section, they regress the fixed effects on budgetary institutions. Their main finding is that states with the strictest budgetary institution – no deficit carry forward – and the line item veto have lower deficits. They also find that states that require referenda ahead of debt issues also have lower deficits. However, in contrast to other studies, they do not find an effect of divided government.

Poterba (1994) looks at the effect of state fiscal rules using state-level data from 1988-92 from the National Association of State Budget Officers. He looks at responses to shocks – calculated as differences between actual and budgeted outcomes – and finds that state fiscal institutions affect the short-run patterns of taxes and spending in response to shocks in a short panel. He also finds that divided state governments affect the extent of adjustment.

Requiring a supermajority in the legislature is also an important form of budgetary rule. These are studied in Knight (2000) who finds that supermajoirity requirements hold down taxes. Having a supermajority requirement reduces taxes as a proportion of state income by between 1.7% and 3.6% in various specifications. He also worries about the potential endogeneity of the budgetary rule which is natural given that twelve out of the thirteen states that have supermajority requirements passed them in the period that Knight (2000) studies (1963-1995). Using two stage least squares, he predicts the probability that a state has a supermajority requirement from three other "institutional" variables: whether the state permits direct legislation, the legislative vote required to pass a constitutional amendment and the sessions required to consider an amendment. He finds that permitting direct legislation is positively correlated with having supermajority requirements and that having a larger number of sessions is negatively correlated with it. The former is not surprising given that half of the states that have implemented supermajority requirements have done so via citizens' initiatives. This is consistent with the findings of both Reuben (2000) and Matsusaka (1995).

A number of studies have looked at the impact of restrictions on debt finance. Bunch (1991) considers the relationship between such limits and the number of state authorities and their scope. She finds some relationship between the two. However, the cross-sectional approach does not make such findings reliable. Moreover, the theoretical link is far from clear. The paper by von Hagen (1991) considers the cross-sectional impact of debt limitations on state debt in 1985 finding little evidence of an effect. Kiewert and Szakaly (1996) look at the effect of state debt limitations on dent in U.S. states using data from 1960-1990. They use a random effects model and find that states that prohibit guaranteed debt and require a referendum for approval had less guaranteed debt than those that required a supermajority or those with revenue based limitations.

Holtz-Eakin (1988) considers the impact of the line-item veto on budgetary outcomes. He uses a variety of measures of state expenditures and tax revenues as the dependent variables from 1965-83, and a range of economic and political variables as controls. As well as looking at cross-sectional differences, Holtz-Eakin also identifies the effect in a fixed effects model by interacting whether a state permits a line-item veto with the nature of political control, in particular with whether there is divided control of the legislature. The cross-sectional results do not find any effect of the line-item veto which is consonant with Dougan and Adams (1986). However, for the fixed effects results, he finds that when government power is divided between two parties – one controlling the executive and the other the legislature – then having the line-item veto reduces spending and raises taxes leading to smaller deficits. This makes sense – the line-item veto has most impact when there is a preference conflict. In contrast to Holtz-Eakin's cross-sectional results, Bohn and Inman (1996) find that mean state deficits are lower in states where the governor has a line-item veto.

We present evidence of the impact of supermajority rules and the gubernatorial line-item veto in Table 7.4. Like much of the literature, we find a large, negative and significant effect of supermajority rules on total taxes collected per capita. On average, and with all else held equal, state taxes per capita are roughly \$60 lower in the years after the state passes a supermajority requirement. This amounts to roughly 12 percent of state taxes during the period over which the estimation is run (1950-91). The supermajority requirement has little effect on state spending, as the last columns of Table 7.4 show.

In this period, only two states changed their rules on allowing the governor a line item veto: Iowa and Washington both moved to allow line item vetoes in 1969. Thus, the indicator that the governor is allowed a line item veto is identified only off of the difference in these two states before and after its passage (and is for that reason not particularly meaningful.) More interesting is the Holtz-Eakin interaction term of the line item veto with divided government. We find, in contrast to Holtz-Eakin, that in a divided government the line item veto reduces both taxes and spending – although the latter more than the former – which would have the effect of reducing budget deficits.

7.6 Indirect Effects of Institutional Rules

Following a theme that we have been developing, we continue with notion that some of the most important mechanisms by which institutions affect policy are indirect – through their effect on the ideology, composition and control of the state legislature. Indeed, above we showed that there can be important responses in these variables to institutional variations.

In this sub-section, we explore the chain of influence in a little more detail for the institutions studied in section 5. Table 7.5 investigates whether the effect of open primaries, voter registration laws and restrictions on corporate campaign contributions works through the impact of "intermediate variables" – party competition or Democratic control of the legislature.

We begin by showing in column 1 of Table 7.4 that these institutions are highly significantly correlated with total taxes per capita. Specifically, open primaries and corporate restrictions are negatively and significantly correlated with taxes, while less-costly voter registration is positively and significantly correlated with taxes. The F-statistic of their joint significance is large (F=20.22, p-value=0.0000). It is difficult to make a case that any of these institutional rules have a direct effect on taxes, which leads us to ask whether we might uncover the chain through which they do affect policy.

Column 2 of table 7.5 presents two-stage-least-squares (2SLS) estimates of the impact of party competition in the state legislature on total taxes, in which our measure of party competition is instrumented on the institutional rules. We find that, instrumented on open primaries, registration rules and corporate restrictions, party competition is negatively and significantly correlated with taxes. The results in column 2 suggest that a one standard deviation increase in competitiveness (an increase in our measure of 0.07 points) is associated with a reduction in total taxes per capita of \$240, measured in \$1982.[34] This is roughly a one standard deviation decrease in taxes over this period (a one standard deviation decrease would amount to \$230). Moreover, we can compare the fit of two regressions – that in column 1, where the institutional rules are allowed to enter in an unrestricted fashion, and that in which the institutional rules enter only through their effect on predicted party competition. The F-test at the bottom of column 2 answers the question of whether the fit of the regression is significantly worse if we force the institutional rules to enter only through their effect on party competition. It is not: F=0.76, p-value =.5483, and we cannot reject that these rules are affecting total taxes solely through their effect on party competition. (Note that this does not prove that party competition is the mechanism at work – but it cannot be rejected by the data.)

We repeat this exercise in column 3, here asking whether the institutional rules might be working through Democratic control. The 2SLS estimate of Democratic control is positive and significant. However, unlike the results for party competition, the fit of the regression in which the institutional rules are entering through the indicator of Democratic control is significantly worse than that when the institutional variables are unrestricted (F=13.57, p-value = 0.0000). When both predicted values are entered in the same regression, as in column 4, it is clear that party competition is the favored mechanism through which these institutional rules are affecting total taxes.

We present this discussion solely as an illustration. However, we believe that more generally this sort of exercise may prove useful when analyzing whether and how the legislative political landscape is affecting policy choice. For many analyses, the institutional rules will provide a source of exogenous variation in political control which can be used to analyze how policies change in response to the character of the legislature. In all of these analyses, it will be important to ask how the institutional rules have been set – a topic we turn to in the next section.

8 Endogenous Institutions

The common assumption in the vast majority of cross-state studies that the institutions are exogenous. This is more or less plausible depending on the time frame and issue in question. Endogeneity is really only part of a broader set of concerns about the possibility that institutions are correlated

[34] That is, –3434.63 × 0.07 = –240.42.

with omitted right hand side variables, and one hope is that state fixed effects and time-varying state-level variables will capture state differences that determine both institutions and policy. To better understand whether and when it is appropriate to take institutions as exogenous, we provide in this section a look at some of the systematic determinants of institutional rules.

Even if institutions do not change over time, there is no guarantee that it is legitimate to take them as exogenous. Moreover, as we observed in our study of citizens' initiatives, it may be difficult to control for sources of unobserved heterogeneity with state fixed effects when institutions are fixed over time. Thus, it remains difficult to distinguish between a genuine institutional effect and the possibility that tastes for citizens' initiatives and taxation are correlated. The main hope here is that some kind of comparative static with respect to some exogenously changing variable can be identified and tested. For example, Besley and Coate (2000a) used the comparative static with respect to fuel prices to gauge the influence of elected versus appointed regulators even where the latter institution did not change.

The charge of endogeneity is not by itself very meaningful. The key empirical issue is to identify possible sources of correlation between institutional variables and the error term in an equation which has either x_{jst} or ℓ_{jst} on the right hand side. The most difficult cases are those where there are good reasons to believe that there are shocks to the policy in question that drive the demand for institutional reform as when states that have a history of deficits implement some kind of balanced budget rule. In each instance, the possible fix (if one exists) must be thought out on a case-by-case basis. It is unlikely that there is any kind of panacea for these problems.

To generate a wholesale account of the endogeneity of institutions of the form studied here is daunting to say the least. However, real progress is most likely to come via beginning with some underlying theoretical account of what motivates institutional change. The large literature on strategic use of long-run policies is relevant here. The early literature – Persson and Svensson (1988) and Tabellini and Alesina (1987) focused on the strategic use of debt to constrain the flexibility of future incumbents. A general account of this type of argument and its relationship to the notion of political failure is in Besley and Coate (1998). Institutional reform is certainly one way of trying to influence future political outcomes.

In some contexts, the theory may give rise to a natural instrument for the institution in question. One of the main cases where this is an issue is the case of tax and expenditure limitations that were adopted comparatively recently and may have been favored in states that have a history of high spending. Reuben (2000) and Knight (2000) both use citizens' initiatives as instruments for tax and expenditure limitations and super-majority requirements respectively. While ingenious, there are a couple of drawbacks. First, for the reasons that we discussed in our treatment of citizens' initiatives above, fixed effects cannot be included and this may be a rather important omission. Second, in instrumenting one institution using another, we must assume that the institution used as an instrument is not itself correlated with the unobservables in the policy equation.

In one of few studies to examine directly the endogeneity of institutions, Hanssen (2001) considers the strategic use of appointing rules for state court judges. He argues that incumbents with firm control of the reins of power are likely to prefer more independent judiciaries given that they might not be in office in future and perceive the judiciary as a check on future incumbent's behavior. However, a party which is expected to remain in office will not benefit from such countervailing powers. He considers the changes in judicial selection rules between 1950 and 1990. There are 27 changes in judicial selection rules over this period. Modeling the timing of the switches as a function of the prevailing political conditions, he finds that firmer single party controls is found to be associated with less independently accountable judiciaries.

The extent to which institutions respond to the political composition of the legislature, and to state demographic change, is explored in Table 8.1. Specifically, we ask whether current institutional rules on primaries, on voter registration, and on corporate campaign restrictions, are significantly correlated with measures of past political legislative control and with past demographic and economic variables. In the first column for each institutional rule, we regress the institution on lagged values of the proportion of Democrats in the state lower house in years t-4, t-6, t-8 and t-10; the proportion of Democrats in the state upper house in years t-4, t-6, t-8 and t-10; indicators that the Democrats controlled both houses in t-4, t-6, t-8 and t-10; and our measure of party competition in the legislature in t-4, t-6, t-8 and t-10. (Our results are robust to the inclusion/exclusion of lags at t-4 and t-12.) In all of these regressions, we control for the same time-varying state-level variables described above, along with state fixed effects and year effects. We find – both for open primaries and for constraints on corporate contributions – that past legislative variables are significant determinants of current state policy. In the second column for each rule, we regress the institution on lagged values of state population, state income per capita, the proportion of the population aged 65 and above, and the proportion aged 5 to 17 in years t-4, t-6, t-8, and t-10. We find, for open primaries, motor-voter registration, and corporate campaign restrictions, that these past economic and demographic variables are significant determinants of current rules. Jointly, the past economic and legislative variables are significant for open primaries, for motor-voter registration and for corporate restrictions.

In studying the impact of a particular institutional rules on policy outcomes, then, we must ask whether the determinants of the institutional rule are thought to have independent effects on current policy. For example, past legislatures may affect both the institutional rule (as we find that they do for corporate contributions) and may also they tie the hands of the current legislature (for example, by taking on debt). As always, this must be addressed on a case-by-case basis.

9 Concluding Remarks

The aim of this paper has been to bring together the large literature that looks at the policy impact of cross-state institutional variations. This is a large and growing area of research. The US states are an important laboratory that can potentially enhance our understanding of the policy process. We have grouped studies into those that consider how the process of political representation works, those that consider how political representation affects policy and those that affect policy directly. These three components are the ingredients of the broader political economy story.

Our focus here is exclusively on the United States. However, there are clear parallels to the work undertaken here and other important work on comparative political economy. Of particular note is the recent cross-country work by Persson and Tabellini (2000). They have been building a data set that models the differences between political institutions across countries. They have focused particularly on different models of the separation of powers and the extent to which systems are closer to proportional representation. It is clear that similar issues to those discussed here, such as identifying chains of influence and possible sources of endogeneity (particularly sources of unobserved heterogeneity) are relevant in this context. The work is clearly complementary in its ambition and scope to that discussed here. There are sources of variation that can be studied trans-nationally, but not using US states – variations in the electoral system being a key example.

There is also parallel work at a subnational level in other federal systems where there are important institutional variations. A good example is the work on citizens' initiatives in Switzerland by Pommerehne (1990). Pande (2001) exploits differences in rules that mandate representation of disadvantaged minorities (scheduled castes and scheduled tribes) in India. She finds that the transfer programs favoring the disadvantaged groups expand (other things being equal) when representation increases. Further sub-national work on these kinds is an important part of the agenda to deepen our empirical understanding of the policy process.

Viewed as applied economics, the challenge presented by empirical studies of the policy process is to proceed in step with political process. Ideally, the empirical agenda will cast light on the relevance of theoretical models. In this regard, the results from the existing empirical research do clearly point to a post-Downsian agenda. One of the key intuitions from a simple lesson in Downsian political economy is that, to a first approximation, the preferences of the median citizen on any given issue are decisive. It has been known for a long time that this approach does not deliver theoretically. Taken literally, the approach generates huge instabilities and no useful empirical predictions. By anchoring the discussion in the data, the agenda has a much clearer focus on building a picture of the kinds of phenomena that theoretical models have to account for.

Even apart from the theoretical issues, the notion that the median voter is decisive is indirectly refuted by two of the key findings from this survey. First, institutions do appear to matter in the way that mediate between preferences and policy. Taken literally, the median voter view would not be able to account for this. Second, representation matters. The results point to important correlations between policies, political competition, party representation and voting rules. This nexus has to be central to explanations that are consistent with the data. Again, the Downsian model, taken literally, encourages us to believe that parties are irrelevant.

Thus, the weight of evidence should lead us to be skeptical about insights built on Downsian foundations. It is clear that the next agenda is to try to build simple models that compete for their ability to match the data. A good starting point would be a role for the internal workings of parties (particular elites), interest groups and electoral competition.

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Table 4.1 Institutions, Legislative Outcomes and Policy Outcomes U.S. States 1950 to 1999

	1950	1960	1970	1980	1990
Institutions: electoral rules	Percent of states covered by the following institutions				
Poll taxes	15.2	10.9	0	0	0
% state population affected by a literacy test	13.5	13.5	1.8	0	0
Voter registration through vehicle registration	0	0	0	4.2	22.9
Voter registration on polling day or no registration	0	2.1	2.1	6.3	6.3
Open primaries	29.2	14.6	14.6	20.8	27.1
Restrictions on corporate campaign contributions	62.8	76.2	71.4	65.1	64.6
Gubernatorial term limits	40.8	43.8	50.0	54.2	58.3
Citizens initiatives	NA	39.6	41.7	45.8	45.8
Institutions: decision-making rules					
Tax and expenditure limitations	0	0	0	2.1	22.9
Super-majority requirements	NA	2.1	6.3	14.6	14.6
Gubernatorial line-item veto	81.3	81.3	85.4	85.4	85.4
Legislative outcomes					
Fraction Dem in lower house	58.5	69.0	55.4	61.6	59.7
Fraction Dem in upper house	54.5	65.7	55.2	64.8	60.2
Indicator: Dem governor	60.4	68.8	35.4	62.5	56.3
Fraction Female lower house	NA	NA	NA	11.4	18.0
Fraction Female upper house	NA	NA	NA	5.5	13.4
Ideology: COPE score governor	NA	55.4	34.7	46.1	48.4
Voter turnout (presidential election years)	63.6	63.6	62.2	55.5	52.2
Party competition in legislature	–.092	–.079	–.058	–.054	–.034
Policy outcomes					
Total taxes per capita $1982	161.4	249.8	464.4	568.2	734.9
Total spending per cap $1982	370.5	534.3	974.1	1200.7	1526.0
Family assistance per capita $1982	NA	17.2	44.9	50.5	42.8
Medicare per capita $1982	NA	NA	81.3	168.7	296.4

Notes: Poll taxes and literacy test data do not include Nebraska or Minnesota. The first column for corporate campaign restrictions presents results for 1952. Tax and expenditure limitations present an indicator for potentially binding tax and expenditure limitations. Voter turnout is the turnout for the highest office in the race in that year, divided by the state's age-eligible voting population, reported here for election years: 1952, 1960, 1968, 1976, 1988.

Table 4.2 Regional Differences in Institutions, Legislative Outcomes and Policy Outcomes

	South	Non-South	South	Non-South
Institutions: electoral rules	1960		1990	
Poll taxes	31.3	0*	0	0
% state population affected by a literacy test	33.6	2.8*	0	0
Voter registration through vehicle registration	0	0	12.5	28.1
Voter registration on polling day or no registration	0	3.1	0	9.4
Open primaries	0	21.9*	12.5	28.1
Restrictions on corporate campaign contributions	71.4	78.6	75.0	59.4
Citizens initiatives	12.5	53.1*	18.8	59.4*
Institutions: decision-making rules				
Tax and expenditure limitations	0	0	12.5	28.1
Super-majority requirements	6.3	0	31.3	6.3*
Gubernatorial line-item veto	93.8	75.0	93.8	81.3
Legislative outcomes				
Fraction Dem in lower house	93.4	60.0*	71.7	53.6*
Fraction Dem in upper house	91.8	51.9*	76.5	51.8*
Indicator: Dem governor	87.5	59.4*	56.3	56.3
Fraction Female lower house	NA	NA	11.3	21.4*
Fraction Female upper house	NA	NA	9.6	15.3*
Ideology: COPE score governor	37.6	64.3*	45.3	49.9
Voter turnout (presidential election years)	47.1	71.8*	46.5	55.1*
Party competition in legislature	–.190	–.020*	–.068	–.015*
Policy outcomes				
Total taxes per capita $1982	251.1	249.1	691.5	756.6
Total spending per cap $1982	500.1	551.4	1388.4	1594.7*
Family assistance per capita $1982	16.1	17.8	28.9	49.7*
Medicare per capita $1982	NA	NA	301.2	294.0

Notes: An asterisk (*) notes that the difference between the South and the Non-South is significant at a 5 percent level. States in the South are AL, AR, DE, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA and WV. Tax and expenditure limitations present an indicator for potentially binding tax and expenditure limitations. Voter turnout is the turnout for the highest office in the race in that year, divided by the state's age-eligible voting population, reported here for election years: 1952, 1960, 1968, 1976, 1988.

Table 5.1 Dependent Variable: Election Turnout of Age-eligible Voters

	(1)	(2)	(3)	(4)	(5)
Poll tax	–.140 (.010)	--	--	--	–.157 (.013)
Literacy test	–.117 (.011)	--	--	--	–.138 (.012)
Citizen initiatives	--	.033 (.014)	--	--	--
Indicator: voter registration through vehicle agency	--	--	.003 (.008)	--	.004 (.007)
Indicator: voter registration possible on polling day or no registration necessary	--	--	.017 (.013)	--	.025 (.014)
Indicator: restriction on corporate campaign contributions	--	--	--	.021 (.006)	.018 (.005)
State fixed effects included?	Yes	No	Yes	Yes	Yes
Years over which regression run	even years 1950-1998	even years 1960-1998	even years 1950-1998	even years 1952-1998	even years 1952-1998
Number obs	1174	958	1198	1060	1038

Notes: Standard errors in parentheses. All regressions control for year effects, and include controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982 and income per capita squared; state population and population squared. Omitted voter registration category in columns 3 and 5 is “conventional” registration. We do not include state fixed effects in column 2 because only 4 states changed whether they allowed initiatives over the period 1960 to 1998. In column 2, we estimate robust standard errors, and allow for correlation in the unobservables from the same state. Campaign finance data are currently not available for 1950.

Table 5.2 Political institutions and representation

	Fraction Democrat in State Upper House			Party Competition in Legislature			Indicator: Democratic Governor		
Poll tax	.032 (.028)	.031 (.030)	--	–.025 (.009)	.004 (.010)	--	.148 (.130)	.046 (.138)	--
Literacy test	.081 (.025)	.082 (.027)	--	–.022 (.008)	.001 (.009)	--	.006 (.116)	–.101 (.124)	--
Indicator: voter registration through vehicle agency	–.015 (.015)	–.015 (.015)	--	–.002 (.005)	–.003 (.005)	--	.048 (.069)	.052 (.069)	--
Indicator: voter registration possible on polling day or no registration necessary	.056 (.029)	.056 (.029)	--	–.039 (.010)	–.043 (.010)	--	.007 (.136)	.025 (.136)	--
Indicator: restriction on corporate campaign contributions	.021 (.011)	.020 (.011)	--	.010 (.004)	.007 (.004)	--	.038 (.049)	.053 (.049)	--
F-test: institutional variables (p-value in parentheses)	4.47 (.0005)	4.10 (.0011)	--	8.36 (.0000)	5.05 (.0001)	--	0.48 (.7913)	0.50 (.7772)	--
Voter turnout	--	–.002 (.067)	--	--	.178 (.022)	--	--	–.693 (.312)	--
IV estimation: Voter turnout	--	--	–.309 (.116)	--	--	.169 (.038)	–	--	–.340 (.531)
F-test:	--	--	3.79 (.0021)	--	--	5.88 (.0000)	--	--	0.54 (.7467)
Number obs	1028	1026	1025	1040	1038	1025	1027	1025	1038

Notes: Standard errors in parentheses. All regressions run over odd-years from 1953 to 1999. All regressions control for year and state fixed effects, and include controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982 and income squared; and state population and population squared. Omitted voter registration category is "conventional" registration. All control variables are lagged one year, to reflect the conditions in place at the time of the election. Results in column 3 are for instrumental variables estimation, where voter turnout is instrumented on the institutional rules that appear in columns 1 and 2. The F-test in column 3 compares the fit of the regression using the predicted value to that in column 1, where the institutional rules are allowed to enter in an unrestricted fashion. Results in column 6 report an analogous comparison for party competition in the legislature. We reject that these institutional rules are affecting fraction Democrats and party competition solely through their effect on voter turnout.

Table 5.3 The impact of primary rules on turnout, ideology and party competition

	Dependent Variable:							
	Turnout		Fraction Democrats in state lower house		Fraction women in state lower house		Absolute difference (citizen-government COPE score)	
Indicator: open primaries	.011 (.007)	.015 (.007)	.001 (.012)	–.001 (.013)	–.015 (.007)	–.014 (.007)	–3.47 (2.30)	–3.41 (.2.44)
Poll tax		–.155 (.134)		.014 (.025)		–		--
Literacy test		–.137 (.012)	--	.045 (.022)	--	--	--	--
Indicator: voter registration through vehicle agency	--	.010 (.009)	--	.021 (.017)	--	.009 (.007)	--	–2.42 (2.25)
Indicator: voter registration possible on polling day or no registration necessary	--	.020 (.016)	--	.039 (.029)	--	–.056 (.017)	--	2.95 (3.72)
Indicator: restriction on corporate campaign contributions	--	.018 (.005)	--	.020 (.010)	--	.015 (.006)	--	1.89 (1.43)
Years over which regression run	even years 1950-1990, 1996,1998		odd years 1951-1991, 1997,1999		odd years 1975-1991, 1997, 1999		even years 1960-1990	
Number obs	1099	942	1067	934	525	498	768	709

Notes: Standard errors in parentheses. All regressions control for year and state effects, and include controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982 and income squared; and state population and population squared. Omitted voter registration category in columns 3 and 5 is "conventional" registration. No registration was necessary in North Dakota from 1951 to 1998, and we have added that state to "registration possible on polling day." For regressions in columns 3-6, all control variables have been lagged one period, to reflect the conditions in place at the time of the election.

Table 5.4 Redistricting and legislative composition

	Dependent Variable:			
	Change in Fraction Dem lower house	Change in Fraction Dem upper house	Change in number of Dems lower house	Change in number of Dems upper house
Indicator: Post-redistricting and Dems held unified legislature and governor during redistricting (year t–2)	.036 (.010)	.008 (.010)	3.81 (1.16)	0.24 (0.40)
Indicator: Post-redistricting and Reps held unified legislature and governor seat during redistricting (year t–2)	!.018 (.013)	!.031 (.013)	!2.06 (1.50)	!0.97 (0.52)
F-test: joint significance of redistricting with power variables (p-value)	10.37 (.0000)	4.16 (.0157)	9.20 (.0000)	2.65 (.0712)
Indicator: Dems held unified legislature and governor seat (year t–2)	!.047 (.006)	!.039 (.006)	!4.73 (0.70)	!1.45 (0.24)
Indicator: Reps held unified legislature and governor seat (year t–2)	.055 (.007)	.056 (.007)	5.81 (0.83)	2.03 (0.29)
Indicator: post redistricting	!.015 (.035)	.033 (.036)	!0.39 (4.04)	0.94 (1.40)
Years over which regression run	all years 1952-1995	all years 1952-1995	all years 1952-1995	all years 1952-1995
Number obs	2024	2024	2024	2024

Notes: Standard errors in parentheses. All regressions control for year and state effects, and include controls for state income per capita in $1982 and income squared; and state population and population squared. Nebraska is removed from the analysis, because it has a unicameral, non-partisan legislature. Observations for Minnesota are present only from 1973 on.

Table 5.5 Reduced form impact of institutional rules on state taxes and spending per capita

	Dependent Variable:				
	Total taxes per capita	Total government spending per capita	Total transfers payments per capita	Total family assistance per capita	Total Medicare payments per capita
Open primaries	–19.25 (8.03)	–18.24 (13.7)	31.8 (10.9)	2.73 (1.37)	11.7 (3.79)
Indicator: voter registration through vehicle agency	35.76 (7.10)	9.78 (11.5)	52.8 (10.1)	9.41 (1.27)	2.77 (3.36)
Indicator: voter registration possible on polling day or no registration necessary	120.38 (13.41)	114.7 (22.2)	0.77 (18.5)	20.5 (2.34)	–42.3 (7.00)
Indicator: restriction on corporate campaign contributions	–16.00 (5.33)	8.35 (8.54)	34.1 (7.57)	5.73 (0.95)	–1.84 (2.66)
F-test: joint significance institutional variables (*p*-value in parentheses)	28.46 (.0000)	7.55 (.0000)	13.87 (.0000)	35.94 (.0000)	13.03 (.0000)
Years over which regression run	All years 1958, 1960-97	All years 1958,1960-96	All years 1958, 1960-98	All years 1958, 1960-98	All years 1966-98
Number obs	1822	1781	1877	1877	1567

Notes: Standard errors in parentheses. All dependent variables are in 1982 dollars. All regressions control for year and state effects, and include controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982 and income squared; and state population and population squared. Omitted voter registration category is “conventional” registration. No registration was necessary in North Dakota from 1951 to 1998, and we have added that state to “registration possible on polling day.” Rules governing registration and voting have been lagged one or two periods, to reflect the conditions in place at the time of the election.

Table 6.1 Legislative composition and policy choice

	Dependent Variable:			
	Total taxes per capita	Total spending per capita	Family assistance per capita	Medicare spending per capita
Fraction Democrat in state lower house	78.71 (19.79)	101.38 (33.33)	28.78 (4.28)	85.97 (13.26)
Fraction Democrat in state upper house	10.49 (18.64)	2.49 (31.50)	9.03 (3.87)	-31.08 (11.47)
Indicator: Democrats control both lower and upper house	12.68 (5.51)	-1.99 (9.36)	3.88 (1.10)	-0.61 (3.08)
Indicator: Dem governor	-5.79 (3.20)	4.56 (5.39)	-0.78 (0.64)	-3.53 (1.77)
Party competition in legislature	-101.13 (41.37)	29.72 (70.10)	4.40 (9.74)	208.20 (28.83)
F-test: joint significance of party variables (p-value)	11.92 (.0000)	3.43 (.0043)	31.19 (.0000)	18.22 (.0000)
Years over which regression run	all years 1950-58 1960-97	all years 1950-58 1960-96	all years 1958, 1960-1998	all years 1966-1998
Number obs	2131	2091	1817	1495

Notes: Standard errors in parentheses. All regressions control for year and state effects, and include controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982 and income squared; and state population and population squared. Nebraska is removed from the analysis, because it has a unicameral, non-partisan legislature. Observations for Minnesota are present only from 1973 on.

Table 6.2 Ideology and policy choice

	Dependent Variable:			
	Total taxes per capita	Total spending per capita	Family assistance per capita	Medicare spending per capita
Fraction Democrat in state lower house	50.62 (21.48)	71.75 (38.25)	29.32 (4.43)	38.84 (10.35)
Fraction Democrat in state upper house	48.24 (18.62)	28.57 (33.14)	16.28 (3.84)	-4.99 (8.82)
Indicator: Dem governor	-1.69 (3.17)	-2.79 (5.66)	-.471 (.655)	-1.78 (1.39)
State citizens' COPE score	.439 (.185)	.620 (.328)	.198 (.038)	.116 (.083)
Years over which regression run	all years 1960-93	all years 1960-1993	all years 1960-1993	all years 1966-1993
Number obs	1576	1583	1583	1307

Notes: Standard errors in parentheses. All regressions control for year and state effects, and include controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982 and income squared; and state population and population squared. Nebraska is removed from the analysis, because it has a unicameral, non-partisan legislature. Observations for Minnesota are present only from 1973 on. All dollar-denominated variables are in 1982 dollars.

Table 6.3 Women's legislative representation and policy choice

	Dependent Variable:					
	Family assistance per capita ($1982)		Child support: immediate withholding upon delinquency		Child support: paternity establishment to age 18	
Fraction female state lower house	.025 (.008)	.038 (.009)	-.053 (.237)	-.321 (.311)	-.369 (.236)	-.555 (.310)
Fraction female state upper house	-.006 (.006)	-.011 (.008)	.712 (.188)	1.06 (0.26)	.311 (.187)	.883 (.255)
F-test joint significance female representation (p-value)	4.85 (.0000)	8.96 (.0000)	7.21 (.0000)	8.62 (.0002)	2.26 (.1044)	6.71 (.0000)
State citizens' COPE score	--	.000 (.000)	--	-.000 (.001)	--	.001 (.001)
Years over which regression run	all years 1975-98	all years 1975-93	all years 1975-97	all years 1975-93	all years 1975-97	all years 1975-93
Number obs	1152	912	1104	912	1104	912

Notes: Standard errors in parentheses. All regressions control for year and state effects, and include controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982 and income squared; and state population and population squared. All dollar-denominated variables are in 1982 dollars.

Table 7.1 Citizens' Initiatives and State Policy Choices

	Dependent Variable:			
	Total taxes per capita	Total income taxes per capita	Government spending per capita	Family Assistance per capita
	OLS with robust standard errors			
Indicator: State Allows Citizens' Initiatives	–30.78 (30.22)	–34.02 (32.96)	–35.00 (51.34)	–.995 (4.69)
	Random effects models			
Indicator: State Allows Citizens' Initiatives	–38.40 (11.82)	–51.98 (9.62)	20.57 (19.25)	–1.76 (2.11)
Years over which regression run	all years 1960-97	all years 1960-97	all years 1960-96	all years 1960-98
Number obs	1817	1824	1776	1872
	Regression on state means			
Indicator: State Allows Citizens' Initiatives	–28.50 (36.11)	–45.83 (37.20)	–74.42 (58.83)	–1.14 (5.26)
Number of observations	48	48	48	48

Notes: Standard errors in parentheses. All regressions include year indicators and controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982 and income squared; and state population and population squared. We do not include state fixed effects because only 4 states changed whether they allowed initiatives over the period 1960 to 1998. (These were: FL 1972, IL 1971, MS 1992, and WY 1968). For all regressions in panel one, we estimate robust standard errors, and allow for an unspecified pattern of correlation in the unobservables from the same state. Panel two allows for state random effects. Panel three estimates between state regressions on state means.

Table7.2 Political business cycles

	Dependent Variable:					
	State income per capita			State unemployment rate		
Indicator: gubernatorial election in *t+1*	–36.80 (197.53)	–5.41 (149.95)	16.48 (54.73)	–.122 (.145)	.011 (.154)	.012 (.113)
Indicator: gubernatorial election in *t+2*	161.98 (196.34)	48.17 (120.60)	25.95 (44.05)	–.050 (.145)	–.088 (.124)	–.081 (.090)
Indicator: gubernatorial election in *t+3*	108.00 (197.20)	26.44 (149.74)	7.07 (54.69)	.136 (.145)	.061 (.154)	.048 (.112)
F-test: joint significance of election variables (p-value)	0.44 (.7246)	0.08 (.9710)	0.13 (.9417)	1.15 (.3270)	0.35 (.7865)	0.51 (.6756)
Year effects?	no	yes	yes	no	yes	yes
State effects?	no	no	yes	no	no	yes
Number obs	1820	1820	1820	1606	1606	1606

Notes: Standard errors in parentheses.

Table 7.3 Binding Term Limits and Policy Choice

	Dependent Variable:			
	Total taxes per capita		Total spending per capita	
Indicator: Incumbent governor cannot stand for reelection	-6.40 (4.28)	1216.73 (514.32)	14.80 (6.73)	1968.81 (820.91)
Indicator: Incumbent cannot stand for reelection × year	--	-0.619 (0.260)	--	-0.990 (0.416)
Years over which regression run	all years 1950-97	all years 1950-97	all years 1950-1996	all years 1950-1996
Number obs	2249	2249	2208	2208

Notes: Standard errors in parentheses. All regressions control for year and state effects, and include controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982, income squared and cubed; and state population, population squared and cubed. All dollar-denominated variables are in 1982 dollars.

Table 7.4 Incumbent Discretion and Policy Choice

	Dependent Variable:							
	Total taxes per capita				Total spending per capita			
Indicator: Non-binding tax or expenditure limitation	8.13 (7.65)				-3.23 (12.01)			
Indicator: Potentially binding tax or expenditure limitation	36.50 (7.50)				20.11 (11.76)			
Supermajority needed to increase taxes		-56.48 (8.13)	-61.40 (8.75)	-61.67 (8.73)		-25.96 (13.16)	2.49 (15.47)	2.00 (15.44)
Indicator: Governor has a line item veto			-21.69 (12.89)	-15.75 (13.03)			11.77 (22.82)	22.95 (23.06)
Indicator: Governor's party is not that of the united majority party in the legislature			-11.40 (5.13)	8.56 (7.62)			-5.03 (5.52)	32.59 (13.48)
Line item veto× divided government				-23.91 (8.32)				-45.04 (14.72)
Years over which regression run	all years 1960-97	all years 1950-91	all years 1950-91	all years 1950-91	all years 1960-96	all years 1950-91	all years 1950-91	all years 1950-91
Number obs	1817	1898	1898	1898	1776	1905	1905	1905

Notes: Standard errors in parentheses. All regressions control for year and state effects, and include controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982, income squared and cubed; and state population, population squared and cubed. All dollar-denominated variables are in 1982 dollars. “Non-binding” tax and expenditure limitations are those that are either advisory or require only a simple legislative majority to amend or overrule.

Table 7.5 Institutional Rules and Legislative Control

	Dependent Variable: Total taxes per capita			
Open primaries	–19.37 (7.04)	--	--	--
Indicator: voter registration through vehicle agency	32.02 (6.86)	--	--	--
Indicator: voter registration possible on polling day or no registration necessary	100.68 (15.92)	--	--	--
Indicator: restriction on corporate campaign contributions	–18.10 (4.95)	--	--	--
F-test: joint significance institutional variables (*p*-value in parentheses)	20.22 (.0000)	--	--	--
IV: Party competition in legislature	--	–3434.63 (776.53)	--	–3376.73 (928.31)
IV: Democrats control both lower and upper house	--	--	165.27 (39.08)	8.44 (76.38)
F-test (see notes to table) (*p*-value in parentheses)	--	0.764 (.5483)	13.57 (.0000)	0.753 (.5561)
Years over which regression run	All years 1950-58, 1960-96			
Number obs	1925	1925	1925	1925

Notes: Standard errors in parentheses. All regressions control for year and state effects, and include controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982 and income squared; state population and population squared. All dollar-denominated variables are in 1982 dollars. In an auxiliary regression, we regress total taxes on all other right side variables and the predicted value of party competition in the legislature, where we use open primaries, voter registration through vehicle agency, voter registration on polling day, and restrictions on corporate contributions as instruments. The F-test in column 2 compares the fit of the regression using the predicted value to that in column 1, where the institutional rules are allowed to enter in an unrestricted fashion. Results in column 3 report an analogous comparison when an indicator that Democrats control both houses is instrumented using the institutional rules. We cannot reject that these institutional rules are affecting total taxes solely through their effect on party competition in the legislature.

Table 8.1 The long-run impact of legislative control on state institutions

	Indicator: Open Primaries			Indicator: Voter Registration through Vehicle Registration			Indicator: Day-of-Election Registration or No Registration			Indicator: Restrictions on Corporate Contributions		
F-test: lagged legislative variables (p-value)	2.19 (.0194)	--	--	0.69 (.7903)	--	--	0.34 (.9893)	--	--	1.70 (.0811)	--	--
F-test: lagged demographic variables (p-value)	--	5.55 (.0000)	--	--	2.77 (.0034)	--	--	0.48 (.9447)	--	--	1.81 (.0593)	--
F-test: lags in both legislative and demographic variables	--	--	6.12 (.0000)	--	--	3.96 (.0000)	--	--	0.40 (.9961)	--	--	2.28 (.0053)
Number of Observations	829	861	829	926	960	926	926	960	926	861	894	861
Years	even years 1960-1990, 1996, 1998			even years 1960-98			even years 1960-98			even years 1960-1998		

Notes: All regressions control for year and state effects, and include controls for the proportion of population aged 65 and above; the proportion of population aged 5 to 17; state income per capita in $1982 and income squared; state population and population squared. The *F*-test for lagged legislative variables is a test for the joint significance of the following variables: the proportion of the lower house held by Democrats in years t!4,t!6,t!8, and t!10; the proportion of the state upper house held by Democrats in years t!4, t!6, t!8, and t!10; indicators that the Democrats controlled both houses in t!4, t!6, t!8, and t!10; and our measure of party competition in the legislature in t!4, t!6, t!8, and t!10. The F-test for lagged demographic variables is a test for the joint significance of the following variables: state income in periods t-4, t-6, t-8 and t-10; state population in periods t-4, t-6, t-8 and t-10; proportion of population aged 65 and above in t-4, t-6,t-8 and t-10; and proportion aged 5 to 17 in t-4, t-6, t-8, and t-10. Results reported are were estimated using robust standard errors.

Figure 1. The changing impact of term limits
on total taxes per capita